THE SPILLOVER EFFECTS OF STATE SPENDING

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June 29, 2001

* This paper was originally prepared for the Joint Center for Poverty Research Conference on Tax and Transfer Programs for Low Income Participants in November, 1999, and appeared as JCPR Working Paper 155. I am grateful to David Cutler, Lawrence Katz, Martin Feldstein, Jonathan Skinner, and Doug Staiger for helpful suggestions, and to Jon Gruber, Tim Conley, and participants at the JCPR Tax and Transfer Conference for their useful comments. I thank Jon Gruber and David Cutler for providing invaluable data on Medicaid expansions. Comments are welcome to Kate Baicker, Department of Economics, Dartmouth College, 6106 Rockefeller Hall, Hanover, NH 03755, or baicker@dartmouth.edu.

ABSTRACT

This paper estimates the degree to which state spending is influenced by the spending of neighboring states. Focusing on mandated increases in welfare spending, I find that each dollar of state spending causes spending in neighboring states to increase by 37 to 88 cents. I use more plausibly exogenous variation than previous studies to abstract from the endogeneity of neighbors' spending, and show that previous estimates may have been biased. I also explore the strength of several different measures of neighborliness. The most predictive measure is the degree of population mobility between states, suggesting that concerns about migration may drive the interdependence of state spending policy.

I. INTRODUCTION

The United States has always had a federal system of revenue-raising and government expenditure, but recent trends have given the states more freedom to determine how funds are spent, especially in the realm of welfare. While this freedom allows state programs to reflect the heterogeneity of state preferences, it comes at a cost: the federal government served as the coordinator state actions and could internalize the externalities generated by state taxation and spending. An individual state may not be able to impose an otherwise desirable tax if businesses or wealthy tax-payers will move out of the jurisdiction in response, for example, but all states might be better off if each were required to impose the same tax. The recent devolution of welfare programs into block grants has prompted concern of a race to the bottom between states. Understanding how one state's actions affect other states is fundamental to understanding both the distribution of spending across states and the effect of policies that change the degree of budget centralization. This paper discusses the reasons one state's spending might affect others', estimates the magnitude of those effects, and evaluates the use of different measures of "neighborliness" in different contexts.

States are clearly concerned about how their expenditures and tax rates compare with those of their neighbors. One reason may be the fear of driving away taxpayers or attracting welfare recipients from other states if their benefits are too generous.[1] Many states have recently enacted rules to limit benefits to new residents out of concern for such immigration. These limits were permitted under the 1996 welfare law, although one such rule was recently overturned by a federal judge in Pennsylvania. The state argued that it "wanted to discourage people from shopping around for the best benefit." Washington State has a similar law "designed to prevent an influx," according to a state spokesman (New York Times, Oct. 14, 1997). Given this concern, a change in spending by one state may trigger a corresponding change by neighboring states.

Another reason states may react to the actions of their neighbors is that voters may judge politicians relative to those in nearby states. Besley and Case (1995) find evidence of this "yardstick competition." Voters may also benchmark their state's performance in other regards against the performance of other states, such as in the realm of health or education. All of these suggest that state spending and revenue decisions may have spillover effects on neighboring states. How big are these effects? Which states have the biggest influence on others? How does or should this affect federal policy?

Case, Hines, and Rosen (1993) estimate the effect of one state's spending on that of its neighbors, using a model of spatially-correlated random effects developed by Case (1991) that relies on variation in neighbors' covariates to separate out correlated errors from true spillovers. They find that states' per capita expenditures are positively and significantly influenced by their neighbors' spending, and that omitting this spillover effect would yield biased estimates of the effects of other covariates on state spending. They focus on a measure of "neighborliness" based on the percentage of the population that is black.

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[1] See Brown and Oates (1987) and Borjas (1996), although see Levine and Zimmerman (1995) for counterevidence. Brueckner (1998) reviews the mixed evidence. See Hoyt (1993) for the implications of voter mobility on tax competition.

Recent work by Figlio, Kolpin, and Reid (1999) estimates spillovers in welfare spending in particular, finding similar results. Consistent with theory about migration concerns, they find greater responses to decreases in neighbors' spending than to increases in spending. Their identification also relies on neighbors' covariates, but they define neighborliness based on interstate mobility.

Using alternate methodology I find that, while many of the basic results hold, variation in neighbors' covariates may be inadequate to correct for the endogeneity of neighbors' spending, and the emphasis on the percent black-based weighting matrix may be misleading. I use several measures of neighborliness (based on geography, income, percent of the population that is black, and interstate mobility), and use more plausibly exogenous variation in state medical spending to instrument for actual spending. Unlike neighbors' covariates, these instruments are validly excluded from the second stage regression. Within this framework I am able to explore the predictive value of different measures of neighborliness.

II. METHODOLOGY

A. Case, Hines and Rosen

I use a model following Case, Hines, and Rosen (CHR) to test the degree to which state spending is affected by the spending of neighbors. They begin with a system of expenditure equations:

$$E_t = \phi W E_t + X_t \beta + u_t \qquad (1)$$

where E_t is a (48 x 1) vector of state expenditures, W is a weighting matrix of "neighborliness," and X_t is matrix of covariates including state and year dummies, population density, state per capita income and income squared, federal grants to states and localities, the fraction of the population above age 65, the fraction of the population between 5 and 17, and the fraction of the population that is black. The CHR data is from the 48 continental states over the period 1970 - 1985.

They allow for spatial correlation between the error terms u of the form

$$u_t = \rho W u_t + \varepsilon_t \tag{2}$$

where the ε terms are uncorrelated across states.

Because of the presence of the other right-hand side variables X, they can (with some restrictions) invert the system to solve for the dependent variable, E_t:

$$E_t = (I - \phi W)^{-1} X_t \beta + (I - \phi W)^{-1} (I - \rho W)^{-1} \varepsilon \tag{3}$$

They can then estimate this system via the maximum likelihood technique developed by Case (1991). All that remains is to specify the form of the weighting matrix W. They suggest three possibilities: geographic proximity, per capita income levels, and percent of the population that is black. In each case, they create a time-invariant weighting matrix based on the average of the variable over time. For example, in the case of per capita income levels, each weight is:

$$w_{ij} = \frac{1}{\left| inc_i - inc_j \right| S_i} \quad where \quad S_i = \sum_j \frac{1}{\left| inc_i - inc_j \right|} \tag{4}$$

and where inc_i is the state's average per capita income over the sample. The percent black weights are defined similarly. Geographic neighborliness is 0 if the states do not share a border, and 1/(number of border states) if they do.

This procedure is roughly equivalent to instrumenting for the endogenous right-hand side variable *neighbors' spending* with the neighbors' *X*s, although the maximum likelihood procedure is more efficient than two-stage least squares and allows the error terms to be spatially correlated.[2] One criticism of this specification is that neighbors' *X*s could be correlated with the error term. One such case, which CHR find to be implausible, is that neighbors' *X*s could be proxying for mismeasurement of a state's own *X*s. Unobserved local economic conditions, for example, could be correlated with neighbors' income, which would produce biased estimates of the effect of neighbors' spending on state spending if neighbors' income were used as an instrument for neighbors' spending.

Their results, which appear in Table 2 of their paper and are summarized in Table 1 here, indicate that using any measure of neighborliness, the spending of neighbors is an important determinant of state spending, in that the joint hypothesis that $\rho = \phi = 0$ is soundly rejected. Their preferred weighting matrix is the one based on percent of the population that is black, because it gives the most significant results. It is from this specification that they draw the conclusion that a one dollar increase in a state's neighbors' spending increases its own spending by 70 cents. This sensitivity appears across several different categories of spending, including health and human services, education, administration, and highways. They also conclude that, since the coefficient on federal grants goes down significantly, traditional measures of the flypaper effect are inflated by the fact that "neighbors" face similar changes in grants over time.

It is puzzling that the only weighting matrix that is completely invariant with respect to economic conditions, changing populations, and changing political climates - geographic contiguity - actually produces a significant coefficient of the *opposite* sign: an increase of one dollar in neighbors' spending is estimated to *decrease* a state's spending by 22 cents. Because of the difficulty in interpreting these coefficients and because of the possibility that neighbors' *X*s may be correlated with omitted variables such as economic conditions, exploring the effect of neighbors using another methodology may be enlightening.[3]

B. Alternate Methodology

My estimation differs from previous work in three regards. First, I use several different weighting matrices to determine neighborliness and explore the implications of using each. Second, I try to disentangle true spillover effects from correlated errors using

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[2] They note that ignoring the spatial autocorrelation would not lead to inconsistent estimates of the parameter β, but would lead to inconsistent estimates of the standard errors.

[3] Conley (1999) describes alternate methodology for efficient IV estimation with spatial dependence. While his estimates are less parametric than CHR in some ways, they require measures of neighborliness that are symmetric (as well as the choice of a cut-off distance). For example, Rhode Island must have as great an impact on New York as New York has on Rhode Island. For this reason, none of the weighting matrices described above can be used. Nevertheless, I also describe rudimentary estimates using this methodology for comparison below.

a source of variation that I argue is more plausibly exogenous. Third, in order to do this, I use a data from a different time period.

There are several reasons, discussed above, that we might expect states' actions to affect their neighbors. States may be concerned about taxpayer (or welfare recipient) mobility: if their taxes (or benefits) are higher than those of their neighbors, taxpayers may move out (or welfare recipients may move in). This would imply that states are most influenced by the actions of those states to which their taxpayers (or from which welfare recipients) may move. Voters may also judge politicians' actions relative to the actions of politicians in neighboring states. This would imply that states are most influenced by the actions of those states that their voters judge to be the most "similar" or whose actions are most salient to the voters.

These motivations suggest several possible weighting matrices for neighborliness. CHR's weights may proxy for voter "yardsticks" or for potential mobility. Actual interstate mobility patterns may be a better proxy for how much each state is influenced by others. Population-based geographic weights, where larger neighboring states are given more weight than smaller ones, may capture this influence more than the equal-weight matrix of CHR.

The problem with the income-based and percent black-based weights is that they may proxy for other omitted variables. Income, for example, is a determinant of the federal match rate for several programs such as Medicaid and AFDC. Although income is included as a covariate, it may not adequately capture the price effects of the federal match. Similarly, percent black could proxy for several unobserved aspects of the state economy or the needs and tastes of the population.[4] Thus, while I will present results using all three of the CHR weighting matrices, I will also add population-based geographic weights and interstate mobility-based weights.[5]

I begin with OLS regressions of the effect of neighbors' spending on state spending. I create a weighted average of neighbors' spending (based on one of these five different criteria), and then estimate:

$$E_{it} = \phi\, w_i \overline{E}_t + X_{it}\beta + \varepsilon_{it} \tag{5}$$

where E_{it} is state per capita general expenditures (in 1987 dollars), $\overline{E}_t$ is a (48 x 1) vector of each state's spending in year t, w_i is a vector assigning a "neighborliness" weight, and X_{it} , similar to CHR, includes population density, state per capita income and its square, federal grants to states and localities, the fraction of the population above age 65, the fraction of the population below 15, the fraction of the population that is black, and state and year dummies.

There are several reasons that we might doubt the validity of these OLS estimates. If, as CHR estimate, there are negatively correlated random errors between neighbors (such as if the political climate made business move from one state to its neighbor), then the OLS estimates would be biased down. If there are positively correlated errors, such as common unobserved economic downturns, the OLS estimates might be biased up.

[4] CHR acknowledge that it might appear that percent black is proxying for some other variable, and address several possibilities, including the income distribution, urbanicity, the cost of providing services, and regional effects.

To correct for this type of problem, we need some source of variation correlated with neighbors' spending but uncorrelated with the error term in equation (5). One potential source of variation is neighbors' *X*s, used by CHR. I create "neighbor" values for these variables in the same way that the neighbor value for spending was created, using five different weight matrices. I use these neighbor-weighted variables to instrument for actual neighbor spending in equation (5). The first stage is thus:

$$w_i \overline{E}_t = w_i \overline{X}_t \beta + \varepsilon_{it} \tag{6}$$

where $\overline{X}_t$ is a (48 x 7) matrix of the 48 state values for each of the seven *X*s (percent old, percent young, percent black, income, income squared, population density, and federal grants) in year *t*, w_i is a (48 x 48) weighting matrix for neighborliness, and $\overline{E}_t$ is a (48 x 1) vector of state spending in year *t*.

In an effort to find instruments that are less likely to be correlated with omitted state characteristics, I use three sources of variation in state medical spending to instrument for the actual state spending of neighbors on the right-hand side.[6] The first instrument is an index created from federally-mandated Medicaid eligibility expansions in the 1980s and 1990s which measures the potential cost to a state of its Medicaid

[5] I use the fraction of all in-migrants to state *i* that come from state *j* to create w_{ij}, rather than net migration between the two states. Figlio et al. argue that this is a better measure because it does not allow large symmetric movements to cancel each other out. Results do not vary substantially with alternate measures.
[6] See Baicker (1998) for details on the construction of these instruments.

eligibility rules in a given year.[7] The index isolates the effect of the legislative environment from other confounding factors.[8]

The second instrument relies on the fact that overall medical price increases had differential effects on state budgets. States with initially larger public medical spending programs faced a greater fiscal shock as medical prices rose. State spending on Medicaid in 1982, inflated by the real growth of national medical expenditures, should capture a state-specific, exogenous budget shock.

The third instrument is based on the Boren Amendment, which in 1980 required states to reimburse nursing homes for Medicaid patients at a "reasonable" rate. Several lawsuits initiated by nursing homes in the late 1980s and early 1990s were successful in the courts and led to increases in reimbursement rates in those states. This change in the cost of Medicaid reimbursement is captured by a dummy variable for the presence of a successful suit.

I create "neighbor" values for these variables in the same way that the neighbor value for spending was created, using five different weight matrices. I use these neighbor-weighted variables to instrument for actual neighbor spending in equation (5). The first stage is thus:

$$w_i \overline{E}_t = \lambda_1 w_i \overline{boren}_t + \lambda_2 w_i \overline{medtrend}_t + \lambda_3 w_i \overline{index}_t + X_{it}\beta + \varepsilon_{it} \qquad (7)$$

[7] There is evidence that these expansions increased access to, utilization of, and spending on Medicaid services. Cutler and Gruber (1996b) find that each additional dollar in their eligibility index leads to 30 cents of increased spending. Currie and Gruber (1996) show that increased eligibility for children, while not fully taken-up, does lead to increased utilization of services.

where $\overline{boren_t}$, $\overline{medtrend_t}$, and $\overline{index_t}$ are each (48 x 1) vectors of the 48 state values in each year, and where X_{it} contains the seven CHR covariates as well as these three Medicaid variables. Each of the five weighting matrices produces significant results in the first stage.[9]

Expenditure data come from the Census of Government Finances. Demographic data come from the Bureau of the Census Current Population Reports. I use the 48 contiguous United States in the years 1983-1994. Table 2 presents summary statistics for this data and for the CHR data. The weighting matrices are constructed analogously to CHR. The population-based geography weight is {population/(total population of neighboring states)} for neighboring states, and 0 otherwise. Interstate mobility data comes from the 1980 PUMS one percent sample, and the weight is the fraction of immigrants to a state that came from each other state.

III. RESULTS

The "OLS" columns of Table 3 present the results of estimating equation (5) with the five different weighting matrices. The coefficients generated using the geography, population-weighted geography, and interstate mobility weights are all similar: a one dollar increase in neighbors' spending increases a state's spending by somewhere between 46 cents and one dollar. The income and percent black weights, however give

[8] See Currie and Gruber (1996) for details. I also show results isolating the mandated portion of spending, or "bite" of the expansions, below.

[9] The joint F statistic for the geography weight was 88.79, for the income weight was 24.23, for the percent black weight was 28.94, for the population-based geography weight was 101.00, and for the interstate mobility weight was 215.14.

very different estimates: the effect of neighbors' spending is negative and statistically insignificant.

Results from using neighbors' covariates as instruments are reported in the "IV1" columns of Table 3. All of the weighting matrices now produce positive estimates of the effect of neighbors' spending on state spending, ranging from .24 to 1.00. The estimates using the percent-black and income-based weights (in this specification as well as those below) are *higher* than the corresponding OLS estimates, consistent with the CHR estimate of negative spatial correlation in errors. More on this below. The biggest difference between these results and the CHR estimates is that I do not obtain a negative estimate using geographic weights. Given the marginal significance (and sign) of their geography-based estimates, this difference does not seem a cause for concern. While CHR find the one-for-one increase in state spending caused by increases in federal grants to diminish in the case of the percent black weights, I do not. No portion of the "flypaper effect" can be attributed to the fact that neighbors are receiving similar changes in federal grants.

As discussed above, we may be concerned that these estimates are picking up mismeasured or omitted variables on the right-hand side. To test this, I perform a Newey test of overidentifying restrictions. These instruments fail the test: neighbors' *X*s are not validly excluded from the second stage regression.[10]

The "IV2" columns in Table 3 present the results of using the three sources of exogenous variation in Medicaid spending as instruments. The results are similar across

[10] The test statistic of NR^2 produces probabilities (from a chi-squared distribution with 6 degrees of freedom) of 1.3×10^{-11} (geography), 4.0×10^{-6} (income), .001 (percent black), 6.0×10^{-6} (population-weighted geography), 2.4×10^{-12} (interstate mobility).

all of the weighting specifications: a one dollar change in its neighbors' spending causes a state to increase its spending by 37 to 88 cents. The only cases where the IV estimate is significantly different from the OLS estimate (using a standard Hausman test) are when the income or percent black weighting matrices are used. These instruments, unlike the neighbors' *X*s used in the IV1 columns, pass the Newey test (with the exception of the specification with income-based weights).[11] The IV2 results are significantly different from the IV1 specification in the case of the percent black weights and the interstate mobility weights.[12]

Again, in the case of the percent black- and income-based weights, the IV estimates are larger than the OLS, consistent with negative spatial correlation of errors. Figlio et al. also find that IV estimates are higher, which is at first pass surprising (although neither they nor CHR identify the source of this negative bias). If omitted variables were biasing the OLS estimates, we might expect those variables to be positively correlated across states, which would bias OLS estimates of spillovers up. There are several candidate explanations for this negative correlation. First (and least plausibly), omitted variables may be negatively correlated across neighboring states. Second, the marginal state affected by the instruments may have a larger response than the average state. For example, if only "stingy" states are affected by the instruments, then the IV estimates will be driven by the neighbors of stingy states, who may feel more constrained to keep spending low than the average state without stingy neighbors would. This seems unlikely to be the case, given that some instruments are more likely to affect

[11] The probabilities are .08 (geography), .04 (income), .27 (percent black), .13 (population-weighted geography), and .14 (interstate mobility).

higher spending states, and, as discussed below, estimates using any subset of the instruments yield the same results. Third, classical measurement error may be biasing the OLS results toward 0. This is perhaps the most plausible explanation, for two reasons: (1) we only see a significant reversal in the case of percent black and income, which seem most likely to be measuring neighborhood with error, and (2) when I restrict the sample to the smallest quintile of states, which are most likely to measure spending with error, the OLS estimates using the other weights are also significantly lower.

We might be concerned that the "index" instrument (which combines required eligibility with voluntary eligibility based on state-year eligibility rules) is picking up state choices for early expansion that may be correlated with changes in regional preferences or fiscal constraints. (Time or state invariant conditions are of course picked up by the fixed effects.) Isolating the portion of this eligibility index that is mandated by the federal government would net out common shocks to preferences or constraints (although federal laws will only be binding in states that are less generous, so we will only pick up the reactions of the neighbors of those less generous states). To this end I replace the "index" instrument with a rough measure of the "bite" of federal eligibility expansions.[13] These results are very consistent. In the specification using mobility-based weights, for example, the spillover coefficient changes from .88 (with robust standard error of .17) to .91 (.19). Removing the eligibility instrument altogether yields an estimate of .89 (.17). This similarity might arise in part because the eligibility instrument

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[12] The "IV3" columns in Table 3 include both neighbors' *X*s and the Medicaid variables as instruments. These results are very similar to the IV1 specification.

[13] Unfortunately I can only use a very crude measure of this "bite": the difference between each state's AFDC need standard (which roughly governs Medicaid eligibility in the absence of intervening federal law)

is not strong, but using just the eligibility index alone also yields a very consistent estimate of .79 (.22).

I can also compare these results to those obtained using the Conley methodology. As discussed above, this requires the use of a symmetric weighting matrix, so I construct an unweighted geographic-contiguity measure of neighborliness. Software limitations restrict me to performing Conley's spatial GMM on less than half of the states and do not allow a full set of controls. As a limited example, I choose the first 20 states alphabetically and run a specification with only per capita income and percent black as controls. In this specification, the usual IV estimate (using the Medicaid instruments) is .86 with a standard error of .09, while the "spatial GMM" estimate is .73 with a standard error of .20.

Spending on Different Budget Categories

We might expect different types of spending to be more sensitive to neighbors' spending than others. There may be more yardstick competition in administrative spending or education spending, or there may be greater fear of recipient immigration in social services spending. This also suggests that different measures of neighborliness may be appropriate for different categories of spending. States may look to geographic neighbors to judge transportation expenses, while looking to states where residents move to or from when concerned with welfare magnet issues.

Table 4 looks at state spending by category, including social services, education, transportation and public safety, and administration, using the IV1 and IV2 specifications

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and the federally mandated lower income bound for eligibility in each year. See Yelowitz for a similar and three different weighting matrices. While CHR find that state spending on all categories responds about equally to neighbors' spending (from .42 for social services to .70 for education), using IV1 I find that some categories are significantly more responsive than others. Again, the results differ greatly when different weighting matrices are used. Social services spending does indeed seem to be most sensitive to neighbor weighting based on interstate mobility patterns under either IV specification.[14]

Unfortunately, we do not have alternate instruments for spending in any category other than social services. Since the Medicaid variables used as instruments in the IV2 specification affect only public welfare spending (see Baicker 1998), it is not appropriate to use them as instruments for other types of spending. This does, however, allow for another specification check: regressing state spending on the other categories on neighboring states' spending instrumented with the Medicaid variables should yield estimates of 0. This is indeed the case. Using mobility weights, for example (filling in the missing values from column (9) of Table 4), results in an effect on education of .04 (.06), on transportation and public safety of .02 (.04), and on administration of .03 (.01).

So, which weighting matrix is the best? If each weight really captured a different aspect of the interactions between states, it would be appropriate to put several differently weighted averages of neighbors' actions on the right-hand side. Table 5 presents such estimations. The percent black and interstate mobility matrices seem to be the most robust, with income and geography being the least, although there is no clear-cut winning

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approach. In this context this measure does not have enough power to use in isolation.

[14] It is important to note that the estimates using the interstate mobility weights are implausibly large in several instances, especially in the case of education, where the coefficients are significantly larger than 1. Such a system would not be stable, with any change in expenditures generating an explosive reaction. The

specification.[15] Column (12) includes all 5 weighting matrices, but column (11) is preferred because of the high correlation between the two measures of geographic proximity. We can interpret these coefficients as weights on the weighting matrices if we divide by the sum of the coefficients. In every specification in which it is included, the mobility-based weighting matrix receives the highest weight. In the case of column (11), geography receives a weight of .1, income of .25, and percent black and mobility each of .33.

Given the importance of geographic mobility, we might imagine that states with high mobility (or in-migration) would be more sensitive to their mobility-defined neighbors than those with low mobility. I test this hypothesis by dividing the states in half based on the fraction of their 1980 population that moved in after 1975. Using the same specification, the spillover effect in neighbor spending is 1.60 (.46) for high mobility states, but only .69 (.19) for low mobility states. These results suggest that the states with the greatest potential in-migration are those that are most concerned with staying in line with their neighbors' spending.

IV. CONCLUSIONS

State spending is significantly influenced by the spending of neighboring states. States raise their spending by something between 37 and 88 cents for every dollar increase in their neighbors' spending. Specification checks suggest that identifying

CHR system can only be inverted to solve for *E* if the coefficient of spatial correlation lies strictly between -1 and 1.

[15] Neighbors' spending based on different weights is of course correlated. The geographic measures are the most correlated, with a correlation coefficient of .90, while geography and percent black and income are the least correlated, with correlation coefficients between .46 and .59.

spillover effects through variation in neighbors' covariates is inappropriate. Using more plausibly exogenous variation in Medicaid expenditures, I obtain more consistent estimates of the spillover effects of state spending across different weighting matrices. In particular, percent black- and income-based weighting matrices produce different results from geographic- and mobility-based measures when identification comes from variation in neighbors' covariates, but not when variation in Medicaid spending is used. The degree of interstate mobility is the strongest predictor of spending spillovers.

This study leaves several unanswered questions for future research. While estimates using Medicaid variables as instruments improve on those using neighbors' covariates in some ways, these instruments cannot be used for other categories of spending, so it is not possible to say whether some categories are more sensitive to neighbors' spending than others. Other instruments should be used for those comparisons, such as changes in incarceration policy, highway funding, or education spending requirements.

Furthermore, we would in theory like to be able to determine the appropriate weighting matrix for different types of state spending. While it seems clear that measures based on percent black are not ideal, it is not clear that any other matrix is best in every situation. With better instruments to identify changes in other spending categories, it would be possible to test which set of neighbors is important for each state spending decision. Different interstate spillover effects in different spending categories might imply very different optimal structures for federal grants to states and localities.

What does seem clear is that an individual state's spending has spillover effects on the spending of its neighbors, and this externality implies a role for the federal government in influencing state spending levels. It is difficult to evaluate whether state spending would be "too high" or "too low" in the absence of federal intervention, or whether or not changes in the financing of different programs would lead to a race-to-the-bottom (or to the top – or even what the right benchmark for spending is), but the level and geographic dispersion of state welfare benefits certainly affect the standard of living of poor Americans. The federal government has powerful tools (such as matching rates that lower the price of state spending on some programs) influence state expenditures, especially in light of the multiplier implied by these spillovers. An analysis of these effects should be a key component of the design of federal and state policies.

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Table 1: Summary of Case, Hines, and Rosen Results

Dependent Variable: State Expenditures (real per capita 1982 dollars)

			Basis of Measure of "Neighborliness"		
			Geography	Income	Percent Black
ϕ	(coefficient of spatial correlation in dependent var)		-0.225 (0.101)	0.096 (0.097)	0.701 (0.035)
ρ	(coefficient of spatial correlation in errors)		0.294 (0.087)	-0.288 (0.098)	-0.753 (0.051)
Federal Grants		1.02 (0.08)	1.04 (0.08)	0.997 (0.078)	0.648 (0.068)
Percent Black		-17.53 (7.18)	-17.52 (6.41)	-22.75 (7.26)	5.38 (5.23)

Notes: From Case, Hines, and Rosen (1993), Table 2.
Sample is 48 states from 1970 to 1985.
Other covariates include fraction of the population above age 65, fraction between 5 and 17, per capita income and its square, population density, and state and year effects.

Table 2: Summary Statistics

	CHR Sample	This Sample
	Mean (Std Dev)	Mean (Std Dev)
Total Expenditures	1865.0	2564.5
	(304.7)	(503.5)
Population Density	156.9	141.8
	(230.5)	(183.3)
Income	100.5	147.5
	(12.74)	(24.1)
Grants	430.1	504.9
	(90.13)	(132.7)
Proportion Elderly	0.109	0.123
	(.017)	(.018)
Proportion Children	0.228	0.223
	(.012)	(.023)
Proportion Black	0.112	0.098
	(.088)	(.093)
Time Period	1970-1985	1983-1993
Base Year	1982	1987

Notes: Summary of "CHR" data comes from Case, Hines, and Rosen (1993).
Data for both samples come from Bureau of the Census annual publications (Census of Government Finances and Current Population Reports).
Dollar amounts are in real per capita terms, with base year as reported.

Table 3: The Effect of Neighbors' Spending

Dependent Variable: Real Per Capita State Expenditures (Robust S.E.s)

Instruments for Neighbors' Spending:
IV1: Neighbors' Xs
IV2: Medicaid Variables
IV3: Neighbors' Xs and Medicaid Variables

Basis of Measure of "Neighborliness"

		Geography				Income				Percent Black			
	OLS	**OLS**	**IV1**	**IV2**	**IV3**	**OLS**	**IV1**	**IV2**	**IV3**	**OLS**	**IV1**	**IV2**	**IV3**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
Neighbors' Spending		0.46 (0.06)	0.53 (0.09)	0.53 (0.11)	0.58 (0.09)	-0.13 (0.13)	0.36 (0.24)	0.52 (0.30)	0.21 (0.24)	-0.02 (0.11)	0.24 (0.14)	0.65 (0.22)	0.28 (0.14)
Federal Grants	0.81 (0.11)	0.93 (0.13)	0.91 (0.13)	0.91 (0.13)	0.91 (0.13)	1.00 (0.15)	1.07 (0.16)	1.03 (0.16)	1.01 (0.15)	1.00 (0.11)	1.14 (0.16)	1.23 (0.18)	1.10 (0.15)

Notes: Sample is 48 states from 1983-1992. All dollar amounts are in real (1987=100) per capita dollars.
All columns except "IV1" specification include Medicaid variables as covariates.
Medicaid variables are eligibility index, Boren Amendment dummy, and medical spending trend.
Other covariates include fraction of the population above age 65, fraction under 15,
fraction black, per capita income and its square, population density, and state and year effects.
"IV" columns instrument for neighbors' spending with neighbors' instruments (same weights).
As in CHR, the weighting matrix is determined by the mean of the variable over the sample period
(except mobility, from 1980 PUMS, and population-weighted geography, from 1983 population).

Table 3: The Effect of Neighbors' Spending (continued)

Dependent Variable: Real Per Capita State Expenditures (Robust S.E.s)

Instruments for Neighbors' Spending:
IV1: Neighbors' Xs
IV2: Medicaid Variables
IV3: Neighbors' Xs and Medicaid Variables

	Basis of Measure of "Neighborliness"							
	Pop. Weighted Geography				Interstate Mobility			
	OLS	**IV1**	**IV2**	**IV3**	**OLS**	**IV1**	**IV2**	**IV3**
	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)
Neighbors' Spending	0.43 (0.06)	0.40 (0.08)	0.37 (0.10)	0.42 (0.09)	1.03 (0.13)	1.00 (0.16)	0.88 (0.17)	1.04 (0.16)
Federal Grants	0.93 (0.14)	0.95 (0.14)	0.94 (0.14)	0.93 (0.14)	0.97 (0.13)	0.98 (0.13)	0.98 (0.13)	0.97 (0.13)

Notes: Sample is 48 states from 1983-1992. All dollar amounts are in real (1987=100) per capita dollars.
All columns except "IV1" specification include Medicaid variables as covariates.
Medicaid variables are eligibility index, Boren Amendment dummy, and medical spending trend.
Other covariates include fraction of the population above age 65, fraction under 15, fraction black, per capita income and its square, population density, and state and year effects.
"IV" columns instrument for neighbors' spending with neighbors' instruments (same weights).
As in CHR, the weighting matrix is determined by the mean of the variable over the sample period (except mobility, from 1980 PUMS, and population-weighted geography, from 1983 population).

Table 4: The Effect of Neighbors' Spending on Different Budget Categories

Coefficient on Neighbors' Spending from Individual Regressions (Robust S.E.s)

Instruments for Neighbors' Spending:
IV 1: Neighbors' Xs
IV 2: Medicaid Variables

Basis of Measure of "Neighborliness"

	Percent Black			Pop. Weighted Geography			Interstate Mobility		
	OLS	**IV1**	**IV2**	**OLS**	**IV1**	**IV2**	**OLS**	**IV1**	**IV2**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Total Expenditures	-0.02	0.24	0.65	0.43	0.40	0.37	1.03	1.00	0.88
	(0.11)	(0.14)	(0.22)	(0.06)	(0.08)	(0.10)	(0.13)	(0.16)	(0.17)
Social Services	-0.19	0.17	0.47	0.25	0.39	0.53	0.59	0.97	0.78
	(0.22)	(0.54)	(0.90)	(0.08)	(0.14)	(0.22)	(0.26)	(0.29)	(0.33)
Education	-0.04	0.09		0.58	0.78		1.39	1.61	
	(0.10)	(0.15)		(0.08)	(0.16)		(0.21)	(0.26)	
Transportation and	0.12	0.46		0.17	0.29		-0.02	0.36	
Public Safety	(0.10)	(0.25)		(0.10)	(0.23)		(0.26)	(0.36)	
Administration	-0.004	-0.47		-0.03	0.40		0.11	1.17	
	(0.13)	(0.30)		(0.10)	(0.21)		(0.28)	(0.48)	

Notes: Sample is 48 states from 1983-1992. All dollar amounts are in real (1987=100) per capita dollars.
Each coefficient is from a separate regression, with spending on different categories as dependent variable.
All columns except "IV1" include Medicaid variables (eligibility index, Boren Amendment dummy, and medical spending trend) as covariates.
Other covariates include pct of pop above age 65, pct under 15, pct black, income and its square, pop density, and state and year effects.
"IV" columns instrument for neighbors' spending with neighbors' instruments (same weights).
As in CHR, the wt matrix is determined by the mean of the variable over the sample (except mobility, from1980 PUMS, and pop-wt geog, from 1983 pop).

Table 5: Multiple Neighbor Weights

Dependent Variable: Real per Capita State Expenditures

Coefficient on Neighbor per Capita Expenditures (varying neighborliness weights)
Instruments for Neighbors' Spending: Medicaid Variables

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Neighbor Weight Based On:												
Geography	0.56	0.50	0.97	0.26							0.20	0.59
	(0.11)	(0.11)	(0.34)	(0.29)							(0.32)	(0.40)
Income	0.37				0.98	0.41	0.13				0.50	0.33
	(0.30)				(0.33)	(0.30)	(0.30)				(0.34)	(0.32)
Percent Black		0.63			0.75			0.69	0.67		0.65	0.53
		(0.21)			(0.24)			(0.21)	(0.20)		(0.19)	(0.22)
Population-Weighted Geography			-0.44			0.38		0.41		-0.21		-0.47
			(0.29)			(0.10)		(0.10)		(0.17)		(0.23)
Interstate Mobility				0.48			0.88		1.00	1.21	0.66	0.81
				(0.47)			(0.17)		(0.17)	(0.29)	(0.51)	(0.54)

Notes: Sample is 48 states from 1983-1992. All dollar amounts are in real (1987=100) per capita dollars.
Medicaid variables are eligibility index, Boren Amendment dummy, and medical spending trend, and are also included as covariates.
Other covariates include pct of pop above age 65, pct under 15, pct black, income and its square, pop density, and state and year effects.
"IV" columns instrument for neighbors' spending with neighbors' instruments (same weights).
As in CHR, the wt matrix is determined by the mean of the variable over the sample
(except mobility, from1980 PUMS, and population-weighted geography, from 1983 population).