Labor supply, learning time, and the efficiency of school spending: evidence from school finance reforms*

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THIS DRAFT IS A REVISION OF MY JOB MARKET PAPER: LINK

Abstract

Over the past half century, legislation in most states raised school funding. Researchers have argued that spending does not raise the quality of schooling, and hence, that money does not matter in education production. This paper provides stronger evidence that new funding did not raise school quality, but then provides new evidence that funding was used to raise the quantity of schooling by extending the school year. Thus, reforms have benefits and private costs that were not measured before. Using an event study design based on the differential timing of reforms in different states, and data from the Current Population Survey (CPS), the American Time Use Survey (ATUS), and the National Assessment of Educational Progress (NAEP), I show that: (1) schools did not use new funding to hire more or more qualified staff; (2) spending did not increase a direct measure of school quality, the rate of learning across weeks of school; (3) schools used new funding to implement a longer school year, adding weeks after NAEP testing; and (4) additional time in school crowded out students' time doing household chores, but not time with parents or other investments in human capital. My results suggest that schools spend efficiently in the narrow sense that they minimize marginal labor costs: the wage for a marginal day is half the average wage. Although additional learning time may be productive, based on the estimated rate of learning, schools do not necessarily internalize costs additional days impose on families.

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1 Introduction

Would more funding for primary and secondary schools lead to better education outcomes? The Coleman Report to Congress, in 1966, argued that the correlation between funding and outcomes is small. Over the past half century, school finance reforms (changes to state school finance laws) have made it possible to establish a causal relationship between funding and outcomes.[1] Recent causal studies of reforms suggest that funding may improve outcomes (Jackson, Johnson, and Persico, 2016; Lafortune, Rothstein, and Schanzenbach, 2016).[2] Yet, critics argue, school finance reforms are a black box: we don't understand what schools do with funding, whether that should improve education outcomes, and what the incentives are for schools to respond similarly to future reforms (Hanushek and Lindseth, 2009).

What do schools do with reform funding? Unsurprisingly, the majority of new funding from reforms goes to compensation for school staff. Nonetheless, the way schools spend on compensation raises a major puzzle: Sims (2011b) reports that salaries grow in response to reforms, but employment does not. School quality depends on the number and qualifications of teachers, so a link in the causal chain from funding to outcomes may be missing, emphasizing the need for better evidence on labor supply. In particular, schools may raise quantity (by extending the school year) rather than quality. Do school staff work more hours in years following reforms? Do schools extend learning time for their students? How does this affect other time allocation by students and their families?

This paper establishes how schools use reform funding. I provide more complete evidence than before on the effects of reforms on labor supply, learning time, family behavior, and achievement. This evidence has important implications for the incentives schools face when implementing reforms, the efficiency of school spending, and the private costs reforms impose. By showing that there are no missing links in the causal chain from funding to outcomes, I also address key concerns about the internal validity of recent evaluations.

To identify the causal impact of reforms on funding and the allocation of funding, I make use of an event study design based on the timing of reforms. I compare states that did and did not have a reform in a given year from 2003 to the present, to estimate the average treatment effect. States that had reforms were not different: nearly every state had a reform in the past five decades. The timing of reform events is arguably exogenous to other factors

[1] The use of reforms to identify exogenous changes in school funding is discussed in Murray, Evans, and Schwab (1998), Hoxby (2001), Downes and Shah (2006), and Corcoran and Evans (2007), among others.

[2] Past contributions in the literature on school finance reforms are more equivocal: Hoxby (2001) finds weak effects on attainment, while Candelaria and Shores (2015) and Hyman (forthcoming) find positive effects on attainment; Downes (1992), Clark (2003), and Dinerstein and Smith (2014) find null or weak average effects on achievement, Downes and Figlio (1998) and Guryan (2001) find mixed effects on achievement, and Card and Payne (2002), Papke (2005), and Roy (2011) find positive effects on achievement.

that could affect labor supply, learning time, or family behavior, too. Two kinds of reforms are studied in the literature: voluntary, and court-ordered. In court-ordered reforms, one plaintiff sets precedent for an entire state, the median case is filed nearly a decade before it results in a reform (separating the conditions at filing from those at reform), and courts necessarily override the intentions of the legislature. Voluntary reforms are determined by the marginal legislator, who in turn is elected according to a bundle of issues that might or might not include school finance. The exogeneity assumption passes falsification tests, for reforms from 1990 through 2013: the U.S. Department of Education's School District Finance Survey (SDFS), which reports on the universe of school districts, shows no evidence of pre-trends or inexplicable post-trends in state, local, or federal funding, or in enrollment.[3]

To measure school quality, school quantity, and family behavior, I use data from the Current Population Survey (CPS), the American Time Use Survey (ATUS), and the National Assessment of Educational Progress (NAEP). I use the CPS (2003-2015) to replicate past findings on the employment and qualifications of school staff, taking advantage of worker reporting to provide an independent check on employer reporting in the U.S. DOE Common Core of Data. I use the NAEP (2005-2013) to measure school quality, defined as the rate of learning. Because NAEP tests are timed quasi-randomly, the gain in test scores across weeks of school identifies the rate of learning; this is the first use of random test timing in the NAEP to measure school quality. I use the ATUS (2003-2015) to measure labor supply, learning time, and the activities of parents and students. Validation studies show that time diaries are more accurate than stylized time use questions about working hours, as in the CPS, and I show that time diaries are more sensitive to known changes in working hours. In addition, no other national dataset measures detailed activities of parents and students.

Schools use reform funding to raise quantity, not quality. The average effect of reforms from 2003 through 2013 on total spending is a 4.3 percent increase, two thirds of the additional spending are allocated to compensation, and the effect of reforms on compensation expenditure is a 3.6 percent increase. Spending has four main consequences:

1. Schools do not employ more or better workers. In the CPS sample, the elasticity of employment for workers in the K-12 industry, with respect to compensation, ranged from -0.2 to 0.3 across specifications,[4] consistent with prior findings.[5] In addition, the

[3]Sections 2 and 3 describe recent reforms, the empirical specification, and the identification assumptions. Overall, my findings suggest that identification from state-level funding impacts (used in most papers on school finance reforms) is as reliable as identification from differential funding impacts across school districts within states (which is argued for by Lafortune, Rothstein, and Schanzenbach, 2016).

[4]The 95 percent confidence intervals around point estimates for changes in employment typically include zero, and imply smaller than unit elasticities.

[5]Sims (2011b) finds an elasticity of employment with respect to compensation of 0.3. Jackson, Johnson, and Persico (2016) find an elasticity of 0.6 for a longer sample period, from 1987 through 2010.

average worker does not have more years of education or experience, after reforms. Thus, similar school staff are paid higher salaries.

2. School quality (measured by the rate of learning across weeks of NAEP administration) does not change. Cumulative gains on the NAEP (for testing years 2003 through 2013) are consistent with zero to small positive effects. Since the rate of learning does not change, cumulative gains would come from additional instruction outside the testing window: tests are taken in January, February, and March.

3. Students and staff spend more time on school and at work, respectively. Children eligible for K-12 and aged 15-19 report 69 more hours of education activities per year (5.5 percent) in the ATUS diaries, per reform; and K-12 staff report 117 more hours of work per year (6.8 percent). The school year is longer, not the school day: schools add time in what would normally be vacation periods, especially May, June, and July.[6]

4. Time at school necessarily crowds out other activities. Increased time allocation to education activities is offset by reduced time allocation to home production activities, but not to leisure or non-school investments in human capital. Parents of children eligible for K-12 and aged 5-19 do not allocate time differently after reforms, even though their children are in school for an additional two weeks per year on average.

These findings are robust in sign and magnitude to changes in the estimation sample, the inclusion of control variables and state trends, and the choice of reform events in regression specifications; triple differences specifications for K-12 staff demonstrate that the results are not driven by changes in the measurement of working hours, or in labor market confounds that affect other industries and occupations. Finally, estimates using the ATUS question measuring usual hours of work, although attenuated by mean-reverting measurement error, are significant and confirm that the results for K-12 staff are not specific to time diaries.

Schools raise quantity because it is more cost efficient (for schools) to raise quantity. Raising quality (defined by the rate of learning) typically means hiring more or better staff, or motivating staff to exert more effort, all of which would mean paying a higher average wage. Raising quantity typically means adding school days for existing staff. Because of fixed costs in employment, and because school staff are underemployed during school vacations, marginal costs for an extended school year may be less than average costs. Indeed, I find

[6] Surprisingly, there is no published evidence of reform effects on learning time. Extended learning time is sometimes required by reforms, or suggested, or is the motive for the original lawsuit itself. In an example of a lawsuit motivated by learning time, the complaint in Butt v. State of California, 842 P.2d 1240 (Cal. 1992) was brought to prevent the Richmond School District from shortening the school year to address budget deficits; the subsequent court ruling forced the state to fund a longer school year.

that the marginal cost of adding a school day to the calendar for existing staff is less than the average cost of school days: the elasticities of student time allocated to education activities, and of staff working hours, to compensation expenditure, are both greater than one. That is, hours grow more than expenditures. I estimate elasticities of 1.5 and 1.9, respectively. These estimates are direct evidence for theories of labor supply with fixed costs set forth by Cogan (1981), Zabel (1993), and Donald and Hamermesh (2009) to explain phenomena including retirement and the scarcity of part-time work.

Raising quantity should consistently raise test scores, if tests were appropriately timed, but the tests used in most current studies (including this study) are not appropriately timed. Therefore, studies that do find effects of reforms on achievement are likely finding something less than the immediate effects of reforms on achievement. The immediate effects of interventions on achievement are relevant because they predict adult outcomes (e.g. Deming, 2009; Chetty et al., 2011). Instead, tests capture gains from additional instruction with a one-year lag, and testing gains are subject to fade-out.[7] Past estimates for one-year fade-out range from 50 to 80 percent (e.g. Chetty, Friedman, and Rockoff, 2014; Jacob, Lefgren, and Sims, 2010). The rate of learning I estimate from the timing of NAEP tests is not subject to fade-out, because measurement is immediate. The estimated rate of learning implies cumulative gains on the NAEP of 0.49 SD per year,[8] and given a 5.5 percent increase in the length of the school year (90 percent CI: 0.010 to 0.100), implies cumulative gains on the NAEP of 0.027 SD per year (90 percent CI: 0.005 to 0.049). My direct estimate of the effect of reforms on cumulative gains on the NAEP is 0.000 SD per year (95 percent CI: -0.004 to 0.005).[9] Measured gains are smaller than implied gains, and the difference is statistically significant: a Welch test of the null hypothesis that implied gains are less than or equal to measured gains has a p-value of 0.021. When schools raise quantity (or could) to raise achievement, evaluations relying on tests should use tests timed after instruction.

Raising quantity imposes private costs on students and families. Time for school is taken out of time for other activities including leisure, home production, or non-school investments in human capital; since parents contribute to these activities, they may also be affected by learning time interventions. The cost of a marginal hour put into school is the value of an

[7] My results rule out some explanations of fade-out in the context of school finance reforms: in particular, because parents allocate very little time to education-related activities with children to begin with, and don't reduce time allocated to children in general, compensatory behavior by parents cannot explain fade-out.

[8] The estimated rate of learning is broadly consistent with findings in Fitzpatrick, Grissmer, and Hastedt (2011), Hansen (2011), and others using similar methods on different tests and populations.

[9] The estimated cumulative gains are consistent with state-average estimates in Lafortune, Rothstein, and Schanzenbach (2016), and their estimate for the most-affected quartile of schools, of 0.007 SD per year, is a quarter as large as the implied cumulative gains based on the rate of learning. My estimates come from a more recent (2003-2015, rather than 1990-2013) but comparable period of reforms: the average effects on funding, compensation, and employment are similar across periods.

hour taken out of another activity. This cost has been recognized at least since Mincer (1958) and Ben-Porath (1967), but its significance for learning time interventions is largely ignored in the recent literature. When schools raise quality, they do not impose an opportunity cost on students, but when schools raise quantity, they do impose an opportunity cost. Thus, quantity interventions that have similar value added in terms of achievement (compared to quality interventions) have lower value added overall once costs are taken into account. How private costs are allocated matters: students allocate costs to home production, rather than to leisure or non-school investments, and since parents do not re-allocate equivalent time to home production, the ultimate cost is lower consumption.[10] Evaluators should recognize that partial equilibrium effects on the family are not restricted to investment in schooling (as in Todd and Wolpin, 2003; Glewwe and Kremer, 2006; and recent empirical contributions from Houtenville and Conway, 2008; Pop-Eleches and Urquiola, 2013; Das et al., 2013; Gelber and Isen, 2013; Fredriksson, Öckert, and Oosterbeek, forthcoming; Kline and Walters, 2016; and Araujo et al., 2016) and may instead appear for activities where schools are not already substituting for parents, including home production. Evaluators should consider measuring these alternative activities in addition to family investments in education.

Overall, the evidence suggests that more funding should lead to better education outcomes, because schools use new funding to provide a longer school year. However, despite the low marginal cost of learning time to schools, private costs matter in ways that previous theoretical and empirical research, and education policy, rarely acknowledge.

The rest of the paper is organized as follows: Section 2 provides background on school finance in the United States, on the reform process, and on the features that make reforms useful for empirical work. Section 3 describes the empirical strategy, assumptions, and falsification tests. Section 4 introduces the sources used to measure labor supply, learning time, school quality, and family behavior, and discusses their validity and advantages compared to other data used in the literature. Section 5 presents evidence that reforms improve school quantity but not school quality, and on patterns of substitution to education activities from alternative activities in the household. Section 6 discusses the implications of my findings for the efficiency of school spending, for our ability to measure the impacts of reforms, and for the private costs of reforms. Finally, Section 7 concludes, arguing that while the findings resolve major puzzles about the efficiency of school spending, they raise a new puzzle: why is the school year so short when the marginal costs of a longer school year are so low?

[10] If home production could be replaced by services schools offer, the impact on welfare would be ambiguous; however, apart from food services, most home production is not substitutable.

2 Background: school finance reforms and the school spending debate

School finance reforms are reforms to state school finance laws. Every state constitution requires that the state legislature provide for publicly funded education through school finance laws, and requires some degree of equity in public education.[11] For example, in New Jersey,

> "The Legislature shall provide for the maintenance and support of a thorough and efficient system of free public schools for the instruction of all the children in the State between the ages of five and eighteen years," (Art. VIII, Sec. IV, Par. 1 of the New Jersey State Constitution).

State legislatures provide half of funding for public school districts. Thus, state law is a major driver of public education outcomes.

In the past half century, most states faced serious political and legal challenges to their school finance laws, and a majority passed school finance reforms to address those challenges. From 1990 to 2013 alone, I estimate that reforms raised annual funding by at least 32 billion dollars.[12] This paper addresses the question of whether increased spending from reforms was productive, using reform timing for identification. This section describes the key issues in reforms, the reform process, and the features of reforms that support identification.

Efforts to increase public school funding from the states are opposed in legislatures, referenda, and courts for at least three reasons:

- **Taxation:** school funding comes out of state taxes.[13] In addition, reforms redistribute tax burdens, and sometimes funding, across school districts. Authors disagree about the extent to which reforms redistribute,[14] but it is clear that some redistribute funding, and others do not, and some are progressive, and others are regressive.[15]

[11] See Parker (2016) for references to each state constitution's education clause.

[12] This figure is based on estimates in the next section of the effect of the average reform on spending. The 27 states that experienced a reform event from 1990 through 2013 covered 59 percent of national enrollment as of 1990. In these states, spending grew by 139 billion constant 2015 dollars. Reforms caused 23 percent of the total increase, enrollment gains contributed another 41 percent of the total increase, and the remainder came from local spending increases and unobserved state policy changes.

[13] Tax reforms are often (effectively) school finance reforms. This paper does not study general tax reforms, though other research in the literature has (Downes and Figlio, 1998).

[14] Corcoran and Evans (2007) argue that recent reforms did not redistribute spending, while Lafortune, Rothstein, and Schanzenbach (2016) argue that reforms did redistribute spending.

[15] An example of a reform that regressively redistributed tax burdens but not spending is New York's School Tax Relief (STAR) credit, enacted in 1997, which rebated local property taxes with revenues from state income taxes, favoring property owners in wealthier districts. When spending is redistributed, reforms often work in favor of poorer districts, but sometimes in favor of richer districts. For example, the creation of Abbott districts in New Jersey in 1985 explicitly favored poorer districts including Newark, Paterson, and

- **Local control:** funding gives states leverage over local governments. Most states respect local control over how funds are spent and how schools are managed, through independent school districts.[16] However, many states control local funding levels through matching, mandates, and caps, so that local districts cannot opt out of reforms.[17]

- **Spending efficiency:** funding does not always lead to better outcomes, and it has been argued in policy circles, as well as in court, that states should not spend more when spending doesn't lead to better outcomes.[18] School districts, not states, choose how to spend, and the (prospective) efficiency of their specific choices is debated.[19]

Because school spending policies have such varied effects on achievement, local control leaves open a wide range of possible reform effects on achievement. These effects are best assessed either directly, or by determining how reform funding was spent. This paper does both, and contributes new evidence on school spending policies following reforms.

2.1 Reforms and the political process

School finance reform legislation can be voluntary, ordered by a referendum, or ordered by a court when existing school finance laws fail to comply with the state constitution. This research focuses on court orders and major voluntary legislation.

Court orders arise from constitutional litigation. In the past five decades, only Delaware, Hawaii, Nevada, and Utah avoided challenges to the constitutionality of their school finance

Trenton; while, by contrast, when California funded K-3 class size reduction grants in 1996, richer districts were more likely to take advantage of the program because they were more likely to have the required classroom space and to attract enough teachers.

[16]Most reforms give full discretion to school administrators to allocate spending: for example, the Massachusetts Education Reform Act (MERA) of 1993 gave substantial discretion to administrators, with the explicit objective to preserve local control. Even when reforms don't give full discretion, local control is a priority: the New York Education Budget and Reform Act of 2007 required administrators in underperforming districts to spend a certain proportion of additional funding on interventions including class size reduction and extended learning time, but all administrators had at least partial discretion.

[17]Examples of state control include Texas Senate Bill 7, enacted in 1994, which rewarded high tax effort localities with matching state funds; the Florida Education Finance Program, enacted in 1973, which set a mandatory minimum local tax rate through its Required Local Effort provisions; and Texas House Bill 1, enacted in 2006, which capped local property tax rates.

[18]For example, in Abbott v. Burke 119 N.J. 287 (June 1990), the New Jersey Supreme Court stated: "We reject the argument, however, that funding should not be supplied because it may be mismanaged and wasted. Money can make a difference if effectively used, it can provide the students with an equal educational opportunity, a chance to succeed."

[19]The debate over class size provides an example: Angrist and Lavy (1999), Krueger (1999), Chetty et al. (2011), and Fredriksson, Öckert, and Oosterbeek (2013) find positive effects on education outcomes, while Hoxby (2000), Chingos (2012) and an older correlational literature summarized in Krueger, Hanushek, and Rice (2002) find null effects; meanwhile, Rivkin, Hanushek, and Kain (2005), Chetty et al. (2011), and others argue that interventions like increasing teacher quality may be more cost-effective.

laws. School districts and advocacy groups initiate litigation on the basis of two clauses found in every state constitution: the equal protection clause (in "equity" cases, where the state allegedly discriminates among districts in taxation or in the provision of public education, on the basis of property wealth or taxable income) and the education clause (in "adequacy" cases, where the state allegedly fails to provide public education up to the required standards); while some state courts have ruled that challenges to school finance systems reach political questions that can only be decided by the legislature, many have overturned existing school finance laws and compelled new legislation.[20] In addition, state legislatures often settle cases, or pre-empt them by raising education appropriations.

Voluntary legislation arises from normal legislative bargaining, under existing constitutional, legal, and political constraints. Small shifts in support among voters or legislators lead to changes in funding for every state school.

2.2 Reforms as an empirical strategy

Two features of reforms make them useful for identification:

First, reforms are typically large and abrupt rather than small and gradual, so that shifts in funding are visible in the time series of state funding to school districts. Thus, reforms are suitable for event study designs such as the one used in this research.

Second, reforms are arguably exogenous. For legislation, only marginal voters influence elections, votes are cast for parties and candidates rather than school finance policies, and legislators bargain over unrelated legislation. For court orders, legal rather than political or fiscal considerations determine funding under new laws.[21] One plaintiff representing one school district sets precedent for the entire state, so that the condition of the average school doesn't determine whether cases are filed or how much of an impact they have on state law. In addition, court orders are rarely related to contemporary political or fiscal conditions: litigation takes years to compel new legislation. Cases must be organized, funded, filed in trial courts, and fought until the state supreme court delivers an order for new legislation.[22] The median time from first filing to final decision is eight years, for cases decided after 2003, and courts often take time to compel legislatures to comply, in some cases after further

[20] Koski and Hahnel (2007) provide a useful review of the intellectual and legal foundations, and the history, of school finance litigation.

[21] According to Corcoran and Evans (2007), even legal considerations don't always determine court orders: Baicker and Gordon (2006), Figlio, Husted, and Kenny (2004), and Card and Payne (2002) demonstrate that high court rulings overturning school funding systems are difficult to predict, even after researchers account for variation in the constitutional language with which rulings are justified.

[22] The first time a case reaches the state supreme court, it is frequently to determine whether the complaint is justiciable (subject to the jurisdiction of the court). Cases may be remanded to trial courts after they are found to be justiciable, and then appealed back up to the state supreme court.

litigation. Court orders also override the preferences of voters and legislators, when they were expressed in overturned school finance laws.

In the next section, I present the specifications used to identify the effects of reforms on funding, and perform falsification checks for the exogeneity assumption.

3 Empirical strategy

The first stage for school spending is an event study of the effect of reforms on school funding.

I identify court-ordered and voluntary reforms at the state level, in the period from 1990 through 2013, using the listings in Jackson, Johnson, and Persico (2016) and Lafortune, Rothstein, and Schanzenbach (2016) and performing further checks for consistency. Court-ordered reforms must have been decided at the state supreme court, or on remand to a trial court from the state supreme court for enforcement; that is, they must have been final. When a state experienced multiple reforms in a single year, I code one event.[23] From 1990 through 2013, the combined list includes 86 reform events in 28 states, covering 59 percent of public school enrollment in the United States; these are the reforms used to confirm identification. In later sections, I use a shorter list of reforms from 2003 onward, which includes 29 reform events in 19 states, covering 48 percent of enrollment.

I measure fiscal variables with the U.S. Department of Education's School District Finance Survey (SDFS). The SDFS collects fiscal variables and enrollment for the universe of public school districts in the United States, in every fiscal year, based on their (audited) administrative records. To compare public school enrollment to private school enrollment, I complement these data with Private School Survey (PSS) records of enrollment for the universe of private schools, in every even-numbered fiscal year.

3.1 Difference-in-difference specification

This paper estimates the impacts of school finance reforms by looking at the change in funding in states that enact reforms, in the years subsequent to the reform, relative to states that did not enact reforms in the same years.

The difference-in-difference (DD) specification is:

$$Y_{it} = \sum_{r \in R_s} \alpha \mathbb{I}_{trs} + A_s + B_t + \gamma X_{it} + \epsilon_{it} \tag{1}$$

[23] All events are checked for visual consistency with a time series of school funding in the relevant state, and verified with documentation of court decisions or legislative acts.

where Y_{it} is a variable measured at the individual (or state, district, or household) level, A_s and B_t are state (or district) and year fixed effects, and X_{it} is a vector of controls. $\mathbb{I}_{trs}$ is an indicator that time t is after reform r in state s, and α is the average effect of a reform. The indicator is defined by $\mathbb{I}_{trs} = 1[t > t_{sr}]$, and is equal to zero in all periods in states that have not had a reform in the period from 1990 to present. Errors ϵ_{it} are clustered at the state level to account for state-level variation not captured in the model.[24]

Equation (1) is the preferred state-level DD estimator used in the remainder of the paper.

I present state-level estimates for three reasons: first, reforms are state-level policies; and second, the key samples from the American Time Use Survey (ATUS) are too small to calculate fixed effects at the county- or school district-level,[25] and are not identifiable at the county- or school district-level.[26]

I model all reforms at once (with the summation term) to avoid attributing the effects of omitted reforms to modeled reforms, to trends, or to error terms. For example, New York enacted several major reforms in quick succession: if I were to include one of these reforms, but not the others, the coefficient for that reform would be overstated because it would pick up the effects of other reforms. In addition, in an event study, failing to include earlier or later reforms would give rise to spurious pre- and post-trends. Including multiple reforms addresses a known source of misspecification error in previous studies, when reforms are not timed independently of each other within states.

The identification assumption is that:

$$E[\epsilon_{it} | \sum_{r \in R_s} \mathbb{I}_{trs}, A_s, B_t, X_{it}] = 0$$

[24] In later sections, I present triple-difference (DDD) specifications comparing the effects on a population of interest, such as K-12 staff, with the effects on a control population, such as other professionals:

$$Y_{it} = \sum_{r \in R_s} (\alpha g_i + \beta) \times \mathbb{I}_{trs} + D_{gs} + E_{gt} + (\gamma g_i + \delta) \times X_{it} + \epsilon_{it}$$

where $g_i = 1$ if the observation is in the group of interest and 0 if the observation is in the control group: β captures the baseline effect of a reform or of any confound associated with the reform, state and year fixed effects A_s and B_t are replaced with group-by-state and group-by-year fixed effects D_{gs} and E_{gt}, and control variables are also interacted with group membership, with the coefficient δ giving the baseline relationship to control variables for both groups.

[25] The ATUS sample is composed of repeated cross sections, so that individual fixed effects are not available. With approximately 7,000 school staff and 9,000 high school students, including fixed effects for the 917 metropolitan and micropolitan statistical areas (MSAs and μSAs) or the 3,143 counties and county equivalents (let alone the 13,000-plus local education authorities) in the United States would consume nearly all of the degrees of freedom afforded by these samples.

[26] CPS and consequently ATUS supplement data are technically identifiable to the Census tract level, but these data are confidential, and due to contracting issues between the Census Bureau and the Bureau of Labor Statistics, geographic identifiers are not available through either: they may become available through a pilot program with the Census Research Data Centers (RDCs) in 2019 or 2020.

or in other words that reform timing is exogenous conditional on fixed effects, controls, and other reforms within the state.

The last section presented the procedural reasons why reform timing would be exogenous to other factors that could affect funding, labor supply, learning time, or family behavior. In summary, the nature of the political process suggests that the causes of litigation and legislation are too local and idiosyncratic to affect statewide outcomes, unless they lead to statewide reforms.

The assumption of exogeneity is also falsifiable in district-level data on school finances and enrollment, and I carry out falsification tests in the next subsection.

3.2 Event study specification and falsification tests

The non-parametric event study specification is:

$$Y_{dt} = \sum_{r \in R_s} \sum_{k \in K} \alpha_k \mathbb{I}_{tkrs} + A_d + B_t + \gamma X_{dt} + \epsilon_{dt} \tag{2}$$

where Y_{dt} is a fiscal variable measured at the district level, A_d and B_t are district and year fixed effects, and X_{it} is a vector of time-varying controls that may contain state time trends. $\mathbb{I}_{tkrs}$ is an indicator that time t is k periods after (or before, for negative values of k) reform r in state s, and α_k is the average effect, across all reforms, of a reform at lag k. The coefficient α_k restricts each reform, including multiple reforms within states, to have the same effect at a given time horizon before or after a reform event: this assumption reduces the scope for overfitting. The reform indicator is defined by $\mathbb{I}_{tkrs} = 1[t - t_{sr} = k]$,[27] with the exception that I truncate lags k at -5 and 5, so that $\mathbb{I}_{t-5rs} = 1[t - t_{sr} \leq -5]$ and $\mathbb{I}_{t5rs} = 1[t - t_{sr} \geq 5]$. The reform indicator is equal to zero in all periods in states that have not had a reform in the period from 1990 to present. Finally, errors ϵ_{dt} are clustered at the state level to account for state-level variation in fiscal variables not captured in the model: this variation could arise because of non-linear trends, omitted reforms, discrepancies between the true reform effect and the average reform effect, mis-measured reform timing, and so on.

Equation (2) is the non-parametric event-study version of the preferred state-level DD estimator used in the remainder of the paper.

School finance reforms pass standard falsification tests in event-study specifications identifying state-level average impacts:

[27] Fiscal data are indexed by the school fiscal year t, which for most states ends on June 30th of the coded year; reforms are indexed by the year t_{sr} in which a court decision was entered, or in which legislation was enacted with the executive's signature. Budgets are typically appropriated well before the beginning of a new fiscal year. Thus, court injunctions and emergency legislation (which can take effect immediately) may affect fiscal variables when $k = 0$, and most reforms should not have effects until $k = 1$, or even $k = 2$.

- Reforms have a significant effect on state revenues to school districts, with no evidence of pre-trends, or of post-trends more than four years after enactment (Figure 1).[28] The absence of pre-trends suggests that modeling all reforms at once reduced specification error, and that the dates of reforms in the listing are coded correctly: measurement error in reform dates would introduce pre- and post- trends.

- State revenue is not offset by local revenue, and neither is it predicted by changes in local revenue, as it would be predicted if the state were trying to remedy recent changes in local fiscal conditions (Figure 2).

- There is no evidence of changing enrollment across states, and hence of selective migration, before or after reforms (Figure 3). There is no evidence, either, of enrollment flowing from private schools into public schools within states (Figure 4).[29]

These findings address the primary concern for identification, which is that changes in unobservables including the endowments and preferences of families and school staff might lead both to reforms, and to other changes in funding and spending in schools. The most obvious ways for these unobservables to change are shocks to economic and fiscal conditions within states, and selective migration across states. Local revenue proxies local economic conditions, and enrollment proxies migration. Further, interstate migration rates are too low to explain rapid changes in school policy of the kind observed in most school finance reforms.[30]

Finally, when changes in unobservables are gradual, but lead to sharp changes in funding (for example, when the marginal vote in a legislature changes because constituents for that district changed) controlling for state-specific time trends should eliminate endogeneity.

These findings do not address the concern that reforms affect school staff and families indirectly through changes in tax burdens: however, this concern is secondary because tax burdens are spread across workers and families regardless of their involvement with the public education system, while school funding targets only school staff (who account for five percent of total employment) and families with school children.

[28]Post-trends of up to four years are consistent with known delays between court orders, enforcement, legislative enactment and appropriations, and the onset of a new school fiscal year, in addition to phased introduction of new laws. For example, the New York Education Budget and Reform Act of 2007 was intended to increase funding in four discrete steps over the four subsequent years.

[29]Steady enrollment is consistent with prior findings. Dinerstein and Smith (2014) find that the New York Education Budget and Reform Act of 2007, by improving funding in public schools, led to the closing of nearby private schools, but not distant private schools. However, because distant private schools were more numerous, the aggregate effect on private school enrollment was negligible.

[30]Migration across states would have to be highly selective according to school funding or taxes, as well, to change the composition of voters, workers, and families. Migration across school districts may be a greater threat for identification strategies that compare reform effects on funding across school districts: in addition to being more frequent, intrastate migration may be more selective as families can relocate across school districts while working for the same employers.

3.3 State average fiscal effects

Table 1 reports the effects of reforms on fiscal variables in the period from 2003 through 2013, which most closely overlaps the period covered by the American Time Use Survey (ATUS). Reforms raised per-pupil spending by $611 (2015 current dollars) on average, per reform, an increase of 4.3 percent. $338 went to compensation (salary and benefits) expenditures, an increase of 3.6 percent.

Further data are needed to tell how funding translates into specific kinds of spending.

4 Data

Reforms raise compensation in schools. Compensation can be used to raise school quality by hiring more or better qualified staff, or to raise school quantity by extending the school year.[31] Alternatively, if neither of these things happens, additional spending may yield economic rents to existing staff without any increase in labor supply. Among these possibilities only the first, that expenditures are used to hire additional staff, has been seriously considered in the literature. Small responses of hiring to spending have led to the interpretation that spending yields economic rents to school staff (Sims, 2011b).

This paper is the first to study the effect of reforms on a direct measure of school quality, the rate of learning on achievement tests. This paper is also the first to study the effect of reforms on the working hours of school staff, and hence on learning time. These contributions depend on new data sources for the literature on school finance reforms: the Current Population Survey (CPS) provides an independent check and extensions of results using the U.S. Department of Education's Common Core of Data, the National Assessment of Educational Progress (NAEP) provides a way to measure the learning rate (a measure of quality) in schools for the first time in the school finance reform literature, and the American Time Use Survey (ATUS) provides a way to measure labor supply, learning time, and family behavior for the first time, accurately, and in nationally-representative samples.

4.1 The Current Population Survey (CPS)

The CPS provides worker-reported data on employment in the K-12 education industry, and in every other industry.

I work with a sample of 10,590,750 respondents in the CPS outgoing rotation groups

[31] Quality adjustment in the pool of teachers necessarily takes longer than hiring, and there is scant evidence of changing qualifications in the literature or in the data used in this paper. This paper focuses on labor supply because it, too, is a marker of quality, but adjusts quickly and is measured objectively.

(ORG, months 4 and 8) from 1990 through 2015 (school fiscal years ending in 1990 through 2016): among these respondents, 48 percent (5,046,983) are employed, and among the employed, 6 percent (311,149) are employed in the K-12 education industry. I work primarily with observations from 2003 onwards, but use the full sample for comparison with other results in the literature based on the U.S. DOE Common Core of Data (CCD) (see Sims, 2011b and Jackson, Johnson, and Persico, 2016).

The CPS has a number of advantages relative to the CCD, which is the only other large, national, regularly reported dataset on employment for school staff:

- The CPS is reported by workers rather than by employers, reducing the scope for error at the school or district level and providing an independent check on results based on the employer-reported CCD. Definitions in the CPS (including occupation and employment) are known and consistent across years and geographic locations.

- The CPS is monthly rather than annual, making it possible to look at the effects of reforms on the school calendar: over the summer, teachers may go on leave while remaining employed, separate from employment, or seek other employment.

- The CPS includes some limited measures of worker qualifications, including education and age (a proxy for experience) that are not available in the CCD.

Despite these advantages, the CPS is known to proxy and impute a large portion of responses for variables other than employment. I complement the CPS with other data sources that better measure compensation (the CCD) and labor supply on the hours margin (the ATUS).

4.2 The National Assessment of Educational Progress (NAEP)

The NAEP assesses a random sample of roughly 3000 students in roughly 50 schools in each state, in each of 4th and 8th grade and math and reading subject tests.

The NAEP can be used to construct a direct measure of school quality, the learning rate. When test takers take the NAEP one week later, their test scores go up by a measurable and significant increment, revealing the rate at which test takers are taught new content relevant to the test.[32] The NAEP is administered in January, February, and March: that is, the interval over which test scores grow is from the fourth week of the new year through the eleventh week. Because the learning rate is measured for a fixed interval, it measures school quality independently of school quantity (the length of the school year).

[32] Researchers have measured learning rates on a variety of other test instruments, for other populations and in different contexts: Sims (2008); Fitzpatrick, Grissemer, and Hastedt (2011); Hansen (2011); Agüero and Beleche (2013); Carlsson et al. (2015)

I work with a sample of 3,364,010 test takers in 80,310 school by grade by subject cells, in alternate years from 2003 through 2013. Because schools are frequently resampled across years, I am also able to construct an imbalanced panel of 2,336,500 test takers in 33,950 school by grade by subject cells, in which each cell is observed at least twice.

Exact testing dates are recorded starting in 2005, so the sample used to measure the learning rate runs from 2005 through 2013.

The learning rate (value added per unit time) is measured by the coefficient of test scores on the scaled testing date, and is given by β in the following specification:

$$A_{ist} = \alpha + \beta T_{st} + \epsilon_{ist} \tag{3}$$

where A_{igst} is achievement for student i in grade g and school s in year t, and T_{gst} is the testing date.

Because test instruments vary across years, and because raw scores vary across subjects and grade levels, I standardize test scores within each year by grade by subject cell: first, I average across the "plausible values" reported by the NAEP for each individual to construct a test score; then, I standardize by the national mean and standard deviation. Thus, estimates of the learning rate are in units of within-cell, national standard deviations.

The identification assumption is that:

$$E[\epsilon_{ist}|T_{st}] = 0$$

or in other words that test timing is unconditionally exogenous. I present results conditioning on school, state, and year fixed effects S_s, $F_{f(s)}$, and Y_t; and controls X_{igst}; as well.

There is strong evidence in favor of the identification assumption:

- **Procedure:** schools are sampled at random, and NAEP coordinators schedule assessments within a fixed window of time, within contiguous geographic areas. Some schools must be assessed early, and others must be assessed late, and this is true among schools that are similar to each other by the virtue of being located in the same school district or nearby school districts. In addition, the NAEP is not used for accountability, leaving school staff no incentive to time the NAEP assessment selectively.

- **Observation:** the assignment of test dates in the NAEP appears as good as random. Test dates observed for schools in the NAEP in one year are visibly orthogonal to test dates observed for the same schools in another year, and the correlation of dates across years is near zero (and in some cases insignificant even in a large sample) whether or not I condition for state-by-year fixed effects (Figure 5 and Tables 2 and 3).

Consistent with randomization, test dates are not explained by student demographics including English Language Learner (ELL) status, Individualized Education Plan (IEP, i.e. special education) status, race, parents' education, or school enrollment. These characteristics are balanced across test dates.[33]. In addition, estimates of the learning rate are highly robust to the inclusion or exclusion of fixed effects and control variables (Klopfer, in preparation).

Klopfer (in preparation) shows that estimates of the learning rate have other desirable properties, including linearity across weeks in non-parametric specifications, consistency across grades and subjects, and correlation with input measures of school quality.

The effect of reforms on the learning rate will be estimated by crossing T_{st} with the reform indicator in Equation (3).

4.3 The American Time Use Survey (ATUS)

The ATUS measures time allocation using time diary methods. Each respondent is asked about his or her activities over 24 hours, starting at 4:00am on the preceding day, in their linear sequence; in addition to coding the stopping and starting times of detailed activities, the ATUS diary captures the location of activities, and the identities of the respondent's other household members and associates who were present.

I work with a subset of the 170,842 time diaries collected in the ATUS from 2003 through 2015 (school fiscal years ending in 2003 through 2016). The ATUS is a stratified random subsample of outgoing CPS rotation groups, and consequently, each respondent reports one diary day, yielding repeated cross-sections. Although time allocation may be uneven across days, the highly scheduled nature of activities like work and schooling ensures that diary days are largely representative of the full week for most respondents; this may be less the case for unscheduled activities like homework and some childcare.[34]

Because the ATUS is a representative sample of the United States population, it captures school staff (workers who report employment in the K-12 education industry; N=7,267), high school children (aged 15 through 19 and eligible for high school: either enrolled in high school or not completed high school; N=9,150), and parents of school children (own household children aged 5 through 19 and eligible for school, and parents themselves are not enrolled in any level of education, or employed or reporting previous employment in the

[33]There is some evidence that urban schools are scheduled earlier than rural schools, but this should not affect test scores because, holding the testing date constant, rural schools do not outperform urban schools on the NAEP.

[34]Stratification likewise ensures that the sample is proportionate to state populations, to calendar days, and to household composition; and stratification ensures that weights do not take on extreme values, despite oversampling for weekends and certain demographic groups.

education industry or occupations;[35] N=43,409).

The ATUS methodically codes activities. For school staff, I focus on measures of work done at school and at home. For high school children, I focus on education activities (class, homework, in-school extra-curriculars, and administrative activities) and all other activities, any of which could be substituted for education activities. For parents of school children, I focus on time with children, and time spent on childcare (activities initiated by or because of children) ranging from helping them with homework and taking them to their activities, to medical care, to playing games, talking, and providing supervision.

Time diaries are considered the most (and in some reports, the only) accurate survey method to collect information on time allocation (Juster and Stafford, 1991; Hamermesh, Frazis, and Stewart, 2005). Time diaries benefit from both from their recent recall period, and the specificity of starting and stopping times. Time diaries capture data that are almost identical to random activity sampling ("beeper studies") and direct observation for the same individuals over the same period, and that are surprisingly accurate even for short-duration activities like walking, searching for lost items, and telephone calls (Robinson, 1985). Since the entire ATUS interview takes only 15-20 minutes (Bureau of Labor Statistics, 2016b), and is administered to experienced CPS respondents, almost all diaries are completed and judged accurate by interviewers and their supervisors.

The ATUS time diaries, and time diaries in general, address measurement error found in the CPS and other surveys that measure work activities. In addition, the ATUS is more responsive than the CPS to changes in work activities:

- The seasonality of work in the K-12 education industry reveals the difference in criterion accuracy between measures of working hours in the ATUS and CPS. Figure 6 shows that for workers employed in the K-12 education industry, ATUS diary hours on weekdays decline by 60 percent from October to July, CPS "actual hours of work last week" decline by only 18 percent, and CPS "usual hours of work" are steady.[36]

- In a validation study of the PSID, hourly workers' reports of their actual hours worked over the previous pay period (a stylized time use question) had a correlation of only 0.614 with company records (Rodgers, Brown, and Duncan, 1993).

- Inaccuracies and unresponsiveness in the CPS working hours questions arise for at least four reasons: First, 38 percent of hours reports in the CPS are proxy reports

[35] The latter restrictions are meant to allay concerns about direct labor market effects of reforms on parents who are educators

[36] For a variety of reasons, the average worker in the K-12 education industry works fewer than eight hours a day: most importantly, the data include employed workers on school holidays, leave, and sick days; and most categories of workers work fewer hours than teachers.

> from household members, while all ATUS reports are provided by the respondent: proxy responses to the CPS usual hours question are less likely to depend on true usual hours, are more likely to be heaped at 40 hours per week, and are more likely to be corrected in subsequent interviews by the respondent. Second, a large proportion of hours reported in the CPS are imputed: 29 percent of usual hours reports are corrected in subsequent interviews even when they were not initially provided by proxies. Third, interviewers for and respondents to the ATUS take time use questions more seriously: responses to identical usual hours questions by individuals who answered both the CPS (in month 8, excluding proxy responses) and the ATUS are more likely to be heaped, and are less dispersed (even when excluding 40-hour responses), in the CPS question. Fourth, even when reports are not given by proxies, imputed, or taken casually, stylized working hours questions may be answered according to social norms about working hours, rather than to the respondent's actual experience.

Measurement error for hours in the CPS and other labor force surveys is especially problematic because it is not classical measurement error. Instead, measurement error for hours is negatively correlated with the true deviation of hours from the trendline, and therefore biases the estimated effect of reforms on hours towards zero. These facts emphasize the value of using the ATUS time diaries, in which measurement error is minimized and is plausibly orthogonal to the ground truth, to measure the effects of reforms on labor supply.

Because both "usual hours worked" and "actual hours worked last week" are poorly measured in the CPS, no effect of reforms on CPS-reported hours was found. Although the ATUS usual hours reports are of higher quality than the CPS usual hours reports, effects of reforms on ATUS usual hours are also highly attenuated relative to effects on ATUS diary hours, as will be shown in the results section.

4.4 Additional data

The School District Universe Survey (SDUS), like the SDFS, is a mandatory, audited annual survey of the universe of public school districts, and collected data on dropout and diploma completion from 1991 through 2010.

5 Results

Schools use reform funding to raise quantity, not quality. I begin by presenting evidence that schools do not use reform funding to hire more or better staff, and that the rate of learning (value added per unit time, a measure of school quality) does not change. Next, I present evidence that schools extend the school year using reform funding. Finally, I present evidence on the opportunity cost of schooling. All evidence is for the period from 2003 through 2015.

5.1 Reforms and school quality

Schools choose to maintain quality. This can be demonstrated with data on inputs, and data on value added.

5.1.1 Inputs

Schools do not hire more staff with funding from reforms. In the period from 2003 through 2015, the proportion of employed workers reporting employment as K-12 staff falls by 0.6 percent, per reform (Table 4, Column 1; implied elasticities to total and compensation expenditure of -0.14 and -0.17 respectively), and the proportion reporting employment as teachers falls by an almost identical 0.5 percent, per reform (Column 2). The estimated decline is larger for K-12 workers reporting public-sector employment (Column 3). Only in DDD specifications are estimates positive and significant, ranging from 0.5 percent to 1.2 percent with comparison groups including professional service industries, colleges and universities, government, state and local government, and non-profit organizations (Columns 4 through 8; implied elasticities at most 0.28 and 0.33 respectively). These estimates are robust to the inclusion of state trends (Table 5) and to including CPS data from 1990 onwards while keeping the set of reforms the same (Table 6). In no case are the changes in employment large enough to match rising expenditures.

In the period from 2003 through 2015, I find smaller elasticities of employment to expenditure than were found in earlier periods by other authors. Sims (2011b) reports an elasticity of staffing to total expenditure of 0.30 for reforms from 1991 through 2002, using data from the SDFS and SDUS, and his results are consistent with my results from the CPS in the full period of 1990 through 2015, including all reforms. I find an employment gain of 1.4 percent for teachers (Table 7, Column 2) with an identical implied elasticity to total expenditure of 0.30. Thus, it appears that earlier reforms had stronger effects on staffing. Further confirming the interpretation that results are driven by the sample period, Jackson, Johnson, and Persico (2016) report an elasticity of the student-teacher ratio to total expenditure that (with constant enrollment) implies an elasticity of teacher employment in the neighborhood of 0.60, based on the SDFS and SDUS for the period from 1987-2010.[37]

[37] The results in Jackson, Johnson, and Persico (2016) also rely on an estimator that exploits substate variation and that may by affected by changes in enrollment within states, in ways that my estimator and the estimator in Sims (2011b) are not.

In any estimate, however, expenditures grow faster than employment. This implies rising salaries. As I will argue in the next section, staff are being paid in reforms to increase their labor supply on the intensive margin (that is, salaries aren't providing rents).

5.1.2 Value added

The rate of learning in schools does not rise after reforms. In the period from 2005 through 2013, the effect of reforms on the rate of learning was a precise zero, with a confidence interval on the order of half a percent of the mean rate of learning across schools (Table 8, Column 4). Thus, any cumulative achievement gains would come from instruction outside the testing window: tests are taken in January, February, and March.

Cumulative gains on the NAEP range from zero to small positive effects. In the period from 2003 through 2013, I find zeroes for three different independent variables: number of reforms to date, cumulative years the school has been exposed to reforms, and cumulative years the students have been exposed to reforms given the number of grades completed (Table 8, Columns 1 through 3). The confidence interval for years of exposure is on the same order of half a percent of the mean rate of learning (compare with Column 4).

My estimates are consistent with those in Lafortune, Rothstein, and Schanzenbach (2016), but somewhat smaller. In Section 6 I compare my achievement results with the literature, and discuss their implications.

Reforms do not raise school quality, as measured by tests, but they may raise school quantity without affecting test scores.

5.2 Reforms and school quantity

Instead of raising quality, schools choose to improve quantity (extended learning time, ELT, in the form of a longer school calendar).[38] Extended learning time does not refer exactly to staff working hours, nor to hours spent in class by students: it may refer to non-academic enrichment activities; or to grading and planning time, collaboration, and professional development for teachers. Thus, the estimates in this section only approximate the effect of school finance reforms on what would officially be called learning time by policymakers.

I show the effects of reforms on school quantity using two independent samples of data from the ATUS (students and school staff), and a third independent sample from the CPS

[38] Learning time is a major focus of federal and state education policy, independent of reforms: the ARRA of 2009 and the Every Student Succeeds Act of 2015 dedicated several billion dollars to extend learning time in failing schools, Arizona and Massachusetts have state grant programs with similar requirements, and large urban districts including Chicago, Boston, and Houston have extended learning time in the past five years. Charter schools are also noted for extending learning time.

(school staff). I present findings on aggregate hours from the two ATUS samples, and on the timing of added hours across the school calendar from the ATUS student sample and the CPS school staff sample.[39] Thus, each school quantity result is replicated in two independent samples, lending confidence beyond nominal levels of statistical significance.

5.2.1 Students

Reforms extend learning time for students, by adding weeks to the end of the school year, in the period from 2003 through 2015.

Children eligible for K-12 schooling and aged 15-19 report 69 more hours of education activities[40] per year (5.5 percent more) in the ATUS diaries, per reform (preferred estimate reported in Table 9, Column 1). The elasticities of education hours to total and compensation expenditure are 1.3 and 1.5 respectively; and the costs are 9 and 5 dollars per hour respectively (combining estimates from Table 9 and Table 1).

These estimates are highly robust. They are similar or larger when controls for employment and full or part time status, the presence and education of parents, and state trends are included, when reforms are allowed to take effect a year earlier, and when the first, second, and third reforms in each state are entered separately (Table 9); and when the sample is altered by winsorizing the sampling weights, restricting responses to the CPS sampling frame that ran from August 2005 through April 2014,[41] and dropping older children, children who do not report being enrolled in high school (who may be on holiday), summers, weekends, and interviews deemed by the interviewer to be low quality (Table 10).[42]

More than two thirds of the additional time is spent in classes (Tables 13 and 14), which

[39] The CPS school staff sample is not suitable to identify aggregate hours effects: as discussed, hours are badly mis-measured in the CPS; and in addition, it is hard to impute aggregate hours effects from reported periods of paid leave and unemployment (which do not capture the length of the school day, in any case). The ATUS school staff sample is not suitable to identify effects across the school calendar: the sample is small, and smaller during the summer because occupations are not reported for school staff who are unemployed or not-in-the-labor-force (NILF) (common among school staff during the summer vacation).

[40] Education activities are class time, homework, extracurriculars, and administrative activities. Class time and homework are the most important activities. I pool all activities because, if a school has additional working hours from teachers and other staff, staff can use the additional hours either for direct interaction with students, or for grading and evaluating additional assignments. Some reforms might favor class time, while other reforms might favor homework time, meaning that there is more room for noise in the separate (class or homework) results than in total education time.

[41] The primary sampling units (PSUs) for the CPS were re-sampled in August 2005 and May 2014, meaning that state average outcomes in periods before and after re-sampling are calculated for slightly different (albeit equally representative) samples.

[42] Preferred estimates include survey responses on summer and weekend days. Particularly when schools may extend learning time, summer and weekend days are the most likely to be affected (to have added learning time). Thus, dropping summer and weekend days would bias the estimated effects of reforms towards zero. Instead of leaving out summers, weekends, and holidays, I control for them in every specification to ensure that random sampling doesn't add noise to estimates.

occur on more days than before reforms (Tables 15 and 16); although estimates are not significant they are of similar magnitude to the estimates for all education activities. Effects on homework are proportionate and significant (Tables 11 and 12).

Additional hours are added primarily in May, June, and July (Figure 9), at the end of the school year, with the largest effects in June. These results come from a specification in which the reform indicator is interacted with an indicator for the month of the year.

Finally, these results are not driven by changes in school participation among children eligible for K-12 schooling and aged 15-19: the increase in attendance is on the order of 0.09 percent (Table 17, Column 3), and diploma completion actually declined by a similar amount (Table 18, Column 1).

5.2.2 School staff

School staff work more days of the year after reforms. Their labor supply rationalizes salary increases paid out by schools after reforms, and enables the extension of learning time seen for students after reforms.

School staff report 117 more hours of work per year (6.8 percent more) in the ATUS diaries, per reform (preferred estimate reported in Table 20, Column 1). The elasticities of work hours to total and personnel spending are 1.6 and 1.9 respectively; and the costs are 5 and 3 dollars per hour respectively (per student; combining estimates from Table 20 and Table 1). The effect of reforms on school staff hours is larger than the effect of reforms on student education activities, because school staff work longer than students do, to prepare lessons, grade assignments, and participate in service and professional development.

Like the estimates for students, these estimates are robust. Estimates range from 80 to 106 hours of work per reform when controls for education, gender, and marital status, for employment and full time status, and for state trends are included; from 124 to 164 hours when either reforms are allowed to have effects a year earlier, or the first, second, and third reforms in each state are entered separately (Table 20). Estimates range from 69 to 164 hours when the sample is altered by winsorizing the sampling weights, restricting responses to the CPS sampling frame that ran from August 2005 through April 2014, adding respondents who were enrolled in school alongside their employment in the K-12 industry, dropping respondents who would not have been included in the sample based on their responses in their last CPS interview, dropping responses in the summer, on weekends, and that were deemed low quality by interviewers (Table 21). In all cases, reform effects on hours are larger than the preferred estimate for high school children over the same period. Finally, effects are robust to triple differences specifications that use other sectors and industries, or a re-weighted sample of workers with matched propensity scores, as control groups: K-12 staff hours grow more when compared to other government and non-profit workers, somewhat less relative to other workers in the professional services industries and the subset of education and health services workers, and about half as much when compared a group of workers matched and re-weighted for their estimated propensities to work in the K-12 education industry; the lowest estimate is 47 hours per reform (Table 22, Column 8).

Confirming that the results are not peculiar to ATUS diary reports, estimates based on usual hours reported in the ATUS are attenuated by two thirds due to measurement error negatively correlated with true hours, but remain significant (Tables 23, 24, and 25).

The ATUS sample is not suitable to estimate the effects of reforms on school staff days of work, because the ATUS does not ask the industry classification question for workers who report being unemployed (as many school staff do during the summer vacation). Thus, I estimate the effect of reforms on school staff days of work using the CPS, which asks the most recent industry in which an unemployed worker was employed.

Additional days for school staff are added primarily in June and July (Figure TKTK, of the probability that school staff are employed; and Figure 9, of the probability that they are on leave conditional on being employed), with the largest effects in June, as was the case with education activities for students. These results come from a specification in which the reform indicator is interacted with an indicator for the month of the year.

5.2.3 Resolving a potential puzzle

Why would schools add days at the end of the school year and not at the beginning of the school year? Adding days at the beginning of the year should improve test scores more than adding days at the end of the year (this will be discussed in more detail Section 6), and there is evidence that some schools in some states strategically move school opening dates earlier to raise test scores and meet accountability standards (Sims, 2008).

However, the puzzle is easily resolved. First, most schools do not face accountability pressure. Second, many states set an earliest date that schools may open, at least in part to prevent a strategic rush among schools to move start dates earlier.[43] School districts may restrict start dates for the same reasons as states. In contrast, most states and school districts do not set a latest date that schools may close. Thus, schools can readily add days at the end of the year, but not at the beginning.

[43]Four states out of the ten most populous have an earliest start date (Texas, Florida, North Carolina, and Michigan), two have proposed state legislation to establish an earliest start date (Pennsylvania and Ohio), and four do not have an earliest start date (California, New York, Illinois, and Georgia) and give discretion to school districts.

5.3 The extended school year and opportunity costs

Time at school necessarily crowds out other activities. But which activities? Do students give up leisure, work, home production, or non-school investment in human capital? Do parents compensate for differences in the kind and amount of education their children are given, or for differences in their children's contributions to the household?

The longer school year crowds out home production, but not home-based human capital investment. Parental activities related to schooling take less time than is often assumed in the education literature, so parental crowd-out is minimal.

5.3.1 Time reallocated by students

Students reduce home production activities in exact proportion to the increase in education activities, after reforms. No significant effects were found for work, leisure, or home-based investments, or for any other category of activities coded in the ATUS.

Children eligible for K-12 schooling and aged 15-19 report 49 fewer hours of household activities[44] per year (20 percent fewer) in the ATUS diaries, per reform (preferred estimate reported in Table 29, Column 1); they report 22 hours fewer of consumer purchases per year (22 percent fewer) per reform (preferred estimate reported in Table 30, Column 1).

These estimates, like those for education activities, are highly robust. They are similar when controls for employment and full or part time status, the presence and education of parents, and state trends are included, when reforms are allowed to take effect a year earlier, and when the first, second, and third reforms in each state are entered separately (Table TKTK); and when the sample is altered by winsorizing the sampling weights, restricting responses to the CPS sampling frame that ran from August 2005 through April 2014, and dropping older children, children who do not report being enrolled in high school (who may be on holiday), summers, weekends, and low quality interviews (Table TKTK).

5.3.2 Time reallocated by parents

Parents do not adjust time spent on children to compensate for school finance reforms, in either the long run or the short run. Parents do not reinforce the effects of school finance reforms, either. Parents, instead, are neutral.

Parents report 8 more hours of time with their own K-12 eligible household children in the ATUS diaries, per year, per reform, within a narrow confidence interval that includes zero

[44] Household activities include cleaning, food preparation, maintenance, yard work, and animal care; and are detailed further in the American Time Use Survey Activity Lexicon, 2003-2015 (Bureau of Labor Statistics, 2016a).

(preferred estimate reported in Table 31, Column 1). The elasticities of time with children to total and compensation expenditure are 0.12 and 0.14 respectively (combining estimates from Table 31 and Table 1). Similarly indicative of small effects on parental behavior, parents report 6 more hours of childcare and 4 fewer hours of education-related childcare per year, per reform (Tables 33 and 35, Column 1). While the implied elasticity of education-related childcare to reforms is large, this is not important for education production: parental contributions of education-related childcare amount to 50 hours per year, or approximately 4 percent of the time their children spend on education annually.[45]

Overall, these null results are highly robust: out of 48 estimates presented in Tables 31 through 36, only two reach conventional levels of significance, and the majority are close in sign and magnitude to the preferred estimates in Column 1 of each table. These tables vary the controls, measures of reforms, and samples used to assess parental behavior. It is hard to attribute the results to measurement error: validation studies show that time diaries closely track direct observation, and this paper reports significant effects of reforms on diary measures of time use by school staff and children.

6 Discussion: school quantity versus school quality

The new findings presented in this paper speak to the efficiency of school spending, to our ability to measure the impacts of reforms, and to the private costs of reforms. When schools improve quantity, and not quality, conventional estimates likely understate both the true achievement effects and the true costs of reforms.

6.1 The efficiency of school spending

Schools raise quantity because it is more cost efficient (for schools, although perhaps not for families) to raise quantity, compared to quality.

Raising quality (defined by the rate of learning) typically means hiring more or better staff, or motivating staff to exert more effort, all of which would mean paying a higher average wage. Higher ability staff command higher wages, and replacing staff means working around rigidities in labor contracts. Expanding employment would mean either hiring more workers

[45] This new evidence from cardinal measures of time allocated to education-related childcare suggests that, contrary to the typical choice of "have you ever helped your child with homework" questions to measure parents' responses to school policies in education research, this group of activities may not be the best place to look for economically relevant offsetting or reinforcing behavior. The small amount of time allocated to education-related childcare is not likely due to rounding-down or under-reporting by parents. 13 percent of parent diary-days (weighted) include some education-related childcare, and, as discussed in Section 4, time diaries are fairly accurate even for minor, sporadic activities.

of a given ability, or retaining more workers. Hiring workers drives up costs for training, professional development, and management, while driving down quality because new staff are less productive. Retaining workers drives up costs for seniority pay, and drives down quality because schools have to be less selective. Motivating staff to exert more effort on a daily basis also means paying a higher wage.

Raising quantity typically means adding school days for existing staff. Marginal costs for an extended school year may be less than average costs, for two key reasons. First, school staff are underemployed during school vacations. Second, fixed costs in employment are large. Cogan (1981), incorporating fixed costs into Heckman's (1974) model of labor supply, estimates in labor force data that the average annual cost of participation for married women is 28 percent of average yearly earnings of working women. Donald and Hamermesh (2009) estimate in time use data that the entry cost for men and women is equivalent to 8 percent of income. Zabel (1993) explores the observational equivalence of employee constraints including entry, credentialing, location, and alternative activities; with employer constraints including minimum hours commitments motivated by production processes.

I find that the marginal cost of adding a school day to the calendar for existing staff is less than the average cost of school days: the elasticities of student time allocated to education activities, and of staff working hours, to compensation expenditure, are both greater than one. That is, hours grow more than expenditures. I estimate elasticities of 1.5 and 1.9, respectively. These estimates contribute direct evidence (rather than indirect evidence from structural models of labor force participation) for the theories of labor supply with fixed costs that Cogan (1981), Zabel (1993), and Donald and Hamermesh (2009) set forth to explain phenomena including retirement and the scarcity of part-time work.

These estimates also contribute to a growing literature on teacher labor supply and the efficiency of spending, by revealing a new margin: days of full-time work. The literature to date has largely focused on effort (e.g. Fryer, 2013; Johnston, 2013; and Goodman and Turner, 2013), ability (e.g. Lazear, 2003 and Rothstein, 2015), and employment (e.g. Falch, 2010).

6.2 The understated benefits of reforms for achievement

Raising the quantity of instruction should consistently raise test scores, if tests were appropriately timed, but the tests used in most current studies are not appropriately timed. Here, I discuss the problems of delayed measurement and fade-out inherent in the NAEP, and their consequences for estimated achievement gains. I estimate the effect of reforms on achievement purged of fade-out, based on this paper's estimates of additional learning time and of the learning rate. Next, I directly estimate the effect of reforms on achievement, and find that they are substantially smaller. The immediate effects of reforms on achievement are relevant because they predict adult outcomes (e.g. Deming, 2009; Chetty et al., 2011).

6.2.1 Test timing, fade-out, and the measurement of achievement gains

Tests capture gains from additional instruction, if any, with a one-year lag. Additional days of instruction are added in May, June, and July, while NAEP tests are administered in January, February, and March, too early to capture the immediate gains from instruction.

Test score gains are subject to fade-out. Estimates of fade-out over one year range from 50 to 80 percent of the immediate treatment effect (Kane and Staiger, 2008 and Chetty, Friedman, and Rockoff, 2014: 50 percent; Rothstein, 2010: 70 percent; Jacob, Lefgren, and Sims, 2010: 80 percent).

Thus, when tests capture gains with a one-year lag, they capture gains that are subject to a substantial degree of fade-out.

Whether the immediate effects of interventions are predictive of adult outcomes depends on the reasons for subsequent fade-out. Test score gains may fade out for any of the following five reasons, the first three of which are articulated by Jacob, Lefgren, and Sims (2010) in their work on teacher value added, and the fifth of which is articulated by Lazear, Shaw, and Stanton (2015) in their work on the value of bosses.

- **Test mismatch:** As students advance in grades, and the curriculum changes, they are tested on different material than what they were taught during the initial year of the intervention: for example, if algebra instruction improves and is tested in the first year, and geometry is taught and tested in the second year, improved algebra instruction should confer greater benefits in the first year than in the second. What is learned is retained, cumulates, and remains relevant for adult outcomes—but is hidden from the investigator by the test instruments used in evaluation.

- **Forgetting by students:** As students advance, they may also forget material that is no longer tested. While forgetting is part of everyone's experience, much of what is taught is clearly retained and put to use on a daily basis, meaning that forgetting cannot explain all of fade-out.

- **Compensation by teachers and parents:** Adults may offset interventions globally (by reducing subsequent investment), or locally (by shifting effort to unobservable investment). For example, when adults compensate globally, students at grade level may get less help than students behind grade level. Because school finance reforms affect all students, and provide additional resources continually after their initial implementation, global compensation by teachers is not a major concern. I find in Section 5 that parents do not compensate globally for reforms, either.

- **Compensation by students:** Students may offset interventions globally or locally, too. Having mastered tested academic content, students may prefer to invest effort in other academic content, in so-called non-cognitive skills or health, in leisure, or in productive activities. I cannot distinguish how time is allocated among tested and untested education activities, but I find in Section 5 that students spend more time on education activities after reforms, and that students do not reduce other investments.

- **Motivation:** The effects of interventions on testing may be temporary if the primary effect of the intervention (such as a better teacher) is to motivate students to perform well on tests. Thus, effects need not reflect actual learning in the first place. Duckworth et al. (2011) provide a meta-analysis and new data on the effects of incentive-based and intrinsic motivation on test scores, and find large effects, validating the mechanism for fade-out in general. However, because reforms are not a transient intervention, they should not have transient effects on motivation.

Based on the nature of reforms, and on the evidence reported in this paper, test mismatch and local compensation are the likeliest causes of fade-out following reform-based investments in students. With these sources of fade-out, investments do not depreciate, but may be understated by delayed measurement. Thus, failing to correct for fade-out may understate the eventual impact of reforms on adult outcomes.

6.2.2 Reform effects: implied by learning time and the learning rate

The rate of learning I estimate from the timing of NAEP tests is not subject to fade-out, because measurement is immediate. The estimated rate of learning implies cumulative gains on the NAEP of 0.49 SD per school year (Table 19, lower bound of the 95 percent CI). In Section 5, I estimated a 5.5 percent increase in the length of the school year (90 percent CI: 0.010 to 0.100), per reform.

Taken together, these estimates imply cumulative gains on the NAEP of 0.027 SD per year (90 percent CI: 0.005 to 0.049), per reform.[46]

[46] Experimental and quasi-experimental studies of the effects of learning time policies validate the claim that extended learning time leads to greater achievement. Reallocated learning time leads to subject-specific achievement gains: Cortes, Goodman, and Nomi (2015); Taylor (2014); and Cortes, Bricker, and Rohlfs (2012). Extended learning time leads to general achievement gains: Parinduri (2014); Bellei (2009); Pischke (2007); and Frazier and Morrison (1998) (but see Meyer and Van Klaveren (2013) for a contrasting null

The estimate of the learning rate that I rely on above is comparable with the literature. Using random test timing, Fitzpatrick, Grissmer, and Hastedt (2011) find a learning rate of 1.1 to 1.8 SD per school year, for kindergarten and 1st grade students in math and reading.[47] Using changes in test timing, Hansen (2011) imputes a learning rate of 0.8 to 2.3 SD per school year, for 3rd and 5th grade students in math; these estimates are backed up by an instrumental variables strategy that delivers similar estimates in other states. Using changes in test timing, Sims (2008) finds a learning rate of 0.2 to 1.2 SD per school year, for 4th grade students in math, language, and reading. These estimates are larger than the learning rate in the NAEP, and imply a larger effect of reforms on achievement than I estimate.

6.2.3 Reform effects: direct measurement

My direct estimate of the effect of reforms on cumulative gains on the NAEP is 0.000 SD per year (95 percent CI: -0.004 to 0.005). This result, from Table 8, Column 4, is discussed in more detail in Section 5.

The difference between the direct and indirect estimates is significant. In a Welch test of the null hypothesis that the indirectly measured gains are less than or equal to the directly measured gains, the p-value is 0.021. I make the conservative assumption that the direct and indirect estimates are uncorrelated in the sample: if the estimates are truly correlated (as we would expect them to be if reforms raise both learning time and achievement), then the true p-value is smaller.

Other authors provide direct estimates of the effect of reforms on NAEP achievement, which are similarly small. My direct estimate is consistent with state-average estimates in Lafortune, Rothstein, and Schanzenbach (2016) of 0.004 SD per year[48] (95 percent CI: -0.002 to 0.010), and their estimate for the most-affected quartile of schools, of 0.007 SD per year[49]

result). Finally, charter school policies including extended learning time lead to greater achievement both in charter schools and in public schools that adopt the same policies: Fryer (2014); Dobbie and Fryer (2013); Hoxby, Murarka, and Kang (2009).

[47] Also using random test timing, Carlsson et al. (2015) find a learning rate of 0.1 to 0.2 SD per school year, for 12th grade students on the language section of a military entrance exam; because the exam is cumulative over many years, learning time may have smaller effects on content knowledge than for grade-specific tests.

[48] While there are some differences between my methods and those in Lafortune, Rothstein, and Schanzenbach (2016), their results are based on the same NAEP testing data, and similar identifying variation. My estimates come from a more recent (2003-2015, rather than 1990-2013) but comparable period of reforms: the average effects on funding, compensation, and employment are similar across periods.

[49] While my study is concerned with the mean effects of reforms, it is worth considering why some schools have greater measured gains from reforms. Two conventional explanations fail to explain the heterogeneity. The first explanation is that reforms direct more funding to poorer-performing schools, but it is ruled out by the fact that higher-performing schools make no testing gains at all, despite getting at least half as much funding as poorer-performing schools. The second explanation is that test scores are easier to raise at the bottom of the distribution, but it is ruled out by the fact that there is only minor convergence in the distribution of test scores over time, even within schools. A more plausible explanation for greater test score

(95 percent CI: 0.001 to 0.013), is a quarter as large as my indirect estimate of cumulative gains based on the rate of learning. Clark (2003) also found null effects of funding from the Kentucky Education Reform Act (KERA) of 1990 on NAEP achievement. These null effects may be attributed to delayed measurement, and to fade-out.[50]

When schools raise quantity (or could) to raise achievement, evaluations relying on tests should use tests timed as soon as possible after instruction.

6.2.4 Fade-out and re-emergence of achievement gains from reforms?

Fade-out masks achievement effects that predict adult outcomes. The behavioral evidence presented in Section 5 of this study suggests that students, parents, and teachers may act in ways that reduce the impacts of reforms on test scores over time, but not in ways that offset reform-driven investments in human capital.

Thus, the effects of reforms may re-emerge in adulthood. Recent findings in Jackson, Johnson, and Persico (2016) suggest that, despite the small effects of reforms on achievement in this study and others in the literature, reforms have large and important effects on adult outcomes: reforms increase educational attainment, raise adult earnings, and reduce adult poverty. Consistent with these national findings, fade-out and re-emergence appear in state-level studies of education reform in Michigan (Proposal A of 1994): Papke (2005) and Roy (2011) find mixed effects of the reform on achievement, with some effects in early grades, while Hyman (forthcoming) finds large effects on college enrollment and completion.

The literature on school finance reforms thus contributes to a growing body of evidence on fade-out and re-emergence: this pattern is found in studies of Head Start preschool (Deming, 2009; Currie and Thomas, 1995; and Garces, Thomas, and Currie, 2002), class size reduction (Chetty et al., 2011; Fredriksson, Öckert, and Oosterbeek, 2013), and teacher value added and classroom quality (Chetty et al., 2011; Chetty, Friedman, and Rockoff, 2014). I contribute evidence on the household behavior that underlies fade-out and re-emergence.

gains at poorer-performing schools is that they face greater accountability pressure, focus more resources on the kind of instruction that raises test scores, and strategically time instruction so that its effects will be measurable; see Chiang (2009) and Rouse et al. (2013) for evidence of this kind of behavior.

[50] Even without delayed measurement, direct estimates of the effect of school finance reforms on achievement should be attenuated by fade-out: the effect of reforms on cumulative achievement is typically estimated by fitting a straight line to the trend in test scores after a reform event, and treating the coefficient as an estimate of gains from one year of exposure to reforms. Were the first year of test data taken immediately after an intervention, the resulting estimate would be one-third as large as the immediate impact of one year of exposure to reforms, because cumulative impacts are an average over immediate impacts and past impacts that have already diminished: based on Chetty, Friedman, and Rockoff (2014), impacts fade out to 50 percent after a year, 33 percent after two years, 25 percent after three years, 20 percent after four years.

6.3 The understated private costs of reforms

Raising quantity imposes private costs on students and families. Time for school is taken out of time for other activities including leisure, home production, or non-school investments in human capital. Since parents contribute to these activities, they may also be affected by learning time interventions and hence by reforms.

The cost of an hour put into school is the value of an hour taken out of another activity. This cost is fundamental in education production, and economists have taken it seriously at least since the work of Mincer (1958) and Ben-Porath (1967). Becker (1965) wrote that, "Most economists have now fully grasped the importance of foregone earnings in the educational process and, more generally, in all investments in human capital, and criticize educationalists and others for neglecting them," yet, in the present literature on learning time (on the expansion of school days and school years, rather than of years of schooling) the opportunity cost of students' time remains neglected.

The private cost of additional instruction is up to an order of magnitude larger than the public cost. Since all people face the same time budget, we may assume the value of time for students is similar to the value of time for staff. Since students outnumber staff eight to one, each day of work by staff (the public cost) is matched by eight days of time from students (the private cost). Thus, for a given gain in achievement, an intervention that raises quantity is more costly than an intervention that raises quality.[51] Before we advocate for extended learning time policies on the basis of achievement gains, economists must carefully weigh their private cost relative to school quality policies.

How private costs are allocated matters. The data used in this paper lead to two novel and potentially surprising conclusions about family behavior:

First, school-related investments by parents are not an important margin of adjustment after reforms. Becker and Tomes (1986), Todd and Wolpin (2003), and Glewwe and Kremer (2006) have all argued that parents may reduce school-related investments in their children following improvements in public education, reducing the impact of education policies on student outcomes.[52] Parents do not adjust school-related investments in children, because

[51] Exactly how costly is it to spend more time in school? The value of time in school and time outside of school need not be equalized on the margin, making it hard to give a concrete answer. Time spent in school could be more valuable (or potentially less valuable) to students than time spent in alternative activities, because students do not choose how much time to spend in school.

[52] Todd and Wolpin (2003) emphasize that whether parental investments should offset or reinforce school inputs depends on the unknown parameters of the education production function, and of parents' preferences: complementarities between investments made by schools and by parents could lead to reinforcing behavior, while substitutabilities could lead to offsetting behavior. It is plain that both complementarities and substitutabilities exist in educational production: for example, efforts made by parents to get children out of bed and onto school buses are complementary to instruction that is provided only to students who are present in the classroom, and efforts made by schools to teach students arithmetic save parents the trouble

school-related investments are already crowded out by the services schools offer. This null result, for quantitative measures of time use, updates qualitatitive results that education interventions reduce or increase school-related investments (Houtenville and Conway, 2008; Pop-Eleches and Urquiola, 2013; Das et al., 2013; Gelber and Isen, 2013; Fredriksson, Öckert, and Oosterbeek, forthcoming; Kline and Walters, 2016; and Araujo et al., 2016). Evaluators should consider the baseline magnitude of school-related investments.

Second, home production is an important margin of adjustment after reforms. Students allocate time costs from the extended school year to home production, rather than to leisure or non-school investments, and since parents do not re-allocate equivalent time to home production, the ultimate cost is lower household consumption. This evidence is the first, to my knowledge, of how children reallocate time from household activities to education activities. This is also the first evidence of how parents adjusted their activities, apart from school-related investments, following an education intervention. These findings drive home Becker's (1965) point that, "the allocation and efficiency of non-working time may now be more important to economic welfare than that of working time." Evaluators should recognize that partial equilibrium effects on the family are not restricted to school-related investments, and focus measurement on more common activities.

7 Conclusion

Whether school funding from reforms is helpful to students depends on how funding is used, a black box in the existing literature.

This paper looked inside the black box of school finance reforms using the CPS, the NAEP, and the ATUS to better measure school quality and school quantity. The main contributions were to measure quality directly as the rate of learning; and to measure quantity in terms of intensive margin labor supply by school staff, and learning time by high school students.

Schools improved quantity, but not quality, resolving two puzzles about spending:

1. Why did schools spend more on the same staff? (It is less costly to have the same staff teach more, than to hire more or better staff.)

2. Why did students do no better on tests, at least statewide, and should that be cause for alarm or for less spending? (The impact of added learning time is measured with a year delay, which implies significant fade-out; this is not a cause for alarm, but it is a good reason to think harder about the test instruments used for impact evaluation.)

of doing so later when this basic skill becomes necessary. Whether complementarity or substitututability dominates, and to what degree, is an empirical question with important implications for program evaluation.

The results in this paper introduce a new puzzle: teachers invest in human capital that is not employed during school vacations. Does the low marginal cost of additional school days, which seems to rule out supply side explanations, mean that there is low demand for school staff during school vacations? In other words, why haven't schools already added days to the school calendar? Why now? One possibility is that families object to a longer school calendar because of the private costs it imposes (and that are measured in this research).[53] Increased state funding might make schools less accountable to families, and more accountable to the state, which cares primarily about student outcomes. However, with extensive state funding of schools already, it is not clear why a reform would make a difference on the margin, leaving the field open for further research on labor demand in schools.

[53] Fischel (2006) suggests that the summer vacation serves a coordinating purpose, for families to relocate among schools while classes are not in session; in this account, the private costs are not vacation time *per se*, but instead the loss of coordinated instruction if there were no summer vacation.

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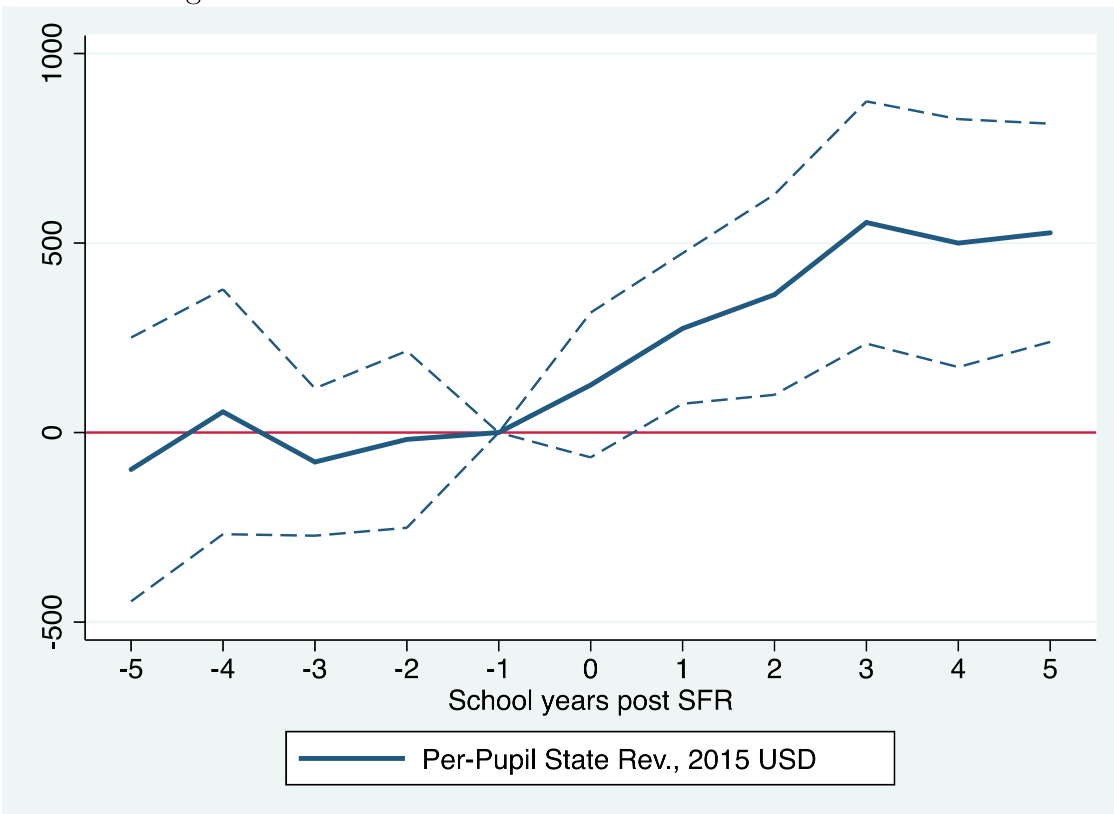
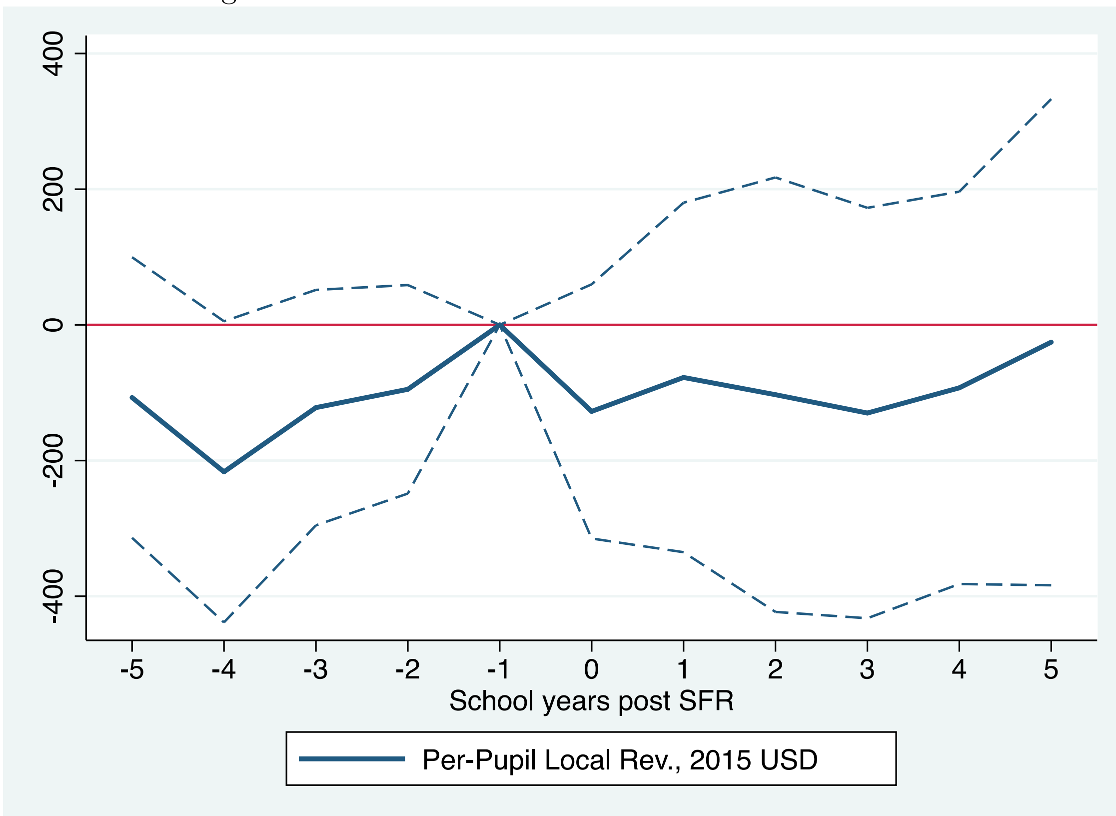
9 Figures and Tables

Figure 1: Reforms Raise State Revenues to School Districts



Data: school finance reforms (independent variable) from 1990 through 2013 (N=86 events, in 28 states). SDFS fiscal variables (dependent variables) annually from 1990 through 2013 (N=311193 observations, from 15087 local education authorities in 50 states and Washington, D.C.; observations from 1991 are omitted because of problems with the data extract).
Regression: reform coefficients are estimated using Equation (2), a state-level non-parametric event study that includes district fixed effects, year fixed effects, and state trends. Year -1 is the last pre-reform year. Standard errors are clustered by state, and 95 percent confidence intervals are displayed with dotted lines.

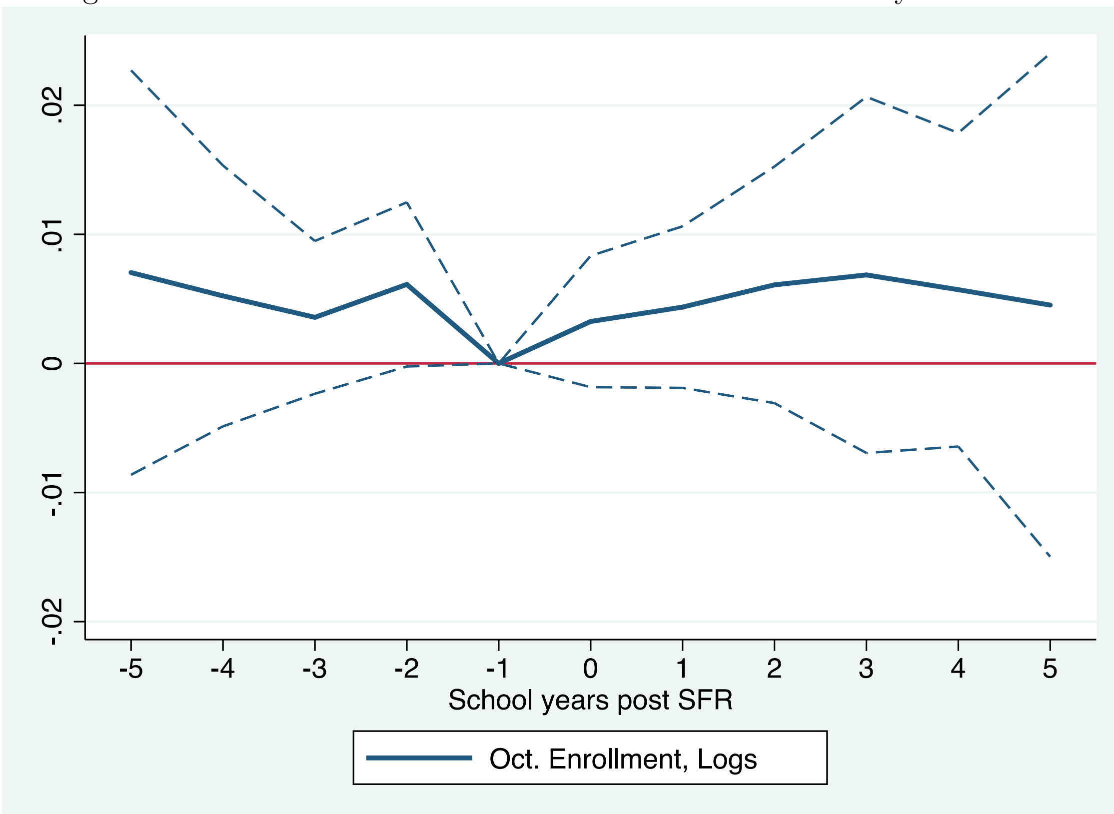
Figure 2: Reforms Do Not Crowd Out Local Revenues



Data: school finance reforms (independent variable) from 1990 through 2013 (N=86 events, in 28 states). SDFS fiscal variables (dependent variables) annually from 1990 through 2013 (N=311193 observations, from 15087 local education authorities in 50 states and Washington, D.C.; observations from 1991 are omitted because of problems with the data extract).

Regression: reform coefficients are estimated using Equation (2), a state-level non-parametric event study that includes district fixed effects, year fixed effects, and state trends. Year -1 is the last pre-reform year. Standard errors are clustered by state, and 95 percent confidence intervals are displayed with dotted lines.

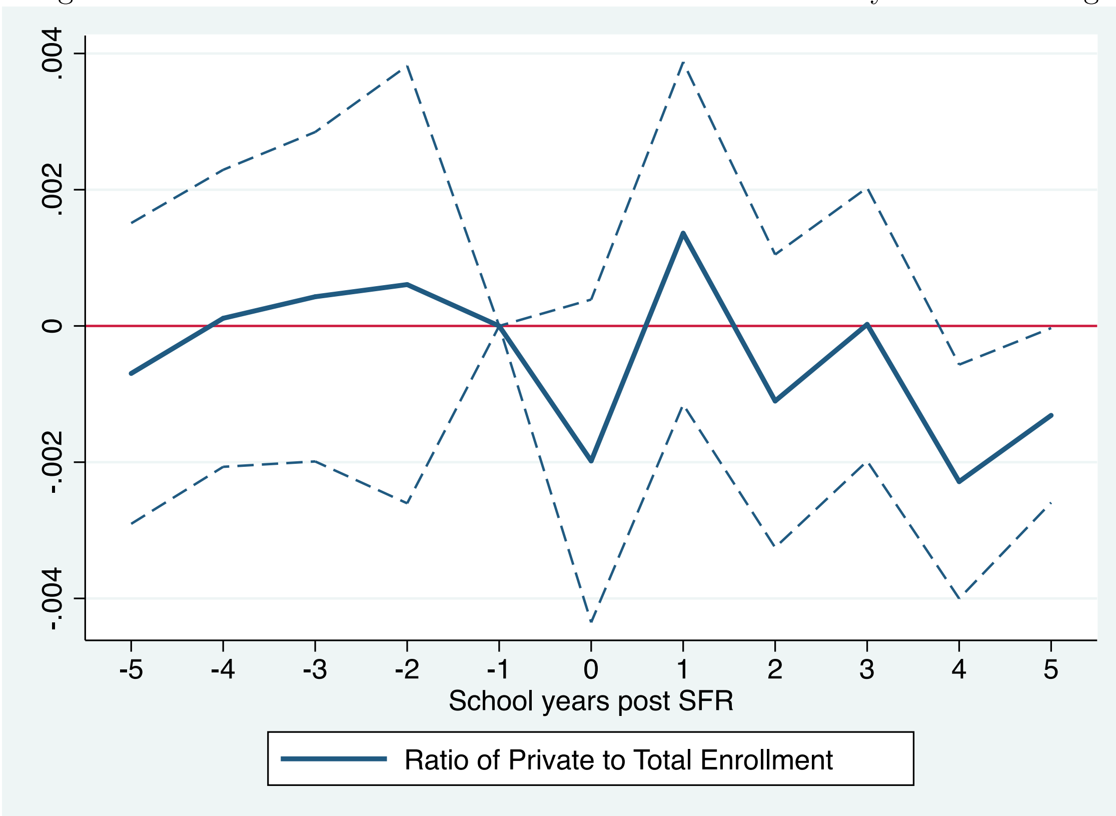
Figure 3: Estimates of Reform Effects Are Not Confounded by Enrollment



Data: school finance reforms (independent variable) from 1990 through 2013 (N=86 events, in 28 states). SDFS fiscal variables (dependent variables) annually from 1990 through 2013 (N=311193 observations, from 15087 local education authorities in 50 states and Washington, D.C.; observations from 1991 are omitted because of problems with the data extract).

Regression: reform coefficients are estimated using Equation (2), a state-level non-parametric event study that includes district fixed effects, year fixed effects, and state trends. Year -1 is the last pre-reform year. Standard errors are clustered by state, and 95 percent confidence intervals are displayed with dotted lines.

Figure 4: Estimates of Reform Effects Are Not Confounded by Sector Shifting



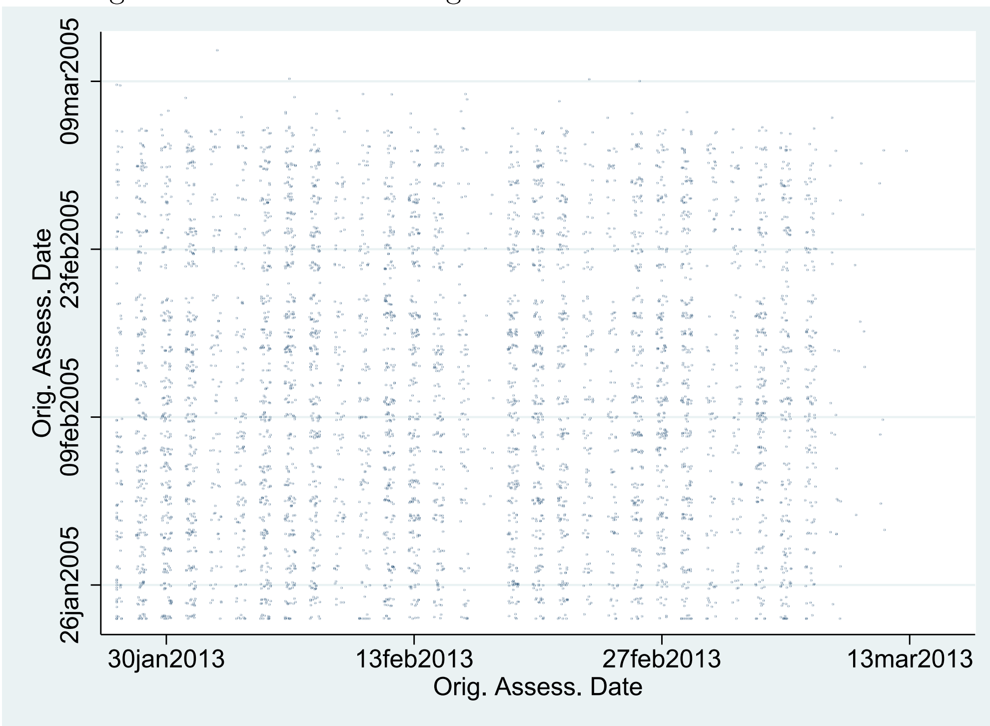
Data: school finance reforms (independent variable) from 1990 through 2013 (N=86 events, in 28 states). SDUS and PSS (dependent variables) in even years from 1990 through 2012 are aggregated up to the county level before the ratio of private school to total enrollment is taken (N=21443 TKTK observations, from 2485 TKTK counties in 50 states and Washington, D.C.).
Regression: reform coefficients are estimated using Equation (2), a state-level non-parametric event study that includes district fixed effects, year fixed effects, and state trends. Year -1 is the last pre-reform year. Standard errors are clustered by state, and 95 percent confidence intervals are displayed with dotted lines.

Table 1: Fiscal Effects of Reforms: 2003 through 2013

	(1) State Rev.	(2) Federal Rev.	(3) Local Rev.	(4) Total Rev.	(5) Total Exp.	(6) Current Exp.	(7) Comp. Exp.
SFR	355***	-19	203	539**	611***	396*	338*
	(122)	(23)	(149)	(209)	(176)	(212)	(199)
Mean of D.V.	6461	1199	6427	14087	14063	11832	9329
β/Mean	0.055	-0.016	0.032	0.038	0.043	0.033	0.036
SFR	2003-	2003-	2003-	2003-	2003-	2003-	2003-
State FE	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y
States	51	51	51	51	51	51	51
Districts	13761	13761	13761	13761	13761	13761	13761
Observations	144999	144999	144999	144999	144999	144999	144999
R^2	0.029	0.101	0.048	0.063	0.036	0.089	0.104

Data: school finance reforms from 2003 through 2013 (N=29 events, in 19 states).
SDFS fiscal variables annually from 2003 through 2013.
Regression: reform coefficients are estimated using equation (4).
Standard errors in parentheses, clustered by state
* $p < .10$, ** $p < .05$, *** $p < .01$

Figure 5: Dates Are Orthogonal Across Years: 2005 and 2013



Data: originally assigned NAEP assessment dates by school and grade in 2005 and 2013 (N=TKTK).

Table 2: Pairwise Correlations of Dates by School and Grade Are Small

	2005	2007	2009	2011	2013
2005	1				
2007	0.0703	1			
2009	0.0447	0.0611	1		
2011	0.0260	0.0505	0.0614	1	
2013	0.0349	0.0733	0.0591	0.0707	1

Table 3: Pairwise Correlations of Dates by School and Grade Are Small: Removing State-by-Year Fixed Effects

	2005	2007	2009	2011	2013
2005	1				
2007	0.0530	1			
2009	0.0336	0.0406	1		
2011	0.0176	0.0195	0.0475	1	
2013	0.0354	0.0628	0.0475	0.0626	1

Figure 6: Accuracy of the ATUS

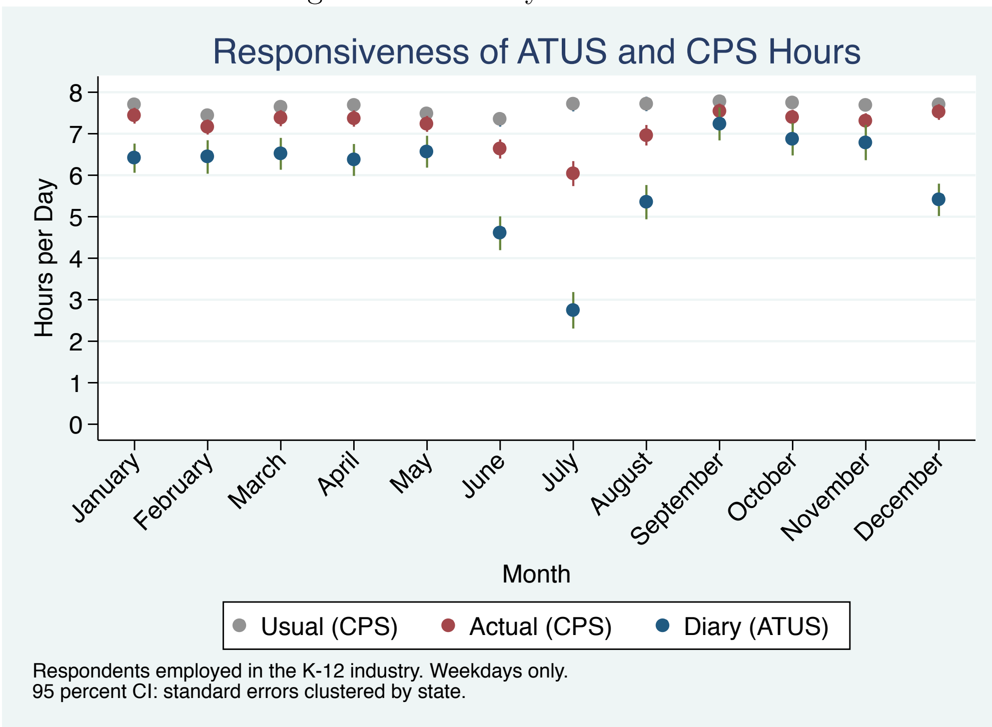


Table 4: SFR and Employment of K-12 Staff: 2003 through 2015

	(1) K-12 Ind.	(2) K-12 Ind. Teacher	(3) K-12 Ind. Public	(4) K-12 Ind.	(5) K-12 Ind.	(6) K-12 Ind.	(7) K-12 Ind.	(8) K-12 Ind.
SFR	-0.00036	-0.00021	-0.00097	0.0010	0.0082**	0.0045*	0.0051*	0.0057
	(0.00056)	(0.00045)	(0.00062)	(0.0023)	(0.0031)	(0.0026)	(0.0027)	(0.0048)
Mean of D.V.	0.062	0.039	0.049	0.224	0.741	0.388	0.460	0.528
β/Mean	-0.006	-0.005	-0.020	0.005	0.011	0.012	0.011	0.011
SFR Definition	Cumul.	Cumul.	Cumul.	Cumul.	Cumul.	Cumul.	Cumul.	Cumul.
State FE	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y
State Trends								
Date Controls	Y	Y	Y	Y	Y	Y	Y	Y
Comparison Group				Profes. Service Industry	Coll. and Uni.	Gov't	State and Local	Non Profit
States	51	51	51	51	51	51	51	51
Respondents	2238477	2238477	2238477	631927	192832	372261	309115	280068
R^2	0.001	0.001	0.001	0.004	0.012	0.012	0.008	0.026

Data: school finance reforms from 2003 through 2013 (N=29 events, in 19 states). CPS ORG (months 4 and 8) from 2003 through 2015, sample of respondents reporting employment, not enrolled. Date controls: month.

Regression: reform coefficients in Columns 1 through 3 are estimated using equation (4), and Columns 4 through 8 are estimated using equation (5). Comparison groups are other workers in: professional service industries, colleges and universities (a subset of professional service industries), government, state and local government, and non-profits. Standard errors in parentheses, clustered by state.

* $p < .10$, ** $p < .05$, *** $p < .01$

Table 5: SFR and Employment of K-12 Staff: 2003 through 2015, with state trends

	(1) K-12 Ind.	(2) K-12 Ind. Teacher	(3) K-12 Ind. Public	(4) K-12 Ind.	(5) K-12 Ind.	(6) K-12 Ind.	(7) K-12 Ind.	(8) K-12 Ind.
SFR	-0.000041	-0.000050	-0.00034	0.0025	0.0087	0.0046	0.0040	0.0057
	(0.00077)	(0.00075)	(0.00091)	(0.0021)	(0.0069)	(0.0058)	(0.0068)	(0.0040)
Mean of D.V.	0.062	0.039	0.049	0.224	0.741	0.388	0.460	0.528
β/Mean	-0.001	-0.001	-0.007	0.011	0.012	0.012	0.009	0.011
SFR Definition	Cumul.	Cumul.	Cumul.	Cumul.	Cumul.	Cumul.	Cumul.	Cumul.
State FE	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y
State Trends	Y	Y	Y	Y	Y	Y	Y	Y
Date Controls	Y	Y	Y	Y	Y	Y	Y	Y
Comparison Group				Profes. Service Industry	Coll. and Uni.	Gov't	State and Local	Non Profit
States	51	51	51	51	51	51	51	51
Respondents	2238477	2238477	2238477	631927	192832	372261	309115	280068
R^2	0.001	0.001	0.001	0.004	0.013	0.012	0.008	0.026

Data: school finance reforms from 2003 through 2013 (N=29 events, in 19 states). CPS ORG (months 4 and 8) from 2003 through 2015, sample of respondents reporting employment, not enrolled. Date controls: month.

Regression: reform coefficients in Columns 1 through 3 are estimated using equation (4), and Columns 4 through 8 are estimated using equation (5). Comparison groups are other workers in: professional service industries, colleges and universities (a subset of professional service industries), government, state and local government, and non-profits. Standard errors in parentheses, clustered by state.

* $p < .10$, ** $p < .05$, *** $p < .01$

Table 6: SFR and Employment of K-12 Staff: 2003 through 2015, with state trends and data from 1990

	(1) K-12 Ind.	(2) K-12 Ind. Teacher	(3) K-12 Ind. Public	(4) K-12 Ind.	(5) K-12 Ind.	(6) K-12 Ind.	(7) K-12 Ind.	(8) K-12 Ind.
SFR	-0.0010	-0.00015	-0.0017**	-0.0013	0.0032	0.0015	0.0018	-0.0054
	(0.00076)	(0.00078)	(0.00081)	(0.0026)	(0.0048)	(0.0045)	(0.0049)	(0.0072)
Mean of D.V.	0.059	0.035	0.048	0.228	0.744	0.372	0.445	0.572
β/Mean	-0.018	-0.004	-0.034	-0.006	0.004	0.004	0.004	-0.010
SFR Definition	Cumul.	Cumul.	Cumul.	Cumul.	Cumul.	Cumul.	Cumul.	Cumul.
State FE	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y
State Trends	Y	Y	Y	Y	Y	Y	Y	Y
Date Controls	Y	Y	Y	Y	Y	Y	Y	Y
Comparison Group				Profes. Service Industry	Coll. and Uni.	Gov't	State and Local	Non Profit
States	51	51	51	51	51	51	51	51
Respondents	4533802	4533802	4533802	1196289	371470	745927	616937	486381
R^2	0.001	0.001	0.001	0.005	0.012	0.013	0.009	0.080

Data: school finance reforms from 2003 through 2013 (N=29 events, in 19 states). CPS ORG (months 4 and 8) from 1990 through 2015, sample of respondents reporting employment, not enrolled. Date controls: month.

Regression: reform coefficients in Columns 1 through 3 are estimated using equation (4), and Columns 4 through 8 are estimated using equation (5). Comparison groups are other workers in: professional service industries, colleges and universities (a subset of professional service industries), government, state and local government, and non-profits. Standard errors in parentheses, clustered by state.

* $p < .10$, ** $p < .05$, *** $p < .01$

Table 7: SFR and Employment of K-12 Staff: 1990 through 2015, with state trends

	(1) K-12 Ind.	(2) K-12 Ind. Teacher	(3) K-12 Ind. Public	(4) K-12 Ind.	(5) K-12 Ind.	(6) K-12 Ind.	(7) K-12 Ind.	(8) K-12 Ind.
SFR	0.00073	0.00049	0.00024	0.0020*	0.0033**	0.0037**	0.0036*	0.0061
	(0.00044)	(0.00030)	(0.00054)	(0.0011)	(0.0015)	(0.0018)	(0.0019)	(0.0043)
Mean of D.V.	0.059	0.035	0.048	0.228	0.744	0.372	0.445	0.572
β/Mean	0.012	0.014	0.005	0.009	0.004	0.010	0.008	0.011
SFR Definition	Cumul.	Cumul.	Cumul.	Cumul.	Cumul.	Cumul.	Cumul.	Cumul.
State FE	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y
State Trends	Y	Y	Y	Y	Y	Y	Y	Y
Date Controls	Y	Y	Y	Y	Y	Y	Y	Y
Comparison Group				Profes. Service Industry	Coll. and Uni.	Gov't	State and Local	Non Profit
States	51	51	51	51	51	51	51	51
Respondents	4533802	4533802	4533802	1196289	371470	745927	616937	486381
R^2	0.001	0.001	0.001	0.004	0.011	0.012	0.008	0.079

Data: school finance reforms from 1990 through 2013 (N=86 events, in 28 states). CPS ORG (months 4 and 8) from 1990 through 2015, sample of respondents reporting employment, not enrolled. Date controls: month.

Regression: reform coefficients in Columns 1 through 3 are estimated using equation (4), and Columns 4 through 8 are estimated using equation (5). Comparison groups are other workers in: professional service industries, colleges and universities (a subset of professional service industries), government, state and local government, and non-profits. Standard errors in parentheses, clustered by state.

* $p < .10$, ** $p < .05$, *** $p < .01$

Table 8: Effect of the Average School Finance Reform on NAEP Scores, in Student Standard Deviations

	(1)	(2)	(3)	(4)
SFR	0.003	0.000	-0.003	-0.001
	[-0.021,0.027]	[-0.004,0.005]	[-0.006,0.001]	[-0.046,0.045]
Week Days Comp. / 195				0.577
				[0.505,0.649]
Week Days Comp. / 195 x SFR				0.000
				[-0.003,0.004]
SFR Variable	Cumul.	Cumul. Years	Cumul. Min(Years,Grade)	Cumul.
Year FE	Y	Y	Y	Y
School FE	Y	Y	Y	Y
N	2937470	2937470	2937470	2463870
States	51	51	51	51
R^2	0.24	0.24	0.24	0.24

95% confidence intervals in brackets

Table 9: Children: SFR and Diary Hours of Education: robustness to controls and reforms

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
SFR	0.19**	0.20**	0.21**	0.17	0.25***	0.23	0.16	0.51***
	(0.092)	(0.090)	(0.099)	(0.17)	(0.093)	(0.15)	(0.20)	(0.094)
Mean of D.V.	3.369	3.369	3.370	3.369	3.369	3.369	3.369	3.369
β/Mean	0.055	0.059	0.063	0.052	0.076	0.069	0.048	0.151
SFR Definition	Cumul.	Cumul.	Cumul.	Cumul.	Cumul. One-Year Lag	First	Second	Third
State FE	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y
State Trends				Y				
Date Controls	Y	Y	Y	Y	Y	Y	Y	Y
Grade Controls	Y	Y	Y	Y	Y	Y	Y	Y
Employment Controls		Y	Y					
Parent Controls			Y					
States	51	51	51	51	51	51	51	51
Respondents	9150	9150	9142	9150	9150	9150	9150	9150
R^2	0.393	0.402	0.408	0.397	0.393	0.393	0.393	0.393

Data: school finance reforms from 2003 through 2013 (N=29 events, in 19 states). ATUS from 2003 through 2015, sample of respondents eligible for K-12 schooling: aged 15 through 19, and either reporting not completed 12th grade or enrolled in high school. Date controls: month, day, and holiday. Grade controls: education, age. Employment controls: employment status (present or absent) and full or part time status. Parent controls: own parent in household, parents' average years of education.
Regression: reform coefficients are estimated using equation (4). Preferred specification in Column 1. Cumulative SFR are as defined in the specification; lagged SFR may have effects one year earlier than the standard variable; and the first, second, and third SFR are zero if that state had fewer SFR in the period from 2003 through 2013. Standard errors in parentheses, clustered by state.
* $p < .10$, ** $p < .05$, *** $p < .01$

Table 10: Children: SFR and Diary Hours of Education: robustness to samples

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
SFR	0.19**	0.13	0.50***	0.18**	0.19*	0.17	0.25*	0.19**
	(0.092)	(0.093)	(0.070)	(0.085)	(0.10)	(0.12)	(0.15)	(0.089)
Mean of D.V.	3.369	3.340	3.457	3.468	4.365	4.186	4.433	3.380
β/Mean	0.055	0.038	0.145	0.052	0.044	0.042	0.057	0.055
SFR Definition	Cumul.	Cumul.	Cumul.	Cumul.	Cumul.	Cumul.	Cumul.	Cumul.
State FE	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y
Date Controls	Y	Y	Y	Y	Y	Y	Y	Y
Grade Controls	Y	Y	Y	Y	Y	Y	Y	Y
Sample Restriction		Trim Weights p5-p95	Frame Aug 05 Apr 14	Age 18 and Under	Report Enrolled in HS	Drop Jun-Aug	Drop W'end	Drop Low-Q.
States	51	51	51	51	51	51	51	51
Respondents	9150	9150	5926	8821	6790	6854	4440	9001
R^2	0.393	0.403	0.401	0.395	0.430	0.391	0.329	0.394

Data: school finance reforms from 2003 through 2013 (N=29 events, in 19 states). ATUS from 2003 through 2015, sample of respondents eligible for K-12 schooling: aged 15 through 19, and either reporting not completed 12th grade or enrolled in high school. Date controls: month, day, and holiday. Grade controls: education, age. Samples: Column 2 winsorizes the ATUS weights at the 5th and 95th percentiles of their distribution, Column 3 restricts the sample to the longest available CPS frame (PSUs) from August 2005 through April 2014, Column 4 drops respondents aged 19, Column 5 drops respondents who do not report being enrolled in high school either in error or because of holidays, Column 6 drops responses in the summer vacation months, Column 7 drops responses on weekends, and Column 8 drops responses flagged as low-quality or incomplete by ATUS interviewers.
Regression: reform coefficients are estimated using equation (4). Preferred specification in Column 1. Standard errors in parentheses, clustered by state.
* $p < .10$, ** $p < .05$, *** $p < .01$

Table 11: Children: SFR and Diary Hours of Homework: robustness to controls and reforms

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
SFR	0.038*	0.040*	0.049*	0.019	0.044	0.072*	0.058	-0.026
	(0.022)	(0.022)	(0.026)	(0.046)	(0.035)	(0.042)	(0.042)	(0.031)
Mean of D.V.	0.658	0.658	0.658	0.658	0.658	0.658	0.658	0.658
β/Mean	0.057	0.061	0.074	0.029	0.066	0.110	0.088	-0.040
SFR Definition	Cumul.	Cumul.	Cumul.	Cumul.	Cumul. One-Year Lag	First	Second	Third
State FE	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y
State Trends				Y				
Date Controls	Y	Y	Y	Y	Y	Y	Y	Y
Grade Controls	Y	Y	Y	Y	Y	Y	Y	Y
Employment Controls		Y	Y					
Parent Controls			Y					
States	51	51	51	51	51	51	51	51
Respondents	9150	9150	9142	9150	9150	9150	9150	9150
R^2	0.096	0.107	0.125	0.102	0.096	0.097	0.096	0.096

Data: school finance reforms from 2003 through 2013 (N=29 events, in 19 states). ATUS from 2003 through 2015, sample of respondents eligible for K-12 schooling: aged 15 through 19, and either reporting not completed 12th grade or enrolled in high school. Date controls: month, day, and holiday. Grade controls: education, age. Employment controls: employment status (present or absent) and full or part time status. Parent controls: own parent in household, parents' average years of education.
Regression: reform coefficients are estimated using equation (4). Preferred specification in Column 1. Cumulative SFR are as defined in the specification; lagged SFR may have effects one year earlier than the standard variable; and the first, second, and third SFR are zero if that state had fewer SFR in the period from 2003 through 2013. Standard errors in parentheses, clustered by state.
* $p < .10$, ** $p < .05$, *** $p < .01$

Table 12: Children: SFR and Diary Hours of Homework: robustness to sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
SFR	0.038*	0.026	0.0057	0.035	0.046*	0.033	0.071*	0.045*
	(0.022)	(0.022)	(0.041)	(0.023)	(0.026)	(0.028)	(0.038)	(0.023)
Mean of D.V.	0.658	0.662	0.683	0.675	0.811	0.803	0.695	0.660
β/Mean	0.057	0.040	0.008	0.052	0.057	0.041	0.102	0.068
SFR Definition	Cumul.	Cumul.	Cumul.	Cumul.	Cumul.	Cumul.	Cumul.	Cumul.
State FE	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y
Date Controls	Y	Y	Y	Y	Y	Y	Y	Y
Grade Controls	Y	Y	Y	Y	Y	Y	Y	Y
Sample Restriction		Trim Weights p5-p95	Frame Aug 05 Apr 14	Age 18 and Under	Report Enrolled in HS	Drop Jun-Aug	Drop W'end	Drop Low-Q.
States	51	51	51	51	51	51	51	51
Respondents	9150	9150	5926	8821	6790	6854	4440	9001
R^2	0.096	0.097	0.098	0.095	0.085	0.080	0.117	0.098

Data: school finance reforms from 2003 through 2013 (N=29 events, in 19 states). ATUS from 2003 through 2015, sample of respondents eligible for K-12 schooling: aged 15 through 19, and either reporting not completed 12th grade or enrolled in high school. Date controls: month, day, and holiday. Grade controls: education, age. Samples: Column 2 winsorizes the ATUS weights at the 5th and 95th percentiles of their distribution, Column 3 restricts the sample to the longest available CPS frame (PSUs) from August 2005 through April 2014, Column 4 drops respondents aged 19, Column 5 drops respondents who do not report being enrolled in high school either in error or because of holidays, Column 6 drops responses in the summer vacation months, Column 7 drops responses on weekends, and Column 8 drops responses flagged as low-quality or incomplete by ATUS interviewers.
Regression: reform coefficients are estimated using equation (4). Preferred specification in Column 1. Standard errors in parentheses, clustered by state.
* $p < .10$, ** $p < .05$, *** $p < .01$

Table 13: Children: SFR and Diary Hours of Class: robustness to controls and reforms

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
SFR	0.12	0.13	0.13	0.15	0.20**	0.15	0.036	0.49***
	(0.093)	(0.094)	(0.095)	(0.14)	(0.079)	(0.14)	(0.20)	(0.070)
Mean of D.V.	2.618	2.618	2.619	2.618	2.618	2.618	2.618	2.618
β/Mean	0.046	0.050	0.051	0.056	0.075	0.057	0.014	0.188
SFR Definition	Cumul.	Cumul.	Cumul.	Cumul.	Cumul. One-Year Lag	First	Second	Third
State FE	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y
State Trends				Y				
Date Controls	Y	Y	Y	Y	Y	Y	Y	Y
Grade Controls	Y	Y	Y	Y	Y	Y	Y	Y
Employment Controls		Y	Y					
Parent Controls			Y					
States	51	51	51	51	51	51	51	51
Respondents	9150	9150	9142	9150	9150	9150	9150	9150
R^2	0.428	0.432	0.433	0.433	0.428	0.428	0.428	0.428

Data: school finance reforms from 2003 through 2013 (N=29 events, in 19 states). ATUS from 2003 through 2015, sample of respondents eligible for K-12 schooling: aged 15 through 19, and either reporting not completed 12th grade or enrolled in high school. Date controls: month, day, and holiday. Grade controls: education, age. Employment controls: employment status (present or absent) and full or part time status. Parent controls: own parent in household, parents' average years of education.
Regression: reform coefficients are estimated using equation (4). Preferred specification in Column 1. Cumulative SFR are as defined in the specification; lagged SFR may have effects one year earlier than the standard variable; and the first, second, and third SFR are zero if that state had fewer SFR in the period from 2003 through 2013. Standard errors in parentheses, clustered by state.
* $p < .10$, ** $p < .05$, *** $p < .01$

Table 14: Children: SFR and Diary Hours of Class: robustness to samples

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
SFR	0.12	0.082	0.48***	0.12	0.11	0.12	0.15	0.11
	(0.093)	(0.087)	(0.075)	(0.093)	(0.10)	(0.12)	(0.14)	(0.089)
Mean of D.V.	2.618	2.586	2.687	2.696	3.446	3.282	3.634	2.626
β/Mean	0.046	0.032	0.178	0.043	0.032	0.035	0.042	0.043
SFR Definition	Cumul.	Cumul.	Cumul.	Cumul.	Cumul.	Cumul.	Cumul.	Cumul.
State FE	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y
Date Controls	Y	Y	Y	Y	Y	Y	Y	Y
Grade Controls	Y	Y	Y	Y	Y	Y	Y	Y
Sample Restriction		Trim Weights p5-p95	Frame Aug 05 Apr 14	Age 18 and Under	Report Enrolled in HS	Drop Jun-Aug	Drop W'end	Drop Low-Q.
States	51	51	51	51	51	51	51	51
Respondents	9150	9150	5926	8821	6790	6854	4440	9001
R^2	0.428	0.441	0.443	0.435	0.495	0.457	0.336	0.430

Data: school finance reforms from 2003 through 2013 (N=29 events, in 19 states). ATUS from 2003 through 2015, sample of respondents eligible for K-12 schooling: aged 15 through 19, and either reporting not completed 12th grade or enrolled in high school. Date controls: month, day, and holiday. Grade controls: education, age. Samples: Column 2 winsorizes the ATUS weights at the 5th and 95th percentiles of their distribution, Column 3 restricts the sample to the longest available CPS frame (PSUs) from August 2005 through April 2014, Column 4 drops respondents aged 19, Column 5 drops respondents who do not report being enrolled in high school either in error or because of holidays, Column 6 drops responses in the summer vacation months, Column 7 drops responses on weekends, and Column 8 drops responses flagged as low-quality or incomplete by ATUS interviewers.
Regression: reform coefficients are estimated using equation (4). Preferred specification in Column 1. Standard errors in parentheses, clustered by state.
* $p < .10$, ** $p < .05$, *** $p < .01$

Table 15: Children: SFR and Diary Days with Class: robustness to controls and reforms

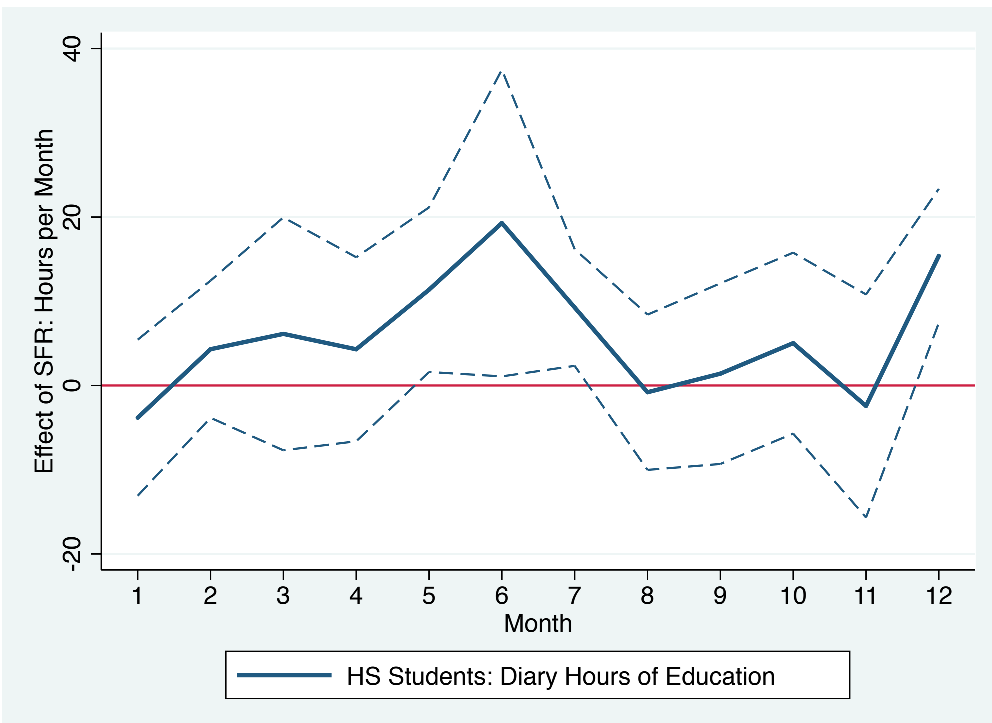
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
SFR	0.017	0.019	0.020	0.025	0.030**	0.019	0.0021	0.082***
	(0.015)	(0.015)	(0.016)	(0.022)	(0.012)	(0.019)	(0.035)	(0.017)
Mean of D.V.	0.425	0.425	0.425	0.425	0.425	0.425	0.425	0.425
β/Mean	0.040	0.045	0.046	0.058	0.069	0.045	0.005	0.194
SFR Definition	Cumul.	Cumul.	Cumul.	Cumul.	Cumul. One-Year Lag	First	Second	Third
State FE	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y
State Trends				Y				
Date Controls	Y	Y	Y	Y	Y	Y	Y	Y
Grade Controls	Y	Y	Y	Y	Y	Y	Y	Y
Employment Controls		Y	Y					
Parent Controls			Y					
States	51	51	51	51	51	51	51	51
Respondents	9150	9150	9142	9150	9150	9150	9150	9150
R^2	0.425	0.430	0.431	0.429	0.425	0.425	0.425	0.425

Data: school finance reforms from 2003 through 2013 (N=29 events, in 19 states). ATUS from 2003 through 2015, sample of respondents eligible for K-12 schooling: aged 15 through 19, and either reporting not completed 12th grade or enrolled in high school. Date controls: month, day, and holiday. Grade controls: education, age. Employment controls: employment status (present or absent) and full or part time status. Parent controls: own parent in household, parents' average years of education.
Regression: reform coefficients are estimated using equation (4). Preferred specification in Column 1. Cumulative SFR are as defined in the specification; lagged SFR may have effects one year earlier than the standard variable; and the first, second, and third SFR are zero if that state had fewer SFR in the period from 2003 through 2013. Standard errors in parentheses, clustered by state.
* $p < .10$, ** $p < .05$, *** $p < .01$

Table 16: Children: SFR and Diary Days of Class: robustness to sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
SFR	0.017	0.010	0.071***	0.015	0.018	0.019	0.020	0.016
	(0.015)	(0.015)	(0.012)	(0.016)	(0.013)	(0.018)	(0.023)	(0.014)
Mean of D.V.	0.425	0.419	0.433	0.436	0.547	0.525	0.584	0.426
β/Mean	0.040	0.025	0.165	0.034	0.033	0.036	0.034	0.038
SFR Definition	Cumul.	Cumul.	Cumul.	Cumul.	Cumul.	Cumul.	Cumul.	Cumul.
State FE	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y
Date Controls	Y	Y	Y	Y	Y	Y	Y	Y
Grade Controls	Y	Y	Y	Y	Y	Y	Y	Y
Sample Restriction		Trim Weights p5-p95	Frame Aug 05 Apr 14	Age 18 and Under	Report Enrolled in HS	Drop Jun-Aug	Drop W'end	Drop Low-Q.
States	51	51	51	51	51	51	51	51
Respondents	9150	9150	5926	8821	6790	6854	4440	9001
R^2	0.425	0.438	0.440	0.434	0.504	0.466	0.321	0.427

Data: school finance reforms from 2003 through 2013 (N=29 events, in 19 states). ATUS from 2003 through 2015, sample of respondents eligible for K-12 schooling: aged 15 through 19, and either reporting not completed 12th grade or enrolled in high school. Date controls: month, day, and holiday. Grade controls: education, age. Samples: Column 2 winsorizes the ATUS weights at the 5th and 95th percentiles of their distribution, Column 3 restricts the sample to the longest available CPS frame (PSUs) from August 2005 through April 2014, Column 4 drops respondents aged 19, Column 5 drops respondents who do not report being enrolled in high school either in error or because of holidays, Column 6 drops responses in the summer vacation months, Column 7 drops responses on weekends, and Column 8 drops responses flagged as low-quality or incomplete by ATUS interviewers.
Regression: reform coefficients are estimated using equation (4). Preferred specification in Column 1. Standard errors in parentheses, clustered by state.
* $p < .10$, ** $p < .05$, *** $p < .01$



Effect of SFR: Hours per Month
40
20
0
-20
1
2
3
4
5
6
7
8
9
10
11
12
Month
HS Students: Diary Hours of Education

Table 17: Attendance Doesn't Explain Learning Time

	(1)	(2)	(3)	(4)
SFR	0.27***	0.20	0.085	0.16
	(0.065)	(0.13)	(0.22)	(0.33)
Mean of D.V.	92.9	92.9	93.2	93.2
SFR	1990-	1990-	2003-	2003-
State FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
State Trends		Y		Y
States	51	51	51	51
Respondents	1224	1224	561	561
R^2	0.065	0.330	0.037	0.432

Data: school finance reforms from 1990 through 2013 (N=86 events, in 28 states). SDFS average daily attendance divided by October enrollment annually from 1990 through 2013, available only in the state-level file.

Regression: reform coefficients are estimated using equation (4).

Standard errors in parentheses, clustered by state

* $p < .10$, ** $p < .05$, *** $p < .01$

Table 18: Retention Doesn't Explain Learning Time

	(1) Diploma Comp. Rate	(2) Diploma Comp. Rate	(3) Annual Dropout G. 9-12	(4) Annual Dropout G. 9-12
SFR	-0.08	-0.26	0.09	-0.30
	(0.85)	(0.94)	(0.13)	(0.19)
Mean of D.V.	82.82	82.82	3.83	3.83
District FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
State Trends		Y		Y
States	51	51	51	51
Districts	12119	12119	12805	12805
Respondents	98530	98530	99148	99148
R^2	0.061	0.112	0.030	0.060

Data: school finance reforms from 1998 through 2010 (N=48 events, in 24 states). SDUS Dropout and Completion data annually from 1998 through 2010: the diploma completion rate is defined as either the average freshman graduation rate or the diploma recipiency rate, depending on the year.

Regression: reform coefficients are estimated using equation (4). Standard errors in parentheses, clustered by state.

* $p < .10$, ** $p < .05$, *** $p < .01$

Table 19: Effect of One Additional Year of Instruction on NAEP Scores, in Student Standard Deviations: Pooled Grades and Subjects

	(1)	(2)	(3)
Subject	Pooled	Pooled	Pooled
Grade	Pooled	Pooled	Pooled
Week Days Comp. / 195	0.50	0.50	0.54
	[0.43,0.57]	[0.43,0.57]	[0.49,0.59]
School FE			Y
State FE	Y		
Year FE	Y		Y
State x Year FE		Y	
N	3364010	3364010	2336500
Schools	80310	80310	33950
R^2	0.04	0.04	0.24

95% confidence intervals in brackets

Table 20: K-12 Staff: SFR and Diary Hours of Work: robustness to controls and reforms

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
SFR	0.32*	0.29*	0.26	0.22	0.34*	0.44*	0.45*	0.36
	(0.17)	(0.17)	(0.22)	(0.21)	(0.19)	(0.26)	(0.24)	(0.45)
Mean of D.V.	4.620	4.620	4.620	4.620	4.620	4.620	4.620	4.620
β/Mean	0.068	0.064	0.057	0.049	0.074	0.096	0.097	0.078
SFR Definition	Cumul.	Cumul.	Cumul.	Cumul.	Cumul. One-Year Lag	First	Second	Third
State FE	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y
State Trends				Y				
Date Controls	Y	Y	Y	Y	Y	Y	Y	Y
Demographic Controls		Y						
Employment Controls	Y	Y			Y	Y	Y	Y
States	51	51	51	51	51	51	51	51
Respondents	6891	6891	6891	6891	6891	6891	6891	6891
R^2	0.500	0.508	0.384	0.397	0.499	0.499	0.499	0.499

Data: school finance reforms from 2003 through 2013 (N=29 events, in 19 states). ATUS from 2003 through 2015, sample of respondents reporting employment in K-12 industry, not enrolled. Date controls: month, day, and holiday. Demographic controls: education, gender, and marital status. Employment controls: employment status (present or absent) and full or part time status.

Regression: reform coefficients are estimated using equation (4). Preferred specification in Column 1. Cumulative SFR are as defined in the specification; lagged SFR may have effects one year earlier than the standard variable; and the first, second, and third SFR are zero if that state had fewer SFR in the period from 2003 through 2013. Standard errors in parentheses, clustered by state.

* $p < .10$, ** $p < .05$, *** $p < .01$

Table 21: K-12 Staff: SFR and Diary Hours of Work: robustness to sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
SFR	0.32*	0.29*	0.19	0.37**	0.23*	0.31*	0.45**	0.32*
	(0.17)	(0.16)	(0.20)	(0.16)	(0.14)	(0.15)	(0.17)	(0.17)
Mean of D.V.	4.620	4.537	4.569	4.688	4.586	4.992	6.031	4.626
β/Mean	0.068	0.064	0.041	0.079	0.051	0.062	0.074	0.069
SFR Definition	Cumul.	Cumul.	Cumul.	Cumul.	Cumul.	Cumul.	Cumul.	Cumul.
State FE	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y
Date Controls	Y	Y	Y	Y	Y	Y	Y	Y
Employment Controls	Y	Y	Y	Y	Y	Y	Y	Y
Sample Restriction		Trim Weights p5-p95	Frame Aug 05 Apr 14	Eligible in CPS ORG	Include Enrolled Resp.	Drop Jun-Aug	Drop W'end	Drop Low-Q.
States	51	51	51	51	51	51	51	51
Respondents	6891	6891	4480	6258	7621	5353	3418	6802
R^2	0.500	0.498	0.508	0.506	0.498	0.527	0.402	0.498

Data: school finance reforms from 2003 through 2013 (N=29 events, in 19 states). ATUS from 2003 through 2015, sample of respondents reporting employment in K-12 industry, not enrolled. Date controls: month, day, and holiday. Employment controls: employment status (present or absent) and full or part time status. Samples: Column 2 winsorizes the ATUS weights at the 5th and 95th percentiles of their distribution, Column 3 restricts the sample to the longest available CPS frame (PSUs) from August 2005 through April 2014, Column 4 drops respondents who would have not been in my ATUS sample based on their responses in their CPS month 8 interview, Column 5 adds K-12 workers who reporting being enrolled in school or university, Column 6 drops responses in the summer vacation months, Column 7 drops responses on weekends, and Column 8 drops responses flagged as low-quality or incomplete by ATUS interviewers.

Regression: reform coefficients are estimated using equation (4). Preferred specification in Column 1. Standard errors in parentheses, clustered by state.

* $p < .10$, ** $p < .05$, *** $p < .01$

Table 22: K-12 Staff: SFR and Diary Hours of Work: robustness to triple differences

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
SFR	0.32*	0.44**	0.52**	0.42**	0.25	0.26	0.17	0.13
	(0.17)	(0.18)	(0.21)	(0.16)	(0.19)	(0.23)	(0.19)	(0.22)
Mean of D.V.	4.620	5.015	4.959	4.877	5.087	4.960	4.847	4.821
β/Mean	0.068	0.088	0.104	0.087	0.048	0.053	0.034	0.027
SFR Definition	Cumul.	Cumul.	Cumul.	Cumul.	Cumul.	Cumul.	Cumul.	Cumul.
State FE	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y
Date Controls	Y	Y	Y	Y	Y	Y	Y	Y
Employment Controls	Y	Y	Y	Y	Y	Y	Y	Y
Comparison Group		Gov't	State and Local	Non Profit	Profes. Service Industry	Educ. / Health Service	PSM Group A	PSM Group B
States	51	51	51	51	51	51	51	51
Respondents	6891	17752	14948	13412	45758	23496	92787	97824
R^2	0.500	0.408	0.415	0.439	0.352	0.389	0.422	0.417

Data: school finance reforms from 2003 through 2013 (N=29 events, in 19 states). ATUS from 2003 through 2015, sample of respondents reporting employment in K-12 industry, not enrolled. Date controls: month, day, and holiday. Employment controls: employment status (present or absent) and full or part time status.

Regression: reform coefficients in Columns 2 through 8 are estimated using equation (5). Comparison groups are other workers in: government, state and local government, non-profits, professional service industries, education and health service industries (a subset of professional service industries), and two groups of propensity-score matched employed workers. Group B is matched on gender, education, sector (class of worker), employment status, and earnings; and group A is additionally matched on decadal age group, full or part time status, usual hours worked, black, and hispanic. Both groups are re-weighted to match the distribution of propensity scores in the sample of K-12 industry workers, and propensity scores are controlled for in bins of 10 percentage points each. Standard errors in parentheses, clustered by state.
* $p < .10$, ** $p < .05$, *** $p < .01$

Table 23: K-12 Staff: SFR and Usual Hours of Work: robustness to controls and reforms

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
SFR	0.10**	0.088*	0.086	0.21*	0.067	-0.0082	0.23***	0.30***
	(0.049)	(0.051)	(0.076)	(0.11)	(0.057)	(0.086)	(0.073)	(0.055)
Mean of D.V.	5.767	5.767	5.767	5.767	5.767	5.767	5.767	5.767
β/Mean	0.017	0.015	0.015	0.037	0.012	-0.001	0.040	0.052
SFR Definition	Cumul.	Cumul.	Cumul.	Cumul.	Cumul. One-Year Lag	First	Second	Third
State FE	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y
State Trends				Y				
Date Controls	Y	Y	Y	Y	Y	Y	Y	Y
Demographic Controls		Y						
Employment Controls	Y	Y			Y	Y	Y	Y
States	51	51	51	51	51	51	51	51
Respondents	6604	6604	6604	6604	6604	6604	6604	6604
R^2	0.536	0.558	0.046	0.061	0.536	0.536	0.537	0.537

Data: school finance reforms from 2003 through 2013 (N=29 events, in 19 states). ATUS from 2003 through 2015, sample of respondents reporting employment in K-12 industry, not enrolled. Date controls: month, day, and holiday. Demographic controls: education, gender, and marital status. Employment controls: employment status (present or absent) and full or part time status.

Regression: reform coefficients are estimated using equation (4). Preferred specification in Column 1. Cumulative SFR are as defined in the specification; lagged SFR may have effects one year earlier than the standard variable; and the first, second, and third SFR are zero if that state had fewer SFR in the period from 2003 through 2013. Standard errors in parentheses, clustered by state.

* $p < .10$, ** $p < .05$, *** $p < .01$

Table 24: K-12 Staff: SFR and Usual Hours of Work: robustness to sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
SFR	0.10**	0.084	0.21***	0.077	0.095**	0.17***	0.090	0.10**
	(0.049)	(0.054)	(0.076)	(0.046)	(0.045)	(0.061)	(0.066)	(0.047)
Mean of D.V.	5.767	5.771	5.771	5.880	5.711	5.746	5.776	5.769
β/Mean	0.017	0.015	0.036	0.013	0.017	0.029	0.016	0.018
SFR Definition	Cumul.	Cumul.	Cumul.	Cumul.	Cumul.	Cumul.	Cumul.	Cumul.
State FE	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y
Date Controls	Y	Y	Y	Y	Y	Y	Y	Y
Employment Controls	Y	Y	Y	Y	Y	Y	Y	Y
Sample Restriction		Trim Weights p5-p95	Frame Aug 05 Apr 14	Eligible in CPS ORG	Include Enrolled Resp.	Drop Jun-Aug	Drop W'end	Drop Low-Q.
States	51	51	51	51	51	51	51	51
Respondents	6604	6604	4277	6046	7303	5126	3275	6521
R^2	0.536	0.536	0.535	0.499	0.559	0.545	0.540	0.538

Data: school finance reforms from 2003 through 2013 (N=29 events, in 19 states). ATUS from 2003 through 2015, sample of respondents reporting employment in K-12 industry, not enrolled. Date controls: month, day, and holiday. Employment controls: employment status (present or absent) and full or part time status. Samples: Column 2 winsorizes the ATUS weights at the 5th and 95th percentiles of their distribution, Column 3 restricts the sample to the longest available CPS frame (PSUs) from August 2005 through April 2014, Column 4 drops respondents who would have not been in my ATUS sample based on their responses in their CPS month 8 interview, Column 5 adds K-12 workers who reporting being enrolled in school or university, Column 6 drops responses in the summer vacation months, Column 7 drops responses on weekends, and Column 8 drops responses flagged as low-quality or incomplete by ATUS interviewers.

Regression: reform coefficients are estimated using equation (4). Preferred specification in Column 1. Standard errors in parentheses, clustered by state.

* $p < .10$, ** $p < .05$, *** $p < .01$

Table 25: K-12 Staff: SFR and Usual Hours of Work: robustness to triple differences

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
SFR	0.10**	0.15*	0.14	0.091	0.073	0.11*	0.12*	0.088
	(0.049)	(0.077)	(0.095)	(0.063)	(0.045)	(0.057)	(0.064)	(0.059)
Mean of D.V.	5.767	5.862	5.839	5.674	5.622	5.642	5.639	5.590
β/Mean	0.017	0.025	0.024	0.016	0.013	0.019	0.022	0.016
SFR Definition	Cumul.	Cumul.	Cumul.	Cumul.	Cumul.	Cumul.	Cumul.	Cumul.
State FE	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y
Date Controls	Y	Y	Y	Y	Y	Y	Y	Y
Employment Controls	Y	Y	Y	Y	Y	Y	Y	Y
Comparison Group		Gov't	State and Local	Non Profit	Profes. Service Industry	Educ. / Health Service	PSM Group A	PSM Group B
States	51	51	51	51	51	51	51	51
Respondents	6604	17126	14403	12845	43331	22501	92787	92787
R^2	0.536	0.472	0.485	0.549	0.537	0.515	0.567	0.585

Data: school finance reforms from 2003 through 2013 (N=29 events, in 19 states). ATUS from 2003 through 2015, sample of respondents reporting employment in K-12 industry, not enrolled. Date controls: month, day, and holiday. Employment controls: employment status (present or absent) and full or part time status.

Regression: reform coefficients in Columns 2 through 8 are estimated using equation (5). Comparison groups are other workers in: government, state and local government, non-profits, professional service industries, education and health service industries (a subset of professional service industries), and two groups of propensity-score matched employed workers. Group B is matched on gender, education, sector (class of worker), employment status, and earnings; and group A is additionally matched on decadal age group, full or part time status, usual hours worked, black, and hispanic. Both groups are re-weighted to match the distribution of propensity scores in the sample of K-12 industry workers, and propensity scores are controlled for in bins of 10 percentage points each. Standard errors in parentheses, clustered by state.
* $p < .10$, ** $p < .05$, *** $p < .01$

Table 26: K-12 Staff: SFR and Diary Days with Work: robustness to controls and reforms

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
SFR	0.0059	0.0029	0.000038	0.013	0.012	0.020	0.011	-0.027
	(0.021)	(0.020)	(0.027)	(0.040)	(0.025)	(0.024)	(0.038)	(0.038)
Mean of D.V.	0.662	0.662	0.662	0.662	0.662	0.662	0.662	0.662
β/Mean	0.009	0.004	0.000	0.019	0.019	0.031	0.017	-0.041
SFR Definition	Cumul.	Cumul.	Cumul.	Cumul.	Cumul. One-Year Lag	First	Second	Third
State FE	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y
State Trends				Y				
Date Controls	Y	Y	Y	Y	Y	Y	Y	Y
Demographic Controls		Y						
Employment Controls	Y	Y			Y	Y	Y	Y
States	51	51	51	51	51	51	51	51
Respondents	6891	6891	6891	6891	6891	6891	6891	6891
R^2	0.397	0.406	0.309	0.318	0.397	0.397	0.397	0.397

Data: school finance reforms from 2003 through 2013 (N=29 events, in 19 states). ATUS from 2003 through 2015, sample of respondents reporting employment in K-12 industry, not enrolled. Date controls: month, day, and holiday. Demographic controls: education, gender, and marital status. Employment controls: employment status (present or absent) and full or part time status.

Regression: reform coefficients are estimated using equation (4). Preferred specification in Column 1. Cumulative SFR are as defined in the specification; lagged SFR may have effects one year earlier than the standard variable; and the first, second, and third SFR are zero if that state had fewer SFR in the period from 2003 through 2013. Standard errors in parentheses, clustered by state.

* $p < .10$, ** $p < .05$, *** $p < .01$

Table 27: K-12 Staff: SFR and Diary Days with Work: robustness to sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
SFR	0.0059	0.0077	-0.0069	0.0077	-0.0059	0.012	0.0085	0.0078
	(0.021)	(0.019)	(0.020)	(0.019)	(0.016)	(0.019)	(0.021)	(0.020)
Mean of D.V.	0.662	0.652	0.655	0.667	0.661	0.696	0.805	0.662
β/Mean	0.009	0.012	-0.010	0.011	-0.009	0.017	0.011	0.012
SFR Definition	Cumul.	Cumul.	Cumul.	Cumul.	Cumul.	Cumul.	Cumul.	Cumul.
State FE	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y
Date Controls	Y	Y	Y	Y	Y	Y	Y	Y
Employment Controls	Y	Y	Y	Y	Y	Y	Y	Y
Sample Restriction		Trim Weights p5-p95	Frame Aug 05 Apr 14	Eligible in CPS ORG	Include Enrolled Resp.	Drop Jun-Aug	Drop W'end	Drop Low-Q.
States	51	51	51	51	51	51	51	51
Respondents	6891	6891	4480	6258	7621	5353	3418	6802
R^2	0.397	0.392	0.400	0.399	0.392	0.412	0.335	0.395

Data: school finance reforms from 2003 through 2013 (N=29 events, in 19 states). ATUS from 2003 through 2015, sample of respondents reporting employment in K-12 industry, not enrolled. Date controls: month, day, and holiday. Employment controls: employment status (present or absent) and full or part time status. Samples: Column 2 winsorizes the ATUS weights at the 5th and 95th percentiles of their distribution, Column 3 restricts the sample to the longest available CPS frame (PSUs) from August 2005 through April 2014, Column 4 drops respondents who would have not been in my ATUS sample based on their responses in their CPS month 8 interview, Column 5 adds K-12 workers who reporting being enrolled in school or university, Column 6 drops responses in the summer vacation months, Column 7 drops responses on weekends, and Column 8 drops responses flagged as low-quality or incomplete by ATUS interviewers.
Regression: reform coefficients are estimated using equation (4). Preferred specification in Column 1. Standard errors in parentheses, clustered by state.
* $p < .10$, ** $p < .05$, *** $p < .01$

Table 28: K-12 Staff: SFR and Diary Days with Work: robustness to triple differences

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
SFR	0.0059	0.016	0.028	-0.0017	-0.0054	-0.0073	-0.0035	-0.011
	(0.021)	(0.022)	(0.023)	(0.024)	(0.023)	(0.024)	(0.024)	(0.027)
Mean of D.V.	0.662	0.672	0.670	0.681	0.688	0.674	0.660	0.658
β/Mean	0.009	0.024	0.042	-0.002	-0.008	-0.011	-0.005	-0.016
SFR Definition	Cumul.	Cumul.	Cumul.	Cumul.	Cumul.	Cumul.	Cumul.	Cumul.
State FE	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y
Date Controls	Y	Y	Y	Y	Y	Y	Y	Y
Employment Controls	Y	Y	Y	Y	Y	Y	Y	Y
Comparison Group		Gov't	State and Local	Non Profit	Profes. Service Industry	Educ. / Health Service	PSM Group A	PSM Group B
States	51	51	51	51	51	51	51	51
Respondents	6891	17752	14948	13412	45758	23496	92787	97824
R^2	0.397	0.355	0.358	0.356	0.284	0.326	0.360	0.355

Data: school finance reforms from 2003 through 2013 (N=29 events, in 19 states). ATUS from 2003 through 2015, sample of respondents reporting employment in K-12 industry, not enrolled. Date controls: month, day, and holiday. Employment controls: employment status (present or absent) and full or part time status.

Regression: reform coefficients in Columns 2 through 8 are estimated using equation (5). Comparison groups are other workers in: government, state and local government, non-profits, professional service industries, education and health service industries (a subset of professional service industries), and two groups of propensity-score matched employed workers. Group B is matched on gender, education, sector (class of worker), employment status, and earnings; and group A is additionally matched on decadal age group, full or part time status, usual hours worked, black, and hispanic. Both groups are re-weighted to match the distribution of propensity scores in the sample of K-12 industry workers, and propensity scores are controlled for in bins of 10 percentage points each. Standard errors in parentheses, clustered by state.
* $p < .10$, ** $p < .05$, *** $p < .01$

.05
0
-.05
Effect of SFR: Probability
1
2
3
4
5
6
7
8
9
10
11
12
Month
Pr(employed, not at work | K-12 staff)

Table 29: Children: SFR Decrease Diary Hours of Household Activities

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
SFR	-49.3***	-49.8***	-50.6***	-40.3	-54.8***	-37.7*	-84.2***	-114.2***
	(15.0)	(15.2)	(15.4)	(28.2)	(15.9)	(22.1)	(31.3)	(35.2)
Mean of D.V.	244	244	244	244	244	244	244	244
β/Mean	-0.202	-0.204	-0.207	-0.165	-0.225	-0.155	-0.346	-0.468
SFR Definition	Cumul.	Cumul.	Cumul.	Cumul.	Cumul. One-Year Lag	First	Second	Third
State FE	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y
State Trends				Y				
Date Controls	Y	Y	Y	Y	Y	Y	Y	Y
Grade Controls	Y	Y	Y	Y	Y	Y	Y	Y
Employment Controls		Y	Y					
Parent Controls			Y					
States	51	51	51	51	51	51	51	51
Respondents	9150	9150	9142	9150	9150	9150	9150	9150
R^2	0.047	0.048	0.051	0.051	0.047	0.046	0.046	0.046

Data: school finance reforms from 2003 through 2013 (N=29 events, in 19 states). ATUS from 2003 through 2015, sample of respondents eligible for K-12 schooling: aged 15 through 19, and either reporting not completed 12th grade or enrolled in high school. Date controls: month, day, and holiday. Grade controls: education, age. Employment controls: employment status (present or absent) and full or part time status. Parent controls: own parent in household, parents' average years of education.

Standard errors in parentheses, clustered by state.

* $p < .10$, ** $p < .05$, *** $p < .01$

Table 30: Children: SFR Decrease Diary Hours of Consumer Purchases

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
SFR	-21.9**	-22.0**	-21.9**	-17.5	-33.7***	-9.96	-46.1**	-51.8***
	(8.37)	(8.30)	(8.21)	(16.1)	(8.35)	(14.4)	(20.9)	(12.3)
Mean of D.V.	101	101	100	101	101	101	101	101
β/Mean	-0.218	-0.219	-0.218	-0.174	-0.335	-0.099	-0.458	-0.515
SFR Definition	Cumul.	Cumul.	Cumul.	Cumul.	Cumul. One-Year Lag	First	Second	Third
State FE	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y
State Trends				Y				
Date Controls	Y	Y	Y	Y	Y	Y	Y	Y
Grade Controls	Y	Y	Y	Y	Y	Y	Y	Y
Employment Controls		Y	Y					
Parent Controls			Y					
States	51	51	51	51	51	51	51	51
Respondents	9150	9150	9142	9150	9150	9150	9150	9150
R^2	0.041	0.042	0.042	0.047	0.041	0.040	0.041	0.041

Data: school finance reforms from 2003 through 2013 (N=29 events, in 19 states). ATUS from 2003 through 2015, sample of respondents eligible for K-12 schooling: aged 15 through 19, and either reporting not completed 12th grade or enrolled in high school. Date controls: month, day, and holiday. Grade controls: education, age. Employment controls: employment status (present or absent) and full or part time status. Parent controls: own parent in household, parents' average years of education.
Standard errors in parentheses, clustered by state.
* $p < .10$, ** $p < .05$, *** $p < .01$

Table 31: Parents: SFR and Diary Hours with Children: robustness to controls and reforms

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
SFR	0.023	0.0086	0.038	0.059	0.028	-0.053	0.071	0.19**
	(0.058)	(0.051)	(0.051)	(0.063)	(0.074)	(0.081)	(0.14)	(0.072)
Mean of D.V.	4.611	4.611	4.611	4.611	4.611	4.611	4.611	4.611
β/Mean	0.005	0.002	0.008	0.013	0.006	-0.011	0.015	0.041
SFR Definition	Cumul.	Cumul.	Cumul.	Cumul.	Cumul. One-Year Lag	First	Second	Third
State FE	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y
State Trends				Y				
Date Controls	Y	Y	Y	Y	Y	Y	Y	Y
Demographic Controls		Y	Y					
Employment Controls			Y					
States	51	51	51	51	51	51	51	51
Respondents	47170	47170	47170	47170	47170	47170	47170	47170
R^2	0.124	0.232	0.273	0.126	0.124	0.124	0.124	0.125

Data: school finance reforms from 2003 through 2013 (N=29 events, in 19 states). ATUS from 2003 through 2015, sample of respondents not working in or enrolled in K-12 schools, with own household children eligible for K-12 schooling: aged 5 through 19, and either reporting not completed 12th grade or enrolled in grade school. Date controls: month, day, and holiday. Demographic controls: gender, 10-year age group, education, number of own household children, average age of own household children among those eligible for K-12 schooling. Employment controls: employment status (present or absent), full or part time status, and enrollment status. Parent controls: own parent in household, parents' average years of education.

Regression: reform coefficients are estimated using equation (4). Preferred specification in Column 1. Cumulative SFR are as defined in the specification; lagged SFR may have effects one year earlier than the standard variable; and the first, second, and third SFR are zero if that state had fewer SFR in the period from 2003 through 2013. Standard errors in parentheses, clustered by state.

* $p < .10$, ** $p < .05$, *** $p < .01$

Table 32: Parents: SFR and Diary Hours with Children: robustness to sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
SFR	0.023	0.023	-0.070	0.028	0.018	-0.026	-0.035	0.024
	(0.058)	(0.055)	(0.12)	(0.065)	(0.055)	(0.044)	(0.082)	(0.054)
Mean of D.V.	4.611	4.646	4.644	4.610	4.611	4.490	3.814	4.614
β/Mean	0.005	0.005	-0.015	0.006	0.004	-0.006	-0.009	0.005
SFR Definition	Cumul.	Cumul.	Cumul.	Cumul.	Cumul.	Cumul.	Cumul.	Cumul.
State FE	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y
Date Controls	Y	Y	Y	Y	Y	Y	Y	Y
Grade Controls	Y	Y	Y	Y	Y	Y	Y	Y
Sample Restriction		Trim Weights p5-p95	Frame Aug 05 Apr 14	Drop Enrolled in Coll.	Drop Educ. Industry	Drop Jun-Aug	Drop W'end	Drop Low-Q.
States	51	51	51	51	51	51	51	51
Respondents	47170	47170	30477	44801	46025	35414	23224	46477
R^2	0.124	0.124	0.127	0.124	0.124	0.139	0.040	0.125

Data: school finance reforms from 2003 through 2013 (N=29 events, in 19 states). ATUS from 2003 through 2015, sample of respondents not working in or enrolled in K-12 schools, with own household children eligible for K-12 schooling: aged 5 through 19, and either reporting not completed 12th grade or enrolled in grade school. Date controls: month, day, and holiday. Samples: Column 2 winsorizes the ATUS weights at the 5th and 95th percentiles of their distribution, Column 3 restricts the sample to the longest available CPS frame (PSUs) from August 2005 through April 2014, Column 4 drops respondents enrolled in college, Column 5 drops respondents who work in the education industry, Column 6 drops responses in the summer vacation months, Column 7 drops responses on weekends, and Column 8 drops responses flagged as low-quality or incomplete by ATUS interviewers.
Regression: reform coefficients are estimated using equation (4). Preferred specification in Column 1. Standard errors in parentheses, clustered by state.
* $p < .10$, ** $p < .05$, *** $p < .01$

Table 33: Parents: SFR and Diary Hours of Childcare: robustness to controls and reforms

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
SFR	0.016	0.0090	0.016	0.013	0.041**	-0.022	0.059	0.069
	(0.024)	(0.025)	(0.023)	(0.037)	(0.018)	(0.031)	(0.048)	(0.061)
Mean of D.V.	0.874	0.874	0.874	0.874	0.874	0.874	0.874	0.874
β/Mean	0.018	0.010	0.018	0.015	0.047	-0.025	0.067	0.078
SFR Definition	Cumul.	Cumul.	Cumul.	Cumul.	Cumul. One-Year Lag	First	Second	Third
State FE	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y
State Trends				Y				
Date Controls	Y	Y	Y	Y	Y	Y	Y	Y
Demographic Controls		Y	Y					
Employment Controls			Y					
States	51	51	51	51	51	51	51	51
Respondents	47170	47170	47170	47170	47170	47170	47170	47170
R^2	0.010	0.181	0.214	0.012	0.010	0.010	0.010	0.010

Data: school finance reforms from 2003 through 2013 (N=29 events, in 19 states). ATUS from 2003 through 2015, sample of respondents not working in or enrolled in K-12 schools, with own household children eligible for K-12 schooling: aged 5 through 19, and either reporting not completed 12th grade or enrolled in grade school. Date controls: month, day, and holiday. Demographic controls: gender, 10-year age group, education, number of own household children, average age of own household children among those eligible for K-12 schooling. Employment controls: employment status (present or absent), full or part time status, and enrollment status. Parent controls: own parent in household, parents' average years of education.

Regression: reform coefficients are estimated using equation (4). Preferred specification in Column 1. Cumulative SFR are as defined in the specification; lagged SFR may have effects one year earlier than the standard variable; and the first, second, and third SFR are zero if that state had fewer SFR in the period from 2003 through 2013. Standard errors in parentheses, clustered by state.

* $p < .10$, ** $p < .05$, *** $p < .01$

Table 34: Parents: SFR and Diary Hours of Childcare: robustness to sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
SFR	0.016	0.018	0.013	0.028	0.018	0.011	0.0077	0.021
	(0.024)	(0.022)	(0.043)	(0.065)	(0.055)	(0.032)	(0.028)	(0.025)
Mean of D.V.	0.874	0.881	0.878	4.610	4.611	0.919	0.903	0.876
β/Mean	0.018	0.021	0.015	0.006	0.004	0.011	0.009	0.024
SFR Definition	Cumul.	Cumul.	Cumul.	Cumul.	Cumul.	Cumul.	Cumul.	Cumul.
State FE	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y
Date Controls	Y	Y	Y	Y	Y	Y	Y	Y
Grade Controls	Y	Y	Y	Y	Y	Y	Y	Y
Sample Restriction		Trim Weights p5-p95	Frame Aug 05 Apr 14	Drop Enrolled in Coll.	Drop Educ. Industry	Drop Jun-Aug	Drop W'end	Drop Low-Q.
States	51	51	51	51	51	51	51	51
Respondents	47170	47170	30477	44801	46025	35414	23224	46477
R^2	0.010	0.010	0.010	0.124	0.124	0.010	0.013	0.011

Data: school finance reforms from 2003 through 2013 (N=29 events, in 19 states). ATUS from 2003 through 2015, sample of respondents not working in or enrolled in K-12 schools, with own household children eligible for K-12 schooling: aged 5 through 19, and either reporting not completed 12th grade or enrolled in grade school. Date controls: month, day, and holiday. Samples: Column 2 winsorizes the ATUS weights at the 5th and 95th percentiles of their distribution, Column 3 restricts the sample to the longest available CPS frame (PSUs) from August 2005 through April 2014, Column 4 drops respondents enrolled in college, Column 5 drops respondents who work in the education industry, Column 6 drops responses in the summer vacation months, Column 7 drops responses on weekends, and Column 8 drops responses flagged as low-quality or incomplete by ATUS interviewers.
Regression: reform coefficients are estimated using equation (4). Preferred specification in Column 1. Standard errors in parentheses, clustered by state.
* $p < .10$, ** $p < .05$, *** $p < .01$

Table 35: Parents: SFR and Diary Hours of Education Childcare: robustness to controls and reforms

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
SFR	-0.011	-0.012	-0.010	-0.012	-0.0096	-0.016	-0.022	-0.0018
	(0.0080)	(0.0081)	(0.0077)	(0.0087)	(0.0092)	(0.013)	(0.016)	(0.025)
Mean of D.V.	0.136	0.136	0.136	0.136	0.136	0.136	0.136	0.136
β/Mean	-0.084	-0.087	-0.077	-0.087	-0.070	-0.114	-0.162	-0.013
SFR Definition	Cumul.	Cumul.	Cumul.	Cumul.	Cumul. One-Year Lag	First	Second	Third
State FE	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y
State Trends				Y				
Date Controls	Y	Y	Y	Y	Y	Y	Y	Y
Demographic Controls		Y	Y					
Employment Controls			Y					
States	51	51	51	51	51	51	51	51
Respondents	47170	47170	47170	47170	47170	47170	47170	47170
R^2	0.041	0.066	0.078	0.043	0.041	0.041	0.041	0.041

Data: school finance reforms from 2003 through 2013 (N=29 events, in 19 states). ATUS from 2003 through 2015, sample of respondents not working in or enrolled in K-12 schools, with own household children eligible for K-12 schooling: aged 5 through 19, and either reporting not completed 12th grade or enrolled in grade school. Date controls: month, day, and holiday. Demographic controls: gender, 10-year age group, education, number of own household children, average age of own household children among those eligible for K-12 schooling. Employment controls: employment status (present or absent), full or part time status, and enrollment status. Parent controls: own parent in household, parents' average years of education.

Regression: reform coefficients are estimated using equation (4). Preferred specification in Column 1. Cumulative SFR are as defined in the specification; lagged SFR may have effects one year earlier than the standard variable; and the first, second, and third SFR are zero if that state had fewer SFR in the period from 2003 through 2013. Standard errors in parentheses, clustered by state.

* $p < .10$, ** $p < .05$, *** $p < .01$

Table 36: Parents: SFR and Diary Hours of Education Childcare: robustness to sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
SFR	-0.011	-0.011	-0.0065	0.028	0.018	-0.013	-0.015	-0.0091
	(0.0080)	(0.0078)	(0.013)	(0.065)	(0.055)	(0.010)	(0.0095)	(0.0084)
Mean of D.V.	0.136	0.137	0.138	4.610	4.611	0.168	0.171	0.136
β/Mean	-0.084	-0.080	-0.047	0.006	0.004	-0.078	-0.091	-0.067
SFR Definition	Cumul.	Cumul.	Cumul.	Cumul.	Cumul.	Cumul.	Cumul.	Cumul.
State FE	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y
Date Controls	Y	Y	Y	Y	Y	Y	Y	Y
Grade Controls	Y	Y	Y	Y	Y	Y	Y	Y
Sample Restriction		Trim Weights p5-p95	Frame Aug 05 Apr 14	Drop Enrolled in Coll.	Drop Educ. Industry	Drop Jun-Aug	Drop W'end	Drop Low-Q.
States	51	51	51	51	51	51	51	51
Respondents	47170	47170	30477	44801	46025	35414	23224	46477
R^2	0.041	0.042	0.043	0.124	0.124	0.035	0.036	0.042

Data: school finance reforms from 2003 through 2013 (N=29 events, in 19 states). ATUS from 2003 through 2015, sample of respondents not working in or enrolled in K-12 schools, with own household children eligible for K-12 schooling: aged 5 through 19, and either reporting not completed 12th grade or enrolled in grade school. Date controls: month, day, and holiday. Samples: Column 2 winsorizes the ATUS weights at the 5th and 95th percentiles of their distribution, Column 3 restricts the sample to the longest available CPS frame (PSUs) from August 2005 through April 2014, Column 4 drops respondents enrolled in college, Column 5 drops respondents who work in the education industry, Column 6 drops responses in the summer vacation months, Column 7 drops responses on weekends, and Column 8 drops responses flagged as low-quality or incomplete by ATUS interviewers.
Regression: reform coefficients are estimated using equation (4). Preferred specification in Column 1. Standard errors in parentheses, clustered by state.
* $p < .10$, ** $p < .05$, *** $p < .01$