

The Effect of the Expansion of the Voting Franchise on the Size of Government

Thomas A. Husted

American University

Lawrence W. Kenny

University of Florida

This paper examines the claim that expansion of the voting franchise has been an important factor in the growth of government. State government spending and state and local spending are explained using a panel of 46 states for 1950–88. Elimination of poll taxes and literacy tests led to higher turnout, particularly among the poor, and a poorer pivotal voter. As predicted, we find that these changes, a fall in the income of voters relative to state income, and the ouster of Republicans from state government led to a sharp rise in welfare spending but no change in other spending.

I. Introduction

A complete explanation of the large growth and change in the composition of government spending over time has eluded economists working in public finance and in public choice. Meltzer and Richard (1978, 1981, 1983), echoing an earlier concern of Tocqueville ([1835] 1965), hypothesize that the growth is due in part to the

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expansion of the voting franchise, which added voters who were drawn mostly from the lower end of the income distribution. The new median voter was poorer, found redistribution to be more advantageous, and accordingly chose larger government. Their empirical analysis (Meltzer and Richard 1983), however, tested other implications of their model for government spending.

Of course, governments do more than process welfare checks. They also provide services (e.g., education, defense, libraries, roads, parks, and police) that benefit all citizens, and the effect of this shift in the median voter on the level of nonredistributive expenditures or government services is not as clear. These expenditures are cheaper to the poorer citizens in a community because their consumption is subsidized by the richer members of the community. Consequently, as the voting franchise expands to include more lower-income individuals, the lower price facing the new, poorer, median voter leads him or her to substitute away from private consumption toward government services. Accompanying this substitution effect, however, is an income effect in which the poorer median voter demands fewer government services.

Kenny (1978), extending work by Lovell (1975), shows that expenditures on government services rise only if the elasticity of substitution between government services and private goods exceeds the income elasticity for government services (or, equivalently, if the uncompensated price elasticity for government services exceeds the income elasticity). Most of the estimates of these parameters from empirical studies of the demand for state and local services imply that this condition is not met, and indeed the estimated income elasticities typically exceed the estimated price elasticities.¹ Thus the expansion of the voting franchise cannot explain the growth of spending on government services.

In summary, as the voting franchise is expanded to include more individuals from the bottom part of the income distribution, expenditures on pure redistribution are expected to increase. Conflicting income and substitution effects for the provision of nonredistributive expenditures, however, accompany the expansion of the voting franchise, and the estimated income and price elasticities suggest that allowing the poor to vote is not going to result in an increase in spending on government services.

¹ According to Fisher (1988, p. 294), the estimated income elasticity typically exceeds the estimated price elasticity for education, police, fire, parks, and recreation expenditure; for public works, there is substantial overlap in the estimates of the two elasticities.

The empirical evidence on the relationship between changes in the voting franchise and the growth in government expenditures is somewhat inconsistent. Peltzman (1980), in a cursory examination of the data, finds that total government spending did not surge following expansions of the voting franchise in Great Britain, Canada, and the United States. He concludes that the "data are too crude to rule out a connection between suffrage and the size of government . . . [but do] suggest that the major changes in the size of government have little to do with extension of the franchise" (p. 254). Voter participation in school budget referenda does not have a consistent impact on educational spending in Inman's (1978) study of 58 Long Island school districts. Mueller and Murrell (1986), in a cross-sectional study of 28 countries, find that countries with higher voter turnout had higher government spending, but Murrell (1985) uses a somewhat smaller sample of OECD countries and finds no significant relationship between voter turnout and the share of public employment in total employment.² Finally, the expansion of the voting franchise is unrelated to changes in the size of the governmental sector in the Brosio and Marchese (1988) examination of Italy between 1866 and 1914.

Despite the lack of statistical support and indeed the meager evidence to the contrary, the expansion of the voting franchise is often cited as an important factor in the growth of government over the last several centuries (see, e.g., Mueller's [1989, pp. 326–33] survey of public choice). Additional evidence clearly is necessary to ascertain whether the expansion of the voting franchise is responsible for the recent growth in government spending. Moreover, is the effect on nonredistributive expenditures different from that on redistributive spending, as theory implies?

In order to answer these questions, we examine state (and local) government spending in the United States using biennial data for 1950–88 (1958–88). This sample allows us to estimate the effects of the removal by the federal government of two major impediments to voter participation *in some states* as a result of the Voting Rights Acts: poll taxes and literacy tests. Removal of these barriers had profound effects on voter registration and turnout in these states. For example, the estimated percentage of voting-age blacks registered in the Old South states increased from 24.9 percent in 1956 to 62 percent in 1968, primarily as a result of the Voting Rights Act of

² These results may be different because of the use of different dependent variables. The number of government workers is more closely related to the amount of government services provided than is total government spending, which includes welfare spending.

1964.³ Filer, Kenny, and Morton (1991), in their study of county voter turnout in the United States in 1948, 1960, 1968, and 1980, show that literacy tests lowered voter turnout among the poorly educated and had the greatest impact on the turnout of blacks and that poll taxes sharply reduced voter turnout, particularly for those with low incomes. Thus we can conclude that exploring the effects of doing away with these devices provides evidence on how expanding the voting franchise to the poor affects the level and composition of government spending.

But this evidence is indirect, since expanding the franchise affects spending by raising voter turnout, particularly among the poor, and lowering the average income of those who are voting. We also provide direct evidence on the effects on spending caused by changes in the income of a state's voters relative to that of its population and whether state government is controlled by Republicans, primarily as representatives of the upper tail of the income distribution, or Democrats, as representatives of the lower tail.

Before we turn to the empirical analysis, we summarize how welfare spending is determined under the pure redistribution model of Meltzer and Richard (1978, 1981, 1983) as well as other major models of redistributive spending. We then review the government services model of Lovell (1975) and Kenny (1978), provide some background on poll taxes and literacy tests, and indicate how we measure their indirect impact on spending and the direct impact on spending of the income of voters relative to state income.

II. Review of Redistribution and Government Services Models

A. *Redistribution*

1. Pure Redistribution Model

In Meltzer and Richard's (1978, 1981, 1983) model of pure redistribution determination, individuals differ in their human capital. Each receives the same lump-sum payment, which is financed by taxes that are proportional to income. As the tax rate rises, fewer hours are worked and consequently there is a tax rate that maximizes tax revenue and the lump-sum payment. Those with the least human capital do not work and prefer this tax rate. For those who work, the preferred tax rate falls as human capital increases, until average human capital is reached. Those with at least average human capital lose from this redistributive activity and consequently prefer a zero

³ Some of the effects are summarized in Alt (1994).

tax rate. The tax rate is determined in this model by the median voter, and as the median voter moves down the community's income distribution, the preferred tax rate and redistributive expenditures rise. Furthermore, in Meltzer and Richard's (1983) Stone-Geary utility specification, welfare expenditures are likely to increase as mean income rises, with the ratio of the decisive voter's income to mean income held constant.⁴

Meltzer and Richard's simple model of pure redistribution can be criticized for being based on assumptions that do not accurately depict the existing tax/transfer programs. Transfer recipients in these programs have income well below the mean or median income levels. Since the median voter would not be a beneficiary in this system, an alternative theoretical framework is required to explain the level of redistribution expenditures. The theories most often advanced to explain welfare payments allow the median voter to receive some benefit from providing welfare for the poor or rely on interest group influence of the poor.

2. Median Voter Models Based on Altruism or on Social Insurance

Redistribution also can be obtained within a median voter framework by assuming that the median voter, who is not a welfare recipient, is motivated by altruism toward the poor (or by a concern over crime or social unrest). Altruism is usually modeled by adding transfer benefit levels, poverty population coverage, or the distribution of income to the median voter's utility function. In these pure altruism models, expenditure on redistribution rises as the median voter's income rises. Thus a fall in the income of the median voter due to enfranchising the poor should lower welfare spending.⁵ The empirical literature based on these pure altruism models uses the average or median income of the state rather than of the state's voters. Of course, this is not a problem if everyone votes or the composition of the voting population does not change. This literature has not considered the effect of changes in the decisive voter's income due to changes in the voting franchise.

An alternative framework allows redistribution to result from both altruism and self-interest. For example, Husted (1989) and Epple

⁴ The effect on the tax rate depends on a parameter of the Stone-Geary utility function and could be positive or negative; in the Cobb-Douglas case, the tax rate is invariant to changes in mean income. As mean income rises, this tax rate is applied to greater income, which results in higher welfare payments.

⁵ Empirical tests of this median voter theory by Orr (1976), Moffitt (1984), and Baumgardner (1993) provide mixed support for this framework.

and Romano (1996) recognize that the self-interest of potential or current welfare recipients will cause them to vote with the altruistic rich for higher welfare payments. Enfranchising the poor brings to the voting booth (1) welfare recipients, who favor a more generous system, and (2) other poor who are not potential welfare recipients and favor lower payments. In this altruism/self-interest model, the new median voter favors greater welfare payments, and redistribution increases if the new welfare recipient voters outnumber the other new poor voters.

Under another scenario, redistribution is valued by the self-interested median voter as social insurance against an unforeseen or temporary loss of income or employment. Because higher-income individuals have alternative safety nets (e.g., private savings or unemployment insurance) or are unlikely to suffer the degree of loss necessary to make them eligible for government redistribution benefits, a negative income elasticity is expected.⁶ Since it results in a new, poorer median voter, enfranchising the poor in this social insurance model is predicted to raise redistributive expenditures.

3. Special Interest Group Model

One can also explain welfare expenditure levels through a special interest group model, based on the theoretical models of Peltzman (1976) and Becker (1983). In this framework, vote-maximizing politicians balance the votes gained from the poor (and possibly welfare providers) against the votes lost from other groups because of higher taxes on all taxpayers for larger welfare benefits.⁷ The sign of the income elasticity in this model is unclear. Legislation that enfranchises more of the recipient population allows the poor to deliver more votes for favorable legislation without incurring any additional organizational cost and thus should result in greater welfare expenditures.

B. Government Services Model

A different theoretical structure is required to explain the demand for government services, which have direct value to all constituents.⁸

⁶ Husted (1989) finds some support for the presence of this social insurance demand in the support for higher welfare benefits by low-income taxpayers.

⁷ Plotnick (1986), Kristov, Lindert, and McClelland (1992), and Baumgardner (1993) test different versions of the political support maximization model in the context of redistribution expenditure.

⁸ The government services and pure redistribution approaches have been integrated in a very similar framework by Tridimas (1993), who considers the simultaneous determination of redistribution and nonredistribution expenditures. He relies on a Stone-Geary utility function to generate comparative statics in this model.

Lovell (1975), like Meltzer and Richard, assumes that taxes are proportional to income. An individual with income M_i is able to purchase C_i units of private goods:

$$M_i = C_i + t \cdot M_i, \quad (1)$$

where t is the tax rate. In this services model, taxes finance a level of government services (G) that is consumed by all in the jurisdiction. More specifically, the government budget constraint requires that total revenue equal total expenditure:

$$t \cdot M^c = P_G \cdot G \cdot N, \quad (2)$$

where total community income equals M^c , P_G represents the price of each unit of government services produced, and N equals the number of individuals in the community. Solving (2) for t , we get

$$t = P_G \cdot G \cdot \frac{N}{M^c} = P_G \cdot \frac{G}{M_m}, \quad (3)$$

where M_m is mean income. Substituting equation (3) into equation (1) yields

$$M_i = C_i + P_G \cdot \frac{M_i}{M_m} \cdot G.$$

The individual budget lines in a particular jurisdiction with mean income M_m all have the same government services intercept (M_m/P_G), and the relative price of government services is proportional to M_i/M_m , the ratio of the individual's income to mean income. Lovell uses specific utility functions to ascertain whether the level of government services preferred by a community's poorer citizens is greater than that preferred by its richer residents. As we move down a community's income distribution, the relative price of government services, $P_G \cdot (M_i/M_m)$, falls, resulting in a substitution toward more government services. On the other hand, the poorer citizen is worse off and demands fewer government services. On net, the poor demand more government services if the substitution effect dominates the income effect. Kenny (1978), generalizing Lovell's work, shows that this occurs when the elasticity of substitution between government services and private consumption is greater than the income elasticity for government services. Equivalently, the poor demand more government services if the uncompensated price elasticity for government services is greater than its income elasticity.⁹

⁹ Note that the uncompensated price elasticity is a weighted average of the income elasticity and the elasticity of substitution.

Thus an expansion of the voting franchise that adds voters from the lower end of the income distribution will result in greater spending on government services only if its price elasticity exceeds its income elasticity. As noted in the Introduction, most estimates of these two elasticities contradict this assumption.

III. Poll Taxes and Literacy Tests

Violence, the white primary, poll taxes, literacy tests, multiple ballot boxes, and other devices were employed over the years to restrict minorities and the poor from voting, particularly in the southern states. Over time a number of these practices were eliminated, and by the late 1950s, the remaining two major obstacles to full political participation were the poll tax and the literacy test.

A. *Poll Tax*

In the 30 years after the end of Reconstruction, all southern states imposed poll taxes as a requirement for voting. Florida, Georgia, Louisiana, and North Carolina repealed theirs by 1945, followed by South Carolina and Tennessee in 1951 and Arkansas in 1964. The Twenty-fourth Amendment to the U.S. Constitution, ratified in 1964, prohibited poll taxes in federal elections, and section 10 of the 1965 Voting Rights Act authorized the U.S. attorney general to challenge the constitutionality of the use of poll taxes in state and local actions. As a result, federal courts quickly struck down the remaining poll taxes in Alabama, Mississippi, Texas, and Virginia.

The poll tax raised the cost of voting, and Rusk and Stucker (1978), Filer et al. (1991), and Heckelman (1995) show that this had a sizable impact on voter turnout, lowering the fraction voting by between .13 and .32.¹⁰ The poll tax should have had its greatest impact on those least likely to vote—the poor. Lloyd (1952) and Ogden (1958) conclude from their studies of the poll tax movement that disenfranchising the poor was a significant part of the motivation for imposing this tax. Filer et al. (1991), using decennial county data for all 50 states pooled over four decades, estimate that the poll tax had a greater impact on turnout in poor counties than in wealthy counties. In regressions based on biennial state data for 46 states for 1950–88, we find some evidence that the poll tax raised our measure of the income of those voting relative to state income

¹⁰ Silver (1973) and Ashenfelter and Kelley (1975) find that an individual's probability of voting is lowered substantially by a poll tax.

$(\text{INCOME}_{\text{VOTER}}/\text{INCOME}_{\text{POP}})$.¹¹ In summary, the results cited above provide strong evidence that eliminating the poll tax results in higher turnout and a new, poorer, decisive voter. The variable POLL TAX in our empirical model equals one if the state has a poll tax and equals zero otherwise.

B. Literacy Test

Numerous studies have found that literacy tests lowered voter turnout, and the evidence suggests that their impact on turnout has fallen over time as illiteracy has become less prevalent.¹² These tests purportedly kept those with little schooling or command over English from voting. Filer et al. (1991) find that literacy tests had their greatest impact on turnout in poorly educated counties, where the fraction voting was estimated to drop by .02. On the other hand, there is considerable anecdotal evidence that voting registrars took advantage of the discretion given them by state laws and administered much more rigorous tests to blacks than to whites. For instance, in Macon County, Alabama, in the late 1950s at least 12 white applicants who had not finished elementary school "passed" the same literacy test that several black college graduates failed (see Hamilton 1973; Lawson 1976).

The 1965 Voting Rights Act targeted those states with the most

¹¹ The construction of this variable is described more fully in the following section. The results from a regression in which the dependent variable and all nondummy independent variables are in logarithms are (with *t*-statistics in parentheses)

$$\begin{aligned} & .082 \cdot \text{PERSONAL INCOME} - .062 \cdot \text{INCOME SPREAD} \\ & (3.26) \qquad \qquad \qquad (-2.27) \\ & - .0022 \cdot \% \text{BLACK} - .0025 \cdot \% \text{ELDERLY} + .0067 \cdot \% \text{METROPOLITAN} \\ & (-.28) \qquad \qquad \qquad (-.12) \qquad \qquad \qquad (.47) \\ & + .031 \cdot \text{LITERACY TEST} + .015 \cdot \text{POLL TAX}, \quad \text{adjusted } R^2 = .958. \\ & (3.42) \qquad \qquad \qquad (1.65) \end{aligned}$$

The results for the comparable regression in which no variable is logged are

$$\begin{aligned} & .55 \cdot 10^{-5} \cdot \text{PERSONAL INCOME} - .090 \cdot \text{INCOME SPREAD} \\ & (2.30) \qquad \qquad \qquad (-3.15) \\ & + .0013 \cdot \% \text{BLACK} + .0017 \cdot \% \text{ELDERLY} + .92 \cdot 10^{-3} \cdot \% \text{METROPOLITAN} \\ & (1.15) \qquad \qquad \qquad (.78) \qquad \qquad \qquad (1.98) \\ & + .030 \cdot \text{LITERACY TEST} + .0098 \cdot \text{POLL TAX}, \quad \text{adjusted } R^2 = .999. \\ & (3.37) \qquad \qquad \qquad (1.08) \end{aligned}$$

Both regressions are based on the full sample, used in tables 3 and 5 below, and also include state and year dummy variables.

¹² Rusk and Stucker (1978) and Heckelman (1995) estimate that these tests caused a .13-.16 drop in the turnout rate at the turn of the century. Filer et al. (1991) find a much smaller impact in the post-World War II era, which was diminishing over time (see also Silver 1973; Ashenfelter and Kelley 1975).

notorious literacy tests. Specifically, it provided for direct federal action in “covered jurisdictions” to prohibit the use of the literacy test; a covered jurisdiction was defined to be a state, county, parish, or town that used a test or device (e.g., a literacy test) and had less than a 50 percent turnout in the 1964 presidential election. Georgia, Louisiana, Mississippi, South Carolina, Virginia, 40 (out of 100) counties in North Carolina, Apache County in Arizona, and Honolulu County in Hawaii attracted federal scrutiny because of their literacy tests and low turnout. The 1970 Voting Rights Act suspended literacy tests and extended this federal attention to eight more counties in Arizona, two counties in California, Elmore County in Idaho, three large counties in New York (Bronx, King’s, and New York), Wheeler County in Oregon, Campbell County in Wyoming, and various towns in Connecticut, Massachusetts, and New Hampshire.

The literacy test variable that we construct captures the effect of those literacy tests that the federal government designated as being particularly onerous on voting rights. LITERACY TEST equals the fraction of the 1970 population covered under either the 1965 or 1970 Voting Rights provisions in those years the covered state had a literacy test. LITERACY TEST equals zero after a test was eliminated in covered states and always equals zero in uncovered states, even if they had literacy tests. In the regressions described above, we find that this literacy test measure raises $INCOME_{VOTER}/INCOME_{POP}$ (see n. 11).

IV. Measuring the Direct Impact of Relative Voter Income

It is clear from the discussion above that literacy tests and poll taxes were eliminated forcefully in the 1960s through federal intervention. This expansion of the voting franchise does not reflect a decision made by state government. Their removal led to increased voter turnout, particularly among the state’s poorer residents, and a new poorer decisive voter.

There are two potential data sources for estimating the income of those voting relative to the income of the general population. Individual surveys conducted by the Survey Research Center have too few respondents from many states to provide reliable estimates of relative voting income, and the income data in the early years are not sufficiently detailed to be very useful. Instead, we utilize turnout, population, and median family income data at the county level. The ratio $INCOME_{VOTER}/INCOME_{POP}$ equals a state’s average county median family income, weighted by the number voting in the county, divided by average county median family income, weighted by the

county's voting-age population. Voter turnout comes from the election that year, and the voting-age population and median family income are interpolated from adjacent censuses, if necessary. Since there are nearly 3,100 counties in the United States, to create this variable we tediously processed over 60,000 turnout statistics from numerous files compiled by the Inter-university Consortium for Political and Social Research for the 20 election years we examine.¹³ An increase in turnout among the poor should be accompanied by an increase in turnout in poor counties, resulting in a fall in $\text{INCOME}_{\text{VOTER}}/\text{INCOME}_{\text{POP}}$.

V. Empirical Analysis

A. Data

We estimate the relationship between the location of the pivotal voter on the income distribution and the size and composition of government spending using a sample of biennial data from 1950 to 1988 for 46 states. To subject our theory to the fullest test, we examine the determinants of both state expenditures and state and local expenditures. The data on state spending begin in 1950, the first date for which comparable state data are available.¹⁴ The state and local data series begins in 1958. State-level data are used because most differences in redistributive policies occur at the state level. As in standard practice, Alaska was eliminated because its oil revenue has resulted in government spending that is much greater than that in any other state. Data for the 1950s were unavailable for Alaska and Hawaii, and they are excluded. Our sample furthermore omits Minnesota and Nebraska, since elections for the state legislature were nonpartisan in Minnesota through 1972 and in Nebraska during the entire time period, which precludes estimating state government party effects on spending.

B. Empirical Model

Our theory predicts that expanding the voting franchise results in a poorer decisive voter, which increases expenditures on pure redistribution but, because of conflicting income and substitution effects,

¹³ In years with a presidential election, turnout in that election is used. In other years, we relied on turnout for a governor's race, a Senate race, or House races, in descending order of preference.

¹⁴ The classification structure used by the census for governmental finances was revised extensively in 1951, and in that year revised data for 1950 were reported in an appendix in *State Government Finances*.

TABLE 1
VARIABLE CHARACTERISTICS, 1950-88

	Mean	Standard Deviation	Minimum	Maximum
State expenditures:				
WELFARE*	140	83	20	531
WELFARE/GENERAL	.150	.058	.038	.370
NONWELFARE*	780	302	221	2,291
State and local expenditures (1958-88):				
WELFARE*	163	84	29	563
WELFARE/GENERAL	.098	.035	.030	.196
NONWELFARE*	1,460	389	601	3,608
FEDERAL GRANTS*	241	122	31	755
PERSONAL INCOME*	8,846	2,688	3,039	18,989
INCOME SPREAD	.905	.141	.543	1.581
PERCENT BLACK	9.6	10.0	.1	45.3
PERCENT ELDERLY	10.0	2.1	4.9	18.1
PERCENT METROPOLITAN	59.8	24.0	12.1	100.0
DEMOCRATIC CONTROL	.211	.738	-1	1
POLL TAX	.045	.206	0	1
LITERACY TEST	.057	.211	0	1
INCOME _{VOTER} /INCOME _{POP}	1.016	.184	.616	1.577

SOURCE.—State expenditures, FEDERAL GRANTS, and population: *State Government Finances*; state and local expenditures: *Government Finances*; PERSONAL INCOME: *State Government Finances* and *Survey of Current Business*; INCOME SPREAD, PERCENT BLACK, and PERCENT ELDERLY: various censuses; PERCENT METROPOLITAN: U.S. Bureau of the Census and *State and Metropolitan Area Data Bank*; DEMOCRATIC CONTROL, POLL TAX, and LITERACY TEST: *Book of the States*; INCOME_{VOTER}/INCOME_{POP}: defined in the text.

NOTE.—The income, expenditure, and grant variables are in 1982 dollars.

* Per capita.

has an uncertain effect on nonredistribution or government services expenditures. To maintain the distinction between expenditures on pure redistributive spending and those on services, government spending is measured by real (in 1982 dollars) per capita welfare expenditures (WELFARE), the fraction of general spending allocated to welfare (WELFARE/GENERAL), and real per capita non-welfare general expenditures (NONWELFARE).¹⁵

Descriptive statistics and sources for these and our other variables are found in table 1. For 1958-88, state welfare expenditures (unreported mean is 155) on average accounted for 95 percent of state and local welfare spending (mean 163). The path of state welfare expenditures over time is further described in table 2. Between 1950 and 1964, per capita welfare spending fell 17 percent and then re-

¹⁵ All government expenditure variables are deflated using the state and local government expenditure deflator. Welfare expenditures include Old Age Assistance, Aid to Families with Dependent Children (AFDC), Aid to the Blind, Aid to the Disabled, other welfare expenditures, and expenditures for the operation of public and private welfare institutions and agencies.

TABLE 2
STATE SPENDING CHARACTERISTICS OVER TIME

	WELFARE			WELFARE/GENERAL			NONWELFARE		
	Mean (Standard Deviation)	Maximum (Minimum)	Mean (Standard Deviation)	Maximum (Minimum)	Mean (Standard Deviation)	Maximum (Minimum)	Mean (Standard Deviation)	Maximum (Minimum)	
1950	90.728 (50.220)	253.424 (20.052)	.177 (.074)	.370 (.044)	428.396 (149.276)	924.726 (221.579)			
1952	81.254 (41.240)	201.860 (19.646)	.167 (.069)	.361 (.047)	412.595 (123.369)	828.968 (221.422)			
1954	81.520 (39.024)	200.645 (23.684)	.159 (.065)	.349 (.058)	438.353 (117.757)	752.134 (247.316)			
1956	75.548 (36.183)	195.727 (22.124)	.138 (.057)	.314 (.055)	474.769 (115.335)	784.574 (285.782)			
1958	76.991 (37.678)	195.491 (22.033)	.125 (.051)	.273 (.046)	549.594 (167.888)	1,050.410 (290.826)			
1960	86.003 (39.420)	218.222 (26.052)	.132 (.054)	.293 (.047)	573.646 (157.586)	1,102.040 (303.049)			
1962	88.037 (37.656)	214.455 (29.903)	.131 (.051)	.282 (.045)	594.517 (148.426)	1,012.920 (372.159)			
1964	91.343 (38.950)	227.415 (27.566)	.123 (.046)	.240 (.041)	667.372 (185.530)	1,242.700 (370.369)			
1966	97.827 (31.786)	230.257 (31.377)	.118 (.044)	.238 (.038)	753.800 (210.713)	1,511.320 (393.949)			

1968	114.974	269.747	.123	.250	828.761	1,486.900
	(49,386)	(35,598)	(.044)	(.042)	(206.157)	(513.017)
1970	139.756	331.010	.140	.311	865.344	1,437.200
	(61,496)	(47,889)	(.053)	(.051)	(192.608)	(570.630)
1972	173.222	367.603	.160	.315	911.114	1,450.320
	(71,230)	(78,081)	(.057)	(.059)	(193.807)	(607.209)
1974	166.980	360.553	.155	.288	902.806	1,384.280
	(71,813)	(69,299)	(.055)	(.064)	(165.949)	(628.956)
1976	186.078	379.700	.159	.291	988.428	1,629.140
	(77,870)	(61,975)	(.058)	(.051)	(196.620)	(667.567)
1978	192.547	393.996	.164	.291	982.092	1,413.370
	(79,208)	(64,576)	(.058)	(.057)	(170.993)	(654.124)
1980	194.226	381.492	.164	.281	990.058	1,670.650
	(78,176)	(64,174)	(.055)	(.059)	(195.256)	(697.144)
1982	199.665	407.975	.171	.285	977.198	1,838.850
	(82,797)	(61,949)	(.060)	(.060)	(236.125)	(642.463)
1984	205.682	451.819	.170	.302	1,014.320	2,111.670
	(84,173)	(105,062)	(.057)	(.063)	(259,142)	(691,922)
1986	219.254	501.959	.164	.291	1,117.710	2,291.070
	(89,633)	(111,783)	(.053)	(.069)	(262,012)	(781,294)
1988	229.577	530.680	.167	.287	1,135.660	2,052.610
	(89,989)	(113,708)	(.048)	(.067)	(245,751)	(818,675)

turned to its original level. Welfare expenditure doubled between 1964 and 1976 and then rose another 23 percent over the next 12 years. Data on the allocation of welfare spending on different welfare programs are unavailable at the state level. National statistics indicate that the share of state welfare spending going to categorical cash assistance programs, including Old Age Assistance, AFDC, Aid to the Blind, and Aid to the Disabled, has fallen steadily over time. Information on public aid payments going to specific programs is available only for total federal, state, and local spending. Per capita spending on Old Age Assistance fell from \$39 in 1950 to \$11 in 1988. Spending on the disabled, most of which does not show up in state welfare spending, increased from almost nothing in 1950 to \$8 in 1964, \$27 in 1976, and \$34 in 1988.¹⁶ Per capita spending on AFDC rose from \$15 in 1950 to \$26 in 1964 and then sharply increased to \$79 in 1976 before dropping back down to \$56 in 1988.

The set of independent variables used in this study is more extensive than that found in most pooled cross-sectional, time-series expenditure determinant studies (see, e.g., Holtz-Eakin 1988; Husted 1989; Elder 1992; Cutler, Elmendorf, and Zeckhauser 1993; Gilligan and Matsusaka 1995). To capture income effects, we include real federal grants per capita (FEDERAL GRANTS) and real personal income per capita (PERSONAL INCOME).¹⁷

In this study, it is important to take account of the number of potential welfare recipients, and three variables taken from census data are used to measure the number of poor families in each state. INCOME SPREAD equals the difference between the first- and third-quartile family incomes, divided by median family income. The first- and third-quartile incomes are estimated through interpolation from census tables giving the income distribution. The other two variables measure the percentage of the state's population who are black (PERCENT BLACK) and who are aged 65 or older (PERCENT ELDERLY). Values for all three variables in the intercensus years are estimated by interpolating between the census years values, which necessarily produces some measurement error.

Workers in urban areas receive higher wages to compensate for the higher cost of living, which makes government services more expensive in urban areas. Since the price elasticity appears to be less than one, this results in higher government expenditures on services in large urban areas. PERCENT METROPOLITAN equals the per-

¹⁶ Since 1974, spending on the disabled arises almost exclusively through the federal Social Security Income program. Roughly 20 percent of spending on this program is part of state welfare spending, under intergovernmental expenditure.

¹⁷ Personal income is deflated using the consumer price index.

centage of the population living in standard metropolitan statistical areas.

There is considerable evidence that electoral competition is better characterized by candidates offering divergent platforms than by a Downsian solution in which candidates' platforms converge to the position preferred by the median voter.¹⁸ Even though elected officials from different parties often vote differently, few studies have found that their political affiliation affects the spending levels they choose.¹⁹ We measure party effects by DEMOCRATIC CONTROL, which equals one if Democrats control both legislative houses and the governor's mansion, negative one if Republicans control the legislative and executive branches, and zero otherwise.²⁰ Because Democrats and Republicans appear to represent the lower and upper tails of the income distribution, respectively, DEMOCRATIC CONTROL should have a positive coefficient if preferred spending rises as income falls.

We also include state and year dummy variables to control for otherwise unmeasured or omitted state-specific and year-specific fixed effects. State dummies capture time-invariant cross-sectional differences in amenities and "tastes" for government programs as well as institutional structure. The period dummy variables pick up changes over time in the relative price of government services, the federal government's state grants-in-aid programs, national business cycle conditions, and nationwide tastes for redistribution programs.

C. Results

Four sets of ordinary least squares regressions are estimated for the three dependent variables measuring purely redistributive and ser-

¹⁸ There are a number of theoretical models, reviewed in Schmidt, Kenny, and Morton (1996), that predict that electoral competition results in divergent platforms. In the empirical literature, Bullock and Brady (1983), Peltzman (1984), Wright (1989), Poole and Rosenthal (1991), and Kenny and Morton (1993) show that senators from the same state but different parties have markedly different voting records; Jung, Kenny, and Lott (1994) conclude that a state's two senators often draw support from different segments of the population. Schmidt et al. (1996) find that a senator's success in being reelected is better explained by how far his voting record has been from that of the typical senator in his party and state than by its distance from a median position in the state.

¹⁹ This literature is reviewed in Dye (1984). More recently in pooled time-series, cross-sectional studies, Blais, Blake, and Dion (1993) and Gilligan and Matsusaka (1995) find some evidence that spending levels are affected by which party controls government.

²⁰ If in one legislative body the two parties held an equal number of seats, control of the legislature was assigned to the majority party in the other legislative body. Note also that *F*-tests accept the restriction, implicit in DEMOCRATIC CONTROL, that Republican control of the legislative and executive branches has an effect that is equal and opposite to that of Democratic control of state government.

vice state spending; another four sets explain state and local spending. Half are based on the log-log specification, which uses logarithms of the dependent variables and the nondummy independent variables. The other half use a linear specification in which no variable is logged. These results are reported in tables 3–6. The indirect effects of the voting franchise on spending are captured in tables 3 and 4 using the poll tax and literacy test variables. The direct effects of the income of voters relative to state income are estimated in tables 5–7; additional estimates of relative income effects for three subsamples are reported in table 7. Overall, the coefficient estimates reported in these tables provide strong support for the hypotheses from our theoretical models, and the goodness-of-fit measures for each of the regressions are quite good. Tests suggested by Belsley, Kuh, and Welsch (1980) show no evidence of multicollinearity.

The political control, literacy test, poll tax, and relative income variables all capture the effects of shifts in the pivotal voter's position on the income distribution. DEMOCRATIC CONTROL, it will be recalled, equals one (negative one) if the Democrats (Republicans) control both the legislative and executive bodies and equals zero if control of state government is shared by the two parties. Since Democrats are more likely to represent the lower half of a state's income distribution, power will shift to a state's poorer voters when Democrats take control from Republicans. Similarly, as shown above, removal of a poll tax or a literacy test brings about an increase in voter turnout, particularly in the lower part of the income distribution, which results in a new, poorer, decisive voter.

1. Effects of Shifting the Pivotal Voter on Welfare Spending

The pure redistribution models of Meltzer and Richard (1978, 1981, 1983) and Tridimas (1993) and the social insurance and special interest group models of welfare spending predict that moving the decisive voter down the income distribution will result in more redistributive spending. The altruism/self-interest model predicts that this will lead to more redistributive spending if the new voters are mostly welfare recipients. Only the pure altruism model predicts that a poorer decisive voter results in smaller welfare expenditures. The positive coefficients of DEMOCRATIC CONTROL and the negative coefficients on $\text{INCOME}_{\text{VOTER}}/\text{INCOME}_{\text{POP}}$ and POLL TAX in the welfare spending per capita and in the share of welfare spending in state spending regressions strongly support the pure redistribution, social insurance, and special interest group models; are consistent with the altruism/self-interest model (under the conditions stated

TABLE 3

STATE SPENDING REGRESSIONS: POLL TAX AND LITERACY TEST EFFECTS ($N = 920$)

	LOC-LOG MODELS			LINEAR MODELS		
	WELFARE	WELFARE/GENERAL NONWELFARE	WELFARE/GENERAL NONWELFARE	WELFARE	WELFARE/GENERAL NONWELFARE	WELFARE/GENERAL NONWELFARE
FEDERAL GRANTS	.957 (27.5)	.572 (16.5)	.268 (15.4)	.464 (18.2)	.24 · 10 ⁻³ (9.50)	1.143 (18.9)
PERSONAL INCOME	-.080 (-.62)	-.395 (-3.11)	.375 (5.87)	.0089 (4.33)	.93 · 10 ⁻⁶ (.47)	.020 (4.18)
INCOME SPREAD	.372 (2.61)	.332 (2.35)	-.086 (-.62)	110.358 (4.49)	.094 (.391)	-30.109 (-.52)
PERCENT BLACK	.138 (3.29)	.123 (2.95)	-.0090 (-.43)	7.375 (7.50)	.0054 (.562)	.418 (.18)
PERCENT ELDERLY	.132 (1.18)	.086 (.77)	-.041 (-.73)	6.338 (3.45)	.0069 (.384)	2.538 (.58)
PERCENT METROPOLITAN	.122 (1.61)	.081 (1.08)	.012 (.33)	-1.933 (-4.74)	-.29 · 10 ⁻³ (-.73)	2.526 (2.61)
DEMOCRATIC CONTROL	.026 (1.91)	.022 (1.60)	-.0038 (-.55)	8.465 (3.79)	.0076 (.351)	1.099 (.21)
LITERACY TEST	.0031 (.07)	-.052 (-1.14)	.061 (2.64)	-17.530 (-2.29)	-.017 (-.229)	14.011 (.77)
POLL TAX	-.149 (-3.24)	-.118 (-2.56)	-.0097 (-.42)	-27.385 (-3.51)	-.027 (-.3.58)	30.959 (1.67)
Adjusted R^2	.9985	.9914	.9998	.9630	.9645	.9922
Root mean square error	.1876	.1867	.0938	31.239	.0304	74.115

NOTE.—State and year coefficients are not reported. The data pertain to 1950–88 for 46 states (Alaska, Hawaii, Minnesota, and Nebraska are excluded). t -statistics are in parentheses.

TABLE 4

STATE AND LOCAL SPENDING REGRESSIONS: POLL TAX AND LITERACY TEST EFFECTS ($N = 736$)

	Loc-Log Models				LINEAR MODELS			
	WELFARE	WELFARE./GENERAL	NONWELFARE	WELFARE	WELFARE	WELFARE./GENERAL	NONWELFARE	NONWELFARE
FEDERAL GRANTS	.745 (21.4)	.488 (14.7)	.194 (15.8)	.519 (19.4)	.18 · 10 ⁻³ (12.3)	1.054 (11.0)		
PERSONAL INCOME	-.214 (-1.74)	-.424 (-3.61)	.246 (5.68)	.0042 (2.10)	-.14 · 10 ⁻⁶ (-.13)	.026 (3.60)		
INCOME SPREAD	.529 (3.92)	.709 (5.48)	-.275 (-5.75)	209.346 (7.58)	.107 (6.97)	-131.259 (-1.33)		
PERCENT BLACK	.154 (3.20)	.261 (5.67)	-.135 (-7.94)	5.697 (5.26)	.0027 (4.57)	2.671 (.69)		
PERCENT ELDERLY	.235 (1.99)	.216 (1.90)	-.0079 (-.19)	6.798 (3.17)	.0037 (3.09)	-4.623 (-.60)		
PERCENT METROPOLITAN	.071 (.96)	-.011 (-.15)	.083 (3.18)	-.966 (-2.22)	-.18 · 10 ⁻³ (-.73)	2.571 (1.66)		
DEMOCRATIC CONTROL	.012 (.98)	.0090 (.79)	-.51 · 10 ⁻³ (-.12)	6.351 (2.89)	.0030 (2.46)	.873 (.11)		
LITERACY TEST	.0069 (.15)	.90 · 10 ⁻³ (.02)	.0022 (.14)	-5.706 (-.70)	.0028 (.63)	-47.438 (-1.62)		
POLL TAX	-.109 (-2.28)	-.136 (-2.97)	.039 (2.31)	-22.590 (-2.53)	-.013 (-.64)	45.490 (1.43)		
Adjusted R^2	.9991	.9964	.9999	.9774	.9786	.9957		
Root mean square error	.1501	.1438	.0531	27.545	.0152	98.557		

NOTE.—State and year coefficients are not reported. The data pertain to 1958–88 for 46 states (Alaska, Hawaii, Minnesota, and Nebraska are excluded). t -statistics are in parentheses.

TABLE 5
STATE SPENDING REGRESSIONS: RELATIVE INCOME EFFECTS ($N = 920$)

	LOG-LOG MODELS				LINEAR MODELS			
	WELFARE	WELFARE/GENERAL	NONWELFARE	WELFARE	WELFARE	WELFARE/GENERAL	NONWELFARE	NONWELFARE
FEDERAL GRANTS	.967 (28.0)	.584 (17.0)	.263 (15.3)	.470 (18.4)	$24 \cdot 10^{-3}$ (9.69)		1.132 (18.8)	
PERSONAL INCOME	-.010 (-.08)	-.321 (-2.55)	.358 (5.66)	.010 (4.98)	$22 \cdot 10^{-3}$ (1.08)		.020 (4.13)	
INCOME SPREAD	.248 (1.81)	.210 (1.54)	-.065 (-.96)	65.568 (2.85)	.051 (2.25)		2.876 (.05)	
PERCENT BLACK	.113 (2.79)	.094 (2.33)	$41 \cdot 10^{-4}$ (.00)	6.659 (6.87)	.0046 (4.89)		1.584 (.69)	
PERCENT ELDERLY	.113 (1.01)	.073 (.66)	-.045 (-.80)	6.008 (3.25)	.0065 (3.61)		3.343 (.77)	
PERCENT METROPOLITAN	.124 (1.65)	.095 (1.26)	$-13 \cdot 10^{-3}$ (-.00)	-1.827 (-4.50)	$-20 \cdot 10^{-3}$ (-.51)		2.604 (2.72)	
DEMOCRATIC CONTROL	.027 (1.97)	.022 (1.63)	-.0034 (-.50)	8.311 (3.71)	.0074 (3.40)		1.693 (.32)	
INCOME _{VOTER} /INCOME _{POP}	-.290 (-1.67)	-.182 (-1.05)	-.061 (-.70)	-82.641 (-2.80)	-.059 (-2.05)		-133.014 (-1.91)	
Adjusted R^2	.9985	.9913	.9998	.9625	.9639		.9922	
Root mean square error	.1884	.1874	.0941	31.424	.0307		74.070	

NOTE.—State and year coefficients are not reported. The data pertain to 1950-88 for 46 states (Alaska, Hawaii, Minnesota, and Nebraska are excluded). t -statistics are in parentheses.

TABLE 6

STATE AND LOCAL SPENDING REGRESSIONS: RELATIVE INCOME EFFECTS ($N = 736$)

	LOG-LOG MODELS			LINEAR MODELS		
	WELFARE	WELFARE/GENERAL	NONWELFARE	WELFARE	WELFARE/GENERAL	NONWELFARE
FEDERAL GRANTS	.744 (21.6)	.494 (14.9)	.188 (15.4)	.520 (19.6)	$.18 \cdot 10^{-3}$ (12.4)	1.039 (11.0)
PERSONAL INCOME	-.156 (-1.30)	-.370 (-3.20)	.247 (5.82)	.0051 (2.59)	$.15 \cdot 10^{-6}$ (.14)	.028 (3.98)
INCOME SPREAD	.446 (3.44)	.605 (4.85)	-.246 (-5.37)	177.510 (6.95)	.094 (6.60)	-133.498 (-1.47)
PERCENT BLACK	.141 (2.99)	.242 (5.33)	-.128 (-7.68)	5.482 (5.13)	.0026 (4.44)	3.582 (.94)
PERCENT ELDERLY	.223 (1.90)	.202 (1.79)	-.0041 (-1.10)	6.771 (3.17)	.0036 (2.99)	-2.987 (-.39)
PERCENT METROPOLITAN	.076 (1.05)	-.0023 (-1.79)	.080 (3.10)	-.839 (-1.97)	$-.18 \cdot 10^{-3}$ (2.99)	3.380 (2.23)
DEMOCRATIC CONTROL	.013 (1.10)	.0099 (.85)	$.56 \cdot 10^{-4}$ (.01)	6.643 (3.03)	.0031 (2.56)	2.071 (.27)
INCOME _{VOTER} /INCOME _{POP}	-.399 (-2.54)	-.214 (-1.41)	-.161 (-2.90)	-99.478 (-3.52)	-.033 (-2.10)	-400.413 (-3.98)
Adjusted R^2	.9991	.9964	.9999	.9776	.9785	.9958
Root mean square error	.1498	.1445	.0530	27.428	.0153	97.630

NOTE.—State and year coefficients are not reported. The data pertain to 1958–88 for 46 states (Alaska, Hawaii, Minnesota, and Nebraska are excluded). t -statistics are in parentheses.

TABLE 7
 INCOME_{VOTER}/INCOME_{POP} RESULTS FOR SELECTED SUBSAMPLES

	Presidential Elections	South	Non-South
State welfare	<i>N</i> = 460	<i>N</i> = 320	<i>N</i> = 600
Log-log model	-.326 (-1.26)	-.366 (-1.32)	-.248 (-1.12)
Linear model	-15.197 (-.35)	-74.891 (-2.31)	-95.884 (-2.52)
State and local welfare	<i>N</i> = 368	<i>N</i> = 256	<i>N</i> = 480
Log-log model	-.454 (-1.66)	-.429 (-1.46)	-.433 (-2.25)
Linear model	-98.671 (-2.29)	-93.959 (-2.54)	-104.585 (-2.88)
State (WELFARE/GENERAL)	<i>N</i> = 460	<i>N</i> = 320	<i>N</i> = 600
Log-log model	-.063 (-.24)	-.295 (-1.06)	.066 (.30)
Linear model	$-.91 \cdot 10^{-3}$ (-.02)	-.053 (-1.29)	-.047 (-1.25)
State and local (WELFARE/GENERAL)	<i>N</i> = 368	<i>N</i> = 256	<i>N</i> = 480
Log-log model	-.011 (-.04)	-.316 (-1.06)	-.146 (-.80)
Linear model	-.0089 (-.37)	-.047 (-1.66)	-.028 (-1.49)

above); and refute the pure altruism model. Only half the coefficients reported for LITERACY TEST, however, are negative.

In the welfare spending regressions in tables 3-6, LITERACY TEST, POLL TAX, and INCOME_{VOTER}/INCOME_{POP} each have four coefficients. Since the DEMOCRATIC CONTROL results in tables 3 and 4 and in tables 5 and 6 are mostly redundant, its results are summarized using its four coefficients from tables 5 and 6. With a two-tailed test, 11 of these 16 coefficients are significant at the 5 percent level and one is significant at the 10 percent level. DEMOCRATIC CONTROL, although significant in the other three specifications, is insignificant in the state log-log model, and only one of the four LITERACY TEST coefficients is significant (and negative). Since poll taxes have been found to have a much stronger impact on turnout than literacy tests, it is not surprising that welfare spending is affected much more by poll taxes than by literacy tests.

Several additional regressions have been estimated to ascertain whether these results are a statistical artifact.²¹ The relative income measure (INCOME_{VOTER}/INCOME_{POP}) was created using voter turnout, which is higher in presidential elections than in other elections.

²¹ Our use of state and time fixed effects makes this unlikely.

Regressions comparable to those in tables 5 and 6 but restricted to presidential election years were estimated to make sure the relative income results were not due to the use of turnout from different types of elections. The results for $\text{INCOME}_{\text{VOTER}}/\text{INCOME}_{\text{POP}}$ are reported in column 1 of table 7. In the state and local regressions, the variable is significant at the 5 percent level in the linear specification and at the 10 percent level in the log-log specification; the variable is not, however, significant in the state regressions. To determine whether the relative income results are attributable to otherwise unmeasured regional effects, we divided the full sample into two regions and estimated separate regressions for each region. Results for the South and non-South are reported in columns 2 and 3, respectively, in table 7.²² It is clear that this is not merely a southern phenomenon. In each region, there is strong evidence that welfare spending rises as the pivotal voter becomes poorer.

Moving down the income distribution to a new poorer decisive voter has a large effect on welfare spending. The coefficients imply that eliminating poll taxes raised welfare spending by 11–20 percent and eliminating literacy tests, when significant, brought about a 13 percent increase in welfare spending. In addition, a fall in $\text{INCOME}_{\text{VOTER}}/\text{INCOME}_{\text{POP}}$ of .2 would result in a 5–12 percent rise in welfare spending, and the ouster of Republicans from state government by Democrats is estimated to lead to 5–12 percent higher welfare spending.

The results are somewhat less significant when the dependent variable is the share of welfare spending in total spending. Again when a two-tailed test is used, nine of the 16 coefficients in tables 3–6 are significant at the 5 percent level. The $\text{INCOME}_{\text{VOTER}}/\text{INCOME}_{\text{POP}}$ coefficients, significant in the linear but not in the log-log models, are not as significant as they were for welfare spending. This is also the case in the regressions represented in table 7. The absolute t -statistics on the coefficients in the linear model are at least 1.25 for each region, but only one is significant at the 10 percent level. None of the coefficients in the log-log regional regressions or in the presidential elections regressions is significant.

Thus, with several very different indirect and direct measures of the location of the pivotal voter on the income distribution, we find strong support for the prediction that welfare spending rises as the decisive voter moves down the income distribution. A shift in control

²² The South is defined to comprise the three census regions that contain the original Confederate states and includes Alabama, Arkansas, Delaware, Florida, Georgia, Kentucky, Louisiana, Maryland, Mississippi, North Carolina, Oklahoma, South Carolina, Tennessee, Texas, Virginia, and West Virginia.

of state government from Republicans to Democrats, a fall in the income of those voting relative to that of the population, and the elimination of the poll tax and, in some specifications, the literacy test all lead to greater welfare spending. These results are robust to the use of state spending or state and local spending and in unreported regressions are insensitive to the exclusion of DEMOCRATIC CONTROL. Furthermore, our finding that welfare spending rises as voters' income relative to state income falls is confirmed in various subsamples.

2. Effects of Shifting the Pivotal Voter on Demand for Government Services

Changing the identity of the decisive voter has conflicting income and substitution effects on the demand for government services. As noted earlier, Kenny (1978) shows that with proportional taxes a jurisdiction's poorer citizens prefer less government services if the income elasticity for the services is larger than the elasticity of substitution between government services and other goods. The estimates of these elasticities from the empirical government spending literature are generally quite close to each other, with the income elasticity typically exceeding the elasticity of substitution.

The evidence in tables 3–6 suggests that the income and price effects roughly cancel out. There is no dominant sign to the coefficients, and the few coefficients that are significant have inconsistent signs. DEMOCRATIC CONTROL never has a significant impact on nonwelfare spending. When a two-tailed test is used, two of the eight coefficients for LITERACY TEST and POLL TAX are significantly positive, but two of the four coefficients for the relative income variable are negative and significant at the 5 percent level. On net, the evidence suggests that increasing voter turnout among the poor results in no change in the level of government services provided.

3. Other Results

As described previously, FEDERAL GRANTS and PERSONAL INCOME capture income effects in these models. Per capita grants from the federal government (FEDERAL GRANTS) raise nonwelfare spending and welfare spending. Federal aid also increases the share of state spending going to welfare.

As expected, a rise in real personal income per capita (PERSONAL INCOME) leads to an increase in total state nonwelfare spending. The income elasticity (.25–.38) is somewhat smaller than estimates summarized by Fisher (1988), which may help to explain

the indirect evidence reported in the previous section that the income and price elasticities are very close to each other.

When a two-tailed test is used, income is not significantly related to welfare spending in the log-log models but has a positive and significant impact on welfare spending in the linear models. These results do not support the social insurance model of welfare spending, which predicts a negative coefficient.

As noted above, increases in *INCOME SPREAD*, *PERCENT BLACK*, and *PERCENT ELDERLY* are associated with an increase in the number of poor families in the state. Therefore, an increase in any one of these measures represents an increase in the actual or potential welfare recipient groups. We find that all three have a positive and generally significant effect on both per capita welfare spending and the share of welfare in the budget.²³ In the state and local regressions, there is weak evidence that nonwelfare spending falls as the number of poor families rises; on the other hand, these measures are unrelated to nonwelfare spending in the state spending regressions. Perhaps local nonwelfare spending falls as the number of poor families rises.

Government services are more expensive in metropolitan areas, where wages are higher. This is expected to result in higher spending on government services. Consistent with this reasoning, *PERCENT METROPOLITAN* has a positive and generally significant impact on nonwelfare spending. Most welfare expenditures go to transfer payments, which are unaffected by this variation in costs, and we find welfare spending to be unrelated to *PERCENT METROPOLITAN* in the log-log models and inversely related to it in the linear models.

The unreported estimated time and state fixed effects for the welfare expenditure models can also be summarized. The coefficients for 1950–54, relative to 1988, are the largest (i.e., most positive) coefficients; the subsequent year coefficients follow no pattern. Not surprisingly, the smallest state coefficients typically are estimated for southern or mountain states, possibly reflecting differences in “tastes” for welfare in these states.

VI. Conclusion

The expansion of the voting franchise to include more poor citizens is often cited as a plausible and perhaps important explanation for the growth of government. The empirical evidence to date on this

²³ Husted (1989) shows that a variable similar to our *INCOME SPREAD*—the variance of state family income—has a positive impact on the maximum state AFDC benefit level.

hypothesis, however, is weak and even unfavorable to any link between changes in the voting franchise and growth in government expenditures.

A closer examination of the theory helps to explain why there is meager support so far for the hypothesis that expanding the franchise results in higher total expenditures. The pure redistribution, social insurance, and political support models of welfare expenditure predict that such a change unambiguously increases government redistributive expenditures. With regard to government services, in a simple model, expanding the franchise has an uncertain, although probably negative, effect on government service expenditures. Thus, to understand the full impact of expansion of the voting franchise, it is necessary to examine both welfare and nonwelfare spending.

The U.S. Voting Rights Acts of the 1960s and early 1970s had a profound impact on voter registration and turnout, especially in the southern states. We have cited evidence that the elimination of the poll tax and literacy test brought about an increase in voter registration and turnout, particularly among the poor, and thus resulted in a new, poorer decisive voter. Consequently, examination of the impact of this legislation yields an excellent test of the relationship between changes in the voting franchise and government spending on redistributive programs and nonwelfare services. Using state expenditure data from 1950 to 1988 and state and local expenditure data from 1958 to 1988, we are able to estimate the indirect effects of poll taxes and literacy tests and the direct effects of the relative income of voters and political party control on welfare and nonwelfare spending, holding state demographic and economic characteristics constant.

With these very different measures, we have found strong support for the prediction of the pure redistribution, social insurance, and special interest group models that welfare spending rises as political power shifts from a state's richer citizens to its poorer citizens. The elimination of poll taxes, a fall in the income of voters relative to that of the population, and a shift from Republican control to Democratic control all lead to higher welfare spending. These results are inconsistent with the pure altruism model but are consistent with the altruism/self-interest model if enfranchisement adds more current or potential welfare recipients than other poor voters. Our evidence also suggests that per capita income has a positive impact on welfare spending, which is inconsistent with the social insurance model of welfare spending. Thus state welfare spending seems to be best described by the pure redistribution, special interest group, or possibly altruism/self-interest models.

In a simple model of the demand for government services, enfranchising the poor results in greater nonwelfare government expenditures only if the income elasticity is smaller than the price elasticity, which does not appear to be the case. We found that nonwelfare government expenditures were unaffected by various measures of political influence of the poor, suggesting that the income elasticity is approximately equal to the price elasticity.

Enfranchising the poor has resulted in noticeably larger welfare spending and has had virtually no impact on nonwelfare spending. Since welfare spending is only 15 percent of the typical state's budget, the surge in welfare spending cannot account for much of the growth in state budgets over the last four decades.

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