The American Recovery and Reinvestment Act: Solely A Government Jobs Program?*

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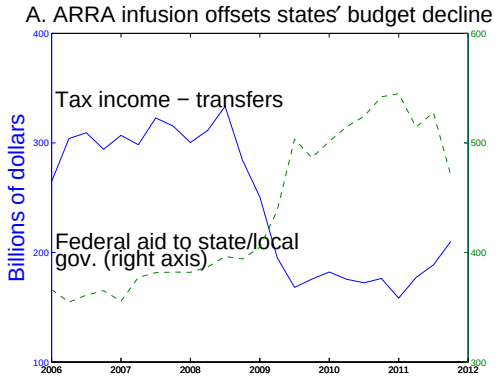
July 17, 2012

Abstract

This paper uses variation across states to estimate the employment effect of the spending component of the American Recovery and Reinvestment Act (ARRA). Our main sources of identification are ARRA highway funding and the extent to which states use inelastic revenue sources. According to our benchmark estimates, average state and local government employment, during the twenty-four months following the program's inception, was between 156 thousand and 563 thousand persons greater as a result of ARRA spending (90% confidence interval). The corresponding estimate for the private sector ranged from a loss of 182 thousand to a gain of 1.095 million jobs. Our benchmark findings are inconsistent with the President's Council of Economic Advisors estimate that the ARRA increased average employment in the same 24 month period by between 1.9 and 2.5 million jobs. One potential explanation for our different findings between government and private-sector jobs effects 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).



*Comments are welcome; all opinions expressed and errors are ours alone. Acknowledgements appear immediately before the bibliography. An earlier version of this paper circulated under the title "The American Recover and Reinvestment Act: Public Sector Jobs Created; Private Sector Jobs Forestalled." First draft: October 2010.

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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. It authorized $288 billion for Federal tax cuts and $499 billion in Federal government spending.[1] This paper seeks to understand the causal effect on employment of the government spending component of the ARRA.[2,3]

Our benchmark estimates imply that average employment in the government sector during the 24 months after the beginning of ARRA spending was 156 thousand to 563 thousand jobs higher as a result of ARRA spending.[4] The corresponding estimates for the private sector imply that average employment in the private sector over that period changed in a range from a loss of 182 thousand to a gain of 1.095 million jobs due to ARRA spending.[5] The analogous interval estimate for total employment is 82 thousand to 1.549 million jobs. These ranges correspond to 90% confidence intervals and are illustrated in Figure 1 by intervals with circle endpoints.

The ARRA spending component was first and foremost a stimulus to state and local governments. Most directly funded jobs were in government (Dupor 2012), 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 educated workers with in-demand skills.

A large fraction of the Federal ARRA dollars was channeled through and controlled by state and local governments.[7] This is important for two reasons. First, it opens the possibility that

[1] Throughout the paper, we use the term spending to include direct government purchases, transfers to state and local governments and entitlements channeled through state and local governments.

In Congressional Budget Office (2011b), the estimated total budget impact of the ARRA was increased from its initial $787 billion to $821 billion.

[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.

[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).

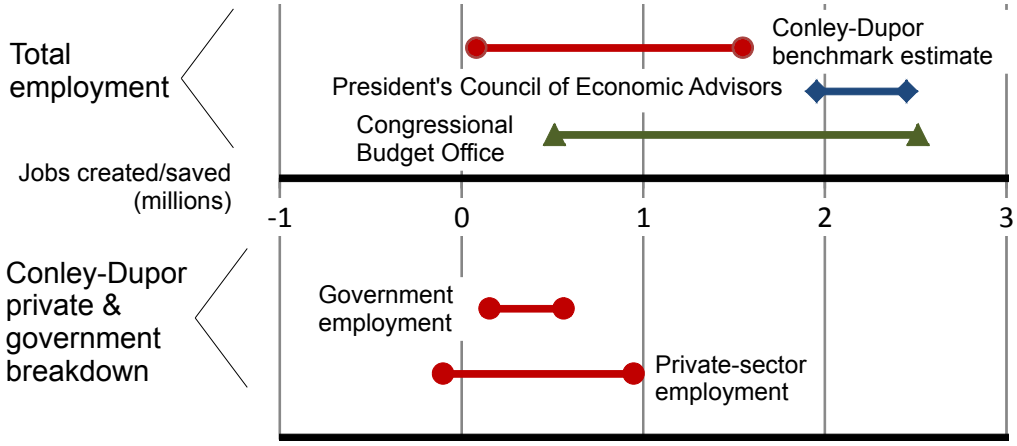
[4] These 24 months are April 2009 to March 2011.

[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 yearly data revision known as benchmarking. Specifically, the BLS re-anchors the Establishment Survey estimates to records of businesses' Unemployment Insurance tax records, which the BLS states corrects for sampling and modeling error in the Establishment Survey.

[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.

[7] This included, for example, $86.6 billion to support states' Medicaid programs, a $53.6 billion 'State Fiscal Stabilization Fund' to (in part) 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

Figure 1: Our benchmark estimates of employment effect (in millions) of the ARRA spending component, with Congressional Budget Office and President's Council of Economic Advisors ranges shown for comparison



Notes: Benchmark estimates are 90% confidence intervals for employment due to ARRA-spending component (averaged between March 2009 and March 2011). Other sources are Council of Economic Advisors (2011) and Congressional Budget Office (2011a). The CEA and CBO ranges each include the effects of tax cuts and spending. See Table 10 in the Appendix for data underlying the figure.

Table 1: State government contributed spending and ARRA spending, various categories and in billions of dollars

Spending category	ARRA	States-contributed spending
Medicaid	88.6	271.8
Elementary and secondary education	53.6	464.7
Highways	28.0	50.1

Notes: ARRA dollars are amounts authorized by the Act. State contributions are 2008 FY multiplied by two: the number of years of ARRA spending covered by this study. 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).

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, and there is arguably 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. 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). The Act did legislate substantial 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, some states could cut their own expenditures on that purpose. As a result, these states could treat substantial portions of ARRA dollars as general revenue, i.e. these funds were effectively fungible.[8]

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 non-Federal sales and income tax revenue net of non-Federal government transfers.[9] These combined revenues experienced a sharp and then persistent decline beginning in 2008:Q4.[10] The reduction in consumer purchases and employment reduced the tax base for sales and income tax revenue. Second, non-Federal

from low income families, and $6 billion for clean water projects.

[8]We present evidence for fungibility in the next section. Also, in Section 2, we discuss economic theory, beginning with Bradford and Oates (1971a), and empirical work that followed, on how local governments' spending changes with receipt of Federal grants.

[9]The two taxes are the main sources of cyclical variation in state and local governments' income; state and local government transfers to individuals, mainly Medicaid, are the main source of their cyclical expenditure fluctuation.

[10]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).

government transfer expenditures, most importantly from Medicaid,[11] increased over this period. 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 was concurrent with an increase in Federal grants (the dashed-dotted line on Figure A), mainly due to the ARRA, of approximately the same amount. Aizenman and Pasricha (2011) use aggregate data to show how in large part the ARRA federal expenditure only offset state fiscal declines.

In our benchmark specifications, we exploit an assumption that ARRA dollars were effectively fungibile and that states spent ARRA dollars to offset lost revenue. We refer to this assumption as our 'fungibility assumption.' 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 construct an instrument to reflect an exogenous component of state budget stress. This instrument is the pre-recession fraction of each state's revenue that comes from short-run inelastic sources. A state that relies on revenue from inelastic sources will experience less fiscal stress during a recession than a state that relies on relatively elastic sources. The relative importance of elastic versus inelastic revenue sources for a state is largely determined by long-run political economy factors which are plausibly unrelated to short run shocks associated with a recession. We operationally define inelastic revenue sources to include: property taxes, revenue from publicly-run enterprises like universities and liquor stores, sales taxes on cigarettes and alcohol, and intergovernment transfers. Elastic sources are mainly income and general sales taxes.

Our benchmark specifications also use exogenous variation in capacity to attract/channel ARRA funds. We exploit variation in funds allocated via the Department of Transportation (DOT), most of which were allocated via formula set prior to the ARRA. The largest component of this, $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. 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. Our instrument is a major component of DOT funding relative to pre-recession state revenue.

We estimate sector-specific employment impacts of ARRA spending for a partitioning into government, goods-producing, and service sectors. The latter two sectors partition the private

[11] Medicaid is a U.S. health care program for low income individuals and households.

sector. We think allowing different parameters and using sector-specific conditioning information across goods-producing and service sectors is critical due to their very different pre-recession trends (illustrated in Figure 3) and because recessions tend to have a more pronounced impact on markets for goods than services.

The two most commonly reported estimates of the ARRA's jobs effect are those of the Congressional Budget Office (CBO) and the President's Council of Economic Advisors (CEA).[12] Figure 1 compares our estimates of the average employment effects in the first 24 months after the beginning of ARRA spending to their analogs derived from CBO and CEA estimates. The line with triangle endpoints represents the CBO and the interval with diamond endpoints is the CEA range of estimates for job creation/saving. [13]

Our results substantially differ from both CEA and CBO ranges, though discrepancies are more pronounced for CEA than CBO. The CEA interval for the total effect is between 1.9 and 2.5 million jobs, which is substantially above our estimates. In contrast, there is some overlap between our interval and that of the CBO. The CBO's range for total job creation is very wide, between 0.5 and 2.5 million jobs.

One potential reason that the CEA and CBO jobs effect estimaates differ from ours is that we do not consider the stimulative effect of the ARRA's tax cut component while both CBO and CEA reflect spending and tax cut components. However, according to several conventional views of fiscal policy, the effect of tax cuts is likely to be much smaller than the effect of government spending. The Congressional Budget Office (2010) takes the conventional Keynesian approach. It argues that the output multiplier for Federal payments to state and local governments for infrastructure is 1.3, whereas the multiplier for middle-income and high-income persons are only 0.9 and 0.35, respectively. Next, the neoclassical view holds that deficit-financed tax cuts do not increase aggregate consumption because people respond by saving their additional income in anticipation of future tax liabilities. Germane to this view, Matthew Shapiro and Joel Slemrod, in a series of papers, surveyed recipients of several recent tax cuts, and found that approximately 20% of households will consume the additional after-tax income. Paying down debt and increasing savings were more common responses to the question.

Our paper is part of a broader line of research on examining the effects of fiscal policy on real economic activity using variation on sub-national government spending and deficit policy. Four existing studies use this methodology to estimate the jobs effect of the ARRA: Council of Economic Advisors (2009b), Chodorow, et. al. (2011), Feyrer and Sacerdote (2011) and Wilson (2011). We discuss our paper's findings relative to these studies later in the paper. Other related recent papers include Shoag (2010) and Clemens and Mirian (2011).[14]

[12] Section 5 explains the methodology used by these two organizations.

[13] We note that neither CBO or CEA ranges are statistical confidence intervals. Instead, they are ranges based on different projection methods reflecting varied assumptions but not sampling uncertainty.

[14] 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).

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. Section 5 discusses other researchers' estimates of employment effects due to the ARRA in the context of our findings. The final section concludes.

2 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.[15]

A key 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 had 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.[16]

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 pre-existing U.S. 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.[17] This formula was set several years prior to the Act's passage and was used to disperse previous highway funds.

Each state selects highway projects on which to spend its dollars. While the Federal Highway Administration (FHWA) must approve each project, our reading is that the approval rate has been very high.[18] The Act does dictate that the FHWA should give priority to "projects that are

[15]Michael Grabell's (2012) book *Money Well Spent?: The Truth Behind the Trillion-Dollar Stimulus, the Biggest Economic Recovery Plan in History*, intended for a general audience, gives an interesting narrative account of the effects of the Stimulus. For an early outline of the plan, see Summers (2008).

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

[17]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.

[18]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.

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 and department, twenty-eight in total, charged with dispersing a fraction of ARRA dollars submits "Weekly Financial and Activity Reports." Agencies enter a new award once a specific grant has been issued, whether it is formulary or discretionary. These reports 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. Each agencies' report provides a list of awards, announced obligated amounts and the total outlays related to each project. 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. Our benchmark specifications use announced obligated amounts to construct our treatment measure; we use actual outlays as an alternative treatment measure in our alternative specification section.

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 for University of California faculty. Establishing this fungibility is important because it will allow us to use exogenous state-level variation in budgetary stress 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 $700 million in ARRA highway funds during 2010.[19] The Texas government *responded* to its receipt of ARRA

Code or Law which defines that phrase.

[19]Only $110 million in ARRA funds were spent in 2009. The budget amounts come from Texas Department of Transportation (2009, 2010)

highway dollars by cutting Texas' own contribution to highway spending, which freed up state dollars to boost suffering state finances.

Georgia provides another good example of transportaion aid fungibility. Georgia's fiscal year (FY) 2010 federally-funded transportation capital expenditures were $580 million greater than its FY 2008 level, due primarily to Recovery Act grants. On the other hand, Georgia's own state funds for these expenditures were $568 million lower in FY 2010 than in FY 2008. Thus, total transportation capital spending was nearly unchanged following the influx of ARRA transportation aid.

Ohio provides another example. Ohio's largest spike in Federally-funded transportation capital expenditures follow the Recovery Act's enactment occurred in its FY 2011. Ohio's FY 2011 federally-funded transportation capital expenditure was $218 million greater than in FY 2008. Ohio's own state-funded expenditures were, on the other hand, $153 million lower in FY 2011 than in FY 2008.

Michigan provides an additional example. Michigan's Federal aid for capital transportation expenditures was substantially greater in each of the three years following the ARRA, relative to year prior to the Act. Federal-funded expenditures in FY2008 were $686 million. The federal funded total across the following three fiscal years was $3.780 *billion*. On the other hand, state-funded expenditures in FY2008 were $470 million versus a total aggregating the following three years of only $779 million. Thus, Michigan's own contribution to transportation capital expenditures was, on average, $207 million per year lower in the Recovery Act period relative to the year before the Recovery Act.[20]

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."[21]

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

[20]Georgia, Ohio and Michigan data are from the National Association of State Budget Officers' State Expenditure Reports. Because of the timing of the reports' data collection, the 2011 numbers are estimated.

[21]This 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."

workers was 1,870 in May of 2008 and 1,890 in May of 2010.[22]

The Medicaid component of the Act provides another channel for fungibility. A Council of Economic Advisors (2010b, 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."

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 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.[23]

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.[24] 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."[25]

There are however a few components of the ARRA where aid funds were not very fungible. 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.

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

[22] Bureau of Labor Statistics' 2008 and 2010 National Industry-Specific Occupational Employment and Wage Estimates.

[23] 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.

[24] 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.

[25] See page 29 of U.S. Department of Education (2009).

simple political economy model. Empirical work that followed, surveyed in Hines and Thaler (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, on the other hand, that after controlling for a particular form of endogenity, the flypaper effect is statistically insignificant 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 similar to that observed during the most recent recession.

3 Econometric Specification

We 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. Our employment outcome data are derived from monthy series of seasonally-adjusted payroll employment from the Bureau of Labor Statistics Establishment Survey. We use the notation $Y_{j,t}^{GOVT}$ for employment in the state and local government sector in state j in month t.[26] Our benchmark outcome variable for this sector is the average growth in employment from the base of March 2009 over the 24 months from April 2009 to March 2011.

$$\text{Employ Growth}_j^{GOVT} = \frac{1}{24} \sum_{k=\text{ Apr 09}}^{\text{Mar 11}} \frac{Y_{j,k}^{GOVT} - Y_{j,Mar\ 09}^{GOVT}}{Y_{j,Mar\ 09}^{GOVT}}$$

We define analogous variables $\text{Employ Growth}_j^{Goods}$ and $\text{Employ Growth}_j^{Services}$ for goods-producing and service sectors, simply by using their employment series in place of $Y_{j,t}^{GOVT}$. We apply our analysis to three distinct employment sectors rather than total employment because of the large differences in trends across these sectors over the past decade. Figure 3 plots national total employment for these sectors with each initial point normalized to 100. State and local government employment fared relatively well during the recession. Employment in the goods-producing sector fell most dramatically during the recession, which was part of a continual decline over the preceding decade.

Time-averaging over a number of months tends to smooth out the idiosyncratic shocks that would be present if we evaluated the ARRA jobs effect at a single particular horizon. This substantially increases our estimates' precision. Moreover, reporting how many jobs were saved/created on average over the two year horizon is a more interesting benchmark–in our view– than reporting how many jobs were saved/created at say a particular (e.g. six or twelve month) horizon.

[26] According to our classifications, educators employed by state and local governments count toward the government sector and Federal workers count towards service employment.

Table 2: Summary statistics

Variable	Mean	Stdev.	10th perc.	90th perc.
Economic and financial variables				
Obligated AID	0.136	0.024	0.108	0.161
$LOSS$	0.178	0.045	0.118	0.231
Avg. Goods-producing employment growth (percent)	-7.01	2.70	-9.63	-4.86
Avg. Services employment growth (percent)	-0.66	0.53	-1.33	0.01
Avg. Government employment growth (percent)	-0.71	1.34	-2.59	1.04
2009 Government employment (millions)	0.43	0.42	0.09	0.72
2009 Services employment (millions)	2.00	2.00	0.42	4.15
2009 Goods-producing employment (millions)	0.42	0.40	0.06	0.84
ARRA obligated dollars (billions)	7.179	7.777	1.516	13.670
Instruments				
USDOT-obligated aid scaled by gov. revenue	0.02	0.01	0.01	0.02
Intensity of inelastically-sourced revenue (percent)	75.10	7.00	67.58	82.24
Democratic governor (fraction)	0.54			
Other variables				
2010 oil production (barrels per capita)	3.63	7.48	0.00	14.74
House price growth, 2003 to 2006 (percent)	0.23	0.13	0.10	0.42
House price growth, 2006 to 2008 (percent)	-0.05	0.14	-0.27	0.07
Midwest/Northeast (fraction)	0.43			
Observations	46			

Notes: The job growth data is from non-farm employment. The four lowest population states (i.e. Alaska, North Dakota, Vermont and Wyoming) are excluded from the sample. See text for variable definitions.

Our first key regressor is AID_j—the ratio of ARRA dollars obligated to state j through March 2011 relative to 2008 state government tax revenue:

$$AID_j = \frac{\text{Total ARRA Obligations April 2009 to March 2011 for State } j}{\text{State } j \text{ Govt Revenue in 2008}}. \tag{3.1}$$

State government revenue includes local government revenue.

Our second key regressor is $LOSS_j$, which measures the decline in state government revenue plus the increase in state Medicaid costs relative to the pre-recession level of government revenue. We include state Medicaid cost increases in this calculation because these increases are mandatory spending and for some states were nearly as substantial as their tax revenue declines. For a state j :

$$\begin{aligned}\text{Net Govt Rev}_j \;\equiv\; &\text{Total Govt Revenue 2009:Q1 to 2011:Q1} \quad - 2.25 \times \text{Govt Revenue in 2008} \\ &-(\text{Approx. Medicaid Expenditures 2009:Q1 to 2011:Q1} - 2.25 \times \text{Medicaid Expenditures in 2008}),\end{aligned}$$

Our regressor then normalizes the sign and scales by pre-recession government revenue:

$$LOSS_j = -1 * \frac{\text{Net Govt Rev}_j}{\text{State } j \text{ Govt Revenue in 2008}}. \tag{3.2}$$

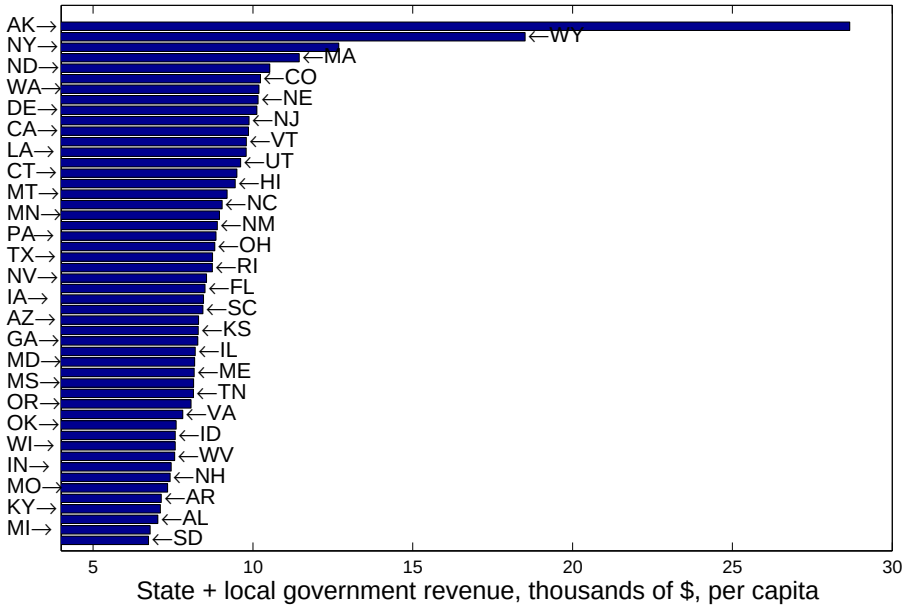
A positive $LOSS$ for a state means its budget has deteriorated. State tax revenue data is from the Census Bureau Quarterly Summary of State Tax Revenue. State-level Medicaid data is from the National Association of State Budget Officers and is calendar-year annual. Details for these data series appear in the Appendix.

We scale ARRA dollars and the decline in states' budget positions by the size of state government revenue 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 2 shows substantial cross-state differences in the size of governments. 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.

Consider an alternative scaling: dividing a state's ARRA obligations by its population. This scaling might be misleading because the cost of creating/saving a government job can differ across 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. Nonetheless, as a robustness check, Section 4.2 reports the alternative specification, scaling aid and loss by population rather than state revenue.

In our benchmark specification, we exclude the four least populous states: Wyoming, Vermont, North Dakota, and Alaska. We feel their small populations render their economies sufficiently

Figure 2: Government revenue (state plus local) per capita, by state.



Notes: Revenue equals all state and local government revenues in 2008 current dollars.

unusual to warrant their exclusion from our benchmark. In addition, natural resources play an unusually large economic role in three of the four, generating large government revenues. This makes the way in which their state governments operate and respond to ARRA money likely to be substantially different from other states. For example, North Dakota experienced an oil shale boom during the ARRA period.[27] Because of similar timing, the oil-boom related employment growth is potentially confounded with ARRA effects. This further motivates the exclusion of North Dakota from our benchmark sample. In our alternate specifications section, we examine the robustness of our results to excluding these four smallest states.

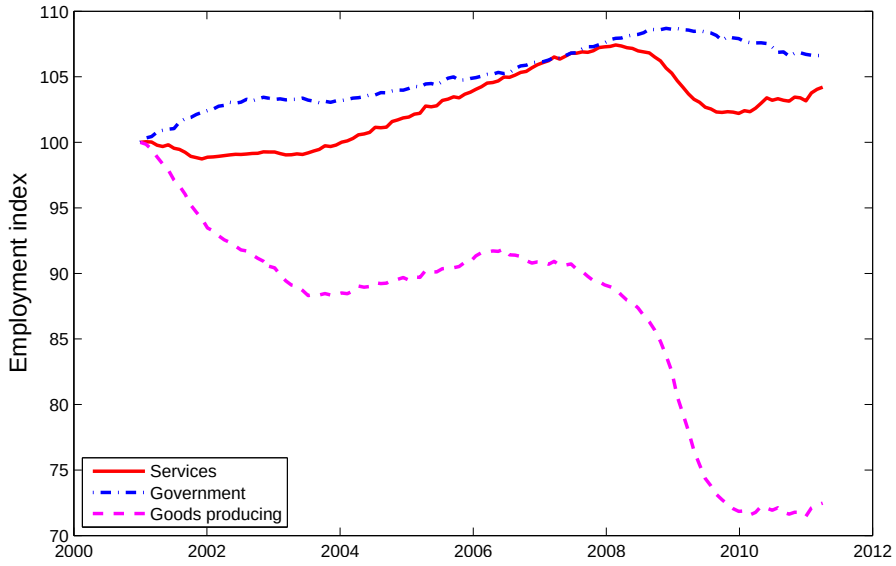
In our benchmark model we impose our fungibility assumption. Recall that this is a joint assumption that ARRA funds were fungible and that states used these dollars to offset lost revenue. Under this assumption the relevant treatment variable is aid net of lost revenue which we denote as $OFFSET_j = AID_j - LOSS_j$ where both AID and LOSS correspond to the same time interval of 24 months past the beginning of the stimulus aid. Our regression specification for the government sector is:

$$\text{Employ Growth}_j^{GOVT} = \alpha OFFSET_j + \beta' ANC_j^{GOVT} + \varepsilon_j. \tag{3.3}$$

Here ANC_j^{GOVT} is a vector of state-specific ancillary regressors: the February 2009 employment level in the government sector, a region indicator (midwest and northeast), house price growth rates

[27] See "Oil Industry Booms–in North Dakota," *Wall Street Journal*, February 26, 2010.

Figure 3: Monthly employment (seasonally adjusted), January 2001 through March 2011, by sector.



Note: Index = 100 in January 2001.

for the run-up period between 2003 and 2006, house price growth rates for the pre-recession decline between 2006 and 2008, 2010 crude oil production per capita, two lags of the annual employment growth in the government sector, and a constant.[28]

We rely on the variables in ANC_j^{GOVT} to parsimoniously control for states' pre-ARRA economic conditions, which should be predictive of its employment trajectory. The three employment and two house price variables measure these conditions. We add a single region dummy (i.e. Midwest/Northeast region) which explains substantial cross-state differences in employment outcomes. This is likely due to the behavior of manufacturing over the cycle combined with cross-state differences in industrial composition. Finally, we include oil production because oil-extracting states' employment situations and tax revenues can be tied closely to the fuel's price. The price of oil was very volatile leading up to and following the ARRA's commencement. We define analogs of Employ Growth$_j^{GOVT}$ and ANC_j^{GOVT} for other sectors by simply substituting their employment numbers for government employment. In our alternate specification section (4.2), we experiment with other sets of conditioning information.

Figure 4 contains a scatter plot of each state's total $LOSS$ and AID, up through March 2011. Note the substantial differences across states on both dimensions. Connecticut (CT) appears in the lower-right corner of the figure. It has a $LOSS$ of 0.325, which means that it experienced a 32.5% decline in its budget position, due to tax losses and Medicaid increases, relative to its 2008

[28]These growth rates are February 2008 to February 2009 and February 2007 to February 2008.

government revenue. It has an AID of 0.115 which means that the ARRA outlays boosted the state's budget by 11.5% relative to its 2008 government revenue. In terms of resources for the Connecticut government to operate, the ARRA dollars were insufficient to cover its losses. Its $OFFSET$ ($= AID - LOSS$) equals -21%.

South Dakota (SD), on the other hand, fared relatively well during the period, and appears in the upper-left corner. South Dakota has a $LOSS$ of 0.118, which means that it experienced a 11.8% decline in its budget position due to tax losses and Medicaid increases. It has an AID of 0.21, which means that the ARRA boosted the state's budget by 21% 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 $OFFSET$ ($= AID - LOSS$) equals 0.092 or 9.2%.

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.[29] This could be due to the fact that states in the upper left region of the figure 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.

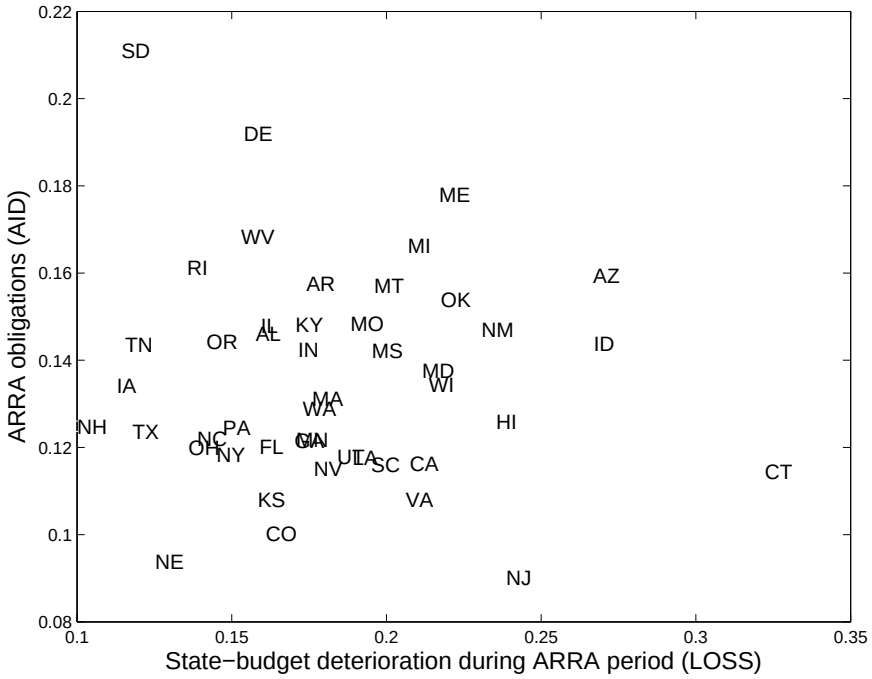
We estimate equation (3.3) via the generalized method of moments using instruments to address the endogeneity of $OFFSET(= AID - LOSS)$, treating ANC as exogenous. The shock to employment growth could reasonably be conjectured to be correlated with the AID and/or $LOSS$ components of $OFFSET$. For example, 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 jointly estimate specifications across our three sectors, allowing for error terms to be correlated within state. When constructing standard errors, we impose independence across states and second moment independence of our errors and instruments. Our procedure differs from sector-specific two-stage least squares (2SLS) only in that we jointly estimate across sectors and allow error terms to be correlated across sectors within a state.

3.1 Instruments

We use instrumental variables methods to identify the employment effects of ARRA spending. 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 (DOT). 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 Admin-

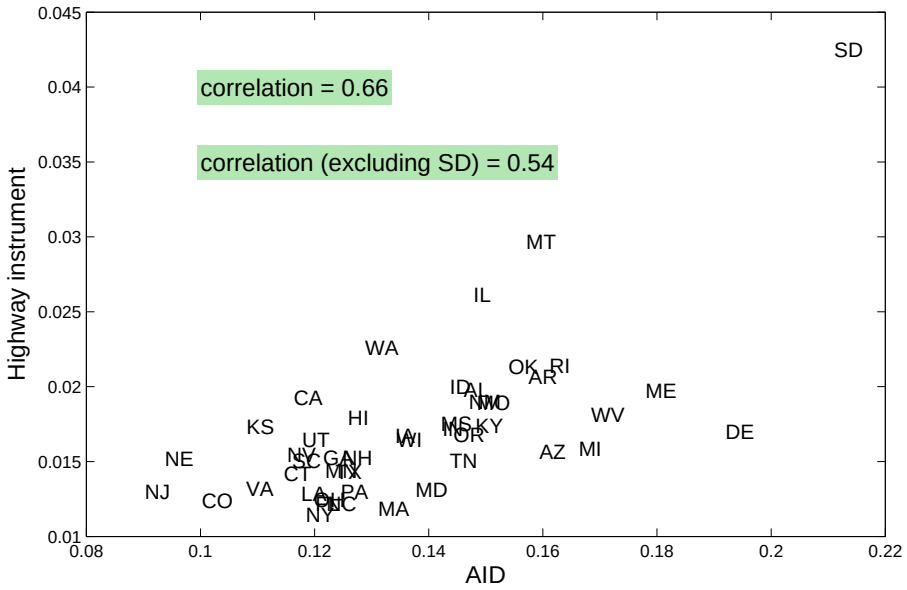
[29] See also Cogan and Taylor (2010b).

Figure 4: Each state's revenue loss net of Medicaid increase and ARRA obligations, through March 2011, relative to its pre-recession revenue, i.e. $LOSS$ and AID



Notes: Excludes four smallest states.

Figure 5: ARRA-obligated aid versus the highway instrument, by state



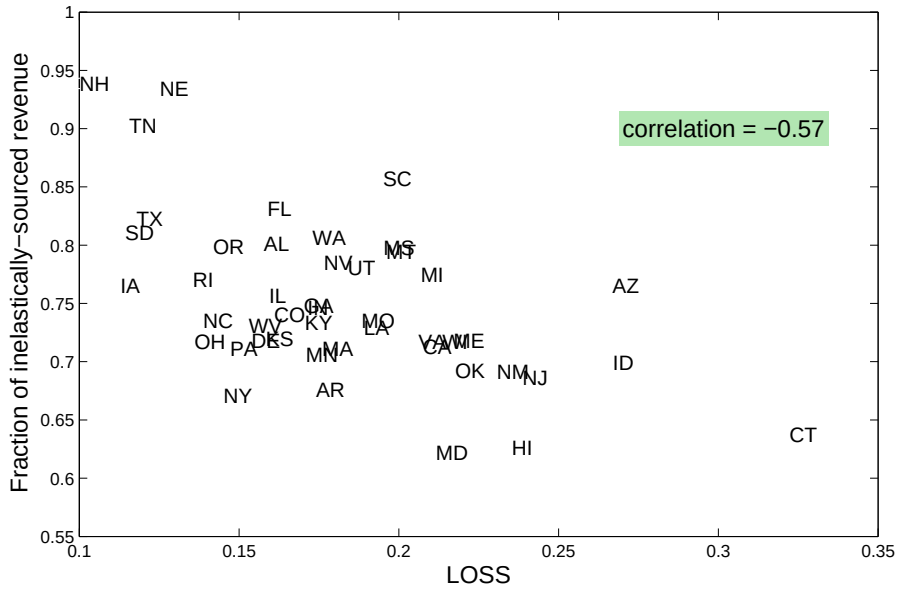
Notes: The highway instrument is the obligated Federal DOT dollars from ARRA, through September 30, 2010, relative to state tax revenue; excludes four smallest states; AID is through March 2011.

istration (FHWA), 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%), share of total vehicle miles traveled on eligible highways (20%), and share of dollar contributed to the Highway Trust Fund paid by highway users (17%).

Specifically, our instrument is each state's ARRA dollars from the Federal DOT obligated through September 30, 2010, divided by that state's 2008 tax revenue. We call this our highway spending instrument. Our choice of the September 2010 end date is motivated by the fact that all formulary FHWA dollars (the largest component of DOT aid) were required to be obligated by this date. After this date, additional obligated dollars were more likely to be non-formulary. Figure 5 contains a scatter plot of AID versus the highway instrument. It demonstrates a strong positive correlation, 0.66, between these two variables. Note that South Dakota is a substantial outlier. The numerator in the highway spending instrument for South Dakota is large because there are two 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 spending instrument is low mainly because of that state's low population. Even excluding South Dakota, the correlation is still large at 0.54.

Table 3 reports statistics from least squares regressions of our benchmark endogenous variable

Figure 6: *LOSS* versus inelastically-sourced revenue ratio instrument, by state



Notes: Inelastically-sourced revenue ratio is the ratio of pre-recession state revenue from inelastic sources to all state revenue (state plus local governments combined); excludes four smallest states; *LOSS* is through March 2011.

$OFFSET = (AID - LOSS)$ on instruments as well as the ANC variables. Also included in the table are results from regressions with the endogenous variable AID as the outcome. As our logic above suggests, highway spending aid has statistically significant, positive partial correlations with these endogenous variables.

Our second instrument is a ratio of a state's revenue from short-run (relatively) inelastic sources to total revenue. We refer to this as the inelastic revenue instrument. Relatively inelastic sources include property taxes[30], revenue from publicly-run universities, hospitals, liquor stores, sales taxes on cigarettes and alcohol, and intergovernment transfers. Their complement is mainly income and general sales taxes.[31] We calculate this ratio for each state based on the Census Bureau's 2007-08 Annual Survey of State and Local Government Finances.

The revenue share from inelastic sources varies substantially across states and is negatively correlated with *LOSS*. The correlation between our inelastic revenue instrument and *LOSS* is -0.57 and the corresponding scatter plot appears in Figure 6. As illustrated in Table 3, this induces

[30]We classify property taxes as relatively inelastic in the short run because most homeowners continue to make their mortgage/insurance/property tax payments even during housing 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.

[31]Additional discussion of this data appears in the data appendix.

Table 3: T- and partial F-statistics for instrument relevance

	March 2011	
	$OFFSET$ (=$AID - LOSS$)	AID
	Goods producing	
T statistics		
Highway spending	2.75	5.23
Inelast. revenue	3.20	-1.18
Partial F-statistic	10.31	13.79
	Services	
T statistics		
Highway spending	2.82	5.10
Inelast. revenue	3.60	-0.81
Partial F-statistic	12.10	12.99
	Government	
T statistics		
Highway spending	2.46	4.63
Inelast. revenue	3.14	-0.63
Partial F-statistic	9.32	10.73

Notes: Above t-statistics are from the single-equation least-squares estimation of alternative endogenous variables on instruments and all control variables. Partial F-statistic reflects joint test against the null that coefficient on every instrument equals zero.

a significant, positive partial correlation with $OFFSET = (AID - LOSS)$. This instrument does not have a significant partial correlation with AID.

The final row of Table 3 reports partial F-statistics for the joint hypotheses of all instrument coefficients being zero. To evaluate the magnitude of these weak instrument test statistics, we use critical values provided by Stock and Yogo (2005) based on a researcher's tolerance for a maximal size distortion of nominal 5% level Wald tests involving 2SLS estimators.[32] We view a range of maximal size distortions from 10% to 15% as tolerable; the corresponding Stock and Yogo (2005) critical values are 11.59 and 8.75. Our weak instrument test statistics are high enough relative to this critical value range that we proceed using strong instrument approximations.

4 The Act's Jobs Impact

4.1 Benchmark Estimates

Table 4 reports our benchmark estimates of the job effect of ARRA spending for total, private-sector and government employment. These jobs estimates result from generalized method of moments es-

[32]We use 2SLS critical values from Stock and Yogo (2005) because our procedure is close to 2SLS, differing only in that we allow for regression error terms within state to be correlated across sectors and jointly estimate across sectors.

Table 4: Employment effects of ARRA spending: monthly average between April 2009 and March 2011, reported as thousands of jobs (ninety percent confidence intervals)

	All sectors	Private	Government
	Instrumental variables		
Fungibility imposed	816	456	359
	(82 , 1549)	(-182 , 1095)	(156 , 563)
Fungibility not imposed	1256	1093	163
	(-194 , 2706)	(-155 , 2340)	(-267 , 594)
	Least squares		
Fungibility imposed	401	144	257
	(-35 , 838)	(-244 , 532)	(141 , 374)
Fungibility not imposed	945	786	159
	(39 , 1850)	(-11 , 1582)	(-96 , 414)

Notes: Results labeled 'Fungibility Imposed' correspond to our benchmark specification. Using all three instruments and 46 states. Ninety-percent confidence intervals appear in parenthesis. With the fungibility restriction, the p-value for the test of overidentifying restrictions is .532.

timates of equation (3.3) treating ANC as exogenous and using our highway spending and inelastic revenue instruments to address endogeneity of $OFFSET$. Private-sector job estimates are computed via aggregating separate estimates for services and goods-producing sectors. Total employment estimates are similarly computed via aggregating across government, goods-producing and service sectors. Estimates of the underlying regression parameters are reported in the Appendix.

Our benchmark estimates of employment effects are reported in the top half of Table 4. The table reports estimates of the ARRA spending effect upon average employment difference in the 24 months after the implementation of the ARRA relative to February 2009, expressed as thousands of jobs.[33] The pair of numbers beneath each point estimate correspond to a 90% confidence interval. This table also includes estimates derived from least squares estimates of equation (3.3) for comparison.

In the state and local government sector, our point estimate is that average employment from April 2009 to March 2011 was 359 thousand persons greater than it would have been in absence of the Act. A 90% confidence interval for this effect is between approximately 156 and 563 thousand. This estimate has a sensible explanation: in states that received relatively less ARRA aid and/or 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 the same action of firing and not filling job openings—resulting in significant government jobs lost.

[33]To calculate the number of jobs 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 (3.3), with fungibility, or (4.1), without fungibility. Then, we sum across all sectors and states to find the 'all sectors' jobs effect reported in the table.

In the private sector, the corresponding point estimate jobs effect is positive 456 thousand persons, with a 90% confidence interval of *negative* 182 thousand to positive 1.1 million jobs. Thus our private sector jobs effect has a substantially wider confidence interval than our government estimates and it easily includes zero. Aggregating our estimates across sectors results in a point estimate of average employment from April 2009 to March 2011 being 816 thousand jobs greater due to ARRA funds. The corresponding 90% confidence interval is 82 thousand to 1.55 million jobs.

There are at least two potential reasons that may be behind our low estimates of private sector job creation: (*i*) ARRA funds being focused on government rather than private sector spending not producing large numbers of private sector jobs or (*ii*) there may have been a substantial private sector job creation that was partially offset by crowding out due to government hiring/retention.

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 worker layoffs. Moreover, as we have argued above, there is evidence that substantial amounts of aid not directly targeted to the government sector remained fungible, allowing state governments to spend it also in the government sector.

The crowding-out effect, (*ii*), is also plausible as a partial explanation. 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. 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;[34] 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.[35] By creating/saving government jobs, the ARRA likely 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.[36]

Even apart from government employment crowding out, to the extent that a private-sector job *directly* funded by ARRA spending was 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.[37]

[34] Greenfield (2010).

[35] 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)).

[36] 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 of Minneapolis President Narayana Kotcherlakota in several speeches.

[37] See also Jones and Rothschild (2011b).

4.2 Estimates Using Alternative Specifications

Our first alternate specification investigates a relaxation of our fungibility assumption. Rather than using $OFFSET$ as our treatment variable we include AID and $LOSS$ individually in the regression specification:

$$\text{Employ Growth}_j^{GOVT} = \alpha_1 AID_j - \alpha_2 LOSS_j + \beta' ANC_j^{GOVT} + \varepsilon_j. \tag{4.1}$$

This specification relaxes our fungibility restriction. 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. The second row of Table 4 reports jobs effects implied by our estimates of equation (4.1). Full parameter estimates are reported in our Appendix.[38]

Our estimates of jobs effects are considerably less precise without our fungibility assumption. For example, the 90% confidence interval for total employment is *negative* 194 thousand to 2.706 million jobs versus an analogous interval estimate of positive 82 thousand to 1.549 million with fungibility. Similar reductions in precision occur for each sector when fungibility is not imposed. While there is a 637 thousand job change in the private-sector point estimates, this is not a large difference relative to the standard errors of the no-fungibility estimates. The decrease in our government sector point estimates of approximately 200 thousand is also substantial but not in our view a large discrepancy given the large no-fungibility standard errors. Thus we consider our intervals' widening when fungiblity is relaxed a more salient feature than their shifts. We do not reject the restriction that the AID and $-LOSS$ coefficients are the same at conventional levels, though this result is unsurprising given the lack of precision in the no-fungibility estimates.[39]

Table 5 reports jobs effect confidence intervals for alternative specifications. Its second column presents results when an additional instrument is used: an indicator of whether a state had a Democrat as governor prior to the recession. Political considerations may have influenced the enthusiasm with which states pursued ARRA funds. There are several well-publicized instances where Republican governors, including Perry of Texas and Sanford of South Carolina, initially refused to accept parts of ARRA aid.[40] The political party of a state's governor prior to the recession is also arguably a valid instrument as it is predetermined relative to recession shocks. Having a Democrat governor is positively correlated with AID and hence $OFFSET$. Adding this third instrument increases estimation precision but qualitatively these results are similar to our benchmarks.

[38] Weak instrument (Cragg-Donald) test statistics for this alternative specification are 10.26 (Goods), 11.87 (Services), and 8.92 (Government). For evaluating these statistics we use results from Stock and Yogo (2005) for 2SLS weak IV tests. The Stock and Yogo (2005) critical value corresponding to a 5% maximal size distortion of nominal 5% level Wald tests involving treatment parameters is 7.03. Our weak instrument test statistics are high enough relative to this critical value that we proceed using strong instrument approximations.

[39] Sector-specific Wald tests of the hypothesis that $\alpha_1 = \alpha_2$ have p-values of .482 (Goods Producing), .621 (Services), and .606 (Government). The p-value for the analogous joint test across all sectors is .528.

[40] See "They're Saying No to the Cash, but Talk is Cheap" in the Washington Post (2009).

Table 5: Employment effects of ARRA spending: monthly average between April 2009 and March 2011, various state combinations, reported as thousands of jobs (ninety percent confidence intervals)

	Benchmark	Add Dem governor instrument	Use per-capita scaling	Outlays instead of obligations	Add population regressor
Private-sector					
Fung. imposed	(-182 , 1095)	(-206 , 847)	(-189 , 1243)	(-162 , 826)	(-150 , 1139)
Fung. not imposed	(-155 , 2340)	(-275 , 1960)	(-303 , 4275)	(-130 , 2063)	(-96 , 2651)
Government					
Fung. imposed	(156 , 563)	(178 , 515)	(169 , 659)	(126 , 435)	(153 , 558)
Fung. not imposed	(-267 , 594)	(-211 , 553)	(-622 , 798)	(-230 , 514)	(-256 , 592)
Total					
Fung. imposed	(82 , 1549)	(66 , 1269)	(105 , 1778)	(48 , 1177)	(115 , 1585)
Fung. not imposed	(-194 , 2706)	(-279 , 2305)	(-530 , 4678)	(-159 , 2376)	(-111 , 3002)

Notes: Negative numbers correspond to jobs destroyed/forestalled.

The third and fourth columns of Table 5 use alternate definitions for our treatment variable. In the third column scaling for AID and $LOSS$ is per capita rather than our benchmark scaling by state revenue. In the fourth column we retain our benchmark scaling but use ARRA outlays instead of obligations to calculate AID. Outlays are the amount of ARRA dollars actually paid out versus obligations which include outlays plus amounts committed to the awardees by the respective Federal agencies. With our fungiblity restriction imposed, the change to per capita scaling does not have a large impact upon our results. Without this restriction, per capita scaling interval estimates are so wide we view them as essentially uninformative. In contrast, using outlays in place of obligations notably improves our estimates' precision and these shorter intervals are qualitatively consistent with our benchmark results.

The fifth and last column of Table 5 adds a population regressor to our conditioning information. The addition of population to our employment level controls makes the specification better able to capture variation in outcomes related to differential labor force participation across states. A priori we viewed this as potentially important. However, this change in conditioning information does not qualitatively change our benchmark results.

Table 6 examines the sensitivity of our results to excluding the four smallest population states. The second column presents results when we estimate our benchmark specifications using all 50 states. When our fungibility restriction is imposed, government sector results are similar to our benchmark. However, there is an appreciable increase in our private sector employment estimates of approximately 300 thousand jobs with little change in precision. A substantial shift upward in private sector interval estimates also occurs without our fungibility restriction but, as with our benchmark results, the no-fungibility estimates are very noisy. Of the four small states, Vermont

Table 6: Employment effects of ARRA spending: monthly average between April 2009 and March 2011, various state combinations, reported as thousands of jobs

	Benchmark	Use all 50 states	46 plus Vermont	46 plus ND and WY	46 plus AK and ND
Private-sector					
Fung. imposed	(-182 , 1095)	(124 , 1456)	(-86 , 1480)	(-205 , 1312)	(124 , 1073)
Fung. not imposed	(-155 , 2340)	(504 , 2932)	(220 , 2557)	(181 , 2804)	(-28 , 2616)
Government					
Fung. imposed	(156 , 563)	(130 , 444)	(186 , 695)	(165 , 563)	(118 , 384)
Fung. not imposed	(-267 , 594)	(-483 , 500)	(-107 , 745)	(-203 , 607)	(-578 , 409)
Total					
Fung. imposed	(82 , 1549)	(351 , 1803)	(221 , 2054)	(89 , 1746)	(331 , 1368)
Fung. not imposed	(-194 , 2706)	(344 , 3109)	(317 , 3098)	(234 , 3155)	(-248 , 2667)

Notes: Negative numbers correspond to jobs destroyed/forestalled.

appears to have the largest individual influence upon our results, illustrated in the third column where it alone is added to our 46 state sample. The analogous results when either Alaska, North Dakota, or Wyoming is added by itself to the sample are similar to the benchmark. However, some pairs of these three states move estimates substantially when added to our 46 state sample. The fourth column illustrates a shift up in our no-fungibility results with the addition of only North Dakota and Wyoming to the sample. The fifth column illustrates a shift in our private sector results with the addition of only Alaska and North Dakota. The bottom line is that our benchmark results are sensitive to excluding the four smallest states; however, this sensitivity is not large enough to change the overall tenor of our results. In particular, the conclusion that our benchmark results are inconsistent with the effects reported by the President's Council of Economic Advisors is robust to these changes in sample.

5 Other Researchers' Estimates of Job Creation

5.1 Jobs Effect 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.

Since the passage of the ARRA there have been two types of studies of its jobs effect. 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. The methodology

used by Congressional Budget Office (2011b) is an example of this pre-ARRA data approach. The CBO estimates that the employment increase attributable to the ARRA was between 0.5 and 2.5 million between April 2009 and March 2011, as shown in Table 10 in the Appendix.[41] To construct these numbers (in their Table 1), the CBO divides the total spending of the ARRA into its components and then applies low and high output multipliers. These multipliers were delivered from the CBOs analysis of existing studies.

The Council of Economic Advisors uses two methodologies to assess the jobs impact of the ARRA. The first of these is a multiplier approach similar to the CBO. As illustrated in Figure 1, the Council of Economic Advisors (2010b) estimates that the Act had the effect of increasing employment by 1.9 to 2.5 million workers on average over the Act's first 24 months. This contrasts with our benchmark estimates of an increase of 82 thousand to 1.549 million workers due to ARRA spending. Our results are more consistent with the CBO estimates as our interval estimate does overlap with the CBO range.

The second CEA methodology involves estimating a time series model used to predict employment outcomes for the post ARRA period and uses these predictions as a counterfactual for employment in the absence of stimulus activity. Specifically, the CEA estimates a vector autoregression of employment and output using data from 1990:Q1 to 2007:Q4. Based on those parameter estimates, they forecast employment from 2009:Q2 to 2010:Q2. They then interpret the vector autoregression's forecast errors for employment as being due to stimulus policy. According to these estimates (Table 5), at the end of 2010:Q2, the Act had increased employment by 3.6 million workers. As the CEA notes, this procedure can only measure the 'net effect' of all combined stimulus components, e.g. fiscal as well as monetary policy actions. Moreover, any stimulus effects are inherently confounded with coincident shocks to the economy, e.g. financial shocks driving European debt concerns. Thus these estimates are not directly comparable to our estimates of the employment effects of ARRA spending.

The second type of studies are more closely related to ours and use employment outcomes observed during and after the ARRA period to estimate a jobs effect. We discuss our paper's findings relative to these studies below.

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 point estimates imply that the Act created/saved 1.9 million jobs, while their time series point estimates imply that the Act created/saved approximately 845 thousand jobs.[42] As in our no-fungibility case, the Feyrer and Sacerdote (2011) estimates are very imprecise. For example, they estimate the cost of a job (employment measured in October 2010) to lie in a 90% confidence interval between $48,000

[41] Note that their ranges are computed based on both government spending as well as tax cut incentives in the Act.

[42] 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.

and $1.85 million.[43]

Chodorow, et. al. (2011) study the employment effect of a particular facet of the ARRA, the Federal Medicaid Assistance Program. This component of the ARRA provided state governments with a one-time supplement to the ongoing Federal aid for state Medicaid programs. This Medicaid assistance made up 17% of the overall cost of the ARRA (excluding tax cuts). The authors estimate that the program was highly effective, creating one job-year at a cost of $26,000. Since Chodorow, et. al. (2011) study one component of the ARRA, their results are not directly comparable to ours.

The most closely related study to ours is Wilson (2011) who estimates the job effects of ARRA spending using state-level variation via an instrumental variables method. Wilson's overall results suggest a larger jobs effect of ARRA spending than we find, though our results are compatible in some aspects. Though we share an econometric method, we use different outcomes, treatment measures, conditioning information, and instruments. Thus, our results provide a complementary look at ARRA job effects.

Wilson's treatment measures differ from *OFFSET* in that he uses per capita scaling. In addition, his preferred stimulus measure is funds at an 'announcement' stage months prior to the funds being obligated to specific recipients, constructed from his own careful collection of non-achived federal funding announcements.

Wilson's conditioning variables (included exogenous regressors) also differ from ours. We think the most important differences are that he uses less information about lagged employment than we do and includes a measure of the change in personal income. The former is simply a difference in judgement calls regarding a parsimonious characterization of state employment benchmark trajectories. The latter is more directly motivated since, in our framework, personal income changes are not a valid exogenous included variable because they are directly related to our endogenous state revenue loss measure, LOSS (and hence OFFSET). They are more easily argued to be exogenous in Wilson's framework.

Wilson's outcomes are per capita changes in employment from February 2009 to different horizons during the following two years, with February 2010 as his preferred benchmark. Our outcome is an average of employment changes from April 2009 to March 2011. We focus on this average effect because it is more precisely estimated than month-by-month effects in our specifications. Wilson's preferred estimates imply a 90% confidence interval estimates of the jobs effect of announced ARRA spending in March 2010 of approximately 600 thousand to 3.5 million jobs. This differs from our benchmark interval estimate for the average jobs effect of 82 thousand to 1.549 million jobs. Leaving aside differences due to announced versus obligated spending, it is possible for the impulse response to spending to be consistent with Wilson's range in March 2010 while still on average being consistent with our range. Our average response measure could be below the level of the response midway simply due to responses in the first few months post ARRA being small.

[43]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 (2011) did not report these confidence intervals directly.

Wilson focuses on aggregate specifications while we allow separate parameters by sector when constructing aggregate employment effects. We pay a cost in precision for the benefit of flexibility. However, our benchmark results are compatible with his sector-specific preferred results. He also finds statistically insignificant private sector effects and positive, statistically significant government job effects for his benchmark treatment variable of announced ARRA funding through February 2010.

6 Conclusion

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

First, researchers must employ additional restrictions or data, beyond simply cross-state variation in spending, to improve the precision of jobs effect estimates. Other researchers working with cross state data find wide confidence intervals and we find very wide confidence intervals when we do not impose fungibility. Our fungibility restriction substantially improves precision, leading us to think that more explicit economic modeling of state governments' aid spending decisions could yield additional useful restrictions.

Second, researchers should allow for cross-state and region spillovers. This might result in estimates of a larger overall 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.[44] If this type of spillover from interstate trade is widespread nationally, then the economy-wide jobs effect of the ARRA may be substantially larger than what we find.

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 think that an accurate economic model of the ARRA experience will need to address three main issues. First, the most recent recession and the Act's implementation happened partway through a sectoral shift from goods-producing to a service (of a

[44] Another type of cross-state spillover is possible because U.S. states share a common monetary policy. Suppose that, as a result of the Stimulus, the Federal Reserve had overseen a change in the expected real interest rate. Then, the interest rate change would have had a largely common effect across all states. Because this common effect itself would not generate cross-state variation, a cross-sectional regression would not pick it up (see Nakamura and Steinsson (2010)). While potentially important, it is unlikely that the common monetary policy effect is relevant for the ARRA episode. During the ARRA period, the nominal federal funds rate was effectively zero and the committed to be kept it at zero for a substantial time. Also, inflation expectations, as measured from the Survey of Professional Forecasters, were unchanged during the months when the surveyed forecasters began predicting a Federal stimulus.

particular type) sector economy as evidenced in Figure 3.[45] The way that state and local governments channeled ARRA funding into different sectors was likely to have consequences for the effects of the Act. Second, as explained in the paper, relatively less educated people faced, and continue to face, a much worse job market than more educated people. For this reason, we think it will be important to differentiate between high and low education workers along with their respective labor markets. Third, we have provided 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, we think it will be important to include a hierarchal intergovernmental component in the spirit of Bradford and Oates (1971a).

Acknowledgements

We are grateful to Dean Baker, Mark Bils, David Blau, Daniel Bunn, Yongsung Chang, Gabriel Chodorow-Reich, Steve Cohen, Jim Donlin, Duane Dupor, Paul Evans, Joel Gratz, Sam Gratz, Christian Hansen, Don Haurin, Dough Hecox, Damian Kozbur, Igor Livshits, Lance Lochner, Jim MacGee, Rabi Mishalani, Matt Shapiro, Jay Shambaugh, Bahi Subhash, Bruce Weinberg, Daniel Wilson, Mark Wolfgram, Sara Vermillion, Matthew Yglesias as well as other government officials and academics for useful conversations and background. Selina Gonzalez provided outstanding research assistance. We also thank seminar participants at the University of Western Ontario, Federal Reserve Bank of Cleveland, the Ohio State University, the University of Michigan and the University of Rochester. We are especially grateful to Chris Robinson for assistance with Census data and Matt Lewis for many useful discussions.

[45] Existing economic modeling of sectoral shifts include Davis and Haltiwanger (2001) and Phelan and Trejos (2000).

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A Appendix

A.1 Data Construction

The numerator for AID_j, as defined by equation (3.1) uses ARRA obligated dollars taken from the Federal agencies' "Weekly Financial and Activity Reports," which are cumulative over time and available on the recovery.gov web site.[46] 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.

The denominator for AID_j requires a measure of 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 as well as local governments' revenue. This is the calendar-year state revenue from all sources, which is reported in the "Annual Surveys of State and Local Government Finances."

This measure of pre-recession state government revenue is also the denominator for $LOSS_j$, as defined by equation (3.2). Its numerator requires a measure of total tax revenue collected by state government. 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.[47] In addition to state revenue, we use state Medicaid outlays (independent of the funding source). Medicaid data is available in National Association of State Budget Officers (2009). 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 timing of state tax revenue exactly.

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 three 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.

Our measures of inelastic revenue share are from the 2007-08 "Annual Surveys of State and

[46] Documentation for the web site data is contained in Recovery Accountability and Transportation Board (2009)

[47] 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.

Table 7: Response of employment growth (monthly average between April 2009 and March 2011) to state government losses and obligated ARRA money, fungibility imposed, all coefficients reported

	Goods-producing	Services	Government
AID-LOSS	0.06330	0.02545	0.14339
	(0.09059)	(0.02151)	(0.04942)
Pre-recession level (in billions)	0.01698	0.00035	-0.00082
	(0.00748)	(0.00037)	(0.00350)
One-year lag employ. growth	0.07855	0.18078	0.05329
	(0.09145)	(0.08097)	(0.12478)
Two-year lag employ. growth	-0.34595	-0.24858	-0.01079
	(0.16146)	(0.13701)	(0.15539)
Midwest/Northeast region	0.02554	-0.00282	0.00045
	(0.00653)	(0.00195)	(0.00353)
Oil-production per capita	0.22684	-0.08748	0.24652
	(0.43185)	(0.10401)	(0.22291)
House price grow (03-06)	-0.01461	0.00019	0.00155
	(0.03056)	(0.00715)	(0.01498)
House price grow (06-08)	0.14633	0.01289	0.03987
	(0.03164)	(0.00846)	(0.01410)
Constant	-0.07357	0.00403	-0.00050
	(0.01240)	(0.00438)	(0.00481)

Local Government Finances." In the denominator, we use reported total state government revenue from all sources. Using categories defined in this survey, our set of inelastic categories is: property taxes, selective sales taxes, charges and miscellaneous revenue, utility revenue, liquor store revenue, and intergovernmental transfers. The main revenue categories implicitly defined as elastic are: income taxes, general sales taxes, and insurance trust revenue.

The remaining data are treated as exogenous variables. Midwest/Northeast indicators are based on the U.S. Census Bureau's region definitions. 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.

A.2 Additional Tables and Figures

Table 8: Response of average employment growth (monthly average between April 2009 and March 2011) to state government losses and obligated ARRA money, fungibility not imposed, all coefficients reported

	Goods-producing	Services	Government
AID	0.15690	0.05978	0.06522
	(0.16930)	(0.04458)	(0.10440)
-LOSS	0.02801	0.01288	0.17357
	(0.10324)	(0.02619)	(0.06097)
Pre-recession level (in billions)	0.01906	0.00054	-0.00244
	(0.00784)	(0.00042)	(0.00404)
One-year lag employ. growth	0.09112	0.18034	-0.02912
	(0.09133)	(0.08220)	(0.14919)
Two-year lag employ. growth	-0.34888	-0.24193	-0.05327
	(0.15586)	(0.13949)	(0.16043)
Midwest/Northeast region	0.02510	-0.00290	0.00007
	(0.00636)	(0.00198)	(0.00356)
Oil-production per capita	0.17971	-0.10343	0.29637
	(0.42367)	(0.10742)	(0.23091)
House price grow (03-06)	-0.01776	-0.00160	0.00649
	(0.02984)	(0.00752)	(0.01600)
House price grow (06-08)	0.14498	0.01282	0.04359
	(0.03067)	(0.00860)	(0.01458)
Constant	-0.09104	-0.00285	0.01642
	(0.02912)	(0.00894)	(0.02018)
Reject fungibility (p-value)	0.482	0.621	0.606

Table 9: Least-square regression of instrument relevance, all coefficients

	$OFFSET$	AID	$-LOSS$
Highway spending	3.98156	3.39619	0.58537
	(1.41021)	(0.66649)	(1.18647)
Inelastically-sourced revenue ratio	0.34633	-0.03663	0.38296
	(0.09615)	(0.04544)	(0.08089)
Democratic governor (divided by 1000)	0.00623	-0.00032	0.00656
	(0.00381)	(0.00180)	(0.00320)
Pre-recession level (in billions)	1.05993	-0.03607	1.09599
	(0.74714)	(0.35311)	(0.62860)
One-year lag employ. growth	-2.25842	-0.69723	-1.56119
	(1.27237)	(0.60135)	(1.07051)
Two-year lag employ. growth	-0.00660	-0.00192	-0.00467
	(0.01837)	(0.00868)	(0.01546)
Midwest/Northeast region	-1.28765	-0.31264	-0.97501
	(0.97700)	(0.46175)	(0.82200)
Oil-production per capita	-0.07555	-0.01739	-0.05817
	(0.06480)	(0.03062)	(0.05452)
House price grow (03-06)	0.07116	0.01375	0.05741
	(0.08164)	(0.03858)	(0.06869)
House price grow (06-08)	-0.29425	0.12293	-0.41718
	(0.08548)	(0.04040)	(0.07192)

Notes: Based on March 2011 AID and LOSS.

Table 10: Our benchmark estimates of jobs created/saved as a result of ARRA-spending component (in millions), with Congressional Budget Office and President's Council of Economic Advisors ranges shown for comparison

	Benchmark (this paper)		President Council of Economic Advisors		Congressional Budget Office	
	Low	High	Low	High	Low	High
2009:Q2			0.3	0.4	0.1	0.5
2009:Q3			1.0	1.1	0.3	1.2
2009:Q4			1.8	1.8	0.5	1.9
2010:Q1			2.2	2.6	0.6	2.7
2010:Q2			2.6	3.2	0.7	3.4
2010:Q3			2.7	3.5	0.7	3.6
2010:Q4			2.5	3.6	0.6	3.5
2011:Q1			2.4	3.6	0.6	3.3
Total employment (avg.)	0.1	1.5	1.9	2.5	0.5	2.5
Private sector (avg.)	-0.2	1.1				
Government (avg.)	0.2	0.6				

Notes: Benchmark low-high estimate is 90% confidence interval for national employment due to ARRA-spending component (averaged between April 2009 and March 2011). Other sources are Council of Economic Advisors (2011) and Congressional Budget Office (2011a). The CEA and CBO estimates each include the effects of tax cuts.