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STATES AS AGENTS

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Macro Fiscal Policy in Economic Unions: States as Agents

by

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ABSTRACT

An important component of the American Recovery and Reinvestment Act's (ARRA's) \$796 billion proposed stimulus budget was \$318 billion in fiscal assistance to state and local governments, yet we have no precise estimates of the impact of such assistance on the macroeconomy. In evaluating ARRA, both the Council of Economic Advisors (CEA) and the Congressional Budget Office (CBO) used instead the impacts of direct federal spending and tax relief. These estimates miss the role of states as agents. We provide estimates of aid's multiplier effects allowing explicitly for state behavior, first from an SVAR analysis separating federal aid from federal tax relief, second from a narrative analysis using the political record for unanticipated federal aid programs, and third from constructed macroeconomic estimates implied by an estimated model of state governments' fiscal choices. We reach three conclusions. First, federal transfers to state and local governments are less stimulative than transfers to households and firms. Second, federal aid for welfare spending is more stimulative than is general purpose aid. Third, an estimated model of state government fiscal behavior provides a microeconomic foundation for the observed macroeconomic impacts of aid.

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I. Introduction

The Great Recession and efforts by the federal government through the American Recovery and Reinvestment Act (ARRA) to stimulate the economy have returned fiscal policy, and in particular the role of state and local governments in such policies, to the center of macroeconomic policymaking. Government spending through ARRA, which was passed within the first two months of President Obama's administration, has now reached over \$796 billion to stimulate the private economy toward full employment: \$381 billion as federal tax relief and expanded unemployment compensation, \$98 billion as direct federal government spending, and \$318 billion as intergovernmental transfers to state and local governments for education spending (\$93 billion), infrastructure spending (\$70 billion), financing of lower-income housing (\$6 billion), lower-income Medicaid funding (\$101 billion), and low-income assistance (\$48 billion).¹ The striking features of this legislation have been its scale, clearly the largest fiscal stimulus since the Great Depression, and its reliance upon intergovernmental transfers to state and local governments for implementing central government macroeconomic fiscal policy.

Central to our analysis is the view that U.S. states act as fiscal agents of the central government and that intergovernmental transfers from the center to member states will be the primary policy instrument for control of their fiscal choices. Large infusions of central government aid such as ARRA can increase aggregate state government spending and, to the extent aid leads to local tax relief, increase private consumption and investment as well. The resulting increases in public and private spending may then have positive consequences for aggregate job and income growth. Unfortunately, at the time ARRA policies were being formulated and implemented, there were no accepted estimates as to how the state and local sector would react to such assistance or how such aid would impact the private economy. The Council of Economic Advisers (Romer and Bernstein, 2009) and the Congressional Budget Office (CBO Report, 2010) both assumed the economy's responses to state and local aid would be similar to those for federal direct purchases and tax cuts. Our analysis here suggests this assumption may have been in error, and we seek to provide

¹ See recovery.gov/arra/Transparency/fundingoverview/pages/fundingbreakdown.aspx

the separate estimates needed to evaluate these policies.²

Recent analyses of the local economic impact of ARRA aid have also sought to fill the gap; see Wilson (2012), Feyrer and Sacerdote (2011), Chodorow-Reich, et al. (2012), and Conley and Dupor (2013). These studies relate changes in state or county employment for the one year after the passage of ARRA to the level of ARRA transfers received by the coincident state or local government, or their contractors, in the previous fiscal year. Each finds a significant positive impact on local private and public employment, with the strongest effects coming from ARRA support for state Medicaid payments.³

These local economy results are valuable but leave three questions unanswered. First, while there are measured gains for the local economy receiving assistance, do those gains come at the expense of, or alternatively might they enhance, the job or income gains of neighboring economies? Recent research, such as that by Beetsma and Giuliodori (2011), Auerbach and Gorodnichenko (2013), and Hebous and Zimmermann (2013) studying aggregate interdependencies among EU economies and by Carlino and Inman (2013) studying economic interdependencies between U.S. states, finds significant spillovers across economic neighbors. Second, because of limitations of the data, the local impact studies can only reveal economic changes for, at most, one year after ARRA spending. Over time, do the positive effects stimulate additional economic activity, do they disappear, or do they perhaps turn negative? Third, the local impact studies estimate the effects of ARRA spending as it is spent. But might the state or local government have used ARRA aid to replace its own planned allocations? Federal aid is fungible within state and local government budgets, and the impact studies will miss the economic consequences of subsequent budgetary adjustments; see Craig and Inman (1982) for general fungibility and Conley and Dupor (2013) for fungibility related to ARRA.

We address each of these questions and provide the first fully integrated analysis of the

² However, both studies did do sensitivity analyses bounding our estimates.

³ Though not our focus here, the central government can also impact local economies through its own spending and tax subsidies paid directly to households and firms. For recent analyses of the effectiveness of these direct spending policies, see Nakamura and Steinsson (2014), Cohen, Coval, and Mallory (2011), Fishback and Kachanovskaya (2010), and Suarez-Serrato and Wingender (2011).

macroeconomic effects of intergovernmental transfers. We estimate the general equilibrium consequences of such assistance over time, allowing for behavioral responses by state and local governments. We do so by first extending the structural vector autoregression (SVAR) approach of Blanchard and Perotti (2002). In contrast to their original specification that combines transfers to state and local governments with transfers to households and firms, we separate out intergovernmental aid as its own policy. In doing so, we drop the implicit assumption that state and local governments are perfect agents for households and firms. We find that separating federal government transfers to households and firms from federal transfers to governments is important. Although transfers to households have an implied peak multiplier of 3.7, the implied peak multiplier that for aggregate intergovernmental transfers is never greater than .8. It is also important to further disaggregate federal transfers to governments into those for general government support (“project aid”) and those targeted for payments to lower income households (“welfare aid”). Estimated GDP multipliers for welfare aid range from 1.6 to 2.3 and are statistically significant; GDP multipliers for project aid range from 0 to 1.0 and are often statistically insignificant. Our SVAR results are robust to a variety of alternative specifications, identification strategies, and sample periods.

Identification of the effects of fiscal policy on the macroeconomy using the SVAR approach follows from a predetermined specification of the timing of policies and their impacts on the economy. While our results are robust to alternative orderings for how policies influence the economy, we also employ the narrative approach to identification as proposed by Ramey (2011a) and Romer and Romer (2010). Separately, we develop a narrative history for federal aid to state and local governments in Carlino and Inman (2014) that identifies the 23 separate policy events of new (arguably exogenous) federal aid programs, or their discontinuation, over our sample period. Nineteen of the events are for project aid; four are for welfare aid. Only the project aid multipliers can be precisely identified, and their estimated values and the time path of impacts are consistent with the SVAR results.

The observed effects of federal aid on the macroeconomy emerge from the microeconomic behavior of the recipients of aid, in our case state and local governments. Money is given to states, and states as “agents” of federal policy in turn allocate those funds to households and firms in the private economy. The importance of this agency relationship has been emphasized originally by

Gramlich (1978, 1979) in his evaluation of federal economic stimulus programs in 1977 and more recently by Taylor (2011) in his evaluation of ARRA. In this paper we estimate state responses to federal project and welfare aid over the sample period 1979-2010 and find the estimated responses rationalize the observed macroeconomic multipliers. States save about half of federal project aid and spend all of matching welfare aid on lower-income assistance and tax relief for general taxpayers. As a consequence, welfare aid has a stronger and longer-lasting impact on the private economy than does project aid or general fiscal relief to state and local governments.

Section II presents the framework for our analysis, outlining the SVAR approach and the narrative approach as alternative identification strategies for estimating the impact of federal aid on the aggregate economy. Section III presents our primary results and then tests for the sensitivity of these results against alternative identification assumptions and sample periods. Section IV provides the microeconomic foundation for our macro analysis by specifying and estimating a model of how state governments might allocate federal assistance to spending, tax relief, and net government savings. Section V applies our estimates to evaluate the relative performance of ARRA-style policies as stimuli for income growth following a recession. Section VI concludes.

II. Estimating the Macroeconomic Effects of Federal Aid

To obtain estimates of the effects of federal aid on the aggregate private economy, we first extend the structural vector autoregression (SVAR) analysis pioneered by Blanchard and Perotti (2002) to explicitly allow for the effects of federal intergovernmental aid and then, as an alternative identification strategy, develop a narrative record of new and abolished federal aid policies to provide a separate time series for exogenous changes in federal intergovernmental aid. Both approaches begin by separating federal transfers to households and firms from federal transfers to the state and local government (SL) sector. Our analysis explicitly recognizes the possible separate effects of these approaches on the aggregate economy.

We first define federal net revenues per capita (“taxation,” denoted R) as federal taxes and fees less federal transfers paid directly to households and firms. We then define federal transfers to state and local governments (“aid,” denoted A) as all federal transfers to the SL sector, *including* federal aid to support income transfers and public services for lower-income households (most

important, health-care services). Total federal aid is then separated into its two main components: general revenue sharing plus project aid (denoted AP) and public welfare plus Medicaid aid (denoted AW): $A = AP + AW$.⁴ This distinction will prove important in our empirical results. The third fiscal policy is direct federal purchases of goods and services (“spending,” denoted G).⁵ Not explicitly modeled, but implicit in our aggregate analysis, are state and local government purchases and state and local net revenues.

Figure 1 shows the time pattern of total federal aid per capita (A), federal project aid (AP), and federal welfare aid (AW) over the postwar period from 1947:1 to 2010:3. The shaded bands indicate periods of economic recessions as dated by the NBER. Real federal aid per capita (2005 dollars) has risen from \$47/person in 1947 to \$1,787/person in 2009:1, where 2009:1 is the last date before the implementation of President Obama’s ARRA fiscal stimulus. ARRA spending over the following year increased aggregate federal aid per capita by \$294/person. For comparison, Figure 2 shows the time path of federal purchases of goods and services (G) and of federal net revenues (R). During the ARRA policy’s first year of operation, federal purchases rose by \$506/person and federal net revenues fell by \$374/person.

A. SVAR Specification and Identification: We begin by estimating a four-variable SVAR for the impact of federal net revenues (R), federal government purchases (G), and aggregate federal aid (A) on national GDP (Y). The SVAR analysis begins with the estimation of a reduced form VAR specified as:

⁴ General revenue sharing/project aid (AP) includes general revenue sharing, elementary and secondary education aid, model cities and urban renewal aid, transportation aid, all federal aid programs meant to assist SL government finances after recessions (including ARRA’s “stability aid”), and tobacco settlement payments; see Carlino and Inman (2014) for full details. The tobacco settlement payments are viewed as *de facto* “federal aid” financed by a “tax” on tobacco companies; see Singhal (2008). The two federal aid programs included in welfare aid (AW) are Aid to Families with Dependent Children (AFDC) and Medicaid. When measuring AP and AW, we specifically allow for the change in funding structure under the Personal Responsibility and Work Opportunity Reconciliation Act of 1996 (PRWORA). PRWORA transformed funding for public welfare from a matching aid program – Aid to Families with Dependent Children (AFDC) – to an unconstrained, lump-sum transfer – Temporary Assistance for Needy Families (TANF). When specifying AW and AP, we remove AFDC from AW and add TANF to AP; see Table 1 and Carlino and Inman (2014).

⁵ Federal purchases of goods and services are specified as federal government purchases of goods and services plus federal defense spending plus federal gross investment.

$$\mathbf{Z}_t = C(L,q) \cdot \mathbf{Z}_{t-1} + \mathbf{u}_t, \text{ where} \quad (1)$$

$$\mathbf{Z}_t' = [r_t, g_t, a_t, y_t],$$

and where r_t is the log of federal net household revenues (R), g_t is the log of federal government purchases (G), a_t is the log of aggregate federal aid (A), and y_t is the log of GDP (Y), each measured at quarterly intervals and measured as real (2005) dollars per capita. Also included in the initial VAR are the trend variables time and time squared, and an indicator variable for “deep recessions” (= 1, if the national rate of unemployment exceeds 8 percent).

The lag structure $C(L,q)$ is a 4 by 4 matrix of three-quarter distributed lag polynomials, and \mathbf{u}_t is a 4 by 1 vector of reduced form innovations, specified as $\mathbf{u}_t' = [u_t^r, u_t^{gt}, u_t^a, u_t^y]$. The three-quarter lag allows for seasonal patterns in the responses of fiscal variables to GDP. The AIC test statistic indicates that three-quarter lags of the endogenous variables are optimal; three-quarter lags are also sufficient to remove serial correlation from the residuals.

To recover the exogenous structural shocks to net federal government revenues, federal government purchases, and federal aid, denoted as v_t^r , v_t^{gt} , and v_t^a , respectively, we follow the methodology of Blanchard and Perotti. First, we take advantage of quarterly variation in our data and impose the restriction that discretionary changes in fiscal policy take at least one quarter to respond to changes in GDP. Thus, the contemporaneous discretionary response of net revenues, purchases, or aid to GDP is zero.

Second, we impose constraints on the ordering of discretionary policy changes, requiring discretionary revenues to be set prior to discretionary spending for either purchases or aid, and then within spending, that purchases (largely defense spending) are set prior to federal aid to the SL sector. The priority of discretionary revenue changes is a result of the institutional rules established by the Congressional Budget and Impoundment Control Act of 1974 and subsequent legislation aimed at establishing a revenue-driven budget constraint for federal fiscal policies; see Keith and Schick (2004) and Auerbach (2003). Formally, federal net revenues are seen to Granger-cause federal purchases.⁶ We assume discretionary government purchases predetermine spending for

⁶ For the full sample period (1947:1 to 2010:3), we reject the null hypothesis that revenues do not Granger-cause spending ($\chi^2 = 14.01$), but we cannot reject the null hypothesis that spending does not Granger-cause revenues ($\chi^2 = 3.84$).

federal aid as, politically, defense spending “trumps” discretionary domestic spending. However, we cannot rule out the possibility that domestic spending may hold priority over defense spending by a Granger-causality test.⁷ As a precaution, however, we redo our analysis with federal purchases predating revenues in the policy process and report those results as a robustness check on our core analyses.⁸

Third, we identify the built-in responses of federal tax and transfer policies and federal purchases to contemporaneous changes in GDP following the specifications proposed originally by Blanchard and Perotti (2002). The built-in contemporaneous elasticity of federal net household revenues to changes in GDP is set equal to 2.08, while the built-in contemporaneous elasticity of government purchases with respect to GDP is set at zero, reflecting the largely contractual nature of these outlays. These estimates come from outside the model and represent the automatic stabilizers for federal net household revenues and government purchases, respectively. We test the sensitivity of our results to alternative estimates of the contemporaneous revenue elasticity, ranging from 1.6 based on direct estimates for the U.S. economy by Follette and Lutz (2010) to 3.0 based upon direct SVAR estimates by Mountford and Uhlig (2009) and Mertens and Ravn (2013). An estimate of the contemporaneous built-in effects of GDP on federal aid to the SL sector is obtained from a panel regression for the period from 1970-2010 relating the log of total federal-to-state aid to the log of gross state product (GSP) conditional on year and state fixed effects; our preferred estimate is -.35 (s.e. = .10).

The vector of reduced form residuals is specified as a linear combination of structural shocks. After subtracting all contemporaneous responses among the reduced form residuals, net residuals ($u^{C,*}$) can be specified as:

$$u^{C,r}_t \equiv (u^r_t - \alpha_{r,y} \bullet u^y_t) = \beta_{r,g} \bullet v^{gt} + \beta_{r,a} \bullet v^a_t + v^r_t$$

⁷ For the full sample period (1947:1 to 2010:3), we cannot reject either the null hypothesis that federal aid Granger-causes federal purchases ($\chi^2 = 14.26$) or the null hypothesis that federal purchases Granger-cause federal aid.

⁸ We have tested for the sensitivity of our core results to the alternative ordering within spending that places domestic spending politically “prior” to defense spending, and the results remain essentially the same.

$$\begin{aligned}
\mathbf{u}^{C,gt}_t &\equiv (\mathbf{u}^g_t - \alpha_{g,y} \bullet \mathbf{u}^y_t) = \beta_{g,r} \bullet \mathbf{v}^{rt} + \beta_{g,a} \bullet \mathbf{v}^a_t + \mathbf{v}^{gt}_t, \\
\mathbf{u}^{C,a}_t &\equiv (\mathbf{u}^a_t - \alpha_{a,y} \bullet \mathbf{u}^y_t) = \beta_{a,g} \bullet \mathbf{v}^{gt} + \beta_{a,r} \bullet \mathbf{v}^r_t + \mathbf{v}^a_t, \\
\mathbf{u}^{C,y}_t &\equiv (\mathbf{u}^y_t - \alpha_{y,r} \bullet \mathbf{u}^r_t - \alpha_{y,g} \bullet \mathbf{u}^{gt} - \alpha_{y,a} \bullet \mathbf{u}^a_t) = \mathbf{v}^y_t,
\end{aligned} \tag{2}$$

where each coefficient $\alpha_{p,y}$ specifies the built-in (programmatic) elasticity of fiscal policy ($p = r, g, a$) to GDP and each coefficient $\alpha_{y,p}$ the response of GDP to contemporaneous (including exogenous) changes in each fiscal policy ($p = r, g, a$).

Our initial estimates set $\alpha_{r,y} = 2.08$, $\alpha_{g,y} = 0$, and $\alpha_{a,y} = -0.35$. From the identification strategy for the timing of fiscal policy decision-making, we set $\beta_{r,g} = \beta_{r,a} = 0$ from the priority of federal revenues over spending and $\beta_{g,a} = 0$ from the priority of government purchases over SL aid. With these restrictions, we can identify the remaining six free parameters ($\beta_{g,r}, \beta_{a,g}, \beta_{a,r}, \alpha_{y,r}, \alpha_{y,g}, \alpha_{y,a}$) and compute a distribution for the exogenous structural errors, $\mathbf{v}'_t = [\mathbf{v}^r_t, \mathbf{v}^{gt}_t, \mathbf{v}^a_t, \mathbf{v}^y_t]$. Returning to the estimated reduced-form VAR specification, we can then compute impulse response functions for GDP following exogenous shocks in fiscal policy, and from the impulse response functions, we estimate multipliers evaluated at the sample means for GDP and each policy.

A similar specification and identification strategy applies when the analysis is extended to a five-variable SVAR. Now the vector of policies and GDP is specified as $\mathbf{Z}' = [r_t, g_t, aw_t, ap_t, y_t]$, where aw_t is the log of federal matching aid for state-funded transfers to lower-income households (AW) and ap_t is the log of federal project aid for general state and local government spending or tax relief (AP). The corresponding vector of exogenous residuals to be estimated is now $\mathbf{v}'_t = [\mathbf{v}^r_t, \mathbf{v}^{gt}_t, \mathbf{v}^{aw}_t, \mathbf{v}^{ap}_t, \mathbf{v}^y_t]$. The five-variable SVAR is specified as:

$$\begin{aligned}
\mathbf{u}^{C,r}_t &\equiv (\mathbf{u}^r_t - \alpha_{r,y} \bullet \mathbf{u}^y_t) = \beta_{r,g} \bullet \mathbf{v}^{gt} + \beta_{r,aw} \bullet \mathbf{v}^{aw}_t + \beta_{r,ap} \bullet \mathbf{v}^{ap}_t + \mathbf{v}^r_t, \\
\mathbf{u}^{C,gt}_t &\equiv (\mathbf{u}^{gt}_t - \alpha_{g,y} \bullet \mathbf{u}^y_t) = \beta_{g,r} \bullet \mathbf{v}^r_t + \beta_{g,aw} \bullet \mathbf{v}^{aw}_t + \beta_{g,ap} \bullet \mathbf{v}^{ap}_t + \mathbf{v}^{gt}_t, \\
\mathbf{u}^{C,aw}_t &\equiv (\mathbf{u}^{aw}_t - \alpha_{aw,y} \bullet \mathbf{u}^y_t) = \beta_{aw,r} \bullet \mathbf{v}^r_t + \beta_{aw,g} \bullet \mathbf{v}^{gt} + \beta_{aw,ap} \bullet \mathbf{v}^{ap}_t + \mathbf{v}^{aw}_t, \\
\mathbf{u}^{C,ap}_t &\equiv (\mathbf{u}^{ap}_t - \alpha_{ap,y} \bullet \mathbf{u}^y_t) = \beta_{ap,r} \bullet \mathbf{v}^r_t + \beta_{ap,g} \bullet \mathbf{v}^{gt} + \beta_{ap,aw} \bullet \mathbf{v}^{aw}_t + \mathbf{v}^{ap}_t, \\
\mathbf{u}^{C,y}_t &\equiv (\mathbf{u}^y_t - \alpha_{y,r} \bullet \mathbf{u}^r_t - \alpha_{y,g} \bullet \mathbf{u}^{gt} - \alpha_{y,aw} \bullet \mathbf{u}^{aw}_t - \alpha_{y,ap} \bullet \mathbf{u}^{ap}_t) = \mathbf{v}^y_t,
\end{aligned} \tag{3}$$

where we again specify $\alpha_{r,y} = 2.08$ and $\alpha_{g,y} = 0$; $\beta_{r,g} = \beta_{r,aw} = \beta_{r,ap} = 0$ by the budgetary priority of revenue over spending; and $\beta_{g,aw} = \beta_{g,ap} = 0$ from the budgetary priority of government purchases (largely defense) over grants to the SL sector. Initially we specify formula welfare aid as prior in budgeting to discretionary federal program aid and set $\beta_{aw,ap} = 0$. For the five-variable SVAR, we use

estimates of the contemporaneous built-in effects of GDP on the two components of federal aid, again obtained from state panel regressions now relating the log of welfare and program aid to the log of gross state product (GSP), conditional on year and state fixed effects. Here the preferred estimates are $\alpha_{ap,y} = -.40$ (s.e. = .15) and $\alpha_{aw,y} = -.19$ (s.e. = .07). With these restrictions, we can identify the 10 free parameters ($\beta_{g,r}, \beta_{aw,r}, \beta_{aw,g}, \beta_{ap,r}, \beta_{ap,g}, \beta_{ap,aw}, \alpha_{y,r}, \alpha_{y,g}, \alpha_{y,aw}, \alpha_{y,ap}$) and the resulting vector of exogenous residuals $\mathbf{v}_t' = [v_t^r, v_t^{gt}, v_t^{aw}, v_t^{ap}, v_t^y]$. Again, we compute impulse response functions for GDP following exogenous shocks in fiscal policy, and from these impulse response functions we estimate multipliers evaluated at the sample means for GDP and each policy, now including those for welfare aid (AW) and program aid (AP).

B. Narrative Specification and Identification: The SVAR approach to identification of exogenous policy shocks is model dependent and relies upon an *a priori* specification for the timing of policy choices. As an alternative identification strategy, we also employ the narrative approach as developed by Ramey and Shapiro (1998), which is independent of the econometrician’s model but rather relies upon political histories of the fiscal policies to identify the level and timing of exogenous innovations to fiscal policy. The important methodological issue is specifying when the private sector, and for us the SL sector as well, is likely to have anticipated the implementation of the new federal policy; see Leeper, Walker, and Yang (2010) and Ramey (2011a).

We have developed the narrative history for federal aid to state and local governments from 1947 to the 2009 adoption of the ARRA; see Carlino and Inman (2014). Table 1 provides a summary of the narrative dates for all important new federal aid programs since 1947.⁹ Our narrative approach identifies 23 separate narrative events, 20 in which federal aid is increased and three in which aid is reduced. Two decisions are required to specify the narrative aid event: first, the level of new aid and, second, the timing of the innovation.

We specify the level of aid available from the new program as the amount appropriated for

⁹ The narrative analysis does not include changes in the federal tax code that impact state and local government revenues, most notably through changes in the federal definition of taxable income used by many states in setting their own tax base. The reason for excluding these policy changes is that the resulting changes in state revenues arise from transfers between residents *within* a state. Our focus here is on changes in revenues paid to states from those *outside* the state and viewed by residents – Ricardian arguments aside – as exogenous increases in state and local governments’ budget constraints. For an analysis of the impact of *within* state revenue “shocks,” see Ladd (1993).

the first full fiscal year of the program's implementation; beyond the initial year, aid is assumed to be anticipated. Because the municipal bond market will lend only against pledged federal assistance within the fiscal year (via Revenue Anticipation Notes, or RANs),¹⁰ we do not discount future aid except for two innovations. The first is the legislated phased reduction in federal funding of general revenue sharing from 1981 to 1983 for state governments and from 1984 to 1987 for local governments. The second is the tobacco settlement revenue specified in the Tobacco Master Settlement Agreement with U.S. tobacco companies beginning in 1998:4 and to be paid through 2025. Although these revenues are paid directly by tobacco firms to states, we treat them as *de facto* exogenous assistance by the federal government; see Singhal (2008). Further, these revenue promises are binding contracts between each state and the firms, and for this reason the bond markets have been willing to lend to the states against these promises. Thus, the discounted present value of these promises is used as our measure for the level of this assistance; see Table 1.

There are three dates on which state and local officials might begin budgeting in anticipation of new federal assistance: (i) the date of introduction of the legislation; (ii) the date of passage by Congress if the legislation had been introduced by the President or the date of signing by the President if there had been a risk of a presidential veto; or (iii) the date of first federal funding. Carlino and Inman (2014) provide all three dates. Following Mertens and Ravn (2012), we use only the first date that money was actually sent to the SL sector, with one institutional adjustment. We allow for the fact that significant changes in state or local budgets, whether a spending increase or a tax reduction, are typically not allowed by state law once the budget has been adopted at the beginning of a fiscal year. This means that an aid innovation that occurs in the middle of a state's fiscal year, for example in calendar Q1, cannot be fully incorporated into state fiscal decisions until the beginning of the next fiscal year, typically beginning in Q3. All shocks are annualized. A first-stage *F*-test that real (2005 dollars) per capita narrative aid is a strong instrument for predicting aggregate federal aid per capita

¹⁰ State and local governments' own savings are insufficient for these governments to do their own "borrowing" in anticipation of future aid. Holtz-Eakin, Rosen, and Tilly (1994) provide evidence that the aggregate state and local government sector is credit constrained on the current accounts.

has a test statistic of 10.64.¹¹

Estimation of federal aid's impact on GDP using a narrative identification follows the VAR approach of Ramey (2011a), ordering the aggregate narrative aid variable first, followed by federal net revenues excluding federal aid, federal purchases, and finally GDP. All variables are now specified in levels to allow for negative innovations. A quadratic time trend and three lags are also included in the specification.

We offer four alternate specifications for the narrative aid variable as robustness checks for our core results. *First*, there are four narrative events (indicated by an * in Table 1) where aid policies were approved following a recession. In the first three cases, project aid was either approved or allocated at least five quarters after the end of the recession. Our estimates of the optimal lag structure for the influence of GDP on fiscal policy and GDP on itself was three quarters, so arguably these policies are exogenous events to the performance of the economy following the aid event. Further, we are careful to specify the “policy surprise” as only that aid in excess of any anticipated federal bailout in response to state deficits following the recession.¹² The one exception was ARRA, which was approved in less than one month after the inauguration of President Obama. The political history suggests that the use of intergovernmental aid, the magnitude of the assistance, and ARRA's quick approval were “surprises” to both the SL sector and the private economy; see Suskind (2011). Still, as a precaution we reestimate our narrative specification excluding these policies. *Second*, there are only four narrative events that apply to matching welfare aid: the introduction of Medicaid

¹¹ Also included in the federal aid regressions with narrative aid are three lag values of net revenues, government purchases, and federal aid. The first-stage *F*-test is for contribution of narrative aid as an explanatory variable for contemporaneous federal aid. The sample period is 1960:1 to 2010:3, the primary period for our analysis.

¹² A conclusion also supported by the GAO; see GAO Report 04-736-R. The one exception to this sequencing of recessions and policies is the passage of ARRA, approved in February 2009. The end of the 2007-2009 recession is dated as December 2009. Project aid (AP) was not received until the third quarter of 2010. Still it is possible that state and local government officials would have anticipated these policy responses by Congress and begun a budget adjustment before aid was to be received. We therefore allow for *anticipated* federal aid under each policy by specifying narrative aid as the difference between federal aid following the recession and anticipated federal aid specified to cover state deficits accumulated during the recessions; see National Association of State Budget Officers (NASBO), *The Fiscal Survey of the States*, recession years. See Carlino and Inman (2014: *Addendum: Is Federal Aid Expected in Downturns?*).

funding beginning in FY 1967, the reform of welfare funding beginning in FY 1997 as a result of the Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA),¹³ and the temporary expansion of Medicaid funding under the Job Growth and Reconciliation Act of 2003 and under ARRA beginning in FY 2009. With only four narrative welfare aid events, we could not separately identify the impact of welfare aid using the narrative strategy. To be sure that these welfare (AW) events were not biasing the estimates of the impact of the 19 narrative project aid (AP) events, we reestimated the impact of narrative aid on GDP, including only the project aid events. *Third*, our core specification allows states to fully react to the federal aid only in the fiscal year following the innovation and for the private sector to react to state policies approved in that year. This specification for the timing of policy effects permits federal aid policies *passed* in Q4 and Q1 (the winter months) to impact the state fiscal policies and the private economy beginning in Q3 (the summer months) of the subsequent state fiscal year. To be certain that the states and the private economy have not already anticipated the funding effects of such policies, thus biasing our narrative estimates, we reestimate the narrative specification using only those programs signed into law in Q2 (the spring) of the state's fiscal year, that is, 90 days or sooner before the implementation of state fiscal policies and the reaction of the private economy. *Fourth*, the Master Settlement Agreement is a singularly large event offering the SL sector close to \$120 billion over 25 years, against which states can, and have, borrowed money. This is not a typical federal aid program. We therefore reestimate the narrative specification for all programs up to the introduction of this agreement.

C. Comparing Identification Strategies: In Figure 3, we compare the quarterly time paths for the dollar-equivalent innovations in lump-sum project aid (Figure 3a) and matching welfare aid (Figure 3b) as estimated from the SVAR (dotted lines) and the narrative (solid line) approaches.¹⁴

¹³ PRWORA did two things to federal funding for state spending on welfare services. First, it removed matching welfare aid for Aid to Families with Dependent Children (AFDC) and then used those released funds to create a new lump-sum project aid program called Temporary Assistance for Needy Families (TANF); see Carlino and Inman (2014). The loss of AFDC matching aid is shown as a decline in narrative welfare aid in Figure 3b in FY 1997, while the gain in TANF project aid is shown as an increase in narrative project aid in Figure 3a, also in FY 1997.

¹⁴ The estimates v^{ap}_t and v^{aw}_t that are used to compute the dollar equivalent change for an unexpected shock to project aid and welfare aid are uncovered from the estimated VAR for the specification reported in Table 3, columns (1)-(4). As specified in the SVAR, v^{ap}_t and v^{aw}_t are measured as innovations

As summarized in Table 1, the narrative approach captures innovations to aid from the legislative introduction of new policies. The SVAR approach captures not only innovations from new policies but also innovations in how governments administer existing aid programs. Even though Congress has *authorized* federal aid spending, the exact timing of expenditures, particularly during any fiscal year, is the responsibility of the President through the decision to *appropriate* spending. In the case of welfare aid, which is allocated to match SL spending, appropriations will also depend on decisions by state governors who determine new recipient eligibility. The simple correlation between SVAR and narrative innovations for project aid is .19, and that between SVAR and narrative innovations for welfare aid is .13. Both correlations are statistically significant at the .95 level of confidence. The two identification strategies provide alternative estimates for how exogenous aid might causally affect GDP; see Favero and Giavazzi (2012). Figure 3 illustrates that both approaches capture each of the new policy innovations as detailed in the legislative record summarized in Table 1, while the SVAR innovations also record additional administrative policy innovations in aid not captured by the narrative record.

III. Results

A. SVAR Results: Tables 2 and 3 present the SVAR estimates of the impact of federal fiscal policy on per capita GDP, first for the four-variable SVAR specifications (Table 2) and then for the disaggregated federal aid five-variable specification (Table 3). The tables report the estimated fiscal multipliers by quarters for GDP for a one-time, one dollar change in each of the federal fiscal policies; one standard deviation (68 percent) confidence intervals are reported within parentheses, and an asterisk indicates when the estimated effect is significantly different from zero at a 95 percent level of confidence. All multipliers are computed from the impulse response functions, evaluated at sample means for the fiscal variable and GDP. To avoid the large breaks in fiscal policy after WWII and from the Korean War, we follow Blanchard and Perotti (2002) and focus our analysis on the period from 1960:1 to 2010:3.

to the log of project aid and welfare aid, respectively. These percentage innovations were multiplied by the levels of project and welfare aid to obtain dollar equivalent innovations for comparison to the narrative aid innovations, also measured in dollars.

For purposes of comparison, columns (1)-(2) in Table 2 reproduce the three-variable SVAR of Blanchard-Perotti for their preferred sample period, beginning in 1960:Q1. This initial-three variable SVAR adopts the Blanchard-Perotti specification for federal net revenues defined as federal taxes less transfers to households and firms (our R) less federal aid to the SL sector (our A): $R - A$. Our estimate of -2.2 for the peak impact of net revenues is larger than that of Blanchard-Perotti (-1.3) but comparable as to the length of impact after the innovation (to quarter 8); Table 2, column (1). The estimates of the impact and timing of federal purchases (G) on GDP in our Table 2, column (2), are similar to those in Blanchard and Perotti (2002, Table IV).

What is central to our analysis is the possible difference between the estimated multipliers for shocks to net revenues for household and firms only (R) and the separate multipliers for shocks to federal SL aid (A); see Table 2, columns (3)-(5). First, estimated multipliers for government purchases (G) are now smaller, peaking at .56, and are statistically significant only in this initial period after the spending innovation, a pattern that holds for the remainder of our analysis and parallels the results in Barro and Redlick (2011) and summarized in Ramey (2011b). Most important, the estimates for this four-variable SVAR show that federal aid paid to the SL sector (A) is far less stimulative of GDP than are taxes paid and transfers received directly by the household sector (R). The peak multiplier for A is .8, while that for R is now -3.75. These results for a household-and-firm-only definition of net revenues are comparable to those reported in Romer and Romer's (2010) narrative study and Mertens and Ravn's (2013) SVAR study for the impacts of tax policy. Importantly, both of those studies are evaluating only taxes and transfers to the private sector, our definition of R. The results here suggest it is important to recognize the separate effects on GDP of innovations in transfers to governments and transfers to households. To ignore this distinction will be to overestimate the impacts on GDP of aid to SL governments and to underestimate the impact of taxes and transfers to households and firms.

Table 3, columns (1)-(4) extend our analysis by disaggregating total SL aid into its two primary components, project aid (AP) and welfare aid (AW), and then estimating a five-variable SVAR. The estimated effects for federal net revenues and federal spending innovations are comparable to those reported in Table 2. The impact of the two forms of aid, however, are significantly different. The multipliers associated with an innovation in project aid (AP) are initially

small, negative, and statistically significant, then positive thereafter though never statistically significant; see Table 3, column (3). The negative effect of AP following the innovation mirrors results in Gramlich (1978), who found that state and local governments temporarily postponed planned government spending, particularly capital projects, so as to utilize forthcoming federal project aid. In contrast, the multipliers associated with an innovation in welfare aid (AW) are as large as 2.3 (in Q2) and are statistically and economically significant into the third year after the innovation; see Table 3, column (4). Our analysis in Section IV seeks to clarify the reasons for these separate impacts of AP and AW on GDP.

Table 4 examines the robustness of our core SVAR results in Table 3 to alternative identification strategies, to the inclusion of monetary policy in the vector of policies, and to the exclusion of the Tobacco Master Settlement Agreement from the list of federal aid programs. Only the results for federal aid, AP and AW, are reported in the Tables. (Estimates for the impact of direct government purchases (G) and federal direct taxes and transfers (R) are similar in magnitude and timing to those reported in Table 3.) Table 4, columns (1) and (2), replace the Blanchard-Perotti identifying specification for the contemporaneous impact of GDP on federal net revenues of $\alpha_{r,y} = 2.08$ with the alternative estimate of $\alpha_{r,y} = 3.00$ provided by Mertens and Ravn (2013). With this adjustment, our estimates for the AP and AW multipliers are somewhat smaller than those reported in Table 3, but the negative impact multiplier for AP assistance in Q1 remains, as do the relatively larger effects of AW over AP.¹⁵ Columns (3) and (4) report estimates for an alternative ordering and timing for the impact of federal policies, both upon each other, and upon GDP. Rather than assume revenues predetermine spending, here we assume spending on purchases and projects (G and AP) predetermines revenues and transfers for aid to households (R and AW). Again, the results parallel those in Table 3.

Table 4, columns (5) and (6), extend the original five-equation SVAR for fiscal policy to now allow for possible confounding effects of monetary policy; see Rossi and Zubairy (2011). We do so

¹⁵ We have also reestimated the core SVAR model setting $\alpha_{r,y} = 1.6$ estimated by Follette and Lutz (2010). With this specification, the peak multiplier for AW is now 2.89, occurring in Q2, and that for AP is .967, also occurring in Q2. Here, too, we see a statistically significant, negative impact multiplier (= -.139) for AP aid. AW aid has a statistically significant effect on GDP into quarter 12 (= 1.2), while AP aid has a significant effect until quarter 8 (= .89).

by adding the federal funds rate and the inflation rate as measures of monetary policy, ordered after the four fiscal variables and GDP. As in Rossi and Zubairy (2011; Figures 9 and 11), we too find fiscal policy is estimated as less stimulative when monetary policy is included in the analysis. Monetary policy is less than fully accommodating. Again, AW is more stimulative than AP assistance. Finally, Table 4, columns (7) and (8), report estimates for the restricted sample, 1960:Q1 to 1998:Q3, excluding transfers from the Tobacco Master Settlement Agreement. Our core results remain in place for this restricted sample.

B. Narrative Results: Table 5 presents estimates for the impact of innovations in total federal aid (ΔA) on GDP identified using the narrative record. The estimated multipliers for the narrative innovations in total federal aid (ΔA) are larger than the multipliers for total federal aid (A) identified from the initial four-variable SVAR; compare Table 2, Column (5), to Table 5, column (1). The peak multiplier is now 1.0 as compared with .80, though both are within each other's error bounds. The reason for the somewhat larger estimated effect with the narrative record, perhaps, is that those shocks include only significant new "policy surprises," while the SVAR fiscal shocks include both those events and smaller "administrative surprises" likely to trigger smaller state fiscal responses; see Figure 3. Table 5, column (2), adds monetary policy to the narrative analysis, and similar to the results for the SVAR estimates, this extension implies a smaller, though still statistically significant, impact of fiscal policy.

Table 5, columns (3)-(6), examine the robustness of our narrative estimates to alternative specifications of the narrative variable. *First*, Table 5, column (3), addresses the concern that some of the programs included in the narrative record (denoted by ⁺⁺ in Table 1) might be endogenously affected by recent economic events and thus not true surprises to either state or local governments. Though the political approval of these programs occurs after our estimated optimal lag for income's effect on policy and though we specify the level of narrative aid for these programs as only aid above or below aggregate state deficits after the recession, it is still prudent to reestimate the model removing these "bailout policies" from the narrative record. The resulting estimates for the effects of aid on GDP are slightly smaller, but still economically and statistically significant for least four to five quarters after the policy introduction. *Second*, there are only four narrative welfare events, which proved insufficient to successfully identify a separate effect for welfare aid from the narrative

record.¹⁶ To be sure that inclusion of those events are not biasing our narrative estimates for the effect of project aid, we reestimated the narrative specification using only the 19 project aid events (ΔAP); see Table 5, column (4). The estimated effects are similar to those reported in column (1). *Third*, Table 5, column (5), seeks to avoid a possible misspecification for the timing of policy and limits our list of narrative events to only those that are approved in Q2 (90 days) before the fiscal year of their implementation by the states. Our original specification relied upon the fact that states could not adjust their budgets within a given fiscal year and thus counted as possible narrative events policies approved as early as Q4 (270 days) or Q1 (180 days) before the start of a state's new fiscal year; here we use only the "90-day events." Again, there are no appreciable changes in the estimates with this respecification. *Fourth*, as with the SVAR analysis, Table 5, column (6), limits the analysis to the period before the Tobacco Master Settlement Agreement. Again, there are no important differences in the resulting narrative estimates.

C. Summary: The prudent conclusion from this analysis is to assign a significant GDP multiplier of 1.5 to perhaps 2.0 for innovations in welfare aid (AW) with an impact on GDP growth lasting up to three years after the innovation. For the AP multiplier, values in the range of .8 to 1.0 are appropriate and with no significant GDP impacts expected after two years. Between the two forms of federal assistance, AW aid appears to be the more effective policy instrument for stimulating the macroeconomy in times of recession.

IV. States as Agents

Because of the fiscal importance of state and local governments in federal public economies, the central government must often rely upon those governments for implementation of its policy objectives. Other than defense, most spending for goods and services in such economies is done by lower-tier governments. State and local governments are often the sole providers of services and the primary providers of income support to lower-income households. The central government's primary means for influencing the spending and transfer policies of these governments are intergovernmental

¹⁶ We did estimate the effect of ΔAW on GDP using the narrative approach. The 95 percent confidence bands were very wide, including both the SVAR estimates for the impact of AW and also for zero.

transfers. Section III has presented evidence that such transfers impact the private economy. But how? Understanding the behavioral responses of lower-tier governments to intergovernmental aid provides the answer.

A. Specification and Identification: We estimate state responses to federal intergovernmental transfers, specified either as lump-sum (income) project aid or matching (price) welfare aid, for the 48 mainland states for the years 1979 to 2010. We do so within a fully specified budgetary framework that accounts for all state spending and all state revenues, following the methodology in Bohn and Inman (1996).¹⁷ The state budget identity is specified as:

$$AP + (rs - b) - (gs + k) \equiv \text{SURPLUS} = \Delta c - \Delta d + \Delta f \quad (4)$$

$$(\$504) + (\$3063 - \$276) - (\$3003 + \$312) \equiv (-\$24) = (\$81) - (\$55) + (-\$50),$$

where:

AP = State project aid per person, defined as all exogenous federal aid to states, including general revenue-sharing, federal aid for education, federal highway and transportation aid, annual payments under the 1998 Tobacco Master Settlement Agreement, and federal aid (other than Medicaid assistance) paid under the ARRA;

rs = State revenues per person, defined as all state taxes plus charges and fees plus miscellaneous revenues plus profits from state-run utilities plus profits from state liquor stores plus net proceeds from lottery sales;

b = States' own expenditures per person for welfare and medical assistance, defined as total state welfare expenditures (B) minus federal aid for welfare and Medicaid (AW = m•B, where m is the federal matching rate for welfare and Medicaid spending: b = (1 - m)•B);

gs = State expenditures per person for current operations plus intergovernmental assistance paid to local governments plus interest on state debt plus state own contributions to state public employee retirement, workers' compensation, and unemployment trust funds (but excluding welfare and Medicaid spending);

¹⁷ Budgetary data for the analysis are from the Census of Governments, *State Government Finances*, various years.

- k = Total capital outlays per person;
- Δc = Changes in cash and security holdings per person, other than in insurance trust funds;
- Δd = Changes in short- and long-term debt outstanding per person; and
- Δf \equiv Changes in contributions per person to insurance trust funds (including pensions) specified as $\Delta f \equiv \text{SURPLUS} - \Delta c + \Delta d$.

Sample means are reported below for each of the fiscal variables within the budget identity, measured as real (2005) dollars per person. Contributions to the trust fund (Δf) are treated as a residual category to ensure adding up in our specification of the state budgetary accounts.

The left-hand-side of Eq. (4) reports all revenues received by the state less all spending by the state. The difference defines the cash flow surplus ($\text{SURPLUS} > 0$) or deficit ($\text{SURPLUS} < 0$) in each fiscal year. Over our sample period, the average SURPLUS indicates a small average deficit of (-) \$24 per person, but the standard deviation of SURPLUS is \$263, reflecting the cyclical sensitivity of state fiscal fortunes over our sample period.¹⁸ The right-hand-side of Eq. (4) shows where the dollars go when there is a positive cash flow, or where the dollars come from when there is a negative cash flow. When there is a positive surplus, extra funds can be saved ($\Delta c > 0$), used to repay outstanding short- and long-term debt ($\Delta d < 0$), or put into insurance trust funds ($\Delta f > 0$). When there is a deficit, then savings must be reduced ($\Delta c < 0$ or $\Delta f < 0$) or short- or long-term government debt must be increased ($\Delta d > 0$).

To understand how states allocate an extra dollar of project aid or welfare aid across r_s , b , g_s , k , Δc , Δd , and Δf , we specify and estimate a behavioral budget model of state finances, specified generally as:

$$(r_s, b, g_s, k, \Delta c, \Delta d, \Delta f) = f(\text{AP}, 1 - m; I, \bar{u}; c_{-1}; \mathbf{X}) + v_s + v_t + v_{st}, \quad (5)$$

where each of the state fiscal choices is determined by a common set of federal policies (project aid, AP, and matching aid for welfare and Medicaid, $1 - m$), the state's economic environment (mean

¹⁸ For example, the average value of SURPLUS in fiscal years of the Great Recession was -\$171/person in FY 2009 and -\$105/person in FY 2010. In nonrecession years, however, average SURPLUS is typically positive, ranging from as small as \$3/person in FY1996 to \$107/person in FY2006.

household income, I , and unanticipated shocks to the state's unemployment rate, \tilde{u}),¹⁹ the state's lagged savings wealth (c_{-1}), and a set (\mathbf{X}) of political, institutional, economic, and natural disaster controls. The estimated budget equations also control for common shocks affecting all state budgets in any one year (e.g., interest rate changes, federal tax reforms) using year fixed effects (v_t) and unchanging state-specific institutions (e.g., balanced budget rules), politics, and economic fundamentals affecting budgetary outcomes using state fixed effects (v_s). Estimation is by generalized least squares. The within-year and state error terms (v_{st}) allow for state-specific autocorrelation following an AR(1) process and heteroskedasticity across states. No spatial autocorrelation is assumed, however.

To control for year-to-year changes within the state in important determinants of fiscal choices – particularly those potentially correlated with federal policies – we include in \mathbf{X} : (i) political controls: the state's vote for the Republican candidate in the last presidential election, the Berry et al. (2010) measure of conservative-liberal preferences of state residents, and whether the budget is set in the year preceding the election of a governor; (ii) an institutional control: the adoption and then presence of a requirement for contributions to a state rainy day fund; (iii) additional economic controls: a state-specific consumer price index, national oil price shocks interacted with whether the state is an energy-producing or energy-consuming state, and unexpected shocks to federal defense spending within the state; and (iv) a control for natural disasters: the total economic damages from disasters lagged one year. Eq. (5) is specified as a linear expenditure system, imposing an adding-up constraint for the impact of each variable on fiscal outcomes.

Key to identifying the effects of federal intergovernmental transfers on state fiscal choices is the assumption that those transfers as measured here are uncorrelated with the unmeasured (v_{st}) determinants of state revenues, spending, and savings decisions. We seek to establish the appropriateness of this assumption by two specification strategies. First, care is taken to ensure that federal aid is specified to include only those transfers exogenous to each state's current period budget. This will not be the case for total welfare and Medicaid aid specified as a federal matching subsidy at a rate m per dollar of state welfare spending (B): $AW = m \cdot B$. Unmeasured shocks to B will be

¹⁹ Measured as the residual of a regression of the state's current level of unemployment on lags of three years of the state unemployment rate. A separate regression is run for each state.

correlated with AW, biasing the estimate of AW's impact on state fiscal outcomes. To remove this source of endogeneity, we estimate the effect not of AW but of $(1 - m)$ on fiscal outcomes, where $(1 - m)$ is exogenous to current state budget choices and can be interpreted as the "net price" of each dollar of state spending on welfare services.

The rate m is known officially as the Federal Medical Assistance Percentage and is set each year based upon the state's three-year average income relative to the national average income beginning five years before the rate applies – e.g., the matching rate that applies in 2012 is based on incomes for the years 2007 to 2009. Poorer states have higher rates than richer states. As a control for possibly omitted influence of swings in the state economy on the value of m , we also include in all regressions state income per capital (I) and the unexpected level of the state unemployment rate, denoted as \tilde{u} . Finally, there have been two important "policy moments" that led to significant changes in the rate – FY 2004, following the Jobs and Growth Tax Relief Reconciliation Act of 2003, and FY2009 and FY2010, following ARRA. Consistent with the budget identity, the dependent variable for welfare spending will be the states' own spending for welfare services: $b = (1 - m) \cdot B$.

Second, project aid (AP) is specified to include only those programs whose funding is, by design or administration, independent of current-period state spending. This specification includes all funding for public education, state transportation infrastructures, jobs and training, general revenue sharing, and, as part of the 1996 reform of welfare financing, all federal aid to support income transfers to lower-income households (known as Temporary Assistance for Needy Families, or TANF).²⁰ Finally, we include in AP the payments to states under the 1998 Tobacco Master Settlement Agreement with U.S. tobacco companies as *de facto* federal project aid; see Singhal (2008).

Even after controlling for the endogeneity of aid because of legislative design, there remains the possibility that special events within the state in a given year might motivate Congress or the bureaucracy to offer, or take away, federal aid, and that these special circumstances might also influence state budget choices. A natural disaster such as Hurricane Katrina seems one obvious

²⁰ Program details supporting the exogeneity assumption for project aid can be found in Craig and Inman (1982) for education, Knight (2002) and the U.S. Department of Transportation (2007, p. 19) for transportation, Gramlich (1978) for jobs and training programs, Reischauer (1975) for general revenue-sharing, and Chernick (1998) for TANF support.

candidate. If so, failing to control for such events will bias our coefficient estimates of aid's effects on budgets. Efforts to find compelling instrumental variables for federal aid such as those proposed in Knight (2002) proved unsuccessful.²¹ Rather than use weak instruments that may worsen the bias in our estimates for aid's impact, we have elected to include in each regression year and state fixed effects, state mean income and unexpected unemployment, and a set of economic, political, institutional, and disaster controls, denoted as (**X**) as described above.

B. Results: Table 6 summarizes our results for the impact of the fiscal policy and economic variables on each of the seven budgetary aggregates. Estimates for Δf are obtained from the budget identity's adding-up constraint. Estimates for the effects of a \$1 increase in the state's mean household income (*I*) show state government activities to be normal goods, even for welfare spending (*b*). From the first row of Table 6, state revenues (*rs*) rise by \$.024/person, government current spending (*gs*) by \$.012/person, welfare spending (*b*) by \$.002/person, and capital spending (*k*) by \$.001/person. This leaves a positive cash flow from the marginal increase in state revenues of \$.009/person, which is then allocated as \$.006/person to noninsurance fund savings (Δc , "rainy day") and \$.004/person to insurance trust fund savings (Δf). There is also a \$.001/person increase in state debt (Δd), presumably to finance the \$.001/person increase in capital spending.

It is instructive, too, to see how state finances react to an unexpected increase in state unemployment (\tilde{u}), particularly in light of the large surprise in state economic fortunes beginning in FY2009. The sample's mean increase in \tilde{u} in 2009 was $\Delta \tilde{u} = .022$. After within-fiscal-year adjustments, our results predict that state revenues would still have declined by \$36.10/person. States are predicted to cut current spending by -\$11.44/person and capital spending by -\$1.48/person. Welfare spending would rise by \$4.40/person, however. The net result is still an end-of-fiscal-year deficit of \$27.58/person, or about 1 percent of the state budget, covered by an increase in state debt

²¹ We followed the approach of Knight (2002), using changes in congressional committee membership for the state's representatives, tenure of the state's congressional delegation, and state party representation relative to party majority in each chamber. In addition, we added changes in the governor's party relative to the state's majority congressional party and whether the state was a potential "swing state" based upon the closeness of the last presidential election. The resulting first-stage *F*-statistics never exceeded 4.0 for our sample period.

of \$29.44/person.²²

Increases in state project aid (AP) have no significant effect on state revenues (rs) or welfare spending (b), but AP does increase spending on current state operations and transfers to local governments (gs) and capital outlays (k); see the second row of Table 6. Total state spending rises by \$.51 for each dollar increase in AP. (Compare this with the spending impact of \$1 of household income of only \$.015; here again, we have evidence of a flypaper effect.) The remaining \$.49 of AP is net savings and equals an increase of \$.33 in the state's rainy day fund (Δc) and \$.19 in the state's insurance trust fund accounts (Δf), offset by a \$.03 increase in state debt (Δd), again used to finance, in part, state capital outlays.²³

Project aid that is saved is spent in subsequent years as it is withdrawn from the state's non-trust fund accounts (c), but the rate of withdrawal is very slow and the added spending effects in the near term are slight. A \$1 increase in the lagged value of noninsurance trust savings (c_{-1}) encourages the state to withdraw only \$.107 from the account each year; see the fourth row of Table 6. That \$.107 is allocated \$.006 to welfare spending, \$.059 to current accounts spending, and \$.01 to capital outlays. Total spending rises by \$.075. The remaining \$.036 is used to pay down outstanding debt. From these estimates, the final spending effects of a \$1 increase in AP will be \$.506 in the year aid is received and only \$.02 in each year thereafter.²⁴

Increases in welfare aid (AW) are made by increasing the federal rate (m) for income transfers

²² The estimated impacts on each budget category are computed from the results in Table 6 as $\Delta \tilde{u} \cdot d(\bullet) / d\tilde{u}$. For example, $\Delta r_s = .022 \cdot (-1641) = -\$36.10/\text{person}$. From the revenue and spending impacts, we then estimate $\Delta \text{SURPLUS} = \Delta r_s - [\Delta b + \Delta g_s + \Delta k] = -\$36.10 - [\$4.40 + (-\$11.44 + (-\$1.48))] = -\27.58 . The estimated impact on debt outstanding = $.022 \cdot 1338.3 = \$29.44$.

²³ We also tested for possible reallocations of AP in recession years and found no significant differences, except for a \$.02 reallocation of spending from current operations (gs) to capital outlays (k). Overall spending from an additional dollar of AP remained constant at \$.50, with the remaining \$.50 saved in rainy day and trust fund accounts as reported in Table 6.

²⁴ The year after the receipt of aid, there is a \$.326 increase in cash savings. This \$.326 increase is withdrawn at the rate of -.107 per dollar or by $-.035 (= -.107 \cdot .326)$ in the next fiscal year. This \$.035 withdrawal is then allocated as \$.025 to increased spending and \$.01 to the paying down of debt. This leaves $$.291 (= .326 - .035)$ in the cash account, which allows for another withdrawal of $-.031 (= -.107 \cdot .291)$ allocated as \$.022 to spending in the third year after the receipt of aid. The sequence is repeated again in year four and thereafter. The final equilibrium increase in aggregate state spending will be about \$.75 per dollar of AP assistance, with \$.506 occurring in the year the aid is received.

paid as aid to families with dependent children (AFDC, until the 1997 reforms) and health-care outlays for lower-income households (Medicaid). A state's own expenditures (b) will equal qualified welfare spending (B) less welfare aid ($AW = m \cdot B$): $b = (1 - m) \cdot B$. The variable $(1 - m)$ is the "net price" of welfare spending and B is the aggregate spending for welfare services for lower-income families. The elasticity of b and B with respect to $(1 - m)$ can be specified as: $\epsilon_{b, (1-m)} = 1 + \epsilon_{B, (1-m)}$. Based upon the results in Table 6, the estimated elasticities evaluated at the sample means are $\epsilon_{b, (1-m)} = .57$ and $\epsilon_{B, (1-m)} = -.43$. Increasing the matching rate lowers the net price for welfare spending and lowers *own* welfare spending (b) by the state but increases total welfare spending going to poor households (B). This was the approach adopted by ARRA.

From Table 6, a .10 percentage point rise in the federal matching rate (m) lowers the average net price $(1 - m)$ from .4 to .3, which in turn leads to lower *own* welfare spending of $-\$40.59$ /person and, from cross-price effects, to a fall in spending on government services of $-\$45.70$ and in capital outlays of $-\$7.57$. Together, total government spending declines by $\$93.86$. This savings is first allocated to lower taxes for the middle class, which decline by $\$52.58$. The remaining $\$41.28$ is then allocated to increase the rainy day fund by $\$.70$, to pay down outstanding debt by $\$15.29$, and to increase savings in insurance trust funds by $\$25.30$. Transfers and services received by poor households are higher because of the increase in m and can be estimated from changes in own welfare spending. For a .10 rise in the matching rate, evaluated at sample means, total support for poor households (B) rises by $\$95$ /person, or, assuming a national poverty rate of 12 percent, by $\$792$ per poor person.²⁵

The rise in state welfare spending and the higher federal matching rate mean increased federal aid to state governments for welfare spending (AW rises). For a .10 percentage point rise in m as in ARRA, and again evaluated at sample means, $\Delta AW = \$135$ /person. How is ΔAW spent? The typical state allocates $\$95$ /person to lower-income households and the remaining $\$40$ /person, along with the cuts in spending, to pay for the fall in middle-class taxes, increased savings, and debt retirement.

²⁵ Own welfare spending is defined as $b = (1-m) \cdot B$, where B is transfers to poor households. At sample means, $m = .6$ and $b = \$276$, so $B = \$690$ /person. After m is increased to $.7$, $b = \$276 - \$40.59 = \$235.41$. At the new levels, $m = .7$ and $b = \$235$, then $B = \$785$. The implied increase in total lower-income transfers per person is therefore $\Delta B = \$785 - \$690 = \$95$ /person.

C. Implied Macro Multipliers: Just as microeconomic estimates of consumer and firm behavior provide plausible bounds on estimated macroeconomic multipliers for direct federal purchases and tax relief, so too can estimates of state government behavior bound our estimates for the macroeconomic effects of federal AP and AW assistance. Each form of assistance has four effects on state budgetary behaviors: on taxes (rs), on transfers to lower-income households ($B = b/(1-m)$), on government purchases ($G = gs + k$), and on changes in public wealth ($\Delta W = \Delta c - \Delta d + \Delta f$). Each budgetary effect has, in turn, a potential multiplier impact on the private economy.

For federal project aid, the implied multiplier allowing explicitly for state behavior is:

$$dGDP/dAP = (dGDP/drs) \cdot [drs/dAP] + (dGDP/dB) \cdot [dB/dAP] \\ + (dGDP/dG) \cdot [dG/dAP] + [dGDP/dW] \cdot [dW/dAP],$$

where impacts within brackets represent state responses to aid and those within parentheses represent the multiplier effects on the GDP of those fiscal responses. The final effect is the multiplier impact of federal project aid on GDP measured as $dGDP/dAP$. We have obtained direct estimates of the aid multipliers in Tables 3, 4, and 5. Can we now replicate those direct estimates from the behavioral channels as shown in Table 6?

To do so, we first use the results in Table 6 to estimate the impact of a dollar of AP on each of the state budget categories impacting the private economy: $drs/dAP = .000$, $dB/dAP = .015$, $dG/dAP = .506$, and $dW/dAP = .488$ from Table 6.²⁶ We then estimate the multiplier effect of each budgetary change as $dGDP/drs = -3.189$, $dGDP/dB = 1.59$, $dGDP/dG = .884$, and $dGDP/dW = .096$. The multiplier estimates for state taxes (rs) and for state purchases ($G = gs + k$) are set equal to our estimates for the federal net revenue (R) and government purchase (G) multipliers after four quarters (one fiscal year) from Table 3. These multipliers are similar to those in the larger literature as well; see Ramey (2011b). The multiplier for state transfer spending, $dGDP/dB$, cannot be specified as the negative of the federal tax multiplier, however. Transfers by state governments to the household sector are not income but rather transfers in kind, most important, subsidized health care. Our estimate for the impact of Medicaid spending on household consumption follows from the analysis

²⁶ From estimates in Table 6, the effect on state revenues (rs) is $-.000$ effect, while the effect on benefits (B) paid to poor households is $\Delta b/(1-m) = .006/.4 = .015$. The effect on government purchases (G) will equal $\Delta gs + \Delta k = .379 + .127 = .506$. The effect on public wealth (W) will equal $(\Delta c - \Delta d + \Delta f) = .488$.

of the Oregon Medicaid program in which each new enrollee received \$788/person in additional health-care spending and, as a result, saved \$390/person in their own health-care spending; see Finkelstein et al. (2012, Tables V and VII). These results suggest that about half of every dollar of Medicaid spending becomes new income for Medicaid recipients. If so, then the multiplier for $dGDP/dB$ can be approximated as half the multiplier for $dGDP/drs$, or (after a sign change for receipt of transfer income) 1.59. Finally, we approximate the multiplier for increases in household public wealth, $dGDP/dW$, as the real interest rate (.03) times the value of the net revenue multiplier: $dGDP/dW = (3.189 \cdot 03) = .096$.

Upon substitution, the behaviorally based estimate for $dGDP/dAP$ is .52, comfortably within the confidence intervals of all four-quarter estimates for the impact of AP on GDP, and about half the estimate for the impact of direct federal government purchases on GDP. As revealed by our behavioral model, the primary reason for the difference between the impact of direct federal spending and indirect state spending initiated through federal grants is that states save about half of the AP transfer and only slowly allocate the saved money into future spending or tax relief. Our state panel estimates confirm the time-series evidence of Gramlich (1978) and Taylor (2011). At least for the objective of stimulating the aggregate economy, states are imperfect agents for federal policies.²⁷

For federal welfare aid, the implied multiplier is:

$$dGDP/dAW = (dGDP/drs) \cdot [drs/dAW] + (dGDP/dB) \cdot [dB/dAW] \\ + (dGDP/dG) \cdot [dG/dAW] + [dGDP/dW] \cdot [dW/dAW],$$

where again estimated behavioral responses are within brackets and multiplier effects are within parentheses. The same values for the multipliers as used for the AP estimate are used here for AW. The impact of one dollar of welfare aid follows from estimates for the impact of a small increase in the matching rate, evaluated at sample means: $drs/dAW = -.389$, $dB/dAW = .704$, $dG/dAW = -.395$,

²⁷ Recent work by Leduc and Wilson (2013) evaluating one targeted program, ARRA highway assistance, is more encouraging, finding that a dollar of aid is fully spent on highway spending. Our AP variable does include highway aid, but it also includes other less targeted programs. Detailed analyses of individual programs will be an important extension of the analysis here, but our basic point remains: Efficient use of federal aid requires understanding the behavior of the agents implementing those policies.

and $dW/dAW = .306$.²⁸ The negative impact of AW on G, also observed in Taylor's (2011) time series analysis, arises because of the positive cross-price effects of $(1 - m)$ on government purchases and the fact that AW increases as m rises and $(1 - m)$ declines. Upon substitution, the implied four-quarter multiplier for welfare aid is 2.05, again close to the four-quarter multipliers directly estimated from the SVAR results. The behavioral consequences of matching aid is to target a large fraction of each new dollar of federal welfare support to lower-income households, to reallocate state resources away from government purchases toward state tax relief, and to save some funds for spending in future budgets. Overall, raising the matching rate for lower-income transfers moves state funds from less stimulative government purchases toward more stimulative welfare spending and general tax relief. Because of these behavioral responses, the AW multiplier is significantly larger than the AP multiplier.

V. Intergovernmental Transfers as a Fiscal Stimulus

Table 7 provides one estimate for the relative effectiveness of the four fiscal policies studied here as alternative stimuli for economic growth. As an example, we use actual federal spending under ARRA. For purposes of our simulations, we have reestimated the five-variable SVAR of Eq. (3) for the pre-ARRA sample period from 1960:1 to 2009:1. Based on these estimates, we then simulate the performance of the economy without, and then with, ARRA policy innovations. The predicted path of GDP without ARRA innovations is provided as a benchmark; see Table 7, column (1). Estimates of the incremental effects of policy on GDP are computed as the difference between the predicted path of GDP with and without the innovation, first individually for all policies (Table 7, columns (2)-(5)) and then for all four ARRA innovations together (Table 7, column (6)). A final simulation shows the estimated impact of using just the two most effective policies (ΔT and ΔAW); see Table 7, column

²⁸ These marginal effects are based upon the effects of an increase in m of .10, percentage points evaluated at sample means, where (from text above) $\Delta rs = -52.58$; $\Delta B = 95$; $\Delta G = \Delta gs + \Delta k = (-45.70) + (-7.57) = -53.27$; and $\Delta W = \Delta c - \Delta d + \Delta f = .70 - (-15.29) + 25.30 = 41.29$. The implied change in AW is $\Delta AW = 135$. The resulting estimates are $drs/dAW = -52.58/135 = -.389$; $dB/dAW = 95/135 = .704$; $dG/dAW = -53.27/135 = -.395$; and $dW/dAW = 41.29/135 = .306$.

(7).²⁹

We specify the timing and size of ARRA's fiscal innovations following Romer and Romer (2010). For ARRA innovations in federal net revenues, we use the total tax savings and direct transfers to households and firms that occurred in the first quarter after the passage of ARRA: $\Delta R = \$45.2$ billion in 2009:2.³⁰ For government purchases, we assign the innovation to the quarter when actual purchases are first observed, with the level of the innovation equal to purchases in that quarter: $\Delta G = \$11.83$ billion in 2010:1.³¹ Innovations in AP include additional funding for three existing federal aid programs: aid to K-12 education (called "stability" aid), aid for infrastructure spending for roads and bridges, and aid for construction of public housing. Education assistance was paid immediately in 2009:2, and this innovation was assigned the actual allocation in that period: $\Delta AP = \$8.686$ billion. Because of a required application review, funding for the infrastructure projects and public housing was not observed until 2010:1 but then equaled $\Delta AP = \$18.753$ billion. Finally, the innovation for welfare aid included added support for SL spending for family services, child support,

²⁹ Simulations for the path of GDP following the fiscal innovations are calculated in four steps. First, each policy innovation is transformed into a corresponding structural shock for the five-variable specification of Eq. (3) denoted as $(v_t^r, v_t^g, v_t^{ap}, v_t^{aw})$, where shocks are the percentage change from the quarter preceding the innovation. Second, estimated and preassigned values of the β 's from the SVAR analysis are then used to provide vectors of the seasonally adjusted reduced form fiscal shocks (u_t^c) for each year (t). Third, given u_t^c , the estimated and pre-assigned values of the α 's are used to solve for the reduced form fiscal shocks associated with each innovation. Finally, the reduced form fiscal shocks and the originally estimated VAR specified by Eq. (1) with the control variable "deep recession" set equal to 1 are used to provide a projected path for GDP following each fiscal innovation. The analysis here should *not* be taken as an evaluation of ARRA per se as a fiscal stimulus. As is clear from estimates for our no-policy benchmark, our simulated economy is predicted to recover much more quickly than has the actual economy – that is, this is a "V" economy not an elongated "U" economy; see Reinhart and Rogoff (2009).

³⁰ Source: www.recovery.gov/News/featured/pages/TaxReliefDec2010.aspx.

³¹ The most important programs included in ΔG are direct federal expenditures for rural water and waste disposal, energy-efficiency and renewable resources, and science and health funding. For these programs we have detailed data on the timing of spending. However, there are smaller initiatives for which detailed funding is not available. The larger programs for which data on timing are available equal 37.7 percent of all proposed ΔG spending. We assume the timing for all other, smaller programs matched that in the waste disposal, energy, and science initiatives. We therefore inflated the total spending in 2010:1 in these measured programs by 2.6 ($=1/.377$). Total measured spending in 2010:1 equaled \$4.464 billion and (estimated) spending for all ΔG programs equaled \$11.83 billion.

Source: www.recovery.gov/Transparency/Agency/reporting/agency.aspx

low-income housing allowances, and most important Medicaid and was first paid in 2009:2 at a level of $\Delta AW = \$37.032$ billion.³²

The most effective of the individual ARRA policies is direct tax relief (ΔR); see Table 7, column (2). The least effective programs are direct federal purchases and project aid. Together, the two spending programs offered about the same size fiscal innovation ($\Delta G + \Delta AP = \$39.3$ billion) as did direct federal tax relief ($\Delta R = \$45.2$ billion). The reason for the significantly stronger effects on growth of tax relief than spending is the significantly larger estimated multipliers for these direct payments to households. Also of comparable impact is federal welfare aid ($\Delta AW = \$37.03$). This assistance is a transfer to households as well but is paid in three separate “installments,” first as direct state tax relief to households, second as an increase in “income-in-kind” for poor households as subsidized health-care spending, and third as increased state savings leading only slowly to future tax or welfare relief. The latter two effects are likely to have smaller multipliers than direct tax relief, but, by our estimates above, stronger multipliers than government purchases. As a result, the impact for welfare aid lies between that of direct tax relief and government purchases; see Table 7, column (5).

Estimates for all programs working in unison are provided in Table 7, column (6). As a policy package, the estimated maximal impact for ARRA spending is \$823/person occurring in 2009:4. The implied increase in the economy’s growth is 1.8 percent over the economy’s no-policy benchmark.

Finally, Table 7, column (7), shows the impact of a targeted stimulus using only the two most effective policies. Under a targeted stimulus, all the federal purchases innovation is reallocated to the innovation in ΔR as \$57.03 billion in 2009:2, and all the project aid innovation is reallocated to the innovation in ΔAW as \$64.473 billion also in 2009:2. For this targeted policy, peak GDP growth again occurs in 2009:4 and now equals \$1094/person. The implied increase in the economy’s growth

³² Full information on the timing of stability aid payments is available; \$8.69 billion was paid in 2009:Q2. For project aid, full information is available for highway aid and for housing aid. What is missing is information on the many small project grants for job training, constituting about 26 percent of all proposed project aid in ARRA. Highway and housing aid for which we do have timing data equal 74 percent of all project aid. We therefore adjust measured project aid spending of \$13.80 billion upward by 1.3589 (= 1/.74) for a total of \$18.753 billion. The actual payments and timing for Medicaid are reported, while funding for the many smaller programs are not. Medicaid constitutes 83 percent of total budgeted ARRA funding for AW assistance. We therefore adjusted the actual Medicaid allocation in 2009:Q2 of \$30.85 billion upward by 1.2 (= 1/.833) for a total AW innovation at this date of \$37.03 billion. Source: www.recovery.gov/Transparency/Agency/reporting/agency.aspx

rate is 2.6 percent over the no-policy benchmark. This is an approximate 30 percent improvement in GDP growth over the original ARRA mix of policies.

VI. Conclusions and Extensions

The Great Recession has renewed both policy and academic interest in the ability of fiscal policy to restore economies to full employment. In the U.S., the response was ARRA, which provided \$381 billion in federal tax cuts, \$98 billion in federal government purchases, and \$318 billion in transfers to state and local governments to fund their own tax relief and spending strategies. As a federal union, the U.S. public economy relies to a significant degree upon its state and local governments to provide nondefense government services, to administer poverty relief, and to share in the financing of such services and transfers. If the central government wishes to expand state and local spending or tax relief in times of an economic downturn, it will need to motivate lower-tier governments to do so. Other than by fiat, which is both constitutionally prohibited or politically infeasible in most economic unions, centrally funded intergovernmental transfers are the required fiscal policy. This paper has sought to provide the first estimates of federal aid's macroeconomic impact on GDP and to offer a microeconomic understanding for the relative effectiveness of alternative aid strategies. Our analysis offers three conclusions.

First, the impact on the aggregate economy of federal transfers to state and local governments is not equivalent to that of federal tax cuts and transfers paid directly to individuals or firms. For the U.S. economy, the average dollar given to the private sector is significantly more stimulative – i.e., it has a larger multiplier—than the average dollar given to lower-tier governments. Transfers to governments must be evaluated as a separate fiscal policy distinct in its macroeconomic impacts from those of taxes and transfers to the private sector.

Second, in economic unions, lower-tier governments are independently elected. As independent agents, they have their own agendas. The structure of incentives matters. First, we find that price (i.e., matching grants) incentives are more effective than income (i.e., unconstrained) incentives in stimulating tax and spending behaviors. In states within the U.S., we find \$.50 of each new dollar of unconstrained or fungible program aid is saved and only slowly spent in future years. In contrast, matching aid targeted for welfare services is paid only when spent. The aid is allocated

to general tax relief and to low-income assistance. Second, the purpose of intergovernmental aid matters too. Assistance for tax relief or income transfers is more stimulative than assistance for government purchases. Together, these two facts help us understand why, at least for the U.S. public economy, welfare aid (AW) is significantly more stimulative to the aggregate economy than is project aid (AP).

Third, recognizing such differences in how, and for what purpose, intergovernmental aid is given is important for the efficient design of macroeconomic stabilization policies. Targeted transfers to households either as direct federal tax relief or federal matching transfers for state welfare spending are most effective. For our simulated recession economy, reallocating ARRA funding from federal government purchases (G) to federal tax relief (R) and federal project aid (AP) to welfare aid (AW) would have improved the simulated economy's estimated income growth by about 30 percent.

Though our data are based on U.S. data and U.S. political institutions, we feel our central lessons have applications for other current, or would-be, federal economies. For example, the current economic crisis within the European Monetary Union has again raised calls for a stronger fiscal union and Union-wide fiscal policies. Given the Union's commitment to the principle of subsidiarity, intergovernmental transfers are the likely fiscal tool for achieving Union-wide objectives. At the moment, EU intergovernmental transfers are a small share of any member country's budget and paid only as fungible project aid. Learning how best to design new transfers to help stabilize the Union's macroeconomy will be an important extension of our work here.

Finally, we need to recognize that states are not only agents of the central government, but through their elected representatives, its principals as well. From this perspective, perhaps, it is no surprise that ARRA and most other aid innovations all incorporate a significant component of general assistance or fungible project aid. In federal public economies, some program inefficiency may be the inevitable political price we pay for the approval of any macroeconomic fiscal stimulus.

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Federal Aid, Federal Purchases, and Federal Net Revenue: 1947 - 2010* (Per Capita, 2005 Dollars)

Figure 1
Total Aid, Welfare Aid, and Project Aid

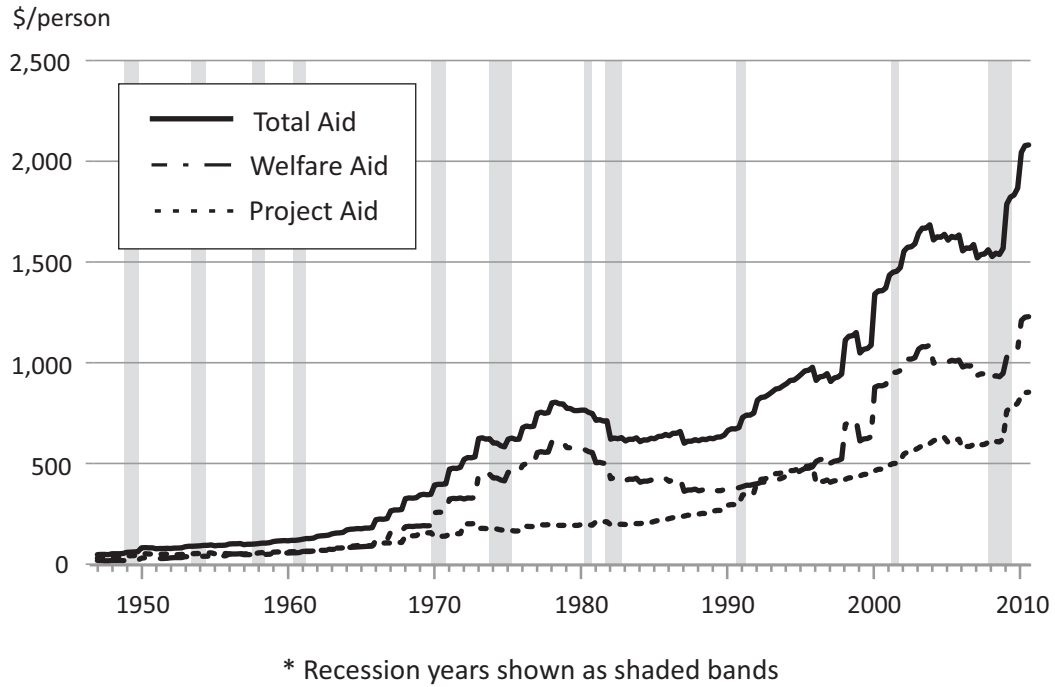


Figure 2
Federal Purchases and Federal Net Revenue

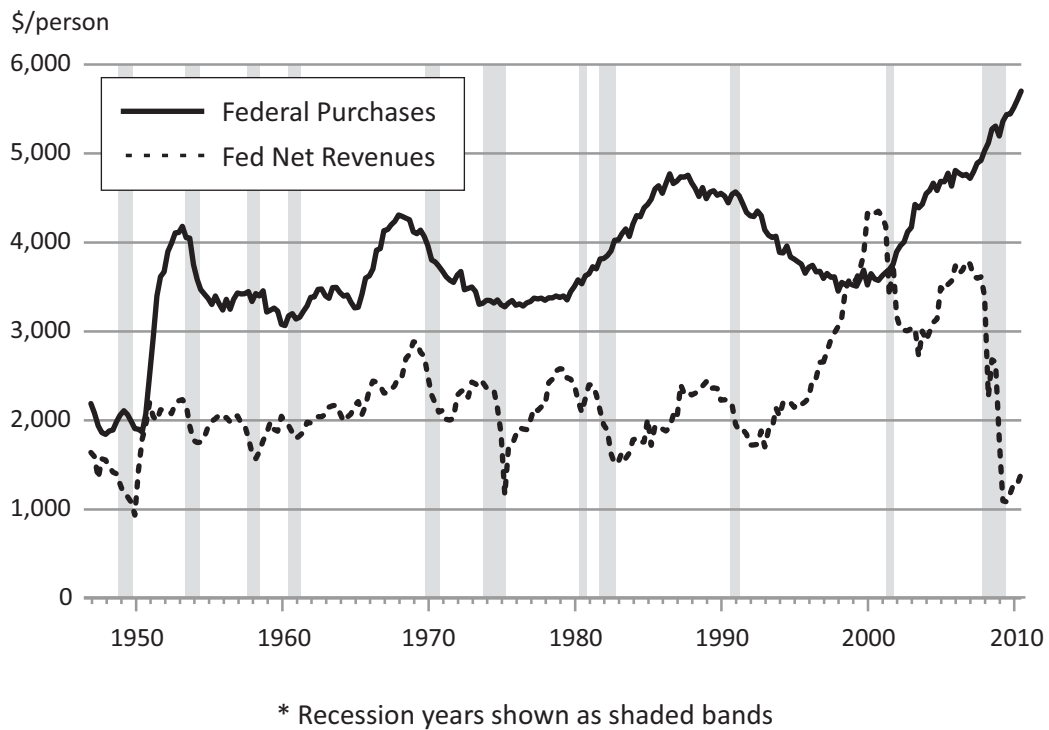


Figure 3: Comparison of Structural and Narrative Aid Innovations
(Dollars per Person)

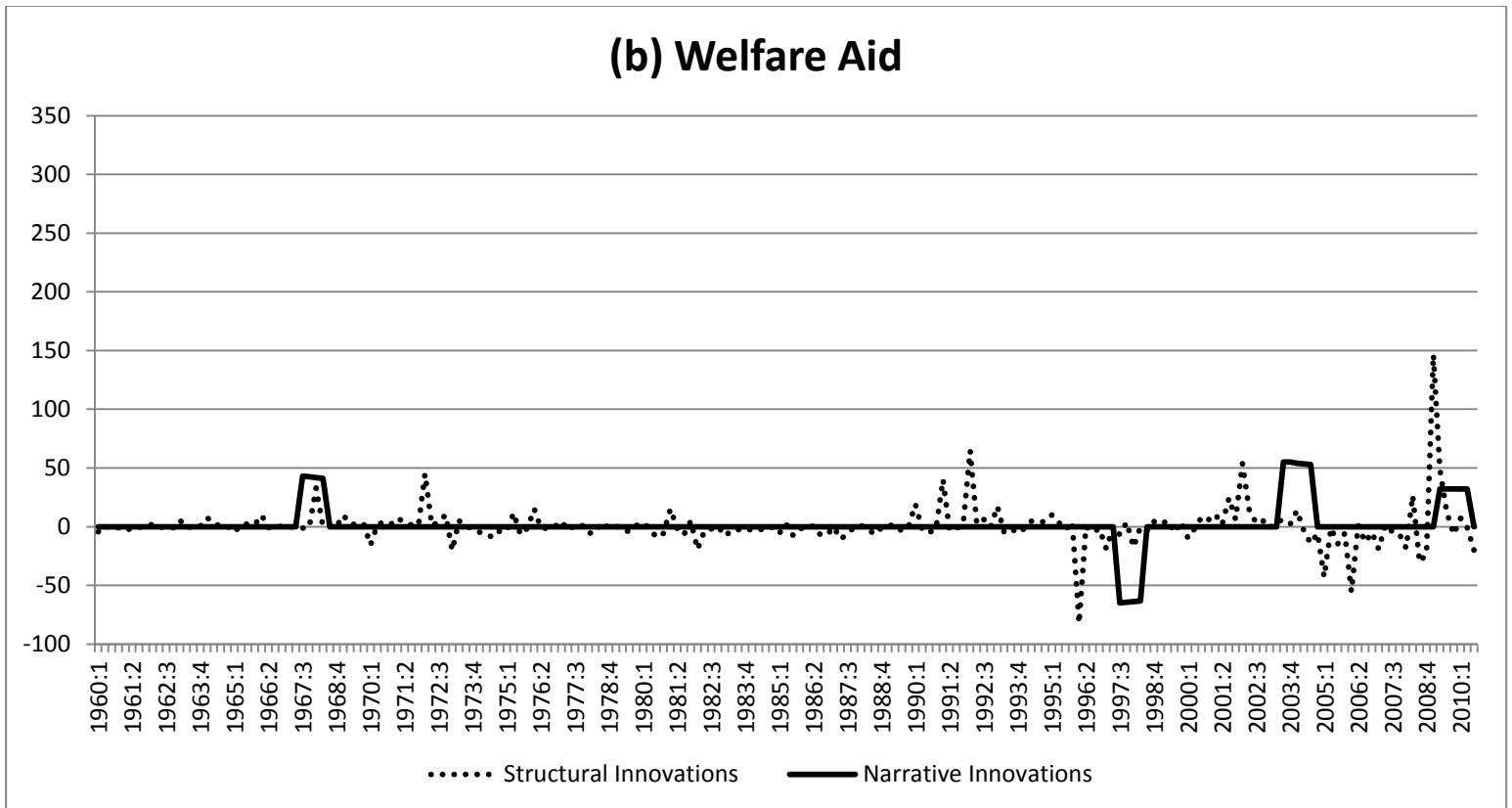
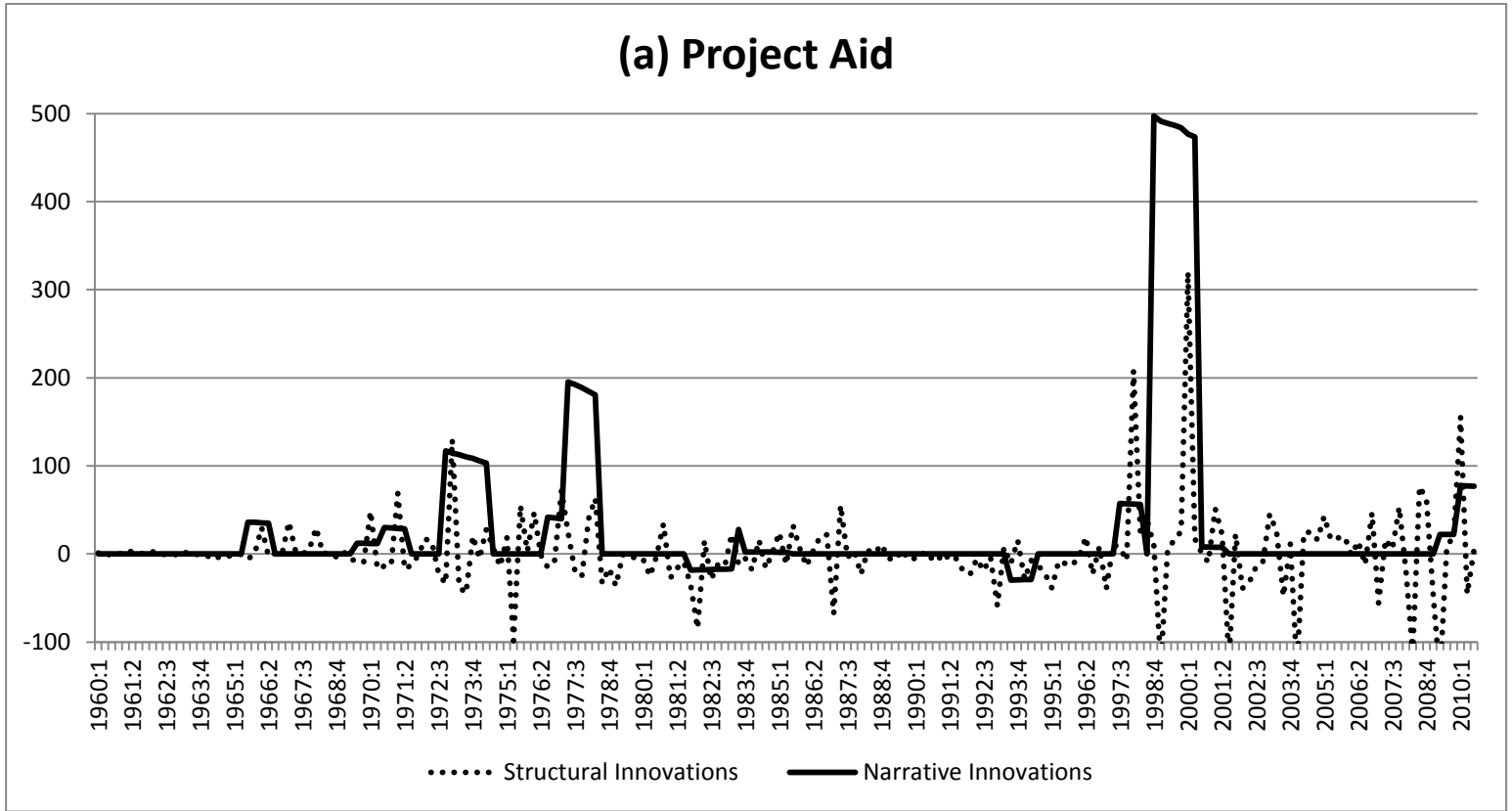


TABLE 1: Narrative Federal Aid Dates and Funding

Program	Timing	Level of Aid [†]
Federal Highway Act of 1956	1957:3 to 1958:2	\$1.518 Billion
Housing Act of 1954	1958:3 to 1959:2	\$.041 Billion
Elementary and Secondary Education Act of 1965	1965:3 to 1966:2	\$1.4 Billion
Medicaid	1967:3 to 1968:2	\$1.805 Billion
Housing and Urban Development Act of 1968	1969:3 to 1970:2	\$.579 Billion
Model Cities	1970:3 to 1971:2	\$1.5 Billion
General Revenue Sharing (GRS)	1972:4 to 1974:2	\$6.636 Billion
Public Works Employment Act of 1976 ⁺⁺	1976:3 to 1977:2	\$3.25 Billion
Economic Stimulus Appropriations Act of 1977 ⁺⁺	1977:2 to 1978:2	\$4.0 Billion
Intergovernmental Anti-Recession Act of 1977 ⁺⁺	1977:2 to 1978:2	\$2.25 Billion
Reauthorization of CETA ⁺⁺	1977:2 to 1978:2	\$6.6 Billion
Discontinuation of GRS (State Portion)	1981:4 to 1983:2	-\$2.283 Billion
Discontinuation of GRS (Local Portion)	1983:4 to 1984:2	-\$3.481 Billion
Emergency Job Act of 1983 (Part I) ⁺⁺	1983:3 to 1984:2	\$4.381 Billion
Emergency Job Act of 1983 (Part II) ⁺⁺	1984:3 to 1985:2	\$4.487 Billion
Welfare Reform of 1996 (PRWORA): Loss of AFDC	1997:3 to 1998:2	-\$14.499 Billion
Welfare Reform of 1996 (PRWORA): Gain of TANF	1997:3 to 1998:2	\$13.284 Billion
Tobacco Settlement Agreement	1998:4 to 2000:2	\$118.28 Billion
No Child Left Behind Act of 2001	2002:3 to 2003:2	\$1.993 Billion
Job and Growth Tax Reconciliation Act of 2003 ⁺⁺ (General Relief Assistance)	2003:2 to 2004:2	\$10.0 Billion
Job and Growth Tax Reconciliation Act of 2003 ⁺⁺ (Medicaid Relief)	2003:3 to 2004:3	\$15.426 Billion
American Recovery and Reinvestment Act of 2009 ⁺⁺ (Medicaid Relief and Stability Aid)	2009:2 to 2010:2	\$37.03 Billion
American Recovery and Reinvestment Act of 2009 ⁺⁺ (Project Aid for Transportation, Housing)	2010:3 to 2011:2	\$18.753 Billion

[†] All levels of aid are in nominal dollars. See Carlino and Inman (2014) for details.

⁺⁺Federal programs passed in response to state fiscal deficits following recessions.

TABLE 2: SVAR Estimates: GDP Responses to Fiscal Shocks for Blanchard-Perotti and Aid Specifications[†]

Specification Sample Period	Blanchard-Perotti 1960:1 to 2010:3 (1)	Blanchard-Perotti 1960:1 to 2010:3 (2)	Aid Specification 1960:1 to 2010:3 (3)	Aid Specification 1960:1 to 2010:3 (4)	Aid Specification 1960:1 to 2010:3 (5)
Identification Strategy	SVAR	SVAR	SVAR	SVAR	SVAR
Fiscal Policy	(R - A)	G	R	G	A
IMPACT	-1.683* (-1.98, -1.55)	.959* (.95, .96)	-2.804* (-2.84, -2.77)	.564* (.56, .57)	.528* (.52, .53)
4 Qtrs	-2.089* (-2.45, -1.72)	1.000* (.59, 1.40)	-3.287* (-3.90, -2.67)	.447 (-.24, 1.13)	.713* (.56, .86)
8 Qtrs	-1.223* (-1.70, -.75)	.619 (.20, 1.03)	-2.186* (-2.96, -1.41)	.404 (-.30, 1.11)	.499* (.33, .67)
12 Qtrs	-.578 (-1.05, -.11)	.340 (-.11, .75)	-1.503* (-2.26, -.74)	.309 (-.44, 1.06)	.360* (.18, .54)
20 Qtrs	-.0962 (-.34, .16)	.058 (-.38, .50)	-.920 (-1.61, -.23)	.165 (-.64, .98)	.234 (.04, .43)
Peak	-2.267* (Q2) (-2.55, -1.99)	1.078* (Q2) (.67, 1.41)	-3.755* (Q2) (-4.22, -3.29)	.564* (Q1) (.56, .57)	.802* (Q2) (.68, .93)

[†]All results are based on a SVAR estimation. For columns (1)-(2), federal net revenues less total federal aid to the SL sector (R - A) is ordered first, then federal government purchases (G), then GDP. For columns (3)-(5), the identifying ordering is federal net revenues (R), then federal purchases (G), then total federal aid to the SL sector (A), and finally GDP. Each cell gives the point estimates of the fiscal multiplier impacting GDP evaluated at the sample means for the fiscal variables and GDP, and then the lower and upper bounds (within parentheses) for the one standard deviation (68 percent) error band. Multipliers that are significantly different from 0 at the 95 percent level of confidence are indicated by an *.

TABLE 3: SVAR Estimates: GDP Response to Fiscal Shocks with Disaggregated Aid Specification[†]

Sample Period	1960:1 to 2010:3 (1)	1960:1 to 2010:3 (2)	1960:1 to 2010:3 (3)	1960:1 to 2010:3 (4)
Identification Strategy	SVAR	SVAR	SVAR	SVAR
Fiscal Policy	R	G	AP	AW
IMPACT	-2.955* (-3.06, -2.91)	.807* (.80, .82)	-.108* (-.11, -.11)	1.637* (1.61, 1.66)
4 Qtrs	-3.189* (-3.71, -2.67)	.884 (.27, 1.51)	.919 (.02, 1.78)	2.108* (1.80, 2.42)
8 Qtrs	-2.067* (-2.73, -1.40)	.677 (.07, 1.28)	.908 (.05, 1.77)	1.453* (1.07, 1.83)
12 Qtrs	-1.312* (-1.97, -.067)	.498 (-.15, 1.15)	.886 (-.03, 1.80)	.988* (.58, 1.40)
20 Qtrs	-.647 (-1.15, -.14)	.301 (-.40, 1.01)	.843 (-.16, 1.84)	.548 (.15, .95)
Peak	-3.604* (Q2) (-3.98, -3.23)	.884 (Q4) (.27, 1.50)	1.005 (Q2) (.19, 1.82)	2.315* (Q2) (2.08, 2.55)

[†]The initial SVAR identification used here orders federal net revenues (R) first, then federal government purchases (G), then welfare aid (AW), then general revenue/project aid (AP), and finally GDP. Each cell gives the point estimates of the fiscal multiplier impacting GDP evaluated at the sample means for each fiscal variable and GDP, and then the lower and upper bounds (within parentheses) for the one standard deviation (68 percent) error band. Multipliers that are significantly different from 0 at the 95 percent level of confidence are indicated by an *.

TABLE 4: SVAR Estimates: Robustness[†]

Specification	$\alpha_{ry} = 3.0$ 1960:1- 2010:3 (1)	$\alpha_{ry} = 3.0$ 1960:1- 2010:3 (2)	Ordering 1960:1- 2010:3 (3)	Ordering 1960:1- 2010:3 (4)	With Monetary 1960:1- 2010:3 (5)	With Monetary 1960:1- 2010:3 (6)	No Settlement 1960:1- 1998:3 (7)	No Settlement 1960:1- 1998:3 (8)
Identification Strategy	SVAR	SVAR	SVAR	SVAR	SVAR	SVAR	SVAR	SVAR
Fiscal Policy	AP	AW	AP	AW	AP	AW	AP	AW
IMPACT	-.076* (-.08, -.07)	1.146* (1.13, 1.17)	-.109* (-.10, -.12)	1.594* (1.57, 1.62)	-.096* (-.09, -.10)	1.642* (1.63, 1.65)	-.002* (-.003, -.001)	1.584* (1.56, 1.61)
4 Qtrs	.954 (.10, 1.81)	1.577* (1.33, 1.82)	.930 (.06, 1.80)	1.882* (1.62, 2.15)	.244 (.09, .40)	1.511* (1.29, 1.78)	.624 (.06, 1.78)	2.040* (1.74, 2.34)
8 Qtrs	.930 (.07, 1.79)	1.109* (.82, 1.39)	.918 (.05, 1.79)	1.253* (.91, 1.59)	.293 (.132, .455)	.427 (.11, .75)	.908 (.05, 1.76)	1.407* (.89, 1.77)
12 Qtrs	.899 (-.02, 1.81)	.769* (.45, 1.08)	.896 (-.03, 1.82)	.809* (.44, 1.18)	.172 (-.00, .35)	-.137 (-.46, .18)	.885 (-.03, 1.80)	.956* (.56, 1.35)
20 Qtrs	.850 (-.15, 1.85)	.441 (.12, .76)	.853 (-.16, 1.87)	.392 (-.07, .718)	-.110 (-.29, .07)	-.213 (-.46, .04)	.850 (-.15, 1.84)	.531 (-.15, .92)
Peak	.969 (Q3) (.12, 1.82)	1.715* (Q2) (1.52, 1.91)	1.017 (Q2) (.20, 1.84)	2.068* (Q2) (1.88, 2.26)	.305 (Q7) (.15, .46)	2.000* (Q2) (1.79, 2.21)	1.005 (Q2) (.19, 1.82)	2.242* (Q2) (2.01, 2.47)

[†]All results are for the sample period from 1960:1 to 2010:3. Columns (1) and (2) use a more elastic coefficient specifying the automatic (same quarter) impact of GDP on federal net revenues. Columns (3) and (4) allow for monetary policy variables (federal funds rate and rate of inflation) within the SVAR specification with fiscal policy ordered before monetary policy. Columns (5) and (6) report results for the alternative ordering of government fiscal policy with government spending (G and AP) ordered first, then revenue policies (R, AW), followed by GDP. Columns (7) and (8) report results for the restricted sample period, 1960:1 to 1998:3 excluding the Tobacco Settlement and subsequent quarters. Each cell gives the point estimates of the fiscal multiplier impacting GDP evaluated at the sample means for fiscal policies and GDP, and then the lower and upper bounds (within parentheses) for the one standard deviation (68 percent) error band. Multipliers that are significantly different from 0 at the 95 percent level of confidence are indicated by an *.

TABLE 5: Narrative Estimates: GDP Response to Fiscal Shocks[†]

Specification Sample Period	All Shocks 1960:1 to 2010:3 (1)	With Monetary 1960:1 to 2010:3 (2)	Exclude Bailouts 1960:1 to 2010:3 (3)	Exclude Welfare 1960:1 to 2010:3 (4)	90-Day Events 1960:1 to 2010:3 (5)	No Settlement 1960:1 to 1998:3 (6)
Identification Strategy	NARRATIVE	NARRATIVE	NARRATIVE	NARRATIVE	NARRATIVE	NARRATIVE
Fiscal Policy	ΔA	ΔA	ΔA	ΔAP	ΔA	ΔA
IMPACT	.470* (.32, .62)	.372* (.23, .51)	.406* (.26, .56)	.390* (.23, .55)	.434* (.28, .58)	.391* (.23, .55)
4 Qtrs.	1.055* (.70, 1.41)	.781* (.46, 1.10)	.854* (.49, 1.22)	1.173* (.78, 1.56)	.913* (.55, 1.27)	1.17* (.78, 1.56)
8 Qtrs.	.987* (.55, 1.42)	.568 (.18, .56)	.755 (.32, 1.19)	.988* (.53, 1.45)	.842 (.41, 1.28)	.987* (.52, 1.45)
12 Qtrs.	.715 (.34, 1.09)	.213 (-.14, .56)	.559 (.18, .94)	.532 (.13, .94)	.603 (.23, .97)	.531 (.13, .93)
20 Qtrs.	.345 (.07, .62)	-.116 (-.43, .20)	.307 (.03, .58)	.083 (-.23, .40)	.275 (.02, .53)	.082 (-.23, .40)
Peak	1.082* (Q5) (.70, 1.50)	.781* (Q4) (.46, 1.10)	.858* (Q5) (.46, 1.25)	1.206* (Q5) (.78, 1.36)	.933* (Q5) (.54, 1.33)	1.21* (Q5) (.78, 1.63)

[†]In columns (1) and (3)-(6), the Narrative identification strategy orders narrative aid first, then federal net revenues, then federal government purchases, and then GDP. Column (2) adds monetary policy to the analysis and orders narrative aid first, then federal net revenues, then federal government purchases, and then GDP, then the federal funds rate, and then the inflation rate as measures of monetary policy. Each cell gives the point estimates of the fiscal multiplier impacting GDP and then the lower and upper bounds (within parentheses) for the one standard deviation (68 percent) error band. Multipliers that are significantly different from 0 at the 95 percent level of confidence are indicated by an *.

TABLE 6: State Budgetary Responses to Federal Aid: 1979-2010[†]

	rs [\$3063] (1)	b [\$276] (2)	gs [\$3003] (3)	k [\$312] (4)	Δc [\$81] (5)	Δd [\$55] (6)	Δf [-\$50] (7)
I [\$60,566]	.024* (.002)	.002* (.0005)	.012* (.002)	.001* (.0005)	.006* (.001)	.001 (.001)	\equiv .004
AP [\$504]	-.000 (.052)	.006 (.010)	.379* (.041)	.127* (.016)	.326* (.064)	.033 (.048)	\equiv .195
(1-m) [.39]	525.8* (224.0)	405.9* (75.1)	457.0* (215.0)	75.66 (66.67)	-7.01 (218.7)	152.9 (160.7)	\equiv -253
c_{-1} [\$2485]	.021* (.010)	.006* (.002)	.059* (.009)	.010* (.003)	-.107* (.011)	-.036* (.008)	\equiv .017
\tilde{u} [.002]	-1641* (464)	200.3 (173.4)	-520.1 (442.8)	-67.15 (162.2)	-384.0 (833.2)	1338.3 (638.7)*	\equiv 468
N	1536	1536	1536	1536	1536	1536	1536
R ²	.93	.81	.94	.76	.20	.12	NA

[†] Budget equations reported in columns (1) to (7) are estimated by generalized least squares allowing for state-specific AR(1) processes. Heteroskedastic-corrected standard errors are reported within parentheses; coefficients twice their standard errors are indicated by an *. Column (7) reports the implied impact of each independent variable required for budgetary “adding up” for the residual category “net contributions to trust fund accounts” denoted as $\Delta f (\equiv AP + rs - (b + gs + k) - \Delta c + \Delta d)$. In addition to I, AP, (1-m), c_{-1} , and \tilde{u} , each regression also includes these independent variables: year and state fixed effects plus state-year controls for the cost of living in the state, citizen preferences measured on a liberal-conservative spectrum, the state’s Republican vote in the previous presidential election, a (1,0) indicator variable for when the budget is decided in an election year for governor, a (1,0) indicator variable for the presence of a state rainyday fund requirement, national oil price shocks interacted with regional indicator variables for consuming states in New England, the Great Lakes, and the Mid-Atlantic or producing states in the Southwest and Rocky Mountain regions, shocks to state military contracts and payroll, and the lagged level of real per capita property damages within the state caused by “billion dollar” natural disasters. Sample means are listed below each variable.

TABLE 7: Fiscal Stimulus and Simulated GDP Growth

Predicted Gains in Real GDP Per Capita for Alternative “Fiscal Innovations” (2005 Dollars)							
PERIOD	BASELINE PREDICTED GDP (1)	TAX RELIEF (ΔR) (2)	FEDERAL PURCHASES (ΔG) (3)	PROJECT AID (ΔAP) (4)	WELFARE AID (ΔAW) (5)	FULL STIMULUS (ALL POLICIES) (6)	“TARGETED” STIMULUS (ΔR ; ΔAW) (7)
2009:Q1	41,279	-	-	-	-	-	-
2009:Q2	41,675	604	0.00	-0.00	116	722	970
2009:Q3	41,887	649	0.00	12	136	800	1062
2009:Q4	42,197	659	0.00	15	147	823	1094
2010:Q1	42,640	640	23	11	141	819	1060
2010:Q2	43,140	603	11	32	132	781	997
2010:Q3	43,649	558	12	38	122	734	921
2010:Q4	44,141	500	9	31	110	653	827
2011:Q1	44,690	442	6	26	98	573	730
2011:Q2	45,052	384	3	25	86	499	635
2011:Q3	45,465	330	.1	24	74	429	547
2011:Q4	45,850	280	-2	22	64	364	465
2012:Q1	46,209	235	-4	19	54	306	393
2012:Q2	46,544	196	-5	16	46	254	329
2012:Q3	46,860	163	-7	14	39	209	273
2012:Q4	47,160	134	-8	11	33	170	226