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CAN U.S. STATES STABILIZE THEIR OWN ECONOMIES?**

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Local Deficits and Local Jobs: Can U.S. States Stabilize Their Own Economies?

by

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ABSTRACT

Using a sample of the 48 mainland U.S. states for the period 1973-2009, we study the ability of U.S. states to expand their 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. 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, when these deficits are scheduled for repayment 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 of state jobs.

JEL: E62, H,74, H77, R23

Key Words: Stabilization Policy, Fiscal Federalism, Local Deficits

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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. 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).

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 U.S. data from 1973-2009 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 II. Section III 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 IV 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 is a small reduction 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 IV (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 (Figure 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 (2012), and Auerbach and Gorodnichenko (2012).

Section V (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 U.S. evidence, the familiar conclusion from Oates stands.

II. 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 minus 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.

Figure 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.

Figure 1b show the ratio of total deficits and total own deficits to national GDP. On average U.S. states have run small surpluses over the years on their combined budgets; the exceptions are the

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.

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 to 3 percent except for the most recent recessions.

Figures 2a-d show 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.

III. State Deficits and State Economies

A. *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:

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

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 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:

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

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 jurisdiction (\mathcal{L}_s); see Haughwout and Inman (2001). Land demanded by firms and households is equal to the exogenous supply of land to define equilibrium gross state product (X):

$$X = X(t_F, G_F, t_H, G_H; r, p; A_F, A_H, \mathcal{L}_s); \quad (3)$$

equilibrium state employment (N):

$$N = N(t_F, G_F, t_H, G_H; r, p; A_F, A_H, \mathcal{L}_s); \quad (4)$$

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

$$H = N/\mu = N(t_F, G_F, t_H, G_H; r, p; A_F, A_H, \mathcal{L}_s)/\mu, \quad (5)$$

where μ is the equilibrium rate of labor force employment.

Equations (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: $\text{OwnD} = (G_F + G_H) - (t_F \cdot X + t_H \cdot H)$. 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 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 scholarships to state universities 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.

B. *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 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 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.

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 U.S. 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

² We use the Bureau of Economic Analysis' 2010 specification of the national input-output matrix; see http://www.bea.gov/iTable/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.

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 U.S. 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

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.

each of the eight economic regions as specified by Crone.⁴

Economic activities specified to influence a given state's growth in jobs and population 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 U.S. states, since they ignore purchases from non-U.S. suppliers. Unfortunately, data on the demand for foreign goods and services by a state's firms and households are not available.

IV. Estimation and Results

A. *Estimation*: We estimate the impact of each state's own deficit on the state's growth in jobs (\dot{N}) and population from 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

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 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.

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

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 (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 restructured 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 (\mathcal{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:

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

where $v_{st} = v_t + v_s + v_{st}$, with year (v_t) fixed effects to control for common changes in aggregate demand and interest rates and state fixed effects (v_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

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

estimation strategy corrects for serial correlation and heteroscedasticity in v_{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 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 sixteen quarters and by results from the application of the

as a control for potential fiscal competition among economic neighbors. Finally, controlling for region-wide fixed effects had no statistically significant effect on our results.

Hansen and difference-in-Hansen tests for instrument exogeneity; see Tables 2 and 3. In particular, see Figure 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 α_s) setting initial conditions. To control for these state fixed effects, each growth equation is estimated in first differences as recommended by Caselli, Esquivel, and Lefort (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, Newey, and Rosen (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 (v_{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

B. *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

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

as “strong” instruments for $\text{OwnD}(-1)$ 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 the F statistic for 10 percent maximal relative bias between OLS and IV estimates is 9.08; see Stock, Wright, and Yogo (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 $\text{OwnNetRev}(-1)$ and $\text{GovServices}(-1)$ as the two components of $\text{OwnD}(-1)$, 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 percent 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 ($= .00003 \times \$390$; Table 2, col. 4) to perhaps .016 ($= .00004 \times \$390$; 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 ($= .012 \times 2.816$

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 below.

million) to 45,000 ($= .016 \times 2.816$ 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 ($= [\$390/\text{person} \times 6.222 \text{ million}]/\text{new jobs}$).

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 percent 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 percent. In contrast, the estimated coefficient for IOSpillovers implies that a 1 percent 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 percent. 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 production relationships between state firms.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

Tables 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 improve 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 well (Romer and Romer, 2010). $GovServices(-1)$ has a positive but statistically insignificant effect on state job growth.¹¹ This

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.

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

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 Figure 3. Finally, Table 3, col. (7) provides an estimate of the impact of OwnD(-1) on the deficit state's rate of employment, μ . The effect is positive but statistically insignificant.

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,

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).

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. From Table 3, col. (7), we estimate that the state rate of employment will rise by .0020 ($= .000005 \times \$390$) from its mean of .942 to .944. For the average state’s labor force of 2.989 million potential workers, this means saving 6,000 jobs of those already working ($= .002 \times 2.989$ million). Finally, new residents are estimated to take about 7,200 of the new jobs. This follows from fact that the \$390/person deficit

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 .0027 ($= .000007 \times \$390$) and increase population in the average state by 17,000 new residents ($= .0027 \times 6.222$ 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 ($= .0027 \times .015$). 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 ($= -2 \times .00004$) or about 500 residents ($= -.00008 \times 6.222$ 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 4,000 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 1950’s “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.

increases own state population from net migration by about 17,000.¹³ About 45 percent of current state residents participate in the labor market. If the new residents participate and are employed at the same rates as current residents, then 7,200 new workers, in jobs, will come from outside the state ($7,200 = 17,000 \times .45 \times .944$). This leaves 20,600 ($= 33,800 - 6,000 - 7,200$) of the new jobs to be filled by current state residents who now enter the labor force. All told, 18 percent of the new jobs are filled by protecting work for residents already employed, 21 percent are filled by non-residents moving into the state, and 61 percent by current state residents who enter the labor force and find work. The majority of beneficiaries of state own deficits, approximately 79 percent, are residents of the deficit state.

The gain in jobs is short-lived, however. Figure 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 percent 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 percent 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

¹³ See footnote 12 above.

to ten years from the date of deficit financing is consistent with jobs declining when taxes rise to repay debt. This third result rejects strong Ricardian equivalence for state deficit financing.

V. 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 have been 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 require 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 the 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

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

panel shows the increase in region jobs if all states 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 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.

¹⁵ Jobs estimates follow from estimates in Table 2, col. 4. First, we compute the increase in the rate of job growth as $\Delta \text{job growth} = .0109 = .000028 \times \$390/\text{person}$. (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.

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. Collectively then, 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.

VI. 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

¹⁷ Though that decision must ultimately rest on the net social benefits of moving residents from unemployment to employment. Whether these net benefits of a created job exceed our 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 that must be compared to our computed costs. For risk-averse workers, the required output gain would be smaller.

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 U.S. 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 to \$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.

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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TABLE 1: Economic Regions[†]

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

[†] 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.

TABLE 2: State Own Deficits and State Rate of Job Growth[†]
 (\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) (Mean = 276; S.D. = 500)	-.000002 (.0000008)*	.00004 (.00002)**	.00004 (.00002)**	.00003 (.00001)**	.000005 (.000002)**	-
ZAID(-1) (Mean = 476; S.D. = 253)	.000006 (.000002)**	-.00006 (.00004)	-.00005 (.00004)	-.00004 (.00002)*	.00002 (.00005)	-.00006 (.00003)*
OwnNetRev(-1) (Mean = 2537; S.D. = 915)	-	-	-	-	-	-.00004 (.00002)**
GovServices(-1) (Mean = 2001; S.D. = 742)	-	-	-	-	-	.00003 (.00003)
IOSpillovers (Mean = .0067; S.D. = .014)	-	-	.093 (.078)	-	-	-
REGSpillovers (Mean = .017; S.D. = .023)	.634 (.033)**	-	-	.685 (.091)**	.887 (.050)**	.676 (.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

† 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. Estimates indicated by ** are significant at the .95 level of confidence and those by * at the .90 level of confidence.

TABLE 3: State Own Deficits and State Rate of Population Growth from Net Migration[†]
 (\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) (Mean = 276; S.D. = 500)	-.0000004 (.0000004)	.000005 (.000008)	.000007 (.000004)*	.000007 (.000005)	-.0000007 (.000002)	-	.000005 (.000004)
Z Aid(-1) (Mean = 476; S.D. = 253)	.000003 (.000001)**	.00002 (.00002)	.000009 (.000009)	-.00002 (.00002)	-.00002 (.000008)**	-.00006 (.00004)*	-.00001 (.000008)
OwnNetRev(-1) (Mean = 2537; S.D. = 915)	-	-	-	-	-	-.000004 (.000006)	-
GovServices(-1) (Mean = 2001; S.D. = 742)	-	-	-	-	-	-.000007 (.000009)	-
IOSpillovers (Mean = .0017; S.D. = .003)	-	-	-2.004 (.937)**	-	-	-	-
REGSpillovers (Mean = .0041; S.D. = .0067)	.345 (.050)**	-	-	.272 (.104)**	.231 (.122)*	.258 (.092)**	.415 (.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	.348	.703	.995	.936
Hansen Exclusion Test	-	.366	.624	.518	.376	.510	.404
Difference-in-Hansen Test	-	.366	.458	.371	.282	.510	.280

† 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. Estimates indicated by ** are significant at the .95 level of confidence and those by * at the .90 level of confidence.

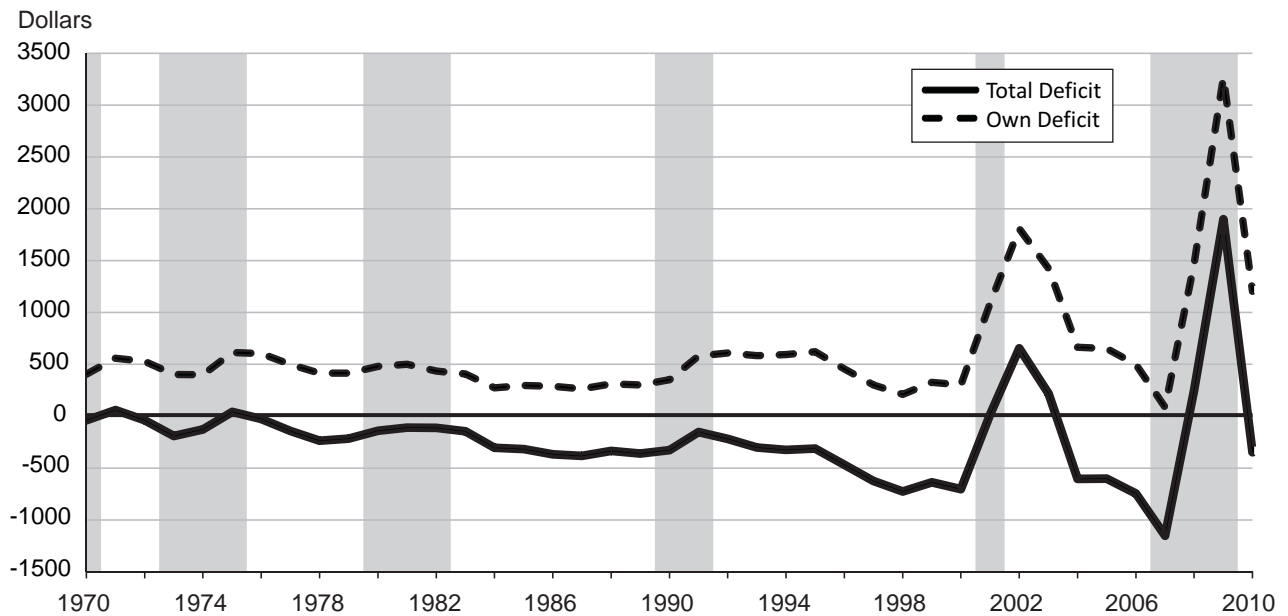
TABLE 4: State Own Deficits, State Jobs, and Benefits of Cooperation: Impact Effects[†]

REGION	NEW ENGLAND	MIDEAST	SOUTHEAST	GREAT LAKES	PLAINS	MOUNTAIN NORTH PLAINS	ENERGY BELT	FAR WEST
LARGEST JOBS STATE (Δ OwnD/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)

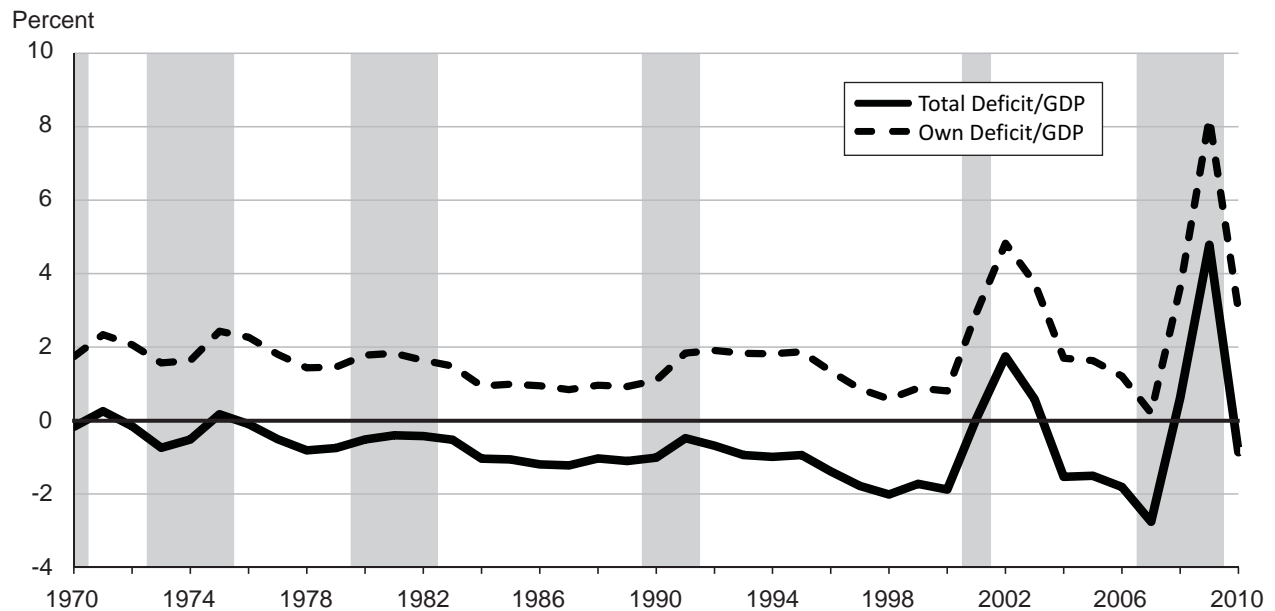
[†] Simulations are for an increase in OwnD(-1) in each region, both on own states and on neighbors based on estimates from Table 2, col. 4. The simulations are for a deficit increase equal to one S.D. of OwnD(-1) for the decade, 2000-2007: \$390/person. Simulations are for a deficit increase in FY2008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

Figure 1: States' Deficits Over Time*

a) Deficits Per Capita

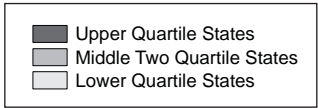


b) Deficits' Share of GDP

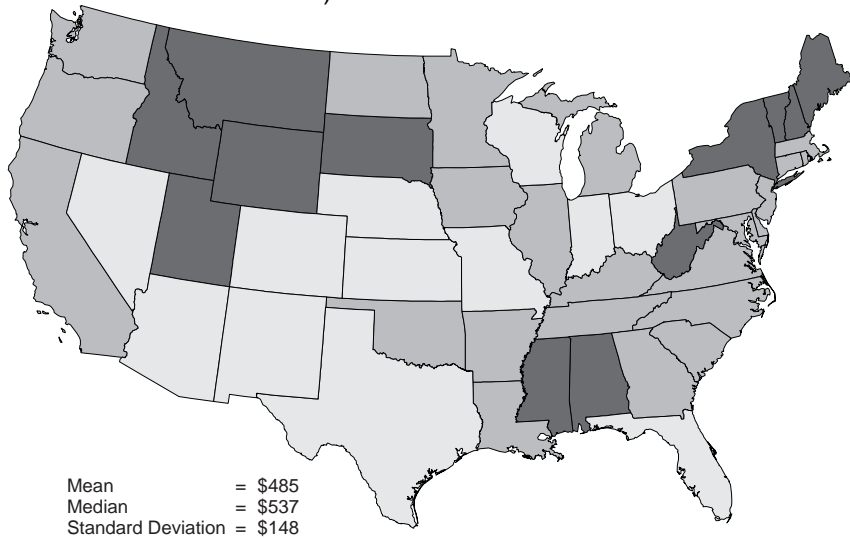


* Figure 1a plots the paths of total deficits per capita (including federal aid as revenues) and state own deficits per capita (excluding federal aid as revenues) for the 48 mainland U.S. states. Figure 1b plots the paths of total and state own deficits as a share of GDP. Total state deficits are represented 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.

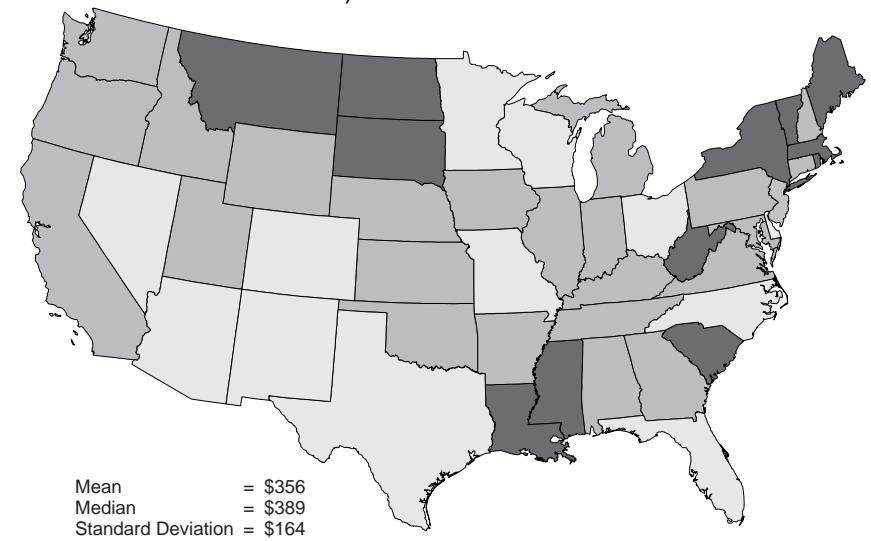
Figure 2: Distribution of State Own Deficits by Decade*



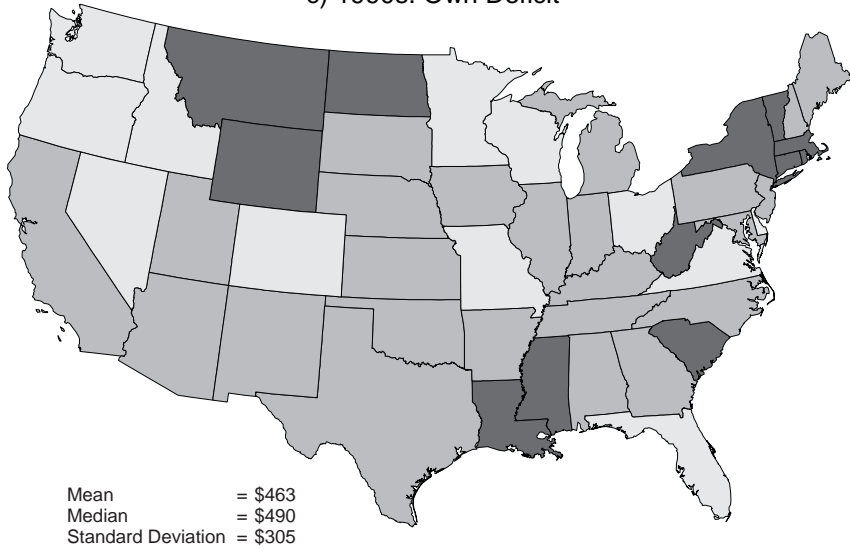
a) 1970s: Own Deficit



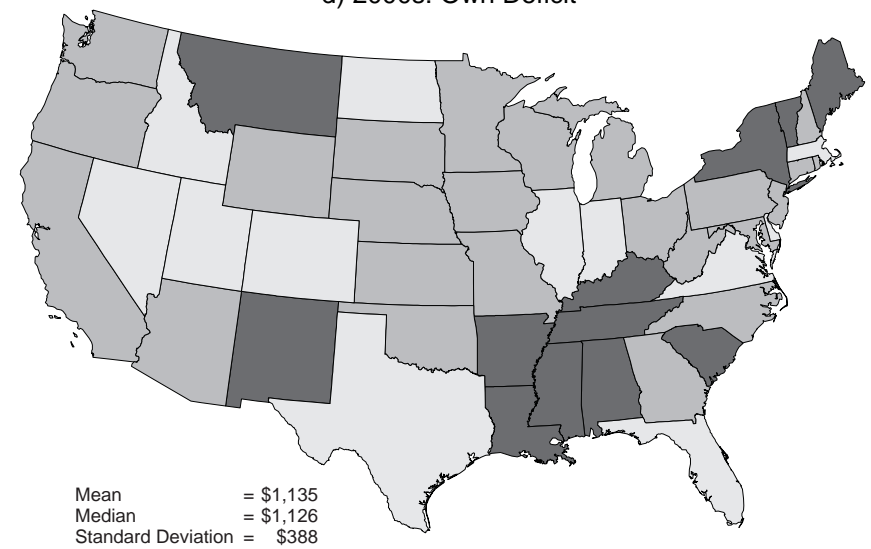
b) 1980s: Own Deficit



c) 1990s: Own Deficit

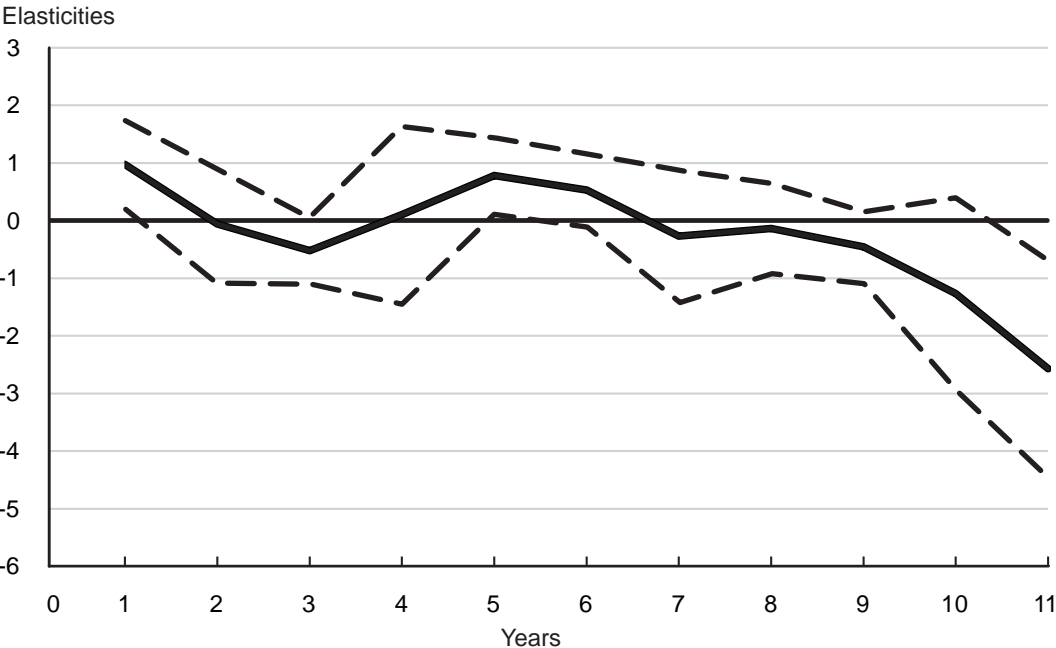


d) 2000s: Own Deficit



* State own deficits exclude federal aid as state revenues. All dollar amounts are measured in 2004 dollars, with Wyoming set equal to 1.00.

Figure 3: Response of State Job Growth to an Increase in State Own Deficits*



* Impulse responses are computed by local projection method for a 1 percent 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 percent confidence bands for each projection.