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ABSTRACT: In this paper we provide new evidence on the importance of the so-called small state advantage for the allocation of the US federal budget. We also provide a new interpretation of the available empirical evidence. Analyzing outlays for the period 1978-2002, we show that not only does the population size of a state matter, but so too does its dynamics. Once population scale and change effects are separated, the impact of population size is substantially reduced, and population change turns out to be an important explanatory variable of current spending patterns. The impact of scale and change effects varies substantially across spending programs. Small states enjoy an advantage in defense spending, whereas fast growing ones are penalized in grants allocations. Our results imply that the interests of the states are not easily aligned around their population size alone. The distortion associated with population dynamics is concentrated on federal grants where formulas play a substantial role in limiting budgetary adjustments. Hence, a large part of the inverse relationship between spending and population appears to be driven by mechanisms of budgetary inertia which are compatible with incrementalist theories of budget allocation.

JEL Codes: D72, H61, H77

Keywords: federal budget, malapportionment, small state advantage, overrepresentation.

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# 1 Introduction

Empirical research on the geographic distribution of US federal spending shows quite convincingly that small states (in population terms) receive disproportionately more dollars per capita. Evidence of small state advantage is usually based on the correlation between federal spending (or some specific spending program) and a linear or non-linear function of state population. The most common explanatory variable used in the literature is senators per capita, since small state advantage is often interpreted as the consequence of Senate overrepresentation. Interpreting the correlation between senators per capita and spending, however, is problematic. In particular, it is not obvious that such correlation represents a causal effect of Senate malapportionment on the allocation of federal spending. This point is very clearly spelled out by Wallis (2001):[1] senators per capita is simply twice the inverse of the state population and the estimated negative relationship between spending per capita and population may be driven by other important factors such as economies of scale,[2] or the fact that several spending programs are directly tied to population levels.[3]

The use of panel data with state fixed effects does not help to solve this problem: in longitudinal data it is difficult to disentangle budgetary lags from changes in over-representation. In other terms, as states grow in population, and therefore fall in terms of representation, they will also lose money per-capita unless the flow of money automatically adjusts to population growth.

These problems can be overcome if an exogenous source of variation in malapportionment could be identified, like in Elis et al. (2009), which uses periodic reapportionments in the House, or in Ansolabehere et. al. (2002) and Ansolabehere and Snyder (2008), which exploit

[1] "*The variable 1/POP represents lots of things. Some, like state flags per capita, have no meaning at all. You, the reader, may interpret 1/POP however you like. But one cannot escape the conclusion that it is a troubled proxy for political influence. (...) If a variable represents two potentially competing hypotheses simultaneously, that variable cannot discriminate between the two hypotheses.*" Wallis (2001), pag. 307.

[2] See for example Alesina and Wacziarg (1998). Wallis (1998), analyzing New Deal spending allocation to the states, finds that economies of scale (for example in the large projects for infractructure building) provide a very plausible explanation for the disproportionately large per capita spending received by small Western states, characterized by a small population dispersed over a large land area.

[3] See for example Hoover and Pecorino (2005) and Levitt and Snyder (1995).

court-ordered reapportionment of state legislatures. Unfortunately, in the case of the Senate, the only determinant of variation in malapportionment is population. Whereas studies that use narrowly defined spending programs can sometimes make a convincing case for the estimation of a malapportionment effect, this is quite difficult for broad spending aggregates. At the same time, studying the allocation of aggregate spending is important if we want to not only show that an effect of malapportionment exists, but to also quantify its overall relevance for the federal budget. This is important because, as pointed out by Larcinese et al. (2006), various and sometimes inevitable distortions introduced by different institutional arrangements may in fact offset each other, leaving a state without a real advantage in the overall budget allocation, even when an advantage can be found in some specific programs.

In this paper we show that, in spite of the difficulties we just mentioned, substantial progress can be made in the estimation of the so-called *small state advantage* in the allocation of large spending aggregates, whether that is due to malapportionment or to other scale effects. By revisiting the estimation methodology used by the existing literature, we provide new results that address this question more directly. First, we show that - while small states enjoy an advantage in the allocation of the federal budget - the estimated advantage is substantially smaller than in previous studies. Second, we find that states with fast growing population loose federal spending to the advantage of slow growing ones. This happens independently of whether they are large or small (in terms of population) and the effect is concentrated on federal grants.

Our estimates, obtained using the standard fixed effect specification for the period 1978-2002,[4] confirm the existence of a strongly positive correlation between senators per capita and total federal outlays. We show, however, that this result is extremely non-robust to specification changes and illustrate a number of rather puzzling findings that cast doubts on the prevalent interpretation of the available evidence. First, we show that the impact of senators per capita vanishes in pure cross-section regressions, i.e. when state fixed effects are omitted. Second, we find that the effect of overrepresentation is particularly strong on aggregates

[4] This represents the longest timespan ever considered in the literature.

such as direct payments to individuals,[5] while we do not find any significant effect on defense spending. This would imply the hardly justifiable claim that direct payments to individuals are somehow more prone to geographic manipulation and targeting than defense spending. Third, if we omit senators per capita from our regressions and analyze the estimated fixed effects (which should then contain the overrepresentation effect) we discover that, after controlling for socio-demographic indicators, larger states often receive more funds than average.

The absence of any effect in pure cross-section regressions may suggest that fixed effects are crucial to correct potential omitted variable bias, and there is certainly no doubt that fixed effects estimates must be preferred in this case. Nevertheless, the inclusion of fixed effects implies that the coefficient of senators per capita is estimated from within-state variation of state population. This point is particularly important because the coefficient of senators per capita is instead used to assess spending differentials between states and, as we will discuss in more detail below, this interpretation of the coefficient conflates two different effects that should instead be kept separate: a scale effect (in each given period states have different population size) and a change effect (in each given state population changes over time). Once population change and scale effects are separated, the small state advantage remains, but is reduced by about one half. Moreover, independently of whether large or small, states that grow faster are penalized in the allocation of the federal budget. According to our estimates, the five fastest growing states lose on average between 1.3% and 5% of their budget during the period 1978-2002. Analyzing different broad spending categories, we also find that the negative effect of population dynamics varies depending on the type of spending. Federal grants are the most affected. Clear evidence of a small state advantage can be found instead in defense spending only.

Hence, our analysis, besides delivering a different assessment on the magnitude of small state advantage, indicates the existence of another important channel through which population affects spending. This resonates with the concerns voiced by several representatives of fast

[5] Direct payment to individuals include mainly entitlement programs such as social security, retirement benefits and health care programs.

growing states on the fairness of budgetary allocations.[6] Even the recent debate surrounding the approval of the stimulus package under the "American Recovery and Reinvestment Act of 2009", suggests that fast growing states are penalized in the allocation of important spending programs.[7]

The factors that can be responsible for this important distortion are numerous and can be traced back to the way the budget allocations are actually determined. First, reallocations of funds are limited by the lack of information available for the drafting of the yearly budget.[8] For example, several programs rely on outdated census data to distribute funds across states.[9] Second, many programs are allocated by formulas that substantially reduce the responsiveness of the budget to population changes. A report issued by the United States Government Accountability Office in 2009 indicated that about 84% of federal aid is allocated through formulas, and that specific rules - such as hold harmless provisions, caps, floors and ceilings - imply that "grant funding may be affected less or entirely unaffected by changes in population" (GAO 2009). Given the nature of the programs involved, the effects of such restrictions are potentially very important. For example, Medicaid - the single largest most important formula grant - is administered under floor and ceiling restrictions (GAO 2009).

Our evidence is consistent with these mechanisms of budgetary inertia highlighted by policy practitioners, and confirms the importance of formulas in the allocation of the budget. In particular, we show that fast growing states are especially penalized in the allocation of formula grants, whereas for non-formula programs the effect of the population dynamics is substantially smaller and has modest statistical significance. In theoretical terms, our results are compatible

[6] Several pieces of legislation introduced in Congress between 1989 and 1993 by the representatives of Florida, Arizona and California point out that the budget allocation based on decennial census data penalizes fast growing states. (Fair share act of 1989, 1992 and 1993. Source: The library of Congress, http://thomas.loc.gov/)

[7] Fast growing states rank at the bottom in the allocation of transportation funds per capita in the stimulus package (*The Wall Street Journal*, Who gets what from the stimulus package, January 27, 2009).

[8] As posited by a voluminous literature of behavioral "incrementalist" theories of budgeting originated with Wildavsky (1964), the limited temporal, financial and cognitive resources available in each year do not allow a rigorous re-examination of the current budget which is then determined by marginal changes to past budgetary allocations.

[9] For an official report see "Federal Formula Programs: outdated population data used to allocate most funds" (GAO 1990).

with behavioral "incrementalist" theories of budgeting (Wildavsky 1964), which claim the current spending to be largely predetermined by past budget allocation.

Our analysis shows that the distinction between population size and growth is important. Some *small* states grow very fast, some *large* states hardly grow. This complicates the negotiations over the budget since the size of the states does not provide a clear line along which coalitions can be formed. Population dynamics represents another important dimension along which the interests of the states may be aligned. Our findings suggest the existence of an important divide between fast and slow growing states, which is at least as important as the divide between small and large states and, for some spending programs, even more relevant. Hence, the procedures that make public spending not sufficiently responsive to population changes are responsible for a substantial part of the distortions that are currently interpreted as a consequence of the size of the states alone.

## 2 Related literature

The literature on small state advantage consists mainly of studies of the consequences of Senate malapportionment. In a purely functionalist view, the double representation principle was devised by the founding fathers of the US constitution in order to balance the interests of the small and big states. The combination of proportional and equal representation, together with the House proposal power on budgetary matters, should grant adequate consideration to the interests of all states, independent of their population size. Ansolabehere et al. (2003) provide a formal model showing how the attribution of proposal power to the lower house may indeed counterbalance the malapportionment in the upper house leading to an equal distribution of per-capita government expenditure.[10]

The functionalist view has been increasingly challenged by recent research. Lee and Oppenheimer (1999) equate Senate apportionment to a "panda's thumb", the residual of a contingent historical situation: "the apportionment of the United States Senate did not result

[10] See Knight (2005) for an empirical investigation of the impact of the proposal power of individual congressional representatives, such as committee members, over spending at the district level.

from the impartial application of any general principle - such as federalism or minority rights - was instead the outcome of a clash between contending political interests within a particular institutional and ideological context".[11] Evidence is now available about various types of distortions generated by the equal representation principle in American politics and policymaking.[12] Some of this literature has focussed on the consequences of malapportionment for the geographic distribution of federal spending, providing support for the idea that small states receive a disproportionate share of the federal budget.[13] The work of Atlas et al. (1995), for example, analyzing biennial data between 1972 and 1990, finds a strongly significant relationship between per capita representation in the US House and Senate and per capita federal spending. These findings are consistent with the results of previous work by Wright (1974) which finds a positive relationship between New Deal spending and electoral votes per capita that - as pointed out by Hoover and Pecorino (2005) - summarizes per capita representation in the House and the Senate. Hoover and Pecorino (2005), considering a different time period (1983-1999) and a broad range of spending aggregates, find that states' representation in the Senate is positively related with total per capita outlays as well as with procurement, grants, wages and pensions.[14] On the other hand, Levitt and Snyder (1995) find that districts from more populous states receive in fact more (rather than less) federal spending.

Another strand in the literature has focused on more specific spending aggregates where the impact of the Senate can be more precisely identified. Lee (1998), using Bickers and Stein (1991) data on domestic outlays from 1983 to 1990, finds evidence of overspending in

[11] Lee and Oppenheimer (1999), p. 27. For a critical view of Senate representation in the US constitution see also Dahl (2002).

[12] Lee and Oppenheimer (1999) consider, among other variables, the number and quality of contacts between Senators and constituents, Senators' fund-rasing efforts and strategies, the competitiveness of the electoral race, the allocation of federal spending. They also find a counter-majoritarian tendency to favor the minority party (in popular vote terms) making it the majority party in Senate. Racial representation has also been shown to be substantially biased against African-Americans and Hispanics (Griffin (2006); Malhotra and Raso (2007)).

[13] The actual process through which Senate overrepresentation could generate a bias in federal budget allocation might be related to congressional bargaining. Since less funds are necessary to obtain the same increase in percapita expenditure in a smaller than in a larger state, senators who need to build winning coalitions to bring federal spending to their constituents will typically ask smaller states to enter the coalition to minimize the cost of buying political allies. Various arguments grounded on this basic premise can be found in Lee (1998), Knight (2004), Knight (2008) and Dragu and Rodden (2010).

[14] They, however, find a negative impact of House representation.

small states for non-discretionary distributive programmes that are allocated via formulas determined by the Congress. Lee (2000) finds that final allocations from the 1991 and 1997-98 reauthorizations of the federal surface transportation programme closely reflect small-state senators' preferences, whereas analyzing surface transportation authorizations between 1956 and 1998, Lee (2004) shows that formulas passed by the Senate are more favorable to small states. Knight (2004) does not find strong effects of Senate overrepresentation on aggregate spending, although he does on earmarked projects: the effect is particularly strong if the earmark comes from the Senate. Hauk and Wacziarg (2007), using the authorizations from the 2005 Highway Bill, confirm the existence of an overrepresentation effect on transportation earmarks. At the district level, Ansolabehere et al. (2002) analyze the effect of unequal representation prior to 1960 and the equalizing impact on state transfers to counties following the court-ordered redistricting in the 1960s.[15]

The evidence provided by existing studies rises some fundamental questions on US bicameralism. According to the estimates of Atlas et al. (1995), the difference in real total spending due to malapportionment between the most overrepresented (Wyoming) and the most underrepresented (California) states amounts in 1990 to $1148 (in current dollars) per capita, which is equivalent to approximately one third of federal spending in Wyoming that year. They estimate that California would gain an additional $25 billion of federal spending if their number of senators were proportional to the state population size. The estimated coefficients of senators per capita from other empirical studies point to similar magnitudes (Fleck (2001); Hoover and Pecorino (2005); Larcinese et al. (2006)).[16] Is small Wyoming really so much more

[15] There is some literature on the consequences of overrepresentation outside of the US context. Rodden (2002) provides evidence on the impact of the overrepresentation of small countries in the EU. He finds that agricultural and regional development transfers as well as total net transfers are disproportionately allocated to small EU member states. See also Aksoy and Rodden (2009) for results on new EU member states. Evidence from Japan is provided in Yusaku and Saito (2003), Shigeo (2006) and Shigeo and Ting (2008). Pitlik, Schneider, and Strotman (2006) provide evidence from Germany.

[16] The magnitudes reported by Lee and Oppenheimer (1999) are substantially smaller. They use seven years of data and a representation index with little within-state variation, which therefore does not allow the inclusion of state fixed effects in the regressions. As we will see, including state fixed effects makes a substantial difference both in terms of the magnitude and significance of the estimated coefficients. Moreover, they focus on programmes that represent an overall 56% of the federal budget, hence the final magnitudes are necessarily smaller than those obtained by using total federal spending.

powerful than California as current empirical investigations seem to suggest? More generally, do small states enjoy such a disproportionate leverage in the allocation of the federal budget? In the remainder of the paper we will address this important question.

## 3 Some puzzling results

Population size varies considerably across US states and so does per capita Senate representation. Table 1 reports an index of average Senate overrepresentation by state during the period 1978-2002.[17] Under or overrepresentation is determined by comparison with a fair representation given by the ratio between the total members of the Senate and the total US population in a given year.[18] States are ordered by average population in the period 1978-2002 (starting with the smallest) and obviously smaller states are overrepresented in the Senate. Table 1 also reports average federal spending per capita by state in the period considered, showing that there is no clear pattern linking Senate over-representation and spending. This can be seen graphically in Figure 1, where the states are ordered along the horizontal axis according to their average population in the period considered, while on the vertical axis we report average per capita outlays. Figure 2 provides yet another visual representation of the spending-overrepresentation link. Although it is apparent that Midwest states tend to be, on average, both overrepresented and better funded, looking at the entire US map it becomes clear that this is far from being a general statement.

A well established procedure to assess the impact of Senate representation on the geographic allocation of the federal budget amounts to estimating the following equation:

$$\begin{aligned} y_{st} &= \alpha y_{st-1} + \beta * SP_{st} + \lambda N_{st} + \boldsymbol{\theta}\mathbf{Z}_{st} + \gamma_s + \delta_t + \epsilon_{st}, \qquad (1) \\ s &= 1, ...48; \quad t = 1978, ...2002; \end{aligned}$$

[17] Like most of literature on the allocation of US federal spending, we focus on the 48 contiguous states.

[18] More specifically, define $N_{st}$ as the population of state $s$ in year $t$ and $USpop_t$ as the total US population (in the 48 states considered) in year $t$. Then the overrepresentation index in year $t$ is given by $\frac{2}{N_{st}}/\frac{96}{USpop_t} = \frac{USpop_t}{48*N_{st}}$. This index is substantially equivalent to that reported in Tab. 6.1 by Lee and Oppenheimer (1999), p.162.

where $y_{st}$ is real per-capita federal expenditure (outlays) in state $s$ at time $t$, $y_{st-1}$ is its lag, capturing the incremental nature of the budget,[19] $SP$ stands for *senators per capita,* measuring Senate representation of the states, $N_{st}$ is population, $\mathbf{Z}_{st}$ is a vector of socioeconomic control variables, and $\gamma_s$ and $\delta_t$ represent respectively state and year fixed effects.[20]

To interpret the coefficients of equation equation (1), two remarks are in order. First, the inclusion of a lagged dependent variable implies that the impact of the independent variables on spending is not transmitted in a single time period, but over a period of subsequent years. The coefficients of the regressors in equation (1) are short run multipliers, i.e. they capture the impact in a single time period. It is then possible to compute long run multipliers, that capture the cumulative effects of the regressors over the years. This is done by dividing each short run multiplier by 1 minus the lag of the endogenous variable.[21] Hence, for example, the long run coefficients for $SP$ is given by $\frac{\beta}{(1-\alpha)}$, and for $N_{st}$ is $\frac{\lambda}{(1-\alpha)}$. Second, since we adopt a functional form that includes both $SP$ (a non-linear population term) and a linear population term, the marginal effect of population ($N_{st}$) on real per capita spending ($y_{st}$) for the short run is given by

$$\left[\frac{\partial y_{st}}{\partial N_{st}}\right]_{SR} = -\left(\frac{2\beta}{N_{st}^2} - \lambda\right) \tag{2}$$

The corresponding long run coefficient is

$$\left[\frac{\partial y_{st}}{\partial N_{st}}\right]_{LR} = -\left(\frac{2\beta}{N_{st}^2(1-\alpha)} - \frac{\lambda}{(1-\alpha)}\right) \tag{3}$$

This implies that the scale effect is non-linear and this must be taken into account while computing the size and significance of the population's coefficient. Hence, whenever both $SP$ and a direct population term are included we also report the overall marginal effect of

[19] For a discussion of this point see Lee and Oppenheimer (1999), p. 172.

[20] Including both a lagged dependent variable and state fixed effects introduces a bias in the estimated coefficients Nickell (1981). This bias is declining in T (see Greene (2003), p. 307) and Monte Carlo simulations tend to show that, for $T > 20$, while the bias in $\alpha$ may remain sizeable, the bias in the other coefficients becomes very small (Kiviet (1995), Judson and Owen (1999)). Moreover, the alternative IV estimates (see for example Arellano and Bond (1991)) tend to be generally less efficient. The time dimension in most of our regressions is equal to 25 and it is never inferior to 20, hence our choice of estimating equation (1) by OLS.

[21] The formal derivation of the long run multipliers is reported in Appendix.

population evaluated at the average population value in our sample (both the short-run and long-run coefficients).

We start by estimating equation (1) using Census data for the US States during period 1978-2002.[22] Summary statistics are reported in Table 2 and estimates in Table 3. We start with a simple regression of real federal spending (outlays) per capita on senators per capita and then progressively include lagged spending, population, year dummies, socioeconomic control variables and, finally, state fixed effects. Only the introduction of fixed effects renders statistically significant the estimated coefficient $\widehat{\beta}$.[23] The population effect at the mean is instead statistically significant when we introduce year fixed effects (column 4) and remains so in the short run if socioeconomic control variables are introduced (column 5). In any event, when we include state fixed effects both the size and the magnitude of the overall impact of population are much larger. The short run coefficient is around sixty times larger, the long run four times.[24] This result is not driven by the inclusion of a lagged dependent variable. In column (7) we remove $y_{st-1}$ and the result remains quite similar (with an overall coefficient of population which is precisely half-way between the short and the long run coefficients of column (6)).

Given the importance of including state fixed effects, it is clear that the estimated impact of malapportionment relies predominantly on the variation of $SP$ within states over time, with a more limited role being played by between variation, despite the large differences in state per capita representation. Although these results suggest the existence of a potentially important omitted variable bias in cross-section regressions (which is corrected by the introduction of fixed effects), they should be interpreted with caution, since within state variation of population can have a direct negative effect on spending independently of overrepresentation.

In Tables 4a and 4b we use as dependent variables the spending aggregates (outlays)

[22] Census data for most spending categories are available starting from 1978, the exceptions being grants (available from 1977) and salaries (available only from 1982 onwards).

[23] Similar results can be obtained from yearly cross-section regressions or by using the between estimator. These estimates are not reported but are available from the authors upon request.

[24] These results are consistent with the findings of Lee (1998), Oppenheimer and Lee (1999) and Knight (2004), who also find a modest impact (at least if compared with studies that use fixed effects estimates) of overrepresentation in cross-section regressions.

available from the Statistical Abstract of the United States.[25] We report specifications both with (Table 4a) and without (Table 4b) fixed effects (but always including year dummies and socioeconomic control variables). Once again, introducing the state fixed effects makes a big difference for the sign and significance of the $SP$ coefficient. In the specification without fixed effects, only for grants the coefficient of senators per capita comes with the expected positive and significant sign. In all other cases, the coefficient is either insignificant, as in the case of direct payments to individuals and salaries, or it is statistically significant but has the "wrong" negative sign, as in the case of defense spending. In any event, if we consider the overall impact of population on spending, the short-run coefficient of direct payment is the only one to be significant.

When state fixed effects are introduced (Table 4b), the impact of senators per capita becomes positive in all the equations and it is statistically significant in the case of direct payments to individuals, salaries and grants. In this last case, the coefficient has almost been doubled by the introduction of state fixed effects. The coefficient of senators per capita is instead insignificant when we consider defense.[26] The overall negative impact of population is strong and statistically significant for grants and salaries (both in the short and long run), and for direct payments to individuals (short run only). The impact of population is never significant for defense. This, however, is a spending aggregate that is at least as likely to be subject to geographic manipulation as direct payments, salaries, and grants.

Finally, we estimate equation (1) without the $SP$ indicator. In this case we expect the effect of malapportionment to be incorporated in the state fixed effects. Figure 3 plots the estimated fixed effects versus the average state population (in the period considered).[27] The

[25] The statistical abstract reports yearly *outlays* at state level by program (direct payments to individuals, salaries and grants) and by agency (defense and non defense). Procurement spending (for which large amounts of funds are appropriated to be spent over the course of many years ) is instead not recorded on an outlay basis. Therefore, a note of caution applies to defense spending for which it is not possible to isolate the pure outlays components from the long term investments (often decided far back in time) that display very limited yearly variation.

[26] Our results are different from Atlas et al. (1995) who find a significant impact of senators per capita on defense. If we run our regression only for the period 1978-1990, we also find a significant effect. However, the significance disappears in the larger sample.

[27] Using average population is a meaningful exercise since the ranking of the various states in population

picture is rather different from what one would expect if population size had any effect. When looking at total federal spending, and after controlling for socioeconomic indicators, larger states appear to receive more funds than smaller ones. Virginia and Maryland, because of their proximity to DC, and New Mexico, because of large defense infrastructure, represent the only exceptions. The advantage of large states is very clear for entitlements (with North Dakota being the sole exception), while no clear pattern can be found for other spending aggregates.

Overall, these results provide a rather puzzling picture which - in light also of the large magnitude of the estimated effects in specifications including fixed effects - cast doubts about what exactly is estimated by using $SP$ as an explanatory variable. Since the number of senators is fixed and equal to 2 for all states, the variable $SP$ in equation (1) is simply a constant divided by the population. In other words, $SP$ varies only because population varies. Interpreting the coefficient of $SP$ as the impact of malapportionment is not an obvious step. How much of the inverse relationship between $SP$ and federal spending is due to malapportionment remains moot.[28]

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terms is relatively stable over the period considered.

[28] To make this point clearer it can be useful to rewrite the basic equation (1) making explicit how it depends on the population term. Omitting for simplicity the error term, the time dummies and the lags, equation (1) can be written as: $\frac{Y_{st}}{N_{st}} = \beta * \frac{2}{N_{st}} + \lambda N_{st} + \boldsymbol{\theta} \frac{\mathbf{z}_{st}}{N_{st}} + \gamma_s$. Where $Y_{st}$ is total federal spending in state $s$ at time $t$, $N_{st}$ is total population, $\mathbf{z}_{st}$ is a vector of control variables expressed in total per state (instead of per capita) levels. The overrepresentation indicator is given by $\frac{2}{N_{st}}$. The above equation, with or without fixed effects, cannot identify the impact of overrepresentation on spending per capita from that of any other effect induced by population variation. In fact, if we multiply both sides of the equation by $N_{st}$, we obtain: $Y_{st} = 2\beta + \lambda N_{st}^2 + \boldsymbol{\theta} z_{st} + \gamma_s N_{st}$. In this equation, the effect of overrepresentation on *total spending* ( $Y_{st}$) is captured by the constant term ($2\beta$). Hence, any factor that induces a positive constant term in the total spending regression would be interpreted as overrepresentation in per capita spending equation. The factors that can possibly be captured by the constant term are very numerous and it is not obvious how to infer whether overrepresentation is the most important of them.

# 4 Small state advantage, population dynamics and federal budget allocation

Having established that the impact of malapportionment cannot be identified by estimating equation (1), even when fixed effects are included, we now turn to a more general question about *small state advantage*. Admitting that we cannot identify the impact of malapportionment does not imply that no progress can be made to establish whether small states indeed receive more federal monies and, in case of an affirmative answer, why. This leads us to another identification problem. Population variation across states may induce variation in per capita federal spending via two main channels. First, states may receive different amounts of spending because they differ in their population sizes (*scale effect*). Second, independently of their size, their spending allocation can vary because of pure population dynamics (*change effect*).

Differences in spending per capita due to the *scale effect* may arise because states are differently represented in the Senate, but also as a consequence of the possible economies of scale in the provision of goods and services in the most densely populated states. Isolating an overall scale effect is important because it would give us an upper bound of the impact of malapportionment on spending. The problem, however, is that an inverse relationship between spending per capita and population can also be observed whenever, because of inertia, yearly changes in per capita spending do not exactly reflect yearly changes in population. In this case, fast growing states, independently of their size, could see a decline of per capita spending because budgetary provisions do not adequately respond to population trends.

When using panel data the scale effect and the change effect - if nothing is done to isolate them - are conflated into one single coefficient. Given the puzzling results reported in the previous section, we have good reasons to think that at least some of the estimated population effect is due to population dynamics rather than to the different population size of the states.

## 4.1 Population dynamics and budgetary inertia

The US states are remarkably different in their population dynamics. During the period we consider (1978-2002), for example, the population of Nevada tripled, while that of Florida and Arizona doubled. At the same time, in states like West Virginia, North Dakota, Iowa or Pennsylvania the population in 2002 is either slightly below or just slightly above the level of 1978.

States with a fast growing population may be disadvantaged in the distribution of federal funds since several factors contribute to generate inertia in the allocation of the federal budget. First, as pointed out by incrementalist theories (Wildavsky (1964) ; Davis et al. (1966); Dempster and Wildavsky (1979)), the complexity of the budget implies that new provisions are determined mainly by marginal changes to previous ones. Second, formulas play an important role in explaining budgetary inertia. For several programs, *hold-harmless provisions* guarantee that the funds allocated to a state will be no less than a specified proportion of a previous year's funding.[29] If a population change results in a decrease in funding below a designated amount, the hold harmless provision would raise the amount to designated one. At the same time, the amount of the increase would be deducted from the funding of other states not affected by the hold-harmless provision. In an analogous way, *caps* impose a limit on the size of an annual increase as a proportion of a previous year's funding so that, if a population change produces an increase in funding above a certain amount, the cap would limit its effect. *Floors* and *ceilings* operate in a slightly different way, but have similar implications: if a change in population reduces funding below the floor, a state would be guaranteed the amount specified by the floor, whereas if the allocation exceeds the ceiling, the state cannot receive more than the ceiling amount.[30] Finally, the use of outdated population data in formulas penalizes states whose population grows fast.[31] As we will see, the budgetary inertia introduced by

[29] For example, a 100% hold-harmless provision is currently in place for the Title I education program and the WIC (Women, Infant and Children). For a detailed report on formula programs see CNSTAT (2003).

[30] For example, the Title I education program state expenditure per pupil is restricted to a range between 80% and 120% of the national average per pupil expenditure. In the special education program no children may receive more than 40% of the average per pupil expenditure in US public elementary and secondary school. Other important programs subject to limits are the Federal Highway Program and Medicaid.

[31] In a testimony (26 February, 2008) to Congress concerning State Children's Health Insurance program

these mechanisms can have important consequences for the allocation of federal money.

A simple graphical analysis can illustrate quite effectively the relationship between spending per capita and state population. We construct two indices that capture for each state the evolution over time of their respective spending and population shares (of the US total).[32] An index equal to 0 means that the state share of US total spending (population) is the same as in 1978, i.e. that the state spending (population) is increasing at the same pace as the US average. An index above 0 means that the state spending (population) grows above the US average and therefore has a higher share of the US total compared to 1978, with 100 indicating that such share has doubled. Negative values indicate instead decreasing shares.

The evolution of these two indices over time, reported in Figures 4a and 4b, shows a remarkable degree of divergence: an above average increase in population is almost always mirrored by a below average increase in federal spending per capita. For example, California and Texas are two underrepresented states with fast growing populations and correspondingly decreasing federal spending per capita. Pennsylvania and Ohio are also heavily underrepresented, but with a decreasing population: they display an increase in the federal spending index, i.e. an above average growth in spending per capita. Similar patterns can be seen among overrepresented states. In Wyoming the population was growing rapidly until the mid-eighties and its share of spending per capita was decreasing correspondingly. Once, however, the population growth decelerates compared to the national average, its share of spending per capita starts increasing. Utah has an increasing population share and a decreasing spending share, whereas the opposite holds in West Virginia. In Nevada - an overrepresented state with the

(SCHIP), the governor of Georgia Sonny Perdue states that "The current funding formula is also flawed because it hurts fast growing states, like Georgia, by lagging behind by as much as four years in factoring in quickly changing population numbers. In our 2007 fiscal year, the federal government was using population numbers from 2004, 2003 and as far back as 2002. Georgia has grown by almost a million peoples since 2002. We need data that is reflective of the actual population and need." (source: http:\\gov.georgia.gov accessed on April 20 2008).

[32]For spending we construct a size invariant index by dividing the state per capita spending in each year by its value in 1978 (and multiplying the result by 100). We also construct an analogous index for the overall spending in the United States. The difference between the state spending index and its corresponding US index will then describe the relative change of spending in a state compared to the US average. We then construct an analogous index for the population of each state by subtracting from our previously computed scale independent index of population its corresponding US index.

fastest growing population in the US - the spending index is always below its 1978 level and continuously decreasing.

The next section confirms the basic intuitions provided by this simple graphic by using regression analysis.

## 4.2 Estimating scale and change effects

To separate the effect due to change from the effect due to scale we construct a *scale independent* index of population change ($POPIND$) that we will then introduce in our baseline regression specification. This index is constructed by dividing the population of every year by the population of the base year (1978). Hence, in 1978 the index ($POPIND$) is equal to 100 for all states, and in all the other years the index measures the deviation of the state population from the same base year. The pattern of $POPIND$ for all states during the entire period is summarized in Figure 5. As we can see, states display very distinct patterns. Moreover, large, medium or small states can be equally found among the fastest growing as well as the slowest growing states. For example, among the three fastest growing states, we have Nevada with an average 1978-2002 population of 1.2 million, Arizona with 3.7 million and Florida with 12.7 million. Similarly, among slow-growing states we have New York with an average population of 18 million, as well as Connecticut with 3.2 million and North Dakota with 0.6 million.

Figure 6 provides a map representing average $POPIND$ by state in the period 1978-2002. It should be compared with Figure 2, which reports the corresponding map for population and federal spending per capita. As we can see, the least populated states in the North-East seem to be advantaged in the allocation of federal spending if compared to populous states such as California, Texas and Florida. For these states we also have an inverse relationship between federal spending and $POPIND$. These states conform to an important claim made by Lee (1998) and Lee and Oppenheimer (1999), that the large states are also those that grow faster and vice-versa: hence the small (and slow-growing) states often secure more funds by negotiating formulas that guarantee minimum allocations.

It is certainly true that, if one takes a very long term perspective on this matter, then the fast growing states will also tend to be larger, and states that do no grow will shrink in relative terms. The differences in size between states, however, are so large that it would probably take many decades if not centuries to reach a good alignment between size and growth. In fact, over the period we consider (twenty five years), there is almost no switch in the ranking by size, despite the very marked differences in population growth. Some small states - like Nevada and Utah - experience a very rapid population growth, whereas some large states like New York and Philadephia grow very little. This implies that when formulas are negotiated, the interests of the states are not easily aligned along the population size dimension and, in fact, if we look at the average spending distribution, states like Nevada and Utah seem to be disadvantaged if compared to states like New York and Pennsylvania no less than if compared to the small and static states of the North-East. If scale and change effects went exactly in the same direction for all or most states, it would be hard to separate the two. We can separately estimate the scale and change effects precisely because this is not the case.

We can use $POPIND$ to purge our scale coefficients of any effect due purely to population change and therefore identify the scale effect (which is an upper bound of over-representation). Returning to equation (1), the new specification becomes:

$$\begin{aligned} y_{st} &= \alpha y_{st-1} + \beta * SP_{st} + \lambda N_{st} + \psi POPIND_{st} + \boldsymbol{\theta}\mathbf{Z}_{st} + \gamma_s + \delta_t + \epsilon_{st}, \quad (4) \\ s &= 1, ...48; \quad t = 1978, ...2002; \end{aligned}$$

The results reported in column 1 of Table 5 show that the scale-independent measure of population change is key to explain federal budget allocation to the states.[33] The coefficient of $POPIND$ is negative and significant, implying that fast growing states are penalized in the allocation of the federal budget, independently of their population size.[34] On the other

[33] An alternative estimation strategy consists of introducing state specific trends, $t_s$, in our basic specification. The results obtained with this alternative specification mirror quite well those obtained with $POPIND$ but have the disadvantage of not making explicit the source of the trends (results are available from the authors upon request).

[34] As a further robustness check, we also introduced an interaction term between $SP$ and $POPIND$. This

hand, once we control for the scale independent population change, the coefficient of senators per capita remains significant, but its magnitude is reduced to about one half of the value estimated in column (6) of Table 3. The same is true for the overall scale effect, evaluated at the average population level, whose size is halved by the introduction of $POPIND$, both in the short and long run.

This analysis leads us to the following conclusions. First, states whose population grows faster are penalized in the budget allocation independently of whether they are large (and hence underrepresented in the Senate) or small (and hence overrepresented): this suggests that the budget fails to respond to population changes at an adequate pace. Second, the coefficient of $SP$ - as well as of the overall scale effect - is reduced by half when change and scale effects are separated. Conflating these two coefficients leads to a serious overestimation of the scale effect and, therefore, of the upper bound of the potential impact of overrepresentation. It is, however, important to stress that our analysis confirms the presence of a pure small state advantage (scale effect) in the allocation of total federal spending.

Finally, the impact of $POPIND$ on spending is of a realistic magnitude. For example, the estimates of Table 5 (column 1) imply that, if in 1990 California had the same $POPIND$ of Wyoming (106.7) then, everything else being equal, California would have received \$57.75 per capita more than what predicted by using its actual $POPIND$ (134.2). This represents less than 2% of the actual California's per capita spending in 1990. In Table 6 we report the average gains and losses (in 1983 USD) implied by our estimates of the change effect reported in column (1) of Table 5. These have been computed by comparing, for each state, the predicted federal spending per capita implied by the average $POPIND$ in the state during the period 1978-2002, with the federal spending per capita that the state would have received if its $POPIND$ was equal to the US average during the same period. The most penalized state, Nevada, is obviously the fastest growing state. Its average per capita loss per year is around 166 USD, or about 5% of its average budget. Such gains and losses do not appear to

term should capture the possibility that small and large states have different bargaining power when different population growth rates induce the renegotiation of budgetary allocations. This interaction term turns out to be statistically insignificant, while the results for $POPIND$ and $SP$ remain robust both in terms of sign and significance of their coefficients.

be related to the population size of the states.

# 5 Scale and change effects in different spending categories: further evidence

Population change and scale effects should play a different role in different spending programs. For some spending categories, such as defense, there is no reason to expect population dynamics to play any particular role, whereas scale effects might actually be quite important. For formula programs, like many types of grants – where population is an important input – fast growing states might be severely penalized by formulas that impose restrictions on yearly funding changes, as well as by the use of outdated population data. This would not rule out possible scale effects either due to economies of scale or to political pressures, since formulas can incorporate economies of scale and are, to a certain extent, manipulable too. The same can be said of public spending in salaries since public services and personnel may not grow at the same pace as the overall population growth and, at the same time, a small state advantage in this type of spending cannot be ruled out. On the other hand, there are no immediate reasons for direct payments to individuals to display any sort of small state advantage. In fact, as pointed out in Section 2, the negative and significant coefficient found for direct payments to individuals using the standard specification (1) is particularly puzzling given the entitlement nature of the programs involved. Equally surprising is the absence of a significant effect on defense spending. When we add $POPIND$ to the basic specification we obtain very different results delivering a more plausible assessment of the advantage enjoyed by small states.

The estimated coefficients, reported in columns 2-4 of Table 5, show that for *grants*, *direct payments* to individuals and *salaries*, introducing $POPIND$ renders the coefficient of $SP$ statistically insignificant (compare columns 2-3-4 of Table 5 with columns 6-7-8 in Table 4b), whereas the coefficient of the linear population term is now negative and significant for salaries only. Most importantly, the overall *scale effect* does not display a significant coefficient neither in the short run nor in the long run in any of the specifications reported in column 2-4 of

Table 5.

On the other hand, for *defense* spending (Table 5, column 5), we find an overall negative and statistically significant scale effect, which becomes substantially larger and more significant in the long run. This result, which refers to an overall scale effect and cannot therefore unambiguously be identified as malapportionment, is nevertheless at least consistent with the idea that defense spending is prone to some manipulation in geographic terms. $POPIND$ has a negative impact on direct payments to individuals, grants and salaries, but the statistical significance is above the 10% threshold for *grants* only. On the other hand, as one would expect, population dynamics plays no significant role in the defense equation. Finally, column 6 shows that the scale effect found on total federal spending (column 1) is mostly due to defense. When we regress all non-defense spending on our explanatory variables, the scale effect loses its statistical significance both in the short and in the long run. The impact of $POPIND$ becomes stronger instead both in magnitude and significance.

Since formulas may play a crucial role in limiting the response of the budget to population changes, we conduct a further check using data on grants that allow us to distinguish between formula and non-formula programs. To this end, we have used the information provided by the Catalogue of Federal Domestic Assistance (CFDA) to identify the programs that are allocated by formula. Formula grants are defined in the CFDA as "allocations of money to States or their subdivisions in accordance with distribution formulas prescribed by law or administrative regulation, for activities of a continuing nature not confined to a specific project". Both formula and non-formula programs in the CFDA are identified by the same codes used in the Consolidated Federal Fund Report (CFFR), which contains data on federal grants allocation to the states on an obligation base, starting from 1983. Hence, by matching the information from the CFDA with the spending data from the CFFR, we have classified federal aid into two categories, formula and non-formula grants. With the exception of Wyoming - which receives on average (during the entire period) roughly equal amounts of formula and non-formula grants - the amount of funds allocated by formula is on average always larger than the non-formula for all states. In the period we analyze, slightly over 67% of federal aid is allocated via formulas.[35] This is not surprising given that formula programs include several large important items such as Medicaid, Title I education grants to local authorities, Highway planning and construction, and Community development block grants. On the other hand, non-formula grants consist mainly of project grants which provide funding for specific projects (such as fellowships, scholarships, research grants, training grants, planning and construction grants) for fixed or known periods.

In columns 1 and 2 of Table 7 we verify that the results obtained by using CFFR data (available from 1983) are very similar to those previously obtained by using data from the Statistical Abstract. We then compare formula and non-formula grants starting from the standard specification without $POPIND$. From columns 3 and 4 it is clear that a small state advantage only appears for formula grants. The short-run marginal effect of population in the case of formula grants is almost seven times larger than that of non-formula grants. The long-run marginal effect is ten times larger. These coefficients are statistically significant at a 5% level for formula grants and very far from statical significance for non-formula grants. Column 5 shows that these results are not robust to introducing $POPIND$. In other words, the small state advantage that seems to characterize formula grants can be attributed in large part to population dynamics, as confirmed by the strong statistical significance of $POPIND$ in the formula grant regression. $POPIND$ is instead only weakly statistically significant (10% level) for non-formula programs and displays a substantially smaller coefficient.

It remains quite possible that a small state advantage is present for some specific programs within our broadly defined spending categories, in particular for grants. As discussed in the Introduction, several studies make a convincing case in that direction. However, not finding a strong effect on the large aggregates implies that the overall magnitude of this effect is confined to some particular or small program that it is compensated by countervailing forces in other programs.

Our conclusion from this analysis of broad spending aggregates is in line with our previous findings: separating scale and change effects is important, since population dynamics matters

[35] Lousiana has the highest average share with 76% and only Colorado, Massachusetts, Maryland and Wyoming have less than 60%. Detailed tables can be provided by the authors upon request.

for the allocation of federal spending. Population change and scale play different roles in different spending aggregates. Population dynamics is an important predictor of spending differentials across states when we consider grants. On the other hand, small states enjoy a substantial advantage in the allocation of defense spending.

# 6 Conclusions

In this paper we have reconsidered the small state advantage hypothesis by analyzing data on the allocation of the US federal budget over the period 1978-2002. We have focused in particular on the limits of the standard econometric specification and on the interpretation of its coefficients to reach the conclusion that, while small states enjoy an advantage in the allocation of the budget, a substantial advantage is also provided by having a slow population dynamics. Hence, the size of the states does not uniquely define a dividing line between their interests. When population dynamics is taken into account, small but fast growing states may end up on the same side of large and fast growing ones. The same is true for large and small, but slow growing states alike. In short, population dynamics is an important predictor of federal budget allocations: *small* but fast growing states lose funds to *large* but slow growing ones.

A small state advantage may occur because of the economies of scale associated with some public programmes. In this case it should not raise much concern since spending differentials would serve the purpose of equalizing welfare across states. A less benign interpretation, however, is that a small state advantage may occur because of differentiated representation in the policy making process, particularly through Senate malapportionment. The standard measure of Senate overrepresentation is the number of senators per capita. This indicator, however, is perfectly correlated with the state population and therefore does not allow to separate the impact of overrepresentation from that of any other variable that might happen to be correlated with the population size of a state. Moreover the use of senators per capita in spending regressions that use longitudinal data and state fixed-effects do not isolate the role of small state advantage (scale effects like malapportionment or economies of scale) from that of population growth (change effects for a given population size). When we include a pure "population change" variable in our estimations, we find that the population scale effect is reduced by half and is mainly driven by defense spending. Our conclusion is that the impact of small state advantage on large spending programs has been substantially overestimated and that we need an alternative (or, at least, a complementary) explanation for the rather puzzling evidence accumulated by the abundant empirical literature on this issue.

Our analysis reveals that, once we disintagle scale and change effects, fast growing states are disadvantaged in the allocation of the federal budget independently of their population size. This may in part be due to the difficulties of collecting and processing all the information necessary to guarantee to every state a fair share of the budget. However, even when such information is available, budgetary rules and formulas, whose determination is not isolated from the political process, can prevent fair reallocations of the budget. The recent reform of Title I education programs provides an instructive example. To meet the increased education needs of fast growing states, decennial Census data on population have been replaced by biennial Census estimates. At the same time, senators of shrinking and slow growing states have managed to obtain the implementation of a 100% "hold harmless provision" that, in the absence of any significant increase in annual appropriations, has *de facto* neutralized the use of updated data, preventing the reallocation of funds toward more needy states. This shows how Congressmen are actively engaged in bargaining over the federal budget allocation to bring bacon home, and how rapid shifts in population can create an important divide between the interests of fast growing as opposed to shrinking or slow growing states. The redistributive effects associated with large population shifts open an important avenue for future research. Understanding how budgetary provisions for specific items are negotiated within Congress when large population changes occur, and whether they are affected by institutional and political features, such as committee representation, party politics and electoral considerations, are very fundamental questions that we leave for future investigation.

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# Appendix

**Short and long run multipliers**

Consider equation (1) in Section 3:

$$y_{st} = \alpha y_{st-1} + \beta * SP_{st} + \lambda N_{st} + \boldsymbol{\theta}\mathbf{Z}_{st} + \gamma_s + \delta_t + \epsilon_{st} \tag{1}$$

This specification implies that the impact of the independent variables on spending is not transmitted in a single time period, but over a number of subsequent years. This assumption is captured in the literature by using the so-called distributed lag analysis (Koyck (1954); Jorgenson (1966)). We use a very common lag structure, known as Koyck (1954) transformation, which assumes that the regression coefficients decline geometrically over time. This means that $(1-\alpha)$ estimates the decline rate of the impact of the independent variables. The coefficients of the regressors in equation (1) are short run multipliers, i.e. they capture the impact in a single time period. The long run multipliers can then obtained by dividing each short run multiplier by 1 minus the lag of the endogenous variable (Pindyck and Rubinfeld (1981), p. 232). In other terms, equation (1) can be written as:

$$y_{st} - \alpha y_{st-1} = \beta * SP_{st} + \lambda N_{st} + \boldsymbol{\theta}\mathbf{Z}_{st} + \gamma_s + \delta_t + \epsilon_{st}. \tag{1.1}$$

On the RHS we have the short-run coefficients. If $\alpha < 1$, then (**??**) converges to steady state and therefore we can write:

$$(1-\alpha) y_{st} = \beta * SP_{st} + \lambda N_{st} + \boldsymbol{\theta}\mathbf{Z}_{st} + \gamma_s + \delta_t + \epsilon_{st}. \tag{1.2}$$

Hence, the long run equation can be written as:

$$y_{st} = \frac{\beta}{(1-\alpha)} * SP_{st} + \frac{\lambda}{(1-\alpha)} N_{st} + \frac{\boldsymbol{\theta}}{(1-\alpha)}\mathbf{Z}_{st} + \frac{\gamma_s}{(1-\alpha)} + \frac{\delta_t}{(1-\alpha)} + \eta_{st} \tag{1.3}$$

where $\frac{\beta}{(1-\alpha)}$ and $\frac{\lambda}{(1-\alpha)}$ represent the long run coefficients of respectively Senators per capita and the linear population term.

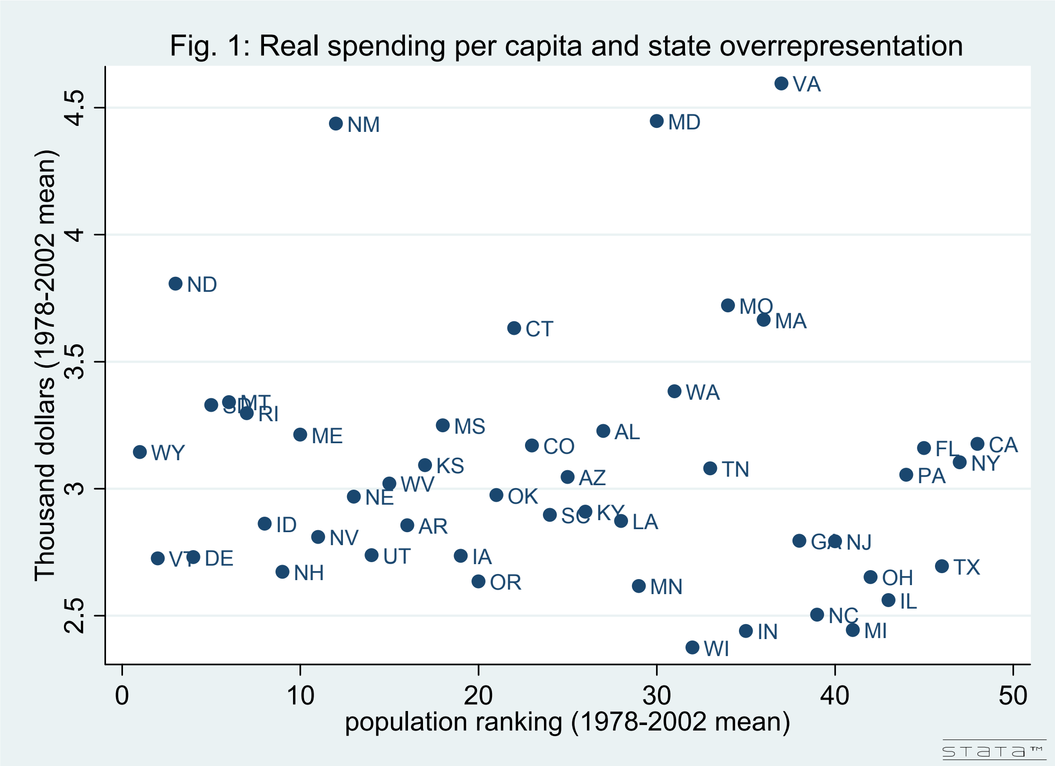
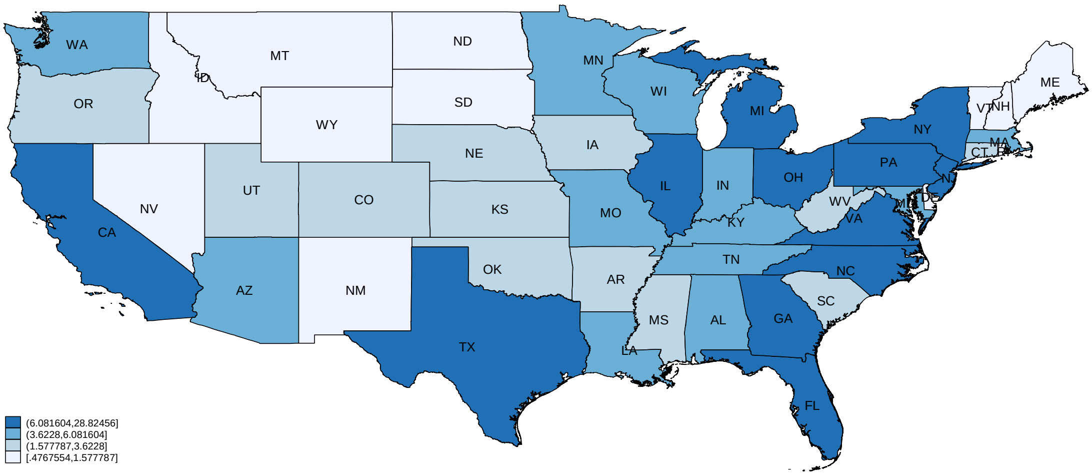
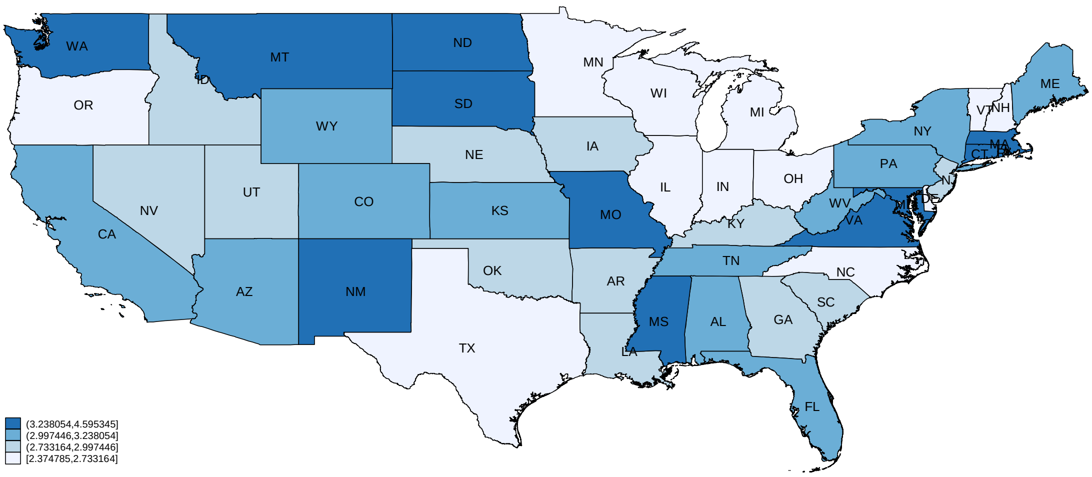


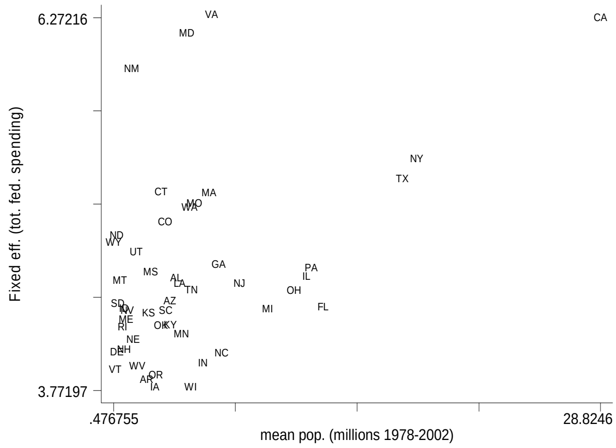
Fig. 1: Real spending per capita and state overrepresentation
Thousand dollars (1978-2002 mean)
population ranking (1978-2002 mean)
4.5
4
3.5
3
2.5
0
10
20
30
40
50
VA
NM
MD
ND
MO
MA
CT
WA
MT
RI
ME
MS
AL
WY
CO
KS
TN
FL
CA
NY
PA
WV
AZ
NE
OK
KY
LA
ID
AR
NV
NJ
DE
UT
IA
TX
NH
OH
OR
MN
IL
NC
IN
MI
WI
stata™

**Fig. 2. Population (top) and real federal spending per capita (bottom): Averages 1978-2002**

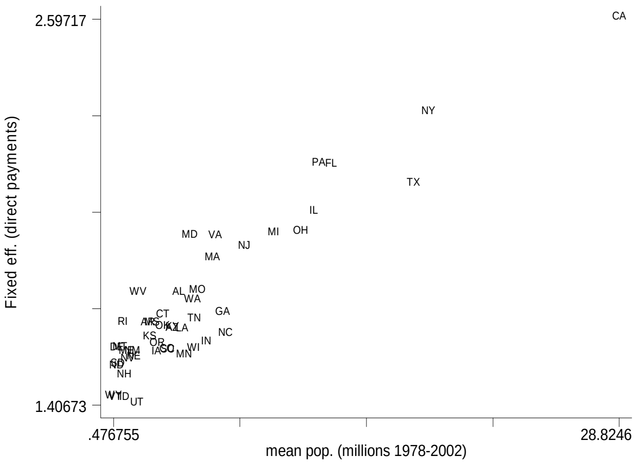


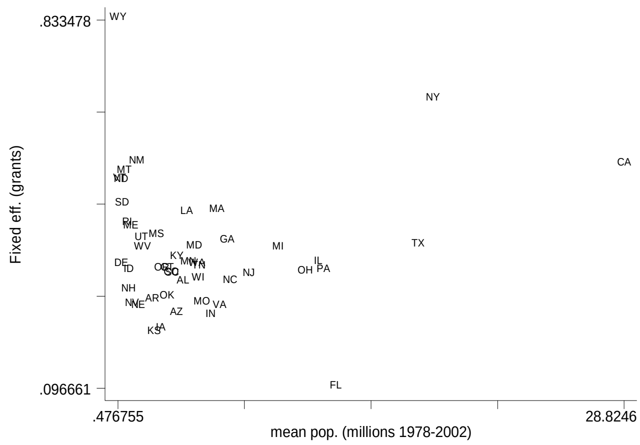
## Figure 3: Estimated fixed effects (from equations without senators per capita) and average state population (1978-2002)



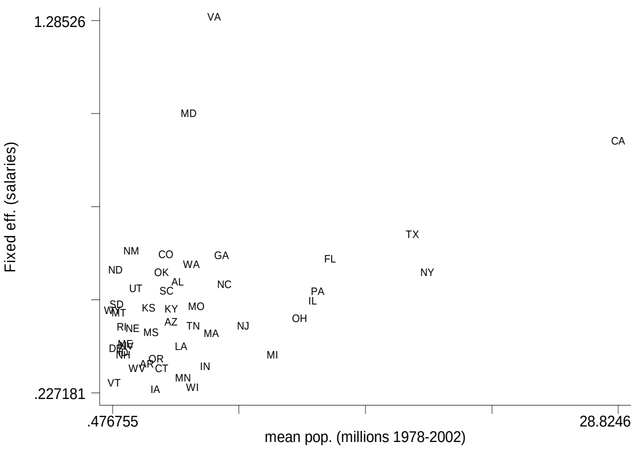

**Total Federal Spending**



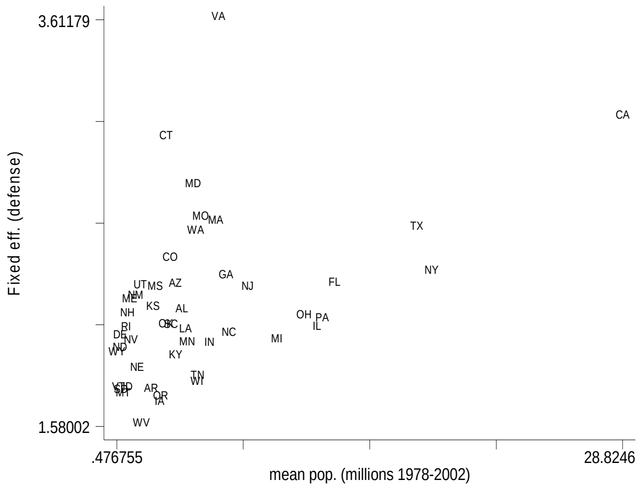

**Direct Payments to Individuals**




**Grants**




**Salaries**




**Defense**

**Fig. 4a: State shares of population and state shares of federal spending (1978=100)**

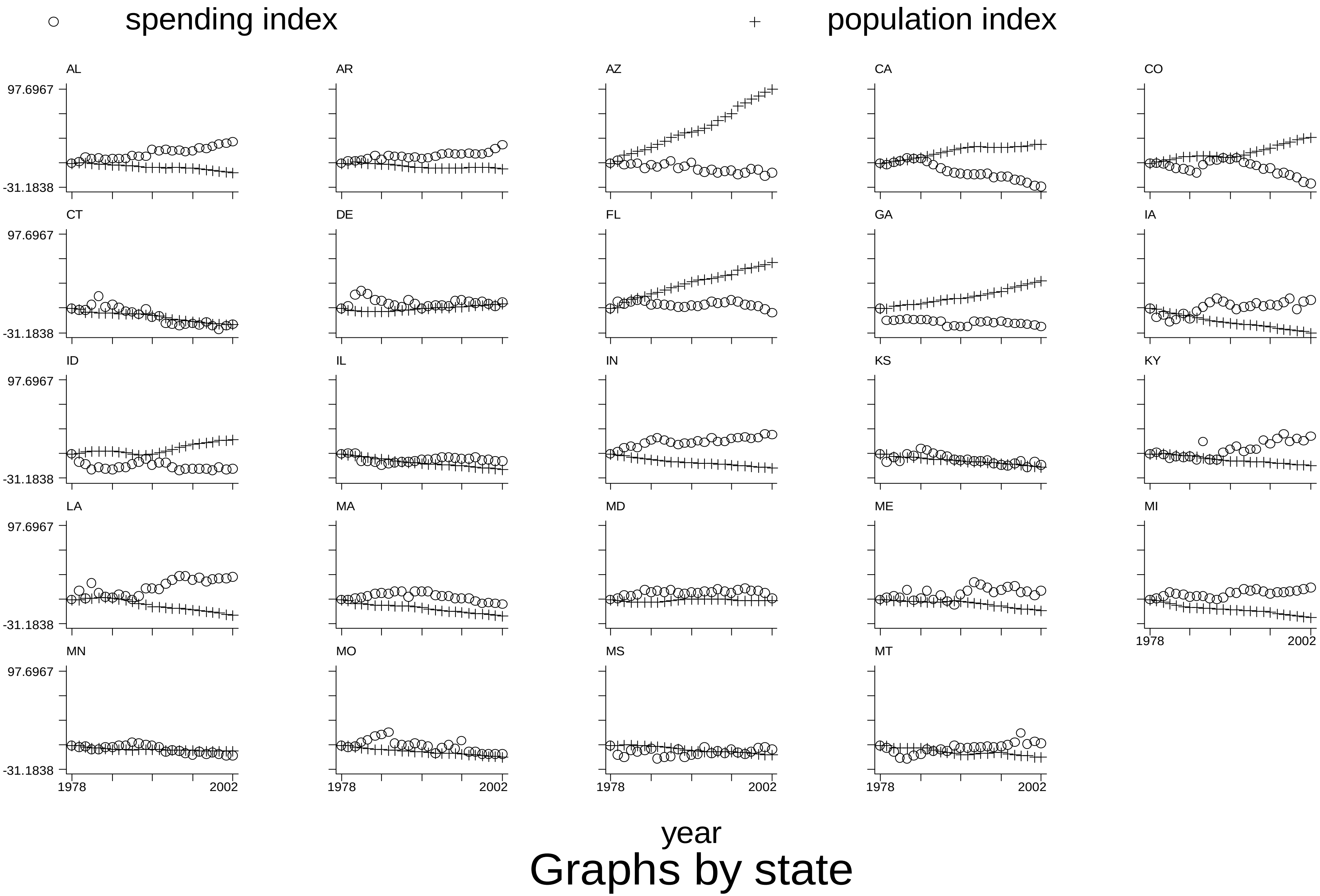


**Fig. 4b: State shares of population and state shares of federal spending (1978=100)**

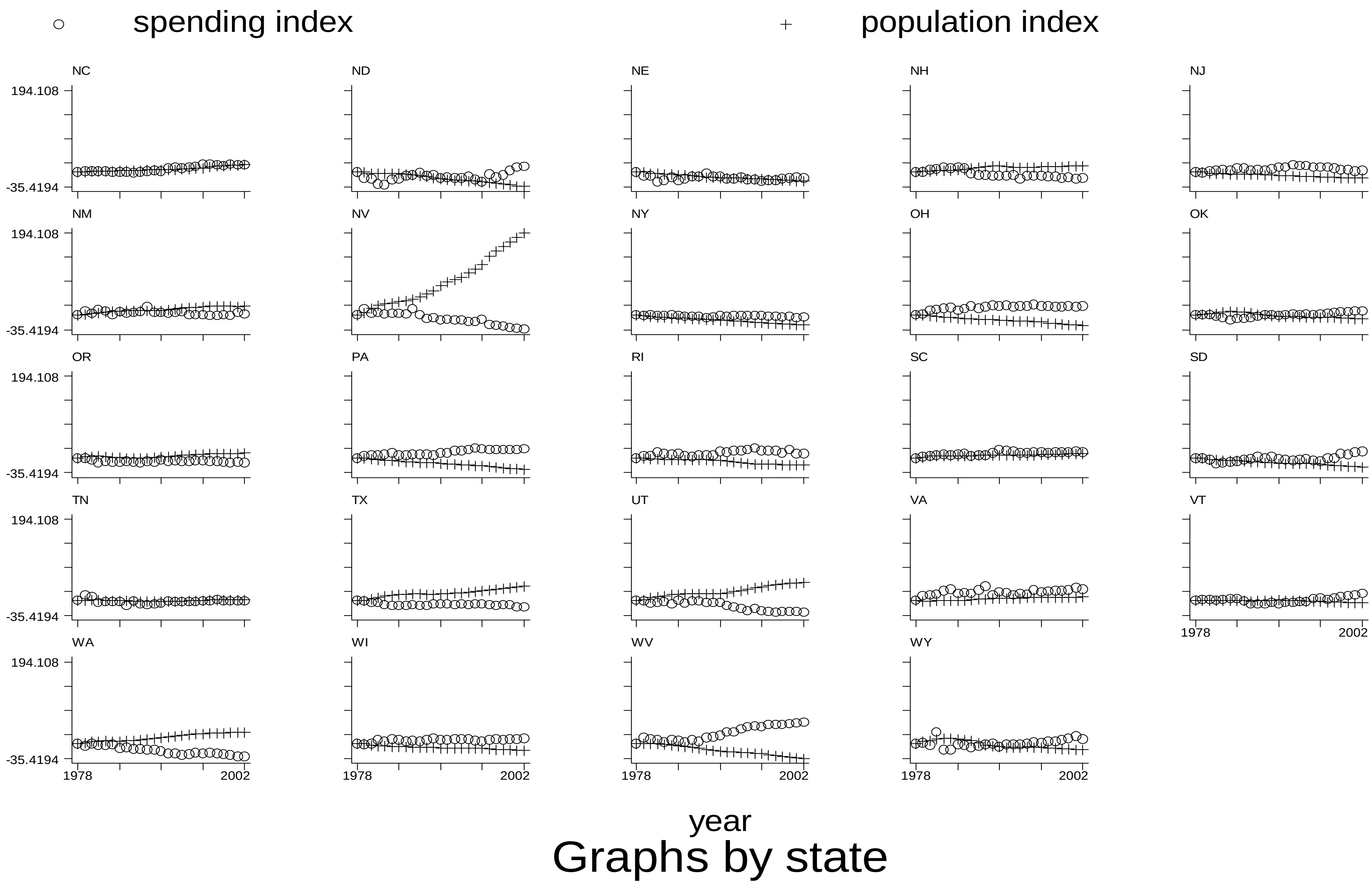
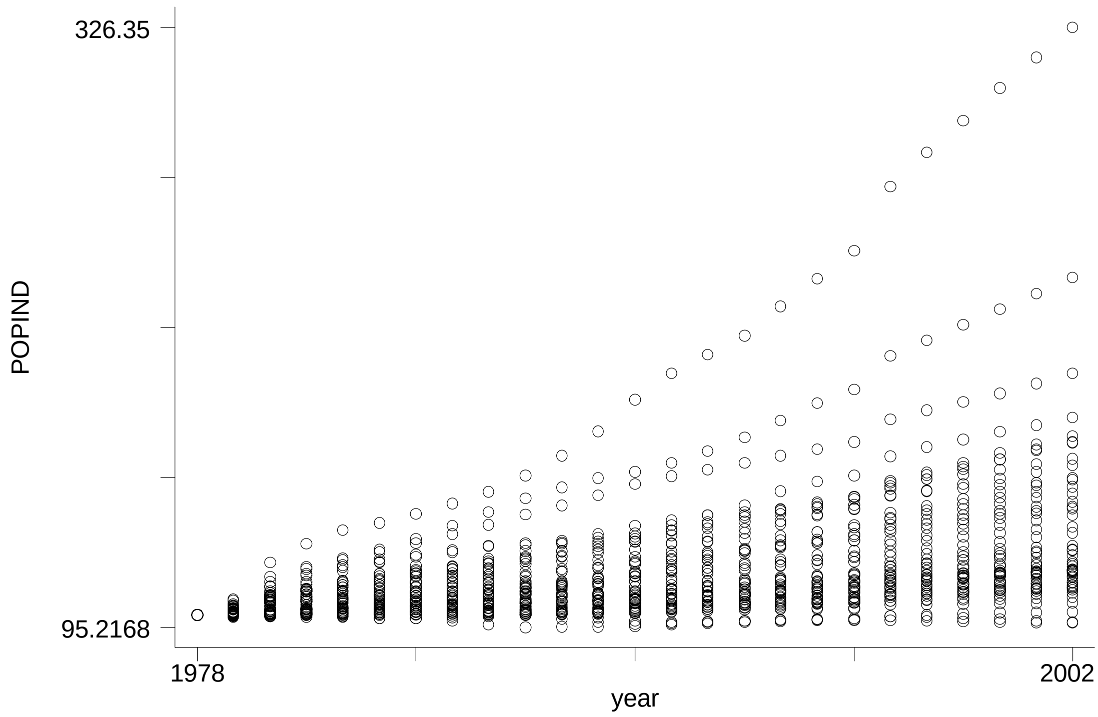


Fig. 5: State Population Index (POPIND) (base year: 1978)



## Fig. 5. US POPIND by state
## Average 1978-2002

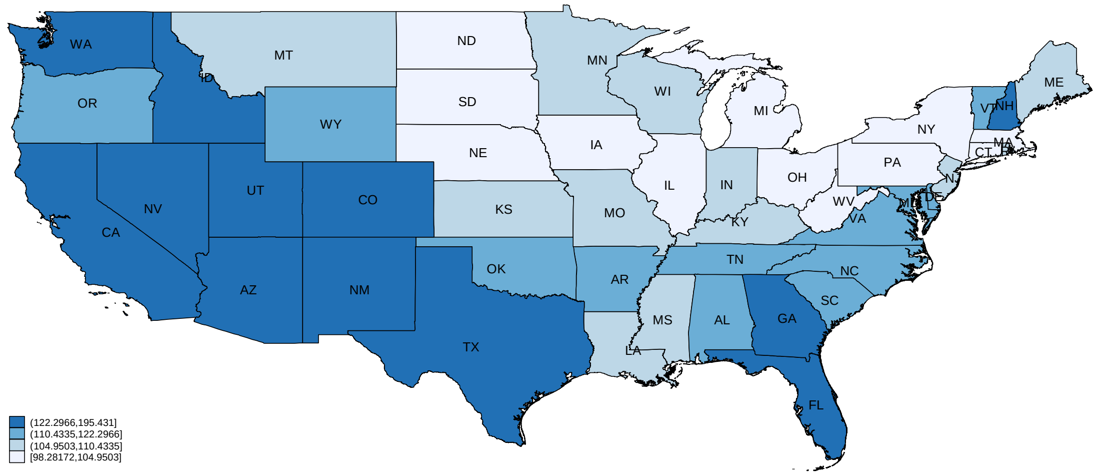


**Table 1: Average population, overrepresentation, and spending in the period 1978-2002**

| state | population (millions) | Senate overrepresentation | Federal spending per capita (real 1983 thousands USD) |
|---|---|---|---|
| WY | 0.480 | 10.844 | 3.144 |
| VT | 0.558 | 9.305 | 2.726 |
| ND | 0.651 | 7.995 | 3.807 |
| DE | 0.677 | 7.692 | 2.731 |
| SD | 0.715 | 7.254 | 3.329 |
| MT | 0.836 | 6.210 | 3.340 |
| RI | 0.993 | 5.227 | 3.297 |
| ID | 1.080 | 4.838 | 2.862 |
| NH | 1.082 | 4.820 | 2.673 |
| ME | 1.204 | 4.310 | 3.212 |
| NV | 1.302 | 4.376 | 2.810 |
| NM | 1.553 | 3.364 | 4.437 |
| NE | 1.618 | 3.207 | 2.969 |
| UT | 1.812 | 2.904 | 2.738 |
| WV | 1.851 | 2.815 | 3.020 |
| AR | 2.419 | 2.146 | 2.856 |
| KS | 2.511 | 2.066 | 3.093 |
| MS | 2.639 | 1.966 | 3.249 |
| IA | 2.856 | 1.820 | 2.736 |
| OR | 2.942 | 1.772 | 2.635 |
| OK | 3.235 | 1.605 | 2.975 |
| CT | 3.260 | 1.592 | 3.632 |
| CO | 3.499 | 1.499 | 3.170 |
| SC | 3.523 | 1.477 | 2.897 |
| KY | 3.781 | 1.372 | 2.910 |
| AZ | 3.805 | 1.418 | 3.046 |
| AL | 4.121 | 1.259 | 3.227 |
| LA | 4.323 | 1.201 | 2.873 |
| MN | 4.439 | 1.170 | 2.617 |
| MD | 4.757 | 1.093 | 4.447 |
| WA | 4.945 | 1.060 | 3.383 |
| WI | 4.977 | 1.043 | 2.375 |
| TN | 5.017 | 1.036 | 3.080 |
| MO | 5.194 | 0.999 | 3.721 |
| IN | 5.671 | 0.915 | 2.440 |
| MA | 6.014 | 0.863 | 3.664 |
| VA | 6.199 | 0.840 | 4.595 |
| GA | 6.663 | 0.789 | 2.795 |
| NC | 6.803 | 0.767 | 2.504 |
| NJ | 7.826 | 0.663 | 2.793 |
| MI | 9.447 | 0.549 | 2.444 |
| OH | 10.978 | 0.473 | 2.652 |
| IL | 11.711 | 0.443 | 2.561 |
| PA | 11.978 | 0.433 | 3.054 |
| FL | 12.854 | 0.412 | 3.160 |
| TX | 17.447 | 0.300 | 2.695 |
| NY | 18.125 | 0.286 | 3.104 |
| CA | 29.102 | 0.180 | 3.176 |

**Table 2: Summary Statistics**

| Variable | | Mean | Std. Dev. | Min | Max | Observations | |
|---|---|---|---|---|---|---|---|
| Population | overall | 5.20 | 5.48 | 0.43 | 35.12 | N = | 1200 |
| | between | | 5.47 | 0.48 | 29.10 | n = | 48 |
| | within | | 0.81 | -1.60 | 11.21 | T = | 25 |
| Senate overrepresentation | overall | 0.97 | 0.99 | 0.06 | 4.71 | N = | 1200 |
| | between | | 1.00 | 0.07 | 4.18 | n = | 48 |
| | within | | 0.13 | 0.16 | 2.25 | T = | 25 |
| Federal spending per capita | overall | 3.08 | 0.61 | 1.79 | 5.68 | N = | 1200 |
| | between | | 0.50 | 2.37 | 4.60 | n = | 48 |
| | within | | 0.35 | 1.53 | 4.91 | T = | 25 |
| Direct payments to individuals | overall | 1.58 | 0.33 | 0.80 | 3.53 | N = | 1200 |
| | between | | 0.18 | 1.12 | 2.07 | n = | 48 |
| | within | | 0.28 | 0.73 | 3.45 | T = | 25 |
| Grants | overall | 0.52 | 0.17 | 0.23 | 1.39 | N = | 1200 |
| | between | | 0.12 | 0.34 | 0.95 | n = | 48 |
| | within | | 0.12 | 0.26 | 1.04 | T = | 25 |
| Formula Grants | overall | 0.391 | 0.15 | 0.15 | 0.95 | N = | 960 |
| | between | | 0.08 | 0.25 | 0.61 | n = | 48 |
| | within | | 0.12 | 0.14 | 0.79 | T = | 20 |
| Non-Formula Grants | overall | 0.183 | 0.08 | 0.08 | 0.77 | N = | 960 |
| | between | | 0.07 | 0.11 | 0.53 | n = | 48 |
| | within | | 0.03 | 0.07 | 0.43 | T = | 20 |
| Formula grants (share of total) | overall | 0.676 | 0.08 | 0.34 | 0.84 | N = | 960 |
| | between | | 0.05 | 0.49 | 0.76 | n = | 48 |
| | within | | 0.06 | 0.45 | 0.82 | T = | 20 |
| No Formula grants (share of total) | overall | 0.324 | 0.08 | 0.16 | 0.66 | N = | 960 |
| | between | | 0.05 | 0.24 | 0.51 | n = | 48 |
| | within | | 0.06 | 0.18 | 0.55 | T = | 20 |
| Salaries | overall | 0.41 | 0.19 | 0.08 | 1.38 | N = | 1008 |
| | between | | 0.19 | 0.17 | 1.22 | n = | 48 |
| | within | | 0.05 | 0.06 | 0.57 | T = | 21 |
| Defense | overall | 0.54 | 0.36 | 0.06 | 2.51 | N = | 1200 |
| | between | | 0.34 | 0.11 | 1.99 | n = | 48 |
| | within | | 0.15 | -0.19 | 1.33 | T = | 25 |

Spending variables are expressed in thousands per capita

**Table 3: OLS regressions with real federal outlays per capita as dependent variable**

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|
| Dep. Variable: real per capita federal spending in all columns | | | | | | | |
| senators per capita | 0.0255 | 0.0026 | 0.0010 | 0.0026 | 0.0052 | 0.3452 | 0.7368 |
| | (0.42) | (0.96) | (0.30) | (0.67) | (1.25) | (5.02)*** | (7.30)*** |
| population | | | -0.0005 | -0.0011 | -0.0007 | -0.0374 | -0.0675 |
| | | | (0.98) | (2.11)** | (1.21) | (5.02)*** | (3.72)*** |
| PRincome | | | | | -0.0042 | -0.0397 | -0.0737 |
| | | | | | (2.41)** | (3.07)*** | (2.14)** |
| unemployment | | | | | 0.0038 | 0.0046 | 0.0014 |
| | | | | | (1.59) | (0.83) | (0.11) |
| aged | | | | | 0.2418 | 3.5910 | 10.0498 |
| | | | | | (0.75) | (2.17)** | (2.99)*** |
| kids | | | | | -0.4785 | -2.7317 | -8.5637 |
| | | | | | (1.24) | (2.16)** | (3.39)*** |
| dependent variable at t-1 | | 0.9896 | 0.9894 | 0.9727 | 0.9735 | 0.6252 | |
| | | (138.45)*** | (139.29)*** | (84.64)*** | (78.61)*** | (12.73)*** | |
| Constant | 3.0513 | 0.0637 | 0.0686 | 0.2900 | 0.2889 | 2.0337 | 4.9250 |
| | (29.65)*** | (3.25)*** | (3.50)*** | (6.71)*** | (2.24)** | (3.84)*** | (4.30)*** |
| Observations | 1200 | 1152 | 1152 | 1152 | 1152 | 1152 | 1200 |
| R-squared (overall) | 0.0017 | 0.9143 | 0.9143 | 0.9417 | 0.9421 | 0.9541 | 0.9177 |
| short run marginal effect of population at the mean | -.0019078 | -.0001964 | -.0000728 | -.0012515 | -.0010832 | -.0632542 | -.1226095 |
| | (0.42) | (0.96) | (0.30) | (2.76)*** | (2.13)** | (6.47)*** | (6.25)*** |
| long run marginal effect of population at the mean | | -.0188326 | -.0567025 | -.0458381 | -.0408237 | -.1687544 | |
| | | (0.90) | (0.99) | (2.02)** | (1.73)* | (6.60 )*** | |

*Robust t statistics in parentheses from standard errors clustered by state.*
** significant at 10%; ** significant at 5%; *** significant at 1%*

**Table 4a: OLS regressions with aggregates from the Statistical Abstract (without state fixed effects)**

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Dep. Variable | **direct payments to individuals (1978-2002)** | **grants (1977-2002)** | **salaries (1982-2002)** | **defense (1977-2002)** |
| senators per capita | 0.0062 | 0.0061 | 0.0021 | -0.0057 |
| | (1.35) | (2.37)** | (0.18) | (2.68)** |
| state population | -0.0002 | 0.0003 | -0.0011 | -0.0005 |
| | (0.56) | (0.94) | (0.76) | (2.10)** |
| income | -0.0030 | -0.0011 | -0.0062 | -0.0019 |
| | (3.04)*** | (1.40) | (1.06) | (1.50) |
| unemployment | 0.0022 | 0.0012 | -0.0101 | -0.0030 |
| | (1.38) | (1.78)* | (1.40) | (1.78)* |
| % aged above 65 | 0.5428 | 0.0698 | -2.3510 | -0.2039 |
| | (2.15)** | (0.76) | (1.53) | (1.48) |
| % in schooling age (5-17) | -0.3818 | -0.1419 | -1.4051 | -0.1620 |
| | (2.15)** | (1.90)* | (1.34) | (0.78) |
| dependent variable at t-1 | 0.9506 | 0.9680 | 0.5690 | 0.9678 |
| | (23.99)*** | (50.82)*** | (2.17)** | (75.48)*** |
| Constant | 0.1520 | 0.0597 | 0.9480 | 0.1175 |
| | (1.92)* | (1.92)* | (1.74)* | (1.64) |
| Year Dummies | yes | yes | yes | yes |
| State Fixed Effects | no | no | no | no |
| Observations | 1152 | 1200 | 960 | 1200 |
| R-squared | 0.9741 | 0.9535 | 0.6617 | 0.9369 |
| short run marginal effect of population at the mean | -.0007055 | 000199 | -.0012742 | -.0000736 |
| | (2.30)** | (0.68) | (0.98) | (0.37) |
| long run marginal effect of population at the mean | -.0142841 | -.00621 | -.0029567 | -.0022901 |
| | (1.11 ) | (0.82) | (1.09) | (0.37) |

*Robust t statistics in parentheses from standard errors clustered by state.*
** significant at 10%; ** significant at 5%; *** significant at 1%*

**Table 4b: OLS regressions with aggregates from the Statistical Abstract (with state fixed effects)**

| | (6) | (7) | (8) | (9) |
|---|---|---|---|---|
| Dep. Variable | **direct payments to individuals (1978-2002)** | **grants (1977-2002)** | **salaries (1982-2002)** | **defense (1977-2002)** |
| senators per capita | 0.0416 | 0.0430 | 0.1104 | 0.0076 |
| | (1.97)* | (2.25)** | (2.96)*** | (0.34) |
| state population | -0.0072 | -0.0034 | -0.0202 | -0.0092 |
| | (2.16)** | (1.49) | (3.83)*** | (1.45) |
| income | -0.0078 | -0.0038 | 0.0001 | -0.0258 |
| | (2.59)** | (1.46) | (0.02) | (2.49)** |
| unemployment | 0.0064 | 0.0029 | -0.0011 | -0.0081 |
| | (4.30)*** | (2.76)*** | (0.38) | (2.05)** |
| % aged above 65 | 0.2514 | 0.4298 | -0.6771 | 0.1964 |
| | (0.48) | (1.57) | (0.61) | (0.15) |
| % in schooling age (5-17) | -1.0944 | -0.6147 | 0.0184 | -0.5527 |
| | (3.22)*** | (3.34)*** | (0.03) | (1.36) |
| dependent variable at t-1 | 0.9177 | 0.7325 | 0.0451 | 0.7011 |
| | (11.13)*** | (20.76)*** | (0.92) | (15.25)*** |
| Constant | 0.4585 | 0.2774 | 0.5761 | 0.7005 |
| | (2.12)** | (3.70)*** | (2.78)*** | (2.37)** |
| Year Dummies | yes | yes | yes | yes |
| State Fixed Effects | no | no | no | no |
| Observations | 1152 | 1200 | 960 | 1200 |
| R-squared | 0.9768 | 0.9596 | 0.9650 | 0.9469 |
| short run marginal effect of population at the mean | -.0103144 | -.0066055 | -.0284184 | -.0097332 |
| | (2.38)** | (2.32)** | (5.37)*** | (1.48) |
| long run marginal effect of population at the mean | -.1253026 | -.0246901 | -.0297612 | -.0325607 |
| | (1.38) | (2.45)** | (5.61)*** | (1.41) |

*Robust t statistics in parentheses from standard errors clustered by state.*
** significant at 10%; ** significant at 5%; *** significant at 1%*

**Table 5: Change and scale effects (OLS regressions)**

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Dep. Variable | federal spending | direct payments to individuals | grants | salaries | defense | federal spending except defense |
| senators per capita | 0.1803 | 0.0016 | -0.0121 | 0.0153 | 0.0573 | 0.1288 |
| | (1.77)* | (0.06) | (0.44) | (0.22) | (1.07) | (1.69)* |
| population index | -0.0021 | -0.0005 | -0.0008 | -0.0011 | 0.0006 | -0.0037 |
| | (2.41)** | (1.73)* | (2.79)*** | (1.65) | (1.28) | (4.62)*** |
| state population | -0.0225 | -0.0037 | 0.0020 | -0.0122 | -0.0139 | -0.0042 |
| | (1.89)* | (1.68) | (1.09) | (1.94)* | (2.14)** | (0.59) |
| income | -0.0420 | -0.0083 | -0.0044 | -0.0012 | -0.0257 | -0.0164 |
| | (3.27)*** | (2.78)*** | (1.59) | (0.29) | (2.45)** | (2.04)** |
| unemployment | 0.0041 | 0.0064 | 0.0029 | -0.0016 | -0.0081 | 0.0141 |
| | (0.72) | (4.27)*** | (2.77)*** | (0.57) | (2.02)** | (2.86)*** |
| % aged above 65 | 3.5114 | 0.2389 | 0.4095 | -0.8069 | 0.2724 | 4.6212 |
| | (2.00)* | (0.45) | (1.39) | (0.72) | (0.21) | (3.51)*** |
| % in schooling age (5-17) | -2.7014 | -1.0738 | -0.5976 | 0.1585 | -0.5921 | -2.1292 |
| | (2.17)** | (3.18)*** | (3.35)*** | (0.23) | (1.46) | (2.21)** |
| dependent variable at t-1 | 0.6128 | 0.9117 | 0.7092 | 0.0425 | 0.6968 | 0.4982 |
| | (12.93)*** | (10.60)*** | (17.76)*** | (0.91) | (14.75)*** | (10.54)*** |
| Constant | 2.3763 | 0.5409 | 0.3898 | 0.7123 | 0.6211 | 2.1081 |
| | (4.36)*** | (2.19)** | (4.55)*** | (3.29)*** | (2.02)** | (4.98)*** |
| Year Dummies | yes | yes | yes | yes | yes | yes |
| State Fixed Effects | yes | yes | yes | yes | yes | yes |
| Observations | 1152 | 1152 | 1200 | 960 | 1200 | 1152 |
| Overall R-squared | 0.9545 | 0.9768 | 0.9604 | 0.9660 | 0.9470 | 0.9539 |
| short run marginal effect of population at the mean | -.0360194 | -.0037719 | .0028852 | -.0133107 | -.0181566 | 0.0138 |
| | (2.10)** | (1.09) | (0.77 ) | (1.45) | (1.96)** | (1.17) |
| long run marginal effect of population at the mean | -.0930315 | -.0427245 | .0099223 | -.0139019 | -.0598846 | 0.0275 |
| | (2.21 )** | (1.01 ) | (0.78) | (1.46) | (2.02)** | (1.17) |

Robust t statistics in parentheses from standard errors clustered by state. * significant at 10%; ** significant at 5%; *** significant at 1%

**Table 6: predicted spending (outlays, real 1983 USD)**

| | POPIND | POPIND | average spending percapita | | predicted difference: total | | predicted spending | |
|---|---|---|---|---|---|---|---|---|
| | | | | | per capita | share of | per capita | share of |
| state | *average* | *% deviation from US* | *total spending* | *grants* | *(real 1983 USD)* | *state average* | *(real 1983 USD)* | *state average* |
| NV | 195.43 | 67.95% | 2784 | 388 | -166.05 | -5.96% | -63.26 | -16.32% |
| AZ | 160.35 | 37.80% | 2998 | 413 | -92.37 | -3.08% | -35.19 | -8.53% |
| FL | 148.41 | 27.54% | 3141 | 343 | -67.31 | -2.14% | -25.64 | -7.48% |
| UT | 137.65 | 18.30% | 2682 | 443 | -44.71 | -1.67% | -17.03 | -3.85% |
| TX | 133.18 | 14.45% | 2637 | 379 | -35.32 | -1.34% | -13.46 | -3.55% |
| GA | 131.28 | 12.82% | 2756 | 459 | -31.34 | -1.14% | -11.94 | -2.60% |
| CA | 130.50 | 12.15% | 3121 | 480 | -29.69 | -0.95% | -11.31 | -2.36% |
| WA | 130.38 | 12.05% | 3291 | 480 | -29.44 | -0.89% | -11.22 | -2.34% |
| CO | 129.32 | 11.13% | 3090 | 408 | -27.20 | -0.88% | -10.36 | -2.54% |
| NM | 127.85 | 9.87% | 4381 | 682 | -24.12 | -0.55% | -9.19 | -1.35% |
| NH | 124.50 | 6.99% | 2636 | 446 | -17.08 | -0.65% | -6.51 | -1.46% |
| ID | 122.48 | 5.26% | 2773 | 474 | -12.86 | -0.46% | -4.90 | -1.03% |
| NC | 122.11 | 4.94% | 2468 | 430 | -12.07 | -0.49% | -4.60 | -1.07% |
| SC | 121.41 | 4.34% | 2861 | 457 | -10.61 | -0.37% | -4.04 | -0.89% |
| OR | 119.97 | 3.10% | 2581 | 527 | -7.58 | -0.29% | -2.89 | -0.55% |
| VA | 119.73 | 2.90% | 4519 | 351 | -7.08 | -0.16% | -2.70 | -0.77% |
| DE | 115.99 | -0.32% | 2699 | 511 | 0.78 | 0.03% | 0.30 | 0.06% |
| TN | 115.79 | -0.49% | 3042 | 496 | 1.20 | 0.04% | 0.46 | 0.09% |
| MD | 114.68 | -1.44% | 4287 | 485 | 3.53 | 0.08% | 1.34 | 0.28% |
| VT | 114.60 | -1.51% | 2676 | 681 | 3.70 | 0.14% | 1.41 | 0.21% |
| OK | 113.84 | -2.17% | 2897 | 458 | 5.29 | 0.18% | 2.02 | 0.44% |
| WY | 112.84 | -3.02% | 3102 | 955 | 7.39 | 0.24% | 2.81 | 0.29% |
| AR | 111.63 | -4.06% | 2774 | 501 | 9.93 | 0.36% | 3.78 | 0.75% |
| AL | 110.55 | -4.99% | 3183 | 482 | 12.20 | 0.38% | 4.65 | 0.96% |
| MN | 110.32 | -5.20% | 2481 | 491 | 12.70 | 0.51% | 4.84 | 0.99% |
| ME | 110.28 | -5.23% | 3183 | 630 | 12.77 | 0.40% | 4.86 | 0.77% |
| MS | 109.98 | -5.49% | 3187 | 575 | 13.41 | 0.42% | 5.11 | 0.89% |
| LA | 108.46 | -6.79% | 2816 | 576 | 16.60 | 0.59% | 6.32 | 1.10% |
| KY | 108.35 | -6.89% | 2871 | 524 | 16.83 | 0.59% | 6.41 | 1.22% |
| MO | 107.17 | -7.90% | 3573 | 452 | 19.31 | 0.54% | 7.35 | 1.63% |
| MT | 107.16 | -7.91% | 3071 | 716 | 19.32 | 0.63% | 7.36 | 1.03% |
| KS | 106.99 | -8.05% | 2938 | 397 | 19.67 | 0.67% | 7.49 | 1.89% |
| NJ | 106.99 | -8.05% | 2776 | 469 | 19.68 | 0.71% | 7.50 | 1.60% |
| RI | 106.53 | -8.45% | 3261 | 674 | 20.66 | 0.63% | 7.87 | 1.17% |
| WI | 106.28 | -8.67% | 2313 | 476 | 21.18 | 0.92% | 8.07 | 1.70% |
| IN | 105.29 | -9.52% | 2373 | 384 | 23.26 | 0.98% | 8.86 | 2.31% |
| CT | 104.61 | -10.10% | 3573 | 520 | 24.67 | 0.69% | 9.40 | 1.81% |
| IL | 104.56 | -10.14% | 2515 | 445 | 24.79 | 0.99% | 9.44 | 2.12% |
| MA | 104.22 | -10.44% | 3560 | 625 | 25.50 | 0.72% | 9.71 | 1.55% |
| SD | 103.68 | -10.90% | 3100 | 678 | 26.63 | 0.86% | 10.14 | 1.50% |
| NE | 103.15 | -11.36% | 2755 | 456 | 27.75 | 1.01% | 10.57 | 2.32% |
| MI | 102.90 | -11.57% | 2407 | 484 | 28.28 | 1.17% | 10.77 | 2.23% |
| OH | 102.59 | -11.83% | 2654 | 447 | 28.91 | 1.09% | 11.01 | 2.47% |
| NY | 102.40 | -12.00% | 3061 | 750 | 29.31 | 0.96% | 11.17 | 1.49% |
| PA | 101.51 | -12.76% | 3016 | 502 | 31.19 | 1.03% | 11.88 | 2.37% |
| ND | 99.71 | -14.31% | 3382 | 726 | 34.96 | 1.03% | 13.32 | 1.84% |
| WV | 99.47 | -14.51% | 2983 | 628 | 35.47 | 1.19% | 13.51 | 2.15% |
| IA | 98.28 | -15.54% | 2502 | 433 | 37.97 | 1.52% | 14.46 | 3.34% |

(1) The Average predicted difference is obtained by substracting the average state spending percapita predicted using the average US population index from the average state spending percapita predicted using the state average population index during the period 1978-2002.

**Table 7: Formula vs non-formula grants from CFFR 1983-2002 (OLS regressions)**

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Dep. Variable | all grants | all grants | formula grants | non-formula | formula | non-formula |
| senators per capita | 0.0402* | -0.0439 | 0.0392* | 0.0013 | -0.0172 | -0.0305 |
| | (1.78) | (1.15) | (1.93) | (0.08) | (0.69) | (0.98) |
| population index | | -0.0010*** | | | -0.0007*** | -0.0003* |
| | | (3.42) | | | (3.31) | (1.87) |
| state population | -0.0054 | 0.0017 | -0.0046* | -0.001 | 0.0002 | 0.0017 |
| | (1.55) | (0.67) | (1.76) | (0.55) | (0.13) | (0.79) |
| income | -0.0043 | -0.0056 | -0.0048* | 0.0013 | -0.0058** | 0.0009 |
| | (1.12) | (1.49) | (1.96) | (0.38) | (2.31) | (0.27) |
| unemployment | 0.0033* | 0.0029* | 0.0021 | 0.0014* | 0.0017 | 0.0012 |
| | (1.96) | (1.71) | (1.61) | (1.83) | (1.40) | (1.55) |
| % aged above 65 | 1.1891** | 1.1579** | 0.6935 | 0.6343** | 0.6658 | 0.5874** |
| | (2.66) | (2.44) | (1.46) | (2.65) | (1.39) | (2.35) |
| % in schooling age (5-17) | -0.6414* | -0.5355* | 0.3906 | -0.2433 | -0.3237 | -0.1978 |
| | (1.86) | (1.75) | (1.50) | (1.07) | (1.28) | (0.98) |
| dependent variable at t-1 | 0.7157*** | 0.6835*** | 0.725*** | 0.5869 | 0.6954 | 0.5779*** |
| | (15.43) | (14.18) | (9.91) | (13.23) | (9.41) | (12.10) |
| Constant | 0.2444** | 0.2506** | 0.2064** | 0.0247 | 0.1742** | 0.0739 |
| | (2.13) | (2.18) | (2.14) | (0.39) | (2.08) | (1.13) |
| Year Dummies | yes | yes | yes | yes | yes | yes |
| State Fixed Effects | yes | yes | yes | yes | yes | yes |
| Observations | 912 | 912 | 912 | 912 | 912 | 912 |
| Overall R-squared | 0.9695 | 0.9702 | 0.9708 | 0.9248 | 0.9714 | 0.9254 |
| short run marginal effect of population at the mean | -0.0084** | 0.0157 | -0.0075** | -0.0011 | 0.0015 | 0.004 |
| | (2.06) | (1.01) | (2.44) | (0.43) | (0.48) | (0.93) |
| long run marginal effect of population at the mean | -0.0296** | 0.005 | -0.0274*** | 0.0026 | 0.005 | 0.0094 |
| | (2.03) | (1.00 ) | (2.67) | (0.41) | (0.47) | (0.99) |

Robust t statistics in parentheses from standard errors clustered by state. * significant at 10%; ** significant at 5%; *** significant at 1%

**2009**

**2009/1. Rork, J.C.; Wagner, G.A.**: "Reciprocity and competition: is there a connection?"
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**2009/3. Rodden, J.**: "Federalism and inter-regional redistribution"
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**2009/6. Jametti, M.; von Ungern-Sternberg, T.**: "Risk selection in natural disaster insurance"
**2009/7. Solé-Ollé, A; Sorribas-Navarro, P.**: "The dynamic adjustment of local government budgets: does Spain behave differently?"
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**2009/11. Libman, A.; Feld, L.P.**: "Strategic Tax Collection and Fiscal Decentralization: The case of Russia"
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**2010**

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