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Abstract

This paper estimates the "jobs multiplier" of fiscal spending using the state-level allocations of federal stimulus funds from the 2009 American Recovery and Reinvestment Act (ARRA). Specifically, I estimate the relationship between state-level federal ARRA spending and the change in states' employment outcomes from the time the Act was passed (February 2009) to some later month (through June 2010). Because the allocation of stimulus spending across states may be endogenous with respect to state economic outcomes, I instrument for stimulus spending using exogenous formula-driven cost estimates made by the *Wall Street Journal* and the *Center for American Progress* around the time that the ARRA was passed. To control for the counterfactual – what would have happened without the stimulus – I include several variables likely to be strong predictors of state employment growth. The results point to substantial heterogeneity in the impact of ARRA spending over time, across sectors, and across types of spending. The estimated jobs multiplier for total nonfarm employment is large and statistically significant for ARRA spending through March 2010, but falls considerably and is statistically insignificant beyond March. The implied number of jobs created or saved by the spending is about 2.0 million as of March, but drops to 0.8 million as of June. Across sectors, the estimated impact of ARRA spending on construction employment is especially large, implying a 23% increase in employment (as of June 2010) relative to what it would have been without the ARRA. Lastly, I find that spending on infrastructure and other general purposes has a large positive impact, while aid to state government to support Medicaid may actually reduce state and local government employment.

"Not for the first time, as an elected official, I envy economists. Economists have available to them, in an analytical approach, the counterfactual.... They can contrast what happened to what would have happened. No one has ever gotten reelected where the bumper sticker said, 'It would have been worse without me.' You probably can get tenure with that. But you can't win office."

U.S. Representative Barney Frank, July 21, 2009. (Washington Post, 2009)

I. Introduction

This paper analyzes the fiscal stimulus spending provided by the 2009 American Recovery and Reinvestment Act and contrasts what happened to what would have happened. The ARRA or "Recovery Act" was enacted into law in February 2009 amidst a great deal of economic and political debate. At the time, it was estimated to cost $787 billion over ten years. More recent estimates put the cost at $814 billion[1], of which about two-thirds comes from increased federal government spending and one third is reduced tax revenues.[2] Proponents saw the stimulus package as a vital lifeline for an economy heading toward a second Great Depression. They pointed to projections from the White House and others suggesting that the stimulus package would "create or save" around 3.5 million jobs in its first two years. Opponents claimed the massive cost of the ARRA would unduly swell the federal deficit while having minimal or even negative impacts on employment and economic growth.

Since the ARRA's passage, a number of studies have attempted to measure these impacts. As the quote above alludes to, the key focus of these studies is isolating the effects on economic outcomes of the stimulus package from what would have occurred in its absence. The methodologies used in these studies can be divided into two broad categories. The first methodology employs a large-scale macroeconometric model to obtain a baseline, no-stimulus forecast and compares that to a simulated forecast where federal government spending includes the ARRA. This is the methodology used in widely-cited reports by the Congressional Budget Office (CBO) (see, e.g., CBO 2010a), the White House's Council of Economic Advisers (CEA) (see CEA (2009, 2010)), private forecasters such as Macroeconomic Advisers, IHS Global Insight, and Moody's Economy.com, as well as a number of academic studies.[3] The key distinction between that methodology and the one followed in this paper is that the former does not use observed data on economic outcomes following the start of the stimulus. Rather, it relies

[1] See Congressional Budget Office (2010a).
[2] See Congressional Budget Office (2010b), Table A-1.
[3] See, for example, Cogan, et al. (2009), Cwik and Wieland (2009), and Drautzberg and Uhlig (2010).

on a macroeconometric model, the parameters of which, including its fiscal spending multiplier(s), are estimated using historical data prior to the ARRA (or pulled from the literature which estimated them using historical data).

The second methodology is an attempt to count the jobs created or saved by requiring "prime" (or "first-round") recipients of certain types of ARRA funds to report the number of jobs they were able to add or retain as a direct result of projects funded by the ARRA. These counts are aggregated up across all reporting recipients by the Recovery Accountability and Transparency Board (RATB) – the entity established by the ARRA and charged with ensuring transparency with regard to the use of ARRA funds – and reported online at www.recovery.gov and in occasional reports to Congress.[4] The number of jobs created or saved, and any fiscal multiplier implied by such a number, reflects only "first-round" jobs tied to ARRA spending, such as hiring by contractors and their immediate subcontractors working on ARRA funded projects, and excludes both "second-round" jobs created by lower-level subcontractors and jobs created indirectly due to spillovers such as consumer spending made possible by the wages associated with these jobs and possible productivity growth made possible by ARRA-financed infrastructure improvements. By contrast, the methodology of this paper uses employment totals as reported by the Bureau of Labor Statistics, and therefore all direct and indirect jobs created by the ARRA should be reflected in the results. Furthermore, only 55% of ARRA spending are covered by these recipient reporting requirements (see CEA 2010, p.27).

The methodology I employ in this paper is distinct from these others in that it uses both observed data on macroeconomic outcomes – namely, employment – and observed data on actual ARRA stimulus spending. It exploits the variation across the 50 states in these outcomes and the amount of federal stimulus allocated to them. By analyzing how states that exogenously received more stimulus fared compared to states which received less stimulus, one can isolate the effects of the stimulus spending from both the macroeconomic cycle as well as other fiscal and monetary stimulus measures, which were implemented on a national basis. These national measures include the Troubled Asset Relief Program (TARP), the Federal Reserve's near-zero Fed Funds rate target, and the Federal Reserve's various balance sheet expansion programs. The stimulus provided by these measures to any given state is roughly proportional to the size of that

[4] For more details and discussion of these data on ARRA job counts, see Government Accountability Office (2009) and CBO (2010b).

state's economy and, regardless, is uncorrelated with the allocation of ARRA spending across states.

The vast majority of ARRA spending is allocated across states according to pre-determined formulas whose factors are exogenous with respect to economic outcomes.[5] For instance, the bulk of the Department of Education's ARRA funds are allocated in proportion to states' youth populations, and the Department of Transportation uses exogenous factors such as the number of highway miles in a state to determine state ARRA (and non-ARRA) funding. Nonetheless, the timing of when these and other funds are announced, and especially when they are obligated or actually disbursed, could be endogenous. First, states whose economies have deteriorated more than anticipated may have received more ARRA funds for social services such as Medicaid (the federally-mandated, state-administered health insurance program for low-income families). Second, some states were slower than others in completing the necessary actions required to receive federal matching grants (such as for education and transportation spending). If such slowness is indicative of problems or inefficiencies in the fiscal governance of those states, it might also be negatively correlated with their economic outcomes. For these reasons, a simple comparison – say, via Ordinary Least Squares (OLS) regression – of stimulus spending to economic outcomes across states may yield misleading results. An Instrumental Variables (IV) technique is required.

I instrument for stimulus spending using exogenous formula-driven cost estimates made by the *Wall Street Journal* and the *Center for American Progress* around the time that the ARRA was passed. These organizations estimated the final (10-year) cost outlays of ARRA's funds by state and category (which maps very closely to federal agency) based on the ARRA formulas mentioned above as well as estimates put out by Congressional subcommittees. These instruments turn out to be good predictors of the actual ARRA spending by state in later months. To control for the counterfactual – what would have happened without the stimulus – I include in the regression model any variables that (1) are likely to be predictive of subsequent employment growth, (2) could potentially be correlated with the instruments for stimulus spending, and (3)

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[5] Many of these formulas, for instance those used to distribute federal highway funds, are just the long-standing, pre-ARRA formulas used by various federal-to-state transfer programs; these formulas were not altered by the ARRA even as the ARRA expanded funding of the programs. For other transfer programs, however, such as that for Medicaid, the additional ARRA funding was allocated according to a new formula laid out in the ARRA legislation. See Section III for more details.

were known at the time of ARRA passage (so arguably exogenous with respect to subsequent economic outcomes).

One limitation of this methodology, however, is that the resulting ARRA multiplier estimates will not include any purely global or national (non-state-varying) impact that ARRA spending may have had. Such "common factors" will be subsumed in the constant term of a cross-state regression. That is, strictly speaking, the multipliers estimated in this paper are "local" multipliers of the type studied in Moretti (2010). The bias this might cause could go in either direction. Cogan, et al. (2009) argue that increases in federal debt resulting from fiscal stimulus measures will push up interest rates and retard private investment and durables consumption, pushing down the ARRA's impact. Other authors, though, have argued that this standard general equilibrium channel was closed off during the recent recession and recovery due to the zero-interest-rate bound (see, e.g., Woodford (2010) and Drautzburg and Uhlig (2010)). Another potential common factor is increased aggregate demand for non-local inputs – particularly, capital goods and intermediate materials. That is, even if just a few states received all of the ARRA's spending, other states could benefit by seeing increased demand for their traded goods. For this reason, as Moretti (2010) points out, the local multiplier may be a lower bound for the national multiplier, at least for tradable-good sectors. Moretti also shows that the local multiplier may be an upper bound for the national multiplier for non-tradable sectors (because of the lower elasticity of labor supply nationally and the possibility that increased jobs in non-tradable sector could crowd out jobs in tradable sector). These theoretical bound predictions help inform the discussion below about how my estimated multipliers vary across sectors.

The remainder of the paper is organized as follows. The next section provides some background on the ARRA legislation and a description of the data used in the analysis. In Section III, I describe the empirical methodology and discuss the endogeneity issues which motivate the instrumental variables strategy employed in the paper. The empirical results are presented and discussed in Section IV. In Section V, I discuss the implications of these results and compare them with those of other studies relating to the ARRA or fiscal stimulus in general. Section VI offers some concluding remarks.

II. Background on the American Recovery and Reinvestment Act

The ARRA is a large and multifaceted piece of legislation. As mentioned above, it is expected to cost more than $800 billion over ten years. Of that total, 64% comes from increased federal outlays (excluding refundable tax credits) and 36% comes from reduced tax revenues and outlays on refundable tax credits (see CBO, 2010b, Table A-1). This paper focuses on the impact of the spending component.

I exclude from the analysis the spending done by the Department of Labor (DOL), which primarily is funds sent to state governments to pay for extended and expanded unemployment insurance (UI) benefits for several reasons. First, these funds are not included in the announcements data. Second, and most importantly, this type of spending in a given state is driven almost entirely by the change in the state's unemployment rate, which is one outcome I consider in the paper and is highly correlated with the others (employment change); there is virtually no source of exogenous variation to use as an instrument for this variable. Third, perhaps because this type of spending is so difficult to predict, one of the two sources (the *Wall Street Journal*) I use for instruments for ARRA spending does not provide estimates of the state allocation of DOL spending. Moreover, the other source (the Center for American Progress) estimates the allocation of DOL spending using numbers based on a forecast (as of January 2009) of unemployment rates by state. If this forecast reflects information not included in my regressions and which has predictive power, then such an instrument would not be valid. The numbers reported in the remainder of the paper reflect non-DOL ARRA spending only. (DOL spending accounted for 17% ($64.5 billion) of total obligations through June 2010.)

Before describing the patterns over time and across states of ARRA spending, it is important to clarify exactly how ARRA spending is measured and reported. A unique aspect of the ARRA relative to previous major fiscal spending initiatives is the heavy emphasis on data transparency and reporting. In particularly, the legislation itself called for the creation of a website, www.recovery.gov, to provide detailed information on ARRA spending to the public. **Figure 1** provides a screen-shot from recovery.gov. The screen-shot illustrates one manner in which the website conveys information on the breakdown of ARRA spending across states.

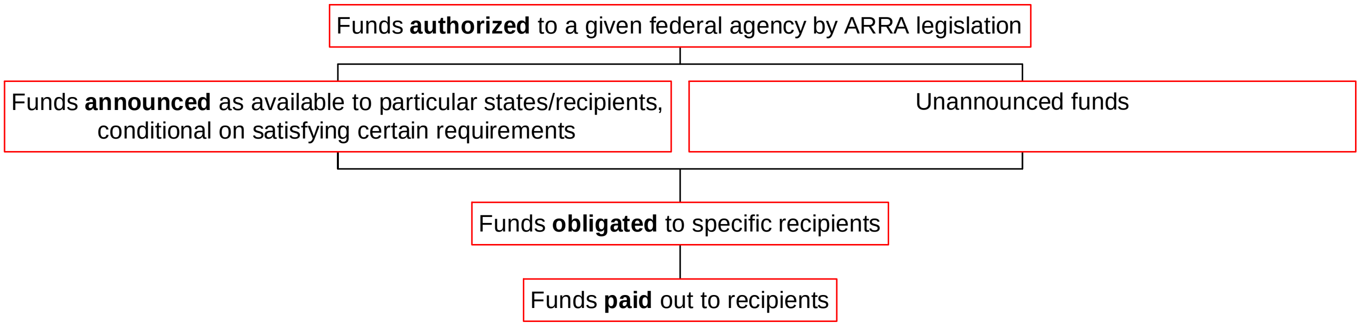
Figure 1 – Screen Shot from Recovery.gov showing ARRA Announcements, Obligations, and Payments by State

Source: http://www.recovery.gov/Transparency/agency/Pages/AgencyLanding.aspx

The website reports on three different metrics of spending and breaks down each of these metrics by federal agency and the state where the recipient individual, organization, or government entity resides or is headquartered.[6] The three different metrics are "announced funds" ("announcements"), "funds made available" ("obligations"), and "funds paid out" ("payments"). **Figure 2** provides a schematic that depicts how these three metrics are related in terms of accounting flow.

[6] Recovery.gov provides both recipient-reported data and agency-reported data. Because the recipient-reported data only cover a little over half of all ARRA spending, I use the agency-reported data, which covers all ARRA spending.

Figure 2. Flow of ARRA Spending



Each federal agency was given authorization by the ARRA legislation to either spend up to an explicit limit or according to formulas that depend on changing conditions (e.g., extended unemployment insurance benefits which will expand with the number of unemployed). Based on that authorization, the agency may subsequently *announce* how much each recipient – private contractors, state governments, local governments, non-profits, etc. – will receive in funds. However, a small portion of authorized funds are never announced. Whether they are announced or not, authorized funds are eventually *obligated* to individual recipients. For example, the Department of Transportation (DOT) might award a contract to a construction firm or municipal agency at which point the DOT is said to have obligated those funds to that recipient. Finally, when recipients satisfy the requirements of their contracts, the agency actually pays out the funds. Data on announcements, obligations, and payments are geocoded by state and reported on recovery.gov. It should be noted, however, that for each spending metric, not all agencies and not all funds are reported separately by state. As of June 2010, about 20% of announcements, 11% of obligations, and 8% of payments were not separated by state.[7] For the remainder of the paper, I will use and discuss only the state-allocated spending data.

Through June 2010, a little over half of the ARRA spending has been obligated and about one-third has been actually paid out. The progression of spending can be seen in **Figure 3**, which shows (state-allocated) ARRA funding announcements, obligations, and payments from April 2009 through June 2010. As of June 2010, announcements, obligations, and payments were $275.3 billion, $309.3 billion, and $180.4 billion, respectively. As indicated by the

[7] The majority of the non-state-allocated funds are from the Department of Agriculture. Given that I only analyze nonfarm employment outcomes in this paper, the exclusion of this spending from my data should have little effect on the results.

schematic in **Figure 2**, obligations can be, and often are, larger than announcements (both at the aggregate level and for any given state) because not all obligations were previously announced.

The ARRA spending (excluding DOL spending) is spread over dozens of separate federal agencies, though three agencies in particular account for the bulk of it. The disaggregation across major agencies is shown in **Table 1**. Through June 2010, the Departments of Education (ED), Health and Human Services (HHS), and Transportation (DOT) are responsible for 65% of the spending announcements, 71% of obligations, and 75% of payments.

Figures 4-6 show the evolution, from April 2009 through June 2010, of announcements, obligations, and payments, respectively, for each of these four major spending agencies and other agencies combined. The first thing to note about these figures is that there is very little time series variation in the announcements data (see **Figure 4**). Rather, these major agencies tend to have one month (or a few months in the case of HHS) when nearly all of their announcements were made and then make only minimal further announcements. Obligations and payments, however, increase more gradually over time. It is also clear that, for each of the three categories, the composition of spending across agencies changes quite a bit over this time. For instance, obligations and payments from HHS have tended to grow faster over time than spending by other agencies (see **Figures 5-6**).

III. Methodology and Data

I perform a cross-sectional (cross-state) analysis, estimating the relationship between cumulative stimulus spending and macroeconomic outcomes, controlling for various likely predictors of these outcomes. Specifically, I estimate the following simultaneous-equations model via IV/GMM:

$$\left(Y_{i,T} - Y_{i,0}\right) = \alpha + \beta S_{i,T} + \mathbf{X}_{i,0}\Gamma + \varepsilon_{i,T} \quad (1a)$$

$$S_{i,T} = \delta + \lambda\left(Y_{i,T} - Y_{i,0}\right) + \mathbf{X}_{i,0}\Theta + \mathbf{Z}_{i,0}\Phi + \upsilon_{i,T} \quad (1b)$$

$\left(Y_{i,T} - Y_{i,0}\right)$ is the change in the outcome variable of interest ($Y_{i,t}$) from the initial period when the stimulus act was passed ($t = 0$) to some later period ($t = T$). $S_{i,T}$ is cumulative ARRA spending per capita in state i as of period T. $\mathbf{X}_{i,0}$ is a vector of control variables (and "included" instruments). $\mathbf{Z}_{i,0}$ is a vector of ("excluded") instruments.

The outcome ($Y_{i,t}$) variables I consider are:

1. Employment, scaled by 2009 population. Annual (2009) population by state comes from the Census Bureau. The employment series I use for most of the regressions in the paper is the state-level payroll employment series from the Bureau of Labor Statistics' (BLS) Current Employment Statistics (CES) payroll survey. These data are seasonally adjusted, available at a monthly frequency (with an approximately two month release lag), and available for the total nonfarm sector as well as by industry. Another set of employment variables I look at comes from the BLS's Business Employment Dynamics (BED) program. The BED data provide gross job gains from opening or expanding establishments, gross job losses from closing or contracting establishments, and the difference between the two (net jobs change). The underlying source for the BED data is the Quarterly Census of Employment and Wages (QCEW), also known as the ES-202 series, which is a census of state administrative (UI) records. The BED data are available quarterly, seasonally-adjusted, and only for the private nonfarm sector. They are released with a considerable lag (latest data as of the time of this writing are for 2009:Q4).
2. Unemployment rate (seasonally-adjusted monthly data from the BLS household survey).[8]

For employment, I estimate the stimulus effect separately for total nonfarm, private nonfarm, state and local government, construction, manufacturing, and (private) education and health services. These latter four subsectors are of particular interest to many analysts because they have been severely impacted by this recession and were expected to be key beneficiaries of the ARRA stimulus act.

Employment and stimulus spending are scaled by population for three reasons. First, many of the agency formulas for allocating ARRA funds to states are expressed in per capita terms. Second, scaling by population puts variables in units that are more comparable across states, mitigating potential inference problems stemming from large outliers.[9] Third, if one is

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[8] One important difference between the household-survey based unemployment rate and the employer-survey based employment data is that the former are geocoded according to state of employee residence whereas the latter are geocoded according to state of employer location. So some of any direct unemployment reduction induced by the stimulus funding provided to a given state may actually show up as lower (than otherwise) unemployment in neighboring states. This should bias the coefficients on the stimulus variable toward zero and positively bias the coefficients on out-of-state stimulus (a variable included in some regressions), when the unemployment rate is the dependent variable.

[9] One argument against scaling is that it gives more weight in the regression to smaller states than they would otherwise have and small states typically have more measurement error in the outcome variable than do large states. In the Results section, I assess whether the results are robust to this concern by estimating the model via weighted

interested in the effect of stimulus on the unemployment *rate*, which is of wide general interest and is a scaled variable, the measure of stimulus spending must by scaled.

I include four control variables in each regressions. Following Blanchard and Katz's (1992) empirical model of state employment growth, I control for lagged employment growth and the initial level of employment. Specifically, I include the change in employment per capita from the start of the recession (Dec. 2007) to when the ARRA was enacted (Feb. 2009) and the initial level of employment per capita as of February 2009. The third control variable is the growth in income per capita from 2006 to 2007. This variable is included because it directly enters the formula determining the state allocations of ARRA "Fiscal Relief" funds. These funds come from the Department of Health and Human Services (HHS) and were meant to help states pay for Medicaid (the federally-mandated, state-administered health insurance program for low-income families). Lastly, I control for estimated ARRA tax benefits received by state residents. This variable is the sum of estimated tax benefits from the ARRA's "Making Work Pay" (MWP) payroll tax cut and its increase of the income thresholds at which the Alternative Minimum Tax (AMT) becomes binding. Following the Center for Budget and Policy Priorities (CBPP), the MWP benefits are estimated by taking each state's share of the national # of wage/salary earners making less than $100,000 for single filers and less than $200,000 for joint filers (roughly the levels above which the MWP benefit phases out), as of 2006, and multiplying by the total cost of MWP tax cuts ($116.2b over 10 yrs, according to CEA (2010)). Similarly, using state-level data from the Tax Policy Center on each state's share of national AMT income, as of 2007, one can estimate AMT benefits by multiplying that share by the total cost of the AMT adjustment ($69.8b, according to CEA (2010)).

These control variables are included because they are likely to be both good predictors of subsequent state economic outcomes <u>and</u> could be determinants of the allocation of stimulus funds across states. That is, they belong on the right-hand side of both equations (1a) and (1b) (which is why they are considered "included" instruments in the parlance of instrumental variables, as opposed to the "excluded" instruments, $\mathbf{Z}_{i,0}$, that are excluded from equation (1a)). It is important to emphasize that the primary goal of this analysis is to obtain an unbiased estimate of β, not necessarily to find the best forecasting model of state economic outcomes from February 2009 to the latest month of data. Note that there are only 50 observations. A fully

regression, weighting by the inverse of the estimated sampling error variance provided by the BLS. These results are similar to the unweighted results.

saturated model – that is, one containing control variables that potentially affect $\left(Y_{i,T} - Y_{i,0}\right)$ but don't affect $S_{i,T}$ – would severely limit the degrees of freedom and the ability to precisely estimate the key parameter of interest, β.

As mentioned earlier, the stimulus variable, $S_{i,T}$, may well be endogenous ($\lambda \neq 0$). There are two potential sources of endogeneity. First, some of the components of $S_{i,T}$ are explicitly functions of current economic conditions. For example, consider the formula determining the state allocation of spending from the Department of Health and Human Services' (HHS) "Fiscal Relief Fund," which is meant to help state governments pay for Medicaid expenses. Each state's per capita receipts from this Fund depend on three factors: (1) the current federal Medicaid share (which is a function of pre-stimulus income per capita), (2) the "hold-harmless" component (a function of 2006-2007 growth in state income per capita), and (3) the change in the unemployment rate from the beginning of the recession through February 2009. These factors determining ARRA Fiscal Relief funds may also be correlated with post-stimulus economic conditions – e.g., states with a rapid pre-stimulus increase in the unemployment rate may be more likely to rebound more quickly than other states because the rapid increase might suggest those states tend to enter and exit recessions earlier than others. However, note that if these factors are controlled for directly in $\mathbf{X}_i$, then this source of endogeneity should be eliminated.

A second potential source of endogeneity, especially for obligations and payments, is that the amount (or share) of ARRA spending going to any given state is partly a function of how successful the state government is at soliciting funds from federal agencies. Most of the state allocation of funding announcements is exogenously determined by formulas, but much of obligations and payments are allocated at the discretion of the federal agencies as they review whether states have satisfied so-called "maintenance of effort" (MOE) requirements and what their plans are for how they intend to spend the money. States with unfavorable MOE's or spending plans may receive funding later or not at all (e.g., DOT funds have a "use it or lose it" requirement[10]). States that are more successful in soliciting funds and starting projects may also be better-run state governments, and better-run states may be more likely to have positive

[10] See http://transportation.house.gov/Media/file/ARRA/Process%20for%20Ensuring%20Transparency%20and%20Accountability%20Highways%201%20YEAR.pdf

outcomes regardless of the stimulus funds. One can address this source of endogeneity via instrumental variables.

I instrument for actual ARRA spending (measured by announcements, obligations, or payments) by state, $S_{i,T}$, using initial 10-year ARRA cost estimates.[11] At least two organizations, the *Wall Street Journal* (*WSJ*) and the *Center for American Progress* (*CAP*), published, around the time the stimulus bill was passed by Congress, their own estimates of how the final (2009-2019) cost of the ARRA's spending would be broken down by state and by category (e.g., Education, Transportation, Health, etc.).[12] For most ARRA programs, both the WSJ and CAP simply compiled allocations from reports made (in January and early February of 2009 as the ARRA was being shaped and debated) by the federal agencies/departments in charge of the major ARRA programs. In other cases, the *WSJ* and *CAP* estimated the allocations based on either past (pre-ARRA) allocations (for programs for which the allocation formula did not change) or data on the program's formulary factors combined with knowledge of the formula itself. Details about the data sources underlying the *WSJ*'s and the *CAP*'s allocations are provided in Appendix A.

These allocations are likely to be strong predictors of subsequent actual ARRA spending. In addition, they should be orthogonal to unanticipated future macroeconomic outcomes (i.e., $\varepsilon_{i,T}$ from equation (1a)) for two reasons.[13] First, they were estimated at the time of the ARRA's enactment, before any information on subsequent economic outcomes was known. Second, both the *WSJ* and *CAP* estimates were based on a combination of (1) formulas that depend on strongly exogenous factors – for example, the Department of Transportation's funds are allocated largely according to the number of highway miles in each state and the Department of Education's funds

[11] Motivated by suggestive results from Inman (2010) and Ruben (2010), I also experimented with using political factors as instrument that, *a priori*, one might suspect as having an influence on the allocation of stimulus funds. In particular, I looked at whether ARRA funds were disproportionately directed to states with more senators or representatives chairing key budgetary committees, with more senators or representatives serving as ranking minority members, with more senators or representatives voting for the ARRA, or whose residents voted in larger proportions for Obama in the 2008 presidential election. I found these variables to have very little predictive power and hence were not useful as instruments for ARRA spending by state.

[12] Both organizations took as given the nationwide 10-year cost estimates, in total and by program, estimated by the Congressional Budget Office at the time the ARRA was passed by Congress. What differs between the two organizations is the estimated allocation of these costs across states.

[13] CAP's estimates were published/posted online in early February 2009. The WSJ estimates were published in mid-April. Based on the source information listed by the WSJ as underlying their estimates, it is unlikely that any information of economic outcomes for March or April (especially given the BLS does not release state-level employment data for a given month until three to six weeks after the month has ended) could have factored into their estimates, contaminating the exogeneity of the instruments. Nonetheless, I have repeated the regressions reported below using only the CAP instrument, and the results are very similar.

are allocated in large part according to each state's youth population – and (2) estimates of past state allocations of federal transfers (for example, by the Department of Health and Human Services). Importantly, according to the *WSJ*'s and *CAP*'s descriptions of their estimation methodologies, there is no indication that their estimates are based on any kind of forecasting exercise, which could have meant that there were additional $\mathbf{X}_{i,0}$ variables that they used for forecasting but which I have omitted from my regressions.

Because these state-level cost estimates are broken down by category, I can also use the category-specific data as instruments for agency-specific stimulus spending. For instance, I use the CAP's and WSJ's estimates for final ARRA spending on "Health" as an instrument for actual ARRA spending to date by the Department of Health and Human Services. Summary statistics for these instruments as well as all of the other variables used in the analysis are shown in **Table 2**.

I will refer to β as a fiscal multiplier. Formally, β represents the marginal effect of per capita stimulus spending on the outcome change from period *0* to *T*. When the outcome variable is the fraction of the population that is employed (in total or in a particular sector), β represents the number of jobs created or saved per dollar of stimulus:

$$\beta^{JOBS} \equiv \frac{\partial\left(\left(L_{i,T} - L_{i,0}\right)/POP_{i,0}\right)}{\partial\left(S^{\$}_{i,T} / POP_{i,0}\right)} = \frac{\partial\left(L_{i,T} - L_{i,0}\right)}{\partial S^{\$}_{i,T}}, \qquad (2)$$

where $L_{i,t}$ is the level of state employment, $POP_{i,0}$ is state population in 2009, and $S^{\$}_{i,t}$ is the level of cumulative stimulus spending in the state ($S^{\$}_{i,t} = S_{i,t} * POP_{i,0}$). I will refer to β^{JOBS} as the "jobs multiplier." The reciprocal of β^{JOBS} represents the stimulus cost per job created or saved. One can obtain the total nationwide number of jobs created or saved up to a particular date *t* by multiplying the estimated marginal effect (jobs multiplier) by the amount of stimulus dollars spent nationally up to date *t* ($S^{\$}_{t}$):

$$JOBS_t = \beta^{JOBS} * S^{\$}_{t}. \qquad (3)$$

The cross-sectional analysis described above smoothes over any variation among states in the timing pattern of stimulus receipts between the ARRA's enactment and the end of the sample period. This timing may contain useful information, but also may be endogenous for two reasons. First, as mentioned above, states with well-run governments may fulfill the requirements necessary to receive certain ARRA funds sooner than other states and having a well-run government may itself lead to better economic outcomes. Second, some components of the ARRA will be doled out to any given state in response to negative economic shocks as they hit the state, so again the timing of the stimulus will be endogenous with respect to the timing of economic outcomes.

Unfortunately, while I arguably have strong and valid instruments for cumulative stimulus spending up to any particular post-ARRA-enactment date, I have no additional instruments that predict the state-specific time path of the spending. Therefore, because I cannot extract an exogenous component of the time path, I cannot exploit the time variation in stimulus. This rules out a panel econometric model.[14]

The cross-sectional analysis also smoothes over variation in the timing/dynamics of economic outcomes between Feb. 2009 and the end period of the analysis. The regression only considers how much employment, state GDP, etc. has changed between Feb. 2009 and the end period; it does not assess how stimulus spending affected the path that the economy took to get to that end period. Because the cross-sectional regression smoothes over the timing of stimulus and the dynamic path of economic outcomes, it should be thought of as estimating the medium-run impact of the ARRA.[15] Nonetheless, I report results below on how the estimated jobs multiplier varies by the choice of sample end date. This variation in the estimated multiplier reflects both the effect of stimulus spending, for a given month, on current and future employment (i.e., the distributed lag structure or impulse response function of the stimulus effect) as well as changes in the quantity and composition (across agencies) of spending over time.

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[14] At first glance, it may seem feasible to estimate a panel regression with monthly changes in economic outcomes as the dependent variable and monthly flows of stimulus as one of the explanatory variables, and then instrument for the monthly stimulus flows using the time-invariant expected stimulus variable. One can show, however, that because of the time-invariance of the instrument, this model reduces to the cross-sectional IV model I estimate in this paper.

[15] These estimates should not be considered long-run or permanent effects because they do not necessarily capture effects long into the future of federal tax increases or spending cuts down the road required to finance the stimulus package. Authors such as Drautzburg and Uhlig (2010) have argued that these future fiscal adjustments can be very costly.

IV. Results

A. <u>Raw Correlations</u>

Before discussing the fiscal multiplier estimates obtained from estimating equation (1) above, it is useful to first get a sense of the raw correlations between the key variables of the analysis – in particular, (1) between the three different measures of ARRA spending, (2) between ARRA spending and employment outcomes, (3) between ARRA spending and the instruments, and (4) between the instruments and employment outcomes.

Correlations between alternative measures of stimulus

The scatterplot in **Figure 7** shows the relationship across states between ARRA announcements per capita (x-axis) and obligations per capita (y-axis), through June 2010. **Figure 8** shows announcements per capita versus payments per capita. The dashed line in each scatterplot is a 45° line. In **Figure 7**, states are divided fairly evenly on each side of the 45° line, meaning there's no general pattern of announcements exceeding obligations or vice-versa. There is, however, a clear positive correlation. As **Figure 8** shows, there is also a positive correlation between announcements and payment, though it is weaker and the slope of the relationship is lower because payments to date are typically lower than announcements (or obligations) to date.

Both figures also show that there one or two outliers in announcements per capita. Alaska, and to a lesser extent, North Dakota and Montana have received much more in announcements per capita than other states. These states, in fact, tend to rank high in announcements per capita for all of the major spending agencies. Alaska's announcements per capita are particularly high relative to other states for Department of Health and Human Services (mainly Medicaid) spending. More generally, states with low population densities tend to receive more ARRA spending announcements per capita. This is driven partly by the fact that low-density states tend to have lower income per capita (a negative factor in many ARRA formulas) and by the fact that the Department of Transportation allocates its ARRA funds in large part in proportion to the number of highway miles (per capita) in the state, which tends to favor states where the population is spread out.

Correlations between stimulus spending and employment change

Figures 9-11 show scatterplots with the January 2009 – June 2010 change in employment on the y-axis and announcements, obligations, or payments on the x-axis. (All variables are scaled by 2009 state population.) The red lines in each figure are OLS regression fit lines. For announcements and obligations, there is a clear positive correlation with the post-ARRA-enactment change in employment, though the fit appears to be stronger for announcements. For payments, on the other hand, there is no clear positive or negative relationship.

Of course, these simple bivariate correlations should not be interpreted as representing a causal link, or lack thereof, from stimulus spending to employment outcomes. These plots/correlations do not control for any other factors that may affect employment and that may be correlated with stimulus spending. More importantly, they do not adjust for any reverse causality from weak employment outcomes leading to more or earlier stimulus spending.

Correlations between ARRA spending and the instruments

Figures 12-14 show the relationship between the Center for American Progress (CAP) instrument – anticipated 10-year cost of ARRA by state at the time of enactment – and announcements, obligations, and payments through May 2010. Again, all variables are in per capita terms. These scatterplots essentially show how strong/relevant this instrument is for each measure of ARRA spending. The instrument is positively correlated with, and strongly predictive of, both announcements and obligations. It is also positively correlated with payments, though the fit is weaker.

Similar scatterplots for the WSJ instrument are shown in **Figures 15-17**. The patterns are similar to those using the CAP instrument, except that the WSJ instrument appears to be better at predicting announcements, while the CAP instrument appears to be better at predicting obligations and payments.

Correlations between the instruments and employment outcomes

It is often useful before presenting IV-type regression estimates to consider the relationship in the data between the instrument and the dependent variable. **Figures 18-19** show scatterplots between each of the two instruments and the post-ARRA-enactment employment change. Both instruments have a strong positive correlation with employment change.

B. Baseline OLS and IV/GMM Results

The results of estimating equation (1), with the initial period equal to February 2009 and the end period equal to June 2010, are shown in **Tables 3-7**. The standard errors are robust to heteroskedasticity. Bold coefficients are statistically significant at the 10% level or below. The dependent variable in each regression is a change in employment per capita (using 2009 population) or the unemployment rate. In addition to the ARRA spending variables, the explanatory variables include the growth in Gross State Product (GSP) per capita from 2006 to 2007 (a factor in the allocation of HHS/Medicaid funds), an estimate of the ARRA tax benefits going to the state, the change in the dependent variable from December 2007 to February 2009 (as a measure of the pre-ARRA employment trend in the state), and the level of the dependent variable in February 2009. The stimulus variables are measured in millions of dollars per capita.

Table 3 shows results for total nonfarm payroll employment. The first two columns show the results with stimulus measured by cumulative announcements through June 2010. The OLS estimate of β is 4.6, with a robust standard error of 2.0. As shown in equation (2), this number can be interpreted as saying that each $1 million of ARRA announced funds is associated with 4.6 jobs created or saved (between February 2009 and June 2010). The IV estimate is 2.8 (s.e. = 3.0). The jobs multiplier is closer to zero and less precisely estimated for obligations. The OLS estimate is 2.7 (s.e. = 3.3), and the IV estimate is 0.7 (s.e. = 4.8). For payments, both OLS and IV, the estimated multiplier is negative and even less precisely estimated than for obligations. For all three measures of stimulus, the IV estimate of the jobs multiplier is statistically insignificantly different from zero. The first-stage F statistics, shown at the bottom of the table, are well above standard critical values indicating minimal weak-instrument bias. Stock and Yogo (2004) provide critical values of first-stage F statistics for weak instrument tests for two-stage least squares (2SLS) regressions; at conventional significance levels, they list a critical value of 11.59 for the case of one endogenous variable and two instruments. It is worth noting that for all three measures of stimulus, the IV estimates are more positive than the OLS estimates, suggesting that the OLS estimates are positively biased.

Table 4 shows the estimated jobs multiplier for the private nonfarm sector. Note that the jobs multiplier for a subsector of total employment, such as private nonfarm, should be expected to be smaller than the total employment multiplier unless stimulus spending has a zero or negative effect on the rest of the economy. For each of the three stimulus measures, the IV estimate of the multiplier for private nonfarm is negative though not statistically significant.

Next I consider four more narrow sectors that are of particular interest with respect to the ARRA. Given large portions of the stimulus package was targeted at aid for state and local governments, infrastructure, high-tech and green manufacturing, healthcare, and education, I look at the sectors of construction, manufacturing, state and local government, and private-sector education and health services.[16] The results for the state and local government sector are shown in **Table 5**. The IV estimated multiplier is positive but insignificant in all three cases. For announcements and obligations, the multiplier estimates are similar, at 1.2 and 0.9, respectively. The payments multiplier is 1.9. **Table 6** gives results for the construction sector. In all cases, the estimated jobs multiplier is large, positive, and statistically significant. Based on the IV estimates, the construction jobs multiplier is 5.2 for announcements and 6.3 for obligations. It is even larger, at 10.3, for payments but the standard error for payments is also large, at 5.0. The results for manufacturing are shown in **Table 7**. For announcements and obligations, the estimated jobs multiplier is positive and significant at 1.5 and 1.7, respectively. The payments multiplier is larger but statistically insignificant. **Table 8** shows results for the (private) education and health services sector. (Employment for education and health services are not available separately for a large number of states.) The multiplier estimates for this sector are small and statistically insignificant. Below, in Section V.A., the magnitudes of these sector-specific multipliers will be evaluated relative to each sector's baseline level of employment.

Table 9 presents results for the unemployment rate. The estimated ARRA spending impact is negative – i.e., spending reduced the unemployment rate – but it is imprecisely estimated and statistically insignificant. This imprecision is likely due to the relatively large measurement error in state-level unemployment rates, which are based on a smaller-scale household survey rather than the large-scale employer survey used for the payroll employment data.[17]

To sum up these baseline results, there is little evidence that *total* ARRA spending has had a statistically significant impact on employment, through June 2010, in the total nonfarm or private nonfarm sectors. However, ARRA spending does appear to have had a positive and

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[16] Unfortunately, employment data is not available for public-sector education and health services.

[17] The unemployment rate is measured from a smaller-scale household survey (approximately 50,000 households nationally) than the employer-based CES survey (approximately 400,000 establishments covering 40% of total nonfarm employment) on which the employment data is based. Moreover, data from the household survey is geocoded according to state of *employee* residence, whereas the employment data from the payroll survey reflects employment by state of *establishment*, which suggests the payroll survey data are more likely than the household survey to reveal employment/unemployment effects of in-state stimulus.

significant impact on employment in the manufacturing, and construction sectors. We will also see below that the impact of ARRA spending varies greatly over time and across different types of spending. First, however, it is important to establish that the results are robust to possible measurement error.

C. Robustness Checks

I perform two robustness checks related to potential measurement error in the CES employment data. The first one addresses the concern that some states, especially less populous states, may have more measurement error in the employment than others and should be given less weight in these regressions. **Table 10** presents results where states are weighted by the inverse of their sampling error variance from the CES payroll survey, as reported by the BLS.[18] The table shows (only) the IV-estimated jobs multiplier for each of the three stimulus measures and each of the four categories of employment investigated in **Tables 3-8**.[19] Along with the coefficient on spending and its standard error, the regression's first-stage F statistic is also displayed (in italics). For ease of comparison, the IV-estimated multipliers from **Tables 3-8** are reproduced in Panel A of **Table 10**. Comparing Panels A and B, one can see that the multipliers obtained in the weighted regressions are similar to those obtained without weighting.

The second robustness check assesses whether using February 2009 as the initial month instead of January 2009 substantially changes the results. Using January has the advantage of being relatively sure that the ARRA, passed on February 19, 2009, could not have impacted January economic activity, unless the ARRA was anticipated. Media reports from the time, though, and the Congressional vote count suggest that the size and composition of the ARRA was largely unknown prior to passage and that passage of the act was far from certain leading up to the final vote. It is more plausible, however, that the ARRA could have had some impact on economic activity as early as February. This would lead to a downward bias on the estimated jobs multiplier. Nonetheless, using an earlier month introduces more noise into the estimation of the change in economic activity due to ARRA spending.

Table 11 compares the estimated jobs multipliers for each of the four sectors and each of the three stimulus measures from using January (Panel B) instead of February (Panel A,

[18] These sampling error variances are highly negatively correlated with state population.
[19] Because the baseline estimate of the stimulus impact on the unemployment rate is so imprecisely estimated, I do not include this specification in the robustness checks.

reproduced from **Tables 3-8**) as the initial month. The IV-estimated jobs multipliers obtained from using January are generally similar to, though somewhat larger (more positive) than, those obtained using February. One exception is that using January as the start date results in a statistically significant jobs multiplier for the state and local government sector for all three measures of stimulus. Consistent with the notion that using January introduces additional noise into the analysis, the standard errors tend to be somewhat larger when January is used as the initial month.

D. Impact of ARRA spending over time

The impact of ARRA spending may increase or decrease over time as the quantity and composition of the spending itself changes over time. Moreover, the impulse response from any given month of ARRA spending will vary over time according to the lag structure governing the effects of spending on employment – that is, how long it takes for spending to maximally affect recipients' hiring/retention decisions and how lasting any ARRA-induced jobs are.[20] **Figures 20-21** show how the IV-estimated jobs multiplier, for each employment category, varies as one advances the last month of the sample from August 2009 through June 2010, which is the latest month of available data (as of the time of this writing). **Figure 20** shows the results when announcements are used as the stimulus measure; **Figure 21** shows the results when obligations are used.[21] In clockwise order starting with the upper left panel, the six panels show the results for employment in total nonfarm, private nonfarm, construction, education and health services, manufacturing, and state and local government. In each panel, the solid line shows the estimated coefficient on cumulative ARRA spending (as of that month). The dashed lines indicate the 90% confidence interval.

Based on announcements, the estimated multiplier for total nonfarm employment was positive and significant throughout the sample period up until and after April 2010. It was at or near its peak of about 7.5 in January, February, and March of 2010. The multiplier for private nonfarm shows a similar pattern over time except that it was only statistically significant in January 2010. The multiplier for state and local government is generally small and statistically

[20] As mentioned in Section III, estimating a distributed lag model of the impact of ARRA spending is precluded, at least with the IV estimator, by the fact that the instruments do not vary over time and therefore I do not have separate instruments for each lag of stimulus spending (each of which is likely to be endogenous).
[21] The multipliers for payments tend to be much more imprecise, especially in the earlier months. Because of this imprecision and to conserve on space, the payments results are not shown here but are available upon request.

insignificant except in September 2009. The jobs multiplier for construction shows a pronounced U shape: it is very high in the fall of 2009, declines gradually going into the winter, and then begins to rise again. It remains positive and statistically significant throughout. The timing of this patterns suggests that there may be a seasonal pattern at work, either because construction-oriented ARRA spending produces jobs only during the times of year conducive to construction or because the seasonal adjustments that the BLS applies to its state employment data are inadequate.[22] The manufacturing sector shows an opposite pattern, with the peak jobs multiplier occurring in the winter months. Nonetheless, the manufacturing jobs multiplier is positive and significant for all months after October 2009. Lastly, the multiplier for the education and health services sector is generally positive but insignificant up until June 2010, at which point it becomes negative but remain insignificant. The patterns for obligations, shown in **Figure 21**, are overall similar to those for announcements. In particular, they also indicate that the peak jobs multiplier of ARRA spending was in the first quarter of 2010. (The peak for obligations occurs in February, whereas the peak for announcements occurs in January.)

E. Impact of ARRA spending by type of spending

The results presented thus far assume that the impact of ARRA spending is the same for all types of spending. However, it is quite likely that funds directed to private contractors for work on infrastructure and other capital projects will have very different employment effects than funds directed to state and local governments for general fiscal aid or funds to support safety-net programs such as Medicaid. To investigate the potential heterogeneity in the jobs multiplier across different types of spending while maintaining a relatively parsimonious specification, I aggregate ARRA spending by federal agency up to three groups: (1) spending by the Department of Education (ED), which consists primarily of fiscal aid to state governments; (2) spending by the Department of Health and Human Services (HHS), which consists primarily of funds for Medicaid (health insurance for low-income families); and (3) spending by all other agencies, much of which comes from the Department of Transportation (DOT). Likewise, I aggregate up the initial 10-year cost estimates by agency from the *WSJ* and *CAP* using this grouping to have separate instruments for each group.

[22] The BLS estimates seasonal factors separately for each 1-digit NAICS supersector (such as Construction) in each state. It is also worth noting that I have repeated the regressions for the construction sector using non-seasonally adjusted data and obtained very similar results, suggesting that pattern over time in the construction jobs multiplier is not driven by some spurious correlation between stimulus spending and the seasonal adjustment factors.

The IV results of allowing the jobs multiplier to vary by these three types of spending (in the same regression) are shown in **Table 12**.[23] The results based on using announcements as the spending measure are shown in Panel A; those based on obligations are shown in Panel B. The results using payments are very imprecisely estimated and hence uninformative, so they are not included here. Each column of each panel represents a single regression, for the sector indicated, containing all three categories of stimulus spending. The jobs multiplier for the total nonfarm sector tends to be positive and large for DOT and Other spending, positive but small for ED spending, and negative and large for HHS spending. Yet, these multipliers is rather imprecisely estimated. It should be noted that the Donald-Cragg minimum-eigenvalue statistics (Cragg and Donald (1993)), which is a multiple-endogenous-variable generalization of the first-stage F statistic, are rather low in some cases. Stock and Yogo (2004) derive critical values for the Donald-Cragg statistic below which indicate weak-instrument bias. In particular, they report that for the case of three endogenous variables and six instruments, which is the present case, the critical value associated with a maximal bias of the IV estimator relative to OLS of 10% is 7.77; the critical value for a maximal bias of 20% is 5.35. The Donald-Cragg statistics in **Table 12**, Panel A (announcements) range from 3.23 for the state and local government regression to 9.11 for that of construction. Those in Panel B range from 4.39 to 5.08. Therefore, these spending-by-type results should be viewed with some caution.

The pattern of results is qualitatively similar for private nonfarm employment compared with total nonfarm, though the multipliers are closer to zero. The pattern is also similar for state and local government employment but the positive multiplier for DOT and Other is statistically significant as is the negative multiplier for HHS. The finding that infrastructure and other general spending has a positive impact on state and local government employment may be because many of the projects funded by this spending must be managed/monitored by state and local government entities (e.g., state Departments of Transportation). The negative multiplier for this sector from HHS spending is somewhat surprising. It could reflect negative burdens placed on state government budgets (and transfers from state to local governments) resulting from states needing to shift general funds to maintain Medicaid benefit levels in order to receive the full amount of Medicaid reimbursement funds for which the state is eligible. Of course, given the somewhat weak instruments, it is also possible that this result reflects a negative bias. As might

[23] Results for the unemployment rate were very imprecisely estimated (even more so that in those in Table 7) and hence are not shown. They are available from the author upon request.

be expected, DOT and Other spending have a strong positive impact on the construction sector, while HHS spending has a negative multiplier. For manufacturing, DOT and Other spending has a positive and significant multiplier; ED and HHS spending each have small and insignificant multipliers. In the education and health services sector, the multiplier for all three categories are small and statistically insignificant.

F. Impact of ARRA spending over time by type of spending

The six panels in **Figures 22-23** show, for each sector, how the estimated jobs multiplier for DOT and Other spending varies as one advances the last month of the sample from August 2009 through June 2010. That is, the estimate shown for a given month and a given sector (e.g., total nonfarm) is the coefficient on combined ARRA spending by the DOT and Other agencies (i.e., non-ED, and non-HHS) in an IV/GMM regression akin to those shown in **Table 12**. **Figure 22** gives the results for announcements as the stimulus measure; **Figure 23** gives the results for obligations. (As with the total ARRA spending results above, the multipliers for payments tend to be much more imprecise, especially in the earlier months. Because of this imprecision and to conserve on space, the payments results are not shown here.) Beginning with the announcements results, the estimated jobs multiplier for total nonfarm employment is positive and significant up until April 2010, at which point it becomes insignificant. For private nonfarm, it is positive but insignificant from December 2009 onward. The multiplier for state and local government employment is always positive and significant, and it has generally risen over the sample period. As I found above for total ARRA spending, DOT and Other spending appears to have had a large and significant positive impact on construction employment in the summer and fall of 2009, then gradually fell until bottoming out in February 2010 and rising again through June 2010. The multiplier on this type of spending for manufacturing is positive and significant for all months after October 2009. Lastly, the multiplier for education and health services is generally near zero and insignificant. Similar patterns are found for obligations in **Figure 23**, except that except that DOT and Other spending does have a positive and significant jobs multiplier for the education and health services sectors for all months from October 2009 through March 2010.

As in **Table 12**, when June 2010 was the end-month, the coefficients on ED spending are generally imprecisely estimated and statistically insignificant throughout this sample period for all six sectors. Hence, the results are not shown here.

The results for HHS spending, for announcements and obligations, are shown in **Figures 24-25**. For both announcements and obligations, the estimated jobs multiplier from HHS spending for the total nonfarm sector is negative but generally insignificant (with the exception of one month, Nov. 2009). The multiplier for private nonfarm tends to hover around zero and is never significant. The difference between total nonfarm and private nonfarm, of course, is government, and state and local government employment comprises roughly 75% of total government employment. Hence, given the generally negative multiplier for total nonfarm and the near-zero multiplier for private nonfarm, it is not surprising to see that the multiplier for state and local government is strongly negative. In fact, the negative impact of HHS spending on state and local government employment is statistically significant in all months but one (Dec. 2009). As mentioned above, this negative impact could reflect an unintended side-effect of the "maintenance of effort" (MOE) requirements that states must meet in order to receive the full amount of Medicaid funds for which they are eligible under the ARRA. The MOE requirements are that states must maintain (or expand) their Medicaid eligibility rules and benefits at their 2008 levels. Thus, it is possible that state governments, faced with dramatically widening budget gaps in 2009 and 2010, were forced to allocate more of their general funds toward transfer to Medicaid recipients and away from other areas of state government (and transfers to local governments), causing job cuts (or fewer job gains) in those areas.

G. An Extension: Impacts on Job Gains versus Job Losses

An important part of the debate on the employment impact of the ARRA spending has been the extent to which the stimulus has increased employment through creating new jobs versus saving existing jobs. Of the net increase in employment for any given month since February 2009 that the cross-state regression attribute to ARRA spending, it is nearly impossible to know how much is from new jobs created versus retention of existing jobs because employment data generally focuses on employment counts rather than tracking individual workers or positions. The ARRA recipient reports offer one possibility of disentangling jobs created versus saved, by asking recipient directly how many jobs were created by the funds they received and how many jobs were saved, but those data come with substantial shortcomings as noted earlier. It is possible, however, to assess the differential impact of ARRA spending on job gains at opening or expanding establishments versus job losses at closing or contracting establishments. As described in Section III, the BLS's Business Employment Dynamics (BED) series contains such data.

Table 13 reports the results of the IV cross-state regressions where the dependent variable is the change from March 2009 to December 2009 in either gross job gains (at opening or expanding establishments), gross job losses (at closing or contracting establishments), net employment change according to the BED data (the difference between job gains and job losses), and net employment change according to the CES data (included for reference). For all three measures of stimulus, I find that ARRA spending increased both job gains and job losses. In fact, for announcements and obligations, the increase in job losses is larger than the increase in job gains. Yet, because I have not imposed any adding-up constraint across the three regressions (job gains, job losses, and net change), the effect of the stimulus on net job changes remains positive (though not significant in the case of announcements).[24]

V. Overall Impact on Employment and Comparisons with Other Studies

A. Overall Impact of ARRA on National Employment

The discussion thus far has focused on the sign and statistical significance of the estimated jobs multipliers. Here I turn to drawing out the economic implications of the results. As mentioned in Section III (see equation (3)), one can calculate the total, nationwide number of jobs created or saved by ARRA spending, implied by a given jobs multiplier estimate, by multiplying that estimate by the amount of ARRA spending to date. The preferred specification from above – IV/GMM using announced funds as the stimulus measure (because it is arguably more exogenous to start with than obligations or payments, and additionally is better predicted by the instruments) – yielded a jobs multiplier for the total nonfarm sector of 2.8 (per million dollars). Announcements through June 2010 totaled $275.3 billion. The jobs multiplier of 2.8 then implies that ARRA spending has created or saved about 0.8 million jobs in the nonfarm sector between February 2009 and June 2010. That number represents just a 0.6% increase (relative to the level in February 2009).

Using an earlier end-month, however, yields a very different implication. In particular, the estimated multiplier (again, for announcements and total nonfarm employment) is

[24] In future drafts, I plan to estimate these three regressions simultaneously using three-stage least squares, which will allow me to impose this adding-up constraint.

approximately 7.5 when January, February, or March of 2010 are used as the end-month. ARRA announcements were roughly constant over that period at about $270 billion. These numbers imply that ARRA spending created or saved 2.0 million jobs (about 1.5%) as of January, February, or March of 2010. The difference in the implied impact between these earlier months and June could be due in part to statistical imprecision, but also may reflect that some jobs created or saved as a result of stimulus spending done in 2009 and early 2010 were short-lived rather than permanent. The results separated by type of spending seem to suggest that this may be especially true for infrastructure and other general spending.

Using the same ARRA spending total, one can calculate similar figures for the private nonfarm, state and local government, construction, manufacturing, and education and health services. The IV-announcements multiplier estimate for private nonfarm implies essentially zero change in employment in the sector, as of June 2010. The multiplier estimates for state and local government, construction, manufacturing, and education and health services imply employment changes from February 2009 to June 2010 of 2.0% (0.3 million jobs), 23.0% (1.4 million jobs), 3.3% (0.4 million jobs), and –0.4% (about 60,000 jobs). The theoretical framework of Moretti (2010) may explain some of these differences across sectors. Moretti finds that in a tradables sector the local multiplier should be a lower bound for the national multiplier, while in a non-tradables sector the local multiplier should be an upper bound. This might help explain why the (non-tradable) construction sector is found to have a much larger multiplier than the (tradable) manufacturing sector. It does not, however, help explain the low multipliers in the state and local government sector and the education and health services sector.

How do these results compare to estimates from other studies of the number of jobs created or saved by the ARRA? A major advantage of this paper relative to other studies is that it is able to provide separate fiscal multipliers by type of spending, by sector, and over time. Other studies that do not consider actual data on observed economic outcomes and on stimulus spending are not able to provide this kind of disaggregation. Nonetheless, it is interesting to compare the "bottom-line" estimate of total nonfarm jobs created or saved by ARRA spending to estimates from other studies. I start with comparing it to the estimates from the most prominent and publicized governmental studies – the quarterly reports of the Council of Economic Advisors (CEA) and the Congressional Budget Office (CBO).

B. Comparison with Government Studies

The most recent CEA report was released July 14, 2010 (see CEA (2010)) and the most recent CBO report is from August 24, 2010 (see CBO (2010b)). Both studies estimate the number of jobs created or saved due to total ARRA costs, including spending and tax cuts for any given quarter. As of June 2010, the CEA reports a range of 2.5 to 3.6 million jobs, whereas the CBO's range is 1.5 to 3.5 million jobs.[25] The range as of March 2010 was 2.2 to 2.8 million for the CEA and 1.3 to 2.8 million for the CBO.

This paper estimates that ARRA spending (excluding tax cuts) created or saved approximately 2.0 million jobs at its peak impact – which occurred from January through March of 2010 – but that the impact fell in the months thereafter, reaching 0.8 million jobs as of June 2010. It should be reiterated that the impact I estimate in this paper relates only to ARRA spending, not ARRA tax reductions. ARRA spending is about half of total ARRA costs through mid-2010 (two-thirds of estimated costs through 2019). This implies that *if* the jobs multiplier of tax cuts is the same as that for spending, then this paper's estimate of 0.8 million jobs through June 2010 from ARRA spending would imply around 1.6 million jobs due to total ARRA costs, which is near the low end of the CBO's estimates and well below the CEA's estimates.[26] This paper's estimate of 2.0 million jobs through March 2010 from ARRA spending would imply 4.0 million jobs from total ARRA costs, which is well above either the CEA's or the CBO's range of estimates. Thus, the key difference between the ARRA employment effects implied by this paper and those estimated by the CBO and CEA has to do with timing. This paper estimates a bigger impact in the first year of the ARRA, but then a steep drop-off in its employment effects in the legislation's second year, while the CBO and CEA estimate a continual, almost linear, increase in the ARRA's employment effects over time.

C. Comparison with Academic Studies

Broadly speaking, there are two veins of modern academic studies on fiscal multipliers. The first analyzes the predicted effects of fiscal policy using a theoretical model. Most papers in this vein calibrate a DSGE model to calculate the predicted effects of one-time or permanent

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[25] The CBO estimates the number of workers, rather than jobs, that the economy had at the end of 2010Q1 that it would not have had without the ARRA. They report that the ARRA resulted in 1.4 to 3.3 million added workers. According to the BLS, in both 2008 and 2009, 5.2% of workers held more than one job. Assuming that these workers primarily held two jobs (as opposed to three or more), the CBO's estimates of 1.4 to 3.3 million added workers translates to 1.4728 to 3.4716 million added jobs.

[26] Of course, there is much debate about whether tax cuts or spending have a larger fiscal multiplier. For studies addressing this issue, see Blanchard and Perotti (2002), Mountford and Uhlig (2008), Alesina and Ardagna (2009), and Barro and Redlick (2009).

change in government spending (or taxes). In particular, Cogan, et al. (2009) and Drautzburg and Uhlig (2010) employ versions of the Smets and Wouters (2007) DSGE model to predict the effects on GDP, consumption, and investment of a government spending shock (or series of shocks) sized to match the ARRA. Though neither paper analyzes employment effects, making their results difficult to compare to those of my paper, it is interesting to note that both find that the GDP multiplier falls rapidly once the flow of stimulus spending begins to wane, which is qualitatively consistent with the time pattern I find for the jobs multiplier.[27]

The second vein of studies typically estimates impulse responses to a generic government spending shock. In contrast to my paper, these studies do not estimate fiscal multipliers specific to the ARRA (i.e., using data on economic outcomes and government spending during the ARRA episode). There is an active debate in this literature regarding how to properly identify these spending shocks. The majority of the literature, dating back at least to the influential paper of Blanchard and Perotti (2002), identify these shocks via a Vector Auto-Regression (VAR) estimation in which government spending is ordered ahead of other variables in a Choleski decomposition. This is done, for example, in the recent studies by Monacelli, Perotti, and Trigari (2010) (hereafter, MPT) and Bruckner and Pappa (2010) (hereafter, BP). Ramey (2010), on the other hand, argues that this identification strategy will incorrectly time the true spending shocks because such shocks are frequently anticipated by agents, and hence influence economic activity, one or more quarters ahead of the observed spending. Ramey, therefore, identifies military spending shocks based on a careful reading of historical publications and real-time private forecasts. One drawback of this narrative approach, at least in so far as it is used to infer the likely effects of fiscal stimulus initiatives such as the ARRA, is that the economic impact of military spending, especially that supporting foreign wars, may be very different than the impact of the type of countercyclical fiscal spending typically enacted and/or debated during downturns. The ARRA, for example, contained very little funding for the Department of Defense.

Despite the considerable differences in data and methodology between my paper and these impulse-response studies, it is nonetheless useful to compare the results as directly as possible. To do so, I consider the estimated impulse response functions for employment from

[27] The main difference between the Cogan, et al. (2009) and Drautzburg and Uhlig (2010) papers is that the latter allows for distortionary taxation and the zero interest rate bound of monetary policy. Consequently, the latter paper finds a larger short-run fiscal multiplier (due to the zero bound) but a more negative long-run multiplier (due to the cost of the distortionary taxation required to repay the debt incurred by the stimulus).

each of the three papers mentioned above (MPT, BP, and Ramey). Specifically, for each I obtain their estimated employment elasticities with respect to an increase in government spending $\left(\varepsilon_s = \frac{dL_{t-s}}{dG_{t-s}} \bullet \frac{G}{L}\right)$, for s = 0 to *T-1* quarters after the initial government spending (*G*) shock. The cumulative response of employment (*L*) after a series of *T* quarters of government spending shocks is then:

$$L_t - L_{t-T} = \sum_{s=0}^{T-1} dL_{t-s} = \frac{L}{G} \sum_{s=0}^{T-1} \varepsilon_s dG_{t-s}.$$

I measure *L* and *G* using their pre-ARRA levels (*G* as 2008 total government spending from the National Income and Product Accounts, Table 3.1, and *L* as total nonfarm employment as of Feb. 2009). Plugging in the flow of ARRA spending (measured using announcements) from 2009:Q1 to 2010:Q2 into this formula, I obtain the total number of jobs created or saved, as of each quarter, implied by each paper's estimated impulse response function. Since Ramey estimates the impulse response for hours rather than employment, I generate two alternative employment estimates based on her results. The first is based on the assumption that the intensive margin – hours per worker – is unaffected by government spending. The second assumes that the intensive margin increases in the same proportion as the extensive margin (hours). **Figure 26** shows the results alongside the estimates provided is this paper. The estimates from this paper are simply the average estimated jobs multiplier for the three months in a given quarter multiplied by cumulative ARRA spending, measured by announcements, as of the end of that quarter.

I find that MPT's results imply ARRA-induced employment that increases steadily over time, reaching 2.6 million jobs by the 2nd quarter of 2010. BP's impulse response implies a peak effect of about 2.2 million in 2009:Q3, but then a steady decline to just 0.7 million by 2010:Q2. Ramey's implied employment effects gradually rise over time but are lower than MPT's, reaching between 0.8 and 1.5 million jobs by 2010:Q2. The estimates from this paper for the last quarter, at 0.8 million, are very similar to those of BP and Ramey with hours per worker fixed. Like BP, I also have a sharp decline in the ARRA employment effect, though the peak effect according to my estimates occurs two quarters later (in 2010:Q1) than in BP.[28]

[28] These results are broadly similar if one measures ARRA spending using obligations instead of announcements.

VI. Conclusion

This paper analyzed the employment impacts of fiscal stimulus spending, using state-level data from the American Recovery and Reinvestment Act (ARRA) enacted in February 2009. Cross-state IV/GMM results imply that ARRA spending created or saved about 2.0 million jobs, or 1.5% of pre-ARRA employment, in the total nonfarm sector by early 2010. However, the results indicate that many of these ARRA-generated jobs were short-lived, as the estimated employment impact fell to just 0.8 million (0.6% of pre-ARRA employment) by June 2010. In addition to this change over time, I also find substantially heterogeneity in the ARRA's employment impact across sectors, and across types of spending. The impact on construction employment was especially large: an 23% increase in employment (as of June 2010) relative to what it would have been in absence of ARRA spending. Across different types of spending, the results suggest that infrastructure and other general spending have large, positive multipliers while "strings-attached" aid to state governments for Medicaid reimbursement may actually reduce state and local government employment.

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Appendix A.
Details of Data Sources Underlying *CAP* and *WSJ* Instruments

The data sources underlying the *CAP* and *WSJ* estimates of state allocations of ARRA spending are described below. The *CAP* and *WSJ* provide estimates for nearly all ARRA spending programs. An important exception is that the WSJ does not report estimated allocations for the approximately $36 billion of Department of Labor (DOL) programs providing for extended and increased unemployment insurance (UI) benefits. The *CAP* estimates allocations for these programs are based on projections of the number of UI recipients for 2009 made by the National Employment Law Project. Because it is possible that these projections could reflect predictive information that I have not controlled for in my regressions, I exclude DOL spending from the measures of ARRA spending included in my analyses and therefore I also exclude *CAP*'s DOL allocations from the *CAP* total-spending instrument used in the analyses.

<u>Center for American Progress Estimates of State Allocations of ARRA Spending</u>

The *CAP* estimates of the state allocations of the 10-year costs of the ARRA, separated by program, were obtained from the *CAP* website at: http://www.americanprogress.org/issues/2009/02/av/recovery_compromise.xls. They provided estimates for each ARRA program costing more than $1 billion and based on funding formulas that were known at the time *CAP* made its estimates (February 13, 2009). The methodology and data sources used by the *CAP* to generate their estimates are described at: http://www.americanprogress.org/issues/2009/02/compromise_map.html/#methodology. The *CAP*'s list of sources, by program, are reproduced below (in *italics*) from this webpage. As one can see, the allocations for most programs are obtained directly from the federal agencies/departments in charge of the major ARRA programs. In other cases, the *CAP* estimates the allocations based on either past (pre-ARRA) allocations (for programs for which the allocation formula did not change) or data on the program's formulary factors combined with knowledge of the formula itself.

CAP's data sources are as follows:

$5.0 billion for the Weatherization Assistance Program. Source: Department of Energy.

$3.1 billion for the State Energy Program. Source: Department of Energy.

$3.2 billion for the Energy Efficiency and Conservation Block Grants. The allocation of $2.8 billion of this money was distributed by population. Sources: U.S. Census Bureau, Energy Information Administration.

$27.1 billion for highway infrastructure investment. Source: Federal Highway Administration.

$8.4 billion for mass transit. Source: Federal Transit Administration.

$4.0 billion for the Clean Water State Revolving Fund. We assumed that allocations would be in line with FY2007 Final Title VI Allotments, including some funding for the territories.

$2.0 billion for the Drinking Water State Revolving Fund. We assumed that allocations would be in line with Tentative Distribution of Fund Appropriations for FY2008, including some funding for the territories.

$13.0 billion for Title I grants. The ESEA Title I Grants to Local Educational Agencies funding formula is set out here.

$12.2 billion for IDEA, Part B state grants. The Special Education Grants to States funding formula is set out here.

$2 billion for Child Care Development Block Grant. Source: Center for Law and Social Policy.

$2.1 billion for Head Start. Source: Appropriations Committee.

$15.6 billion for Pell Grants. The Federal Pell Grants funding formula is set out here.

$4.0 billion for Workforce Investment Act employment services. Proportions were taken from the House Appropriations Committee for the $2.95 billion that will be distributed to states.

$26.9 billion for unemployment insurance benefits extensions. We are grateful to the National Employment Law Project for their help with these calculations.
[Excluded from the total-spending instrument due to endogeneity concerns – see above.]

$8.8 billion for unemployment insurance increased benefits. We used the CBO assumption that less than $9 billion would be spent including some for the territories. We used NELP data to estimate how this would be split among states.
[Excluded from the total-spending instrument due to endogeneity concerns – see above.]

$1.1 billion for temporary assistance for states with advances. We are grateful to the National Employment Law Project for their help with these calculations.

$3.0 billion for the Unemployment Insurance Modernization Act. Proportions were in line by research from NELP. Source: Center for American Progress Action Fund, Half in Ten, and National Employment Law Project.

$2.0 billion for the Neighborhood Stabilization Program. We assumed that the allocations would be in line with current state and local NSP allocations including some funding for the territories.

$2.3 billion for the HOME Program. The same funding formula is used as in FY2008, including some funding for the territories.

$4 billion for Public Housing Capital Funds. We assumed that the allocation of $3.0 billion to states would be in line with FY2008 grants, including some funding for the territories.

$1.5 billion for Emergency Shelter Grants. The same funding formula will be used as in FY2008, including some funding for the territories.

$1 billion for the Community Development Block Grant. The same funding formula is used as in FY2008, including some funding for the territories.

$19 billion for Supplemental Nutrition Assistance Program. Source: Center on Budget and Policy Priorities.

$1 billion for child support enforcement. The allocated funds total more than $1 billion as some states will not get the full allocation over time. Source: Center for Law and Social Policy.

$14.3 billion for seniors, disabled veterans, and SSI. We used the funding formula set out by the Senate Finance Committee. Sources: U.S. Social Security Administration, U.S. Railroad Retirement Board, U.S. Department of Veterans Affairs.

$1.0 billion for Community Services Block Grant. Source: Appropriations Committee.

$53.6 billion for the State Fiscal Stabilization Fund. $62.7 billion will be distributed through the states using the funding formula set out in the 2008 Recovery and Reinvestment Act. Source: U.S. Census Bureau

$86.6 billion for Medicaid Federal Medical Assistance Percentages. This will be distributed through the funding formula set out in the act. We made estimations for 2009 and multiplied by 2.25 for the recession window. Sources: Congressional Budget Office, Statehealthfacts.org, Bureau of Labor Statistics.

$2.0 billion for Byrne Justice Assistance Grants. We assumed that allocations would be in line with the 2008 JAG Allocation, including some funding for the territories.

$116.2 billion for Make Work Pay. We are grateful to the Institute on Taxation and Economic Policy for their help with these calculations.

$4.6 billion for Earned Income Tax Credit increase. We are grateful to the Institute on Taxation and Economic Policy for their help with these calculations.

$14.8 billion for the Child Tax Credit. We are grateful to the Institute on Taxation and Economic Policy for their help with these calculations.

$5.4 billion for financial assistance for national recovery zones. We used the funding formula set out in the Senate bill. Source: Bureau of Labor Statistics.

$69.8 billion for the Alternative Minimum Tax. We are grateful to the Institute on Taxation and Economic Policy for their help with these calculations.

Wall Street Journal Estimates of State Allocations of ARRA Spending

The WSJ's estimates were obtained online at: http://online.wsj.com/public/resources/documents/info-STIMULUS0903.html. Under these estimates, the WSJ listed its data sources as follows: Department of Transportation, Department of Education, Department of Housing and Urban Development, Department of Health and Human Services, Department of Labor, Environmental Protection Agency, Department of Energy, Department of Defense, National Endowment for the Arts, Department of Veterans Affairs, U.S. Census Bureau, CIA World Factbook.

Table 1
Agency Totals (Bill.) and Percentages

	Announcements		Obligations		Payments	
ED	89.1	(32.4)	85.6	(27.7)	51.8	(28.7)
DOT	34.7	(12.6)	35.2	(11.4)	12.4	(6.8)
Other	95.8	(34.8)	90.7	(29.3)	45.2	(25.0)
HHS	55.6	(20.2)	97.8	(31.6)	71.0	(39.4)
Sum	275.3	(100.0)	309.3	(100.0)	180.4	(100.0)

Table 2

Summary Statistics, Sample Period: Feb 09-Jun 10

Panel A: Dependent Variables

	Mean	SD	Min	Max	N
Change in Employment (p.c.), Total Nonfarm	-0.0081	0.0055	-0.0243	0.0079	50
Change in Employment (p.c.), Private Employment	-0.0093	0.0047	-0.0241	0.0025	50
Change in Employment (p.c.), S&L Government	-0.0001	0.0019	-0.0032	0.0067	45
Change in Employment (p.c.), Construction	0.0000	0.0034	-0.0113	0.0108	44
Change in the Unemployment Rate	0.0104	0.0092	-0.0090	0.0410	50

Panel B: Explanatory Variables

	Mean	SD	Min	Max	N
Dec07-Feb09 Employment (p.c.) trend, Total Nonfarm	-0.0225	0.0105	-0.0554	-0.0012	50
Dec07-Feb09 Employment (p.c.) trend, S&L Government	-0.0003	0.0008	-0.0025	0.0015	45
Dec07-Feb09 Employment (p.c.) trend, Total Private	-0.0221	0.0101	-0.0539	-0.0007	50
Dec07-Feb09 Employment (p.c.) trend, Construction	-0.0039	0.0030	-0.0138	0.0000	47
Dec07-Feb09 Employment (p.c.) trend, Unemployment	0.0312	0.0111	0.0110	0.0540	50
Feb09 Employment (p.c.) Level, Total Nonfarm	0.4474	0.0414	0.3767	0.5661	50
Feb09 Employment (p.c.) Level, S&L Government	0.0704	0.0126	0.0498	0.1167	45
Feb09 Employment (p.c.) Level, Total Private	0.3676	0.0362	0.2922	0.4479	50
Feb09 Employment (p.c.) Level, Construction	0.0224	0.0057	0.0139	0.0461	47
Feb09 Employment (p.c.) Level, Unemployment Rate	0.0758	0.0181	0.0420	0.1200	50
ΔGSP_{06-07}	0.0094	0.0175	-0.0225	0.0675	50
Announcements (p.c.)	1,043.8	299.8	740.6	2,451.4	50
Obligations (p.c.)	1,286.6	259.2	912.4	2,445.6	50
Payments (p.c.)	788.5	138.9	481.7	1,081.3	50
Tax Benefits (p.c.)	567.1	110.1	435.8	923.7	50

Panel C: Instruments

	Mean	SD	Min	Max	N
American Progress Estimates (p.c., less DOL)	1,606.2	171.4	1,292.3	2,073.5	50
Wall Street Journal Estimates (p.c.)	674.8	168.0	482.7	1,313.7	50
American Progress DOT Estimates (p.c.)	133.2	52.8	89.4	310.8	50
Wall Street Journal DOT Estimates (p.c.)	133.2	52.8	89.4	310.8	50
American Progress ED Estimates (p.c.)	297.8	26.4	241.7	372.7	50
Wall Street Journal ED Estimates (p.c.)	253.8	17.8	215.8	295.3	50
American Progress HHS Estimates (p.c.)	264.8	94.0	129.8	665.7	50
Wall Street Journal HHS Estimates (p.c.)	83.7	28.3	45.0	182.7	50
American Progress Other Agency Estimates (p.c.)	910.4	76.7	754.0	1,120.2	50
Wall Street Journal Other Agency Estimates (p.c.)	204.1	118.0	89.3	614.4	50
American Progress DOL Estimates (p.c.)	121.9	58.9	31.9	275.8	50

Table 3

Change in Employment:Population Ratio, Feb 09-Jun 10

Total Nonfarm

	OLS β/SE	IV/GMM β/SE	OLS β/SE	IV/GMM β/SE	OLS β/SE	IV/GMM β/SE
Announcements (Mill. Per Cap)	**4.568**	2.774	-	-	-	-
	(2.044)	(2.965)				
Obligations (Mill. Per Cap)	-	-	2.668	0.703	-	-
			(3.260)	(4.754)		
Payments (Mill. Per Cap)	-	-	-	-	-3.295	-5.317
					(6.871)	(11.015)
ΔGSP_{06-07}	0.033	0.035	0.047	0.054	0.053	0.071
	(0.053)	(0.052)	(0.054)	(0.051)	(0.059)	(0.058)
Tax Benefits (Mill. per cap)	0.326	-2.375	-4.863	-4.888	-5.575	-4.412
	(5.957)	(5.742)	(5.753)	(5.121)	(5.709)	(5.169)
Dec07-Feb09 trend	**0.211**	**0.227**	**0.233**	**0.232**	**0.273**	**0.246**
	(0.082)	**(0.083)**	**(0.097)**	**(0.094)**	**(0.094)**	**(0.091)**
Feb09 level	-0.023	-0.015	-0.020	-0.014	-0.018	-0.018
	(0.026)	(0.023)	(0.025)	(0.023)	(0.026)	(0.024)
Constant	0.003	0.002	0.005	0.005	0.011	0.010
	(0.011)	(0.011)	(0.012)	(0.012)	(0.012)	(0.014)
N	50	50	50	50	50	50
R^2	0.312	0.303	0.275	0.266	0.267	0.260
Robust First-Stage F		24.779		27.415		14.613

Table 4

Change in Employment:Population Ratio, Feb 09-Jun 10

Private Nonfarm

	OLS β/SE	IV/GMM β/SE	OLS β/SE	IV/GMM β/SE	OLS β/SE	IV/GMM β/SE
Announcements (Mill. Per Cap)	0.967	-0.162	-	-	-	-
	(1.960)	(2.570)				
Obligations (Mill. Per Cap)	-	-	-0.337	-3.142	-	-
			(2.555)	(4.271)		
Payments (Mill. Per Cap)	-	-	-	-	-5.031	-11.502
					(5.199)	(10.293)
ΔGSP_{06-07}	0.039	0.037	0.042	0.046	0.052	0.069
	(0.048)	(0.046)	(0.046)	(0.044)	(0.046)	(0.052)
Tax Benefits (Mill. per cap)	0.701	-1.252	-0.488	-0.843	-0.435	-0.088
	(4.811)	(4.514)	(4.689)	(4.263)	(4.660)	(4.281)
Dec07-Feb09 trend	**0.202**	**0.221**	**0.221**	**0.247**	**0.233**	**0.247**
	(0.084)	**(0.083)**	**(0.092)**	**(0.088)**	**(0.085)**	**(0.083)**
Feb09 level	-0.020	-0.015	-0.021	-0.019	-0.021	-0.023
	(0.024)	(0.022)	(0.024)	(0.022)	(0.024)	(0.022)
Constant	0.001	0.002	0.003	0.007	0.006	0.011
	(0.010)	(0.009)	(0.009)	(0.010)	(0.009)	(0.011)
N	50	50	50	50	50	50
R^2	0.257	0.252	0.255	0.235	0.266	0.246
Robust First-Stage F		27.279		26.156		13.984

Table 5

Change in Employment:Population Ratio, Feb 09-Jun 10

State and Local Government

	OLS β/SE	IV/GMM β/SE	OLS β/SE	IV/GMM β/SE	OLS β/SE	IV/GMM β/SE
Announcements (Mill. Per Cap)	1.642	1.232	-	-	-	-
	(1.097)	(0.946)				
Obligations (Mill. Per Cap)	-	-	**1.225**	0.929	-	-
			(0.722)	(0.817)		
Payments (Mill. Per Cap)	-	-	-	-	1.332	1.918
					(2.026)	(2.013)
ΔGSP_{06-07}	-0.003	-0.001	0.002	0.000	-0.000	-0.004
	(0.012)	(0.011)	(0.013)	(0.012)	(0.014)	(0.013)
Tax Benefits (Mill. per cap)	-0.331	-0.621	-1.623	-1.422	-1.562	-1.352
	(1.693)	(1.594)	(1.715)	(1.512)	(1.647)	(1.481)
Dec07-Feb09 trend	0.188	0.164	0.103	0.097	0.123	0.138
	(0.263)	(0.250)	(0.273)	(0.251)	(0.269)	(0.259)
Feb09 level	0.038	**0.044**	**0.053**	**0.055**	**0.061**	**0.059**
	(0.023)	**(0.021)**	**(0.019)**	**(0.018)**	**(0.021)**	**(0.019)**
Constant	**-0.004**	**-0.004**	**-0.004**	**-0.004**	**-0.004**	**-0.005**
	(0.002)	**(0.002)**	**(0.002)**	**(0.002)**	**(0.002)**	**(0.002)**
N	45	45	45	45	45	45
R^2	0.277	0.274	0.252	0.250	0.232	0.229
Robust First-Stage F		15.824		24.396		19.706

Table 6

Change in Employment:Population Ratio, Feb 09-Jun 10

Construction

	OLS β/SE	IV/GMM β/SE	OLS β/SE	IV/GMM β/SE	OLS β/SE	IV/GMM β/SE
Announcements (Mill. Per Cap)	**5.232**	**5.156**	-	-	-	-
	(0.747)	**(0.881)**				
Obligations (Mill. Per Cap)	-	-	**5.831**	**6.320**	-	-
			(1.059)	**(1.327)**		
Payments (Mill. Per Cap)	-	-	-	-	**10.211**	**10.327**
					(4.200)	**(5.001)**
ΔGSP_{06-07}	0.038	0.039	**0.057**	**0.055**	0.035	0.042
	(0.026)	(0.024)	**(0.026)**	**(0.024)**	(0.036)	(0.032)
Tax Benefits (Mill. per cap)	3.691	**3.595**	-1.015	0.740	-1.064	0.383
	(2.535)	**(2.130)**	(2.603)	(2.081)	(2.567)	(2.212)
Dec07-Feb09 trend	**0.508**	**0.509**	**0.485**	**0.468**	**0.615**	**0.542**
	(0.129)	**(0.118)**	**(0.154)**	**(0.136)**	**(0.178)**	**(0.151)**
Feb09 level	-0.062	-0.061	-0.023	-0.007	0.048	0.049
	(0.069)	(0.063)	(0.070)	(0.059)	(0.075)	(0.069)
Constant	-0.003	**-0.003**	-0.004	**-0.006**	-0.005	-0.006
	(0.002)	**(0.002)**	(0.003)	**(0.002)**	(0.003)	(0.004)
N	44	44	44	44	44	44
R^2	0.704	0.704	0.676	0.672	0.591	0.583
Robust First-Stage F		20.055		31.543		17.525

Table 7

Change in Employment:Population Ratio, Feb 09-Jun 10

Manufacturing

	OLS β/SE	IV/GMM β/SE	OLS β/SE	IV/GMM β/SE	OLS β/SE	IV/GMM β/SE
Announcements (Mill. Per Cap)	**1.623**	**1.494**	-	-	-	-
	(0.397)	**(0.481)**				
Obligations (Mill. Per Cap)	-	-	**1.662**	**1.734**	-	-
			(0.546)	**(0.623)**		
Payments (Mill. Per Cap)	-	-	-	-	2.369	3.073
					(1.818)	(2.004)
ΔGSP_{06-07}	-0.008	-0.007	-0.003	-0.004	-0.004	-0.005
	(0.007)	(0.007)	(0.007)	(0.007)	(0.010)	(0.010)
Tax Benefits (Mill. per cap)	1.421	1.252	-0.317	-0.338	-0.697	-0.718
	(1.276)	(1.229)	(1.113)	(1.051)	(1.104)	(1.060)
Dec07-Feb09 trend	-0.137	-0.125	-0.105	-0.112	-0.066	-0.080
	(0.090)	(0.083)	(0.090)	(0.083)	(0.098)	(0.088)
Feb09 level	**-0.064**	**-0.063**	**-0.065**	**-0.064**	**-0.067**	**-0.063**
	(0.015)	**(0.014)**	**(0.016)**	**(0.015)**	**(0.015)**	**(0.014)**
Constant	**-0.003**	**-0.002**	**-0.002**	**-0.002**	-0.001	-0.002
	(0.001)	**(0.001)**	**(0.001)**	**(0.001)**	(0.001)	(0.001)
N	47	47	47	47	47	47
R^2	0.590	0.590	0.575	0.574	0.521	0.514
Robust First-Stage F		17.228		26.403		20.165

Table 8

Change in Employment:Population Ratio, Feb 09-Jun 10

Education and Health

	OLS β/SE	IV/GMM β/SE	OLS β/SE	IV/GMM β/SE	OLS β/SE	IV/GMM β/SE
Announcements (Mill. Per Cap)	-0.032	-0.247	-	-	-	-
	(0.511)	(0.610)				
Obligations (Mill. Per Cap)	-	-	-0.005	-0.542	-	-
			(0.553)	(0.906)		
Payments (Mill. Per Cap)	-	-	-	-	-1.516	-2.148
					(1.552)	(2.934)
ΔGSP_{06-07}	0.006	0.006	0.006	0.005	0.008	0.008
	(0.009)	(0.009)	(0.009)	(0.008)	(0.009)	(0.010)
Tax Benefits (Mill. per cap)	-1.853	**-2.343**	-1.808	**-2.317**	**-2.282**	**-2.538**
	(1.349)	**(1.381)**	(1.225)	**(1.192)**	**(1.178)**	**(1.313)**
Dec07-Feb09 trend	-0.087	-0.048	-0.091	-0.071	-0.079	-0.076
	(0.243)	(0.233)	(0.252)	(0.230)	(0.246)	(0.227)
Feb09 level	0.008	0.008	0.008	0.012	0.014	0.018
	(0.015)	(0.014)	(0.016)	(0.016)	(0.015)	(0.019)
Constant	**0.002**	**0.002**	**0.002**	**0.003**	**0.003**	**0.003**
	(0.001)	**(0.001)**	**(0.001)**	**(0.001)**	**(0.001)**	**(0.001)**
N	49	49	49	49	49	49
R^2	0.047	0.042	0.047	0.030	0.068	0.065
Robust First-Stage F		14.825		14.165		13.089

Table 9

Change in Unemployment, Feb 09-Jun 10

	OLS β/SE	IV/GMM β/SE	OLS β/SE	IV/GMM β/SE	OLS β/SE	IV/GMM β/SE
Announcements (Mill. Per Cap)	-7.772	-4.439	-	-	-	-
	(4.954)	(4.973)				
Obligations (Mill. Per Cap)	-	-	-7.062	-8.167	-	-
			(5.690)	(6.791)		
Payments (Mill. Per Cap)	-	-	-	-	-11.619	-25.344
					(13.387)	(20.519)
ΔGSP_{06-07}	-0.104	**-0.121**	-0.117	**-0.117**	-0.095	-0.053
	(0.087)	**(0.058)**	(0.087)	**(0.057)**	(0.096)	(0.088)
Tax Benefits (Mill. per cap)	-9.323	-4.142	-2.286	-1.736	-1.941	-2.000
	(12.886)	(10.025)	(12.126)	(9.112)	(12.230)	(9.435)
Dec07-Feb09 trend	-0.246	-0.222	-0.235	-0.282	-0.179	-0.267
	(0.259)	(0.223)	(0.267)	(0.223)	(0.262)	(0.221)
Feb09 level	0.108	0.096	0.137	0.151	0.132	0.189
	(0.153)	(0.116)	(0.160)	(0.130)	(0.165)	(0.148)
Constant	**0.023**	**0.017**	0.018	**0.019**	0.015	**0.022**
	(0.012)	**(0.010)**	(0.011)	**(0.010)**	(0.011)	**(0.012)**
N	50	50	50	50	50	50
R^2	0.131	0.120	0.113	0.112	0.098	0.076
Robust First-Stage F		30.558		31.899		13.886

Table 10

IV/GMM Results, Weighted vs. Unweighted Regressions

Panel A: Unweighted

	Total Nonfarm β/SE/F	Private Nonfarm β/SE/F	S&L Govt β/SE/F	Construction β/SE/F	Manufacturing β/SE/F	Educ. & Health β/SE/F
Announcements	2.774	-0.162	1.232	**5.156**	**1.494**	-0.247
(Mill. Per Cap)	(2.965)	(2.570)	(0.946)	**(0.881)**	**(0.481)**	(0.610)
	24.779	*27.279*	*15.824*	*20.055*	*17.228*	*14.825*
Obligations	0.703	-3.142	0.929	**6.320**	**1.734**	-0.542
(Mill. Per Cap)	(4.754)	(4.271)	(0.817)	**(1.327)**	**(0.623)**	(0.906)
	27.415	*26.156*	*24.396*	*31.543*	*26.403*	*14.165*
Payments (Mill.	-5.317	-11.502	1.918	**10.327**	3.073	-2.148
Per Cap)	(11.015)	(10.293)	(2.013)	**(5.001)**	(2.004)	(2.934)
	14.613	*13.984*	*19.706*	*17.525*	*20.165*	*13.089*

Panel B: BLS Weights

	Total Nonfarm β/SE/F	Private Nonfarm β/SE/F	S&L Govt β/SE/F	Construction β/SE/F	Manufacturing β/SE/F	Educ. & Health β/SE/F
Announcements	3.288	0.344	**1.634**	**5.066**	**1.580**	-0.186
(Mill. Per Cap)	(2.742)	(2.458)	**(0.953)**	**(0.779)**	**(0.460)**	(0.608)
	23.147	*26.618*	*14.320*	*19.161*	*16.081*	*15.986*
Obligations	1.512	-3.084	1.249	**6.127**	**1.978**	-0.497
(Mill. Per Cap)	(4.772)	(4.524)	(0.794)	**(1.123)**	**(0.576)**	(0.943)
	26.823	*24.356*	*22.401*	*31.499*	*25.087*	*13.599*
Payments (Mill.	-5.178	-13.182	2.800	**11.541**	**3.508**	-2.270
Per Cap)	(11.445)	(10.787)	(2.065)	**(4.971)**	**(2.060)**	(3.369)
	13.625	*12.164*	*18.950*	*17.365*	*18.516*	*12.758*

Table 11

IV/GMM Results, Alternative Start Dates

Panel A: Feb 09

	Total Nonfarm	Private Nonfarm	S&L Govt	Construction	Manufacturing	Educ. & Health
	β/SE/F	β/SE/F	β/SE/F	β/SE/F	β/SE/F	β/SE/F
Announcements	2.774	-0.162	1.232	**5.156**	**1.494**	-0.247
(Mill. Per Cap)	(2.965)	(2.570)	(0.946)	**(0.881)**	**(0.481)**	(0.610)
	24.779	*27.279*	*15.824*	*20.055*	*17.228*	*14.825*
Obligations	0.703	-3.142	0.929	**6.320**	**1.734**	-0.542
(Mill. Per Cap)	(4.754)	(4.271)	(0.817)	**(1.327)**	**(0.623)**	(0.906)
	27.415	*26.156*	*24.396*	*31.543*	*26.403*	*14.165*
Payments (Mill.	-5.317	-11.502	1.918	**10.327**	3.073	-2.148
Per Cap)	(11.015)	(10.293)	(2.013)	**(5.001)**	(2.004)	(2.934)
	14.613	*13.984*	*19.706*	*17.525*	*20.165*	*13.089*

Panel B: Jan 09

	Total Nonfarm	Private Nonfarm	S&L Govt	Construction	Manufacturing	Educ. & Health
	β/SE/F	β/SE/F	β/SE/F	β/SE/F	β/SE/F	β/SE/F
Announcements	3.201	-0.263	**2.019**	**6.324**	**1.808**	-0.075
(Mill. Per Cap)	(3.410)	(3.378)	**(0.905)**	**(1.156)**	**(0.519)**	(0.606)
	18.615	*19.420*	*14.674*	*19.478*		
Obligations	2.608	-1.503	**1.680**	**7.247**	**2.100**	-0.532
(Mill. Per Cap)	(4.739)	(4.239)	**(0.722)**	**(1.597)**	**(0.759)**	(0.928)
	25.728	*25.815*	*17.736*	*24.805*		
Payments (Mill.	-0.106	-3.893	**3.803**	10.090	1.823	-2.251
Per Cap)	(9.689)	(7.997)	**(1.804)**	(6.288)	(2.013)	(2.162)
	28.470	*27.939*	*16.854*	*18.677*		

Table 12

Agency Categories

Feb 09-Jun 10

Panel A: Announcements

	Total Nonfarm	Private Nonfarm	S&L Govt	Construction	Manufacturing	Educ. & Health
	β/SE	β/SE	β/SE	β/SE	β/SE	β/SE
DOT + Other	8.224	0.708	**6.619**	**10.019**	**1.899**	-0.154
	(5.204)	(4.797)	**(1.777)**	**(1.691)**	**(0.692)**	(0.945)
ED	1.842	1.698	11.228	16.956	4.168	1.323
	(36.453)	(32.926)	(12.994)	(15.057)	(4.325)	(6.280)
HHS	-19.019	-0.924	**-9.260**	-3.670	0.899	0.487
	(14.184)	(12.755)	**(4.138)**	(5.241)	(2.001)	(2.802)
Donald-Cragg	5.738	6.346	3.225	9.110	5.518	3.573

Panel B: Obligations

	Total Nonfarm	Private Nonfarm	S&L Govt	Construction	Manufacturing	Educ. & Health
	β/SE	β/SE	β/SE	β/SE	β/SE	β/SE
DOT + Other	8.981	0.476	**5.859**	**15.280**	**2.836**	0.090
	(6.367)	(6.308)	**(2.096)**	**(1.292)**	**(0.784)**	(0.929)
ED	3.215	12.390	-2.644	12.293	2.758	4.504
	(25.229)	(23.772)	(9.870)	(11.646)	(4.431)	(5.293)
HHS	-14.308	-3.343	**-7.658**	**-10.770**	-0.078	-0.141
	(9.423)	(9.627)	**(2.676)**	**(2.440)**	(1.338)	(1.640)
Donald-Cragg	4.846	5.079	4.305	4.572	4.655	4.390

Table 13

BED Employment Measures

Mar 09-Dec 09

	Gross Job Gains	Gross Job Losses	Net Job Changes	Total Private (CES)
	β/SE/F	β/SE/F	β/SE/F	β/SE/F
Announcements	**5.720**	**8.740**	1.853	2.250
(Mill. Per Cap)	**(3.227)**	**(1.947)**	(2.626)	(2.412)
	25.235	*22.700*	*28.238*	*35.088*
Obligations	**8.944**	**12.597**	**6.948**	4.064
(Mill. Per Cap)	**(3.599)**	**(3.101)**	**(3.473)**	(4.652)
	51.922	*41.628*	*59.254*	*60.275*
Payments (Mill.	**24.199**	**24.893**	**21.600**	4.227
Per Cap)	**(11.668)**	**(10.448)**	**(9.103)**	(11.577)
	11.329	*13.248*	*22.075*	*14.128*

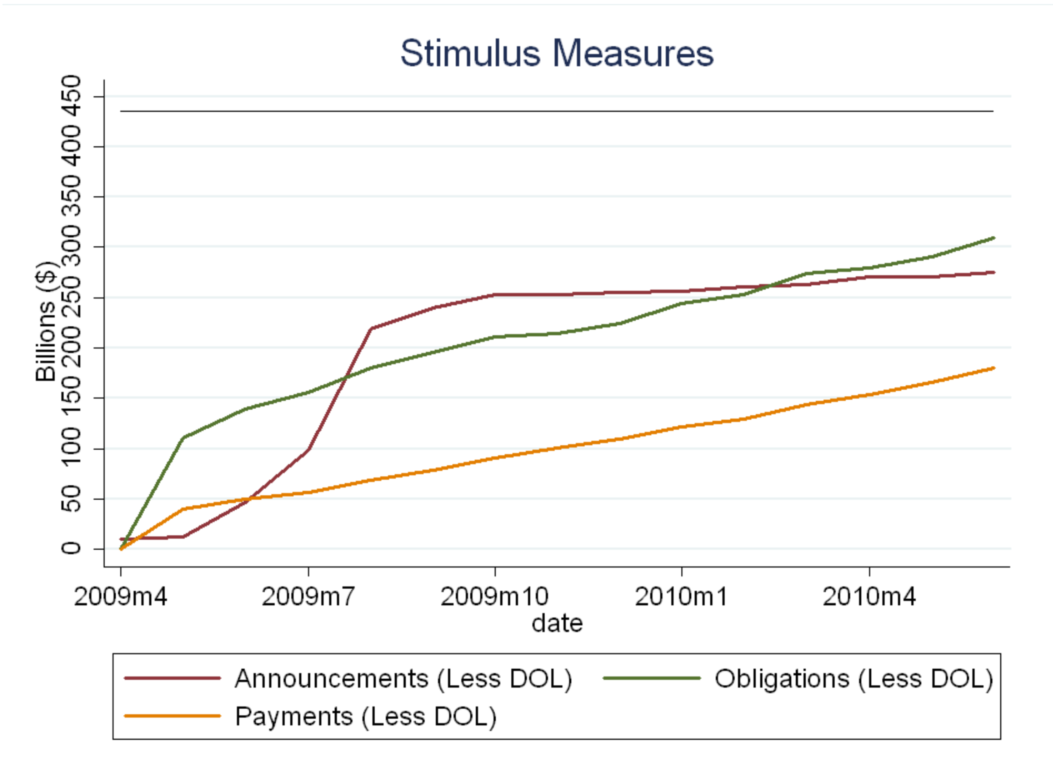

Figure 3

Stimulus Measures

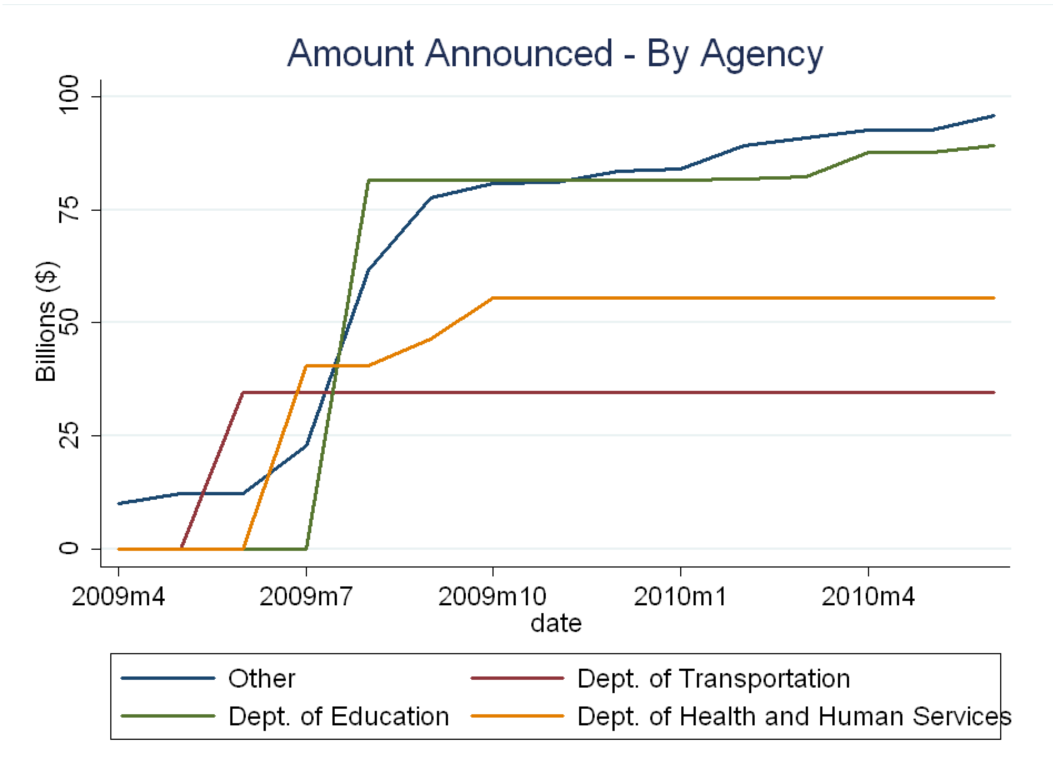

Figure 4

Amount Announced - By Agency

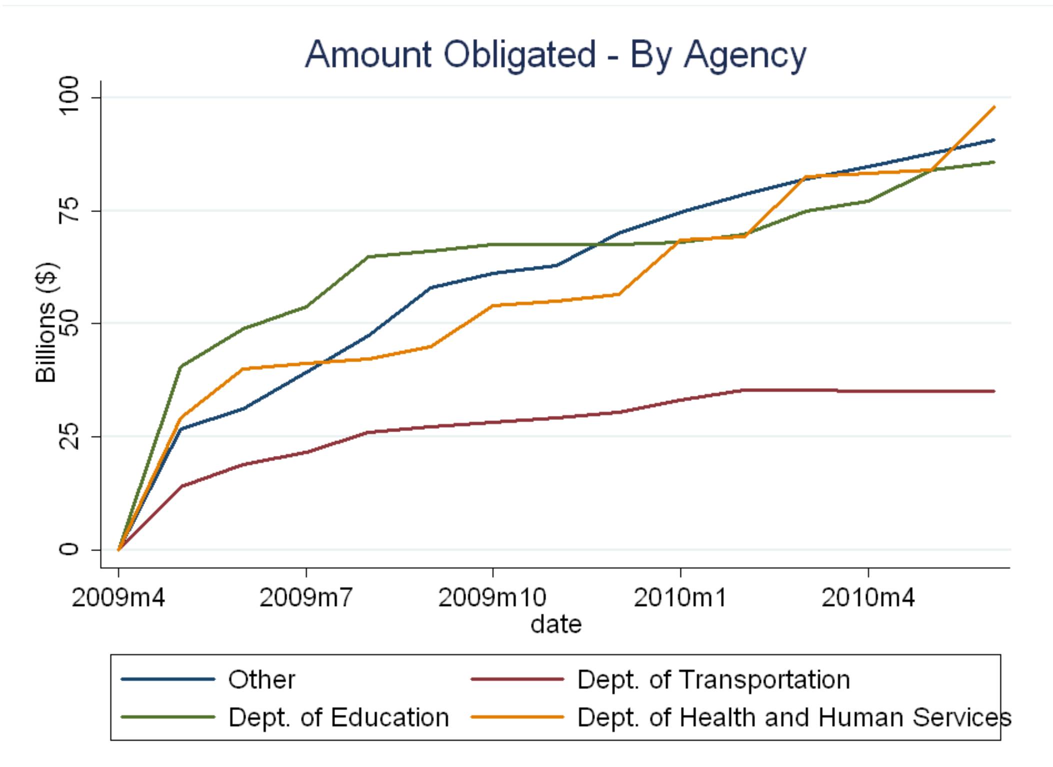

Figure 5
Amount Obligated - By Agency

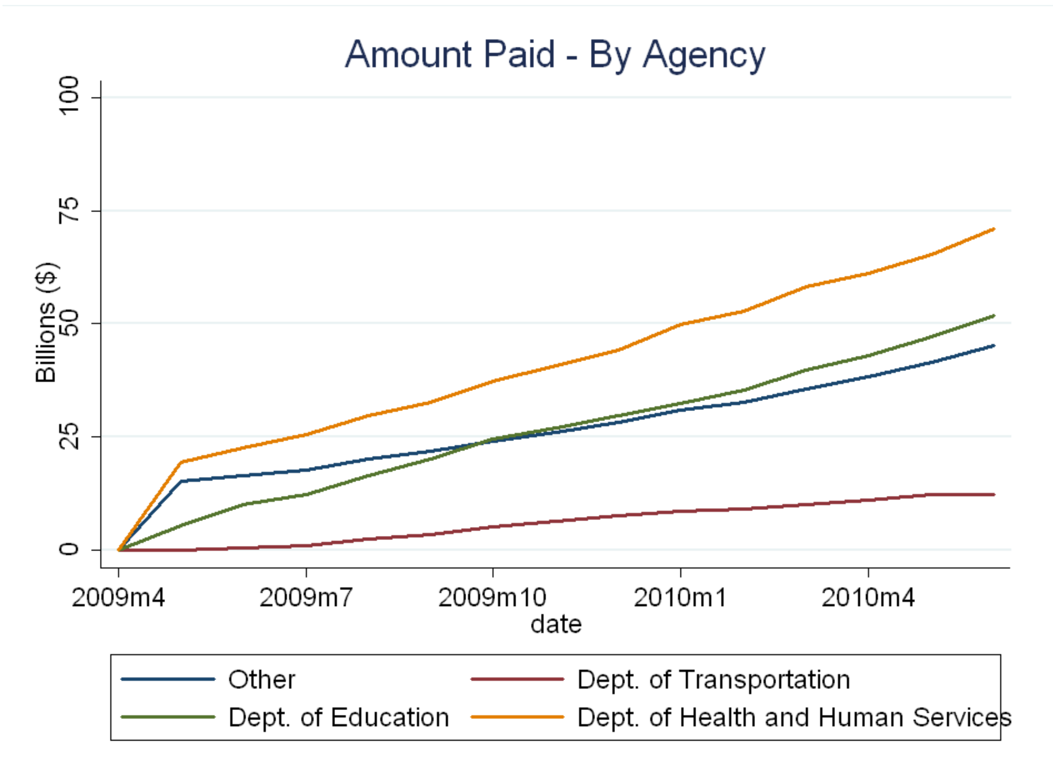

Figure 6

Amount Paid - By Agency

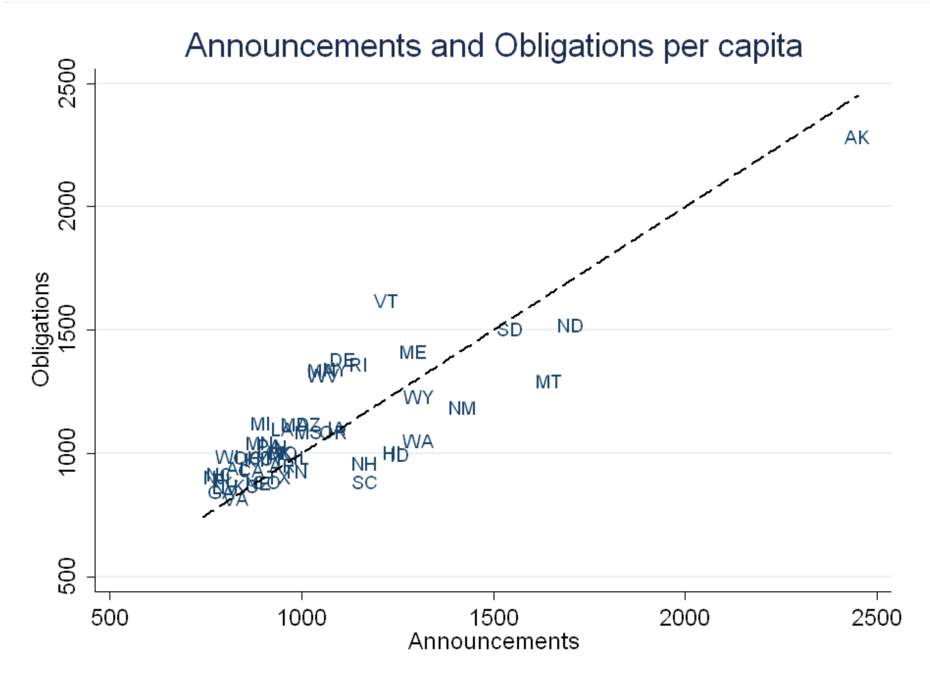

Figure 7
Announcements and Obligations per capita

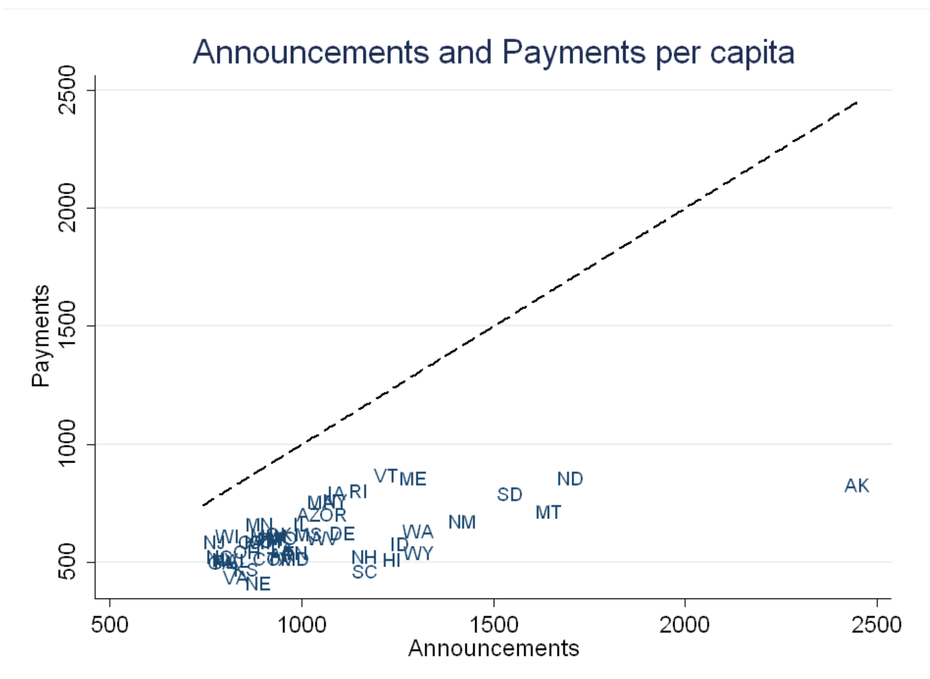

Figure 8
Announcements and Payments Per Capita

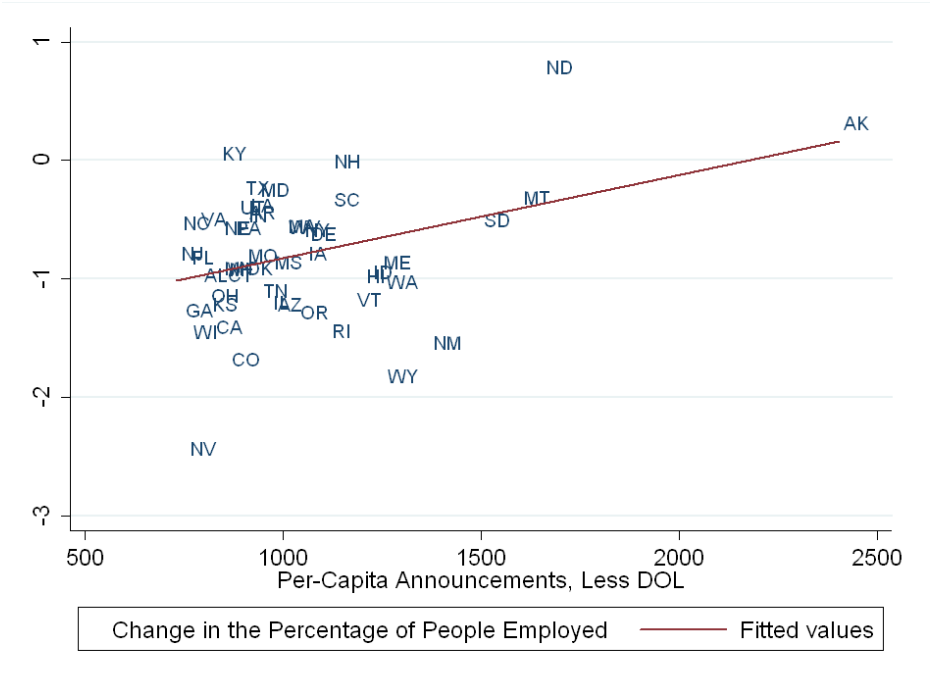

Figure 9
Change in Employment Rate v. Announcements

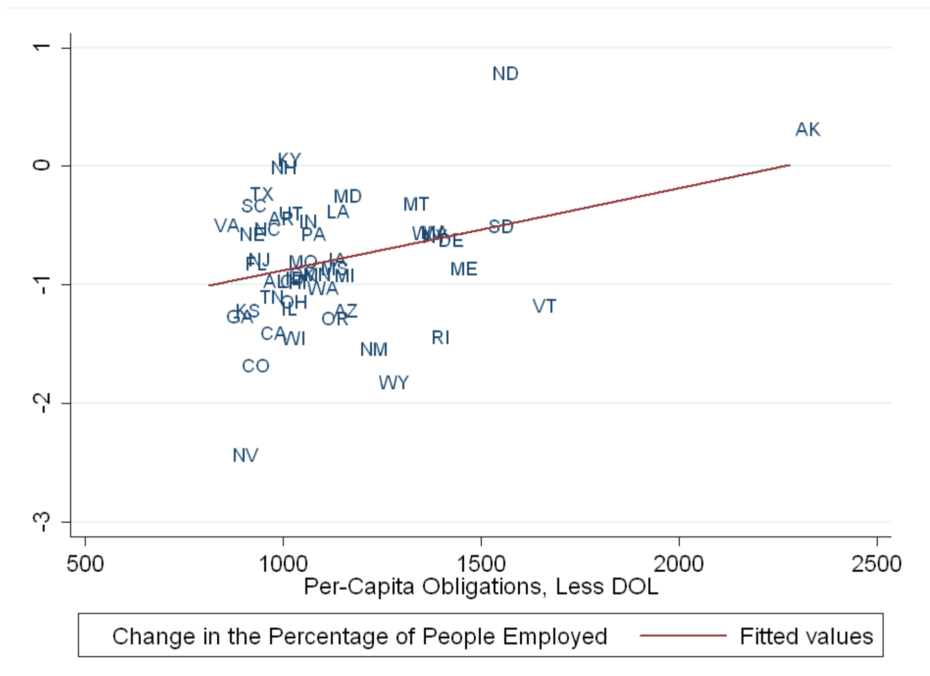

Figure 10
Change in Employment Rate v. Obligations

Figure 11

Change in Employment Rate v. Payments

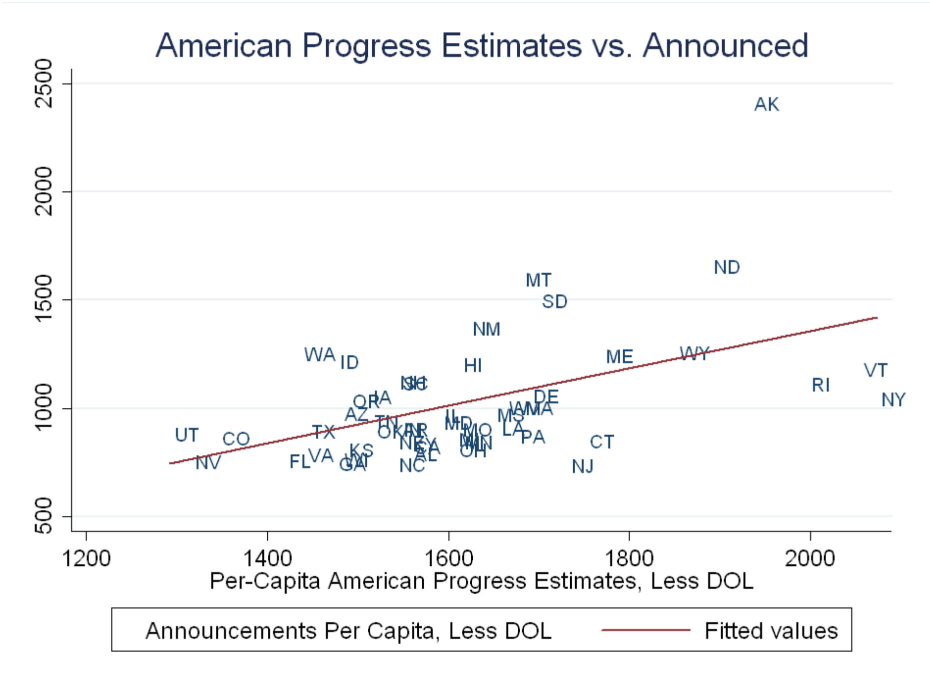

Figure 12
American Progress Estimates vs Announcements

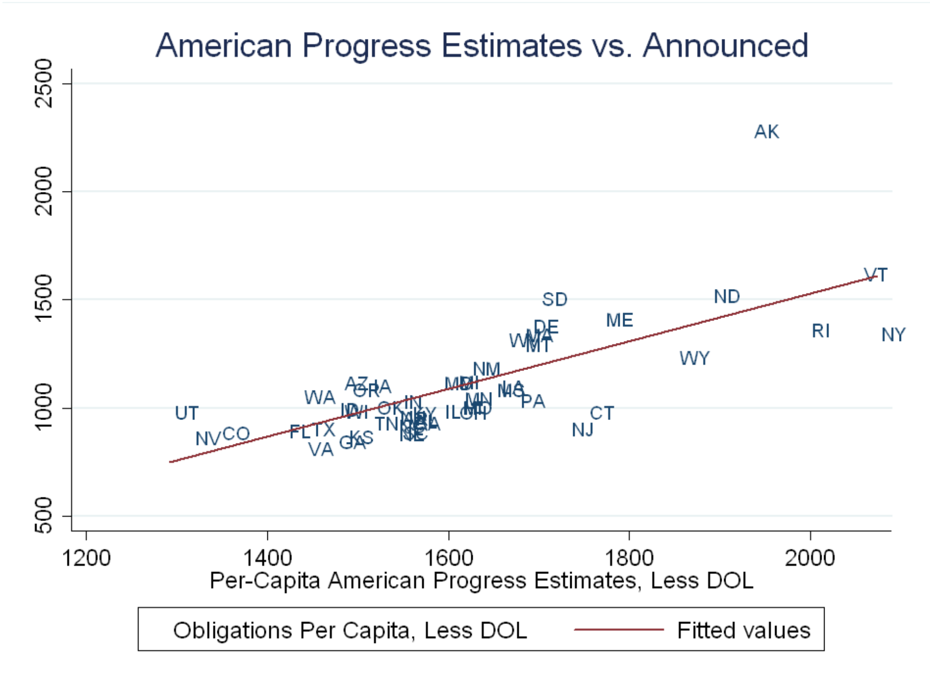

Figure 13
American Progress Estimates vs Obligations

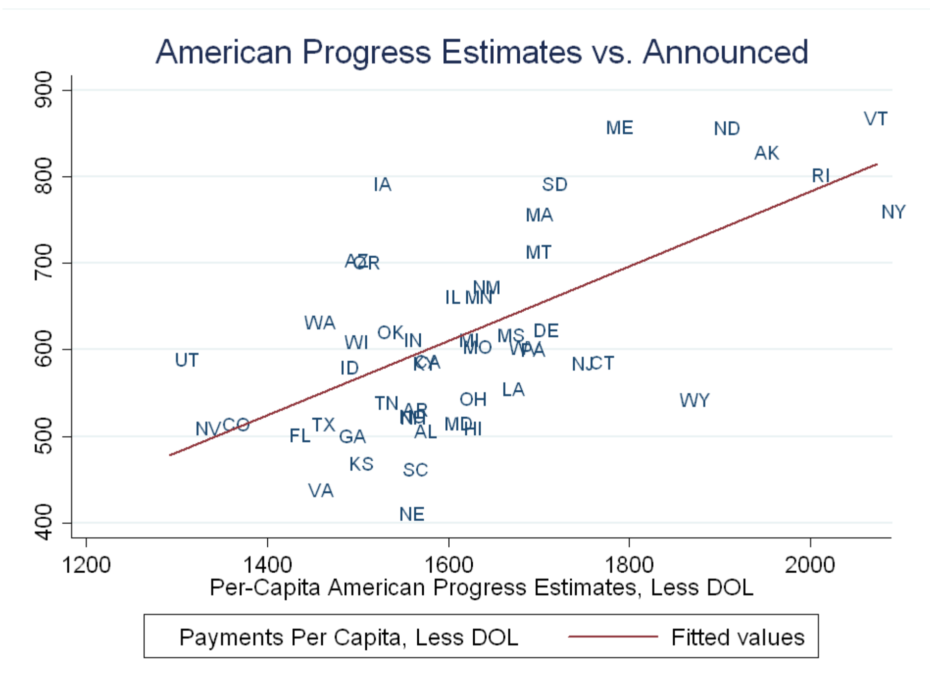

Figure 14
American Progress Estimates vs Payments

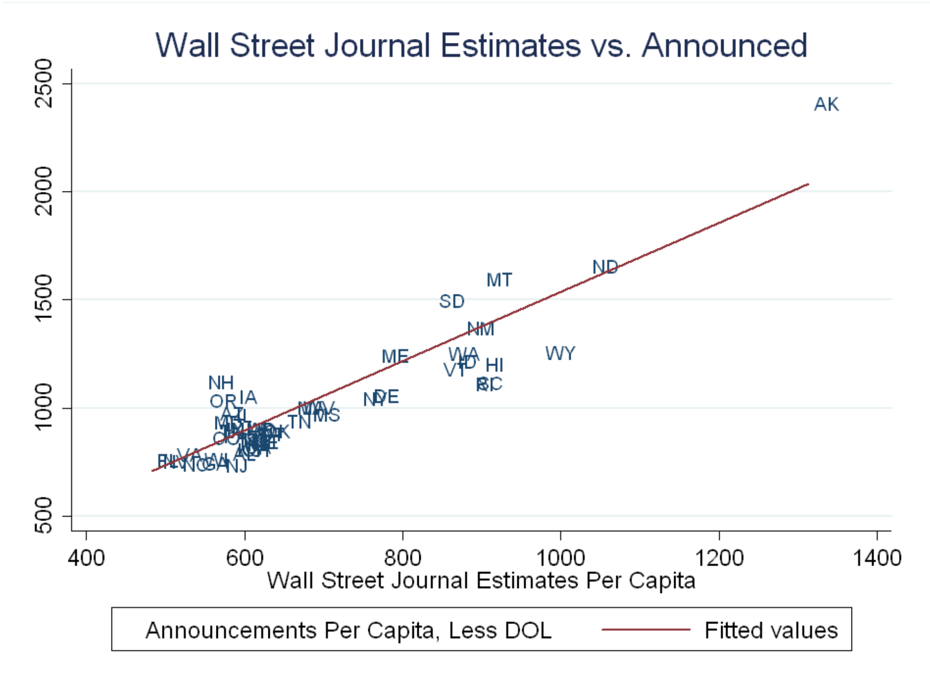

Figure 15
WSJ Estimates vs Announcements

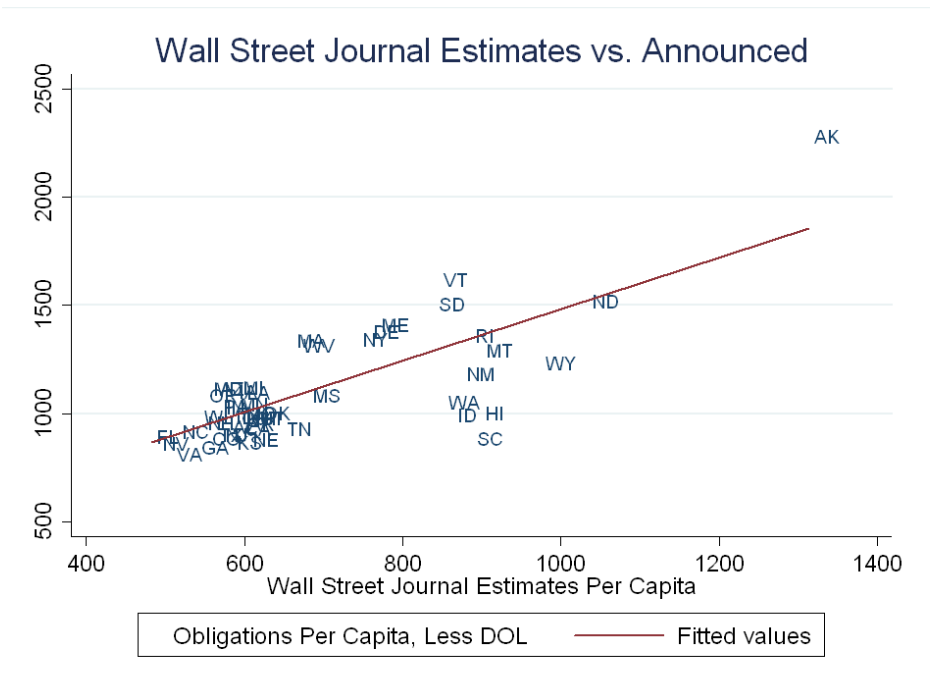

Figure 16
WSJ Estimates vs Obligations

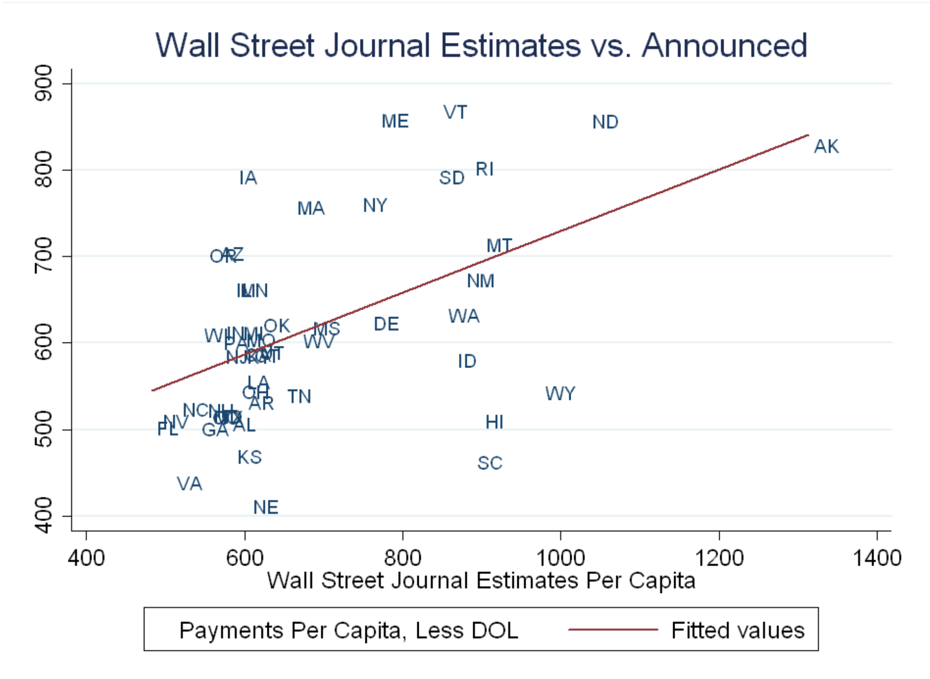

Figure 17
WSJ Estimates vs Payments

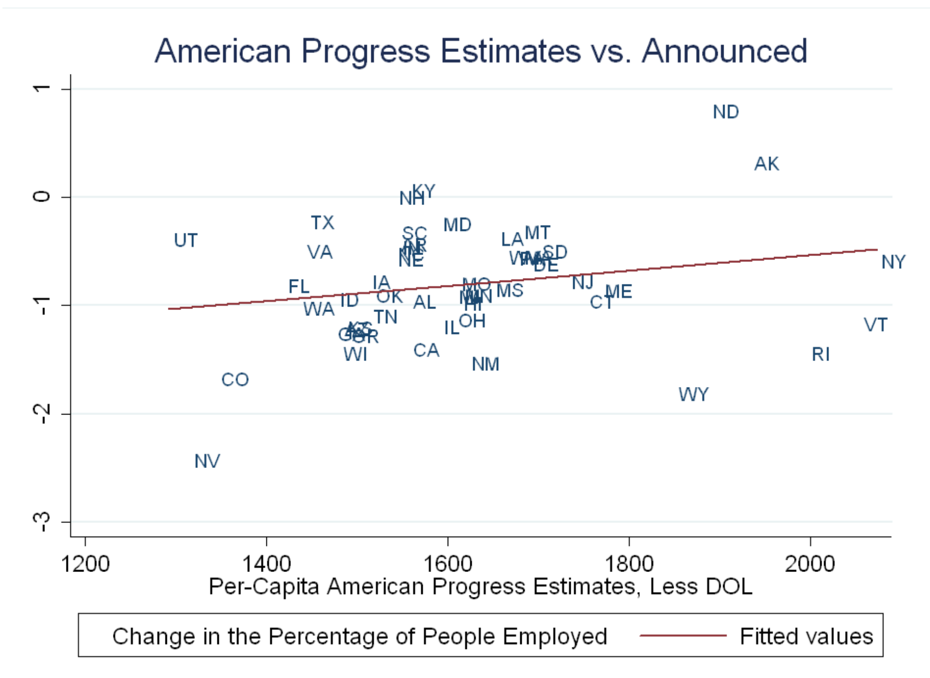

Figure 18
American Progress Estimates vs Change in Employment Rate

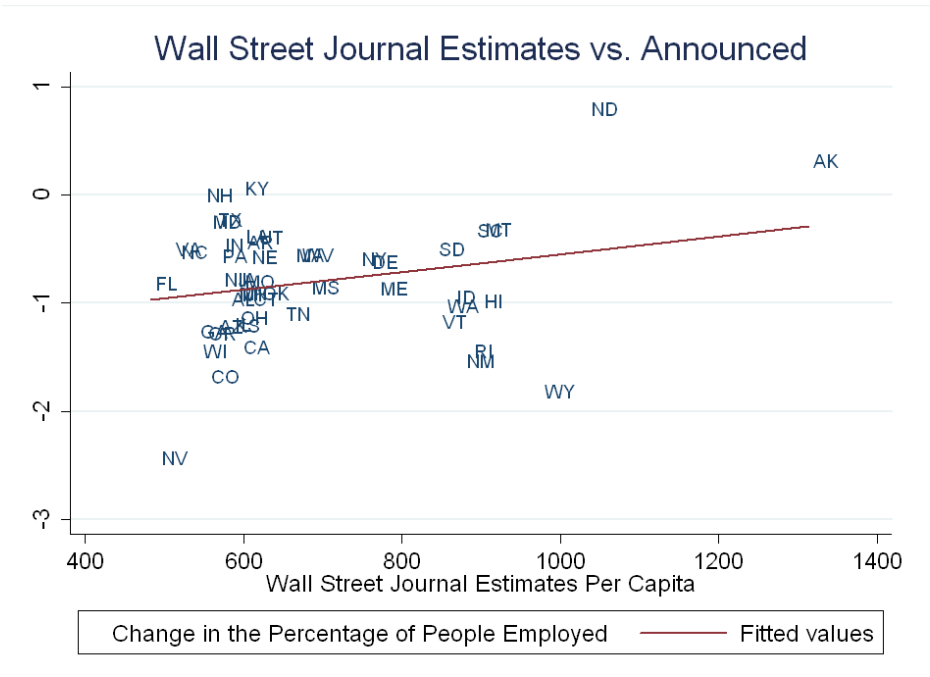

Figure 19

WSJ Estimates vs Change in Employment Rate

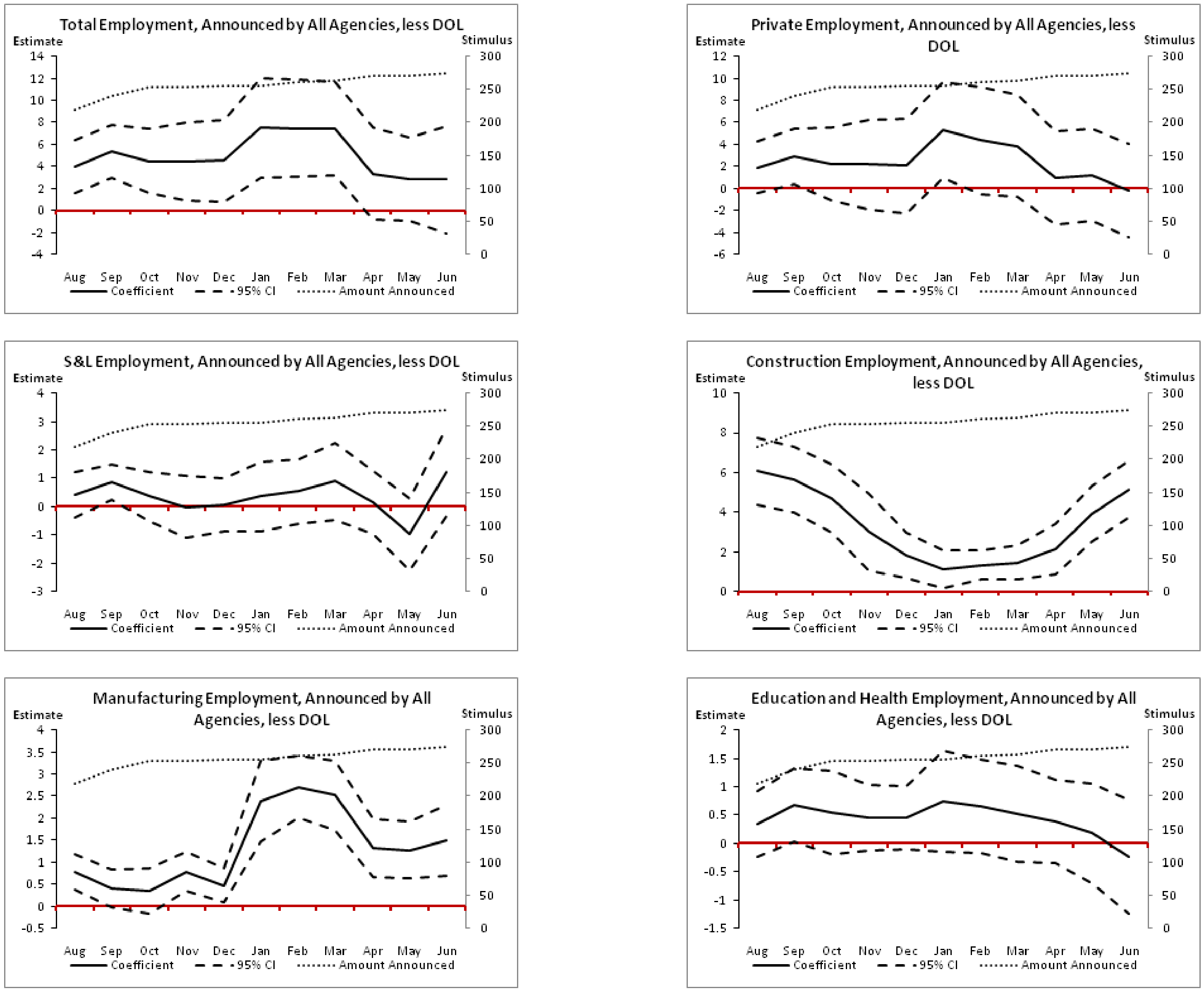

Figure 20

Coefficients over time, All Agencies

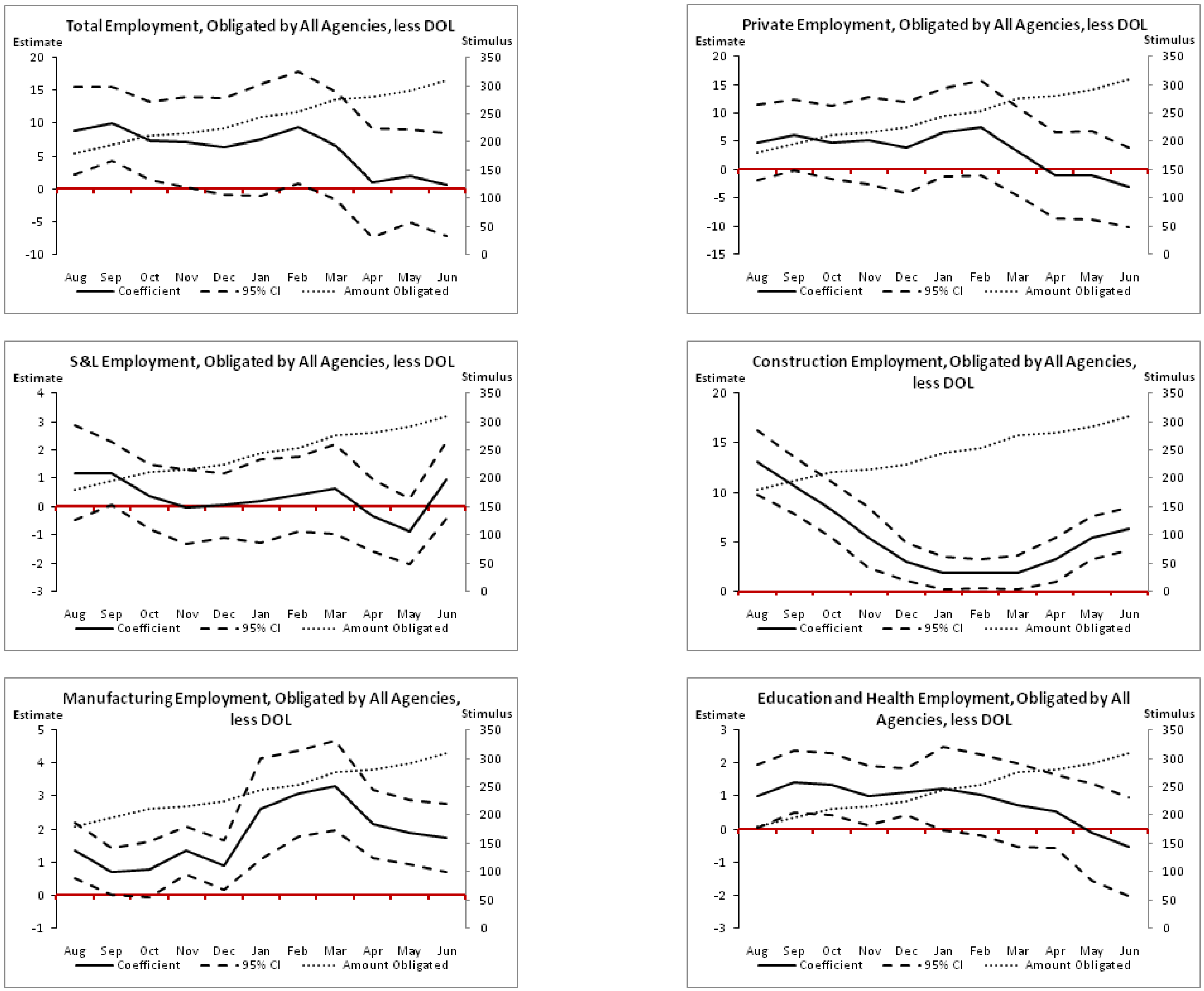

Figure 21

Coefficients over time, All Agencies

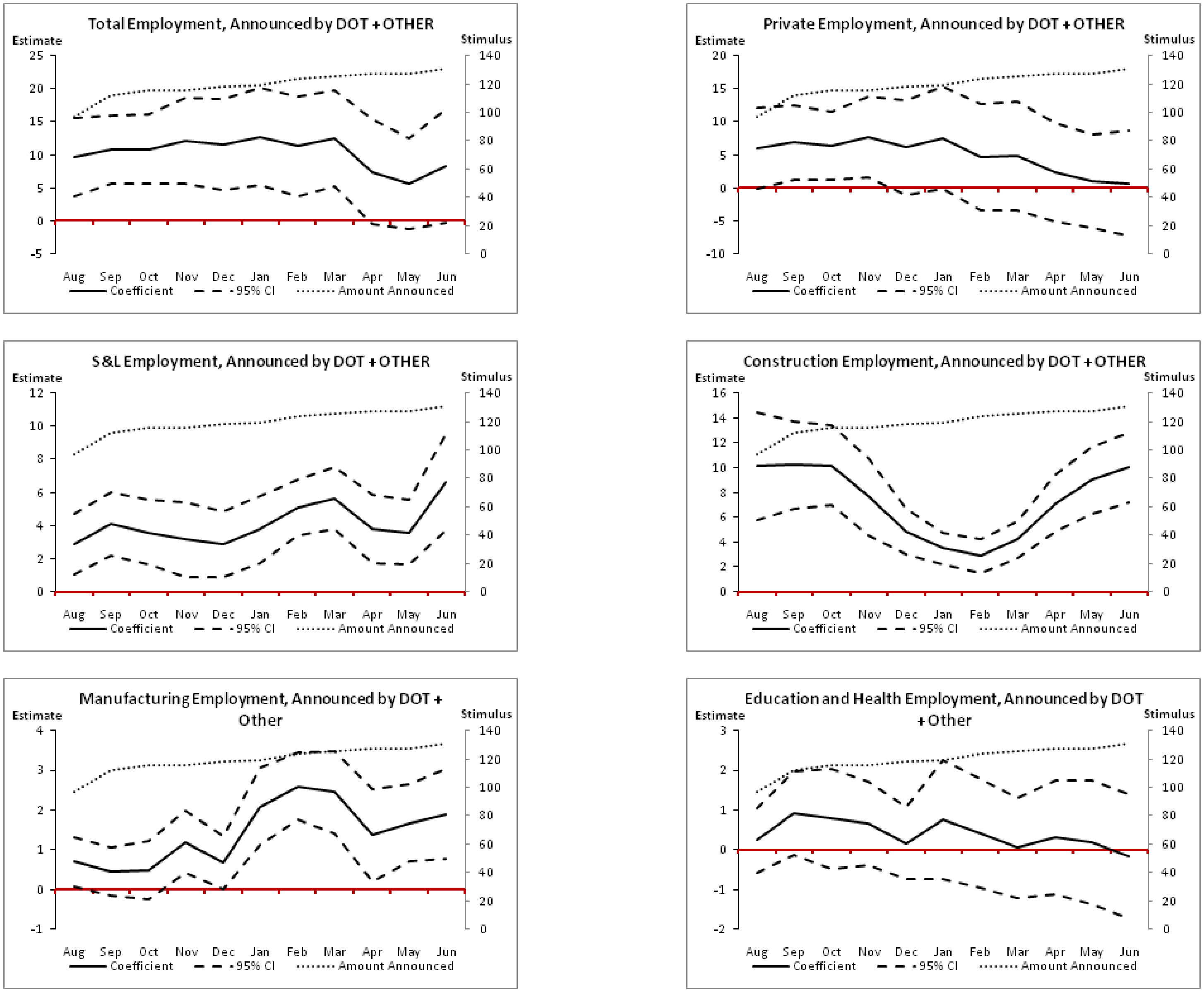

Figure 22

Coefficients over time, DOT + Other

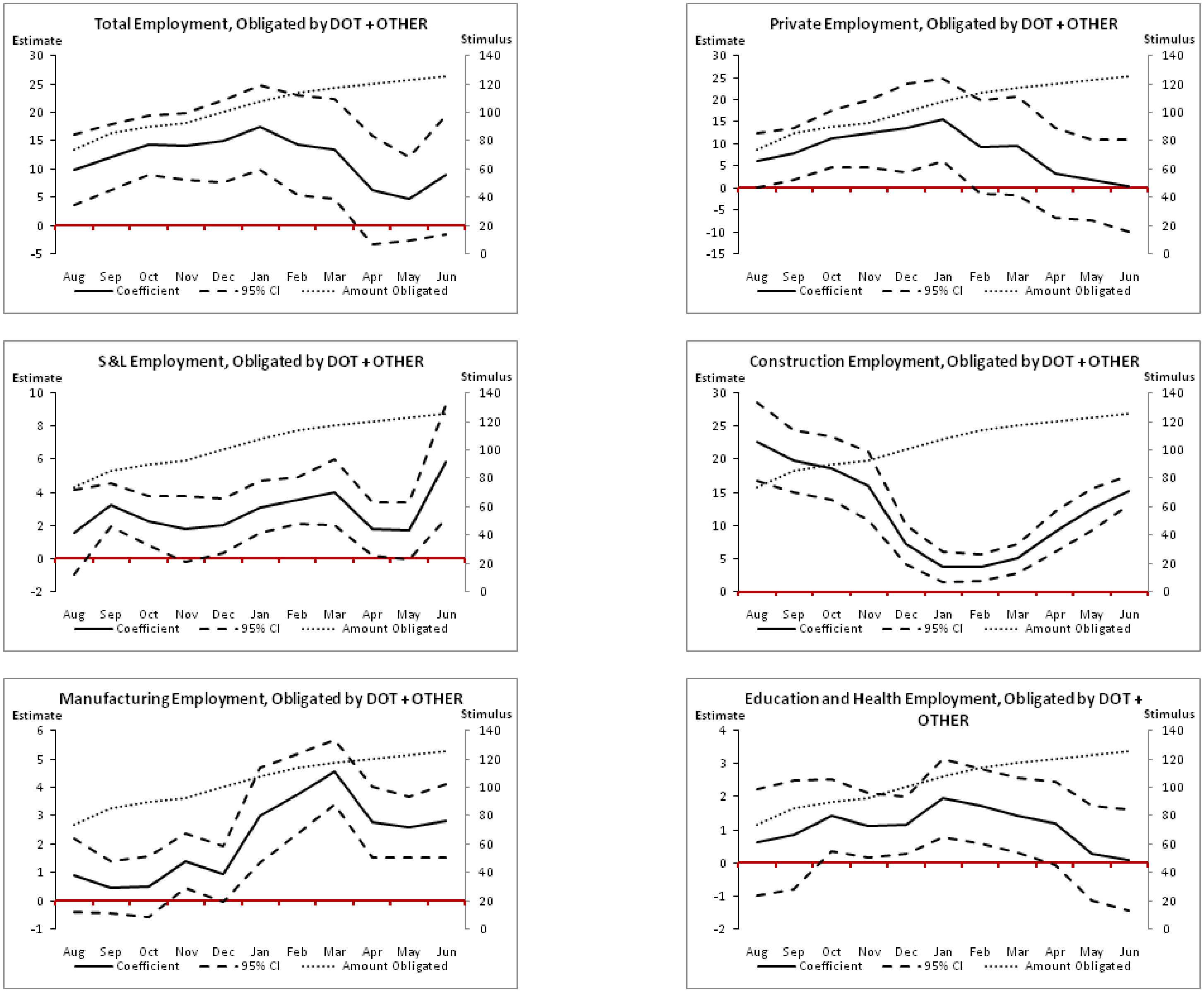

Figure 23

Coefficients over time, DOT + Other

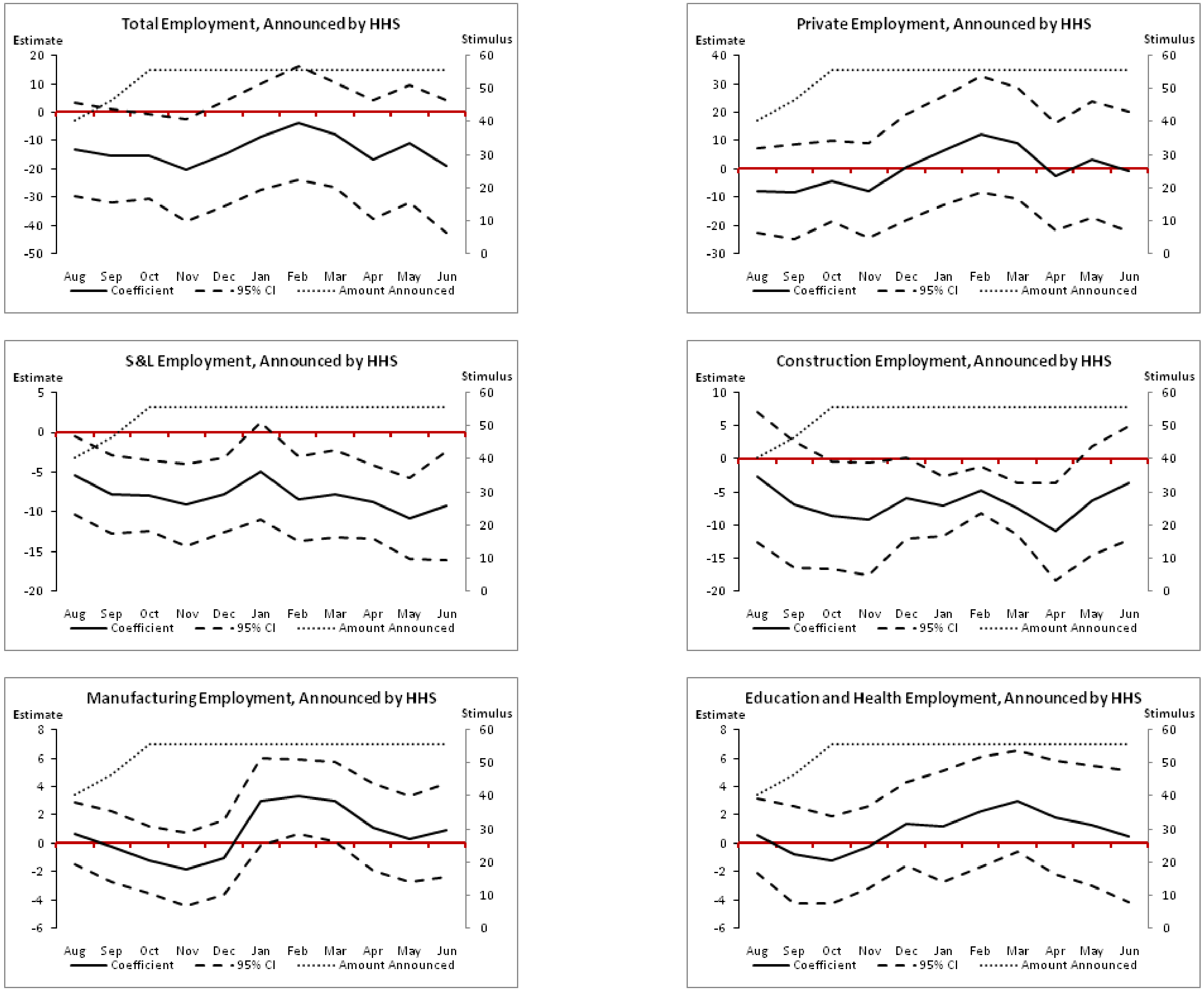

Figure 24

Coefficients over time, HHS

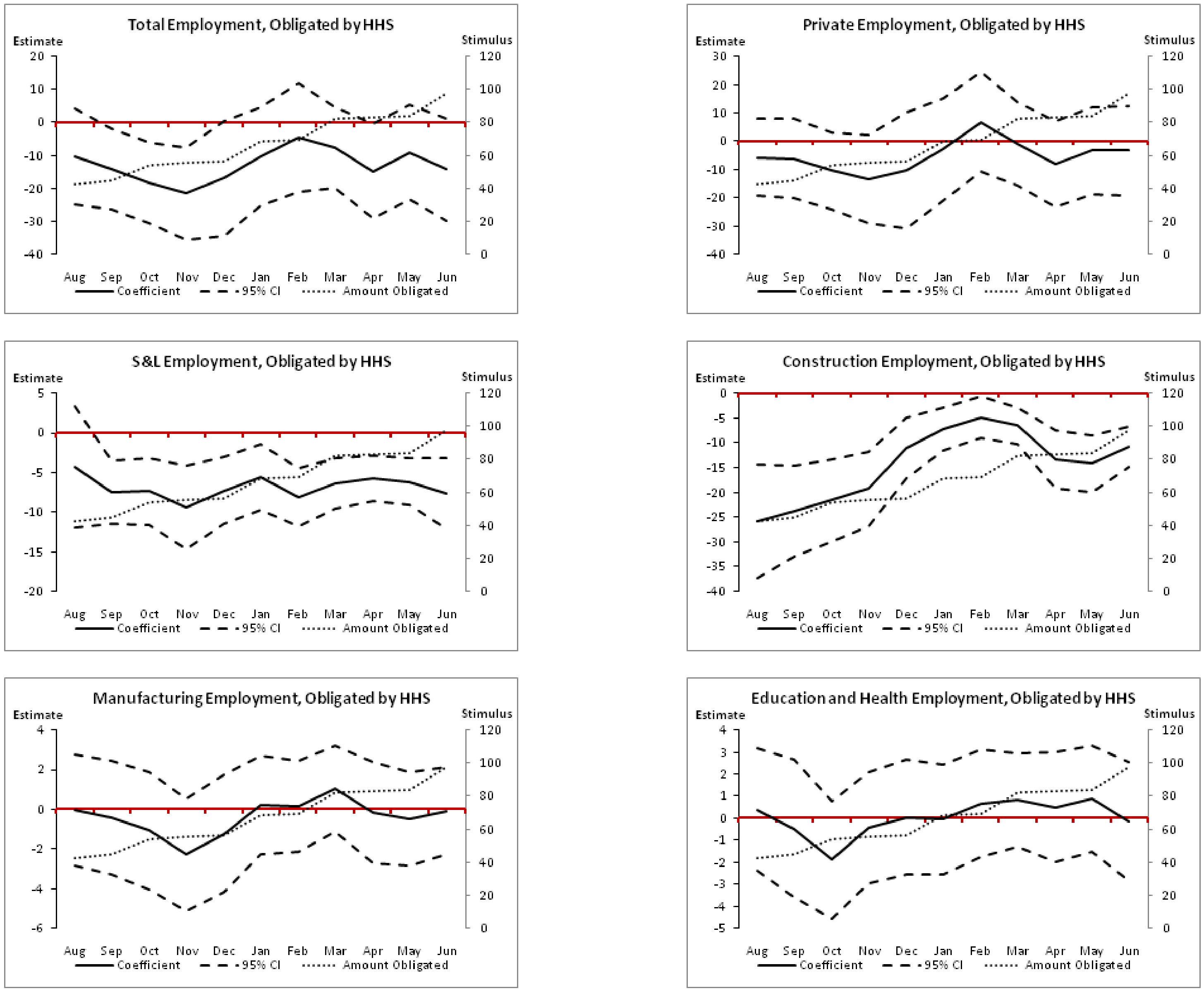

Figure 25

Coefficients over time, HHS

Figure 26. Jobs Created or Saved by ARRA Spending, as Implied by Results of Recent Academic Studies (based on ARRA announcements through quarter indicated)