

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

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Abstract

We examine the relationship between legislative representation and public finances. *Baker v. Carr* and subsequent court cases eradicated severe disparities in the population in U.S. state legislative districts. Most prior research, which has focused on the level of state aggregates, found little effect of equalization of representation on the distribution of public money. We examine the geographic distribution of money (to counties), and demonstrate that the equalization of legislative representation had a large effect on who received state expenditures. First, cross-sectional analysis shows that counties with relatively more legislative seats per person prior to redistricting received relatively more transfers 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 over-represented to formerly under-represented 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. Prior to judicial intervention, unequal representation was the norm in U.S. legislatures, and very unequal representation was not uncommon. In 1960, only New Hampshire's and Wisconsin's state legislatures approximated one-person, one-vote in both chambers (David and Eisenberg, 1961). But, 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 transfer of state funds 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 funds 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.

At the time of the *Baker* decision it was thought that equalization of representation would lead to equalization of public expenditures on highways, schools, and other important public programs. An intuitive view of coalition making within legislatures underlies this thinking. To increase their reelection chances, legislators would try to secure funds and programs for their constituencies. In a legislature of equals, each has the same coalition power and each will get an equal share of expenditures (at least, on average across programs). But, citizens would receive markedly different amounts of government funding, on a per capita basis, if legislators represent very different sized populations. Gaining their “fair share” of public funding was, in fact, a central motivation of the plaintiffs in *Baker v. Carr*. The Intervening Petition to the District Court in *Baker* filed by Mayor Ben West of Nashville provides extensive statistical data showing “the direct relationship between excess representation in the unlawfully constituted General Assembly of Tennessee and the obtaining of an excess share in the monies collected by the State of Tennessee ... and the converse, the direct relationship between the lack of proportionate

representation in the General Assembly and the bearing of an excessive and disproportionate share of the expense of government.”¹

Despite these strong expectations, social science research provides at most only weak support for the conclusion that equalizing state legislative representation altered policy outcomes. A spate of papers published in the late 1960s and early 1970s looked at the relationship between state-level measures of unequal 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).² Studies examining changes in state expenditures in the years immediately after redistricting found some effects, but typically the results were mixed and the methods problematic (Pulsipher and Weatherby, 1968; Hanson and Crew, 1973; Fredrickson and Cho, 1970). 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, 1991, pages 292-303).³

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

¹The quote is from pages 2 to 3 of the “Amendment and Supplement to the Intervening Petition Filed by the Plaintiff, Ben West, Mayor, City of Nashville, Tennessee. Civil Action No. 2724. The data covered are school funds and highway revenues, which are distributed through state transfers to local governments. We are grateful to Harris Gilbert, attorney for the city of Nashville in the Baker suit, for providing these materials.

²For a critique of many of these early studies, see Bicker (1971).

³For 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 effects of under-representation in the Senate, but Lee (1998) finds substantively small and statistically weak effects.

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. 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 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; also see Robeck, 1972).⁴ 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. 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 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

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For example, Erikson (1971) shows that the partisan effects of reapportionment varied dramatically across states. He finds that for some states reapportionment helped Republicans (e.g. Arizona, Nevada, Iowa, Washington) while in others the Democrats benefitted (e.g. California, Connecticut, Idaho, New Hampshire). Averaging across states, there was only a small shift in favor of the Democrats.

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 1991). 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, *et al*, 1999; Samuels and Snyder 2000; Jones, *et al*, 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. We call this the Relative Representation Index (RRI)⁵ A county with an index value equal to 1 has representation equal to the ratio one would expect under an exact one-person, one-vote rule. Values less than 1 reflect under-representation 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:

$$C_i = \sum_{(j|f_{ij} > 0)} f_{ij} \frac{M_j}{P_j}$$

⁵ David and Eisenberg use the term “Right-To-Vote Index.”
 $RRI_i = C_i / [J/P]$.

The RRI is equal to C_i normalized by average fraction of representatives per county in the state:

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 1.5.⁶

Following David and Eisenberg, the RRI 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. By 1972, populations of state legislative districts were nearly equal in every state (*Book of the States, 1972-1973*, pages 64 and 65). Throughout, we treat the RRI 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 RRI was 1.65, indicating that the average county had about 65 percent more representation than it would have if its share of the legislature equaled its share of the state population. The average within-state standard deviation in the RRI was 1.23. For the upper houses, the average value of the RRI 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 one or two percent. In an ideal world, this would make the RRI equal to 1 in all counties; the mean would then be 1 and the standard deviation would be 0.

Table 1 presents the means and standard deviations of the RRI for the upper and lower houses for each state in 1960, as well as the RRI for each state's entire legislature (the average of

⁶When a county is split across more than one district the RRI 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 $C_i = 2/3 (1/20,000) + 1/3 (1/10,000)$.

lower and upper house RRI). New Hampshire's upper and lower houses are closest to equal county representation, with mean RRI 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 RRI 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 RRI 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 RRI 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 tended to reflect urban-rural divisions, but this is only part of the story. The correlation between the RRI (in logarithms) and the county population (in logarithms) is -.58. In 20 states, the correlation is above -.9. However, suburban counties were often as poorly represented as urban counties. In New York state, Nassua, Suffolk, and Westchester counties were more under-represented than New York City. In Illinois, Lake and Dupage counties had less representation than Cook county (Chicago). In Maryland, the City of Baltimore had three times as many legislative seats per person as neighboring Baltimore county. Some rural counties were also badly under-represented in some states. Tennessee, for example, gave fewer legislative seats per capita to rural counties in the eastern half of the state. Because urban residents tend to vote Democratic and suburbanites tend to vote Republican, the expansion of the franchise often had uneven effects on the partisan composition of state legislatures (see also Erikson, 1973).

The exceptions to the under-representation of metropolitan areas deserve mention for a methodological reason. Noting the under-representation of urban areas, some studies have examined the

level of state expenditures on “urban” programs, or total state intergovernmental transfers to the largest urban counties, using the states as the units of observation (Brady and Edmonds, 1967; Fredrickson and Cho, 1970). This approach introduces a large amount of measurement error, because “urbanness” is a highly imperfect measure of “under-representation,” especially when the data are aggregated to the state level. As a result these studies probably yield biased estimates.

B. Measuring Public Expenditures

We seek to explain the distribution of public money to counties. The Census of Governments is conducted every 5 years; we use data from 1957, 1962, 1967, 1972, 1977, and 1982. We focus on the first two and last two years in this series. The years 1957 and 1962 depict expenditures before Baker. Battles over equalization of district populations occurred mainly from 1962 through 1968. Because of lags in districting, budgeting, and legislative organization, it is difficult to pinpoint when changes in representation should begin to affect transfers. Transfers to counties in 1977 and 1982, thus, measure the distribution of expenditures once one-person, one-vote is in place. To smooth over year-to-year variations in expenditures, we average the 1957 and 1962 reports and the 1977 and 1982 reports. Analysis of each year separately shows the same pattern as the pairs of years combined.

We study total transfers from states to all local governments within counties. Though certainly not all state money, these transfers account for a large share of state expenditures: 35 to 38 percent of all money in this time frame. Roughly half of all money transferred to local governments is for education, one-fifth is for highways and roads, and one-sixth is for general aid to local governments.⁷

Our motivations for examining this variable are three-fold. First, this is the variable used

⁷For more details on the composition of intergovernmental transfers see the *Census of Governments* volume, *State Payments to Local Governments*.

in much of the past research on this topic, and with which researchers looking at the state-level measures found no effects. We will show that using the same variable, completely different results hold once the data are disaggregated. Second, the geographic distribution of transfers is readily identified. It is very hard to determine the geographic distribution of the remaining 60 percent of state expenditures. The data referred to by the Courts in *Baker* and other cases usually focused on an even smaller subset, usually just highway funds. Third, total state transfers to counties cumulates a large number of different programs, and we expect should reflect influence of legislators on public finances generally. Some researchers have tried to assess the effects of redistricting on specific programs. Measuring the effects of representation on isolated programs may be problematic if there is vote trading or logrolling across programs. 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.

There is one important detail regarding our main dependent variable. State intergovernmental transfers to local governments is not entirely composed of money that originates inside the state, but includes some federal “pass through” money -- funds sent from federal accounts to the states that is then transferred to local governments. This is an accounting issue and practices vary state by state.⁸ In only a few states are federal pass-throughs larger than about 15% of total state intergovernmental transfers. When we exclude these states from our analysis, the results are unchanged.

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

⁸In all states, pass throughs include some portion of federal spending on Title I aid to education, school lunch programs, and vocational and adult education. In some states, pass throughs also include federal funds for programs such as hospital construction and local health services, disaster relief, airport construction, and forest reserve payments. For further details, see *Census of Governments* volume, *State Payments to Local Governments*. For a discussion of the implementation of federal education programs, see Chubb 1985.

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.

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 counties' 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 under-represented 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. 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 our measure of county representation, many other variables might affect the distribution of public money. We address this in three ways.

First, we include variables for demographic, socioeconomic, and political factors. To minimize the danger of omitted variable bias we include in our multivariate analysis the poverty and unemployment rates, median income, percentages of the population that are school-aged, black, and elderly, and population change (growth). Poverty and income are included because many state spending programs transfer money to low income citizens. Unemployment rates are included because some of the programs consist of short-term relief. School-aged population is one of the most important predictors of transfers because approximately half of all money transferred from states to locales is for education. Percent black and elderly represent important populations often targeted for assistance. Population change captures the fact that budgets may lag behind demographic changes, and so per capita transfers may be temporarily depressed in fast growing counties.

Aside from per capita representation, several political factors that may affect the distribution of state money include county voter turnout and partisanship, party control of the legislature and governorship, and the relative power of the governor versus the legislature. Counties that have high turnout rates may be expected to receive more state money, since for state level elected officials the political rewards to providing per capita transfers to high turnout counties will, all things equal, be greater. We measure county partisanship by county gubernatorial vote. We interact county partisanship with variables for party control of the state legislature and party of the governor to control for the possibility that politicians favor those

counties with more agreeable partisan leanings.⁹

Second, we run “over time regressions” in which we regress the change in each county’s share of state transfers on the change in the county’s representation. This eliminates the influence of any omitted variables that are approximately constant over time.

Finally, we include dummy variables for each state in all model specifications. In our cross-sectional regressions, inclusion of state level dummy variables eliminates the need to include any additional variables that are restricted to the values zero or one, such as variables for region of the country or “democratic majority in the state legislature.” In our “over-time” regressions, where we regress the change in a county’s share of state transfers on the change in that county’s representation, including a dummy variable for each state captures the effect of any contemporaneous changes in state level variables, such as the state’s adoption of the line item veto or a change in majority party control of the state legislature. Descriptions of the variables employed in the analysis can be found in a table in the appendix (Table A).

3. Representation and the Distribution of Funds within States

Our measurement of the effects of representation on spending breaks neatly into two empirical 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?

We address the first question by examining the relationship between representation and transfers across counties in 1960. We examine the second question by examining the relationship between representation and transfers across counties in the 1980s, and by examining changes in

⁹There is a very extensive literature investigating partisan and ideological effects on budgeting both at the federal and the state levels, with mixed findings. See, for example, Key (1949), Dawson and Robinson (1963), Dye (1966, 1984), Sharkansky (1969), Sharkansky and Hofferbert (1969), Winters (1976), Marquette and Hinckley (1981), Wildovsky (1984), Plotnick and Winters (1985), Garand (1985, 1988), Barilleaux and Miller (1988), Erikson, Wright and McIver (1989), and Levitt and Snyder (1995). Others variables discussed in the literature include local factors such as local lobbying efforts to secure grants, political ideology, and bureaucratic capacity (see Chubb 1984, Rich 1989).

transfers and representation over this twenty-year period.

A. Differences Across Counties

A.1. The Case of Florida

We begin by considering a single state: Florida. Florida is, by our measure, the state with the most malapportionment in the lower house (where budgets typically originate), and the second most malapportionment overall. The high variation in per capita representation in Florida, across a large number of counties, provides us with a good case for drawing attention to the overall pattern of interest.¹⁰

The pattern of Florida's district populations prior to *Baker* is typical of many states. Large cities and their surrounding suburbs were under-represented because the state legislature refused to draw new district boundaries in response to dramatic demographic changes. The 1962 election in Florida was held under district lines dating to the 1920s. Since the 1920s, Dade county, which includes the city and most of the suburbs of Miami, had grown tremendously. This population growth did not lead to additional legislative representation. The legislature refused to redistrict in 1930, and intense battles in the 1940s and 1950s turned back districting plans, even though the state passed an initiative "requiring" the legislature to abide by the state law requiring equal populations. By refusing to approve new boundaries the rural counties maintained a disproportionate share -- though not a majority -- of seats in both the upper and lower houses of the legislature. As a consequence of the malapportionment of the legislature, counties with relatively small populations dominated state legislative politics. Rural legislators'

¹⁰The extreme malapportionment in Florida corresponds to large variation in per capita representation across counties. This case therefore satisfies the "principal of maximum difference." (See King, Keohane, and Verba, 1994, Chapter 4 for more details on case selection). Although the degree of malapportionment in Florida is extreme, we report later that the pattern observed there repeats itself in nearly every state. We chose Florida rather than Nevada because Nevada has too few counties to permit systematic data analysis.

share of committee positions mirrored their share of state legislative seats, and they dominated the leadership posts (Havard and Beth, 1962, Chapter 5).

Figure 1 shows that Florida's public finances at the time reflected the degree of malapportionment of the legislature. Figure 1 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 RRI (in logarithms). Because the measures are in logarithms, a value of zero means that the county received a share of revenues equal to its share of the state population. The observations for 1960 are signified by a "o" and the observations for 1980 with a triangle. In Figure 1 there are 67 observations for each year, 1 observation for each of Florida's counties.

[Figure 1]

The strong relationship between county representation and county transfers per capita is clear from visual inspection. A bivariate regression of log of share of state revenues in 1960 on log of RRI in 1960 is plotted in the figure. The slope equals .41 (se=.02); the R-squared = .83. A county with twice as much representation as another county received approximately forty percent more of the state's transfers.

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. It is always possible that the relationship between transfers and legislative representation is spurious, and more money just happens to be going to under-represented areas for some other reason. Unless these (alleged) omitted factors are transient though, we would expect that the pattern linking transfers to county representation observed in 1960 would remain very strong in 1980, even after the legislative redistricting that equalized representation. On the other hand, if representational inequalities drive state transfers, we expect substantial equalization of transfers by 1980. Figure 1 also plots the relationship between transfers and representation for 1980, and it shows a much flatter relationship. Regressing revenues shares in 1980 on vote shares in 1960 yields a slope of .15 (se = .04) and an R-squared of just .22. There is still some tendency to favor the formerly under-represented counties, but the coefficient has fallen by over 50% once legislative power is equalized.

The results are even more impressive when we control for other factors that are expected to affect the flow of state money. Controlling for county per capita income, gubernatorial turnout, the percent in poverty, percent school-aged, percent elderly, percent black, population growth, unemployment, and committee and leadership positions of representatives, the slope in the early period equals .29 (se = .031), and, in the latter period, the slope falls substantially, to .03 (se=.04)--no longer different from 0 at the .05 level. This implies that the advantages enjoyed by the over-represented counties vanish once they lose their political edge.

A.2. National Sample

Looking across all states, we find that the results from Florida illustrate a more general relationship. Figures 2A and 2B display the relationship between revenue shares and vote shares (both in logarithms) for the national sample. 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. Figure 2A presents the relationship for 1960; Figure 2B the relationship between revenues in 1980 and RRI in 1960.

[Figures 2A and 2B]

Immediately before the Court imposed one-person, one-vote on the state legislatures, there was a strong positive relationship between legislative seats per person and state intergovernmental transfers per person. 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 distribution of funds to counties was equalized substantially. The slope on the line relating revenues to 1960 RRI falls to .10 (se= .01).

Controlling for other factors reduces the estimated effect of representation somewhat, but the effect is still very large and highly significant. Table 2 reports regressions predicting counties' relative shares of state transfers controlling for political and demographic factors. The estimates in Table 2 confirm that unequal district populations correspond with substantial inequalities in the shares of funds in the 1960s. The first two columns of Table 2 present different specifications of the regression. The coefficient on the Relative Representation Index is .34 (se = .01) without any control variables. Controlling for other demographic and political

factors, the coefficient on relative representation is .17 (se = .01). Doubling a county's representation is predicted to increase its share of state money by 17 percent. Holding other factors constant, then, the counties' representation has a substantial effect on its share of state revenue.

[Table 2]

Focusing on the second column of Table 2, we find that the effects of the control variables in the regression generally square with our intuitions regarding the ideological commitment to redistribution associated with the New Deal and Great Society, and arguments from the public finance literature about government spending. Areas with higher poverty rates and lower median income received substantially more state revenue per person. More money flowed to areas with higher unemployment rates. Counties with more persons in school received more state money, though there was no significant effect of high percentages of old or black citizens. Consistent with the possibility that budgeting lags population growth, areas experiencing faster population growth receive less per capita state transfers. With the exception of the variable for county turnout, the political controls have no important effect. Consistent with Husted and Kenny (1997) and Stromberg (1999), counties with relatively high turnout in statewide elections receive relatively more revenue per person.

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 RRI in 1960 should have little or no effect on the distribution of revenues. The last two columns display regression results using the values of the dependent variable and the control variables twenty years later. For the later period, the coefficient on RRI 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 .04 (se = .01). A county with twice as much representation in 1960 as the typical county is predicted to receive only 4 percent more money by 1980. The residual effect might reflect the persistence of agrarian legislators in positions of power in many state legislatures.

The signs and magnitudes of the coefficients on the control variables in the 1980 regression are similar to those observed in the 1960 equation. As in 1960, counties with higher poverty and unemployment rates received substantially more transfers per person, as did

counties with a higher percentage of the population in school. Higher turnout was also associated with higher transfer levels. One interesting change is that in 1980 spending appears to be somewhat more targeted to counties with a higher percentage of black citizens. Also, counties with a higher percentage of Democratic voters received higher transfers, at least in states where the legislature was not solidly Republican. In Republican-controlled states, Democratic counties fared no better than Republican counties.

Given the well known regional differences in American politics, we examined whether the patterns we detected nationally also held within the South by re-running the regressions reported in Table 2 separately for the Southern and non-Southern states. Regional variation did not prove substantively important. We also examined whether the percentage black in a county altered the relationship between RRI and transfers, and we found no interaction effect.

The key result from the cross sectional analysis is that counties that had less representation received relatively less money in the pre-*Baker* era. 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 qualitative conclusions 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 over-representation in the legislature, and a large share of the state's transfers. 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 1968. 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 and changes in shares of state legislative seats. 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 RRI in the mid 1970s (after the 1972 redistricting) and log of RRI in the 1960s. Because district populations are virtually identical by 1974, the change in representation is equal to one minus the RRI in 1960, or, in the logarithmic scale, the change in the RRI measure equals $-\log(\text{RRI})$. The horizontal axis in Figure 3, then, equals the negative of log of RRI 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]

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 following redistricting 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 an analysis predicting changes in revenue shares from 1960 to 1980. All regressions contain dummy variables for each state. The first column contains the regression results using only the negative of the log of RRI in 1960 as a predictor. The second column contains the regression results including changes in the control variables shown in Table 2, as well as state dummy variables.

[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 16 percent. The estimated coefficient on $-\log(\text{RRI})$ in the multivariate regression reported in Table 3 equals .16 ($se = .01$). Significantly, this result is very close to the cross-sectional estimates we report in Table 2, as is expected if the cross-sectional results are not spurious. The effects of the control variables are, again, generally consistent with our expectations and the results reported in Table 2. We also tested for possible interactions between our representation index and race, region, and gubernatorial power. None of these variables have statistically significant or substantively important effects.

Looking state-by-state shows that the pattern is remarkably robust. We performed bivariate regressions of the effect of RRI on county transfers state-by-state for all states with more than 10 counties. In 32 cases the coefficient on RRI was positive and statistically significant at the .05 level. In 8 cases the coefficient was positive, but statistically insignificant. In only 4 cases was the coefficient negative. In only one case (South Carolina) was the estimate statistically significant; the remaining three cases (North Dakota, Oregon and Washington) were among the least malapportioned states.

Our analysis shows that areas that gained voting power during the 1960s and 1970s received disproportionately large increases in state transfers. We have interpreted this as a causal relationship. It is theoretically possible that the association is spurious; cities tended to gain seats following redistricting, and the 1960s witnessed greater public attention to the problems of urban areas. To test for this possibility we reran the regressions reported in Table 3, after excluding the 100 counties with the largest 1960 populations (the cutoff was counties with at least 300,000 people). We find that the estimated coefficient on RRI falls only slightly, from .23 to .21, when no controls are included, and from .16 to .15 when control variables are included. This shows that changes in state transfers tracked changes in political power even in the nation's suburbs and rural areas.

C. The Effects of Political Institutions

It is possible that the effects of malapportionment vary across political circumstances. For example, having a disproportionate share of legislative power may be more valuable in those

states where the legislature is relatively more powerful. We attempt to gauge the importance of this possibility by adding an additional variable capturing the interaction between the Relative Representation Index and a measure of Governors' power. We experimented with a variety of measures, including the presence of a line item veto, appointment power, budgetary power, and overall power.¹¹ None of these variables is statistically significant, and including the variables has no effect on the coefficient on RRI. There was also no difference in the results in states with and without the initiative.¹²

In addition, it is possible that the amount of transfers a county receives is influenced by such factors as whether the county's representatives are legislative leaders or on key committees. This historical data can be recovered, but requires examination of each state's legislative handbook or journal year by year, and then, in many cases, also matching up districts to counties. To the best of our knowledge, no previous work has assembled a comprehensive data set containing this information. We undertook an exploratory analysis, gathering leadership and committee data for 8 states (California, Colorado, Florida, Iowa, Maryland, Michigan, Nebraska, and New York). Detailed studies of these states show that political leadership and positions on powerful committees had no statistically significant or substantively large effect on the distribution of transfer spending.¹³ The coefficient on RRI did not change when the additional control variables were added to the model. It is possible that a more comprehensive analysis would find some effects in some states, but it is unlikely that this would alter our conclusions regarding the importance of reapportionment.

D. Effects on the Level of Overall State Transfers

¹¹There is an extensive literature assessing the effect of the line-item veto on state spending levels. See, for example, Wells (1924), Abrams and Dougan (1986), Carter and Schap (1990), and Abney and Lauth (1997).

¹²We also examined the effect of adding additional variables to measure interactions between RRI and open meeting laws, party control of the legislature, and divided government in the cross-sectional regressions. We find that none of these effects were either substantively or statistically significant.

¹³This is consistent with the findings in many studies of federal spending, such as Ritt (1976), Ray (1980), Owens and Wade (1984), Rich (1989), and Levitt and Snyder (1995).

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, under-represented areas may have had higher demand for public expenditures than over-represented areas. Under-represented 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 benefitted voters who were over-represented 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 (*e.g.*, Lindert, 1996; Husted and Kenny, 1997).

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 state's 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 RRI, the standard deviation of the log of RRI, and the difference in log of RRI between the county with the most representation and the county with the least representation (labeled RRI range). 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 RRI and of SD Log RRI are approximately .2 and the standard deviation of Hi-To-Lo Log RRI 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 Appendix B, 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:

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

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, RRI_{60} is the RRI 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).¹⁴

Using this formula we can generate predicted transfers to each county. Because the RRI does not follow the normal distribution, the means and variances of the RRI 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 RRI measure: 5th percentile, 10th percentile, 25th percentile, 50th percentile, 75th percentile, 90th percentile, and 95th percentile.

Table 5 shows the distribution of RRI 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 RRI measure and the value of the RRI at that percentile. Equalization of district populations creates an RRI equal to 1; this is approximately the value of the RRI 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 RRI, equal votes increased

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In 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.

state revenues by \$88 per person per year. In the most over-represented counties, one-person, one-vote reduced state revenues by \$268 per person per year. The cumulative effect was to shift approximately \$7 billion toward counties that had been under-represented prior to the imposition of one-person, one-vote. This redistribution amounts to taking approximately 10 percent of the revenues received by people in over-represented counties and shifting them to the under-represented counties.¹⁵

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.

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 quite responsive to constituency demands. It is difficult 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

¹⁵The very small standard errors for the RRI coefficient imply that the prediction standard errors for Table 4 are very small. In the extreme categories the 95 percent confidence interval is plus or minus 2.2 percent of the predicted values. Among counties with RRI in the lowest 5 percentile, the predicted gain in revenues transferred is \$5.3 billion plus or minus \$11.8 million. Among counties with RRI in the highest 5 percentile, the predicted loss in revenues transferred is \$717 million plus or minus \$16 million. At the mean the prediction confidence interval is even smaller.

directly to specific mechanisms, such as the pivotalness of the median, but they do reinforce the general point that public policy strongly reflects constituents' interests.

Second, our results suggest that at least with respect to the geographic distribution of public funds, legislative politics involves bargaining among equals, rather than bargaining among pre-determined blocs that may be quite unequal in strength. In the pre-*Baker* era, rural legislators and interest groups, and some scholars, argued that over-representation of rural areas was necessary in order to prevent the large urban areas from dominating the state legislatures via their bloc voting and thereby taking more than their “fair share” of public expenditures (see, *e.g.*, Perrin, 1962, and de Grazia 1962, chapter 5). That is, equal representation would lead to the tyranny of the *urban* majority. We find that the over-represented areas, which were predominantly rural, received more than their “fair-share” before *Baker*, and the equalization in representation associated with *Baker* led to greater equalization of per capita transfers. In fact, in the post-*Baker* era there was essentially *no* relationship between urbanization and the distribution of intergovernmental transfers.¹⁶ These results are more consistent with a simple view that, in distributive politics, it is every district for itself.

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. 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.

¹⁶

If we add percent urban or county population density to the cross-sectional specification shown in Table 2 for 1980, we find no substantively or statistically significant effects for either variable.

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 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.

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Table A. Variable Definitions and Data Sources	
Variable Name	Definition
State Transfers Per Capita, 1960 ¹	$\log((\text{relative per capita intergovernmental transfers in 1957} + \text{relative per capita intergovernmental transfers in 1962})/2)$
State Transfers Per Capita, 1980 ¹	$\log((\text{relative per capita intergovernmental transfers in 1977} + \text{relative per capita intergovernmental transfers in 1982})/2)$
Population Growth Rate, 1950-60 ²	$\log(\text{relative population in 1960}) - \log(\text{relative population in 1950})$
Population Growth Rate, 1970-80 ²	$\log(\text{relative population in 1980}) - \log(\text{relative population in 1970})$
Population Change, 1960-1980 ²	$\log(\text{relative population in 1980}) - \log(\text{relative population in 1960})$
Per Capita Income, 1960 ²	$\log(\text{relative per capita income in 1960})$
Per Capita Income, 1980 ²	$\log(\text{relative per capita income in 1980})$
Percent Poor, 1960 ²	$\log(\text{relative percent of families} > \$3000 \text{ in 1960})$
Percent Poor, 1980 ²	$\log(\text{relative percent of families in poverty in 1980})$
Percent in School, 1960 ²	$\log(\text{relative percent of population enrolled in grades K-12 in 1960})$
Percent in School, 1980 ²	$\log(\text{relative percent of population enrolled in grades K-12 in 1960})$
Percent Unemployed, 1960 ²	$\log(\text{relative unemployment rate, civilian labor force, in 1960})$
Percent Unemployed, 1980 ²	$\log(\text{relative unemployment rate, civilian labor force, in 1980})$
Percent Black, 1960 ²	Relative percent of population that is black in 1960
Percent Black, 1980 ²	Relative percent of population that is black, 1980
Percent 65 and over, 1960 ²	$\log(\text{Relative percent of population that is age 65 or older, 1960})$
Percent 65 and over, 1980 ²	$\log(\text{Relative percent of population that is age 65 or older, 1980})$
Percent Turnout, 1960 ³	$\log(\text{relative percent of voting age population voting in gubernatorial elections, average 1958-62})$
Percent Turnout, 1980 ³	$\log(\text{relative percent of voting age population voting in gubernatorial elections, average 1978-83})$
Percent Democratic Vote, 1960 ³	$\log(\text{relative percent voting democratic in gubernatorial elections, average 1958-1962})$
Percent Democratic Vote, 1980 ³	$\log(\text{relative percent voting democratic in gubernatorial elections, average 1978-1983})$
Republican Control, 1960	Republicans had a majority in both chambers of the legislature 60% or more of the time from 1953 to 1962.
Republican Control, 1980	Republicans had a majority in both chambers of the legislature 60% or more of the time from 1973 to 1982.

Note: Footnotes refer to data source. ¹Census of Governments, various years. ² City and County Databook, various years. ³ General Election Data for the United States, 1950-1990, ICPSR study #13. Data on line item veto is from Book of the States. Gubernatorial power indices are from Schlesinger 1965, Beyle 1983. Data on committee and party leadership positions from various state legislative handbooks. Party control of legislatures from Book of the States, selected years.

Appendix B. 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:

$$\frac{Y_{80}}{Y} = \alpha + \beta \left(\frac{R_{80}}{R} \right) + \gamma Z,$$

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

$$\frac{Y^*}{Y} = \alpha + \beta \left(\frac{R_{60}}{R} \right) + \gamma Z,$$

where Y^* is the predicted level of transfers to the county in 1980, when RRI equals RRI_{60} , all else held equal to the 1980 values.

First, solve equations (1) and (2) to express expected revenues in terms of current and past levels of RRI:

$$Y_{80} = \alpha Y + \beta \left(\frac{R_{80}}{R} \right) Y + \gamma Z Y,$$

$$Y^* = \alpha Y + \beta \left(\frac{R_{60}}{R} \right) Y + \gamma Z Y,$$

Subtracting the first equation from the second yields:

$$\frac{Y^*}{Y} - \frac{Y_{80}}{Y} = \beta \left(\frac{R_{60}}{R} - \frac{R_{80}}{R} \right).$$

This can be rewritten as:

$$Y^* - Y_{80} = Y \left(\frac{R_{60}}{R} - \frac{R_{80}}{R} \right) \beta.$$

Second, contrast Y^* with Y_{80} . We are interested in the difference between what would have been 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),

$$Y^* - Y_{\theta} = Y_{\theta} \left[\left(\frac{R}{R_{\theta}} \right)^{\beta} - 1 \right].$$

Since county representation is virtually identical in 1980, $RRI_{80} = 1$. Using our estimates above, set $\beta = .2$, so:

$$Y^* - Y_{\theta} = Y_{\theta} [(R / R_{\theta})^{.2} - 1].$$

Table 1			
Relative Representation Index in State Legislatures, 1960			
State	2-Chamber Average Average [St. Dev.]	Lower House Average [St. Dev.]	Upper House Average [St. Dev.]
Nevada	5.64 [6.37]	3.55 [3.17]	7.74 [9.62]
Florida	3.83 [3.15]	4.28 [4.31]	3.38 [2.81]
California	3.54 [3.17]	1.20 [.37]	5.89 [6.25]
Arizona	2.71 [2.00]	1.39 [.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 [.91]	2.36 [1.80]	1.46 [.62]
Colorado	1.81 [.61]	1.81 [.74]	1.82 [.64]
Alaska	1.73 [.70]	1.71 [.80]	1.74 [.88]
Michigan	1.69 [.72]	1.45 [.45]	1.93 [.77]
Missouri	1.69 [.72]	2.22 [1.36]	1.17 [.24]
Oklahoma	1.69 [.79]	1.67 [.84]	1.70 [.93]
Hawaii	1.66 [.74]	1.23 [.27]	2.08 [1.22]
New Jersey	1.66 [1.18]	1.13 [.49]	2.20 [1.94]
New York	1.66 [.74]	2.00 [1.37]	1.33 [.27]
Alabama	1.60 [.85]	1.65 [.95]	1.55 [.92]
Connecticut	1.57 [.77]	1.87 [1.14]	1.27 [.47]
North Carolina	1.55 [.95]	1.86 [1.66]	1.24 [.40]
Pennsylvania	1.55 [1.05]	1.75 [1.94]	1.36 [.45]
Tennessee	1.50 [.82]	1.56 [1.31]	1.45 [.64]
Texas	1.49 [.34]	1.44 [.48]	1.54 [.40]
Ohio	1.48 [.67]	1.90 [1.28]	1.06 [.22]
Delaware	1.45 [.73]	1.43 [.71]	1.46 [.76]
South Carolina	1.44 [.74]	1.11 [.28]	1.78 [1.26]
Louisiana	1.43 [.62]	1.54 [.84]	1.32 [.66]

Minnesota	1.43 [.45]	1.50 [.64]	1.37 [.50]
Wyoming	1.43 [.64]	1.26 [.46]	1.61 [.96]
Illinois	1.42 [.46]	1.18 [.21]	1.68 [.77]
Iowa	1.41 [.56]	1.50 [.68]	1.33 [.57]
Vermont	1.37 [.82]	1.40 [.81]	1.34 [.96]
Mississippi	1.32 [.63]	1.40 [.74]	1.25 [.60]
Nebraska	1.31 [.30]	1.31 [.30]	-----
Washington	1.31 [.50]	1.19 [.40]	1.42 [.71]
Oregon	1.27 [.33]	1.31 [.48]	1.26 [.34]
Arkansas	1.26 [.42]	1.43 [.77]	1.09 [.16]
Kentucky	1.26 [.28]	1.34 [.43]	1.18 [.24]
Massachusetts	1.25 [.76]	1.52 [1.50]	.98 [.10]
Rhode Island	1.25 [.48]	1.00 [.22]	1.50 [.77]
Indiana	1.23 [.42]	1.24 [.54]	1.23 [.45]
North Dakota	1.23 [.34]	1.23 [.34]	1.23 [.43]
West Virginia	1.22 [.38]	1.31 [.70]	1.13 [.19]
South Dakota	1.19 [.27]	1.19 [.41]	1.19 [.30]
Virginia	1.17 [.30]	1.23 [.70]	1.16 [.34]
Maine	1.16 [.26]	1.09 [.16]	1.24 [.38]
Wisconsin	1.13 [.20]	1.13 [.24]	1.12 [.19]
New Hampshire	1.07 [.19]	1.07 [.15]	1.07 [.26]
Note: Relative Representation Index is based on David and Eisenberg (1961).			

Table 2				
Cross Sectional Analyses: Transfers and Votes, 1960 and 1980				
Dependent Variable: Relative Per Capita Intergovernmental Transfers				
Independent Variables	1960	1960	1980	1980
Relative Representation	.34 (.01)	.17 (.01)	.11 (.01)	.04 (.01)
Population Growth Rate		-.19 (.03)		-.33 (.03)
Average Income		-.01 (.04)		-.17 (.05)
% Poor		.22 (.03)		.12 (.02)
% Unemployed		.06 (.01)		.11 (.01)
% In School		.40 (.05)		.45 (.04)
% Age 65 or Older		-.03 (.02)		-.02 (.02)
% Black		-.02 (.02)		.08 (.02)
% Turnout		.19 (.02)		.08 (.03)
% Democrat x Non-Republican Control		.04 (.03)		.14 (.03)
% Democrat x Republican Control		-.02 (.04)		-.08 (.06)
N	3048	3048	3048	3048
R ²	.33	.50	.05	.35
Note: Dummy Variables for each state included in all regressions.				

Table 3		
Panel Analyses: Changes in Transfers and Votes, 1960 and 1980		
Dependent Variable = Change in Relative Per-Capita Intergovernmental Transfers		
Independent Variables		
Changes in Relative Representation	.23 (.01)	.16 (.01)
Change in Population Growth Rate		-.32 (.03)
Change in Population, 1960 to 1980		-.26 (.02)
Change in Average Income		.04 (.03)
Change in % Poor		.14 (.02)
Change in % Unemployed		.02 (.01)
Change in % In School		.56 (.05)
Change in % Age 65 or Older		-.22 (.03)
Change in % Black		.18 (.05)
Change in % Turnout		.07 (.02)
Change in % Democrat x Non-Republican Control Both Periods		.17 (.04)
Change in % Democrat x Republican Control Both Periods		-.36 (.10)
Change in % Democrat x non-R to R-Control		-.36 (.23)
Change in % Democrat x R Control to non-R control		-.07 (.11)
N	3048	3048
R ²	.16	.37
Note: Dummy Variables for each state included in all regressions.		

Table 4						
Effects of Redistricting on Growth in State Transfers						
Independent Variable	Dependent Variable = Change in Total Transfers per Capita					
Change in average RRI	.28 (.18)			.31 (.24)		
Change in Standard Deviation of RRI		.25 (.19)			.26 (.26)	
Change in Range of RRI			.07 (.04)			.05 (.05)
Change in population				-.26 (.24)	-.31 (.23)	-.37 (.22)
Change in average income				-1.45 (.62)	-1.36 (.61)	-1.29 (.61)
Change in % poor				.42 (.26)	-.40 (.27)	-.44 (.26)
Change in percent unemployed				-.41 (.17)	-.41 (.17)	-.38 (.17)
Change in % in School				.48 (.83)	.58 (.83)	.58 (.83)
Change in % age 65 or older				.03 (.14)	.03 (.15)	.03 (.15)
Change in % Black				.71 (.37)	.72 (.39)	.67 (.37)
N	48	48	48	48	48	48
R squared	.05	.04	.09	.31	.30	.30
Note: Observations are for states excluding Alaska and Hawaii.						

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 RRI_{1960}	Average RRI_{1960}	Total 1980 Population	Average Predicted Difference in Annual Per Capita Transfers ($Y^* - Y_{80}$)	Predicted Difference in Annual Total Transfers ($Pop_{80}(Y^* - Y_{80})$)
0 to 5	.49	61,168,000	-87.62	-5,360,000,000
5 to 10	.72	23,984,000	-41.91	-1,005,000,000
10 to 25	.94	72,395,000	-8.10	-587,000,000
25 to 50	1.22	32,074,000	26.74	858,000,000
50 to 75	1.58	20,409,000	63.13	1,288,000,000
75 to 90	2.09	10,130,000	104.69	1,060,000,000
90 to 95	2.85	2,721,000	153.56	418,000,000
95 to 100	5.51	2,678,000	267.74	717,000,000

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

