Should we expect a race to the bottom in welfare benefits? Evidence from a multistate panel, 1979-1995

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Abstract

Evidence exists that welfare recipients migrate between states to seek more generous benefits, potentially leading states to lower AFDC benefits to avoid such welfare migration. Taken further, this raises the specter of states competitively lowering benefits in reaction to similar moves by other states. The naïve model of policymaking assumes that benefits are solely a function of state characteristics. If benefits depend on the threat of welfare migration or other interstate competition, however, one must account for possible spillovers. This paper presents tests for the presence of welfare-policy spillovers in a panel of 47 states over the period 1979-1995. I find weak evidence of spillovers even in the presence of state fixed effects and political and budgetary-control variables.

I. Introduction

The primary cash support program for poor Americans is Temporary Assistance to Needy Families (TANF), long called Aid to Families with Dependent Children (AFDC).[1] AFDC was financed jointly by states and the federal government. States set income and asset limits within federal guidelines, set maximum benefit levels, and administer the program (House of Representatives 1994). As an entitlement program, every qualified applicant received AFDC benefits. Congress recently changed AFDC to a federal block-grant program, however, raising each state's marginal cost of benefits by 100% or more. How will states react? They may lower benefit levels during recessions to avoid extra expenditures, limit the duration of benefits, or simply continue the long-term downward trend in real AFDC benefits (House of Representatives 1994). They may also try to pass off some of the additional cost to federally financed programs such as Food Stamps and Medicaid.[2]

The rise in marginal benefit cost also puts states in potential competition. Because the poor may migrate, there is an increased incentive for states to engage in a tit-for-tat reduction of benefits in an attempt to discourage in-migration of other states' poor.

To learn how states will react to the new funding scheme, we must understand what determines state benefit levels. This paper extends the literature on benefit determination by testing for interstate policy spillovers in the presence of a wide array of political-party and budgetary controls. It also determines whether the apparent effect of these variables is due to unobserved state-specific factors. I find that states do adjust their own welfare benefits in

[1] Throughout I will refer to AFDC as it was structured during the period of the study (1979-1995) and not to its replacement, TANF.

[2] Regarding Food Stamps, see Orr (1979), Hulten *and others* (1982), Gramlich (1982), and Plotnick and Winters (1985). Although results have been mixed, the most recent study finds evidence in favor of such substitution (Moffitt 1990). For similar evidence concerning Medicaid, see Coughlin *and others* (1994).

response to those of contiguous neighboring states. This spillover effect is not consistent, however, with the large-scale benefit reductions that some have predicted (Peterson and Rom 1989). Less-populous states, and those historically gaining the most poor people through migration, theoretically should be the most sensitive to welfare migration. Even these states do not show a clear pattern of spillovers in state welfare policymaking, however. I conclude that welfare migration is not a prominent concern of legislators in most states.

Theories of Benefit-Setting

Models of welfare by economists are usually based on the interdependent utility model of Orr (1976) in which state residents are either taxpayers or poor. While the poor care only for their own income, taxpayers are altruistic and care about both groups. Because everyone values the poor's income, its transfer-induced rise is a public good.

Several other explanations for the existence of welfare also have been suggested, including income security for the nonpoor (Varian 1980) and securing electoral support (Peltzman 1980). Welfare may result from competition among political parties (Key 1949; Kasper 1971; Jennings 1979; Plotnick and Winters 1985, 1990) or pressure from interest groups (Becker 1983; Plotnick 1986).

Several studies have found that when the poor move, they are more likely than the nonpoor to move to states with higher benefit levels (Southwick 1981; Gramlich and Laren 1984; Blank 1988). A recent study of migration between border counties of several sets of adjacent states similarly finds evidence of welfare migration (Walker 1996). Migration of the poor may depend on other factors, of course, such as personal circumstances of recipients (Allard and Danziger 1997).

In current policy discussions, the term "race to the bottom" is applied to the notion that states may competitively lower AFDC benefit levels in order to encourage potential recipients to move away, as well as to discourage in-migration of other states' poor. In both cases, the goal is to reduce state welfare costs. This has only recently been subjected to empirical testing by Scheve and others (1996), who find weak support for the thesis. This paper improves on their analysis by controlling for a wider range of factors in benefit-setting. It also reports results from two models that further test the effect of state population on spatial spillovers in benefits.

The next section derives an appropriate utility function for benefit setting, that of state legislators. In Section 3 I detail how the spatial spillover theory may be tested. The data and models are discussed in Section 4, followed by estimation results in Section 5. Section 6 presents the two additional models to test the spatial spillover hypothesis. Section 7 concludes.

II. Modeling Legislators' Utility

Political scientists have long debated the role of the party system in determining aid to the poor. Key (1949) first suggested that the poor would fare better in states with active competition among parties. Jennings (1979) modified this, arguing that party competition only aids the poor if parties split along class lines. Orr (1976) follows what political scientists label the party convergence theory: under certain conditions, parties converge to the preferred policy of the median voter (Downs 1957). When such convergence does not occur--the current American situation--it may be that stable, long-standing differences exist among parties. The basic premise of interparty competition theories is that the party in power implements its preferred policies (Jennings 1979; Plotnick and Winters 1985, 1990).

In the public good model, taxpaying (nonpoor) citizens demand welfare transfers in order to raise their own utility. Orr (1976) further assumes that the actual level of transfers is determined by majority vote. Implicitly he assumes that lawmakers follow the dictates of voters, but this is likely to be untrue. Politicians are not constrained to follow voters' desires, and citizens cannot directly reveal their welfare preferences through a vote. Nor can general elections be construed as mandates on welfare, since they could equally be mandates on taxes or highway speed limits.

One modification of the electoral school of politics ascribes a greater role to politicians' own preferences. State legislators choose policies according to their own interests, but within boundaries or constraints of public opinion (Oberlander 1995). Because state legislators set AFDC benefit levels (and thereby the sum of AFDC and Food Stamps), it makes sense to model their utility. I hypothesize that it is a function the perceived chances of reelection (E), the income of the poor (Y_p), and preferences about the welfare system or its recipients (Ts):

$$(2) \qquad U_s = U(E; Y_p; T),$$

where s indexes the state. Legislators are assumed to know the value of other states' benefits, as well as economic and demographic information about their own states.

Likelihood of reelection is assumed to be a function of household income and public support for the current benefit levels, both of which would be affected by changes in benefits. A natural measure of income is state per-capita income. Although they clearly affect income, tax rates should be avoided in general. They are set by legislators concurrently with welfare benefits and so cannot be treated as exogenous variables.

The unemployment rate may also figure into reelection chances. Suppose that a weak economy hurts incumbents at the polls. A politician may react to high unemployment by seeking to cushion the impact of job loss through higher transfers, including welfare payments. Alternatively, legislators may wish to avoid budget overruns. In the face of higher unemployment – and hence higher demand for welfare – they may decide to lower benefits.

Next, I assume legislators directly gain utility from raising the income of the poor. Raising Y_p may also increase legislators' utility indirectly by encouraging electoral support (Peltzman 1980).

Preference term T is added for several reasons. There is deep disagreement as to whether raising welfare benefits is the proper way to raise Y_p. The utility function must be able to accommodate a desire to raise Y_p and a simultaneous desire to lower welfare benefits. Otherwise, there would be no way to explain lower benefits except through electoral politics. If welfare recipients as a group are politically unpopular, this will also be captured in T. Moffitt and others (1996) find the long-term fall in benefits to relate to a similar fall in the bottom of the real wage distribution, consistent with the theory that the nonpoor (or politicians) desire not just to aid the poor but also to maintain a consistent work incentive for them.

All politicians in a state are assumed to have the same utility function, so T becomes a measure of state (or even regional) preferences, whether political or social in nature. I improve on the traditional regional dummies by estimating models with state fixed effects and by allowing interaction between the Southeast-state dummy and the political variables.

III. Spatial Autocorrelation

If we suppose that states are affected by the policies of neighboring states, we must ask how "neighbor" is defined. When the primary concern is welfare migration, it is natural to define neighborhood geographically.[3] I will do so here, labeling a state as another state's neighbor if the two share a common border.

To create a weighted average of the benefit levels in contiguous states, there must be a matrix containing the weights assigned to each state relative to each other state. Call this NxN weight matrix W, where N is 47, the number of states. Each row of W corresponds to a single state. The matrix W will be row standardized, meaning that the sum of weights for each row is 1. At first each contiguous state will have equal weight, while all noncontiguous states have weight 0.

Consider two examples. In Florida's row of W, the columns corresponding to Georgia, Alabama, and Mississippi each have a weight of 1/3, while all other columns contain 0. Similarly, in the row corresponding to Maine, the column for New Hampshire has a value of 1 while all other columns contain zeros. Thus $W*B_{t-1,s'}$ is the weighted average value of welfare benefits in contiguous states, where B_{t-1} is the Nx1 matrix of welfare benefits in each state in year t-1. Neighboring state benefits are lagged relative to the dependent state's (hence subscript *t-1*) since benefits are set a year in advance. Thus when estimating the Maine benefit for 1986, one would consider New Hampshire's 1985 benefit.

[3] Policymaking may follow other neighborhood patterns, however. In a study of state expenditures, Case and others (1993) find that defining neighborhood by the percentage of Black residents best fits their data. On auto emissions, a group of Northeastern states simultaneously adopted California's strict controls.

Applying equal weight to each neighboring state is an arbitrary choice. It is more plausible to weight the states based on their relative populations, such that larger contiguous states receive more weight than smaller ones. This makes sense if states are concerned about population flows, since larger states are larger potential sources of migratory poor people. Models with this alternative set of population weights will be estimated as well.

One shortcoming of these spatial models is that when considering the benefits of an individual state, they take the other states' benefit levels as fixed. A possibility unexplored here is a game-theoretic model of benefit-setting among states. Brown and Oates (1987) derive a simple two-state general equilibrium model in which each state's benefits depend partly on welfare migration from the other state, based on the difference in benefits between the two. They find that an increase in mobility of the poor will typically decrease the average benefit level of the two states. Figlio and others (1997) develop a game-theoretic model that implies that states should react more to decreases in neighboring states' benefits than to increases. Using a panel of state data from 1983-1994, they find evidence that supports their hypothesis. Smith (1991) presents a model in which state legislators consider the income, previous benefits and other features of other states when setting their own benefit levels. She finds that states do take into account the features of others within a 750-mile radius.

These game-theoretic models have two shortcomings. They rely on strong assumptions about the structure of preferences to obtain reaction functions. Even when the data seem consistent with the model, as in Figlio and others (1997), it is unclear whether the data would also support models with substantially different reaction functions. This matters if the goal of estimating reaction functions is to find the theoretic equilibrium of the game. A second

difficulty is that game-theoretic models to date generally lack measures of political-party control or fiscal constraints, thereby ignoring a potentially important set of determinants.

Spatial autocorrelation models may be misleading in that a spatial effect may appear due to omitted variables. That is, states in a region may simultaneously react to similar economic (or other) variables not included in the regression. Given the similarity of benefit levels year to year, this could lead to a spurious finding of spatial correlation across states. I handle this in two ways. First, I explicitly test for spatial dependence in the error terms and find that I cannot reject the null hypothesis of no dependence; this will be detailed in the following section. Second, to avoid any remaining regional correlation I use predicted benefit values in place of real values in building the weighting average benefit matrices in equations (3.c), (3.e) and (3.f).

IV. Models

I model the underlying data-generating function in equation (3.1). Equations (3.2) and (3.3) allow for various assumptions about the error term in (3.1).

$$(3.1) \quad \mathbf{B}_{s,t} = \beta \mathbf{X}_{s,t-1} + \rho_L \mathbf{W}_L \mathbf{B}_{s',t-1} + \rho_H \mathbf{W}_H \mathbf{B}_{s',t-1} + \gamma_t \mathbf{t} + \eta_s \mathbf{s} + \varepsilon_{s,t},$$

$$(3.2) \quad \varepsilon_{s,t} = \lambda_s \mathbf{W} \varepsilon_{s',t-1} + \delta_{s,t},$$

$$(3.3) \quad E[\delta_{i,j}, \delta_{k,l}] = \Omega,$$

The analysis includes 47 states for the period 1979-1995.[4] Equation (3.1) models benefit levels as a function of own-state characteristics in the previous year ($\mathbf{X}_{s,t-1}$).[5] Equation (3.1) also

[4] Alaska and Hawaii are omitted because they lack contiguous neighbors. The District of Columbia and Nebraska are omitted because they lack partisan, bicameral legislatures.

lets benefit levels in neighboring states ($\mathbf{B}_{s',t-1}$) to have a spillover effect. Weighting matrices W_L and W_H allow lower-benefit and higher-benefit neighbors to have separate spillover effects.[6] They each incorporate either the equal or population-based weighting assumptions described in Section III. Vector **t** includes 16 year dummies to account for national trends in benefit levels (1995 is omitted).

Alternative Assumptions about the Model

Equations (3.1)-(3.3) can incorporate a large number of testable restrictions. In this section I discuss a number of these and the resulting models tested.

Due to the possibility of unobserved state characteristics correlated with welfare generosity, equation (3.1) includes **s**, a vector of 47 state dummies. Even in the presence of state fixed effects, apparent spillovers may simply reflect regional movements in state benefits. I therefore estimate models with three variants: no state fixed effects; state fixed effects; and region fixed effects, in which I restrict the state dummies to be constant within each of 9 regions defined by the Census Bureau. The t-statistic reveals the value
of adding either kind of state effect, while an F-test assesses the importance of the distinguishing between region and state fixed effects.

Equation (3.2) models autocorrelation in the error term of (3.1). The term $W\varepsilon_{s',t-1}$ allows for spatial dependence in the error term reflecting "spatial autocorrelation in measurement errors or in variables that are otherwise not crucial to the model," such as omitted variables (Anselin 1996). Alternatively I can restrict W to be the identity matrix, yielding first-order

[5] See Appendix 1 for definitions and descriptive statistics.

[6] The weighting matrices W_H and W_L vary by state and year, but these subscripts are omitted for notational clarity.

(temporal) autocorrelation in the residuals. Tests for spatial autocorrelation are discussed in the next section.

Explicitly modeling state fixed effects may still leave a state-specific component in the error terms. In order to capture any such differences, equation (3.1) should be estimated with heteroskedastic errors. What if the apparent similarity of contiguous states' policies simply reflects omitted regional economic circumstances? One solution is to restrict the state dummies to be regional dummies, as mentioned above. Another method would estimate a covariance matrix in which $E[\delta_{i,t} , \delta_{k,t}] = \sigma_{ik}$. The number of estimated covariance parameters would be (47*48)/2 or 1128, however, which is more than the 799 observations available. I therefore restrict the covariance matrix Ω to allow nonzero state variances but zero covariances:

(3.3') $\Omega_{ii} = \sigma_i , \quad \Omega_{ik} = 0$

The Gauss-Markov theorem states that OLS is optimal only if, among other requirements, the independent variables are uncorrelated with the error term. If in the true model the spatial coefficients ρ_L or ρ_H is nonzero, OLS estimation of equation (3.1) is biased and inconsistent (Anselin 1988; Case 1991). The difficulty arises from the presence of a spatially lagged value of the dependent variable on the right-hand side of the equation, which is correlated with the error term. This can be overcome through the method of instrumental variables.

The choice of instruments is determined by the assumptions made about the source of correlation. If it reflects factors fixed over the duration of the study, it will be absorbed by the state fixed effects but not by region effects. Region-specific factors that are constant will be removed by either state or region fixed effects. Fixed factors may also be instrumented through

time-varying regressors that correlate with own-state benefits but not with neighbors' benefits. Another possibility is that potential endogeneity reflects time-varying unobserved factors that change substantially over over the period of the study. In this case time-varying instruments must be used.

I test two-stage least squares models under several different assumptions. The first is a naive model that assumes no endogeneity, so lagged neighbors' benefits ($\mathbf{B}_{s',t-1}$) are used directly. The remaining models include either state or region fixed effects, alternately accounting for potential time-invariant sources of correlation. I also instrument lagged neighbors' benefits with neighbors' other characteristics lagged an additional year:

$$(3.4) \quad \underline{B}_{s,t-1} = \varphi \mathbf{X}_{s,t-2} + \upsilon_{s,t} \quad ,$$

where the error term $\upsilon_{s,t}$ is assumed to be distributed Normal with mean zero.[7] The identification here comes from the presumed correlation between a state's benefit levels and its economic and political conditions of the prior year. I include dummy variables to denote the year within the gubernatorial cycle, as well as others for the political control of the state legislature and governorship, all of which should be unrelated to any regional economic trends not found in per-capita income and unemployment. The instruments perform well. The F-statistic for joint significance of all regressors is highly significant ($F=29.3$, $p < .001$), as is the F-statistic for the subset of political variables described above ($F=11.6$, $p < .01$). Equation (3.1) may therefore be rewritten to take account of this two-stage method:

[7] I adjust the second-stage standard errors to account for the additional error caused by the use of a predicted regressor. See Murphy and Topel (1985).

$$(3.1') \quad \mathbf{B}_{s,t} = \beta\mathbf{X}_{s,t-1} + \rho_L\mathbf{W}_L\underline{\mathbf{B}}_{s',t-1} + \rho_H\mathbf{W}_H\underline{\mathbf{B}}_{s',t-1} + \gamma_t\mathbf{t} + \eta_s\mathbf{s} + \varepsilon_{s,t},$$

where $\underline{B}_{s,t}$ refers to instrumented benefits in state *s* in year *t*.

Testing for Spatial Autocorrelation

Among several tests for spatial autocorrelation, the most common is Moran's I (Moran 1950b; see Case 1991 for details). Referring to equations (3.1) and (3.2), the joint null hypothesis is $\rho_L = \rho_H = \lambda = 0$. It was performed once using the equal-weighting scheme and again with the population-based weights. In both cases I could reject the null at the 99%-confidence level. This suggests that there is considerable spatial autocorrelation of some form in my data. I then employed a Lagrange Multiplier test of the hypothesis $\lambda = 0$ (Anselin and Bera 1996). The test did not reject the null hypothesis at the 90% confidence level, so I will assume that there is no spatial dependence in the error term. I still allow for first-order autocorrelation, however, so that equation (3.2) may be rewritten as follows:

$$(3.2') \quad \varepsilon_{s,t} = r_s\varepsilon_{s,t-1} + \delta_{s,t}.$$

The presence of autocorrelated errors suggests an iterative procedure to obtain the greatest efficiency.[8]

I expect that both ρ_L and ρ_H will be positive, with ρ_H being significantly smaller. If its neighbors raise their benefits, a state has some freedom to raise its own benefits without increasing the risk of welfare migration. As well, if a neighbor lowers its benefits, there is

[8] Briefly, the process is as follows: benefit levels are predicted using exogenous variables. Using these predicted values, OLS estimates of the primary equation are computed. The resulting errors are used to obtain a first estimate of the autocorrelation coefficients r_s. The Prais-Winsten transformation is carried out using r_s, and then FGLS is done to allow for heteroskedasticity. The errors of these new estimates are used to obtain the next estimates of r_s, which are employed in a new transformation of the data, and so on.

incentive for a state to lower its own. In both cases the correlation is positive between the direction of change in own benefits and neighbor's benefits. The value of separating high- and low-benefit states will be assessed through F-tests of the difference between ρ_L and ρ_H. If they are similar in magnitude and sign it would suggest that high- and low-benefit states need not be treated separately.

The spatial-autoregressive models will be estimated alternately with equal weights and with population-based weights. In all cases year fixed effects are included. In models that also include state fixed effects, time-invariant variables may not be included. The goal is to reveal the extent to which the partial correlation of the exogenous variables and benefits actually reflects underlying unobserved variables across the states.

Explanatory Variables

The primary economic variable in these models is annual per-capita state income. Because the federal matching rate for AFDC (and Medicaid) was based on per-capita income, it is a measure of the cost of providing benefits. It also measures the ability of the state's residents to support benefits through taxes. The state share of AFDC spending for each state will not be included separately, since it is highly correlated with per-capita income.[9] It also changes little over time, so most variation would be washed out by the state fixed effects.

Due to federal matching, one would expect per-capita income to have a negative effect on AFDC benefits: higher income leads to a higher price-per-dollar. Yet every study has found per-capita income to have a positive coefficient (e.g., Orr 1976; Ribar and Wilhelm 1994; Scheve

[9] As of 1981, all but two states used the Medicaid formula based on per-capita income. The state share is $.45*(Y/Z)^2$, where Y is per-capita state income and Z is the national average per-capita income. The minimum state share is .17, the maximum .50.

and others 1996). This may occur for several reasons. Wealthier states can collect greater tax revenues, and they may have fewer recipients to support. There may also be an unobserved preference for higher welfare benefits in wealthier states. Or it may be that poorer states have a taste for less government support. Whatever the reason, the positive (and statistically significant) effect of per-capita income on benefit levels is the most regular empirical finding in this literature.

I include the unemployment rate since higher unemployment increases the number of welfare recipients. If lawmakers primarily wish to hold down costs, one would expect them to lower benefits in response, leading to a negative coefficient. If instead their goal were to cushion job-losers during a bad economy, a positive coefficient would result.

In order to provide a more thorough analysis of the political economy of welfare benefits than has been found in earlier papers, I include a number of measures of state political control. The first is a dummy variable signifying that the state legislature and governorship are both controlled by Democrats (see Appendix A for details). Unified and divided governments may react differently to economic factors as well as demographic. Alt and Lowry (1994) and Roubini and Sachs (1989) study the impact of party control on budget deficits at the state and federal levels. Both find that divided governments are more likely to run deficits than are those with single-party control.

A considerable political science literature has studied whether the poor fare better under Democrat regimes, and why this pattern seems not to hold in the South (Key 1949; Jennings 1979; Dye 1984; Erikson and others 1989; Brown 1995). One explanation is that the effect of Democrat control may not be positive once one controls for other major factors. A dummy variable may not capture the true effect of political control, however, so I also add measures for

the percentage of each house of the state government that is held by Democrats. All else equal, I expect stronger Democratic control to be correlated with higher welfare benefits.[10] If Jennings (1979) is correct, the traditional lack of party competition in Southeastern states should lead to worse outcomes for the poor. To test the theory that Southern states react differently to party control than do others, I allow Southeastern states to have separate interactions with the political control variables.[11]

Two demographic variables analyzed are the percentage of Blacks and the percentage of people aged 65 or older. I expect the latter to be negatively correlated with welfare benefit levels because the nonelderly poor and the elderly compete for state social-service dollars. Blacks are poorer on average than Whites, and so a higher proportion of Blacks will tend to mean a higher number of welfare recipients. The eventual impact on welfare benefits is ambiguous, however; states could react either by lowering benefits to keep spending constant or by raising benefits to help more people. At the simplest political level, Blacks may have less political clout or be deemed less worthy of support, leading to a negative coefficient on the proportion of Blacks. In order to allow a more complex relationship, I interact each state's proportion of Black population with the political control variables. Again, Jennings (1979) implies that there will be a positive correlation between benefits and the proportion of Blacks only in states with substantial competition between parties.

[10] Political party labels do not necessarily carry the same meaning across states or over time. Measures of average state political ideology, as in Wright, Erickson and McIver (1985), would be preferable but are not regularly collected.

[11] South (or Southeastern) refers to the following states: Mississippi, Alabama, Georgia, Florida, South Carolina, North Carolina, Virginia, Maryland, West Virginia, Tennessee, and Kentucky.

A measure of the state's fiscal condition is also included: the ratio of per-capita state debt to per-capita personal income. States may reduce spending on welfare as their debts and deficits rise relative to their tax bases, corresponding to an expected negative coefficient on the debt-to-income ratio.

Three variables measure budgetary restrictions on the governor and the legislators. Gubernatorial budget authority is measured by the presence of a line-item veto, and by the ability to reduce appropriations bills unilaterally when coupled with a line-item veto. More authority in the governor's hands is expected to reduce welfare benefits, since governors are hypothesized to face less scrutiny on this issue than local representatives. The ability to carry expected deficits over to the next year will be associated with higher benefits if it allows state governments to avoid benefit reduction in lean years. The effect of these variables on states' general funds is considered in Bohn and Inman (1996). They find "tentative evidence" that line item vetoes lead to lower deficits, and stronger evidence that rules disallowing carryover have substantial effects on state budgets. The influence of governors is also at stake in the final three variables, which measure the extent of gubernatorial political cycles. If governors attempt to vary tax rates and spending over their terms to maximize their chances of reelection, the coefficients will be significant. Their signs are unclear in theory, however, since spending more and spending less are both ways to win electoral support.

V. Estimation Results

In this section I discuss estimation results. In order to make them concrete, Table 4 illustrates the dollar-value of the impact of per-capita income and of the spatial spillover terms, and their implied elasticities. Besides the national average effects, I also focus on three

particular states: Illinois, Iowa, and Wisconsin. They are of particular interest for several reasons. First, they are geographically contiguous, meaning each state's benefit levels will have the chance to affect the other two state's benefits in my models. Second, there has been much debate in Wisconsin about welfare migration from the Chicago area. Finally, Walker's (1996) study found evidence of welfare migration between border counties of Wisconsin and its neighbors. Results from estimation of equation (3.1) are found in Table 1; Table 2 includes results from equation (3.1'), a 2SLS model that treats neighbors' lagged benefits as endogenous.

Spatial Autocorrelation Coefficients

Table 4 summarizes the spatial coefficients from each model, divided according to the weighting scheme used for neighboring states. The values are small and often insignificant, although a few achieve at least the 90% confidence level. The bottom row of Tables 4.A and 4.B shows the results of F-tests of the null hypothesis that the two spatial coefficients are equal. Only one case yields a test statistic close to 3.00, the 95% confidence value, implying that we may treat all neighboring states jointly.

I find only weak support for the welfare migration hypothesis. The low F-statistics contradict it since lower-benefit neighbors should influence own-state benefits more than higher-benefit neighbors. The coefficient on lower-benefit neighbors (ρ_L) also ought to be positive and larger than that of higher-benefit neighbors (ρ_H). I do find that most coefficients are positive, but in half the models ρ_L is less than ρ_H, not greater.

How much do spillovers affect state benefit levels? Table 5.A provides estimates for three states and for the nation as a whole. Using models with population-based weights, a 5% rise in neighboring states' 1994 benefits leads to a rise in own-state benefits of less than $1.25.

The implied elasticity of benefits is roughly 3 percent. These findings imply that spatial spillovers are not substantially determining state benefit levels.[12]

Economic Variables

Economic variables seem to play a substantial role in explaining benefit levels. The variables representing per-capita income are not always individually significant, but F-tests reveal joint significance at the 99-percent confidence level. Table 5.B illustrates the impact on 1995 benefits of a 1 percent rise in 1994 income. The implied elasticities range from .18 to .55, with a national average of .22 for models that include state fixed effects. Considering that income has risen steadily over the period 1979-1995, we must look to other explanations for the long-term downward trend in welfare benefits.

The remaining economic variables are often statistically significant as well. Unemployment's coefficients are positive and significant but small: a 2% rise would yield less than a $5 change in welfare benefits, all else equal. Its positive sign suggests that state governments raise welfare benefits in order to cushion job loss during a recession. The debt-to-income ratio, a measure of the state's fiscal condition, is positive in the absence of fixed effects but negative with them. This implies that states with higher debt also have a preference for higher benefits, but that within each state higher debt leads to lower spending.

Political and Demographic Variables

Tables 1-3 also report the coefficients of variables measuring legislative party control. As discussed earlier, political scientists have long theorized that party control has a different effect in the South than in other regions. In order to capture this possibility, a Southern-state

[12] I simulated a counterfactual in which spillovers were allowed to affect other states' benefits in repeated iterations while other independent variables were held constant at their 1994 levels. In most cases it failed to converge. Those that did reached convergence within 10 iterations and sustained an average fall in benefits of less than 2-3 percent.

dummy is interacted with the political variables. The proportion of Blacks, usually measured by a single variable, is allowed to have a more complex relationship with welfare as well, again through interactions with the political
variables.

Relative to Republican control, I find Democrat control outside the Southeast to be associated with roughly a $35 increase in monthly benefits, while divided control leads to a $10 increase. The story is reversed in Southeastern states. There, states with stronger Democrat control of the state legislature have lower benefits, supporting Jennings' (1978) theory that it is party competition that raises benefits.

Overall a higher proportion of Blacks is associated with slightly lower benefits, even holding constant Southern region, income, and other factors. This may reflect a lack of political clout among Blacks, or instead a political focus among Blacks on issues besides welfare. I do find support for the interaction of Black population and party control, although the mixed pattern of signs and significance levels has no obvious explanation.

The proportion of senior citizens was expected to have a negative and significant coefficient. I find these results in most cases, suggesting that senior citizens compete with the poor for program funds. The small coefficients imply that the proportion of senior citizens explains benefit differences across states more than within them, since cross-sectional variation is large but within-state change is quite slow.

The final political variables represent the year within the governor's term. If there are political business cycles in state spending, welfare benefits may rise in the year preceding the gubernatorial election year. These variables have the expected negative coefficient for years 2 and 3 of the governor's term, but are significant in the final year as well. The implied political

cycle is that of larger budgets in the first year followed by slower welfare spending every year after. This is not consistent with governors attempting to buy votes from the poor.

Budgetary-control Variables

Across all models, I find the three budgetary-control variables to have sizable impacts on welfare benefits. They support the theory that greater gubernatorial power--in the form of a line-item veto and the ability to unilaterally reduce appropriations bills--leads to substantially lower welfare benefits. This may reflect unobserved state factors variously described as 'political culture' or 'ideology' as in Wright and others (1987). If so, then state fixed effects should reduce these to statistical insignificance. To date, however, there is insufficient variation across time to test this hypothesis. Over the period 1979-1995, two of the variables are fixed and so drop out once state effects are added. The remaining variable--the ability to carry over deficits--keeps its positive sign but falls dramatically and loses statistical significance.

VI. Extensions

This section presents two extensions that further test the link between welfare migration and spatial spillovers in benefits.[13]

Smaller states should be more sensitive to welfare migration than large ones, all else equal, since an equal number of migrants will have a greater relative fiscal impact.

In the previous models estimated with population weights, larger states' benefits were given more weight than smaller states' benefits. Here I allow states with smaller populations to have separate spillover coefficients. If the smaller states have positive coefficients on the spillover

[13] The results of each, not shown here, are available on request.

variable for lower-benefit neighbors that are larger than the coefficients for larger states, the welfare migration theory will gain support.

Because the definition of 'small' is arbitrary, I test two population groups. The first contains states whose populations in a given year are less than one-half of the 17-year average of all 47 states ($\mu = 5.2$ million). There are 15-19 states in this group depending on the year. The second group includes states with populations up to one-quarter of the 17-year average; 10-12 states fall here each year. Appendix A lists the members of each group.

The models modify equation (3.1') by the addition of interaction terms between the spatial terms and the low-population-state dummy. For example, equation (4) illustrates a model that includes state fixed effects and defines low-population states (LPS_{25}) by the more restrictive definition:

$$(4) \quad B_{s,t} = \beta \mathbf{X}_{s,t-1} + \rho_{L1} W_L \underline{B}_{s',t-1} + \rho_{H1} W_H \underline{B}_{s',t-1} + \rho_{L2} LPS_{25} W_L \underline{B}_{s',t-1} + \rho_{H2} LPS_{25} W_H \underline{B}_{s',t-1} + \gamma_t \mathbf{t} + \delta_s \mathbf{s} + \varepsilon_{s,t}$$

All of these models utilize population weights in matrix W. As before, the errors are assumed to be heteroskedastic across states and to exhibit first-order autocorrelation. The goal is to determine the signs and magnitudes of the new spillover coefficients ρ_{L2} and ρ_{H2}.

I find the lower-benefit spillover terms to have small, positive coefficients that rise as expected for low-population states. Together they are significantly only in the model with the more inclusive definition of low population. The coefficients on higher-benefit neighbors are also positive but become negative and significant when interacted with the low-population dummy. This suggests that smaller states react more strongly to neighbors'

benefits, as expected, but the opposing signs do not yield a single conclusion. Altogether, the results of this model do not support the theory that welfare migration is a major determinant of welfare benefits.

State legislators may instead be concerned about the total number of migrants and not the relative number. This possibility is addressed in the second extension. Frey and others (1995) collated the 1990 Census migration data by income group, dividing migrants into those above and below the poverty line. They determine three groups: those states with the greatest net loss of poor people; those with the greatest net gain of poor people; and those with the greatest net gain of nonpoor people.[14] In the final model I allow each of these state groups to have a separate set of spatial spillover coefficients. I expect net *gainers* and net *losers* of the poor to act differently, with the former group being most sensitive to neighboring states' benefits. Comparing net gainers of the *poor* to net gainers of *nonpoor*, I again expect the former group to be more sensitive. If F-tests reveal no statistically significant difference among these, it would tend to contradict the theory that welfare migration concerns are substantially determining welfare benefits. A single model is estimated, similar to equation (3.1'), with population-based weights but without state fixed effects. Dropping state effects allows estimation of dummy variables for each state group. I also drop the Southeast dummy, since it overlaps considerably with the dummy representing poor-population gainers.

In order to avoid potential endogeneity between population flows and benefit levels, I limit the data to the period 1991-1995, the years following the data collection.
The resulting short panel precludes correction for autocorrelation or heteroskedasticity in the errors, since five observations per state is too few for reliable estimation of the necessary

[14]Not all states could be included. See Appendix 2.B for the members of each group.

parameters. The overall effect is to increase the likelihood of insignificant coefficients. It may also lead to large spillover coefficients, since the iterative process used for other models tends to reduce the original estimates from two-state least squares.

The results again do not support the purported link between welfare migration and state welfare benefits. Considering states gaining poor population versus those gaining nonpoor population, I find that the spatial spillover coefficients are significantly different from each other in both cases. This is surprising since the two groups could have overlapped by six states (see Appendix B.2, note 2). States that gain poor population in theory ought to be concerned about welfare benefits in lower-benefit neighbors, but in fact their spillover coefficient is insignificant. Moreover, the coefficients for poor-population gainers are smaller than for nonpoor gainers, the opposite of what the welfare migration theory would suggest.

Comparing gainers and losers of poor population , I again find that coefficients differ substantially across the groups. In this case the pattern better fits the welfare migration theory, since gainers of the poor are more sensitive to neighbors' benefits that gainers of the nonpoor.

In sum, the two extensions presented in this section do not support the theory that fears of welfare migration, whether inspired by actual population flows or by relative state populations, are exerting a substantial effect on welfare benefits.

VII. Conclusion

Most discussion of an impending “race to the bottom” has taken place in the context of the funding status of AFDC (Scheve and others, 1996). Federal support for AFDC has turned from open-ended matching to a closed-end block grant, leaving states to bear the entire cost of the marginal dollar of benefits. This adds new pressure to lower the number of welfare recipients. The analysis presented here suggests that although there may be some spillover in the

setting of AFDC benefits, it is not substantial. In addition, I find that states seem to treat higher- and lower-benefit neighbors similarly. Both of these results suggest that we should not expect benefit levels to fall due to legislators' fears of welfare migration.

My research confirms earlier findings that economic and demographic factors like per-capita income and the percentage of Blacks play a significant role in welfare policy. Employing a more extensive array of political and budgetary factors than others have, I also find strong evidence that political party control has a substantial impact, both alone and interacted with regional and demographic factors.

One question that remains is whether previous patterns of benefit setting will continue now that federal funding has moved to a block-grant system. In the next recession when state experience deficits instead of the current widespread surpluses, politicians may become more sensitive to the possibility of welfare migration and its attendant costs. Another possibility is that states may choose to raise spending on other aspects of the welfare system, such as child care, while allowing benefits to continue falling. Finally, the new freedom that states enjoy to determine their welfare plans should lend increasing importance to policy leadership among states. Future research should consider, for example, how the professionalism of state legislatures affects the likelihood that they actively seek guidance from other states instead of merely reacting to unintended spillovers.

Table 1

2SLS Model of Monthly Welfare Benefits With Two Spatial Effects

	(1)	(2)		(1)	(2)
Fixed Effects	None	None	**Percent Black * Percent Dem't -Lower House**	-1.50	-1.70
				(1.20)	(1.50)
Weighting Scheme	Equal	Population			
			Southeastern State	-28.4	140.
Benefit Average: Lower Benefit States (ρ_L)	-.096	.047		(1.03)	(7.08)
	(2.88)	(2.08)			
			South * Democrat Control	.053	-52.8
Benefit Average: Higher Benefit States (ρ_H)	.008	.011		(.586)	(4.82)
	(.325)	(.509)			
			South * Divided Control	.053	-25.0
Per-Capita Income ($100s)	-7.25	-1.77		(.620)	(2.31)
	(2.80)	(1.01)			
			South * Percent Democrat-Upper House	-1.79	-.451
Per-Capita Income^2 (x 1000^{-1})	34.7	4.11		(6.96)	(2.48)
	(2.69)	(.477)			
			South * Percent Democrat - Lower House	-.306	-1.25
Per-Capita Income^3 (x $1{,}000{,}000^{-1}$)	-45.0	6.71		(.825)	(6.58)
	(2.15)	(.475)			
			Percent Over Age 65 (%)	-.195	-.161
Unemployment (%)	2.93	-1.63		(.271)	(.265)
	(4.01)	(4.57)			
			No Line-Item Veto	22.0	-101.
Per-Capita Debt-to-Income Ratio	19.3	20.2		(2.28)	(12.4)
	(7.02)	(11.8)			
			Carryover Expected Deficit	25.0	30.7
Democrat Government	3.06	.863		(6.28)	(7.26)
	(.658)	(.219)			
			Governor Can Reduce Appropriations Bills	-113.	-95.8
Divided Government	5.18	12.6		(20.2)	(16.5)
	(1.52)	(3.77)			
			Political Cycle - Year 2	-1.27	.347
Percent Democrats - Upper House (%)	1.10	.436		(.961)	(.342)
	(5.29)	(2.72)			
			Political Cycle - Year 3	-1.51	-.649
Percent Democrats - Lower House (%)	.527	.843		(1.01)	(3.44)
	(2.40)	(4.19)			
			Political Cycle - Year 4	1.50	.607
Percent Black (%)	1.29	.908		(1.06)	(.707)
	(2.71)	(2.03)			
			F-test of Joint Significance of Income Variables	84.4	49.3
Percent Black * Democrat Control	-.376	.323			
	(1.59)	(1.46)			
			F-test of Difference Between ρ_L and ρ_H	2.76	.394
Percent Black * Divided Control	-.414	-.649			
	(2.61)	(3.44)			
Percent Black * Percent Dem't –Upper House	.250	.607	**R^2**	.992	.992
	(.247)	(.707)			

Table 2
FGLS Model of Monthly Welfare Benefits With Two Spatial Effects

	(1)	(2)		(1)	(2)
Fixed Effects	None	None	**Percent Black * Percent Democrat - Upper House**	.049 (.050)	.002 (.231)
Weighting Scheme	Equal	Pop'n	**Percent Black * Percent Democrat - Lower House**	-.014 (1.17)	-.015 (1.19)
Weighted Benefit Average: Lower-Benefit States (ρ_L)	-.008 (.376)	.018 (.930)	**Southeastern State**	-3.05 (.111)	-8.76 (.316)
Weighted Benefit Average: Higher-Benefit States (ρ_H)	.049 (1.95)	.016 (.659)	**South * Democrat Control**	.456 (.050)	.051 (.006)
Per-Capita Income ($100s)	-8.36 (3.21)	-8.27 (3.19)	**South * Divided Control**	1.14 (.134)	2.13 (.252)
Per-Capita Income^2 (x 1000^{-1})	39.5 (3.07)	39.1 (3.05)	**South * Percent Democrat - Upper House**	-1.78 (6.98)	-1.74 (6.80)
Per-Capita Income^3 (x $1{,}000{,}000^{-1}$)	-51.8 (2.50)	-51.5 (2.48)	**South * Percent Democrat - Lower House**	-.597 (1.63)	-.564 (1.53)
Unemployment (%)	2.73 (3.81)	2.74 93.81)	**Percent Over Age 65** (%)	-.780 (1.10)	-.957 (1.33)
Per-Capita Debt-to-Income Ratio	20.0 (7.37)	20.6 (7.58)	**No Line-Item Veto**	19.2 (2.02)	18.4 (1.91)
Democrat Government	2.79 (.615)	3.27 (.721)	**Carryover Expected Deficit**	22.4 (5.84)	22.4 (5.85)
Divided Government	8.04 (2.37)	7.81 (2.32)	**Governor Can Reduce Appropriations Bills**	-110. (19.4)	-110. (19.5)
Percent Democrats - Upper House (%)	1.10 (5.43)	1.05 (5.18)	**Political Cycle - Year 2**	-.644 (.487)	-.796 (.602)
Percent Democrats - Lower House (%)	.604 (2.80)	.662 (3.08)	**Political Cycle - Year 3**	-1.14 (.763)	-1.37 (.914)
Percent Black (%)	1.15 (2.41)	1.09 (2.30)	**Political Cycle - Year 4**	1.66 (1.16)	1.59 (1.11)
Percent Black * Democrat Control	-.175 (.747)	-.224 (.956)	**F-test of Joint Significance of Income Variables**	85.2	82.4
Percent Black * Divided Control	-.394 (2.50)	-.413 (2.62)	**F-test of Difference Between ρ_L and ρ_H**	.815	.002
			R^2	.961	.962

Table 3
FGLS Model of Welfare Benefits With Separate Spatial Effects
and Geographic Fixed Effects

	(1)	(2)	(3)	(4)
Fixed Effects	Region	Region	State	State
Weighting Scheme	Equal	Population	Equal	Population
Weighted Benefit Average: Lower-Benefit States (ρ_L)	.025 (1.00)	.024 (1.08)	.004 (.529)	.008 (1.12)
Weighted Benefit Average: Higher-Benefit States (ρ_H)	.042 (1.86)	.035 (1.64)	.012 (.825)	.020 (1.46)
Per-Capita Income ($100s)	-2.41 (1.01)	-2.58 (1.07)	-5.44 (2.15)	-5.39 (2.13)
Per-Capita Income^2 (x 1000^{-1})	6.23 (.544)	7.05 (.611)	19.9 (1.59)	19.6 (1.57)
Per-Capita Income^3 (x $1{,}000{,}000^{-1}$)	.400 (.022)	-.791 (.044)	-17.2 (.849)	-16.8 (.829)
Unemployment (%)	1.44 (2.33)	1.45 (2.33)	-1.30 (2.98)	-1.27 (2.98)
Per-Capita Debt-to-Income Ratio	-8.62 (2.39)	-8.05 (2.24)	-8.48 (3.18)	-8.81 (3.32)
Democrat Government	4.72 (1.08)	4.47 (1.02)	1.81 (.429)	1.50 (.356)
Divided Government	-5.88 (2.03)	-5.82 (2.01)	6.07 (2.33)	5.87 (2.27)
Percent Democrats - Upper House (%)	.823 (4.67)	.842 (4.77)	.232 (1.56)	.251 (1.70)
Percent Democrats - Lower House (%)	-.610 (3.29)	-.625 (3.37)	.334 (1.78)	.328 (1.75)
Percent Black (%)	-.747 (1.61)	-.779 (1.67)	.436 (1.23)	.487 (1.38)
Percent Black * Democrat Control	.772 (3.45)	.755 (3.36)	.571 (2.89)	.584 (2.96)
Percent Black * Divided Control	.375 (2.73)	.355 (2.58)	-.065 (.491)	-.055 (.421)
Percent Black * Percent Democrat - Upper House	-.021 (2.42)	-.020 (2.36)	.003 (.310)	.002 (.234)

Table 3, cont'd

	(1)	(2)	(3)	(4)
Percent Black * Percent Democrat - Lower House	.019 (1.98)	.019 (2.02)	-.014 (1.62)	-.015 (1.66)
Southeastern State				
South * Democrat Control	-36.4 (4.63)	-35.9 (4.55)	-37.0 (4.83)	-37.1 (4.85)
South * Divided Control	-9.70 (1.40)	-9.31 (1.33)	-3.07 (.430)	-2.87 (.403)
South * Percent Democrat - Upper House	-.584 (2.43)	-.592 (2.44)	-.319 (1.84)	-.334 (1.94)
South * Percent Democrat - Lower House	.313 (1.11)	.309 (1.09)	-.495 (2.40)	-.456 (2.21)
Percent Over Age 65 (%)	-3.03 (4.45)	-2.96 (4.34)	-3.53 (4.90)	-3.66 (5.11)
No Line-Item Veto	-4.31 (.396)	-3.56 (.325)		
Carryover Expected Deficit	-11.1 (2.96)	-11.0 (2.93)	3.57 (.620)	3.79 (.659)
Governor Can Reduce Appropriations Bills	-137. (19.2)	-138. (19.6)		
Political Cycle - Year 2	-3.51 (2.31)	-3.62 (2.37)	-3.68 (2.80)	-3.58 (2.74)
Political Cycle - Year 3	-5.75 (3.56)	-5.85 (3.62)	-2.86 (2.12)	-2.99 (2.22)
Political Cycle - Year 4	-5.64 (3.61)	-5.71 (3.65)	-2.55 (1.94)	-2.48 (1.90)
F-test of Joint Significance of Income Variables	14.0*	14.3*	36.4*	36.3*
F-test of Difference Between ρ_L and ρ_H	.090	.040	.031	.086
R^2	.965	.966	.970	.970

* Significant at the 99-percent confidence level

Notes

a. Absolute value of t-statistics shown in parentheses. All models contain year fixed effects; models (3) and (4) also contain a constant term.

b. All coefficients adjusted for first-order serial autocorrelation in the errors.

Table 4
Spatial Autocorrelation Coefficients Across Models, By Weighting Scheme

4.A Weights Proportional to State Population

	Table 1	Table 2	Table 3	Table 3
Neighbors' Benefits Treated As:	exogenous	endogenous	endogenous	endogenous
Fixed Effects:	none	none	region	state
Weighted Benefit Average:	.047	.018	.024	.008
Lower-Benefit States (ρ_L)	(2.08)	(.930)	(1.08)	(1.12)
Weighted Benefit Average:	.011	.016	.035	.020
Higher-Benefit States (ρ_H)	(.509)	(.659)	(1.64)	(1.46)
F-test of H_o: $\rho_L = \rho_H$	.394	.002	.040	.086

4.B Equal Weights

	Table 1	Table 2	Table 3	Table 3
Neighbors' Benefits Treated As:	exogenous	endogenous	endogenous	endogenous
Fixed Effects:	none	none	region	state
Weighted Benefit Average:	-.096	-.008	.025	.004
Lower-Benefit States (ρ_L)	(2.88)	(.376)	(1.00)	(.529)
Weighted Benefit Average:	.008	.049	.042	.012
Higher-Benefit States (ρ_H)	(.325)	(1.95)	(1.86)	(.825)
F-test of H_o: $\rho_L = \rho_H$	2.76	.815	.090	.031

Table 5

Partial Effect of Changes in 1994 Independent Variables on 1995 Own-State Benefits for Selected States

5.A .Rise in Neighbors' Benefits

	Average 1994 Benefits in Neighbors With		Impact of 5% Rise in Average 1994 Benefits [Implied Elasticity] [1]	
	Lower Benefits	**Higher Benefits**	**without State Effects**	**with State Effects**
State				
Illinois	648.52	723.11	$ 1.16 [.03]	$ 0.68 [.02]
Iowa	624.43	710.96	$ 1.13 [.03]	$ 0.96 [.03]
Wisconsin	702.44	723.89	$ 1.21 [.03]	$ 1.00 [.03]
Nat'l Average	622.04	716.59	$ 1.13 [.03]	$ 0.97 [.03]

5.B Rise in Own-State Per Capita Income

	1994 Income	Impact on 1995 Benefits of a 1% Rise in Real Income [Implied Elasticity] [1]	
		Model with No State or Region Fixed Effects	Model with State Fixed Effects
State			
Illinois	24,368	$ 3.73 [.55]	$ 2.89 [.43]
Iowa	20,915	$ 2.80 [.39]	$ 1.30 [.18]
Wisconsin	21,686	$ 3.11 [.40]	$ 1.65 [.21]
Nat'l Average	21,277	$ 2.95 [.44]	$ 1.46 [.22]

Notes

[1]Figures based on Table 2, col. 2 and Table 3, col. 4

Appendix A.1

Data - Definitions

AFDC + Food Stamps: the maximum monthly combined benefit of AFDC plus Food Stamps for a family of three with no other income, in real (1995) dollars, by state and year.[a] The family is assumed to have no other countable income, but to have the full deductions allowed under the Food Stamp program. See House of Representatives (1994).

Per-Capita Income: the average per-capita personal income, in hundreds of real (1995) dollars, by state and year[a]

Unemployment: the unemployment rate, by state and year[a]

Percent of Blacks: the percentage of Blacks in total state population, by state and year[a]

Percent Age 65 and Over: the percentage of state residents aged 65 and over, by state and year[a]

Democrat Government: a dummy variable equal to 1 if the governor is a Democrat and both houses of the state legislature have Democrat majorities.[a,d]

Divided Government: a dummy variable equal to 1 if the Democratic party controls eitherthe governship or both houses of the legislature while Republicans control the other, or vice versa. (Note that all non-Democrats are counted as Republicans.) [a,d]

Percent Democrat in Lower (Upper) House: percentage of Democrats (including Independents) in the lower (upper) house of the state legislature, by year[e]

Line-Item Veto: a dummy variable equal to 1 if the governor does not have a line-item veto, by state and year[e]

Governor Can Reduce Appropriations Bills: a dummy variable equal to 1 if the governor can reduce items in appropriations bills and governor has a line-item veto, by state and year[e]

State Can Carryover Expected Deficits: a dummy variable equal to 1 if the state may carry over anticipated deficits into the next fiscal year, by state and year[e,f,g]

Per-capita Debt-to-Income Ratio: per-capita total state debt divided by per-capita person income, by state and year[a,e]

Appendix A.1, cont'd

Gubernatorial Cycle: the year of the governor's term; omitted is Year 1, the first year following the election year; by state and year[e]

Southeastern States: Mississippi, Alabama, Georgia, Florida, South Carolina, North Carolina, Virginia, Maryland, West Virginia, Tennessee, and Kentucky.

Sources

[a]Statistical Abstract of the United States
[b]Some data were downloaded from the website of the Administration for Children and Families, Department of Health and Human Services:*www.acf.dhhs.gov*
[c]CAWP Fact Sheet: Women in State Legislatures
[d]Alamanac of the 50 States
[e]The Book of the States
[f]Stanley and Niemi (1992), pp. 325-326
[g]ACIR. Significant Features of Fiscal Federalism, 1992, 1994, 1995 eds.; Bohn and Inman (1996), Table 2
[h]ACIR. Significant Features of Fiscal Federalism, 1994 ed., Table 98

Appendix A.2

Data - Descriptive Statistics[1]

	Without State Fixed Effects		With State Fixed Effects	
	Mean	Std. Dev.	Mean	Std. Dev.
AFDC + Food Stamps ($)	737.55	134.47	0	66.87
Per-Capita Income ($100's)	193.82	29.16	0	20.00
Per-Capita Income^2 x 10^{-3}	38.42	11.96	0	8.25
Per-Capita Income^3 x 10^{-6}	7.79	3.80	0	2.65
Unemployment Rate (%)	6.64	2.11	0	1.66
Democrat Government	.349	.477	0	.342
Divided Government	.343	.475	0	.428
Percent Democrat in Lower House (%)	59.44	18.34	0	43.39
Percent Democrat in Upper House (%)	60.92	19.01	0	5.60
Percent Black (%)	11.10	12.10	0	9.18
Percent Age 65 and Over (%)	11.38	3.36	0	2.98
Debt-to-Income Ratio	.850	.587	0	.193
Line-Item Veto[2]	.149	.356	--	---
Carry Over Expected Deficits	.299	.458	0	.193
Governor May Reduce Bills[2]	.192	.394	--	---
Gubernatorial Cycle - Year 2	.278	.448	0	.434
Gubernatorial Cycle - Year 3	.214	.410	0	.399
Gubernatorial Cycle - Year 4	.247	.431	0	.419
Southeastern State	.234	.424	--	---
Number of Observations	799		799	

Notes

[1]All dollars figures are in real 1995 dollars.

[2]This variable is time-invariant and therefore cannot be used in regressions with fixed state effects.

Appendix B.1

I. Definitions of Low-Population State Groups

	.25 * mean population	.50 * mean population
Northeast:	Delaware	Delaware
	Maine	Maine
	New Hampshire	Michigan*
	Rhode Island	New Hampshire
	Vermont	Rhode Island
		Vermont
Southeast:	(none)	West Virginia
Northwest:	Idaho	Idaho
	Montana	Kansas*
	North Dakota	Montana
	South Dakota	North Dakota
	Wyoming	Oregon*
		South Dakota
		Utah
		Wyoming
Southwest:	Nevada*	Arizona*
	New Mexico*	Arkansas
		Nevada
		New Mexico

* this state does not appear in the group in every year

Appendix B.2, cont'd

II. Definitions of State Migration Groups
(in descending order of total net migration, by category)

Poor-Population Net Losers[1] : New York, Illinois, Texas, New Jersey, California, Louisiana, Connecticut, Wyoming

Poor-Population Net Gainers : Florida, North Carolina, Washington, Arizona, Georgia, Oregon, Tennessee, Wisconsin, Missouri, Alabama

Nonpoor-Population Net Gainers[2] : Virginia, California, Nevada, Maryland

Notes:
[1]Alaska and Hawaii are eliminated because they do not appear in my data.

[2]The remaining six of the top ten are eliminated because they also appear in the Poor-Population Net Gainers group.

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