Schools and Stimulus*

Bill Dupor† and M. Saif Mehkari‡

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Abstract

This paper analyzes the impact of the education funding component of the American Recovery and Reinvestment Act of 2009 (the Recovery Act) on public school districts. We use cross-sectional differences in district-level Recovery Act funding to investigate the program's impact on staffing, expenditures and debt accumulation. To achieve identification, we use exogenous variation across districts in the allocations of Recovery Act funds for students with special needs. We estimate that $1 million in grants to a district had the following effects: expenditures increased by $570 thousand, district employment saw little or no change, and an additional $370 thousand in debt was accumulated. Moreover, 70% of the increase in expenditures was in the form of capital outlays. N ext, we build a dynamic, decision-theoretic model of a school district's budgeting problem, which we calibrate to district-level expenditure and staffing data. The model can qualitatively match the employment and capital expenditure responses from our regressions. We also use the model to conduct policy experiments.

Keywords: fiscal policy, K-12 education, the American Recovery and Reinvestment Act of 2009.

JEL Codes: D21, D24, E52, E62.

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†Federal Reserve Bank of St. Louis, william.d.dupor@stls.frb.org, billdupor@gmail.com.

‡University of Richmond, smehkari@richmond.edu.

1 Introduction

The American Recovery and Reinvestment Act of 2009 (the Recovery Act) was signed into law with a primary goal of creating and saving millions of jobs during and following the 2007-2009 recession. A large share of the appropriations from the act consisted of grants. Public school districts constituted one of the largest groups of these recipients, receiving $64.7 billion in Department of Education Recovery Act funds.[1,2]

The act's education component has been touted as one of the success stories by the law's supporters. Shortly after its passage, Vice President Joe Biden stated that funds from the act would "help to keep outstanding teachers in America's schools."[3] According to the Executive Office of the President of the United States (2009), "the rapid distribution of SFSF [State Fiscal Stabilization Funds] funding helped fill the gaps and avert layoffs of essential personnel in school districts and universities across the nation." The act's official website, Recovery.gov, used surveys of recipient organizations to track the number of jobs payrolled by the act's funds. The Council of Economic Advisers (various quarterly reports) used the jobs counts data from these surveys as evidence of the act's success.[4] According to these reports, Department of Education Recovery Act dollars *alone* directly created and saved over 750 thousand jobs during the first two school years following its passage.[5]

This paper analyzes the act's impact on schools using cross-sectional differences in district-level Recovery Act grants and expenditures, staffing and debt accumulation. We compare the behavior of districts receiving relatively little grant money with those of districts receiving generous grants. From this comparison, we infer what districts would have done without the grants.

To address the potential endogenity of spending, we employ two instruments. Our first instrument is the ratio of the number of special-needs students relative to the total number of students in each district. Our second instrument is the Recovery Act dollars received by a district through the act's Special Education Fund (SEF). The SEF was one category of the Recovery Act education component and constituted one-fifth of the education grants. Its allocation across districts was determined primarily by the requirement that districts finance their special-needs programs. Although each instrument is highly correlated with overall Recovery Act education spending, each is

[1]This includes the Office of Special Education and Rehabilitative Services Special Education Fund ($12.2 billion) and the following Office of Elementary and Secondary Education programs: Education Stabilization funds ($42.0 billion), Compensatory Education for the Disadvantaged ($12.4 billion) and the School Improvement Program ($0.7 billion).

[2]The federal government's objectives for the each of the programs were explicit and usually involved, in part, an attempt to stimulate economic activity. For example, recipients were advised that "Among other things, the Education Stabilization funds may be used for activities such as: paying the salaries of administrators, teachers, and support staff; purchasing textbooks, computers, and other equipment," according to a U.S. Department of Education (2009a) implementation guidance.

[3]See Biden (2011).

[4]See also Congressional Budget Office (various quarterly reports).

[5]See Table A.1 for a quarterly breakdown of the payroll count data extracted from Recovery.gov. Here, a job is measured as lasting one year and as a "full-time equivalent" of one respective position.

plausibly uncorrelated with the short-run business cycle and tax revenue situations faced by school districts.

We have four main findings. First, the grants had either no or only a small impact on education jobs. Each \$1 million of aid to a district resulted in roughly 1.5 additional jobs within that district. The point estimate implies that, in the first two school years following its passage, the act increased education employment by 95,000 persons nationwide. Moreover, this estimate is not statistically different from zero.

We find no evidence that the grants increased the number of classroom teachers. Intuitively, district administrators may have shown a strong preference for maintaining teacher/student ratios and, to a lesser extent, staff/student ratios. As such, school officials may have found margins other than firing or hiring with which to cover shortfalls or spend surpluses.

Second, each \$1 million of grants to a district increased its expenditures by \$570 thousand. Because districts already had substantial funds from local and state sources, the additional Recovery Act funds were effectively fungible. Thus, upon receipt of Recovery Act funds, state and local funding sources may have reduced their own contributions to district funding, thereby offsetting the act's grants.

Third, districts that received grants tended to accumulate more debt. Fourth, roughly 70% of the spending increases were capital expenditures; that is, construction and purchases of land, existing structures and equipment. Why might districts have used these funds for capital improvement? Since this aid was temporary, school districts may have smoothed the benefits of the aid over time by making long-lived physical investments. In Section 4, we build and calibrate a model of dynamic decision making by a forward-looking school district. We show that the small employment effect and relatively large investment effect fall out of a fully specified and realistic dynamic programming problem.

We also use our theoretical model as a laboratory to understand the effect of different types of policy. Our main finding is that forcing school districts to use all the stimulus money on labor has no additional effect on the employment outcome. School districts that are forced to use stimulus money only on employment reduce their labor spending from state and local funding sources and substitute this shortfall with stimulus money, leaving the net employment outcome unchanged. We show that an alternative policy requiring districts to spend most of their revenue (from both stimulus and state and local sources) has a more significant effect on employment.

With respect to existing work, there is little economic research on the act's education component. Two exceptions are Dinerstein, et.al. (2013), who study the impact of the act on universities, and Chakrabart and Setren (2011), who examine the impact of the recession and the early part of the Recovery Act on school districts in the state of New York. More generally, other studies using microeconomic evidence that study the overall impact of the Recovery Act have focused mainly on economy-wide labor market outcomes. These include Chodorow-Reich et al. (2012), Conley and

Dupor (2013), Dupor and McCrory (2015), Feyrer and Sacerdote (2012) and Wilson (2012).

Another line of research studies how federal grants to schools influence school spending. Gordon (2004) studies the impact of additional federal grants to school districts serving economically disadvantaged children through the No Child Left Behind Act of 2001. She finds that, although the additional federal grants initially caused a dollar-for-dollar increase in school spending, over time school districts offset those increases with reductions in their own contributions to education funding.

Lundqvist, Dahlberg and Mörk (2014) study the impact of intergovernmental grants to local governments in Sweden and find that the grants do not stimulate local public employment. Evans and Owens (2007) study the extent to which federal grants to fund new police hires increased the size of local police forces versus simply supplanting local funding. They found that for every four officers payrolled by a grant, in an accounting sense, a police force actually increased by only a little over two officers.

2 Empirical Analysis

2.1 The Data

The Sample

Our unit of observation is a public school district.[6] During the 2010 school year (SY), there were 16,117 such districts in the United States. We restrict our attention to districts with more than 500 student during that year. We also exclude districts missing requisite data, which leaves 6,786 districts.[7]

Outcome Variables (ΔJob-Years, ΔExpenditures and Debt accumulation)

Our first outcome variable measures school district employment. It is the change in employment from a base of the 2007SY over the first two school years in which the act was fully in effect: i.e. the 2009SY and the 2010SY.[8] Employed persons include teachers, aides, guidance counselors, librarians, district administrators and other support staff. The data are self-reported by school districts in the annual Common Core of Data Local Education Agency Universe Survey.

[6] Our usage of the term "school district" is synonymous with the term "local education agency" (LEA) used in the education policy area. In the education policy jargon, our sample is made up of school districts and a small number of regional educational service agencies.

[7] For example, we were forced to exclude data from all districts in Iowa, Montana, New Hampshire, Pennsylvania and Vermont because the Recovery Act spending information was reported in a manner that did not allow us to match the Recovery Act grants to school district spending and employment variables. We also excluded Hawaii because the entire state is a single school district.

[8] We exclude the 2008SY because it includes only a few months during which the Recovery Act was in effect.

Let $Y_{j,k}$ denote employment by district j during school year k. Then,

$$\Delta\text{Job-years}_j = \frac{1}{\text{Pop}_j}\sum_{k=2009}^{2010}(Y_{j,k} - Y_{j,2007})$$

where Pop_j is the district j enrollment in the 2007SY.

Our data on total expenditures $S_{j,t}$ and debt are from the annual Local Education Agency Finance Survey. From these variables, we calculate our next two outcome variables. We measure expenditure as the per student cumulative spending in the 2009SY and 2010SY relative to a pre-act baseline:

$$\Delta\text{Expenditure}_j = \frac{1}{\text{Pop}_j}\sum_{k=2009}^{2010}(S_{j,k} - S_{j,2007})$$

Debt accumulation is the change in the per student debt of a district over the two school years following the act's passage.

$$\text{Debt accum}_j = \frac{1}{\text{Pop}_j}(\text{End of 2010SY Debt}_j - \text{End of 2008SY Debt}_j)$$

Treatment Variable (V)

First, let $\tilde{V}_j$ be the Recovery Act dollars outlaid to school district j, from the time of enactment through 2011Q2.[9] Outlaid dollars are defined as dollars paid by the federal government to a recipient organization. These amounts are constructed using quarterly reports filed by recipients to the website FederalReporting.gov.[10] Finally, we scale by the district enrollment and report values in millions of dollars:

$$V_j = \frac{\tilde{V}_j}{(1e+6)\times \text{Pop}_j}.$$

Nearly all of the education dollars authorized by the act were outlaid by the end of 2011Q2.

Instrument Variables (V^{SN} and V^{SEF})

Since the allocation of the act's school funding was perhaps in part endogenous, we use instrumental variables. We have two instruments. Our first instrument is the per student value of special education funding outlaid as part of the Recovery Act, defined as V_j^{SEF}, through 2011Q2.

The main channel through which the federal government supports special education is the Individuals with Disabilities Education Act (IDEA), a comprehensive statute originally passed in 1990 to ensure all students with disabilities are entitled to a free appropriate public education. Most of the Recovery Act special education money was tied to the IDEA program. While there

[9] We use outlays through 2011Q2 because this aligns our Recovery Act data sample with the end of the 2010 school year.

[10] After processing and data verification by the Recovery Accountability and Transparency Board, these data were posted on the website Recovery.gov. A user's guide for these data is contained in Recovery Accountability and Transparency Board (2009).

are several subprograms within IDEA, the lion's share of funding comes through Part B of IDEA. The Recovery Act funding formula follows the IDEA Part B formula.[11]

Recovery Act IDEA Part B grants were add-ons to regular annual IDEA Part B grants to states. The national federal fiscal year (FFY) 2009 regular grant amount was \$11.5 billion. The first \$3.1 billion (from both regular funding and the Recovery Act add-on) was divided among states so they were guaranteed to receive their FFY1999 awards. Once this requirement was met, the remaining part of the national award was allocated among the states according to the following rule: "85% are allocated to States on the basis of their relative populations of children aged 3 through 21 who are the same age as children with disabilities for whom the State ensures the availability of a free appropriate public education (FAPE) and 15% on the relative populations of children of those ages who are living in poverty."[12] The Recovery Act add-on totaled \$11.3 billion. Since, at the margin, the FFY1999 requirements had already been met by the regular awards, every Recovery Act dollar was in effect assigned according to the 85/15 percent rule.

Next and importantly, we address how funds were assigned from state education agencies to local education agencies (LEAs). These initial allocations too were made at the federal level. Each LEA was first allocated a minimum of its FFY1999 award.[13] Beyond these minimums, which were already met by the regular annual award amounts, a slightly different 85/15 rule was used. Within each state, 85% of the dollars was allocated according to the share of school-age children in the LEA and 15% was allocated according to the LEA's childhood poverty rate. After meeting these stipulations, states were allowed to reallocate funds as explained below. Before we explain how reallocations worked, we ask whether the observed spending data at the within-state level are explained by the simple formulary rule.

Let $P_{j,s}$ and $\tilde{P}_{j,s}$ be the enrollment of students and students in poverty, respectively, in district j and state s. Let $IDEA_{j,s}$ denote the total Recovery Act special-needs funding in district j in state s. Based on the above formula, the distribution of Recovery Act IDEA funds would be

$$IDEA_{j,s} = \left(0.85 \times \frac{P_{j,s}}{\sum_{i=1}^{N_s} P_{i,s}} + 0.15 \times \frac{\tilde{P}_{j,s}}{\sum_{i=1}^{N_s} \tilde{P}_{i,s}} \right) IDEA_s$$

Letting P_s and $\tilde{P}_s$ denote the sum within state s of the two district-level enrollment variables, we can rewrite the above equation as

$$\frac{IDEA_{j,s}}{P_{j,s}} = \left[0.85 \times \frac{1}{P_s} + 0.15 \times \frac{1}{\tilde{P}_s} \left(\frac{\tilde{P}_{j,s}}{P_{j,s}} \right) \right] IDEA_s$$

[11] See U.S. Department of Education (2009b) and New America Foundation (2014).

[12] See Enclosure B of U.S. Department of Education (2009b), which contains a description of the allocation of Recovery Act funds across states.

[13] Federal code also describes how minimum awards are determined for LEAs created after 1999.

Thus, within each state, the district-level per pupil IDEA amount would be perfectly predicted by the ratio of the low-income enrollment to the overall enrollment in the district if the simple formula were used. Next, we run state-level regressions to check this conjecture for the 46 states for which we have fully reported IDEA amounts. The R^2 values from these regressions are generally very low: twenty-five values are less than 0.01. Only six of the R^2 are greater than 0.1 and only one is greater than 0.3.[14] This tells us that other factors besides the poverty rate in each district are influencing the allocation of IDEA funds.

This brings us to the rules for redistribution of dollars within state across LEAs, given by Code of Federal Regulation 300.707(c)(1). It states:

> If an SEA determines that an LEA is adequately providing FAPE to all children with disabilities residing in the area served by that agency with State and local funds, the SEA may reallocate any portion of the funds under this part ... to other LEAs in the State that [are] not adequately providing special education and related services to all children with disabilities residing in the area served by those LEAs.

Based on the legislation and given the low set of R^2 values above, we conclude that the primary reason that IDEA money was allocated differently from the formulary rule is that some states were able to meet their funding requirements of special-needs students in some districts without drawing on Recovery Act IDEA funds. Those funds were then reallocated to districts with additional funding requirements for special needs students. Differences in funding requirements across districts were likely due to factors such as the number of special needs students, the types of disabilities and their associated costs and the districts' own funding contributions for providing services to these special-needs students. Our exogeneity assumption is that this set of factors driving redistributions of IDEA funds is orthogonal to the error term in second-stage equation.

Our second instrument is the ratio of the number of special needs students within a district relative to the overall student enrollment in that district in 2007.[15] Denote this variable as V_j^{SN}. While the fraction of special needs students in a school district is likely to affect the Recovery Act funding that a district receives, it is plausibly uncorrelated with the business cycle conditions and tax revenue stress that the district faced.

Conditioning Variables (X)

We include the following conditioning variables, which we partition into three types:

- *Pre-recession education variables*: the 2007SY values of the teacher/student ratio, staff/student ratio, expenditure per pupil; the change in debt per pupil over the 2007SY;

[14] As an additional measure, we include the poverty rate as an additional control in our estimation.

[15] These data are also from the Common Core of Data Universe Survey. As the data documentation explains, special-needs students are defined as "all students having a written Individualized Education Program (IEP) under the Individuals with Disabilities Act (IDEA), Part B."

Table 1: Summary statistics

Variable	Mean	SD	10^{th} perc.	90^{th} perc.
Change in total revenue (pp)	838.85	3186.31	-1778.23	3635.67
Change in expenditure (pp)	689.81	5140.74	-3492.91	4976.10
Recovery Act education spending (pp)	1013.20	766.98	446.04	1569.25
Recovery Act IDEA spending (pp)	178.48	480.00	0.00	288.82
Change in the wage bill (pp)	642.68	1397.71	-926.05	2256.92
Change in the number of job-years (pp)	-0.00	0.03	-0.03	0.02
Debt accumulation (pp)	59.69	7443.66	-2381.30	2984.66
Log of enrollment	7.83	1.09	6.55	9.32
SY2007 values of:				
Number of teachers (pp)	0.06	0.01	0.05	0.08
Number of staff (pp)	0.12	0.03	0.08	0.16
End of school year debt (pp)†	10.88	2.99	8.23	14.40
One-year debt change (pp)	3653.55	30046.28	-3000.00	9662.00
Minority rate	0.24	0.27	0.02	0.69
Poverty rate	0.03	0.02	0.01	0.05
Self-sufficiency ratio	0.41	0.20	0.19	0.71
Total Recovery Act education spending = \$36 billion				
Total Recovery Act IDEA spending = \$7 billion				
Number of observations = 6,786				

Notes: The unit of observation is a U.S. school district. The above sample excludes districts with enrollments less than 500 in the 2010SY. † denotes a variable has been divided by 1000. IDEA, Individuals with Disabilities Education Act; perc., percentile; SD, standard deviation; pp, per pupil.

- *Nonfinancial variables*: the ratio of African American plus Hispanic enrollment to total enrollment, the natural log of enrollment, 7 regional dummy variables, a constant; and
- *School district financials*: the poverty rate, the fraction of revenue from local sources, the cumulative change in revenue from nonfederal sources.

Details regarding a few of these variables are in order. The poverty rate is the number of young persons living in poverty relative to the total population of persons living within each school district's borders. The change in revenue from nonfederal sources variable is given by

$$\frac{1}{\text{Pop}_j} \sum_{k=2008}^{2010} \left(R_{j,k}^{nonfed} - R_{j,2007}^{nonfed} \right)$$

where $R_{j,k}^{nonfed}$ is the district j revenue from nonfederal sources in school year k. The primary nonfederal sources are from within the district and the state government.

Table 1 provides summary statistics for the variables in our analysis.

2.2 The econometric model

We use two-stage least-squares regression for our estimations. The statistical model for the ΔJob-years equation is

$$\begin{aligned} V_j &= \theta_1 V_j^{SEF} + \theta_2 V_j^{SN} + \psi X_j + v_j \\ \Delta\text{Job-years}_j &= \beta_{JY}\hat{V}_j + \gamma X_j + \varepsilon_j \end{aligned} \tag{2.1}$$

where $\hat{V}_j$ are the fitted values from the first-stage regression. The parameter of interest is β_{JY}. The statistical model for the other two outcome variables simply replaces $\Delta\text{Job-years}_j$ with $\Delta\text{Expenditure}_j$ or Debt Accum_j. Our estimates are weighted by district enrollment and we report robust standard errors (SEs).

3 Results

3.1 Benchmark results

The employment effect

Table 2 contains our benchmark estimates. Column (i) shows the job-years response to grants. The coefficient on education spending equals 1.47 (SE = 1.32): Every $1 million in grants increased district employment by 1.47 relative to a no-Recovery Act baseline. Note that our construction of the outcome variable is such that one job should be interpreted as lasting one year. This estimate is not statistically different from zero, but is estimated sufficiently precisely to conclude that the jobs effect was small at best. At the upper end of the 95% confidence interval, the employment effect was 4.05 persons per million dollars spent. We view this as quantitatively small, bearing in mind that the average education industry wage was roughly $50,000 during this period.[16] The estimates for other outcome variables (presented below), elucidate two reasons why there was a small, if any, education jobs effect. First, a large portion of the grants did not translate into greater district-level expenditures. Second, district-level expenditures that did arise from the grants were used mainly for capital expenditures.

Next, using the job-years response estimate, we calculate the implied total number of education job-years resulting from the act's education component. Taken at the upper end of its 95% confidence interval, our estimate is that the effect was 260,000 jobs.[17] As explained in the introduction, this is substantially lower than the corresponding number based on the payroll count data reported

[16]The mean annual wage for U.S. workers in the "Education, Training and Library" occupation was $49,530 in 2009.

[17]We calculate this number by multiplying the 95% upper bound of the job-years coefficient confidence interval by the cumulative total Recovery Act education spending through the 2010SY. This calculation assumes that the treatment effect is the same for districts within our sample as for those excluded from the sample.

Table 2: Estimates of the impact on staff employment, expenditures and debt accumulation in $1 million of Recovery Act education grants, benchmark results

	Δ Job-years (i)	Δ Expenditure (ii)	Debt accum. (iii)
Recovery Act education grant	1.47	570.10***	340.63*
($1 mil)	(1.32)	(196.60)	(185.12)
Ln(population)	0.04	148.93**	-38.88
	(0.55)	(64.57)	(97.72)
Minority ratio	-0.02***	1.47***	0.77**
	(0.00)	(0.36)	(0.39)
Poverty rate	-0.03	-15.22***	7.49
	(0.04)	(5.61)	(10.62)
Nonfederal spending change	1.62***	632.00***	78.36*
	(0.23)	(45.11)	(44.11)
Self-supporting school	-0.01***	-1.21**	-0.72
district	(0.00)	(0.60)	(0.68)
Teachers per pupil, lag	-0.05	67.66***	27.08**
	(0.15)	(9.41)	(12.92)
Staff per pupil, lag	-0.36***	5.49	-12.39**
	(0.04)	(3.67)	(5.13)
Total expenditure per pupil,	0.00***	-0.00***	0.00
lag	(0.00)	(0.00)	(0.00)
Debt change, lag	-0.00***	-0.00	0.00
	(0.00)	(0.00)	(0.00)
Region dummies	Yes	Yes	Yes
No. of Observations	6786	6786	6786
First stage results			
Special Ed. ratio (t-stat)	4.29	4.29	4.29
IDEA Recovery Act aid (t-stat)	10.33	10.33	10.33
Partial F-stat	78.93	78.93	78.93

Notes: Each estimation also includes additional conditioning variables described in the text. The regressions are enrollment weighted. Standard errors appear in parentheses. *** denotes 1% , ** 5% and * 10% statistical significance. The expenditure and debt accumulation variables are in units of thousands of dollars.

at Recovery.gov.[18]

The bottom rows of Table 2 report key statistics from the first-stage regressions. The first-stage results indicate that we have two strong instruments. The partial F-statistic is 78.93, with a pointwise t-statistic of 4.29 for the special education student ratio instrument and 10.33 for the Special Education Funds instrument.

Our jobs effect finding raises the question: Why were so few, if any, education jobs created as a result of the act? One possibility is that district administrators viewed their staff, particularly teachers, as so important to their mission that districts receiving relatively little aid found ways to close budget gaps without firing many staff. Also, districts that received relatively generous Recovery Act grants may have been less willing to hire new staff for risk that, once the short-lived grants were spent, the new staff would need to be laid off. Adjusting the capital outlays was an alternative way to spend grant dollars. We provide empirical evidence of and theoretical justification for a capital outlay response later in the paper.

If neither districts receiving large grants nor those receiving small grants significantly adjusted their staff levels in response to the shock, then we should expect our instrumental variables (IV) estimates to reflect a small jobs effect. An absence of significant changes in staffing levels is consistent with narrative descriptions of districts' responses to the most recent recession. Cavanaugh (2011) explains that school officials initially responded to budget stress caused by the recession "at the periphery," e.g., cutting travel, delaying equipment upgrades as well as scaling back extracurricular activities and art and music programs. As further evidence, based on surveys of school administrators, the AASA (2012) lists many ways that school administrators filled budget gaps during the period without firing employees. These include furloughing personnel, eliminating or delaying instructional improvement initiatives, deferring textbook purchases and reducing high-cost course offerings. While each of these strategies may have marginally reduced the quality of education services provided by the schools, the changes did not directly affect the total number of district employees.

Note that if jobs were created outside the district, perhaps because of a "Keynesian multiplier" effect, this is not reflected in our estimates because we examine only school district employment.

The expenditure effect

Column (ii) of Table 2 reports estimates for the ΔExpenditure specification. The point estimate on Recovery Act education spending equals 570,000 (SE = 197,000). This implies that \$1 million in education grants resulted in an increase in expenditures of approximately \$570,000 over the first two full school years following the act's passage. Thus, only about half of the aid to a district actually translated into more expenditures in that district. One explanation for this result may be that there was substantial "crowding out" of contributions by local and state governments' to public education when school districts received Recovery Act dollars.

[18]See Table A.1 for a tabulation of the Council of Economic Advisers payroll count data.

This finding relates to previous research on whether federal grants crowd out state and local spending. In a simple political economy model, Bradford and Oates (1971) show conditions under which crowding out occurs. Leduc and Wilson (2014) present evidence that crowding out was not an issue for the highway component of the Recovery Act.

The debt accumulation effect

Column (iii) of Table 2 presents the results with debt accumulation per pupil over the two years following the act's passage as the outcome variable. The point estimate on the Recovery Act spending variable is 340,000 (SE = 185,000). Based on the point estimate, districts that received relatively more aid tended to increase their debt positions. The estimate is statistically different from zero, but only at a 10% level.

3.2 Additional results

Table 3 gives the responses of the outcome variables for several variations on the benchmark specification. Panels A and B provide the weighted and unweighted specifications, respectively. The first row contains the benchmark estimates. The "Ordinary least squares" row is identical to the benchmark specification except we estimate via OLS rather than instrumental variables. The next two rows estimate the model for each instrument separately. The final two rows sequentially drop the region dummies and then drop all lagged variables.

Column (i) of Table 3 presents the job years estimates for all of the alternative specifications. The majority of estimates are close to the benchmark one. There are three things worth noting. First, not weighting by enrollment has very little effect on the estimate. Second, the OLS estimate is very similar to our benchmark IV case. This suggests that the endogeniety problem is not severe in this case.

Third, instrumenting with only the special education ratio generates a substantial increase in the jobs as well as the expenditure effect relative to the benchmark specification. The job years estimate increases to 8.04 (SE = 7.36). Note that we are unable to reject a zero jobs effect for this specification. This specification results in the strongest jobs and expenditure effects of all of the alternative estimated models. Interestingly, the large jobs and expenditure effects are diminished substantially in the corresponding unweighted estimates (see panel B).

Column (ii) of Table 3 presents the total expenditure estimates. (Recall that the coefficient is interpreted as the thousands of dollars by which expenditures increase for a $1 million Recovery Act education grant to the district.) Thus, if the value is less than 1,000, then there is some crowding out of grants because part of the aid is not passing through to expenditures. The majority of estimates are close to the benchmark estimate and exhibit substantial crowding out.

Column (iii) of Table 3 presents the debt accumulation estimates. The benchmark estimate shows a statistically significant positive effect. All of the alternative specifications have a positive point estimate, with roughly one-half being statistically different from zero. The only outliers are

Table 3: Estimates of the impact on job-years, total expenditure and debt accumulation of $1 million in Recovery Act education funding, alternative specifications

	Δ Job years	Δ Expenditure	Debt accum.	1st stage partial F-statistics
	(i)	(ii)	(iii)	(iv)
Panel A—Weighted by Enrollment				
Benchmark	1.47	570.10***	340.63*	79
	(1.32)	(196.60)	(185.12)	
Ordinary least squares	2.11**	165.46	30.25	N/A
	(1.00)	(116.72)	(242.32)	
IDEA instrument only	1.39	524.99***	229.73	100
	(1.32)	(190.14)	(181.77)	
Special ed ratio instrument only	8.04	2,339.31***	3,977.68***	30
	(7.36)	(815.61)	(1,352.86)	
Drop region dummies	1.28	621.54***	232.14	89
	(1.21)	(209.81)	(195.77)	
Drop all lagged variables	0.97	216.97**	388.78**	78
	(1.07)	(105.19)	(186.16)	
Panel B—Unweighted Results				
Benchmark	0.12	346.08***	461.15***	540
	(0.63)	(125.01)	(164.84)	
Ordinary least squares	-0.32	60.45	199.82	N/A
	(0.37)	(97.03)	(132.80)	
IDEA instrument only	0.18	349.87***	401.69	1,027
	(0.68)	(127.50)	(159.89)	
Special ed ratio instrument only	3.29	772.27	8,515.06***	31
	(0.24)	(994.30)	(2,437.75)	
Drop region dummies	0.09	323.39***	588.84***	572
	(0.53)	(125.15)	(195.27)	
Drop all lagged variables	-0.19	145.13**	463.07***	528
	(0.24)	(66.51)	(163.62)	

Notes: Each estimation includes the conditioning variables described in the text. Standard errors are listed in parentheses. *** denotes 1% , ** 5% and * 10% statistical significance.

Table 4: Estimates of the impact on staff employment of $1 million in Recovery Act education grants, by job type

	Δ Teacher-JY (i)	Δ Non Teacher-JY (ii)
Recovery Act education	-0.03	1.50
spending per pupil	(0.51)	(0.99)
Full Controls	Yes	Yes
No. of Observations	6786	6786
Partial F-stat	78.93	78.93

Notes: Each estimation includes the benchmark conditioning variables described in the text. The regressions are enrollment weighted. Standard errors are listed in parentheses. *** denotes 1% , ** 5% and * 10% statistical significance.

the "Special education ratio instrument only" cases, both weighted and unweighted. The point estimates for these specifications jump to $4.0 million and $8.5 million, respectively. We view these values as implausibly large.

Column (iv) of Table 3 contains the partial F-statistic for each specification. None of the values indicate a weak instrument problem, although the statistic is dramatically lower for the "Special education ratio instrument only" specifications.

Next, we consider the type of education jobs affected. Did the grants create and save teachers' jobs or those of other employees? Table 4 presents the estimates for the benchmark specification, except we estimate the equation separately for the change in the number of teaching and nonteaching employees.

Column (i) of Table 4 shows that there was no statistically significant effect on the number of teacher jobs created/saved. The point estimate equals -0.03 (SE = 0.51). District administrators may have sought, as a top priority, to maintain class sizes at their pre-recession levels. This constancy may have been achieved by neither hiring nor firing teachers on net.

The employment effect came through nonteaching jobs. As seen in column (ii), each $1 million resulted in 1.50 (SE = 0.99) additional job-years of nonteaching employment, although this too is not statistically different from zero.

Next, Table 5 examines the categories of spending that account for most of the effect on total expenditures. In columns (ii) through (iv), we estimate the benchmark model except we in turn replace the change in total expenditures with the change in a component of total expenditures.

Column (ii) shows that Recovery Act aid had a substantial effect on capital outlays. Roughly 70% of all expenditures came in the form of capital outlays.[19] Why might districts have used so much of their grant money for investments? First, suppose a district seeks to maximize its provision of education services as well as keep those provided services relatively smooth over time,

[19] Capital outlays include construction and purchases of equipment, land and existing structures.

Table 5: Estimates of the impact on expenditure of $1 million of Recovery Act education funding, by major expenditure categories

	Δ Expenditure (i)	Δ Capital (ii)	Δ Salaries (iii)	Δ Benefits (iv)
Recovery Act education spending per pupil	570.10*** (196.60)	390.82*** (149.34)	8.92 (41.23)	79.38* (48.00)
Full Controls	Yes	Yes	Yes	Yes
No. of Observations	6786	6786	6786	6786
Partial F-stat	78.93	78.93	78.93	78.93

Notes: Each estimation includes the conditioning variables described in the text. The regressions are enrollment weighted. Standard errors in parentheses. *** denotes 1% , ** 5% and * 10% significance. Expenditures and debt accumulation variables are in units of thousands of dollars.

in a manner similar to the permanent income model of consumption smoothing. Second, suppose education services are a function of labor (i.e. the number of staff) and capital. In this case, a district that receives a one-time grant may seek to spread the benefits of this grant over many periods by using part of the grant to increase its capital stock.

Likewise, a district that received a relatively small amount of aid may have found that the best way to close budget gaps was to temporarily cut back on capital investment rather than lay off staff. Because the capital stock depreciates slowly, a temporary interruption in investment would likely have only a small effect on the quality of education services that the school could provide.

Recall that earlier, we document that Recovery Act aid tended to increase debt accumulation. This effect may be related to the positive effect of aid on capital expenditure shown in Table 5. Suppose that, upon receipt of Recovery Act funds, a district decided to spend part of its funds on capital, such as construction. The district may have chosen to boost the dollars available for construction by leveraging up the grant aid by borrowing. Under this scenario, had the district attempted to finance the entire capital project only with debt, it may have been unable to secure the funds or be offered a reasonable financing rate. Thus, it is possible that grants may have led to borrowing rather than saving by some districts.

Note that the construction spending itself is likely to have a positive jobs effect because of building contractors the district might hire. These numbers are not reflected in our employment estimate because we restrict attention to school district employees.[20]

Column (iii) of Table 5 reports the impact of aid on salaries, which was small and not statistically different from zero.[21] Since the employment effect was so small, it is not surprising that we do not

[20]Dupor and McCrory (2015) conduct a cross-regional analysis of the act in a broader context than only education. That paper examines employment from all sectors and the act's entire spending component, in contrast to that solely from education. They find a larger jobs effect than that estimated in the current paper.

[21]The salary and benefits variables are constructed in the equivalent manner as the variable for total expenditures was constructed.

recover a substantial wage effect. Column (iv) of Table 5 implies that \$1 million in aid increased benefits paid by the school district by \$79 million.

4 A model of school district hiring and capital decisions

In this section, we study the dynamic optimization problem of a school district facing stochastic revenue shocks.

In the previous section, we found that the ratio of stimulus spending for paying education workers relative to capital investment was 0.25. This may be puzzling since, as we explain below, the long-run average of this ratio equals 8. Second, there was a small effect on nonteacher staffing and no effect on the number of teachers employed. Our model simulations roughly match both of these findings.

Moreover, our model allows us to estimate the medium- and long-run effects of these grants and provides a laboratory to study the effects of alternative hypothetical stimulus programs aimed at schools.

4.1 The stylized facts

We begin by documenting two stylized facts about education spending by analyzing a 17-year panel of district-level data ending with the 2011SY.[22] The facts provide guidance for building and then calibrating our economic model.

Our panel covers a long time span and some of our series contain time trends. As such, we detrend every variable x_t by its aggregate (over districts) gross growth rate between period t and Q, the final period in our sample. The cumulative growth rate is

$$cg_{x,t} = \frac{\sum_{i \in I} x_{i,t}}{\sum_{i \in I} x_{i,Q}}$$

where I is the set of all districts. The detrended district-level variable is then $\tilde{x}_t$ is thus

$$\tilde{x}_{i,t} = \frac{x_{i,t}}{cg_{x,t}}$$

Unless otherwise noted, each variable is scaled by its district enrollment.

Stylized Fact 1: *The teacher/student ratio is less volatile than the nonteacher/student ratio.*

For each district i, we compute the time-series variance of the log deviation of the employment levels of teachers, T, and non-teaching workers, N.[23]

[22]We use the merged Universe and Finance surveys of the Common Core School District dataset. The 1994SY is the first year for which the entire dataset is available. As in the paper's previous section, we drop districts that report less than 500 students.

[23]Nonteaching staff includes instructional aides, guidance counselors, library/media staff, administrative support

$$v_{x,i} = \text{ variance across } t \text{ of } log\left[\frac{\tilde{x}_{i,t}}{\frac{1}{Q}\sum_t \tilde{x}_{i,t}}\right]$$

for $x \in (T, N)$.

Columns (i) and (ii) of Table 6 contain the across-district median values (along with the 10th, 25th, 75th, and 90th percentile values) of $v_{T,i}$ and $v_{N,i}$. Observe that the nonteacher/student ratio is more variable than the teacher/student ratio. The difference in variability ranges from 3 times as high for the 10th percentile, 4 times as high for the median, and over 5 times as high for the 90th percentile.

As further robustness, columns (iii) and (iv) contain the statistics for a smaller subsample that includes data from the most recent 6 years. Whereas the shorter time horizon results in a reduced value of the magnitude of the variance, as in the full sample in this subsample, the teacher/student ratio remains less variable than the nonteacher/student ratio.

Table 6: Volatility of the teacher/student and noneacher/student ratio

	Time-Series Variance of Log Deviations from the Aggregate Trend of the Per Student Ratio			
	(i) Teacher	(ii) Non-Teacher	(iii) Teacher	(iv) Non-Teacher
10th Perc	0.0012	0.0036	0.0004	0.0008
25th Perc	0.0018	0.0062	0.0007	0.0017
Median	0.0033	0.0122	0.0016	0.0038
75th Perc	0.0062	0.0245	0.0039	0.0093
90th Perc	0.0104	0.0555	0.0092	0.0232
Years	1994-2011		2006-2011	
Number of districts	1901		4291	

Stylized Fact 2: *Capital spending is more volatile than labor spending.*

Next, we consider the behavior of two categories of spending: capital expenditure and labor expenditure. Capital expenditure is the sum of spending on construction, land and existing structures (CLS), and equipment with an expected life of 5 or more years. Labor expenditures includes salaries and benefits of district employees.[24] We convert each variable into real terms using the gross domestic product deflator with a base year of 2011.

Table 7 reports the across-district median values (along with the 10th, 25th, 75th, and 90th

staff, and so on.

[24]We exclude services and nondurable good expenditures in our descriptions here. In regression results not provided in the paper (but available on request), we establish that there was a negligible effect of grants on these types of spending. We also exclude debt service payments, payments to other districts and expenditures on non-elementary/secondary programs because they make up only 10% of the average district's spending and our outside of our model.

percentile values) of the time-series volatility of expenditures on total real salary plus benefits and real capital outlays, where each volatility is calculated as the time-series variance of the log deviations of the variable from its aggregate trend using equation (4.1). Note that investment is significantly more variable than labor expenditures. At the median level of variability, capital expenditures are 250 times more variable than expenditures on labor.

Table 7: Volatility of pay/student and capital/student ratios

	Time-Series Variance of Log Deviations from the Aggregate Trend of the Per Student Ratio	
	(i) Salary + Benefits	(ii) All Capital Outlays
10th Perc	0.0013	0.3339
25th Perc	0.0021	0.5860
Median	0.0036	0.9580
75th Perc	0.0064	1.4720
90th Perc	0.01113	2.0509
Dates	1994-2010	
Number of districts	6092	

Table 8: Volatility of the (salary + benefits)/student and the investment/student ratios

	Time-Series Variance of Log Deviations from the Aggregate Trend of the Per Student Ratio		
	(i) Constr./Land/Struc.	(ii) Equipment	(iii) All Capital Outlays
10th Perc	0.5657	0.1141	0.3339
25th Perc	1.0089	0.1861	0.5860
Median	1.7008	0.3255	0.9580
75th Perc	2.6626	0.5898	1.4720
90th Perc	3.9451	1.1045	2.0509
Dates		1994-2010	
Number of districts		6092	

We then divide the capital category into spending of two types: CLS and equipment. Table 8 reports the volatility of these variables. Even though the equipment component itself is volatile, most of the volatility in capital is driven by CLS. This fact, coupled with the facts that CLS makes up roughly 80% of all capital investment and that labor expenditure is not highly volatile, pushes us towards a theory in which districts: (i) tend to use large revenue gains and (ii) make up for revenue shortfalls largely by either investing in, or delaying expenditure on, long-lived capital goods.

4.2 The economic model

Consider a school district that uses an exogenous stream of revenue, R, to hire workers and buy capital to provide education services to its students. Its revenue process is given by the following AR(1) process:

$$R' = \rho R + (1-\rho)\bar{R} + \epsilon_R \text{ with } \epsilon_R \sim \mathcal{N}(0, \sigma_R) \tag{4.1}$$

where $\rho \in (0,1)$ and $\bar{R}$ is fixed. Revenue, as well as other variables in the model, are per pupil.

A district's one-period welfare function is

$$W(T,N,K) = \alpha U\left(T;\xi_T\right) + \gamma U\left(N;\xi_N\right) + \eta U\left(K;\xi_K\right) \tag{4.2}$$

where T, N, and K are the number of teachers, number of nonteachers, and quantity of capital, respectively. Moreover, let $U\left(X;\xi\right) = X^{1-\xi}/\left(1-\xi\right)$.

The district's dynamic optimization problem is given by the following recursive functional equation:

$$V(K;R) = \max_{T,N,I}\left\{W(T,N,K) + \beta E\left[V(K';R')|R\right]\right\}$$

subject to

$$R = w_T T + w_N N + I \tag{4.3}$$

$$K' = (1-\delta)K + I \tag{4.4}$$

and nonnegativity constraints on T, N and K. Also, I represents investment in the capital good, and values with a superscript prime give the next-period realization of that variable. For example, K' gives the next period realization of capital, K.

Next, (4.3) is the district budget constraint, with w_T and w_N representing the teacher wage and nonteacher wage, respectively. Also, equation (4.4) is the capital law of motion, and δ is the capital depreciation rate.

Every period the school district receives revenue which it optimally allocates to: (i) the hiring of teacher and nonteachers, and (ii) capital acquisition. Whereas the number of teachers and nonteachers hired affects only the current period's welfare, the durable nature of capital results in a multiperiod effect. As we discuss in the next section, the dynamics that result from allowing the district to choose a durable input are important for understanding why the 2009 Recovery Act had a small effect on hiring but a large effect on capital outlays.

4.3 Calibration and simulations

Table 9 provides the parameter values for the model. The model period is 1 year. We begin our calibration by setting the discount factor $\beta = 0.96$ to match a 4% annual real interest rate.

Next, in the data, the capital stock is composed of two different basic types: equipment with more than a 5-year lifespan and CLS. CLS account for roughly 75% of the capital outlays and depreciate at a 1.88% annual rate, while equipment accounts for 25% of capital outlays and depreciate at a 15% annual rate.[25] As such, we set $\delta = 0.0516$ $(= 0.75 \times 0.0133 + 0.25 \times 0.16)$.

Across districts, the median wage bill per student is \$8128, for which 48% goes toward teacher pay and 52% goes toward nonteaching staff pay. Teacher compensation per pupil is thus \$3901 and nonteaching staff compensation equals \$4227. The median teacher-student ratio is $1 : 15.5$ and the median number of nonteaching staff per student is $1 : 16$. As a result, we set teacher and nonteacher wage $w_T = \$60472$ $(= 15.5 \times 3901)$ and $w_N = \$67625$ $(= 16 \times 4227)$.

The persistence of the AR(1) revenue process is directly estimated from the data. The median autocorrelation of expenditures is 0.47. The average revenue is set at $\bar{R} = \$8128 + \$988 = \$9116$.

Six parameters remain: The welfare elasticities, ξ_T, ξ_N and ξ_K, the relative shares of teachers, α, and nonteachers, γ, and the standard deviation of the revenue process, σ_R.

First, we set $\xi_K = 1.0$ and then jointly calibrate the remaining five parameters to match the following five targets: The average teacher/student ratio is 0.064; the average nonteacher/student ratio is 0.062; the nonteacher/student ratio is 4 times as volatile as the teacher/student ratio; the average salary volatility is 0.0036; and the average investment volatility 0.95.

Table 9: Parameter Values

Parameter	Value	Description	Explanation
β	0.96	Discount factor	Standard value for annual discount factor
δ	0.0516	Depreciation rate	Calculated using the BEA data on the depreciation of buildings and equipment.
w_T	$\$60,472$	Wage rate for teachers	Set equal to average teacher wage in the data.
w_N	$\$67,625$	Wage rate for nonteachers	Set equal to average non-teacher wage in the data.
$\bar{R}$	$\$9,116$	Average revenue per student	Sum of average revenue on labor + capital.
ρ	0.47	Persistence of revenue process	Set equal to the autocorrelation of district-level expenditure in the data.
ξ_K	1.0	Welfare elasticity of capital	Normalized to 1
ξ_T	1.52	Welfare elasticity of teachers	Jointly calibrated to match: (1) Avg. teacher/student
ξ_N	0.76	Welfare elasticity of non-Teacher	= 0.064, (2) Average non-teacher/student = 0.062 (3)
α	0.086	Welfare share for teachers	teacher/student 4x more volatile than non-teacher
γ	0.751	Welfare share for non-teaching staff	/student (4) Volatility of total salary = 0.0036,
σ_R	1150	SD of shock to revenue	(5) Avg. volatility of investment = 0.95

[25] See "BEA Depreciation Estimates" at http://www.bea.gov/national/FA2004/Tablecandtext.pdf

4.4 The effect of a Recovery Act-sized shock

To simulate the effects of the Recovery Act, we alter equation (4.3) to

$$R + A = w_T T + w_N N + I \tag{4.5}$$

where A denotes the net magnitude of the Recovery Act shock to revenue after accounting for any loss in revenue at the district level. From our benchmark regression analysis (see Table 2), we estimate the size of this shock to be \$570 per student. As a result, we set $A = \$570$ in the period of the shock and $A = \$0$ otherwise. For a transparent comparison with our regression results, all results below are for a \$1 million shock. In the data, the gross magnitude of the Recovery Act shock before accounting for any loss in revenue at the district level was approximately \$1000 per student. As a result, to find the \$1 million response we multiply the per student values by 1000.

Figure 1 plots the effect of the spending shock. The left panels show the per period impulse responses and the right panels show the cumulative responses. As seen in the figure, over the first two years the additional revenue creates 1.4 nonteaching staff jobs and 0.7 teaching jobs, and increases investment by \$435,000 for each million dollars spent. Note that other than the size of the shock, the model was calibrated independently of the regression results. Consequently, the consistency between our regression results and the dynamic model provides further evidence for a small effect of the Recovery Act on employment.

The large effect on investment is driven by a motive to smooth the value of education inputs over time. For the purpose of intuition, suppose a school district had two mutually exclusive uses of new funds: (i) increasing the number of staff for one year, or (ii) engaging in additional investment for one year. The latter option leads to more capital in both the short and the intermediate run, which increases education services. Also, since the capital is now higher, the district can cut back marginally on investment in periods after the shock and use the funds saved to increase its staffing levels. The latter option leads to an increased and smoother path of inputs over time, as well as higher welfare.

To illustrate this effect, Figure 1 graphs the responses of the district in an calibration where $\delta = 1.0$, i.e. capital depreciates fully after one period. As the figure shows, once the district loses access to interperiod savings, the employment effect increases.

Note that our environment does not permit the district to smooth the benefit of the revenue shock over time using savings or a similar deficit reduction. If we were to extend the model to permit these options, districts would use these financial instruments as well as capital accumulation in an optimal policy. Note, however, that our regression results instead find that deficits increased upon receipt of Recovery Act grants. As explained earlier, the deficit results may be a result of districts pairing new capital spending with increased leverage through higher debt levels.

Next, one of our stylized facts was that the volatility of the number of teachers is significantly lower than that of nonteachers. We conjecture that this occurs because there may be little flexi-

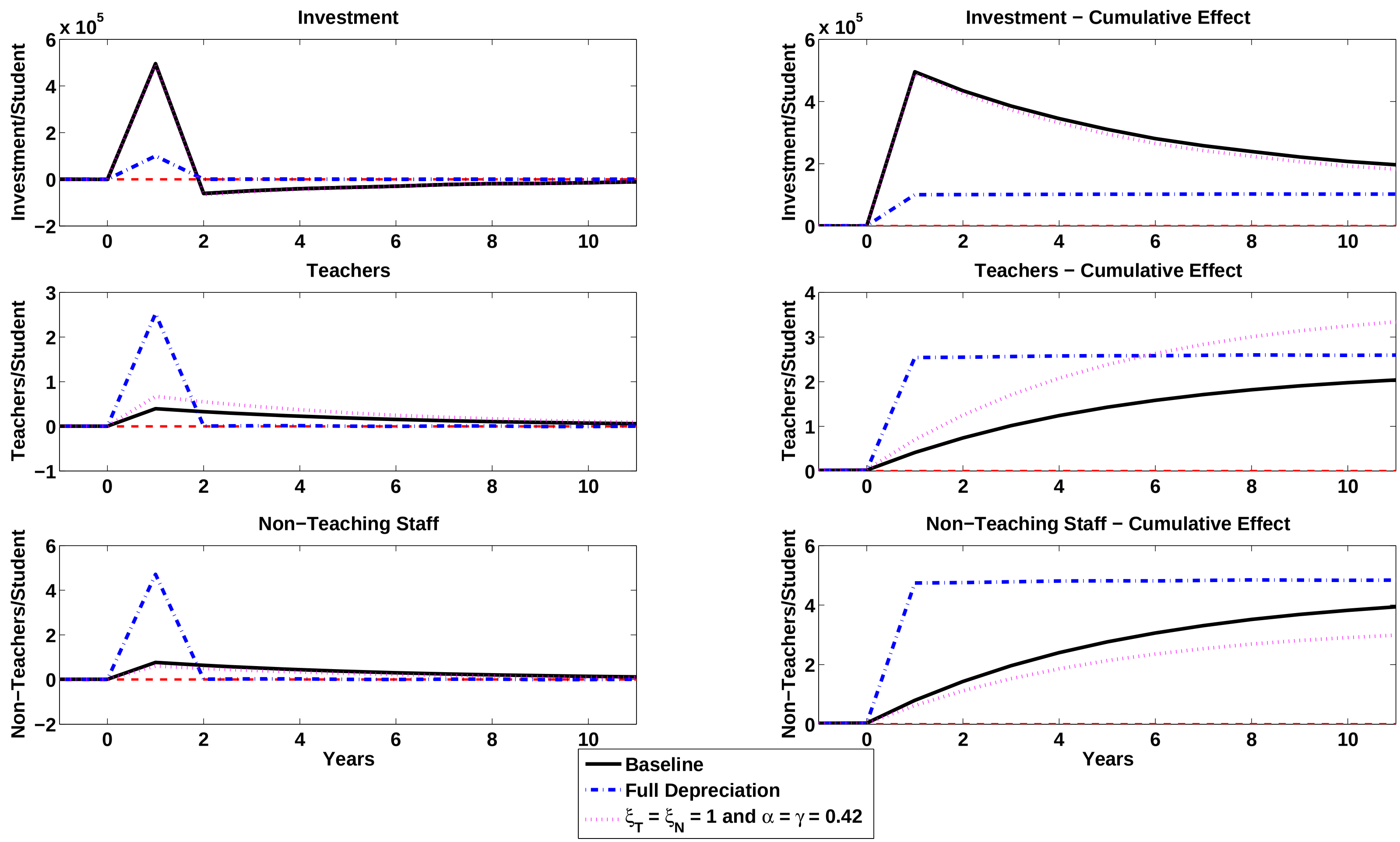

Figure 1: Impulse Responses to a Recovery Act-sized shock

bility in hiring or laying off teachers. Consider a school that teaches five subjects—math, English, Spanish, social studies, and science—to 80 students and currently hires one teacher for each subject. This school may be unable to lay off a teacher because doing so would lead to one fewer subject being taught. If the school wanted to add one teacher, the additional teacher could not teach a bit of all five subjects. Thus, the marginal benefit of hiring one extra, say math, teacher is very low. On the other hand, hiring non-teaching staff across the district likely would not face classroom indivisibilty constraints. The relatively low volatility of teacher employment can be achieved in the model with a high value of ξ_T relative to ξ_N. Thus, $\xi_T > \xi_N$ proxies for a relatively low flexibility in changing the teaching staff level. Figure 1 plots impulse responses if $\xi_T = \xi_N$.[26] When the elasticities for teachers and nonteachers are identical, the response between them is more closely aligned.

Our model also permits us to estimate the shock's long-run effects. As discussed earlier, the initial effect of the shock is driven largely by an education-services-smoothing motive that results in accumulating capital initially. This, in turn, frees up future resources for hiring teachers and non-teachers. As shown in Figure 1, the cumulative 10-year effect is approximately 2 teachers and 4 non-teachers per million dollars spent. Note that these effects are larger than the two-year effect. The long run effect still dwarfs the Council of Economic Adviser's estimate that over 750,000 education jobs were created/saved by the act. At 2.25 jobs per \$1 million in 2 years and 6 jobs per million in 10 years, the \$64.7 billion spent by the Department of Education creates 146,000 jobs in the first 2 years and 388,000 jobs in the first 10 years following the act's passage.

Policy analysis

Our model provides a laboratory to study the effects of alternative ways to implement a stimulus program. First, a simple—and it turns out simplistic—policy would require all districts to use stimulus money only on employment, i.e.

$$A \leq w_T T + w_N N \tag{4.6}$$

Figure 2 plots the response to this policy. The policy has no effect relative to the "no constraint" case presented above. This is because a district's existing revenue and the stimulus money are fungible. In response to a stimulus shock, a district can cut back on using its existing revenue to pay labor and instead use the stimulus money to hire workers. The district would meet the requirement of using stimulus money to hire workers and maintain the no constraint outcome.

Consider an alternative policy where, instead, the federal government requires that in the period of the shock

$$\phi(R + A) \leq w_T T + w_N N \tag{4.7}$$

[26] The value of α and γ are jointly determined with ξ_T and ξ_N.

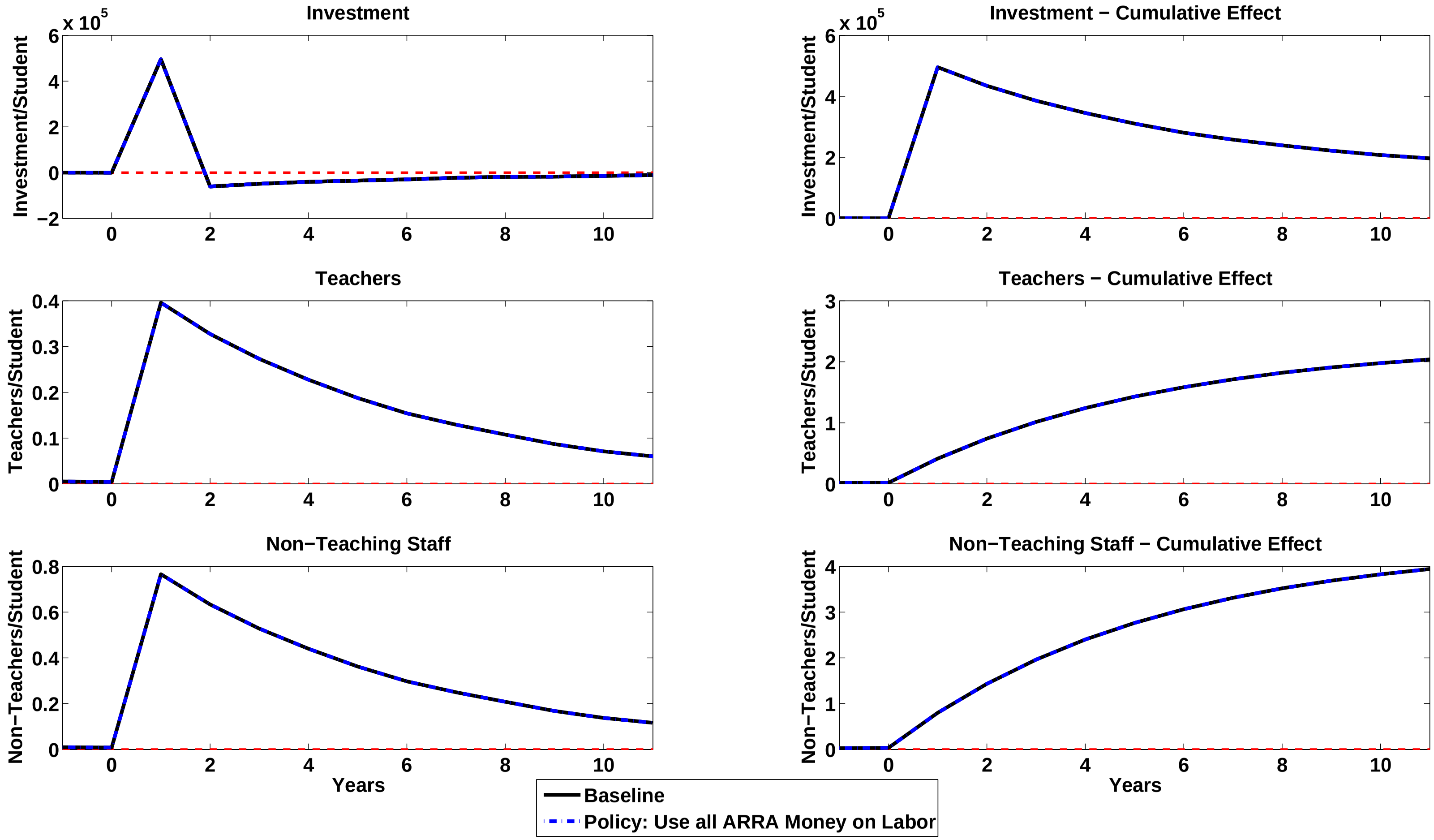

Figure 2: Impulse Responses to a Recovery Act-sized shock

where ϕ gives the percentage of the *all revenue* that must be used to pay workers. We simulate the model under this policy, setting $\phi = 0.875$, which we find achieves the maximum employment effect (while keeping investment constant). Figure 3 gives the results of this exercise. This has a significantly larger response of 9 new jobs (3 teaching plus 6 nonteaching) in the year of the shock.

Our model also allows us to consider much richer policy alternatives where the percentage of revenue depends on the amount of revenue and capital at the district level. In Figure 4, we first calculate the pre-stimulus response of the district and then require the district to use all its stimulus revenue plus all revenue it would have used toward hiring labor had it not received the stimulus revenue. The figure then plots what percentage of the total post-stimulus revenue this amount would have been.

As Figure 4 shows, as an optimal policy the government should impose a larger percentage of revenue used on labor for districts with lower revenues and high levels of capital. Districts with lower levels of revenue in particular are motivated to use the additional stimulus revenue received from the government on capital.

5 Conclusion

This paper explores the impact of countercyclical government spending on the education sector. Empirically, we find that the Recovery Act's education component had a small impact on nonteacher employment, no effect on teacher staff levels, and a substantially less than one-for-one response of district-level expenditures. To the extent that government grants increased district expenditures, the increases largely took the form of capital outlays. The grants also stimulated district debt accumulation.

These findings should not be entirely surprising given the decentralized nature of the act's implementation plan. The allocation process was multitiered, with local and state governments allowed latitude as to how Recovery Act dollars were spent. First, state governments maintained substantial control over how they spent their own revenue. This created an environment where stimulus dollars might be used to replace state contributions.[27]

After passing through the state level, the Recovery Act dollars were spent by individual districts largely at their own discretion. Given that the stimulus dollars were temporary, districts had an incentive to smooth out the spike in additional education services that they could provide by investing in equipment and structures. This objective is one potential explanation for the small education jobs effect estimated in this paper.

[27] As Inman (2010) writes, "States are important 'agents' for federal macro-policy, but agents with their own needs and objectives."

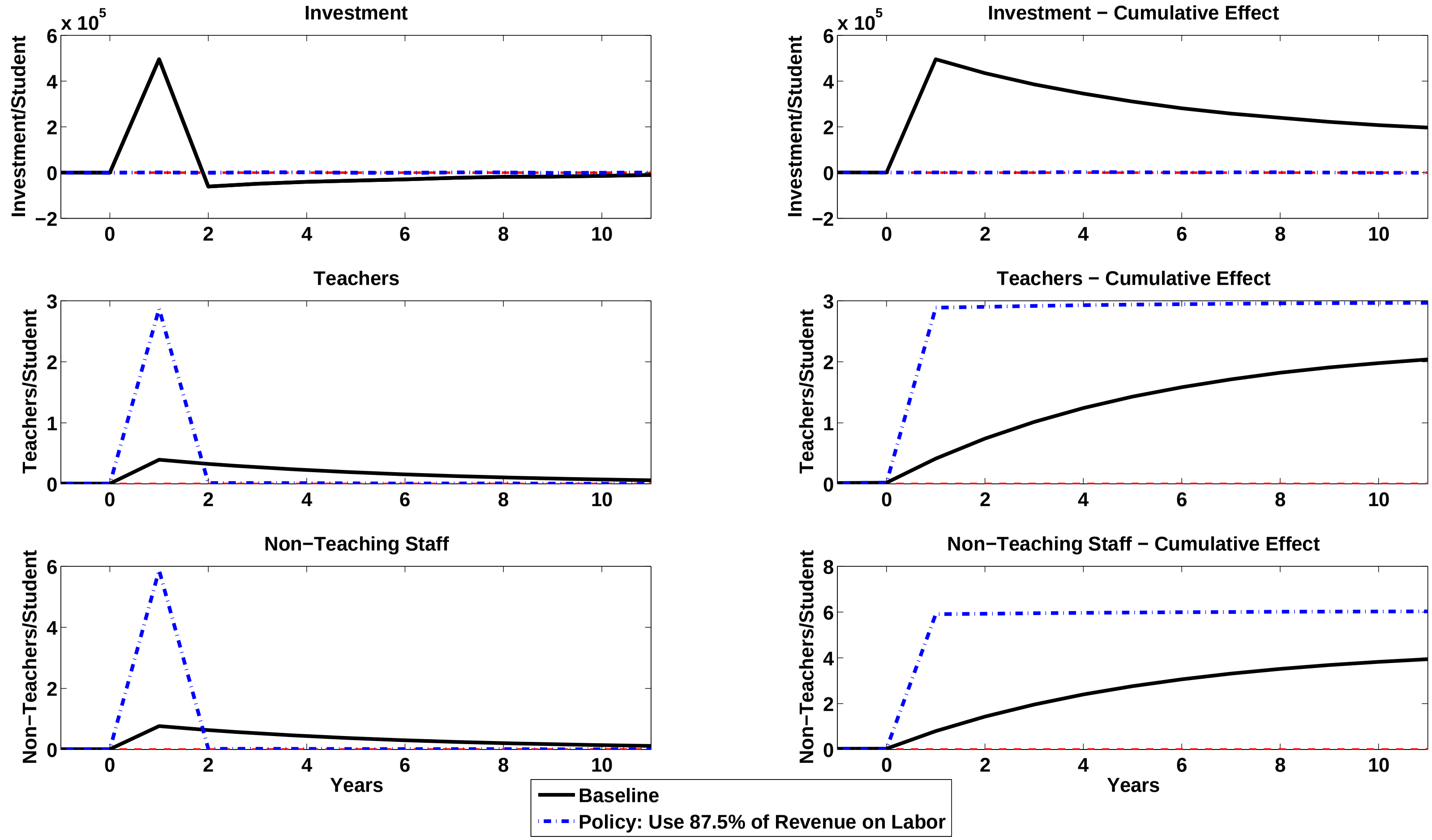

Figure 3: Impulse Responses to a Recovery Act-sized shock

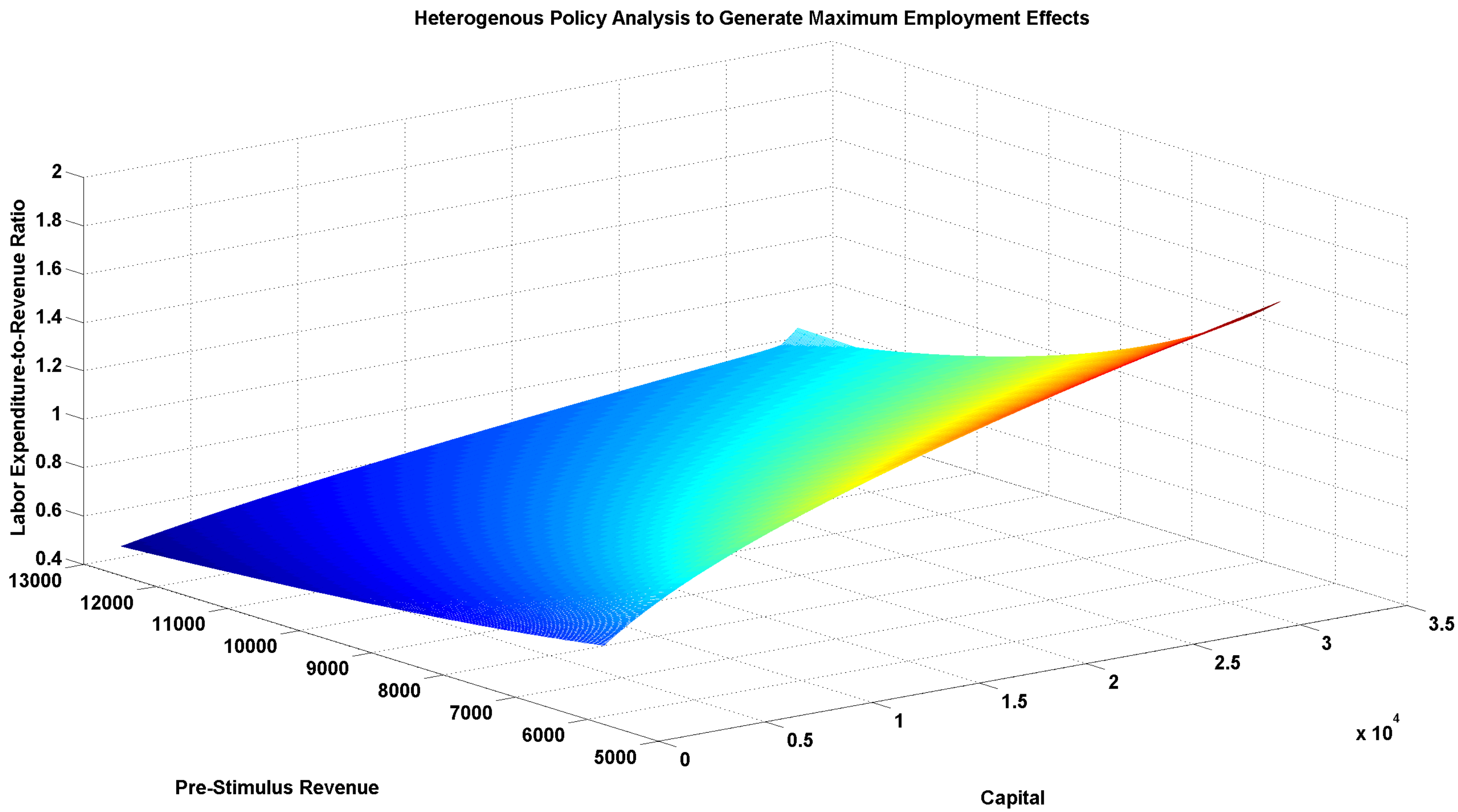

Figure 4: Optimal Policy

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Appendix

Table A.1: Number of jobs directly created and saved through grants, contracts and loans administered by the U.S. Department of Education, first two school years following enactment

Quarter	Education jobs
2009Q3	397,982.43
2009Q4	423,616.33
2010Q1	470,197.34
2010Q2	454,281.08
2010Q3	344,308.14
2010Q4	309,187.21
2011Q1	319,494.26
2011Q2	307,901.15
Total (annualized)	756,741.99

Notes: Jobs are measured in units of full-time equivalents. Source: Recovery.gov.