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Abstract

This paper estimates the "jobs multiplier" of fiscal spending – by sector, by type of spending, and over time – using the state-level allocations of federal stimulus funds from the 2009 American Recovery and Reinvestment Act (ARRA). To control for the potential endogeneity of state ARRA receipts, I employ an IV estimator using ex-ante expected-cost estimates from the *Wall Street Journal* and the *Center for American Progress*. The results point to substantial heterogeneity in the impact of ARRA spending across sectors, across types of spending, and over time. The estimates suggest ARRA spending created or saved over 2 million jobs in its first year (ending February 2010), but that most of these jobs were short-lived. As of October 2010, the estimates point to just 0.8 million jobs attributable to ARRA spending. Across sectors, the estimated impact of ARRA spending on construction employment is especially large, implying a 19% increase in employment (as of October 2010) relative to what it would have been without the ARRA. Looking across types of spending, I find spending on infrastructure and other general purposes had a large positive impact, while aid to state governments to support Medicaid may have actually reduced 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 (ARRA) and contrasts "what happened to what would have happened" by exploiting the variation in ARRA spending across states. The ARRA 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 from 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. Critics claimed the massive cost of the ARRA would unduly swell the federal deficit while having minimal or even negative impact on employment and economic growth.

The policy debate over the effectiveness of the ARRA has centered around, and revived interest in, the long-standing debate in economics over the size of fiscal multipliers. Neoclassical models typically imply relatively small fiscal multipliers, whereas New Keynesian models, characterized by sticky prices and/or wages, generally imply larger multipliers, especially when monetary policy is less active (e.g., at the zero bound).[3] Each side of the debate can point to a number of empirical studies using historical time-series data yielding supportive evidence. For instance, recent studies by Ramey (2010) and Barro and Redlick (forthcoming), which consider the multiplier associated with defense spending and utilize narrative information on the timing of spending anticipation, find relatively small multipliers – for example, a GDP multiplier peaking below one. On the other hand, a number of other studies, especially those

[1] See Congressional Budget Office (2010a).
[2] See Congressional Budget Office (2010b), Table A-1.
[3] See Woodford (2010) and Eggertsson (2010).

using structural VAR techniques *a al* Blanchard and Perotti (2002), have found much larger multipliers.[4]

As the quote above alludes to, the key challenge faced by researchers estimating fiscal multipliers is isolating changes in economic outcomes due solely to government spending from what would have occurred in the absence of that spending. Recently, a number of studies, including this one, have turned to sub-national variation in government spending to identify fiscal multipliers, exploiting the fact that other potentially confounding nationwide factors such as monetary policy are independent of relative spending and relative economic outcomes across regions. Nakamura and Steinsson (2010) use cross-region variation in U.S. military spending to estimate an "open economy" fiscal multiplier, instrumenting for actual spending using a region's historical sensitivity to aggregate defense spending. Similarly, Serrato and Wingender (2010) consider variation in federal spending directed to U.S. counties and exploit the natural experiment afforded by the fact that much federal spending is allocated based on population estimates that are exogenously "shocked" after each Decennial Census. Shoag (2010) estimates the multiplier associated with state-level government spending driven by exogenous shocks to state pension fund returns. Chodorow-Reich, et al. (2010) use cross-state data to estimate the fiscal multiplier associated with federal Medicaid spending. They exploit the fact that one component of the ARRA stipulated a 6.2 percentage point increase, for each state, in the share of the state's Medicaid spending reimbursed by the federal government. Because this share already varied across states prior to the ARRA, this legislative component resulted in exogenous variation in federal Medicaid spending across states.[5] Lastly, Fishback and Kachanovskaya (2010) estimate a fiscal multiplier using variation across states in federal spending during the Great Depression. The results of Fishback and Kachanovskaya are particularly relevant for this paper in that both investigate the fiscal multiplier during a time of considerable factor underutilization, when the multiplier should be at its largest according to the New Keynesian model. Fishback and Kachanovskaya find that government spending had a negligible impact on

[4] See, for example, Monacelli, Perotti, and Trigari (2010).

[5] Though my paper is concerned with the fiscal multiplier associated with overall ARRA spending, of which Medicaid is less than a third, I also report results below on the fiscal multiplier of Medicaid spending. In contrast to Chodorow-Reich, et al., I find no evidence of a positive Medicaid multiplier for total nonfarm employment, even when I replace my *CAP* and *WSJ* instruments for HHS spending with an instrument based on pre-ARRA Medicaid spending, as is used by Chodorow-Reich, et al.. (In fact, I find a negative Medicaid multiplier for state and local government employment – see Table 15 below.) The difference likely derives from the control variables that I include and the fact that in my regressions I am also simultaneously controlling for the other two-thirds of ARRA spending.

employment during the 1930s. As we will see below, I find a relatively large employment impact in the short-run but a small and insignificant impact after the first year of the ARRA spending.

All of these papers, as well as mine, share in common that they are, strictly speaking, estimating "local" multipliers. That is, they apply to contexts in which output and factors of production are fairly mobile across borders. To the extent that mobility is greater among sub-national regions than among countries, the local multiplier may well be a lower bound on the national multiplier, especially in the tradable goods sector (see Moretti (2010)). [6] On the other hand, the multipliers estimated from these sub-national studies may be larger than a national multiplier because of the independence between the geographic allocation of spending and the geographic allocation of the financing of that spending. For instance, suppose that a single region received 100% of federal government spending. The impact of that spending on the federal government's budget constraint will be shared by taxpayers in all regions. Thus, these studies provide estimates of the multiplier associated with unfunded government spending.[7] This multiplier will be lower than the national multiplier if agents in the economy are Ricardian, or forward-looking, in the sense that they recognize that increased government spending now translates into future reduced spending or increased taxes.

That the local multiplier may not equal the national multiplier does not, however, mean that the local multiplier is not of independent interest, nor does it mean that the local multiplier cannot inform the debate surrounding the effectiveness of federal stimulus. In the U.S. and many other countries with federalist systems, a large share of federal spending comes in the form of regional transfers. The economic impact of these transfers is of first-order importance. In fact, much of the ARRA spending consisted of transfers to state and local governments with the goal of bolstering employment and output in those areas. In addition, this paper provides evidence on how the fiscal multiplier associated with the ARRA evolved over time. The factors potentially causing a gap between the local and national multiplier (the degrees of regional factor mobility and myopia among firms and households) are likely to be relatively constant over time, implying that the national multiplier evolved similarly to the local multiplier.

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[6] Mendoza, Ilzetski, and Végh (2010), in their cross-country panel study, find evidence that the fiscal spending multiplier is lower in open economies than in closed economies. To the extent that sub-national regions within the U.S. are more open than the national economy, this result suggests that the local multiplier estimated for these regions may indeed be a lower bound for the national multiplier.

[7] This is true for Shoag (2010) as well, even though he uses state instead of federal spending because the spending increases he considers are driven by "windfall" pension returns and therefore do not require tax financing.

This paper is not the first to attempt to evaluate the economic effects of the ARRA, though it is the first, to my knowledge, to exploit the cross-regional variation in ARRA spending for this purpose. Since the ARRA's passage, a number of studies have sought to measure its economic effects. 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.[8] 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 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).[9]

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.[10] 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

[8] See, for example, Cogan, et al. (2009), Blinder and Zandi (2010), and Drautzburg and Uhlig (2010).
[9] CEA (2010) also estimates the ARRA's economic impact using a VAR approach that compares forecasted post-ARRA outcomes (employment or GDP), based on data through 2009:Q1, to actual post-ARRA outcomes.
[10] For more details and discussion of these data on ARRA job counts, see Government Accountability Office (2009) and CBO (2010b).

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 the above two methodologies 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.[11] 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 independent of the amount of ARRA spending that the state received.

The vast majority of ARRA spending is allocated across states according to statutory formulas whose factors are exogenous with respect to post-ARRA economic outcomes.[12] **Appendix A** provides a description of the state allocation formula/mechanism for each major spending category in the ARRA (i.e., all programs with at least $5 billion of authorized funding). 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

[11] Another paper that exploits geographic variation in a fiscal stimulus program to assess its impact is Mian and Sufi (2010). This paper estimates the impact of the 2009 "Cash for Clunkers" program (which was not part of the ARRA) on auto purchases using cross-city variation in ex-ante expected benefits of the program.

[12] 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.

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

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 baseline empirical results, using data through the latest available month, are presented and discussed in Section IV. Section V explores how the estimated ARRA employment effects have evolved over time. In Section VI, I discuss the implications of these results and compare them with those of other studies relating to the ARRA and fiscal stimulus in general. Section VII 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. The numbers reported in the remainder of the paper reflect non-DOL ARRA spending only. (DOL spending accounted for 14% ($66.5 billion) of total obligations through December 2010.)

Before describing the patterns over time and across states in 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 particular, 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 (from late April 2010). The screen-shot illustrates one manner in which the website conveys information on the breakdown of ARRA spending across states.

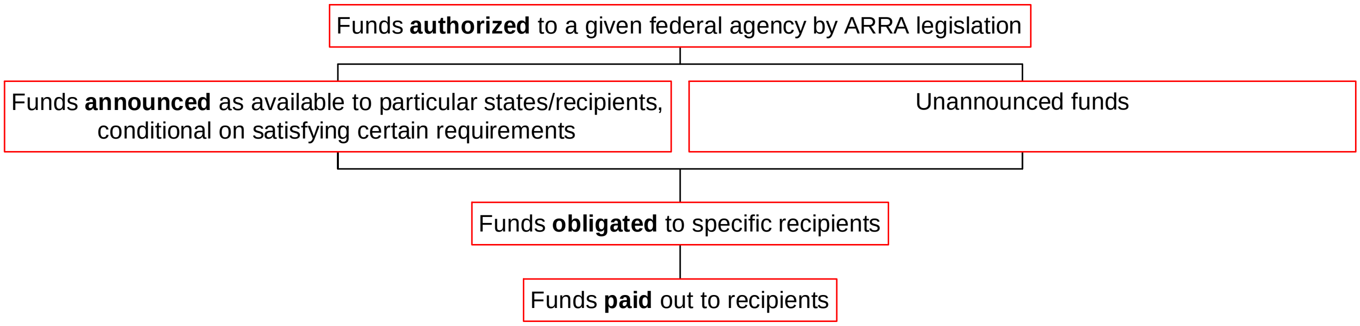
Figure 1 – Screen Shot from Recovery.gov showing ARRA Announcements, Obligations, and Payments by State

Source: http://www.recovery.gov/Transparency/agency/Pages/AgencyLanding.aspx. Accessed 4/23/2010.

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.[13] The three different metrics are "announced funds" ("announcements"), "funds made available" ("obligations"), and "funds paid out" ("payments"). Announcements are reported by agency in so-called Funding Notification Reports, while obligations and payments are reported in weekly Financial and Activity Reports. **Figure 2** provides a schematic that depicts how these three metrics are related in terms of accounting flow.

[13] 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 – generally state or municipal governments – 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 the end of 2010, 18% of announcements, 12% of obligations, and 12% of payments were not separated by state.[14] For the remainder of the paper, I will use and discuss only the state-allocated spending data.

As of the end of 2010, nearly 90% of the expected10-year ARRA spending total (from CBO) had been obligated and 65% had been 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 October 2010.[15] By the end of this period, announcements,

[14] The Dept. of Agriculture accounts for the largest share, at 40%, of non-state-allocated announcements. 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 for announcements. For obligations and payments, the non-allocated funds are spread out over many agencies; no one agency accounts for more than 25% of non-allocated obligations or payments.

[15] Note that total announcements are observed only for August 2009 onward. Recovery.gov does not provide archived Funding Notifications (the source of announcements data) and Aug. 2009 was the first month in which I

obligations, and payments were $282.7 billion, $348.4 billion, and $232.4 billion, respectively. As suggested by the 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 October 2010, the Departments of Education (ED), Health and Human Services (HHS), and Transportation (DOT) are responsible for 64% of the spending announcements, 70% of obligations, and 75% of payments.

Figures 4-6 show the evolution, from April 2009 through August 2010, of announcements, obligations, and payments, respectively, for each of these major spending agencies and for other agencies combined. The first point that emerges from these figures is that there is very little time series variation in the announcements data. 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.

Although I report regression results for all three measures of spending, it is worth discussing the relative merits of each as a measure of fiscal stimulus. An appeal of announcements and obligations relative to payments is that the former two measures are likely to lead (affect) employment and other economic activity, whereas payments are likely to lag activity. For instance, private contractors are most likely to make job hiring or retention decisions when they begin a project, which will occur after they have been awarded a contract. If the contract is awarded directly by a federal agency, the timing of the contract award will be reflected in the timing of the obligations data. If the contract is awarded by a state or local government agency, which received funding from the ARRA, the contract award will occur at some point after the announcement and obligation of those funds to the state or local agency.

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began regularly downloading the Funding Notification reports. For some agencies, however, announcements are known for earlier months because their Aug. 2009 Funding Notifications indicated that the reported level of announced funds is "as of" a specified earlier month. The earlier "as of" month is reflected in the announcements-by-agency levels shown in **Figure 4**.

A payment will not occur until the contract is completed, and possibly even later if there are bureaucratic delays in disbursements. Announcements generally lead obligations by several months. For job creation/retention of private contractors funded directly by federal agencies, obligations are likely the most relevant measure because they reflect contract awards to a specific contractor. For job creation/retention decisions by state and local governments or decisions by contractors funded by state or local government agencies, announcements may be the most relevant measure since the timing of announcements reflect when a state or local government first learns that it will receive (or are at least is eligible to receive, based on satisfying certain requirements) a particular amount of funds, and it can then act upon that information in its budgeting and personnel decisions. Note that state and local governments are easily able to avoid any temporary cash flow shortage through short-term borrowing (e.g., issuing revenue anticipation notes or warrants). Thus, in terms of obligations versus announcements, announcements has the advantage of being the more leading indicator of funding, but obligations has the advantage of reflecting only guaranteed funding (as opposed to funding conditional on meeting certain requirements) and, at least for private projects funded directly by federal agencies, may be timed closer to the start of project when hiring is most likely to occur.

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:

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

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

$\left(Y_{i,T} - Y_{i,0}\right)$ 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. The CES data are originally based on a payroll survey of about 400,000 business establishments and some model-based adjustments for establishment entry and exit. These data are revised annually to incorporate information on state employment levels from state UI records. As of the time of this writing, the last benchmark revision was done in March 2010, revising state employment counts for months from April 2008 through September 2009 (with the exception of one state which revised only through June 2009).
2. Job gains and losses. 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 2010:Q1).
3. Unemployment rate (seasonally-adjusted monthly data from the BLS household survey).[16]

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

[16] 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.

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.[17] Third, if one is 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 regression. 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 change, from 2005 to 2006, in a three-year trailing average of personal income per capita. This variable is included because it directly enters the formula determining the state allocations of ARRA "Fiscal Relief" funds, which come from the Department of Health and Human Services (HHS) and were meant to help states pay Medicaid expenses.[18] 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

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

[18] The hold-harmless component of the ARRA's Medicaid funds calls for states whose FY2009 FMAP (an inverse function of mean personal income per capita from 2004-2006) is greater than FY2008 FMAP (an inverse function of personal income per capita from 2003-2005) to receive Medicaid funds based on FY2008 FMAP (plus other adjustments to this percentage specified in the ARRA). So the hold-harmless component of a state's ARRA Medicaid funds is increasing in FY2009 FMAP – FY2008 FMAP, which in turn is a function of the change in the three-year moving average of personal income per capita lagged three years.

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 and 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 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 level and timing 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[19]). 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 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.[20] 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.). 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 B**.

These allocations are strong predictors of subsequent actual ARRA spending. In addition, they should be orthogonal to unanticipated future macroeconomic outcomes (i.e., $\varepsilon_{i,T}$

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

[20] 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.

from equation (1a)) for two reasons.[21] 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 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.[22]

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:

[21] *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.

[22] The one component of ARRA spending for which spending is quite likely to be endogenous (because spending in a given state depends in part on its unemployment rate) and there could be a possible concern that the *CAP* or *WSJ* 10-year cost estimates reflect some kind of forecast of unemployment rates is HHS spending. To address this possibility, below I also estimate alternative IV estimates where the *CAP*'s and *WSJ*'s HHS cost estimates by state are replaced by cost estimates formed by allocating the CBO's nationwide HHS cost estimate ($89.6 billion) across states in proportion to states' pre-ARRA (FY2007) Medicaid spending. This follows Chodorow-Reich, et al. (2010), who use FY2007 state Medicaid spending as an instrument for actual ARRA HHS spending by state. I find very similar estimates of the jobs multiplier using these alternative instruments as I do using the original *CAP* and *WSJ* instruments. The alternative estimates are available upon request.

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

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* ($S_t^{\$}$):

$$JOBS_t = \beta^{JOBS} * S_t^{\$}. \quad (3)$$

The cross-sectional analysis described above smoothes over any variation among states in the intertemporal pattern of stimulus spending and outcomes between the ARRA's enactment and the end of the sample period. For example, for a given level of cumulative spending to date, one state may have received most of the spending early in the sample period whereas another may have received most of the spending later in the period. This timing variation may contain useful information, but it is likely to 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 *flow* of spending (i.e., the first-difference of cumulative spending) by month. Absent some exogenous determinant of the *monthly flow* of ARRA spending, the exogenous component of this monthly flow is unidentified. This rules out using a dynamic panel model to estimate a distributed lag structure or impulse response function for stimulus spending.

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 with respect to ARRA spending) and how the flow and composition (across agencies) of spending changed over time.

IV. Baseline Results

A. Raw Correlations

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, between (1) the alternative measures of ARRA spending, (2) ARRA spending and the instruments, (3) ARRA spending and employment change, and (4) the instruments and employment change.

(1) Correlations between alternative measures of ARRA spending

The scatterplot in **Figure 7** shows the relationship across states between ARRA announcements per capita (x-axis) and obligations per capita (y-axis), through October 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 are one or more 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 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. I will show below that down-weighting these less populous states in the regressions (weighting by the inverse of BLS's sampling error variance estimates) has little effect on the fiscal jobs multipliers that I estimate.

(2) Correlations between ARRA spending and the instruments

Figures 9-11 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 October 2010. Again, all variables are in per capita terms. The solid red line in each figure is an OLS regression fit line. 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 12-14**. 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.

(3) Correlations between ARRA spending and employment change

Figures 15-17 show scatterplots with the February 2009 – October 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.) As before, 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 is stronger for announcements. For payments, the correlation is weaker, though still positive.

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. Moreover, they do not adjust for possible reverse causality from weak employment outcomes leading to more or earlier stimulus spending.

(4) 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 the instruments and the post-ARRA-enactment employment change. Both instruments have a strong positive correlation with employment change.

B. <u>Baseline OLS and IV/GMM Results</u>

The results of estimating equations (1a) and (1b) via IV/GMM are shown in **Tables 3-7**. The initial period ($t = 0$) for these regressions is February 2009, the month in which the ARRA was enacted. For the purposes of these tables, I choose the end period ($t = T$) to be February 2010. This choice is basically arbitrary – below I show the fiscal jobs multiplier estimates for other end-months – though February 2010 is of particular interest given that many fiscal multiplier studies in the literature focus on the multiplier one year past the initial government spending shock. 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 2005 to 2006 change in a three-year average real personal income per capita (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 February 2010. The OLS estimate of the jobs multiplier, β, is 10.2, with a robust standard error of 1.7. As shown in equation (2), this number can be interpreted as saying that each \$1 million of ARRA announced funds is associated with 10.2 jobs created or saved (between February 2009 and February 2010). The IV estimate is 9.0 (s.e. = 2.3). This estimate implies that the ARRA spending's cost per job created or saved at its one-year mark was \$111,111. The jobs multiplier is less precisely estimated for obligations. The OLS estimate is 8.9 (s.e. = 4.6), and the IV estimate is 10.8 (s.e. = 4.7). This IV estimate implies a cost per job of \$92,593. For payments, using either OLS and IV, the estimated multiplier is much less precisely estimated than for announcements or obligations. The OLS estimate is 7.9 and the IV estimate is 9.4, similar to the IV estimates for announcements and obligations, but neither are statistically significant. For all three measures of stimulus, the first-stage F statistics, shown at the bottom of the table, are well above standard critical values associated with weak-instrument bias.[23] Also shown are the p-values corresponding to the Hansen (1982) J-test of overidentifying restrictions. For announcements, the p-value is well above conventional significance levels, however the p-values for obligations and payments are rather low, suggesting that those two results should be viewed with caution.

It is also worth mentioning the estimated coefficients on the control variables. I find that the 2005-2006 change in the three-year average of personal income is negatively associated with employment change over February 2009 to February 2010. This may reflect the fact that states that grew faster during the mid-2000s boom tended to be experience larger economic declines during the 2007-2009 recession and its aftermath. Estimated ARRA tax benefits are found to have had a positive effect on employment, though the effect is large and statistically significant only when announcements is the measure of ARRA spending.[24] The pre-ARRA trend (from December 2007 to February 2009) in employment change (per capita) is positively associated with the post-ARRA employment change; the coefficient on this variables is statistically significant in all cases. This results likely reflects positive momentum or inertia in employment growth during this period. Lastly, I find that the initial level of employment in February 2009 is negatively associated with post-February 2009 employment change, suggesting some conditional convergence across states in terms of employment-population ratios, though this effect is only statistically significant in the case of announcements.

Table 4 shows the estimated jobs multiplier for the private nonfarm sector. The estimates are somewhat smaller than those for the total nonfarm sector, though they are again positive and statistically significant for both announcements and obligations. Using announcements, the IV multiplier is 6.4 (s.e. = 2.3). Using obligations, it is 9.2 (s.e. = 4.1). The IV estimate for payments is larger but statistically insignificant. It is also worth noting that the p-values for the overidentifying restrictions test are above conventional significance levels for all three measures of stimulus spending.

Next I consider four, more narrow sectors that are of particular interest with respect to the ARRA. Given large portions of the stimulus package were targeted at aid for state and local

[23] In particular, 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.
[24] The coefficient on estimated tax benefits should be interpreted with caution. This variable is an estimated of the expected tax benefits the state will receive over the entire 10-year horizon of the ARRA rather than actual tax benefits from February 2009 to February 2010, which is unobserved.

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.[25] **Tables 5 – 8** present the regression results for each sector. In **Section VI.A.**, the magnitudes of the sector-specific multipliers will be evaluated relative to each sector's pre-ARRA level of employment.

The results for the state and local government sector are shown in **Table 5**. The IV estimated multiplier is positive but statistically insignificant for all three measures of spending. **Table 6** gives results for the construction sector. For announcements and obligations, the IV estimated jobs multiplier is positive, and statistically significant; it is insignificant for payments. The results for manufacturing are shown in **Table 7**. For all three measures of spending, the IV estimated jobs multiplier is positive and significant. **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.) For all three measures of stimulus spending, the IV multiplier estimate for this sector is positive but statistically insignificant.

Table 9 presents results for the unemployment rate. In all cases, the estimated impact of ARRA spending on unemployment is negative – i.e., spending reduced the unemployment rate – but it is not statistically significant. The imprecision of the estimates of ARRA spending's impact on the unemployment rate may be due to the relatively large measurement error in state-level unemployment rates, which are based on a smaller-scale household survey than the large-scale employer survey used for the payroll employment data.[26]

Before proceeding to assessing robustness and exploring extensions to these baseline results, it is worth commenting on the importance of the control variables in these regressions. **Table 10** shows the results from simple univariate regressions of each instrument on each control variable. The Dec07 – Feb09 trend in employment per capita and the Feb09 level of employment per capita are positively and significantly correlated with both instruments. The estimate of ARRA tax benefits is positively and significantly correlated with the *CAP* instrument, but it is not significantly related to the *WSJ* instrument. Nonetheless, the R^2's for

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

[26] 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.

these univariate regressions are quite small except in the case of the relationship between the *CAP* instrument and the pre-stimulus trend, suggesting that the inclusion of this control variable in the final model is likely important for obtaining unbiased estimates. Indeed, I find this to be the case: **Table 11** shows IV results (with end-period equal to February 2010) when controls are excluded, compared with baseline IV results. The point estimates for obligations and payments, for total nonfarm and private nonfarm, are considerably larger when controls are excluded. A closer investigation (omitting each control one at a time) reveals that it is specifically the inclusion/exclusion of the Dec07 – Feb09 trend in employment per capita that matters most. Taken together, **Tables 10** and **11** suggest that not controlling for the pre-stimulus trend in employment could lead to positively biased estimates of the ARRA's employment impact because this trend is positively correlated with both the post-enactment employment change and the exogenous component of stimulus spending (predicted by the instruments).

To sum up the baseline results, total ARRA spending had a positive and statistically significant impact on both total and private nonfarm employment at the one-year mark after the legislation was enacted. ARRA spending had a positive and significant impact on employment after one year in manufacturing and construction, but little impact on employment in state and local government or in education and health.

We will see below, however, 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 baseline results are robust to possible measurement error.

C. Robustness of Baseline Results

I perform three 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 employment than others and should be given less weight in the regressions. **Table 12** presents results where states are weighted by the inverse of their sampling error variance from the CES payroll survey, as reported by the BLS. This weighting will also mitigate any undue influence of outlier states in terms of ARRA spending (such as Alaska, North Dakota, and Montana) because these sampling error variances are highly negatively correlated with state population. The table shows (only) the IV-estimated jobs multiplier for each of the three stimulus measures and each of the six categories of employment investigated in **Tables 3-8**.[27] 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 12**. Comparing Panels A and B, one can see that the multipliers obtained in the weighted regressions are generally quite similar to those obtained without weighting.

The second robustness check also investigates the importance of measurement error in the CES employment data. An alternative measure of state employment comes from the BLS' Quarterly Census of Employment and Wages (QCEW), previously known as the ES-202 series. The QCEW data are based on a census of state administrative (UI) records and thus have minimal measurement error. Like the CES, they are available at a monthly frequency (though they are released quarterly) and for total nonfarm as well as by industry.[28] However, the QCEW data are not available on a seasonally adjusted basis and are released with a substantial lag (of between seven and nine months). To assess the importance of CES measurement error to the baseline results, I estimate the same set of IV regressions underlying **Tables 3-8**, but using the QCEW data. As before, employment change is measured from February 2009 to February 2010.

The results are shown in **Table 13**. The results based on QCEW data (Panel B) are generally quite similar to those based on the CES (Panel A), with a few exceptions. The multiplier for state and local government, when either obligations or payments are used as the measure of spending, are larger and statistically significant when QCEW data are used. The estimated multipliers for construction also tend to be larger using QCEW data, while the multipliers are smaller (and insignificant for obligations and payments) for manufacturing. Overall, there's little indication from **Table 13** that CES-based results, at least for announcements and obligations, are likely to be systematically biased toward zero due to measurement error.

Lastly, I assess whether using February 2009 as the initial month instead of earlier months has a substantial effect on the results. If the passage, size, and composition of the ARRA was substantially anticipated prior to February 2009, then the Act may have had an economic impact prior to passage. In particular, such anticipation would imply that a state's February 2009 employment level is an invalid measure of "pre-stimulus" employment. Data from the Survey of

[27] 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.
[28] QCEW data are available for the agricultural sector as well, however the BLS Handbook of Methods notes that only 47% of agricultural employment are covered by state UI records.

Professional Forecasters (SPF) provide helpful guidance on this question. The SPF in 2008:Q4 and 2009:Q1 contained special survey questions related to the possibility of a fiscal stimulus package. They asked panelists whether their economic forecasts reflected any influence of a new stimulus package, and if so, what they expected for its size and composition (in terms of government consumption plus investment, transfers, and taxes). For responses received on or before Nov. 10, 2008 (the 2008:Q4 SPF), 69% expected a stimulus package in 2009, the mean estimate of its size was $211 billion, and it was estimated to be 2/3 spending and 1/3 taxes. For responses received on or before Feb. 10, 2009, 91% expected a stimulus package, the mean estimate of its size was $806 billion, and it was estimated to be 2/3 spending, and 1/3 taxes (this despite the fact that the bill overcame the filibuster threshold for passage in the Senate by only one vote).[29] Thus, as of November 2008, though many forecasters expected an ARRA-like stimulus package to be passed in the year to come, it's expected scale was far lower than what was eventually passed. But by early February 2009, the passage of a stimulus package very similar to the ARRA was widely expected. This suggests that fiscal foresight of the ARRA *could* have begun having significant effects on real economic activity as early as December 2008, but probably not earlier. I therefore report here how the baseline results differ if one uses either December 2008 or January 2009 as the initial month in defining the "post-stimulus" employment change (as well as in defining the pre-stimulus level and trend control variables). Note that while employment in months prior to February are more likely to be unaffected by ARRA anticipation, using an earlier month also introduces more noise into the measurement of employment change due to ARRA spending.

Table 14 compares the estimated jobs multipliers for each of the four sectors and each of the three stimulus measures from using December 2008 (Panel C) or January 2009 (Panel B) instead of February (Panel A, reproduced from **Tables 3-8**) as the initial month. The IV-estimated jobs multipliers obtained from using either December or January are quite similar to those obtained using February. It is also notable that the jobs multiplier for state and local government increases as the initial month is pushed back earlier. It is possible that, unlike private agents, some state and local governments, faced with severe and growing fiscal imbalances at the time, may have incorporated hoped-for future federal fiscal aid, however

[29] The widespread anticipation of the ARRA's passage, size, and composition reflected in SPF responses on or before Feb. 10, 2009 is not surprising. The House bill version of the ARRA passed on Jan. 28. The Senate voted to end debate on the Senate bill on Feb. 9; the Senate passed the bill on Feb. 10.

uncertain, in their revenue projections used to balance their prospective budgets. This could lead to earlier employment impacts in this sector than seen in the private sector. It should also be noted that the standard errors are typically larger when December or January is used as the initial month, consistent with the notion that using earlier initial months introduces additional noise into the analysis.

D. Extension 1: Heterogeneous Effects 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.

The IV results of allowing the jobs multiplier to vary by these three types of spending (in the same regression) are shown in **Table 15**.[30] 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 one regression, for the sector indicated, containing all three categories of stimulus spending.

The results based on announcements and obligations are quite similar. In both cases, the IV-estimated jobs multiplier for the total nonfarm sector is positive and significant for DOT and Other spending, and negative but insignificant for HHS spending. For ED spending, the estimated multiplier is positive for announcements and negative for obligations, but in both cases

[30] Results for the unemployment rate were very imprecisely estimated and hence are not shown. They are available from the author upon request.

it is estimated very imprecisely. 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 for the regressions based on announcements. 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 case here, 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 15** are generally below 5 when announcements are used, but above 8 when obligations are used.

Looking across subsectors, DOT and Other spending is found to have a positive and statistically significant effect on jobs in the state and local government, construction, and manufacturing sectors. It also has a positive and significant effect on the (private) education and health sectors when obligations are used. For both announcements and obligations, HHS spending is estimated to have a negative and significant effect on state and local government and construction employment. On the other hand, HHS spending is found to have a positive effect on employment in manufacturing, though the effect is small and insignificant when spending is measured by obligations.

The negative multiplier for the state and local government and construction sectors from ARRA HHS/Medicaid spending may seem somewhat surprising. However, it may well reflect negative burdens placed on state government budgets 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. That is, the HHS' ARRA funds for Medicaid reimbursement have "strings attached" in the form of maintenance of effort (MOE) requirements which may lead to cuts in state and local government spending in non-Medicaid areas. These non-Medicaid areas would include state governments' own payrolls, transfers to local governments, and state government funded construction works. This hypothesis that ARRA-induced state MOE Medicaid spending crowded out non-Medicaid state spending also is bolstered by the findings of Cogan and Taylor (2010). They find that ARRA Medicaid grants to states had a negative effect on purchases of goods and services by the state and local government sector.[31]

[31] It should be noted that the negative estimated HHS effect on state and local government employment holds true even if one replaces the *CAP*'s and *WSJ*'s HHS instruments with an alternative instrument formed by allocating the

E. Extension 2: Effects 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 attributes to ARRA spending, it is nearly impossible to know how much is from new jobs created versus retention of existing jobs because aggregate 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 16 reports the results of cross-state regressions where the dependent variable is the change from March 2009 to March 2010 in either gross job gains (at opening or expanding establishments), gross job losses (at closing or contracting establishments), or net employment change (the difference between job gains and job losses). Note that the BED data are only available at a quarterly frequency, so March 2009 (i.e. 2009:Q1) is chosen as the initial month here because it is the closest quarter-end to February 2009. Similarly, March 2010 is chosen as the end-month. These three separate regression equations are estimated simultaneously as a system using 3SLS in order to (1) improve efficiency given that errors across equations are likely to be correlated, and (2) impose the constraint that the effect of ARRA spending on job gains minus the effect on job losses should equal the effect on net employment change. For comparison, the IV results based on the CES data on net employment change for the same March 2009 – March 2010 period are provided in the right-most column. For all three measures of stimulus, I find that ARRA spending increased both job gains and job losses. However, the

CBO's nationwide 10-year ARRA HHS cost estimate of $89.6 billion to states in proportion to the states' FY2007 Medicaid spending. This alternative instrument is used in Chodorow-Reich, et al. (2010). The logic is that one component of the ARRA's HHS spending is simply a uniform increase of 6.2 percentage points to the shares (called "FMAPs") of states' Medicaid expenditures that the federal government reimburses. Because these shares varied considerably across states prior to the ARRA, the 6.2 percentage points increase resulted in exogenous variation in ARRA-induced federal Medicaid spending across states.

effect on job gains is estimated to be somewhat larger than the effect on job losses, yielding a positive and significant net increase in employment, similar to the net employment effect found using CES data.

Another extension I explored that is worth noting was adding a measure of stimulus spending in other states in the regressions to assess the extent to which a given state benefits from ARRA spending received by other states. To do so, for each state, I computed a weighted-average (also known as the spatial lag) of other states' announcements, obligations, or payments, using bilateral export trade flows as weights.[32] I then constructed a *CAP* and a *WSJ* instrument for out-of-state by taking a weighted average of those cost estimates using the same weighting matrix. The inclusion of this variable in the regression yielded rather imprecise estimates for the coefficients on both in-state and out-of-state spending, likely because these variables are highly collinear. Nonetheless, for announcements, the inclusion of out-of-state spending had little impact on the in-state spending jobs multiplier for total nonfarm employment (which remained positive and significant, with a value of 8.9 and a standard error of 5.0). The coefficient on out-of-state spending itself was small and insignificant (2.0 with a standard error of 8.7).[33]

V. The Evolution of the Jobs Multiplier Over Time

A. Impact of Overall Spending

The jobs multiplier of ARRA spending may increase or decrease over time as the intertemporal distribution (i.e., how front-loaded or back-loaded is the spending for a given level of cumulative-to-date spending) and the composition of the spending (across agencies) changes over time. Moreover, the cumulative response of employment to past ARRA spending to date will reflect the lag structure (or impulse response function) 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.[34] **Figures 20-21** show

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[32] Specifically, using the 2007 Commodity Flows Survey, the weight assigned to state *j* in state *i*'s spatial lag was given by the share of state *i*'s exported commodities that go to state *j*. I also tried spatial lags based on which other states border a given state and based on the inverse of geographic distance between states. All three spatial lags yielded similar results.

[33] The results for obligations and payments were very imprecise, but suggested a potentially large positive effect from out-of-state spending. These results are available from the other upon request.

[34] As mentioned in Section III, estimating a distributed lag model of the impact of ARRA spending is precluded 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). In other words, absent some exogenous determinant

how the IV-estimated jobs multiplier, for each employment category, varies as one advances the last month of the sample from the earliest month possible (which differs by spending measure) through October 2010, which is the latest month of available data (at the time of this writing). Data on obligations (and payments) are available on Recovery.gov for months as early as May 2009. The earliest available data on announcements varies by agency, but the earliest month for which all agencies report announcements is August 2009. **Figure 20** shows the results when announcements are used; **Figure 21** shows the results for obligations.[35] 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 IV coefficient on cumulative ARRA spending (as of that month). The dashed lines indicate the 90% confidence interval. The dotted lines show the path of observed ARRA spending (in billions of dollars, indicated on the right axis) for that category.

Based on announcements, the estimated multiplier for total nonfarm employment was positive and significant from August 2009 through July 2010, after which it dropped sharply and became small and statistically insignificant. The multiplier peaked at 11.9 in January 2010, after which it generally declined gradually except for an uptick in June and July of 2010. The multiplier for private nonfarm shows a similar pattern over time, though the estimate becomes statistically insignificant as early as April 2010. It is virtually zero by September 2010. The multiplier, based on announcements, for state and local government is generally small and statistically insignificant except in September 2009 and July 2010. 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, rises throughout the spring and early summer, and then begins to fall again in August 2010. It remains positive and statistically significant throughout. The timing of this pattern strongly suggests that there is a seasonal pattern at work, most likely because construction-oriented ARRA spending produces jobs only during the times of year conducive to construction, though it is also possible that it reflects some inadequacy in the seasonal

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of the *monthly flow* of ARRA spending, the exogenous component of this monthly flow is unidentified. It is only the exogenous component of the *cumulative stock* of ARRA spending to date that is identified by the instruments used in this paper.

[35] 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.

adjustments the BLS applies to its state employment data.[36] The manufacturing sector shows somewhat of 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 throughout the sample period.

The patterns for obligations, shown in **Figure 21**, are roughly similar to those for announcements. One difference, however, is that the obligations data, which are available further back than the announcements data, suggest a large and significant jobs multiplier in the total nonfarm sector as early as June 2009. As with announcements, the multiplier based on obligations drops sharply after March 2010. In fact, the multiplier on obligations is small and statistically insignificant for all months after March 2010. The results for the subsectors using obligations are qualitatively quite similar to those using announcements, except that the obligations data point to a positive and significant jobs multiplier in the education and health sector from September through December of 2009.

B. Robustness and Placebo Checks

To assess whether these dynamic patterns documented above are driven by some idiosyncratic feature of one or the other instruments, or by variation over time in mismeasurement of "true" stimulus spending, I estimate the following reduced-form regression,

$$\left(Y_{i,T} - Y_{i,0}\right) = \alpha_T + \varphi_T Z_{i,0} + \mathbf{X}_{i,T}\Gamma_T + \varepsilon_{i,T} \tag{4}$$

for each of the two instruments ($Z_{i,0}$) and for each end-month (T) from March 2009 through August 2010. The dependent variable in each regression is the change in total nonfarm employment (scaled by 2009 population) from February 2009 to the end-month T. The estimated coefficient on the instrument (φ_T) and its 90% confidence interval are shown in **Figures 22** and **23**. It is clear from these figures that the dynamic pattern found above for the IV estimates for total nonfarm employment is also seen in the reduced-form regressions, and that the same pattern is revealed by either instrument. This indicates that the IV time pattern is driven by the reduced form relationship and not by changes over time in the first-stage relationship between the measures of actual ARRA spending to date and expected 10-year ARRA spending.

[36] 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.

In particular, the drop in the IV-estimated jobs multiplier after March 2010 is also seen in the reduced form regressions, and thus this drop cannot be explained by changes in the relationship between stimulus spending to date and the instruments. In addition, the fact that the time pattern from the reduced-form regressions is the same for either of the two instruments suggests that if the results are driven by some source of endogeneity in the instruments, it would have to present in both.

Next, I perform a kind of placebo test by extending the series of reduced-form regressions estimated above to include "end-months," T, prior to February 2009. That is, as above, I regress $L_{i,T} - L_{i,Feb09}$, scaled by 2009 pop, the instruments and controls, for T = February 2008 to August 2010. If the positive relationship I find between ex-ante expected ARRA spending and employment change subsequent to February 2009 is truly a causal effect, then there should be no reduced-form relationship (conditional on the controls) between employment change leading up to February 2009 and expected ARRA spending. Note that the controls for all regressions include the change in employment from the start of the recession, December 2007, and February 2009, so the coefficient on expected ARRA spending will reflect any relationship above and beyond that between this pre-stimulus trend and employment change. To be able to estimate a single coefficient summarizing this reduced-form relationship, I use a simple average of the two instruments as the measure of ex-ante expected ARRA spending.

The estimated coefficient on the instruments' average and its 90% confidence interval are shown in **Figure 24**. The estimated coefficient is near zero and far from statistical significant for all months up to January 2009. Aside from the correlation in this last pre-stimulus month, the lack of correlation for all earlier months indicates there is no general, spurious correlation between employment change (relative to February 2009) and the instruments. Note the negative and significant relationship between the instruments and January 2009 employment less February 2009 employment indicates a *positive* partial correlation between month-over-month employment change as of February 2009 and expected ARRA spending. This could suggest some early anticipation effects prior to ARRA passage, as discussed in Section IV.C above.

An alternative placebo test is to again estimate equation (4), but replacing February 2009 as $t = 0$ with February of earlier years and replacing post-February 2009 months as the end-months with post-February months of those earlier years. This replacement applies to both the dependent variable and to the control variables. For example, in the "2004" set of regressions, the dependent variables are the change in employment (per capita) from February 2004 to end-months from March 2004 through August 2005, and the control variables are the percentage change in the three-year average of income per capita from 2000 to 2001, the change in employment (per capita) from December 2002 to February 2004, the level of employment per capita in February 2004, and the estimate of ARRA tax benefits. For all regressions, the population scaling factor is based on 2009 population to ensure that differences across the years only reflect differences in employment changes and the control variables.

The results for earlier years, from 2004 through 2007, as well as the reduced-form results for post-February 2009 months, are shown in **Figure 25**. The partial correlation between expected ARRA spending and employment change (relative to February of that year) in the earlier years is generally close to zero, have modest or no trends, and do not display large month-to-month swings. By contrast, this partial correlation for months from July 2009 through March 2010 is greater than for corresponding months in earlier years, and the dramatic drop after March 2010 is unlike any month-to-month change in any prior year.

In sum, the results from this and the earlier placebo tests suggest that the dynamic pattern of both the reduced-form and IV coefficients documented in **Figures 20 – 23** are unique to the post-ARRA-enactment time period and therefore unlikely to be spurious.

I also repeated these rolling end-month series of regressions weighting states by the inverse of their BLS sampling error variances, as was done in **Table 12**. The time pattern in the estimated IV multiplier was found to be very similar. Similarly, I repeated the series of regressions using, alternately, December 2008 and January 2009 as the initial (pre-ARRA) month rather than February 2009. Again, the results shown in **Figures 20-23** are robust to this alternative specification.

Lastly, one might be concerned that the variation in per capita stimulus spending across states may not be stable over time – in particular, that the variation may have declined over time as the states that were slower in applying for grants and taking the other steps necessary to receive the full amount of eligible funds eventually caught up. A decline in the variance of spending per capita should not cause bias in the spending coefficient; rather, it should be reflected in larger confidence intervals as one rolls the regression sample forward in time. Though there is no noticeable increase in these confidence intervals in **Figures 20-21**, it is nonetheless worth assessing whether the variance in spending per capita is stable over time. **Figure 26** shows the cross-sectional coefficient of variation for each measure of stimulus spending . If anything, it appears that the variation in spending rose slightly over the course of 2010.

C. Impact by type of spending

The six panels in **Figures 27-28** show, for each sector, how the estimated jobs multiplier for DOT and Other spending varies as one advances the last month of the sample. 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 15** (that is, including all three agency categories). **Figure 27** gives the results for announcements as the stimulus measure; **Figure 28** 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 on DOT and Other spending for total nonfarm employment peaks in January 2010, after which it generally declines and becomes statistically insignificant as of August 2010. For private nonfarm, it is also declines after January 2010 is virtually zero by August 2010. The multiplier for state and local government employment is always positive and significant (except in Nov. 2009) and peaks in June 2010. As was found above for total ARRA spending, DOT and Other spending appears to have had a large positive impact on construction employment in the summer and fall of 2009, then gradually fell until bottoming out in February 2010, rose again through July 2010, and slowly declined thereafter. 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 28**, except that the DOT and Other obligations' multiplier shows a more pronounced decline for total nonfarm after March 2010 (becoming insignificant in subsequent months except for July 2010) and the multiplier for education and health services is positive and significant for all months from October 2009 through May 2010.

As in **Table 15**, when February 2010 was the end-month, the coefficients on ED spending generally are 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 29-30**. For both announcements and obligations, the estimated jobs multiplier from HHS spending for the total nonfarm sector is negative but generally insignificant. 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 two for announcements, and for all months but one for obligations. 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 such 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 fiscal years 2009 and 2010, were forced to allocate more of their general funds toward transfers 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.

VI. 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 (or, inversely, the cost per job created or saved), 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) – yielded a jobs multiplier for the total nonfarm sector of 9.0 per million dollars of announcements through February 2010. Announcements through February 2010 totaled $258.8 billion. The jobs multiplier of 9.0 then implies that there were about 2.3 million more jobs in the economy in February 2010 than there would have been without the ARRA's spending. That number represents a 1.7% increase relative to the level in February 2009.

The implied number of jobs created or saved, however, drops sharply over the course of 2010. As discussed in the previous section, the estimated jobs multiplier for total nonfarm employment steadily declined after the one-year mark of the ARRA. By October 2010, the estimated multiplier, based on announcements, was 2.8 (and not statistically significant). Cumulative ARRA announcements through October 2010 were $282.7 billion. These numbers imply that there were roughly 0.8 million (a 0.6% increase from February 2009) more jobs in October 2010 than there would have been without the ARRA spending. The sharp decline in the implied impact of ARRA spending after the one-year mark of the legislation suggests that many of the jobs created in the ARRA's first year were relatively short-lived.

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 results are shown in **Table 17** below. The IV-announcements multiplier estimate for private nonfarm implies 1.6 million jobs (1.5%) created or saved as of February 2010, but just 0.1 million jobs (0.1%) created or saved as of October 2010. Similarly, there is a drop-off in the estimated jobs impact of the ARRA for manufacturing. The estimated impact goes up for construction and state and local government, and is flat for education and health services.

Table 17. Estimated number of jobs created/saved by ARRA spending (in millions and percentages relative to Feb. 2009)

	February 2010	**October 2010**
Total Nonfarm	2.3 (1.7%)	0.8 (0.6%)
Private Nonfarm	1.6 (1.5%)	0.1 (0.1%)
S&L Government	0.1 (0.6%)	0.2 (1.2%)
Construction	0.4 (6.5%)	1.2 (19.4%)
Manufacturing	0.7 (5.8%)	0.4 (3.3%)
Education & Health	0.2 (1.0%)	0.2 (1.0%)

B. <u>Comparison with Government Studies</u>

How do these results compare to estimates from other studies of the number of jobs created or saved by the ARRA? One 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).

As of the time of this writing, the most recent CEA report was released on Nov. 18, 2010 (see CEA (2010)) and the most recent CBO report is from Nov. 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. The reported ranges, alongside the estimates from this paper, are shown in **Table 18** below. As of the end of the 2010:Q1 (March), the CEA reports a range of 2.2 to 2.7 million jobs created or saved (see their Table 9), whereas the CBO's range is 1.2 to 2.8 million jobs (see their Table 1).[37] The range as of 2010:Q3 (September) was 2.7 to 3.7 million for the CEA and 1.4 to 3.7 million for the CBO.

Table 18. Estimated number of jobs created/saved by ARRA (Total Nonfarm sector)

	March/Q1 2010	**September/Q3 2010**
This paper (spending only)	2.4 million	0.7 million
Congressional Budget Office	1.2 – 2.7 million	1.4 – 3.6 million
Council of Economic Advisors	2.2 – 2.7 million	2.7 – 3.7 million

This paper estimates that ARRA spending (excluding tax cuts) created or saved approximately 2.4 million jobs through March 2010, but that the impact fell in the months thereafter, reaching just 0.7 million jobs as of September 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 around 60% of total ARRA costs through September 2010 (and 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 0.7 million jobs through September 2010 from ARRA spending would imply around 1.2 million jobs due to total ARRA costs, which is slightly

[37] The CBO estimates the number of workers, rather than jobs, that the economy had at the end of each quarter that it would not have had without the ARRA. For example, they report that the ARRA resulted in 1.4 to 3.3 million added workers as of 2010Q2. 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.

below the low end of the CBO's estimates and well below the CEA's estimates.[38] This paper's estimate of 2.4 million jobs through March 2010 from ARRA spending would imply roughly 4.0 million jobs from total ARRA costs (again, if the multiplier for tax cuts was equal to that for spending), 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, near-linear increase in the ARRA's employment impact 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 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.[39]

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 structural Vector Auto-Regression

[38] 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).

[39] 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).

(VAR) estimation in which government spending is ordered ahead of other variables in a Choleski decomposition. This is done, for example, in the recent study by Monacelli, Perotti, and Trigari (2010). Following the technique of Mountford and Uhlig (2009) (which does not include employment or hours in their VAR), Bruckner and Pappa (2010) estimate a similar structural VAR but also impose theory-based sign restrictions. Ramey (2010), on the other hand, argues that these VAR identification strategies 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 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.[40] The cumulative response of employment (*L*) after a series of *T* quarters of government spending shocks is then:

[40] The impulse responses reported by Ramey correspond directly to ε_s (in terms of hours, which I transform to employment as described in the text). The impulse responses reported in MPT, however, are in terms of a G shock that is 1% of GDP. Since G is approximately 33% of GDP (in 2009), this translates to a 3%-of-G shock. I thus divide their reported impulse responses by 3 to obtain the approximate impulse responses with respect to a 1%-of-G shock. The impulses responses reported in BP are for a 1% of G shock and their employment variable is the employment population ratio. That is, their impulse responses (assuming constant population) are:

$$\frac{d\left(L_{t-s}/P\right)}{dG_{t-s}/G} = \frac{dL_{t-s}}{dG_{t-s}} \bullet \frac{G}{P} = \left(\frac{dL_{t-s}}{dG_{t-s}} \bullet \frac{G}{L}\right)\frac{L}{P} = \varepsilon_s \bullet \frac{L}{P} \ .$$

Thus, I back out their implied elasticities by multiplying their L/P impulse response coefficients by 2.2, which is the ratio of population to (average) total nonfarm employment in 2009.

$$L_t - L_{t-T} = \sum_{s=0}^{T-1} dL_{t-s} = \frac{L}{G} \sum_{s=0}^{T-1} \varepsilon_s dG_{t-s} \,. \tag{5}$$

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 the flow of ARRA spending from 2009:Q1 to 2010:Q3 into equation (5) for dG_{t-s}, 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). **Figures 31-32** show the results alongside the estimates provided is this paper. The results in **Figure 31** are based on announcements as the measure of spending; those of **Figure 32** are based on obligations. Both spending measures exclude funds from the DOL as my estimated multipliers are based on non-DOL spending. 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 as of the end of that quarter.

I find that MPT's results imply ARRA-induced employment that increases steadily over time, reaching 3.1-3.3 million jobs by the end of 3rd quarter of 2010, depending on which spending measure is used. BP's impulse response, based on announcements, implies a sharp peak effect of about 2.1 million in 2009:Q3, but then a steady decline to just 0.3 million by the end of 2010:Q3. Based on obligations, the implied BP effect also peaks in 2009:Q3, at 1.4 million jobs, but declines more gradually thereafter, reaching 0.9 million by the end of 2010:Q3. Ramey's implied employment effects gradually rise over time but are lower than MPT's, reaching between 0.5 and 1.0 million jobs by the end of 2010:Q2, depending on what is assumed for the response of hours per worker to stimulus spending. This range is the same whether announcements or obligations are used. My announcements-based estimate, averaged over the months of 2010:Q3, is 1.1 million, which is slightly above those of BP and Ramey. My obligations-based estimate for 2010:Q3 is very similar to those implied by BP and Ramey. The pattern of my estimates over time is much more consistent with BP than either Ramey or MPT, which both increase monotonically. Like BP, I also have a decline in the ARRA employment effect in the latter part of the sample, though the peak effect according to my estimates occurs two quarters later (in 2010:Q1) than in BP.

VII. 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 indicate that in its first year ARRA spending yielded roughly ten jobs per million dollars spent, or about $100,000 per job. Extrapolating from that marginal local effect to the national level, the estimates imply ARRA spending created or saved about 2.3 million jobs, or 1.7% of pre-ARRA total nonfarm employment, in that first year. However, results based on later months 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 October 2010. This pattern in ARRA spending's impact over time is true whether one measures spending based on announcements of funds or on obligations of funds. It is also seen in the reduced-form relationship between post-February employment change, conditional on the control variables, for either instrument. Furthermore, the pattern is unique to the post-February 2009 pattern of employment change; it is not seen in post-February employment change for prior years.

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: a 19% increase in employment (as of October 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. Lastly, I find that ARRA spending appears to have increased both jobs gains (from opening/expanding businesses) and job losses (from closing/contracting businesses).

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Appendix A.

Details on Formulas for Major ARRA Spending Programs (Programs ≥ $5 billion)

	Program (Agency)	Expected ARRA cost	Description of formula(s)	Source
1	Fiscal Relief Fund (HHS)	$86.6 billion	For fiscal years 2009 and 2010, all states receive increase (relative to 2008 FMAP) of 6.2 percentage points in the percentage of state Medicaid expenses reimbursed by federal government (FMAP). States with high unemployment rates get additional FMAP increase.	NCSL*
2	State Fiscal Stabilization Fund (ED)	$53.6 billion	$48 billion allocated to state governments according to a weighted-average of school-aged population and total population (subject to education spending maintenance of effort requirements); $5 billion in state education incentive grants.	NCSL*
3	Federal Highway Administration Grants (DOT)	$27.5 billion	50% allocated same as the 2008 allocation of DOT obligations; 50% based on existing (per-ARRA) Surface Transportation Program formula, which depends on total lane miles of federal-aid highways, total vehicle miles traveled of federal-aid highways, and estimated tax payments attributable to highway users paid into the highway trust fund.	NCSL*
4	UI benefit extensions and enhanced benefits (DOL)	$35.7 billion	Full reimbursement by federal government of states expenses on unemployment insurance (UI). **Excluded from the analysis in this paper.**	NCSL*

	Program (Agency)	Expected ARRA cost	Description of formula(s)	Source
5	Supplemental Nutrition Assistance Program (USDA)	$19.0 billion	ARRA simply increased the SNAP benefits per eligible family. [*CAP* used CBPP state allocation of ARRA SNAP funds, which was based on the number of SNAP participants in each state in December 2008. See http://www.cbpp.org/files/1-22-09bud.pdf.]	http://www.usda.gov/wps/portal/usda/arrapie?navid=PIE_NUTRITION
6	Federal Pell Grants (ED)	$15.6 billion	ARRA increased federal funding of Pell grants for individual higher-education expenses.	NCSL*
7	Payments for seniors, disabled veterans, and SSI recipients (SSA)	$14.3 billion	Lump-sum $250 extra to each recipient of social security, disabled veteran, and SSI benefits.	Social Security Administration (SSA)
8	ESEA Title I, Part A grants to local educational agencies (ED)	$13.0 billion	$3 billion based on competitive "school turnaround" grants; $10 billion for low-income "college and career-ready students", based on pre-ARRA statutory allocation formula, which depends in part on poverty rate.	Dept. of Education (ED)
9	IDEA, Part B state grants for special education	$12.2 billion	Uses pre-ARRA statutory formula, which depends on number of children in state with disabilities or special education needs.	Dept. of Education (ED)
10	High-speed Rail and Intercity Passenger Rail (DOT)	$8.0 billion	Discretionary grants for High Speed Rail and Intercity Passenger Rail; issuance of grants determined by Federal Transit Administration, an agency within the Department of Transportation (DOT).	NCSL*

	Program (Agency)	Expected ARRA cost	Description of formula(s)	Source
11	Financial assistance for national recovery zones (Dept. of Treasury)	$5.4 billion	Funds for "recovery zone" bonds. 50% of bond funding limitation allocated equally to each state (not on per capita basis); 50% allocated based on 2008 employment decline by state.	Sec. 1400U-1 of final ARRA bill (Senate compromise bill of H.R. 1)
12	Weatherization Assistance Program (Dept. of Energy)	$5.0 billion	Increased funding of pre-ARRA weatherization assistance program and increased household income eligibility requirement from 150% to 200% of poverty level.	NCSL*

*National Conference of State Legislatures, www.ncsl.org/?TabId=16779

Appendix B.

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.

Center for American Progress Estimates of State Allocations of ARRA Spending

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), such as with ARRA Supplemental Nutrition Assistance Program (SNAP) funds, 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. **[According to the CBPP, the estimated allocation of SNAP ARRA costs across states is assumed to be proportional to the number of SNAP participants in each state in December 2008. See http://www.cbpp.org/files/1-22-09bud.pdf]**

$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.

<u>Table 1</u>

Agency Totals (Bill.) and Percentages

Oct 10

	Announcements		Obligations		Payments	
Dept. of Education (ED)	89.1	(31.5)	94.6	(27.2)	64.2	(27.6)
Dept. of Transportation (DOT)	34.7	(12.3)	38.0	(10.9)	20.9	(9.0)
Other	103.3	(36.5)	103.2	(29.6)	58.5	(25.2)
Dept. of Health and Human Services (HHS)	55.6	(19.7)	112.6	(32.3)	88.8	(38.2)
Total (excluding Dept. of Labor)	282.7	(100.0)	348.4	(100.0)	232.4	(100.0)

Table 2

Summary Statistics, Sample Period: Feb 09-Feb 10

Panel A: Dependent Variables

	Mean	SD	Min	Max	N
Change in Employment (p.c.), Total Nonfarm	-0.0114	0.0048	-0.0233	0.0057	50
Change in Employment (p.c.), Private Employment	-0.0115	0.0043	-0.0235	0.0029	50
Change in Employment (p.c.), S&L Government	-0.0002	0.0011	-0.0025	0.0023	45
Change in Employment (p.c.), Construction	-0.0028	0.0016	-0.0093	-0.0003	44
Change in the Unemployment Rate	0.0145	0.0091	-0.0040	0.0310	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
Change in PI 3-yr Moving Average (p.c.), 2005 to 2006	0.0008	0.0005	-0.0002	0.0025	50
Announcements (p.c.)	961.2	259.7	711.3	2,255.0	50
Obligations (p.c.)	1,059.8	202.2	744.5	1,904.1	50
Payments (p.c.)	596.6	115.4	348.3	848.3	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-Feb 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)	**10.172**	**9.004**	-	-	-	-
	(1.733)	**(2.388)**				
Obligations (Mill. Per Cap)	-	-	**8.896**	**10.769**	-	-
			(4.643)	**(4.722)**		
Payments (Mill. Per Cap)	-	-	-	-	7.912	9.396
					(7.557)	(11.529)
Change in PI Moving Average	**-2.882**	**-2.806**	-2.080	-1.370	-1.590	-1.509
	(1.324)	**(1.230)**	(1.490)	(1.520)	(1.490)	(1.409)
Tax Benefits (Mill. per cap)	**10.781**	**9.616**	1.228	0.887	-1.398	0.940
	(5.447)	**(5.139)**	(5.271)	(5.298)	(5.836)	(5.115)
Dec07-Feb09 trend	**0.141**	**0.155**	**0.181**	**0.158**	**0.243**	**0.197**
	(0.055)	**(0.057)**	**(0.075)**	**(0.084)**	**(0.074)**	**(0.069)**
Feb09 level	**-0.033**	**-0.033**	-0.028	-0.025	-0.025	-0.022
	(0.016)	**(0.014)**	(0.018)	(0.018)	(0.018)	(0.017)
Constant	-0.005	-0.003	-0.002	-0.006	0.004	-0.001
	(0.007)	(0.007)	(0.009)	(0.010)	(0.009)	(0.010)
N	50	50	50	50	50	50
R^2	0.560	0.556	0.397	0.386	0.314	0.297
Robust First-Stage F		22.699		44.287		16.973
Overidentifying restrictions test (p-value)		0.822		0.048		0.090

Table 4

Change in Employment:Population Ratio, Feb 09-Feb 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)	**7.793**	**6.368**	-	-	-	-
	(1.773)	**(2.301)**				
Obligations (Mill. Per Cap)	-	-	**7.444**	**9.198**	-	-
			(3.528)	**(4.118)**		
Payments (Mill. Per Cap)	-	-	-	-	9.162	11.798
					(6.500)	(10.768)
Change in PI Moving Average	**-3.117**	**-3.304**	-2.531	-1.955	-2.058	-1.885
	(1.580)	**(1.318)**	(1.636)	(1.605)	(1.560)	(1.571)
Tax Benefits (Mill. per cap)	**14.396**	**13.597**	7.736	6.977	5.473	5.899
	(5.456)	**(4.884)**	(5.031)	(4.922)	(5.050)	(5.049)
Dec07-Feb09 trend	**0.115**	**0.132**	**0.151**	**0.142**	**0.206**	**0.173**
	(0.054)	**(0.057)**	**(0.062)**	**(0.071)**	**(0.064)**	**(0.061)**
Feb09 level	**-0.038**	**-0.041**	**-0.041**	**-0.039**	**-0.041**	**-0.035**
	(0.015)	**(0.014)**	**(0.017)**	**(0.017)**	**(0.018)**	**(0.016)**
Constant	-0.007	-0.004	-0.002	-0.004	0.003	-0.002
	(0.006)	(0.007)	(0.007)	(0.009)	(0.007)	(0.008)
N	50	50	50	50	50	50
R^2	0.525	0.517	0.421	0.411	0.362	0.352
Robust First-Stage F		24.859		46.157		16.982
Overidentifying restrictions test (p-value)		0.515		0.205		0.143

Table 5
Change in Employment:Population Ratio, Feb 09-Feb 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)	0.881	0.442	-	-	-	-
	(0.669)	(0.767)				
Obligations (Mill. Per Cap)	-	-	0.393	0.247	-	-
			(0.822)	(0.827)		
Payments (Mill. Per Cap)	-	-	-	-	-0.394	0.059
					(1.981)	(2.252)
Change in PI Moving Average	-0.508	**-0.595**	**-0.537**	**-0.639**	**-0.578**	**-0.644**
	(0.319)	**(0.262)**	**(0.297)**	**(0.246)**	**(0.297)**	**(0.252)**
Tax Benefits (Mill. per cap)	-0.421	-0.478	-0.813	-0.529	-0.636	-0.458
	(1.564)	(1.366)	(1.546)	(1.359)	(1.560)	(1.402)
Dec07-Feb09 trend	0.235	0.199	0.193	0.159	0.182	0.160
	(0.239)	(0.225)	(0.242)	(0.224)	(0.241)	(0.226)
Feb09 level	**0.032**	**0.039**	**0.042**	**0.045**	**0.045**	**0.046**
	(0.017)	**(0.016)**	**(0.012)**	**(0.012)**	**(0.013)**	**(0.012)**
Constant	**-0.002**	**-0.003**	**-0.002**	**-0.003**	-0.002	**-0.003**
	(0.001)	**(0.001)**	**(0.001)**	**(0.001)**	(0.001)	**(0.001)**
N	45	45	45	45	45	45
R^2	0.378	0.368	0.346	0.344	0.343	0.340
Robust First-Stage F		19.407		35.762		18.580
Overidentifying restrictions test (p-value)		0.636		0.494		0.471

Table 6
Change in Employment:Population Ratio, Feb 09-Feb 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)	**1.619**	**1.437**	-	-	-	-
	(0.430)	**(0.501)**				
Obligations (Mill. Per Cap)	-	-	1.113	**1.806**	-	-
			(0.849)	**(0.900)**		
Payments (Mill. Per Cap)	-	-	-	-	1.856	2.449
					(2.046)	(2.986)
Change in PI Moving Average	0.198	0.206	0.206	0.192	0.240	0.212
	(0.406)	(0.379)	(0.430)	(0.389)	(0.434)	(0.416)
Tax Benefits (Mill. per cap)	0.344	0.247	-0.694	-0.672	-0.875	-0.546
	(1.360)	(1.276)	(1.507)	(1.401)	(1.555)	(1.521)
Dec07-Feb09 trend	**0.278**	**0.288**	**0.297**	**0.274**	**0.319**	**0.293**
	(0.066)	**(0.062)**	**(0.078)**	**(0.070)**	**(0.076)**	**(0.069)**
Feb09 level	**-0.111**	**-0.109**	**-0.094**	**-0.095**	**-0.084**	**-0.079**
	(0.045)	**(0.042)**	**(0.049)**	**(0.045)**	**(0.048)**	**(0.046)**
Constant	-0.001	-0.001	-0.000	-0.001	-0.000	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)
N	44	44	44	44	44	44
R^2	0.689	0.688	0.629	0.622	0.620	0.616
Robust First-Stage F		19.765		42.765		22.102
Overidentifying restrictions test (p-value)		0.583		0.133		0.096

Table 7

Change in Employment:Population Ratio, Feb 09-Feb 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)	**2.485**	**2.656**	-	-	-	-
	(0.654)	**(0.531)**				
Obligations (Mill. Per Cap)	-	-	**3.301**	**2.927**	-	-
			(0.911)	**(0.797)**		
Payments (Mill. Per Cap)	-	-	-	-	**3.984**	**5.324**
					(2.161)	**(2.272)**
Change in PI Moving Average	0.012	0.015	-0.005	0.084	-0.315	0.041
	(0.369)	(0.342)	(0.380)	(0.337)	(0.543)	(0.449)
Tax Benefits (Mill. per cap)	**2.307**	**2.356**	0.185	0.412	-0.257	-0.080
	(1.032)	**(0.932)**	(1.025)	(0.893)	(1.246)	(1.045)
Dec07-Feb09 trend	-0.104	-0.095	-0.055	-0.073	0.039	-0.020
	(0.084)	(0.078)	(0.081)	(0.074)	(0.095)	(0.079)
Feb09 level	**-0.085**	**-0.082**	**-0.084**	**-0.084**	**-0.091**	**-0.084**
	(0.012)	**(0.011)**	**(0.011)**	**(0.010)**	**(0.014)**	**(0.011)**
Constant	**-0.003**	**-0.004**	**-0.003**	**-0.003**	-0.000	**-0.002**
	(0.001)	**(0.001)**	**(0.001)**	**(0.001)**	(0.001)	**(0.001)**
N	47	47	47	47	47	47
R^2	0.813	0.812	0.813	0.809	0.720	0.702
Robust First-Stage F		21.638		49.382		23.923
Overidentifying restrictions test (p-value)		0.381		0.035		0.103

Table 8

Change in Employment:Population Ratio, Feb 09-Feb 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)	0.346	0.590	-	-	-	-
	(0.632)	(0.514)				
Obligations (Mill. Per Cap)	-	-	0.886	0.855	-	-
			(0.714)	(0.774)		
Payments (Mill. Per Cap)	-	-	-	-	0.627	2.255
					(1.897)	(3.574)
Change in PI Moving Average	**0.723**	**0.692**	**0.712**	**0.691**	**0.768**	**0.723**
	(0.273)	**(0.260)**	**(0.261)**	**(0.239)**	**(0.259)**	**(0.248)**
Tax Benefits (Mill. per cap)	-1.750	-1.382	-1.707	-1.634	**-2.185**	-1.722
	(1.271)	(1.145)	(1.117)	(1.022)	**(1.046)**	(1.088)
Dec07-Feb09 trend	-0.180	-0.215	-0.154	-0.147	-0.138	-0.146
	(0.199)	(0.184)	(0.199)	(0.184)	(0.200)	(0.192)
Feb09 level	0.014	0.013	0.008	0.009	0.013	0.007
	(0.013)	(0.012)	(0.013)	(0.013)	(0.013)	(0.019)
Constant	0.001	0.000	0.000	0.000	0.001	0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
N	49	49	49	49	49	49
R^2	0.160	0.153	0.189	0.188	0.149	0.124
Robust First-Stage F		19.450		12.674		13.579
Overidentifying restrictions test (p-value)		0.930		0.678		0.483

Table 9

Change in Unemployment, Feb 09-Feb 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)	-4.444	-2.721	-	-	-	-
	(4.846)	(4.700)				
Obligations (Mill. Per Cap)	-	-	-2.627	-3.638	-	-
			(6.623)	(6.882)		
Payments (Mill. Per Cap)	-	-	-	-	-23.603	-9.128
					(15.022)	(22.611)
Change in PI Moving Average	**9.843**	**9.760**	**9.701**	**9.810**	**9.194**	**9.607**
	(2.738)	**(1.744)**	**(2.754)**	**(1.756)**	**(2.697)**	**(1.673)**
Tax Benefits (Mill. per cap)	-18.781	**-17.548**	-15.544	**-15.403**	-12.062	-14.101
	(11.475)	**(9.869)**	(11.027)	**(8.950)**	(10.983)	(9.203)
Dec07-Feb09 trend	-0.328	-0.296	-0.287	-0.298	-0.351	-0.281
	(0.236)	(0.233)	(0.243)	(0.240)	(0.225)	(0.238)
Feb09 level	**0.359**	**0.353**	**0.359**	**0.363**	**0.399**	**0.361**
	(0.140)	**(0.118)**	**(0.146)**	**(0.128)**	**(0.141)**	**(0.134)**
Constant	0.004	0.001	-0.000	0.000	0.005	0.000
	(0.011)	(0.010)	(0.010)	(0.009)	(0.009)	(0.010)
N	50	50	50	50	50	50
R^2	0.295	0.293	0.284	0.284	0.320	0.305
Robust First-Stage F		30.402		61.719		16.814
Overidentifying restrictions test (p-value)		0.984		0.820		0.710

Table 10

Results From a Univariate Regression of Each Instrument on Each Control

Independent Variable	Dependent Variable	
	WSJ β/SE/R^2	CAP β/SE/R^2
Change in PI Moving Average	0.040	0.052
	(0.052)	(0.052)
	0.013	0.020
Tax benefits (mill. p.c.)	-0.198	**0.580**
	(0.218)	**(0.209)**
	0.017	0.139
Dec07-Feb09 trend in employment (p.c.)	**0.005**	**0.010**
	(0.002)	**(0.002)**
	0.102	0.362
Feb09 employment (p.c.) level	**0.001**	**0.001**
	(0.001)	**(0.001)**
	0.068	0.110

Table 11

IV/GMM Results, With and Without Controls

Panel A: With Controls

	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	**9.004**	**6.368**	0.442	**1.437**	**2.656**	0.590
(Mill. Per Cap)	**(2.388)**	**(2.301)**	(0.767)	**(0.501)**	**(0.531)**	(0.514)
	22.699	*24.859*	*19.407*	*19.765*	*21.638*	*19.450*
Obligations	**10.769**	**9.198**	0.247	**1.806**	**2.927**	0.855
(Mill. Per Cap)	**(4.722)**	**(4.118)**	(0.827)	**(0.900)**	**(0.797)**	(0.774)
	44.287	*46.157*	*35.762*	*42.765*	*49.382*	*12.674*
Payments (Mill.	9.396	11.798	0.059	2.449	**5.324**	2.255
Per Cap)	(11.529)	(10.768)	(2.252)	(2.986)	**(2.272)**	(3.574)
	16.973	*16.982*	*18.580*	*22.102*	*23.923*	*13.579*

Panel B: Without Controls

	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	**6.518**	4.593	**1.479**	-0.130	**3.479**	0.646
(Mill. Per Cap)	**(3.499)**	(3.381)	**(0.602)**	(1.116)	**(0.762)**	(0.459)
	26.379	*26.379*	*24.797*	*23.724*	*25.903*	*28.309*
Obligations	**13.314**	**12.264**	**1.793**	1.377	**4.716**	0.808
(Mill. Per Cap)	**(4.631)**	**(4.207)**	**(1.048)**	(1.639)	**(1.398)**	(0.721)
	39.429	*39.429*	*34.854*	*37.757*	*39.530*	*37.725*
Payments (Mill.	**26.490**	**27.995**	1.248	**11.022**	4.384	1.051
Per Cap)	**(11.774)**	**(10.421)**	(3.381)	**(5.666)**	(3.417)	(2.404)
	20.775	*20.775*	*16.917*	*19.812*	*20.873*	*28.252*

Table 12

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	**9.004**	**6.368**	0.442	**1.437**	**2.656**	0.590
(Mill. Per Cap)	**(2.388)**	**(2.301)**	(0.767)	**(0.501)**	**(0.531)**	(0.514)
	22.699	*24.859*	*19.407*	*19.765*	*21.638*	*19.450*
Obligations	**10.769**	**9.198**	0.247	**1.806**	**2.927**	0.855
(Mill. Per Cap)	**(4.722)**	**(4.118)**	(0.827)	**(0.900)**	**(0.797)**	(0.774)
	44.287	*46.157*	*35.762*	*42.765*	*49.382*	*12.674*
Payments (Mill.	9.396	11.798	0.059	2.449	**5.324**	2.255
Per Cap)	(11.529)	(10.768)	(2.252)	(2.986)	**(2.272)**	(3.574)
	16.973	*16.982*	*18.580*	*22.102*	*23.923*	*13.579*

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	**9.464**	**7.034**	0.656	**1.299**	**2.901**	0.582
(Mill. Per Cap)	**(2.287)**	**(2.195)**	(0.763)	**(0.468)**	**(0.524)**	(0.538)
	21.707	*24.490*	*17.937*	*18.567*	*19.686*	*21.315*
Obligations	**11.688**	**9.230**	0.559	**1.681**	**3.181**	0.876
(Mill. Per Cap)	**(4.737)**	**(4.206)**	(0.791)	**(0.810)**	**(0.796)**	(0.805)
	39.808	*42.407*	*33.336*	*39.834*	*46.955*	*12.675*
Payments (Mill.	8.583	9.317	1.131	2.459	**6.244**	2.646
Per Cap)	(12.055)	(11.112)	(2.366)	(2.964)	**(2.482)**	(3.987)
	14.290	*14.069*	*15.659*	*17.448*	*20.432*	*12.748*

Table 13

Sensitivity to Alternative Employment Measures

Feb 09-Feb 10

Panel A: CES

	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	**9.004**	**6.368**	0.442	**1.437**	**2.656**	0.590
(Mill. Per Cap)	**(2.388)**	**(2.301)**	(0.767)	**(0.501)**	**(0.531)**	(0.514)
	22.699	*24.859*	*19.407*	*19.765*	*21.638*	*19.450*
Obligations	**10.769**	**9.198**	0.247	**1.806**	**2.927**	0.855
(Mill. Per Cap)	**(4.722)**	**(4.118)**	(0.827)	**(0.900)**	**(0.797)**	(0.774)
	44.287	*46.157*	*35.762*	*42.765*	*49.382*	*12.674*
Payments (Mill. Per Cap)	9.396	11.798	0.059	2.449	**5.324**	2.255
	(11.529)	(10.768)	(2.252)	(2.986)	**(2.272)**	(3.574)
	16.973	*16.982*	*18.580*	*22.102*	*23.923*	*13.579*

Panel B: QCEW

	Total Nonfarm α/SE/F	Private Nonfarm α/SE/F	S&L Govt α/SE/F	Construction α/SE/F	Manufacturing α/SE/F	Educ. & Health α/SE/F
Announcements	**6.896**	**5.227**	0.861	**2.803**	**0.786**	**1.260**
(Mill. Per Cap)	**(1.829)**	**(1.871)**	(0.662)	**(1.090)**	**(0.466)**	**(0.690)**
	29.345	*32.241*	*19.452*	*26.080*	*39.263*	*17.048*
Obligations	**11.665**	**9.482**	**1.368**	**3.645**	0.369	1.318
(Mill. Per Cap)	**(4.039)**	**(3.959)**	**(0.689)**	**(1.125)**	(0.574)	(1.115)
	56.105	*59.615*	*40.328*	*51.687*	*83.303*	*13.881*
Payments (Mill. Per Cap)	**18.741**	14.851	**3.651**	**9.672**	0.478	1.790
	(10.829)	(9.793)	**(1.970)**	**(2.758)**	(1.509)	(4.451)
	11.553	*12.138*	*18.174*	*37.944*	*22.661*	*13.134*

Table 14

IV/GMM Results, Alternative Sample Start Months

Panel A: Feb 09

	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	**9.004**	**6.368**	0.442	**1.437**	**2.656**	0.590
(Mill. Per Cap)	**(2.388)**	**(2.301)**	(0.767)	**(0.501)**	**(0.531)**	(0.514)
	22.699	*24.859*	*19.407*	*19.765*	*21.638*	*19.450*
Obligations	**10.769**	**9.198**	0.247	**1.806**	**2.927**	0.855
(Mill. Per Cap)	**(4.722)**	**(4.118)**	(0.827)	**(0.900)**	**(0.797)**	(0.774)
	44.287	*46.157*	*35.762*	*42.765*	*49.382*	*12.674*
Payments (Mill.	9.396	11.798	0.059	2.449	**5.324**	2.255
Per Cap)	(11.529)	(10.768)	(2.252)	(2.986)	**(2.272)**	(3.574)
	16.973	*16.982*	*18.580*	*22.102*	*23.923*	*13.579*

Panel B: Jan 09

	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	**10.325**	**7.967**	0.619	**1.772**	**3.057**	0.829
(Mill. Per Cap)	**(2.661)**	**(2.623)**	(0.935)	**(0.634)**	**(0.531)**	(0.519)
	19.781	*21.556*	*17.825*	*19.567*	*21.250*	*26.295*
Obligations	**12.507**	**10.119**	0.874	1.797	**3.895**	1.194
(Mill. Per Cap)	**(5.057)**	**(4.537)**	(0.810)	(1.116)	**(1.032)**	(0.747)
	29.254	*29.420*	*23.287*	*25.722*	*30.469*	*13.655*
Payments (Mill.	5.598	6.438	2.385	0.013	2.912	1.397
Per Cap)	(11.038)	(9.867)	(2.190)	(3.183)	(2.493)	(2.510)
	19.017	*18.899*	*11.091*	*12.761*	*9.810*	*7.283*

Panel C: Dec 08

	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	**9.004**	**6.541**	**1.241**	0.057	**2.721**	0.748
(Mill. Per Cap)	**(2.961)**	**(3.145)**	**(0.700)**	(0.856)	**(0.695)**	(0.469)
	20.379	*22.138*	*18.857*	*18.416*	*19.900*	*29.677*
Obligations	**12.724**	**10.313**	**1.885**	-0.638	**2.530**	**1.204**
(Mill. Per Cap)	**(4.650)**	**(4.567)**	**(0.671)**	(1.233)	**(1.199)**	**(0.578)**
	29.393	*29.528*	*25.719*	*24.313*	*31.232*	*14.703*
Payments (Mill.	17.844	16.711	**5.737**	-2.443	1.979	3.140
Per Cap)	(12.572)	(11.495)	**(2.153)**	(2.759)	(2.376)	(2.263)
	19.982	*19.790*	*11.862*	*12.777*	*10.461*	*7.287*

Table 15

Jobs Multiplier Estimates by Agency

Feb 09-Feb 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	**11.332**	**5.460**	**4.922**	**2.690**	**2.422**	0.944
	(3.581)	**(3.003)**	**(1.245)**	**(0.792)**	**(0.547)**	(0.781)
ED	15.816	14.828	6.420	9.272	5.375	6.167
	(32.449)	(33.483)	(10.175)	(8.264)	(5.249)	(4.605)
HHS	-2.405	13.428	**-7.363**	**-4.990**	**3.632**	-0.322
	(10.755)	(9.617)	**(3.604)**	**(2.176)**	**(1.898)**	(2.559)
Donald-Cragg	4.220	4.939	3.118	5.429	5.105	3.854

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	**15.518**	**8.129**	**3.017**	**4.122**	**3.431**	**1.989**
	(4.960)	**(4.217)**	**(1.005)**	**(1.345)**	**(0.943)**	**(0.753)**
ED	-24.601	-26.061	7.669	6.922	3.091	4.155
	(36.811)	(33.661)	(7.258)	(7.672)	(4.962)	(3.771)
HHS	-6.523	7.715	**-7.005**	**-5.245**	0.431	-1.789
	(9.552)	(7.901)	**(2.264)**	**(2.484)**	(1.534)	(2.095)
Donald-Cragg	9.305	9.422	9.654	7.729	9.694	11.085

Table 16

ARRA Spending's Impact on Job Gains vs. Job Losses

(based on Business Employment Dynamics data)

Mar 09-Mar 10

	Gross Job Gains	Gross Job Losses	Net Job Changes	Total Private (CES)
	β/SE/F	β/SE/F	β/SE	β/SE/F
Announcements	**39.088**	**33.878**	**5.211**	**6.082**
(Mill. Per Cap)	**(5.504)**	**(5.786)**	**(1.974)**	**(1.811)**
	16.877	*14.252*		*25.598*
Obligations	**51.078**	**44.617**	**6.461**	6.150
(Mill. Per Cap)	**(9.240)**	**(9.209)**	**(2.981)**	(3.799)
	11.197	*10.860*		*49.064*
Payments (Mill.	**105.033**	**92.482**	12.551	2.399
Per Cap)	**(32.884)**	**(31.854)**	(8.551)	(9.317)
	5.260	*5.818*		*15.661*

Figure 3

Stimulus Measures

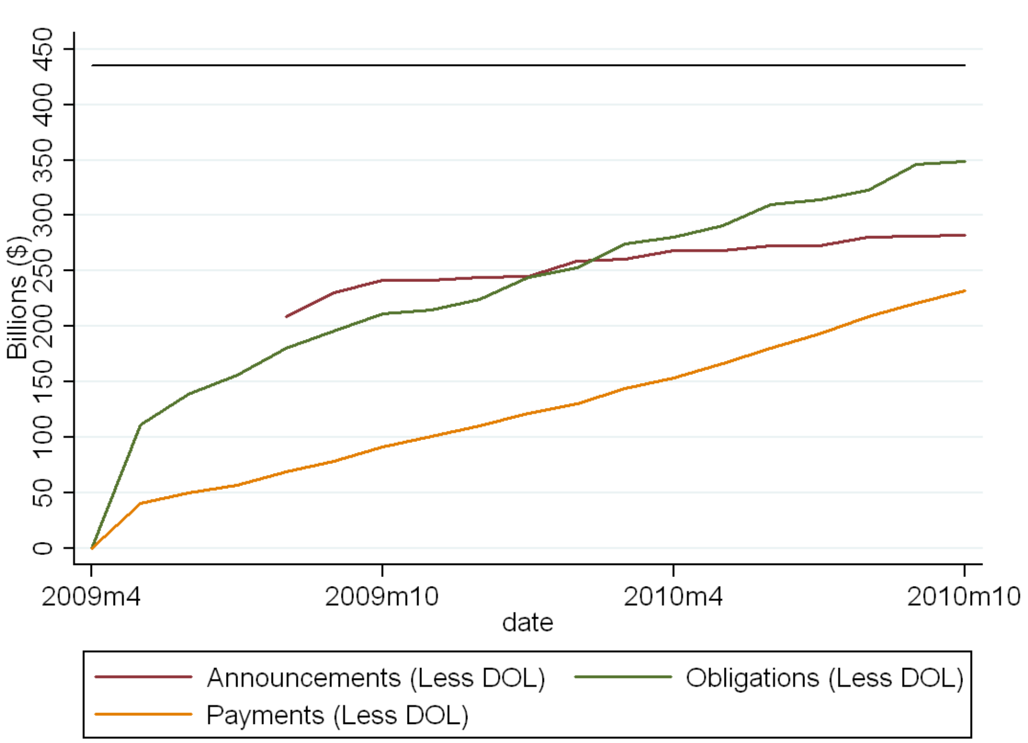


Figure 4

Amount Announced - By Agency

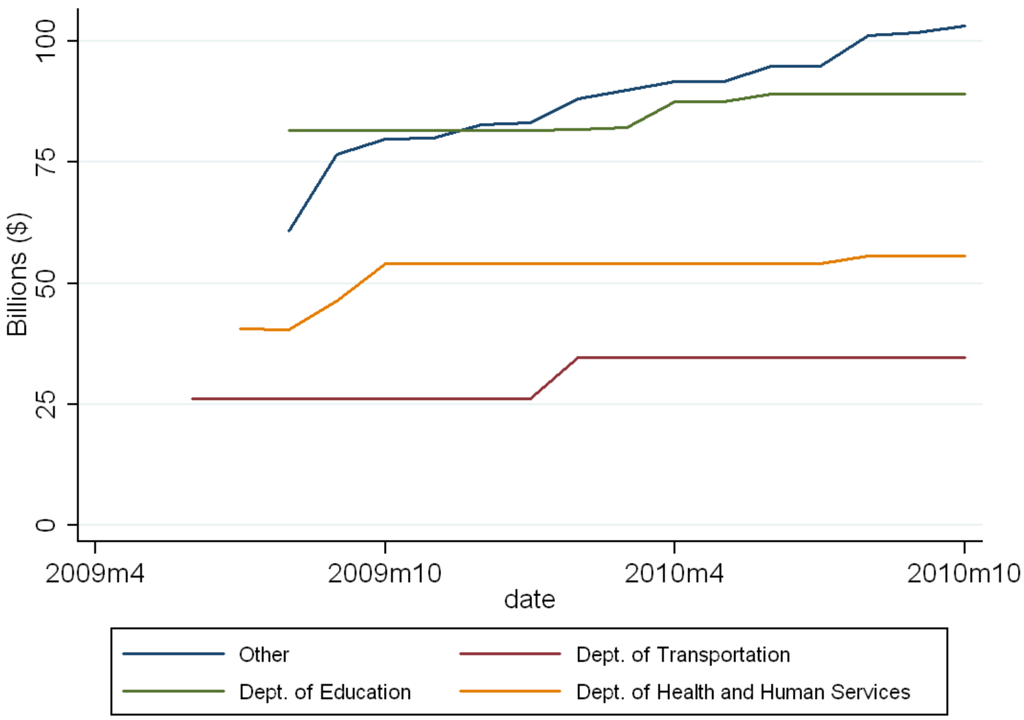


Figure 5

Amount Obligated - By Agency

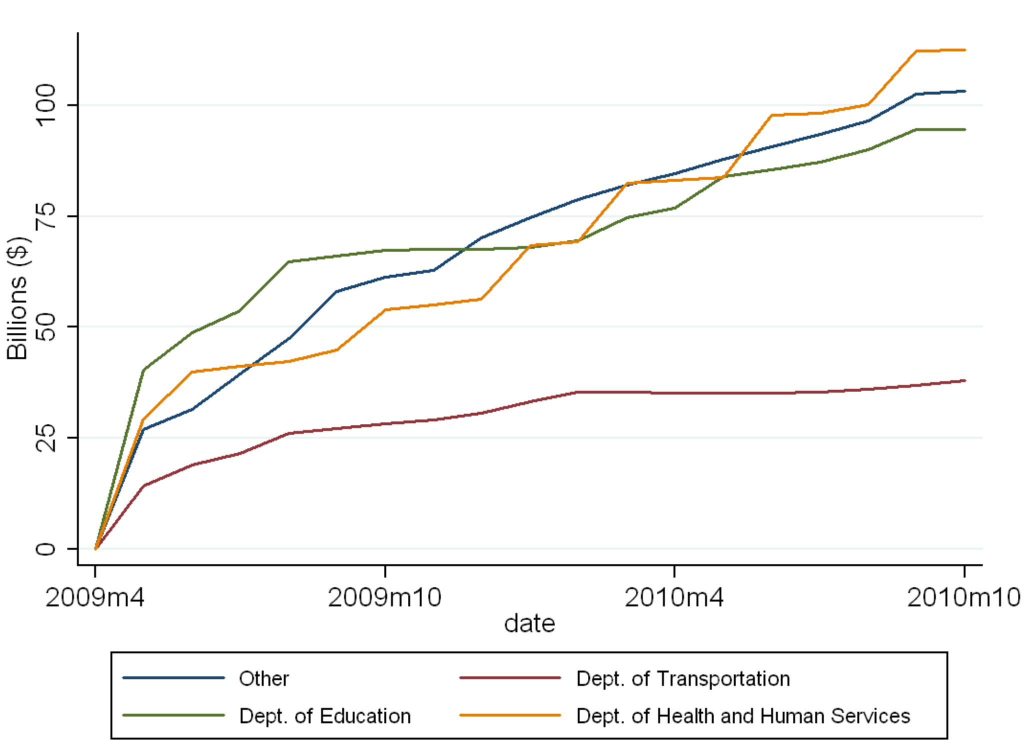


Figure 6

Amount Paid - By Agency

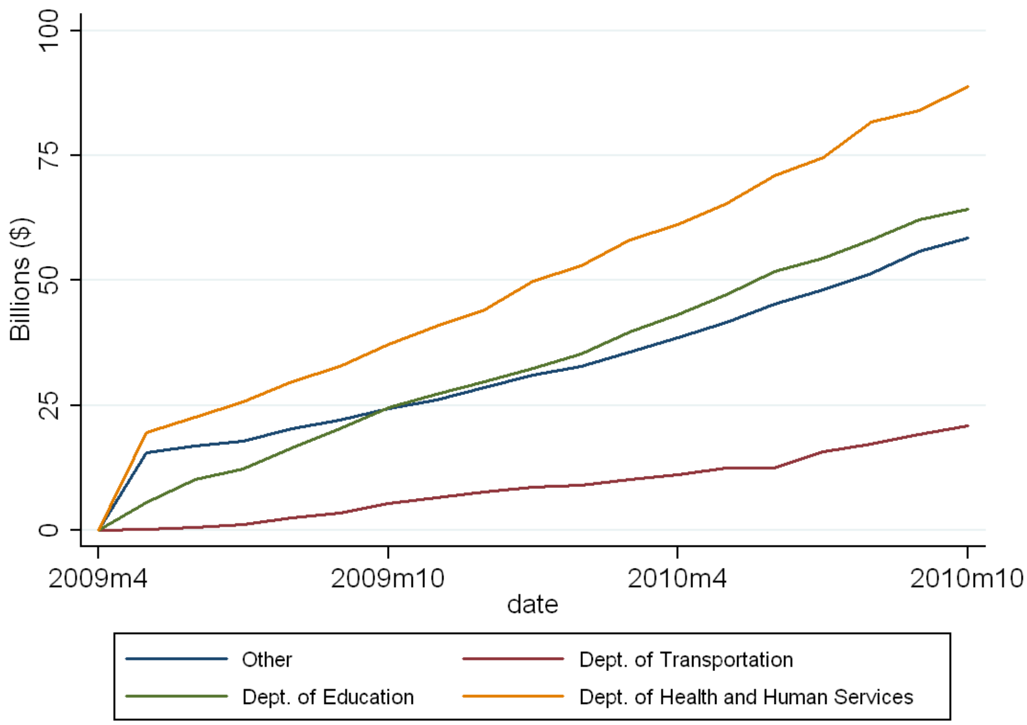


Figure 7
Announcements and Obligations per capita
Feb 10

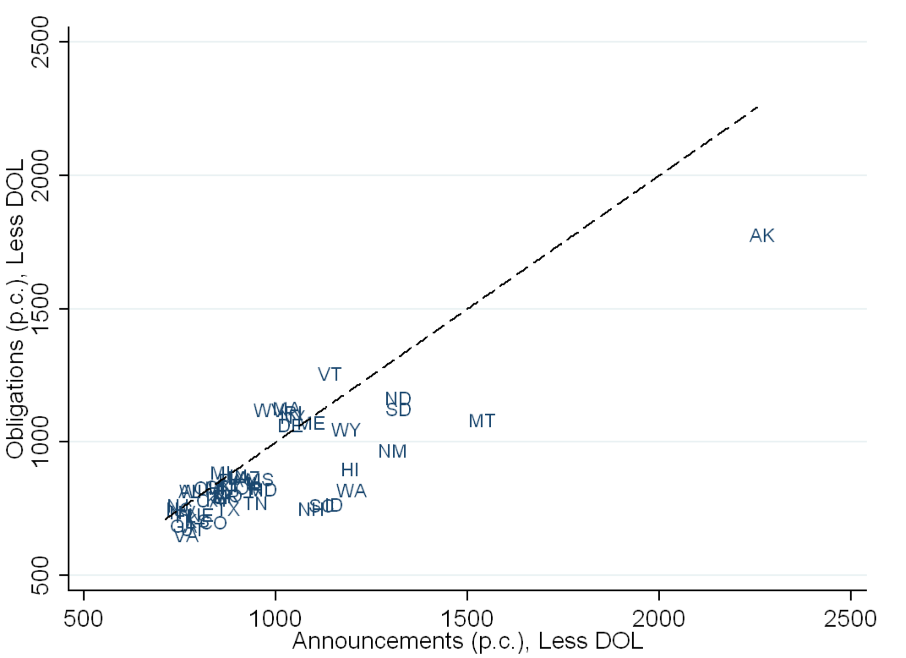


Figure 8
Announcements and Payments Per Capita
Feb 10

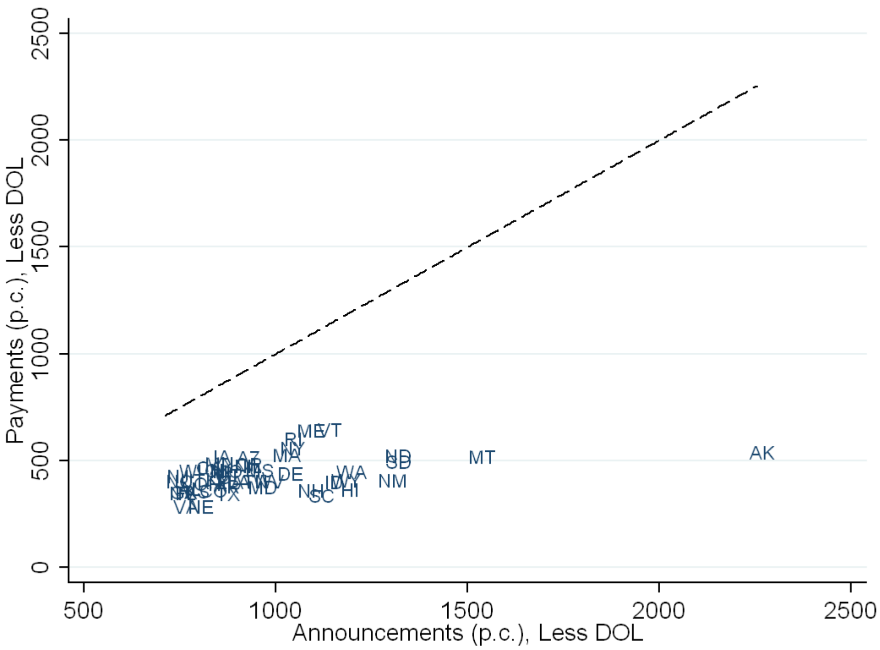


Figure 9
Center For American Progress Estimates vs Announcements
Feb 10

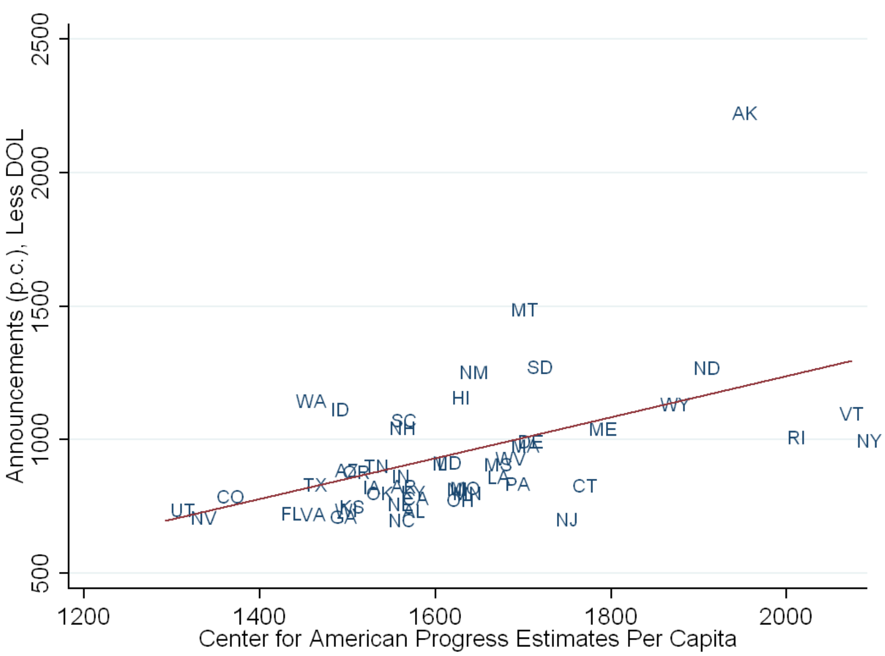


Figure 10

Center For American Progress Estimates vs Obligations

Feb 10

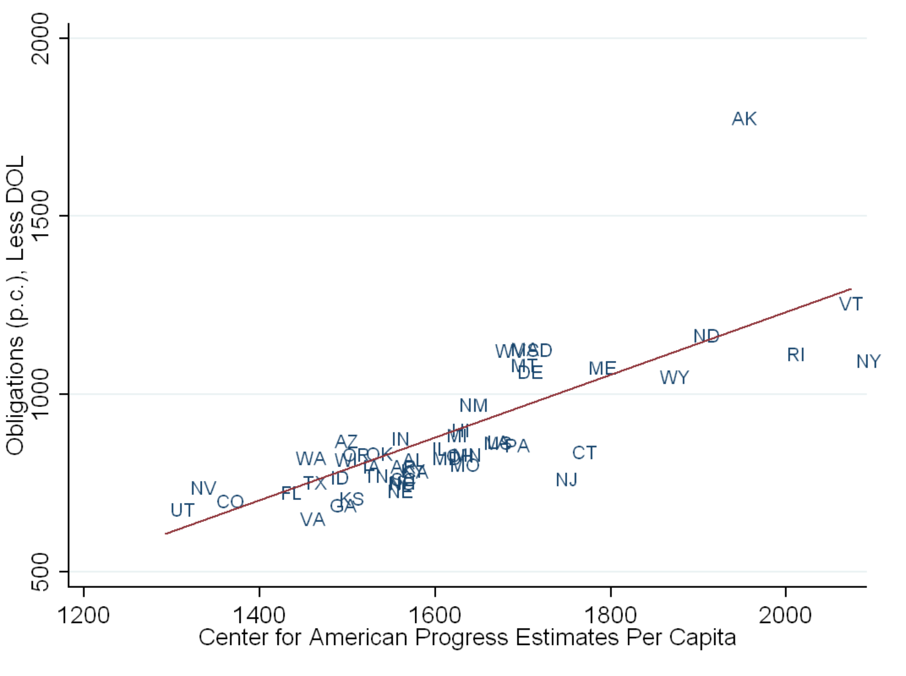


Figure 11

Center For American Progress Estimates vs Payments

Feb 10

Figure 12

Wall Street Journal Estimates vs Announcements

Feb 10

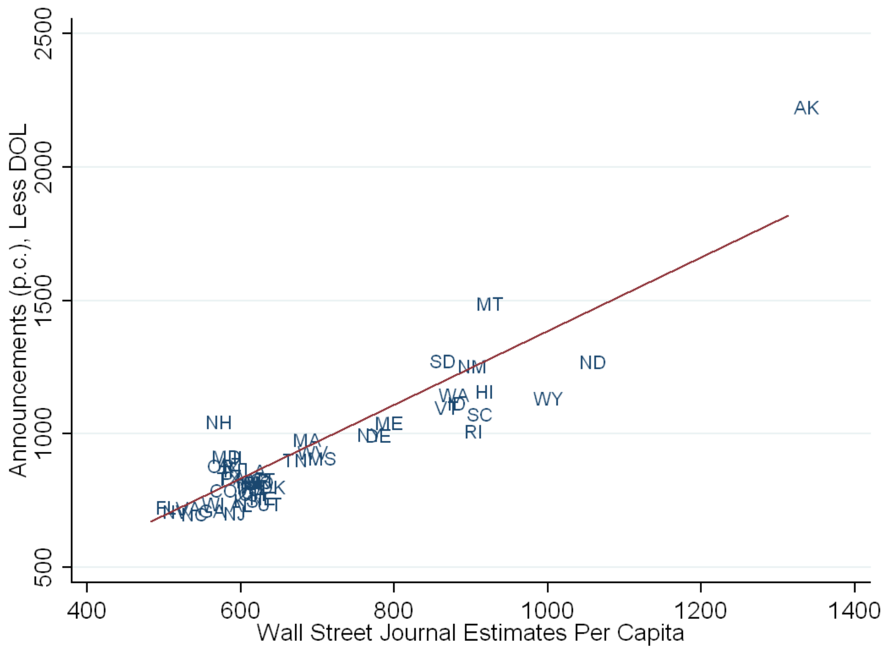


Figure 13

Wall Street Journal Estimates vs Obligations

Feb 10

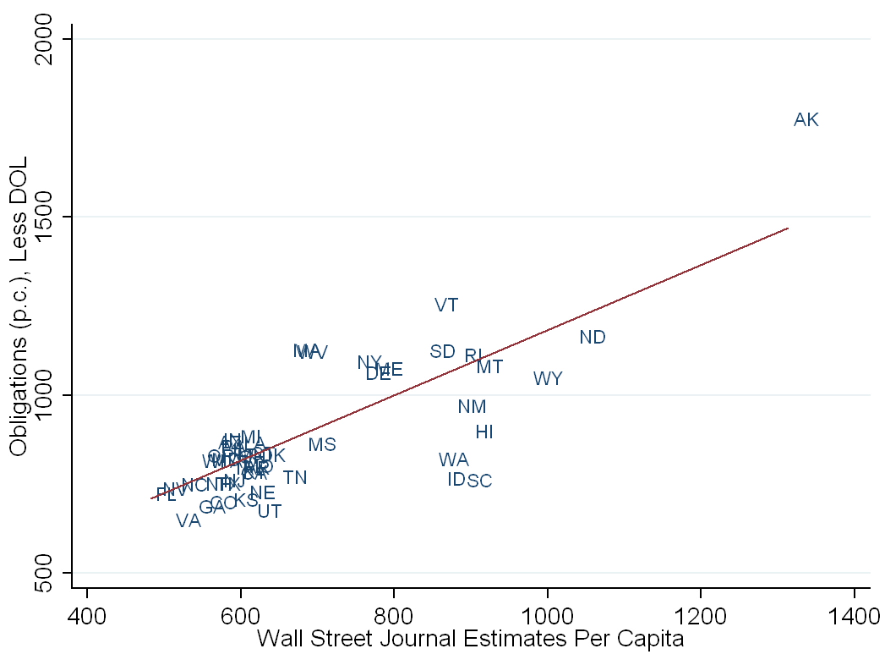


Figure 14

Wall Street Journal Estimates vs Payments

Feb 10

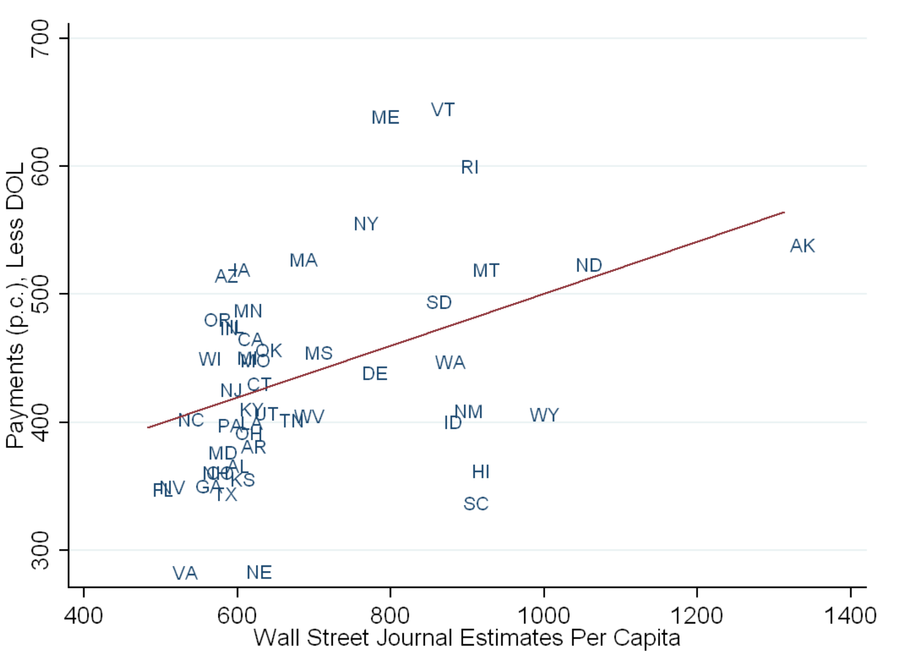


Figure 15

Change in Employment:Population Ratio v. Announcements

Feb 10

Figure 16

Change in Employment:Population Ratio v. Obligations

Feb 10

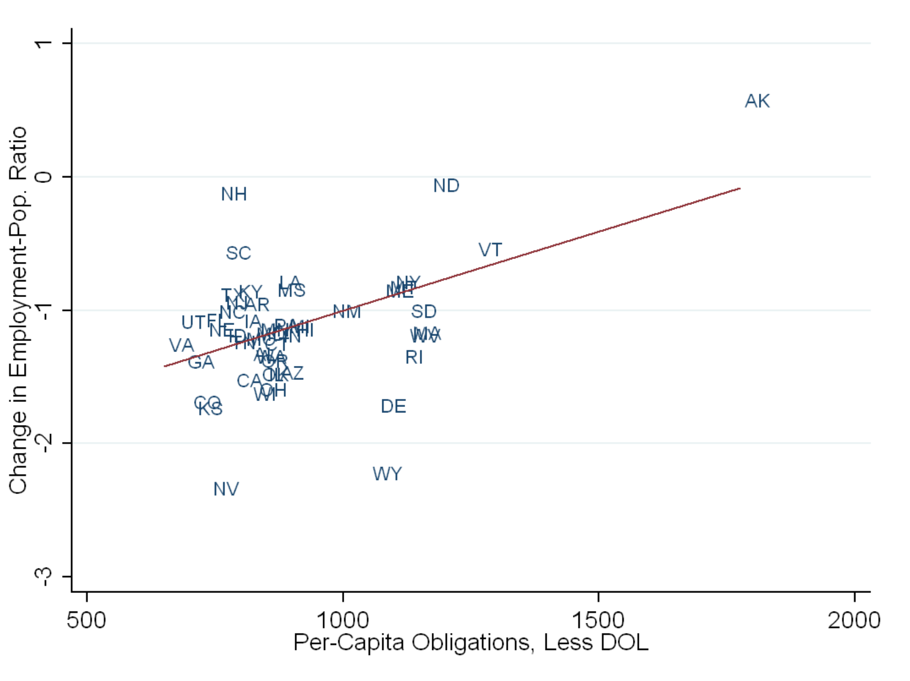


Figure 17

Change in Employment:Population Ratio v. Payments

Feb 10

Figure 18

Center for American Progress Estimates vs Change in Employment:Population Ratio

Feb 10

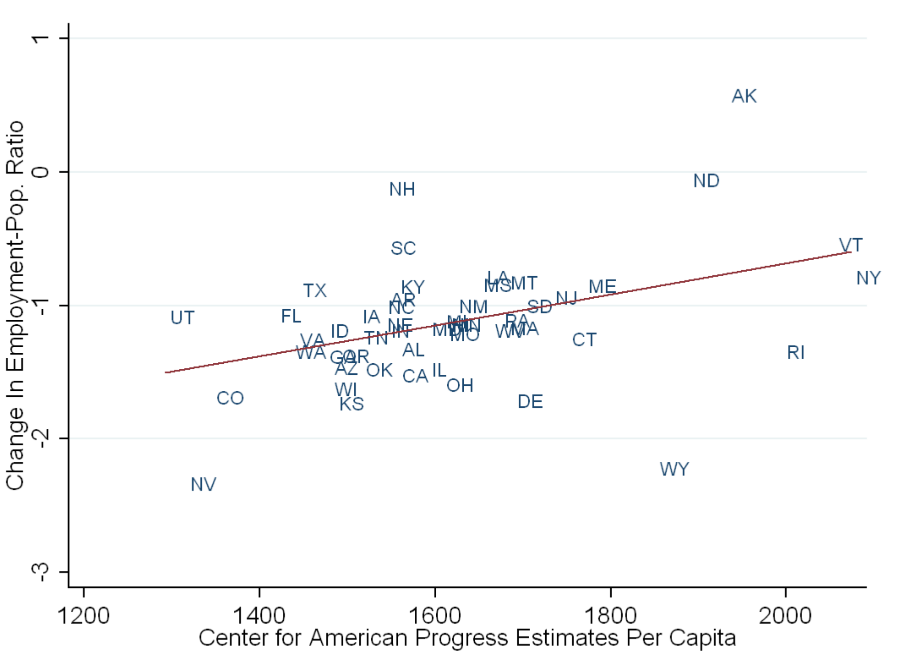


Figure 19

Wall Street Journal Estimates vs Change in Employment:Population Ratio

Feb 10

Figure 20

Coefficients over time, Announcements by All Agencies

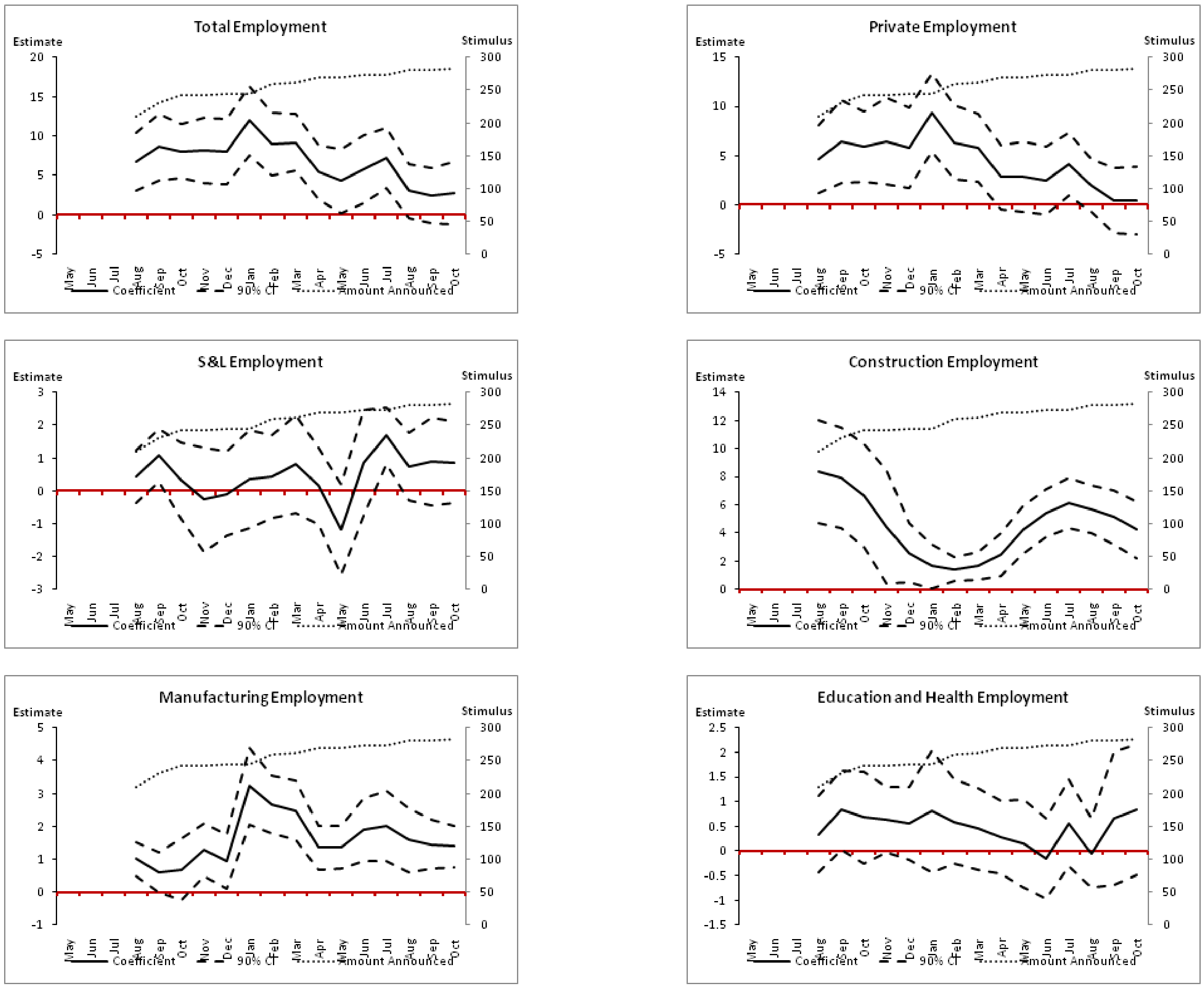


Figure 21

Coefficients over time, Obligations by All Agencies

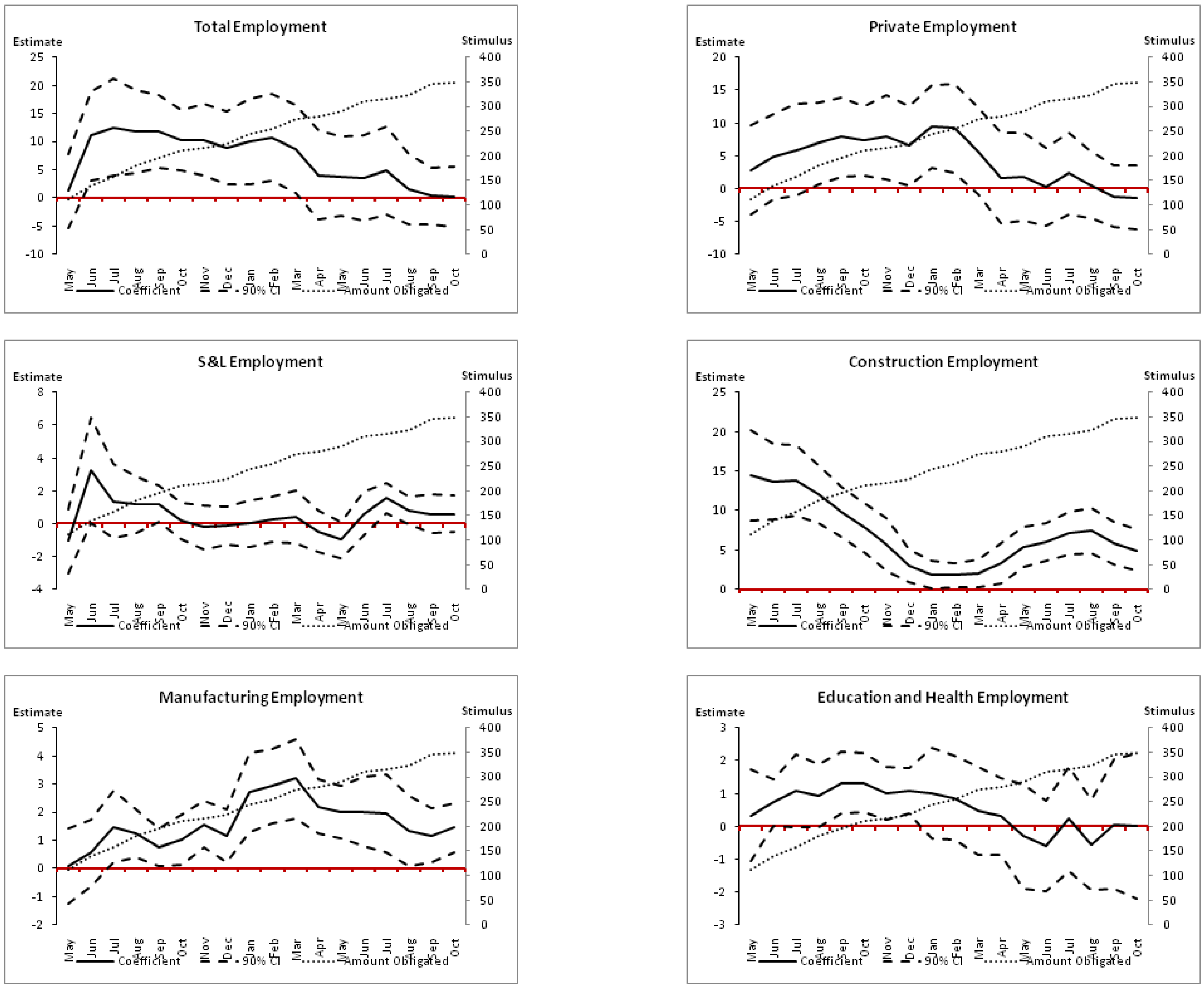


Figure 22

Reduced Form Coefficients 2009, Center for American Progress Instrument

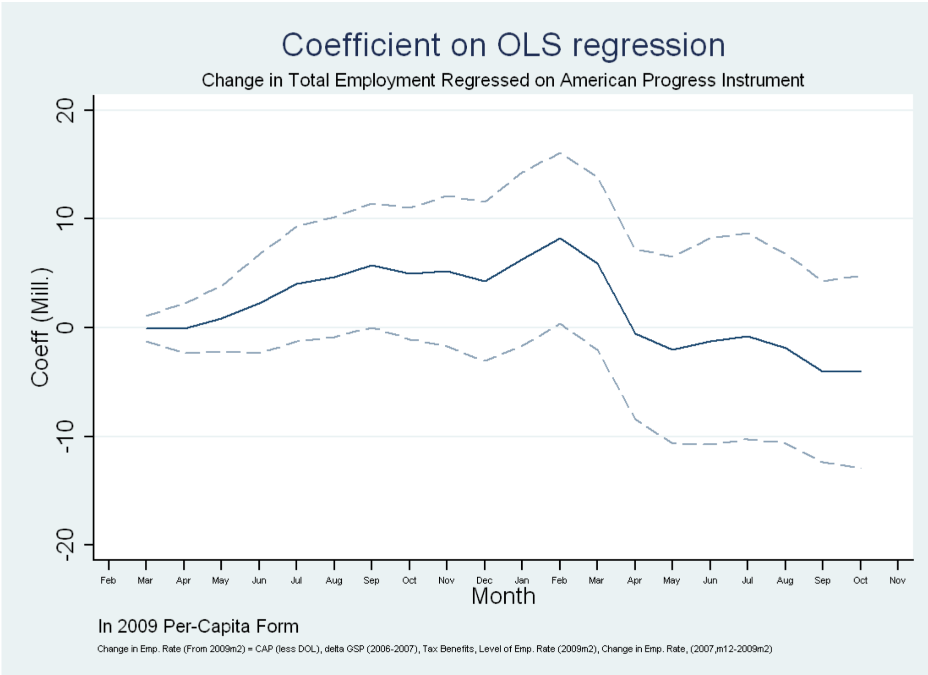


Figure 23

Reduced Form Coefficients 2009, Wall Street Journal Instrument

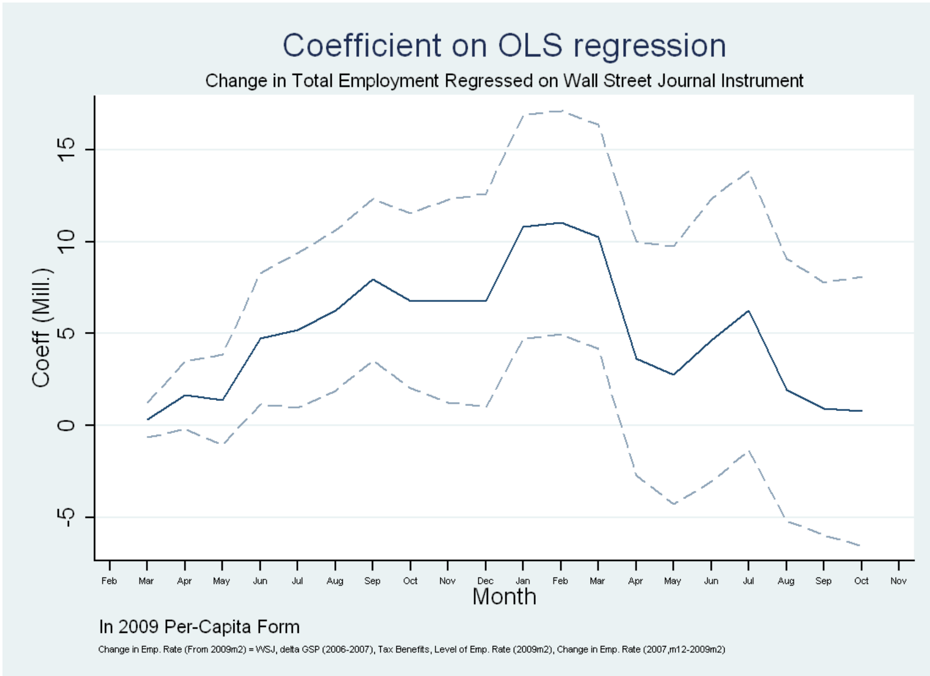


Figure 24

OLS coefficients, Average of both Instruments, 2008 - 2009

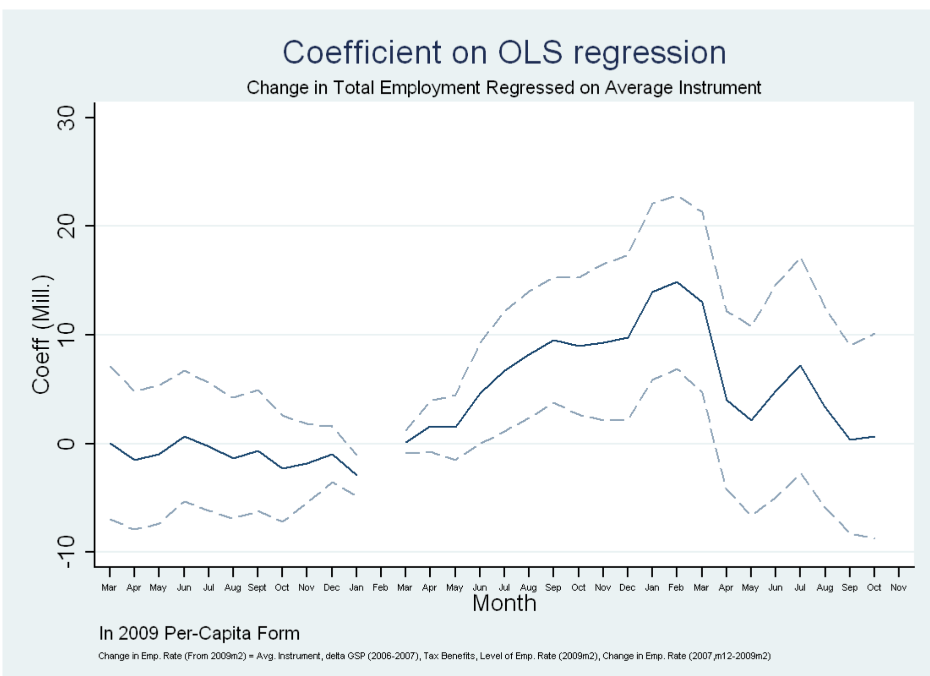


Figure 25

OLS coefficients, Average of both Instruments

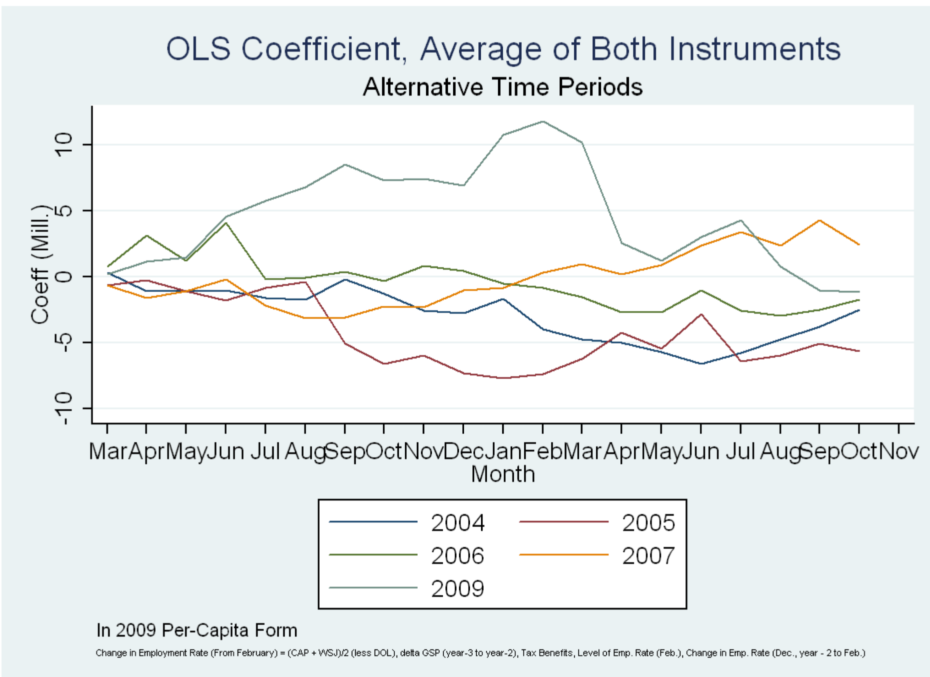


Figure 26

Coefficient of Variation

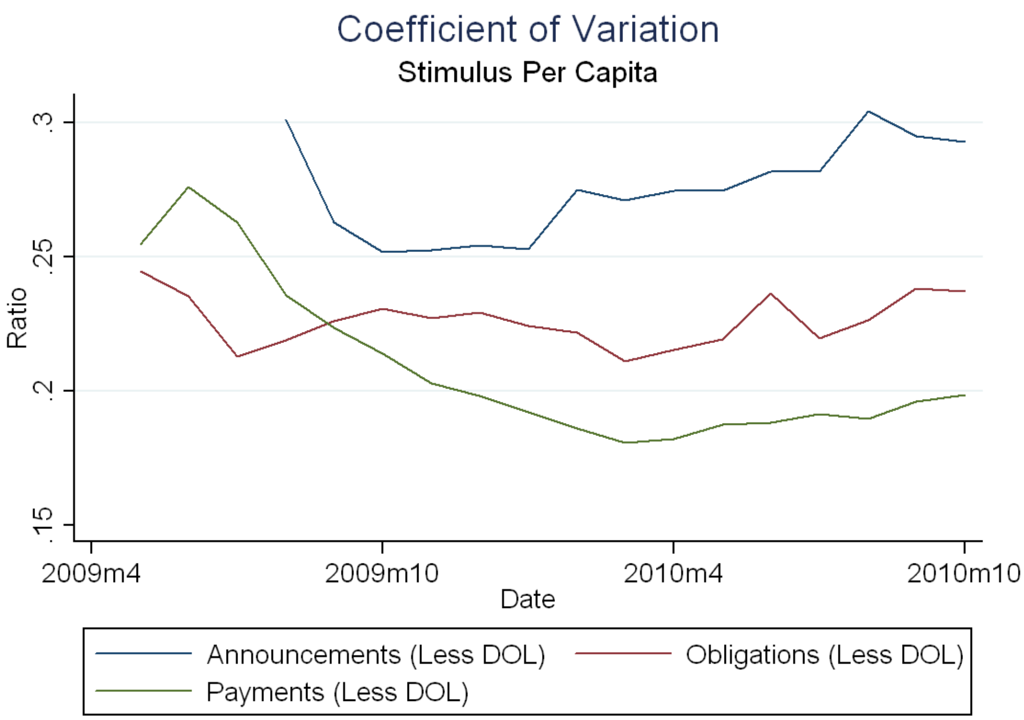


Figure 27

Coefficients over time, Announcements by DOT + Other

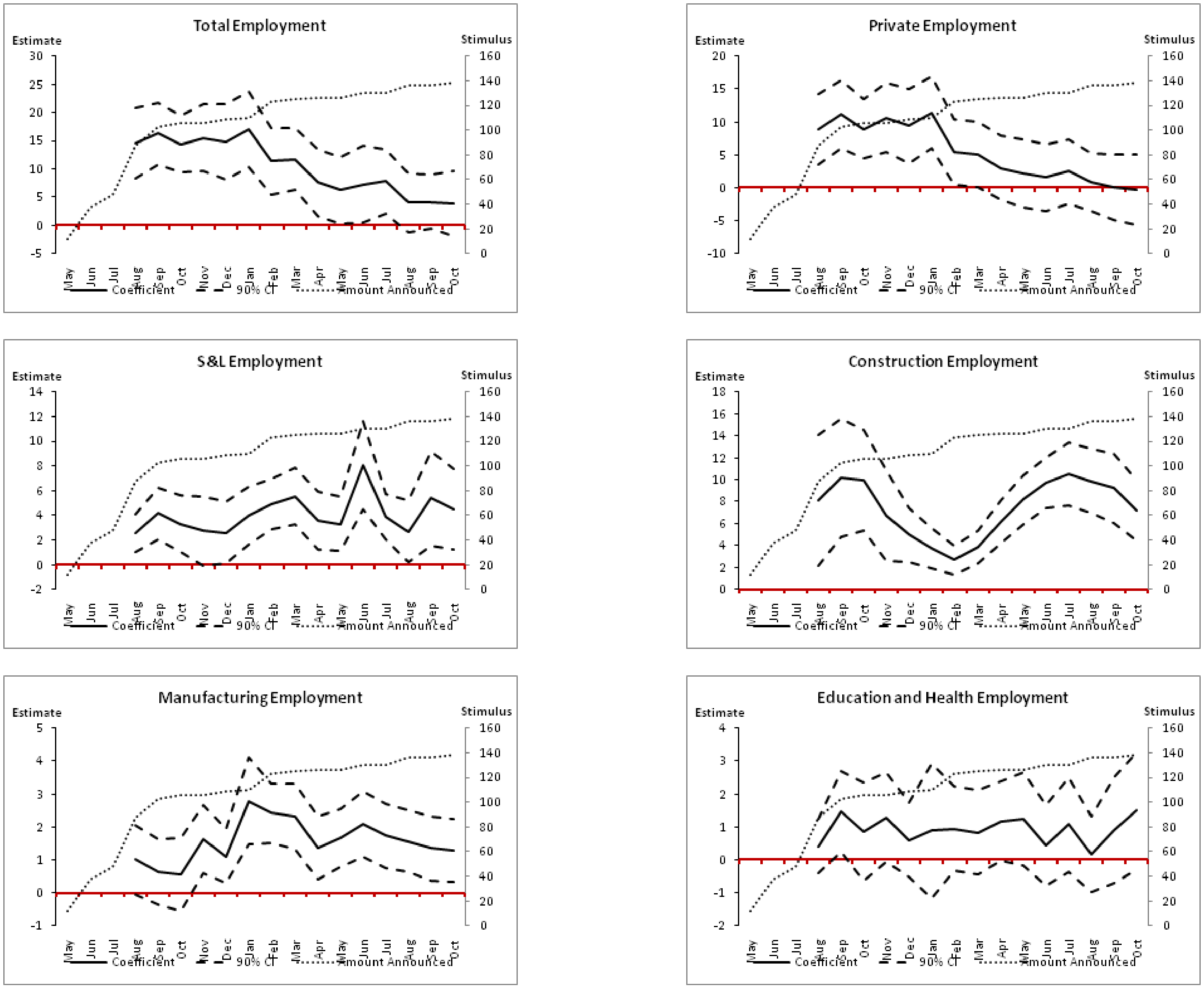


Figure 28

Coefficients over time, Obligations by DOT + Other

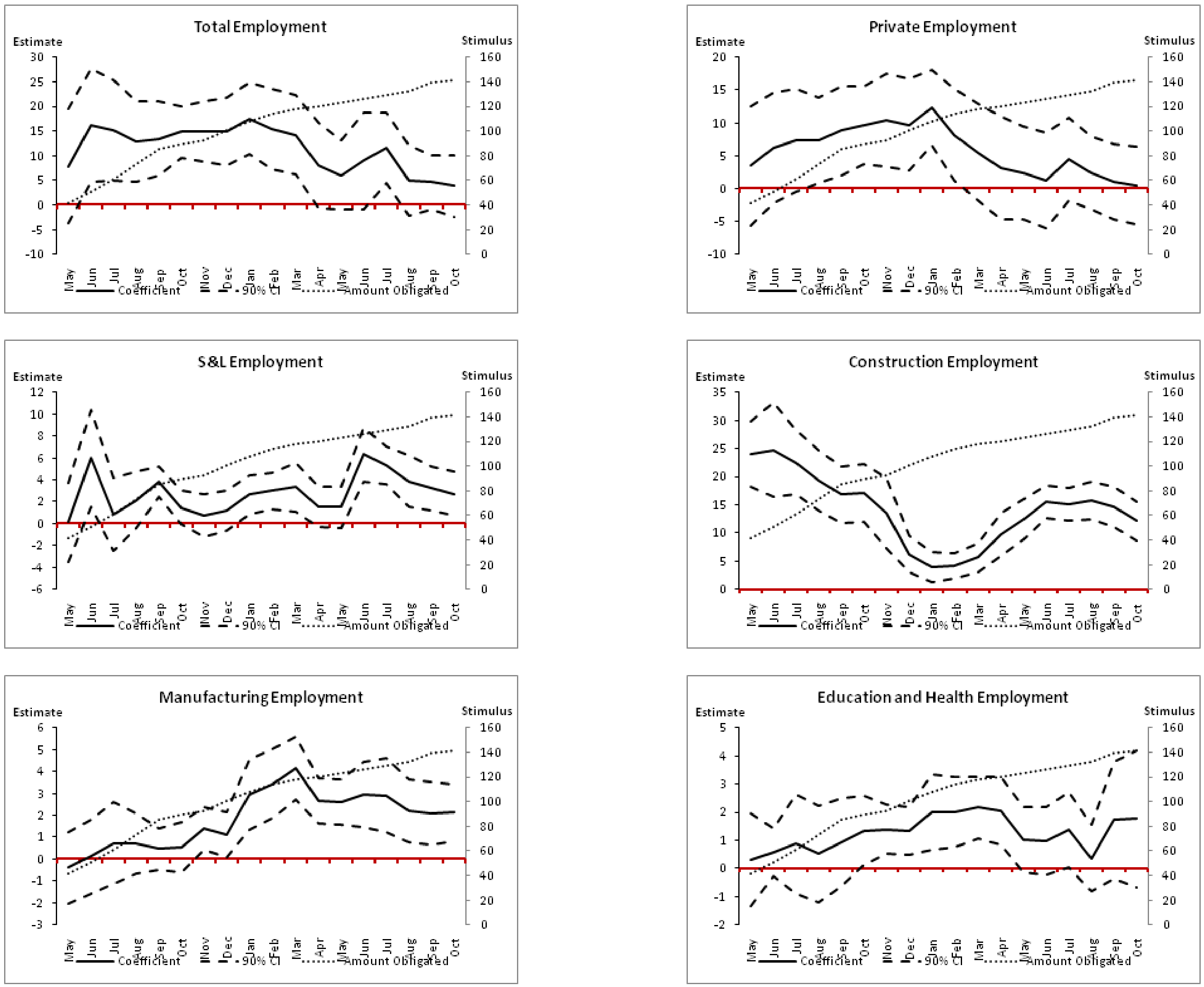


Figure 29

Coefficients over time, Announcements by HHS

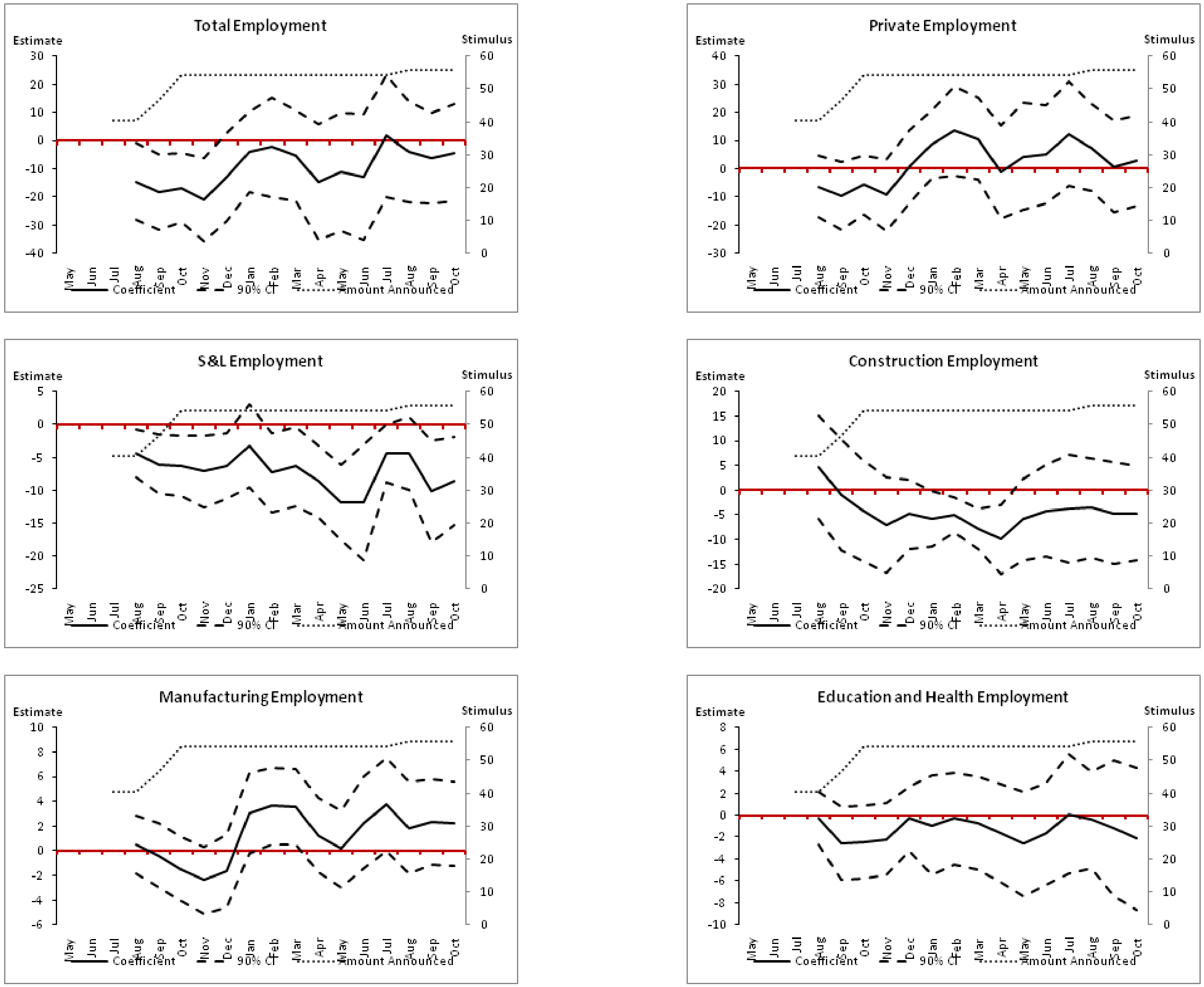
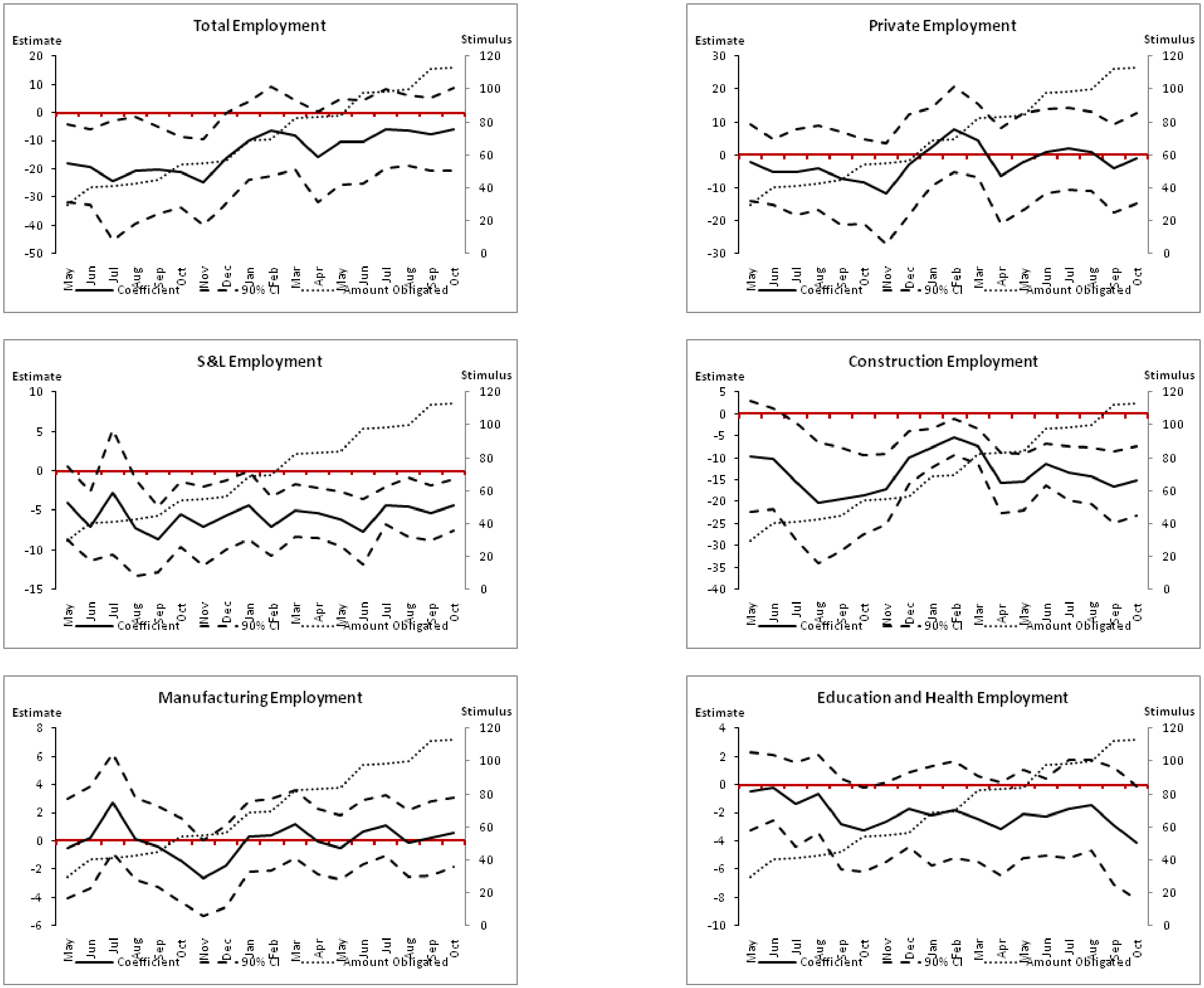


Figure 30

Coefficients over time, Obligations by HHS



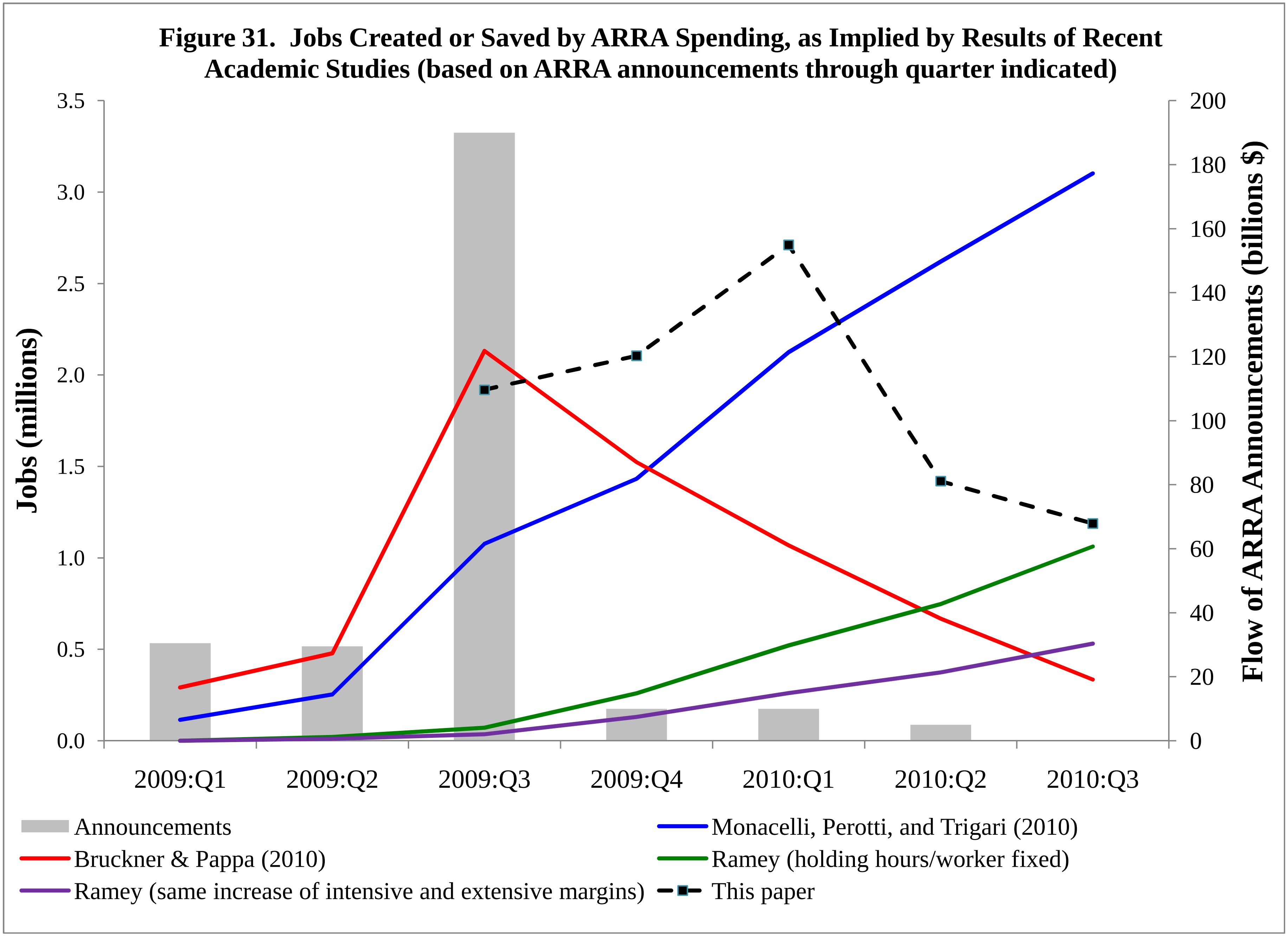


Figure 31. Jobs Created or Saved by ARRA Spending, as Implied by Results of Recent Academic Studies (based on ARRA announcements through quarter indicated)
Jobs (millions)
Flow of ARRA Announcements (billions $)
0.0
0.5
1.0
1.5
2.0
2.5
3.0
3.5
0
20
40
60
80
100
120
140
160
180
200
2009:Q1
2009:Q2
2009:Q3
2009:Q4
2010:Q1
2010:Q2
2010:Q3
Announcements
Monacelli, Perotti, and Trigari (2010)
Bruckner & Pappa (2010)
Ramey (holding hours/worker fixed)
Ramey (same increase of intensive and extensive margins)
This paper

Figure 32. Jobs Created or Saved by ARRA Spending, as Implied by Results of Recent Academic Studies (based on ARRA obligations through quarter indicated)