# Schools and Stimulus*

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**Abstract**

This paper analyzes the impact of the education funding component of the 2009 American Recovery and Reinvestment Act (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 special needs students. We estimate that $1 million of 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 came in the form of capital outlays. Next, 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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## 1 Introduction

The 2009 Recovery Act was signed into law with a primary goal of creating and saving millions of jobs during and following the most recent recession. A large share of the appropriations from the act was made as 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 passage, Vice President Joe Biden stated that funds from the act would "help to keep outstanding teachers in America's schools."[3] According to 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, tracked the number of jobs which were payrolled by the act's funds using surveys of recipient organizations. The Council of Economic Advisers (various quarterly reports) used these 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 made decisions relative to that of districts receiving plenty of grant money. From this comparison, we infer what all districts would have done had the act's grants not been available.

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 overall 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, constituting one-fifth of the education grants. Its allocation across districts was determined primarily by the requirements that districts finance their special needs programs. Although each instrument is highly correlated with overall Recovery Act education spending, each is plausibly uncorrelated with the short-run business cycle and tax revenue situations faced by school districts.

[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), 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, "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. Dept. of Education (2009a) implementation guidance.

[3] See Biden (2011).

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

[5] See Table E.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.

We have four main findings. First, the grants had either zero or else a small education jobs impact. Each $1 million of aid to a district resulted in roughly 1.5 additional jobs at that district. The point estimate implies that, in the first two school years following 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 ratio. As such, school officials may have found other margins besides firing or hiring along 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 which offset the act's grants.

Third, districts receiving grants tended to accumulate greater debt.

Fourth, roughly 70% of the spending increase occurred as capital expenditures, i.e. 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 falls 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 our forced to only use stimulus money on employment reduce the spending they do on labor from their own sources of revenue and substitute this shortfall with stimulus money leaving the net employment outcome unchanged. We show that an alternative policy where school districts are required to spend 87.5% of all revenue (both from stimulus + their own sources) has a much more significant effect on employment.

With respect to existing work, there is almost no economic research on the act's education component. One exception is, Dinerstein, et.al. (2013), which study the impact of the Act on universities. Also, Chakrabart and Setren (2011) examines 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 Recovery Act's impact have focused mainly on economy-wide labor market outcomes. These include Chodorow-Reich et al. (2012), Conley and Dupor (2013), 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 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 supplanted local funding. They found that for every four officers payrolled by a grant, in an accounting sense, a police force actually increased by 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 2010SY, there were 16,117 such districts in the U.S. We restrict attention to districts with more than 500 student during that year. After additionally excluding districts missing requisite data, we are left with 6,786 districts.[7]

**Outcome Variables** ($\Delta$Job-Years, $\Delta$Expenditures and Debt accum)

Our first outcome variable measures school district employment. It is the change in employment from a base of 2007SY over the first two school years in which the act was fully in effect, i.e. 2009SY and 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.

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 $\text{Pop}_j$ is the district $j$ enrollment in the 2007SY.

[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 them 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 in which the Recovery Act was in effect.

From the annual Local Education Agency Finance Survey, we have data on total expenditures $S_{j,t}$ and debt. From these variables, we calculate our next two outcome variables. We measure expenditure as the per student cummulative 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 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 $\tilde{V}_j$ to be the Recovery Act dollars outlaid to school district $j$, from enactment through 2011Q2. 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 web site FederalReporting.gov.[9] 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 employ 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 by which the federal government supports special education is through 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 education. Most of the Recovery Act special education money was tied to the IDEA program. While there are several subprograms within IDEA, the lion's share of monies comes through Part B of IDEA. The Recovery Act funding formula follows the IDEA Part B formula.[10]

Recovery Act IDEA Part B grants were add-ons to regular annual IDEA Part B grants to states. The national FFY2009 regular grant amount was \$11.5 billion. The first \$3.1 billion (both from regular funding and the Recovery Act add-on) was divided up amongst states so that they were guaranteed to receive their FFY1999 awards. Once this requirement was met, the remaining

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

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

part of the national award was allocted 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."[11] The Recovery Act add-on totaled \$11.3 billion. Since, at the margin, the FY1999 requirements had already been met by the regular awards, every Recovery Act dollar was in effect assigned across according to the 85/15 percent rule.

Next and importantly, we address how funds were assigned from state education agencies to local education agencies (LEA). These initial allocations too were made at the federal level. Each LEA was first allocated a minimum of its FFY1999 award.[12] 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 dollars was allocated to according to the share of school age children in the LEA and 15% was allocated according the LEA's childhood poverty rate. After this, states were allowed to do reallocations 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 rules.

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 dollars 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 thie above equation as:

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

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. Next we run state-level regressions to check this conjecture for the 46 state for which we have fully reported IDEA amounts. The set of $R^2$ from these regression are generally very low: 25 are less than 0.01. Only six of the $R^2$ are greater than 0.1 and only one is greater than 0.3.[13] This tells us that other factors besides the poverty rate in each district are influencing the allocation of IDEA funds.

[11] See Enclosure B of U.S. Dept. of Education (2009b), which contains a description of how Recovery Act funds were allocated across states.

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

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

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 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 $R^2$ 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 using drawing on Recovery Act IDEA funds. Those funds were then reallocated to districts with additional funding 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 the services to these special needs students. Our exogenity assumption is that this set of factors driving redistributions of IDEA funds are 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.[14] Denote this variable as $V_j^{SN}$. While the fraction of special needs students in a school district is likely to impact 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;
- *Non-financial variables*: the ratio of African American plus Hispanic enrollment to overall enrollment, the natural log of enrollment, 7 region dummy variables, a constant;
- *School district financials*: the poverty rate, the fraction of revenue from local sources, the cumulative change in revenue from non-federal 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 overall population of persons living within each school

[14] This data also comes 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

| | 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 variable has been divided through by 1000. IDEA, Individuals with Disabilities Education Act; SD, standard deviation; pp, per pupil.

district's borders. The change in revenue from non-federal 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.

Summary statistics for the variables in our analysis appear in Table 1.

## 2.2 The econometric model

We use two-stage least squares in estimation. The statistical model for the $\Delta$Job-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 $\beta_{JY}$. The statistical model for the other two outcome variables simply replaces $\Delta\text{Job-years}_j$ with

$\Delta$Expenditure$_j$ or Debt Accum$_j$. Our estimates are weighted by district enrollment. We report robust standard errors, which are calculated with clustering by states.

## 3 Results

### 3.1 Benchmark results

Table 2 contains our benchmark estimates.

**The employment effect**

We report the job-years response to grants in column (i). The coefficient on education spending equals 1.47 (SE=1.31): 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 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.[15] The estimates for other outcome variables, presented below, elucidate two reasons why there was a small, if any, education jobs effect. First, a large part of the grants did not translate into greater district-level expenditures. Second, district level expenditures that did arise from the grants were mainly used 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% confident interval, our estimate is that the effect was 260,000 jobs.[16] As explained in the introduction, this is substantially lower than the corresponding number based on payroll count data reported at Recovery.gov.[17]

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 2395, with a pointwise $t$-statistics of 9.88 for the special education student ratio instrument and 67.8 for the Special Education Funds instrument.

Our jobs effect finding begs 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, and in particular 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

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

[16]We calculate this number by multiplying the job-years coefficient 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 those excluded from the sample.

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

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

| | Δ Job-years | Δ Expenditure | Debt accum. |
|---|---|---|---|
| | (i) | (ii) | (iii) |
| Recovery Act education grant | 1.47** | 570.10*** | 340.63* |
| ($1 mil) | (0.70) | (109.22) | (176.18) |
| Ln(population) | 0.04 | 148.93*** | -38.88 |
| | (0.23) | (35.94) | (57.98) |
| Minority ratio | -0.02*** | 1.47*** | 0.77** |
| | (0.00) | (0.21) | (0.35) |
| Poverty rate | -0.03 | -15.22*** | 7.49 |
| | (0.02) | (3.58) | (5.78) |
| Nonfederal spending change | 1.62*** | 632.00*** | 78.36*** |
| | (0.10) | (16.16) | (26.06) |
| Self-supporting school | -0.01*** | -1.21*** | -0.72 |
| district | (0.00) | (0.30) | (0.49) |
| Teachers per pupil, lag | -0.05 | 67.66*** | 27.08** |
| | (0.04) | (6.66) | (10.74) |
| Staff per pupil, lag | -0.36*** | 5.49** | -12.39*** |
| | (0.01) | (2.29) | (3.69) |
| 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) | 9.88 | 9.88 | 9.88 |
| IDEA Recovery Act aid (t-stat) | 67.79 | 67.79 | 67.79 |
| Partial F-stat | 2395.26 | 2395.26 | 2395.26 |

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

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 let go. Adjusting the capital outlays was 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 large grant nor small grant districts significantly adjusted their staff levels in response to the shock, then we should expect our 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, art and music programs. As further evidence, based on surveys of school administrators, 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 offering. While each of these may have marginally reduced the quality of education services provided by the schools, the changes did not directly impact the total number of district employees.

Note that if there was job creation outside of the school 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 $\Delta$Expenditure specification. The point estimate on Recovery Act education spending equals 570 (SE=109) thousand. This implies that $1 million of education grants resulted in an increase in expenditures of approximately $570 thousand over the first two full school years following the act's passage. Thus, only about one-half of 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 local and state governments' contribution to public education when school districts received Recovery Act dollars.

This findings relates to previous research on whether federal grants crowd out state and local spending. In a simple political economy model, Bradford and Oates (1971) shows conditions under which crowding out occurs. Leduc and Wilson (2013) present evidence that crowding out was not a problem 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 (SE = 176) thousand. Based on the point estimate, districts which received relatively more aid tended to increase their debt positions. The estimate is statistically

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

| | Δ Job years<br>(i) | Δ Expenditure<br>(iii) | Debt accum.<br>(ii) | 1st stage partial F-statistics<br>(iv) |
|---|---|---|---|---|
| Benchmark | (1.47)** | 570.10*** | 340.63* | 2,395 |
| | (0.70) | (109.22) | (176.18) | |
| Not enrollment weighted | 1.56 | 1,149.12*** | 284.38 | 859 |
| | (1.47)** | (226.22) | (432.93) | |
| No weights & large districts | 0.12 | 346.08*** | 461.15*** | 5,719 |
| | (0.54) | (89.60) | (158.97) | |
| Ordinary least squares | 2.11*** | 165.46** | 30.25 | N/A |
| | (0.43) | (66.81) | (104.84) | |
| IDEA instrument only | 1.39** | 524.99*** | 229.73 | 4,627 |
| | (0.71) | (110.30) | (177.93) | |
| Special ed ratio instrument only | 8.04** | 2,339.31*** | 3,977.68*** | 140 |
| | (3.21) | (526.71) | (843.64) | |
| Drop region dummies | 1.28* | 621.54*** | 232.14 | 2,369 |
| | (0.75) | (108.73) | (172.08) | |
| Drop all lagged variables | 0.97 | 216.97* | 388.78** | 2,464 |
| | (0.74) | (114.79) | (173.68) | |

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

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. The first row contains the benchmark estimates. The row labeled "Not enrollment weighted" does not weight the error terms by the district's enrollment. The "Large districts and no weights" specification restricts attention to districts with enrollments greater than 2,000 and also does not weight the error terms by enrollments. 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.

Table 4: Estimates of the impact on staff employment of $1 million of 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 | (0.32) | (0.59) |
| Full Controls | Yes | Yes |
| No. of Observations | 6786 | 6786 |
| Partial F-stat | 2395.26 | 2395.26 |

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

Third, instrumenting with only the special education ratio generates a substantial increase in the jobs effect. The estimate increases to 8.04 (SE = 3.41). Note that besides the increase in the point estimate relative to that of the benchmark specification, there is also a large increase in the standard error.

Column (ii) of Table 3 presents the total expenditure estimates. Recall that the coefficient is interpreted as the thousands of dollars that 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 the grants because part of the aid is not passing through to expenditures. The majority of estimates are close to the benchmark one and exhibit substantial crowding out. There are two exceptions. First, the "Not enrollment weighted" estimate increases to 1.1 million dollars, indicating that there is no crowding out. This suggests that crowding out is a larger problem for large districts relative to small districts. Second, the "special ed. ratio instrument only" estimate is 2.4 million dollars.

Column (iii) of Table 3 presents the debt accumulation estimates. The benchmark estimate shows no statistically significant effect. Similarly, for 4 of 7 of the alternative specifications, the estimates are not statistically different from zero. The estimates are statistically significant and negative for three of the specifications: "Large districts and no weights," "Special ed ratio instrument only" and "Drop all lagged variables." They imply that the receipt of Recovery Act aid *encouraged* districts to take on new debt. Note that for the "special ed. instrument only" case, the estimate equals -4.3 million, which we view as implausible.

Column (iv) of the table contains the partial $F$-statistic for each specification. None of the values indicate a weak instrument problem.

Next, we consider what type of education jobs were impacted. 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 non-teaching employees.

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 | 570.10*** | 390.82*** | 8.92 | 79.38*** |
| spending per pupil | (109.22) | (99.60) | (24.13) | (18.06) |
| Full Controls | Yes | Yes | Yes | Yes |
| No. of Observations | 6786 | 6786 | 6786 | 6786 |
| Partial F-stat | 2395.26 | 2395.26 | 2395.26 | 2395.26 |

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.

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 -0.17 (SE = 0.32). 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 non-teacher jobs. As seen in column (ii), each $1 million resulted in 1.50 (SE = 0.59) additional job-years of non-teacher employment.

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 there is a substantial effect on capital outlays of Recovery Act aid. Roughly 70% of all expenditures came in the form of capital outlays.[18] 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, in a similar manner as 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 a part of its 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 investment in capital rather than layoff 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. Anecdotally, based on a survey of school administrators, AASA (2012) found that nearly half of those surveyed deferred some maintenance in the 2010SY.

Recall that earlier in the paper, we document that Recovery Act aid tended to increase debt

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

accumulation. This effect may be related to the positive effect of aid on capital expenditure seen 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 via borrowing. Under this scenario, had the district attempted to finance the entire capital project with only debt, it may have been unable to secure the funds or else 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.

Column (ii) of Table 5 reports the impact of aid on salaries, which was small and not statistically different from zero.[19] Since the employment effect was so small, it is not surprising that we do not recover a substantial wage effect. Column (iii) 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 for the purpose of understanding our empirical findings.

First, the ratio of stimulus going to pay education pay education workers relative to capital investment was 0.2. This is a puzzle since, as we explain below, the long run average of this ratio equals 8.

Second, there was a small but statistically insignificant effect on non-teacher staffing but no effect on the number of teachers in the classroom. First, why was the ratio of stimulus going to pay education workers relative to capital investment equal to 0.2, when the long run average of this ratio equals 8? In addition, our model allows us to estimate the medium and long-run effects of these grants and provides a laboratory to the effects of alternative hypothetical stimulus programs aimed at school districts.

## 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 between the 1994SY and the 2011SY.[20] The facts provide guidance for building

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

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

and then calibrating our economic model. Unless otherwise noted, each variable is scaled by the district's enrollment.

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

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

**Stylized Fact 1:** *The teacher to student ratio is less volatile than the non-teacher to student ratio.*

For each district $i$, we compute the time series variance of the log deviation of the scaled employment levels of teachers, $T$, and non-teacher staff, $N$, (each measured on a per pupil basis).[21]

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

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

Columns (i) and (ii) of Table 6 contain the across-district median value (along with the 10th, 25th, 75th, and 90th percentile values) of $v_{T,i}$ and $v_{N,i}$. As can be seen the Non-Teacher/Student ratio is systematically 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, in columns (iii)-(iv) we report 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 is still consistently less variable than the Non-Teacher/Student ratio.

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

Next, we consider the behavior of two categories of expenditures: capital expenditures and labor expenditures. Capital expenditure is the sum of spending on construction, land and existing structures, and equipment with an expected life of 5 or more years. Labor expenditures includes salaries and benefits of district employees.[22] We convert each variable into real terms using the

[21]Non-teacher staff includes instructional aides, guidance counselors, library/media staff, administrative support staff, etc.

[22]We exclude services and non-durable 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

Table 6: Volatility of Teacher/Student & Non-Teacher/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 | |
| # of Districts (N) | 1901 | | 4291 | |

GDP deflator with a base year of 2011.

Table 7 reports the across-district median value (along with the 10th, 25th, 75th, and 90th percentile values) of the time-series volatility of expenditures on Total Real Salary + Benefits and Real Capital Outlays (Real Investment), where the volatility is calculated as the time-series variance of the log deviations of the variable from its aggregate trend using (4.1). Note that investment is significantly more variable than labor expenditures. At the median level of variability, expenditure on capital is 250 times more variable than expenditure on salary and benefits.

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 | |

Next, we break this expenditure category into spending of two types: construction, land, and existing structures (CLS) and equipment. Table 8 reports the volatility of these variables. Even though equipment itself is volatile, most of the volatility in capital is driven by the volatility of

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.

Table 8: Volatility of (Salary + Benefits)/Student and Capital/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 | |

expenditure on CLS. This fact coupled with the facts (1) that CLS makes up roughly 80% of all capital investment and (2) that labor expenditure at the district level is not very volatile leads us towards a theory where districts tend to use large revenue gains and make-up for revenue shortfalls by largely either investing in, or delaying expenditure on, capital goods such as construction, land, and existing structures

## 4.2 The Economic Model

Consider a school district that uses an exogenous stream real revenue, $R$, to hire workers and buy goods to provide education services to its students. Its revenue process is given by:

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

where $\rho \in (0,1)$. Revenue, as well as other variables in the model, are per pupil. A $'$ denotes next period's value.

A district's one-period welfare function is

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

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

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

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

subject to

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

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

and non-negativity constraints on $T, N$ and $K$. Also, $I$ represents investment in the capital good and values with a prime subscript 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 non-teacher wage, respectively. Also, (4.4) is the capital law of motion and $\delta$ is the capital depreciation rate.

Our model above allows for dynamic decision making. Every period the model school district receives revenue which it optimally allocates to the hiring of teacher and non-teachers, and the acquisition of capital. Whereas, the amount of teachers and non-teachers hired effect only the current period's welfare, the durable nature of capital results in the capital choice having a multi-period effect. As we discuss in the next section, the dynamics that result from allowing the district to choose a durable input, capital, are extremely important to understanding why the 2009 Recovery Act had such a small effect on hiring, but a large effect on capital outlays.

## 4.3 Calibration and Simulations

The parameter values for the model are given in Table 9. The model period is equal to 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, capital stock is comprised of two different basic types: Construction, land, and existing structures (CLS), and equipment (such as furniture) with more than a 5 year lifespan. 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.[23]. As a result we set the depreciation rate in our model to equal $(0.75 * 0.0133 + 0.25 * 0.16) = 0.0516$.

Across districts, the median wage bill per student is \$8128, for which 48% go towards teacher pay and 52% go towards non-teaching staff pay. Teacher compensation per pupil is thus \$3901.44 and non-teaching staff compensation equals \$4226.56. The median teacher-student ratio is 1/15.5 and the median number of non-teachers staff per student is 1/16. As a result we set teacher and non-teacher wage $w_T$ to be \$60472 (= $15.5 \times 3901$) and $w_N$ at \$67625 (=$16 \times 4227$.).

The persistence of the exogenous AR(1) process for revenue is directly estimated from the data where the median auto-correlation of expenditure across time is 0.47. The average revenue is set at $\bar{R} = \$8128 + \$988 = \$9116$.

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

Six parameters remain: The welfare elasticities ($\xi_T$, $\xi_N$ and $\xi_K$, the relative shares of teachers, $\alpha$, and non-teachers, $\gamma$, and the standard deviation of the revenue process, $\sigma_R$.

First, we set $\xi_K = 1$ and then jointly calibrate the remaining 5 parameters to match the following 5 targets: The average teacher/student ratio is 0.064; the average non-teacher/student ratio is 0.062; the non-teacher/student ratio is 4 times as volatile as the teacher/student ratio; the average salary volatility equals 0.0036; and an average investment volatility of investment.

Table 9: Parameter Values

| Parameter | Value | Description | Explanation |
|---|---|---|---|
| $\beta$ | 0.96 | Discount factor | Standard value for annual discount factor |
| $\delta$ | 0.0516 | Depreciation Rate | Calculated using the BEA data on the depreciation of buildings and equipment. |
| $w_T$ | \$60472 | Wage rate for teachers | Set equal to average teacher wage in the data. |
| $w_{NT}$ | \$67625 | Wage rate for non-teachers | Set equal to average non-teacher wage in the data. |
| $\bar{R}$ | \$9116 | Average revenue per student | Sum of average revenue on labor + capital. |
| $\rho$ | 0.47 | Persistence of revenue process | Set equal to the auto-correlation of district-level expenditure in the data. |
| $\xi_K$ | 1.0 | Welfare elasticity of capital | Normalized to 1 |
| $\xi_T$ | 1.52 | Welfare elasticity of teachers | Jointly calibrated to match: (1) Avg. teacher/student |
| $\xi_N$ | 0.76 | Welfare elasticity of non-Teacher | = 0.064, (2) Average non-teacher/student = 0.062 (3) |
| $\alpha$ | 0.086 | Welfare share for teachers | teacher/student 4x more volatile than non-teacher |
| $\gamma$ | 0.751 | Welfare share for non-teaching staff | /student (4) Volatility of total salary = 0.0036, |
| $\sigma_R$ | 1150 | Std. Dev. of shock to revenue | (5) Avg. volatility of investment = 0.95 |

## 4.4 The Effect of a Recovery Act Size Shock

To study the effects of the Recovery Act shock we alter equation (4.3) to:

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

where $A$ gives 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 in table 2 we estimate the size of this shock to be \$570 per student. As a result we set $A = \$570$ for the period of the shock and $A = \$0$ otherwise. To be consistent with our empirical results in the previous section we report all our results below as resulting from a million dollar Recovery Act spending shock. The gross magnitude of the Recovery Act shock before accounting for any loss in revenue at the district level was $\approx$ \$1000 per student in the data, as a result to find the million dollar response we multiply our per student simulations by a factor of 1000.

Figure 1 plots the per million dollar effect of the Recovery Act spending shock for our baseline model. The left hand panels give the per period impulse response and the right hand panels give the cumulative effects of the shock. As can be seen in the figure, consistent with our regression estimates, our model predicts that in two years the stimulus created 1.4 non-teaching staff jobs, 0.7 teaching jobs, and increased investment by $435,000 for each million dollars spent. It is important to note that other than the size of the ARRA shock the dynamic model of this section was calibrated independently of the regression results. Consequently, the consistency inbetween our regression result and dynamic model give further evidence for the small effect of the Recovery act on employment and large effect on investment.

The large effect of the Recovery Act on investment is driven by a motive to smooth the value of education inputs over time. The school districts had two options (1) They could have used the money to increase the number of teachers and non-teachers for the year in which they got the stimulus or (2) They could have saved the money and used it over time for a smaller but more persistent increase in the number of teachers and non-teachers. The latter option leads to a smoother and higher welfare. One way to save money is to invest in durable goods such as capital, which is what the districts did. To illustrate this effect in figure 1 we plot the responses of the district in an alternate economy where the depreciation rate of capital is 1.0, that is capital is not durable. As can be seen in the figure once the district loses access to a saving instrument the employment effect rises.

Next, as documented in the stylized facts subsection the volatility of the number of teachers is significantly lower than that of non-teachers. We conjecture that the reason for this difference is that their is very little flexibility in adding or subtracting teachers. Consider a simple school that teaches 5 subjects - math, English, Spanish, social studies, and science - to 80 students and currently hires one teacher for each subject. This school has very little flexibility in subtracting a teacher because if it does it would not be able to teach one of the subjects. On the hand if it wanted to add a teacher it would not be able to hire a teacher who can teach a little bit of all 5 subjects and thus would have to hire a teacher to teach only one more subject. The marginal benefit of hiring one full extra math teacher is very low. This is in contrast to non-teaching staff across the schooling district which does not face classroom or subject related constraints.The relatively low volatility of teachers translates to a high value of $\xi_T$ relative to $\xi_N$ in the model. Mathematically $\xi_T > \xi_N$ indicates the relatively low flexibility in changing the number of teachers. In figure 1 we also plot out the responses if $\xi_T = \xi_N$.[24]. When the elasticities for teachers and non-teachers are identical the response between them is more even.

Our dynamic model also lets us estimate the long term effects of the stimulus. As discussed above the initial effect of the stimulus is driven largely by a "welfare-smoothing" motive that results in saving in capital to free up future resources for hiring teachers. As seen in figure 1 the 10 year effect of the stimulus stands at 2 teachers and 4 non-teachers per million dollars spent. It should be noted that this effect is definitely larger than the 2 year effect but still dwarfs the claim of 750,000 education jobs. At 2.25 jobs per million in 2 years and 6 jobs per million in 10 years, the $64.7

[24]The value of $\alpha$ and $\gamma$ are jointly determined with$\xi_T$ and $\xi_N$ so we also set them equal to each other

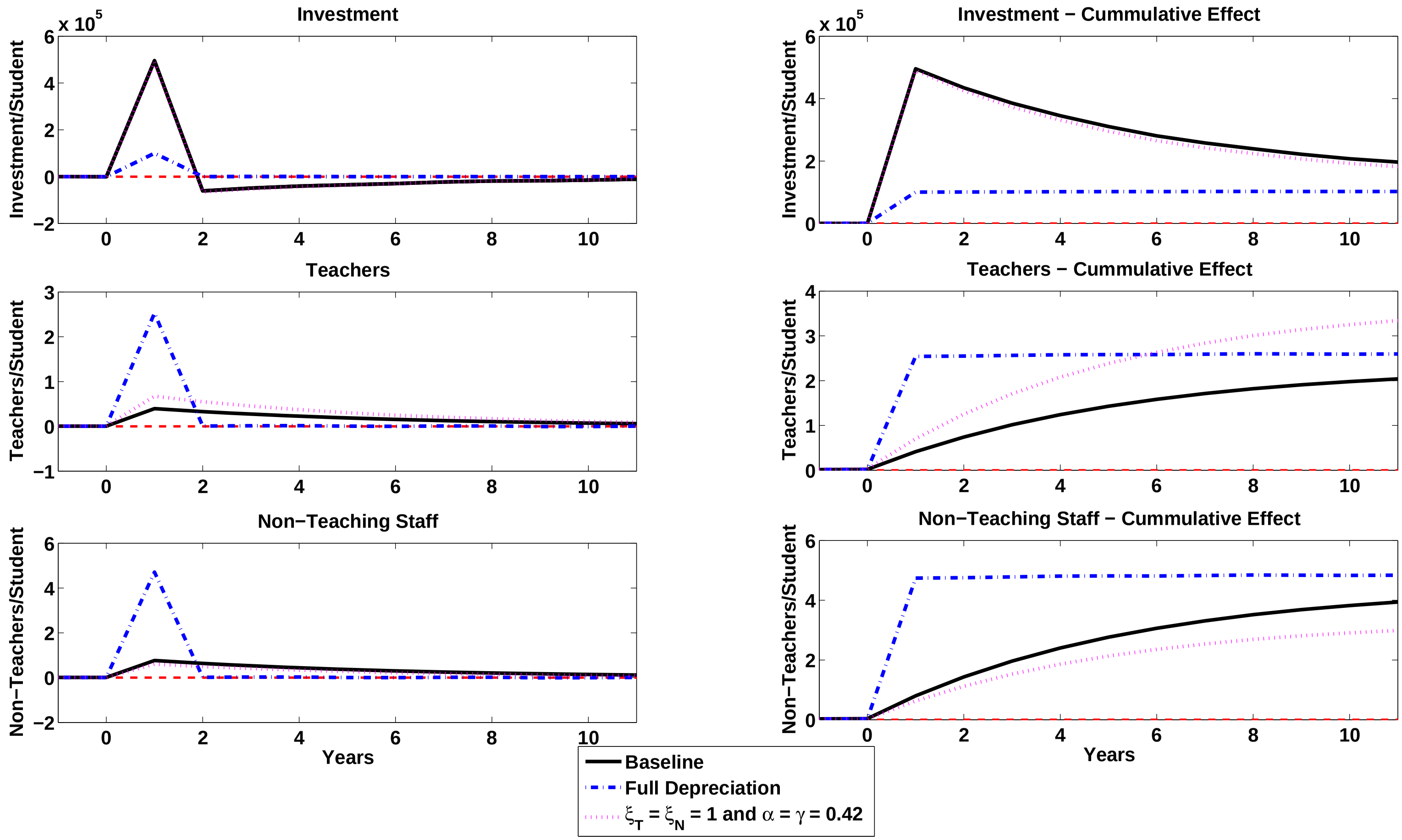


Figure 1: Impulse Responses to ARRA Shock

billion spent by the Department of Education by our dynamic model's estimate would only create 146,000 jobs in 2 years and 388,000 jobs in 10 years.

**Policy Analysis**

Our dynamic model provides us with a laboratory to study the effects of different policies that can be used to generate a larger employment effect in response to the stimulus. The simplest policy that can be used is to require all districts to use all the stimulus money on employment only. That is to require:

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

In figure 2 we plot the response to such a policy in our dynamic model. As can be seen such a policy has no effect. The reason being that a districts regular revenue and the stimulus money are indecipherable as revenue streams. In response to a stimulus shock a district can cut back on using its own revenue stream for hiring and instead use the stimulus money to hire workers thereby meeting the requirement of using stimulus money to hire workers and at the same time keeping the net effect the same.

The policy advice thus has to be based not on the Recovery Act money but the total revenue the district has in the period of the recovery act shock. That is in the period of the recovery act shock to get the maximum effect the government should require:

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

where $\phi$ gives the percentage of the revenue the government requires the district use to hire labor. We simulate our model with such a policy, setting $\phi = 87.5\%$ which we find is the optimal value to generate the maximum employment effect (while keeping investment constant). Figure 3 gives the results of this exercise. As can be seen implementing such a policy can lead to a significantly large response of 9 new jobs (3 teaching + 6 non-teaching) in the year of the shock.

Our model also allows us to consider a much richer policy advice were the percentage of revenue depends on the amount of revenue and capital at the district level. For figure 4 we first calculate the pre-stimulus response of the district and then require that the district use all its stimulus revenue + all revenue it would have used toward hiring labor had it not gotten the stimulus revenue. The figure then plots what percentage of the total post-stimulus revenue this amount would have been.

As can be seen in the figure as an optimal policy the government should impose larger percentage of revenue used on labor restrictions on districts with lower revenues and high levels of capital. Districts with lower level of revenue in particular are very likely to use the additional stimulus revenue they receive from the government on capital.

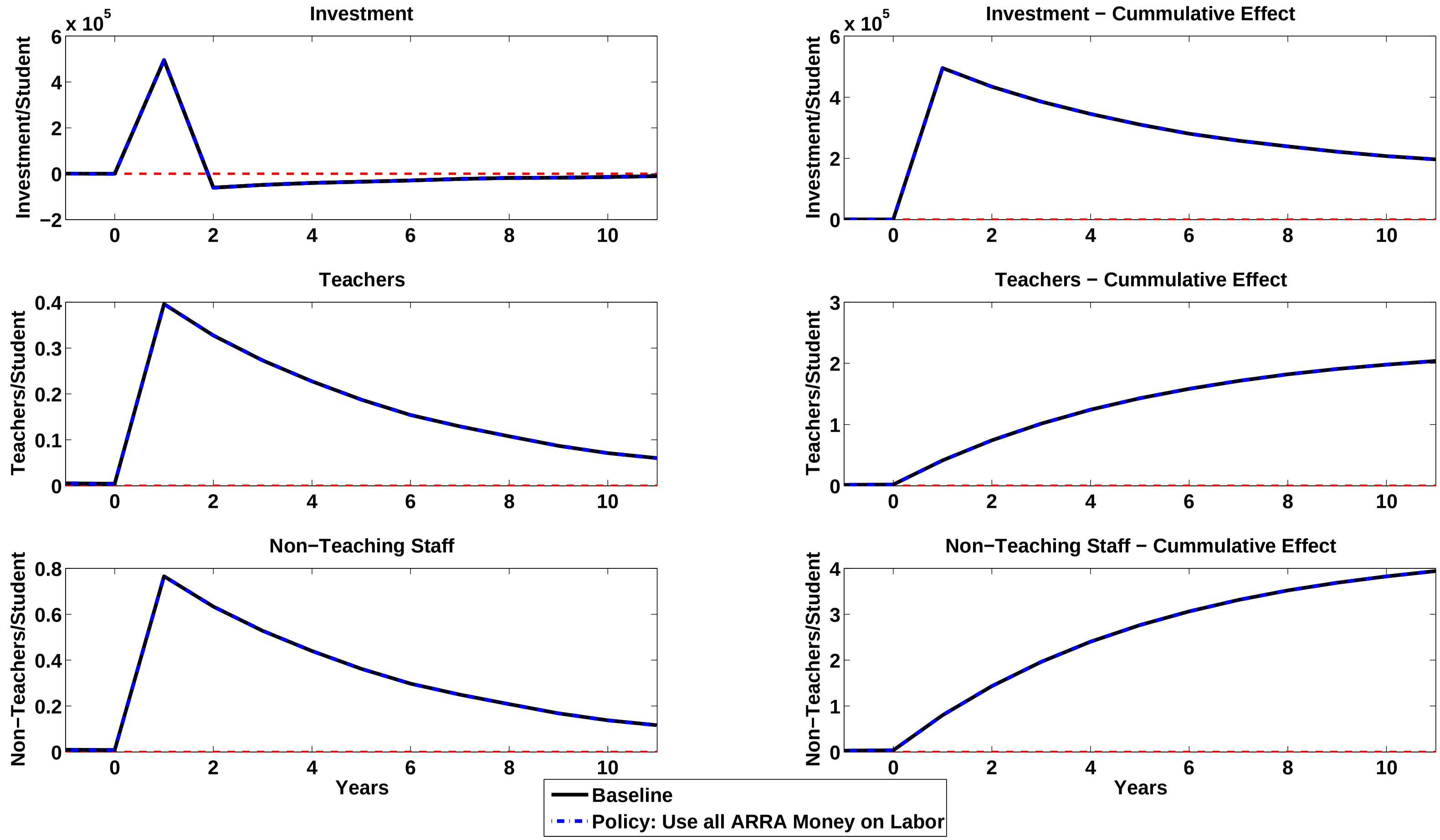


Figure 2: Impulse Responses to ARRA Shock

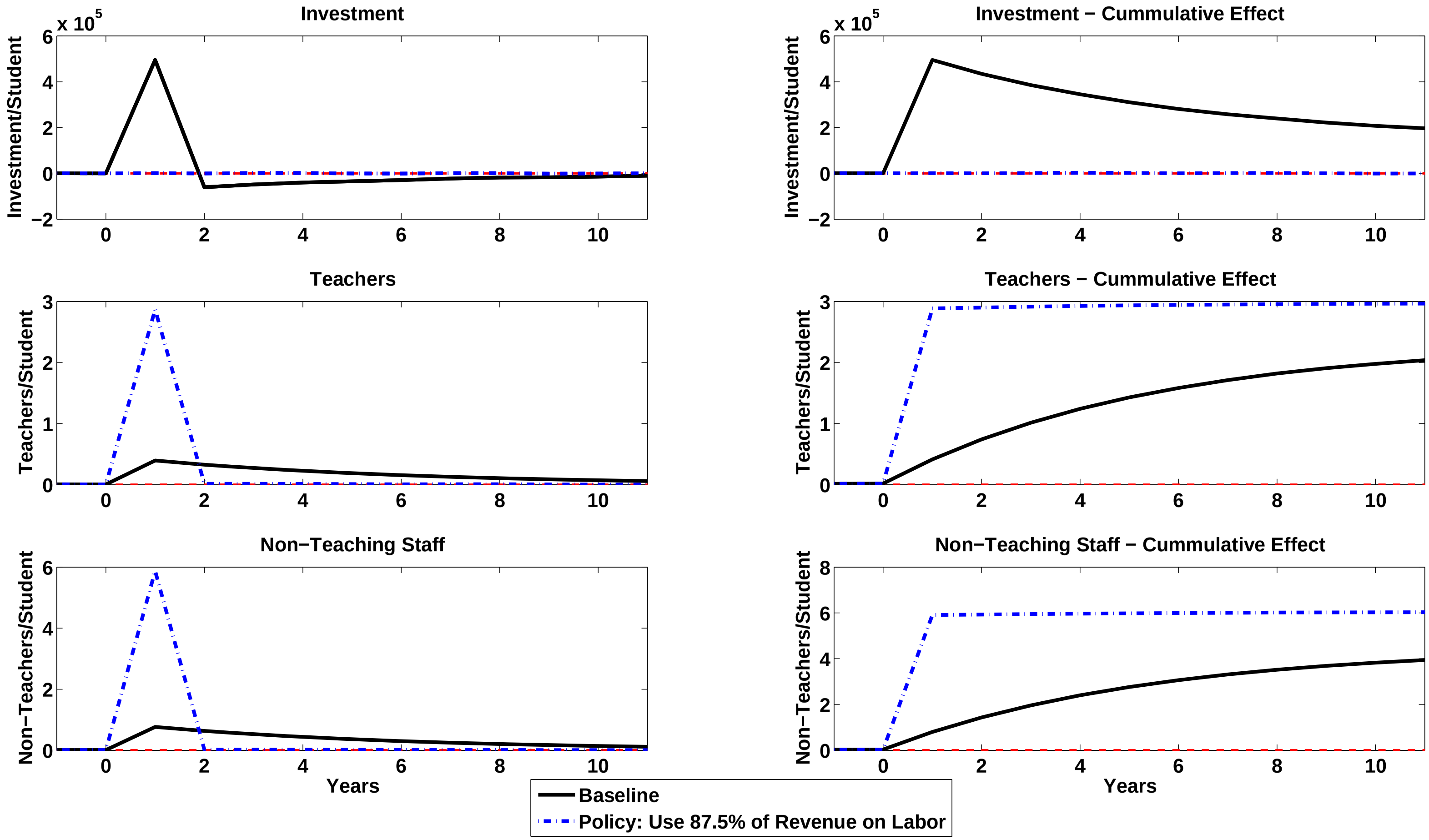


Figure 3: Impulse Responses to ARRA Shock

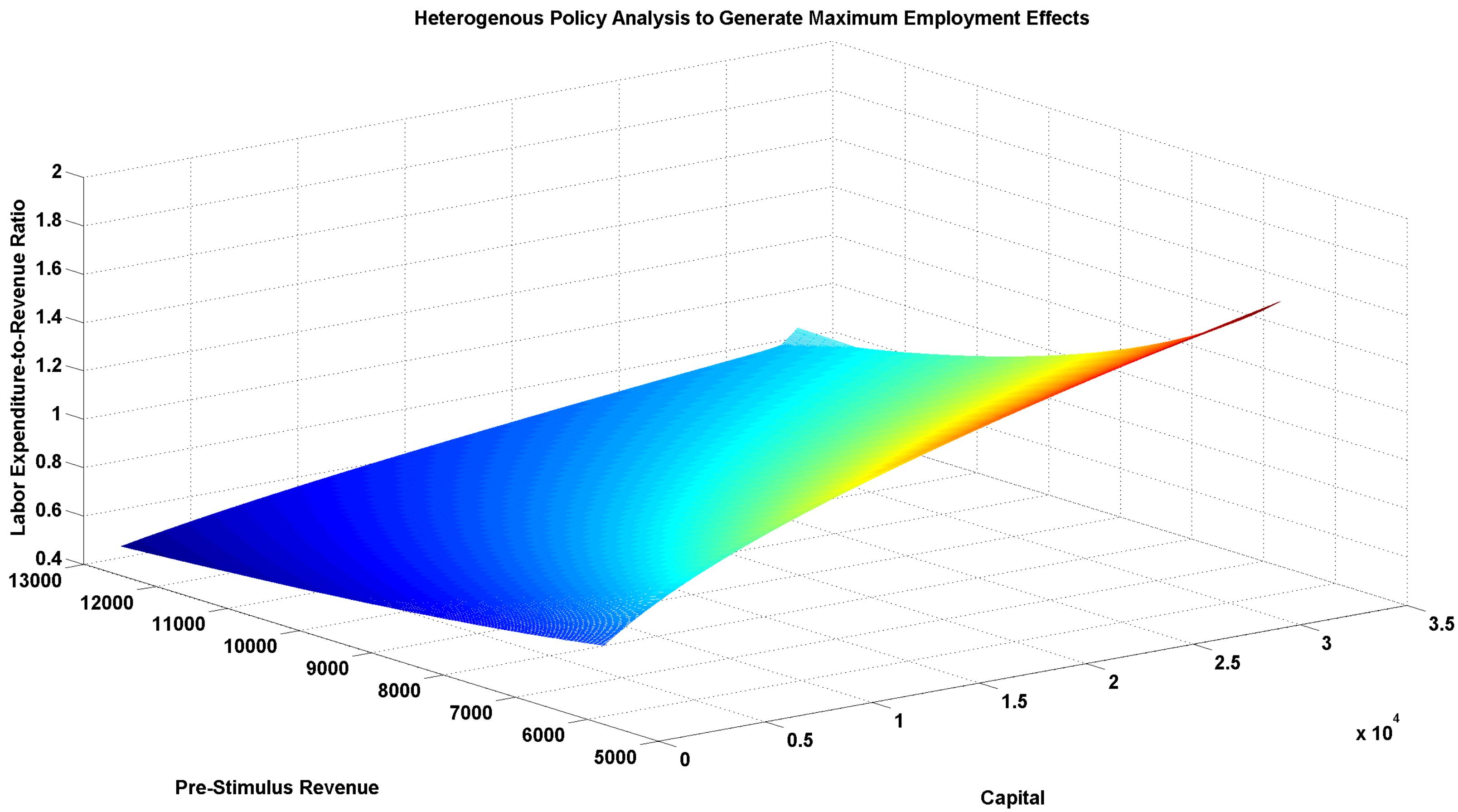


Figure 4: Optimal Policy

## 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 non-teacher 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.

These findings should not be entirely surprising given the decentralized nature of the act's implementation plan. The allocation process was multi-tiered, with local and state governments having latitude regarding 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.[25]

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 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 that we estimate in this paper.

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

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## Appendix

Table E.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 is Recovery.gov.