UNIVERSITY OF CALIFORNIA,
IRVINE

Understanding Societal Investments in Children

DISSERTATION

submitted in partial satisfaction of the requirements
for the degree of

DOCTOR OF PHILOSOPHY

in Education

by

Michelle Spiegel

Dissertation Committee:
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DEDICATION

To

Nana and Poppa,

my biggest supporters,
always,

I love you.

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Hill, Z., **Spiegel, M.**, Gennetian, L., Hamer, K. A., Brotman, L., & Dawson-McClure, S. (2021). Behavioral economics and parent participation in an evidence-based parenting program at scale. *Prevention Science*, *22*(7), 891-902. https://doi.org/10.1007/s11121-021-01249-0

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ABSTRACT OF THE DISSERTATION

Understanding Societal Investments in Children

By

Michelle Spiegel

Doctor of Philosophy in Education

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Distinguished Professor Greg J. Duncan, Chair

The support that society provides to children from low-income families plays a critical role in helping them grow and thrive. This support encompasses a wide range of policies, including increased funding for education and direct assistance to families in the form of cash or in-kind benefits. While it is well established that these forms of support significantly improve child outcomes, questions remain about the most effective strategies for targeting, distributing, and designing programs to maximize the impact of these resources.

This dissertation focuses on deepening our understanding of three specific types of social supports for low-income children: education policies that target resources to schools serving low-income students, social policies that provide unconditional cash transfers to low-income families, and policies that focus on improving the environmental conditions of schools.

First, many education policies depend on valid measures of school economic disadvantage. Recent research raises questions about the validity of commonly used free-or-reduced-price-lunch measures, particularly in light of the increasing availability of

universal free school meals. The first study links confidential federal tax return data and program participation data housed at the U.S. Census Bureau to examine the validity of several measures of school economic disadvantage. Results suggest that direct certification measures provide the best widely available measure, both over time and across the distribution of school poverty.

Second, parental spending on children is important for child development. Using data from a randomized control trial of an unconditional cash transfer to low-income mothers of young children, the second study examines the extent to which the cash transfer is spent on goods directly related to children, relative to other sources of household income. I find that the unconditional cash transfer is more likely to be spent on children than any other household income source, including mothers' earned income alone. The results suggest that money in the household is differentiated for spending on children.

Finally, no level of lead is safe in a child's blood. Moreover, low-income children have higher blood lead levels than non-economically disadvantaged children. Most research and policy has focused on lead abatement in home environments. However, researchers estimate that 73 percent of schools have lead in the drinking water. The third study uses school water lead data linked to education administrative records to estimate a causal effect of school water lead exposure on educational outcomes. The results suggests that water lead exposure may negatively affect students, although the effect is sensitive to model specification. In addition, students' exposure to lead in schools is curiously correlated with students' prior achievement, making a causal effect of lead in schools unclear.

My dissertation concludes with a discussion of themes and lessons from the three studies for improving social policies to support low-income families.

INTRODUCTION

The United States invests in schools and families in the hope of improving the life chances of low-income children. Indeed, government spending on low-income children improves educational and health outcomes, and leads to upward economic mobility (Hoynes, Schanzenbach, Jackson, Johnson, Persico, 2016; Mayer and Lopoo, 2008; Angrist, Autor, and Pallais, 2022; Hoynes, 2019). Recent events raise questions about the implementation and effectiveness of specific societal expenditures on children in schools and families. The three chapters in this dissertation address concerns around implementation and design of three important societal expenditures on children.

The first study focuses on a question relevant to a recent policy change to the National School Lunch Program (NSLP), which was established in 1946, to provide free or -reduced-price school meals to low-income children. The Community Eligibility Provision (CEP) to the NSLP, implemented nationwide in 2015, allows schools and school districts to offer universal free meals to all students regardless of their families' incomes, if 40 percent or more of enrolled students participate in means-tested programs (e.g., SNAP). While beneficial to student educational outcomes (Radsky, Domina, Clark, Bhaskar, 2022), CEP further undermines an already flawed measure of school economic disadvantage - an indicator of free or reduced-price lunch enrollment - that is central to targeting resources to schools (Domina et al., 2018; Harwell and LeBeau, 2010). As a result, policymakers and researchers need to construct – and evaluate the validity of – alternative measures of school economic disadvantage to ensure that resources designated for low-income students reach the schools that serve them (Greenberg et al, 2021). The first study broadly asks: How well do different measures capture school economic disadvantage?

The second study focuses on an issue that in part came to the fore during the COVID-19 pandemic. The pandemic ushered in an unprecedent level - and type - of cash assistance for families (Bitler, Hoynes, Schanzenbach, 2023). Policies such as child tax credits, economic impact payments, and expansions to unemployment insurance provided cash to families through multiple disbursement channels, including through the Internal Revenue Service and through existing social safety net mechanisms. While many cash transfer policies are discontinued (e.g., economic impact payments), the multiple and overlapping policies focus attention on whether - and how - increasing income for children in low-income families improves child outcomes. In particular, the variety of cash assistance offered during the COVID-19 pandemic raises the question of whether specific mechanisms for disbursing cash to families may be important in determining whether cash improves child well-being. The second study asks: Are all household resources treated equally for spending on children in low-income families?

The final study sheds light on a question relevant to recent infrastructure policy, which includes substantial funding for testing and removing lead from water in school drinking fountains. President Biden's 2023 infrastructure bill dedicates an unprecedented $15 billion to removing lead from drinking water in educational facilities (Department of Health and Human Services, 2023). Much of the research on lead exposure and effects focuses on sources of exposure from the home environment. There is a dearth of evidence on the effect of lead exposure in schools. Proposed expenditures to address lead in school drinking water raise questions about whether and to what extent lead exposure in schools harms children. The third study asks: Does exposure to lead in school drinking water harm students?

Chapter 1: Measuring school economic disadvantage

While scholars disagree about the role that public schools play in the production and reproduction of economic inequality (Labaree, 1997), efforts to narrow educational inequalities have animated American educational policy and practice for more than the past half century (Coleman, 1966; Ladd, 2012). One longstanding effort to narrow inequalities has been to provide additional funding to school districts that serve higher proportions of economically disadvantaged students. In Oregon, for example, schools receive additional funding based on the percentage of economically disadvantaged students (State of Oregon, 2021). California, as another example, applies a multiplier of 1.20 to the base per-pupil amount for economically disadvantaged students and provides an additional grant for districts where at least 55% of students are economically disadvantaged (California Department of Education, 2021). However, recent research raises questions about the validity of the measure of school economic disadvantage that is central to the allocation of resources to schools - data from the NSLP on free or reduced-price lunch enrollment.

The first study provides new information on the validity of measures of economic disadvantage by comparing different measures to a benchmark measure. The benchmark measure is constructed using highly confidential IRS tax records, which, because of their breadth and accuracy, provide the best benchmark available in the United States against which we can compare the various measures. I use confidential federal tax return data and program participation data housed at the U.S. Census Bureau linked with education administrative data to examine the validity of several measures of school economic disadvantage.

I find that direct certification – a measure that uses students' participation in means-tested programs - provides the best widely available measure, both over time and across the distribution of school poverty. In addition, I find that neighborhood-based measures do a poor job of capturing school economic disadvantage. Finally, I find that the quality of free or reduced-price lunch measures varies and that their accuracy deteriorates over time.

Chapter 2: *Which* money matters for spending on children in low-income households?

Research consistently documents the positive effects of income on children's development and life outcomes (Brooks-Gunn and Duncan, 1997). The study of cash transfer policies to augment income often relies on, albeit implicitly, a neoclassical model of the household in which all household money is assumed to be fungible (Samuelson, 1956, Becker, 1981). However, decades of literature show that household income is not fungible (Hastings and Shapiro, 2018; Hoynes and Schanzenbach, 2009). Policymakers and researchers seeking to optimize the effects of cash transfers are particularly interested in how these policies lead to increased parental investment in children (Duncan, Brooks-Gunn, Klebanov, 1994; Brooks-Gunn, Duncan, 1997). The second study begins with the understanding that not all money is equal and asks how an increase in family income translates into improved child outcomes.

I use data from the Baby's First Years study, a randomized control trial of a labeled cash transfer to low-income mothers with young children in the United States, to test for differences in the marginal dollar spent on child-related goods from the labeled cash transfer, mothers' earned income, and all other household income. I find evidence that spending on babies depends on the source of income, with the highest spending on babies from the cash transfer compared to both mothers' earned income alone and all other household income combined. I also find that mothers' earned income is more likely to be spent on infants than all other household income combined.

The higher spending on infants from the cash transfer – above the spending on infants from mothers' earned income – suggests that resource control cannot explain the spending patterns. Instead, the results suggest an effect of spending on children that is due to a feature of the cash transfer alone. The findings suggest that cash transfer policies aimed at promoting child development may benefit from taking into account the nuanced ways in which families view and use money.

Chapter 3: Does exposure to lead in schools harm students?

No level of lead in children's blood is safe. Yet, an estimated 1.2 million U.S. children have blood lead levels twice the Center for Disease Control's threshold for medical action (5 micrograms per deciliter as of 2012) (Madrigal et al., 2017; CDC, 2013). Researchers estimate that lead is present in the water of 73 percent of U.S. schools (Patel and Hampton, 2011). Most research on lead exposure examines exposure in the home environment (Bolser and Holman, 2019), resulting in a lack of evidence on the causal effect of lead exposure in schools.

In the third study, I construct a measure of student water lead exposure by linking fixture-level water lead measurements from classroom water fountains to students in classrooms. Using this measure, I estimate the effect of exposure on students' math and reading scores, absenteeism, and likelihood of suspension, comparing students to themselves in years when they were exposed to higher and lower levels of lead, and comparing students in a school who were exposed to higher levels of lead than other students in the same school. These approaches are arguably improvements over regression control-based approaches.

I find that higher water lead exposure results in lower math test scores, but not reading scores, absences, or suspensions. However, the negative effect on math is not robust to alternative specifications. I also find that students exposed to higher water lead levels have *higher* reading scores, and small but insignificant higher math scores, absenteeism, and suspensions. Finally, I discuss conceptual reasons for the counterintuitive results, and where data are available, empirically test mechanisms that might explain the counterintuitive findings.

Conclusion

The studies outlined above deepen understanding around pressing policy issues for supporting children from low-income families in their school and home environments. While each study focuses on different policy areas, together, they demonstrate the importance of paying close attention to measurement and policy implementation strategies, and of using inter-disciplinary knowledge, to design more effective societal investments in children.

CHAPTER 1

Measuring School Economic Disadvantage

We live in an era of profound economic inequality and large gaps in academic achievement between low-income and non-low-income students (Reardon 2011; Hashim et al., 2020; Bailey and Dynarski 2011; Duncan, Kalil, and Ziol-Guest 2017; Ziol-Guest and Lee 2016). Long standing efforts have sought to improve the educational resources and opportunities available to economically disadvantaged students. Many of these compensatory strategies, including Title I of the federal *Elementary and Secondary Education Act* and a variety of state and district-level programs, earmark supplementary funds and educational programs to schools and districts that educate relatively high proportions of low-income students (Greenberg, Blagg, & Rainer, 2019; U.S. Department of Education, 2019). For example, Oregon's state-level school finance system provides additional funding for students who are economically disadvantaged by applying a multiplier of 1.25 to the base per pupil amount for these students (State of Oregon, 2020). Similarly, California applies a multiplier of 1.20 to the base per-pupil amount for economically disadvantaged students and provides an additional grant for districts where at least 55 percent of students are economically disadvantaged (California Department of Education, 2021).

To operate efficiently and equitably, compensatory school funding programs require valid measures of the concentration of economic disadvantage at the district and school levels (Marar, 2020). However, substantial questions exist about the degree to which the standard existing measure – the proportion of students enrolled in means-tested free or reduced-price lunch (FRPL) via the National School Lunch Program (NSLP) – can accurately capture economic disadvantage in schools (Domina et al. 2018; Michelmore & Dynarski 2017; Harwell & LeBeau 2010).

In this paper, we use confidential data housed at the U.S. Census Bureau to analyze the validity of various measures of school economic disadvantage that are currently in use or are recently constructed. Specifically, we link federal tax return and program participation microdata to administrative public school enrollment data for the state of Oregon. We then construct a school economic disadvantage measure that, thanks to the breadth and accuracy of federal tax data, in particular, offers a benchmark against which we compare an assortment of candidate measures of school economic disadvantage. Specifically, the benchmark captures the proportion of students enrolled in each Oregon public school who were either enrolled in SNAP or whose family reported a household income of less than 185 percent of the federal poverty level on their tax return.

We assess the following policy- and research-relevant candidate measures of school poverty: two different sources of FRPL enrollment rates, so-called direct certification rates (which measure the proportion of students who are enrolled in SNAP and other means-tested programs targeted at low-income families), aggregated data describing the rate of poverty in the neighborhoods in which students live, and two experimental measures (one from the Urban Institute and one from the National Center for Education Statistics). We posit that any measure used to distribute school finances aimed at compensating for school poverty ought to mirror variation in poverty rates over time and across schools relative to the benchmark in an unbiased fashion. As such, our analyses compare temporal trends for our benchmark measure and available candidate measures; examine the cross-school correlations between these measures; and focus on the extent to which the benchmark and candidate measures correspond across the distribution of school poverty.

Our analyses reveal that, in Oregon, many widely available measures of school poverty correlate reasonably highly with our school economic disadvantage benchmark measure. However, each of these measures is subject to important shortcomings. We find that the validity of widely used FRPL enrollment rate depends on how these data account for a growing number of schools that participate in universal free meals programs, the most notable of which is the Community Eligibility Provision. By contrast, direct certification rates correlate highly with benchmark economic disadvantage rates, as direct certification rates reflect participation in means-tested programs distinct from NSLP. Not all states make this measure available for public use and fewer use it for policy purposes. But our analyses suggest that, among measures with widespread availability, it provides the most valid measure of economic disadvantage at the school level.

Other existing alternatives, including measures that use population-based estimates of household poverty rates or income levels of households living near students or their schools, are less promising. Such measures also preclude understanding which specific students are in poverty, which, though not a focus of our analyses, is an important feature for research efforts that investigate or attempt to control for within-school variation in economic status, including, as examples, the literature on peer effects (see Sacerdote, 2011), within-school income segregation (see Dalane and Marcotte, 2022), and teacher effects literatures (see Jackson, Rockoff, Staiger, 2014).

Defining the construct: "Students from low-income families"

Our focus in this paper is on the valid measurement of student economic disadvantage rates at the school level. While other measures of student disadvantage and academic need are clearly important, we focus on economic disadvantage (and the closely related constructs of poverty and low-income) because of the central role that it has long played in U.S. educational policy. Over the last several decades, policymakers at the federal, state, and local levels have authorized a wide array of policies that direct supplementary funds to public schools and districts based on the concentration of economically-disadvantaged students.[1] These policies conceptualize economic disadvantage as a unidimensional and dichotomous construct – a student is either economically disadvantaged or not, based on their household income. In line with this policy tradition, we focus largely on school economic disadvantage measures constructed from dichotomous indicators based on household income.

The largest and most influential policy aimed at providing additional support for economically disadvantaged students is Title I, Part A of the 1965 *Elementary and Secondary Education Act* (ESEA). Title I allocates supplemental federal educational funds to "local educational agencies serving areas with concentrations of children from low-income families to expand and improve their educational programs." The policy's language notes the "special educational needs of children of low-income families and the impact that

[1] Throughout this paper, we use the terms "economic disadvantage" and "low-income" interchangeably. At several points in the manuscript, we make specific reference to the federal poverty line, a construct that is defined around a family's ability to secure a minimal standard of living. Since this construct is dated and does not account for regional variation in costs of living, scholars debate its validity (Hauser, 1994). Our discussion does not take a stand on this issue, but our use of the terms "economic disadvantage" and "low-income" rely on thresholds defined around the federal poverty line.

concentrations of low-income families have on the ability of local educational agencies to support adequate educational programs" (Public Law 89-10-Apr. 11 1965, p. 27).

Currently 35 states also provide additional funding for pre-primary, elementary, secondary, and higher education targeting low-income students (Edbuild, n.d.a). Furthermore, many of the nation's largest districts have implemented weighted student funding formulae that inform the distribution of district resources across schools. Although existing district-level systems consider a range of student attributes, including the concentration of English Language Learners and special education students, most include student poverty as a key factor for targeting compensatory resources (Roza, Hagan & Anderson 2021; Edbuild, n.d.b). These federal-, state-, and district-level policies vary in many regards. Different policies operationalize the notion of economic disadvantage or poverty in different ways, with some emphasizing the federal poverty line and others emphasizing different income-based thresholds. Policies also differ in the degree to which they supplement allocations based on student poverty or other forms of economic disadvantage and the range of other characteristics that inform resource allocations (EdBuild, n.d.b; Roza, Hagan & Anderson 2021). Nevertheless, common to each of these policies is the notion that school finance mechanisms can offset disparities associated with socioeconomic inequality by directing funds toward the schools and districts that educate students from low-income families.

Measures of school-level economic disadvantage

In this section, we provide an overview of strategies currently being used for the measurement of student economic disadvantage at the school level in U.S. policy contexts as well as several promising alternative strategies.[2]

FRPL enrollment

To measure economic disadvantage at the school level, policymakers typically rely on data produced in the administration of means-tested programs designed to benefit children living in low-income households. Historically, the most widely used measure is based on the proportion of students enrolled in FRPL, often referred to as the FRPL rate. Currently approximately 30 U.S. states use FRPL data to measure school economic disadvantage for funding purposes (EdBuild, n.d.b).

Scholars and practitioners have long known that FRPL rates provide error-prone measures of school economic disadvantage (Domina et al. 2018; Harwell and LeBeau, 2010). Historically, students enroll in FRPL when their households self-report income at or below 185 percent of the federal poverty line. This self-reporting process produces two-sided error. Some students from families with incomes above 185 percent of the federal poverty line enroll in FRPL; other students from low-income families do not enroll (Domina et al. 2018). Since administrative burdens for families completing NSLP applications, concerns about being stigmatized as poor at school, and governmental distrust likely vary

[2] Before summarizing these school-level measures, we note that under Title I of the ESEA, the federal government allocates supplementary funds to districts based on data generated by the U.S. Census Bureau's Small Area Income and Poverty Estimates Program (SAIPE). SAIPE uses state and county-level American Community Survey (ACS) data as well as summary data from federal income tax returns, SNAP benefits data, data from the decennial Census, and other sources to estimate the percent of children ages 5-17 living in the district's geographic boundaries whose household income falls below the poverty line using (U.S. Census Bureau 2021). Since these measures are largely derived from data reported by families sampled to participate in the ACS, they are not available at the school level. As such, the SAIPE data are beyond this paper's analytic scope.

across schools and communities (USDA, 2022, Richman, 2019, Domina et al. 2018; ERS, 2006; Harwell and LeBeau, 2010), the measurement error in the FRPL rate likely varies by school location and enrollment demographics.

Recent efforts to improve student access to school meals may have further exacerbated concerns regarding the ability of FRPL data to measure economic disadvantage (Cookson, 2020; Koedel & Parsons 2021). Specifically, Provisions 1, 2, and 3 of the National School Lunch Program (implemented in the 1980s as alternative provisions to the normal requirements for annual determinations of eligibility for FRPL) and the Community Eligibility Program (CEP, available nationally in 2015) permit schools to provide free lunch widely, and directly or indirectly reduce data collection obligations. During the 2019-20 school year, approximately 69 percent of eligible schools participated in CEP (FRAC, 2020). Some schools continue to report traditional FRPL enrollment data after enrolling in CEP, while others stop reporting or report that all students are enrolled (Greenberg et al. 2019). The pandemic potentially brought additional changes to FRPL enrollment data as schools struggled to collect forms from eligible families and federal pandemic relief efforts allowed schools across the country to offer free meals to all students beginning in the spring 2020 (Burnette II, 2020). While the pandemic relief effort ended in the fall of 2022, it is not clear when and to what extent schools and districts will resume FRPL data collection.

Direct Certification

To reduce the administrative burden associated with enrolling in FRPL, the USDA authorized districts to directly certify children from households enrolled in the Supplemental Nutrition Assistance Program (SNAP), Temporary Aid for Needy Families (TANF) or other means-tested programs for free lunch beginning the 1990s and early

2000s. More recently, some five states piloted a program to incorporate Medicaid data for direct certification (Hulsey et al., 2015) in the 2012-2013 school year. This program continues to expand, with some 14 additional states – including Oregon - planning to incorporate Medicaid to the direct certification process in the 2023-2024 school year for both free and reduced-price meal eligibility (USDA, 2023).

Many states and school districts now use the proportion of students who enroll in FRPL via direct certification either as a supplement to or a replacement for traditional FRPL rates for resource allocation. To date, limited evidence is available regarding the properties of direct certification as a measure of school economic disadvantage. These data are not strictly comparable to FRPL rates, since the criteria for receiving benefits via SNAP and TANF are more restrictive than the criteria for enrolling in FRPL directly.[3] Furthermore, direct certification rates have both potential strengths and weaknesses relative to NSLP data as a measure of school economic disadvantage. Since families must provide income documentation to enroll in SNAP and TANF, direct certification rate data may be less subject to some forms of bias than FRPL enrollment data. At the same time, the policies and administrative practices that govern the process of enrolling in SNAP, TANF, and Medicaid vary across states, within states, and over time (Ganong and Liebman, 2018; Dickert-Conlin et al., 2021). This variation likely leads to both geographic and temporal variation in the characteristics of families who are captured in direct certification. Direct certification rates may substantially understate economic disadvantage rates in contexts where program enrollment processes place heavy burdens on potential beneficiaries relative to contexts

[3] Families must demonstrate household income less than 130 percent of poverty and have less than $2250 in cash savings to enroll in SNAP. TANF eligibility rules vary across states but are typically more restrictive than SNAP eligibility rules.

where administrative burdens associated with program enrollment are less pronounced (Herd & Moynihan 2018; Heinrich et al. 2022). Furthermore, direct certification data may understate economic disadvantage in schools serving immigrant, rural, and other communities in which income-qualified families are relatively less likely to enroll in means-tested programs.

Newly introduced alternative measures

To address the shortcomings associated with measures of economic disadvantage based on program participation data, researchers and policymakers have begun to experiment with alternative approaches. Some states and districts have begun to construct school-level measures of student neighborhood economic disadvantage using ACS data (see Morath, 2019 for an example in Texas and see Maryland State Department of Education, 2022). While these data do not directly measure the household income of students enrolled in schools, they use student home address data to identify the Census block groups or tracts in which students live. Then, publicly available data on neighborhood economic conditions are used to proxy for students' own economic circumstances. If students' economic circumstances correlate highly with those of their neighbors, aggregating these measures to the school level may accurately approximate school low-income rates.

The Urban Institute recently developed a measure of school economic disadvantage that builds on two sources of publicly available data: the school-level direct certification or FRPL data available via the National Center of Education Statistics' Common Core of Data (CCD) and the Census-constructed district-level Small Area Income and Poverty Estimates Program (SAIPE) (Gutierrez, Blagg, and Chingos, 2022). This measure, known as Model Estimates of Poverty in Schools (MEPS), uses a linear mixed effects model to predict school-level estimates of economic disadvantage based on average household economic disadvantage rates for families with children in the districts in which schools are located. MEPS makes two assumptions as it attempts to address error associated with FRPL or direct certification data: (1) that school-level biases in FRPL or direct certification data cluster at the district level (as opposed to varying across schools within the same district), and (2) that SAIPE data capture public school student poverty rates at the district level, despite being based on a sample that is not designed to specifically generalize to the subset of families enrolling children in public schools. SAIPE data captures the proportion of school-aged children below 100 percent of the federal poverty line within a school district, which is a lower threshold of poverty than other typical definitions. Because of this, MEPS identifies fewer students as economically disadvantaged compared with other measures.

The National Center for Education Statistics (NCES) reports School Neighborhood Poverty (SNP) statistics. This measure uses ACS data to estimate the average household income relative to the poverty line for the households with school-aged children who live in close physical proximity to each school's campus (Geverdt and Nixon, 2018). Recent work by Fazlul et al. (2021) provides a transformation of the SNP measure designed to better match the dichotomous conception of poverty embedded in policy and seeks to capture the share of students at or below 130 percent of the federal poverty line for each school. To the degree that the economic circumstances of households located near a school's campus are representative of the economic circumstances of all the households in which enrolled students live, these SNP-based measures can provide a proxy for school economic disadvantage (Fazlul et al. 2021). This may be a problematic assumption, particularly for schools that serve geographically dispersed student bodies.[4]

Current study

Our analyses investigate the validity of various measures of student economic disadvantage at the school level, with the goal of identifying measures that are both reliable and widely available for use in educational policy and practice. Our argument-based approach to validation (Kane, 2006, 2012) proceeds by establishing a benchmark measure and then assessing alternative measures against our benchmark.

Specifically, our analyses address the following research questions:

1. To what degree do available measures of economic disadvantage track temporal trends in economic disadvantage for students enrolled in Oregon public schools?

2. To what degree do available school-level measures capture variation in economic disadvantage across Oregon public schools, and how has this changed over time?

3. To what degree do available measures capture variation across the distribution of school economic disadvantage? Do candidate measures over- or under-estimate school economic disadvantage rates in schools enrolling high or low concentrations of students from low-income families?

Data and measures

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[4] Through an additional program called BlindSIDE, NCES has partnered with states to provide policymakers with a spatially interpolated school poverty measure based on student addresses, rather than school addresses (Institute of Education Sciences, 2021). While we are unable to assess the properties of this proposed measure here, we note that it has a greater degree of face validity than the existing SNP measure. In FY2019, multiple State Departments of Education received additional funds from the State Longitudinal Data Systems Grant Program to help the National Center for Education Statistics to test this new Spatially Interpolated Demographic Estimates (SIDE) poverty measure. States participating in the testing of the SIDE measure collected and cleaned student addresses and obtained poverty estimates using the SIDE program and compared this measure with existing measures of poverty and other academic indicators, including FRPL data and test scores.

We link student-level administrative records from the Oregon Department of Education (ODE) for the school years 2009-10 through 2016-17 with administrative records stored at the U.S. Census Bureau. These data are linked using protected identification keys (PIKs) assigned via the Census's Person Identification Validation System.[5] The overwhelming majority of students receive PIKs: in the 2016-17 school year, we link over 95% of K-12 students in the ODE data to PIKs.

Using PIKs, we link students in the ODE data to 1) the Oregon SNAP enrollment records for each school year and 2) the IRS form 1040 records (tax returns) in which they are claimed as dependents for the most recent and two prior calendar years (spanning from 2007 to 2016). Details about the 9 measures used in the analyses are included in Table 1.1 and described below. We refer to all measures using their bolded name as listed in Table 1.1.

If a student does not receive a PIK, we cannot link them to IRS and SNAP data. We exclude students without PIKs from our calculations of school-level economic disadvantage rates. Broadly speaking, students might not be assigned a PIK due to missing or erroneous information in ODE administrative records for the student, or because the student's information is not present in the reference file used to assign PIKs, which means a PIK has never been created for them. By excluding students without PIKs, we assume that students are missing income at random at the school-level: that is, we assume that students whose economic disadvantage we do not observe due to missing PIKs have, on average, the economic disadvantage rate of the school. The same missing income at random assumption

[5] For a detailed description of the Person Identification Validation System, see Wagner and Layne (2014).

is used for students who have PIKs but do not appear in IRS 1040 records. Clark and Bhaskar (2023) document that this assumption is more plausible than assuming students with missing values are categorically low-income.

Benchmark Measures

Benchmark (SNAP/IRS)

Our benchmark measure of school economic disadvantage defines students as economically disadvantaged in a given year if they meet one of two tests: SNAP enrollment or family income less than 185 percent of the federal poverty line. Students are considered economically disadvantaged via SNAP receipt if they were enrolled in SNAP at any time during the school year. For students who were not enrolled in SNAP, the income determination is made using IRS 1040 records from the most recent calendar year (e.g., 2016 IRS records for the 2016-17 school year). Specifically, we identify students as dependents in their families' tax returns and calculate their ratio of income to poverty by dividing their adjusted gross income by the poverty line for their family size (also derived from the 1040). For students who are not enrolled in SNAP or claimed on a 1040 in the most recent calendar year, we check 1040 records from the prior two calendar years and use this income information. In summary, we deem the following students economically disadvantaged in the 2016-17 school year: a student enrolled in SNAP at any time during the school year regardless of their family's tax-reported income; a student not enrolled in SNAP whose family's 2016 income-to-poverty ratio is below 185 percent of the federal poverty line; and a student not enrolled in SNAP and not linked to a 2016 tax return, but whose family met the income standard in their 2015 tax return (or, if that is also missing, their 2014 tax return).

Eight percent of Oregon students in the 2016-17 school year are missing this measure. As with all the microdata-derived measures used in this paper, we use listwise deletion for these missing cases, implicitly assuming they are missing-at-random at the school level.

Our benchmark is interpreted as the proportion of students who are economically disadvantaged, with 185 percent federal poverty as our target threshold[6]

Benchmark (IRS)

We construct an alternative version of our benchmark measure that uses only IRS 1040 data on family income and size from the most recent calendar year to measure the proportion of students with family income below 185 percent of the federal poverty line. This measure systematically excludes students whose families did not recently file tax returns, but it offers a measure that is independent of program participation, and one that states may be able to replicate with state level tax data more easily. This measure results in some 15.4 percent of students without an income value, and we, once again, assume students are missing-at-random at the school level. While we prefer the SNAP/IRS benchmark because it captures more students overall, and more low-income students in non-tax-filing households, in particular, it is also useful to have a benchmark that is not mechanically correlated with our SNAP-derived direct certification measure.

Traditional FRPL Measures

[6] SNAP eligibility falls at 130% federal poverty, but students occasionally only briefly enrolled in SNAP during the school year may have income more than 185% federal poverty. However, our analysis suggests that these students overwhelmingly fall below the 185% threshold, so this measure still targets that threshold though it may not perfectly comply with it.

Two distinct measures of school-level economic disadvantage rates based on FRPL enrollment are currently available for Oregon public schools. Our analyses use both measures, since they highlight potentially important differences in how FRPL rates address measurement questions associated with CEP and other programs that offer free school meals to all students at participating schools.

CCD FRPL

The NCES Common Core of Data (CCD) collects FRPL rates for Oregon public schools, which we refer to as "CCD FRPL". NCES state coordinators responsible for submitting school economic disadvantage information to the CCD receive guidance to "*estimate the count of students by multiplying current year membership by the percentage of eligible students in the most recent year for which the school collected that information" (U.S. Department of Education, 2021; p.10)*. Thus, since the introduction of CEP, some schools enrolled in CEP have continued to report their pre-CEP FRPL rate to the CCD in lieu of an updated rate.

ODE Economic Disadvantage

The ODE data flags students as "economically disadvantaged" based on their FRPL enrollment and other factors, including homeless children, as determined by the McKinney-Vento Homelessness Assistance Act (enacted in 2001). In 2015, certain schools participating in CEP or other universal meals programs show at or near 100 percent of students as "economically disadvantaged". Thus, our ODE Economic Disadvantage variable is the proportion of students in each school designated economically disadvantaged in the ODE microdata.

Direct Certification Measure

SNAP

Although many states have started publishing school-level direct certification rates, Oregon did not during the period of study. Thus, we construct an estimate of the direct certification rates for Oregon schools using Oregon SNAP data housed at the U.S. Census Bureau. Most of the SNAP administrative data that the U.S. Census Bureau possesses have been acquired since 2012 as part of the *Census-FNS-ERS Joint Project*, a cross-agency collaboration between the State SNAP agencies, the U.S. Census Bureau, and USDA's Food and Nutrition Service (FNS) and Economic Research Service (ERS).[7] This measure is simply the proportion of students at each school that are enrolled in SNAP at any point during the school year.

An important distinction between our direct certification proxy and actual measures of direct certification is that different states use different means-tested programs – and typically more than one – to directly certify students. They also have different methods for matching students to direct certification records. In Oregon, Medicaid data will be added to direct certification beginning in the 2023-2024 school year. We are not able to assess the quality of Oregon's matching approach nor the effect of the introduction of Medicaid data on the quality of the approach.[8]

Alternative Candidate Measures

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[7] For more information about the *Census-FNS-ERS Joint Project*, see the webpage: https://www.ers.usda.gov/topics/food-nutrition-assistance/food-assistance-data-collaborative-research-programs/snap-and-wic-administrative-data/

[8] Using SNAP enrollment directly instead of state-reported direct certification data avoids potential issues that arise due to matching issues in state administrative data systems. In school years 2017-18 and 2018-2019, the USDA estimates that 98 percent of SNAP-participant children were directly certified for free school meals. This rate was lower in Oregon, where 88 percent of children enrolled in SNAP were directly certified for free school lunch (USDA, 2021). Broadly speaking, eligible students might not be directly certified due to issues in the data linkage procedures between SNAP and NLSP staff, or due to the timing of direct certification efforts compared with SNAP enrollment.

ACS Block Group

We construct an ACS-based measure by linking students in each school to their residential census block group, then assigning them the economic disadvantage rate (proportion of households with income less than 185 percent of the federal poverty line according to the ACS 5-year estimate) of their block group.[9] We average these rates across students to arrive at a school-level measure of economic disadvantage.

Urban Institute MEPS

We utilize the publicly available measures from the Urban Institute's website, including the Model Estimates of Poverty in Schools (MEPS) and the modified MEPS. The Urban Institute suggests that analysts use the original MEPS when looking at schools of varying enrollment sizes. We follow the Urban Institute's suggestion and report findings from the original MEPS in our main analyses and provide information about the modified MEPS in footnotes.

NCES-SNP

As described above, SNP measures the household income-to-poverty ratio for ACS respondent households living near schools. We include this measure for the year 2016-2017.

Transformed NCES-SNP.

Fazlul et al. (2021) propose a transformation of the SNP designed to better match the dichotomous conception of economic disadvantage embedded in policy and other

[9] There are different potential options for translating ACS income and poverty measures into school economic disadvantage rates. We found this approach superior to designating students in a binary fashion as either low or high income based on their neighborhood low-income rate or neighborhood average income.

widely used measures. We reproduce this transformation and report results of this transformed variable as well as the untransformed version.[10]

Sample of Schools

Table 1.2 provides descriptive statistics for the analytic sample. To construct a panel of schools comparable across all years of our study, we restrict the sample to schools (a) that appear in ODE administrative data in all 8 years of our study, and (b) for which all our candidate measures are available. All the analyses in this paper are restricted to the approximately 1,060 schools in our analytic sample. We lose approximately 300 schools with small enrollments driven in large part by instances for which there is no school-level data available in NCES data sources like the CCD and SNP. Our analyses consider only measures that are widely available throughout the state, but we note that some measures are not available for all schools. There are a small number of low-enrollment Oregon public schools in which no enrolled students are linked to data with which we construct the benchmark measure (they may be missing a PIK or, if not, do not appear in form 1040 or SNAP records). While the analytic sample offers good coverage of the universe of schools in the ODE data (see Appendix Table 1.A1), the lack of universal coverage might restrict education administrators' efforts to allocate compensatory funds using certain measures.

Methods

To answer our first research question, below we plot state-wide trends in school economic disadvantage for Oregon public schools on all available measures between 2010 and 2017.

[10] To do so, we first multiplied the provided standard errors of the NCES-SNP measure by five and calculated the proportion of the area under the normal curve that lies below 185 percent federal poverty, given the NCES-SNP mean and standard error.

To answer our second research question, we correlate all the candidate school economic disadvantage measures with our Benchmark (SNAP/IRS). Correlation coefficients indicate the extent to which the various measures move together, and how well each preserves the relative rankings of schools according to our benchmark. Our preliminary analyses focus on correlations in measurements drawn from the 2016-17 academic year. For the subset of measures that are available for three or more years, we further consider trends in measurement properties by estimating correlations separately for each school year, in light of concerns about the way changes in policy and practice induce change in proxy measure performance. Correlations are unaffected by whether the school economic disadvantage measure targets students at 100, 130, or 185 percent of the federal poverty line.

However, a correlation is a simplistic summary of the relationship between two variables, and in particular, correlation analyses may overlook systematic divergences between measures in very low- or high-income schools. Our third research question draws attention to the performance of candidate measures across the distribution of school economic disadvantage. To do so, we focus on our most recent year of data (2016-2017). We standardize each of our measures, group schools into 20 quantiles based on their rank according to the Benchmark (SNAP/IRS) measure and calculate the mean and standard deviation of the candidate measures for schools in each quantile. These quantiles are enrollment-weighted, so each bin is comprised of schools enrolling approximately 5 percent of students. We then plot these binned statistics. Each point on this plot represents the mean for the candidate measure in each quantile bin, providing a visual summary of how consistently the benchmark and candidate measures agree about the relative economic disadvantage of schools. Each of the whiskers represents the standard deviation of the candidate measures in each quantile bin, providing a visual summary of the extent of variation in the candidate economic disadvantage rates in each bin.

Results

Temporal trends in measures of student economic disadvantage

Figure 1.1 provides an overview of each of the candidate measures of economic disadvantage in Oregon public schools between the 2009-10 and 2016-17 school years. Since these measures are enrollment-weighted, they can be interpreted as the proportion of Oregon public school students designated as economically disadvantaged. Since the six measures represented on this graph use different income thresholds to define student economic disadvantage, it is not surprising that these lines depict different levels of economic disadvantage. However, we generally expect the measures to follow qualitatively similar trends; as the share of students receiving SNAP falls, so too should the share enrolled in FRPL, and the share designated as economically disadvantaged by MEPS.

The solid line near the top of the graph represents the Benchmark (SNAP/IRS) measure, which is the statewide trend in the proportion of Oregon public school students whose family reported household income less than 185 percent of poverty in IRS filings and/or enrolled in SNAP. This rate increases modestly at the beginning of the study period as the state slowly emerged from the 2008 recession, peaking at approximately 52 percent before declining to approximately 47 percent in 2017.

The dashed line with the circle marker in this graph represents the trend in CCD FRPL, as reported by the state of Oregon to the Common Core of Data; the dashed line with a triangle marker represents trends in the ODE Economic Disadvantage measure. The contrast between these two measures – both of which are ostensibly built around FRPL enrollments – is instructive. While the CCD FRPL rate appears similar to the year-to-year changes in the Benchmark (SNAP/IRS) measure, the ODE Economic Disadvantage rate – which counts schools that participate in CEP as 100 percent economically disadvantaged – rises as schools across the state implement CEP. The apparent reliability of the CCD FRPL measure may be deceptive: CEP schools have the option of reporting their pre-CEP FRPL rates to the CCD. Since school rates tend to be similar over time this creates a reasonable approximation, but it does not conceptually meet the needs of administrators and researchers because rates of economic disadvantage do not change over time in the schools that are using data from prior years.

As expected, the remaining measures – SNAP, ACS Block Group, and Urban Institute MEPS – record very different levels of economic disadvantage than the Benchmark (SNAP/IRS). SNAP enrollment rates, which reflect students whose families demonstrate household income below 130 percent of poverty, vary between 32 and 37 percent across the time period; ACS Block Group rates range similarly from 30 to 35 percent; and the Urban Institute MEPS rate, which reflects students whose families have household income below 100 percent of poverty, varies between 20 percent and 15 percent in the years for which it is available. Economic disadvantage rates using these measures follow the general economic disadvantage rates of the Benchmark (SNAP/IRS) over time.

Correlations between the benchmark measure and candidate measures

Table 1.3 summarizes the school-level correlations between the Benchmark (SNAP/IRS) and each of the two traditional FRPL measures, direct certification measure, and the four alternative candidate measures described above in the 2016-17 school year, the most recent year for which we have data. Our analyses indicate that the CCD FRPL measure correlates well with the benchmark in 2017, at 0.934, but since many schools are using data from previous years, we expect that the quality of this measure will grow worse over time. The ODE Economic Disadvantage measure, which is largely based on FRPL enrollment and is affected by school participation in universal meals programs, has a lower correlation with the benchmark of 0.863. The difference in the correlation between the two traditional FRPL measures and the Benchmark (SNAP/IRS) indicates that the expansion of access to free meals differentially affects the quality of FRPL enrollment measures, depending on the decisions that data providers make as they construct and report data.

The analyses in Table 1.3 show that the direct certification measure, constructed using SNAP enrollment, correlates extremely highly with the Benchmark (SNAP/IRS) at 0.957.[11] It also is the measure with the highest correlation with the Benchmark (IRS) measure, confirming that its strong performance against the Benchmark (SNAP/IRS) measure is not mechanical. This suggests SNAP-based direct certification measures have strong potential as measures of school economic disadvantage.

Concerns about the measurement quality of school economic disadvantage measures based on FRPL enrollment have also led some to consider the use of alternative measures. Our ACS Block Group measure, which describes the degree of economic disadvantage in the neighborhoods in which schools' students reside, correlates with the Benchmark (SNAP/IRS) at 0.796. The Urban Institute MEPS, which is based on CCD,

[11] Since SNAP enrollment data contribute to our Benchmark (SNAP/IRS) measure, some of this correlation is mechanical. However, an alternative version of our benchmark measure based exclusively on the proportion of students whose families report income below 185 percent of the poverty line in their tax filings – referred to in Table 1 as the Benchmark (IRS) - also correlates very highly with the proportion enrolled in SNAP measure, a correlation of 0.942.

correlates with the Benchmark (SNAP/IRS) as highly as the CCD FRPL measure at 0.935.[12] Meanwhile, the NCES-SNP measure, which captures the economic disadvantage of families living in schools' immediate vicinity, is -0.694. The negative correlation is expected because the measure is an average of families' income-to-poverty ratios and thus should be inversely related to proportion low income. A Transformed NCES-SNP measure (cf., Fazlul et al. 2021) has a correlation of 0.661. Taken together, the results in Table 1.3 indicate that alternative candidate measures that use data from the students' ACS Block Group correlate less well with the Benchmark (SNAP/IRS) than do more readily available SNAP or FRPL enrollment-based measures.[13]

Figure 1.2 graphs correlations between the Benchmark (SNAP/IRS), traditional FRPL-based measures, direct certification measure, and alternative candidate measures over time. We see that the quality of traditional FRPL-based measures varies over time. The correlation between the Benchmark (SNAP/IRS) and the CCD FRPL measure improves across the study period, while the correlation between the Benchmark (SNAP/IRS) and the ODE Economic Disadvantage measure rises and then falls over time. While we do not have an explanation for the improvement that we observe in the ODE Economic Disadvantage measure's correlation with the Benchmark (SNAP/IRS) between 2010 and 2014, we note that the post-2014 decline corresponds with the CEP's implementation in schools across

[12] The modified MEPS measure has a lower correlation with the benchmark than the original MEPS, with a correlation of .865.

[13] One reason that neighborhood-based measures might fare poorly is if large numbers of students at the school do not live in the neighborhood surrounding school, as might happen if many students attend charter schools. Oregon is relatively average in terms of its charter school enrollment (5.7% of students attended charter schools in Oregon and 6.0% nationally in 2016-2017, see Wang, Rathbun, and Musu, 2019). Although charter schools are not the only kind of choice available in Oregon, this suggests that the relatively low correlation of the NCES-SNP measure here is not due to an unusually large amount of school choice in Oregon.

the state. Some 39 percent of eligible schools adopted CEP beginning in the 2014-2015 academic year (Neuberger et al., 2015).

Encouragingly, this figure indicates that our proxy for direct certification – the measure of the proportion of students enrolled in SNAP (the dashed line with the square marker) – consistently correlates extremely highly with the Benchmark (SNAP/IRS) in each of the available years. If anything, this measure appears to improve in quality over time, with a correlation of 0.944 in 2010 and 0.957 in 2017.

We note that although the ACS Block Group measure does not correlate particularly highly with the Benchmark (SNAP/IRS) at any point in the study period, its correlation is relatively stable over time. While we only have access to data on the Urban Institute MEPS measure for the last five years in the study period, we note that this measure appears to be somewhat more stable than both traditional FRPL enrollment-based measures.

Michelmore and Dynarski (2017) suggest flagging students as economically disadvantaged if they were economically disadvantaged for the prior 3 years. Since this requires multiple observations of the same student, it may not be a practical measure of year-to-year school-level economic disadvantage. It is nonetheless instructive to observe how highly correlated it is with our Benchmark (SNAP/IRS). We construct this measure of persistent economic disadvantage measure by flagging students as economically disadvantaged if they are indicated as such by the ODE Economic Disadvantage flag for the current school year and the two years prior. The school-level rate of persistent economic disadvantage correlates at 0.835 with the Benchmark (SNAP/IRS) in 2016-17. Generally speaking, the correlation of the persistent measure with the Benchmark (SNAP/IRS) is lower than that of the single-year ODE Economic Disadvantage rate, except for 2014-15 and

2015-16, where it briefly outperforms the single-year rate. We suspect this reflects the fact that the persistent measure holds onto the higher-quality pre-CEP data through 2015-16, improving its reliability relative to the single-year ODE Economic Disadvantage rate. However, once it is limited to data from after the introduction of CEP in 2016-17, it returns to slightly underperforming the single-year rate as a measure of school economic disadvantage.

Relationship between benchmark measure and candidate measures across the distribution

Figure 1.3 presents a series of binned scatterplots depicting the relationship between the Benchmark (SNAP/IRS) and candidate measures across the school economic disadvantage distribution. These graphs represent data from the 2016-17 academic year. We use z-scores to standardize all measures, thus effectively setting all measures on the same scale, allowing us to focus on variation in measure rankings, rather than variation in levels of economic disadvantage. For purposes of legibility, we separate these scatterplots into panels: Panel A depicts the traditional FRPL-based measures, Panel B depicts the direct certification measure (SNAP), and Panel C depicts the ACS Block Group and Urban Institute MEPS measures. Each point on these graphs represents a bin of 5 percent of schools (approximately 55 schools).[14] In the graphs, we plot bin averages of the standardized candidate measure of school economic disadvantage (on the y-axis) for each bin of the standardized Benchmark (SNAP/IRS) measure (on the x-axis). We also mark one standard deviation above and below the mean of the binned Benchmark (SNAP/IRS) measure,

[14] We plot binned scatterplots due to privacy constraints with data housed at the U.S. Census Bureau.

depicting how much variation each candidate measure has at different levels of the Benchmark (SNAP/IRS) measure.

The graphs in Panel A investigate the correspondence between the traditional FRPL-based measures and the Benchmark (SNAP/IRS) measure, with the graph on the left focusing on the CCD FRPL measure and the graph on the right focusing on the ODE Economic Disadvantage measure. These traditional FRPL-based measures correspond closely with the benchmark across the distribution, although CCD FRPL measures school economic disadvantage with greater precision across the distribution than the ODE Economic Disadvantage measure. At the high end of the distribution, these measures appear to understate economic disadvantage relative to the Benchmark (SNAP/IRS) measure, and this is particularly true for the ODE Economic Disadvantage measure. This is due in large part to the fact that the ODE Economic Disadvantage measure reports 100 percent of students enrolled in many CEP schools as FRPL - in 2017, 23 percent of schools in Oregon report a proportion of economically disadvantaged students greater than .99, while in the CCD FRPL measure only one school reports a proportion economically disadvantaged greater than .99. Because the ODE Economic Disadvantage measure reports many CEP schools at the scale's ceiling, this measure does not distinguish between those schools, and this dramatically alters the relative rank of many relatively lower-income schools.

While the graph in Panel B indicates that the direct certification proxy (SNAP) corresponds closely with the Benchmark (SNAP/IRS) across the bulk of the distribution of school economic disadvantage, we note that the SNAP measure captures school economic disadvantage rates less precisely at the very top of the distribution compared with other points of the distribution. This suggests heterogeneity in SNAP enrollment in more economically disadvantaged schools. Our data do not allow us to definitively account for why this might be, but SNAP under-enrollment in economically disadvantaged schools is likely due to a relatively large concentration of children whose families qualify for SNAP benefits but choose not to enroll, a phenomenon that is particularly well-documented in immigrant communities (FRAC, 2020).[15] Also, the Food Distribution Program on Indian Reservations (FDPIR) may take the place of SNAP enrollment in schools near and in reservations. At the bottom of the distribution, we observe higher levels of the SNAP measure than expected based on the Benchmark (SNAP/IRS). This finding suggests that the relatively few students from low-income families in economically advantaged schools have high levels of access to SNAP benefits.[16]

Looking at the alternative candidate measures in Panel C, we see that the ACS Block Group measure overstates economic disadvantage at the low end of the distribution and understates economic disadvantage at the high end of the distribution. Further, the ACS Block Group measure is relatively imprecise across the distribution of school economic disadvantage compared to other available measures. Finally, the Urban Institute MEPS corresponds to the benchmark measure similarly to the CCD FRPL measure, which makes sense given that Urban Institute MEPS relies on CCD FRPL enrollment data. Like the CCD

[15] It is also possible that SNAP overcounts economic disadvantage in some schools due to provisions in SNAP enrollment that allow families to remain enrolled without recertifying every year or that over-enrollment might be an artifact of the timing of income measurement.

[16] One might be worried that the high correspondence between the SNAP measure and the Benchmark (SNAP/IRS) is attributable to a mechanical correlation driven by the use of SNAP to construct the Benchmark (SNAP/IRS) measure. To assess this possibility, we looked at the correspondence of the SNAP based measure across the poverty distribution using an alternative benchmark – Benchmark (IRS) - that relies only on IRS data and find a similarly high degree of correspondence. Thus, we conclude that the high correspondence is not due to a mechanical correlation.

measure, the Urban Institute MEPS understates economic disadvantage in highly economically disadvantaged schools. Unlike the CCD FRPL measure, the Urban Institute MEPS also slightly understates economic disadvantage in schools with relatively few economically disadvantaged students.

Conclusion

As the incomes becomes increasingly unequal, a broad array of federal, state, and local policies aim to target resources and opportunities toward students from low-income families and the schools that educate them. Valid measures of school economic disadvantage are an essential component of this policy agenda.

Our analyses compare several widely used measures of school economic disadvantage as well as recently proposed alternatives against a benchmark measure constructed by linking student-level administrative data from the state of Oregon with IRS and SNAP data housed at the U.S. Census Bureau. Our findings build on a growing body of research highlighting the shortcomings of measures that rely on FRPL enrollment information. Using data from Oregon public schools, we demonstrate that the proportion of students who qualify as economically disadvantaged according to the state's official measure diverges from our Benchmark (SNAP/IRS) economic disadvantage measure starting in 2014-15 as a growing number of schools begin to provide free meals to all students under the NSLP's Community Eligible Provision. Trends in FRPL enrollment data reported in the CCD continue to correspond to the Benchmark (SNAP/IRS), though that may be due to CEP schools reporting old rates to the CCD. These CCD-reported FRPL data correlate highly with our benchmark measure throughout the 2010 to 2017 period for which data are available, although we find some evidence to suggest that they understate the concentration of economic disadvantage in the state's most economically disadvantaged schools. While this may create the appearance of a reliable measure, awareness of the reporting instructions to CCD indicates the measure is not being fully updated and will likely degrade over time. Notably, beginning in 2019-2010, Oregon no longer reported FRPL data to the CCD and not all states have CCD FRPL rates that track trends in state poverty (Spiegel, 2023).

How, then, should policymakers and researchers who lack access to detailed household income data measure the concentration of economic disadvantage in public schools? Our findings from Oregon indicate that school-level measures of the proportion of students enrolled in SNAP provides a highly valid measure of school economic disadvantage. We find that temporal trends in SNAP enrollment rates parallel trends in our benchmark measure of student economic disadvantage. Further, we find that these two measures consistently correlate quite highly across the period for which we have data. Finally, we find that the correspondence between SNAP enrollment rates and our Benchmark (SNAP/IRS) measure holds equally well for Oregon public schools across the distribution of school economic disadvantage. Notably, Oregon has a relatively high SNAP take up rate, and it is possible that SNAP-based measures might not fare as well in states where this is not the case.

These SNAP findings suggest that direct certification measures based on enrollment in SNAP and other means-tested federal programs may be accurate broadly, though further analysis of their reliability given variation in take-up across states is warranted. While direct certification measures are not currently publicly available for all U.S. public schools, a growing number of states make them available via the CCD and it is possible for all states to do so. Notably, SNAP enrollment reflects a different family income threshold than FRPL enrollment, so switching from one measure to the other requires attention to the corresponding level change in rates of economic disadvantage.

We find similarly encouraging evidence regarding the use of the Urban Institute MEPS measure. We note, however, that the Urban Institute MEPS measure is designed as an adjustment for publicly available data, rather than an alternative source of data. In the Oregon context, the Urban Institute MEPS is calculated based on the relatively high-performing FRPL data reported in the CCD. It is not clear whether the Urban Institute MEPS measure would perform as well if it were calculated based on data in other states that were less highly correlated with economic disadvantage. Conversely, it is possible that the Urban Institute MEPS approach could help further improve the validity of school economic disadvantage measures in contexts where direct certification data might not be as strong of a measure. Thus, the extent to which these findings hold for other states is an area for future research.

Our analyses proceed from the assumption that compensatory school policies intend to target resources at schools based on their enrolled concentration of students from economically disadvantaged families. While this focus is an explicit component of many educational policies, including Title I of the ESEA and many state and local school finance plans, we recognize that it is a policy choice. In the future it may be desirable to broaden our view, redirecting attention from the dichotomous and unidimensional notion of student economic disadvantage and toward richer, more multidimensional conceptions of student advantage and disadvantage (Singer, 2023). More sophisticated measures of student household economic status might consider household income as a continuous variable or incorporate local or regional cost of living data to better approximate household spending power. Further, in light of growing evidence regarding the role of wealth in the intergenerational reproduction of educational advantage (Hallsten and Pfeffer, 2017; Pfeffer and Killewald, 2018), policymakers and researchers may build wealth into their consideration of household economic status, though such an undertaking would likely necessitate new data-collection efforts. Alternatively, researchers and policymakers may attempt to broaden their view to consider non-economic factors such as household composition, educational attainment, occupational status, and availability of learning opportunities. Our analyses are not designed to speak to these broader constructs, but future research should.

What our research makes clear, however, is that commonly used measures of school economic disadvantage differ appreciably in their capacity to capture the variation in the proportion of students who reside in economically disadvantaged households across Oregon public schools. These findings validate growing concerns over the quality of widely available measures of school economic disadvantage. At the same time, however, our findings indicate that direct certification data can perform well.

Table 1.1

List of measures of school economic disadvantage used in analyses

Table 1. List of measures of school economic disadvantage used in analyses

Name	*Source*	*Measure*	*Notes*
Benchmark measures			
Benchmark (SNAP/IRS)	Census (IRS/SNAP) 2009-10 through 2016-17	Proportion enrolled in SNAP at any point during relevant school year or with documented family income below <1.85 poverty	Students are designated low income if they (a) are enrolled in SNAP at any point during relevant school year or (b) have family income less than 1.85x poverty in their family's most recent IRS 1040 filling (within prior three tax years).
Benchmark (IRS)	Census (IRS) 2009-10 through 2016-17	Proportion with documented family income below <1.85 poverty	Students are designated low income if they have family income less than 1.85x poverty in their family's IRS 1040 from the most recent calendar year.
Traditional FRPL enrollment measures			
CCD FRPL	NCES - CCD 2009-10 through 2016-17	Proportion enrolled in FRPL	Based on state's reports to the NCES.
ODE Economic Disadvantage	Oregon Department of Education 2009-10 through 2016-17	Proportion flagged as "economically disadvantaged" based on NSLP enrollment and other factors	Other factors include children experiencing homelessness as defined by McKinney-Vento Homeless Assistance Act. In the 2015-2016 school year, all students are flagged as "economically disadvantaged" if they attend schools that provide free lunch to all regardless of economic status. This includes schools that are participating in CEP other universal meals programs.
Direct certification measure			
SNAP	Census (SNAP) 2009-10 through 2016-17	Proportion enrolled in SNAP (proxy for direct certification)	Received any SNAP benefits during school year (e.g., for 2017, enrollment between August 2016 and July 2017), excluding students who only receive SNAP via summer school-based program.
Alternative candidate measures			
ACS Block Group	ACS 2009-10 through 2016-17	Proportion <1.85 poverty in student's block group	Defined as the proportion of children ages 5-17 in households with income below 1.85x poverty in the student'sblock group.
Urban Institute MEPS	Urban Institute 2013-14 through 2018-19	Predicted poverty rate from SAIPE and CCD	This measure uses a linear mixed effects model to predict school-level estimates of economic disadvantage based on average household poverty rates for families with children in the districts in which schools are located.
NCES-SNP	NCES 2016-17	Income-to-poverty ratio	Based on the average income of households in the immediate vicinity of the school using ACS data.
Transformed NCES-SNP	NCES and author calculations 2016-17	Proportion <1.3 poverty	Transformation of the SNP measure designed to capture the share of students at or below 130 percent of the federal poverty line for each school (see Fazlul et al., 2021).

Source: 2010-2017 ODE, IRS 1040, OR SNAP
DRB Approval Numbers: CBDRB-FY2022-CES010-023, CBDRB-FY21-CES014-017.

Table 1.2

Enrollment weighted descriptive statistics of analytic sample

	Analytic sample	
	Mean	*Std. Dev.*
Benchmark measures		
Benchmark (SNAP/IRS)	0.506	0.185
Benchmark (IRS)	0.433	0.171
Traditional FRPL Enrollment Measures		
CCD FRPL	0.522	0.211
ODE Economic Disadvantage	0.539	0.242
Direct certification measure		
SNAP*	0.345	0.164
Alternative candidate measures		
ACS Block Group*	0.327	0.093
Urban Institute MEPS+	0.176	0.075
NCES-SNP (Income-to-poverty ratio)+	304.9	140.5
Transformed NCES-SNP+	0.399	0.106
Demographic characteristics		
Proportion Black	0.024	0.048
Proportion White	0.641	0.191
Proportion Hispanic	0.221	0.174
Proportion Multi-ethnic	0.048	0.031
Proportion American-Indian	0.016	0.041
Proportion Asian	0.049	0.063
Proportion ESL	0.227	0.219
School Enrollment	733.5	520.2
N (School-years)	8500	

Source: 2010-2017 ODE, IRS 1040, OR SNAP

DRB Approval Numbers: CBDRB-FY2022-CES010-023, CBDRB-FY21-CES014-017.

Notes. Statistics are enrollment weighted.

*Available for >99% of school-year observations in the full sample, but missing for a small number of school-years. Measures with suppressed values in the full sample have many more missing values. By construction, all measures are available for all schools in the longitudinal sample.

+ Urban Institute MEPS measures are available 2013-2017; the NCES-SNP measure in the analysis is from 2017.

Table 1.3

Correlations between benchmark measures and candidate measures, 2016-2017 school year

Table 3. Correlations between benchmark measures and candidate measures, 2016-2017 school year

	Benchmark (SNAP/IRS)	*Benchmark (IRS)*
Benchmark measures		
Benchmark (SNAP/IRS)	1	
Benchmark (IRS)	0.991	1
Traditional FRPL Enrollment Measures		
CCD FRPL	0.934	0.923
ODE Economic Disadvantage	0.863	0.859
Direct certification measure		
SNAP	0.957	0.942
Alternative candidate measures		
ACS Block Group	0.796	0.796
Urban Institute MEPS	0.935	0.929
NCES-SNP (Income-to-poverty ratio)	-0.694	-0.679
Transformed NCES-SNP	0.661	0.653
N Schools	1,100	

Source: ODE, IRS 1040, OR SNAP
DRB Approval Numbers: CBDRB-FY2022-CES010-023, CBDRB-FY21-CES014-017.

Note. Statistics are not enrollment weighted.

Statistics include schools from the final year of the analytic sample (2016-2017) for which there is coverage from all measures. We lose approximately 300 schools with small enrollments driven in large part to whether schools are represented in the NCES data sources like the CCD and SNP measures. The analytic sample offers a reasonable snapshot of the universe of schools in the ODE data (see Appendix Table 1.A1).

The Urban Institute also released a MEPS modified measure which they recommend to be used for analyses in large school districts with large school enrollments. Because we are conducting a state-wide analysis with schools of varying enrollment sizes, we do not focus on the MEPS modified measure in the paper. Consistent with Urban Institute's recommendation, the MEPS modified measure has a lower correlation with the Benchmark (SNAP/IRS) than the MEPS, with a correlation of .865.

Figure 1.1

Trends in student economic disadvantage in Oregon across measures, 2010-2017

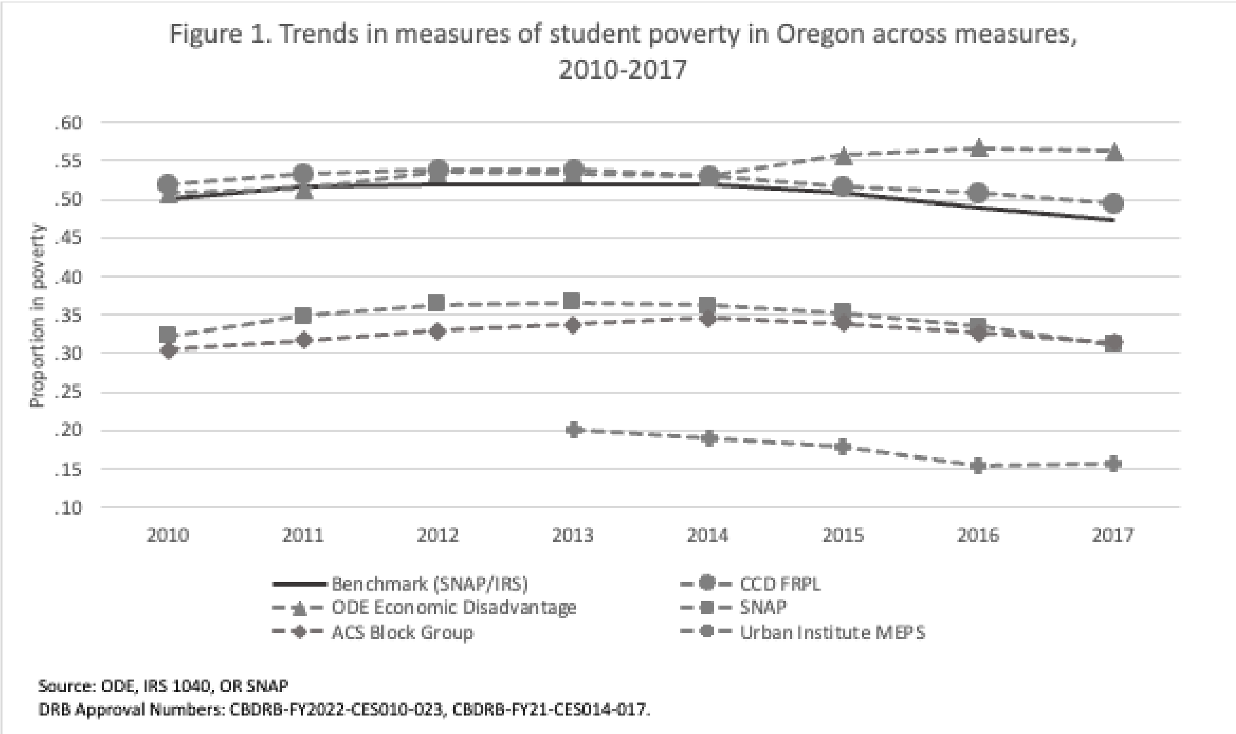
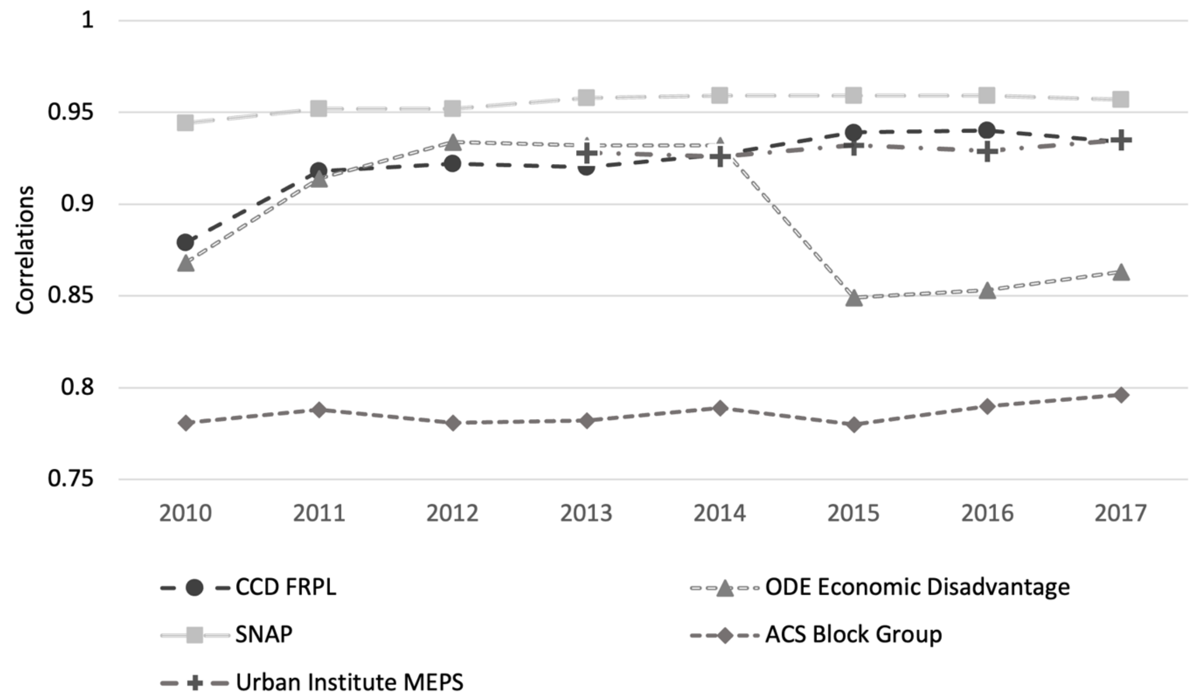


Figure 1.2

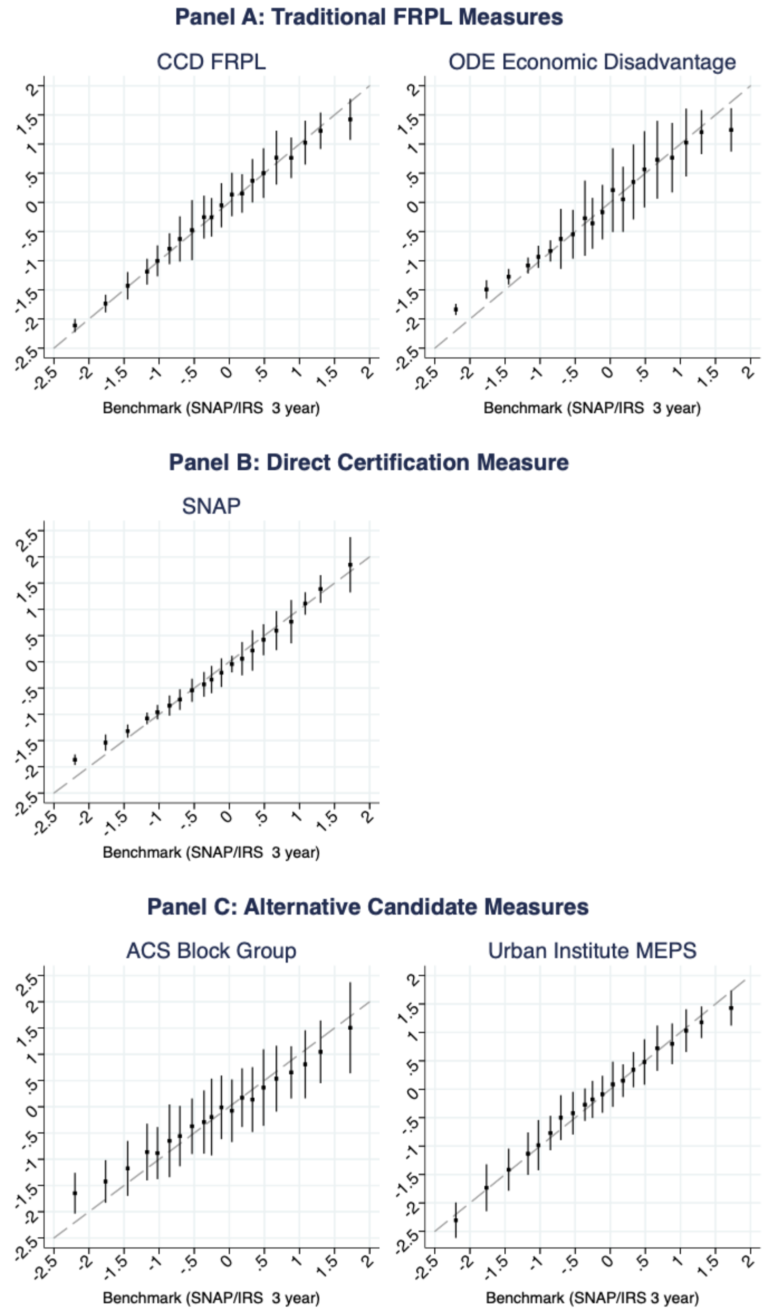
Trends in the correlation between the Benchmark (SNAP/IRS) measure and candidate measures, 2010-2017



Source: ODE, IRS 1040, OR SNAP
DRB Approval Numbers: CBDRB-FY2022-CES010-023, CBDRB-FY21-CES014-017.

Figure 1.3

Relationship between candidate measures and Benchmark (SNAP/IRS) across the distributions



Source: ODE, IRS 1040, OR SNAP; DRB Approval Numbers: CBDRB-FY2022-CES010-023, CBDRB-FY21-CES014-017.
Note. Axes ranges differ slightly to optimize visibility. Data is from academic year 2016-2017.

CHAPTER 2

Which money matters for spending on children in low-income households?

Children's development depends in part on parental spending on enriching activities and materials (Becker, 1981; Becker and Thomes, 1986). Policymakers and researchers seeking to improve child development are particularly interested in whether cash transfer policies lead to increased parental investments in children (Duncan, Magnuson, Votruba-Drzal, 2014; Aizer, Eli, Ferrie, Lleras-Muney, 2016). Existing research has primarily framed households within a common preference model, assuming income pooling and the fungibility of money (Becker, 1981). Nevertheless, decades of interdisciplinary research has highlighted the non-fungible nature of income, challenging the notion that all income sources are interchangeable (Zelizer, 1989; Thaler, 1999). This line of inquiry has examined how individuals and families perceive and utilize different sources of income, revealing distinctions in how money is allocated (Bandelj, Wherry, Zelizer, 2017; Zelizer, 1997). Despite the significant role of income to parental investment theories, research on parental spending on children has largely overlooked considerations of fungibility.

To address this research gap, the current study uses data from the Baby's First Years study, a randomized control trial of a cash transfer labeled as "For My Baby" provided to low-income mothers with young children in the United States, to compare the marginal propensity to spend on child-focused goods from the labeled cash transfer, mothers' earned income, and other household income sources. I find notable differences in spending on children across various exogenous and existing income sources, suggesting that income type affects spending on children in low-income households. Moreover, the analysis reveals that while control over resources likely plays a role in spending on children, the labeling of the cash transfer impacts spending decisions.

Understanding the uniqueness of income sources and their implications for spending on children is crucial for optimizing the child development effects of cash-based welfare policies. The study emphasizes the importance of considering the nuanced ways in which families perceive and utilize money, offering valuable insights for researchers and policymakers looking to improve children's development from cash-based welfare programs.

Income and parental investment in children

Research consistently finds a strong correlation between family income and child development (Barrow and Schanzenach, 2012; McLoyd, 1998; Brooks-Gunn & Duncan, 1997; Yoshikawa, Aber, Beardslee, 2012). The investment model helps to explain the relationship between income and child development, which applies economic theory to the study of families (Becker, 1964). Within this model, parental investment in the form of spending on children is widely recognized as a key mechanism through which income affects children's development (Yeung et al., 2002; Cunha and Heckman, 2008). Higher income parents, with greater financial resources at their disposal, are better able to provide their children with intellectually stimulating materials, activities, and experiences. Conversely, limited financial resources in low-income households impose constraints on the quantity and quality of parental investment, potentially explaining the developmental disparities observed between low-income children and their higher income peers (Brooks-Gunn and Duncan,1997).

However, these studies have primarily viewed households through the framework of a common preference model, assuming income pooling and the fungibility of money (Samuelson, 1956; Becker, 1981). According to this perspective, all income is treated as equal and interchangeable. However, literature suggests that income is often perceived as non-fungible, meaning that different sources of income may be utilized differently (Zelizer, 1997; Thaler, 1999). Moreover, income allocated towards children is particularly "sacred" (Zelizer, 1985). Researchers have shown that when it comes to spending on children, income is demarcated and concretely put aside, never to be touched except for spending on children, even if it means that families must go into debt (Bandelj, 2020; Zaloom, 2019). Studies on spending on children and its central role in family life have mostly focused on middle-income households and have not investigated the extent to which income is demarcated for children in low-income households (Hays, 1996; Lareau, 2011). Because low-income households have greater economic constraints, it is possible that household resources are pooled to meet basic needs, rather than having certain income sources siphoned off towards spending on directly child-related goods (see Rao, 2022). The current study explores whether – and the extent to which - distinct household resources are allocated towards children in low-income families.

The influence of maternal control over income on spending on children

Numerous studies have examined the relationship between control over money and its impact on spending on children (Phipps & Burton, 1998). Notably, a growing body of literature demonstrates that when women control household financial resources, children have better outcomes across a variety of domains, including nutrition, health, and education (Thomas, 1990; Zelizer, 2010).

Building on this, research examining spending on children shows that when mothers have financial control, a higher proportion of income is allocated towards children's needs (Kenney, 2008). Studies have looked at the impact of exogenous income changes on spending patterns. Lundberg, Pollak, & Wales (1997) explored a policy change in the UK that augmented women's income, relative to men's income, within the same household. They discovered that this redistribution of income towards women resulted in increased expenditures on women and children's clothing, relative to men's clothing expenses. Similarly, Ward-Batts (2008) examined a comparable policy shift in the United Kingdom during the late 1970s, which allocated a child allowance to wives. Their findings indicated that households did not pool their income, with mothers allocating a larger portion of the child allowance towards expenditures on children, such as toys, clothes, and children's pocket money. However, these studies did not compare spending on children from existing household resources, and although located in developed countries, were conducted outside the U.S. context. Questions remain about whether control over income in low-income households in the U.S. effects spending on children.

Labelling money and its impact on spending on children

While maternal control over resources provides some insight into spending increases on children resulting from cash transfers, further research suggests that control alone cannot fully explain the increased spending on children. Kooreman (2000) conducted a study in the Netherlands that examined spending from an untaxed child benefit provided to all households with at least one child, regardless of income. Like the studies described above, Koooreman (2000) found that the cash benefit was more likely to be spent on child-focused goods than other household income sources. Additionally, Kooreman (2000) found that the propensity to spend the cash transfer on children held true for single mothers as well. This finding suggests that control over resources alone cannot fully account for the heightened investment in children resulting from the cash transfer. Kooreman (2000) speculates that parents may have considered the child benefit as a benchmark for child-related expenses or feel a moral obligation to allocate a significant portion of it towards child goods. In a similar vein, Del Boca and Flynn (1994) found a notable difference in the marginal propensity to consume child-focused goods between child support income and alimony income among divorced mothers.

Recognizing that how money is perceived might affect spending behavior, researchers designing cash transfer programs have introduced labels to shape spending patterns (Abler and Marklein, 2008; Fiszbein and Schady, 2009). This approach aims to direct recipients' attention towards the intended purpose of the money. Such cash transfers, known as "labeled cash transfers", have been found to induce spending in a desired manner. For example, a randomized control trial in Morocco conducted by Benhassine et al. (2015) found that a cash transfer explicitly labeled as for education led to significant improvements in school participation, irrespective of whether a parent had to fulfill a condition to receive the transfer. In the United States, Halpern-Meekin et al. (2015) explored how mothers perceive the Earned Income Tax Credit (EITC) and found that their interpretations shaped how the money was spent: mothers viewed the EITC as a well-deserved treat suitable for spending on both wants and needs, illustrating how labels and social associations with money influence spending behaviors. However, questions remain about how the perception of money – independent of control – shapes spending on children among low-income families in the U.S.

Baby's First Years RCT

The current study uses data from Baby's First Years (BFY), an ongoing randomized control trial designed to estimate the causal impact of income on children's development. Mothers are randomly assigned to a high cash gift group, receiving $333 per month, or a low cash gift group, receiving a nominal $20 per month.[17] At ages 1, 2, 3 and 4, surveys were conducted to assess a wide range of family, maternal, and child-level outcomes. These surveys ask detailed information on income and expenditures specifically directed towards the child, enabling the estimation of differential spending patterns across various sources of household income.

The mothers in BFY were recruited at their hospital bedside after giving birth.[18] They were offered "the opportunity to receive a cash gift every month for the next 3 years and 4 months (40 months total)". The monthly cash gift is independent of the research, with additional incentives offered for participation in data collection activities.[19] Once consent was obtained, mothers were given a green debit card with the words "For My Baby" printed on it, which is the label in "labeled cash transfer". The mode of cash disbursement makes the BFY gift a labeled cash transfer (Benhassine et al., 2015;

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[17] The amount of the high cash gift group was chosen because it is in the range of the amount received (in today's dollars) of typical income supplement programs in the United States, which amounts to around 20 percent of the average low-income family's total budget.

[18] To be eligible for the study, mothers had to have household incomes below the federal poverty threshold in the calendar year prior to the interview, counting the newborn. The full set of inclusion conditions include (all must be met): 1. mother 18 years or older 2. household income below the federal poverty threshold in the calendar year prior to the interview, counting the newborn 3. infant admitted to the newborn nursery and not requiring admittance to the intensive care unit 4. residence in the state of recruitment 5. mother reports not "highly likely" to move to a different state or country in the next 12 months 6. infant to be discharged in the custody of the mother 7. Mother English or Spanish speaking (necessary for administration of instruments used to measure some of the child outcomes)

[19] The full script inviting mothers to receive the cash gift can be found here: https://www.babysfirstyears.com/_files/ugd/88a466_b0e50ba6dd9444208f9ba16c59e4bf08.pdf

Kooreman, 2000). The cash is initially disbursed at hospital bedside and is automatically loaded onto the card each month around the child's birth date along with a text message reminder about the cash availability.[20] The text message is sent to a number provided by the mother.[21]

Current study

The current study uses disaggregated household income data to estimate marginal propensities to consume (MPCs) child-focused goods, which are the shares of an additional dollar of a given income category devoted to a given expenditure. The analysis focuses on comparisons of MPCs of child-focused goods from BFY income, mothers' earned income, and all other household income combined.

Informed by the literature described above, I test three hypotheses:

1. The MPC child-focused goods from mothers' earned income is greater than the MPC child-focused goods from all other household income.
2. The MPC child-focused goods from BFY income is greater than the MPC child-focused goods from total household income.
3. The MPC child-focused goods from BFY income is greater than the MPC from mothers' earned income.

While my hypotheses focus on child-related expenditures, I include an outcome that is a general household expenditure – specifically, money spent on food eaten outside the

[20] The card functions similarly to a typical debit card, available for use at an ATM or point-of-sale, but mothers cannot deposit money onto the card.

[21] I assume that mothers control the BFY gift money because: a) the care was given to the mother; b) the text messages go to the mother, c) and because of anecdotal evidence from interview data where mothers share with interviewers that they do not tell others about the money.

household - as a point of comparison with the MPCs on child-related goods. I choose a general household expenditure as a point of comparison because the existing literature suggests that income is pooled for general household expenditures (see Phipps and Burton, 1998). Consistent with existing literature, I expect that the MPCs will not differ across income types for this category of expenditures.

Methods

I estimate MPCs from different income sources using the following equation:

$$Y_{is} = B_0 + B_1 BFY_{is} + B_2 \text{mothers' earned income}_{is} + B_3 \text{all other income}_{is} + Z_{is}\gamma + \delta_s + \varepsilon_{is}$$

where i is a mother or household in site s, Y is the expenditure outcome of interest, δ is a site fixed-effect, and ε is the error term (with robust standard errors). The model includes the following baseline covariates (γ) with the goal of improving precision of the impact estimate: mother's characteristics (mother's age, maximum education level attained, race and ethnicity, marital status, general health, an indicator of maternal depressive symptoms, cigarettes and alcohol consumption during pregnancy), household characteristics (number of children born to mother, number of adults in the household, father living with the mother, household net worth), and baby's birth characteristics (weight at birth and gestational age). I also include a continuous variable of total household income at baseline to control for the concern that spending differences from non-BFY income sources might be due to differences in overall income levels.

BFY is takes a value of \$3.33 for mothers in the high cash gift group and \$2.00 for the low cash gift group and B_1 is the causal effect of being in the high cash gift group on spending on outcome Y. B_1 can be interpreted as the change in spending on outcome Y per \$100 increase in BFY income. Mothers' earned income represents earned income for mom *i* in site *s* and, thus, B_2 is the estimate of the marginal dollar spent on outcome Y from every $100 increase in mothers' monthly earned income. Lastly, all other income represents all other household income for mom *i* in site *s* (excluding BFY income and mother's earned income), and thus B_3 is the estimate of the marginal dollar spent on outcome Y from every $100 increase in all other household income. Parameters B_2 and B_3 are correlations between maternal-reported income sources and spending on a given good.

My hypotheses are about differences in spending by income category. Given that all my hypotheses have specific directions, I employ a one-tailed Wald test to assess the differences between the MPC coefficients. I also provide p-values from two-tailed tests in my analysis for a more conservative approach.

My first hypothesis, presented in the first row of Table 2.1, is that the MPC child-focused goods from mothers' earned income is greater than the MPC child-focused goods from all other household income ($B_2 > B_3$).

The second hypothesis, presented in row 2 of Table 2.1, is that the MPC child-focused goods from BFY income is greater than the MPC from total household income ($B_1 > B_2 + B_3$).

Lastly, my third hypothesis is presented in row 3 of Table 2.1. I test whether the MPC child-focused goods from BFY income is greater than the MPC from mothers' earned income ($B_1 > B_2$).

Sample

The study relies on data from Baseline, Age 1, and Age 2 of BFY surveys.[22] The sample used in this study are mothers with valid data on variables that are central to the hypotheses (N=816).[23]

Measures

In this section I describe the key measures used in the study. All spending measures are presented in monthly units.

Monthly income sources

BFY income. Mothers in the high cash gift group are assigned a value of $3.33 and mothers in the low cash gift group are assigned a value of $2.00, which are the dollar amounts deposited onto the "For My Baby" debit card each month for the treatment and control groups, respectively, scaled to $100.

Household income. At each wave of the survey, interviewers ask mothers to report household income by source for the previous calendar year. Mothers interviewed at any point in 2020 report income for January and December of 2019 and mother s interviewed in 2021 report income for January through December of 2020. I use income reported at the age 2 interview, which provides income from the year in which the child expenditures were

[22] The surveys can be found here: https://www.icpsr.umich.edu/web/DSDR/studies/37871/datadocumentation#

[23] The key variables include child-focused expenditures at Age 1 and mothers' earned income and all other household income at Age 2. I use income from Age 2 because income is asked about the prior year. The income that mothers report at Age 2 refers to the year they are reporting the expenditures (at Age 1). There are 931 mothers at Age 1 who have valid child-focused expenditure data. Of these 931 mothers, 115 do not have valid earned income data and other household income data at Age 2. This results in a sample of 816 mothers.

reported (at the age 1 interview).[24] I divide each annual income source by 12 to present results in monthly terms.

Figure 2.1 shows the five income categories that mothers are asked about: their owned earned income, income earned by a spouse, income from anyone residing in the household, income from the government, and income from any others who do not live in the household.[25]

Mothers' earned income. This is measured by the mothers' earned income as reported by each mother. The specific question asks: "*How much did you earn from all your employers before taxes and deductions during [previous year]?"* If mothers report having no earned income, this has a value of zero.

All other income. This variable includes all household income other than the mothers' earned income. As Figure 2.1 shows, to create the category "all other income", this variable adds together income received from a spouse, income received from anyone living in the household, income received from the government, and income received from anyone not living in the household. The items read: "*How much did [spouse/husband/wife/domestic partner] earn from all employers before taxes and deductions during [previous year]?"; "Now let's think about the other members of your household, that is, the people who have been living with you and are related to the child by blood, marriage, adoption, or domestic*

[24] As robustness checks, in Appendix Tables 2.A1 and 2.A2 I show the marginal propensities to consume from income measured at baseline, which is the effect of income prior to receipt of the cash transfer and so these estimates are not confounded by changes in behavior due to the birth of a child or receipt of the cash transfer. While my preferred specification uses Age 2 income because it is the income that overlaps with spending, Appendix Tables 2 and 3 show that empirically this distinction does not matter for my conclusions. My results are robust the timing of measurement of income.

[25] The specific sequence of survey questions can be found in the on babysfirstyears.com (Duncan et al., 2020). If mothers do not have income from a given category or do not know the income from a given category, I treat this as zero income in the category.

partnership. How much did other members of this household, earn from all employers before taxes and deductions during [previous year]?"; "How much income did you and other members of your household receive from the government, such as welfare, SSI, unemployment benefits and social security during [previous year]"; and *"How much income did you and anyone in your household receive from all other sources such as money from any businesses, help from friends or relatives, child support and any other money income during [previous year]? This should include any regular contributions from people who did not live with you. Please DO NOT include the gift you are currently receiving from our study."*

Total household income. I create a variable that is total household income. This variable adds income from all five categories, excluding the BFY cash gift.

Expenditures

Child-expenditures. Mothers were asked about how much they spent in the prior month on the following child-focused goods: books, toys, clothes, shoes, diapers, videos/apps (such as Disney+ or ABC mouse). Mothers are asked, "*In the past month, have you or any member of your household purchased [good] for [child name]*"? The item about videos/apps asks specifically about "*videos, apps, or on demand programs for use on phone, tablet, desktop or laptop computer and/or TV.*" I use this information from the Age 1 survey. In addition to presenting results for each individual item, I present results of an additive index of all the items together. If more than three items have missing data, the additive index is considered missing. For the individual items, however, I show results for all mothers with valid data, which explains the sample size discrepancies between the individual items and the additive index.

Other expenditure. I use the total amount mothers reported spending on food used outside the home. The interviewer asked, "*In the prior month, about how much did you and everyone else in your family spend EATING OUT in an average week? Include any carry-out or drive-through orders, too.*" I multiplied this value by 4.35 to obtain the monthly amount.

Results

Baseline balance

Table 2.2 shows that mothers in the high cash gift group are statistically similar to mothers in the low cash gift group on baseline characteristics (X^2 (30, N=816) = 331.82, p=.112). I conducted tests of mean differences for each individual characteristic and a joint test of significance of all the characteristics together. The p=.112 is from the joint test, and four out of 29 individual tests produced marginally significant p-values.[26]

Mothers are similar across racial and ethnic groups. Approximately 42 percent of mothers are Black, non-Hispanic, 43 percent are Hispanic, 10 percent are white, non-Hispanic, 4 percent are multi-racial, and 3 percent other race or unknown race. The average age of mothers is 27. Mothers have an average of 2.5 children. Approximately 37 percent of mothers reported that the biological father of the BFY study target child was living in the household at baseline. About 20 percent of the mothers had less than a high school education, almost 50 percent of the mothers had completed high school, and about 18 percent had completed some college. Finally, some 48 percent of mothers report being

[26] This includes whether race is unknown or other (p=.026), whether mother is single or never married (p=.050), mother health is good or better (p=.086), and number of alcoholic drinks per week (p=.089).

single and never married at baseline, about 24 percent report being single and living with a partner, and about 24 percent report being married.[27]

Descriptive statistics at Age 1

Table 2.3 presents descriptive statistics of key variables and household composition characteristics at the time of the target child's first birthday. I include household composition characteristics to understand who is present in the household who might contribute to financial decisions or control household resources. The first panel of Table 2.3 presents income statistics. Total household income averaged $2,229 per month, with a standard deviation of $2,298. Mothers earned an average of $856 per month with a standard deviation of $1,201. About 31 percent of mothers have no earned income. All other household income (excluding mothers' earned income) averaged $1,373 per month, with a standard deviation of $1,859. For 30 percent of mothers, all other income includes income from a biological father of the BFY target child who is reported as contributing to the household income.

The second panel of Table 2.3 presents descriptive spending statistics. Mothers spent an average of $322 on all child-related items in the past month (sd=$320). Mothers spent the most on clothing/shoes (mean=$149; sd=$223), and the second most on diapers (mean=$72; sd=$65) and toys (mean=$73; sd=$93). Mothers spent an average of $215 on food eaten outside the home in an average month (sd=$327).

The third panel provides information on who is in the household. About 29 percent of mothers live with no other adults in the household. For the 71 percent of mothers who

[27] Numbers may not add to one hundred due to rounding.

live with other adults, the average number of additional adults is 1, with a maximum of 5 adults. About 47 percent of mothers have a romantic partner living in the household and about 25 percent live with the biological father of the BFY target child. Finally, about 24 percent of mothers live with unrelated adults.

MPC child-focused goods across household income types

Table 2.4 shows the MPC results. In interpreting the results, it is important to recognize the differences between the nature of the increase in BFY income and the sources of household income. Specifically, BFY coefficients are based on differences across participants in the amounts of BFY payments they receive. Because all receive either a randomly assigned $333 or $20 per month payment, the cross-sample variation in this source of income is based on randomly-assigned (exogenous) income. In contrast, cross-sample variation in earnings and other family income arises from many endogenous sources and the coefficient estimates that it supports are subject to the usual kinds of omitted-variable bias.

The first row shows the MPCs on an index of child-focused goods. First, there is a significant increase in spending on child-focused goods for mothers in the high cash gift group relative to the low cash gift group. Specifically, mothers in the high cash gift group spend $20.63 more (se=7.48) per $100 of BFY income on the index of child-focused goods than mothers in the low cash gift group (p<.05). Mothers in the high cash gift group spend a significant $2.02 more on books (se=.057; p<.01) and about $6.10 more on toys (se=2.13; p<.01) per $100 increase in BFY income than mothers in the low cash gift group. In addition, mothers in the high cash gift group spend $8.68 more on clothes/shoes (se=5.44; p>.10), about $2.10 more on diapers (se=1.54; p<.10) and about $1.65 more on videos and apps (se=1.47; p>.10) per $100 increase in BFY income, although not statistically significant.

The second column of Table 2.4 shows the MPCs for child-focused goods from mothers' earned income. These coefficients should be interpreted as associations between household income and expenditures on children. Column 2 shows that an additional $100 in mothers' earned income per month is associated with an increase of $2.69 in spending on child-focused goods (p<.10). Looking at the individual goods, the positive association between mothers' earned income and spending on children is driven primarily by spending on clothing/shoes. A $100 (observational) increase in mothers' earned income per month is associated with an increase in spending on clothes/shoes of $1.68 (se=.69; p<.05). There are also positive associations between mothers' earned income and spending on books, toys, diapers, and videos and apps, although none are statistically significantly different from zero.

Finally, the third column shows the MPCs of all other household income. There is no significant relationship between all other household income and spending on child-focused goods, with a non-significant $0.04 decrease in spending on child-focused goods for each additional $100 in all other household income.

Differences in MPCs across household income types

Table 2.5 presents the p-values corresponding to the one- and two-tailed Wald tests for equality of coefficients.

Research Question 1: Is the MPC child-focused goods from mothers' earned income greater than the MPC from all other household income?

The first row of Table 2.5 reports the results of the test of the first hypothesis - that the MPC for child-focused goods from mothers' earned income is greater than the MPC from all other household income (excluding the BFY income) (B_2 > B_3). Mothers spend $2.73 more out of each additional hundred dollars of their earned income on child-focused goods than out of all other household income, statistically significant at the 5 percent level using a two-tailed test and below 5 percent using a one-tailed test.

Research Question 2: Is the MPC child-focused goods from BFY income greater than the MPC from total household income?

The second row of Table 2.5 shows the results of hypothesis two. Mothers spend $17.98 more from BFY income than from a $100 increase in total household income. This difference is significant using a one-tailed test (p=.01) and a two-tailed test (p=.02).

Research Question 3: Is the MPC child-focused goods from BFY income greater than the MPC from mothers' earned income?

The third row of Table 2.5 presents the results of the third hypothesis. I find that mothers spend more on child-focused goods from their BFY income than from their labor income. This difference is highly significant using both a two-tailed and one-tailed test (p=.02 and p=.01, respectively).

Falsification tests

To further examine whether BFY income is allocated specifically to children rather than to general expenditures, I employ two approaches. First, I analyze the difference in MPC between BFY income and total household income spent on eating out, which is a general household expense. Second, I examine whether expenditures on children from BFY income exceed expenditures on eating out from BFY income.

First, I find that I cannot reject the null that the MPC on general household expenditures from BFY income is different from the MPC on general household expenditures from total household income (p=.20 using a one-tailed test). This finding is consistent with the expectation that BFY income is fungible with other sources of household income when it comes to general household expenditures.

Second, I test for the difference in spending of BFY income between child-focused goods (*b*=20.63*)* and eating out (*b*=7.43). I cannot reject the null, indicating that BFY income is spent similarly on child-focused goods and eating out (p=.17). While this suggests that an increase in BFY income led to increased spending not only on child-focused goods but also on general household items, it is worth noting that the point estimates indicate a relatively larger amount spent on child-focused goods compared to the spending on general household items.

Sensitivity of investments in children from BFY income to having a romantic partner in the household

A romantic partner in the household might influence investments in children, either positively or negatively, depending on the partner's preferences. To explore this dynamic, I conduct an additional analysis asking whether the presence of a romantic partner in the household affects the MPC child-focused goods from each income source. This analysis sheds light on the extent to which differentiation of BFY income specifically for investments in children might be affected by a romantic partner. The results are presented in Appendix Table 2.A1.

Unfortunately, due to limited statistical power, I could not detect an effect of having a romantic partner in the household on spending on children from BFY income. Using

Bloom's (1998) rule of thumb, the minimum detectable effect of having a romantic partner on child investments from BFY income is approximately $42.00, on a base of $20.63. The study lacks sufficient power to detect an effect of this magnitude and therefore cannot provide conclusive evidence on the role of a romantic partner in influencing spending on children from BFY income.

Discussion

Parental investment through monetary expenditures on children links income to child development and long-term outcomes (Aizer et al., 2016). Previous research has shown that as income increases, so do parental expenditures on children (Kaushal, Magnuson, and Waldfogel, 2011; Duncan and Murnane, 2011). However, the extent to which exogenously increasing family income leads to higher parental expenditures on children remains an open question (Duncan, Magnuson, Votruba-Drzal, 2014). Moreover, the variation in child expenditures across income sources, particularly within low-income families, remains largely unexplored. This study aims to assess the impact of an exogenous income shock on investment in children relative to investments from other sources of household income, providing valuable insights for designing effective child-focused social policies.

Using data from a randomized control trial of an unconditional cash transfer to low-income mothers, this study examines differences in the marginal dollar spent on child-focused goods across different sources of household income. The results support the notion that not all income sources are equal when it comes to spending on children. First, the results show that mothers' income is a stronger predictor of child expenditures than other sources of household income, particularly among a sample of low-income mothers in the

United States. Second, the study shows that income from a cash transfer labeled as "For My Baby" is more likely to be used for investments in children than from total household income. Finally, the study highlights that BFY income increases investment in children above and beyond investments from mothers' labor income alone, suggesting that control over resources cannot fully explain differences in income sources for investment in children.

The study provides some evidence that differences in spending by household income type are specifically observed in investments in children. In particular, there is no significant effect of income type (BFY vs. total household income) on general household spending, as measured by the money spent on food consumed outside the home. However, while more of the BFY income is allocated to investing in children compared to eating out, this difference does not reach statistical significance.

Insights from a variety of disciplines shed light on why BFY income is allocated to investments in children, beyond total household income and mothers' earned income alone. Behavioral economics provides a perspective that highlights the role of mental accounting and environmental cues (Thaler, 1985). The "For My Baby" label, along with the text message reminders related to the child's birthday, serve as a nudge or gentle reminder to spend the money in a specific manner. This behavioral economic approach suggests that the increased investment in children from BFY income relative to existing household income is due to the label influencing mothers' preferences for child-related expenditures (see Kooreman, 2000; Milkman & Beshears, 2009).

On the other hand, economic sociology offers an alternative viewpoint that emphasizes the social and cultural aspects of spending decisions. Zelizer (1996, 2000) argues that money is spent in the context of relationships and cultural meanings. According to this perspective, the label “For My Baby” invokes the cultural notion of intensive motherhood (Hays, 1996), which may explain the increased investment in children from BFY income relative to existing household income. Furthermore, the specific types of goods purchased with the cash transfer provide insights into mothers’ values and priorities, rather than indicating a shift in preferences driven solely by the label. Gowayed (2018) shows that mothers already had preferences aligned with a cash transfer earmarked for children’s education, and the money allowed them to spend in ways that reflected their pre-existing values, with the label serving as a facilitator rather than a catalyst for changing preferences.

By drawing on theories from behavioral economics and economic sociology, we gain a comprehensive understanding of the nuanced factors that influence the allocation of income to investments in children.

Conclusion

The current study goes beyond the traditional concept of fungibility and introduces a broader understanding of the role of money in child development to theories of parental investment. The findings highlight the differentiation of money within low-income households when it comes to spending on children. At the same time, different sources of household income are similarly allocated to general household expenditures. The results support existing literature that an exogenous increase in household income increases expenditures on low-income children in the United States. Moreover, the results show that the appearance of the money, independent of income control, can increase investments in children relative to existing investment from other maternally controlled income sources.

The findings have implications for the design of social policies aimed at implementing cash transfers to improve child development. The findings reveal that seemingly secondary concerns related to implementation of cash transfers, such as the method of disbursement, are central to whether these transfers lead to investments in children. The question of whether income improves child development remains fundamental to enhancing the well-being of children (Duncan and Magnuson, 2005). The results of this study encourage researchers and policymakers to explore the nuanced ways in which cash transfers facilitate increased spending on children particularly among low-income families.

Table 2.1

List of hypotheses

(1)	The MPC child-focused goods from mothers' earned income will be greater than the MPC from all other household income.
(2)	The MPC child-focused goods from the BFY income will be greater than the MPC from all other household income.
(3)	The MPC child-focused goods from the BFY income will be greater than the MPC from mothers' earned income.

Table 2.2

Baseline balance between high and low cash gift groups

	Low Cash Gift	High Cash Gift	Std Mean Difference		
	Mean (sd)	Mean (sd)	Hedges' g	Cox's Index	p-value
Child is female	0.49	0.47		-0.085	0.383
Child weight at birth (pounds)	7.14 (1.04)	7.11 (1.01)	-0.05		0.396
Child gestational age (weeks)	39.08 (1.22)	39.03 (1.19)	-0.06		0.325
Mother age at birth (years)	27.24 (5.84)	27.61 (5.82)	0.11		0.137
Less than high school	0.22	0.21		-0.074	0.420
High school or GED	0.51	0.51		0.031	0.760
Some college	0.17	0.17		0.017	0.868
Associate's	0.04	0.04		0.196	0.561

Bachelor's	0.06	0.06		-0.044	0.835
Unknown	0.00	0.01		.	0.399
White, non-Hispanic	0.10	0.09		-0.165	0.246
Black, non-Hispanic	0.41	0.43		0.103	0.101
Multiple, non-Hispanic	0.04	0.03		-0.140	0.397
Hispanic	0.42	0.43		0.052	0.756
Other or unknown	0.04	0.02		-0.543	0.026
Single, never married	0.45	0.49		0.137	0.050
Single, living with partner	0.25	0.22		-0.161	0.102
Married	0.23	0.23		0.000	0.932
Divorced	0.04	0.03		-0.180	0.385
Other or unknown	0.04	0.04		0.060	0.993
Mother health is good or better	0.90	0.92		0.248	0.086
Mother depression (CESD)	6.67 (4.30)	6.63 (4.28)	-0.02		0.873
Cigarettes per week during pregnancy	4.44 (18.07)	3.46 (11.68)	-0.09		0.145
Alcohol drinks per week during pregnancy	0.11 (1.39)	0.03 (0.41)	-0.10		0.089
Number of children born to mother	2.48 (1.41)	2.57 (1.43)	0.10		0.142
Number of adults in household	2.04 (0.97)	2.02 (0.97)	-0.04		0.564
Biological father lives in household	0.39	0.36		-0.120	0.173
Household income unknown	0.06	0.07		0.226	0.282

Household net worth unknown	0.10	0.09		-0.128	0.449
Joint Test: Chi2(29) = 29.30, p-value= 0.398, n=816.					

Notes: n=339 treatment moms and n=477 control moms.
p-values were derived from a series of OLS bivariate regressions in which each respective variable was regressed on the treatment status indicator using robust standard errors and site-level fixed-effects.
Standardized mean differences were calculated using Hedges' g for continuous variables and Cox's Index for dichotomous variables.
All variables are measured at baseline.

Table 2.3

Descriptive statistics of sample at age 1

	mean	*sd*	*min*	*max*	*count*
Panel A: Income					
BFY monthly money (hundreds)	1.5	1.54	0.2	3.33	816
Household income (hundreds)	22.29	22.98	0	233.5	816
Mother earned income (hundreds)	8.56	12.01	0	166.67	816
All other income (hundreds)	13.73	18.59	0	233.5	816
Mother has no earned income	0.31	0.46	0	1	816
Panel B: Spending on children					
Total dollar amount spent on child-focused goods in last month	321.77	320.35	0	5145	816
Money spent on Books	14.59	23.97	0	200	809
Money spent on Toys	72.68	93.36	0	1000	811
Money spent on Clothes/shoes	149.35	223.02	0	5060	814
Money spent on Diapers	72.19	64.54	0	962	811
Money spent on Videos/apps	14.44	58.63	0	759	811
Money spent eating out per month	214.94	326.58	0	3910.5	804
Panel C: Household characteristics					
Biodad contributes to income	0.3	0.46	0	1	816
Number of adults in household	1.07	0.97	0	5	815
Number of children in household	1.68	1.41	0	10	815
Living with no other adults	0.29	0.45	0	1	815
Mother has romantic partner in household	0.47	0.5	0	1	815
Lives with baby's biodad	0.25	0.43	0	1	816
Lives with unrelated adults	0.24	0.43	0	1	816
Number of unrelated adults in household	0.33	0.65	0	4	816
Number of grandparents in household	0.28	0.53	0	2	816
Observations	816				

Notes. Income is reported monthly and in hundreds. Income variables come from Age 2 survey data collection. Age 2 income overlaps with the time of spending data reported. For example Age 1 interviews in July of 2019 report spending from June of 2019. Age 2 income data refers to January through December of 2019. Spending data comes from Age 1 survey.

Table 2.4

Marginal propensities to consume child-focused goods from different household income sources

	BFY income	*Mothers' earned income*	*All other household income*	*N*
Total dollar amount spent on child-focused goods in last month	20.63** (7.48)	2.69* (1.23)	-0.04 (0.49)	816
Books	2.02** (0.57)	0.09 (0.07)	-0.04 (0.04)	809
Toys	6.10** (2.13)	0.41 (0.30)	0.22 (0.22)	811
Clothes/shoes	8.68 (5.44)	1.68* (0.69)	-0.30 (0.30)	814
Diapers	2.10 (1.54)	0.33 (0.24)	-0.02 (0.11)	811
Videos/apps	1.65 (1.47)	0.16 (0.14)	0.11 (0.13)	811
Money spent eating out per month	7.43 (7.02)	0.69 (0.99)	0.57 (0.70)	804

Notes. Robust standard errors in parenthesis. + $p<0.10$; * $p<0.05$; ** $p<0.01$
Model includes site fixed-effects.
Coefficients in the 'BFY income column' should be interpreted as the difference in spending between the low and high cash gift group. For example, mothers in the high cash gift group spend $20.63 more on child-focused goods than mothers in the low cash gift group.
Coefficients in the 'mothers' earned income' and the 'all other household' income columns should be interpreted as the association between a $100 increase in income on spending. For example, a $100 increase in mothers' earned income is associated with a $2.69 increase in spending on child-focused goods.
All income – including BFY income - is in monthly denominations and scaled to $100. BFY income takes a value of $3.33 for treatment mothers and $2.00 for control mothers.
All other household income includes income from: government sources, spouses (if present), anyone else who contributes to the household.
Covariates from baseline survey: Mother age, completed Schooling, net worth, general health, mental health, race/ethnicity, marital Status, number of adults in the household, number of other children born to the mother, smoked during pregnancy, drank alcohol during pregnancy, father living with the mother, child's sex, birth weight, Gestational age at birth, and total household income at baseline.

Table 2.5

Test of differences of marginal propensities to consume across household income

Hypothesis		Coefficient comparison	Difference	p-value of difference (two-tailed)	p-value of difference (one-tailed)
Child-focused goods					
(1)	The MPC child-focused goods from mothers' earned income will be greater than the MPC from all other household income.	$\beta_2 > \beta_3$	\$2.73	0.05	0.03
(2)	The MPC child-focused goods from the BFY income will be greater than the MPC from all other household income.	$\beta_1 > \beta_2 + \beta_3$	\$17.98	0.02	0.01
(3)	The MPC child-focused goods from the BFY income will be greater than the MPC from mothers' earned income.	$\beta_1 > \beta_2$	\$17.94	0.02	0.01
Other goods and services					
	The MPC general household expenditures (food eaten outside the home) from the BFY income will be the same as the MPC from all other household income.	$\beta_1 > \beta_2 + \beta_3$	\$6.17	0.40	0.20

Note. p-values are derived from a postestimation test of coefficients presented in Table 2.4. I include both one- and two-tailed tests because, while my hypotheses are directional, the two-tailed test is more conservative.

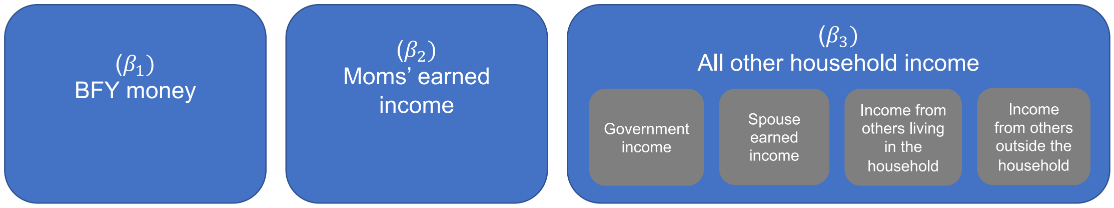

Figure 2.1

Household income categories

CHAPTER 3

Does Exposure to Lead in Schools Harm Students?

Lifetime income and responsible citizenship in a democratic society depend in part on learning in school (Angrist and Kreuger, 1991; Allen, 2016). Learning in school depends, in turn, on factors like teaching methods, curriculum, and the behavior of one's peers (Dee and Penner, 2016; Xu, Zhang, and Zhou, 2022; Clotfelter, Ladd, Vigdor, 2007). A less appreciated feature of schools that matters for student learning is the quality of the physical environment (cf. Heissel, Persico, Simon, 2020; Persico and Vernator, 2021). This chapter explores another potentially critical aspect of the school environment: lead in drinking fountains. Previous research has documented the detrimental effects of lead on various body systems, and it is estimated that lead is present in the water of a significant portion of U.S. schools (Aizer et al., 2018; Aizer and Currie, 2019; ATSDR, 2007; Lambrinidou, Triantafyllidou, and Edwards, 2010). However, the causal links between lead exposure in schools and educational outcomes have not been explored.

The current study uses lead readings from individual fixtures within schools and student observations over two years to isolate the effect of lead exposure on student outcomes. I construct a student-level lead exposure metric based on the classrooms students occupy and the drinking fountains within those classrooms. I model the effect of lead exposure by comparing students to themselves over time and comparing peers within the same school who have varying levels of lead exposure. These approaches hold constant individual-level and school-level factors to isolate the effect of lead exposure.

I find that higher lead exposure is associated with lower math test scores, although the results are sensitive to non-linear approaches. This finding aligns with the general understanding of lead's detrimental effects. However, it departs from existing literature that documents adverse effects on reading scores (Aizer et al., 2018; Aizer and Currie, 2019). I find no significant effect on absences or suspensions. Additionally, I find counterintuitive results: higher levels of lead are associated with higher reading scores and a lower likelihood of suspension, while math scores and absences remain similar across exposure levels.

Prevalence of lead in school drinking water

The Environmental Protection Agency (EPA) is responsible for regulating water quality in accordance with the Safe Drinking Water Act, which was established in 1974. However, the EPA's jurisdiction over water quality ends once the water reaches schools (Lambrinidou, Triantafyllidou, Edwards, 2010). While water is regularly tested for lead at the point of treatment, there is a risk of lead leaching from leaded pipes into the water between treatment and delivery. Leaded pipes were commonly used until they were prohibited by the Safe Drinking Water Act's Amendments in 1986. Nevertheless, most schools were built prior to 1986 (Triantafyllidou, 2012). In fact, research indicates that approximately 73 percent of all schools have lead present in their water (Patel and Hampton, 2011; Lambrinidou, Triantafyllidou, and Edwards, 2010). Recent examples include the discovery of lead in school water in Missouri, Delaware, and Montana (Liberman, 2023).

Different public health agencies and research associations have established various thresholds for acceptable water lead concentrations in schools. The EPA sets a threshold for action at 15 parts per billion (ppb) of lead in water. The American Academy of Pediatrics recommends a threshold of 1 ppb in schools, although there is no enforcement mechanism in place (Lanphear et al., 2016). As further illustration, North Carolina's threshold is 10 ppb, while Colorado's threshold is 5 ppb.

In response to the issue of lead in school drinking water, state and city governments have recently allocated significant funding for schools to test and remove lead pipes (Environmental Protection Agency, 2020; Kieffer, 2020). For instance, North Carolina recently announced a plan to use $150 million in COVID-19 funds for more extensive water lead testing (Norman and Redmon, 2023). Furthermore, Biden's infrastructure plan allocates billions of dollars to eliminate all lead pipes and service lines across the nation's schools (The White House, 2023).

Effect of lead on educational outcomes

According to the CDC (2023), no level of lead in a child's blood is considered safe. Even low levels of blood lead, defined by the CDC as under 5 micrograms per deciliter, have detrimental neurobehavioral effects on children (Needleman et al., 1990; Needleman and Gatsonis, 1991; Banks, Ferretti, Shucar, 1997). These effects include cognitive problems, impaired executive functioning, increased aggression, and decreased fine motor control (Cecil et al., 2008). When absorbed, lead generally has negative impacts on almost every system in the human body (Meyer, McGeehin, and Falk 2003).

Recent research has examined the impact of low blood lead levels on educational outcomes (Gronquist et al., 2014; Reyes, 2007, 2015; Ferrie et al., 2012). Aizer, Currie, Simon, Vivier (2018) linked blood lead levels of children under 6 years old with their third-grade test scores. The average blood lead level observed in children was 3.1 micrograms per deciliter, below the threshold for action set by the CDC. Using variation in the timing of lead remediation policies in children's homes, the authors discovered that reducing the mean blood lead levels before the age of 6 increased reading and math scores by .09 and .06 standard deviations, respectively. The effects were more pronounced at the tail of the test score distribution, with the same reduction in lead levels decreasing the likelihood of being below proficient in reading by 22 percent and in math by 13 percent.

In a subsequent study, researchers found that a one microgram per deciliter increase in blood lead levels among children aged three to five increased the likelihood of suspension in third grade by 6 percent for boys. The effect on suspensions for girls was small and estimates were imprecise (Aizer and Currie, 2019). Additionally, the authors found that the same increase in blood lead levels (one microgram per deciliter) increased the probability of detention for boys by 56 percent.

These studies clearly demonstrate that low levels of blood lead in early childhood and the remediation of lead in young children's home environments have measurable impacts on educational outcomes in third grade. However, the literature currently lacks causal links between lead exposure in schools and educational outcomes.

Current study

The current study investigates the effect of exposure to lead in school drinking water on concurrent student educational outcomes using a case of elevated water lead levels discovered in Portland, Oregon in 2016. The full timeline of events is shown in Figure 3.1. Elevated water lead levels were discovered in May of 2016. On May 27th, 2016, administrators shut off the water and began providing bottled water for the remainder of the year. From May until June 2016, Portland Public Schools conducted water lead testing at the fixture-level throughout the district. The lead concentrations of each fixture were made publicly available on the district's website (PPS.net, 2021). The district then began an extensive effort to remove lead service lines.

The current study uses the fixture-level lead data and links it to state administrative data that includes student classroom assignments for two school years – 2014-2015 and 2015-2016. Using these linked data, I construct a student-level lead exposure measure by linking students to water lead levels from drinking fountains in their classrooms. I estimate the effect of water lead exposure in two ways: first, by comparing students' test scores in different years when their lead exposure was higher or lower depending on the classrooms to which they were assigned; and second, by comparing students in a school to other students in the same school, who were exposed to more or less lead depending on their classroom assignment. These approaches hold constant individual- and school-level factors that correlate with both lead exposure and test scores, helping to isolate the unique effect of lead exposure in schools. Using these approaches, I estimate the effect of lead exposure on math and reading scores, as well as absences and suspensions. This is the first study of the effect of potential water lead exposure in schools.[28]

Methods

Data

Lead data

In May and June of 2016, the city of Portland contracted with a state-certified laboratory to conduct initial water testing of all cold-water fixtures in all public school

[28] The current study specifically examines the effect of external exposure (e.g., concentration in water) on educational outcomes. The remainder of the paper uses *exposure* and *potential exposure* interchangeably. The use of *exposure* in this study is distinct from other studies, like that of Gazze, Persico, and Spirovska (2022), which use *lead exposure* and *lead poisoning* interchangeably when referring to blood lead levels.

buildings and made the results available on their website. An example of the data available is shown in Figure 3.2, where each row is a test from a single fixture. As can be seen, the fixtures were labeled by type (i.e., sink, drinking fountain, faucet) and location (i.e., hallway, classroom, bathroom). Each fixture was tested for two metals –copper and lead. Water for testing was sampled using a first draw protocol.[29] The fixtures that exceeded the Environmental Protection Agency (EPA) threshold of 15 ppb were highlighted in yellow on the district's records.

A total of 9,848 fixtures in 78 schools were tested for lead. The average ppb concentration of lead in the water was 49 ppb, but the median was only 8 ppb, reflecting the presence of several very high readings. The mode was 2 ppb. The current study is restricted to a) fixtures labeled as water fountains or otherwise indicated to be for consumption; and b) those labeled as being in a classroom. This results in a total of 669 fixtures in 64 schools. Fixtures labeled as drinking fountains had markedly lower average lead levels than all fixtures, with a mean of 7 ppb, a median of 3 ppb, and a mode of 2 ppb. The mean water lead level is below the EPA's action threshold of 15 ppb.

Oregon Department of Education administrative data

I rely on de-identified student-level administrative data from the Oregon Department of Education, which includes all students enrolled in Portland Public Schools. The data include state standardized test scores (for the Fall of kindergarten and the spring for grades 3 through 8), and, for all grades, includes absences and suspensions from 2005

[29] According to the EPA, first-draw samples are "representative of the water that may be consumed at the beginning of the day or after infrequent use." Further, first-draw samples, "maximize the likelihood that the highest concentrations of lead will be found because the first…sample is collected after overnight stagnation (the water sat in the pipes for at least 8 hours)" (EPA, 2021). Therefore, the lead assays should be interpreted as an upper bound for the lead a particular fixture might leech into water over the course of a day.

through 2017. The data also include an indicator of the physical classroom in which a given student was enrolled for each of his or her courses for the 2014-2015 through 2016-2017 school years, and information on the number of school days spent in each physical classroom.

Measures

Student water lead exposure

Using these data sources, I construct a student-level measure of water lead exposure. I first construct a spell-level dataset that contains separate observations for each unique student-classroom spell. I link classroom IDs in the student-classroom level data to classroom water fixture lead levels (ppb). I then collapse the data to the student level, using an arithmetic mean and weighting the observations by the percent of the year the student spent in the given classroom.[30] Because the student water lead exposure metric is constructed using water lead levels, the student water lead exposure metric represents the

[30] An alternative approach would be to use a geometric mean (see, e.g., Aizer and Currie, 2019). We do not suspect that our results would differ if we used the geometric mean.

level of potential exposure (in ppb) in school drinking fountains.[31] I use *potential exposure* interchangeably with *exposure*.[32]

Outcomes

I use the state standardized math and reading test scores included in the administrative data for students in grades 3 through 8. In the 2014-15 through 2016-17 school years, the state administered the Smarter Balanced Assessment System. I further standardized math and reading test scores within grades. Math and reading tests are given in the spring of each school year. On average, math tests are administered 10.6 days after reading tests. I also rely on data on student absenteeism, which is measured as the proportion of the school year that students are absent. Lastly, I use an indicator of whether a student was suspended in a given year.

Threat to validity of student water lead exposure metric

Water lead assays were collected in May and June of 2016. I use these lead assays to construct the student water lead exposure measure for academic years 2014-2015 and 2015-2016. My analysis thus assumes that water fixtures leach lead into water in a rank-

[31] Existing research modelling BLLs from WLLs will consider the amount of lead ingested to be $WLL_i \times Q_i$, where Q_i represents the amount of water ingested at day i (Ngueta et al., 2016). Then, this value is multiplied by k, where k represents the gastrointestinal absorption rate of lead from the water. The student lead exposure metric has error. Sources of error might be correlated with test scores and other student characteristics. For example, a lower income student might drink more water from school fountains than higher income peers who may bring a water bottle from home. Nutrition also affects the degree to which lead is absorbed by the gastrointestinal tract. Students with worse nutrition might absorb more ingested lead than students with better nutrition, and nutrition might also be correlated with test scores. The preferred specification of this study compares a student to him or herself. Thus, within a student over time, we consider mismeasurement to be generally random, so should lead to attenuation bias. We do not have any data on how much students drank the water, or on student blood lead levels, which are typically not measured in children over 6 years old.

[32] I constructed alternative versions of student water lead exposure metric that included ppb water lead readings from sinks and drinking fountains in or near to classrooms. Because including sinks and fixtures outside of classrooms requires additional assumptions about drinking behavior, I chose to include only the in-classroom drinking fountains for the current study.

preserved manner over time, within schools. This assumption may or may not be true. To the degree that factors that contribute to lead leaching from pipes are relatively consistent over time, such as the age of a fixture or the temperature of the water (Cartier et al., 2011), I would expect fixtures to maintain the same rank of water lead levels relative to other fixtures in the same school. To the extent that the frequency of use of a particular fixture changes over time, the degree of lead leaching in the fixture will vary, as less frequently used fixtures will leach more lead (Triantafyllidou, 2012). For example, if a classroom is not used for a few years, and then at some point is used more frequently, this could result in a change in the rank of the lead in that fixture relative to other fixtures in a school building.

To assess whether fixtures in schools are leaching the same relative amount of lead into water over time, I use publicly available data from Portland Public Schools' fixture-level water lead testing conducted in 2012, four years prior to the lead testing used to construct the student exposure metric in the current study. By relying on fixture labels and locations in the 2012 and 2016 data, I was able to match lead readings from 143 unique fixtures across 45 schools. Using this linked data, I first rank fixtures within a school. I then correlate these rankings using a Spearman correlation. Within-school rankings of fixture lead levels are correlated at 0.71 ($p<0.00$). However, Spearman correlations of water lead levels (measured in ppb) across all observed fixtures in the district are not as highly correlated over time (Spearman's correlation=0.39; p-value<0.00). This suggests that rank order maintenance of fixture lead leaching within schools is a plausible assumption, but that fixtures across the district do not maintain their rank over time as highly as they do within schools.

Analytic Strategy

My first analytic strategy effectively compares students in the same school who were exposed to different levels of water lead. I estimate OLS and linear probability models, depending on the dependent variable, using the following specification:

$$y_{it} = B_0 + student\_exposure_{it} + y_{it-1} + \omega + e_i \quad (1)$$

where *y* is outcome (math score, reading score, ever suspended, proportion of school year absent) of student *i* in year t. $student\_exposure$ is student *i*'s exposure in year t and *y* is student *i*'s test score in year t-1. ω is a school fixed-effect and e is an error term. Standard errors are clustered at the student-level. The sample is pooled across the 2014-2015 and 2015-2016 school years to study the effect of exposure (see Figure 3.1 for a timeline). The coefficient of interest is on $student\_exposure_{it}$, and can be interpreted as the change in student *i*'s outcome from *t*-1 associated with an increase in 10 ppb exposure between t and *t*-1.

My second analytic strategy addresses additional potential bias stemming from student-level characteristics by incorporating a student fixed-effect (and dropping the lagged dependent variable, y_{it-1}). The model specified below compares students in a year when they were exposed to a higher level of lead to themselves in a year when they were exposed to a lower level of lead:

$$y_{it} = B_0 + student\_exposure_{it} + \partial + \omega + e_i \quad (2)$$

where all parameters are the same as model (1), with the additions of the student fixed-effect, ∂, and removal of the lagged dependent variable.

To assess the possibility that the effect of lead is worse for younger children (see Ngueta et al., 2016), I estimate models (1) and (2) by grade.

Lastly, to assess the possibility of non-linear effects of lead exposure on academic outcomes (Lanphear et al., 2005 and CDC 2004), I estimate models (1) and (2) replacing the continuous student exposure metric with indicators for quintiles of exposure, using the first quintile as the reference category. Following Ngeuta et al. (2016) and Aizer et al. (2018), I expect one of two possible monotonic patterns of exposure: first, that exposure has the greatest effect at low levels, or second, that exposure has the greatest effect at higher levels.

Results

Descriptive statistics of sample and lead exposure

The analytic sample includes students in grades 4 through 8 in the 2014-2015 and 2015-2016 school years who are in at least one classroom with a drinking water fountain with a lead assay and who have a current and lagged test score. Table 3.1 compares descriptive statistics for students included in the analytic sample with those excluded from the analytic sample in the same grades and years. The analytic sample includes about 14,290 student-year observations in grades 4 through 8, representing about 40 percent of the universe of Portland Public School students. The analytic sample is predominantly white, with Hispanic as the second largest racial/ethnic group. Additionally, 45 percent of the students are economically disadvantaged.

Students are generally similar on observable characteristics other than test scores. Students in the analytic sample are more likely to be white than students excluded from the sample (53 percent white compared to 59 percent white, respectively). The analytic sample has lower average reading and math test scores on average than students excluded from the analytic sample (with math test scores in the analytic sample averaging 0.12 standard deviations and an average of 0.21 in the comparable universe of students excluded from the sample and reading scores in the analytic sample averaging 0.12 standard deviations and 0.23 in the excluded students).

Table 3.1 also shows that students are exposed to an average of 7.2 ppb of water lead each year. The minimum exposure is undetectable (0 ppb), and the maximum lead exposure is 87 ppb.[33]

Looking within a student over the two years, the average range of ppb lead that an individual student is exposed to is 5.4 ppb, with a standard deviation of 8.7 ppb, a minimum of 0 (meaning they are exposed to the same amount over the study period), and a maximum range of 83 ppb.[34]

Association between exposure and educational outcomes

To assess the association between water lead exposure in schools and student outcomes, I first report bivariate results in the first columns of Table 3.2 (and Figure 3.3 shows the bivariate relationship visually). I successively add covariates and layers of fixed-effects in columns 2 through 4.

Looking at columns 1 in Panel A, I find that students in classrooms with higher levels of lead have lower math scores and reading scores, although the difference is statistically significant only for math. Looking at columns 1 in Panel B, I find that students who are in classrooms with higher water lead levels have fewer absences and are

[33] The kernel density of exposure is in Appendix Figure 3.A1.

[34] Black and Hispanic students are exposed to more water lead than their white peers; this mostly due to differences in water lead levels across schools as opposed to sorting into lead exposure within schools. For information on racial/ethnic and other demographic characteristic differences in student exposure, see Spiegel, Penner, Penner (2023).

marginally significantly less likely to be suspended. These results do not hold constant factors that are important for students' educational outcomes, such as their economic status or the school they attend. The following columns include student and school fixed-effects, which help to isolate the unique effect of exposure on outcomes.

Comparing students with varied exposure levels over time to themselves

In Table 3.2. columns 4 incorporate student and school fixed-effects, effectively controlling for student- and school-level characteristics that remain constant over time, such as the influence of a school principal and the economic status of students. I observe that students tend to have lower math scores in the year of higher levels of lead exposure compared to the year of lower levels of lead exposure (*b*=-0.035; se=0.010). However, I puzzlingly find that students have higher reading scores (*b*=0.033; se=0.013). I find that students have similar absence rates and suspension probabilities regardless of whether they are exposed to higher or lower lead.

To investigate the relationship between lead exposure and age, I examine the results by grade and present them in Table 3.3. First, I conducted a joint test of grade-exposure interaction coefficients in a fully interacted model. The findings indicate that the effect of exposure varies by grade for both math and reading. For math, contrary to the expectation that the youngest students would be the most affected and the harmful effects would decrease as students age, the impact of exposure on math scores is largest for the oldest students in the sample. Specifically, students transitioning from grades seven to eight show a significant negative effect of exposure (*b*=-0.073; se=0.022*)*, and to a lesser extent, students going from grades four to five (*b*=-0.061; se=0.023) and six to seven (*b*=-0.039;

se=0.019). Regarding reading scores, the effect of exposure is positive for all grades, which is the opposite of the expected effect.

In Appendix Table 3.A1, I explore the sensitivity of the observed negative effect on math scores to outlier exposure levels. Upon removing outlier values that exceed the 99th or 95th percentile of student exposure, I find that students still perform significantly worse on their math tests in the year with higher lead exposure.[35]

I also estimate the effect of lead exposure allowing the effect to vary based on the level of lead (see model (2) above). The results of this analysis are found in Appendix Table 3.A3. For reference, column 1 replicates the effect of exposure obtained from the linear estimation model, which is also reported in columns 4 of Table 3.2. Columns 2 through 4 in Appendix Table 3.A3 show the bivariate results of model (2) with successive covariates and fixed-effects. Columns 5 incorporates both student- and school-level fixed-effects.

A joint test reveals that the effect of exposure on math does not depend on the level of exposure, suggesting that a linear estimation strategy is appropriate. For reading scores, however, a joint test reveals that the effect of exposure varies statistically depending on quintile of exposure. Looking at column 5, I observe that the effect of level of lead exposure on reading scores follows a curvilinear pattern, which is not consistent with existing literature.

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[35]As another robustness check, I estimate the effect of exposure on math by year (academic years 2014-2015 and 2015-2016) in Appendix Table 3.A2. The exposure coefficient in Appendix Table 3.A2 can be interpreted as the effect of exposure on concurrent math test scores, comparing students within a school to each other, controlling for prior year test score, race/ethnicity, economic disadvantage status, and whether in special education. The first column shows the effect of exposure in academic year 2014-2015 and the second column shows the effect of exposure in academic year 2015-2015, the two years during which I have classroom information before water bottles were brought into schools. The results are null for both academic years. In academic year 2014-2015, a 10 ppb increase in water lead exposure is associated with an increase math test scores of .017 standard deviations (se=0.012), and 0.003 standard deviation decrease (se=0.010) in academic year 2015-2016, both statistically insignificant.

Comparing students with higher or lower exposures in the same school to each other

To further examine whether exposure to water lead in drinking fountains affects educational outcomes, I estimate the effect of lead exposure by comparing students exposed to higher and lower levels of lead within the same school. Model (1) drops the student-level fixed-effect but holds constant the school-level factors common to all students. This model assumes that the degree to which students are exposed to water lead within schools is unlikely to be correlated with individual characteristics because lead is invisible and thus cannot itself be considered in classroom assignment decisions (see Spiegel, Penner, Penner, 2023).

Comparing students with higher and lower levels of water lead based on their classroom assignment within a school in columns 4 of Table 3.2, the effect of exposure to higher levels of water lead is positive for both math and reading scores. In other words, students exposed to higher levels of lead within a school have higher math scores and reading scores than students exposed to lower levels of lead within the school. Further, as shown in Panel B, students exposed to higher levels of lead are statistically significantly less likely to be suspended and to be absent for a lower percentage of the school year, although this difference is not statistically significant.

I assessed the sensitivity of the counterintuitive, statistically significant positive effect on reading in several ways. First, I estimate model (1) with trimmed outlier observations of water lead exposure above the 95th and 99th percentiles, shown in Appendix Table 3.A4. I find that the positive and significant effect on reading is robust to trimmed outlier student exposure values.

In addition, I estimate model (1) using indicators for the quintile of water lead exposure and present the results in Appendix Table 3.A3. Columns 4 report the estimates from model (1). I find that the effect of exposure on both math and reading scores becomes increasingly positive as exposure increases. In other words, relative to students exposed to the first quintile of water lead, students exposed to the higher levels of lead have the highest reading and math scores.

Discussion

Lead in school drinking fountains may pose a significant threat to children's intelligence, ability to pay attention, and academic performance (CDC, 2023). In addition, millions of dollars are spent annually to remediate lead in school water sources (The White House, 2023). However, there is a lack of evidence on whether lead exposure from water in school drinking fountains is uniquely detrimental to students. The current study is the first to attempt to disentangle the effect of lead exposure in schools from other sources of exposure – and other characteristics of students and schools – to identify a causal relationship between exposure and educational outcomes.

First, I find that drinking fountains have lower lead levels than other fixtures in the school, and that the highest water lead levels come from fixtures that are not intended for consumption. Second, I find that in a year when students are exposed to a higher level of lead, they have lower math scores, but higher reading scores, and similar absenteeism, and likelihood of suspension as when they are exposed to lower levels of lead in another year. But I also find that the negative effect on math is not robust to alternative specifications. Moreover, the negative effect on math, but not reading, is inconsistent with other findings in the literature that show negative effects on reading but not math (see Aizer, Currie, Simon, and Vivier, 2018), as well as epidemiological research suggesting that lead has worse effects on verbal reasoning (Bellinger, Stiles, and Needleman, 1992 and CDC, 2004).

Third, counterintuitively, I find that students exposed to higher levels of lead within a school are also higher performing students. Students in classrooms with higher water lead levels have higher reading scores and lower likelihood of suspension, and small but insignificant higher math scores and a lower absenteeism.

A direct positive effect of lead exposure on test scores is implausible. This suggests the presence of a mechanism that leads to the sorting of students into higher water lead classrooms. In the following analysis, I explore potential mechanisms for sorting students into classrooms with higher lead levels and discuss possible explanations for the lack of clear, negative effects.

Why are higher-performing students exposed to higher levels of lead in schools?

What explains the positive effect of water lead exposure in schools? To answer this question, I quantify several characteristics of a classroom and correlate them with water lead levels in the classroom. In the following sections, I briefly explain the methods and results for three possible classroom characteristics that might correlate with lead levels in the following paragraphs.

First, classrooms facing certain cardinal directions (e.g., north, south, etc.) may receive more sunlight and thus be hotter than other classrooms within the same school. Scientists have documented that lead increases in water as temperatures rise (Cartier et al., 2011). Therefore, I first examined whether the cardinal direction of a classroom predicted lead levels in classroom water. I was able to code the cardinal direction of 378 classrooms in 59 schools based on a compass rose on school blueprints. I regressed classroom water lead on dummy indicators of cardinal direction, with northeast as the omitted category. The results are presented in Appendix Figure 3.A2. The results show that classroom cardinal direction is not a statistically significant predictor of classroom water lead. Therefore, I consider the classroom cardinal direction to be an unlikely sorting mechanism to explain the positive effect of water lead levels on educational outcomes.

Second, I examined whether classrooms that are close together within a school have more similar water lead levels than classrooms that are farther apart. This could indicate that water lead exposures within schools are not independent, perhaps due to shared plumbing, similar welding materials, or proximity to the point of entry of water from the municipal water supply into the school building. I used a binomial probability distribution function to assess whether neighboring classrooms have similar lead levels beyond what would be expected by chance. Of the 50 schools with applicable data examined,[36] approximately 37 had classrooms with shared lead levels beyond what would be expected due to chance, and 11 schools had classrooms with shared lead levels consistent with what would be expected by chance. This exercise suggests some degree of spatial patterning of water lead levels such that students may be sorted into areas of a building rather than specific classrooms.

Third, I asked whether the age of the classrooms within a school predicted lead levels in classroom water. Older buildings and older pipes are more likely to leach lead into the water than newer buildings and pipes (Sampson and Winter, 2016). Classrooms that

[36] I examined the publicly available blueprints for 67 schools. Of the 67 schools, 50 schools had water lead data for classrooms that were near to each other within the building. The remaining 17 were removed from the analysis because the available water lead data was for classrooms that were in different areas within a building, and so we were unable to assess spatial patterning for these schools.

were constructed as additions to a building and are therefore newer than other classrooms in the same building may also leach lower levels of lead. I use historical building records for schools in Portland, Oregon, that provided the date of construction for specific classrooms within a school. My analytic sample includes the age for 581 classrooms in 57 schools. In Appendix Figure 3.A3, I present a scatterplot of the classroom age and classroom water lead levels. In addition, I estimate the relationship between classroom age and classroom water lead levels using a school fixed-effect regression. I find that classroom age does not significantly predict classroom water lead levels. This suggests that the age or newness of a classroom is unlikely explaining why higher-achieving students are in classrooms with higher water lead levels.

Students are sorted into classrooms with higher water lead based on prior achievement. I find that students' lagged scores predict future classroom water lead levels (see Appendix Table 3.A5). Neither classroom age nor cardinal direction of classroom are likely reasons for the observed sorting into higher water lead classrooms. I find some evidence of spatial patterning on school water lead. Yet, the results suggest that lead exposure within schools is biased by prior achievement. Some mechanism is sorting higher-achieving students within a school into classrooms with higher water lead levels, making a conclusion about the true effect of water lead exposure unclear.

What explains the conflicting effects of lead exposure on educational outcomes?

As described above, I find that students have worse math scores during years they are exposed to higher levels of lead, but that this result is not robust to alternative specifications. Why might water lead exposure in this district context not be harmful for students? It is possible that students do not regularly consume water while at school. A study examining the water drinking behavior of middle school students discovered that approximately 30 percent of students were unlikely or very unlikely to drink water at school (Patel et al., 2014). Exploring students' drinking behavior in educational settings is crucial for understanding the risk associated with exposure to water lead in schools.

Additionally, different regions and governing bodies have established various action thresholds for water lead levels, recognizing the potential health risks associated with lead exposure. These thresholds typically range from 5 parts per billion (ppb) to 15 ppb, depending on the jurisdiction. The CDC has an action threshold of 15 ppb. It is possible that most of the time in school is spent being exposed to water lead that is not particularly harmful. I find that approximately 90 percent of the time that students spend in school is spent in classrooms that are below 15 ppb (the CDC action threshold), and 50 percent of time is spent in classrooms that are below 5 ppb. On the other hand, students spend 90 percent of their time in classrooms with water lead levels greater than 1 ppb, which is the most stringent threshold from the Academy of Pediatrics. It is possible that student lead exposure from classroom drinking fountains in Portland, Oregon was in a relatively safe range, although this depends on the threshold you are using, so more research is needed to provide a stronger evidence base for threshold action levels.

Furthermore, there is evidence to suggest that ingested water lead might not be readily absorbed into students' bloodstream. Studies have indicated that adults tend to excrete low levels of lead ingested through water rather than absorbing it into their bloodstreams (Lidsky and Schneider, 2003). Although children's gastrointestinal systems typically absorb higher quantities of ingested lead compared to adults, the relationship between age and gastrointestinal absorption is not yet well understood (National Research

Council, 1993). Some literature supports the notion that school-aged children may not be susceptible to harmful effects of lead exposure at the levels observed in school water fountains.[37] Specifically, researchers have suggested that further investigation of school drinking water impacts would only be warranted if younger children consume water or if water lead concentrations exceed those examined in their study, which were similar to levels observed in the current study (Sathyanarayana, Beaudet, Omri, Karr, 2006; p. 291).

In summary, the negative impact of water lead exposure on students' math scores in a year of higher lead exposure is sensitive to alternative specifications, and the effect on reading is significant and positive, casting doubt on the conclusive nature of the findings. One possible explanation could be that students either do not consume water or do not consume enough water to pose a risk, considering the observed levels of water lead and their age. Future research should delve deeper into students' drinking habits in schools, rely on multiple samples of water collected at various times of the day from individual water fixtures, and concurrently gather blood lead data from school-aged children. By addressing these factors, we can gain a better understanding of the risk that exposure to water lead in schools poses.

Conclusion

No level of lead in a child's blood is known to be safe (CDC, 2023). Recent research documents the effect of slightly elevated blood lead levels in preschool with later

[37] For instance, a study conducted in Seattle, Washington utilized a modeling technique to predict student blood lead levels based on water lead readings from 1,905 drinking fountains across 71 elementary schools (Sathyanarayana, Beaudet, Omri, Karr, 2006). The lead levels ranged from below the detection limit (less than 1 ppb) to 1600 ppb, with a median of 5 ppb. The study's worst-case scenario predictions estimated blood lead levels of 1.7 to 5.0 micrograms per deciliter. Consequently, the authors concluded that "elevated school drinking water lead concentrations are not a significant source of lead exposure in school-age children" (p. 288).

educational outcomes. Lead is present in the water of some 73 percent of U.S. schools. However, research has yet to identify a causal link between exposure to lead in schools and educational outcomes. This is the first paper to attempt to disambiguate the effect of lead exposure in school drinking water from other sources of exposure, and other student- and school- characteristics, on educational outcomes.

I find that in a year during which students are exposed to a higher water lead level, they have worse math scores than a year during which they were exposed to a lower water lead level, but similar absences and suspension rates. Moreover, the negative effect of exposure on math goes away when allowing for a non-linear relationship between lead exposure and outcomes. Further, I find that students exposed to higher levels of lead in a school have higher reading scores and a lower likelihood of suspension than students exposed to lower levels of lead within the same school. Exploratory analyses suggest that higher achieving students are sorted into classrooms within schools with higher levels of lead, though the mechanism explaining this sorting is elusive.

Table 3.1

Descriptive statistics of analytic sample compared with remainder of Portland Public School students; grades 4 through 8; pooled 2014-2015 through 2015-2016

	Included in analytic sample		Excluded from analytic sample		p-value of mean difference
	mean	*sd*	*mean*	*sd*	
White	0.53	0.50	0.59	0.49	0.00
Hispanic	0.18	0.38	0.15	0.36	0.00
Asian	0.10	0.30	0.07	0.26	0.00
Black	0.09	0.29	0.09	0.29	0.53
Multi-racial	0.09	0.28	0.09	0.29	0.52
American-Indian	0.01	0.09	0.01	0.09	0.73
Is in special education	0.14	0.34	0.18	0.39	0.00
Is economically disadvantaged	0.45	0.50	0.39	0.49	0.00
Math score	0.12	1.04	0.21	1.10	0.00
Reading score	0.12	1.06	0.23	1.08	0.00
Percent of year absent	0.05	0.05	0.05	0.05	0.00
Suspended	0.04	0.19	0.03	0.18	0.12
Water lead ppb (10s)	0.70	0.75	-	-	
Range of water lead ppb (10s)	0.54	0.87			
Observations	14290		22828		

Note. Observations are student-year. The analytic sample includes students with lead exposure readings and lagged test scores.

Table 3.2

Effect of exposure to lead on student outcomes, 2014-2015 and 2015-2016

Panel A: Test score outcomes

	Math				Reading			
	(1)	*(2)*	*(3)*	*(4)*	*(1)*	*(2)*	*(3)*	*(4)*
	Bivariate OLS	*OLS w/ lagged DV*	*School FE*	*Student + School FE*	*Bivariate OLS*	*OLS w/ lagged DV*	*School FE*	*Student + School FE*
Water lead ppb (10s)	-0.039**	-0.005	0.009	-0.035***	-0.016	0.014*	0.032***	0.033*
	(0.013)	(0.006)	(0.008)	(0.010)	(0.014)	(0.006)	(0.008)	(0.013)
Lagged math score		0.841***	0.810***			0.810***	0.762***	
		(0.005)	(0.005)			(0.005)	(0.006)	
Observations	14290	14290	14290	14290	14174	14074	14074	14174

Panel B: Behavioral outcomes

	Percent of school year absent				Likelihood of suspension			
	(1)	*(2)*	*(3)*	*(4)*	*(1)*	*(2)*	*(3)*	*(4)*
	Bivariate OLS	*OLS w/ lagged DV*	*School FE*	*Student + School FE*	*Bivariate OLS*	*OLS w/ lagged DV*	*School FE*	*Student + School FE*
Water lead ppb (10s)	-0.001	-0.000	-0.001	0.001	-0.005*	-0.003	-0.005*	0.007
	(0.001)	(0.000)	(0.000)	(0.001)	(0.002)	(0.002)	(0.002)	(0.004)
Lagged absent rate		0.730***	0.724***			0.297***	0.276***	
		(0.014)	(0.015)			(0.025)	(0.024)	
Constant	0.051***	0.017***	0.018***	0.071***	0.040***	0.030***	0.004*	0.102*
	(0.001)	(0.001)	(0.004)	(0.013)	(0.002)	(0.002)	(0.002)	(0.045)
Observations	14289	14280	14280	14173	14290	14290	14290	14173

Notes. Standard errors in parentheses.
All models are limited to the sample of students with a lead exposure metric and a lagged test score (grades four through eight).
Test scores are standardized by year-grade.
Observations are student-year.
* p<0.05; ** p<0.01; *** p<0.001

Table 3.3.

Effect of exposure on test scores by grades

	Math				Reading			
	(1)	*(2)*	*(3)*	*(4)*	*(1)*	*(2)*	*(3)*	*(4)*
	Grades 4 and 5	*Grades 5 and 6*	*Grades 6 and 7*	*Grades 7 and 8*	*Grades 4 and 5*	*Grades 5 and 6*	*Grades 6 and 7*	*Grades 7 and 8*
Water lead ppb (10s)	-0.061**	0.074**	-0.039*	-0.073**	0.004	0.099**	0.032	0.024
	(0.023)	(0.029)	(0.019)	(0.022)	(0.024)	(0.037)	(0.023)	(0.021)
Constant	-0.031***	0.007	0.268*	0.325***	0.123***	0.463*	0.129*	0.056
	(0.003)	(0.150)	(0.118)	(0.028)	(0.003)	(0.186)	(0.065)	(0.063)
Observations	3805	5469	7058	6763	3821	5493	7121	6836

Notes. Standard errors in parentheses and clustered at the student level.
Estimates are from by-grade specific regressions of model specification (2), which includes student and school fixed-effects.
A joint test of exposure interacted with grade coefficients from a fully interacted model suggests that the effect on math and reading is statistically different across grades, which p-values adjusted for multiple comparisons using Sidak's method. The p-value of the joint test of interacted coefficients is .02 for math and .00 for reading.

Figure 3.1

Timeline of potential for lead exposure, discovery of elevated water lead levels, and remediation in Portland, Oregon. *Source:* Associated Press, 2016

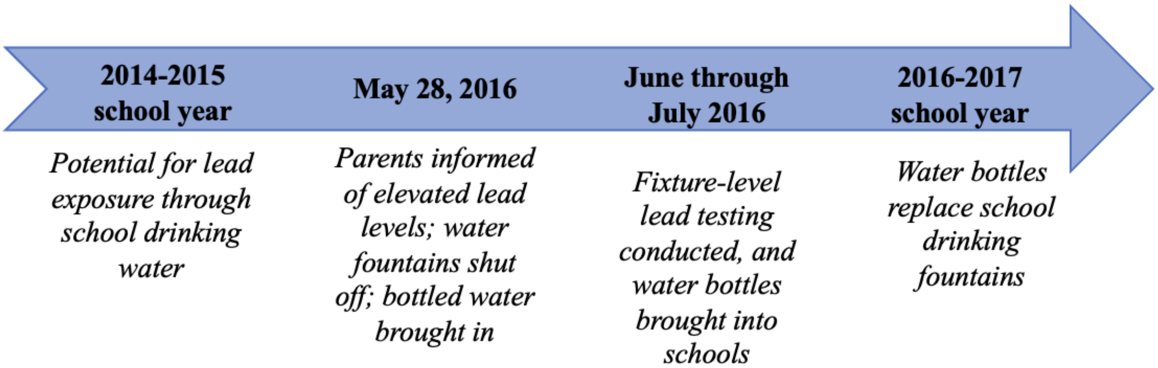


Figure 3.2

Example of Available Water Lead Data

Portland Public Schools
Lead and Copper Water Sampling Results
A (First Draw) Sample Data
School -redacted

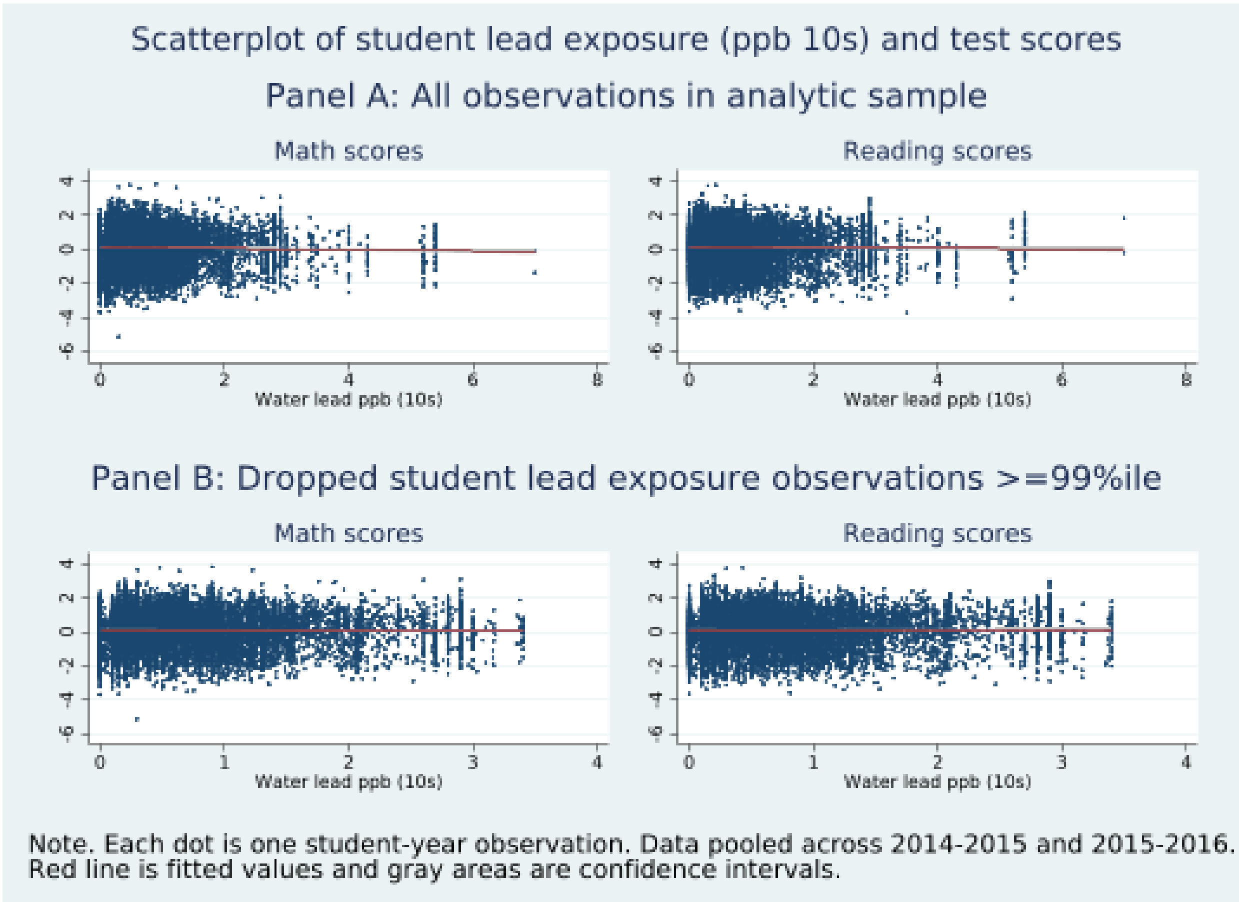
Sample Identification # and Location	Date & Time Collected	Date & Time Analyzed	ContainerID	Analyte	Result	EPA Action Limit	Units
002-BRS-01A Boiler Room Sink Faucet	6/17/16 8:39	6/24/16 14:52	606206001	Copper	0.0	1.3	ppm
002-BRS-01A Boiler Room Sink Faucet	6/17/16 8:39	6/24/16 14:52	606206001	Lead	5	15	ppb
002-K-02A Kitchen Sink Faucet Left, SW Dishwash	6/17/16 8:46	6/24/16 14:53	606206002	Copper	0.1	1.3	ppm
002-K-02A Kitchen Sink Faucet Left, SW Dishwash	6/17/16 8:46	6/24/16 14:53	606206002	Lead	3	15	ppb
002-K-03A Kitchen Sink Faucet Food Prep Left	6/17/16 8:48	6/24/16 14:53	606206003	Copper	0.1	1.3	ppm
002-K-03A Kitchen Sink Faucet Food Prep Left	6/17/16 8:48	6/24/16 14:53	606206003	Lead	21	15	ppb
002-K-04A Kitchen Sink Faucet Food Prep Middle	6/17/16 8:49	6/24/16 14:53	606206004	Copper	0.1	1.3	ppm
002-K-04A Kitchen Sink Faucet Food Prep Middle	6/17/16 8:49	6/24/16 14:53	606206004	Lead	2	15	ppb
002-K-05A Kitchen Sink Faucet Food Prep Right	6/17/16 8:49	6/29/16 14:24	606206005	Copper	0.1	1.3	ppm
002-K-05A Kitchen Sink Faucet Food Prep Right	6/17/16 8:49	6/29/16 14:24	606206005	Lead	4	15	ppb
002-KB-06A Kitchen Sink Faucet Bathroom	6/17/16 8:53	6/29/16 14:24	606206006	Copper	0.1	1.3	ppm
002-KB-06A Kitchen Sink Faucet Bathroom	6/17/16 8:53	6/29/16 14:24	606206006	Lead	5	15	ppb
002-KJC-07A Kitchen Sink Faucet Janitors Closet	6/17/16 8:56	6/29/16 14:25	606206007	Copper	0.1	1.3	ppm
002-KJC-07A Kitchen Sink Faucet Janitors Closet	6/17/16 8:56	6/29/16 14:25	606206007	Lead	7	15	ppb
002-K-08A Kitchen Right NE Sink Faucet Small White Sinl	6/17/16 8:58	6/29/16 14:25	606206008	Copper	0.2	1.3	ppm
002-K-08A Kitchen Right NE Sink Faucet Small White Sinl	6/17/16 8:58	6/29/16 14:25	606206008	Lead	23	15	ppb
002-BGF-09A Girls Bathroom Faucet 1	6/17/16 9:02	6/29/16 14:25	606206009	Copper	0.2	1.3	ppm
002-BGF-09A Girls Bathroom Faucet 1	6/17/16 9:02	6/29/16 14:25	606206009	Lead	10	15	ppb
002-BGF-10A Girls Bathroom Faucet 2	6/17/16 9:02	6/29/16 14:25	606206010	Copper	0.1	1.3	ppm
002-BGF-10A Girls Bathroom Faucet 2	6/17/16 9:02	6/29/16 14:25	606206010	Lead	14	15	ppb
002-DF-11A Drinking Fountain	6/17/16 9:08	6/29/16 14:36	606206011	Copper	0.0	1.3	ppm
002-DF-11A Drinking Fountain	6/17/16 9:08	6/29/16 14:36	606206011	Lead	3	15	ppb
002-DF-12A Drinking Fountain	6/17/16 9:08	6/29/16 14:36	606206012	Copper	0.0	1.3	ppm
002-DF-12A Drinking Fountain	6/17/16 9:08	6/29/16 14:36	606206012	Lead	2	15	ppb
002-JSC-13A Janitors Closet Sink Faucet	6/17/16 9:10	6/29/16 14:25	606206013	Copper	0.1	1.3	ppm
002-JSC-13A Janitors Closet Sink Faucet	6/17/16 9:10	6/29/16 14:25	606206013	Lead	11	15	ppb
002-BBF-14A Boys Bathroom Faucet 1	6/17/16 9:13	6/29/16 14:25	606206014	Copper	0.1	1.3	ppm
002-BBF-14A Boys Bathroom Faucet 1	6/17/16 9:13	6/29/16 14:25	606206014	Lead	10	15	ppb
002-BBF-15A Boys Bathroom Faucet 2	6/17/16 9:13	6/29/16 14:38	606206015	Copper	0.1	1.3	ppm
002-BBF-15A Boys Bathroom Faucet 2	6/17/16 9:13	6/29/16 14:38	606206015	Lead	14	15	ppb
002-BBS-16A Boys Bathroom Spigot	6/17/16 9:14	6/29/16 14:38	606206016	Copper	0.2	1.3	ppm
002-BBS-16A Boys Bathroom Spigot	6/17/16 9:14	6/29/16 14:38	606206016	Lead	112	15	ppb
002-GBS-17A Girls Bathroom Spigot	6/17/16 9:04	6/29/16 14:38	606206017	Copper	0.1	1.3	ppm
002-GBS-17A Girls Bathroom Spigot	6/17/16 9:04	6/29/16 14:38	606206017	Lead	39	15	ppb
002-CR2-18A Classroom 2 Sink Faucet Left	6/17/16 9:17	6/29/16 14:38	606206018	Copper	0.1	1.3	ppm
002-CR2-18A Classroom 2 Sink Faucet Left	6/17/16 9:17	6/29/16 14:38	606206018	Lead	17	15	ppb
002-CR2-19A Classroom 2 Sink Faucet Right	6/17/16 9:19	6/29/16 14:38	606206019	Copper	0.2	1.3	ppm
002-CR2-19A Classroom 2 Sink Faucet Right	6/17/16 9:19	6/29/16 14:38	606206019	Lead	12	15	ppb
002-CR1-20A Classroom 1 Sink Faucet	6/17/16 9:22	6/30/16 19:13	606206020	Copper	0.8	1.3	ppm
002-CR1-20A Classroom 1 Sink Faucet	6/17/16 9:22	6/29/16 14:38	606206020	Lead	10	15	ppb
002-CR1-21A Classroom 1 Sink Faucet Drinking	6/17/16 9:22	6/30/16 19:13	606206021	Copper	1.1	1.3	ppm
002-CR1-21A Classroom 1 Sink Faucet Drinking	6/17/16 9:22	6/29/16 14:38	606206021	Lead	6	15	ppb
002-CR3-22A Classroom 3 Sink Faucet	6/17/16 9:25	6/29/16 14:38	606206022	Copper	0.4	1.3	ppm
002-CR3-22A Classroom 3 Sink Faucet	6/17/16 9:25	6/29/16 14:38	606206022	Lead	4	15	ppb

Analyzed by: Pixis Labs   Reviewed by: TRC Environmental Corp   Page 1 of 6

Notes. This example comprises one of six pages of fixture-level lead readings for one specific school. Each fixture appears twice – one entry for lead results and one entry for copper results. The first column includes information about the fixture location and type. The sixth column includes the results from the test in parts per billion (ppb). Fixtures than tested above the EPA's Action Limit are highlighted in yellow. Other information provided includes the dates and times the sample was collected and analyzed and the container ID. The current study is restricted to lead results, fixtures labeled as used for drinking, and fixtures that are in classrooms. We linked fixtures to classrooms using publicly available school blueprints.

Figure 3.3

Scatterplot of student lead exposure (ppb 10s) and test scores



CHAPTER 4
SUMMARY AND CONCLUSION

Children are the future workforce and are our future voters. Low-income children have fewer resources to grow and thrive. The United States government allocates resources to low-income children to equalize opportunity, and these resources have positive effects for children and society (Hoynes, Schanzenbach, Almond, 2016; Currie, 2001). The three studies in this dissertation shed light on measurement and policy design concerns of three societal investments in low-income children. In the following sections, I summarize each study, discuss its implications for each policy area, and suggest potential future directions for research. Finally, I reflect on general lessons that can be drawn from the three studies to improve the effectiveness of societal investments in children.

Chapter one

Income-based gaps in academic achievement are profound (Reardon 2011; Hashim et al., 2020). A wide array of educational policies and practices attempt to bridge socioeconomic gaps in educational opportunity. To target opportunities efficiently and effectively to low-income students and the schools that educate them, researchers and policymakers require accurate measures of school economic disadvantage. Historically, data on students' enrollment in free or reduced-price lunch (FRPL) has been central to compensatory resource allocation. However, FRPL has long been recognized as an imperfect proxy for family income (Domina et al. 2019; Harwell & LeBeau 2010). The implementation of the National School Lunch Program's Community Eligibility Provision (CEP) further undermines the reliability of FRPL data. Therefore, scholars and policymakers have been searching for and constructing new measures of school economic disadvantage (Greenberg, 2019).

By linking state education administrative data with IRS tax records and program participation data housed at the U.S. Census Bureau, the current study comprehensively examines the validity of different measures of school economic disadvantage. I find that a measure relying on student enrollment in means-tested programs provides a more accurate representation of school economic disadvantage relative to other available measures. I also find that neighborhood-based measures of school economic disadvantage perform poorly in capturing the true economic disadvantage rate of schools.

The results of the study - namely, the validity of direct certification as a measure of school economic disadvantage - holds considerable importance for education policy and research. Firstly, having a valid and readily available measure of school economic disadvantage in a context of the worsening quality of FRPL-based measures enables policymakers to target resources and interventions more efficiently and effectively. By accurately identifying low-income students and the schools that serve them, policymakers can allocate resources where they are most needed, ultimately working towards reducing socioeconomic gaps in educational opportunities.

Secondly, the evidence for the validity of direct certification as a measure of economic disadvantage enables the promotion of equity considerations when evaluating educational policies. Without a valid measure of school economic disadvantage, policymakers and researchers are left in the dark about the state of opportunities and the academic progress of economically disadvantaged students. Having validated enrollment in means-tested programs as a sound measure of school economic disadvantage, policymakers can make more informed decisions and ensure that policies and practices support schools that serve economically disadvantaged students.

Moreover, the study's findings shed light on the limitations of FRPL enrollment as a proxy for school economic disadvantage. While research has long acknowledged the limitations of FRPL, the current study provides empirical evidence on the ways in which it is further deteriorating in quality in the post-CEP era. The results show that once universal meals programs are implemented, FRPL data begins to overestimate the true proportion of economically disadvantaged students. With this information, policymakers and researchers should recognize the urgent need to explore opportunities to link administrative data, such as IRS tax records and program participation data, with education administrative data, to construct measures of economic disadvantage based on enrollment in means-tested programs. While many states already link program administrative data with education administrative data, and more states are beginning to do so, the results of this study emphasize the importance of these efforts.

In conclusion, the study's findings regarding the validity of direct certification as a measure of school economic disadvantage have significant policy implications. The findings inform the targeting of resources, evaluation of policies, and promotion of equity in education policy and practice. By employing measures of school economic disadvantage derived from enrollment in means-tested programs, researchers, policymakers, and school administrators can take meaningful steps towards reducing educational inequalities and providing all students with opportunities to succeed.

Chapter two

Research consistently documents the positive effect of income on children's development and life opportunities (Blau, 1999; Akee et al., 2010). Policymakers and researchers are particularly interested in understanding the causal impact of income on children's development. (Duncan, Brooks-Gunn, Klebanov, 1994; Brooks-Gunn, Duncan, 1997; Currie, 1998). However, existing literature has yet to explore how diverse income sources may shape parental spending on children in low-income families, which in turn affects children's development.

The second study draws on data from a randomized control trial of the causal impact of an unconditional cash transfer on child development. It uses disaggregated household income data to examine the likelihood of spending on children from different income sources: the unconditional cash transfer, mothers' earned income, and other household income. The findings reveal that not all money within the household is equally likely to be spent on children. The unconditional cash transfer, specifically labeled as "For My Baby", is the most likely to be allocated to child-focused expenditures, followed by mothers' earned income, and finally, all other household income.

What does showing that low-income families demarcate specific money for children, and are more inclined to spend an unconditional cash transfer on their children, compared to mothers' income and other household income, mean for cash transfer policies? It suggests the meaning attached to money is a pertinent factor in child-related spending, even in the context of limited economic resources. The study shows that economic scarcity does not negate the influence of social significance of money on spending decisions on children. In other words, even in conditions of economic hardship, mothers prioritize specific money for spending on children, and particularly so when money is labelled as "For

My Baby". Future policy research should explore the specific social or physical attributes of money that shape why cash transfers are spent for children within low-income families.

Moreover, the study highlights the significance of mothers' earned income in contributing to child-related expenditures. Policymakers can consider pursuing legislation that enhances mothers' economic standing in families, such as promoting pay equity and increasing the availability of childcare arrangements. The current research shows that these policies are likely to translate into increased investments in children. However, it is also important that policies are designed to strike a balance between various aspects of child development, including intellectual stimulation through books and educational resources, and meeting children's basic needs. Striking this balance will support comprehensive and holistic child outcomes.

Chapter three

Exposure to lead has detrimental effects on students' educational outcomes (Aizer and Currie, 2018). Furthermore, elevated blood lead levels are disproportionately found among low-income children (Currie, 2011). Research supports the notion that schools, particularly for low-income children, are a significant source of lead exposure (Latham and Jennings, 2022). In recognition of this issue, Biden's infrastructure plan has allocated $15 billion for the removal of leaded pipes serving schools and daycare facilities. However, to date, there has been limited investigation into the impact of water lead exposure in schools on educational outcomes.

The third chapter examines whether exposure to lead in school drinking fountains adversely affects students. By using within-school and within-student differences in lead exposure, I aim to estimate the direct effect of exposure on students' concurrent educational outcomes, independent of other school- and student-level characteristics. The findings indicate that water lead exposure in schools does not appear to adversely affect students' reading scores, absences, or suspensions. I find that lead exposure negatively affects students' math scores but that this result is not robust to a non-linear specification, and the finding is inconsistent with previous research that suggests a negative effect on reading rather than math (Aizer et al., 2017).

Additionally, the study uncovers an intriguing pattern where students with higher test scores tend to be placed in classrooms with higher lead levels. I find suggestive evidence that certain areas within a school might have higher lead levels than others, but the specific characteristics that lead to the sorting of higher-achieving students into these areas remain elusive.

What do these unexpected findings mean for environmental and education policy? Can remediating lead in school drinking fountains enhance students' learning? Unfortunately, the current study provides inconclusive evidence. To establish a true null effect of exposure, I would expect to see a series of null results. Instead, some evidence of a negative effect on math scores is found, but the effect is not robust across alternative specifications. Moreover, other counterintuitive findings, such as the positive effect on reading across empirical approaches, raises doubts about the quality of the data. While theoretical and empirical literature suggests that any level of lead exposure is harmful, the current study cannot offer empirical support either for or against water lead remediation policies.

To better inform the question of whether lead exposure in schools harms students, researchers might consider collecting more detailed information on students' water consumption habits and investigate the absorption and bioavailability of lead exposure through water in school-aged children. Longitudinal studies that track both water lead levels in schools and blood lead levels in school-aged children would also provide valuable insights as to whether water lead exposure in schools harms students. These studies would help public health officials to establish more precise thresholds for lead in school water systems and ensure the safety of all students.

Researchers and policymakers might also be interested in considering the issue of lead in schools through an environmental justice lens. Environmental justice emphasizes the fair distribution of environmental burdens and benefits, ensuring that no group bears a disproportionate burden of environmental hazards (Mohai, Pellow, and Roberts, 2009). From an environmental justice perspective, it is essential to address any unequal exposure of specific student groups to lead in school water systems, even if the exposure does not have a direct negative impact on their educational achievement. Even in the absence of clear negative effects, from an environmental justice standpoint, there remains an ethical imperative to address disproportionate environmental burdens. Relatedly, the precautionary principle of public health states that it is important to protect students from potential harm even in the absence of documented negative effects.

Enhancing social policy for low-income children: Common themes and policy implications from the three chapters

For redistributive policies to have their intended positive effect, it is essential that resources are allocated in a manner that effectively benefits children. The three studies in this dissertation demonstrate that allocating resources so that they benefit children is not necessarily straightforward. Together, the three studies highlight the need for careful attention to multiple factors when it comes to targeting resources to children in ways that matter for their well-being. First, they draw attention to the need to carefully measure key study and policy constructs to target resources more efficiently. Second, they demonstrate the importance of interdisciplinary knowledge for implementing effective policies.

First and foremost, having valid measures of constructs relevant to research and policy is crucial. The first study shows that not all measures of school economic disadvantage accurately capture the proportion of students who are low-income. Without close analysis of measures of school economic disadvantage, resources may not reach the schools that need them, and policymakers and administrators may not have a clear understanding of how well their schools are serving economically disadvantaged students. The second study brings the importance of valid measures and carefully thought-out constructs to the fore as well. Common research and policy practices such as understanding of household income as pooled or fully interchangeable can obscure more nuanced relationships between household resources and child development. By paying close attention to the money dynamics within a household, and critically examining the conceptualization of money, researchers can better inform cash-welfare policy towards improving child outcomes. In summary, these studies underscore the importance of accurate and valid measurement in various domains, including education and social policy, to support child development and address disparities in outcomes.

The three studies also highlight the importance of interdisciplinary knowledge in the effective targeting of resources to maximize their potential impact. For example, identifying the effect of water lead exposure in schools requires expertise from econometrics, epidemiology, and biology, and chemistry. Each discipline bears on aspects of the important and policy-relevant question of whether exposure to lead in schools harms students. Lastly, thorough understanding of the dynamics of money in the household requires the tools and knowledge of multiple disciplines, including sociology, psychology, and economics. While initiatives such as lead remediation in schools or providing financial support to families may appear like straightforward paths for improving child outcomes, the three studies show that specific contexts are more complicated, and high-quality research requires the expertise of scholars across disciplines. A comprehensive and inter-disciplinarily approach is essential to conducting research and implementing policy that truly enhances children's well-being.

In summary, these three studies underscore the critical need for careful attention when allocating resources to children. The three studies bring to the fore both the importance of interdisciplinarity for effectively answering policy-relevant research questions and the valid measurement and understanding of key study constructs. Whether it is addressing the risks of lead exposure, implementing social welfare programs, or channeling money to schools, policymakers would benefit from harnessing interdisciplinary knowledge to improve child outcomes. The three studies in this dissertation bring us a step closer to achieving this important goal.

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APPENDIX TO CHAPTER 1

Appendix Table 1.A1

Enrollment weighted descriptive statistics of analytic sample and the universe of schools in ODE data, 2009-10 through 2016-17

	Universe of schools in ODE data		Analytic sample	
	Mean	*Std. Dev.*	*Mean*	*Std. Dev.*
Benchmark measures				
Benchmark (SNAP/IRS)	0.508	0.186	0.506	0.185
Benchmark (IRS)	0.435	0.172	0.433	0.171
Traditional FRPL enrollment measures				
CCD NSLP	-	-	0.522	0.211
ODE Economic Disadvantage	0.530	0.245	0.539	0.242
Direct certification measure				
SNAP*	0.346	0.165	0.345	0.164
Alternative candidate measures				
ACS Block Group*	0.327	0.092	0.327	0.093
Urban Institute MEPS+	-	-	0.176	0.075
NCES-SNP (Income-to-poverty ratio)+	-	-	304.9	140.5
Transformed NCES-SNP+	-	-	0.399	0.106
Demographic characteristics				
Proportion Black	0.025	0.052	0.024	0.048
Proportion White	0.646	0.190	0.641	0.191
Proportion Hispanic	0.216	0.173	0.221	0.174
Proportion Multi-ethnic	0.049	0.032	0.048	0.031
Proportion American-Indian	0.016	0.041	0.016	0.041
Proportion Asian	0.048	0.062	0.049	0.063
Proportion ESL	0.220	0.219	0.227	0.219
School Enrollment	720.5	564	733.5	520.2
N (School-years)	12000		8500	

Source: ODE, IRS 1040, OR SNAP

DRB Approval Numbers: CBDRB-FY2022-CES010-023, CBDRB-FY21-CES014-017.

Notes. Statistics are enrollment weighted.

*Available for >99% of school-year observations in the full sample, but missing for a small number of school-years. Measures with suppressed values in the full sample have many more missing values. By construction, all measures are available for all schools in the analytic sample.

+ Urban Institute MEPS measures are available 2013-2017; the NCES-SNP measure used in the analysis is from 2017.

APPENDIX TO CHAPTER 2

Appendix Table 2.A1

Marginal propensities to consume child-focused goods from different household income sources using baseline income

	BFY income	Mother earned income	All other household income	N
Total dollar amount spent on child-focused goods in last month	21.37**	2.20*	-0.37	761
	(7.91)	(1.10)	(0.69)	
Money spent on Books	2.12**	0.04	0.06	756
	(0.59)	(0.07)	(0.07)	
Money spent on Toys	5.77*	0.78*	-0.04	756
	(2.25)	(0.38)	(0.23)	
Money spent on Clothes/shoes	9.33	1.17+	-0.49	760
	(5.74)	(0.61)	(0.38)	
Money spent on Diapers	2.25	0.05	0.06	757
	(1.62)	(0.22)	(0.17)	
Money spent on Videos/apps	1.70	0.11	0.04	757
	(1.59)	(0.20)	(0.17)	
Money spent eating out per month	5.99	0.40	0.95	753
	(7.32)	(1.56)	(0.87)	

Notes. Robust standard errors in parenthesis. + $p<0.10$; * $p<0.05$; ** $p<0.01$
Model includes site fixed-effects.
Coefficients in the 'BFY income column' should be interpreted as the difference in spending between the low and high cash gift group. For example, mothers in the high cash gift group spend $21.37 more on child-focused goods than mothers in the low cash gift group.
Coefficients in the 'mothers' earned income' and the 'all other household' income columns should be interpreted as the association between a $100 increase in income on spending. For example, a $100 increase in mothers' earned income is associated with a $2.20 increase in spending on child-focused goods.
All income – including BFY income - is in monthly denominations and scaled to $100. BFY income takes a value of $3.33 for treatment mothers and $2.00 for control mothers.
Incomes are converted to monthly amounts by dividing the reported yearly amount by 12.
All other household income includes income from: government sources, spouses (if present), anyone else who contributes to the household.
Covariates from baseline survey: Mother's age, Completed Schooling, Net Worth, General Health, Mental Health, Race and Ethnicity, Marital Status, Number of adults in the household, Number of other children born to the mother, Smoked during pregnancy, Drank alcohol during pregnancy, Father living with the mother, Child's sex, Birth weight, Gestational age at birth.

Appendix Table 2.A2

Test of differences of marginal propensities to consume across household monies using income measured at baseline

Hypothesis		**Coefficient comparison**	**Differenc e**	**p-value of difference (two-tailed)**	**p-value of difference (one-tailed)**
Child-focused goods					
(1)	The MPC child-focused goods from mothers' earned income will be greater than the MPC from all other household income.	$\beta_2 > \beta_3$	$2.57	0.04	0.02
(2)	The MPC child-focused goods from the BFY cash gift will be greater than the MPC from all other household income.	$\beta_1 > \beta_2 + \beta_3$	$21.74	0.02	0.01
(3)	The MPC child-focused goods from the BFY cash gift will be greater than the MPC from mothers' earned income.	$\beta_1 > \beta_2$	$19.17	0.02	0.01
Other goods and services					
	The MPC general household expenditures (food eaten outside the home) from the BFY cash gift will be the same as the MPC from all other household income.	$\beta_1 > \beta_2 + \beta_3$	$4.64	0.53	0.27

APPENDIX TO CHAPTER 3

Appendix Table 3.A1

Sensitivity of within-student effect on math scores to exposure outlier cutoffs at 99th and 95th percentile of water lead level

	(1)	*(2)*	*(3)*
	All observations	*Dropped at 99th percentile WLLs*	*Dropped at 95th percentile WLLs*
Water lead ppb (10s)	-0.035**	-0.031*	-0.050*
	(0.010)	(0.013)	(0.019)
Observations	14290	14213	13508

Notes. Column 1 reproduces the estimate shown in Table 3.2 column 4. Standard errors in parentheses are clustered at the student level. Models include student and school fixed-effects.
* p<0.05; ** p<0.01; ** p<0.01

Appendix Table 3.A2

Effect of exposure on math test scores, by year

	(1) *2014-2015*	*(2)* *2015-2016*
Water lead ppb (10s)	0.017	-0.003
	(0.012)	(0.010)
School Fixed-effects	X	X
Student covariates	X	X
Observations	7465	6825

Notes. Standard errors clustered at the student level. Models include school fixed-effects, lagged test scores, and control for student race/ethnicity, economic disadvantage status, and whether special education.

* p<0.05 ** p<0.01 *** p<0.001

Appendix Table 3.A3

Non-linear specification of exposure on test scores

	Math					Reading				
	(1)	(2)	(3)	(4)	(5)	(1)	(2)	(3)	(4)	(5)
	Linear effect (replicates Column 4 from Table 3.2)	*Bivariate OLS*	*OLS w/ lagged DV*	*School FE w/ lagged DV*	*Student + School FE*	*Linear effect (replicates Column 4 from Table 3.2)*	*Bivariate OLS*	*OLS w/ lagged DV*	*School FE w/ lagged DV*	*Student + School FE*
Water lead ppb (10s)	-0.035**					0.033*				
	(0.010)					(0.013)				
First quintile (0-1.9ppb)	omitted category					omitted category				
Second quintile (1.9-3.0ppb)		0.301***	0.054***	0.041*	0.039		0.294***	0.061***	0.042*	-0.024
		(0.029)	(0.014)	(0.018)	(0.028)		(0.030)	(0.015)	(0.021)	(0.030)
Third quintile (3.0-6.7ppb)		0.225***	0.030*	0.037	0.012		0.240***	0.039*	0.052*	-0.014
		(0.030)	(0.015)	(0.020)	(0.029)		(0.030)	(0.016)	(0.023)	(0.031)
Fourth quintile (6.7-10.7ppb)		0.178***	0.051***	0.041*	0.012		0.234***	0.102***	0.101***	0.040
		(0.029)	(0.014)	(0.021)	(0.030)		(0.030)	(0.016)	(0.024)	(0.031)
Fifth quintile (10.7+ppb)		0.074*	0.037**	0.056**	-0.018		0.099**	0.073***	0.120***	0.031
		(0.030)	(0.014)	(0.021)	(0.030)		(0.031)	(0.015)	(0.023)	(0.031)
Observations	14290	14290	14290	14290	14290	14174	14174	14074	14074	14074

Notes. Standard errors in parantheses. All models are limited to the sample of students with a lead exposure measure and a lagged test score (grades four through eight). Test scores and standardized by year-grade. Observations are student-year. A joint test of the statistical significant of the quintile exposure indicator variables indicatesthat the effect of exposure on math does not depend on the level of exposure (p=.25) but does depend on level of exposure for reading (p=.06).
* p<0.05; ** p<0.01; *** p<0.001

Appendix Table 3.A4

Sensitivity of within-school effect on reading scores to exposure outlier cutoffs of at 99th and 95th percentile of WLL

	(1)	*(2)*	*(3)*
	All observations in analytic sample	*Dropped above 99th percentile exposure*	*Dropped above 95th percentile exposure*
Water lead ppb (10s)	0.034***	0.041***	0.078***
	(0.008)	(0.010)	(0.016)
Lagged reading score	0.763***	0.762***	0.761***
	(0.006)	(0.006)	(0.006)
Constant	0.221***	0.215***	0.182**
	(0.064)	(0.064)	(0.065)
Observations	14437	14351	13643

Notes. Estimates are from model (2). Column 1 reproduces estimates from the reading results of Table 2, Column 3. Standard errors clustered at the student level. Models include school fixed-effects and lagged test scores.

* $p<0.05$ ** $p<0.01$ *** $p<0.001$

Appendix Table 3.A5

Lagged variables predicting lead exposure in following year, pooled 2014-2015 and 2015-2016

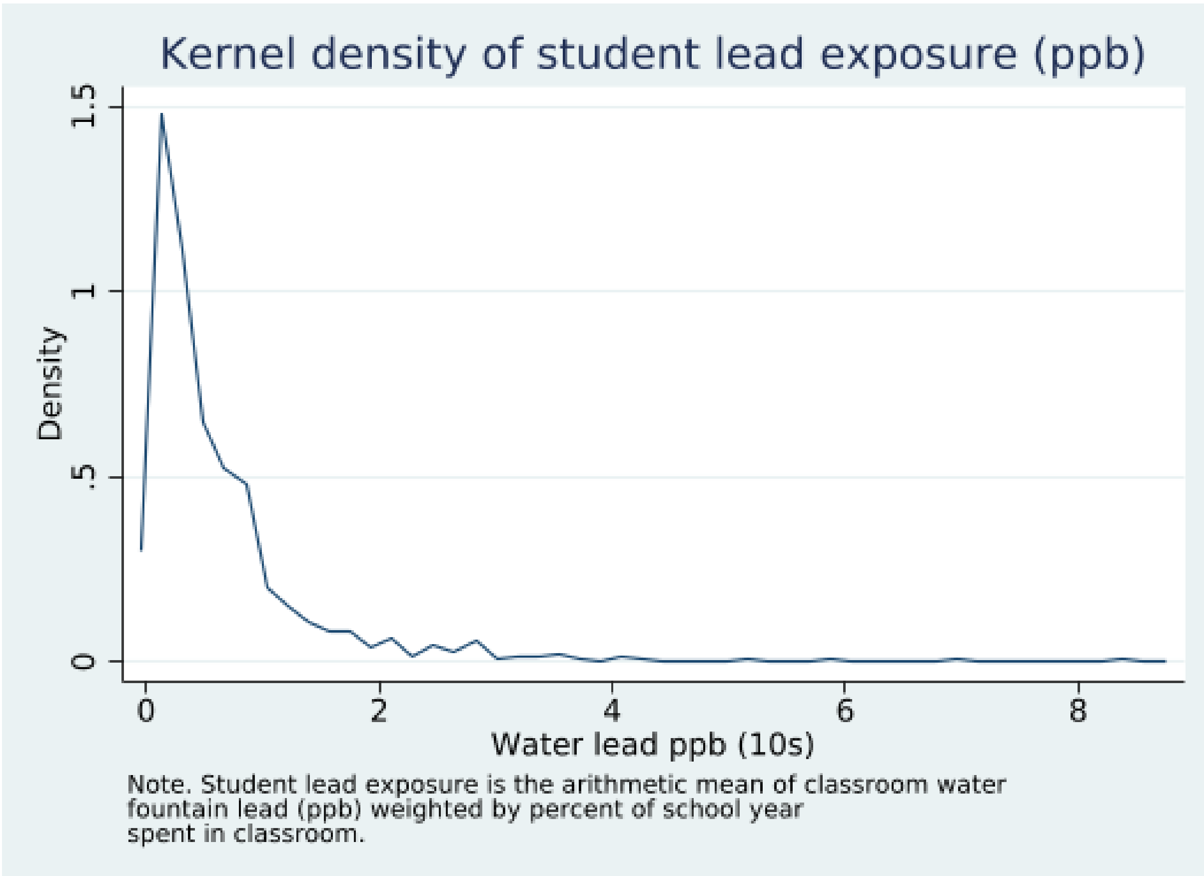
	(1)
	School FE
Lagged math score	0.015**
	(0.005)
Lagged reading score	0.016**
	(0.005)
Lagged absences	-0.058
	(0.11)
Lagged suspensions	-0.061*
	(0.024)

Notes. Robust standard errors in parentheses. Outcome is ppb water lead exposure (10s). Estimates are from separate regressions with individual lagged variable predicting water lead.

* p<0.05 ** p<0.01 *** p<0.001

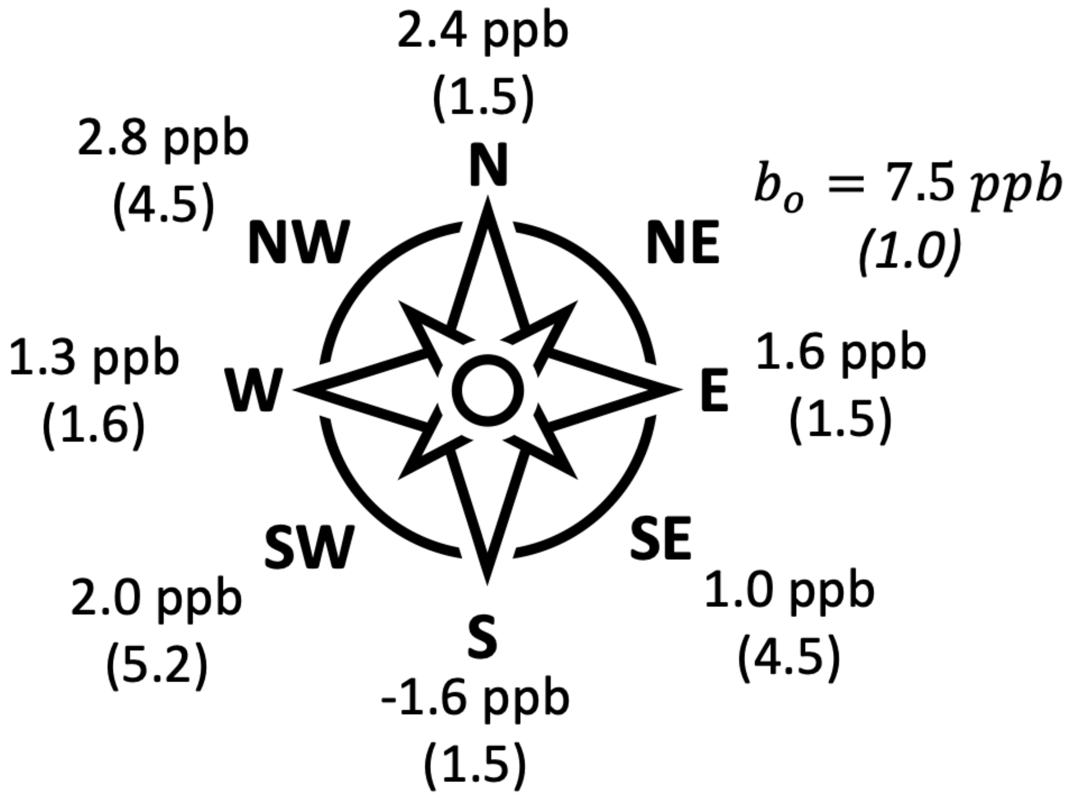
Appendix Figure 3.A1

Kernel density plot of student lead exposure (ppb)



Appendix Figure 3.A2

Using classroom cardinal direction (e.g., North, South, East, West) to predict classroom water lead levels (ppb).



Notes. Estimates come from a school fixed-effect regression that is unique at the school classroom level using classroom direction to predict classroom water lead level (ppb). Standard errors are in parenthesis. Northeast is the omitted category because Northeast side of the building tends to be the most moderate temperature, and higher temperatures causes lead to lead more in to water. The expectation is that all sides will have higher lead levels compared with classrooms facing Northeast. As we can see, this is not the case. The northeast classrooms have on average 7.5ppb of lead (se=1.0). Compared to classrooms facing Northeast, classrooms facing North have 2.5 more ppb water lead, though statistically insignificant. There is no statistically significant difference in classroom lead level between Northeast and any of other direction. This includes a total of 378 classrooms in 56 unique schools for which I could determine directionality of classroom.

Appendix Figure 3.A3

Scatterplot of classroom age and classroom WLL (ppb)