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Abstract

This paper uses variation across states to estimate the number of jobs created/saved due to the spending component of the American Recovery and Reinvestment Act (ARRA). Our main sources of identification are ARRA highway funding, how much states use inelastic revenue sources, and a political variable. Our benchmark estimates imply that, in December of 2010, the Act created/saved between 158 and 883 thousand state and local government jobs and had no statistically significant effect on private sector employment. The range for private-sector jobs created/saved is between -390 thousand and 1.69 million jobs. Across alternative specifications, such as varying the instrument set or changing the horizon of program evaluation, confidence intervals typically widen; however, the qualitative finding described above is robust. For our benchmark specification, we therefore reject with 99% confidence the President’s Council of Economic Advisors’ claim that the ARRA created/saved between 2.4 and 3.5 million private-sector jobs. One explanation for the potentially different government and private-sector jobs effect is that state and local government jobs were saved because ARRA funds were largely used to offset state revenue shortfalls and Medicaid increases (Fig. A) rather than directly boost private sector employment (e.g. Fig. B). We also show that the President’s Council of Economic Advisors (2009b) failure to include states’ budget conditions in their analysis led it to improperly infer a statistically-significant positive jobs effect of the ARRA’s Medicaid component.

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*Preliminary and not for wide distribution. Comments are welcome; all opinions expressed and errors are ours alone. Acknowledgements appear at the end of the paper. First draft: October 2010. Copyright 2011 by Timothy Conley and Bill Dupor. All rights reserved.

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1 Introduction

As a response to a recession that began in December of 2007, President Barack Obama signed into law The American Recovery and Reinvestment Act, hereafter ARRA, (Public Law 111-5) in February of 2009.\(^1\) It authorized $288 billion for Federal tax cuts and $499 billion in Federal government spending. This paper seeks to understand the causal effect on employment of the government spending component of the ARRA.\(^2\) We estimate how many jobs were created/saved by the Act.\(^3\)

Our benchmark estimates imply that, in December of 2010, the Act created/saved between 158 and 883 thousand state and local government jobs and had no statistically significant effect on private-sector employment.\(^4\) The confidence interval for private-sector employment effect is between -300 thousand and 1.7 million jobs.\(^5\) Across alternative specifications, such as varying the instrument set or changing the horizon of program evaluation, confidence intervals change somewhat and typically widen; however, the qualitative finding described above is robust. For our benchmark specification, we reject with 99% degree confidence the President’s Council of Economic Advisors’ statement that the ARRA created between 2.4 and 3.5 million private-sector jobs in mid-2010.

The ARRA’s spending component was first and foremost a stimulus to state and local governments. Most directly created/saved jobs were in government, which is consistent with our finding of a strong government jobs effect. Thus, a positive private-sector jobs effect must be indirect. While there may be indirect forces towards creating private-sector jobs, there is an opposing force. The boost in government employment generates a partially offsetting negative employment effect on the private sector, known as crowding-out.\(^6\) This effect is likely to be more intense to the extent that state and local governments rely on in low unemployment regions persons and with in-demand skills.

A large fraction of the Federal ARRA dollars was channeled through and controlled by state

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\(^1\)Useful background reading on the ARRA includes Auerbach, Gale and Harris (2010).

\(^2\)We focus on employment rather than: (a) the unemployment rate because of well-known issues related to movements in and out of the labor force, and (b) GDP because it is only available annually for our unit of observation, a U.S. state, and it is subject to long data-collection lags. We focus on government spending because of the relative dearth of this topic relative to research on tax change.

\(^3\)Section 5 discusses other researchers’ estimates of the jobs effects of the ARRA in the context of our findings. Also, several economists have written insightful pieces, in the popular press, on the macroeconomic effects of government stimulus spending, such as Barro (2010), Frank (2009) and Ohanian (2009).

\(^4\)Unless otherwise stated, confidence intervals are at the 90% range.

\(^5\)In an earlier circulated version of our paper (May 2011), we found that the ARRA had a negative jobs effect in some private-service sectors. The change in our results seen in the present version arose from a Federal government data revision, by which the BLS matches the Establishment Survey results to records of businesses’ Unemployment Insurance premiums paid.

\(^6\)Undergraduate textbook discussions of crowding-out, e.g. Krugman and Wells (2009) and Mankiw (2008) cover this phenomenon in the context of private-sector investment rather than employment. For previous research on employment crowd-out of private-sector employment, see Malley and Moutos (1996) and references therein.
Table 1: State government contributed spending and ARRA spending, various categories and in billions of dollars

<table>
<thead>
<tr>
<th>Spending category</th>
<th>ARRA</th>
<th>States-contributed spending</th>
</tr>
</thead>
<tbody>
<tr>
<td>Medicaid</td>
<td>88.6</td>
<td>254.1</td>
</tr>
<tr>
<td>Elementary and secondary education</td>
<td>53.6</td>
<td>434.5</td>
</tr>
<tr>
<td>Highways</td>
<td>28.0</td>
<td>46.8</td>
</tr>
</tbody>
</table>

Notes: ARRA dollars are amounts authorized by the Act. State contributions are 2008 FY multiplied by the number of years that ARRA has spanned, 1.83. Elementary and secondary education of ARRA dollars refers to State Fiscal Stabilization Fund. State-contributed education spending does not include $557 billion 2008 spending by local governments. Sources are Public Law 111-5 (2009), National Association of State Budget Officers (2009) and U.S. Census Bureau (2009).

and local governments. This is important for two reasons. First, it opens the possibility that states might receive different ARRA allocations due in part to differing exogenous capacities to channel or attract Federal funding. Approximately two-thirds of all ARRA spending is formulary, of which there is substantial exogenous state-level variation in formula parameters.

Second, channeling through states creates an environment where Federal dollars might be used to replace state and local spending. The Act legislated ARRA funds to go to state and local governments for specific programs, such as schools in high poverty neighborhoods and highway construction. Importantly, as depicted in Table 1, states and local governments were already spending significant amounts of their own dollars on many of these programs before the ARRA. Often state spending was substantially higher than nominally targeted ARRA funding.

Upon acquisition of ARRA funds for a specific purpose, a state or local government could cut its own expenditure on that purpose. As a result, these governments could treat the ARRA dollars as general revenue, i.e. the dollars were effectively fungible.

Federal aid arrived when state and local governments were entering into budget crises. The solid line in Figure A in the abstract illustrates these budget woes. It plots the non-Federal sales and income tax revenue net of non-Federal government transfers. These combined budgets experienced a sharp and then persistent decline beginning in 2008:Q4. The reduction in consumer purchases and employment reduced the tax base for sales and income tax revenue. Second, non-Federal government transfer expenditures, most importantly from Medicaid, increased over this period.

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7 This included, for example, $86.6 billion to support states’ Medicaid programs, a $53.6 billion ‘State Fiscal Stabilization Fund’ to aid local school districts, $48.1 billion for transportation infrastructure investment, $40 billion for states to pay unemployment benefits, $13 billion for programs supporting public schools with students from low income families, and $6 billion for clean water projects.

8 These together constitute the most cyclical component of non-Federal government finance. The main non-Federal government transfer is Medicaid.

9 For timely background on the states’ budget crisis between 2008 and 2010, see Boyd and Dadayan (2010), Inman (2010) and McNichol, Oliff and Johnson (2010).

10 Medicaid is a U.S. health care program for low income individuals and households.
As the economy worsened, Medicaid participation rates and, thus, the states’ burdens increased. Moreover, state and local governments are, with few exceptions, legally prohibited from borrowing to pay for non-capital expenditures.

The deterioration of the non-Federal government budget position occurred concurrently with an increase in Federal grants (the dashed-dotted line on Figure A), mainly due to the ARRA, of approximately the same amount.\footnote{The spike in Federal aid is less than the authorized $499 billion because several spending categories (such as Department of Energy grants) had largely been unspent through the first quarter of 2010.} In fact, a substantial component of the ARRA was authorized specifically to cover states’ tax losses (through the State Fiscal Stabilization Fund) and the most dramatic cost increases (through support for state Medicaid programs). Aizenman and Pasricha (2011) use aggregate data to show how the ARRA federal expenditure largely only offset state fiscal declines.

States were able to re-purpose some ARRA dollars. For example, despite the fact that the ARRA gave states $22 billion, of the total $28 billion available, through September of 2010 to spend on infrastructure, the number of highway, bridge and street construction workers, nationwide, fell dramatically over the past several years (Figure B in the abstract).\footnote{No doubt part of the decline is due to a fall in local street construction due to a slowdown in home building during the recession. This component is unrelated to highway and bridge spending. Calculating the amount of the employment decline due to reduced street construction versus reduced highway funding (because of ARRA crowding out) will be possible with the 2010 Bureau of Labor Statistics Occupational Outlook, which should become publicly available in mid-2011.}

In our benchmark specifications, we exploit the effective fungibility of ARRA dollars along with an assumption that states spent ARRA dollars to offset lost revenue. Suppose California loses one dollar in sales tax revenue. If at the same time, California receives an additional ARRA dollar and that ARRA dollar is fungible, then we assume California spends the aid dollar for the same purpose it would have spent its just lost tax dollar. Under this scenario, the relevant treatment is ARRA funding net of state budget shortfalls. This presents the opportunity to use exogenous variation in budget shortfalls to identify the effect of ARRA spending.

We use two instruments to capture exogenous variation in capacity to attract/channel ARRA funds and state budget stress. The first is the component allocated via the Department of Transportation for highway and bridge construction. $27.5 billion was allocated for highway improvements. These dollars were allocated by formulary rule to states, based on pre-defined factors, mainly highway-lane miles, highway usage, and each state’s previous contribution to the Federal highway fund.\footnote{The Act specifies a small number of set asides, e.g. $60 million for forest highways on Federal land.} This formula was set several years prior to the ARRA’s passage and was used to disperse previous highway funds. These outlays should be uncorrelated with each state’s short-run budget, employment and general economic situation. Second, we instrument using the political party of the governor. We intend for this to capture political considerations relevant for attracting
ARRA dollars that are plausibly uncorrelated with a state’s particular economic situation. Third, we instrument for state budget stress using the pre-recession fraction of each state's revenue that comes from, short-run, inelastic sources. These sources include, for example, property taxes, revenue from publicly-run universities, hospital, liquor stores, sales taxes on cigarettes and alcohol and revenue from mineral and resource extraction. A state that relies, to a relatively greater extent, on inelastically-sourced revenue will experience less fiscal stress during a recession than a state that relies on other sources, mainly income and general sales taxes.

The next section provides background necessary to understand the Act as well as our approach. Section 3 describes the data and the estimation equation and Section 4 presents our empirical results including the number of jobs created/saved because of the ARRA. Section 5 discusses other estimates of job creation due to the ARRA in the context of our findings. The final Section concludes.

2 Required Background on the Act

2.1 The Legislation

The American Recovery and Reinvestment Act of 2009 (Public Law 111-5) was enacted on February 17, 2009. The Act contains approximately 175,000 words and makes references to hundreds of existing U.S. codes and existing laws. As such, a comprehensive explication of the Act is beyond the scope of this paper.

One paramount feature of the Act is that a large fraction of the Federal dollars are channeled through state and local governments. The Act specifies dollar amounts allocated for various categories and often formula for divvying each categories’ dollars across states; however, local and state governments have much latitude regarding when and on what projects ARRA dollars are spent. Moreover, each state and local government maintained substantial control over how it spent its own non-Federal revenues. This is important because it created an environment where Federal ARRA dollars might be used to replace state and local spending.\(^\text{15}\)

Let us consider a specific section of the Act: highway infrastructure improvement. Title XII of Division A of the Act specifies that $27.5 billion shall be allocated to “restoration, repair, construction and other eligible activities,” where the eligible activities are spelled out in a particular U.S. pre-existing code.

These dollars are divvied up between states based on pre-defined factors, mainly highway-lane miles, highway usage, and each state's previous contribution to the Federal highway fund.\(^\text{16}\) This formula was set several years prior to the Act’s passage and was used to disperse previous highway

\(^{15}\)As Inman (2010) writes, “States are important ‘agents’ for Federal macro-policy, but agents with their own needs and objectives.”

\(^{16}\)The Act specifies a small number of set asides, e.g. $60 million for forest highways on Federal land, $20 million for training as well as Federal Highway Administration overhead costs.
funds. More generally, roughly two-third of all ARRA spending is formulary.

Each state selects highway projects on which to spend its dollars. While the FHWA must approve each project, our reading is that the approval rate has been very high.\textsuperscript{17} The Act does dictate that the FHWA should give priority to “projects that are projected for completion within a 3-year time frame.” The Act also gives a deadline for when grant applications are due, when dollars must be allocated and when the grant dollars must be spent. Agencies provide some guidelines for potential applicants beyond the language of the legislation, e.g. U.S. Federal Highway Administration (2009).

For some components, Federal agencies have additional discretion in allocating amounts. For example, the Act allocates $1.1 billion as grants-in-aid for airports. The Act states: “such funds shall not be subject to apportionment formulas, special apportionment categories or minimum percentages the Secretary shall distribute funds provided under this heading as discretionary grants to airports, with priority given to those projects that demonstrate to his satisfaction their ability to be completed within 2 years of enactment of this Act.”

Each Federal agency, twenty-eight in total, charged with dispersing a fraction of ARRA dollars issues regular “Agency Funding Notification Reports” which summarize the agency’s intention to communicate the availability and requirements an applicant must meet to receive funding. The agency posts the total dollar value of current and past notifications, by state when applicable. State governments, local governments, citizens and companies may apply for ARRA dollars. In this paper, these are referred to as announced dollars. These are compiled by the Recovery Accountability and Transparency Board, which was established by the Act, and posted at this Board’s web site Recovery.gov.

Besides announcements, the Recovery Accountability and Transparency Board also tracks awards and outlays. These come from the “Weekly Financial and Activity Report” made by the participating Departments and Agencies. These reports are also posted on the Recovery.gov web site. Each agency provides a list of awards and the total outlays related to each project. Some awards have either territory codes, such as Puerto Rico, or no code at all. We drop these from the sample. In terms of the chain of events described above, the agency enters a new award once a specific grant has been issued, whether it is formulary or discretionary. Our benchmark specification uses outlaid dollars, and we use announced dollars as an alternative treatment measure.

Outlays are payments from the U.S. Treasury as directed by the managing Federal agency to the grant recipient. Most outlays are paid as reimbursements for expenses the grant recipient has made.

\textsuperscript{17}The Act does specify that priority will go to “projects located in economically distressed areas.” Often, a phrase that might be open to interpretation, such as “economically distressed,” is followed by reference to a specific U.S. Code or Law which defines that phrase.
2.2 Evidence for Fungibility

As stated previously, states have substantial capacity to treat ARRA dollars as fungible. For example, if California receives $100 million ARRA dollars to improve its highways, it might cut its own contribution to the state highway budget by $100 million and use this money to finance pay increases of UC system faculty. Establishing this fungibility is important because it will allow us to use state-level variation in budget differences as one way to infer the effects of ARRA spending on employment—thus providing additional variation besides that from exogenous ARRA spending itself.

Texas provides a case in point. In Texas, ARRA dollars arrived and simultaneously the number of Texas highway, bridge and street construction workers declined. Employment in that sector fell from 34,600 workers in May of 2008 to 28,500 workers in May of 2010. Total capital outlay on highways in Texas (fiscal year ending on August 31) went from $3.38 billion in 2009 to $2.82 billion in 2010. This decrease in state expenditures occurred even though Texas spent $0.70 billion in ARRA highway funds during 2010.\(^{18}\) The Texas government *responded* to its receipt of ARRA highway dollars by cutting Texas’ own contribution to highway spending, which freed up state dollars to boost suffering state finances.

The State of New York provides a second example.\(^{19}\) For the year ending in March 2009, which contains only three months of the ARRA period, the New York Department of Transportation capital project spending was $3.42 billion. For the year ending in May 2010, in which ARRA spending was in full swing, this spending was $3.47 billion (i.e. nearly unchanged). On the other hand, the US Department of Transportation reported that it outlaid $522 million in ARRA monies to New York by May of 2010. Interestingly, the reduction in state transportation dollars simultaneous with its spending of ARRA dollars may not have been planned in advance by the state government; the *planned* 2009-2010 budget allocated $3.95 billion towards transportation capital spending. This was nearly $500 million more than it *actually* spent.

Michigan provides another example. For the fiscal year ending on September 30, 2009, Michigan’s revenue from Federal aid had increased by $189.2 million over the previous fiscal year;\(^{20}\) however, over the same horizon, transportation capital outlays had risen by only $17.4 million. What might explain this gap? Taxes and miscellaneous revenues received by the Department fell by $140.6 million relative to the previous fiscal year. The US DOT reported that it outlaid $110 million to Michigan through September 2009, $105 million of which was FHWA money. In the following year, Michigan reported Federal aid to Michigan for transportation increased by an additional \(^{21}\) As in Texas and New York, ARRA dollars were substituting for Michigan government

\(^{18}\)Only $110 million in ARRA funds were spent in 2009. The budget amounts come from Texas Department of Transportation (2009, 2010)
\(^{19}\)The following numbers are taken from the State of New York (2009, 2010) Enacted Financial Plan reports.
\(^{20}\)See Michigan Department of Transportation (2010).
\(^{21}\)In the fiscal year ending in September of 2010, Michigan Department of Transportation (2011) Michigan re-
Poten and Poten, a private company that collects, analyzes and sells information about the asphalt industry, describes the situation clearly: “The lack of demand for asphalt is largely due to constrained public road funding and a weak private and commercial market for the product. Most state and local governments have major budget problems. Federal funds related to the transportation budget and Stimulus are a critical source of current road funding, but it hardly makes up for the declines from state and local public funding sources, as well as scant private and commercial demand.”

Ohio provides an example where ARRA transportation aid has been received, but total transportation spending or even contracts awarded by the amount of that aid. According to Ohio Department of Transportation (2011), the ARRA provided $1.04 billion in funding across Ohio’s 2009 and 2010 fiscal years. Over the same two fiscal years, the Ohio Department of Transportation’s capital contracts awarded increased, relative to 2008, by only roughly $515 million summed across both years; moreover, capital expenditures, as opposed to the value of contracts awarded, fell in 2009 and 2010 relative to their 2008 level.

As further evidence that ARRA highway dollars did not translate into a significant increase in highway construction nationwide, consider the number of rebar workers employed in bridge and road building over the past few years. Rebar, i.e. steel bars and rods used to reinforce concrete, is a key material in building highways and bridges. Yet, after the ARRA’s implementation, there was only a one percent increase in the number of reinforcing iron and steel rebar workers (SOC 47-2171) in the highway, bridge and construction industry (NAICS 237300). Specifically, the number of these workers was 1,870 in May of 2008 and 1,890 in May of 2010.

The Medicaid component of the Act provides another channel for fungibility. A Council of Economic Advisors (2010, pg. 7) report states that ARRA Medicaid dollars were “intended to boost the level of discretionary funds available to states and not simply to relieve Medicaid burdens.” The Act does reference the proper use of funds to support states’ Medicaid program. For example, Section 5001(f)(3) of the Act reads “A State is not eligible for an increase [in Federal Medicaid funds] . . . if any amounts attributable (directly or indirectly) to such increase are deposited or credited into any reserve or rainy day fund.” The dire straits of the typical state’s budget gave it little incentive to stockpile the aid; therefore, the above requirement was unlikely to bind.

For some components of the stimulus program, language in the Act does try explicitly to prohibit states from cutting state funding upon the receipt of ARRA dollars. U.S. General Accountability

22This quote appears in the August 16, 2010 issue of Asphalt Weekly Monitor in the article headlined “US Asphalt Prices Slide Despite It Being Peak Demand Season.”

23The 2010 fiscal year ended on June 30, 2010.

Office (2009) states that $101 billion of the spending, including funds for transportation, education and housing, have such restrictions in place. For example, each state governor was required to certify, by March 19, 2009, to the USDOT that the state would maintain a certain contribution to its spending in an area as a condition of accepting ARRA transportation funds. This is part of a “maintenance of effort” requirement of the Act.

However, the governors were not required maintain their pre-Act levels of spending. Rather, it was acceptable for a governor to promise to spend less than their expenditure in recent years if she could justify the reduction based on other fiscal considerations, such as falling tax revenues.\(^{25}\)

Moreover, the U.S. Department of Transportation (2010) reported that twenty-one states were failing to meet maintenance of effort requirements as of November 2010. The penalty for a state that fails certification or does not meet that certification’s obligations is a prohibition from participation in redistribution of unobligated funds set to occur on August 1, 2011.

The use of matching grants, a potential tool to discourage crowding-out, is almost entirely absent from the Act. The section on highway infrastructure investment in the Act states “the Federal share payable on account of any project or activity carried out shall be, at the option of the recipient, up to 100 percent of the total cost thereof.”

Next, the section on grants to support schools with a high poverty population (i.e. Title I schools), states the funds shall serve to supplement and not supplant planned expenditures for such activities from other Federal, State, local and corporate sources.\(^{26}\) This restriction, however, is unlikely to bind states that have seen declining tax revenues. A guide for local education agencies provided by U.S. Department of Education on ARRA funding states that the supplement not supplant restriction would not be violated if there was “a reduction in the amount of non-Federal funds available to pay for the activities previously supported by non-Federal funds.”\(^{27}\)

For a few components of the ARRA, the “effective fungibility” we will sometimes impose on the econometric model may be inappropriate. For example, the Act authorized $6 billion for clean-up of nuclear waste sites. Since states spent very little on nuclear clean up before the ARRA, those state governments could not free up state dollars by cutting their own spending in response to this $6 billion.

**GRAMLICH, GRAMLICH, GRAMLICH**

Prior to the Act, researchers had studied whether Federal grants crowd out state and local spending. Early analysis by Bradford and Oates (1971a) shows that crowding out occurs in a simple political economy model. Empirical work that followed, surveyed in Hines and Thaler

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\(^{25}\)The issue of maintenance of effort is complicated. The language of the Act and administrative guidances that followed appear to have interpretations that differ across Federal agencies.

\(^{26}\)Similar language requiring that ARRA dollars ‘supplement rather than supplant [non-Federal] funding’ appears in six other sections of the Act, which deal with airports, Amtrak, public housing, child care assistance for low income families, community college and career training as well as community economic development. The total dollars subject to this qualification is very small as a fraction of the Act’s total dollars.

(1995), finds evidence against crowding out, which has been termed the “flypaper effect.” When a higher-level government issues grants for a particular purpose to a lower-level government, then this money sticks like flypaper towards its intended purpose, with little or no reduction in the lower-level government’s contribution. Knight (2002) shows that, on the other hand, that after controlling for a particular form of endogeneity, the flypaper effect is not present in Federal Highway Aid from 1983 through 1997. In the context of our paper, it is worth noting that we are unaware of any studies on the flypaper effect when state and local governments have been under the tremendous budget pressure witnessed during the most recent recession.

3 Statistical Specification

We use the Generalized Method of Moments on a panel of states to estimate a linear model of employment growth as a function of state budget loss, ARRA aid and ancillary variables. Table 2 contains summary statistics for all variables used. The dependent variable is $EMPLOY_{j,t}^i$, the growth rate of state $j$ employment in sector $i$ ending in month $t$. Here $i$ denotes sector and $j$ denotes state. The start point for the growth rate is February of 2009. Our employment data is the number of seasonally-adjusted payroll employees and comes from the Bureau of Labor Statistics Establishment Survey.

We apply our analysis to three distinct employment sectors rather than total employment because of the large differences in trends across the sectors over the past decade. The sectors are: state and local government, goods-producing and services.\(^{28}\)

Differences in trends across the sectors can be seen in Figure 2, which plots national totals where the initial point is normalized to 100 for each sector. Government employment has fared relatively well during the recession. Employment in the goods-producing sector has fallen most dramatically during the recession, which has been part of a continual decline over the preceding decade.

Our first key regressor is $AID_{j,t}$—the ratio of ARRA dollars actually spent relative to 2008 state government tax revenue for state $j$ through period $t$. We also estimate the model using obligated ARRA dollars (see Section 2).\(^{29}\)

Our second key regressor is $LOSS_{j,t}$, measured as the decrease in the sum of the state $j$ tax revenue change net of its Medicaid cost increase through period $t$, relative to 2008 tax revenue for state $j$. Finally, when it does not cause confusion, we will sometimes omit subscripts $j$ and $t$ as well as superscript $i$. A positive number for a state means its budget has deteriorated. State tax data is from the Census Bureau Quarterly Summary of State Tax Revenue. State-level Medicaid

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\(^{28}\)Educators employed by state and local governments count toward the government sector. Federal workers count towards service employment.

\(^{29}\)We try two different measures because macro analyses of the effects of fiscal policy (such as Mertens and Ravn (2009), Leeper, Walker and Yang (2009) and Ramey (2010)) often find that whether measured fiscal shocks in vector-autoregressions are unanticipated or anticipated has important implications for estimated impulse responses.
Table 2: Summary statistics

<table>
<thead>
<tr>
<th>Variable</th>
<th>Mean</th>
<th>Stdev.</th>
<th>10th perc.</th>
<th>90th perc.</th>
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<tbody>
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<td><strong>Economic and financial variables</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
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<tr>
<td>Outlay AID</td>
<td>0.079</td>
<td>0.016</td>
<td>0.059</td>
<td>0.094</td>
</tr>
<tr>
<td>Obligation AID</td>
<td>0.107</td>
<td>0.020</td>
<td>0.086</td>
<td>0.125</td>
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<tr>
<td>LOSS</td>
<td>0.056</td>
<td>0.024</td>
<td>0.028</td>
<td>0.076</td>
</tr>
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<td>Government employment growth (percent)</td>
<td>-1.31</td>
<td>2.32</td>
<td>-4.95</td>
<td>1.69</td>
</tr>
<tr>
<td>Services employment growth (percent)</td>
<td>-1.03</td>
<td>0.90</td>
<td>-2.21</td>
<td>0.05</td>
</tr>
<tr>
<td>Goods-producing employment growth (percent)</td>
<td>-10.91</td>
<td>4.67</td>
<td>-14.53</td>
<td>-7.37</td>
</tr>
<tr>
<td>2009 Government employment (millions)</td>
<td>0.426</td>
<td>0.424</td>
<td>0.088</td>
<td>0.716</td>
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<tr>
<td>2009 Services employment (millions)</td>
<td>1.999</td>
<td>1.999</td>
<td>0.419</td>
<td>4.147</td>
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<tr>
<td>2009 Goods-producing employment (millions)</td>
<td>0.420</td>
<td>0.397</td>
<td>0.063</td>
<td>0.842</td>
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<td>ARRA outlaid dollars (billions)</td>
<td>6.388</td>
<td>6.959</td>
<td>1.149</td>
<td>13.104</td>
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<td>ARRA announced dollars (billions)</td>
<td>8.532</td>
<td>9.282</td>
<td>1.769</td>
<td>16.873</td>
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<td><strong>Instruments</strong></td>
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<td>USDOT obligated aid</td>
<td>0.02</td>
<td>0.01</td>
<td>0.01</td>
<td>0.02</td>
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<tr>
<td>Intensity of inelastically-sourced revenue (percent)</td>
<td>75.100</td>
<td>6.995</td>
<td>67.577</td>
<td>82.244</td>
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<tr>
<td>Democratic governor (fraction)</td>
<td>0.54</td>
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<td></td>
<td></td>
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<tr>
<td><strong>Other variables</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
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<tr>
<td>Max monthly unemployment insurance (dollars)</td>
<td>435.22</td>
<td>131.79</td>
<td>275.00</td>
<td>572.00</td>
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<tr>
<td>2010 oil production (barrels per capita)</td>
<td>3.63</td>
<td>7.48</td>
<td>0.00</td>
<td>14.74</td>
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<tr>
<td>House price growth, 2003 to 2006 (percent)</td>
<td>0.23</td>
<td>0.13</td>
<td>0.10</td>
<td>0.42</td>
</tr>
<tr>
<td>House price growth, 2006 to 2008 (percent)</td>
<td>-0.05</td>
<td>0.14</td>
<td>-0.27</td>
<td>0.07</td>
</tr>
<tr>
<td>Northeast (fraction)</td>
<td>0.22</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>West (fraction)</td>
<td>0.22</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Midwest (fraction)</td>
<td>0.22</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>Observations</strong></td>
<td>46</td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Notes: The job growth data is from non-farm employment. The four lowest population states (i.e. Alaska, North Dakota, Vermont and Wyoming) excluded from sample. AID, LOSS and employment growth rates above are through December 2010.
Figure 1: Revenue-based and employment-based sizes of governments (state plus local governments)

Notes: Excludes four smallest states. Revenue equals all state and local government revenues in 2008.

data from the National Association of State Budget Officers is calendar-year annual. Details on these data series appear in the Appendix.

We scale ARRA dollars and the decline in states’ budget positions by the size of state government rather than the state population because: (i) ARRA funds were channeled largely though the state and local governments that, in turn, used this aid to cover functions that otherwise may have been cut, (ii) the size of government varies substantially across states. Figure 1 shows substantial cross-state differences in both the size of governments and the number of government employees. Intuitively, even if two states have the same population, each dollar of aid is likely to have a greater impact on a state with a small government relative to a state with a large government. For example, consider the alternative scaling: dividing a state’s ARRA outlays by its population. This alternative scaling might be misleading because the cost of creating/saving a government job can differs states. For example, according to the May 2010 Occupational Employment and Wage Estimates, secondary school teachers in Massachusetts earn 17% more than their counterparts in Georgia.

Using by-population scaling would not reflect of how wages differ across states (in particular for government workers) and how governments have different overhead costs. Nonetheless, one alternative to our benchmark specification, reported in Section 4.2 will scale aid and loss by population rather than state revenue. Our benchmark results are robust.

Our benchmark estimation equation exploits the fungibility of ARRA aid:

\[ EMPLOY = a \times (AID - LOSS) + c' \times ANC + e \]  

\( AID - LOSS \) is referred to as the \( OFFSET \) later in the paper, because fungibility implies that each state used its aid to first and primarily offset its losses of revenue.
Here $\text{ANC}$ is a column vector of state-specific ancillary regressors: two lags of employment growth, the pre-recession employment level, the maximum monthly unemployment insurance payment, three region dummies, state population, two house price growth rates (one for the run-up period between 2003 and 2006 and one for the pre-recession decline between 2006 and 2008), 2010 crude oil production per capita and a constant.

When estimating (3.1) we are assuming that, at the margin, spend an additional thousand dollars of ARRA aid in the same way it would spend an additional thousand dollars it received in state tax revenue. In turn, the result of ARRA outlays should have the same effects on state employment as a reduction in the budget loss that occurs when tax revenues are higher.

Figure 3 contains a scatter plot of each state’s $\text{LOSS}$ and $\text{AID}$. Note the substantial differences across states on both dimensions. New Mexico (NM) appears towards the lower-right corner of the figure. It has a $\text{LOSS}$ of 0.073, which means that it experienced a 7.3% decline in its budget position due to tax losses and Medicaid increases. It has an $\text{AID}$ of 0.057, which means that the ARRA outlays boosted the state’s budget by 5.7% relative to its pre-recession size. In terms of resources for the New Mexico government to operate, the ARRA dollars were insufficient to cover its losses. Its $\text{OFFSET} (= \text{AID} - \text{LOSS})$ equals -1.6%.

Rhode Island (RI), on the other hand, has fared relatively well during the recession, appears in the upper-left corner. Rhode Island has a $\text{LOSS}$ of 0.028, which means that it experienced only a 2.8% decline in its budget position due to tax losses and Medicaid increases. It has an $\text{AID}$ of 0.068, which means that the ARRA boosted the state’s budget by 6.8% relative to its pre-recession size. In terms of resources for the government to operate, the ARRA dollars have more than compensated for the fiscal loss due to the recession in South Dakota. Its $\text{OFFSET} (= \text{AID} - \text{LOSS})$ equals 0.191 or 19.1%.

The 45 degree line is added to the figure to demarcate states that had greater outlays than losses (above the line) from states that had greater losses than outlays. In a related paper, Cogan and Taylor (2010a) provide evidence that summed across all state and local governments, there was positive accumulation of financial assets during the ARRA period. This could be due to the fact that states above the forty-five degree line found ways to save (because of fungibility) for precautionary motives rather than spend ARRA aid. This precautionary savings would undo the intended purpose of the ARRA and underscores the need to include states’ budget positions in any analysis of the jobs effect of the ARRA.

In an alternative specification, we do not impose fungibility

$$\text{EMPLOY} = b \times \text{AID} - d \times \text{LOSS} + k' \times \text{ANC} + e \tag{3.2}$$

We will be able to assess whether the fungibility restriction, used in the benchmark specification, is rejected by the data using standard hypothesis test for whether $b = d$ from (3.2). We will find

---

30See also Cogan and Taylor (2010b).
Figure 2: Monthly employment (seasonally adjusted), January 2001 through December 2010, by sector

Note: Index = 100 in January 2001.

Figure 3: Each state’s revenue loss net Medicaid increase and ARRA outlays, through December 2010, relative to its pre-recession revenue, i.e. LOSS and AID

Notes: The dashed-dotted line is the 45 degree line with a zero intercept. Excludes four smallest states.
that the restriction is almost never rejected.

Next, we must address endogeneity. The equation’s error term, the shock to employment growth, could reasonably be conjectured to be correlated with $AID$ and $LOSS$. For example, a negative shock to employment growth in a state might: (i) increase $AID$ if that state received more ARRA dollars because its economy was in worse shape; (ii) increase $LOSS$ if a worse employment situation resulted in lower state tax receipts. We use instruments to control for endogeneity.

3.1 Instruments

To achieve identification, we first exploit the ARRA’s purpose, apart from stimulating a weak economy, of improving the nation’s infrastructure. Our first instrument is based on dollars allocated by the Federal Department of Transportation. Roughly $48$ billion of ARRA funds were allocated for improving transportation infrastructure, with the largest share going to highway, bridge and intercity rail construction. Much of this was allocated by formula. For example, the Federal Highway Administration, an agency within the Department of Transportation, was authorized by the ARRA to apportion $27.5$ billion. Criteria of the FHWA formula for appropriation to the states include: each state’s share of total eligible highway lane miles (which counts for $13\%$); each state’s share of total vehicle miles traveled on eligible highways ($20\%$); each state’s share of dollar contributed to the Highway Trust Fund paid by highway users ($17\%$).

The contribution of DOT dollars to cross-state variation in $AID$ is not due to particular economic conditions of any state during the great recession. As such, endogeneity that may be present between the error term and $AID$ is likely to be orthogonal to the component for transportation infrastructure.

Specifically, our instrument is each state’s ARRA dollars available from the Federal DOT divided by that state’s tax revenue. We call this USDOT-obligated aid. Figure 4 contains a scatter plot of $AID$ versus the highway instrument. It demonstrates a strong positive correlation, $0.49$, between these two variables. Note that South Dakota is a substantial outlier. The numerator in the highway instrument for South Dakota is large because there are two Federal interstate highways (I-90 East/West and I-29 North/South) that each run from one side of the state to another as well as many state highways. The denominator in the highway instrument is low mainly because of that state’s low population. Even excluding South Dakota, the correlation is still large at $0.39$.

The second instrument is the ratio of inelastically-sourced revenue to all revenue of each state’s combined local and state governments. We refer to this as the inelastically-sourced revenue ratio. These include property taxes, selective sales taxes (such as tobacco and alcohol), revenue derived from mineral and resource extraction, as well as revenues from publicly-run universities, hospitals, airports and liquor stores.\footnote{A complete list of these components appears in the data appendix.} Note that property taxes are relatively acyclical because most homeowners continue to make their mortgage/insurance/property tax payments even during housing
Figure 4: USDOT-obligated aid versus the highway instrument, by state

Notes: The highway instrument is the obligated Federal DOT dollars from ARRA relative to state tax revenue; excludes four smallest states.

Figure 5: LOSS versus inelastically-sourced revenue ratio instrument, by state

Notes: Inelastically-sourced revenue ratio is the ratio of state revenue from inelastic sources to all state revenue (state plus local governments combined); excludes four smallest states.
downturns. Also, the downward reassessment of house values, which cause property tax revenues to decline, happens with a substantial lag for most of the housing stock. We calculate this ratio for each state based on the Census Bureau’s 2007 Census of Governments.

The usage of these sources as opposed to the more cyclical income and general sales taxes vary significantly across states. The inelastically-sourced revenue intensity is determined by historical, legislative decisions and other factors that are likely to be uncorrelated with the state-level variation over the business cycle.

Thus, a part of the variation in LOSS is due to the historical evolution of each state’s revenue system. The correlation between the two variables is -0.46 and the corresponding scatter plot appears in Figure 5.

Our third instrument is an indicator equal to one if a state’s governor is a Democrat. Why might Democratic governors be more likely to have larger offsets? The rate of spending of ARRA dollars by a state, and therefore the amount of outlays, may be larger when Democrats are in charge. There are several well-publicized instances where Republican governors, including Jindal of Louisiana, Perry of Texas and Sanford of South Carolina, initially refused to accept parts of ARRA aid.\footnote{See “They’re Saying No to the Cash, but Talk is Cheap” in the Washington Post (2009).}

\section{The Act’s Jobs Impact}

\subsection{Benchmark Estimates}

Table 3 reports the jobs effect of ARRA aid for total, private-sector and government employment.\footnote{Private-sector estimates are computed from the separate estimates of services and goods-producing sectors. Total employment estimates are similarly computed from the government, goods-producing and service sectors.} These are our benchmark estimates. Each imposes fungibility, uses ARRA outlays, the forty-six largest states, all three instruments and all control variables. The table reports estimates of the thousands of jobs that existed in December 2009, June 2010 and December 2010 that would have not existed (i.e. jobs saved or created) had the Act not been implemented.\footnote{To calculate the number of jobs created/saved, for each state we compute the product of the state’s offset times the state’s pre-recession employment level times the point estimate from either (3.1), with fungibility, or (3.2), without fungibility. Then, we sum across all states to find the total jobs effect reported in the table.} The pair of numbers beneath each point estimate correspond to a 90\% confidence interval. A negative number implies that the ARRA, on net, destroyed or prevented employment growth in that sector over that period.

First, our point estimate states that in December of 2010 state and local government employment was 521 thousand persons greater than it would have been in absence of the Act, as seen in Table 3, with a confidence interval between approximately 160 and 880 thousand.

This estimate has a sensible explanation. In states that received relatively less ARRA aid and/or else had greater budget losses, government employment was cut or did not significantly expand. In the counterfactual world without the Act, all states would have been forced to take

\addcontentsline{toc}{section}{4 The Act’s Jobs Impact}
Table 3: Number of jobs created/saved as a result of ARRA, benchmark results, reported as thousands of jobs

<table>
<thead>
<tr>
<th></th>
<th>All sectors</th>
<th>Private</th>
<th>Government</th>
</tr>
</thead>
<tbody>
<tr>
<td>December 2009</td>
<td>925</td>
<td>687</td>
<td>238</td>
</tr>
<tr>
<td></td>
<td>(73, 1778)</td>
<td>(-64, 1437)</td>
<td>(-33, 510)</td>
</tr>
<tr>
<td>June 2010</td>
<td>802</td>
<td>389</td>
<td>413</td>
</tr>
<tr>
<td></td>
<td>(-251, 1855)</td>
<td>(-537, 1315)</td>
<td>(76, 750)</td>
</tr>
<tr>
<td>December 2010</td>
<td>1215</td>
<td>695</td>
<td>521</td>
</tr>
<tr>
<td></td>
<td>(116, 2314)</td>
<td>(-300, 1690)</td>
<td>(158, 883)</td>
</tr>
</tbody>
</table>

Notes: Negative numbers correspond to jobs destroyed/forestalled. Using all three instruments, outlaid-based offset, fungibility imposed and 46 states. Ninety-percent confidence intervals appear in parenthesis.

the same action of firing and not filling job openings—resulting in significant government jobs lost. Based on the additional other months we estimate (December of 2009 and June of 2010), we see the government jobs effect grows over time. Note that in December of 2009, the lower bound of the confidence interval is approximately zero.

Second, the point estimate for the December 2010 private-sector jobs effect is positive 695 thousand persons, with a confidence interval of negative 300 thousand and positive 1.69 jobs. The negative lower bound on private-sector employment means that, with greater than 90% confidence, we reject the hypothesis that ARRA outlays resulted in net private-sector job creation. This negative lower bound holds for December 2009 and June 2010 as well.

There are two key reasons that the estimates in the lower half of the confidence interval, i.e. between a negative effect and less than a million private-sector jobs, might be explained by the data: (i) the intensive spotlight of the ARRA on funding state and local government programs, and (ii) crowding-out.

Evidence of reason (i) was described in detail in Sections 1 and 2. Over one-hundred billion dollars of stimulus spending was targeted directly at the government-sector rather than the private sector, e.g. the State Fiscal Stimulus Fund, grants to state and local law enforcement, and support for public schools with students from low income families. These dollars were used largely to pay salaries to avoid government workers layoffs. Moreover, dollars not directly targeted to the government sector were administered through state and local governments. Given the effective fungibility of these ARRA, government could use these dollars in the government sector as well.

Reason (ii) is crowding-out. A portion of the growth in government employment from the ARRA’s government-stimulus component was sourced from a pool of potential workers with skills and from regions where there was still private-sector demand. By creating/saving government jobs, the ARRA prevented some private-sector job creation. The size of this portion remains an open
question and the answer is crucial for understanding the Act’s jobs effects.\textsuperscript{35}

We note that government workers tend to be well educated. In 2006, the most recent available data, 49\% of state and 47\% of local government workers had at least a bachelor’s degree;\textsuperscript{36} for private-sector workers, this proportion is only 25\%. The labor market for well-educated individuals was relatively strong during and after the recession. In June of 2011, the unemployment rate for the college-educated was 4.4\% versus 9.2\% for high school graduates without college degrees.\textsuperscript{37}

Even apart from government employment crowding out, to the extent that a private-sector job directly funded by ARRA spending were filled by an in-demand potential worker, there may be zero net private employment increase from this ARRA job. Jones and Rothschild (2011a) survey finding, that approximately one-half of the individuals filling positions directly created by the ARRA were leaving other jobs, is germane to this point.\textsuperscript{38}

Next, although our benchmark estimate does not reject the hypothesis of zero private-sector job creation, it does reject the President’s Council of Economic Advisor’s private-sector job creation estimate as being too high. The confidence interval’s upper bound for our benchmark case, for December 2010, is that 1.7 million private-sector jobs were created/saved. This upper bound is well below the President’s Council of Economic Advisors (2010) estimate that the ARRA created/saved between 2.4 and 3.5 million private-sector jobs over this period.\textsuperscript{39}

Next, Table 4 reports the jobs effect when fungibility is not imposed. That is, we estimate 3.2 which does not require that $b = d$. In words, the effect on employment growth of an exogenous increase in tax revenue, or reduction in Medicaid cost, is not required to have the identical effect as an increase in ARRA aid.

There are three differences relative to the case with fungibility. First and most importantly, confidence intervals widen dramatically. For example, the confidence interval on the jobs effect summed across sectors in December 2010 is $[116,2314]$ thousand with fungibility and $[-548,3296]$ thousand without fungibility. This widening occurs for each sector and for each month estimated. From this, we learn that studies that rely solely on cross-state variation in ARRA aid, without additional restrictions such as fungibility, may be unable to estimate jobs effects that are sufficiently precise to be considered useful to policy-makers. This lack of precision (if we do not impose fungibility) is a feature of every existing study that uses cross-state variation to this purpose.\textsuperscript{40}

\begin{thebibliography}{99}
\bibitem{fn35}The magnitude of this type of crowding-out is closely related to the labor market ‘mismatch’ issue which has been raised, for example, by Federal Reserve Bank President of Minneapolis Bank Narayana Kotcherakota in several speeches.
\bibitem{fn36}Both rates apply to high school graduates 25 years or older. The former number applies to those with at least a bachelor’s degree and the latter applies to those without bachelor degrees (Bureau of Labor Statistics (2011)).
\bibitem{fn37}See also Jones and Rothschild (2011b).
\bibitem{fn38}Regarding the CEA’s private-sector jobs estimate, note that Table 4 of Council of Economic Advisors (2009a) estimates that 97\% of all ARRA created/saved jobs are in the private sector. Next, Table 9 of Council of Economic Advisors (2011) reports estimates that in the second quarter of 2010 jobs effect across all (non-farm) sectors was 2.51 million jobs, using its projection approach, and 3.61 million jobs, using its model approach.
\bibitem{fn39}None of these studies impose additional restrictions (e.g. imposing fungibility) to increase precision of estimates.
\end{thebibliography}

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Table 4: Number of jobs created/saved as a result of ARRA, benchmark specification except fungibility not imposed, reported as thousands of jobs

<table>
<thead>
<tr>
<th></th>
<th>All sectors</th>
<th>Private</th>
<th>Government</th>
</tr>
</thead>
<tbody>
<tr>
<td>December 2009</td>
<td>1637</td>
<td>1637</td>
<td>0</td>
</tr>
<tr>
<td></td>
<td>(-50, 3323)</td>
<td>(113, 3160)</td>
<td>(-606, 606)</td>
</tr>
<tr>
<td>June 2010</td>
<td>2213</td>
<td>1503</td>
<td>710</td>
</tr>
<tr>
<td></td>
<td>(-664, 5089)</td>
<td>(-961, 3966)</td>
<td>(-277, 1698)</td>
</tr>
<tr>
<td>December 2010</td>
<td>1374</td>
<td>1208</td>
<td>167</td>
</tr>
<tr>
<td></td>
<td>(-548, 3296)</td>
<td>(-484, 2899)</td>
<td>(-520, 853)</td>
</tr>
</tbody>
</table>

Notes: Negative numbers correspond to jobs destroyed/forestalled. Using all three instruments, outlaid-based offset, fungibility not imposed and 46 states. Ninety-percent confidence intervals appear in parenthesis.

discuss these other papers in Section 5.

The second difference is that point estimates on the private-sector jobs effect increase. For example, the jobs effect summed across sectors in December of 2010 increases from 695 to 120 thousand by no longer imposing fungibility.

Third, the government employment confidence interval in Table 4 includes substantially negative jobs saved/created. This runs counter to the fact that the Federal government reports that hundreds of thousands of public-school teacher and support staff jobs were directly saved by the ARRA. For example, the official ARRA web site Recovery.gov reports that in the fourth quarter of 2010, the State Fiscal Stabilization Fund, Special Education Grants to States and Title I Grants to Local Educational Agencies together created/saved over 290 thousand jobs directly.

REMAINDER OF THIS SUBSECTION ON ELASTICITIES MUST BE REWRITTEN

A second, but equivalent, way to report the jobs effect is as the elasticity of employment growth with respect to ARRA aid. This coefficient, for each of three sectors in December of 2010, appears in Table 5 for the goods-producing, service and government sectors. It reports the estimates for two cases: fungibility is imposed, $a$ from equation (3.1) and fungibility is not imposed, $b$ from equation (3.2). For example, this elasticity equals equals 0.420, with a standard error of 0.201, for the government sector when fungibility is imposed. *This means that a one-percent increase in ARRA outlays relative to the state’s pre-recession revenue results in employment that is 0.42% greater in June of 2010.* This is positive and statistically greater than zero. The point estimates on the goods-producing and services sectors are positive but not statistically greater than zero.

Table 5 reports the employment effect as elasticities rather number of jobs, when fungibility is not imposed. All three coefficient estimates on $AID$ are positive but none have $t$-statistics greater than 1.5. Without fungibility, the jobs effect estimates are very imprecise. This imprecision is a feature not only of this specification, but as we discuss below, of every other study based on
Table 5: Response of employment growth through December 2010 to state government losses and outlaid ARRA money, reported as elasticities

<table>
<thead>
<tr>
<th></th>
<th>Goods-producing</th>
<th>Services</th>
<th>Government</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Panel A: Using instruments (benchmark)</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>Fungibility imposed</td>
<td></td>
<td></td>
</tr>
<tr>
<td>AID - LOSS</td>
<td>0.013</td>
<td>0.097</td>
<td>0.351</td>
</tr>
<tr>
<td></td>
<td>( 0.235)</td>
<td>( 0.059)</td>
<td>( 0.144)</td>
</tr>
<tr>
<td></td>
<td>Fungibility not imposed</td>
<td></td>
<td></td>
</tr>
<tr>
<td>AID</td>
<td>0.262</td>
<td>0.118</td>
<td>0.112</td>
</tr>
<tr>
<td></td>
<td>( 0.383)</td>
<td>( 0.103)</td>
<td>( 0.272)</td>
</tr>
<tr>
<td>-LOSS</td>
<td>-0.225</td>
<td>0.077</td>
<td>0.577</td>
</tr>
<tr>
<td></td>
<td>( 0.370)</td>
<td>( 0.097)</td>
<td>( 0.264)</td>
</tr>
<tr>
<td><strong>Panel B: Least-squares</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>Fungibility imposed</td>
<td></td>
<td></td>
</tr>
<tr>
<td>AID - LOSS</td>
<td>-0.247</td>
<td>0.061</td>
<td>0.283</td>
</tr>
<tr>
<td></td>
<td>( 0.167)</td>
<td>( 0.041)</td>
<td>( 0.098)</td>
</tr>
<tr>
<td></td>
<td>Fungibility not imposed</td>
<td></td>
<td></td>
</tr>
<tr>
<td>AID</td>
<td>0.255</td>
<td>0.032</td>
<td>0.036</td>
</tr>
<tr>
<td></td>
<td>( 0.296)</td>
<td>( 0.077)</td>
<td>( 0.184)</td>
</tr>
<tr>
<td>-LOSS</td>
<td>-0.452</td>
<td>0.072</td>
<td>0.383</td>
</tr>
<tr>
<td></td>
<td>( 0.190)</td>
<td>( 0.049)</td>
<td>( 0.114)</td>
</tr>
</tbody>
</table>

Notes: Negative numbers correspond to jobs destroyed/forestalled. Outlaid-based offset.
cross-state variation.\footnote{These papers are discussed in the introduction. None of the other existing papers attempt to increase precision using fungibility or another outside restriction.}

The final row of the Table reports the coefficients on $-LOSS$. If fungibility holds, then we should be unable to reject the hypothesis that the coefficient on $AID$ equals the coefficient on $-LOSS$ for any of the three sectors, as seen in the Table. This is due both to the fact that the point estimates are relatively close (although only for services and government), but unfortunately also due to the fact that all of the parameters are estimated imprecisely. As seen at the top of the table, imposing fungibility improves precision substantially.

Table 5 also tells us that the data does not reject the fungibility restriction. Under the heading “fungibility restriction not imposed,” we see the elasticity estimates when $b$ is not required to equal $d$. Examining the government column, the elasticity for the ARRA outlay-based offset equals 0.112 and the elasticity for $-LOSS$ equals 0.577. Taking into account the standard errors of the estimates, these two values are very close. Formally, the Chi-squared statistic for the test is sufficiently low that we fail to reject fungibility at all conventional significance levels. This failure to reject fungibility also holds for the other sectors. Moreover, our finding of jobs forestalled for the three private sectors is maintained even when the fungibility restriction is not imposed (although the precision of the estimates fall).

4.2 Estimates using alternative specifications

Table is in paper, but text in body to be completed.

5 Other Researchers’ Estimates of Job Creation

5.1 Job Creation Estimates

Even before the legislation was passed, Bernstein and Romer (2009) reported that 3.6 million jobs would be created or saved by the then envisioned legislation, relative to a no stimulus baseline. This was based on existing estimates of fiscal policy multipliers. Their estimates included both the tax and spending components of the ARRA.

There have been two types of studies of the jobs effect of the ARRA. The first type uses all pre-ARRA data and information except for the actual ARRA spending. They fail to look at the actual employment outcomes experienced after the program starts. Studies of this type provide no new information about jobs effect. As an analogy, suppose a physicist conducts a new experiment with a particle reactor. If rather than recording and analyzing the observed outcome of the experiment, he simply reported the experiment’s results based upon on previous experiments, then there is no scientific value of his efforts. Similarly, an social scientist estimating the treatment effect of a new
Table 6: Confidence intervals for jobs created/saved as a result of ARRA in December 2010, miscellaneous specifications, reported as thousands of jobs

<table>
<thead>
<tr>
<th></th>
<th>Benchmark</th>
<th>Per-capita scaling</th>
<th>All 50 states</th>
</tr>
</thead>
<tbody>
<tr>
<td>Private-sector</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Fung. imposed</td>
<td>(-300, 1690)</td>
<td>(-444, 1835)</td>
<td>(12, 1917)</td>
</tr>
<tr>
<td>Fung. not imposed</td>
<td>(-484, 2899)</td>
<td>(-1190, 5937)</td>
<td>(69, 4345)</td>
</tr>
<tr>
<td>Exclude LOSS</td>
<td>(-223, 3089)</td>
<td>(-530, 5872)</td>
<td>(610, 4667)</td>
</tr>
<tr>
<td>Government</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Fung. imposed</td>
<td>(158, 883)</td>
<td>(195, 1045)</td>
<td>(86, 581)</td>
</tr>
<tr>
<td>Fung. not imposed</td>
<td>(-520, 853)</td>
<td>(-1478, 1513)</td>
<td>(-1205, 686)</td>
</tr>
<tr>
<td>Exclude LOSS</td>
<td>(-78, 1313)</td>
<td>(-173, 2435)</td>
<td>(-73, 1373)</td>
</tr>
<tr>
<td>Total</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Fung. imposed</td>
<td>(116, 2314)</td>
<td>(39, 2592)</td>
<td>(293, 2303)</td>
</tr>
<tr>
<td>Fung. not imposed</td>
<td>(-548, 3296)</td>
<td>(-1676, 6457)</td>
<td>(-390, 4285)</td>
</tr>
<tr>
<td>Exclude LOSS</td>
<td>(200, 3901)</td>
<td>(156, 7450)</td>
<td>(1024, 5553)</td>
</tr>
</tbody>
</table>

Notes: Negative numbers correspond to jobs destroyed/forestalled. Ninety-percent confidence intervals appear in parenthesis. For the “exclude LOSS” specification, we also drop the inelastically-sourced revenue instrument.

Congressional Budget Office (2010), an example of this first type, estimates that the employment increase “attributable to the ARRA” was in the range of 500 to 900 thousand in 2009 and is in the range of 1.3 to 3.3 million for 2010. Their ranges are computed based on both government spending as well as tax cut incentives in the Act. To construct these numbers (in their Table 1), they divide the total spending of the ARRA into its components and then apply low and high output multipliers. These multipliers were delivered from previous studies.

Much of the Presidents’ Council of Economic Advisors’ analysis also belongs to this first type. They use a multiplier approach similar to the Congressional Budget Office, the Council of Economic Advisors (2010) estimates that the Act had the effect of increasing employment by 2.5 million workers (Table 4).

The second type of study, most closely related to ours, uses employment outcomes observed during and after the ARRA period to estimate a jobs effect. These existing studies, all of which we were unaware of when doing the majority of our first draft, are Wilson (2011), Council of Economic Advisors (2009b) and Chodorow, et. al. (2011).

Wilson (2011) estimates the job effects of the Recovery Act using state-level variation in a manner similar to ours. He instruments for endogeneity using several formulary rules. He considers the effect on employment at different horizons following the ARRA’s implementation.

Wilson finds a large jobs effect estimate that is statistically greater than zero. His point estimates imply the ARRA created/saved 2 million jobs in its first year and more than 3 million by
March of 2011.

However, as with our no-fungibility case, Wilson job effect is estimated imprecisely. For example, in one specification, Wilson’s ninety-percent confidence interval for the cost of an additional private-sector job (one year following the ARRA start) is between $62,000 and $241,000.\footnote{This confidence interval calculation is based upon his jobs per million dollars point estimate of 10.1 with standard deviation 3.5 (see Wilson’s Table 4 panel A), where employment is measured through March 2010 and the ARRA treatment is measured as obligated dollars.}

In our analysis, including the four smallest states increased the jobs effect. While Wilson does not exclude small states in any specifications, in one robustness check he applies BLS weights to account for measurement error in employment. This has an effect of down-weighting the importance of smaller states, which are measured less accurately. He reports estimates in Table 5 panel B which imply that the cost range of a private-sector job above changes to between $77,000 and $1.47 million.

Feyrer and Sacerdote (2011) conduct both a cross-sectional and time series analysis to estimate the employment effects of the ARRA. Based on state-level data, their cross-section estimate implies that the Act created/saved 1.9 million jobs, while their time series estimate implies that the Act created/saved approximately 845 thousand jobs.\footnote{Feyrer and Sacerdote (2011) do not report results in units of employment; however, we can calculate this figure based upon numbers they report: per capita stimulus is $1100, one job per $170,000 of stimulus (cross-section), one job per $400,000 (time series). These statistics together with a U.S. population of 307 million imply the job totals reported in the text.} As in our no-fungibility case, Feyrer and Sacerdote (2011) estimates are very imprecise. For example, they estimate the cost of a job (employment measured in October in 2010) to lie in a 90% confidence interval between $48,000 and $1.85 million.\footnote{This confidence interval calculation is based upon their jobs effect point estimate of 1.064 and standard deviation 0.594 from column (3) of Table 3. Feyrer and Sacerdote did not report these confidence interval directly.}

Given that the above studies find a positive and often statistically significant jobs effect, we would like to pinpoint what accounts for these differences. Table 7 accomplishes this as best we can.\footnote{Note that this table does not contain a replication exercise. We neither use the other researchers’ instruments, control variables or exactly the same data definition; rather, we generate ballpark estimates similar to those studies by moving our specifications closer to theirs.} Two reasons we find smaller jobs effects and cannot reject a zero jobs effect, excluding the government sector than existing studies are: (i) others omit states’ fiscal stress; (ii) others use the four smallest population states. Reading from the left, Table 7 shows our benchmark estimates, except not imposing fungibility. We do not impose fungibility to mimic the existing studies.

The next column of Table 7 moves our specification along dimension (i), i.e. omitting states’ fiscal stress. The total employment point estimate increases by three-hundred thousand and its confidence interval shrinks and excludes zero. This is due both to changes in private-sector and government employment.

The third column of estimates moves our specification along dimension (ii), i.e. not dropping the four smallest states, but not (i). In this case, the total employment point estimate increases by six-hundred thousand relative to our benchmark and the lower bound on its confidence interval
Table 7: Number of jobs created/saved in December 2010 as a result of ARRA, implementing changes in order to resemble other studies, reported as thousands of jobs

<table>
<thead>
<tr>
<th></th>
<th>Our benchmark, except no fungibility</th>
<th>Omit fiscal stress</th>
<th>Do not drop smallest states</th>
<th>Both changes</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Total</strong></td>
<td>1374</td>
<td>1829</td>
<td>1948</td>
<td>2880</td>
</tr>
<tr>
<td></td>
<td>(-548, 3296)</td>
<td>(23, 3634)</td>
<td>(-390, 4285)</td>
<td>(708, 5051)</td>
</tr>
<tr>
<td><strong>Private</strong></td>
<td>1208</td>
<td>1289</td>
<td>2207</td>
<td>2367</td>
</tr>
<tr>
<td></td>
<td>(-484, 2899)</td>
<td>(-327, 2905)</td>
<td>(69, 4345)</td>
<td>(423, 4312)</td>
</tr>
<tr>
<td><strong>Government</strong></td>
<td>167</td>
<td>540</td>
<td>-259</td>
<td>512</td>
</tr>
<tr>
<td></td>
<td>(-520, 853)</td>
<td>(-147, 1226)</td>
<td>(-1205, 686)</td>
<td>(-193, 1217)</td>
</tr>
</tbody>
</table>

Notes: Negative numbers correspond to jobs destroyed/forestalled. Outlaid-based offset. Ninety-percent confidence intervals appear in parenthesis.

Increases and equals approximately zero.

The final column implements both (i) and (ii) simultaneously. With both changes, our point estimate on total jobs created/saved as of December 2010 is 2.88 million with a confidence interval between 708 thousand and 5.05 million jobs. By comparison, in one specification Wilson estimates that in March of 2011, the ARRA created/saved 3.2 million jobs with a confidence interval between roughly 1.1 and 5.5 million jobs.46

**INCLUDE LOSS; EXCLUDE FOUR SMALL**

Finally, the CEA estimates a vector autoregression which includes employment from 1990:Q1 to 2007:Q4. Based on those parameter estimates, they forecast gross domestic product for the period after the Act’s implementation. They then interpret the vector autoregression’s forecast error for employment from 2009:Q2 to 2010:Q2 as being due to the policy. According to these estimates (Table 5), at the end of 2010:Q2, the Act had increased employment by 3.6 million workers. Note the CEA departs from standard practice in statistics of reporting confidence intervals and standard practice in vector autoregression work of conducting robustness analysis.

5.2 The Council of Economic Advisors’ Analysis and Omitted Variables

In Section 4, we established that by omitting LOSS from the specification one could induce a larger, positive private-sector effect. Perhaps related is the fact that every existing study using state-level variation in ARRA spending that finds such an effect (specifically Council of Economic Advisors (2009b), Chodorow, et. al. (2011), Feyrer and Sacerdote (2011) and Wilson (2011)) does not include a variate controlling for states’ budget stress.

46Wilson reports this point estimate on page 32 but does not report numerical values for it confidence interval or standard errors. We infer the corresponding confidence interval roughly by inspecting his Figure 3.
To explore whether omitted variable bias helps induce a positive (statistical) jobs effect in a study besides our own, we redo the analysis of one of these papers, Council of Economic Advisors (2009b), and then augment it with the LOSS variable. As we show next, the inclusion of LOSS will reduce the estimated jobs effect and make it statistically not different from zero.

The Council of Economic Advisors’ analysis begins with the observation ARRA funding to support states’ Medicaid costs is divvied up between states partially according to each state’s Federal Medicaid support to states before the recession. In particular, the ARRA increased the Federal share of states’ Medicaid costs by 6.2 percentage points in each state.

Moreover, before the recession there was substantial variation in spending on Medicaid per capita across states. States with relatively generous Medicaid costs before the recession were automatically, and for factors plausibly unrelated to the recession, given relatively generous contributions in the ARRA’s Medicaid-support program.

The Council then constructs each state’s “estimated non-cyclical ARRA Medicaid payouts” variable as 0.062 multiplied by the 2007 Medicaid spending per capita in that state.\footnote{The reader can envision this construction as executing the first stage in two-stage least squares estimation. With the first stage completed, the authors then use least-squares in the remainder of their analysis.}

The authors then regress the change in non-farm employment per capita on the above constructed variable and a constant.\footnote{The employment change is between January and July of 2009. The authors, in addition, report statistics that add region controls and weight the regression errors by state populations. To Tim, we have not yet added manufacturing share or the vintage employment data that the CEA uses.}

The first column of Table 8 in our paper repeats the CEA analysis as closely as we are able. As in the original study, we find a statistically-significant positive jobs effect. Our coefficient on the estimated payout equals 0.0077 and the CEA estimate equals 0.0078 and both coefficients are statistically different from zero at the 95% confidence interval.

The second column augments the CEA model—still using least squares—by $-\text{LOSS}$ per capita. This additional variable reduces the coefficient on the Medicaid variable from 0.0092 to 0.0064 and the $t$-statistic against the null of zero jobs effect falls from 2.08 to 1.46. The coefficient on $-\text{LOSS}$ per capita is -0.0002 with a standard error of 0.0001. It would be inappropriate to interpret the parameter on the loss variable as structural because of its endogeneity.

Therefore, to control for the endogeneity of states’ fiscal loss, we instrument using the percent of inelastic revenue sources. These estimates are reported in the final column of Table 8. The coefficient on the Medicaid falls even further to 0.0049. The $t$-statistic becomes 0.0049 and we cannot longer reject the hypothesis that the Medicaid component of the ARRA created/saved no jobs as of June 2009.\footnote{We have not done a more complete revamping of the CEA analysis; it is beyond the scope of this paper.}

Through the robustness check in our own model (i.e. Table 7) and analyzing the CEAs’ model above, we conclude that including a state’s fiscal position in an analysis of the jobs effect is critical.
Table 8: Correcting the omitted variable bias in the Council of Economic Advisors (2009b) study of the jobs effect of the ARRA; the elasticity of non-farm employment change with respect to non-cyclical ARRA Medicaid aid

<table>
<thead>
<tr>
<th></th>
<th>Least-squares CEA model</th>
<th>Instrumental variables Include LOSS</th>
</tr>
</thead>
<tbody>
<tr>
<td>Non-cyclical ARRA Medicaid aid per capita</td>
<td>0.0077 (0.0040) [1.93]</td>
<td>0.0010 (0.0060) [0.17]</td>
</tr>
<tr>
<td></td>
<td>Include LOSS</td>
<td>Include LOSS</td>
</tr>
<tr>
<td>LOSS per capita</td>
<td>-0.0002 (0.0001) [-2.65]</td>
<td>-0.0004 (0.0003) [-1.43]</td>
</tr>
</tbody>
</table>

Notes: “CEA model” corresponds to specification described on page 8 of Council of Economic Advisors (2009b). Change in employment is from January to July of 2009. LOSS per capita is from January to June 2009. Instrument used in IV, “Inclusion of LOSS,” is each state’s pre-recession fraction of revenue from inelastic sources. Each specification includes gdp per capita, a measure of house price growth, region controls and a constant. Standard errors appear in parentheses and t-statistics appear in brackets.

6 Conclusion

Much work on the cyclical effects of the ARRA remains to be done.

First, researchers must employ additional restrictions or data, beyond simply cross-state variation in spending, to shrink the confidence intervals on the jobs effect. Other researchers find very wide confidence intervals and we find very wide confidence intervals when we do not impose fungibility. Our fungibility restriction improves precision greatly and, hopefully, future work will find other restrictions that do the same job.

Second, researchers must allow for cross-state positive spillovers. This might result in estimates of a large positive jobs effect. Suppose, for example, that Georgia received relatively more ARRA aid, which in turn stimulated that state’s economy. If, as a result, Georgia residents’ vacation spending in Florida increased, then the increased vacationing might generate jobs in Florida. Our methodology cannot pick up this effect.

If this type of spillover from interstate trade is widespread nationally, then the economy-wide jobs effect of the ARRA may be actually larger than what we find. To address this, we are planning to redo our analysis by adding time series variation to the current cross-state variation. Given data collection lags and the fact that ARRA spending did not begin in earnest until mid-2009, we did not have a sufficiently long time series to use this approach in the present paper.

Next, research on the ARRA, and in particular our empirical findings, demands greater structural economic modeling. In this study, we deliberately chose the relatively ‘model-free’ approach for one of the first studies on this new government program and data set. The drawback is that,
at this point, we can only conjecture on the underlying, economic mechanisms that give rise to our findings.

Equilibrium modeling, with specification of preferences, endowments and technologies, should allow us to pinpoint these mechanisms. We predict that an accurate economic model of the ARRA experience will have three essential features. First, the Great Recession and the Act’s implementation happened partway through a sectoral shift from goods-producing to a service (of a particular type) sector economy as evidenced in Figure 2. Second, as explained in the paper, relatively less educated people faced, and continue to face, a much worse job market than more educated people. The way that state and local governments channeled ARRA funding into different sectors is likely to have consequences for the effects of the Act. For this reason, an accurate model should differentiate between high and low education workers along with their respective labor markets. Third, we have provided substantial evidence that state and local governments have used part of ARRA aid in a way not explicitly intended by Congress and the President. As such, an accurate model of this period will include a hierarchal intergovernmental component in the spirit of Bradford and Oates (1971a).

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50Existing economic modeling of sectoral shifts include Davis and Haltiwanger (2001) and Phelan and Trejos (2000).
Appendix

Data Construction

ARRA outlaid and obligated dollars are taken from the Federal agencies’ “Weekly Financial and Activity Reports,” which are cumulative over time and available on the recovery.gov web site.\(^{51}\) ARRA aid is measured through three distinct periods: December 2009, June 2010 and December 2010. The Social Security Administration dollars are excluded entirely because these are direct transfers paid by the Federal government to citizens. We also exclude the following agencies from our data: General Services Administration, National Science Foundation, Small Business Administration, Department of Interior, Federal Communication Commission, Department of State, Retirement Railroad Board, Department of Veterans Administration, NASA. Each of these exclusions is due to one of the following reasons: data was not reported in a useful manner, none of that agencies’ funding was reported as going through the states. The total funding outlays from excluded agencies (expect for the SSA) make up less than 2% of total ARRA outlays. Call this \(AID_t\). Every variable, unless otherwise noted, is indexed by state which we suppress for simplicity.

Next, we use the total tax revenue collected by state. This is collected by the U.S. Census in the “Quarterly Summary of State and Local Government Tax Revenue,” which is available on the Census web site.\(^{52}\) We construct annual total state tax collections from the quarterly data. Call this \(STATETAX_t\), where \(t\) represents one year ending in June.\(^{53}\)

Next, we need a measure of the pre-recession size of state revenue. Because we do not need data since the recession’s beginning, we are able to use a more accurate measure of the size of state government—one that includes non-tax state revenue sources. \(STATEREVALL_t\) is the calendar-year state revenue from all sources, which is reported in the “Census of Governments.”

In addition to state revenue, we will use state Medicaid outlays (independent of the funding source). Medicaid data is available in National Association of State Budget Officers (2009). There are two issues. First, unlike the quarterly census-collected tax revenue data, the state-level NASBO Medicaid data is reported in annual, calendar amounts. Therefore, we are unable to match Medicaid cost to the growth rate timing of state tax revenue exactly. Second, NASBO reports the 2009 Medicaid expenditures as an estimate only. Let \(MEDICAID_t\) equal the annual spending in a state on Medicaid.

Using the above variables, we compute our outlay-based measure of ARRA aid relative

\(^{51}\)Documentation for the web site data is contained in Recovery Accountability and Transportation Board (2009)

\(^{52}\)Although the word ‘Local’ is in the title of the data series, in actuality the data does not report local tax information.

\(^{53}\)Note that state tax is less than actual state government income. This is because we do not have up-to-date data on the two main others sources of state income: earnings from other sources, e.g. university tuition, and non-ARRA Federal aid.
to the size of state government. Let

$$OFFSET = \frac{OUTLAY}{1.75 \times STATEREVALL_{2008}}$$

In our robustness analysis, we estimate the model using $OFFSET$ computed using the ARRA announced rather than outlaid dollars.

$$OFFSET_{announced} = \frac{ANNOUNCE}{1.75 \times STATEREVALL_{2008}}$$

Next, we compute a measure of fiscal stress faced by each state because of the recession. We use the annualized decline in state tax revenue plus the increase in Medicaid costs. We do so over seven quarters with the final quarter being 2010:Q3.

$$LOSS = -\frac{\Delta T - \Delta M}{1.75 \times STATREVALL_{2008}}$$

where

$$\Delta T = 0.5 \times STATETAX_{2009} + STATETAX_{2010} + STATETAX_{AUTUMN} - 1.75 \times STATETAX_{2008}$$

$$\Delta M = 1.75 \times MEDICAID_{2009} - 1.75 \times MEDICAID_{2008}$$

Employment is the non-farm payroll series, de-seasonalized, from the Bureau of Labor Statistics Establishment Survey. This gives the number of workers in each state by month. We use measures from four sectors: state plus local government, goods-producing and services. Each series has been deseasonalized either by us, using the Census’ X12 algorithm, or by the Census itself. Further details on the employment data appear in the body of the paper.

We have three instruments. Our first is the ARRA dollars announced by the Federal Highway Administration relative to state government revenue from all sources in 2008. Second, Democratic governor is a dummy variable constructed using the Wikipedia entry “List of Current U.S. Governor” (extracted in July 2010). We spot several of the entries against the respective state government websites. Third, the intensity of inelastically-sourced revenue is from the “Quarterly Summary of State and Local Government Tax Revenue.” It measures the ratio of revenue from inelastic sources to all revenue, in 2008. The inelastic revenue sources are: property taxes, selective sales taxes, charges and miscellaneous revenue, utility revenue and liquor store revenue. All revenue, i.e. the denominator in the above ratio, excludes inter-government revenue.
The remaining data are exogenous variables used in estimation. Northeast, West and Midwest are dummy variables based on the U.S. Census Bureau’s region definitions. The unemployment insurance (UI) generosity variable is the maximum weekly benefits amount from the 2010 U.S. Department of Labor’s “Comparison of State UI Laws.” In the case that a maximum is reported as a range in the report, we use the maximum from that range. House price growth rates are computed using the Federal Housing Finance Agency State House Price Indexes (seasonally adjusted, purchase-only index). Oil production is the U.S. Energy Information Administration’s 2010 crude oil production, measured in barrels.

Partial Correlations

Table 9: Partial correlations of instruments on $OFFSET$ and $-LOSS$

<table>
<thead>
<tr>
<th></th>
<th>Offset</th>
<th>- Loss</th>
</tr>
</thead>
<tbody>
<tr>
<td>Highway dollar and percent sales tax instruments</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Highway spending</td>
<td>0.890</td>
<td>0.775</td>
</tr>
<tr>
<td>Inelastically-sourced revenue ratio</td>
<td>-0.038</td>
<td>0.129</td>
</tr>
<tr>
<td>All three instruments</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Highway instrument</td>
<td>0.947</td>
<td>0.906</td>
</tr>
<tr>
<td>Inelastically-sourced revenue ratio</td>
<td>-0.035</td>
<td>0.135</td>
</tr>
<tr>
<td>Democratic governor</td>
<td>0.004</td>
<td>0.009</td>
</tr>
</tbody>
</table>

Notes: Above are regression coefficients from the single-equation least-squares estimation of alternative endogenous variables on instruments and all control variables.
References


National Association of State Budget Officers, State Expenditure Report, various years.


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