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WHY DO SMALL STATES RECEIVE MORE FEDERAL MONEY? U.S. SENATE REPRESENTATION AND THE ALLOCATION OF FEDERAL BUDGET

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Empirical research on the geographic distribution of U.S. federal spending shows that small states receive disproportionately more dollars per capita. This evidence, often regarded as the consequence of Senate malapportionment, in reality conflates the effects of state population size with that of state population growth. Analyzing outlays for the period 1978–2002, this study shows that properly controlling for population dynamics provides more reasonable estimates of small-state advantage and solves a number of puzzling peculiarities of previous research. We also show that states with fast-growing population loose federal spending to the advantage of slow-growing ones independently of whether they are large or small. The two population effects vary substantially across spending programs. Small states enjoy some advantage in defense spending, whereas fast-growing ones are penalized in the allocation of federal grants, particularly those administered by formulas 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.

1. INTRODUCTION

Empirical research on the geographic distribution of U.S. federal spending shows that small states (in population terms) receive disproportionately more dollars per capita. Existing evidence, however, conflates the effects of state population size with that of state population growth. This study shows that properly controlling for the latter provides more reasonable estimates of small-state advantage and solves a number of puzzling peculiarities of previous research. We also show that states with fast-growing population loose federal spending to the advantage of slow-growing ones independently of whether they are large or small.

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, as small-state advantage is often interpreted as the consequence of Senate over-representation. 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

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[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), p. 307.

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 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 funds automatically adjusts to population growth.

These problems could 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 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.[4]

Our estimates, referred to the period 1978–2002, confirm the existence of a strongly positive correlation between senators per capita and total federal outlays. We show, however, that this result is 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 over-representation is particularly strong on aggregates such as direct payments to individuals,[5] whereas we do not find any significant effect on defense spending. The extent to which geographic targeting affects defense spending is an issue open to debate,[6] but there is consensus among existing studies that direct payments to individuals (consisting mainly of hardly manipulable entitlement programs) should be the least affected by over-representation or other sources of political influence (see Hoover and Pecorino, 2005; Levitt and Snyder (1995), among others). Third, if we omit senators per capita from our regressions and analyze the estimated fixed effects (which should then contain the over-representation effect), we discover that, after controlling for sociodemographic indicators, larger states often receive more funds than average.

[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 infrastructure building) provide a very plausible explanation of the disproportionately large per capita spending received by small western states, characterized by a small population dispersed over a large land area.

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

[4]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.

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

[6]Existing evidence suggests that not all defense spending items are subject to manipulation (Carsey and Rundquist, 2002; Mayer, 1992).

The absence of any effect in pure cross-section regressions may suggest that fixed effects are crucial to correct potential omitted variable bias.[7] Nevertheless, the inclusion of fixed effects implies that the coefficient of senators per capita is estimated from within-state variation in 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 is substantially reduced. 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. The negative effect of population dynamics is particularly strong for federal grants, especially those administered by formulas limiting budgetary adjustments. Some evidence of a small-state advantage can be found in defense spending only.

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. This resonates with the concerns voiced by several representatives of fast-growing states on the fairness of budgetary allocations.[8] 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 U.S. constitution 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.[9]

The functionalist view has been increasingly challenged by recent research. Lee and Oppenheimer (1999) equate Senate apportionment to a "panda's thumb", the residual

[7]At the same time, malapportionment effect is arguably a long-term effect. In this case state fixed effects could remove part of the malapportionment effect from the estimated coefficient.

[8]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/). 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 (*The Wall Street Journal*, Who gets what from the stimulus package, January 27, 2009).

[9]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.

of a contingent historical situation: "the apportionment of the United States Senate did not result 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".[10] A substantial empirical literature provides evidence about various types of distortions generated by the equal representation principle in American politics and policy making.[11] 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.[12] 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 U.S. 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.[13] 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 small states for non-discretionary distributive programs that are allocated via formulas determined by the Congress. Lee (2000) finds that final allocations from the 1991 and 1997–1998 reauthorizations of the federal surface transportation program 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 over-representation 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 over-representation effect on transportation earmarks. At the district level, Ansolabehere et al. (2002) analyze the effect of unequal

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

[11]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 countermajoritarian 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).

[12]The actual process through which Senate over-representation could generate a bias in federal budget allocation might be related to congressional bargaining. As less funds are necessary to obtain the same increase in per capita 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).

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

representation prior to 1960 and the equalizing impact on state transfers to counties following the court-ordered redistricting in the 1960s.[14]

The evidence provided by existing studies rises some fundamental questions on U.S. bicameralism. According to the estimates of Atlas et al. (1995), the difference in real total spending due to malapportionment between the most over-represented (Wyoming) and the most under-represented (California) states amounts in 1990 to $1,148 (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).[15] Is small Wyoming really so much more 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 study we will address this important question.

3. SOME PUZZLING RESULTS

Population size varies considerably across U.S. states and so does per capita Senate representation. Federal spending per capita also varies substantially across states, but there appears to be no systematic link between Senate over-representation and spending. This can be seen graphically in Figure 1, by comparing maps A and B.[16] Although it is apparent that Rural Midwest states tend to be, on average, both over-represented and better funded, looking at the entire U.S. map it becomes clear that this is far from being a general statement.[17]

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:

[14]There is some literature on the consequences of Over-representation outside of the U.S. context. Rodden (2002) provides evidence on the impact of the over-representation 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), Hirano (2006), and Hirano and Ting (2008). Hans et al. (2006) provide evidence from Germany.

[15]The magnitudes reported by Lee and Oppenheimer (1999) are substantially smaller. They use 7 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 programs that represent an overall 56% of the federal budget, hence the final magnitudes are necessarily smaller than those obtained by using total federal spending.

[16]Like most of literature on the allocation of U.S. federal spending, we focus on the 48 contiguous states.

[17]In the online Appendix we report more details on the link between spending and population. We construct an index of average Senate over-representation by state during the period 1978–2002. Under- or over-representation is determined by comparison with a fair representation given by the ratio between the total members of the Senate and the total U.S. population in a given year. More specifically, define N_{st} as the population of state s in year t and $USpop_t$ as the total U.S. population (in the 48 states considered) in year t. Then the over-representation 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. In Table S1 states are ordered by average population in the period 1978–2002 (starting with the smallest) and obviously smaller states are Over-represented in the Senate. Table S1 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.

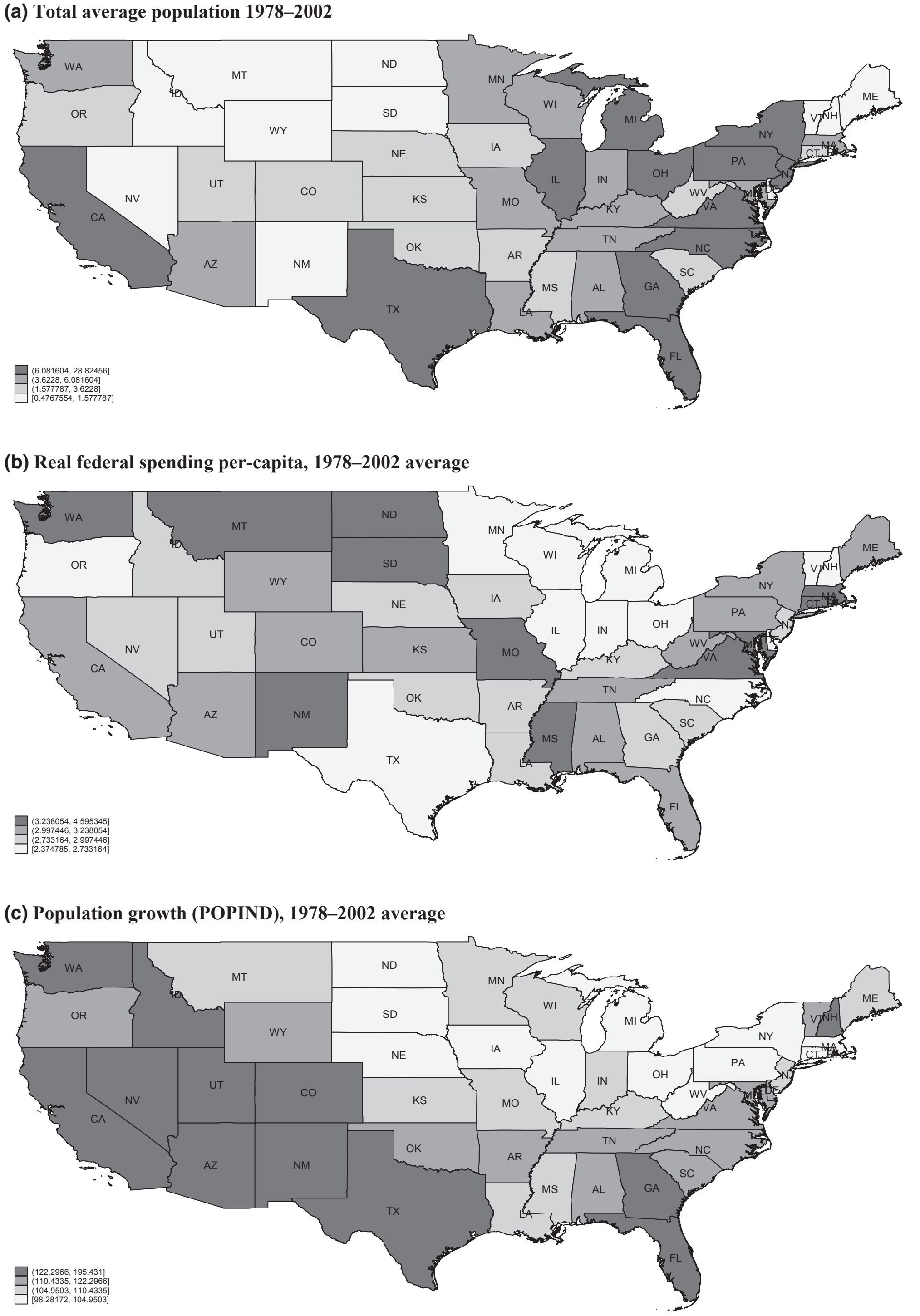

Figure 1. U.S. population and federal spending

$$y_{st} = \alpha y_{st-1} + \beta * SP_{st} + \lambda N_{st} + \boldsymbol{\theta}\mathbf{Z}_{st} + \gamma_s + \delta_t + \epsilon_{st}, \quad s = 1, \ldots 48; t = 1978, \ldots 2002; \tag{1}$$

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,[18] 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 γ_s and δ_t represent, respectively, state and year fixed effects.[19]

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. Long-run multipliers, capturing the cumulative effects of the regressors over the years, can be calculated by dividing each short-run multiplier by $(1 - \alpha)$. As 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 population evaluated at the average population value in our sample (both the short-run and long-run coefficients).[20]

We start by estimating equation (1) using Census data for the U.S. States during period 1978–2002.[21] Summary statistics are reported in Table S2 (see appendix) and estimates in Table 1. We start with a simple regression of real federal spending 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 $\widehat{\beta}$.[22] The population effect at the mean is instead statistically significant when we

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

[19] 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 $T > 20$, whereas the bias in α may remain sizeable, the bias in the other coefficients becomes very small (?), 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.

[20] We use average population in equations (2) and (3) to provide a representative estimate of the multipliers. However, as these expressions are non-linear, the multipliers also vary in a non-linear fashion with population growth. Exact shapes and magnitude of the multipliers for varying population levels can be derived using equations (2) and (3).

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

[22] 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.

Table 1. OLS Regressions with Real Federal Outlays per capita as Dependent Variable

Dep. variable: real per capita federal spending in all columns	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Senators per capita	0.0255	0.0026	0.0010	0.0026	0.0052	0.3452	0.208	0.7368
	(0.42)	(0.96)	(0.30)	(0.67)	(1.25)	(5.02)***	(0.26)	(7.30)***
Population			−0.0005	−0.0011	−0.0007	−0.0374	0.122	−0.0675
			(0.98)	(2.11)**	(1.21)	(5.02)***	(1.00)	(3.72)***
PR income					−0.0042	−0.0397	0.0022	−0.0737
					(2.41)**	(3.07)***	(0.06)	(2.14)**
Unemployment					0.0038	0.0046	0.0147	0.0014
					(1.59)	(0.83)	(0.49)	(0.11)
Aged					0.2418	3.5910	3.1088	10.0498
					(0.75)	(2.17)**	(0.62)	(2.99)***
Kids					−0.4785	−2.7317	8.5343	−8.5637
					(1.24)	(2.16)**	(1.65)	(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	5.5025	4.9250
	(29.65)***	(3.25)***	(3.50)***	(6.71)***	(2.24)**	(3.84)***	(2.89)	(4.30)***
Time fixed effects	No	No	No	Yes	Yes	Yes	Yes	Yes
State fixed effects	No	No	No	No	No	Yes	No	Yes
Observations	1,200	1,152	1,152	1,152	1,152	1,152	1,200	1,200
R^2 (overall)	0.0017	0.9143	0.9143	0.9417	0.9421	0.9541	0.2563	0.9177
Short-run marginal effect of	−.0019078	−.0001964	−.0000728	−.0012515	−.0010832	−.0632542	−0.0138	−.1226095
population at the mean	(0.42)	(0.96)	(0.30)	(2.76)***	(2.13)**	(6.47)***	(1.37)	(6.25)***
Long-run marginal effect of		−.0188326	−.0567025	−.0458381	−.0408237	−.1687544		
population at the mean		(0.90)	(0.99)	(2.02)**	(1.73)*	(6.60)***		

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

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.[23] This result is not driven by the inclusion of a lagged dependent variable. In columns (7) and (8) we replicate, respectively, specifications (5) and (6), but we remove y_{st-1}. The results remain quite similar: in the specification without state fixed effects (column 7) *SP* and *population* (as well as the overall effect of population) are not statistically distinguishable from zero. In the specification which includes state fixed effects (column 8), both *SP* and *population* display large and statistically significant coefficients [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 in *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 may suggest the existence of a potentially important omitted variable bias in cross-section regressions, they should be interpreted with caution, as within-state variation in population can have a direct negative effect on spending independently of over-representation.

In Table 2 we use as dependent variables the spending aggregates available from the Statistical Abstract of the United States. We report specifications with and without 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 2, columns 5–8), 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.[24] 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, although at least some important defense items should, in fact, be subject to geographic manipulation (Carsey and Rundquist, 2002).

Finally, we estimate equation (1) without the *SP* indicator. In this case we expect the effect of malapportionment to be incorporated into the state fixed effects. Figure 2

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

[24]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.

TABLE 2. OLS REGRESSIONS WITH AGGREGATES FROM THE STATISTICAL ABSTRACT

Dep. variable	Without state fixed effects (1) Direct payments to individuals (1978–2002)	(2) Grants (1977–2002)	(3) Salaries (1982–2002)	(4) Defense (1977–2002)	With state fixed effects (5) Direct payments to individuals (1978–2002)	(6) Grants (1977–2002)	(7) Salaries (1982–2002)	(8) Defense (1977–2002)
Senators per capita	0.0062	0.0061	0.0021	−0.0057	0.0416	0.0430	0.1104	0.0076
	(1.35)	(2.37)**	(0.18)	(2.68)**	(1.97)*	(2.25)**	(2.96)***	(0.34)
State population	−0.0002	0.0003	−0.0011	−0.0005	−0.0072	−0.0034	−0.0202	−0.0092
	(0.56)	(0.94)	(0.76)	(2.10)**	(2.16)**	(1.49)	(3.83)***	(1.45)
Income	−0.0030	−0.0011	−0.0062	−0.0019	−0.0078	−0.0038	0.0001	−0.0258
	(3.04)***	(1.40)	(1.06)	(1.50)	(2.59)**	(1.46)	(0.02)	(2.49)**
Unemployment	0.0022	0.0012	−0.0101	−0.0030	0.0064	0.0029	−0.0011	−0.0081
	(1.38)	(1.78)*	(1.40)	(1.78)*	(4.30)***	(2.76)***	(0.38)	(2.05)**
% Aged above 65	0.5428	0.0698	−2.3510	−0.2039	0.2514	0.4298	−0.6771	0.1964
	(2.15)**	(0.76)	(1.53)	(1.48)	(0.48)	(1.57)	(0.61)	(0.15)
% In schooling age (5–17)	−0.3818	−0.1419	−1.4051	−0.1620	−1.0944	−0.6147	0.0184	−0.5527
	(2.15)**	(1.90)*	(1.34)	(0.78)	(3.22)***	(3.34)***	(0.03)	(1.36)
Dependent variable at $t-1$	0.9506	0.9680	0.5690	0.9678	0.9177	0.7325	0.0451	0.7011
	(23.99)***	(50.82)***	(2.17)**	(75.48)***	(11.13)***	(20.76)***	(0.92)	(15.25)***
Constant	0.1520	0.0597	0.9480	0.1175	0.4585	0.2774	0.5761	0.7005
	(1.92)*	(1.92)*	(1.74)*	(1.64)	(2.12)**	(3.70)***	(2.78)***	(2.37)**
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects	No	No	No	No	Yes	Yes	Yes	Yes
Observations	1,152	1,200	960	1,200	1,152	1,200	960	1,200
R^2	0.9741	0.9535	0.6617	0.9369	0.9768	0.9596	0.9650	0.9469
Short-run marginal effect of	−.0007055	000199	−.0012742	−.0000736	−.0103144	−.0066055	−.0284184	−.0097332
population at the mean	(2.30)**	(0.68)	(0.98)	(0.37)	(2.38)**	(2.32)**	(5.37)***	(1.48)
Long-run marginal effect of	−.0142841	−.00621	−.0029567	−.0022901	−.1253026	−.0246901	−.0297612	−.0325607
population at the mean	(1.11)	(0.82)	(1.09)	(0.37)	(1.38)	(2.45)**	(5.61)***	(1.41)

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

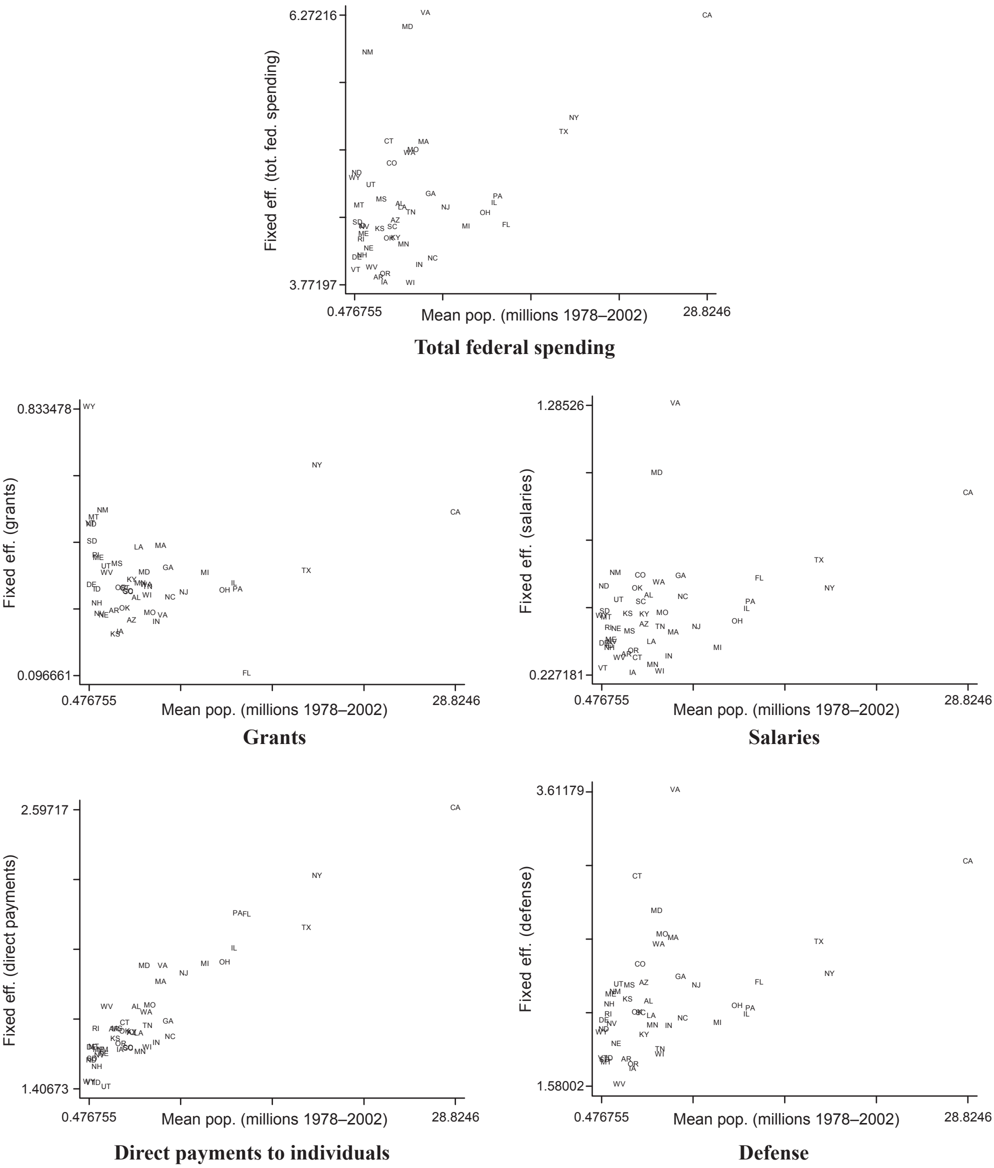

Figure 2. Estimated fixed effects (from equations without senators per capita) and average state population (1978–2002).

plots the estimated fixed effects vs. the average state population (in the period considered).[25] 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

[25]Using average population is a meaningful exercise as the ranking of the various states in population terms is relatively stable over the period considered.

states is very clear for entitlements (with North Dakota being the sole exception), whereas 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. As 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.[26]

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*. This leads us to another identification problem. Population variation across states may induce variation in per capita federal spending because they differ in their population sizes (*scale effect*) but also, independently of their size, 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.

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 U.S. states are remarkably different in their population dynamics. During the period we consider (1978–2002), for example, the population of Nevada tripled, whereas

[26]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 over-representation indicator is given by $\frac{2}{N_{st}}$. The above equation, with or without fixed effects, cannot identify the impact of over-representation 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: $Y_{st} = 2\beta + \lambda N_{st}^2 + \boldsymbol{\theta}\mathbf{z}_{st} + \gamma_s N_{st}$. In this equation, the effect of over-representation on *total spending* (Y_{st}) is captured by the constant term (2β). Hence, any factor that induces a positive constant term in the total spending regression would be interpreted as over-representation 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 over-representation is the most important of them.

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 as several factors contribute to generate inertia in the allocation of the federal budget. First, as pointed out by incrementalist theories (Davis et al., 1966; Dempster and Wildavsky, 1979; Wildavsky, 1964), the complexity of the budget implies that new provisions are determined mainly by marginal changes to previous ones. Second, demand-side explanations of budgetary provisions stress that former allocations may have a strong impact on current ones because states and local governments accumulate experience from past grants applications and knowledge of the federal decision-making process (Rich, 1989). Third, many federal programs are administered through formulas that – through hold-harmless provisions, caps, floors, and ceilings – introduce inertia in budgetary allocations. *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.[27] If a population change results in a decrease in funding below a designated amount, the hold-harmless provision would raise the amount to the 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.[28] Finally, the use of outdated population data in formulas penalizes states whose population grows fast.[29] As we will see, the budgetary inertia introduced by these mechanisms can have important consequences for the allocation of federal money. Given the incremental nature of the budget, inertia may of course also be driven by the limited responsiveness of allocations to other changing characteristics of the states (besides population). For this reason we always prefer to include lagged spending in our regressions. In this way, we can control for any other source of inertia not related to population.

A simple graphical analysis can illustrate quite effectively the relationship between spending per capita and state population. We construct two indices that capture for

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

[28]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 U.S. public elementary and secondary school. Other important programs subject to limits are the Federal Highway Program and Medicaid.

[29]In a testimony (26 February, 2008) to Congress concerning State Children's Health Insurance program, 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 4 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).

each state the evolution over time of their respective spending and population shares (of the U.S. total).[30] An index equal to 0 means that the state share of U.S. total spending (population) is the same as in 1978, i.e., that the state spending (population) is increasing at the same pace as the U.S. average. An index above 0 means that the state spending (population) grows above the U.S. average and therefore has a higher share of the U.S. total compared to 1978, with 100 indicating that such share has doubled. Negative values indicate decreasing shares instead.

The evolution of these two indices over time, reported in Figure 3a and b, 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 under-represented states with fast-growing populations and correspondingly decreasing federal spending per capita. Pennsylvania and Ohio are also heavily under-represented, 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 over-represented 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 with 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 over-represented state with the fastest growing population in the United States – 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 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 in the state population from the same base year. *POPIND* focuses on percentage change and therefore a given *absolute change* in population has a higher impact on *POPIND* in a small rather than in a large state. When we include *POPIND* in spending equations, the assumption we make is that percentage rather than absolute population changes matter for spending, which is probably reasonable, to a certain extent, for most spending aggregates.[31] It is also important to remark that both *SP* and *POPIND* vary only as a function of population. Our empirical strategy, there-

[30]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 U.S. index will then describe the relative change in spending in a state compared with the U.S. 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 U.S. index.

[31]In other terms, 1,000 more people in California should have a smaller impact on spending than 1,000 more people in Wyoming.

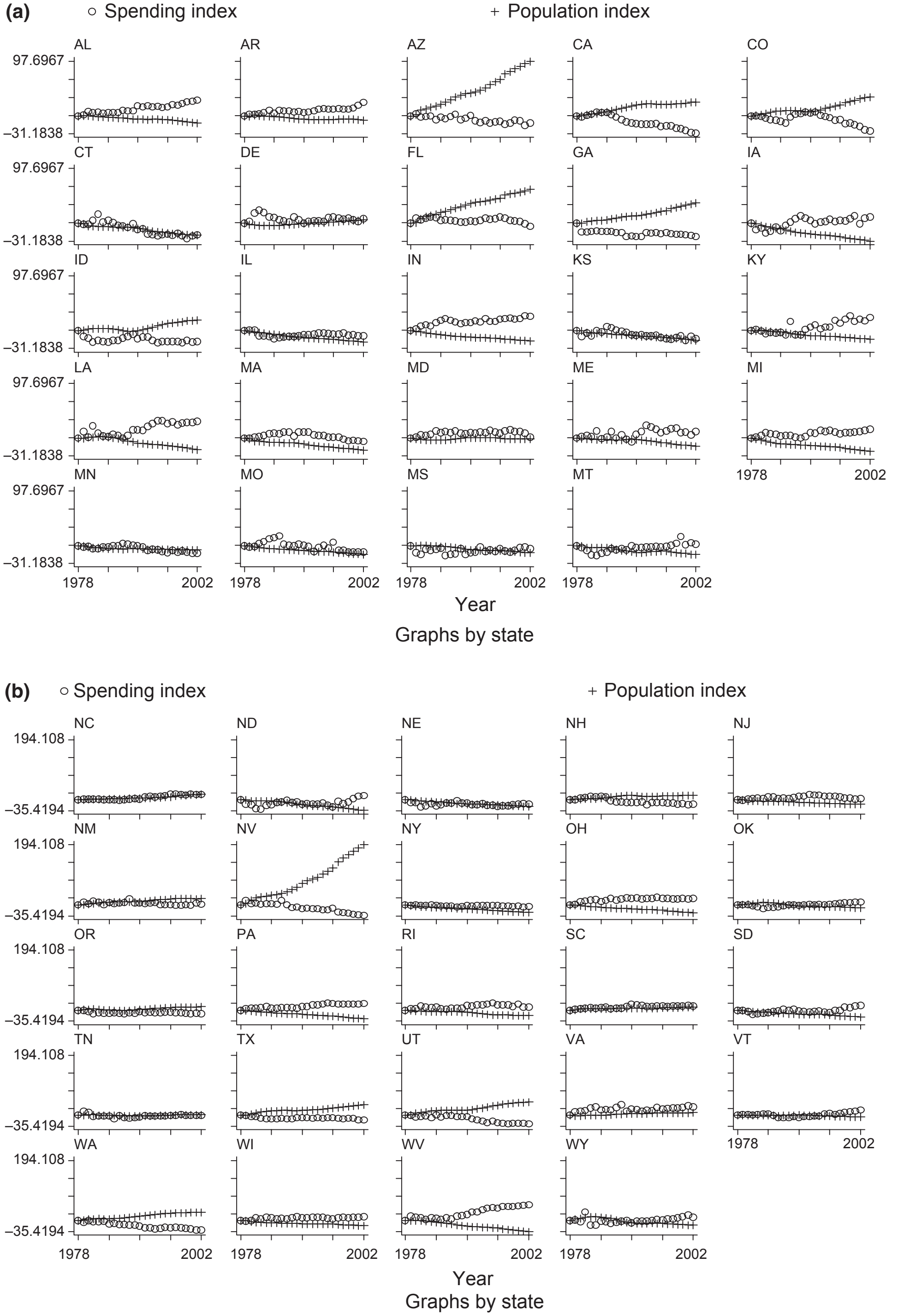

Figure 3. A State shares of population and state shares of federal spending B State shares of population and state shares of federal spending (1978=100).

fore, consists in isolating the role of different mechanisms of budgetary allocation by using different non-linear functional forms of population.

The pattern of *POPIND* for all states during the entire period is summarized in Figure 4. 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.

Going back to Figure 1, some small states, such as those in rural Midwest, seem to be advantaged in the allocation of federal spending if compared with populous states such as California, Texas, and Florida. For these states we also have an inverse relationship between federal spending and average *POPIND* in the period 1978–2002 (map C). These states conform to the claim of 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. A comparison of maps A and C, however, also reveals that population growth is often substantially different from population size. Moreover, it is not obvious that spending (map B) is related to size (map A) better than to growth (map C).

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 not 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 (25 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 Pennsylvania 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

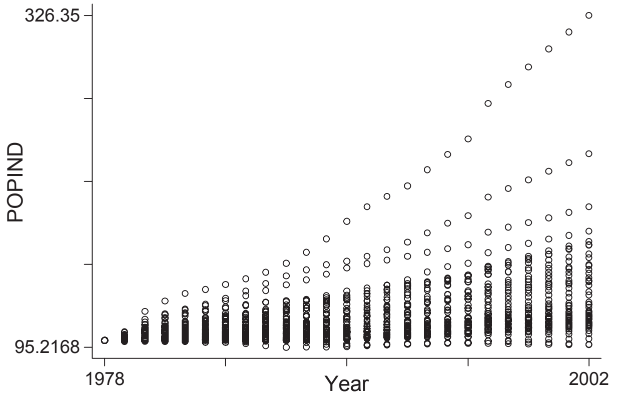

Figure 4. State Population Index (base year: 1978).

if compared with states like New York and Pennsylvania no less than if compared with the small and static states of the industrial Midwest. 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 the effect of over-representation). Returning to equation (1), the new specification becomes:

$$y_{st} = \alpha y_{st-1} + \beta * SP_{st} + \lambda N_{st} + \psi POPIND_{st} + \boldsymbol{\theta} Z_{st} + \gamma_s + \delta_t + \epsilon_{st}, \quad s = 1, \ldots 48; t = 1978, \ldots 2002; \tag{4}$$

The results reported in column 1 of Table 3 show that the scale-independent measure of population change is key to explain federal budget allocation to the states.[32] The coefficient of *POPIND* is negative and significant, implying that fast-growing states are penalized in the allocation of the federal budget.[33] On the other 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 1. 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.

Our main findings are robust to several specification changes summarized in Tables S3–S5 (see appendix).[34] An important robustness check is the introduction of an interaction term between *SP* and *POPIND*. This allows us to verify whether the effect of population change is in fact independent of the effect of population size. For example, small and large states could have different bargaining power when different population growth rates induce the renegotiation of budgetary allocations. The interaction term turns out to be statistically insignificant for federal spending and all other spending categories, except grants. On the other hand, *POPIND* remains negative and significant, whereas *SP* remains statistically insignificant. However, the interaction term for grants is positive and significant implying that the negative impact of population dynamics is reduced by the size of a state (Table S5).[35]

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 under-represented in the Senate) or small (and hence over-represented): this suggests that the budget fails to respond to population changes at an ade-

[32] An alternative estimation strategy consists of introducing state-specific trends, t_s, in our basic specification. Results in this case 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).

[33] A negative relationship between spending and population growth has also been found at counties' level by Ansolabehere et al. (2002).

[34] Table S3 in the online Appendix shows that our main results are not affected by the inclusion of further demographic and political variables such as population density and the closeness of presidential races. Table S4 also shows that the coefficient of POPIND remains positive and significant if we use a simpler functional form, excluding the lagged dependent variable and the non-linear population term. The same is true if we use lagged population terms, capturing per capita representation during the year in which the budget is appropriated rather the year in which funds are actually spent. In line with our previous findings, only the exclusion of state fixed effects drastically reduces the significance of our coefficients of interest.

[35] This result points in the direction of a "large state advantage", at least for what concerns the impact of population dynamics.

TABLE 3. CHANGE AND SCALE EFFECTS (OLS REGRESSIONS)

Dep. variable	(1) Federal spending	(2) Direct payments to individuals	(3) Grants	(4) Salaries	(5) Defense	(6) Federal spending except defense
Senators per capita	0.1803*	0.0016	−0.0121	0.0153	0.0573	0.1288
	(1.767)	(0.06)	(0.44)	(0.22)	(1.07)	(1.69)*
State population	−0.0225*	−0.0037	0.0020	−0.0122	−0.0139	−0.0042
	(−1.891)	(1.68)	(1.09)	(1.94)*	(2.14)**	(0.59)
Population index	−0.0021**	−0.0005	−0.0008	−0.0011	0.0006	−0.0037
	(−2.411)	(1.73)*	(2.79)***	(1.65)	(1.28)	(4.62)***
Income	−0.0420***	−0.0083	−0.0044	−0.0012	−0.0257	−0.0164
	(−3.267)	(2.78)***	(1.59)	(0.29)	(2.45)**	(2.04)**
Unemployment	0.0041	0.0064	0.0029	−0.0016	−0.0081	0.0141
	(0.723)	(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
	(2.003)	(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
	(−2.175)	(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
	(12.925)	(10.60)***	(17.76)***	(0.91)	(14.75)***	(10.54)***
Constant	1.7481***	0.5409	0.3898	0.7123	0.6211	2.1081
	(3.552)	(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	1,152	1,152	1,200	960	1,200	1,152
Overall R^2	0.9545	0.9768	0.9604	0.9660	0.9470	0.9539
Short-run marginal effect of	−.0360194	−.0037719	.0028852	−.0133107	−.0181566	0.0138
population at the mean	(2.10)**	(1.09)	(0.77)	(1.45)	(1.96)**	(1.17)
Long-run marginal effect of	−.0930315	−.0427245	.0099223	−.0139019	−.0598846	0.0275
population at the mean	(2.21)**	(1.01)	(0.78)	(1.46)	(2.02)**	(1.17)

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

quate 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 over-representation. Our analysis, however, 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 3 (column 1) imply that if in 1990 California had the same *POPIND* as Wyoming (106.7), everything else being equal, then 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 S6 (see appendix), we report the average gains and losses (in 1983 USD) implied by our estimates of the change effect reported in column (1) of Table 3. 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 U.S. 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 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, fast-growing states might be 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, as formulas can incorporate economies of scale and are, to a certain extent, manipulable too. The same can be said of public spending in salaries as 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. Somewhat surprising is the absence of any 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 2–6 of Table 3, show that for *grants*, *direct payments* to individuals, and *salaries*, introducing *POPIND* renders the coefficient of *SP* statistically insignificant (compare columns 2–4 of Table 3 with columns 5–7 in Table 2), 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 3.

On the other hand, for *defense* spending, 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.[36] *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 play no significant role in the defense equation. Finally, column 7 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.

As 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.[37] 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 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.[38] 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 4, 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 mar-

[36]The result we obtain using overall defense spending is likely to be driven by the geographic targetability of some important defense spending items, such as expenditures for employment and military bases Goss (1972), Mayer (1992), rather than by military procurement, which has been found to be less sensitive to political influence Mayer (1991).

[37]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".

[38]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.

TABLE 4. FORMULA VS. NON-FORMULA GRANTS FROM CFFR 1983–2002 (OLS REGRESSIONS)

Dep. variable	(1) All grants	(2) All grants	(3) Formula grants	(4) Non-formula	(5) Formula	(6) 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)
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)
Population index		−0.0010***			−0.0007***	−0.0003*
		(3.42)			(3.31)	(1.87)
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^2	0.9695	0.9702	0.9708	0.9248	0.9714	0.9254
Short-run marginal effect	−0.0084**	0.0157	−0.0075**	−0.0011	0.0015	0.004
of population at the mean	(2.06)	(1.01)	(2.44)	(0.43)	(0.48)	(0.93)
Long-run marginal effect	−0.0296**	0.005	−0.0274***	0.0026	0.005	0.0094
of population at the mean	(2.03)	(1.00)	(2.67)	(0.41)	(0.47)	(0.99)

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

ginal 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. Columns 5 and 6 show 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. This indicates that formulas play a very important role in explaining the limited responsiveness of grants to population dynamics, although the small effect estimated for non-formula programs suggests that other sources of inertia may also limit the adjustment of budgetary allocations to population dynamics.[39]

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, some studies point 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.

How should we interpret our findings? Focussing on the way budget allocations are actually determined, we can think of several mechanisms that could generate the distortion we uncover. First, reallocations of funds are limited by the lack of information available for the drafting of the yearly budget. For example, several programs rely on outdated census data to distribute funds across states.[40] Second, many programs are allocated by formulas that substantially reduce the responsiveness of the budget to population changes.[41]

Our evidence is consistent with these mechanisms of budgetary inertia, also highlighted by policy practitioners, and confirms the importance of formulas in the allocation of the budget. In theoretical terms, our results are compatible with theories claiming the existence of a substantial inertia in budgetary allocations. According to behavioral "incrementalist" theories of budgeting (Wildavsky, 1964), current spending is largely predetermined by past provisions because the drafting of the yearly budget is a very complex task that can only be tackled by incremental changes.[42] On the other hand, demand-side explanations of budgetary provisions stress that former allocations may have a strong impact on current ones because receivers of federal aid

[39]For example, states that in the past have been major receivers of federal aid may continue to secure large shares of federal funds (independently of their population dynamics) because of their experience with grant's applications and federal decision making – as pointed out by demand-side theories of budgetary provisions [Rich (1989)].

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

[41]A report issued by the U.S. 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).

[42]These theories stress the limited temporal, financial, and cognitive resources available in each year when re-examining the budget, which is then mostly determined by marginal changes to past budgetary allocations.

(i.e., state and local governments) accumulate experience in grants applications and may have better access to federal decision-makers (Rich, 1989). These theories provide plausible explanations of the role played by population dynamics in our regressions. An important question, however, still remains unanswered: how do legislators overcome the small-state advantage? In this case, more institutionally focused stories provide plausible interpretations of our results. Empirical evidence, for example, shows that whether a piece of legislation originates from the House or from the Senate does make a difference because the chamber enjoying proposal power is able to sway legislation in its favor (Strom and Rundquist, 1977). The importance of proposal power has been stressed by models of distributive politics showing how it provides an advantage in the so-called "divide-the-dollar" bargaining (Ansolabehere et al., 2003; Baron and Ferejohn, 1989; Cutrone and McCarty, 2006). In particular, Ansolabehere et al. (2003) show that, in bicameral legislatures where the lower chamber has proposal power, malapportionment in the upper chamber does not in general lead to maldistribution of public expenditures.[43] Thus, the fact that the U.S. constitution grants to the House proposal power on money bills provides one important rationale for the limited small-state advantage we uncover in our empirical investigation. Also, in legislative bargaining, targeted spending interacts with general redistributive programs and ideological considerations.[44] Hence, theoretical reasons to expect small-state advantage due to malapportionment are probably less compelling than what is usually believed.

6. CONCLUSIONS

In this study we have reconsidered the small-state advantage hypothesis by analyzing data on the allocation of the U.S. 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 programs. In this case it should not raise much concern as 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 over-representation 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 over-representation 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

[43]They show that malapportionment only matters in some special circumstances such as supermajority rules, Senate proposal power, and non-targetability of expenditure to electoral districts.

[44]See for example Huber and Ting (2009).

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 of the rather puzzling evidence accumulated by the abundant empirical literature on this issue.

Our analysis reveals that, once we disentangle 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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SUPPORTING INFORMATION

Additional Supporting Information may be found in the online version of this article:
Table S1. Average Population, Overrepresentation, and Spending in the Period 1978–2002
Table S2. Summary Statistics
Table S3. Change and Scale Effects (OLS regressions)
Table S4. Change and Scale Effects (OLS regressions)
Table S5. Change and Scale Effects (OLS regressions)
Table S6. Predicted Spending (outlays, real 1983 USD)