



ELSEVIER

Journal of Public Economics 88 (2004) 1771–1792

JOURNAL OF
PUBLIC
ECONOMICS

www.elsevier.com/locate/econbase

Do federal grants boost school spending? Evidence from Title I

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Received 6 April 2003; received in revised form 5 August 2003; accepted 17 September 2003

Abstract

One of the federal government's main elementary and secondary education programs is Title I, which allocates money for compensatory education to school districts based on child poverty. I use sharp changes in per-pupil grant amounts surrounding the release of decennial census data to identify effects of Title I on state and local education revenue, and how much the program ultimately increases spending by recipient school districts. I find that state and local revenue efforts initially are unaffected by Title I changes, but that local governments substantially and significantly crowd out changes in Title I within in a 3-year period.

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JEL classification: H7; H4; I2

Keywords: Fiscal federalism; Intergovernmental grants; Education finance; Compensatory education

1. Introduction

Title I is widely recognized as the federal government's single most important education program. It attempts to increase the resources of school districts that serve economically disadvantaged children, and cost \$10.4 billion in FY 2002. It thus represents one-third of the US 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. Not only has Title I traditionally been the main way the federal government directly aids poor local schools, but among the 10% of school districts that

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rely most heavily on the program, Title I accounts for between 5% and 10% of total spending. Under the No Child Left Behind Act of 2001 (NCLB), the Title I program has taken on a new accountability role as well: schools designated as in need of improvement may lose Title I funds.¹

If other revenue sources to school districts systematically offset gains from Title I, the program will have less than its intended effect on the schooling experienced by poor children. School districts' budgets are determined by as many as three levels of government, in addition to the federal government: states, local governments such as counties and municipalities, and school districts. Any of these other levels of government could potentially offset Title I revenue. 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 how 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. 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; Kosters and Mast, 2003; 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. 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 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

¹ NCLB requires states to set subject- and grade-specific academic standards and to assess students in relation to these standards. The law has several accountability provisions specifying penalties for schools that fail to make sufficient progress in meeting these standards (US Department of Education, 2002). After 2 years of failing to make "adequate yearly progress", schools must reserve up to 20% of Title I Part A funds for transporting students to schools that are not designated as in need of improvement and reserve 10% of Title I Part A funds for professional development. After 3 years, schools must allow students to essentially cash out their Title I benefits and purchase supplemental instructional services from a private provider.

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 traditionally had depended on child poverty counts from the decennial Censuses of Population, and these counts are updated only at 10-year intervals.² Thus, Title I allocations jumped discretely every 10 years while poverty (and the state and local revenues that depend on poverty) changed continuously. Moreover, decennial census counts are first used in Title I allocations approximately 3 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 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 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.

² The Census Bureau began using administrative data to make projections of district-level child poverty counts for the Title I allocation process with the allocation for the 1997–1998 school year. I consider only years using the decennial data in this paper.

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

The remainder of this paper is structured as follows. In Section 2, I present background information on the Title I program and review the literature on Title I. In Section 3, I review the theory and empirical literature on the intergovernmental grants. In Section 4, I discuss the methodology, in Section 5 the data, and in Section 6 the results. Section 7 concludes.

2. 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 While the current legislation details requirements of the Title I program, focusing on standards, assessments, and accountability, the guidance on *how* school districts are to use Title I funds is and traditionally has been 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.⁵

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% of districts receiving more than \$1000 per low-income pupil and more than \$250 per pupil.⁶

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. For example, 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 described how “arts and crafts is paid for out of Title I funds on the theory that it will increase ‘small muscle’ coordination” as just one of multiple non-compliance problems in the district (Harvard Center for Law and Education, 1972).

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 district spending. It mandates that either state and local revenue per pupil or aggregate state and local revenue cannot fall below 90% of their levels in the

⁴ This paper considers only Part A of Title I, “Improving Basic Programs Operated by Local Educational Agencies”, which gives grants to school districts based primarily on their child poverty counts. Other parts of Title I include provisions for migrant education, neglected and delinquent children, and dropout prevention, among other programs. Policy discussion of “Title I” generally refers to Part A of the program, while the other parts typically are referred to more specifically. 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.

⁵ Local guidance on use of Title I funds is at times much more specific than the general federal guidance. The degree to which Title I funds are restricted thus varies by district.

⁶ 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.

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
<i>N</i>	7030	7047	7047

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

preceding fiscal year without penalty.⁷ In 1992, Title I provided about 2% of total spending for the average district. For the 1% of districts relying most heavily on Title I, their Title I revenue approached 10% 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%, 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”.

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 his 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, and found that for every additional dollar of Title I revenue, total spending was about 80 cents higher.

3. Intergovernmental grants and the flypaper effect

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

⁷ 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 to the reduction of state and local effort in the school district.

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 only a small share) going to 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. 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.

There is a large literature focused on estimating the effect of various intergovernmental grants to state and local governments. Hines and Thaler (1995) provide an excellent review of this literature, and Fisher and Papke (2000) provide a review of education-specific flypaper research. 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, corresponding to a strong flypaper effect.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 (in his particular case, federal highway funding to states, he considers political endogeneity of grants) reveals significant crowd-out, suggesting that some observed flypaper effects may be statistical artifacts.

The flypaper literature is generally concerned with how targeted expenditures respond to intergovernmental grants. When the spending jurisdiction receives revenue from multiple sources, however, the individual revenue responses that ultimately determine the net effect on spending are of independent interest themselves. In this case, because the typical school district today receives approximately the same amount from the state as it raises at the local level, it is important to consider the effects that federal grants may have on both state revenue to local school districts and 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 increases by some amount less than the federal grant. Local revenue responses can come through school districts themselves changing their tax rates, or, in some cases, through parent governments.⁹

⁸ Government spending is estimated to rise by about 5–10% 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 5–10% of the increase in the Title I grant amount, but the maintenance of effort clause would not be violated in most cases.

⁹ Some school districts have a parent government that aids them—for instance, a county that aids its county school district or a municipality that aids the district that is, typically, geographically aligned with it. A subset of districts with parent governments are dependent on them, meaning that the district receives all local revenue through the parent government and cannot raise revenue at the district level.

4. 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, Eq. (1) would be used:

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

where d indexes the school district, and the change is taken over at 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 variation in Title I grants is exogenous to state and local spending because poverty counts influence both Title I and spending, and 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. Finally, I explain how the use of average state education spending in the title I allocation formula poses an endogeneity problem for OLS and describe an instrumental variables (IV) approach to this problem.

4.1. 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 1991 through 1995 (see [US Department of Education, 1990](#), for more detail). I use the formula in its entirety to predict a district's grant before and after the census updating; the reader should focus on three facts from the following description of the Title I formula. First, the grants were mainly determined by decennial census child poverty data. Decennial child poverty figures jump discretely whereas state and local revenue change more continuously with continuous changes in poverty; furthermore, 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. Second, the grants were partially determined by state-level education spending, which is obviously related to key dependent variables, such as instructional spending and state revenue to local districts; when I use census-determined changes in Title I to instrument for actual changes in Title I, this purges the effect of changes in state education spending from changes in Title I. Third, the Title I allocation formula is sufficiently complex and 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 distributes two types of grants to the states, with allocations specified at the county level.¹⁰ States then distribute grants to school districts within the counties. The Title I formula used child poverty data from the 1980

¹⁰ Counties with at least 10 poor children ages 5–17 were eligible for “basic grants”. Basic grants accounted for about 90% of the total Title I budget in the early 1990s. Counties with either 6500 or more poor children or 15% or more children in poverty were eligible for “concentration grants”.

census for allocations through 1992, and then switched to the 1990 data beginning with 1993.¹¹ Title I allocations also reflected adjusted mean state per-pupil expenditure (SPPE), used as an education cost index.¹²

The Title I formula allots a set share of SPPE per poor child, then revises allocations through an iterative process to comply with hold-harmless and small state minimum requirements.¹³ Once a state had the Title I grant for each of its counties, it redistributed the grants to school districts within each county based on poverty, following the same eligibility and distribution rules as the federal distribution to counties.¹⁴ I can, therefore, summarize a district's Title I allocation in a given year as a non-linear function TI of the most recent decennial child poverty counts (POOR) and adjusted mean SPPE, which is updated annually to the 3-year lagged value (for simplicity, my notation indexes SPPE by the actual year rather than the year of the lagged value): $TI_{92} = TI(POOR_{80}, SPPE_{92})$ and $TI_{93} = TI(POOR_{90}, SPPE_{93})$.

4.2. Regression discontinuity surrounding the release of 1990 census data

One would expect the OLS approach in Eq. (1) in which the change in Title I is regressed on the change in instructional expenditures to be 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. The infrequent updating of child poverty data used in the Title I allocations allows me to 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 10 years. I analyze the effects of discontinuous changes in Title I revenue due to changes in reported poverty (reflecting actual changes over a 10-year period) on changes in other revenue sources and spending correlated with changes in actual poverty (over 1-, 2- and 3-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).

¹¹ To be precise, the number of “formula count” children determines allocations, rather than the number of poor children. The number of formula count children is determined nearly entirely by child poverty, but also includes counts of neglected and delinquent children. I use child poverty instead of the full formula count, due to data availability.

¹² The adjusted amount is equal to average per pupil spending in the state 3 years earlier, less Title I funds received. States below 80% of the national average per pupil spending were brought up to that level, and states above 120% were brought down to that amount.

¹³ In the mid-1990s, the hold-harmless clause stated that, as long as a county or school district remained eligible, it could not receive less than 85% of the basic grant it had received in the previous year. Concentration grants were not held harmless at that time.

¹⁴ 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.

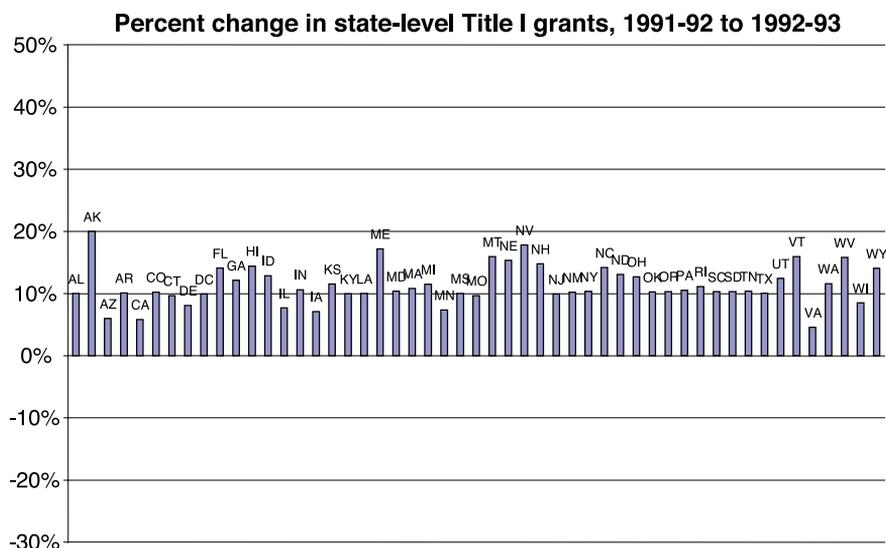


Fig. 1. Changes in aggregated state Title I grants from 1991 to 1992 clustered around a 10% increase, which was the amount by which the total appropriation for Title I increased over those years. Source: US Department of Education administrative data.

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. Fig. 1 shows how Title I revenue changed by state from 1991 to 1992, a year with about a 10% increase in the total amount allocated. Without new poverty data, the increase was distributed in a relatively uniform way. The distribution is not completely uniform because of varying state-level growth rates of the education cost index. In comparison, Fig. 2 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 1992.¹⁵ 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 IV analysis.

4.3. Isolating census-induced changes in Title I: an instrumental variable approach

Choosing a longitudinal period surrounding a year with new child poverty data introduces variation in funding that is not perfectly collinear with demographic change

¹⁵ 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% for 1992. Unfortunately, 1990 data on district budgets are not available.

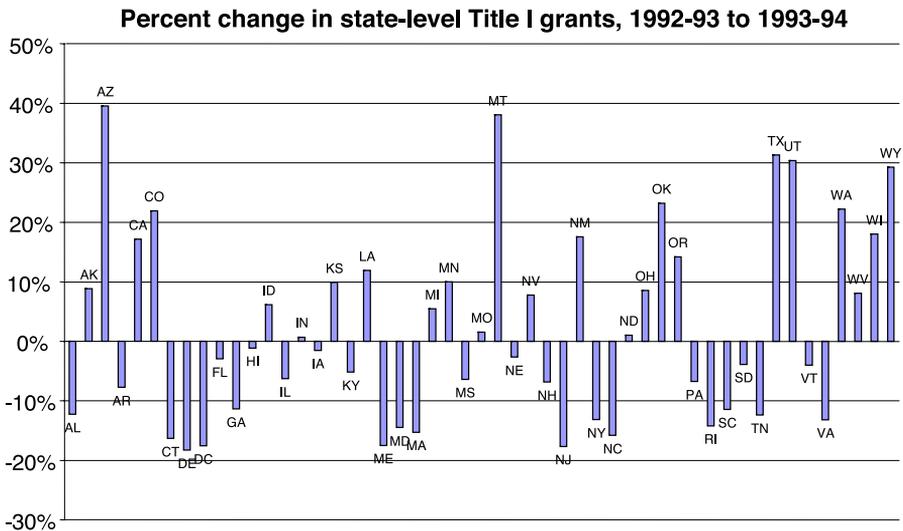


Fig. 2. The switch from using 1980–1990 census child poverty data in allocating Title I grants yielded varied changes in aggregated state Title I grants from the 1992 to 1993. Source: US Department of Education administrative data.

is likely to affect state and local revenue independently. Actual changes in Title I revenue, even surrounding this discontinuity, still reflect short-term changes in mean state education spending, however. The actual change in Title I between 1992 and 1993 is expressed in Eq. (2):

$$\text{Actual } \Delta \text{TI} = \text{TI}(\text{POOR}_{90}, \text{SPPE}_{93}) - \text{TI}(\text{POOR}_{80}, \text{SPPE}_{92}) \quad (2)$$

Table 2

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

	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
<i>N</i>	7046	7047

Source: census of governments public elementary–secondary finance data and school district data book. All amounts are in real 1992 SY dollars.

Because I wish to consider the effect of an exogenous shift in Title I funds (based solely on introduction of 1990 census data), I calculate how Title I revenue would change with the new poverty data, holding mean per-pupil spending in each state constant. This “census-determined” change in Title I is given by the following equation:

$$\text{Census-determined } \Delta\text{TI} = \text{TI}(\text{POOR}_{90}, \text{SPPE}_{93}) - \text{TI}(\text{POOR}_{80}, \text{SPPE}_{93}) \quad (3)$$

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

To compute the census-determined change, I first calculate 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 calculation, the total amount of Title I grants distributed is equal to the 1993 amount, and SPPE is equal to the level used for that state in the 1993 allocations; the allocation is denoted by $\text{TI}(\text{POOR}_{80}, \text{SPPE}_{93})$. I then calculate the difference between the actual 1993 per-pupil Title I revenue amount and this calculated per-pupil amount for each district.

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.¹⁶

$$\frac{\text{TI}_{93}(\text{POOR}_{90}, \text{SPPE}_{93})}{\text{ENROLLMENT}_{93}} - \frac{\text{TI}_{\text{SIM}}(\text{POOR}_{80}, \text{SPPE}_{93})}{\text{ENROLLMENT}_{92}}$$

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

$$\frac{\text{TI}_{93}(\text{POOR}_{90}, \text{SPPE}_{93})}{\text{ENROLLMENT}_{93}} - \frac{\text{TI}_{92}(\text{POOR}_{80}, \text{SPPE}_{92})}{\text{ENROLLMENT}_{92}}$$

I also consider 3-year changes in Title I revenue per pupil. In this case the simulated variable is:

$$\frac{\text{TI}_{95}(\text{POOR}_{90}, \text{SPPE}_{95})}{\text{ENROLLMENT}_{95}} - \frac{\text{TI}_{\text{SIM}}(\text{POOR}_{80}, \text{SPPE}_{95})}{\text{ENROLLMENT}_{92}}$$

4.4. 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, spending on support services (the next largest category of

¹⁶ 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.

educational spending), and capital outlay expenditures. I use all measures at the per-pupil level throughout the analysis.

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 concurrent 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 with changes in state and local revenue per pupil from 1986 to 1991 (spending is highly correlated with the sum of state and local revenue).

Eq. (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 $\Delta LOCAL REV PP$), and enrollment changes:

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

where the census-determined 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 1- and 3-year specifications, as well as in the first stage.

4.5. 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 1- and 3-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 0.58 and the standard error (S.E.) is 0.04, with

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, 1992–1993	Simulated change in Title I per pupil, 1992–1993	0.584 (0.040)	91	0.526
Actual change in Title I per pupil, 1992–1995	Simulated change in Title I per pupil, 1992–1995	0.672 (0.022)	291	0.630

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 S.E. are in parentheses. All amounts are in real 1992 SY dollars. One-year changes include Michigan; 3-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 and for enrollment changes from 1992 to 1993, but are not sensitive to the exclusion of these controls.

an R^2 of 0.526 and an F -statistic of 91. The simulated census-determined per-pupil change over the 3-year period is a strong predictor of that actual change as well: the coefficient on the simulated change is 0.67 and the S.E. is 0.02, with an R^2 of 0.630 and an F -statistic of 291.

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 SPPE 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.¹⁷ 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 my computed *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 my computed census-determined *changes* in Title I per pupil from 1992 to 1993 on actual corresponding changes gives a coefficient of 0.58.

5. 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, capital outlays, enrollment, local revenue, state formula aid, and state categorical aid from these data.

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. Decennial data on the total number of children and children in poverty at the school district-level come from the Summary Tape File 3F for the 1980 US Census of Populations and from the joint Census–National Center for Education Statistics School District Data Book for the 1990 US Census of Populations.

¹⁷ 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.

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 (231,159)	36,338 (75,345)	63,985 (179,061)
Title I revenue per pupil	138 (108)	148 (132)	143 (120)
Total expenditure per pupil	5742 (1662)	6089 (1940)	5902 (1804)
Elementary and secondary expenditure per pupil	4980 (1362)	5373 (1665)	5161 (1522)
Instructional expenditure per pupil	3073 (937)	3329 (1081)	3191 (1014)
Support services expenditure per pupil	1667 (506)	1804 (646)	1730 (579)
Expenditures for capital outlay per pupil	498 (577)	449 (599)	475 (588)
Expenditures for other educational services per pupil	264 (231)	267 (301)	266 (265)
State revenue per pupil	2710 (875)	2668 (1139)	2691 (1006)
State formula aid per pupil	1937 (814)	1778 (977)	1864 (897)
State categorical aid per pupil	773 (478)	890 (530)	827 (506)
Local revenue per pupil	2531 (1584)	3038 (1964)	2765 (1787)
Number of observations	3475	3572	7047

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

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.¹⁸ 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 problematic in this context.¹⁹

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 3475 districts predicted to gain funds and 3572 predicted to lose funds. Districts predicted to

¹⁸ Because Title I funds are given to districts per formula count student, while administering a Title I program incurs some level of fixed administrative costs, small districts have lower take-up rates on Title I participation given eligibility.

¹⁹ 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 3-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. Results for 1-year changes are not sensitive to the inclusion of Michigan.

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

6. 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, over the 2-year period from 1992 to 1994, and longer-run responses for the 3-year change from 1992 to 1995. My discussion focuses on the IV results in columns 1 and 3 in Table 5, which present results for 1- and 3-year changes. The 2-year changes, in column 2, generally fall about midway between the 1- and 3-year changes. OLS results, which are largely consistent with the IV results in Table 5, are reported in the Appendix A.²⁰ All regression results are in per-pupil terms.

6.1. Short-run responses to census-determined 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-determined changes in Title I per pupil for the 1-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.²¹ A \$1 increase in Title I translates into a \$0.98 increase in total revenue (with a S.E. of 0.41) and a \$1.40 increase in instructional spending (with a S.E. of 0.55), with both effects significant at the 5% level. S.E. in all of the analyses are sufficiently large, however, that I emphasize the direction 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 1-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.²²

²⁰ The main difference between the OLS and IV results arises in predicting changes in state aid to districts. This is unsurprising, as the use of instrument is motivated by potential correlations between actual changes in Title I and trends in state aid to districts.

²¹ Note that the federal revenue effects are insignificantly different from one for all 3 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.

²² 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.

Table 5

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

	1-year change, 1992–1993 (1)	2-year change, 1992–1994 (2)	3-year change, 1992–1995 (3)
<i>Revenue</i>			
Total revenue	0.981** (0.406)	0.538 (0.485)	– 0.036 (0.469)
State revenue	0.348 (0.308)	0.465 (0.487)	0.251 (0.396)
Formula aid	0.019 (0.315)	0.072 (0.555)	– 0.576* (0.309)
Categorical aid	0.329 (0.250)	0.393 (0.298)	0.828*** (0.259)
Local revenue	– 0.199 (0.337)	– 0.952** (0.452)	– 1.215*** (0.338)
Federal revenue	0.832*** (0.118)	1.026*** (0.120)	0.928*** (0.130)
<i>Expenditures</i>			
Instructional spending	1.401** (0.551)	0.960* (0.509)	0.119 (0.501)
Support services	– 0.425* (0.241)	– 0.028 (0.264)	– 0.293 (0.208)
Capital outlays	0.478 (0.332)	0.527 (0.390)	0.392 (0.341)

***, **, 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 and for relevant changes in enrollment (1992–1993, 1993–1994, 1994–1995). Robust S.E. are in parentheses. All amounts are in real 1992 SY dollars. Two- and 3-year changes exclude Michigan. OLS results for these specifications are in Appendix A.

The effect of Title I changes on total state revenue is small and insignificantly positive, as are the effects on formula aid and categorical aid, the two components of state revenue. The local (combined school district and parent government) revenue response is small and insignificantly different from zero in the very short run; the point estimate suggests that a \$1 increase in Title I per pupil leads to a 20-cent decrease in local revenue per pupil, with a S.E. of 0.34. The federal revenue response is significantly positive and insignificantly different from one.²³

Next, Table 5 presents results for the impact of changes in Title I revenue per pupil on instructional spending.²⁴ 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. Table 5 shows that instructional spending (about 60% of total expenditures for the mean district) changes about dollar for dollar (a coefficient of 1.40 with a S.E. of 0.55, which is significantly positive and insignificantly different from one) with Title I. This point estimate is consistent with school district administrators wanting to

²³ 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.

²⁴ 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.

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 43-cent *cut* in support services, statistically significant at the 10% level. This cut makes sense if districts are looking to other potential revenue sources to supplement Title I gains to allow for particular instructional expenditures. Title I revenue is positively related with capital expenditures, but not statistically significantly so.

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. Popular reported uses of new Title I funds are class-size reduction, adding aides to classrooms, purchasing instructional materials, and initiating staff development, pre-school, and before- and after-school programs. Similarly, Title I losses prompt reported cuts in spending: administrators describe purchasing fewer new instructional materials, cutting back on staff development, and reducing staff through attrition. 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 would yield a drop in spending despite no loss of programs publicly attributed to Title I, making Title I analogous to an unfunded mandate for particular visible and popular programs.

6.2. Longer-run responses to census-determined 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 2 years after the census updating, a \$1 increase in Title I caused a local revenue decline of 95 cents, and an increase in total revenue of 54 cents. In Column 3, we see that by 3 years after the census updating, a \$1 increase in Title I was associated with an insignificant 4-cent decline in total revenue. This is because over time, local (but not state) revenue responds more negatively to Title I increases, falling about dollar for dollar (the coefficient is 1.21), with Title I gains over the 3-year period.

While the response of total state revenue remained positive but insignificant over the 3-year period, rising 25 cents with each dollar of Title I, it is interesting to note that this results from significant and opposing changes in formula and categorical aid. Formula aid declined

Table 6
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)
<i>Revenue</i>			
Total revenue	0.683 (0.484)	0.515 (0.973)	$F=0.02$ $P=0.878$
State revenue	0.123 (0.476)	0.924 (0.809)	$F=0.73$ $P=0.394$
Formula aid	- 1.113*** (0.396)	0.880 (1.032)	$F=3.25$ $P=0.071$
Categorical aid	1.237*** (0.391)	0.044 (0.440)	$F=4.11$ $P=0.043$
Local revenue	- 0.527* (0.294)	- 1.010 (0.741)	$F=0.37$ $P=0.545$
Federal revenue	1.086*** (0.170)	0.601** (0.278)	$F=2.21$ $P=0.137$
<i>Expenditures</i>			
Instructional spending	0.214 (0.195)	1.486 (1.129)	$F=1.23$ $P=0.267$
Support services	- 0.283 (0.186)	0.539 (0.590)	$F=1.77$ $P=0.184$

***, **, 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 S.E. are in parentheses. All amounts are in real 1992 SY dollars.

58 cents on the Title I dollar, while categorical aid rose 83 cents with each dollar of Title I. This 83-cent increase is statistically significant at the 1% level, and is unsurprising if states use any census data in their allocations.²⁵ Any inclusion of census data in the categorical formulas could yield the observed result. 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.

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 3-year change, a \$1 increase in Title I per pupil caused only an insignificant 12-cent increase in instructional spending per pupil. (Note that the significant large 1-year estimate and the insignificant smaller 3-year estimate have similar S.E.) The significant cuts in support services in the 1-year results have decreased in magnitude and become insignificant over the 3-year period. Again, effects on capital outlays are insignificantly positive.

6.3. Effects of aggregate state-level changes in Title I

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

²⁵ 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. These student characteristics can affect formula aid in addition to or instead of categorical aid; this varies by state (see Gold et al. (1995) for more on State school finance programs).

Table 7

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

	“Pre” period change
<i>Revenue</i>	
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)
<i>Expenditures</i>	
Instructional spending	– 0.327 (0.261)
Support services	0.121 (0.184)

***, **, 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 S.E. are in parentheses. All amounts are in real 1992 SY dollars.

could certainly 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 6 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 \$1 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.

6.4. Specification tests

For further evidence that my estimates can be interpreted causally, 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, I control for pre-existing *state-level* trends in spending per pupil from 1986 to 1990.²⁶

Table 7 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 trends 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 S.E. of 0.37, controlling for state trends.

While the 1992–1993 Title I change does have predictive power in the preceding period for local revenue, the direction of this effect suggests that any pre-existing trend is swamped by Title I, rather than the Title I effect being driven by a pre-existing trend. A district receiving a \$1 increase in Title I would have experienced a 78-cent *increase* in local revenue in the *previous* year (with a S.E. of 0.45); when controlling for district-level trends in the later period (rather than state-level trends in the “pre” period), however, local revenue *falls*.

7. 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-determined 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 3 years, I can examine the dynamics of the flypaper effect. A 1-year analysis of these data would suggest that the grant is quite sticky, while the 3-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

²⁶ While district-level trends would be a preferable control to state-level trends, district-level spending per pupil is available only every 5 years before the beginning of the “pre” period. The end of the trend period, the 1991 school year, is the start of the “pre” period, causing a mechanical correlation between the trend period and the “pre” period.

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 changes in Title I at state and local levels. Because the identification relies on the discrete change in funding surrounding the release of new census data, it limits our interpretation of the estimate. Title I has existed since 1965, and while the average district experienced full crowd-out of the changes studied, it is possible that earlier changes were in fact supplemental.

Important questions about Title I's effects on social welfare remain. First, what form do the crowd-out responses to Title I take, and how do they affect welfare? Does the incidence in benefits or costs from Title I-induced changes in local property tax rates or state and/or local spending patterns favor the disadvantaged population targeted by the program? Second, how are any supplemental Title I funds distributed within school districts and schools? 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. Answers to these questions can provide important guidance in assessing the efficacy of the federal government's targeting of education funds to the poor.

Acknowledgements

I am especially grateful to Caroline Hoxby. I thank Katherine Baicker, David Cutler, Amy Finkelstein, Claudia Goldin, Jonathan Gruber, Brian Jacob, Mireille Jacobson, Christopher Jencks, Lawrence Katz, Sarah Reber, Janice Seinfeld, Tara Watson, and two referees for their suggestions, as well as seminar participants at Harvard, UCSD, RAND, UC Santa Cruz, the Harris School, the New York Fed, Georgetown Public Policy Institute, Berkeley, UT Dallas, UCLA, UC Davis, and the NBER Government Expenditure Universities' Research Conference. Paul Brown of the US Department of Education generously provided administrative data and detailed explanation of the Title I formula. Any remaining errors are my own. I am grateful for support from the Spencer Foundation and the American Educational Research Association, neither of which are responsible for the contents of this paper.

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

	1-year change, 1993–1994 (1)	2-year change, 1993–1995 (2)	3-year change, 1993–1996 (3)
<i>Revenue</i>			
Total revenue	0.882*** (0.317)	1.099*** (0.345)	0.563* (0.342)
State revenue	0.248 (0.187)	1.180*** (0.386)	0.711** (0.325)
Formula aid	0.028 (0.218)	1.111* (0.441)	0.111 (0.227)
Categorical aid	0.220 (0.158)	0.069 (0.242)	0.600*** (0.220)
Local revenue	–0.155 (0.264)	–1.095*** (0.374)	–1.231*** (0.238)
Federal revenue	0.789*** (0.095)	1.043*** (0.074)	1.082*** (0.079)
<i>Expenditures</i>			
Instructional spending	0.994** (0.473)	0.955** (0.401)	0.330 (0.348)
Support services	–0.073 (0.177)	0.118 (0.195)	–0.052 (0.142)
Capital outlays	–0.055 (0.218)	0.016 (0.309)	–0.008 (0.274)

***, **, 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 S.E. are in parentheses. All amounts are in real 1992 SY dollars. Two- and three-year changes exclude Michigan.

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