Local Deficits and Aggregate Stabilization: Evidence From U.S. States

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November, 2012

ABSTRACT

Using a sample of the 48 mainland U.S. states for the period 1973-2009, we study the ability of states to expand own state employment through the use of state deficit policies. The analysis allows for the facts that U.S. 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. We test for spillovers using three definition of neighbors: geographic proximity, input-output connections, and common business cycle patterns. We find only limited ability of U.S. states to impact their own or their neighbors' economies through state deficit policies. The implied cost per job created ranges from $340,000 to over $1 million.

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[For Discussion Only]

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The European Union's recent struggles to manage the public debts of its member countries raises in a fresh light an old issue of fiscal federalism: What is the appropriate role of fiscal policy for member countries in a monetary union? The received doctrine of fiscal federalism, which the Union has adopted under the title of *subsidiarity*, stresses the virtues of decentralized decision-making for mapping citizen preferences to the financing and provision of public services. Taxes by each member country should be designed to be as close as possible to benefit payments, services should be provided to meet the revealed demands of country citizens, and public debt should be used to smooth citizen tax payments for large lumpy investments or to ease the economic consequences of temporary economic (recessions), political (wars), or natural (disasters) crises. A central government, however configured, should step in when there are significant economic spillovers between the policy choices of member countries.

From these broad precepts follow specific recommendations for the management of each tool of fiscal policy. This paper examines the arguments for and against the use of country-specific deficits for the management of short-run macro-economic performance, specifically, returning country labor markets to full employment and output to its long-run growth path following an adverse, asymmetric economic shock impacting the local economy. The analysis is for open economies within monetary unions and explicitly allows for the fact that within such unions the real effects of member country deficits may "spillover" to impact the economies of both geographic and economic (i.e., trading) neighbors. If such spillovers occur and are economically significant then there is a case for coordinated, central government deficit policies for managing union-wide adverse shocks to member economies – that is, a fiscal union.

The presumption in the established public finance literature is that local deficits in monetary unions

will have no significant effect on either local, or union-wide, aggregate demand and thus on the temporary paths of employment and output, again either locally or globally. Any successful demand stimulus within the local economy will dissipate through the wider union economy as local firms and households buy (i.e., import) goods and services globally or re-locate from declining to expanding economies. Further, the comparatively small local deficits will have only trivial impacts on union-wide aggregate demand. That said, local deficits may remain a tempting local policy to redistribute income to current residents, either as a transfer from future residents or, in the case of defaults and bailouts, from other union members to the deficit state. The efficiency consequences of such local policies may be significant. Wallace 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 policies and fiscal programs involving deficit finance; consequently, the stabilization problem must be resolved primarily at the central government level. (p. 30).

We test empirically the assumptions behind this familiar conclusion.[1] Using United States data from 1973-2009 for the 48 mainland states, we first examine the impact of state government deficits on each individual state's own rate of employment of its work force, establishment employment growth, and population growth due to net migration. We find state deficits do have a positive impact on the rate of employment in the state and the rate of growth of state employment. Deficits do not have an impact on net migration into the state, at least over the horizon we study here. In addition, we specify three alternative variables to measure the connection of the state's economy to the economies of other states' as a means for

[1] Gramlich (1987) provides the only other empirically based analysis of the usual conclusion and concludes that in a two state federal economy calibrated to estimated import-export propensities between the regions that each of the two states would find it in their own domestic interests to adjust deficits to smooth shocks to local unemployment. While each state would like to use local deficits to smooth asymmetric economic shocks, there are significant "beggar-thy-neighbor" spillovers between the states suggesting an inefficient over-use of local deficits. Thus the need to centralize deficit policies remains. On the more general problem of coordination of deficit policies between open economies *outside of monetary unions*, see Mundell (1963), Kehoe (1987) and Chari and Kehoe (1990).

testing for economic spillovers of state fiscal deficits.

The first spillover measure called "spatial distance" assumes state economies are connected by distance alone and is a variant of the familiar spatial weighting for independent variables as developed by Cliff and Ord (1981), Anselin (1988), and Case (1991). The presumption here is that transportation costs proportional to distance define the economic connections between states. The variable reveals no significant spillovers between states for employment rates or growth, but there is a significant connection for net migration. The second measure called "economic distance" assumes states are connected by their production functions, as specified by the 2010 Bureau of Economic Analysis national input-output table. The presumption here is that expanded production in one state requires expanded production by its main supplier states. We find statistically weak and economically modest spillover effects in this case. The third measure called "economic region" groups states into eight regions based upon similarities of the states' underlying business cycles, weighting each state in the region according to the overall level of employment (for employment rate and growth of employment) or population (for net migration) for the other states within the region. Here we find statistically significant, and now more economically important, spillovers from one state's economy to its neighbors. In all cases, however, the aggregate effects of state deficits on a state's own economy or its neighbors is modest, even for large deviations in state deficits from their sample means. Implied income multipliers are very small, never larger than .35. The results are consistent with either of three propositions: (i) small open economies spending a significant fraction of their consumption dollar on "imported" goods; (ii) government deficits spent on inelastically supplied "nontradables" such as local construction or state and local government services; and/or (iii) local firms and households that recognize the tax implications of the deficits and adjust investment and savings behaviors accordingly.

Section II briefly reviews what we now know about how local fiscal policies impact local economies in fiscal unions. Recent results evaluating individual state and federally financed subsidies to local businesses show such subsidies can have a statistically and economically significant impact on local employment. These

local results suggest a more systematic economy-wide empirical analysis of the Oates presumption is in order. Section III outlines our estimation strategy, defines our key deficit variable, and specifies each of our three measures for economic spillovers. Section IV provides our central empirical results. Section V concludes with a few brief comments for how the U.S. evidence might inform the current debate within in other federal economies, and perhaps the European Union going forward, for the appropriate design of deficit policies in times of economic downturns.

II. Can Local Fiscal Policies Impact Local Economies?

Beginning with the important insights of Charles Tiebout (1956), public finance economists have appreciated the importance of taxes, services, and (more recently) deficits for the performance of the private economy within the boundaries of the local government. Public services are important inputs to household welfare and firm production. Taxes are a burden on household private incomes and firm profits. Deficits signal future taxes or service reductions and possible declines in future welfare and profits. In federal economies with mobile households, labor, and capital among government jurisdictions, a government's public finances will impact private decisions to consume, work, save, and invest, and perhaps most importantly, where to locate when making those decisions.

In an important pair of papers, Rosen (1979) and Roback (1982) provide an equilibrium framework for firm and household location in an open economy with an explicit specification for the impact of government fiscal policy on those decisions. Fiscal policies where service benefits more than compensate for current and future taxes will attract firms and households to the local jurisdiction leading to an increase in local land prices and, if increases in firm demand exceed the equilibrium increase in labor supply, local wages as well. Conversely so, for inefficient public policies. Haughwout and Inman (2001) have calibrated this model for a paradigmatic local public economy and find the impacts on land prices and local wages of even modestly inefficient fiscal policies – local taxes not matched by fully compensating benefits – can be sizeable. There is now a vast supporting literature estimating the effects of local taxes and local services on

household mobility, firm location, land prices, and local wages. There is little doubt that local fiscal policy matters for the economic performance of local private economies. The only question is: How much? That is our concern here. Specifically, can local deficit financing increase local employment in economic downturns?

The recent recession and the passage of the American Recovery and Reinvestment Act (ARRA) of 2009 has provided a unique opportunity to begin this analysis. ARRA has provided a significant inflow of revenues into local economies for the provision of public infrastructure and support for public employee salaries (primarily teachers), state unemployment benefits, and health services for lower income households (Medicaid). Total transfers to state and local governments over the two and half years of the policy is nearly $280 billion dollars, financed by increases in the federal deficit. Four studies deserve mention.[2] Each studies the change in local jobs after the introduction of ARRA's deficit financed increases in local government budgets. Arguably, ARRA funding has been an exogenous increase in local resources. The law was passed within the first six weeks of the Obama Administration and had not been anticipated by local officials.[3] Allocation of funding was based upon historically specified grant formulas or tied to the importance of locally elected officials in federal budgeting; see Inman (2010). The authors view ARRA funding as a quasi-experimental treatment and use variation in the level of funding across states and counties to reveal the one-year impact of temporary, deficit financed spending on local economies.

Feyrer and Sacerdote (2011) provide an overview analysis of the effects of ARRA on state and county job growth from the date of passage on February 23, 2009 to the end of October, 2010. The authors

[2] Three additional, recent studies have estimated the effect of direct federal spending on local economies. Nakamura and Steinsson (2011) estimate the impact of federal defense spending on state economies and find an output multiplier of about 2 with impacts nearer 3 during recessions. Suárez-Serrato and Wingender (2011) study the impact of direct federal income transfers to households and firms and find local income multipliers also near 2. Fishback and Kachanovskaya (2010) estimate the impact on state economies during the Great Depression of federal spending for farm subsidies and public works projects. They find income multipliers ranging from .9 to 1.7; the impact of federal spending on local employment, however, was never significant.

[3] For the political (narrative) history of ARRA, see Grunwald (2012, Chapters 6-9).

find from their cross-section study of states that each $100,000 of ARRA funding creates from .5 to perhaps 2.0 jobs. The county level analysis, however, finds much smaller effects – only .04 to .15 jobs per $100,000 of ARRA spending – suggesting possibly significant spillover effects across county jurisdictions within a state. Important for understanding our results below, the effects were strongest for targeted ARRA support of Medicaid, weakest for general funding for public education.[4] Unconstrained grants to local governments created only .2 jobs per $100,000 of funding, suggesting a cost/job of about $500,000.

A closer look at the impact of ARRA funding is provided by Wilson (forthcoming) and Chodorow-Reich, et. al. (2011). Wilson (Table 7) finds a significant impact of promised funds on employment growth. Each additional $100,000 of announced ARRA spending creates from .43 (OLS estimates) to .80 (IV estimates) total new jobs implying a cost per job of from $232,560 to perhaps as low as $125,000. Disaggregating the .80 total of new jobs by employment sectors, Wilson finds that each $100,000 of ARRA funding adds .08 jobs in manufacturing, .30 jobs in state and local government employment, and .36 jobs in construction. If the construction jobs created are in response to ARRA funding of governmental infrastructure then over 80 percent of the new jobs created are arguably employment directly tied to state and local budgets.

Chodorow-Reich et. al. (2011; Table 8) examine the impact on local jobs of expanded matching aid for state Medicaid expenditures. They find a strong effect on overall government employment, roughly 1.5 jobs for every $100,000 of additional assistance for state Medicaid funding. In the private health care sector itself, however, only .3 additional jobs are created. There is some "spillover" of funding into education, creating an additional .3 private sector jobs in this sector. Overall they estimate that roughly two jobs have been created for every $100,000 of ARRA funding targeted for Medicaid support, implying a cost of

[4] This distinction between targeted Medicaid assistance and unconstrained general transfers is an important one for evaluating the overall impact of ARRA funding. Additional ARRA assistance for Medicaid was given as a temporary increase in the federal matching rate for state spending on Medicaid services. This is a price grant and may have a significant price effect to encourage additional state spending. However, general fund support, such as that for education, is an income grant which may be saved or spent. For evidence that the savings response can be important, see Carlino and Inman (2012a).

$50,000/job. As from Wilson's study most of the job creation is in the governmental sector.[5]

Finally, Conley and Dupor (2011) estimate the impact of ARRA funding on changes in state employment in each of four sectors of the economy: state and local government employment including teachers, private sector service employment (primarily in education and health care), goods production, and federal government employment. Consistent with the results of Wilson and Chodorow-Reich, et. al., they find only state and local government employment is significantly increased. Their best estimate of net jobs created or saved is 685,000 at a cost of approximately $395,000/job.[6] Though total jobs are increased, the striking result from this study is the strong and statistically significant negative effect of ARRA funding on the level of private sector service jobs. Why? The work of Cohen, et. al. (2011) suggests an answer. These authors examine the effect of plausibly exogenous increases in federal spending on a state's economy arising from exogenous changes in congressional committee chairmanships. They find that public funds spent on public infrastructure or increases in public employment crowd out firm investment and private sector employment within the state. Increased spending by the public sector raise local wages and thus firm costs, leading to the exit of private capital and a decline in private sector jobs. These offsetting supply side effects are less in times of recessions, but remain significant.

As a group these four studies suggest deficit financed local spending can increase local employment in the short-run. The effect is strongest on public sector employment with possible offsets in competitive private sector service employment. Deficit costs per job created range from $50,000/job (Chodorow, et. al.)

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[5] The fact that most of the new jobs created (saved) are in the state and local government sector generally is consistent with the use of a targeted matching grant for a service with a relatively low price elasticity of demand. In this case, most of the funds received from the price subsidy become "released" funds or what public finance economists called "fungible aid" that can then be spent on other government services or returned to taxpayers as tax relief. Conley and Dupor (2011) note that a Council of Economic Advisors report (July 14, 2010) comments that ARRA Medicaid funding was "intended to boost the level of discretionary funds available to states and not simply to relieve Medicaid burdens." That is, even targeted Medicaid assistance was viewed as fungible. Carlino and Inman (2012) provide direct evidence on the point using estimates of state budgeting behavior in response to adjusted federal matching rates.

[6] Conley and Dupor do not provide an estimate of cost per job. However, mean ARRA spending per state is $5.9 billion for the 46 states in their sample. Total spending is therefore $271 billion. Dividing by 685,000 jobs implies a cost per job of $395,000/job.

to perhaps as much as $400,000/job (Conley and Dupor, Feyrer and Sacerdote). These results provide a useful benchmark against which we will evaluate the plausibility of our own results. They do not, however, directly address our question: Can *local* government deficits create local jobs and are there significant spillovers across jurisdictions?

For our question Clemens and Miran (2011) offer the first empirical estimates of the impact of exogenous changes in state deficits on a state's economy. Unexpected shocks to state deficits are estimated as the difference between unexpected levels of state spending minus unexpected levels of state revenues, where expected levels are from fiscal year forecasts for the sample states from the National Association of State Budget Officers (NASBO) annual survey of state budget officers. These unexpected deficits will need to be adjusted over the course of a fiscal year depending on the strength of the states balanced budget rules; see Bohn and Inman (1996). State with weak balanced budget rules will have larger end of year deficits than states with more stringent rules. Weak rule states do not cut spending as much as do states with strong balanced budget rules. Thus unexpected deficits represent additional state spending. Clemens and Miran estimate the marginal effect of this additional, deficit financed spending on state personal income and find an income multipliers ranging from .1 to .5, most centering on .3, but with wide confidence intervals. Clemens and Miran's results are directly comparable to our own reported below.

III. Local Deficits and Local Economies

A. *The Local Economy*: Our understanding of the potential impact of local deficits on the local economy builds off the Rosen-Roback framework of labor markets in open economies as specified in Haughwout and Inman (2001).[7] Demand for labor is by firms producing goods (X) for a competitive world market selling at the world price (p). Local jurisdictions within the monetary union can produce different

[7] Gali and Monacelli (2008) provide alternative structural model of the impacts of fiscal policy in a network of small open economies. Both our model and theirs allow for common monetary policy setting price levels and for the free mobility of capital across jurisdictions. Our model also allows for free mobility of labor while their model holds households in their original local jurisdiction. Their model use monopolistic competition to determine market outcomes while we allow perfectly competitive firms but constraint on available productive land to set equilibrium output levels.

products, but all jurisdictions compete in world markets. 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. The price of capital ® is set in the world market, influenced by monetary policy of the monetary union. In addition firms pay a tax per unit output (t_F) and obtain productive public goods and services (G_F) from the local government impacting firm production as Hick's neutral technology. The demand for labor (n), land (ℓ), and capital (k) per unit output is specified as:

$$n = n(W, R; t_F, G_F; r, p, A_F);$$

$$\ell_f = \ell_f(W, R; t_F, G_F; r, p, A_F); \text{ and,}$$

$$k = k(W, R; t_F, G_F; r, p, A_F).$$

Together, factor prices and factor demands determine the average cost (c) of producing a unit of output. Profits will equal firm average revenues (p) minus average costs (c) minus taxes per unit output (t_f). In equilibrium, firms are not allowed to make profits above the competitive rate of return:

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

Labor is supplied by each resident household at a constant level of hours per year; there is no labor-leisure trade-off. Each working resident earns the market wage (W) and allocates that income less local taxes (t_H) to the consumption of the local economy's produced good (x), to housing services (h), and to the purchase of land (ℓ_H). Allocation conforms to the maximization of a household utility function specified as $V(x, h, \ell_H; G_H, A_H)$, where in addition purchased goods (x, h, ℓ_H) each household receives utility from locally provided residential public goods and services (G_F) and local residential "amenities" (A_H). Households pay the world price (p) for the locally produced good (x), a rental price (R) for housing services (h), and the local market's price for land (R). The household's demand for residential goods is specified as:

$$x = x(W, R; t_H, G_H; r, p, A_H);$$

$$\ell_H = \ell_f(W, R; t_H, G_H; r, p, A_H); \text{ and}$$

$$h = h(W, R; t_H, G_H; r, p, A_H).$$

Together the demands imply an indirect utility function specified as:

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

where in equilibrium every household in a local jurisdiction must receive at least the exogenously determined level of utility ($\hat{U}$) available in all other jurisdictions within the union. Thus full labor market mobility is assumed.

Equilibrium allocations within the local jurisdiction are determined in two steps. First, equations (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 ® and p), and local production and residential amenities (A_F, A_H):

$$W = W(t_F, G_F, t_H, G_H; r. p; A_F, A_H) \textit{ and } R = R(t_F, G_F, t_H, G_H; r. p; A_F, A_H). \quad (3)$$

Second, aggregate output and thus the final size of the local economy is determined by the availability of productive land within the jurisdiction, denoted as $\mathcal{L}_s$. Demand for land depends upon the demands by individual firms (ℓ_F) times the output of local firms (X) plus the demand by individual households (ℓ_H) times the number of households in the jurisdiction (H): $\mathcal{L}_d = \ell_F \bullet X + \ell_H \bullet H$. Finally we assume there is an exogenously given search rate of unemployment ($\tilde{u}$) that relates the total number of households to total labor supplied to firms: $H\bullet(1 - \tilde{u}) = n\bullet X$. Thus, $\mathcal{L}_d = [\ell_F + \ell_H \bullet n/(1 - \tilde{u})]\bullet X$.

Equating the demand for land to the exogenous supply of land implies total equilibrium production for the jurisdiction of:

$$X = X(t_F, G_F, t_H, G_H; r. p; A_F, A_H,, \mathcal{L}_s);$$

total equilibrium employment for the region of:

$$N = n\bullet X = N(t_F, G_F, t_H, G_H; r. p; A_F, A_H,, \mathcal{L}_s); \qquad (4)$$

and total equilibrium households for the region of:

$$H = N/(1 - \tilde{u}) = N(t_F, G_F, t_H, G_H; r. p; A_F, A_H,, \mathcal{L}_s)/(1 - \tilde{u}). \qquad (5)$$

We propose to estimate equations (4) and (5) along with a short-run employment rate (e) equation allowing

for the possibility of unexpected labor demand shocks to the local economy (Ñ) and temporary Keynesian "sticky" wages. The result is a temporary deviation, denoted as Δu, from the search rate of unemployment; Δu may be positive (adverse shock) or negative (favorable shock).[8] The resulting employment rate equation will be specified as:

$$e = (1 - [\tilde{u} + \Delta u]) = E(t_F, G_F, t_H, G_H; r.\ p; A_F, A_H,, \mathcal{L}_{s,}\ \tilde{u}; \tilde{N}). \qquad (6)$$

For our analysis we focus on the net impact of taxes and spending as specified by aggregate local deficits (D), defined here as $D = (G_F + G_H) - (t_F \bullet X + t_H)$. A positive impact of local deficits on the local private economy may occur in either of two cases. First, if firms and households fully anticipate the future tax burden of current deficits – the case of Ricardian equivalence – and local services add more to productive efficiency and household utility than current and anticipated taxes, then firms will increase their demand for labor and households and workers will be attracted to the region. In equilibrium N and H both rise and perhaps for the short-run, as household mobility responds more slowly than firm demand, the rate of employment increases above that implied by the rate of search unemployment (i.e., $\Delta u < 0$). But if the productive and utility benefits of public services do not compensate for the burden of deficit's future taxes, then firms and households will leave the jurisdiction and N and H both fall, and perhaps the short-run rate of employment as well ($\Delta u > 0$).

In the second case where firms and households do not anticipate the future tax burdens of current deficits. Deficits are seen as fiscal transfers from future residents, or in the case of bailouts, from residents of the full monetary union. A reduction in the output tax on firms increases firm profits attracting firms into the region thereby expanding the aggregate demand for labor. If there is a corresponding "free" increase in $G_{F,\backslash}$, and G_F enhances the overall productivity of firm labor, then again the demand for labor increases. The reduction in residential taxes and any "free" increase in residential services will attract households into the region. Again, both N and H increase as the local deficit rises. This may or may not be an equilibrium

[8] See Blanchard and Katz (1992) for a specification of such an adjustment process. .

outcome. If local deficits must eventually be paid by local residents and firms, then at some future date when the required tax burdens are realized, there will be an offsetting decline in N and H. [9] In the short-run, however, N and H may increase and the short-run rate of employment may fall as well.[10]

Because capital and labor are assumed fully mobile within a monetary *cum* economic union, deficit created economic expansions in one jurisdiction may have implications for the level of employment and households in other jurisdictions. These spillover effects may be negative or positive. In our first case where deficits in one jurisdiction are fully recognized by local firms and households and the funds are spent on profit improving and utility enhancing local public services, then this improved fiscal performance may attract households and firms from other jurisdictions and employment and residents may decline elsewhere, and in the short-run, perhaps also local rates of employment. These are efficient re-allocations of union-wide resources.

In the second case where future tax burdens of local deficits are not anticipated, however, spillovers may be positive. Again, households and firms may relocate to the deficit jurisdiction, but the deficit financed service increase and/or tax reductions may also increase local demand for private goods and services (x). If the deficit state is a net importer of private goods – $x \bullet H > X$ – then rising local demands will stimulate production in other jurisdictions. In this case the levels of employment, population, and perhaps the short-run employment rate in demand favored neighboring communities may rise. We will seek to specify each local jurisdiction's economic "neighbors" and then test for negative or positive deficit-induced spillovers onto employment, households, and the short-run employment rate.

Table 1 summarizes the predicted impact of local deficits in these two cases, first for the deficit

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[9] These future taxes will be paid in either of two ways. First, directly, when the deficits must be re-paid. Or indirectly as a fall in R when the deficit burden is finally realized by firms and households considering the attractiveness of the local jurisdiction. In this case, future deficits are capitalized into the value of land, or more generally, any fixed assets in place at the time the burdened is realized. What happens to wages in the long-run is less obvious as both the demand and supply of labor decline.

[10] These short-run effects are equivalent to those observed for an increase, and then decrease, in central government fiscal transfers into the local public economy; see Haughwout and Inman (2001; Tables 6 and 7).

jurisdiction and then for its neighbors.

B. Measuring Deficits and Spillovers: Central to our analysis is the definition and measurement of state deficits (D). Here we measure deficits as the difference between all state expenditures and transfers and all current revenues for all state funds including the state's general fund, the state's capital fund, all state insurance funds, and state administered pension funds. Included in a state's general fund expenditures are 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. Included in capital fund expenditures are all new construction. Included in insurance fund expenditures are all payments to households for workmen's compensation and for unemployment benefits. Included in pension fund expenditures are all payments to retirees and disabled public employees. Counted as current revenues for the general fund are all taxes, fees, licenses, interest earnings from non-insurance funds (i.e., "rainy day" funds), and federal aid paid to the general fund. Counted as current revenues to the capital fund are interest earnings on bond funds. Counted as revenues to insurance funds are contributions from state employers. Counted as revenues to the state pension fund are contributions from covered employees, from local governments whose employees are members of the state pension plan, and the state's own contributions. Since these revenues are also counted as a state general fund expenditures, these payments are an internal transfer and have no implication for the aggregate state deficit. This expanded measure of a state's deficit on the state's private economy is meant to capture the potential impact of deficit financing from all channels. Importantly, even when a state's general fund budget is constrained by an effective balanced budget rule, the state's capital, insurance trust, and pension budgets are not.

We make one adjustment to our specification of state deficits when estimating our employment, household, and employment rate equations. We remove all federal aid from state revenues, thus re-defining state deficits as the states's *own deficit*, denoted *OWN-D*. We do so for three 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 choices. Second, D and OWN-D are both likely to be endogenous to our analysis of state deficits' impacts on state private economies. We will need instruments that can isolate the exogenous changes in state deficits so that consistent estimates of deficit's effects on N, H, and e can be obtained. Those instruments are available from the cross-state variation in state fiscal environments. Third, we do include the level of exogenous federal aid received by the state, denoted as *Z-AID*, as a separate regressor in our estimation. Z-AID excludes all matching assistance tied to the level of state spending.[11] The estimated marginal effect of Z-AID on local jobs and local employment rates will be informative as to the potential job creation benefits of such assistance within a federal fiscal stimulus strategy. We will lag each fiscal variable one year when estimating their separate effects on state employment, population as affected by net migration, and the rate of employment. Both OWN-D and Z-AID are measured in real dollars per capita, with our state specific price deflator set equal to 1.00 for Wyoming in 2004.

The potential spillovers of deficit policies requires a choice of economic neighbors for the original local jurisdiction. We use three alternative measures.[12] First, following the original approach of Case (1991) we specify neighbors as those that are geographically close to one another, assuming transportation costs for goods and households is the important determinant of economic interactions. A "spatial" weighting matrix is specified to link each state (s, representing a "sell" state) to distance weighted average of the economic

[11] Included as exogenous federal aid to states are all federal programs paid to states including those with explicit "pass-through" requirements for the states to fund local governments, for example, programs funded by the Elementary and Secondary Education Act. The state treats these programs as potential substitutes for its own support of local schools; see Craig and Inman (1982). Excluded from exogenous federal aid are Medicaid funding, AFDC funding (to 1996), and funding for highways; each of these programs involved matching 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 exogenous federal aid. See Carlino and Inman (2012a) for a complete description of federal aid programs to state governments used in this analysis.

[12] Ideally, we would include the economic outcomes or fiscal policies of all forty-seven other states directly as independent variables for each state and allow the regression coefficients themselves to identify the relevant economic neighbors. This is impractical. The approach adopted here is to first specify a measure for "neighborliness" ***a priori***, and then interact that measure of neighborliness with the spillover variable of interest to estimate a common effect for neighbors on outcomes in an affected state. The impact of any individual state's policies on another will be it's ***a priori*** measure of neighborliness to that state times the estimated common effect.

performance (measured alternatively by N, H, and e) in all other states (b, representing a "buy" state). We apply the weighting matrix to job and population outcomes in neighboring states in the current period to capture the fact that policies in the initial state impact its neighbor through the economic outcomes in that state.[13] If the state's employment or population grows, then those outcomes may complement (positive effect) or compete (negative effect) with the outcomes of its neighbors. The elements of the spatial weighting matrix equal $1/\delta(s,b)^2$ with $\delta(s,b)$ as the geographic distance from the center of state *s* to the center of all other states *b*. We call this spillover measure *Spatial Distance.* Geographically small states surrounded by other states will have relatively high measures of Spatial Distance. By this measure the economies of Rhode Island and Delaware, the two states with the highest values of Spatial Distance, will be most affected by the economic policies of other states.

Our second measure of spillovers links states by their production connections as specified by the input-output coefficients of each sell state s's outputs to the economies of each buy state *b*'s need for inputs. Here the weighting matrix connecting state economies is specified by the share of each of 63 industry inputs needed to produce one dollar of a buy state b's total output times the transpose of the national input-output matrix times the national share of a sell state s's production of those 63 industry inputs. We use the Bureau of Economic Analysis's 2010 specification of the national input-output matrix.[14] The resulting weighting matrix is then used to construct an average of each sell state s*'s* "input-output" connection to the economic activities (e.g., N, H, and e) in all other buy states, b. We call this spillover measure *Input-Output Distance.* States that are major "sellers" of inputs demanded by a "buy" state will be closely linked to that state. In the U.S. economy, however, most inputs have multiple providers and most outputs use many inputs. As a

[13] In addition to providing a direct estimate of economic spillovers, outcome measures avoid a problem of interpretation that might arise were we to apply the weighting matrix to the fiscal policy itself, here own deficits. The work of Besley and Case (1995) show that states compete by choice of fiscal policies. Applying the weighting matrix to a policy choice of neighboring states, here OWN-D, would compound the joint effects of economic spillovers from the policy with fiscal competition in the choice of the policy. Our estimates will include weighted OWN-D of neighboring states as a separate regressor to control for the effects of fiscal competition.

[14] See http://ww.bea.gov/iTabke/index_industry.cfm.

consequence, Input-Output Distance reduces to measure of the relative importance of a state's economy in aggregate U.S. economy. By this measure, California, Texas, and New York are the states most "exposed" to the economic performance of their neighbors.

Our third measure of spillovers uses a new grouping for states as specified by Crone (2004) to define the economic regions of the U.S. economy. In constructing the regions, Crone uses the Philadelphia Federal Reserve Bank's indices of coincident economic activity to identify through trend-cycle decompositions the cyclical components of a state's economy. The indices constructed use each state's employment growth, unemployment rate, manufacturing hours, and wages and salaries. Crone 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 each sell state s to the economic activities in its neighboring buy states b is the regional aggregate of employment activities (for N and e) or population activities (for H) of the buy only states within its economic region. For example, when Pennsylvania is the sell state, its Crone defined neighbors' (Delaware, New Jersey, Maryland and New York) employment activities and population levels will define the region's spillover variables for Pennsylvania. We call this spillover measure *Regional Distance*. Table 2 lists the states in each of the eight economic regions as specified by Crone (2004).

A correlation matrix of the three spillover measures for each of our three dependent variables show little common co-movements. The correlation of the three weightings for population changes are never larger than .26, for employment rates never larger than .07, and for rates of employment growth never larger than .53.[15]

C. *Estimation and Data*: Because our state economic performance measures of employment (N) and

[15] Conley and Ligon (2002) also study economic spillovers between economic regions. They seek to explain cross-country growth rates. In addition to geographic distance between countries, they measure economic linkages by the costs of shipping a standard 20 Kg package between capital cities and by the cost of airfare between capital cities. Since shipping of people or packages involves a large fixed cost, low per unit costs between countries, signaling close neighbors, will be associated with countries that engage in significant between-country trade. They find economic spillovers between geographic neighbors are relatively unimportant but spillovers between "shipping" neighbors are important for understanding common growth rates. We will find similar results for our measures linking state employment paths.

population (H) are likely to display unit roots when estimated in levels, we specify these variables as the growth rate in state establishment employment, for eq. (4), and the growth rate in state population due to net migration for eq. (5). As it turns out there is evidence of a possible unit root in the levels of the state employment rate for state residents as specified in eq. (6) as well. Table 3 provides panel unit root tests for each of our three dependent variables and for our fiscal policy variable of central interest, OWN-D. The null hypothesis is that all state panels contain a unit root for the variable under consideration, while the alternative is that a non-zero fraction of panels follow a stationary process. We are able to reject the null of a unit root. All estimates provided below are for the stationary series in state growth rates or changes in the state employment rate. Our focus is therefore on the short-run dynamic impacts of fiscal policies, not the longer-run structural effects. OWN-D is stationary as well.

Since there is likely to be a significant dynamic component to the time path of employment and population growth and perhaps to the employment rate as well, we include in each performance equation one and two year lags of the dependent variable. These lags will allow us to compute the "staying power" of any performance benefits of a temporary increase in state deficits. In addition, we specify the independent variable to be lagged one year, OWN-D(-1) to give fiscal policy a chance to influence the state economies.[16]

There are two important econometric concerns when estimating our employment and population growth equations and our employment rate equation. 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 OWN-D(-1) and contemporary performance as well. The bias is likely to be downward when measuring the effects of deficits on economic outcomes.[17] To

[16] From the many empirical studies of the influence of fiscal policy on the national macro economy , the time period needed for most fiscal policies to be fully effective is from 2 to 6 quarters; see for example, Blanchard and Perotti (2002) and also Carlino and Inman (2012b).

[17] OLS estimates of the impact of OWN-D(-1) on our performance outcomes were always statistically insignificant and negative.

correct for possible endogeneity, we instrument for OWN-D(-1) using as instruments the one year lagged level of the state's rainy day fund, a narrative-based estimate of the unexpected changes in the state's own exogenous aid, a one year lag of the state's federally specified share of welfare and Medicaid spending, and a two year lag of the state's own deficit. The two federal aid variables are assumed exogenous to any state's fiscal choices. The narrative-based estimate of unexpected changes in a state's exogenous aid is provided by Carlino and Inman (2012a).[18] The share of welfare and Medicaid spending is set by a federally decided formula independent of current or lagged state fiscal choices.[19] We will provide instrumental variable estimation (IV) for the effects of OWN-D(-1) in economic performance.

Second, each of our performance equations will employ state fixed effects to control for state unique variables that do not change over time. In our specifications above, this will be household (A_H) and firm (A_F) amenities, state land area ($\mathcal{L}_s$), and the state's structural rate of unemployment ($\tilde{u}$). We will therefore estimate our performance equations in first differences to remove the influence of these fixed effects. However, in dynamic panels as here 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. To correct this bias, Anderson and Hsiao (1982) suggest first estimating the serial correlation of the level equation errors, imposing that structure on the first differenced equation, and then using twice (or more) lagged levels or differences of the lagged dependent variable as instruments. Holtz-Eakin, Newey, and Rosen (1988, hereafter H-ENR) provide a more efficient GMM estimator employing the Anderson-Hsiao strategy. In addition to providing instruments for estimating the effects of lagged dependent variables, the Anderson-Hsiao approach will allow us to use lagged values of the

[18] Our narrative specification of exogenous state aid also allows us to address the important question of whether private market agents anticipate future state deficits and thus react economically before actual state deficits are realized. This would lead to a potential "errors-in-variable" problem and a downward bias in our estimate of the effects of OWN-D(-1) employment and population. See Section IV below.

[19] The federal formula setting federal matching rates for Medicaid and welfare does change with lagged changes in state income, but the adjustments involve a quadratic specification from income to matching rates. To be fully precise, therefore, the federal share instrument is a quadratic specification of lagged state incomes.

dependent variables as additional instruments for OWN-D(-1) as well. Finally, the GMM estimation procedure corrects standard errors for heteroscedasticity and serial autocorrelation. Crucial to the validity of this approach is the identifying assumptions that there is no second-order (or higher) autocorrelation and that the proposed instruments are in fact exogenous to the second stage estimates. Arellano and Bond (1991) provide tests for these two key assumptions. The GMM estimator and Arellano and Bond test statistics for second-order or higher autocorrelation and for instrument exogeneity are available in STATA. This is the estimator we use for our results here.[20]

Our final estimating equations will apply the H-ENR GMM estimator to the following three equations derived from the underlying specifications in eqs. (4)-(6):

$$\dot{y} = f(\dot{y}(-1), \dot{y}(-2); \Gamma_s, \Gamma_t; \text{OWN-D}(-1), \text{Z-AID}(-1), p, \tilde{N}) \qquad (7)$$

where $\dot{y}$ is either $\dot{N}$ as the rate of growth in state establishment employment, $\dot{H}$ as the rate of state population growth due to net migration, or e as the state employment rate. Γ_s, are state fixed effects controlling for the state's household (A_H) and firm (A_F) amenities, state land area ($\wp_s$), and the state's structural rate of unemployment ($\tilde{u}$). These effects are removed when we estimate eq. (7) as first differences. Γ_t are year fixed effects meant to capture the effects of year to year variation of common macro-economic shocks and monetary policy (r). OWN-D(-1) is the lagged value of the state's own deficit. Z-AID(-1) is the lagged value of exogenous federal aid to the state. Price shocks for the demand of state output (p) will be measured by the change the price for West Texas Intermediate crude oil over the previous twelve months; see Hamilton (2003). These price shocks will be interacted with a (1,0) indicator variable for whether the state is an energy producing state: OIL PRICE SHOCK*PROD. Shocks to the demand for workers ($\tilde{N}$) is measured by our estimates of a Hick's neutral shift in state productivity in manufacturing for each state in each year: Q

[20] We use the "Two-Step Differenced GMM" estimator as proposed Windmeijer (2005) and implemented by Roodman (2009).

SHOCK.[21] We also include as a measure of demand shocks, the Distance weighted average of OWN-D(-1) in other neighboring states, where the weights used correspond to the Distance measure used to specify spillovers. This variable may play another role as well as a measure of the impact of between state fiscal competition.

Estimation is for the sample of the 48 contiguous mainland states for the years 1973-2009 for the job growth ($\dot{N}$) and net migration ($\dot{H}$) equations and for the years 1979-2009 for the employment rate equation (e). Means and standard deviations for all variables are provided in Tables 4-6 below.

IV. Local Deficits and Aggregate Stabilization

Tables 4-6 provide estimates of the impact of a state's own deficits and the spillover effects of other states' economies. Col. (1) of each table presents the IV estimates using as instruments for OWN-D(-1) the one year lagged level of the state's rainy day fund, unexpected changes in the state's own exogenous aid, a one year lag of the state's federally specified share of welfare and Medicaid spending, and a two year lag of the state's own deficit. The F-test statistics for the ability of these instruments to account for exogenous variation in OWN-D(-1) are approximately 5, suggesting potentially weak instruments.[22] For this reason, and to improve the estimates of the coefficient standard errors, we employed the H-ENR GMM estimator allowing us to use two and three year lags of the dependent variable as instruments for the lagged values of the dependent variable. Instruments used for OWN-D(-1) are those used for the IV estimation. The Arellano-Bond test statistics for second order autocorrelation and the Hansen test statistics for exogeneity of the chosen

[21] Estimated as $\ln(\kappa_{st}) = \ln(Q_{st}) - .33\cdot\ln(K_{st}) - .67\cdot\ln(L_{st})$, where we use level of κ_{st} as our measure of a state productivity shock. We also included as demand shocks the presence in the state of a major natural disaster and an interaction variable of our measure of an oil price with measure the state is a major consumer of energy. Neither variable proved statistically significant and the results are not reported here.

[22] To test for the weakness of these core IV instruments, Stock and Yogo (2004) provide the critical values for a test of the maximal degree of bias in the IV estimates relative to the endogeneity bias of the OLS estimates. The test statistic is based on the F-statistic for the collective influence of the instrumental variables. Our F statistics are above the critical value of 4.79 for a maximal bias of no more than 30% relative to the OLS estimates. The fact that our simple IV estimates and those of the expanded GMM approach are all positive, and significant using the GMM estimators, leads us believe there is a positive effect of OWN-D(-1) on employment outcomes in the state. Our estimates of the exact size of this positive effect have to be considered tentative, however. Any remaining bias is likely to imply an underestimate of the true effect.

instruments do not allow use to reject the null hypotheses of no second-order autocorrelation or exogeneity of the instruments.

The results are consistent across the three Tables. First, OWN-D(-1) has a statistically significant positive effect on both the rate of job growth (Table 4) and the rate of employment (Table 6). There is no evidence that over the two year time horizon studied here deficits impact the rate of net migration into the state (Table 5). From the state's perspective then, the primary beneficiaries of increased deficit financing will be the state's own residents, at least for the year during and the year after the adoption of the deficit policy.[23]

Second, there is no evidence of employment spillovers using Spatial Distance as the measure of neighborliness, weak evidence of positive employment spillovers using Input-Output Distance, and strong evidence of positive employment spillovers using Regional Distance. Since each specification also includes a set of controls for the wider economic environment – year fixed effects, Q SHOCK, OIL PRICE SHOCK*PROD – we are confident the spillover variables are measuring the separate effect of one state's economy on another, beyond the influence of common shocks to all neighbors. We conclude employment growth in a state's regional, and perhaps input-output, neighbors implies higher employment growth for the state itself. Together, the significant positive effect of a state deficits on state employment coupled with the evidence for positive spillovers suggest that the underlying mechanism involves stimulating state aggregate demand through unanticipated (non-Ricardian) fiscal transfers from future to current residents via state deficit policies; see Table 1.

Third, the estimated coefficients for the lagged dependent variables are all statistically significant and imply that any initial increase in state employment growth or rate of employment from a temporary increase in a state's own deficit will themselves be temporary, returning to initial levels in four to five years.

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[23] We also tested for whether the positive effects of OWN-D(-1) on employment were larger in times of recession. While the interaction effect of OWN-D(-1) with a recession years was positive, it was never statistically significant.

Fourth, state deficit policies do not affect the rate of net migration between states in the short-run. Thus deficit-induced employment gains for state residents are not "diluted" by an influx of out-of-state residents. This result is consistent with the results in Blanchard and Katz (1992) showing improved employment opportunities in a state will attract residents from other states but this relocation takes from five to ten years to reach the new population equilibrium. Blanchard and Katz are examining population movements after a structural change – for example, from Michigan to California and Texas after the decline of the auto industry. The adjustment process implied by our results, however, is fully exhausted after four years, and we are studying population movements following a temporary fiscal change. Finally, and perhaps consistent with the Blanchard-Katz analysis, is the observed *negative* spillover of net migration between neighbors defined by our Input-Output Distance; Table 5, col. 4. In-migration to a seller implies out-migration from a buyer. Population spillovers measured by Spatial Distance or Regional Distance are both positive.

Fifth, exogenous federal aid, Z-AID(-1), has no statistically significant positive effect on state employment levels or net-migration in the short-run. This result is broadly consistent with the state level analyses of Feyrer and Sacerdote (2011) and Conley and Dupor (2011) for exogenous grants through ARRA funding as well as with the aggregate U.S. time-series analysis of Carlino and Inman (2012b). Our measures of state productivity shocks, Q SHOCK, and of oil price shocks for energy producing states, OIL PRICE SHOCK*PROD, have no important positive effects on the rate of growth of state employment or the overall level of employment.

Knowing that state deficits can impact state employment and that there may be spillovers between states raises the next question: Are the observed effects important? Table 7 provides an answer for our sample's average state economy. The answer in brief is: *No*. Table 7 first shows the impact of a one standard deviation increase in our average state's deficit equal to $532/resident – approximately 10 percent of all state spending – on the number of jobs created as estimated from the impact of deficits on (establishment) job growth (Table 7; cols. 1-3) and then from the impacts of deficits on the (resident) employment rate (Table

7; cols. 4-7).[24] Though statistically significant, the estimated effects are very modest ranging, from 1/10th of 1 percent of initial employment for estimates from the employment rate equation to perhaps 3/10th's of 1 percent of initial employment for estimates from the employment growth equation. Since the growth in establishment employment measures jobs actually created in the state, rather than jobs held by residents (who may work outside the state), results from job growth equation are more appropriate. Table 7 also provides estimates of the aggregate spillover effects from a "buy" state's decision to increase it deficit by $532/resident.[25] Since spillover effects must initiate from the original employment effect in the deficit state, spillover effects will be small if the original stimulus effect is small. It is, and they are; see Table 7.

Total jobs created will be the sum of the original deficit state employment effect and the resulting spillover effects. The dollar cost per job created will be aggregate level of the deficit in the deficit state divided by the total number of jobs created. The costs per job are very high, comparable in magnitude to those of Feyrer and Sacerdote (2011) and Conley and Dupor (2011) for ARRA spending; see Table 7. Finally we can make an estimate of the average state's output multiplier for a dollar increase in the state's deficit applying Okun's Law relating a 1 percent increase in the rate of state employment to a 2% increase in state output.[26] Those multipliers are reported in Table 7. For estimates based on job growth ($\dot{N}$) the output multiplier is approximately .3, consistent with the direct estimates of the impact of deficits on state output provided by Clemens and Miran (2011).

A final question then remains: Why are the effects so small? Our results are consistent with one or

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[24] Own state effects are computed by multiplying the estimated coefficients of OWND(-1) from Table 4 or 6 times the proposed one standard deviation increase in an average state's deficit $532/resident times the average level of establishment employment for the estimates from the job growth equation (Table 4) or times the level of establishment employment adjusted for the mean rate of employment for the employment rate equation (Table 6).

[25] This spillover effect will be the sum of all effects of an increase in the "buy" state's employment growth, or rate of employment, on all other states. This estimated effect for a single state is computed as the buy state's employment change from the deficit increase times the average measure of "neighborliness" times the estimated impact of spillover DISTANCE on a sell state's economy. This single state spillover estimate is then summed over all neighbors :47 for the INPUT-OUTPUT DISTANCE, and the average number of states in each region, 6, for the REGIONAL DISTANCE.

[26] See for example, Abel and Bernanke (2005, pp. 99-101).

all of four possible explanations.[27] First, our estimate of the effect of deficits on jobs and (implied) state output are only for the two years after the introduction of the deficit; perhaps larger effects may occur in years three and four. Estimates of the time path of spending and tax multipliers from aggregate studies, however, suggest that two years (eight quarters) is sufficient to capture most of the impact of deficit financed fiscal policies on the aggregate economy.[28]

Second, state government deficits are not being spent on activities that create jobs. If deficits are used to fund increases in public employee pay, either directly for state employees or indirectly for local government employees through transfers to local governments, then no new jobs are created.[29] Or if state spending goes to construction projects and there is a constraint (unions perhaps) on the supply of labor to those projects, then again the primary effect may be an increase in wages, not employment. Third, the majority of state residents realize the increase in deficits will be matched by a future increase in state taxation; thus there is no net increase in household wealth and therefore in private spending for these families. Credit-constrained households may benefit if deficits are paid to poorer households as income transfers, but those households are typically no more than 25 percent of the state population as measured by eligibility for Federal government transfer programs. And even for these families, if the deficits pay for transfers-in-kind, particularly health services via Medicaid, then the impact on additional private consumption may be limited.

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[27] There is an econometric explanation for our small coefficients turning on a possible mismeasurement of the timing of the true policy effects. If private agents anticipate future state deficits and react before our measure of deficits is observed, then all changes in employment may be completed before our measured employment outcomes are observed. A failure to get the timing right in this case would lead to a downward bias in the estimate of OWN-D(-1)'s impact on employment. To check for the possibility of such bias, we re-ran our employment growth and employment rate equations substituting the twice lagged value of our narrative federal aid variable for OWN-D(-1) under the specification that narrative aid would be a true "surprise" determinant of OWN-D(-1). The estimated coefficients in the employment equations for twice lagged narrative aid are of the same order of magnitude as our reported OWN-D(-1) coefficients, though only marginally larger than their standard errors. While certainly not a strong rejection of the "timing" explanation for the low coefficients in Tables 4-6, these results lead us to look for an economic rather than an econometric reason for our results.

[28] See Blanchard and Perotti (2002) for federal policies and Carlino and Inman (2012b) for state and local policies.

[29] For evidence that increases in state aid to education increases teacher wages, see Craig and Inman (1982).

Finally, if as our results suggest deficits fund expanded household consumption as a (non-Ricardian) transfer from future to current state residents and state residents consume a significant fraction of that additional income on goods produced outside the state, then the demand stimulus will be shared throughout the union. For economic unions composed of many small jurisdictions, these spillovers will be spread widely across union members. Our spillover measures have sought to capture these effects, but they are no doubt imperfect. Table 7 provides an estimate of state residents' marginal propensity to spend income on import goods consistent with each estimated GSP multiplier.[30] From the job growth equations, the implied marginal propensities to import are close to those estimated from cross-state trade flows for a typical small state; see Gramlich (1987).

V. Concluding Comments

The analysis here provides empirical support for the usual recommendation of public finance economists that stabilization policies should be centralized and state and local government deficit policies should be limited to the task of smoothing taxation in the face of large, lumpy expenditures. While state deficits can increase state employment and state output in the short-run, the effects are very small. Doubling deficit financing for all government activities in the average U.S. state is estimated to increase state employment by no more than 3/10 of 1 percent. Estimated spillover effects are never more than one-half of the state's own employment gains. The source of these small effects is not formally identified here, but high import propensities stands as one credible explanation. If so, then in large currency and economic unions deficit policies for aggregate stabilization, if used at all, will need to be centrally decided. For the U.S. at least, Oates is right.

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[30] The multiplier for a deficit financed reduction in state taxes for a simple open economy Keynesian model is specified as:

$$\partial Y/\partial T = - \mu[1 - \beta)/[1 - \mu[1 - \beta)],$$

where μ is the marginal propensity to consume from after-tax income and β is the share of consumption purchased from imported goods. For the estimated values of $\partial Y/\partial T$ in Table 7 and assuming $\mu = .6$, we can compute β, the marginal propensity to spend additional income on imported goods.

Moving stabilization policies to the central government in federal public economies is not without its own perils, however. Here the recent U.S. experience with the passage , and final impact, of the American Recovery and Reinvestment Act is instructive. That Act used federal deficit financing to allocate $349 billion directly to households and firms as tax cuts and increased unemployment compensation, $123 billion as direct federal spending, and $315 billion as intergovernmental transfers to state and local governments for budgetary relief, and presumably maintenance of state and local services.[31] In federal economies, this makes sense. The central virtue of a federal system is the allocative efficiency arising from decentralized service provision and fiscal competition. Once this decentralized political and administrative structure for public spending is in place, it is efficient for the central government to use those local institutions for appropriate national (spillover) objectives. Macro-economic stabilization can be one such objective. Intergovernmental transfers is typically the policy instrument of choice, as was the case with ARRA.

The effectiveness of these transfers, however, depends significantly on how state governments as "agents" for federal policies choose to spend the resources. While these funds do have an impact when they are spent, particularly to maintain state transfers to lower income households, states remain independent political actors with their own agendas. Gramlich (1978), Taylor (2011), and Carlino and Inman (2012b) all find that a significant share of federal aid to states is not spent when received, but saved for future spending. This makes sense if there is uncertainty as to program renewals by the central government, and particularly so for explicitly temporary assistance as is likely to be case for macro-stabilization aid.

In their aggregate time-series analysis of federal assistance to state-local governments, Carlino and Inman (2012b) find that the income multipliers for federal project aid are never statistically different from 0. More effective as a stabilization policy are transfers to maintain income support and services (Medicaid) for lower income households. Here their estimated multipliers center on 2.0. The reason for the difference is two-fold. First, lower income households are more likely to be credit-constrained. Second, the low income

[31] The website www.recovery.gov provides the details of ARRA allocations.

transfers are tied directly to spending through a matching (price) subsidy. ARRA did earmark $94 billion of the $315 billion in total state and local transfers for matching Medicaid assistance. Still $221 billion were allocated for the generally ineffective unconstrained project grants.

Why then did the ARRA allocate so much funding to an inefficient policy? Grunwald's (2012, Part II) narrative of the passage of ARRA suggests one answer: Budgetary politics in a federal legislature. While the President proposes a budget, it is Congress representing the interests of the individual states that ultimately decides the final allocations. To ensure the ARRA stimulus reached the private sector as quickly as possible, quick Congressional approval of the President's proposal was required. That happened. ARRA legislation was presented to Congress on January 26, 2009 and approved and signed into law four weeks later. To achieve quick approval, ARRA needed to do two things. First, do not propose new programs. In federal, decentralized legislatures "divide-the-pie" policies such as ARRA are susceptible to endless voting cycles.[32] To avoid such protracted negotiations the decision was made to use long-established formula grants where the division of the pie had been settled and accepted. Second, be sure all states receive funding, ideally nearly equal funding. To these ends, the programs chosen were Medicaid, which favored the urban states, highway and transportation grants which favored the rural states, and the plethora of education grants which were given (on average) as grants per child. As finally approved, total ARRA funding became a *de facto* per capita transfer to all states, whether or not the local economies were in deep recession or the state budgets in deep deficit; see Inman (2010).

The U.S. experience is instructive as to the management of deficit policies in federal economies, and possibly for an emerging European fiscal union too, on at least three levels. First, local deficits should be used only for smoothing the financing of state expenditures over time. If deviations from this prescription carry a risk of significant adverse financial externalities for the wider union, then union imposed balanced budget rules should be considered. The analysis here suggests there will no significant welfare loss for

[32] See, for example, Besley and Coate (2003).

member states if, as for U.S. states, local deficit policies have no economically significant effect on own state employment. Second, the central government must recognize the important role that member states play as "agents" for the implementation of union-wide stabilization policies. States will have their own agendas. Deficit financed spending policies must use appropriate incentives to ensure states spend their allocated resources in a timely manner. Finally, if states are agents, they may also be their own "principals" as well. Legislatures based upon locally elected state representation run the risk of catering to local, not national, interests when setting deficit stabilization policies. If so then budgetary rules that strengthen the hand of union-wide elected leadership, whether presidential or dominant parties in the case of parliamentary governance, should be considered.

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TABLE 1: Deficits and Local Economies

Deficit Financing, *Fiscal Allocation*: Jurisdiction	N	H	e
Fully Anticipated, *Efficient Services*: Local Jurisdiction	+	+	+
Fully Anticipated, *Efficient Services*: Neighboring Jurisdictions	-	-	-
Fully Anticipated, *Inefficient Services*: Local Jurisdiction	-	-	-
Fully Anticipated, *Inefficient Services*: Neighboring Jurisdictions	+	+	+
Unanticipated, *Fiscal Transfer*: Local Jurisdiction	+	+	+
Unanticipated, *Fiscal Transfer*: Neighboring Jurisdictions	+	+	+

TABLE 2: Economic Regions[1]

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

[1] 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 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 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.

TABLE 3: Panel Unit Root Tests[†]

	Im-Persaran-Shin Unit Root Test: No Trend Specification	Im-Persaran-Shin Unit Root Test: Trend Specification
Job Growth ($\dot{N}$)	-2.706*	-3.269*
Net Migration Rate ($\dot{H}$)	-2.697*	-2.963*
Employment Rate (e)	-1.574	-1.070
Change in Employment Rate (Δe)	-3.409*	-3.516*
OWN-D	-3.421*	-3.923*

[†] The critical value defining the .99 confidence level for the Im-Persaran-Shin test of the null hypothesis of unit roots in panel data is -1.815 with the no trend specification and -2.44 with a trend specification. An * indicates all variable specifications for which we can reject the null hypothesis at this level of confidence.

TABLE 4: State Deficits and State Rate of Job Growth[†]
($\dot{N}$: Mean = .018 ; S.D. = .024)

	$\dot{N}$ (1) (IV)	$\dot{N}$ (2) (GMM)	$\dot{N}$ (3) (GMM)	$\dot{N}$ (4) (GMM)	$\dot{N}$ (5) (GMM)
$\dot{N}$ (-1)	.661** (.102)	.769** (.069)	.765** (.072)	.771** (.071)	.575** (.084)
$\dot{N}$ (-2)	-.089* (.047)	-.071* (.037)	-.071* (.036)	-.067* (.036)	-.065* (.032)
OWN-D(-1) (Mean = 276 S.D. = 500)	.000005 (.000004)	.000006** (.000002)	.000007** (.000002)	.000006** (.000002)	.000005** (.000002)
Z-AID(-1) (Mean = 476; S.D. = 253)	.000007 (.000009)	-.000054 (.000042)	-.000055 (.000042)	-.000049 (.000042)	-.000043 (.000037)
p: Q SHOCK (Mean = 2857; S.D. = 1299)	-.0000001 (.0000009)	-.000001 (.0000008)	- .000001 (.0000008)	-.000001 (.0000008)	-.000001* (.0000005)
Ñ: OIL PRICE SHOCK*PROD. (Mean = .050 S.D. = .245)	.0064** (.002)	.0039 (.0027)	.0038 (.0027)	.0038 (.0027)	.0017 (.0023)
SPATIAL DISTANCE (Mean = .012; S.D. = .026)	-	-	.014 (.028)	-	-
INPUT-OUTPUT DISTANCE (Mean = .007; S.D. = .014)	-	-	-	.093 (.071)	-
REGIONAL DISTANCE (Mean = .018; S.D. = .022)	-	-	-	-	.503** (.079)
"NEIGHBORS" OWN-D(-1)	-	-	.0000009 (.0000011)	-.0000015 (.000002)	-.0000007 (.000001)
F-Test for OWN-D(-1)	5.05	-	-	-	-
Arellano-Bond Test: AR(2)	-	.173	.172	.170	.398
Hansen Exclusion Test	-	.539	.534	.542	.415

[†] 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 year fixed effects and use a differenced specification to remove state fixed effects. All results instrument for $\dot{N}$ (-1), $\dot{N}$ (-2) and OWNDEF(-1) with non-insurance fund cash and security holdings (Rainy Day Fund) lagged one year, (narrative) policy shocks to state aid lagged one year, one minus the matching rate for welfare spending lagged one year, a two year lag of the state's own deficit, and two and three year lags of changes in the dependent variable. Results in Column (1) are obtained by two-stage least squares. Results in Columns (2)-(5) are obtained using the difference GMM dynamic panel estimator in STATA. Standard errors are reported within parentheses. Estimates indicated by ** are significant at the .95 level of confidence and those by * at the .90 level of confidence.

TABLE 5: State Deficits and State Rate of Population Growth from Net Migration†
($\dot{H}$: Mean = .005; S.D. = .010)

	$\dot{H}$ (1) (IV)	$\dot{H}$ (2) (GMM)	$\dot{H}$ (3) (GMM)	$\dot{H}$ (4) (GMM)	$\dot{H}$ (5) (GMM)
$\dot{H}$ (-1)	.607** (.162)	.512** (.141)	.504** (.141)	.505** (.135)	.486** (.139)
$\dot{H}$ (-2)	.128 (.076)	.139** (.021)	.138** (.022)	.135** (.020)	.127** (.022)
OWN-D(-1) (Mean = 276 S.D. = 500)	.0000015 (.0000012)	.0000003 (.000001)	.0000002 (.000001)	.0000002 (.0000009)	.0000002 (.000001)
Z-AID(-1) (Mean = 476; S.D. = 253)	.000004 (.000007)	.000013 (.000019)	.000014 (.000019)	.000013 (.000018)	.000015 (.000019)
p: Q SHOCK (Mean = 2857; S.D. = 1299)	-.00000009 (.0000005)	-.00000003 (.0000002)	-.00000003 (.0000002)	-.00000005 (.0000002)	-.00000006 (.0000002)
Ñ: OIL PRICE SHOCK*PROD. (Mean = .050 S.D. = .245)	-.001 (.0009)	-.002* (.001)	-.002* (.001)	-.0018* (.0009)	-.0014 (.0010)
SPATIAL DISTANCE (Mean = .007; S.D. = .007)	-	-	.135** (.052)	-	-
INPUT-OUTPUT DISTANCE (Mean = .004; S.D. = .005)	-	-	-	-1.602** (.501)	-
REGIONAL DISTANCE (Mean = .010; S.D. = .008)	-	-	-	-	.230** (.072)
"NEIGHBORS" OWN-D(-1)	-	-	.00000006 (.0000004)	-.0000010** (.0000003)	-.0000001 (.0000007)
F -Test for OWN-D	5.22	-	-	-	-
Arellano-Bond Test: AR(2)	-	.084	.084	.095	.157
Hansen Exclusion Test	-	.708	.700	.638	.805

† Dependent variable is the rate of population growth due to net migration into or from 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 year fixed effects and use a differenced specification to remove state fixed effects. All results instrument for $\dot{H}$ (-1), $\dot{H}$ (-2) and OWNDEF(-1) with non-insurance fund cash and security holdings (Rainy Day Fund) lagged one year, (narrative) policy shocks to state aid lagged one year, one minus the matching rate for welfare spending lagged one year, a two year lag of the state's own deficit, and two and three year lags of changes in the dependent variable. Results in Column (1) are obtained by two-stage least squares. Results in Columns (2)-(5) are obtained using the difference GMM dynamic panel estimator in STATA. Standard errors are reported within parentheses. Estimates indicated by ** are significant at the .95 level of confidence and those by * at the .90 level of confidence.

TABLE 6: State Deficits and State Employment Rate[†]
(e: Mean = .942 ; S.D. = .020)

	e (1) (IV)	e (2) (GMM)	e (3) (GMM)	e (4) (GMM)	e (5) (GMM)
e(-1)	1.059** (.160)	1.049** (.049)	1.042** (.051)	1.046** (.049)	.836** (.057)
e(-2)	-.205** (.063)	-.209** (.033)	- .206** (.034)	- .198** (.031)	- .172** (.027)
OWN-D(-1) (Mean = 283; S.D. = 532)	.000002* (.000001)	.000002** (.0000006)	.000002** (.0000006)	.000002** (.0000006)	.000002** (.0000006)
Z-AID(-1) (Mean = 486; S.D. = 264)	-.0000008 (.000003)	-.000012 (.000010)	-.000012 (.000010)	-.000011 (.000010)	-.000012 (.000011)
p: Q SHOCK (Mean = 3109; S.D. = 1214)	.0000002 (.0000004)	.00000001 (.0000004)	.00000002 (.0000004)	.00000003 (.0000004)	.00000006 (.0000003)
Ñ: OIL PRICE SHOCK*PROD. (Mean = .050 S.D. = .245)	.0006 (.001)	.0011 (.0010)	.0010 (.0010)	.0011 (.0098)	.0007 (.0008)
SPATIAL DISTANCE (Mean = .812; S.D. = .817)	-	-	.020 (.061)	-	-
INPUT-OUTPUT DISTANCE (Mean = .368; S.D. = .435)	-	-	-	.171 (.148)	-
REGIONAL DISTANCE (Mean = .941; S.D. = .017)	-	-	-	-	.377** (.050)
"NEIGHBORS" OWN-D(-1)	-	-	.0000003 (.0000009)	.0000010 (.000002)	-.0000014* (.0000007)
F-Test for OWN-D(-1)	4.77	-	-	-	-
Arellano-Bond Test for AR(2)	-	.487	.466	.453	.128
Hansen Exclusion Test	-	.748	.737	.810	.810

[†] Dependent variable is (1- State UE Rate). Sample includes the 48 mainland states for the years 1979-2009; means and standard deviations for all variables are for this sample. All regressions include year fixed effects and use a differenced specification to remove state fixed effects. All results instrument for e(-1), e(-2) and OWNDEF(-1) with non-insurance fund cash and security holdings (Rainy Day Fund) lagged one year, (narrative) policy shocks to state aid lagged one year, one minus the matching rate for welfare spending lagged one year, a two year lag of the state's own deficit, and two and three year lags of changes in the dependent variable. Results in Column (1) are obtained by two-stage least squares. Results in Columns (2)-(5) are obtained using the difference GMM dynamic panel estimator in STATA. Standard errors are reported within parentheses. Estimates indicated by ** are significant at the .95 level of confidence and those by * at the .90 level of confidence.

TABLE 7: Local Fiscal Policy and Aggregate Stabilization[†]

	$\dot{N}$: *SPATIAL* DISTANCE (1)	$\dot{N}$: *INPUT-OUTPUT* DISTANCE (2)	$\dot{N}$: *REGIONAL* DISTANCE (3)	e: *SPATIAL* DISTANCE (4)	e: *INPUT-OUTPUT* DISTANCE (5)	e: *REGIONAL* DISTANCE (6)
OWN STATE JOBS: (% of Ave. State Jobs)	8673 (.37 %)	7433 (.32 %)	6195 (.27%)	2630 (.11 %)	2630 (.11 %)	2630 (.11%)
"SPILLOVER" JOBS:	≃ 0	299	3134	≃ 0	96	1023
TOTAL JOBS: (% from Spillovers)	8673 (0 %)	7732 (3.8 %)	9329 (33.6 %)	2630 (0 %)	2726 (4 %)	3653 (28 %)
DOLLAR COST PER JOB	$368,000	$413,000	$340,000	$1,170.000	$1,214,000	$875,000
TOTAL GSP MULTIPLIER (MPC Imports)	.324 (μ =.59)	.288 (μ =.63)	.344 (μ =.57)	.088 (μ =.87)	.096 (μ =.85)	.122 (μ =.82)

[†] Estimates are jobs created within an average state, and then across all relevant "spillover" states, for a one standard deviation increase (= $532/resident) in a state's total own deficit, based on the estimated effect of TOTAL OWNDEF(-1) on the rate of state employment (Table 1) and on the rate of growth of state employment (Table 2). All Job estimates assume the own state and the average state receiving spillovers has the sample mean level of establishment employment of 2,328,817. Costs per job are for an average state with a population of 6 million residents. The GSP multiplier assumes an Okun's Law that relates each 2% increase in GSP to a 1% increase in rate of employment.