February 20, 2015

Dear Professor Brown,

My colleague, Jerry Carlino, and I would like to submit the attached paper for this fall's *Tax Policy and the Economy* conference and later publication. The paper will combine our recent macro-econometric work with recently published and new micro-econometric work for how state fiscal policies can influence state, and ultimately, the national economies. We see the project as a bridge between the macro and micro literatures on stabilization within federal economies. We apply our work to an evaluation of ARRA. We hope you find the project interesting and suitable for the coming conference.

Best regards,

Bob Inman

FISCAL STIMULUS IN FEDERAL ECONOMIES: WHAT ROLE FOR STATES?

by

Gerald Carlino and Robert P. Inman

PRELIMINARY DRAFT

(February 17, 2015)

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Gerald Carlino and Robert P. Inman[1]

I. Introduction

The Great Recession and the subsequent passage of the American Recovery and Reinvestment Act (ARRA) has returned fiscal policy, and in particular the role of state and local governments in such policies, to center stage in our efforts to return the US economy to full employment. Passed within the first two months of President Obama's administration, ARRA has now spent over $796 billion to stimulate the stimulate the private economy: $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).[2] 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 fiscal policy.

Lying behind ARRA are the implicit assumptions that fiscal policies can stimulate job growth during recessions, that state fiscal policies are not up to the task and thus federal policies are needed, and that intergovernmental transfers to state and local governments must be an important component

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[2] See Recovery.gov/transparency/fundinggoverview/pages/funding.breakdown.aspx.

of any central government's stimulus package. This has been the received wisdom in the scholarly and policy literature on the design of fiscal policy in economic unions, at least since the foundational writings of Richard Musgrave (1959) and Wallace Oates (1972). There have been few empirical tests of these propositions, however, with the exception of important early work by Edward Gramlich (1978, 1979).[3] And Gramlich was skeptical, finding the federal effort to escape the 1976 recession with grants to states was too little and too late. ARRA funding has provided scholars with another opportunity to evaluate the stimulus impact of intergovernmental aid, and the results are more encouraging; see Wilson (2012), Feyrer and Sacerdote (2011), and Chodorow-Reich et al. (2012). 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 study finds a significant positive impact on local private and public employment, with the strongest effects coming from ARRA support for state Medicaid payments.

These new results are valuable, but they leave three important questions unanswered. First, while there are measured gains for the local economy receiving assistance, might they come at the expense of, or alternatively might they enhance, the job or income gains of neighboring economies? Specifically, how do these gains aggregate? Second, the local economy studies have (so far) only been used to reveal economic changes for, at most, one year after ARRA spending. We still need to know: How long will the stimulus effects last? Third, the local impact studies estimate the effects

[3] At the time passage of ARRA, there were no accepted estimates as to how the state and local sector would react to such intergovernmental assistance or how such aid would impact the private economy. The Council of Economic Advisors (Romer and Bernstein, 2009) and the Congressional Budget Office (CBO Report, 2010) both assumed the money would be spent immediately and the economy's responses to increased state and local spending would be similar to those for federal direct purchases and tax cuts.

of ARRA spending as it is spent, but federal aid is fungible; see Craig and Inman (1982) generally and Conley and Dupor (2013) for ARRA. Might state and local governments have saved ARRA funds for spending after the recession had subsided or might ARRA aid been used to replace states' own planned allocations? This paper seeks to provide answers to these three questions. Our answers suggest ARRA policies might have been re-designed to provide a significantly larger impact on national economic growth following the Great Recession.

Section II summarizes the analysis in Carlino and Inman (2013a), where we estimate the impact of state own deficit spending and tax relief on state employment growth. State own deficits are financed either by state own borrowing. We find a state's own deficits do have positive impacts on the state's own employment growth, but also, importantly, on job growth in surrounding states. Total jobs created inclusive of "spillover jobs" are from 60 to 70 percent greater than own jobs created. Own deficit financed state spending and tax relief can stimulate job growth. Because of job spillovers, however, coordinated state fiscal policies at the national level are preferred.

Section III reports on our research in Carlino and Inman (2013b) that extends the micro-econometric analysis of state economies to the aggregate economy, with a particular focus on the impact of federal intergovernmental transfers. Previous macro-econometric research has estimated the effects of intergovernmental transfers on income growth by including those payments as part of federal transfers to households; see, for example, Blanchard and Perotti (2002). The implicit assumption with such a specification is that state and local governments are perfect "agents" for the household sector when allocating transfer income – that is, there is no "flypaper effect." The preponderance of the evidence examining state and local government spending and tax responses to federal aid rejects this assumption, however; see Inman (2009). State and local officials appear

to have their own spending agendas. Our econometric analysis for the macro-economic effects of federal aid respects this fact by examining separately the effects on national income growth of exogenous changes in federal spending, federal tax and transfer relief for the household sector, and federal transfers to the state and local sector. Further federal aid is divided into transfers to states for general government spending or tax relief and for lower income assistance. Our results summarized here confirm the importance of first separating household and intergovernmental transfers and then of separating total federal aid into transfers for lower income assistance and for general government activities.

Section IV provides the micro-econometric foundations for the aggregate results reported in Section III. Just as it is important to provide estimates of how households and firms respond to exogenous changes in federal tax and transfer policies to clarify macro-econometric estimates of federal fiscal policies, so too is it important to understand how state and local governments respond to exogenous changes in federal intergovernmental transfers. Here we specify and estimate a budgetary model of state government spending, taxation, and borrowing for the 48 mainland state for the sample period, 1979 to 2010 to highlight the full budgetary effects, both in the current year and in future years, of exogenous changes in federal to state aid. As for our macro-econometric estimates, we separate total federal aid into general aid for spending and tax relief and targeted aid for lower income assistance. The resulting micro-econometric estimates of how states allocate both forms of aid are shown to be consistent with the observed macro-econometric estimates in Section III.

Section V uses our macro-econometric estimates of the impact of federal spending, federal tax relief, and federal intergovernmental aid to simulate the effects to 2014 of ARRA policies on the

private economy. We estimate that ARRA is likely to have been an important stimulus for the current recovery, but that the stimulus might have been 30 percent more effective (or 30 less expensive) had it focused all federal spending on tax relief to households and intergovernmental transfers to the states for lower income assistance.

Section VI concludes our analysis with a mention of possible lessons from the US experience for other federal economies.

II. Local Deficits and Local Jobs

A. *State Fiscal Policy and State Economies*

1. [This section will be a short summary – without equations – of the general equilibrium local economy model used in Carlino and Inman (2013a), Sections 3.1 and 3.2. A PDF of Carlino and Inman (2013a) is attached to this proposal.]

2. [This section summarizes our measure of fiscal policy for estimating the impact of state policies on the private economy. For the analysis here we use state own deficits defined as the difference between state expenditures and state revenues for all state funds – that is, "flow" accounting. Included in our measure of state own deficits are the state's general fund, the state's capital fund, all state insurance funds including unemployment insurance, and state administered pension funds. State expenditures by these funds are for wages and salaries, pension payments to state employees, transfers to households, transfers to local governments, maintenance and new construction of state infrastructure, and payments for supplies. Included in state own revenues are all taxes, all fees, licenses, and all payments into the insurance trust funds. Since all state funds are included here, even states with enforceable balanced budget rules on the general fund can run own deficits in the aggregate budget. Own deficits are supported by short and long term borrowing.]

3. [This section will summarize the empirical results and estimation strategy for identification of the economic impact of state fiscal policies on state economies. Figure 3 from Carlino and Inman (2013a) will be presented here to summarize our empirical results for states; again see the attached PDF version of Carlino and Inman

(2013a).

B. *Local Deficits and Local Jobs*

> [This section draws upon the main conclusions from Carlino-Inman (2013a) as to spillovers across states and implications for aggregate fiscal policy. We will reproduce the relevant Table from Carlino-Inman (2013a, Table 4) as Table 1 for this paper. The discussion which follows is from Carlino-Inman (2013a, Section 5) but will be redrafted and shortened for this paper.]

Table 1 provides a policy experiment based on the results summarized above, asking what an increase in state own deficits would mean for jobs both in the deficit state(s) and in its surrounding economic neighbors. We focus on the largest state in each of the eight Crone economic regions; the economic neighbors are the other states within the region. The year chosen for the deficit increase is FY 2008, the first year of the Great Recession. Projected job impacts will occur over calendar year 2009.[4] The level of the simulated state deficit increase is set equal to $390/person, the one standard deviation of all state deficits over the period 2000-2007. These deficits may be funded by increases in the general fund deficit, the capital fund deficit, the unemployment insurance fund deficit, or the pension fund deficit. Use of the latter three funds for deficit financing is still available to states constrained by strong balanced budget rules on their general fund. This $390/person deficit is from 6 to as much as 15 percent of each of the largest state's own fund revenues in FY 2008 and will imply a sizeable increase in state spending and transfers or a significant reduction in state taxes and fees.

Three alternative simulations are presented in Table 1. The upper panel illustrates the impact on state jobs of a deficit increase in a region's largest state alone, with no new deficits by its (Crone)

[4] There is no evidence of significant job creation after the first year following the temporary increase in state own deficits; see Figure 3.

regional neighbors. The middle panel shows the impact of deficits by all other states in the region, *except* the largest state. These two panels illustrate the potential spillovers across states, first from the largest state to its neighbors and then from the neighbors to the largest state. The lower panel shows the increase in region jobs if all states in the region agree to cooperate in a common policy where each state increases its own deficit by $390/person.

A $390/person deficit is estimated to lead to an increase of .011 in the deficit state's rate of job growth, which leads in turn to the change in jobs reported in Table 1.[5] The job growth in the deficit state then spills over into neighboring states through changes in the growth of regional jobs surrounding each neighbor. This change, which varies by each neighboring state, allows a prediction of new job growth and thus new jobs in each neighboring state. Those new jobs are summed to provide an estimate of the overall level of job spillovers.[6] Finally, the large state's own jobs and the spillover jobs are summed to give the total jobs created in the region. We also report the present value tax cost/job created defined as the total level of deficits in the largest state divided by jobs created. For the second panel, jobs created and the present value cost/job from increasing deficits in each region's smaller states, but now excluding the largest state, are computed in a similar way. Finally, the third panel aggregates the results of the upper two panels to show the impact on total

[5] Jobs estimates follow from estimates in Table 2, col. 4 in Carlino-Inman (2013a). First, we compute the increase in the rate of job growth as Δjob growth = .0109 = .000028 *x* $390/person. The predicted rate of job growth is then multiplied by actual employment in each state in 2009 to obtain the predicted level of new jobs.

[6] The largest state's predicted increase in employment is added to existing growth for each of the state's regional neighbors to obtain a new estimate of each neighboring state's REGSpillovers. That value is then multiplied by the estimated coefficient for impact of REGSpillovers on job growth from Table 2, col. (4) (= .685) to obtain a new value of each neighbor's job growth. Each neighbor's new jobs are then estimated as the new growth rate times the neighbor's existing employment in 2009. Neighbors' new jobs are then summed to obtain the estimate of "spillovers to other states" reported in Table 1.

regional jobs and the average tax cost/job if all states agreed to jointly increase their deficits by $390/person.

Three conclusions are evident from Table 1. First, from the first row of the first two panels (OWN JOBS), state own deficits can create state jobs, at least for one year. But sizeable deficits will be required. The present value tax cost/job created *in the state* for the state running the deficits ranges from $72,000 per job in Massachusetts to $91,000 per job in California. These cost estimates are comparable to those obtained by the recent evaluative literature of the impact of the American Reinvestment and Recovery Act's fiscal assistance to state and local governments on local job growth; see, for example, Wilson (2012).

Second, there can be significant aggregate job spillovers onto the deficit state's neighbors. The costs of these "spillover jobs" will be $0 for the neighbors. This spillover benefit creates a strong incentive for the neighbors of the largest state to free-ride on that state's deficit behavior. For comparable deficit levels, large states can often create more jobs for its neighbors than the neighbors can create for themselves. For example, in the Far West region, spillovers from California (108,561) exceed its neighbors' own job creation (90,301).

The second panel in Table 1 shows that the incentive to free-ride runs both ways. If the largest state's neighbors were to collectively increase their deficits, but the large state did not, then the large state would receive the spillover jobs. In this panel, the region's total jobs will be the sum of the jobs created in the small states plus spillover jobs created in the large state plus spillover jobs created amongst all the small states (in the total, but not shown separately). With significant spillovers, all states may choose to "sit on their hands," hoping that the other states in the region will run deficits in times of recessions. Or if each state does run a deficit to create jobs within the state

– as would occur if the state benefits of a new job exceed the state's own tax cost/job – there will likely be a downward adjustment in own state's deficit behavior to the job spillovers received from the other states. The Nash equilibrium to such a "race to the bottom" policy game will be Pareto inferior to a cooperative allocation in which all states collectively agree to deficit finance region-wide job creation; see Pauly (1970).

Third, the lower panel of Table 1 shows the outcome of such a cooperative policy, if all states in the region agreed to run a common $390/person deficit. Total jobs created is the sum of total jobs created when the two sets of governments operated separately (REGION'S TOTAL JOBS) and tax cost/job becomes the weighted average of cost/job after allowing for spillovers. The tax cost/job under the cooperative policy is significantly lower than when each state, or set of states, operated independently ignoring spillovers. For example, in the New England region the "private" cost/job to Massachusetts of the deficit policy would be $72,103, or to the other smaller states $76,724, but working together and allowing for spillover jobs, the "social" cost/job falls to $44,250. A deficit policy that was not attractive for an individual state when comparing the benefits of a state job to its present value tax costs may become attractive when all states agree to cooperate and collectively share the deficit costs of job creation.[7] If so, then there is an argument for centralizing stabilization fiscal policy at the level of a regional or national government.

[7] Though that decision must ultimately rest on a comparison of the social benefits of job creation to the social costs of the increase in state own deficits. Whether the benefits of a created job exceed estimated social costs remains an open question. For example, as part of an effort to understand fluctuations in employment rates, Hall and Milgrom (2008, Table 2) estimate the annual (flow) benefits to a risk-neutral worker of remaining unemployed (and searching or providing home production) as 70 percent of the overall gain in added output. The net social surplus of moving from unemployed to employed would therefore be 30 percent of the worker's added output. It is this net output benefit from job gains today offset by job losses tomorrow that must be discounted and then compared to the present value of all taxes (including their excess burdens) needed to finance today's increase in state own deficits.

III. Estimating the Macroeconomic Effects of Federal Aid

A. *Specifying the Federal Fiscal Policy With Federal Aid*

[Here we specify the variables to be used in the macro-econometric analysis of federal intergovernmental aid on the aggregate economy. The discussion will provide a short, non-technical introduction for how to think about the impact fiscal policy in the macro-economy, following Blanchard and Perotti (2002). We will focus on specifying the variables for study: federal purchases of goods and services (denoted as G), federal net revenues (taxes less transfers to households and firms, denoted as R), and federal intergovernmental aid (denoted as A). The discussion will stress the potential importance of separating intergovernmental aid from R. Blanchard and Perotti and all current macro-econometric research (with the exception of Romer and Romer (2010)) include A within R and estimate the effects of the joint variable (R - A). This specification requires identical impacts for intergovernmental aid and household transfers on the private economy. Our contribution is to separate these effects. In addition we argue it may be important to separate A into its two main components as well, where A is the sum of project aid (denoted AP) and public welfare plus Medicaid aid (AW): A = AP + AW.[8] To illustrate the importance of federal aid to the overall time path of national fiscal policies; see Figures 1 and 2; attached.]

FIGURE 1: Will show the historical time path since WW II of total federal aid (A), project aid (AP) and welfare aid (AW).

FIGURE 2: Will show the historical time path since WW II of federal purchases (G) and federal net revenues (R).

B. *The Impact of Federal Aid on the Aggregate Economy*

1. Identifying Fiscal Impacts: [This section will provide a motivation

[8] 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 Carlino and Inman (2014).

for, and summarize strategies that allow, the identification of the impacts of exogenous shocks of federal fiscal policies on the private economy. The discussion will be informal but clarify briefly the identifying assumptions needed for each of the two prominent strategies: structural VAR techniques pioneered by Blanchard and Perotti (2002) and the political "narrative" approach used by Ramey (2011) and by Romer and Romer (2010). Both approaches have been used in Carlino-Inman (2013). Since our original work in that paper, we have been encouraged to adopt the "hybrid" technique pioneered by Mertens and Ravn (2013), and we are in the process of revising our 2013b paper using these techniques. We anticipate sharper estimates.]

2. The Fiscal Impacts of Federal Aid: [Current Tables 2 and 3 from Carlino and Inman (2913b) are provided here and summarize our main results. The analysis confirms the value in separating federal transfers to governments from tax and transfers to households and, further, the value of separately estimating the effects of project aid from welfare aid. The discussion below, from Carlino-Inman (2013b) summarizes these 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 specification (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 1960:1 to 2010:3.

For purposes of comparison, Cols. (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 length of impact after the innovation (to Q8); Table 2, Col. (1). The estimates of the impact and timing of federal purchases (G) on GDP in our Table 2, Col. (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, Cols. (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 importantly here, the estimates for this four variable SVAR show 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 the household-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, Cols. (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 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 statistically and economically significant into the third year after the innovation; see Table 3, Col. (4). Our analysis in Section IV seeks to clarify the reasons for these separate impacts of AP and AW on GDP.

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 to expect no significant GDP impacts 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

> [This material currently appears in Carlino-Inman (2013b). That paper is now being revised and will focus exclusively on the macro-econometric estimates summarized above in Tables 2 and 3, with additional identification and specification checks for

robustness. The results presented below originally appeared in Carlino-Inman (2013b) but will now dropped from that paper and included here as original material along with additional robust checks for our core results. We present our current discussion below, which we anticipate will be robust to our new work.]

[COMMENT ON PRESENTATION: In the interest of conserving space in Carlino-Inman (2013b), the results below may seem a bit dense. We anticipate in this paper offering some simple tables or flow charts that provide a more graphical presentation of how aid is allocated by state governments.]

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 provider 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 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).[16] The state budget identity is specified as:

$$AP + (rs - b) - (gs + k) \equiv SURPLUS = \Delta c - \Delta d + \Delta f \qquad (4)$$
$$(\$504) + (\$3063 - \$276) - (\$3003 + \$312) \equiv (-\$24) = (\$81) - (\$55) + (-\$50),$$

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

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 Settlement, and federal aid (other than Medicaid assistance) paid under the American Reinvestment and Recovery Act;

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 = State 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);

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

$\Delta f \equiv$ Changes in contributions per person to insurance trust funds (including pensions) specified as $\Delta f \equiv$ 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 LHS of Eq. (4) reports all revenues received by the state less all spending by the state. The difference defines the cash flow surplus (SURPLUS > 0) or deficit (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 $263 reflecting the cyclical sensitivity of state fiscal fortunes over our sample period.[17] The RHS of Eq. (4) shows where the dollars go when there is a positive cash flow, or where the dollars come from when there is a negative cash flow. When there is a positive surplus, extra funds can be saved ($\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 rs, b, gs, k, Δc, Δd, and Δf, we specify and estimate a behavioral budget model of state finances, specified generally as:

$$(rs, b, gs, k, \Delta c, \Delta d, \Delta f) = f(AP, 1 - m; I, \tilde{u}; c_{-1}; \mathbf{X}) + \upsilon_s + \upsilon_t + \upsilon_{st}, \qquad (5)$$

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 household income, I, and unanticipated shocks to the state's unemployment rate, $\tilde{u}$),[18] the state's lagged savings wealth (c_{-1}), and a set (**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 (υ_t) and

[17] 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 non-recession years, however, average SURPLUS is typically positive, ranging from as small as $3/person in FY1996 to $107/person in FY2006.

[18] 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.

unchanging state-specific institutions (e.g., balanced budget rules), politics, and economic fundamentals affecting budgetary outcomes using state fixed effects (υ_s). Estimation is by generalized least squares. The within-year and state error terms (υ_{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 **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 (υ_{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•B. Unmeasured shocks to B will be 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 ũ. 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 own state spending for welfare services: b = (1 - m)•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).[19] Finally, we include in AP the payments to states under the 1998 Master's Settlement Agreement with US 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 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.[20] Rather than use weak instruments which 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 4 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

[19] Program details supporting the exogeneity assumption for project aid can be found in Craig and Inman (1982) for education, Knight (2002) and 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.

[20] 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.

spending (b). From the first row of Table 4, 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 non-insurance 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 (ũ), particularly in light of the large surprise in state economic fortunes beginning in FY2009. The sample's mean increase in ũ in 2009 was Δũ = .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 of $29.44/person.[21]

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 4. Total state spending rises by $.51 for each dollar increase in AP. (Contrast to 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

[21] The estimated impacts on each budget category are computed from the results in Table 6 as Δũ•d(•)/dũ. For example, Δrs = .022•(-1641) = - $36.10/person. From the revenue and spending impacts, we then estimate ΔSURPLUS = Δrs - [Δb + Δgs + Δk]) = -$36.10 - [$4.40 + (- $11.44 + (- $1.48)] = - $.27.58. The estimated impact on debt outstanding = .022•1338.3 = $29.44.

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.[22]

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 non-insurance trust savings (c_{-1}) encourages the state to withdraw only $.107 from the account each year; see the fourth row of Table 4. 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 debt outstanding. 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.[23]

Increases in welfare aid (AW) are made by increasing the federal rate (m) for income transfers 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•B): b = (1 - m)•B. The variable (1 - m) is the "net price" of welfare spending and B is the aggregate spending for welfare services for lower-

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[22] 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 4.

[23] 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•.326) in the next fiscal year. This $.035 withdrawal is then allocated as $.025 to increased spending and $.01 to paying down of debt. This leaves $.291 (= .326 - .035) in the cash account, which allows for another withdrawal of -$.031 (= -.107•.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.

income families. The elasticity of b and B with respect to (1 - m) can be specified as: $\varepsilon_{b,(1-m)} = 1 + \varepsilon_{B,(1-m)}$. Based upon the results in Table 4, the estimated elasticities evaluated at the sample means are $\varepsilon_{b,(1-m)} = .57$ and $\varepsilon_{B,(1-m)} = -.43$. 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 4, a .10 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.[24]

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 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

[24] Own welfare spending is defined as $b = (1-m) \bullet B$, 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.

in spending, to pay for the fall in middle-class taxes, increased savings, and debt retirement.

C. *Implied Macro Multipliers*: Just as micro-econometric 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:

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

where impacts within brackets represent state responses to aid and those within parentheses represent the multiplier effects on 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 2 and 3 Can we now replicate those direct estimates from the behavioral channels as revealed in Table 4?

To do so, we first use the results in Table 4 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 4.[25] 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.

[25] 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 $(\Delta c - \Delta d + \Delta f) = .488$.

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 specified as the negative of the federal tax multiplier, however. Transfers by state governments to the household sector are not income bur rather transfers in kind, most importantly, subsidized health care. Our estimate for the impact of Medicaid spending on household consumption follows from the analysis of the Oregon Medicaid program where each new enrollee received $788/person in additional health care spending and, as a result, saved $390/person in 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•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 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 slowing 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.[26]

For federal welfare aid, the implied multiplier is:

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

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 impacts 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, and dW/dAW = .306.[27] 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 re-allocate state resources away from government purchases towards state tax relief, and to save some funds for spending in future budgets. Overall, raising the matching rate for lower income transfers

[26] 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 also other less targeted programs as well. 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.

[27] These marginal effects are based upon the effects of an increase in m of .10, 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.

moves state funds from less stimulative government purchases towards 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

> [This section will update the analysis and discussion now in Section V of Carlino-Inman (2013b). That analysis went to 2012 and was based on the estimates reported there. The Table and discussion below is from our preliminary results in Carlino-Inman (2014b). The new presentation to appear here will be based on the revised estimates of our macro-econometric model summarized in Section III above. The simulations reported here will not appear in the revised version of Carlino-Inman (2013b).]

Table 5 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 re-estimated the five variable SVAR of Eq. (3) for the pre-ARRA sample period 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 5, Col. 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 5, Cols. (2)-(5)) and then for all four ARRA innovations together (Table 5, Col. (6)). A final simulation shows the estimated impact of using just the two most effective policies (ΔT and ΔAW); see Table 5, Col. (7).[28] We specify the timing and size of ARRA's fiscal innovations following Romer and Romer

[28] 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^r_t, v^g_t, v^{ap}_t, v^{aw}_t), where shocks are the percentage change from the quarter preceding the innovation. Second, estimated and pre-assigned values of the β's from the SVAR analysis are then used to provide vectors of the seasonally adjusted reduced form fiscal shocks (u^C_t) for each year (t).

(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: ΔR = \$45.2 billion in 2009:2.[29] 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: ΔG = \$11.83 billion in 2010:1.[30] 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: ΔAP = \$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 ΔAP = \$18.753 billion. Finally, the innovation for welfare aid included added support for SL spending for family services, child support, low-income housing allowances, and most importantly Medicaid and was first paid in 2009:2 at a

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Third, given u^C_t, 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 Reinhardt and Rogoff (2009).

[29] Source: www.recovery.gov/News/featured/pages/TaxReliefDec2010.aspx.

[30] 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.

level of ΔAW = $37.032 billion.[31]

The most effective of the individual ARRA policies is direct tax relief (ΔR); see Table 5, Col. (2). The least effective are direct federal purchases and project aid Together the two spending programs offered about the same size fiscal innovation (ΔG + ΔAP = $39.3 billion) as did direct federal tax relief (ΔR = $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 (ΔAW = $37.03). This assistance is a transfer to households as well, but 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 later two effects are likely to have smaller multipliers than direct tax relief, but by our estimates above, stronger than government purchases. As a result, the impact for welfare aid lies between that of direct tax relief and government purchases; see Table 5, Col. (5). Estimates for all programs working in unison are provided in Table 5, Col. (6). As a policy package the estimated maximal impact for ARRA spending is $823/person occurring in 2009:Q4. The implied increase in the economy's growth is 1.8 percent over the economy's no-policy benchmark.

Finally, Table 5, Col. (7) shows the impact of a targeted stimulus using only the two most

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[31] 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.

effective policies. Under a targeted stimulus, all of the federal purchases innovation is reallocated to the innovation in ΔR as \$57.03 billion in 2009:2, and all of 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 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. Conclusion

National, state, and local governments each has an important role to play in providing government services and in protecting against the adverse consequences of uncertain events not well covered by private insurance. In federal public economies, each public good, service, or insurance program will be "assigned" to one level of government or another, and in some instances shared among all three levels. The accepted wisdom has been that government protection against large, adverse economic shocks affecting the entire economy could only be provided by the national government using national monetary and fiscal policies.

The research presented here suggests a shared approach may be preferred, for at least two reasons. First, in Section II we found state own deficits can stimulate a state's own economy, but there are significant spillover effects across states from such policies suggesting an important role for policy coordination. The federal government is the appropriate center for such coordination. Second, because the efficient provision goods and services and implementation of social insurance assigns an important role to state and local governments, much of total government spending and taxation will be done by these governments. National policies are appropriate, but they must be implemented to an important extent through state and local governments. In federal economies,

therefore, federally financed deficits for the funding of intergovernmental transfers becomes an important policy tool for economic stabilization against adverse aggregate economic shocks.

We have seen here how best to design such assistance, at least for the US public economy. Section III draws out federal intergovernmental aid for a separate analysis of its impact on national economic growth, and importantly, separates such assistance into its two major components. General federal assistance paid to state and local governments has modest effects and are similar in impact and timing to direct federal spending for goods and services. Federal assistance paid to state and local governments for lower income spending is significantly more stimulative and is comparable in its impact and timing to direct federal tax relief and expanded transfers. If one wishes to have the maximal effect on the private economy, then direct and indirect (through states) transfers to households appear to be the more stimulative fiscal policies.

Section IV's analysis of state budgeting helps us to understand why this is so. First, intergovernmental transfers for lower income assistance goes to households, either to lower income households as income transfers (though Medicaid) or to taxpayers as state tax relief. From our macro-econometric estimates, transfer to households are more stimulative than spending on goods and services. Second, intergovernmental transfers for low income assistance is matching aid and must be spent (for receipt of aid) in the year in which the aid is received. In contrast, intergovernmental aid paid as a general transfer may be saved by recipient governments. Our evidence in Section IV suggests it will be, at the rate of about $.50 for each dollar of aid received. Together these two results suggests matching assistance to state and local governments for lower income assistance should be more stimulative, and occur more quickly, than lump-sum transfers for state spending or general tax relief.

The analysis from Sections III and IV suggest a re-configuration of ARRA policies away from direct federal spending and general aid towards federal tax and transfer relief and matching aid for lower income transfers for maximal impact of GDP growth. Simulations in Section V based upon the macro-econometric estimates from Section III confirm this to be the case. Had ARRA spending been totally allocated to general tax and transfer relief and to intergovernmental aid for poverty spending, the aggregate impact on economic growth could have been improved by as much as 30 percent.

Though based on US data and US 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 using Union-wide fiscal policies. As we have found for US states, recent research by Beetsma and Giuliodori (2011), Auerbach and Gorodnichenko (2013), and Hebous and Zimmermann (2013) have found significant income spillovers of national fiscal policies across the EU economies. Given the Union's commitment to the principle of subsidiarity, intergovernmental transfers are the politically most likely fiscal tool for achieving Union-wide full employment. At the moment, EU intergovernmental transfers are a small share of any member country's budget and paid only as income aid for national or regional infrastructure projects. Were EU governments to decide to collectively pursue EU-wide fiscal stabilization policies, our results suggest EU funding for matching transfers for each country's existing unemployment insurance or lower income transfer programs might be the preferred policy.[32]

[32] A similar conclusion follows from the theoretical analysis in Fahri and Werning (2014).

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

Figure 1
Total Aid, Welfare Aid, and Project Aid

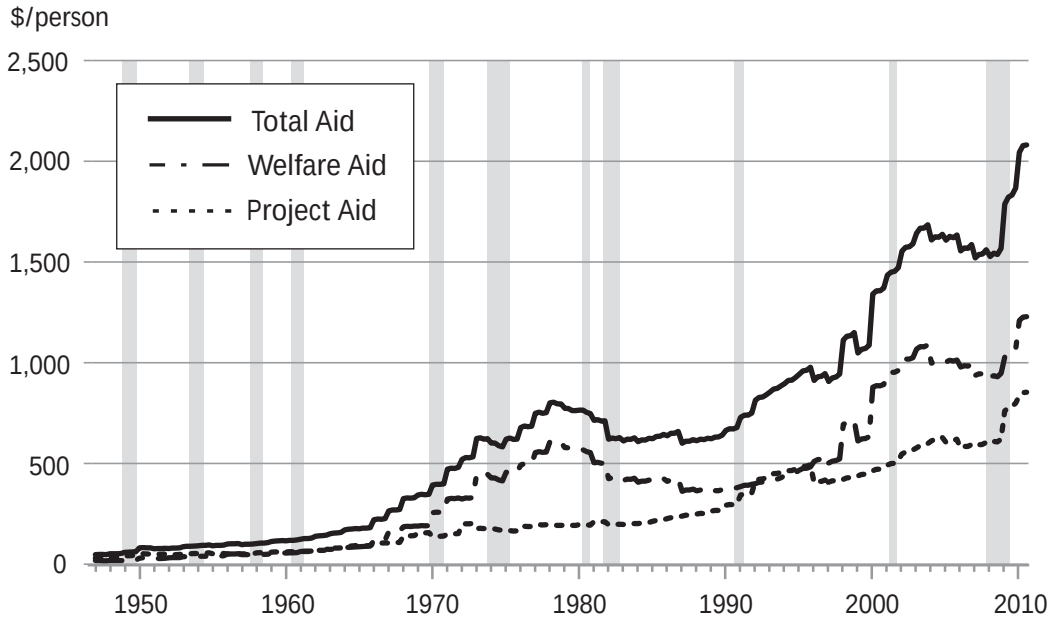
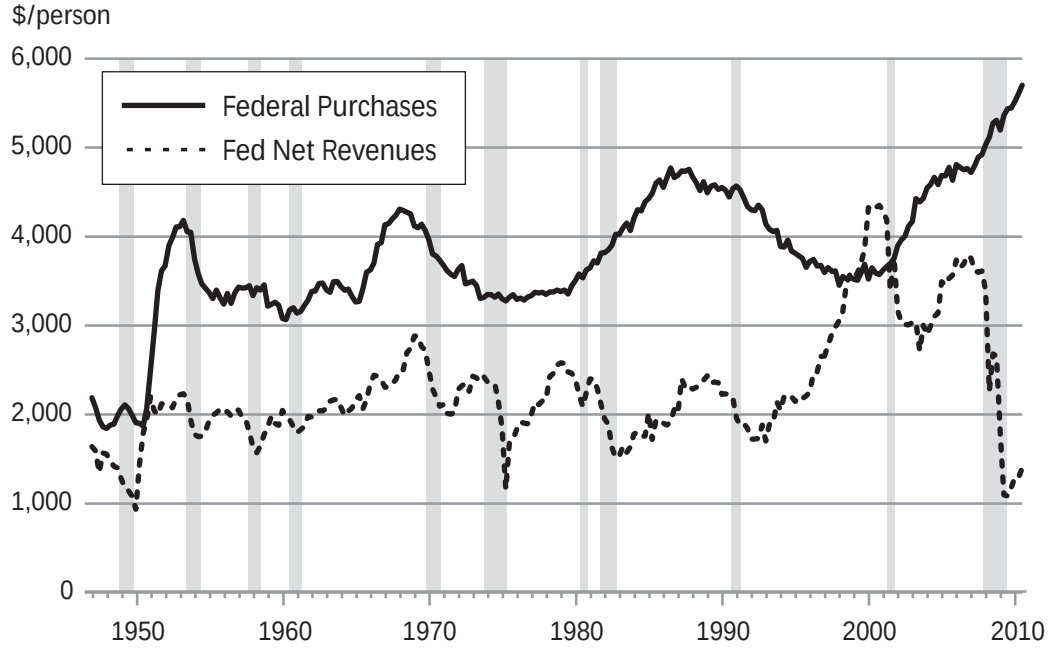


Figure 2
Federal Purchases and Federal Net Revenue



* Recession years shown as shaded bands

TABLE 1: State Own Deficits, State Jobs, and The Benefits of Cooperation: Impact Effects[†]

REGION	NEW ENGLAND	MIDEAST	SOUTHEAST	GREAT LAKES	PLAINS	MOUNTAIN NORTH PLAINS	ENERGY BELT	FAR WEST
LARGEST JOBS STATE (ΔOwnD/Own Rev)	Massachusetts (.06)	New York (.07)	Florida (.15)	Illinois (.11)	Missouri (.14)	Idaho (.12)	Texas (.11)	California (.10)
LARGE STATE'S OWN JOBS (Cost/Job)	35,253 ($72,103)	95,237 ($79,063)	81,337 ($89.467)	63,294 ($78,851)	29,831 ($77,934)	6,850 ($88,502)	113,563 ($85,175)	158,483 ($90,956)
JOB SPILLOVERS TO OTHER STATES (Cost/Job)	24,149 ($0)	65,237 ($0)	55,716 ($0)	43,356 ($0)	20,434 ($0)	4,692 ($0)	77,791 ($0)	108,561 ($0)
REGION'S TOTAL JOBS (Cost/Job)	59,402 ($42,791)	160,475 ($46,922)	137,053 ($53,078)	106,650 ($46,796)	50,265 ($46,251)	11,542 ($52,523)	191,354 ($50,549)	267,043 ($53,980)
OTHER STATES' OWN JOBS (Cost/Job)	40,086 ($76,724)	137,926 ($79,299)	242,881 ($84,819)	199,416 ($79,574)	41,730 ($71,759)	13,201 ($72,556)	88,759 ($81,294)	90.301 ($84,234)
JOB SPILLOVERS TO LARGE STATE (Cost/Job)	15,286 ($0)	48,756 ($0)	46,429 ($0)	38,907 ($0)	14,936 ($0)	3,965 ($0)	37,577 ($0)	43,836 ($0)
REGION'S TOTAL JOBS (Cost/Job)	67,544 ($45,534)	232,406 ($47,061)	409.254 ($47,061)	336,015 ($47,225)	70,315 ($42,587)	22,245 ($43,057)	149,559 ($48,245)	152,158 ($49,990)
REGIONAL POLICY JOBS (Cost/Job)	126,946 ($44,250)	392,881 ($47,004)	546,307 ($51,025)	442,665 ($47,121)	120,580 ($44,114)	33,787 ($46,293)	340,913 ($49,538)	419,201 ($52,532)

† Simulations are for an increase in lagged values of state's own deficits in each region, both on own states and on neighbors based on estimates from Inman and Carlino (2013) Table 2, col. 4. The simulations are for a deficit increase equal to a one standard deviation increase in own deficits for the decade, 2000-2007: $390/person. Simulations are for a deficit increase in FY2008 impacting jobs in calendar 2009. Only impact effects one year following the increase in state deficits are reported here.

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 Cols. (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 Cols. (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: 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)	≡ .004
AP [$504]	-.000 (.052)	.006 (.010)	.379* (.041)	.127* (.016)	.326* (.064)	.033 (.048)	≡ .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)	≡ -253
c_{-1} [$2485]	.021* (.010)	.006* (.002)	.059* (.009)	.010* (.003)	-.107* (.011)	-.036* (.008)	≡ .017
ũ [.002]	-1641* (464)	200.3 (173.4)	-520.1 (442.8)	-67.15 (162.2)	-384.0 (833.2)	1338.3 (638.7)*	≡ 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 Δf ($\equiv$ AP + rs - (b + gs + k) - $\Delta c + \Delta d$). In addition to I, AP, (1-m), c_{-1}, and ũ 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 whether the budget is decided in an election year for governor, a (1,0) indicator variable for the presence of a state rainy-day fund requirement, national oil price shocks interacted with regional indicator variables for consuming states in New England, Great Lakes, and the Mid-East 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 5: 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

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Local deficits and local jobs: Can US states stabilize their own economies?☆

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ABSTRACT

Using a sample of the 48 mainland US states for the period 1973–2009, we study the ability of US states to expand their own state employment through the use of state deficit policies. The analysis allows for the facts that US states are part of a wider monetary and economic union with free factor mobility across all states and that state residents and firms may purchase goods from "neighboring" states. Those purchases may generate economic spillovers across neighbors. Estimates suggest that states can increase their own state employment by increasing their own deficits. There is evidence of spillovers to employment in neighboring states defined by common cyclical patterns among state economies. For large states, aggregate spillovers to its economic neighbors are approximately two-thirds of the large state's job growth. Because of significant spillovers and possible incentives to free-ride, there is a potential case to actively coordinate (i.e., centralize) the management of stabilization policies. Finally, the job effects of a temporary increase in state own deficits persist for at most one to two years, and there is evidence of a negative impact on state jobs when these deficits are scheduled for repayment.

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1. Introduction

The recent Great Recession in the United States and the European Union has generated renewed interest in the management of macro-economic fiscal policy within economic and monetary unions. The received wisdom is that such fiscal policies will be efficient only if they are carried out by an overarching central government. Oates (1972) in his classic treatise on fiscal federalism concludes:

> The case for having the central government assume primary responsibility for the stabilization function appears, therefore, to rest on a firm economic foundation. (L)ocal government cannot use conventional stabilization tools to much effect and must instead rely mainly on beggar-thy-neighbor policies, which from a national standpoint are likely to produce far from the desired results. The central government, on the other hand, is free to adopt monetary

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> policies and fiscal programs involving deficit finance; consequently, the stabilization problem must be resolved primarily at the central government level (p. 30).

Within economic and monetary unions, deficits by lower tier governments, either states or member countries, are viewed as ineffective for restoring employment within the local economy. Deficits may stimulate local demand for goods and services, but because those goods and services are produced and traded within the wider union, the impact on local employment is thought to be modest at best. Even if there are increases in local employment opportunities, they may be "diluted" by the entry of workers from another state or country. Because of demand spillovers or worker mobility, therefore, any employment benefits derived from the deficit will accrue to all members of the union, while future tax costs remain the responsibility of the deficit-creating jurisdiction. If so, we have a public goods spillover and a need for coordinated fiscal policies managed by the union's central government.

We test empirically for the two important "facts" behind this familiar conclusion: do local fiscal policies impact the aggregate performance of the local economy for the benefit of state residents, and are there significant economic spillovers? Using US data from 1973 to 2009 for the 48 mainland states, we first examine the impact of state governments' own deficits on each individual state's own rate of establishment employment growth and, because these are open economies, state population growth due to net migration. Unique to this study, our measure of state deficits is explicitly specified to capture the full impact of state budgets on state economies and includes not just the usual general fund deficit but also deficits in the state capital accounts and in the state trust fund accounts, including public employee pensions; see Section 2. Section 3 provides our specification for the impact of fiscal policies on state economies within an economic and monetary union as well as two specifications for how one state's policies might impact the economies of its "economic neighbors."

Section 4 presents our core empirical results. We find that temporary state deficits do have a positive impact on the rate of employment growth within the state (Table 2) and a modest and statistically insignificant effect on state population growth because of net migration (Table 3). There are no significant changes in the state's rate of unemployment (Table 3). We conclude that most of a state's employment growth must be satisfied by residents returning to the work force—that is, by a rise in a state's rate of labor force participation. Section 4 (Table 2) also provides estimates for the effects of the deficit's two main components on job growth. It is state net revenues (taxes and fees minus transfers) rather than state spending for goods and services that has the primary effect on local employment. We compute an impulse response function for a temporary one-year increase in state deficits and find that the employment effects are relatively short-lived; state employment growth returns to its equilibrium level after one year (Fig. 3). Job growth declines eight years after the initial increase in debt as taxes rise to cover scheduled debt repayments.

In addition to the positive, though temporary, impact on its own economy, state own deficits also impact the job growth of their economic neighbors, specified as states with common patterns of economic fundamentals, e.g., the older industrial states of the upper Midwest or the energy region of the Gulf Coast and Mountain states. The aggregate regional impact is roughly two-thirds of a state's own impact, suggesting significant spillover effects (Table 2). The results parallel those found for fiscal policy interdependencies across EU economies; see, for example, Beetsma and Giuliodori (2011), Hebous and Zimmerman (2013), and Auerbach and Gorodnichenko (2012).

Section 5 (Table 4) summarizes the job impacts and the present value tax cost per job created from a temporary one-standard-deviation increase in state own deficits. Estimates are calibrated to states' economies at the start of the Great Recession. The present value of the future tax costs per job created for each deficit state range from $72,000/job to perhaps as much as $91,000/job. Importantly, when we include positive job spillovers to neighboring states, "collective" tax costs fall to $44,000/job to $53,000/job. Given significant spillovers, each state has an incentive to free-ride on the deficit policies of its economic neighbors. If so, the results argue for fiscal policy coordination for macro-economic stabilization. From the US evidence, the familiar conclusion from Oates stands.

2. Measuring state deficits

In contrast to previous studies focusing on state *general fund* deficits and state economies, we are the first to develop and use an encompassing definition of state deficits, one defined as the difference between state expenditures and state revenues for *all* state funds. Included in our measure of state deficits are the state's general fund, the state's capital fund, the state's unemployment and workers' compensation insurance funds, and state-administered pension funds. For each of the state's four fund accounts, we use a cash accounting measure of deficits, defined as the difference between money spent by the state in each account less revenues received by each account during the government's fiscal year (FY). Deficits are dated by the fiscal year typically beginning on July 1 and ending on June 30—for example, deficits in FY 2000 are for the period July 1, 1999 to June 30, 2000.

Included in the general fund deficit are expenditures for wages and salaries and pension contributions for state employees, transfers to households (including Medicaid payments), transfers to local governments, maintenance of state infrastructure, and payments for supplies. General fund revenues include all taxes, fees, licenses, and federal aid (including "aid" paid as part of the national tobacco settlement) into the general fund. Included in the capital fund are expenditures for all new construction; fees collected from the use of existing infrastructure have been included as part of general fund revenues. Included in insurance and pension fund expenditures are all payments to households for workers' compensation, unemployment benefits, and all payments to retired and disabled public employees. Included as revenues for the insurance

and pension funds are the annual payments by state employers into the unemployment and workers' compensation funds plus pension contributions from covered state and local employees, from local governments whose employees are members of the state pension plan, and the state's own pension contributions. Since the state's own contributions to the pension fund are also counted as a state general fund expenditure, these payments are an internal transfer and have no implication for the aggregate state deficit as measured here. Importantly, even when a state's general fund deficit is constrained by an effective balanced budget rule, the state's capital, insurance trust, and pension deficits are not. Deficits in each account can be funded by short-term or long-term borrowing or by drawing down accumulated reserves in each of the funds. Fund reserves are fungible across accounts. Importantly, states with effective balanced budget rules can still run significant deficits for aggregate demand management. All deficits and supporting fiscal variables are measured in real dollars with a state-specific price deflator set equal to 1.00 for Wyoming in 2004.[1]

In our empirical analysis of the impact of state deficits on state economies, we do not use total state deficits, but rather total state *own* deficits, denoted hereafter as *OwnD* and defined as total state deficits excluding all federal aid paid to the state. We use OwnD for two reasons. First, we wish to focus on the role of the state's own fiscal policies on its own economy. Including federal aid in our definition of state deficits compounds federal and state fiscal policies. Second, both total and own deficits are likely to be endogenous to the state's economy. We will therefore need instruments so that unbiased estimates of the effects of state deficits on state employment and population growth can be obtained. Effective instruments are more likely to be available from variation in state fiscal environments. Our analysis includes an exogenous measure of federal aid as a separate regressor, however.

Fig. 1a shows the historical path of the national (population weighted) average of total and state own deficits for the 48 mainland states for our sample period, where both deficit measures are defined as expenditures minus revenues and will be positive (negative) when there is a deficit (surplus) in the budget. The gap between own and total deficits is the average level of federal aid given to the sample states. It is clear that, without this assistance, states would require a significant increase in state revenues or cuts in state spending to balance their aggregate budgets.

Fig. 1b show the ratio of total deficits and total own deficits to national GDP. On average US states have run small surpluses over the years on their combined budgets; the exceptions are the two most current recession periods. Comparing the share of total deficits and own deficits to GDP over time again shows the importance of federal aid to the final balancing of state budgets. Nonetheless, aggregate state own deficits as a share of national GDP, what states do without federal assistance, is modest and usually no more than 2–3% except for the most recent recessions.

Fig. 2a–d shows the spatial pattern of state own (exclusive of federal aid) deficits per capita averaged over the four decades of our sample. States whose average own deficits fall in the upper quartile (darkest shading) over at least three of the four decades include Louisiana, Maine, Mississippi, Montana, New York, South Carolina, West Virginia, and Vermont (the one state without a balanced budget rule). With the exception of New York, they are all relatively poor states and heavily dependent on federal aid to balance their final budgets. States consistently ranking in the lowest quartile (lightest shading) of own deficits include Colorado, Delaware, Florida, Missouri, Nevada, Ohio, Texas, and Wisconsin. All but Nevada and Wisconsin have strong balanced budget rules based on constitutional provisions requiring balanced general fund budgets at the *end* of each fiscal year without the ability to carry over a deficit from one fiscal year to the next. The remaining states show significant variation over time in their relative rankings for own deficits, many moving between quartile rankings, and some dramatically so (Massachusetts, Utah, Idaho, New Mexico, and North Dakota). It will be this across-state and within-state over time variation in own deficits that we use to identify the impact of state fiscal policy on state economies.

3. State deficits and state economies

3.1. State economy

Our understanding of the potential impact of state deficits on state economies begins with the equilibrium framework for open economies in economic and monetary unions; see Haughwout and Inman (2001). All states compete in the world economy and receive the competitive world price (p) for their outputs. Technology is constant returns to scale in labor, capital, and land, defined uniquely for each local jurisdiction by the production "amenities" of the jurisdiction (A_F). Factor prices for labor (W) and land (R) are determined in the local market, while the price of capital (r) is set in the world market. Firms pay a tax per unit of output (t_F) and obtain productive public goods and services (G_F). Profits will equal the world price for the state's output minus average costs minus taxes per unit of output (t_F). In equilibrium, firms make zero excess returns. Thus:

$$\Pi(W, R; t_F, G_F; r, p, A_F) = 0. \tag{1}$$

Labor is supplied by state residents at a constant level of hours per year. Each working resident earns the market wage (W) and that income less local household taxes (t_H) is allocated to the consumption of goods and services produced locally or imported, to housing services, and to the purchase of land. Households within a state pay the market price (R) for the land

[1] All fiscal data for our analysis come from the *Census of Governments, State Government Finances*, various years. The price deflator is from Craig and Inman (1982), updated by the national urban CPI for those years for which state-specific indices are not available.

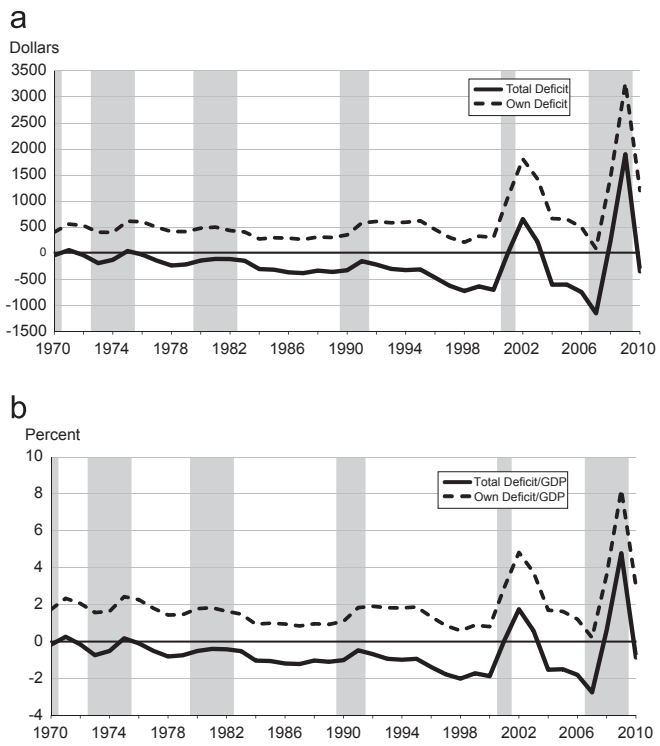


Fig. 1. States' deficits over time. Fig. 1(a) plots the paths of total deficits per capita (including federal aid as revenues) and state own deficits per capita (excluding federal aid as revenues) for the 48 mainland US states. Fig. 1(b) plots the paths of total and state own deficits as a share of GDP. Total state deficits are represented by solid lines; state own deficits are represented by dashed lines. Positive dollar amounts indicate a deficit; negative dollar amounts indicate a surplus. Both are measured in 2004 dollars. NBER recession periods are indicated by shaded bands.

they consume. Households receive residential public goods and services (G_H) and enjoy residential amenities (A_H). Household welfare is specified by their indirect utility function as:

$$U(W, R; t_H, G_H; r, p, A_H) = \hat{U}, \tag{2}$$

where in equilibrium every household in a state receives the exogenously determined level of utility ($\hat{U}$) available from living in any other state. Labor mobility is assumed.

Equilibrium allocations within the state are determined in two steps. First, Eqs. (1) and (2) are solved jointly to specify equilibrium wages and rents, conditional upon local fiscal policies (t_F, G_F, t_H, G_H), world prices (r and p), and local production and residential amenities (A_F, A_H). Second, aggregate output and thus the final size of the local economy is determined by the availability of productive land within the state (L_s); see Haughwout and Inman (2001). Land demanded by firms and households is set equal to the exogenous supply of land to define equilibrium gross state product (X):

$$X = X(t_F, G_F, t_H, G_H; r, p; A_F, A_H, L_s); \tag{3}$$

equilibrium state employment (N):

$$N = N(t_F, G_F, t_H, G_H; r, p; A_F, A_H, L_s); \tag{4}$$

and the equilibrium number of households (H) needed to supply N:

$$H = N/\mu = N(t_F, G_F, t_H, G_H; r, p; A_F, A_H, L_s)/\mu, \tag{5}$$

where μ is the equilibrium rate of labor force employment.

Eqs. (3)–(5) provide the starting point for our empirical analysis. We focus on the net impact of state own deficits (OwnD), defined here as state expenditures minus state own revenues: OwnD$=(G_F+G_H)-(t_F \cdot X+t_H \cdot H)$. In particular we are interested in the effects of a temporary increase in OwnD on changes in gross state product (X), state employment (N), state population (H), and temporary changes in the rate of employment (μ) as the economy adjusts to a new equilibrium. Our empirical analysis will also test for the separate effects of state spending (G_F+G_H) and state own revenues ($t_F \cdot X+t_H \cdot H$), denoted hereafter as *GovServices* and *OwnNetRev,* respectively.

A positive impact of temporary state deficits on the local private economy may occur in either of three cases. First, if firms and households fully anticipate the future tax burden of current deficits—the case of Ricardian equivalence—but local government services add more to productive efficiency and household utility than current and anticipated taxes, then firms

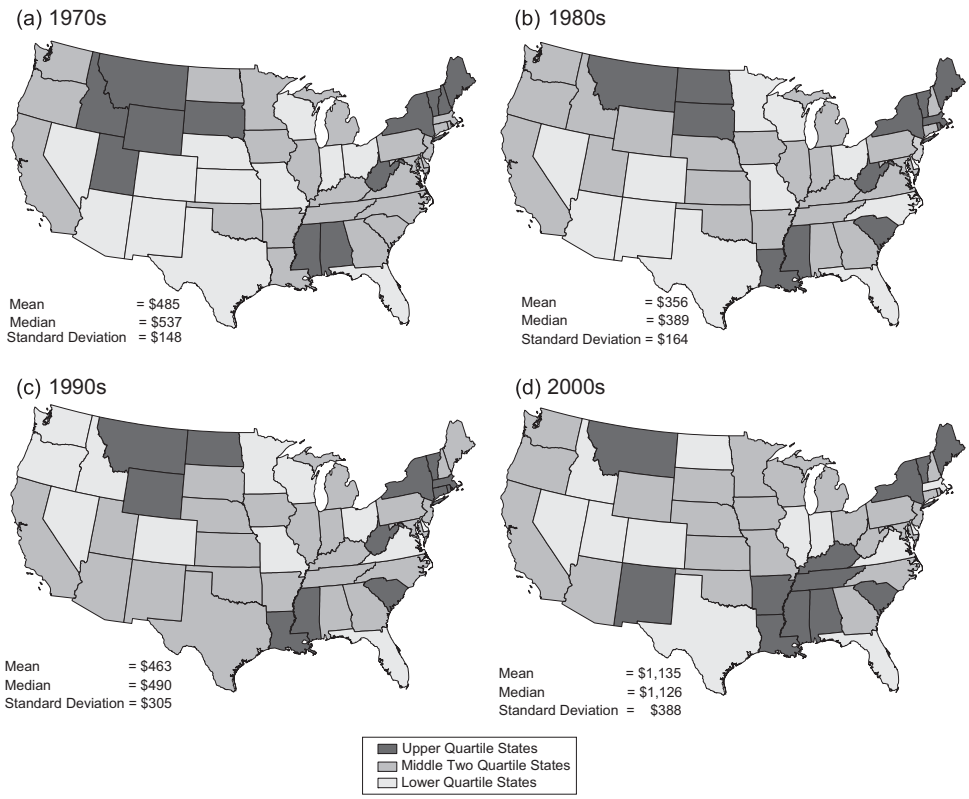


Fig. 2. Distribution of state own deficits by decade. State own deficits exclude federal aid as state revenues. All dollar amounts are measured in 2004 dollars, with Wyoming set equal to 1.00.

will increase their demand for labor, and workers will be attracted to the region. Here the *temporary* deficit creates a *permanent* improvement in the state's productive or residential environments. Capital spending may be an example.

Second, if firms or households within the state are credit-constrained, temporary state deficits can ease that constraint by financing current period tax cuts for constrained firms and households to allow desired investment and consumption. If such state deficit financing overcomes a failure in the local capital markets, credit-constrained firms and households will be attracted to the state. Again, X, N, and H will rise in equilibrium. Deficit financing of student loans may be an example.

Third, if firms and households are myopic and do not anticipate the future tax burdens of state own deficits, then deficits act as *de facto* fiscal transfers from future (perhaps future self) state residents or, in the case of bailouts, from residents of the full monetary union. Such transfers act to increase current period consumption and investment within the state, leading to an increase in X, N, and H. When deficits are repaid, however, firm and household taxes will rise, causing X, N, and H to fall back to their original equilibrium levels. If the eventual tax consequences of temporary deficits are perceived immediately, then there will be no change in X, N, and H, even in the short run. That is, Ricardian equivalence will hold.

In each case it is plausible to assume a period of adjustment to the new economic equilibrium or, in the case of myopic firms and households, a period before the tax consequences of state deficits are recognized and the economy returns to its original equilibrium; see, for example, Wildasin (2000). We will estimate the effects of temporary own state deficits per capita (OwnD) on annual changes in N and H, each specified as an annual state growth rate $(\dot{N}, \dot{H})$. The impact of OwnD on the growth rate of state economic output is not possible because of a re-definition of real state GSP by the Bureau of Economic Analysis during our sample period, making annual comparisons inappropriate.

3.2. State spillovers

In addition to any impact a state's deficit may have on its own economy, there may be consequences for other states' economies as well—that is, spillover effects onto the levels of jobs, output, and population of its neighbors. Temporary deficits that finance a permanent improvement in the state's firm or household amenities may draw firms, workers, and residents from other states. Here the spillover effects on neighbors are negative. In contrast, temporary deficits that finance an expansion of household consumption or firm investment may expand state aggregate demand and then economic activity in neighboring states as state residents buy goods and services produced outside their state economy. Here the spillover effects on neighbors are positive. Spillovers, whether negative or positive, may have global welfare implications and therefore argue for fiscal coordination of deficit policies among states within the union.

Table 1
Economic regions.[a]

Economic regions	Member states
New England	Maine, New Hampshire, Vermont, Massachusetts, Rhode Island, Connecticut
Mideast	New York, New Jersey, Pennsylvania, Delaware, Maryland
Southeast	Virginia, North Carolina, South Carolina, Georgia, Florida, Kentucky, Tennessee, Alabama, Mississippi, Arkansas
Great Lakes	West Virginia, Michigan, Ohio, Indiana, Illinois, Wisconsin, Minnesota
Plains	Missouri, Kansas, Nebraska, Iowa
Mountain/Northern Plains	South Dakota, North Dakota, Montana, Idaho
Energy Belt	Louisiana, Wyoming, Utah, Colorado, Texas, Oklahoma, New Mexico
Far West	Arizona, California, Nevada, Oregon, Washington

[a] Economic regions are defined as in Crone (2004). Crone's economic regions differ from the BEA definitions by moving West Virginia into the Great Lakes region and Louisiana into the "Energy Belt" region, both from the BEA's Southeast region. Minnesota is added to the Great Lakes region from the BEA's Plains region. South Dakota and North Dakota are moved to a new Mountain/Northern Plains region from the BEA's Plains region. Wyoming. Utah, and Colorado are moved to the "Energy Belt" region from BEA's Rocky Mountain States region. Finally, Arizona is moved to the Far West region from the BEA's Southwest region. The BEA's Southwest region is now omitted.

We will test for the presence of such fiscal spillovers among the 48 mainland states in two ways. Each measure uses an alternative specification of trade linkages between member states to define the reach of spillovers. Direct information on trade flows between US states is not available, however. As a first alternative we combine information on each state's major industries with the national input–output matrix to approximate trade flows between productive enterprises across states. The matrix is specified by the shares of each of 63 industry inputs needed to produce one dollar of a "buy" state's gross state product multiplied by the national input–output matrix and then multiplied by the share of a "sell" state's production of those 63 industry inputs.[2] The resulting matrix connects each state's purchases of inputs needed to make one dollar of the state's output to the inputs supplied by all other states. States that are major "sellers" of inputs demanded by "buy" states will be closely linked to those states. We call this measure of states' economic interdependence input–output spillovers, denoted *IOSpillovers*. Because most states have multiple providers of their inputs and sell their outputs to many states, the state input–output spillover indices closely approximate the relative importance of a state's economy in the aggregate US economy.[3]

The input–output measure of state linkages has the weakness, however, that it is a production-based measure of state interdependencies and omits the final demands by the household sector for goods and services produced in other states. We therefore offer a second, more inclusive measure of state interdependencies that includes demand by the household sector. In this measure states are grouped by economic regions as specified by Crone (2004). Crone uses the Philadelphia Federal Reserve Bank's indices of coincident economic activity to first identify the cyclical components of each state's aggregate economy. He then applies cluster analysis to the resulting cyclical components to group the 48 contiguous states into eight regions with similar business cycles. Here the spillover measure connecting states will be the change in economic activity in a state's Crone-specified regional neighbors, excluding the change in the state itself. We call this measure of state interdependence the *regional* spillover measure, denoted *REGSpillovers*. Table 1 lists the states in each of the eight economic regions as specified by Crone.[4]

Economic activities specified to influence a given state's growth in jobs and populations are the jobs and population growth, respectively, for that state's spillover "neighbors."[5] Weightings used to reflect "neighborliness" are either the input–output weights or the Crone-region (state shares) weights. The correlation of the two measures of spillovers is .47 for job growth spillovers and .24 for population spillovers. We test for spillovers with both specifications. We anticipate the spillover variable based on the more economically inclusive within-region connections to show a larger spillover impact than those obtained using the input–output connections. Finally, both our measures may overstate spillovers among US states, since they ignore purchases from non-US suppliers. Unfortunately, data on the demand for foreign goods and services by a state's firms and households are not available.

[2] We use the Bureau of Economic Analysis' 2010 specification of the national input–output matrix; see http://ww.bea.gov/iTabke/index_industry.cfm. In computing this measure we assume that all inputs into state production are imported from outside the state. Unfortunately, there are no compelling measures for state "self-sufficiency" in production.

[3] For example, to produce an additional dollar of gross state product (GSP) in California the production in the average "seller" state needs to rise by about $.01 (S.D.=.01). California's most important suppliers are New York ($.03) and Texas ($.04). Conversely, a $1 increase in GSP production elsewhere in the country will mean an average $.05 increase in production of California goods and services. It is this latter weighting—how exposed is California to demands elsewhere in the country—that we use as our measure of national spillovers that impact each state.

[4] For evidence that the Crone economic regions are likely to capture most of the important economic spillovers across state economies, see Bronars and Jansen (1987).

[5] We use outcome measures (growth in jobs and population) rather than policies (e.g., own state deficits) to avoid compounding policy interdependencies, say through yardstick competition, with economic interdependencies. We did test for the effects of a population weighted average of lagged OwnD(−1) of neighboring states as a separate regressor to control for regional fiscal competition. The variable was never a statistically significant determinant of state growth rates.

4. Estimation and results

4.1. Estimation

We estimate the impact of each state's own deficit on the state's growth in jobs ($\dot{N}$) and population due to net migration ($\dot{H}$) for a panel of the 48 mainland states for the period 1973–2009.[6] The growth specification for state jobs and population is preferred, first because it allows for adjustment lags of states' economies with respect to fiscal policies, and second, because it ensures stationarity of the relevant dependent variables. One- and two-period lagged values of growth rates are included in each equation to allow for adjustments in the dependent variables. Levels of state jobs (N) and population (H) are non-stationary by the Im–Persaran–Shin Im et al. (2003) test for stationarity in panel data allowing for unit roots to differ across states; growth rates are stationary.

A state's own deficit per capita, denoted OwnD, is defined as the sum of state deficits from all accounts—general fund, capital fund, and insurance trust fund (including public employee pensions)—less federal aid paid to the state. OwnD is also stationary by the Im–Persaran–Shin test. Because our data are annual data and because it takes from one to four quarters before fiscal policy impacts the private economy, we lag OwnD one period; see Ramey (2011). Contemporary values and one-period lags of the two spillover variables, IOSpillovers and REGSpillovers, are as specified above and included alternatively in each growth equation. All fiscal variables are denominated in 2004 dollars, allowing for across-state variation with Wyoming 2004 prices as the base deflator.

In addition to OwnD we also include federal aid to states as a separate regressor in each growth equation, specified as *non-matching* federal aid to state and local government, denoted as *ZAID*. Matching aid is endogenous to state government spending and thus potentially endogenous to variations in state economic outcomes. Matching aid includes Medicaid, Aid to Families with Dependent Children (AFDC, to 1996), and federal highway aid. In 1996, AFDC funding was re-structured as a lump-sum transfer without matching, known as Temporary Assistance for Needy Families (TANF), and was then included in ZAID. Always included in ZAID are all non-matching aid programs paid to states, including programs with explicit "pass-through" requirements for the states to fund local governments—for example, programs funded by the Elementary and Secondary Education Act. States have treated these programs as potential substitutes for their own support of local activities; see Craig and Inman (1982). Finally, also included in ZAID are payments to states as a result of the 1998 settlement with tobacco companies for reimbursement of state health expenditures due to smoking. As for OwnD, ZAID will be lagged one year in our regressions.

The underlying specifications for our job and population growth equations follow from Eqs. (3)–(5) above and suggest the need to control for initial levels of state firm and residential amenities (A_F, A_H,) and state land area (L_s). We do so by including state fixed effects in each growth equation. Also included in each specification are common national interest rates and prices for state inputs and outputs. The potential influence of changes in these macro variables are captured by the inclusion of year fixed effects. The estimated equations have the final specification as:

$$(\dot{N}, \dot{H}) = f(OwnD(-1), ZAID(-1), Spillovers; Controls) + \upsilon_{st},$$

where $\upsilon_{st} = \nu_t + \nu_s + \nu_{st}$, with year ($\nu_t$) fixed effects to control for common changes in aggregate demand and interest rates and state fixed effects (ν_s) to control for state amenities, state political and legal environments, and the land area of each state. The two spillover specifications—IOSpillovers and REGSpillovers will be tested separately. Control variables included in each regression are lagged values of the spillover variables as a control for shocks to "neighboring" economies, changes in world energy prices interacted with whether the state is an energy-producing state, and changes in state productivity as measured from the state production function for manufacturing.[7] Our estimation strategy corrects for serial correlation and heteroscedasticity in υ_{st}.

Two important econometric issues remain when estimating our growth equations. First is the potential endogeneity of state own deficits, even when lagged one year. Unmeasured shocks to a state's economy will lower the measures of economic performance and at the same time increase the state's deficit. If these unmeasured shocks have an autocorrelated path, then they will be correlated with lagged OwnD and contemporary performance as well. Since deficits rise when the economy declines, the bias is likely to be downward; OLS results reported below confirm this negative bias.

To correct for the possible endogeneity of OwnD, we instrument for OwnD using as instruments four- to six-year lags of OwnD. The identifying assumptions are, first, that deficit changes arising from fiscal choices of preceding legislative regimes have an institutional persistence helping to predict contemporaneous state deficits and, second, that those lagged deficit changes are not correlated with the current economic performance of the state except through their impact on OwnD. For example, accumulated cash reserves from prior general fund surpluses or in capital or pension funds available from prior long-term debt each allows for

[6] The year 2010 is omitted from the analysis, since it stands as a significant outlier for both state deficits and state economies.

[7] Changes in world energy prices are from Hamilton (2003). Changes in state productivity in manufacturing is estimated as $d\ln(\kappa_{st})/dt$, where $\ln(\kappa_{st})$ is computed as the residual from the state Cobb–Douglas production function linking state manufacturing output to state capital stocks and labor. Other within-state year controls generally found to be statistically insignificant and therefore excluded from our final results include decade-to-decade changes in the level of advanced education in the state (percent with college degrees or more) and in state urbanization (percent of population living in urban areas); losses from major natural disasters thought to impact the state economy; oil price changes interacted with whether the state could be considered a major consumer of energy; and population weighted changes in OwnD(−1) of the other states in each state's economic region as a control for potential fiscal competition among economic neighbors.

Table 2
State own deficits and state rate of job growth.[a] ($\dot{N}$: Mean=.018; S.D.=.024).

	$\dot{N}$ (OLS) (1)	$\dot{N}$ (GMM) (2)	$\dot{N}$ (GMM) (3)	$\dot{N}$ (GMM) (4)	$\dot{N}$ (GMM) (5)	$\dot{N}$ (GMM) (6)
OwnD(−1)	−.000002	.00004	.00004	.00003	.000005	–
(Mean=276; S.D.=500)	(.0000008)*	(.00002)**	(.00002)**	(.00001)**	(.000002)**	
ZAID(−1)	.000006	−.00006	−.00005	−.00004	.00002	−.00006
(Mean=476; S.D.=253)	(.000002)**	(.00004)	(.00004)	(.00002)*	(.00005)	(.00003)*
OwnNetRev(−1)	–	–	–	–	–	−.00004
(Mean=2537; S.D.=915)						(.00002)**
GovServices(−1)	–	–	–	–	–	.00003
(Mean=2001; S.D.=742)						(.00003)
IOSpillovers	–	–	.093	–	–	–
(Mean=.0067; S.D.=.014)			(.078)			
REGSpillovers	.634	–	–	.685	.887	.676
(Mean=.017; S.D.=.023)	(.033)**			(.091)**	(.050)**	(.609)
Year Fixed	Yes	Yes	Yes	Yes	No	Yes
IV F-Test for OwnD(−1)	–	10.06	10.06	10.06	10.06	–
IV F-Test for OwnNetRev(−1)	–	–	–	–	–	13.58
IV F-Test for GovServices(−1)	–	–	–	–	–	164.29
Arellano–Bond Test: AR(2)	–	.479	.389	.329	.312	.359
Hansen Exclusion Test	–	.732	.651	.785	.733	.515
Difference-in-Hansen Test	–	.765	.714	.745	.635	.515

[a] Dependent variable is the rate of job growth in the state. Sample includes the 48 mainland states for the years 1973–2009; means and standard deviations for all variables are for this sample. All regressions include state fixed effects; the GMM estimator uses a differenced specification to remove state fixed effects. All equations also include one and two lags of the dependent variable. Coefficients for the one-year lag of the dependent variable range from .47 (for REGSpillovers) to .60 (for all other specifications) and are statistically significant at the .95 level of confidence; the coefficient for the two-year lag is typically near −.10 and never statistically significant. Other variables included in all regressions are lagged measures of the spillover variable, a measure of shocks to state manufacturing productivity, and shocks to the price of oil interacted with whether the state is an oil-producing state. For the one regression excluding year fixed effects (col. 5) we included the national rate of unemployment as a control for the national economy and the coefficient estimate was negative and statistically significant. Standard errors are reported within parentheses.

* Significant at the .90 level of confidence.

** Significant at the .95 level of confidence.

funding current period OwnD. This first assumption is tested directly using the Stock–Yogo F test for weak instruments in the first stage of the IV regression. For our job growth equation we can reject the null hypothesis of weak instruments at the .95 level of confidence; see Table 2. For our population growth equation we can reject the null hypothesis of weak instruments at the .90 level of confidence; see Table 3. The second assumption is supported by the preponderance of macro-economic evidence for the time horizon of fiscal policy impacts as never longer than 16 quarters and by results here from the application of the Hansen and difference-in-Hansen tests for instrument exogeneity; see Tables 2 and 3. In particular, see Fig. 3, which shows the estimated impact of state own deficits on the state's economy as exhausted within two years.

Second, each of our growth equations uses state fixed effects to control for state unique variables (A_F, A_H, and L_s) setting initial conditions. To control for these state fixed effects, each growth equation is estimated in first differences as recommended by Caselli et al. (1996). Because our data and specification define a dynamic panel, however, the resulting error term in the first differenced estimating equation is likely to be correlated with the first differences of the lagged dependent variables. Thus, the estimated coefficients for the lagged dependent variables will be biased. Holtz-Eakin et al. (1988) provide an efficient GMM estimator for dynamic panels with fixed effects using long-lagged values of the dependent variables as instruments. There must be no evidence of serial correlation of the within state errors (ν_{st}) for the longer lags. Arellano and Bond (1991) develop the test for within-state serial correlation for second-order and longer lags, and we report those results for our GMM estimates; see Tables 2 and 3. The preferred GMM estimator also corrects standard errors for heteroscedasticity and first-order autocorrelation.[8]

4.2. Results

Col. (1) of Tables 2 and 3 provides the OLS estimates of the job growth and population growth equations, respectively. Both show a negative correlation between OwnD(−1) and the two outcome variables as would be expected when there are common omitted events causing both the local economy to decline and local deficits to rise. To control for this downward bias we instrument for OwnD(−1) using lags of OwnD(−1) from a previous legislative session, dated four, five, and six fiscal years prior. Prior to GMM estimation, we test for whether these instruments qualify as “strong” instruments for OwnD(−1) from the IV estimation of each equation. The resulting Stock–Yogo F test statistic for the null hypothesis of weak instruments is 10.06 for the job growth equation and 9.05 for the population growth equation; see Tables 2 and 3. The threshold value for

[8] We use the “two-step differenced GMM” estimator as proposed in Windmeijer (2005).

Table 3
State own deficits and state rate of population growth from net migration.[a] ($\dot{H}$: Mean=.005; S.D.=.010).

	$\dot{H}$ (OLS) (1)	$\dot{H}$ (GMM) (2)	$\dot{H}$ (GMM) (3)	$\dot{H}$ (GMM) (4)	$\dot{H}$ (GMM) (5)	$\dot{H}$ (GMM) (6)	$\Delta\mu$ (GMM) (7)
OwnD(−1)	−.0000004	.000005	.000007	.000005	−.0000007	–	−.0000009
(Mean=276; S.D.=500)	(.0000004)	(.000008)	(.000004)*	(.000006)	(.000002)		(.000004)
ZAID(−1)	.000003	.00002	.000009	.00001	−.00002	−.000002	−.000002
(Mean=476; S.D.=253)	(.000001)**	(.00002)	(.000009)	(.00002)	(.000008)**	(.00001)	(.00001)
OwnNetRev(−1)	–	–	–	–	–	−.00001	–
(Mean=2537; S.D.=915)						(.000007)	
GovServices(−1)	–	–	–	–	–	−.000005	–
(Mean=2001; S.D.=742)						(.000008)	
IOSpillovers	–	–	−2.004	–	–	–	–
(Mean =.0017; S.D.=.003)			(.937)**				
REGSpillovers	.345	–	–	.221	.231	.078	.426
(Mean =.0041; S.D.=.0067)	(.050)**			(.075)**	(.122)*	(.300)	(.074)**
Year Fixed	Yes	Yes	Yes	Yes	No	Yes	Yes
IV F-Test for OwnD(−1)	–	9.05	9.05	9.05	9.05	–	7.02
IV F-Test for OwnNetRev(−1)	–	–	–	–	–	16.83	–
IV F-Test for GovServices(−1)	–	–	–	–	–	188.89	–
Arellano–Bond Test: AR(2)	–	.240	.206	.262	.703	.597	.915
Hansen Exclusion Test	–	.366	.624	.489	.376	.732	.334
Difference-in-Hansen Test	–	.365	.458	.333	.282	.732	.578

[a] Dependent variable is the rate of population growth due to net migration into the state including immigrants from outside the United States. Sample includes the 48 mainland states for the years 1973–2009; means and standard deviations for all variables are for this sample. All regressions include state fixed effects; the GMM estimator uses a differenced specification to remove state fixed effects. All equations also include one and two lags of the dependent variable. Coefficients for the one-year lag of the dependent variable range from .60 (for REGSpillovers) to .90 (for all other specifications) and are statistically significant at the .95 level of confidence; the coefficient for the two-year lag is typically equal to −.20 and is never statistically significant. Other variables included in all regressions are lagged measures of the spillover variable, a measure of shocks to state manufacturing productivity, and shocks to the price of oil interacted with whether the state is an oil-producing state. For the one regression excluding year fixed effects (col. 5) we included the national rate of unemployment as a control for the national economy and the coefficient estimate was negative and statistically significant. Standard errors are reported within parentheses.

* Significant at the .90 level of confidence.

** Significant at the .95 level of confidence.

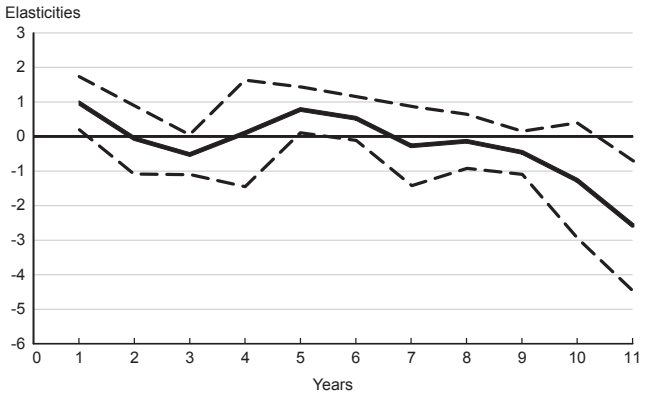


Fig. 3. Response of state job growth to an increase in state own deficits. Impulse responses are computed by local projection method for a 1% increase in state own deficits introduced in period 0. Responses are reported as a percentage increase in a state's own jobs. The dashed lines represent the 95% confidence bands for each projection.

the F statistic for 10% maximal relative bias between OLS and IV estimates is 9.08; see Stock et al. (2002). We conclude that lags of (−4) to (−6) for OwnD qualify as strong instruments. In addition, Tables 2 and 3 report the Arellano–Bond test statistic for the null hypothesis of no second-order autocorrelation of the residuals for each of our regressions. In each case we cannot reject the null hypothesis of no second-order serial correlation supporting the use of long-lagged values of the dependent variable as additional valid instruments.

For estimation of the job and population growth equations using OwnNetRev(−1) and GovServices(−1) as the two components of OwnD (−1), we use the (−4) to (−6) year lags of the two fiscal variables as instruments. In this case the Stock–Yogo F test statistics for lagged revenues and lagged spending as strong instruments are 13.58 and 164.29,

respectively, in the job growth equation and 16.83 and 188.89, respectively, in the population growth equation; see Tables 2 and 3; col. 6. We conclude that lags of (−4) to (−6) of OwnNetRev and GovServices are strong instruments for the two component fiscal variables of OwnD.

Also important for the validity of our instruments is that they are exogenous to the second-stage job and population growth equations. We use the Hansen test of overidentifying restrictions to test this assumption; see Tables 2 and 3. We cannot reject the null hypothesis that the instruments can be excluded from the second-stage equations. There is the potential problem in the GMM estimation of dynamic panels, however, that the Hansen test loses its power to identify exclusion when there are many instruments in the first stage where candidate instruments include long-lagged values of the dependent and other included exogenous variables; see Roodman (2008). Restricting the set of instruments can offer substantial gains in power for exclusion tests; see Bowsher (2002). We have done so by limiting the number of lags of the dependent and exogenous variables used as instruments in first-stage estimation to no more than three. In an alternative specification of our core model, we have also estimated our job and population growth equations excluding year fixed effects; see Tables 2 and 3; col. (5). Here too we fail to reject the null hypothesis of instrument exclusion from the second stage.[9] Finally, we report the test statistic for the difference-in-Hansen test for exclusion of only our primary instruments for OwnD(−1), OwnNetRev(−1) and GovServices(−1) specified as lags (−4) to (−6) of each variable; by this test too we fail to reject the null hypothesis that the instruments are excluded from the second-stage equations.

Our core results appear in Tables 2 and 3. After instrumenting for the endogeneity of OwnD(−1), we find that own state deficits now have a positive impact on state job growth one year after deficit spending; see Table 2; cols. (2)–(5). The GMM estimated coefficients are statistically significant at the 95% confidence level and the resulting estimated impact on job growth is economically important. For example, a one-standard-deviation increase in state deficits over the last ten years of our sample period of $390/person will increase the rate of job growth in the subsequent calendar year by .012 (=.00003 × $390; Table 2; col. 4) to perhaps .016 (=.00004 × $390; Table 2; cols. 2 and 3), compared to a sample mean rate of job growth of .018. For an average state in our sample period with employment of 2.816 million jobs, this is an increase of 33,800 (=.012 × 2.816 million) to 45,000 (=.016 × 2.816 million) state jobs. For our sample's average state with a population of 6.222 million residents, the present value cost/job ranges from $53,857/job to $71,809/job (=[$390/person × 6.222 million]/new jobs).

State deficits not only create jobs within the deficit state, but there is direct evidence of positive job spillovers to its economic neighbors. Both IOSpillovers and REGSpillovers show a positive effect on other states' job growth. The estimated effects are strongest, both statistically and economically, for neighbors within the economic region as defined in Table 1. The estimated coefficient for REGSpillover implies that a 1% increase in the rate of job growth among *all* of a state's *regional* neighbors will increase the state's rate of job growth by six-tenths of 1%. In contrast, the estimated coefficient for IOSpillovers implies that a 1% increase in the rate of job growth among *all* of the state's *national* neighbors will increase the state's rate of job growth by one-tenth of 1%. The larger estimated impact for regional spillovers seems plausible. The regional spillover variable is defined to include all economic activities between the states, including consumer demand, while the input–output measure is restricted to only production relationships between state firms.[10]

Table 2; col. (6) provides separate estimates for the fiscal components of OwnD(−1). State own deficits are defined as spending on government services less state own revenues by all funds. We follow the specification used in studies of the macro-economic effects of tax policy and define revenues as *net* state revenues specified as own state revenues collected *minus* all transfers paid to households including welfare payments, Medicaid payments, unemployment insurance, and pensions for retired public employees. We also deduct state payments to local governments, viewing such payments as transfers to households for local tax relief or local spending. As for own state deficits we lag the variable one year, denoted as *OwnNetRev*(*−1*). Spending on government services is also lagged, denoted as *GovServices*(*−1*), and now excludes all transfer payments. Service spending does include wages paid to state public employees, spending on purchased goods and services, and state infrastructure spending. As noted, we instrument for GovServices(−1) and OwnNetRev(−1) with their four- to six-year lags.

OwnNetRev(−1) has a statistically significant negative effect on state job growth. Raising state tax revenues depresses job growth, while returning revenues as transfers to households improves growth. The negative effect of net revenues on job growth is consistent with previous estimates for state and local economies (e.g., Helms, 1985) and for the macro-economy as

[9] While omitting year fixed effects gives us confidence that our primary instruments can be thought of as excluded from the growth equation, those variables are necessary to control for possible bias in our estimates of the marginal effects of OwnD(−1) on job and population growth. See footnote 10.

[10] Even the estimates for REGSpillovers may be an underestimate of the full spillover effects. Our referee suggested one additional test for aggregate spillover effects and that was to repeat our core regression but omit year fixed effects. Including year fixed effects not only controls for common macro-economic shocks to state economies but also for the level of deficits in all other states. To the extent that a state's own deficit correlates with other states' deficits in a year—i.e., all states trying to stimulate their economies—these "aggregate" effects will be captured as part of year fixed effects. By omitting year fixed effects, the OwnD(−1) variable could then include these correlated effects of other states' deficits. This alternative specification for job growth appears in Tables 2 and 3; col. 5. The estimated coefficient for OwnD(−1) remains positive and is statistically significant but is in fact smaller than the estimate with year fixed effects included. Even after including the national rate of unemployment in these regressions, we suspect that omitting controls for year-to-year variation in the aggregate economy leads to a downward bias in the estimate of the effects of OwnD(−1) on state job growth. When the aggregate economy is in decline, state job growth is low and state deficits are high. Including year fixed effects controls for this possible bias. Still, to the extent that our estimates with year fixed effects do control for the spillover benefits from other states' deficits, our reported estimates for OwnD(−1) should be viewed as a lower bound to total job effects when all states react in unison.

well (Romer and Romer, 2010). GovServices(−1) has a positive but statistically insignificant effect on state job growth.[11] This result too mirrors previous estimates; see Holtz-Eakin (1994). Importantly for our work here, the revenue and expenditure components provide impact estimates of the same order of magnitude and statistical significance as those for OwnD(−1).

The effects of OwnD(−1) on state population growth through net migration (including possible foreign migrants) are statistically insignificant or, if significant, quantitatively small in impact; see Table 3; cols. (2)–(5). Similar insignificant effects are observed when OwnD(−1) is separated into OwnNetRev(−1) and GovServices(−1); Table 3; col. (6). This makes sense if state job gains from the temporary deficit are thought to be short lived, a conclusion consistent with our impulse response function for the effects of OwnD on job growth in Fig. 3. Finally, Table 3; col. (7) provides an estimate of the impact of OwnD(−1) on the deficit state's rate of employment (μ) for those currently in the labor force. The effect is negative but statistically insignificant and quantitatively trivial.

Spillover effects of population growth outside the state are statistically significant, however, but small in magnitude as well and consistent with within-state effects. For the IOSpillovers specification, a temporary increase in OwnD(−1) leads to an increase in the deficit state's population and, from the negative coefficient on IOSpillovers, to a *decline* in the population of all other states nationally. For the REGSpillovers specification, a temporary increase in OwnD(−1) leads to more residents in the deficit state and, from the positive coefficient on REGSpillovers, to an *increase* in the populations of the other states in the economic region. Together the two results imply a gain in population for the deficit state and its regional neighbors and a population loss for all other states, a result suggestive of a region-wide job search by new residents.[12]

Combining the estimated effects of OwnD(−1) on new jobs, on new residents, and on changes in the rate of state employment allows us to estimate who benefits from the increase in state debt. Will it be current residents through protection of their existing jobs, current residents through new employment opportunities, or outside residents moving in to "steal" new state jobs? A simple example provides the answer. From Table 2, col. (4), we find that a one-standard-deviation increase in state deficits of $390 will increase the rate of state job growth by .012 ($=.00003\times\$390$) or, for the average state in our sample with 2.816 million jobs, an increase of 33,800 jobs. These new job are seen to have no impact on the rate of state employment, however; see Table 3; col. 7. Thus, the new jobs must be being filled by new residents or current state residents leaving "home production" and entering the labor force. New residents are estimated to take about 7200 of these new jobs. This follows from fact that the $390/person deficit increases own state population from net migration by about 17,000 residents.[13] If we assume new residents participate in the labor market at the same rate (=.45) as current resident and face the same rate of employment (.942) as current residents, then 7200 of the 17,000 new residents have jobs ($=17{,}000\times.45\times.942$). (Each new resident worker moves into the state with their family, who then account for the remaining 9800 new residents.) This leaves 26,600 (=33,800–7200) of the new jobs to be filled by current state residents who now enter the labor force. All told, 21% of the newly created jobs are filled by non-resident workers moving into the state, and 79% by current state residents who enter the labor force and find work. The primary beneficiaries of state own deficits are residents of the deficit state.

The gain in jobs is short-lived, however. Fig. 3 provides an estimate of the impulse response function using the direct projection method of Jordá (2005) for state employment growth following a one year, 1% increase in state own deficits represented as the percent increase in state job growth. The estimated effect is shown as a solid line and the 95% confidence interval as dashed lines. Three conclusions follow from these estimates. First as noted, the impact of a current period increase in state own deficits will increase state employment one year hence. An increase in a state's own deficit during FY 2008, for example, will increase jobs over the calendar year 2009, a response time as short as two quarters or as long as ten quarters. By calendar year 2010, however, there are no longer significant effects on state jobs. Second, the lack of a permanent effect on jobs suggests that historically at least state deficit financing has not been funding productive new state infrastructure (A_F) or residential amenities (A_H), leading to a permanently more attractive state economy. Third, the observed statistically significant *negative* effect of today's deficits on jobs eight to ten years from the date of deficit financing is consistent with jobs declining when taxes rise to repay debt. Job gains today are offset by job losses in the future.

[11] While not a central concern of this study, the estimated effect of non-matching federal-to-state aid, denoted as ZAid(−1) in Tables 2 and 3, deserves comment. The effect is generally negative and most often statistically insignificant. We are reluctant to conclude that such assistance depresses state economic growth. More likely such programs include a redistributive component that channels more aid to declining states. To correct for this downward bias one would need an instrument for ZAid. For more precise estimates of the effects of federal aid on local economies, see Suárez Serrato and Wingender (2011).

[12] Two calculations provide a sense of the magnitude of these estimated population effects. First, we estimate that with a one-standard-deviation increase in state own deficits of $390/person, state population growth rates will increase by a maximum of .0027 ($=.000007\times\$390$; Table 3; col. 3) and increase population in the average state by 17,000 new residents ($=.0027\times6.222$ million residents). Second, from the spillover variables we can compute the change in populations outside the state. For IOSpillovers, the average "weight" for one state's increase in population growth is .015 so that the deficit state's impact on the value of IOSpillovers will therefore be .00004 ($=.0027\times015$). From the estimated spillover effect of −2.004 for IOSpillovers, this means a decline in the population growth rate of each of the other states of about −.00008 ($=-2\times00004$) or about 500 residents ($=-.00008\times6.222$ million). The typical economic region has six states. Thus 42 states will be "sending" 500 residents each into the region for a total in-migration of 21,000 residents, 17,000 of whom will settle in the original deficit state and 4000 of whom will settle in the other five states. REGSpillover measures this population gain for the region's other states. The implied regional job search process is reminiscent of the 1950s "Caterpillar trail," where job-seekers from the South went first to Peoria, Illinois (home of Caterpillar Tractor), then to Gary, Chicago, Milwaukee, Green Bay, and finally Minneapolis—St. Paul in search of work.

[13] See footnote 12 above.

Table 4
State own deficits, state jobs, and benefits of cooperation: impact effects.[a]

Region	New England	Mideast	Southeast	Great lakes	Plains	Mountain north plains	Energy belt	Far west
Largest jobs state (ΔOwnD/Own Rev)	Massachusetts (.06)	New York (.07)	Florida (.15)	Illinois (.11)	Missouri (.14)	Idaho (.12)	Texas (.11)	California (.10)
Large state's own jobs	35,253	95,237	81,337	63,294	29,831	6850	113,563	158,483
(Cost/Job)	($72,103)	($79,063)	($89.467)	($78,851)	($77,934)	($88,502)	($85,175)	($90,956)
Job spillovers to other states	24,149	65,237	55,716	43,356	20,434	4692	77,791	108,561
(Cost/Job)	($0)	($0)	($0)	($0)	($0)	($0)	($0)	($0)
Region's total jobs	59,402	160,475	137,053	106,650	50,265	11,542	191,354	267,043
(Cost/Job)	($42,791)	($46,922)	($53,078)	($46,796)	($46,251)	($52,523)	($50,549)	($53,980)
Other states' own jobs	40,086	137,926	242,881	199,416	41,730	13,201	88,759	90.301
(Cost/Job)	($76,724)	($79,299)	($84,819)	($79,574)	($71,759)	($72,556)	($81,294)	($84,234)
Job spillovers to large state	15,286	48,756	46,429	38,907	14,936	3965	37,577	43,836
(Cost/Job)	($0)	($0)	($0)	($0)	($0)	($0)	($0)	($0)
Region's total jobs	67,544	232,406	409,254	336,015	70,315	22,245	149,559	152,158
(Cost/Job)	($45,534)	($47,061)	($47,061)	($47,225)	($42,587)	($43,057)	($48,245)	($49,990)
Regional policy jobs	126,946	392,881	546,307	442,665	120,580	33,787	340,913	419,201
(Cost/Job)	($44,250)	($47,004)	($51,025)	($47,121)	($44,114)	($46,293)	($49,538)	($52,532)

[a] Simulations are for an increase in OwnD(−1) in each region, both on own states and on neighbors based on estimates from Table 2, col. 4. The simulations are for a deficit increase equal to one S.D. of OwnD(−1) for the decade, 2000–2007: $390/person. Simulations are for a deficit increase in FY2008 impacting jobs in calendar 2009. Only impact effects one year following the increase in state deficits are reported here.

Interestingly perhaps, losses will occur after most governors have left office. This third result rejects strong Ricardian equivalence for state deficit financing.

5. Local deficits and local jobs

Table 4 provides a policy experiment based on the results in Table 2, asking what an increase in state own deficits would mean for jobs both in the deficit state(s) and in its surrounding economic neighbors. We focus on the largest state in each of the eight Crone economic regions; the economic neighbors are the other states within the region. The year chosen for the deficit increase is FY 2008, the first year of the Great Recession. Projected job impacts will occur over calendar year 2009.[14] The level of the simulated state deficit increase is set equal to $390/person, the one standard deviation of all state deficits over the period 2000–2007. These deficits may be funded by increases in the general fund deficit, the capital fund deficit, the unemployment insurance fund deficit, or the pension fund deficit. Use of the latter three funds for deficit financing is still available to states constrained by strong balanced budget rules on their general fund. This $390/person deficit is from 6 to as much as 15% of each of the largest state's own fund revenues in FY 2008 and will imply a sizeable increase in state spending and transfers or a significant reduction in state taxes and fees.

Three alternative simulations are presented in Table 4. The upper panel of Table 4 illustrates the impact on state jobs of a deficit increase in a region's largest state alone, with no new deficits by its (Crone) regional neighbors. The middle panel shows the impact of deficits by all other states in the region, *except* the largest state. These two panels illustrate the potential spillovers across states, first from the largest state to its neighbors and then from the neighbors to the largest state. The lower panel shows the increase in region jobs if all states in the region agree to cooperate in a common policy where each state increases its own deficit by $390/person.

A $390/person deficit is estimated to lead to an increase of .011 in the deficit state's rate of job growth, which leads in turn to the change in jobs reported in Table 4.[15] The job growth in the deficit state then spills over into neighboring states through changes in the growth of regional jobs surrounding each neighbor, measured by a change in REGSpillovers. This change, which varies by each neighboring state, allows a prediction of new job growth and thus new jobs in each neighboring state. Those new jobs are summed to provide an estimate of the overall level of job spillovers.[16] Finally, the large state's own jobs and the spillover jobs are summed to give the total jobs created in the region. We also report the

[14] There is no evidence of significant job creation after the first year following the temporary increase in state own deficits; see Fig. 3.

[15] Jobs estimates follow from estimates in Table 2; col. 4. First, we compute the increase in the rate of job growth as Δjob growth = .0109 = .000028 × $390/person. (The coefficients in Table 2 have been rounded to save space.) The predicted rate of job growth is then multiplied by actual employment in each state in 2009 to obtain the predicted level of new jobs.

[16] The largest state's predicted increase in employment is added to existing growth for each of the state's regional neighbors to obtain a new estimate of each neighboring state's REGSpillovers. That value is then multiplied by the estimated coefficient for impact of REGSpillovers on job growth from Table 2; col. (4) (=.685) to obtain a new value of each neighbor's job growth. Each neighbor's new jobs are then estimated as the new growth rate times the neighbor's existing employment in 2009. Neighbors' new jobs are then summed to obtain the estimate of "spillovers to other states" reported in Table 4.

present value tax cost/job created defined as the total level of deficits in the largest state divided by jobs created. For the second panel, jobs created and the present value cost/job from increasing deficits in each region's smaller states, but now excluding the largest state, are computed in a similar way. Finally, the third panel aggregates the results of the upper two panels to show the impact on total regional jobs and the average tax cost/job if all states agreed to jointly increase their deficits by $390/person.

Three conclusions are evident from Table 4. First, from the first row of the first two panels (own jobs), state own deficits can create state jobs, at least for one year. But sizeable deficits will be required. The present value tax cost/job created *in the state* for the state running the deficits ranges from $72,000 per job in Massachusetts to $91,000 per job in California. These cost estimates are comparable to those obtained by the recent evaluative literature of the impact of the American Reinvestment and Recovery Act's fiscal assistance to state and local governments on local job growth; see, for example, Wilson (2012).

Second, there can be significant aggregate job spillovers onto the deficit state's neighbors. The costs of these "spillover jobs" will be $0 for the neighbors. This spillover benefit creates a strong incentive for the neighbors of the largest state to free-ride on that state's deficit behavior. For comparable deficit levels, large states can often create more jobs for its neighbors than the neighbors can create for themselves. For example, in the Far West region, spillovers from California (108,561) exceed its neighbors' own job creation (90,301).

The second panel in Table 4 shows that the incentive to free-ride runs both ways. If the largest state's neighbors were to collectively increase their deficits, but the large state did not, then the large state would receive the spillover jobs. In this panel, the region's total jobs will be the sum of the jobs created in the small states plus the spillover jobs created in the large state plus the spillover jobs created amongst all the small states (in the total, but not shown separately). With significant spillovers, all states may choose to "sit on their hands", hoping that the other states in the region will run deficits in times of recessions. Or if each state does run a deficit to create jobs within the state—as would occur if the state benefits of a new job exceed the state's own tax cost/job—there will likely be a downward adjustment in own state's deficit behavior to the job spillovers received from the other states. The Nash equilibrium to such a "race to the bottom" policy game will be Pareto inferior to a cooperative allocation in which all states collectively agree to deficit finance region-wide job creation; see Pauly (1970).

Third, the lower panel of Table 4 shows the outcome of such a cooperative policy, if all states in the region agreed to run a common $390/person deficit. Total jobs created is the sum of total jobs created when the two sets of governments operated separately (region's total jobs) and tax cost/job becomes the weighted average of cost/job after allowing for spillovers. The tax cost/job under the cooperative policy is significantly lower than when each state, or set of states, operated independently ignoring spillovers. For example, in the New England region the "private" cost/job to Massachusetts of the deficit policy would be $72,103, or to the other smaller states $76,724, but working together and allowing for spillover jobs, the "social" cost/job falls to $44,250. A deficit policy that was not attractive for an individual state when comparing the benefits of a state job to its own tax costs may become attractive when all states agree to cooperate and collectively share the deficit costs of job creation.[17] If so, then there is an argument for centralizing stabilization fiscal policy.

6. Conclusion

The accepted wisdom for managing stabilization policy in an economic and monetary union, as summarized by the opening quote from Oates, assigns these policy responsibilities to the central government for either or both of two reasons. First, no state can have a significant impact on its own level of economic activity as firms and households within the state purchase much of their inputs and consumption from producers outside the state. As a result, the primary beneficiary of any temporary fiscal stimulus will be workers outside the state because of economic spillovers. Second, even without significant spillovers, workers from other states may move into the state adopting the fiscal stimulus with the consequence that job opportunities will not improve for state residents. In either case, state officials may under-provide job creation policies for their residents. If so, only the central government can implement efficient macro-stabilization policies.

Our results here suggest that for the US economic union the familiar conclusion is correct. While states can implement effective deficit policies and the primary beneficiaries of the resulting increases in state jobs will be state residents, the present value costs per job of such policies to each individual state—ranging from $70,000 to $90,000 per job may exceed the benefits of the jobs created. After allowing for the resulting positive job spillovers to neighboring states, however, the social cost of created jobs falls to $45,000–$50,000 per job. What may not be optimal from the perspective on an individual state may be justified collectively because of spillovers. If so, only a central government, or a fiscal treaty as in the case of the European Union, will be capable of coordinating state deficits for efficient fiscal stabilization policies.

[17] Though that decision must ultimately rest on a comparison of the social benefits of job creation to the social costs of the increase in state own deficits. Whether the benefits of a created job exceed estimated social costs remains an open question. For example, as part of an effort to understand fluctuations in employment rates, Hall and Milgrom (2008) Table 2) estimate the annual (flow) benefits to a risk-neutral worker of remaining unemployed (and searching or providing home production) as 70% of the overall gain in added output. The net social surplus of moving from unemployed to employed would therefore be 30% of the worker's added output. It is this net output benefit from job gains today offset by job losses tomorrow that must be discounted and then compared to the present value of all taxes (including excess burdens) needed to finance today's increase in state own deficits.

Whether central government policy-makers will be able to achieve efficient coordination of state fiscal policies is another matter, however. Within fiscal unions, the central government typically relies on state or provincial governments for the implementation of its domestic fiscal policies, including transfers to households. Rather than spending funds as the central government desires, states often have their own policy agendas. Having now examined states as their own "principals", the next task for understanding stabilization policy in economic unions should be to evaluate states as "agents" for central government fiscal policies.

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