DO FEDERAL GRANTS BOOST SCHOOL SPENDING? EVIDENCE FROM TITLE I

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Abstract

Title I, which allocates money for compensatory education to school districts based on their child poverty, is recognized as the single most important federal education program. This is largely because of its size: it cost $9.6 billion in 2001 and represents 35 percent of the Department of Education's elementary and secondary spending. Whether Title I is actually important is controversial, however, because it is not clear that it raises the spending of schools that serve poor children. Title I money must make its way through as many as three other levels of government (states, local parent governments such as counties or municipalities, and school districts), each of which can offset changes to Title I so that spending on poor students changes less than the federal government intends. I overcome the simultaneity problems inherent in estimating the effect of Title I by using sharp changes in per-pupil grant amounts resulting from the release of decennial census data to identify how state and local education revenues and school district spending react to changes in Title I. I find that state education revenue and school districts' own revenue efforts are initially unaffected by Title I changes so that Title I raises instructional spending dollar for dollar. Three years later, however, local governments have offset changes in Title I, so that the federal spending has only small and statistically insignificant net spending effects on schools.

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I. INTRODUCTION

Title I is widely recognized as the federal government's single most important education program. Title I attempts to increase the resources of school districts that serve economically disadvantaged children, and cost $11.3 billion in FY 2002. It thus represents 34 percent of the U.S. Department of Education's elementary and secondary budget. The program makes non-matching grants to school districts based on their number of poor children, and specifies that the grants be used so that educationally disadvantaged children receive compensatory education, such as small group instruction outside the classroom. The defining characteristic of Title I traditionally has been that it is the main way the federal government directly aids poor local schools: among the 10 percent of school districts that rely most heavily on the program, Title I accounts for between 5 and 10 percent of total spending. Under the No Child Left Behind Act of 2001, the Title I program has taken on a new accountability role as well: the federal government may penalize schools designated as failing by allowing their students to take their Title I funds elsewhere for compensatory education activities.

There is controversy, however, about whether Title I is *actually* important, or only appears to be important because it is a large item on revenue accounts. School districts' budgets are determined by as many as three levels of government, in addition to the federal government: states, local parent governments such as counties and municipalities, and school districts. Any of these other levels of government could potentially offset Title I revenue, making the program have less than its intended effect

on the schooling experienced by poor children.[1] If this is the case, federal dollars subsidize other levels of government rather than supplement instructional resources for poor children. In this paper, I estimate the effect of Title I on school spending, and examine in detail how both local and state governments respond to changes in the federal program.

One of this paper's benefits is that it will begin to untangle some of the controversy about the effects of Title I on *achievement*. Ultimately, Title I aims not merely to provide supplemental educational services to poor children, but to improve educational outcomes for these disadvantaged children. Yet, without knowing how much Title I actually increases spending, it is impossible to know whether services like those funded by Title I have an effect on achievement. As a rule, the Title I evaluation literature looks for achievement to change as a direct result of Title I revenue, ignoring the possibility that some or all of the services it funds might have been provided in its absence (Borman and D'Agostino, 1996; Mast, 2001; Puma et al., 1993). To the extent that state or local governments offset Title I by lowering their own spending on services to poor students, Title I will have diminished impact on students' educational experiences, and a finding of an insignificant treatment effect (as in the congressionally-mandated *Prospects* study, Puma et al., 1993) should be no surprise.[2] Indeed, the common finding that Title I students exhibit no relative improvement could be entirely due to their having experienced few additional resources. The impact of a classroom aide, for example, should be the same regardless of whether her salary comes from Title I

[1] The maintenance of effort, supplement not supplant, and comparability clauses of the Title I legislation aim to prevent this substitution. Much of the substitution would be difficult to detect, however, even in a perfect enforcement regime.

revenue or more local revenue. Given legislatures' current push for accountability in schools, it is important to understand whether the services funded by Title I are ineffective because they are poorly designed or because they do not represent net service increases.

Assessing the impact of Title I has been a challenge for previous empirical studies. This is because a district's poverty determines its Title I allocation, but poverty also affects a district through other channels. In particular, poverty affects a district's ability to raise revenue from its own residents, simply because their ability to pay is a continuous function of their incomes. State aid to school districts is also a function of local poverty, although states generally use measures of poverty based on a district's property wealth per pupil. It may seem impossible, therefore, to separate the effects of Title I on state and local revenue from the effects of poverty on all three revenue streams (Title I, state, and local). In this paper, I use an innovative identification strategy that exploits a key difference between Title I and state and local funds. State and local revenue both depend on a district's *current* ability to pay and change continuously, as ability to pay changes continuously. In contrast, Title I depends on child poverty counts from the decennial Censuses of Population, and these counts are updated only at 10-year intervals. Thus, Title I allocations jump discretely every 10 years while poverty (and the state and local revenues that depend on poverty) changes continuously. Moreover, decennial census counts are first used in Title I allocations approximately three years *after* the information is gathered, so the census-based changes in poverty do not even include current changes in poverty (and it is *current* changes in poverty that affect state

[2] The *Prospects* study used a propensity score methodology to compare achievement changes of Title I participants to demographically matched non-participants and found no treatment effect.

and local revenue). Because actual poverty is likely to change only slightly between adjacent years but the census-based child poverty count may change substantially, my identification strategy is essentially a regression discontinuity one.

Understanding the effects of Title I is not only important because the policy is important; it is also a rich problem in fiscal federalism that can reveal a great deal about how different levels of government interact. Title I is particularly well-suited for studying fiscal federalism for three reasons. First, because so many levels of government are involved in the determination of school spending, the problem is rich in potential interactions among governments. Second, because the data are detailed, I can show not just the immediate effects of Title I, but also district- and state-level reactions over several years, as they have time to respond. Third, the evaluation of many fiscal federalist policies is plagued by identification problems like the one that plagues Title I: because districts with more Title I funds are necessarily poorer than other districts, it is unlikely that they would have similar spending behavior, even in the absence of the program. That the Title I funding formula creates large, discrete changes in Title I funding when new decennial census data appear allows me to credibly identify the effects of Title I and overcome empirical problems that have plagued previous studies.

In short, I investigate the impact of Title I funding on schools' revenues and spending, distinguishing the effect of Title I from the effect of poverty by exploiting sharp census-based changes in per-pupil grants between the 1992 and 1993 school years (I refer to school years by the calendar year of the fall throughout).[3] I find that school

[3] Ideally one could identify changes in spending on disadvantaged students due to changes in Title I revenue: because budgetary data are reported for aggregate categories at the district level, such as total spending, instructional salaries, and instructional equipment, in this analysis I am limited to analyzing the

revenues and spending initially experience dollar-for-dollar increases with Title I, but that—over time—school districts' revenues respond, significantly offsetting the impact of the Title I revenue. Three years after receiving increases in Title I, poor school districts have little to no increases in school spending over what would have been the case without the Title I increase. States respond heterogeneously to Title I changes, but generally appear to respond only to the general pattern of Title I among poor districts in their state, not to individual districts' receipts of Title I funds.

The remainder of this paper is structured as follows. In section two, I present background information on the Title I program and review the literature on Title I. In section three, I review the theory and empirical literature on the intergovernmental grants. In section four, I discuss the methodology, in section five the data, and in section six the results. Section seven concludes.

II. BACKGROUND ON TITLE I

Title I, the largest federal education program, was passed into law in the 1965 Elementary and Secondary Education Act as part of the Johnson administration's War on Poverty.[4] The guidance on how school districts are to use Title I funds is broad: they should be used to improve academic performance of children at risk of school failure, either targeting only the educationally neediest students in the school or, in some circumstances, using a schoolwide approach.

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effects of Title I revenue on spending overall rather than spending on the most disadvantaged students in a district.

[4] This paper considers only Part A of Title I, which gives grants to school districts based primarily on their child poverty counts. Other parts of Title I include provisions for migrant education, homeless children, and the Even Start early childhood program. For simplicity, I will refer to Title I, rather than Title I, Part A, throughout. The set of programs now known as Title I since 1994 were called Title I originally, then Chapter 1; I will refer to them as Title I throughout this paper for consistency.

Table 1 shows the distribution of Title I funds per low-income pupil, per pupil, and as a percentage of all spending for all school districts in 1992, the base year for my analysis. The median participating district received about $800 per low-income pupil and about $100 per pupil from Title I, with just over 10 percent of districts receiving more than $1000 per low-income pupil and more than $250 per pupil.[5]

Federal efforts to prevent offsetting state and local responses

In the early years of Title I in the late 1960s and early 1970s, several clear cases of school districts using Title I funds to replace other types of revenue emerged and were the subject of federal audits. A complaint brought by the Harvard Center for Law and Education on behalf of the children of the Bernalillo school district in Sandoval, New Mexico in 1970 outlines non-compliance problems in the district which typify general complaints of the era:

> "Librarians, teachers, nurses, and counselors are paid from Title I funds even though they provide services to students who are not eligible for Title I assistance… Some of the programs financed by Title I are unrelated to the needs of poor Indian children, and consistently have been opposed by the Pueblo communities. For example, arts and crafts is paid for out of Title I funds on the theory that it will increase 'small muscle' coordination… *In general, Title I… funds are treated as non-categorical aid which the board may spend as it deems appropriate* (emphasis added)." (Harvard Center for Law and Education 1972, pp. 156-57)

Complaints such as this one led to the inclusion of several enforcement mechanisms in the legislation. The "maintenance of effort" requirement attempts to ensure that Title I "sticks" to school spending. It mandates that either state and local revenue per pupil or aggregate state and local revenue cannot fall below 90 percent of

[5] The Title I funding formula, which I discuss in detail later, introduces variation in grant amount per poor pupil along dimensions of state education spending, concentration of poverty, and previous level of Title I funding.

their levels in the preceding fiscal year without penalty.[6] In 1992, Title I provided about 2 percent of total spending for the average district. For the 1 percent of districts relying most heavily on Title I, their Title I revenue approached 10 percent of total spending, but their new Title I funds in any given year are only a fraction of that. Thus, even if a state or district wanted to completely substitute new Title I revenue for old state or local revenue, it would be able to do so by cutting combined state and local revenue by less than 10 percent, and the maintenance of effort requirement would not bind. In short, *the maintenance of effort clause is irrelevant for even the poorest districts* (and thus for this empirical investigation), except perhaps as "moral suasion."

Literature on the impact of Title I of school district budgets

To my knowledge, Feldstein (1978) is the only empirical analysis that examines the effect of Title I on state and local revenue while explicitly considering poverty's simultaneous influence on Title I, state, and local revenue. At the time of this study, Title I funds were distributed to school districts based in part on the rank of their poverty rate within their county, not just on the number of poor children living in the district (this is no longer the case). Feldstein exploited the cross-sectional variation in Title I funding per pupil resulting from the fact that rankings were not fully collinear with absolute poverty. He compared revenues and expenditures of school districts with similar poverty rates and other characteristics, but different Title I revenues due to their differing poverty rankings in their counties. Feldstein found that for every additional dollar of Title I revenue, total spending was about 80 cents higher.

[6] School districts can choose whichever measure is beneficial to them. If a school district failed to maintain effort, the state education agency was required to reduce the school district's Title I allocation in proportion

III. Theory and Literature on Intergovernmental Grants

My investigation is related to a substantial literature on an empirical puzzle dubbed "the flypaper effect." The puzzle is the following. Economic theory predicts that a jurisdiction receiving an intergovernmental lump-sum grant will view the grant as income and will spend it just as it would spend other income, with a fraction (equal to the jurisdiction's marginal propensity to spend on the targeted service, and possibly a small share) going to the that area, and the remainder going to other projects or to tax reduction. Many empirical studies, however, have observed that the marginal propensity to spend an intergovernmental grant on the targeted government service is higher than the marginal propensity to spend other income on that service. Arthur Okun called this empirical regularity the flypaper effect because money "sticks where it hits" unduly.[7] Depending on whether the flypaper effect is strong or weak for Title I, the program is very important or much less important than the accounting data suggest.

The flypaper effect and the level of local public goods: theory

The typical school district today receives approximately the same amount from the state as it raises at the local level. It is thus important to consider the effects that federal grants may have on *state* revenue to local school districts in addition to effects on revenue raised locally. A state may respond to its poor districts' receipt of large Title I grants by redirecting money away from education aid to poor districts and towards other areas (e.g., tax reduction, health care, criminal justice), such that the total revenues received by the school district increase by some amount less than the federal grant. Some school districts have a parent government that aids them—for instance, a county that aids

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to the reduction of state and local effort in the school district.

[7] Hines and Thaler (1995) provide an excellent review of the flypaper literature.

its county school district or a municipality that aids the district that is geographically aligned with it. (It is generally but not always true that a parent government covers the same geographic area as its school district. Suppose it does, to keep the logic reasonably simple.)

The three-panel diagram labeled Figure 1 shows how school spending would change with Title I if grants were passed through each layer of government, from higher to lower, and each layer reacted strictly as theory predicts. The first panel shows the state's spending decision; the second shows the parent government's (a municipality's, say) spending decision; and the last panel shows the school district's spending decision. In the first panel of figure 1, the state divides its budget between other goods and aid to the poor municipality. When the state's budget constraint shifts rightward by the amount of the Title I grant, the state spends only a fraction on aid to the poor municipality. In the second panel of the figure, the poor municipality divides its budget between other goods and aid to its dependent school district. When its budget constraint shifts rightwards by the fraction of the Title I grant that was passed on to it, the municipality spends only part on aid to its school district. School districts divide their spending only between schools and tax reduction, so when the fraction of the grant passed on to the district shifts its budget constraint, it raises spending on education (e) somewhat and reduces taxes somewhat.

The same result would obtain regardless of the order in which the jurisdictions receive the grant *if* the jurisdictions respond as theory predicts and are sufficiently knowledgeable and flexible about one another's response actions. Suppose that the school district receives the entire Title I grant and divides it between tax reduction and

education spending, as predicted. The municipality's indifference curves are defined over a space that has tax reduction, education spending, and other public goods as its dimensions. Thus, the municipality should reduce its school district aid and change its tax reduction by just enough so that the ultimate changes in education spending, tax reduction, and other public goods match its marginal propensity to spend in these three areas. The dimensions over which a state's indifference curves are defined are: tax reduction for the residents of the poor municipality, education spending for the residents of the poor municipality, other public goods for the residents of the poor municipality, and all other goods for all the other residents of the state. The state should change its aid to the municipality, its aid to the school district, and its taxes on the municipality's (equivalently, district's) residents just enough so that the ultimate changes match its marginal propensity to spend in each area.

Needless to say, this equivalence not only can break down at many points, but is likely to break down. For example, a state may be constrained to tax residents of all districts very similarly because it has only a few tax instruments and is not permitted to charge different rates in different areas. Or, a state might be unable to process sufficiently detailed information about what happens in each district and municipality to respond optimally to each of them. Adding realistic politics would only reduce the probability of equivalence further. Bureaucrats may place relative values on government spending and tax reduction that do not correspond to those of voters. Bureaucrats likely know more about grants than voters, just as local voters may know more about local spending than the state does. In short, Title I grants have potentially rich effects on

multiple layers of government, and empirical evidence is likely to elucidate jurisdictions' fiscal interactions.

The flypaper effect: evidence and explanations

There is a large literature focused on estimating the effect of various intergovernmental grants to state and local governments. Researchers typically find that an additional dollar of intergovernmental grant increases expenditures on the targeted program by much more than the receiving government's propensity to spend on that program out of regular income.8 Estimates range from $0.25 for every $1.00 of grant received to $1.00 for every $1.00 of grant received, with most estimates clustered at the top end of this range. Knight's (2001) recent addition to this literature, however, indicates that controlling for endogeneity of grant amounts reveals significant crowd-out, suggesting that some observed flypaper effects may be statistical artifacts.

Two popular explanations of why the flypaper effect occurs are well-suited to the particular case of Title I. The fiscal illusion hypothesis posits that voters in the receiving jurisdiction are unaware of the new funding, and that bureaucrats in the receiving jurisdiction expand their budgets without voters fully realizing what has happened. In the case of Title I, school district administrators may increase school spending and state and parent governments may increase spending on education and other unrelated government programs. Hines and Thaler (1995) suggest that voters and/or bureaucrats act irrationally, failing to note that grants are fungible and may keep them in separate

[8] Government spending is estimated to rise by about 5 to 10 percent of the additional potential revenue when state tax bases increase (Hines and Thaler, 1995). Legislators almost certainly would be disappointed if total education expenditures rose by only 5 to 10 percent of the increase in the Title I grant amount, but the maintenance of effort clause would not be violated in most cases.

"mental accounts" from which they spend for separate programs. In the case of Title I, such mental accounting would direct all Title I funds to instructional spending.[9]

IV. METHODOLOGY

A typical test of the flypaper effect exploits longitudinal changes in intergovernmental grant amounts to estimate the effect of a change in the grant amount on the change in targeted expenditures at the state or local level. In the most basic ordinary least squares (OLS) specification, equation (1) would be used:

$$\Delta \textit{ INSTRUCTIONAL SPENDING}_d = \beta_0 + \beta_1 \Delta \textit{ TITLE I GRANT}_d + \varepsilon_d \qquad (1)$$

where *d* indexes the school district, and the change is taken over any period in which Title I grants change.

I alter this basic approach to better suit the particular problems posed by Title I. In this section, I first explain how Title I grants are allocated. I then discuss how not all longitudinal variation in Title I grants is exogenous to longitudinal change in state and local spending because poverty counts influence both changes in Title I and changes in spending. Next, I discuss how decennial updating of the poverty data used in the allocation formula yields immediate changes in Title I revenue, even if actual poverty levels change slowly. I outline my specification and present first-stage results.

The structure of Title I grants and the grant allocation process

My identification strategy relies on the formula used to allocate Title I funds. In this section, I describe the formula used for the school years I analyze, 1991 through

[9] A narrower interpretation (consistent with the intent of the program) would have the funds restricted to instructional spending for the most educationally needy students. These two cases are indistinguishable in the school district budgetary data I use here, and would require a close examination of resources provided at the school level rather than at the district level.

1995.[10] Although it is clear that I need to use every part of the formula to predict a district's grant before and after the census updating, it is not essential that readers know the formula equally well. What I hope is that a reader will derive two facts from the following description of the Title I formula: (1) that the grants were mainly determined by decennial census child poverty data, and (2) that the relationship between a district's grant and its child poverty was highly non-linear. The non-linearity of the grants means that there are actually *three* reasons why the census updates are a good source of identification: (1) the updates jumped discretely whereas state and local revenue changes more continuously with continuous changes in poverty; (2) the updates were not a function of current changes in poverty (which might have affected outcomes) but changes in poverty that were already out of date; and (3) the updates were a highly non-linear, even "jumpy" function of changes in child poverty whereas state and local revenue is likely to be a more linear function of poverty.

The federal Department of Education distributed two types of grants to the states, with allocations specified at the county level. States then distributed grants to school districts within the counties. Counties with at least ten poor children ages 5 to 17 were eligible for "basic grants." Basic grants accounted for about 90 percent of the total Title I budget in the early 1990s. Counties with either 6,500 or more poor children or 15 percent or more children in poverty were eligible for "concentration grants." The Title I formula used data from the 1980 census through 1992, and then switched to the 1990 data beginning with 1993. Title I allocations also reflected current mean per-pupil spending at

[10] The funding process was supposed to change considerably beginning in 1997-98, with the federal government directly allocating funds to school districts without the intermediate level of allocations to counties, and more frequent updating of poverty data from other sources. I present the allocation process that was both mandated and used in the mid-1990s.

the state level, used as an education cost index. I address the endogeneity of state spending levels in determining Title I through a simulated instrumental variables approach.

In a typical year, Congress appropriated enough money to Title I to fund about one-third of what the formula required. In order to make the actual grants add up to the actual appropriation, each grant was proportionately reduced. (Thus, if Congress appropriated an amount equal to 35 percent of the formula, each grant was set equal to 35 percent of its formula amount.) A "hold-harmless clause" applied at both the county and school district levels for basic grants only. The hold-harmless clause stated that, as long as a county or school district remained eligible, it could not receive less than 85 percent of the basic grant it had received in the previous year.[11] Two other rules also generated non-linearities in the relationship between grants and poverty: the "small state minimum" and the adjustment for a state's mean spending per pupil. These two rules are described in Appendix A.

Once a state had the Title I grant for each of its counties, it redistributed the grants to eligible school districts within each county based on poverty. However, states were allowed to choose poverty indicators, so that while within-county distribution relied mainly on census child poverty counts, in some cases, Food Stamps, AFDC, and free lunch data were also used. Eligibility of school districts was determined using the same rules that governed the eligibility of counties, for both basic and concentration grants. Within a district, funds were distributed to each school based on how many of its pupils

[11] The hold-harmless clause meant that two districts with the same child poverty count in 1990 might get different Title I grants. For instance, when the 1990 data were used to update Title I, the hold-harmless clause generated smaller grants per poor child in the West than in the East because Eastern states had had relatively high child poverty in the 1980 data compared to Western states.

were eligible for free or reduced lunch. Within each school, the resources purchased with Title I funds were supposed to be targeted at the most educationally disadvantaged children. *Educational* disadvantage was usually based on achievement test scores, so the most economically disadvantaged children within a school were not necessarily targeted. In short, Title I dollars follow poor students from the federal government to their county, to their school district, and to their school; but then, within a school, the dollars are targeted to low achievers.

Simultaneous determination of Title I grants and other sources of revenue

The OLS approach in equation (1) in which the change in Title I is regressed on the change in instructional expenditures is problematic because both the Title I grant and other components of instructional spending are determined by the number of poor children residing in the school district. I address this problem by analyzing changes in spending and revenue surrounding the release of 1990 census data. Most non-Title I revenue sources and district spending do not experience discontinuous changes with the release of census data; they are correlated with *actual* poverty, which changes continuously, while Title I revenue is determined by *reported* poverty, which changes every ten years. I analyze the effects of discontinuous changes in Title I revenue due to changes in reported poverty (reflecting actual changes over a ten-year period) on changes in other revenue sources and spending correlated with changes in actual poverty (over one- and three-year periods). For example, I consider the impact of Title I on state revenue to a school district, which is often determined by the relative property wealth of the school district, and thus highly correlated with (actual) poverty. I also consider effects on local revenue, which depends on local property values and ability to pay for

education, both of which are functions of family income (and, thus, highly non-linear functions of actual poverty).

The impact of the switch to 1990 census data

The release of 1990 census data had a significant impact on the distribution of Title I allocations to local school districts beginning with 1993 allocations. The funding changes from 1992 to 1993 corresponded with geographic population trends. Figure 2 shows how Title I revenue changed by state from 1991 to 1992, a year with about a ten percent increase in the total amount allocated. Without new poverty data, the increase was distributed in a relatively uniform way. In comparison, Figure 3 shows state-level changes in Title I funding with the release of the new census data for the 1993 allocations: here clear winners and losers emerge.

Table 2 shows the district-level distribution of the change in Title I revenue per pupil from 1992 to 1993. The change at the mean and median is small, but districts at the tails (above the 90th percentile and below the 10th percentile) experienced large gains and losses due to the census updating. In comparison, changes in the tails of the distribution were smaller from 1991 to 1992.[12] Local districts that gain or lose Title I funding due to the release of the 1990 child poverty counts provide the variation for the simulated instrumental variable analysis.

As demonstrated in Figures 2 and 3 and in Table 2, much of the variation in Title I funding from 1992 to 1993 relies on the change in child poverty from 1980 to 1990. State and local revenue are not based on census data, and thus reflect demographic

[12] In a typical year not affected by the introduction of new Census data, changes in Title I per pupil would be quite small across the distribution. The changes from 1991 to 1992 are so large only because the total amount allocated to Title I rose by about 10 percent for 1992. Unfortunately, 1990 data on district budgets are not available.

change continuously, unlike Title I revenue.[13] I exploit the introduction of the 1990 census data, which acts as an exogenous shock to the distribution of Title I funding but not to other revenue sources, to identify the role of Title I on instructional spending and other aspects of school district budgets. The lag in the release of census data is helpful in the identification strategy: changes in spending from 1992 to 1993 depend on corresponding changes in state and local revenue, which are affected by changes in child poverty from 1992 to 1993, and also on corresponding changes in Title I, which are affected by changes in child poverty from 1980 to 1990. The Title I change thus not only relies on a sharper change in poverty than the changes in state and local revenue, but also relies on a change in poverty entirely preceding the time period analyzed.

Simulated instrumental variable regression approach

The formula for allocating Title I grants considers an adjusted, lagged mean level of per-pupil spending in the state, in an attempt to adjust for geographic differences in educational prices. Because I wish to consider the effect of an exogenous shift in Title I funds (based solely on introduction of 1990 census data), I simulate a change in Title I revenue, holding mean per-pupil spending in each state constant. My notation indicates that Title I revenue in a particular year results from the non-linear allocation formula using the child poverty count (*POOR*), updated decennially, and adjusted mean per pupil expenditure in the state (*SPPE*), which is updated annually to the three-year lagged value (for simplicity, my notation indexes *SPPE* by the actual year rather than the year of the lagged value). Thus, $TI_{92} = \text{TI}(POOR_{80}, SPPE_{92})$ and $TI_{93} = \text{TI}(POOR_{90}, SPPE_{93})$. The actual change in Title I between 1992 and 1993 is expressed in equation (2):

[13] A few states use Census data for small parts of their funding decisions, such as compensatory programs. The vast majority of state funding relies on property values rather than Census reports of poverty, however.

$$Actual\ \Delta TI = \text{TI}(POOR_{90}, SPPE_{93}) - \text{TI}(POOR_{80}, SPPE_{92}) \quad (2)$$

I want to use only the change in Title I coming from the census updating. This "census-determined" change in Title I is given by the following equation:

$$Census\text{-}determined\ \Delta TI = \text{TI}(POOR_{90,}\ SPPE_{93}) - \text{TI}(POOR_{80,}\ SPPE_{93}) \quad (3)$$

We do not observe this change, but I can simulate it using the Title I formula, poverty data, and state per-pupil spending. I use the simulated census-determined change as an instrument for the actual change in Title I revenue.

To simulate the census-determined change, I first simulate how much Title I revenue per pupil school districts would have received in 1993 if the poverty counts had not been updated but all other inputs to the allocation had changed. In this simulation, the total amount of Title I grants distributed is equal to the 1993 amount, and state per-pupil expenditure is equal to the lagged level used for that state in the 1993 allocations; the allocation is denoted by $\text{TI}(POOR_{80,}\ SPPE_{93})$. I then calculate the difference between the actual 1993 per pupil Title I revenue amount and this simulated per pupil amount for each district, as summarized in equation 2. The simulated difference I estimate thus is due solely to the introduction of the 1990 census child poverty counts into the allocation framework.

Because any given change in total funding is differentially important to districts with more or fewer students, I analyze changes in Title I funding *per student*. Thus, my simulated variable is the census-determined change in Title I per pupil: [14]

[14] The instrument divides simulated Title I by enrollment in 1992 rather than 1993 in case districts experience large changes in enrollment between 1992 and 1993 that would drive the difference between the instrument and the actual Title I change per pupil. Results are nearly identical, however, using the 1993 enrollment in the denominator.

$$\frac{TI_{93}(POOR_{90}, SPPE_{93})}{ENROLLMENT_{93}} - \frac{TI_{SIM}(POOR_{80}, SPPE_{93})}{ENROLLMENT_{92}}$$

It is an instrument for the actual change in Title I *per pupil*:

$$\frac{TI_{93}(POOR_{90}, SPPE_{93})}{ENROLLMENT_{93}} - \frac{TI_{92}(POOR_{80}, SPPE_{92})}{ENROLLMENT_{92}}$$

I also consider three-year changes in Title I revenue per pupil. In this case the simulated variable is:

$$\frac{TI_{95}(POOR_{90}, SPPE_{95})}{ENROLLMENT_{95}} - \frac{TI_{SIM}(POOR_{80}, SPPE_{95})}{ENROLLMENT_{92}}$$

Impact of Title I on school district budgets: estimation

I assess the impact of the simulated exogenous change in Title I revenue per pupil on a variety of school district budgetary variables. I examine impacts on total revenue, local revenue, state revenue and its components, and federal revenue. I also consider effects on instructional spending and spending on support services, the next largest category of educational spending. I use all measures at the per-pupil level throughout the analysis.

Both the fiscal illusion and behavioral explanations of the flypaper effect describe situations in which voters are not aware that targeted intergovernmental grants could be spent on other programs, and suggest that the flypaper effect could lessen over time. In the fiscal illusion case, voters have more time to learn the new grant amount and eventually should rein in bureaucratic spending. In the behavioral case, the new grant may not seem fungible, while over time, voters and bureaucrats may mentally lump the grant together with other revenue. I consider two time periods to investigate this possibility, a shorter-run period of one year and a longer-run three-year period.

Because I use first-differences at the district level, I am controlling for all *fixed* district-level characteristics. The differencing does not, however, control for district-specific changes unrelated to the causal impact of Title I during the relevant period. I therefore control for pre-existing district-level trends in state and local revenue per pupil (spending is highly correlated with the sum of state and local revenue).

Equation 4 shows the regression specification for the effect of changes in Title I per pupil ($\Delta TI\ PP$) on changes in instructional expenditure per pupil ($\Delta INST\ EXP\ PP$), controlling for lagged changes in district-level state and local revenue per pupil (from 1986 to 1991, *lag* $\Delta STATE\ REV\ PP$ and *lag* $\Delta LOCAL\ REV\ PP$):

$$\Delta INST\ EXP\ PP_d = \alpha_d + \beta * \Delta TI\ PP_d + \phi * lag\Delta STATE\ REV\ PP_d + \gamma * lag\Delta LOCAL\ REV\ PP_d + \varepsilon_d \quad (4)$$

where the simulated census-induced change in Title I per pupil instruments for the actual change in Title I per pupil and *d* indexes the school district. The specification remains the same for other dependent variables. I use the same lagged changes in district-level state and local revenue per pupil for both the one- and three-year specifications. (I also use them in the first stage; as I explain later, it is not possible to use them for the sensitivity test on changes preceding census updating.)

First stage results

Table 3 shows that the simulated change in Title I grants is a strong predictor of the actual change. These are effectively the first stage regressions of the IV procedure for the one- and three-year changes. The simulated census-determined change in Title I grants per pupil from 1992 to 1993 (at the district level) predict the actual change in Title I grants per pupil over that period quite well: in a simple regression predicting the actual

change, the coefficient on the simulated change is 0.62 and the standard error is 0.05, with an R-squared of 0.504 and an F-statistic of 98. The simulated census-determined per-pupil change over the three-year period is a strong predictor of that actual change as well: the coefficient on the simulated change is 0.67 and the standard error is 0.02, with an R-squared of 0.630 and an F-statistic of 385.

That the coefficients on the simulated per-pupil changes are consistently less than one is not inconsistent with the strong predictive power of the instrument. To isolate the effect of the poverty data updating, the census-determined per-pupil changes are simulated using different levels of mean state per-pupil expenditures than were used in the actual allocation process. There are also several potential sources of measurement error. There is likely reporting error in the Census of Governments, particularly about which parts of Title I are reported.[15] The census poverty data from 1980 and 1990, coded at the school district level, also contain reporting error. The hold-harmless clause may introduce some simulation error. These factors contribute to classical measurement error, which is exacerbated by taking first differences, as this approach requires. Regressing simulated *levels* of Title I per pupil for 1992 on actual corresponding levels of Title I per pupil gives a coefficient of 0.97, while regressing simulated *changes* in Title I per pupil from 1992 to 1993 on actual corresponding changes gives a coefficient of 0.62. Appendix B further discusses sample exclusions and measurement error in the first stage.

[15] Examination of administrative data suggests that some districts report revenue for migrant education or Even Start, technically Title I programs, while other districts with migrant education or Even Start funds only report revenue for Title I, Part A.

V. DATA

My empirical strategy of identifying exogenous changes in Title I funding and analyzing how these changes affect expenditures and revenues requires school district-level data on the number of children and poor children in each district as measured in the 1980 and 1990 censuses and school district-level enrollments, Title I grant amounts, expenditures, and revenues for 1991 through 1995.

Annual financial data at the school district level for 1991 through 1995 come from the Elementary-Secondary School District Financial Data collected by the Bureau of the Census. This data set gives the total Title I allocation for each district in each year without distinguishing between basic and concentration grants. It also provides revenues and expenditures, by category, for each school district. I use measures of Title I revenue, spending on instruction and on support services, enrollment, local revenue, state formula aid, and state categorical aid from these data.[16]

In the simulation process, I use Department of Education administrative data at the county level for 1991 through 1995 on the number of formula count children eligible for basic and concentration grants, adjusted spending per pupil by state, and actual basic and concentration grant Title I allocations. These data improve accuracy in the Title I revenue simulation process.

Decennial data on the total number of children and children in poverty at the school district level come from the Special Tabulation File 3F for the 1980 U.S. Census

[16] I focus on instructional spending because results for total spending are quite sensitive to capital outlays. The timing of reported capital outlay expenditures does not correspond to when districts decide to undertake those expenditures, but rather to a schedule of payments to the state.

of Populations and from the joint Census-National Center for Education Statistics School District Data Book for the 1990 U.S. Census of Populations.

Per-pupil amounts of Title I changes are more accurately replicated (and thus simulated) for larger school districts. I use a combination cutoff and weighting method to minimize the impact of small school district replication error, limiting the sample to school districts with enrollments of at least 200 students in each year of the analysis and weighting school districts by their 1992 enrollments. This strategy avoids using the most error-laden school districts with fewer than 200 students, and relies more heavily on the larger districts with the cleanest replication. These districts are also of greater policy interest, as they receive the bulk of Title I funding. The majority of dropped districts were dropped because they were missing in the data from at least one of the key years and thus did not merge into my final sample. I also dropped all districts from certain states. I dropped Alaska, the District of Columbia, and Hawaii because of their unique geographic and political characteristics. I dropped Montana, Nebraska, New Hampshire, and Vermont because these states have undistributed concentration grants, making it difficult to simulate Title I allocations. Finally, I exclude Texas for all years, and exclude Michigan for the three-year changes, due to dramatic state school finance reforms which make it impossible to determine which changes in state and local revenue result from changes in Title I rather than changes in school finance regimes.[17]

Table 4 presents summary statistics for my key variables, dividing the sample into school districts predicted to gain Title I funds with the census updating and those predicted to lose funds. This divides the sample into roughly equal groups, with 3,475 districts predicted to gain funds and 3,572 predicted to lose funds. Districts predicted to

gain funds are on average larger than those losing funds, but other differences between districts are small.

VI. Results

I examine short-run responses to Title I changes over the first year following the use of the 1990 census in the allocations, from the 1992 to 1993 school years, and longer-run responses for the three-year change from 1992 to 1995. My discussion focuses on the IV results in Table 5. OLS results, which are consistent with the instrumental variable (IV) results in Table 5, are reported in Appendix C. All regression results are in per-pupil terms.

Short-run responses to census-induced changes in Title I

In the first year following census updating, Title I exhibits classic flypaper properties. It sticks about dollar for dollar to total revenue and to instructional spending, without inducing offsetting responses in local or state education revenue. Column 1 of Table 5 reports IV estimates of the effects of census-induced changes in Title I per pupil for the one-year period following the introduction of the new census data. The first line shows the effect on total revenue, which is the sum of effects on state, local, and federal revenue.[18] A one-dollar increase in Title I translates into a $1.12 increase in total revenue (with a standard error of 0.38) and a $1.25 increase in instructional spending (with a standard error of 0.44), with both effects significant at the one-percent level. Standard errors in all of the analyses are sufficiently large, however, that I emphasize the direction

[17] Results for one-year changes are not sensitive to the inclusion of Michigan.
[18] Note that the federal revenue effects are insignificantly different from one for all three years because they are dominated by actual changes in Title I, but are not exactly one because school districts receive other federal revenue and because this category includes actual Title I revenue rather than simulated.

and significance of results throughout and caution against strict interpretation of specific coefficients. More generally, then, changes in total revenue and instructional spending for the one-year period are significantly positive and insignificantly different from one.

Table 5 first breaks down the response in total revenue into state, local, and federal components. I also group state revenue to school districts into two categories: formula aid, which typically is determined by formulas dependent on property values and local revenue effort, and categorical aid. Categorical aid is distributed for specific programs, including programs such as compensatory education and special education that disproportionately go to poor districts, and is based on characteristics of students in the school district. About two-thirds of state education revenue nationwide is distributed through formula aid, and about one-third through categorical aid. These proportions, and the types of categorical aid provided, vary by state.

The effect of Title I changes on total state revenue is close to zero, but masks potentially different responses to Title I from different types of state aid. Title I increases are associated with small and statistically insignificant declines in formula aid in the very short run. Categorical state aid to districts *rises* with Title I in the first year the 1990 census data were used in Title I allocations. This 41-cent increase per additional Title I dollar is statistically significant at the 10 percent level, and is unsurprising if states use any census data in their allocations. Most state categorical aid is determined by administrative data on student characteristics, such as limited English proficiency, eligibility for free or reduced price lunch, or special education classification.[19] Any inclusion of census data in the categorical formulas could yield the observed result.

[19] These student characteristics can affect formula aid in addition to or instead of categorical aid; this varies by state.

California, for example, uses census poverty data to allocate their Title VI class size reduction funds. The extent to which other states use census data in addition to administrative data in determining their categorical grants is not well documented.

The local (school district and/or parent government) revenue response is small and insignificantly different from zero in the very short run; the point estimate suggests that a one-dollar increase in Title I per pupil leads to a 17-cent increase in local revenue per pupil, with a standard error of 0.51. That is, school districts do not change their own revenue-raising efforts immediately following an influx or outflow of Title I funds. The federal revenue response is significantly positive and insignificantly different from one. I report these federal revenue results as a check that the simulated instrument is in fact highly correlated with the actual change. School districts receive other types of federal revenue in addition to Title I, so Title I is not a perfect predictor of changes in federal revenue.

Next, Table 5 presents results for the impact of changes in Title I revenue per pupil on instructional spending.[20] Short-run spending results should be interpreted in the context of the findings on revenue: Title I gains initially translate about dollar for dollar into gains in total revenue for school districts. If districts do not anticipate permanent changes in total revenue, they may be hesitant to increase total spending. At the same time, school districts are aware of their Title I changes and know that they are mandated to use Title I dollars for instructional purposes. Table 5 shows that instructional spending

[20] I primarily emphasize results for instructional spending, because Title I revenue is intended to supplement instructional spending and not other components of total spending. Also, instructional spending is more stable within a district over time than total spending. Also note that total spending typically does not equal total revenue for a school district. Part of this is due to changes in assets and liabilities; debt is reported, but assets are not in the Census of Governments, so it is not possible to systematically equate changes in total revenue with changes in total spending.

(about 60 percent of total expenditures for the mean district) changes about dollar for dollar (a coefficient of 1.25 with a standard error of 0.44, which is significantly positive and insignificantly different from one) with Title I.

The estimate for the short-run impact of Title I on instructional spending is greater but insignificantly different from one. This point estimate is consistent with school district administrators wanting to increase instructional spending with increases in Title I, perhaps due to pressure from federal or state Title I administrators, parents, teachers and aides, school administrators, or advocacy groups. If districts are concentrating on increasing instructional spending, they may overshoot slightly, and then go elsewhere in their budgets (for example, to support services) to make up for spending not covered by the Title I increase. It is possible that such overshooting may not be accidental: if a district receives a grant that requires a relatively small additional amount of revenue to allow a particular purchase, such as a full-time teacher, it may choose to increase instructional spending by more than the grant amount.

It appears that school districts do go elsewhere in their budgets, in the very short run, to make up these differences. Changes in per-pupil spending on support services (including pupil support, instructional staff support, general and school administration, operation and maintenance of plant, transportation, and other costs), falls with Title I gains. An extra dollar of Title I causes a statistically significant 48-cent *cut* in support services. This cut makes sense if districts are looking to other potential revenue sources to supplement Title I gains to allow for particular instructional expenditures.

Anecdotal evidence on how districts and schools respond to gains and losses in Title I funding is consistent with these short-run spending results. Districts and schools

gaining Title I funds describe spending these funds in a purely supplemental manner. In particular, popular staffing uses of new Title I funds are class-size reduction (in school-wide programs, which are widespread now but were not in the period studied here), and adding teaching assistants (aides) to classrooms. Districts and schools also purchase instructional materials with Title I funds. One administrator said his district largely purchases books and readers with these funds, as there are many other sources of funding for technology but not for books. Initiating staff development, pre-school, and before- and after-school programs are also common uses of new Title I dollars.

Title I losses prompt cuts in spending: administrators describe purchasing fewer new instructional materials and cutting back on staff development. Although administrators are not able to fire certified staff working under contracts (which often span multiple years), they can choose not to fill newly vacated positions. Districts losing funds may reshuffle funds away from other non-Title I programs to maintain some of their Title I expenditures. For example, highly visible uses of Title I funds, such as pre-school and before- and after-school programs, can be extremely difficult for administrators to cut. Cutting less visible programs to maintain these yields a drop in spending despite no loss of programs publicly attributed to Title I.

Longer-run responses to census-induced changes in Title I

Changes in Title I initially significantly increased total revenue about dollar for dollar, but over time, the effect of Title I on total revenue (and, correspondingly, on instructional spending) became smaller: Column 2 of Table 5 shows that by three years after the census updating, a one-dollar increase in Title I caused only an 11-cent increase in total revenue and a 27-cent increase in instructional spending, neither of which are

statistically significant. This is because over time, local (but not state) revenue responds more negatively to Title I increases.

The main reason Title I gains no longer translated into revenue gains in the three-year period is that local revenue fell, about dollar for dollar, with Title I gains over this somewhat longer run. A one-dollar increase in Title I (which compounded by increased state categorical aid) prompted a decrease in local revenue of $1.26. Though this result would likely be discouraging to federal policymakers, it is not possible to prevent local districts from lowering their revenue if they choose.

Local revenue, as defined in the Census of Governments, is comprised of several sources. The primary source of local revenue is property taxes collected in the school district. These taxes may be collected directly by an independent school district (generally the case in the Midwest). Alternatively, a "parent government," a geographically coterminous jurisdiction such as a municipality (typical in New England) or a county (common in the South), may levy the property tax and then give revenues to the school district. School districts may also receive additional revenues from their cities or counties, other school systems, taxes other than property taxes (natural resources, for example), or non-tax revenue such as private donations. Because these different systems of local revenue are historically determined and typically were in place long before Title I, they may be considered somewhat exogenous.

I test the robustness of the local revenue finding in several ways. First, I allow districts gaining and losing Title I funds to respond differently by interacting "winner" and "loser" dummies with the simulated Title I change. I find that the local revenue result is robust: I cannot reject that responses of winner and loser districts are of equal

magnitude. Also, both winner and loser responses are insignificantly different from full crowdout of local revenue (a coefficient of negative one). I then perform a variation of this specification, comparing responses on districts with relatively large losses to those with relatively large gains. Again, I cannot reject that the two groups have equal responses of full crowdout. I also allow dependent districts (I define these as districts reporting at least half of their revenue from a parent government) to respond differently from independent districts. The result is similarly robust in this case: I cannot reject that the two types of districts respond with equal magnitude, nor that both independent and dependent districts reduce local revenue to fully offset Title I gains.

Next, I examine the role of city and county contributions to independent school districts. This excludes districts in much of the Northeast, which are dependent on municipal revenues, and in several Southern states with county-level school districts. The city and county contributions I analyze here are supplemental, rather than primary sources of support for school districts. I find that school districts receiving city or county aid had the same general local reaction described previously: Title I crowded out local revenue dollar for dollar. School districts without city or county revenues, however, had a local revenue response that was insignificantly different from zero, and was significantly different from the response of school districts with city or county aid. This may suggest that these intermediate levels of government are important forces in offsetting federal aid, but because districts receiving city or county aid differ from districts without city or county aid in other ways, it is not possible to establish a causal relationship.

Total state revenue, which had little response to Title I changes in the one-year period, had a positive but insignificant response over the three-year period, rising 55 cents with each dollar of Title I. This effect was driven by the categorical aid rather than formula aid: the magnitude of the earlier effect of targeted state aid outside of the general formula moving with Title I grew larger, although its standard error increased as well and the result is not statistically significant (a 49-cent increase in state categorical aid for each new Title I dollar). Formula aid, in contrast, responded little over the longer period. Three years after the census updating, a one-dollar increase in Title I was associated with an (insignificant) $0.05 increase in state formula aid. Because different states have such different fiscal federalist institutions, it is not surprising that state education revenue responses are heterogeneous. In Appendix D, I present a case study of Washington state. In Washington, the state plays a much larger role than the local districts in school finance, and the state responds more significantly than in the nation overall.

As the impact of Title I on total revenue fell over time, so, unsurprisingly, did the impact of Title I on instructional spending. By the three-year change, a one-dollar increase in Title I per pupil caused only an insignificant 27-cent increase in instructional spending per pupil. (Note that the significant large one-year estimate and the insignificant smaller three-year estimate have similar standard errors.) Cuts in support services, present in the one-year change, are not evident over the longer run. If districts are no longer focusing on using Title I funds for instructional purposes (because those Title I gains are no longer associated with gains in total revenue), there is no need for them to go elsewhere in their budgets to find extra funding to round out purchases.

Specification tests

I test the validity of my empirical strategy in two ways. In Table 6, I show that the simulated census-induced change in Title I had no impact on changes in dependent variables *before* the census updating took place. I also test that the general finding that Title I changes are extremely sticky in the short run and are offset by local revenue reactions in the longer run is robust to the exclusion of any geographic region, and is thus not driven by a particular region or state.

It is key for my identification strategy that the district-level changes in dependent variables are driven by causal responses to changes in Title I, and that spurious correlation between these variables caused by coincident shocks at the district level does not pose a significant problem. By controlling for district-level trends in state and local revenue per pupil in all the analyses, I aim to address this concern. As a further test, I check that changes in Title I per pupil from 1992 to 1993 do not significantly affect *earlier* changes (from 1991 to 1992) in district budgets. I cannot check for this in my main specification with district-level trends, however, because the end of the trend period, the 1991 school year, is the start of the "pre" period, causing a mechanical correlation between the two. I therefore control for pre-existing *state-level* trends in spending per pupil, from 1986 to 1990.

Table 6 shows that the change in Title I per pupil from 1992 to 1993 does not have predictive power for earlier changes in state, federal, or total revenue, or for spending on instruction or support services, in the "pre-period" test controlling for state changes in spending per pupil. For example, a dollar increase in Title I per pupil from 1992 to 1993 (using the simulated change as an instrument for the actual change) is

associated with a 34-cent increase in total revenue per pupil from 1991 to 1992, with a standard error of 0.37, controlling for state trends.

The 1992 to 1993 Title I change does have predictive power in the preceding period for local revenue: a district receiving a one-dollar increase in Title I would have experienced a 78-cent *increase* in local revenue in the *previous* year (with a standard error of 0.45). Controlling for district-level trends in the later period (rather than state-level trends in the "pre" period), however, I find that local revenue falls, rather than increases, with Title I gains, suggesting that any pre-existing trend is swamped by Title I, rather than the Title I effect being driven by a pre-existing trend. Overall, these results provide further evidence that any significant effect of the instrument on *concurrent* changes in budgets should be interpreted as causal.

I also test that the results are not driven by any particular state or geographic region. This is particularly relevant because Title I changes are linked to changes in relative numbers of poor children, resulting with the West gaining funds and the Northeast losing funds. In all cases, the main findings hold: effects on total revenue and instructional spending are insignificantly different from zero, and effects on local revenue are insignificantly different from negative one.

Heterogeneity of responses

While the central finding is not sensitive to the exclusion of any particular region or state, effects are heterogeneous across regions and states. In Table 7, I group districts into two regions, Northeast and Midwest combined, and South and West combined. Districts in the Northeast/Midwest sample experienced significantly positive changes in total revenue and instructional spending with Title I changes, while districts in the

South/West sample did not. Because these two groups of regions have different changes in Title I, different demographic changes, and often have different political structures, it is difficult to isolate the source of the heterogeneity in response. Overall, the West gained Title I funds from the census updating, while the Northeast lost funds. School districts in the Northeast and Midwest tend to be smaller than those in the South and the West; some states in the South and the West have county-level districts, while none do in the Northeast or Midwest; independent districts are common in the Northeast and not in the other regions. I use the full sample of districts and interact simulated Title I changes per pupil with region (Northeast/Midwest or South/West). Each row of Table 7 comes from one regression. Column 1 of Table 7 reports the coefficients for districts in the Northeast or Midwest, column 2 reports coefficients for districts in the South or West, and column 3 reports F-statistics and corresponding probabilities that the two coefficients are equal.

Table 7 reveals that differences between the two larger regions are significant at the 5-percent level for total revenue. This difference comes from state revenue, which moves with Title I changes in the Northeast and Midwest and moves against Title I changes in the South and West. State education revenue differences, in turn, are driven by differences in state formula aid; state categorical aid responses are not significantly different for the two larger regions. Local and federal revenue changes are not different for the two regions.

State formula aid in the Northeast and Midwest significantly increases with Title I gains, while it falls significantly with Title I gains in the South and West (these effects are estimated with more noise here than in the basic specification in Table 5). Why

might state formula aid respond negatively to Title I changes in the South and West and not in the Northeast and Midwest? One possibility is that states with many districts experiencing large gains in Title I, which are concentrated in the South and West, are more aware of the changes brought about by the census updating and thus lower the amount they spend on formula aid overall or change their general formula to be more sensitive to the Title I changes. While any explicit acknowledgement of the Title I funds in a state formula would be difficult to justify, particularly given the complexity of the Title I formula, states can tweak their formulas in ways that will benefit or penalize districts who happen to share characteristics with districts likely to gain or lose Title I funds.

To test this possibility, I group districts by whether their state, in the aggregate, gained or lost Title I funds per pupil. Table 8 shows that changes in state formula aid and state categorical aid to districts differ by whether the state overall gained or lost Title I funds per pupil; other revenue and spending effects, in contrast, do not differ for districts by this grouping, as column 3 shows insignificant F-values for all other variables. States losing Title I funds per pupil have state formula aid and categorical aid responses to districts' changes in Title I that are insignificantly different from zero, while states gaining Title I funds per pupil respond by lowering formula aid by $1.11 and increasing categorical aid by $1.24 for a district with a one-dollar increase in Title I per pupil. While this yields an insignificant response in total state revenue for states gaining Title I funds, it does suggest an interesting reorganization of educational funds within states. One potential explanation of this behavior could be that states with large Title I gains view the changes in Title I grants as a focal point: they know that child poverty is

increasing in their state relative to other states. This might prompt a switch from formula aid to categorical aid throughout the state; within a state, districts benefiting from the census updating are likely to be those districts benefiting from categorical aid as well.

VII. CONCLUSIONS

This paper finds that while school districts comply with the letter of the law, Title I ultimately fails to fully meet the spirit of its mandate to supplement instructional spending. Title I increases initially boost total school district revenue and instructional spending about dollar for dollar, but by the third year following the census-induced Title I changes, the effects are no longer significantly positive in the full sample, due to local government reactions countering the effects of Title I. These local reactions occur across all regions and regardless of aggregate state-level changes, but state revenue reactions to individual districts differ by *state-level* changes in Title I. States with increases in Title I are more likely to move from formula to categorical aid. Because the local reactions rendering Title I changes insignificant to instructional spending generally do not violate the maintenance of effort mandate of the legislation, the federal government cannot counter these responses simply by increasing enforcement of existing compliance mechanisms.

These results further the literature on the flypaper effect along several dimensions. First, by following the effects of Title I changes on school district budgets for three years, I can examine the dynamics of the flypaper effect. A one-year analysis of these data would suggest that the grant is quite sticky, while the three-year analysis shows otherwise. This suggests that research on other flypaper effects would benefit from looking at changes in responses over time, rather than longitudinal changes immediately

surrounding a policy change. Second, the finance structure of education allows me to consider the response of the receiving jurisdiction (the school district) as well as an intermediate jurisdiction (the state) to the grant from the issuing jurisdiction (the federal government). I observe that local districts are more active in their revenue responses to intergovernmental grants than are the intermediate state agencies. Finally, my identification strategy relies on changes in reported data rather than changes in actual conditions, thus providing a particularly strong foundation for drawing inference. Other work may find flypaper effects that are statistical artifacts, if it does not use adequately exogenous variation in the grants received or does not follow effects over time.

Education research should be informed by this work as well. Researchers asking if money matters must first establish that the money is *spent* in ways that should matter, rather than evaluating partial equilibrium effects of any particular revenue stream. This research furthers the evaluation literature on Title I specifically by revealing how revenue and spending react to Title I at state and local levels. The question of how any supplemental Title I funds are distributed within school districts and schools, however, remains. Even in districts where large shares of Title I revenue continue to stick to instructional spending *at the district level* over time, benefits may not be appropriately targeted to poor schools within school districts, or to educationally-disadvantaged students within schools. These questions are important next steps in assessing the efficacy of the federal government's targeting of education funds to the poor.

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Table 1

Distributions of Title I funds, per poor pupil, per pupil, and as a share of school spending, by school district, 1992 school year

	Title I per poor pupil	Title I per pupil	Title I / total spending
1st percentile	$0	$0	0
5th percentile	395	25	0.4%
10th percentile	542	37	0.6%
25th percentile	679	60	1.0%
50th percentile	811	99	1.8%
75th percentile	931	161	3.0%
90th percentile	1101	242	4.6%
95th percentile	1296	300	5.8%
99th percentile	2231	463	9.2%
Mean	838	123	2.3%
Standard deviation	473	94	1.9
N	7,030	7,047	7,047

Source: Census of Governments Public Elementary-Secondary Finance data and School District Data Book. All amounts are in real 1992 SY dollars.

Table 2

Distribution of change in Title I revenue per pupil weighted by enrollment, 1991 to 1992, and 1992 to 1993

	Change in Title I revenue per pupil from 1991 to 1992	Change in Title I revenue per pupil from 1992 to 1993
1st percentile	-64	**-98**
5th percentile	-17	**-48**
10th percentile	-6	**-30**
25th percentile	2	-12
50th percentile	9	0
75th percentile	21	19
90th percentile	41	**51**
95th percentile	60	**77**
99th percentile	141	**149**
Mean	14	5
Standard deviation	33	44
N	7,046	7,047

Source: Census of Governments Public Elementary-Secondary Finance data and School District Data Book. All amounts are in real 1992 SY dollars.

Table 3

First-stage results: correlations between simulated and actual changes in Title I revenue per pupil

Dependent variable	Independent variable	Coefficient (s.e.)	F	R^2
Actual change in Title I per pupil, 1992 to 1993	Simulated change in Title I per pupil, 1992 to 1993	0.620 (0.050)	98	0.504
Actual change in Title I per pupil, 1992 to 1995	Simulated change in Title I per pupil, 1992 to 1995	0.671 (0.021)	385	0.630

Note: Simulated change in Title I per pupil are calculated holding child poverty at 1980 levels. Regression results are weighted by 1992 enrollment of district. Robust standard errors are in parentheses. All amounts are in real 1992 SY dollars. One-year changes include Michigan; three-year changes do not. One-year results excluding Michigan are quite close to those reported here including Michigan. All results exclude the following states: AK, DC, HI, MT, NE, NH, TX, and VT. Regressions control for district-level trends in state and local per-pupil revenue from 1986 to 1991, but are not sensitive to the exclusion of these controls.

Table 4

Summary revenue and expenditure statistics for 1992, by whether school districts are predicted to gain or lose Title I funds with census updating, weighted by district enrollment

	Gainers	Losers	All
Fall enrollment	87,641	36,338	63,985
	(231,159)	(75,345)	(179,061)
Title I revenue per pupil	138	148	143
	(108)	(132)	(120)
Total expenditure per pupil	5,742	6,089	5,902
	(1,662)	(1,940)	(1,804)
Elementary and secondary expenditure per pupil	4,980	5,373	5,161
	(1,362)	(1,665)	(1,522)
Instructional expenditure per pupil	3,073	3,329	3,191
	(937)	(1,081)	(1,014)
Support services expenditure per pupil	1,667	1,804	1,730
	(506)	(646)	(579)
Expenditures for capital outlay per pupil	498	449	475
	(577)	(599)	(588)
Expenditures for other educational services per pupil	264	267	266
	(231)	(301)	(265)
State revenue per pupil	2,710	2,668	2,691
	(875)	(1,139)	(1,006)
State formula aid per pupil	1,937	1,778	1,864
	(814)	(977)	(897)
State categorical aid per pupil	773	890	827
	(478)	(530)	(506)
Local revenue per pupil	2,531	3,038	2,765
	(1,584)	(1,964)	(1,787)
Number of observations	3,475	3,572	7,047

Notes: means are reported, with standard deviations in parentheses. All figures are in real SY 1992 dollars. Results are weighted by 1992 enrollment.

Table 5

IV estimates of effects of change in Title I funds per pupil on changes in revenue and expenditures per pupil

	One-year change, 1993 (1)	Three-year change, 1993-1996 (2)
REVENUE		
total revenue	1.120***	0.109
	(0.380)	(0.450)
state revenue	0.098	0.547
	(0.433)	(0.441)
formula aid	-0.316	0.054
	(0.452)	(0.559)
categorical aid	0.414*	0.494
	(0.227)	(0.340)
local revenue	0.173	-1.262***
	(0.508)	(0.324)
federal revenue	0.849***	0.823***
	(0.127)	(0.172)
EXPENDITURES		
instructional spending	1.247**	0.269
	(0.435)	(0.489)
support services	-0.481**	0.001
	(0.209)	(0.305)

Notes: ***, **, and * indicate statistical significance at the 0.01, 0.05, and 0.10 levels, respectively. Each cell in the table represents its own regression. Simulated changes in Title I hold poverty constant at 1980 levels, and instrument for actual changes in Title I. All regressions are weighted by district enrollment in 1992, and control for district-level changes in state and local revenue per pupil from 1986 to 1991. Robust standard errors are in parentheses. All amounts are in real 1992 SY dollars. Three-year changes exclude Michigan. OLS results for these specifications are in Appendix C.

Table 6

IV estimates of effects of changes in Title I per pupil (1992 to 1993) on earlier (1991 to 1992) changes in revenue and expenditures per pupil

	"Pre" period change
REVENUE	
total revenue	0.340
	(0.369)
state revenue	-0.528
	(0.340)
formula aid	-0.339
	(0.289)
categorical aid	-0.188
	(0.272)
local revenue	0.780*
	(0.448)
federal revenue	0.088
	(0.103)
EXPENDITURES	
instructional spending	-0.327
	(0.261)
support services	0.121
	(0.184)

Notes: ***, **, and * indicate statistical significance at the 0.01, 0.05, and 0.10 levels, respectively. Each cell in the table represents its own regression. Simulated changes in Title I hold poverty constant at 1980 levels, and instrument for actual changes in Title I. All regressions control for state-level changes in spending per pupil from 1987 to 1990 and are weighted by district enrollment in 1992. Robust standard errors are in parentheses. All amounts are in real 1992 SY dollars.

Table 7

Testing differences in three-year IV coefficients on Title I change by geographic region

	Northeast & Midwest (1)	South & West (2)	F-test on equality of coefficients (3)
REVENUE			
total revenue	1.970***	-1.247**	F = 15.27
	(0.579)	(0.585)	p = 0.0001
state revenue	2.726***	-1.593***	F = 30.94
	(0.631)	(0.453)	p < 0.00001
formula aid	1.670**	-2.317***	F = 22.89
	(0.667)	(0.501)	p < 0.00001
categorical aid	1.056***	0.724	F = 0.36
	(0.318)	(0.453)	p = 0.548
local revenue	-1.617***	-0.673	F = 2.20
	(0.457)	(0.443)	p = 0.138
federal revenue	0.862***	1.019***	F = 0.59
	(0.166)	(0.118)	p = 0.441
EXPENDITURES			
instructional	1.548**	-0.847***	F = 10.60
spending	(0.684)	(0.272)	p = 0.001
support services	0.608*	-0.945***	F = 14.01
	(0.351)	(0.221)	p = 0.0002

Notes: ***, **, and * indicate statistical significance at the 0.01, 0.05, and 0.10 levels, respectively. Each row in the table represents its own regression; column 1 gives coefficients for the interaction between a dummy for being in the Northeast or Midwest and the simulated change in Title I from 1993 to 1996, and column 2 gives coefficients for the interaction between a dummy for being in the South or West and the simulated change in Title I from 1993 to 1996. Simulated changes in Title I hold poverty constant at 1980 levels, and instrument for actual changes in Title I. All regressions are weighted by district enrollment in fall 1992, and control for district-level changes in state and local revenue per pupil from 1986 to 1991. These district-level trends and the constant term is allowed to differ by regional grouping. Robust standard errors are in parentheses. All amounts are in real 1992 SY dollars.

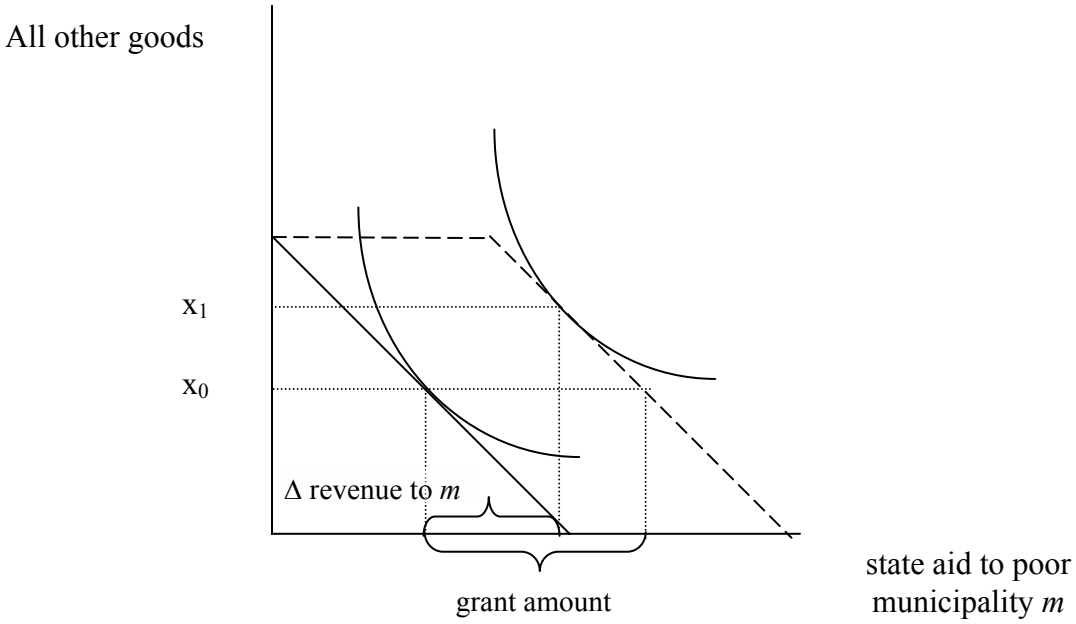
Table 8

Three-year IV estimates, by aggregate state changes in Title I

	State gains Title I per pupil (1)	State loses Title I per pupil (2)	F-test on equality of coefficients (3)
REVENUE			
total revenue	0.683	0.515	F = 0.02
	(0.484)	(0.973)	p = 0.878
state revenue	0.123	0.924	F = 0.73
	(0.476)	(0.809)	p = 0.394
formula aid	-1.113***	0.880	F = 3.25
	(0.396)	(1.032)	p = 0.071
categorical aid	1.237***	0.044	F = 4.11
	(0.391)	(0.440)	p = 0.043
local revenue	-0.527*	-1.010	F = 0.37
	(0.294)	(0.741)	p = 0.545
federal revenue	1.086***	0.601**	F = 2.21
	(0.170)	(0.278)	p = 0.137
EXPENDITURES			
instructional	0.214	1.486	F = 1.23
spending	(0.195)	(1.129)	p = 0.267
support services	-0.283	0.539	F = 1.77
	(0.186)	(0.590)	p = 0.184

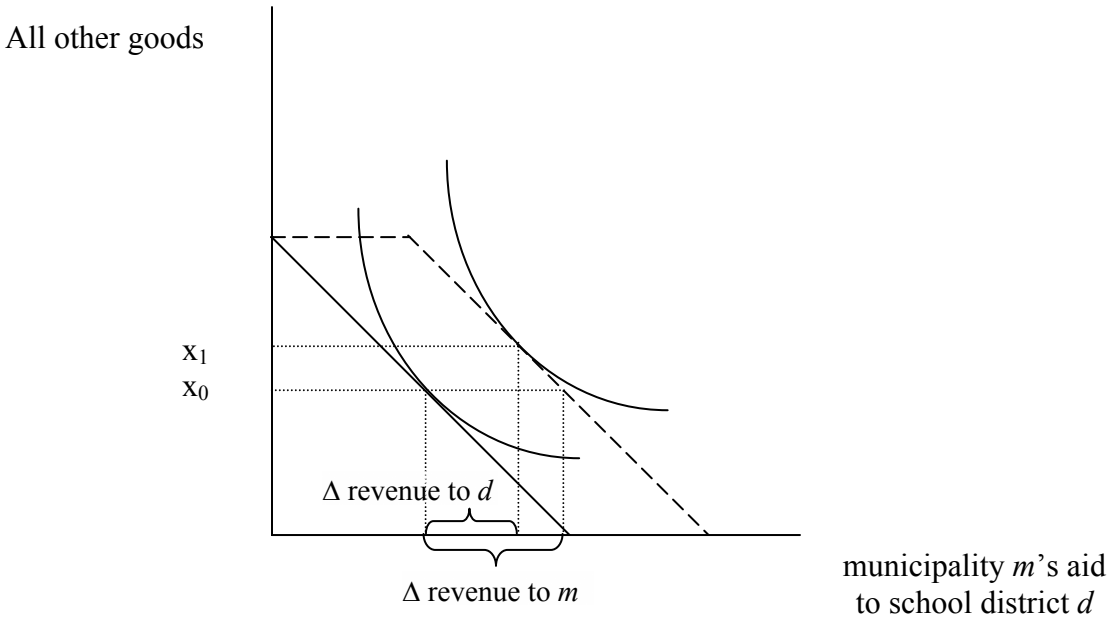
Notes: ***, **, and * indicate statistical significance at the 0.01, 0.05, and 0.10 levels, respectively. Each row in the table represents its own regression. Simulated changes in Title I hold poverty constant at 1980 levels, and instrument for actual changes in Title I. All regressions are weighted by district enrollment in fall 1992, and control for district-level changes in state and local revenue per pupil from 1986 to 1991. I also interact district-level trends in state and local revenue with a dummy variable indicating whether the state gained or lost Title I dollars per pupil, and allow the constant to differ for the two groups of states. Robust standard errors are in parentheses. All amounts are in real 1992 SY dollars.

Figure 1a: State response to a lump-sum federal grant



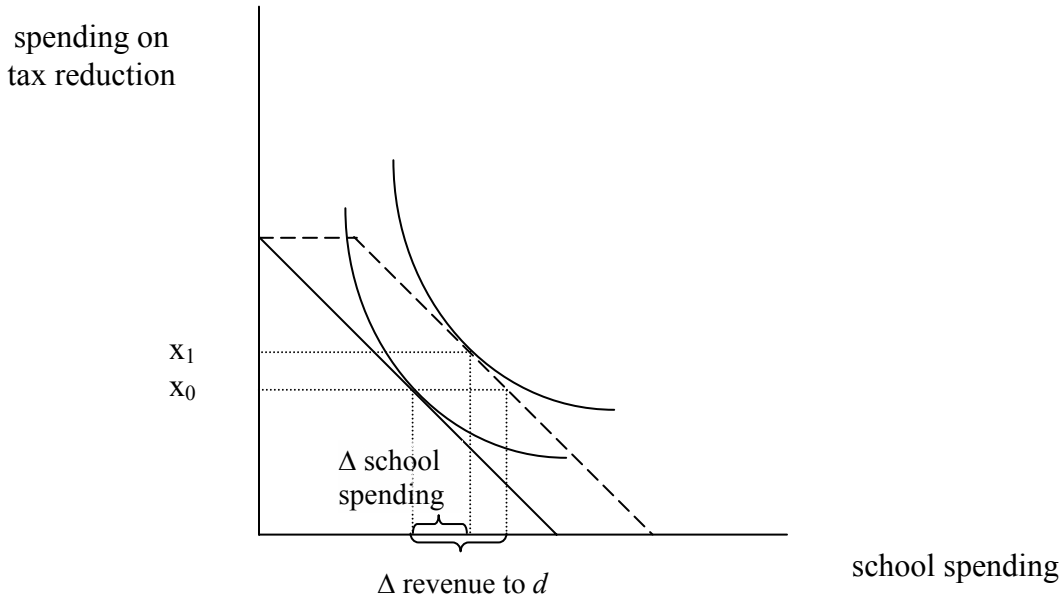
The state's budget constraint expands by the full grant amount. The grant can be used to increase state revenue to a particular poor municipality, *m*, or on all other goods, *x*, which include aid to other municipalities, other government spending, and tax reduction.

Figure 1b: Municipality *m*'s response to a lump-sum federal grant



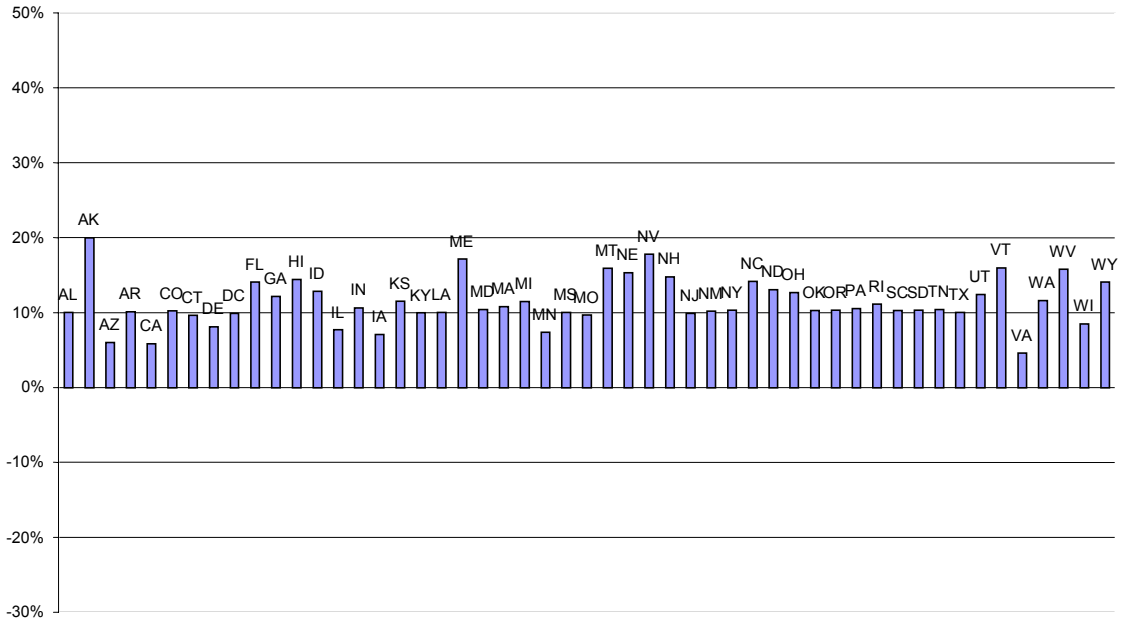
The municipality's budget constraint expands by the amount of the grant remaining after the state has offset its revenue to the municipality. The remainder of the grant can be used to increase municipal revenue to the municipality's dependent school district, *d*, or on all other goods, *x*, which include other government spending and tax reduction.

Figure 1c: School district *d*'s response to a lump-sum federal grant



The school district's budget constraint expands by the amount of the grant left after offsetting responses from the state and parent government. The remainder of the grant can be used to reduce taxes or to increase school spending.

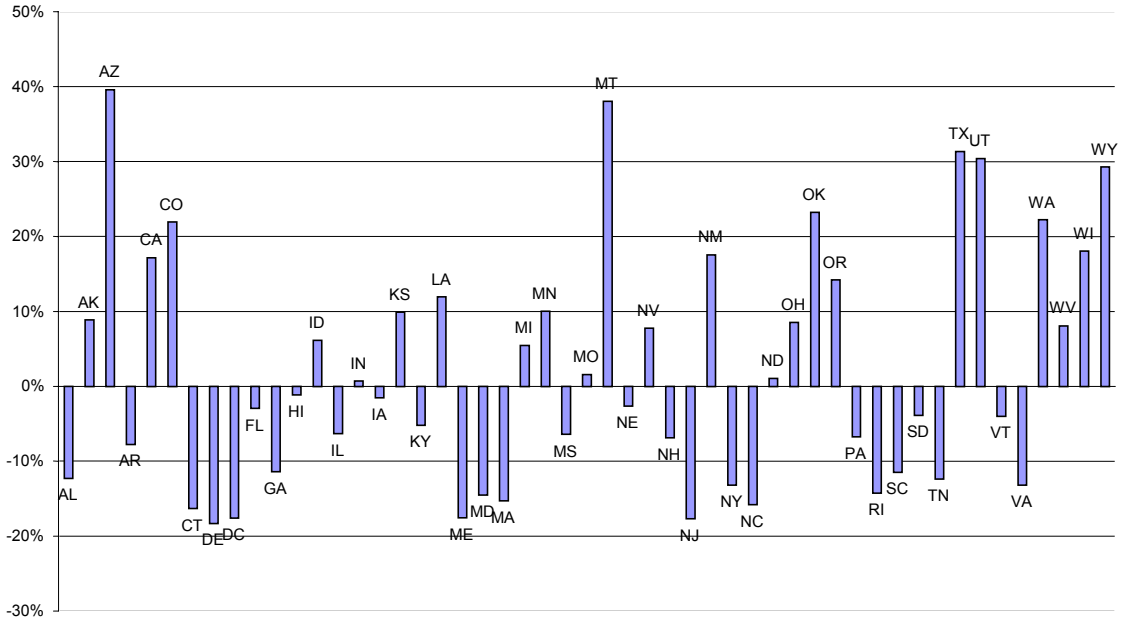
Figure 2
Percent change in state-level Title I grants, 1991-92 to 1992-93



Changes in aggregated state Title I grants from the 1991 to 1992s clustered around a 10 percent increase, which was the amount by which the total appropriation for Title I increased over those years.

Source: U.S. Department of Education administrative data.

Figure 3
Percent change in state-level Title I grants, 1992-93 to 1993-94



The switch from using 1980 to 1990 census child poverty data in allocating Title I grants yielded varied changes in aggregated state Title I grants from the 1992 to 1993s.

Source: U.S. Department of Education administrative data.

APPENDIX A

Description of Title I grant allocation process

The processes I describe here are those that were used in the years for which I have data, 1991 through 1995. In these years, the program now known as Title I was called Chapter 1. The current process of funding Title I is significantly different, with the federal government making allocations directly to school districts, more stringent accountability requirements, and more frequent updating of poverty data for the formula count. The process I describe is for the fifty states, the District of Columbia, and Puerto Rico. One percent of the total appropriation is withheld for the Secretary of the Interior, Guam, American Samoa, the Virgin Islands, the Northern Mariana Islands, and the Republic of Palau.

Chapter 1/Title I grants come in two forms, basic and concentration. Basic grants constitute about 90 percent of total funding, and are easier to qualify for. The two types of grants are disbursed in separate processes. With both types of grants, the federal government first made allocations to counties, and states then redistributed those funds to school districts within the county under the previous allocation system. There were some exceptions: in eight states where school districts frequently cross county lines, the federal government made a grant to the state level, and the state distributed it among school districts without regard to county lines. Also, in states where school districts are coterminous with counties, the federal allocation was the final step and the state did not have redistributive power.

Allocating county-level basic grants

The process of allocating federal funds to counties for basic grants involved first calculating what each county would receive according to an unadjusted formula without regard to limited funding overall. The county grants were then ratably reduced so that the sum of the grants was equal to the total allocation from Congress. Counties were then held harmless: for the period from 1991 to 1995, this means that no eligible county could receive less than 85 percent of its basic grant from the previous year, regardless of changes in the poor population. County-level grants were then summed within states to check that each state received at least the small state minimum, and the hold-harmless clause was then revisited.

1. Determining eligibility

A county was eligible for a basic grant in 1991 through 1995 if it had at least ten formula count children. The formula count is the sum of children ages 5 to 17 who reside in the county in the following categories: children in families below poverty from the most recent decennial census (approximately 96 percent of the formula count), children whose families receive AFDC payments in excess of the poverty line, children in institutions for neglected and delinquent children which are not state operated, and

children in foster homes supported with public funds. The formula count does not take into account whether children are enrolled in public or private schools.

2. Unadjusted county-level basic grants

I first allocate to each county an unadjusted grant equal to 40 percent of adjusted per pupil expenditure in its state (see explanation below) multiplied by basic grant formula count children in the county.

unadjusted basic grant = 40% * adjusted state PPE * basic formula count kids

The adjusted state per-pupil expenditure is the average current expenditure net of federal Chapter 1 and Chapter 2 expenditures, per pupil in average daily attendance three fiscal years prior to the allocation year. Thus the 1992 allocation relies on spending in 1989-90. If this figure is below 80 percent of the national average, it is adjusted up to be 80 percent of the national mean. If it is above 120 percent of the national average, it is adjusted down to be 120 percent of the national mean.

3. Ratable reduction

The process outlined above would distribute approximately three times the funds allocated to Title I by Congress. The next step is thus a ratable reduction of the unadjusted grants. This reduction is in proportion to unadjusted grant size rather than in proportion to formula count children, so the state mean expenditure per pupil is a factor in the reduction. After the ratable reduction each county *c* receives a basic grant such that:

adjusted grant_c = total basic grant appropriation * (unadjusted grant_c / Σunadjusted grants)

4. Hold harmless iterations

The hold harmless clause for basic grants ensures that as long as a county remains eligible for a basic grant, its grant cannot fall below 85 percent of its grant the previous year regardless of any fall in its formula count population. Counties are held harmless through an iterative process. I first assign each county a hold harmless status indicating whether it needs additional funds to be held harmless, has excess funds to redistribute to other counties, or is at the hold harmless line. I bring up all eligible counties below hold harmless to 85 percent of the previous year's basic grant, and ratably reduce the grant of all counties above hold harmless to fund the new increases. I then iterate this process to make sure that none of the ratable reductions bring counties that were originally above hold harmless to below their hold harmless level. This requires three or four iterations.

5. Small state minimum iterations

After holding counties harmless, I sum the county-level basic grants by state, in order to check the small state minimum requirement is being met. Each state is guaranteed a minimum basic grant allocation that is equal to the lesser of:

(a) 0.25 percent of the total basic grant allocation (the national allocation),
(b) 150 percent of the state's basic grant allocation from the previous year, and
(c) 150 percent of the national average per pupil basic grant payment (total basic grant allocation divided by total formula count nationwide) times the state's previous year's basic grant formula count.

If a state is receiving basic grants less than the minimum of (a), (b), and (c), it receives additional funds to bring it up to that level. The additional funds are redistributed from counties that are not being held harmless in states that are not bound by the small state minimum, using a ratable reduction. This process is iterated to make sure that additional states do not fall below their small state minimum after losing funds to states that initially qualified.

6. Final hold harmless/small state minimum iterations

I repeat the hold harmless iteration process to make sure that no counties fell below their hold harmless level while helping other states to reach their small state minimums.

Allocating county-level concentration grants

The concentration grant process is quite similar to the basic grant process. Exceptions are highlighted below.

1. Eligibility and formula count determination

In order for a county to be eligible for a concentration grant in 1991 through 1995, it was required to have either more than 6,500 formula count children or 15 percent of its 5 to 17 year old population as formula count *in the preceding year*. If a county had 6500 or more formula children but they constituted less than 15 percent of the county's 5 to 17 year old population, only those children in excess of 6500 were counted for allocation purposes. If at least 15 percent of the population qualified as formula count, the full formula count population was used in the allocation process.

2. Unadjusted county-level concentration grants

The unadjusted county concentration grant award is equal to the county's concentration grant formula count multiplied by the county's unadjusted basic grant and divided by the county's basic grant formula count in the previous year.

3. Ratable reduction

The unadjusted concentration grant is then ratably reduced so that the total allocation is equal to the sum of the adjusted county grants.

4. Hold harmless does not apply

During the 1991 to 1995 period, there was no hold harmless clause for concentration grants.

5. Small state minimum

The small state minimum requirement for concentration grants parallels that of basic grants. Each state is guaranteed a minimum concentration grant allocation that is equal to the greater of (1) $250,000, and (2) the minimum of (a), (b), and (c) outlined below:

(a) 0.25 percent of the national basic grant appropriation,
(b) 150 percent of the state's concentration grant allocation from the previous year, and
(c) 150 percent of the national average per pupil concentration grant payment (total concentration grant allocation divided by total concentration formula count nationwide) times the state's previous year's concentration grant formula count.

If a state is receiving concentration grants less than the maximum of (1) and (2), it receives additional funds to bring it up to that level. The additional funds are redistributed from states that are not bound by the small state minimum, using a ratable reduction. This process is iterated to make sure that additional states do not fall below their small state minimum after losing funds to states that initially qualified.

Allocating district-level basic grants

The state education agency (SEA) allocates county-level basic grants to school districts (local education agencies, or LEAs) within those county lines. Some states are exempt from county-level distribution because their LEAs cross so many county lines, and some states have LEAs coterminous with counties, and thus have no district-level allocation process.

SEAs use “best available” data to allocate these funds, which in some cases are identical to the data used by the federal government, and in some cases are entirely different (for example, Food Stamps administrative data).

Those states with the standard county-to-school district allocation process follow the steps below.

1. Determining LEA eligibility for basic grants

An LEA must be in an eligible county to be eligible for a basic grant. The SEA may choose to allocate funds to LEAs with fewer than ten formula count children, but is not required to do so.

2. Determining per-pupil basic grant amount

Within a county, the per-pupil grant amount across districts should be constant. The state thus calculates a per-pupil basic grant amount for each county equal to the county-level grant from the federal government divided by the number of formula count children in eligible LEAs, and gives each eligible LEA that per-pupil basic grant multiplied by its basic formula count.

3. Hold harmless iterations

No eligible LEA can receive less than 85 percent of its previous year's total basic grant allocation. As with the federal-to-county part of the allocation process, the hold harmless adjustments are made using an iterative process. First all eligible LEAs below their hold harmless levels are brought up to the hold harmless level, with funds taken away from LEAs within their county that are initially above their hold harmless levels. I then check that no new LEAs are brought below the hold harmless level by this process, iterating until no LEAs are below their hold harmless levels.

Allocating district-level concentration grants

States use the same best available data for allocating concentration grants as they do for allocating basic grants. States that are exempt from using county lines in allocating basic grants are also exempt from using county lines in allocating concentration grants, and states that make no sub-county basic grant allocations also make no sub-county concentration grant allocations.

There are two key differences between the district-level basic and concentration grant allocation processes. First, there is no hold-harmless provision for concentration grants at the district level, as at the county level. Second, it is possible for a county to be eligible for concentration grants, but none of its LEAs to be eligible, or for an LEA to be eligible without its county being eligible. These are not possible for basic grants since they rely completely on a count rather than on a percentage in some cases.

1. District eligibility for concentration grants

A district, like a county, must have either more than 6,500 formula count children or have at least 15 percent of its 5 to 17 year-old population qualify as formula count *in the preceding year* in order to be eligible for a concentration grant.

2. Division of concentration grants within counties

If there is at least one eligible district in an eligible county, its concentration grant is divided solely among eligible districts in the county. Allocation of concentration grants to LEAs within a county relies on basic formula count numbers, unlike the allocation of concentration grants to the counties, which uses the concentration formula count described in that section.

If there are no eligible districts in an eligible county, the county's grant is divided proportionately among districts whose percentage of formula count children exceeds the county's mean. If an eligible district is located in an ineligible county, it may or may not receive a concentration grant, depending on the state. States have the option of retaining up to 2 percent of total state concentration grants to divide among concentration eligible districts located in concentration ineligible counties. If a state chooses not to exercise that option, eligible districts in ineligible counties will receive no concentration grant.

APPENDIX B

Sources and effects of measurement error and sample selection

The data in this project are reported in several years of the Census of Governments and the Census of Populations. The instrumental variable is simulated using data from these sources and using administrative data from the Department of Education on county-level child poverty counts and Title I allocations. The first stage is strongest for aggregate levels, and weakest for per-pupil changes (divided and differenced aggregate levels). These differences suggest that measurement error is causing attenuation bias in the first stage of per-pupil changes.

There are several sources of measurement error in these data. Reporting error may occur in the Census of Governments school district financial data and in the school district child poverty census files. The covariance among these errors is unclear: errors may be correlated within data sources within a year, within variables across years, or with no other errors or variables. Simulation error occurs to a small degree in the instrumental variable. I test this by looking at replicated rather than simulated values for Title I in 1992, using the actual inputs. The replicated values correlate closely with actual values, but differ considerably for the smallest school districts, as discussed in the data section. Replication error is highest in several states subject to the small state concentration grant minimum. These states receive a minimum amount of concentration grant funds, and at times have undistributed grants because few or no counties or school districts are eligible for the concentration grants. For this reason, I exclude Montana, Nebraska, New Hampshire, and Vermont.

While I have been able to replicate federal allocations to counties using the federal administrative data, the allocations made by state to districts within counties have proved more messy. First of all, I do not have the state administrative data used in the allocations. States choose their own measure of poverty to use in allocating federal funds within counties. I do not have these data for states choosing measures other than census poverty data, such as administrative data from AFDC, Medicaid, Food Stamps, or free and reduced lunch, and use the census child poverty counts as a proxy. I also do not have the data on neglected and delinquent children (who are counted as eligible in addition to poor children in the census) used in the allocation process. Second, states may intentionally or unintentionally deviate from the rules in making their allocations.

I exclude Alaska, the District of Columbia, and Hawaii because their geographic and political differences from the other states may be important.

Finally, not all districts may pursue Title I funds. If eligible districts do not apply for Title I funds and do the necessary administrative work to retain them, the simulated change in funds can be a worse estimator of the actual change than the simulated level will be of the actual level.

There were about 14,500 local educational agencies in the US in the early 1990s. Of these, 7,047 are unified school districts (serving elementary and secondary grades), present in each year of financial data, with non-missing Title I data, are in both census years, have legitimate county identifiers, and are not excluded from the sample based on their state or on their district enrollment.

APPENDIX C

OLS estimates of effects of changes in Title I per pupil on changes in revenue and expenditures per pupil

	One-year change, 1993 (1)	Three-year change, 1993-1996 (2)
REVENUE		
total revenue	1.013***	0.637*
	(0.314)	(0.336)
state revenue	0.041	0.897***
	(0.337)	(0.337)
formula aid	-0.264	0.519
	(0.387)	(0.344)
categorical aid	0.305**	0.378
	(0.152)	(0.253)
local revenue	0.164	-1.263***
	(0.452)	(0.234)
federal revenue	0.808***	1.002***
	(0.103)	(0.074)
EXPENDITURES		
instructional spending	0.902***	0.426
	(0.324)	(0.349)
support services	-0.157	0.142
	(0.168)	(0.193)

Notes: ***, **, and * indicate statistical significance at the 0.01, 0.05, and 0.10 levels, respectively. Each cell in the table represents its own regression. All regressions are weighted by district enrollment in fall 1992, and control for district-level changes in state and local revenue per pupil from 1986 to 1991. Robust standard errors are in parentheses. All amounts are in real 1992 SY dollars. Three-year changes exclude Michigan.

APPENDIX D

Case study of Washington state

Washington state was one of the greatest beneficiaries of the census-based updating in child poverty counts. In the 1992-93 school year, Washington received $65.4 million in Title I revenue to local school districts; by the 1995-96 school year, the state's Title I allocation had risen to $97.2 million. Washington's Title I money was concentrated in both years. In 1992-93, 25 percent of districts received less than $50 per student. In 1993-94, the corresponding number was 15 percent.

For three reasons, Washington is a particularly good state for getting "inside the black box" of responses to Title I. First, as noted, its Title I funding changed substantially. Second, although the state's *share* of school revenue is unusually high in Washington, the state's school finance formula is fairly typical of an American state. Thus, its responses are likely to be reasonably representative of state responses. Third, Washington meets important data criteria: it has a reasonable number of school districts (296), and experienced no obvious confounding changes in school finance from 1992-93 to 1995-96.

Background on Washington state school finance

In the 1993-94 school year, the state provided $3,779 million for education, with local districts raising $832 million (Gold, Smith, and Lawton, 1995). Of the state revenue for education in 1993-94, 78 percent was distributed through a foundation formula that essentially reimburses districts on a per-student basis. Two percent was distributed through a relatively simple equalization formula: only school districts with mean property values below the statewide average are eligible for participation in this program. The remainder of state education funds was distributed to districts for categorical programs as detailed in Table D-1. Transportation, school construction assistance, special education, compensatory education, and bilingual education are the main categorical programs in Washington state.

Table D-1: Total state education revenue in Washington state, 1993-94 school year

Program	Funds (in millions of nominal dollars)	Percent of all state education revenue
All state education revenue	$3,779.0	100%
Basic education allocation formula	2,958.0	78.3
Transportation	152.6	4.0
School construction assistance	192.4	5.1
Special education	421.8	11.2
Compensatory education	53.0	1.4
Gifted and talented	4.1	<1
Bilingual education	22.7	<1
Local effort assistance	76.4	2.0
Local education program enhancement	22.9	<1

Impact of Title I changes on other revenues within Washington state

In order to see how a district in Washington state with a Title I gain of one dollar per pupil fares relative to another district in Washington with no change in its per pupil Title I allocation, I repeat the instrumental variable regression analysis limiting the sample to school districts in Washington. This analysis, presented in Table D-2, is based on a sample of 236 school districts (of 296 statewide), and has large standard errors for many coefficients. There are only significant effects on two variables: state formula aid, and state revenue for bilingual education (bilingual education is one small categorical program out of many—none of the others were significantly affected, nor was total categorical aid). In summary, Table D-2 shows that within Washington state, districts gaining Title I funds tended to lose state general formula revenue and to gain state revenue for bilingual education.

Table D-2: IV estimates of effects of changes in Title I per pupil on changes in revenue per pupil, Washington state

	Three-year change, 1993-1996
REVENUE	
total revenue	1.804
	(2.315)
state revenue	1.400
	(1.932)
formula aid	-0.503*
	(0.276)
categorical aid	1.902
	(2.016)
bilingual education	0.345***
	(0.076)
local revenue	-0.995
	(0.753)
federal revenue	1.400***
	(0.372)

Ability to of the state to respond to Title I changes through formula aid

Washington's formula for distributing revenue to school districts consists of two parts: (1) a foundation plan, distributing funds on a per-pupil basis,[21] and (2) a power equalization plan to redistribute resources to school districts based on their assessed mean property values.[22] No census variables are used in determining allocations. Because the formulas for its foundation and equalization aid are relatively simple, the likeliest response through these formulas is simply to change the amount of funding the programs receive overall.

The school finance rules in Washington would allow legislators to target districts with Title I changes only in a very crude fashion, however, as the complex and non-linear formula used in allocating Title I revenue is far from the relatively simple rules of the state. Most obviously, legislators may realize that districts they perceive to be poor are receiving Title I gains, and that these districts also receive state equalization aid. This perception would be correct: as Table D-3 shows, the greater the Title I gain per pupil in a district, the more likely the district is to be eligible for equalization aid (eligibility is determined by having a mean property value below the statewide average).

Table D-3: Changes in Title I per pupil and eligibility for state equalization aid

Title I change per pupil, 1993-96	Percent of districts eligible for state equalization aid
<$50	70%
>=$50	94%

Legislators did in fact cut back on equalization aid in Washington state immediately following the impact of the census updating on Title I allocations. Table D-4 shows that in 1993-94, the first year that census updating affected Title I revenues, state equalization spending fell to about $75 million from about $85 million in 1992-93,[23] while state foundation spending continued to grow. State equalization aid fell further in 1994-95, while Title I's hold-harmless clause promoted further changes in revenues to reflect the census updating.

[21] A small fraction of funds are affected by the number of staff employed by the district.
[22] Note: the entire discussion of Washington's school finance rules draws heavily on Gold, Smith, and Lawton (1995). Please refer to their work for further details.
[23] Note that equalization spending did not drop below its 1991-92 level.

Table D-4: State formula revenue for education in Washington state

School year	Foundation aid (nominal dollars)	Equalization aid (nominal dollars)
1991-92	$2,728,043,127	$69,829,315
1992-93	2,896,547,438	85,136,599
Census updating first affects Title I revenue in 1993-94 school year		
1993-94	2,959,423,987	75,361,371
1994-95	3,030,199,418	71,204,331
1995-96	3,289,546,747	78,564,648

State responses through categorical aid

While the estimated response through categorical aid broadly is quite noisy, the change in Title I produced a modest and statistically significant change in state revenue allocations for transitional bilingual education among districts within Washington state. Districts receive a set bilingual allotment per student identified as eligible. Eligibility is determined by student interview and, if necessary, performance on a state English test and is *not* affected by any U.S. Census of Populations data. Districts receiving bilingual dollars, however, disproportionately gained from the census updating of Title I.

Table D-5 classifies districts by what share of their students are Hispanic or Asian. It shows that districts with greater potential for receiving bilingual funds (that is, districts with a higher share of Hispanic and Asian students) were more likely to experience significant positive gains in Title I after the census updating. State bilingual revenues to school districts were rising over this period, explaining the correlation between Title I changes and state revenue for bilingual education.

Table D-5: Changes in Title I per pupil and potential eligibility for state transitional bilingual education revenue

Percent of students Hispanic or Asian (combined)	Percent of districts gaining at least $50 Title I revenue per pupil, 1993-96
0 – 5%	27%
5 – 20%	36%
>= 20%	61%

Conclusion

In summary, the experience of Washington state shows that a state may respond to a statewide revenue gain from Title I by shifting funds away from equalization aid to poor districts without having to alter the equalization formula. It also shows that in allocating formula and categorical aid, the state may view districts roughly similarly to how they are treated by the Title I allocation formula.