

EQUAL VOTES, EQUAL MONEY:
COURT-ORDERED REDISTRICTING AND THE DISTRIBUTION OF
PUBLIC EXPENDITURES
IN THE AMERICAN STATES¹

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Abstract

We examine the relationship between state legislative representation and public finances. *Baker v. Carr* and subsequent court cases led to the equalization of population in U.S. state legislative districts. Analysis of state transfers to counties finds strong evidence that the equalization of legislative representation had a large effect on the distribution of state money. First, cross-sectional analysis shows that counties with relatively more legislative seats per person prior to redistricting received relatively more money from the state per person. Second, observing counties before and after the court ordered redistricting, counties that lost seats subsequently received a smaller share of state funds per capita. We calculate that population equalization significantly altered the flow of state transfers to counties, diverting approximately 7 billion dollars annually from formerly overrepresented to formerly underrepresented counties.

1. Introduction

Court-ordered redistricting in the 1960s radically altered representation in the United States. Through a series of important cases, beginning with *Baker v. Carr* in 1962, the U.S. Supreme Court established a criterion of strict equality of state legislative and U.S. House district populations.¹ Prior to judicial intervention, unequal representation was the norm in U.S. legislatures, and very unequal representation was not uncommon. Only New Hampshire's and Wisconsin's state legislatures, David and Eisenberg (1961) report, approximated one-person, one-vote in both chambers. In less than a decade, every state in the country reshaped their legislative districts to comply with the Court's rulings.

Baker revolutionized representation and, we argue, fundamentally transformed the politics of public finance in the American states. We examine the distribution of state revenue to all local governments within each county in the United States from 1957 through 1982. Two striking patterns hold. First, the more votes per person that a county had prior to 1962, the more state revenue per person the governments in that county received. Second, equalization of voting strength of counties produced equalization of transfers of state funds to the counties.

In many ways, our conclusions sound unsurprising. One-person, one-vote offers people a more equal opportunity to influence public decisions. And in practice, unequal representation in state legislatures before the 1960s appeared to produce strong biases in public policy. In the years leading up to the redistricting revolution, rural areas received extra political weight because of unequal district populations, and, as a result, the "rural agenda" was thought to be especially privileged.² Equalization of legislative representation should have erased an important bias in state politics.

However, the existing literature provides at most only weak support for the conclusion that equalizing representation altered policy outcomes. Empirical research conducted around the time of *Baker* suggested that malapportionment had little, if any, effect. A spate of papers published in the late 1960s and early 1970s looked at the relationship between state-level measures of unequal

¹*Baker v. Carr* 369 US 186 (1962) was the first in a series of important court decisions, which includes *Reynolds v. Sims* 377 US 533 (1964), *Wesberry v. Sanders* 376 US 1 (1964), and dozens of other significant rulings, leading to the establishment of the principle of one-person, one vote and the equalization of population across legislative districts. Lowenstein (1995, pages 71-113) provides an excellent summary of the cases, their progression, and the legal and constitutional issues involved.

²Indeed, this was a major point of policy activism by political scientists, involving, among other things, an APSA committee in the 1940s and 1950s that recommended equalization of district populations.

representation prior to *Baker* and statewide levels of public expenditures on a variety of programs. On the whole, these studies found no or slight effects of unequal district populations on public spending overall or on specific programs (Brady and Edmonds 1967, Hofferbert 1966, Dye 1965, 1966, Jacob 1964, Fry and Winters 1970, Erikson 1973).³ Studies examining changes in state expenditures in the years immediately after redistricting found at least some effects, but typically the results were mixed and the methods problematic (Pulsipher and Weatherby 1968, Hanson and Crew 1973; Fredrickson and Cho 1970).⁴ Nearly forty years after *Baker*, the conventional wisdom among legal scholars and political scientists is that court-ordered equalization of legislative district populations had little if any effect on how states allocate public funds (Carp and Stidham, 1993, page 370; Rosenberg, 1993, pages 292-303).⁵

Why did social science research find small or no measurable effects of redistricting? Redistricting may have truly had only minimal effects on policy. If so, a provocative and theoretically significant interpretation of the existing literature is that the rules of the electoral system have little substantial effect on public policy decisions, as some economic theorists and political scientists have argued. Alternatively, we argue, the immense change in representation that occurred in the American states from 1962 to 1972 actually had large and clearly demonstrable effects, but these were overlooked in previous research or obscured by methodological problems.

Early research implicitly assumed that the effect of malapportionment would be reflected primarily in levels of spending, overall or on specific programs. We study how one-person, one-vote changed the distribution of state spending as it changed the distribution of seats to geographic areas.⁶ Even if equalization of voting power led to a significant redistribution of spending within states, there may still be no strong link between the degree of malapportionment

³For a critique of some early studies, see Bicker (1971).

⁴How mixed were the results? Fredrickson and Cho perform the most detailed assessment of how policy might have changed following redistricting. They performed nearly 1,000 regressions, one for each combination of many different dependent variables and measures of malapportionment. They report only those equations that produce statistically significant coefficients on the malapportionment measure, approximately 10 percent of the regression analyses they performed.

⁵For other statements to this effect, see for instance Erikson, 1973, page 280; McCubbins and Schwartz, 1988, page 388. An analysis of *Baker* at the federal level concludes that policy did shift away from rural areas as a consequence (McCubbins and Schwartz, 1988). Analysis of malapportionment arising from representation of states in the US Senate produces additionally ambiguous results. Atlas, et al, (1995) find very substantial effects due to underrepresentation in the Senate, but Lee (1998) finds substantively small and statistically weak effects.

⁶The closest analysis to ours is Brady and Edmonds (1967) who compare state expenditures in counties of different population sizes across states with different levels of malapportionment. Even this poorly approximates the actual distribution of funds to counties.

and overall state spending levels. It turned out that equalization of legislative district populations generally altered the partisan balance of legislatures only slightly, chiefly because both Democratic cities and Republican suburbs were under represented (Erikson 1971, 1973).⁷ After redistricting, the increased power of fiscally conservative suburban districts countered the more pro-spending impulses of new urban representatives. As a result, the net effect of redistricting on legislative support for higher taxes and more spending, or changing spending priorities, was slight, and depended subtly on the exact features of each state's malapportionment.⁸ In retrospect, it is perhaps no surprise that previous studies concluded that the poor quality of a state's apportionment was unrelated to its level of spending on particular programs.

The direct consequence of the post-*Baker* population equalization, a consequence which holds across party lines, regional boundaries, and economic interests, is that redistricting dramatically changed the number of legislators representing specific areas, increasing the political power of some places and reducing the political power of others. We investigate perhaps the simplest possible hypothesis about the effect of redistricting: when a geographic area gets more power it will receive a greater share of state money. We know of no previous study that has looked for such redistribution directly. We examine both the cross-sectional relationship between each county's representation and its share of state transfers, and the change in each county's share of state transfers that occurs following redistricting. We find strong and consistent evidence in support of this hypothesis.

This is, in many ways, an old debate. Legal and legislative battles in the 1960s ended unequal representation in state legislatures. Our interest in the consequences of *Baker v. Carr*, though, derives from three much broader and contemporary problems. First, recent judicial scholarship argues that the courts have little impact on public policy in the United States: the early studies of the effects of redistricting are taken as a case in point (Carp and Stidham 1993; Rosenberg 1993). Second, among comparative political scientists and development economists there is growing concern that unequal political representation produces unequal distribution of public money in a variety of federal systems (Atlas, et al, 1998; Gibson, Calvo, and Falletti 1999;

⁷For other work that considered party effects see Roebuck, 1972.

⁸Also, even plans that clearly favor the cities may have mixed results on overall tax and spending levels. Urban areas are typically heterogeneous, containing high densities of both the wealthy and very poor. Depending on the mix of low and high income residents, urban areas may not provide strong support for increases in taxes in exchange for income based redistribution, much of which will go to rural parts of the state.

Samuels and Snyder 2000; Jones, Sanguinetti, and Tomasi 2000). Finally, there is a persistent and nagging question for political scientists: does representation matter? Some political scientists argue the key determinants of public policy are the activities of interest groups or the state of public opinion, an emphasis which minimizes the importance of formal political representation, and there are those in economics who argue that government policy responds to market forces rather than the voters' interests. Our findings indicate that the details of political representation and the Court's intervention in this matter had substantial policy consequences: the post-*Baker* reapportionment redistributed billions of dollars per year.

Section 2 of this paper describes the measures of representation and other data used in the analysis. Section 3 presents the findings for the distribution and levels of state revenues to county governments. Section 4 calculates the effects of equalization of representation. Section 5 discusses the implications of our results.

2. Data and Methods

We analyze the distribution of state money to and the political representation of the 3,100 U.S. counties. Counties are the basic unit of analysis in this study for three reasons. First, state governments report their electoral and government finance data at the county level. Reports of electoral and finance data are available at other levels, such as cities, but the county data are much more complete. Second, counties have very stable boundaries that are determined exogenously to the districting process. We can, therefore, measure changes in the dependent and independent variables over time for these units. Using political units, such as legislative districts, creates potential endogeneity problems because the legislatures determine these boundaries and make the revenue decisions. Third, there is ample variation in the political strength of counties prior to court-ordered redistricting.

A. Measuring Representation

Our primary independent variable of interest is the representation of individuals in the state legislatures. We measured this at the county-level by computing the number of legislative seats per person in each county. Because the sizes of legislatures and populations vary across states, any measure of voting power that is to be compared nation-wide must be normalized. Following

David and Eisenberg (1961), we measure the number of legislative seats per person in a county relative to the number of seats per person in a given state. David and Eisenberg term this the Right-To-Vote Index (or RTV). Thus, a county with an index value equal to 1 has representation equal to the ratio one would expect under a one-person, one-vote rule. Values less than 1 reflect underrepresentation and values over 1 indicate over representation.

The index is defined by the following formula. Suppose a state has I counties (indexed i) and J legislative districts (indexed j) and population P . Consider the case of a typical legislative district, j , with population P_j and a number of seats M_j (which is typically equal to 1). Let f_{ij} denote the fraction of county i 's people in legislative district j . The number of representatives per person in county i is:

$$C_i = \sum_{(j|i \cap j \neq \emptyset)} f_{ij} \frac{M_j}{P_j}.$$

The Right-To-Vote index equals C_i normalized by average fraction of representatives per county in the state: $RTV_i = C_i / [J/P]$.

An example helps with the interpretation of the index. Suppose that the state has 40 legislative districts and 2,000,000 people; thus, there are, on average 2 seats for 100,000 people. The denominator of the index, then, is $2/100,000$. If a county contains three legislative districts and 100,000 people, the numerator of the index equals $3/100,000$. In this particular example, the county in question has 50 percent more representation than the typical county in the state and thus has an index of 1.5.⁹

Following David and Eisenberg, the Right-To-Vote index for a state's entire legislature is the average of the index for the upper and lower chambers. For the legislative district lines in the 1950s and early 1960s, we rely on David and Eisenberg's measurement of this index.¹⁰ Following the 1972 redistricting, district populations were nearly equal. Throughout, we treat the RTV index as equal to 1 after 1972.

In 1960, the disparities in county representation in state legislatures were substantial. For the lower houses, the average value of the Right-To-Vote index was 1.65, indicating that the average county had about 65 percent more representation than it would have if its share of the legislature

⁹When a county is split across more than one district the RTV is the weighted average of the representation of the various parts of the county. For example, suppose that one-third of a county is in district A and there are 10,000 people for every seat in A and that two-thirds of a county are in district B and that there are 20,000 people for every seat in B, then $C_i = 2/3 (1/20,000) + 1/3 (1/10,000)$.

¹⁰We validated their construction using data from the California state legislature.

equaled its share of the state population. The average within-state standard deviation in the Right-To-Vote index was 1.23. For the upper houses, the average value of the Right-To-Vote index was also 1.65. The average within-state standard deviation was 1.47. After establishing the one-person, one-vote standard, the Court allowed deviations from equal population of no more than a couple of percent, which would, in an ideal world, have created mean RTV equal to 1 and standard deviation of 0.

Table 1 presents the means and standard deviations of the RTV for the upper and lower houses for each state in 1960, as well as the RTV for each state's entire legislature (the average of lower and upper house RTV). New Hampshire's upper and lower houses are closest to equal county representation, with mean RTV scores of 1.07 for each chamber and standard deviation of .19. Of the larger states, Florida appears to have the greatest discrepancies in representation, with a mean of 3.83 and a standard deviation of 3.15. Close behind follows California, which has the greatest discrepancies in any single legislative house, because the state uses a system of "one-county, one-vote" in the state Senate.

[Table 1]

The Right-To-Vote measure shows that unequal representation was a nation-wide phenomenon. Although many prominent court cases involved southern states – *Baker* involved Tennessee – political inequality was not a distinctly southern problem. The standard deviation of the Right-To-Vote within each state offers one measure of the inequality of representation within each state. Table 1, which lists states in descending order by their average RTV measure, shows that only a handful of the worst cases of malapportionment were found in southern states.

Unequal legislative district populations prior to the 1960s, instead, reflected schisms between urban, suburban, and rural areas. The correlation between the right-to-vote index (in logarithms) and the county population (in logarithms) is -.58. Every state shows a negative relationship between population and votes per person. In some states the correlation exceeds -.9. Underrepresentation of higher population counties effectively lowered the voting power of urban *and* suburban voters. Because urban residents tend to vote Democratic and suburbanites tend to vote Republican, the expansion of the franchise of both groups often had no net effect on the partisan distribution of the state legislatures. Several important exceptions to the association between county population and right-to-vote deserve mention. In Illinois, Louisiana, Maryland, Michigan, New York, Ohio, Pennsylvania, and Wisconsin the most populous county was not as underrepre-

sented as other large population counties. Elsewhere, the most populous counties, containing the largest metropolitan areas, had the least representation in the state legislatures.

The exceptions to the underrepresentation of metropolitan areas deserve mention for a methodological reason. Noting the underrepresentation of urban areas, some studies have examined the effect of equal representation (Brady and Edmonds, 1967; Fredrickson and Cho, 1970) on spending by examining expenditures on metropolitan and non-metropolitan programs. This approach misses the fact that the largest counties in many states were not badly underrepresented. This approach also misses the fact that the suburban counties, which might not have had a "metropolitan" spending agenda, often received the least representation. In New York state, Nassau, Suffolk, and Westchester counties had less representation than New York City. In Illinois, Lake and Dupage had much less representation than Chicago (Cook county). In Maryland, the City of Baltimore had three times as many votes as neighboring Baltimore county. With this in mind, we set out to examine a more appropriate dependent variable.

B. Measuring Public Expenditures

We seek to explain the distribution of public money to counties. The Census of Governments reports total transfers from the state and the federal government to all local governments in a county. The Census of Governments is conducted every 5 years (e.g., 1957, 1962, 1967, 1972, 1977, and 1982). We are interested in the changes that occur from before the imposition of one-person, one-vote to the immediately following period. The years 1957 and 1962 provide a picture of expenditures before *Baker*. Battles over redistricting occurred mainly from 1965 through 1972. Transfers to counties in 1977 and 1982, thus, measure the distribution of revenues to counties once one-person, one-vote is in place. To smooth over year-to-year variations in expenditures, we average the 1957 and 1962 revenue reports and the 1977 and 1982 reports. Analysis of each year separately shows the same pattern as the pairs of years combined.

We look at total transfers from states to counties. Transfers span a large class of programs, including highways and roads, health and welfare, and education. Ideally one might ascertain the effects of redistricting on specific programs. One problem with measuring programs in isolation is that logrolls across programs might mean that specific programs do not reflect the overall influence of a county over the state or federal budget. Given these complexities with the analysis of specific programs we chose to look at the effects of political representation on total transfers.

Further research might tease out the program-specific effects of one-person, one-vote.

Transfers from states to counties vary considerably across states and over time. The average state transfer to counties equaled \$71 per person (all dollar figures in this paragraph are in 1967 dollars) in 1962 and \$131 in 1977. New Hampshire had the lowest average transfers per capita to counties in 1962 of \$13 and in 1977 of \$52. Colorado had the highest average transfers per capita to counties in 1962 of \$140, and New York had the highest average transfers per capita to counties in 1977 of \$258.

In order to compare across states, we calculate the amount of money per person transferred to each county relative to the average amount transferred to all counties in a given state. We compute the total transferred to each county divided by the county's population, and then divide this by the average per capita amount transferred to counties in the state. This is equivalent to the county's share of total state revenues transferred to all counties per capita.¹¹

One conjecture of our research is that some degree of equalization of transfers did occur from the 1950s to the 1970s. This is borne out in the descriptive statistics of our dependent variable. Both the mean and variance of county's shares of per capita transfers shrunk over this time period. Equality predicts a mean relative per capita transfer near 1 with a small variance. The average relative per capita expenditures in 1957 and 1962 equal 1.25, and the variance around this average is .17. The average relative per capita expenditure in 1972 and 1977 equals 1.06, and the variance around this average is .09. In other words, in the wake of the redistricting cases of the 1960s, the transfers to the typical county more closely approximated the equal division of funds (with a mean near 1), and disparities across counties were cut in half (the approximate reduction in the variance).

Some of the early research on this question correlated various state level measures of inequality of representation with (state level) measures of disparities in public finances. These studies often found only weak associations between state level measures of equal representation and equal dispersion of public money. There are several important drawbacks to such an aggregate approach. First, the correlations do not measure whether the underrepresented areas indeed received less money than other parts of the state. It is entirely possible that other factors contribute to the variation in transfers to counties. Second, the findings depend strongly on the measures used.¹²

¹¹We exclude Alaska and Hawaii because they joined the union in 1958.

¹²Brady and Edmonds, for example, compare the total representation and total revenues of the top 5 counties in

Using the standard deviation measures as an indicator of districting equality reveals a correlation of only .09 between state-level equality of representation and equality of revenues. However, using the ratio of the largest county to the smallest county to measure state level inequalities in representation and in revenues transferred to counties yields a correlation of .47 across states. The sensitivity of these correlations to which measure is used raises serious doubts about the validity of state level studies of the effects of political equality, which unfortunately covers most of the literature on redistricting in the United States.

C. Other Factors

In addition to the number of representatives per person, many other factors affect the distribution of public money. We are particularly concerned about the possible confounding effects of poverty and income. The decade of the 1960s witnessed the expansion of programs designed to reduce poverty. Rising transfers to poorer counties might confound any possible effects of redistricting because inner cities were disproportionately underrepresented before the imposition of one-person, one-vote. To control for such confounding factors, we hold constant county poverty rates, per capita income, and turnout (which Stromberg (1999) argues influences the distribution of government expenditures).

3. Representation and the Distribution of Funds within States

Our analysis of the effects of representation on spending breaks neatly into two questions. First, did counties with relatively more legislative seats per person prior to 1962 receive relatively more money per person? Second, did equalization of voting strength produce a more equal distribution of state transfers per person to counties?

A. Differences Across Counties

Did unequal votes correspond with unequal money? If representation influences the shares of revenues, then we expect that, prior to 1962, counties' voting strength maps into their shares of government revenue. The association between votes and revenues across counties is striking.

a state. They find no association. Using the ratio of the least represented to the most represented county, however, shows a strong association.

We begin by considering a single state: Florida. Florida's lower house had the highest average RTV measure of all states in 1960, and the overall average RTV measure is second only to Nevada.¹³ Dade county, which includes the city and most of the suburbs of Miami, was badly underrepresented and the rural counties in the panhandle held a disproportionately large number of state legislative seats.¹⁴ plots the relationship between counties' shares of state revenues per person (in logarithms) in 1960 (1957 and 1962 averaged) and in 1980 (1977 and 1982 averaged) against the Right-To-Vote measure (in logarithms). Because the measures are in logarithms, values equal to 0 mean that the county received a share of revenues equal to what one would expect under equal distribution of funds or a share of votes equal to what one would expect under one-person, one-vote. The observations for 1960 are represented with an "o" and the observations for 1980 with a triangle.

[Figure 1]

There is a strong association between votes per person in 1960 and revenues per person in 1960. The bivariate regression of log of share of state revenues in 1960 on log of Right-To-Vote in 1960 has a slope of .398 (se=.022); the $R^2 = .833$. A county with twice as much representation as another county received approximately forty percent more of the state's revenue.

There are two ways to check whether the association between revenue and votes in 1960 is spurious. First, we can contrast the 1960 pattern with the 1980 pattern. By 1980 district populations were equalized. If some other feature of the county actually accounts for the relationship between votes and revenue then we expect to see the same relationship between revenues and votes in 1980 as we do in 1960. Figure 1 shows a much flatter relationship. Regressing revenues shares in 1980 on vote shares in 1960 yields a slope of .152 (se = .035) and an R^2 of just .23. Second, we can control for other factors. Controlling for per capita income, gubernatorial turnout, and the percent in poverty, the slope in the early period *rises*, slightly, to .417 (se = .031), and, in the latter period, the slope falls substantially, to .063 (se=.034)—no longer different from 0 at the .05 level.

Looking across all states, we find that Florida, though an extreme case, reveals a more general

¹³As we demonstrate later in the paper, though the degree of malapportionment in Florida is extreme, the effects are not at all atypical. We have chosen to illustrate the case of Florida rather than Nevada since Florida has many more counties than Nevada and therefore more observations.

¹⁴Havard and Beth (1964) provide an excellent history of these battles. There were no constitutional constraints on the state, as in California. Rather, the state legislature had deadlocked several times during the 1940s and 1950s over redistricting and so the lines dating back to the 1920s remained in place.

relationship. Figure 2 displays the relationship between revenue shares and vote shares (both in logarithms) for all counties in the U.S. We convert the revenue and vote measures to logarithms to reduce the heavy skew in these measures, making the relationships between the transformed variables nearly linear. The top panel presents the relationship for 1960; the bottom panel, the relationship between revenues in 1980 and RTV in 1960.

[Figure 2]

Immediately before the Court imposed one-person, one-vote on the state legislatures, there was a strong positive relationship between legislative seats per person apportioned to counties and state revenues per person transferred to counties. The slope on the line in the top panel is .30 (se = .01). A county with twice as much representation as another county is predicted to have received 30 percent more state money per capita. By 1980 the counties the distribution of funds equalized substantially. The slope on the line relating revenues to votes falls to .10 (se= .01).

Controlling for other factors affects the estimated relationship somewhat, but the pattern displayed in Figure 2 still holds. Table 2 reports the results of regression analysis in which the Right-To-Vote measure, the percent in poverty, per capita income, and turnout predict the share of state funds received by each county in the US. Each regression contains fixed effects for each state. The share of state funds is the average of the amounts reported in the 1957 and 1962 Census of Governments for the earlier period and 1977 and 1982 for the later period. As in the Figures, the Right-To-Vote index and the per capita share of state revenues are measured in logarithms. As with the right to vote and revenue measures, we divided other independent variables – per capita income, the poverty rate, and the gubernatorial turnout rate – by the state average for each variable, so each variable is normalized relative to the state.

[Table 2]

The estimates in Table 2 reveal that unequal district populations correspond with substantial inequalities in the shares of funds in the 1960s. The three columns on the top of Table 2 present three different specifications of the regression. The coefficient on the Right-To-Vote index equals .33 (se = .009) without any control variables. Controlling for county poverty rates, per capita income, and turnout rates, the effect falls to .18 (se = .010). Doubling a county's representation is predicted to increase its share of state money by 20 percent. Holding other factors constant, then, the counties' representation has a substantial effect on its share of state revenue.¹⁵

¹⁵An alternative set of regressions weights by population. Weighting alters the results only slightly: the coefficient

Control variables in the regression also square with intuitions, the ideological commitment to redistribution associated with the New Deal and Great Society, and arguments from the public finance literature about the redistributive nature of government. Areas with higher poverty rates received substantially more state revenue per person; areas with higher income received substantially less. Consistent with Stromberg (1999), counties with relatively high turnout in statewide elections receive relatively more revenue per person. We do not give much of a causal interpretation to this finding, but rather include turnout to capture not only direct turnout effects but also other local factors that may affect representation, such as the implementation of election laws, and the preferences of other players in the budget process, such as governors.

Analysis of the distribution of funds two decades after *Baker* serves as a further check that the result for 1960 is not spurious. By 1980, after district populations had been equalized, the Right-To-Vote in 1960 should have little or no effect on the distribution of revenues. The bottom panel displays regression results using the values of the dependent variable and the control variables twenty years later. For the later period, the coefficient on Right-To-Vote in 1960 is a quarter to a third the size as the coefficient for the earlier period. With all of the controls, for example, the coefficient is .05 (se = .010). A county with half as much representation as the typical county is predicted to receive only 5 percent less money by 1980. The residual effect might reflect the persistence of agrarian legislators in positions of power in many state legislatures. Much of the residual effect comes from the southern states: outside the south the coefficient is not different from 0.¹⁶

The other factors in the multivariate regressions for 1980 comport with expectations, except for turnout. As in 1960, areas with higher poverty rates received substantially more state revenue per person; areas with higher income received substantially less. In the late 1970s and early 1980s, counties with relatively high turnout in statewide elections receive relatively less revenue per person. This reverses the effect from the 1960s analysis, and is inconsistent with other

on log RTV, controlling for population, turnout, poverty, income, and state, becomes .15 (se = .01) for revenues in 1960 and .02 (se = .01) for revenues in 1980. Because the coefficients from those regressions are somewhat harder to interpret, especially when population is an included variable, we prefer the specifications reported in Table 2.

¹⁶The results reported in this table are robust to the inclusion of additional control variables. The regressions in Table 2, column 2 and column 4 were re-estimated including several additional control variables: percentage black in county, percentage voting democratic in county, last 10 year population growth in county. The coefficient on the RTV measure was .17 for the 1960 revenue regression, and .06 for the 1980 revenue regression. As a further check on the results, we reran the regressions after breaking the sample into quintiles based on county population. The effects of RTV on the distribution of public funds was consistent across these subsamples.

empirical research. We are reluctant to give a causal interpretation to the turnout variable: it may pick up many other effects, such as percent urban, or may be due to sampling error. Our focus is on the Right-To-Vote measure, and by the late 1970s and early 1980s areas that were over represented in 1960 maintain only slight advantages in the distribution of state revenues.

As a further check on the robustness of the estimates in Table 2 we considered possible interactions between Right-To-Vote and other factors. The literature on state politics suggests three such interactive effects: gubernatorial power, race, and region. Neither race (percent black in each county) nor gubernatorial power interact significantly with the Right-To-Vote measure. Specifically, having a formally weak governor does not magnify the effects of unequal legislative representation on the distribution of state money. Nor does having a high concentration of blacks in a county interact with the right-to-vote measure.

Regional variations, by contrast, did prove important. Even though the South and non-South show no differences in terms of average Right-To-Vote, the marginal effect of the votes on revenues appears to be much larger for the South in 1960 than for the non-South. Regressing log revenue shares per capita in 1960 on log Right-To-Vote in 1960 yields a coefficient of .19 in the South and .13 in the non-South. The estimated marginal effects are proportionately lower in 1980: .06 in the South and .03 outside the South. Indeed, the persistent effect of the Right-To-Vote measure in 1960 on revenues in 1980, as noted in the previous section, stems from the relationship between these variables inside the South. One possible explanation lies in the continued domination of many southern legislatures by rural Democrats throughout the 1970s. It might also take a long time for these effects to work their way through the political system. Even still, the influence of the overrepresented counties on public expenditures falls in the wake of *Baker*, and it falls by nearly equal amounts in the two regions.

The county-level data analyzed in Table 2 show that counties that had less representation received relatively less money in the pre-*Baker* era. The pattern holds among all counties in the U.S. and as well as within nearly every state. What is more, the analysis of the data from 1980 suggests that inequalities in representation in the 1960s had little or no effect on counties' shares of revenues in 1980.

B. Changes from 1960 to 1980

The cross-sectional analysis in Table 2 provides significant evidence that over-representation

translated into a larger share of state revenue. The fact that our results are robust to alternative model specifications is reassuring. However, it is well known that cross-sectional analysis, even when control variables are included, can generate spurious results due to omitted variables. One possible critique of the cross-sectional work is that the malapportionment merely *reflects* a county's political power, rather than causes it. If so, some common feature might lead a county to have both overrepresentation in the legislature, and a large share of the state's transfers.¹⁷ To minimize the chances that omitted variables lead to incorrect inferences about the marginal effect of districting, we next look at how revenue to a county changes over time in response to a change in the county's voting power.

District populations were equalized in a very short period of time – from 1962 to 1972. Court-ordered redistricting raised the representation of some counties such as Dade County, Florida, as much as ten-fold, and reduced the representation of other counties, such as Lafayette County, Florida, by as much as one-hundred fold. The county data allow us to map changes in an area's representation into changes in an area's revenues. If political representation influences the distribution of public finances, then counties, especially those extremely over or under represented, should have witnessed substantial changes in the revenues per person that they received from the state, relative to the amounts other counties received.

Figure 3 displays the relationship between changes in revenue shares (in logarithms) and changes shares of state legislative seats (in logarithms). To measure changes in revenue we calculate the difference in the same dependent variable used for the analysis in Table 2. To measure changes in representation we calculate the difference between log of Right-To-Vote in the mid 1970s (after the 1972 redistricting) and log of Right-To-Vote in the 1960s. Because district populations are virtually identical by 1974, changes in representation equal one minus the Right-To-Vote in 1960, or, in the logarithmic scale, the change in the RTV measure equals $-\log(\text{RTV})$. The horizontal axis in Figure 3, then, equals the negative of log of Right-To-Vote in 1960. Because all of the variables are measured in logarithms, the differences between the two time periods can be thought of as percentage changes in the variables.

[Figure 3]

¹⁷For example, if public opinion in the state supports rural versus urban interests, this may be reflected in both state spending patterns and tolerance of overrepresentation for rural areas. Despite this theoretical possibility, since in many cases the roots of the malapportionment stretch back many decades, it is very plausible that the districting schemes can be safely viewed as exogenous.

Figure 3 shows that counties that received more representation following *Baker* saw their shares of state revenue increase substantially. The slope of the regression line in Figure 3 is .23 (se = .01). A county whose representation doubled between 1960 and the mid 1970s received a 23 percent increase in its share of state money over this time.

Regression results confirm the robustness of the pattern in Figure 3. Table 3 presents regression results from analysis predicting changes in revenue shares (in logarithms) from 1960 to 1980. All regressions contain fixed effects for each state. The first column contains the regression results using only the negative of the log of Right-To-Vote in 1960 as a predictor. The second column contains the regression results including changes in poverty rates, per capita income, and turnout, as well as state fixed effects.

[Table 3]

Increases in representation produced dramatic increases in revenues per person. Controlling for other factors, doubling a county's representation increases its revenues per capita by almost 20 percent. The estimated coefficient on $-\log(\text{RTV})$ in Table 3 equals .17 (se = .010).¹⁸ Looking state-by-state reveals that the pattern is remarkably robust. In 47 of 48 states, regressions of the change in county revenue share on the change in a county's RTV measures yields a coefficient with the expected sign.¹⁹

The effects of the control variables are, again, consistent with other arguments about the distribution of public money. An increase in a county's poverty rate, relative to other counties in the state, corresponded with higher transfers from the state to the governments in the county. Income changes produced no significant changes in intergovernmental transfers to counties. And consistent with Stromberg (1999), increases in a county's statewide turnout, relative to the rest of the state, corresponded with higher revenues per capita.

Across specifications, the effect of changes in representation on changes in revenues remains fairly stable. A one-hundred percent increase (or decrease) in representation corresponds with approximately a 20 percent increase (or decrease) in revenues. Significantly, this result is very close to the cross-sectional estimates we report in Table 2, as is expected if the cross-sectional

¹⁸Weighting by population produces a slightly smaller effect. The estimate coefficient on change in log RTV, once we have weighted by population, equals -.145 (se = .009).

¹⁹Regressions were performed for all states for which data was available (Alaska and Hawaii were excluded). The results reported in Table 3 are robust to alternative model specification. Including additional control variables (change in county population from 1960 to 1980, change in percent black, change in democratic vote percentage) raises the coefficient on RTV reported in column 3 from .17 to .19.

results are not spurious.

As a further check on the robustness of the specification we tested for possible interactions between representation and race, region, and gubernatorial power. None of these factors interacts significantly with the Right-To-Vote measure. Given regional differences in the slopes discussed earlier, we were especially concerned that equalization of representation had differential effects between the south and non-South. It did not. The effect of changes in county representation on changes per capita state revenues (i.e., the coefficient on $-\log(\text{RTV})$) equals $-.25$ in the South and $-.22$ outside the South. Changes in representation, it appears, translate directly into changes in state revenues received by the counties.

C. Effects on the Level of Overall State Transfers

Equalization of legislative district populations in the mid-1960s may have affected the level of spending, in addition to altering the distribution of public expenditures. Transfers from states to counties grew from \$365 per person in 1960 to \$659 per person in 1980 (in 1999 dollars). One might expect that *Baker* contributed to an expansion of state government in the 1960s and 1970s for three reasons. First, underrepresented areas may have had higher demand for public expenditures than overrepresented areas. Underrepresented areas typically had higher per capita income, and demand for public spending tends to increase with income. Second, expanding the size of government may have been politically more expedient than cutting programs that benefited voters who were overrepresented in the past. Third, expansions of democracy have generally been associated with expansions in government spending, as new voters bring new demands for public expenditures (Lindert, 1996).

Most earlier research on the effects of malapportionment examined its effects on levels of spending overall and on particular programs. Consistent with past research we find that equalization contributed little to the growth of transfers from state governments to counties. Table 4 presents estimates of the association between changes in a states' overall degree of malapportionment and the growth in total state transfers to counties. As noted earlier there are many ways to measure state-level equality of representation. We considered three: the mean of the log of RTV, the standard deviation of the log of RTV, and the difference in log of RTV between the county with the most representation and the county with the least representation (labeled Hi-to-Lo Log of RTV). We regressed the change in the log of total intrastate transfers to counties on the various

measures of malapportionment. We considered the simple bivariate regression, shown in the first three columns, and the multivariate regression, controlling for changes in population, income, and poverty, as shown in the last three columns.

[Table 4]

Changes in total state transfers to all counties in a state per capita from 1960 to 1980 are weakly and positively associated with the degree of equalization of representation that occurred. All three measures suggest that states that had greater improvements in representation showed, on average, higher growth in expenditures. The magnitudes of the effects, however, are small, and none reach conventional levels of statistical significance. The standard deviations of Avg Log RTV and of SD Log RTV are approximately .2 and the standard deviation of Hi-To-Lo Log RTV is .88. A one standard deviation change in one of these independent variables, then, corresponds to only a 3 to 4 percent growth in total revenue per capita transferred from states to counties, a shift that is not statistically distinguishable from 0.

4. What if Carr Had Won?

Our findings clearly show that changing a county's voter power had important effects on a county's relative per capita share of state spending. Using the regression estimates, we can investigate who won and lost from *Baker* by considering a counterfactual. Had the Court not imposed one-person, one-vote, what would the distribution of state revenues have looked like in 1980?

We construct the counterfactual using predicted values from the regressions presented in section 3. Algebraic manipulation of the regression specification, as sketched in the Appendix, produces a simple formula for calculating how much per capita state revenues in 1980 differed from what would have been had the county representation remained as unequal as it was in 1960. The difference between the predicted state revenues per capita in a county had the 1960 distribution of votes held and the predicted state revenues under the 1980 district lines equals:

$$Y^* - Y_{80} = Y_{80}[(RTV_{60})^2 - 1],$$

where Y^* is the hypothetical level of per capita spending in a county if the 1960 districting held, Y_{80} is the actual level of per capita state spending in the county, RTV_{60} is the right to vote

measure for the county in 1960, and .2 is the elasticity from Table 3. We make one simplifying assumption in making this calculation: equalization of voting power did not contribute to the growth in overall state transfers to counties, which seems reasonable from section 3(C).²⁰

Using this formula we can generate predicted transfers to each county. Because the RTV does not follow the normal distribution, the means and variances of the RTV measure will not fully capture the changed distribution of voting power and its consequent effects on spending. Instead, we sketch the change in the distribution of funds across various percentiles of the distribution of the right-to-vote measure: 5th percentile, 10th percentile, 25th percentile, 50th percentile, 75th percentile, 90th percentile, and 95th percentile.

Table 5 shows the distribution of RTV by sample percentiles along with the change in per capita transfers associated with the change in voting power. The first two columns show the percentile of the RTV measure and the value of the RTV at that percentile. Equalization of district populations creates an RTV equal to 1; this is approximately the value of the RTV at the 25th percentile county in 1960. In other words, about three-quarters of all counties had more voting power before *Baker* than they did afterward. This distribution of county power, though, does not map uniformly into persons. The third column presents the number of persons in the counties in each percentile. Under represented counties – those in the bottom 25 percent of the distribution of representation – had 71 percent of the nation’s population. Over represented counties had just 29 percent of the population. The last two columns of Table 5 show the effect of redistricting on the distribution of money. The fourth column presents the difference in dollars transferred to the county (per person) between the actual 1980 transfers and the amount that the county would have received (per person) under the 1960 districts.

[Table 5]

The amount of redistribution that followed from *Baker* was substantial. The final column shows the calculated gain in revenue that a county received from the imposition of one-person-one-vote relative to what would have happened without equal representation. In the most under-represented counties, the lowest 5 percent in terms of Right-To-Vote, equal votes increased state revenues by \$88 per person per year. In the most overrepresented counties, one-person, one-vote reduced state revenues by \$268 per person per year. The cumulative effect was to shift approxi-

²⁰In addition, this assumption does not affect our conclusions, since a change in the overall amount of spending does not affect the relative shares of counties.

mately \$7 billion toward counties that had been underrepresented in the prior to the imposition of one-person, one-vote. This redistribution amounts to taking approximately 10 percent of the revenues received by people in overrepresented counties and shifting them to the underrepresented counties.

5. Discussion

The Supreme Court's doctrine of one-person, one-vote transformed political representation in the United States. It resulted, we have documented, in substantial redistribution of public spending in the states. Transfers of revenue from state governments to all governments within counties were highly unequal before the imposition of one-person, one-vote, and the degree of inequality corresponded strongly with the degree of county representation. By the late 1970s and early 1980s, the distribution of state revenue to counties equalized substantially and transfers to counties were only weakly related to the pre-*Baker* distribution of political representation. Treating court-ordered redistricting as a natural experiment, we find changes in a county's state legislative representation resulted in substantial changes in that county's shares of state revenues.

Our findings come with an important caveat. We look at transfers from states to all local governments within each county. In and of themselves, such transfers are an important vehicle of state government finance, amounting to approximately 20 percent of all state revenue. We believe that the redistribution of transfers to local governments traces broader changes in public finances that happened as a consequence of more equal representation. However, it is very difficult (if not impossible) to reconstruct how the remaining 80 percent of state funds were distributed geographically.

Beyond the immediate transformation of public finance, the one-person, one-vote revolution carries four broader lessons about political institutions and public policy. First, our findings suggest that public policy is highly responsive to the electorate. It is typically very hard to study the relationship between policy change and electoral preferences because of potential simultaneous relationships and because of difficulties measuring exogenous changes in voters' preferences. *Baker v. Carr* is a rare natural experiment in which an agent (the court) outside of electoral or legislative politics dramatically changed the scope of representation. Our results do not speak directly to specific mechanisms or equilibria, such as the pivotalness of the median, but they do

reinforce the general point that public policy strongly reflects constituents' interests.

Second, and relatedly, our results suggest that understanding the electorate is what is important for understanding the distribution of public money. We find not only reduced inequalities in the distribution of funds, but that the distribution of state transfers became very equal in the post-*Baker* era and unrelated to pre-*Baker* representation. The lesson, we infer, is that legislative politics substantially involves bargaining among equals. As long as people have equal representation they will get a fair share of public expenditures. This view is echoed by findings that constituents receive very little material benefit from the institutional positions held by their representatives in the legislature. Previous studies have generally found that districts represented by a member of the majority, a committee chair, or party leader do not receive significantly more spending (e.g. Levitt and Snyder 1995, Owens and Wade 1984, Rich 1989, Rundquist and Griffith 1976). Such a broad conclusion fits with some past studies of the distribution of public money, but it awaits more extensive analysis: greater attention should be given to voting rules and participation in understanding legislative output.

Third, our investigation has important implications for constitutional design. Malapportionment is still widely observed in contemporary political systems; it is widespread in federal legislatures outside of the U.S. and is an especially common feature of legislatures in poorer democracies or newly democratic nations.²¹ The upper chambers of several national legislatures, including those of Argentina and Brazil, are more malapportioned than the U.S. Senate (Samuels and Snyder, 2000). Our empirical findings suggest that malapportionment is not a minor detail of legislative design, but can have important consequences for political decision making. Our conclusion that equalization of district populations altered the distribution of state spending suggests that malapportionment does not merely reflect the underlying distribution of power in a political system, but is itself at least one important source of political power.

Finally, our findings demonstrate the power of the courts. At the time of *Baker v. Carr*, it was widely believed that Court actions to equalize district populations would have an important effect on state politics. The empirical work following population equalization led to the formation of a new conventional wisdom that the court-led redistricting had little or no impact on political

²¹For a description of the extent of malapportionment in 78 countries, see Samuels and Snyder (2000). For a comparative analysis of malapportionment and public spending in several federal systems in South America, see Gibson, Calvo, and Falletti 1999.

outcomes; this conventional wisdom was subsequently relied on to bolster the view that courts can, in general, have only minimal impact on democratic politics. Our work, which finds that the post-*Baker* reapportionment actually redistributed billions of dollars per year, strongly contradicts the conventional wisdom on the effects of population equalization, and as a result provides clear evidence of how the Court can, in fact, shape public policy.

APPENDIX

Formula for Generating Predicted Values

Consider again the regression results. For a county in 1980 the level of spending per capita relative to the state is

$$\log\left(\frac{Y_{80}}{\bar{Y}}\right) = \alpha + \beta \log(RTV_{80}) + Z\gamma, \quad (1)$$

where Y_{80} is the state transfer to county i per person in 1980 and \bar{Y} is the state's overall transfer to all counties per person in 1980 and a vector of other variables, such as poverty and income in 1980. If the old regime held in 1980, then the county's representation would have been RTV_{60} and the regression predicts that that relative transfer to the county would have been

$$\log\left(\frac{Y^*}{\bar{Y}}\right) = \alpha + \beta \log(RTV_{60}) + Z\gamma, \quad (2)$$

First, solve equations (1) and (2) to express expected revenues in terms Y^* of current and past RTV. Assuming that the level of total state transfers to counties was not affected by redistricting, as seems reasonable given the results in section 3(C), we may bring \bar{Y} to the right-hand side of these equations. The regressions, then, may be rewritten as

$$\log(Y_{80}) = \alpha + \log(\bar{Y}) + \beta \log(RTV_{80}) + Z\gamma$$

$$\log(Y^*) = \alpha + \log(\bar{Y}) + \beta \log(RTV_{60}) + Z\gamma$$

Subtracting the first equation from the second yields

$$\log\left(\frac{Y^*}{Y_{80}}\right) = \beta \log\left(\frac{RTV_{60}}{RTV_{80}}\right).$$

Taking exponentials and isolating Y^* :

$$Y^* = Y_{80} \left(\frac{RTV_{60}}{RTV_{80}}\right)^\beta \quad (3)$$

Second, contrast Y^* with Y_{80} . We are interested in the difference between what would have received had the courts not mandated one-person, one-vote and what the counties did receive in the aftermath of *Baker*. Subtracting Y_{80} from (3),

$$Y^* - Y_{80} = Y_{80} \left[\left(\frac{RTV_{60}}{RTV_{80}}\right)^\beta - 1 \right]$$

Because county representation is virtually identical in 1980, $RTV_{80} = 1$. Using our estimates above, $\beta = .2$, so

$$Y^* - Y_{80} = Y_{80} \left[(RTV_{60})^{.2} - 1 \right]$$

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○ Share of Transfers, 1960 (logs) △ Share of Transfers, 1980 (logs)

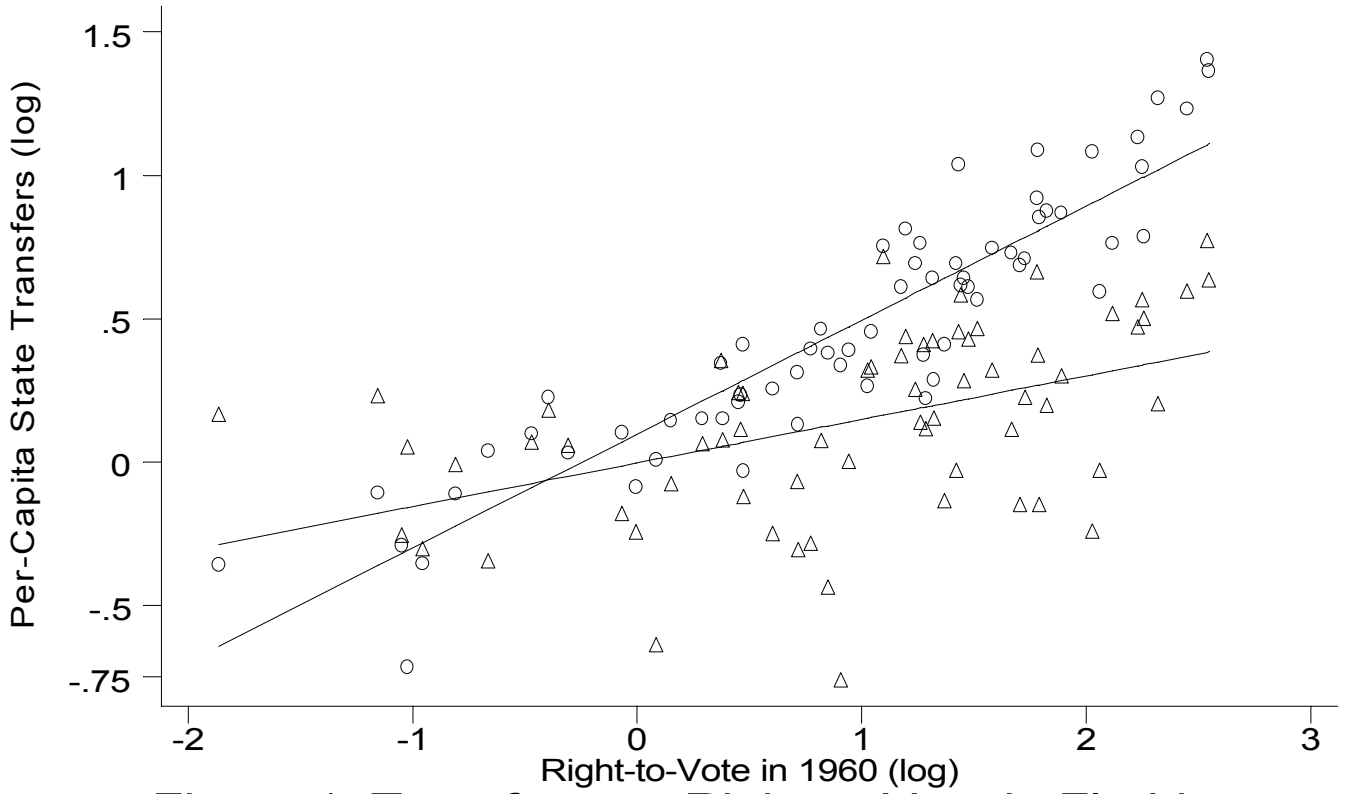


Figure 1: Transfers vs. Right-to-Vote in Florida

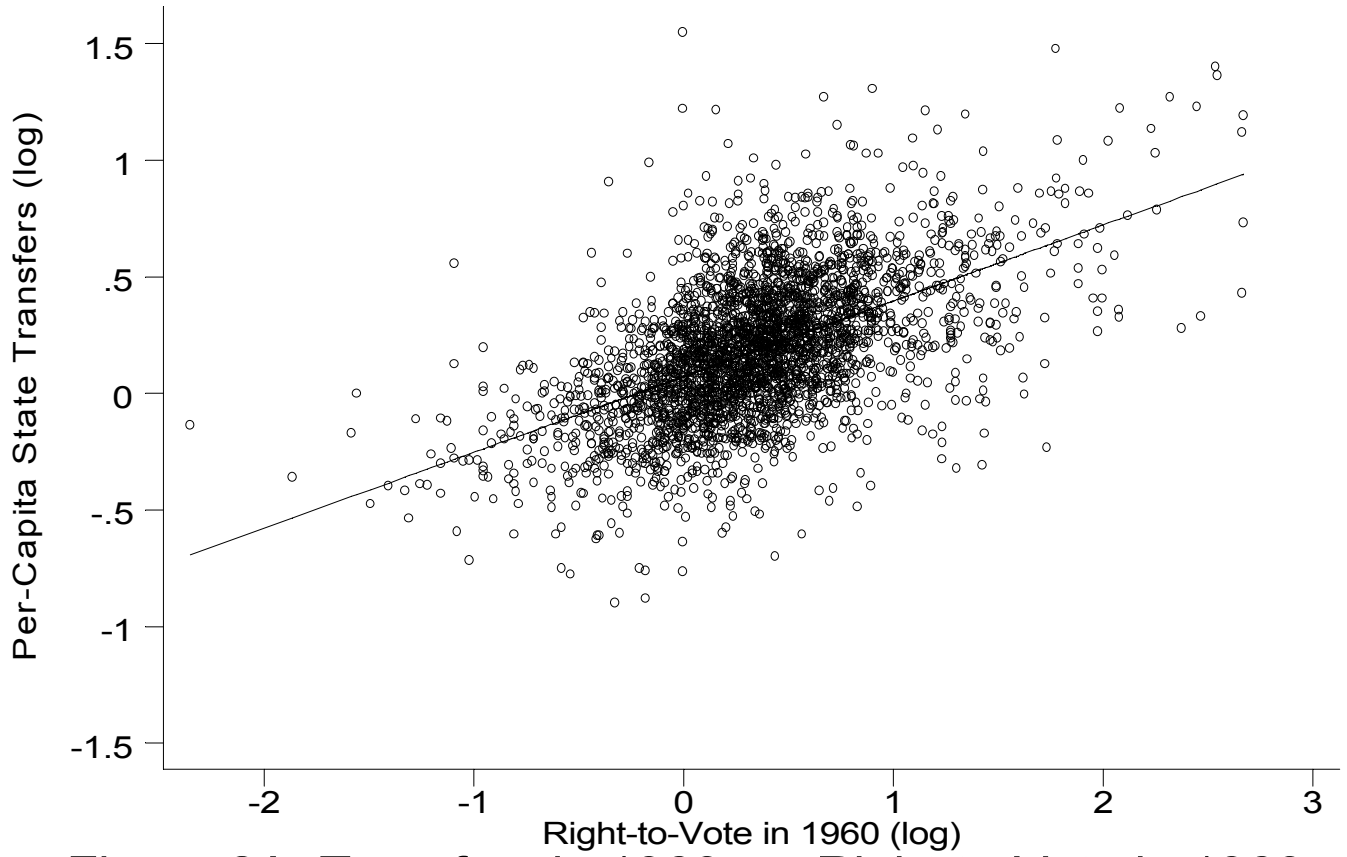


Figure 2A: Transfers in 1960 vs. Right-to-Vote in 1960

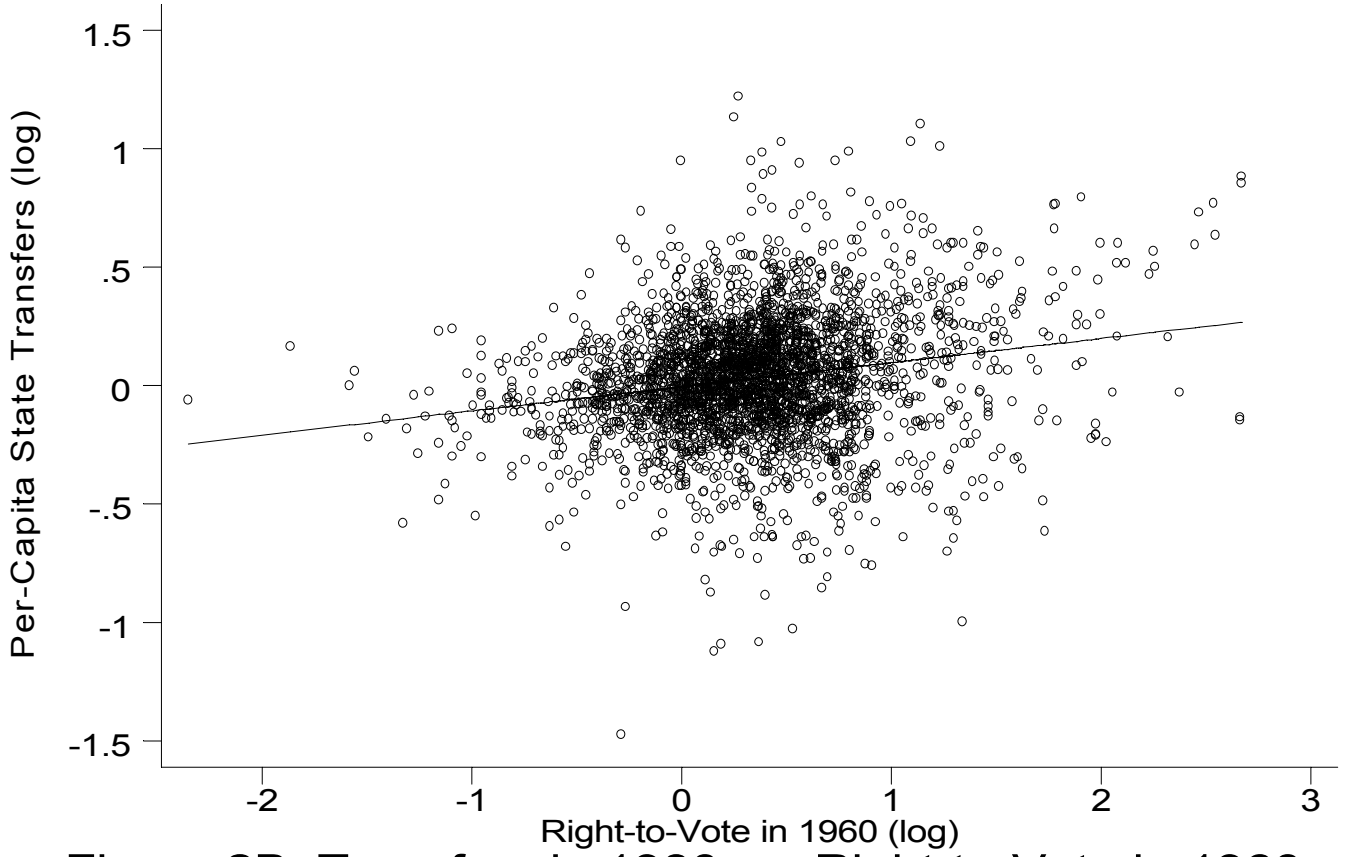


Figure 2B: Transfers in 1980 vs. Right-to-Vote in 1960

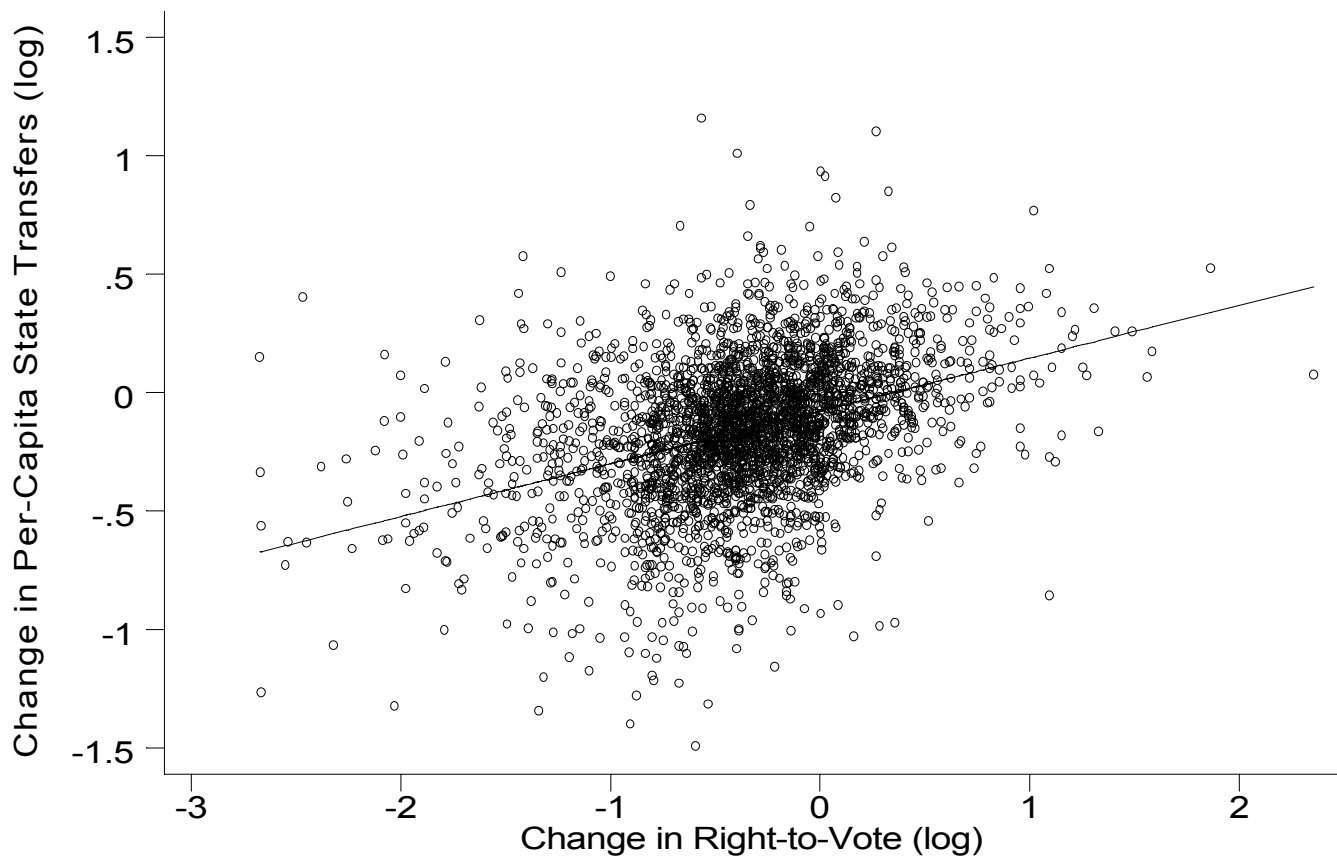


Figure 3: Change in Transfers vs. Change in Right-to-Vote

Table 1. Right-To-Vote Index in 1960, Upper and Lower Houses of State Legislatures

State	Upper & Lower House Average [St. Dev.]	Lower House Average [St. Dev.]	Upper House Average [St. Dev.]
Florida	3.83 [3.15]	4.28 [4.31]	3.38 [2.81]
California	3.54 [3.17]	1.20 [0.37]	5.89 [6.25]
Arizona	2.71 [2.00]	1.39 [0.69]	4.04 [3.48]
Utah	2.65 [1.84]	2.92 [2.93]	2.39 [1.15]
Maryland	2.53 [1.62]	2.05 [1.12]	3.01 [2.21]
New Mexico	2.52 [2.48]	1.92 [1.51]	3.13 [3.48]
Idaho	2.50 [3.02]	2.19 [2.55]	2.81 [3.50]
Montana	2.19 [2.01]	1.73 [1.44]	2.65 [2.60]
Georgia	1.92 [1.05]	2.06 [1.42]	1.77 [1.05]
Kansas	1.91 [0.91]	2.36 [1.80]	1.46 [0.62]
Colorado	1.81 [0.61]	1.81 [0.74]	1.82 [0.64]
Alaska	1.73 [0.70]	1.71 [0.80]	1.74 [0.88]
Michigan	1.69 [0.72]	1.45 [0.45]	1.93 [0.77]
Missouri	1.69 [0.72]	2.22 [1.36]	1.17 [0.24]
Oklahoma	1.69 [0.79]	1.67 [0.84]	1.70 [0.93]
Hawaii	1.66 [0.74]	1.23 [0.27]	2.08 [1.22]
New Jersey	1.66 [1.18]	1.13 [0.49]	2.20 [1.94]
New York	1.66 [0.74]	2.00 [1.37]	1.33 [0.27]
Alabama	1.60 [0.85]	1.65 [0.95]	1.55 [0.92]
Connecticut	1.57 [0.77]	1.87 [1.14]	1.27 [0.47]
North Carolina	1.55 [0.95]	1.86 [1.66]	1.24 [0.40]
Pennsylvania	1.55 [1.05]	1.75 [1.94]	1.36 [0.45]
Tennessee	1.50 [0.82]	1.56 [1.31]	1.45 [0.64]
Texas	1.49 [0.34]	1.44 [0.48]	1.54 [0.40]
Delaware	1.45 [0.73]	1.43 [0.71]	1.46 [0.76]
South Carolina	1.44 [0.74]	1.11 [0.28]	1.78 [1.26]
Louisiana	1.43 [0.62]	1.54 [0.84]	1.32 [0.66]
Minnesota	1.43 [0.45]	1.50 [0.64]	1.37 [0.50]
Wyoming	1.43 [0.64]	1.26 [0.46]	1.61 [0.96]
Illinois	1.42 [0.46]	1.18 [0.21]	1.68 [0.77]
Iowa	1.41 [0.56]	1.50 [0.68]	1.33 [0.57]

Table 1 (Continued). Right-To-Vote Index in 1960

State	Upper & Lower House Average [St. Dev.]	Lower House Average [St. Dev.]	Upper House Average [St. Dev.]
Vermont	1.37 [0.82]	1.40 [0.81]	1.34 [0.96]
Nebraska	1.31 [0.30]		
Washington	1.31 [0.50]	1.19 [0.40]	1.42 [0.71]
Oregon	1.27 [0.33]	1.31 [0.48]	1.26 [0.34]
Kentucky	1.26 [0.28]	1.34 [0.43]	1.18 [0.24]
Massachusetts	1.25 [0.76]	1.52 [1.50]	0.98 [0.10]
Rhode Island	1.25 [0.48]	1.00 [0.22]	1.50 [0.77]
Indiana	1.23 [0.42]	1.24 [0.54]	1.23 [0.45]
North Dakota	1.23 [0.34]	1.23 [0.34]	1.23 [0.43]
West Virginia	1.22 [0.38]	1.31 [0.70]	1.13 [0.19]
South Dakota	1.19 [0.27]	1.19 [0.41]	1.19 [0.30]
Virginia	1.17 [0.30]	1.23 [0.70]	1.16 [0.34]
Maine	1.16 [0.26]	1.09 [0.16]	1.24 [0.38]
Wisconsin	1.13 [0.20]	1.13 [0.24]	1.12 [0.19]
New Hampshire	1.07 [0.19]	1.07 [.15]	1.07 [0.26]

Note: State indices are based on David and Eisenberg (1961).

Table 2. Cross-Sectional Analyses: Revenues and Votes, 1960 and 1980

Independent Variable	1960 Log Revenue Per Capita	1960 Log Revenue Per Capita	1980 Log Revenue Per Capita	1980 Log Revenue Per Capita
Log (RTV_{1960})	.33 (.01)	.18 (.01)	.11 (.01)	.05 (.01)
1960 Poverty Rate		.16 (.03)		
1960 Per Capita Income		-.23 (.04)		
1960 Governor Turnout		.21 (.02)		
1980 Poverty Rate				.16 (.01)
1980 Per Capita Income				-.32 (.03)
1980 Governor Turnout				-.08 (.02)
N	3022	3022	3022	3022
R^2	.30	.41	.04	.17

Note: Dummy Variables included for each state in all regressions.

Table 3. Panel Analyses: Change in Revenue and Change in Votes, 1960 and 1980

Independent Variable	Δ Log Revenue Per Capita, 1960-1980	Δ Log Revenue Per Capita, 1960-1980
$-\text{Log}(RTV_{1960})$.21 (.01)	.17 (.01)
Δ Poverty Rate		.14 (.01)
Δ Per Capita Income		-.01 (.03)
Δ Governor Turnout		.14 (.03)
N	3022	3022
R^2	.14	.18

Note: Dummy Variables included for each state in all regressions.

Table 4. Effects of Redistricting on Growth in State Transfers
 Dependent Variable = Δ Log Total Revenue Transferred per Capita
 Observations are changes in state aggregates from 1960 to 1980

Independent Variable	Specification					
-Avg(Log RTV)	.23 (.17)			.22		
-SD(Log RTV)		.21 (.18)			.11 (.22)	
-Hi to Lo (Log RTV)			.06 (.04)			.04 (.04)
Δ Log of State Population				-.24 (.27)	-.34 (.24)	-.33 (.21)
Δ Log of State Poverty Rate				-.28 (.22)	-.24 (.21)	-.28 (.22)
Δ Log of State Income				-1.42 (.69)	-1.26 (.66)	-1.28 (.65)
N	47	47	47	47	47	47
R^2	.02	.01	.03	.09	.08	.09

Table 5. Difference in the Predicted Distribution of 1980 Spending Between Two Scenarios: (1) Had the 1960 State Legislative District Boundaries Remained (Y^*) and (2) Under the 1980 Boundaries (Y_{80})

Percentile of <i>RTV</i> ₁₉₆₀	Average <i>RTV</i> ₁₉₆₀	Total 1980 Population (<i>Pop</i> ₈₀)	Avg. Predicted Difference in Annual Per Capita Transfers ($Y^* - Y_{80}$)	Predicted Difference in Annual Total Transfers ($Pop_{80}(Y^* - Y_{80})$)
0 to 5	0.49	61,737,000	-86.62	-5,409,000,000
5 to 10	0.72	24,964,000	-41.91	-1,046,000,000
10 to 25	0.94	82,363,000	-8.10	-668,000,000
25 to 50	1.22	32,089,000	26.74	858,000,000
50 to 75	1.58	20,405,000	63.13	1,288,000,000
75 to 90	2.09	10,222,000	104.69	1,070,000,000
90 to 95	2.85	2,721,000	153.56	418,000,000
95 to 100	5.50	2,682,000	267.74	718,000,000

Note: Calculations assume that a 1% increase in RTV results in a 2% increase in state transfers. See Table 2 and Table 3 for the complete regression results.