THE POLICY IMPACT OF REAPPORTIONMENT

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A considerable amount of conflict has been generated over the selection of legal standards for legislative apportionment.[1] Prior to 1962, seats in state legislatures were generally assigned to counties. In some houses of some state legislatures, a limited number of additional seats were given to the most populated districts. Population (size) was seldom used, however, either as the sole basis for designing districts or as the sole basis for determining the number of seats for given districts. With the development of the social force of urbanization, a limited number of counties became the residence for a majority of the citizens. Since state legislatures continued to operate with counties as the fundamental unit of legislative apportionment, significant differences in the population of districts were created.

Individuals residing in most populated districts constructed the argument that the design of districts on the basis of non-population factors violated the U.S. Constitution. They claimed that the value of their votes was less than the value of the votes of individuals living in sparsely populated areas. By measuring the value of a vote in terms of its theoretical probability of affecting the selection of a candidate (election outcome), these individuals argued that the size of representational districts determined the value of a vote. They posited, first, that each individual's vote is some fraction of the total possible number of votes in a district. Their second claim was that as the magnitude of a district's population increases, that fraction decreases. Consequently, they observed that as the size of a district expands, the probability of a voter electing a candidate is reduced and the value of the vote diminishes.[2] Moreover, since electoral choices about office seekers are the summation of individual votes, the proportion of a state's population living in a district is an index of the value of all of the votes in that district. This means that different sized proportions implies

that entire districts are more influential than other districts in electing candidates.

Individuals who regarded their votes as having been diluted by the existing apportionment arrangements took action to remedy this situation by seeking relief in the federal courts. They focused the issue of what criteria should govern the distribution of seats on the fourteenth amendment's "equal protection of the laws" clause. On the basis of that provision, they reasoned, the disparities in the size of districts and the resultant inequalities in the value of the votes in different sized constituencies were discriminatory practices. They believed that residency should not be a basis for the unequal weighting of votes.

From 1946 until 1962, the U.S. Supreme Court and the lower federal courts chose not to adjudicate directly the disputes concerning the legality of non-population based methods of apportionment.[3] The U.S. Supreme Court declined to intervene in the challenges of the existing modes of apportionment on the grounds that such matters involved "political, nonjusticiable questions." This meant that the state legislatures continued their practice of allocating legislative seats to various sized districts. Litigants who opposed the maintenance of disparate sized legislative districts, however, were eventually rewarded for their perserverance. They managed to convince the U.S. Supreme Court of the unconstitutionality of nonpopulation factors as standards for allocating legislative seats. In a series of decisions, the U.S. Supreme Court has acted to establish population as the defining characteristic of legislative districts and to require that the ratio of the legislative seats assigned to a district *to* the size of the constituency be the same for all districts. (*Baker v. Carr,* 1962; *Gray v. Sanders,* 1963; *Lucas v. Colorado Gen. Assembly,* 1964; *Reynolds v. Sims,* 1964; *Wesberry v. Sanders,* 1964).

Because of the binding character of federal court decisions, state legislatures have had to attempt to construct districts that are equal in population. Legislators and advisory panel members have undergone the exercise of using 1970 census tract data in order to satisfy the judicially imposed requirements. Without necessarily accepting the content of the arguments behind the Court's reapportionment decisions, what knowledge can legal scholars and political scientists offer about the probable nationwide impact on major governmental institutions?

REVIEW OF THE LITERATURE

The law reviews contain innumerable essays concerning

major court decisions relevant to the question of legislative apportionment. The content of many of these analyses can be classified under the following general headings: constitutional histories of legislative apportionment (Dixon, 1962; McCloskey, 1962); descriptions of the political context surrounding legislative apportionment (Tyler, 1962; Silva, 1962); difficulties confronting the courts in specifying judical standards in adjudicating disputes about legislative apportionment (Black, 1962; Bickel, 1962; Emerson, 1962; McKay, 1963; Lucas, 1963); and problems of techniques for measuring inequalities in district size (Goldberg, 1962; Weaver and Hess, 1963; Israel, 1962).[4] In some studies, the writers raised questions about the policy impact of the change in the mode of apportionment (Schattschneider, 1962; Sindler, 1962). These essays were written, however, without the benefit of systematic data. The conclusions suggest hypotheses about the policy impact of judicial decisions, but the authors do not test any empirical generalizations in this regard.

Some political scientists and politicians responded to the Court's decisions in *Baker v. Carr* and its companion cases with great enthusiasm and high expectations. A striking example of these reactions is Landau's view that the reapportionment cases have the causal potency of first "urbanizing" the state legislatures and then shifting the political party system to the "urban-national side as against the state-local side" (1965). While Landau's projection remains untested, available evidence does not suggest that the short-run effects of reapportionment are dramatic shifts in the partisan composition of legislative bodies (Erikson, 1971). Nevertheless, some of the acute observers of state politics perceived definite connections between reapportionment and policy impact.

The authors of numerous publications appear to have linked the probability of a voter affecting the selection of a candidate with the ability of a legislator to affect the selection of proposals for allocating resources across some defined set of issues (Adrian, 1960: 202-209; Key, 1956: 64-67; Lockhard, 1969: 322-323; Blair, 1967: 108; Jewell, 1962: 17-21, 1966: 71-73; Baker, 1966). Implicitly, they assumed, first, that each legislative district was very cohesive and, second, that legislators attempted to express faithfully the preferences of their constituents. Given these assumptions, these scholars argued that, if the numerically advantaged districts were similar in voter composition, the legislators representing a minority of the citizens would form a voting bloc and exercise dominant influence over

the selection of proposals.[5] This general outline of the relationship between vote values and policy outcomes was considered to be an accurate description of the existing state of affairs in many legislatures. Because of significant malapportionment, urban legislators constituted a minority within the legislature although they represented a majority of the state's population. It was argued that the meager number of urban legislators emasculated all of their capacity to direct the flow of policy outcomes. On the other hand, a coalition of non-urban legislators was described as acting in concert to withhold programs that could benefit urban districts. With the revitalization of reapportionment, the alleged negative influence of non-urban legislators was expected to become less effective. As a result, major policy changes in the areas of taxation, welfare, education, and transportation were forecast as consequences of reapportionment.

Like the articles in the law reviews, such prognostications were the product of insight, experience, and shrewd observation. Little in the way of "hard data" supported these opinions. These opinions were seriously questioned, moreover, by a second wave of literature in political science journals, which found few, if any, empirical associations between malapportionment and policy outcomes. Based upon empirical analysis, these writings encouraged the expectation that reapportionment would not produce widespread changes in state policy outcomes (Hoffebert, 1966; Dye, 1965, 1965b, 1966; Jacob, 1964; Brady and Edmonds, 1967). Since the publication of this compendium of findings, other empirical studies have demonstrated that malapportionment was related to particular types of state policy outcomes (Pulsipher and Weatherby, 1968; Walker, 1969). Consequently, until evidence is obtained about possible changes in policy outcomes after reapportionment, some scholars can remain committed to the belief that reapportionment is likely to produce cataclysmic changes while others do not anticipate even mild tremors.

The findings of the current research are that reapportionment is associated with important policy changes in the 1960's. By means of a longitudinal analysis, evidence is gathered which can be interpreted as showing that reapportionment preceded changes in the pattern of policy outcomes.

RESEARCH DESIGN

The basic function of the design is to provide a valid test of the association between reapportionment and policy out-

comes. Because the authors of prior studies claim that malapportionment index scores are not highly related to policy outcomes, it is reasonable to suppose that reapportionment might not be a causal determinant of interstate variations in such outcomes. With that presumption, a research format is developed which permits us to determine if an adjustment in apportionment precedes any major policy changes within a given state. The verification of hypothesized connections between reapportionment and variation in intrastate policy outcomes is a basic step in obtaining knowledge about the impact of the judicially ordered changes in the legislative structure.

The methodological framework selected for the purpose of examining the causal efficacy of reapportionment within the boundaries of the individual states is a before and after test. In this context, reapportionment is conceptualized as an event which occurs within the broader time frame of the ongoing process of policy-making. The before period includes observations about the policy outcomes prior to the date of the application of a reapportionment plan. The after period takes in measurable decisional outcomes which happen subsequent to the implementation of the structural reform. The first task of the empirical analysis is to ascertain whether any significant changes in policy outcomes are evident after reapportionment is introduced. The second task is to ascertain whether it is reapportionment or some other antecedent condition that is the source of any observable policy change. In order to attempt to satisfy these research goals, the data are analyzed in a manner approximating the standards of inference that have been proposed for quasi-experimental designs.[6]

Forty-eight states (Hawaii and Alaska are excluded) are the subjects for comparisons of intra-unit variations in policy outcomes. By looking at forty-eight units, this research effort complements the scope of prior analyses of the impact of reapportionment which are close examinations of legislative roll call voting and legislative committee occupancy in a single state (Sharkansky, 1970; Hawkins and Welchel, 1968). Each state is examined for the *first* state election held under the guidelines of a reapportionment plan. Because reapportionment plans are not necessarily adopted simultaneously for both houses of a state legislature, we consider a change in *one* house to be sufficient for a state to be classified as reapportioned. Since state governmental expenditures are adopted as the indicators of policy outcomes, the fiscal year expenditures that

are the result of legislative activity prior to reapportionment are the data set of the before period. Those fiscal year expenditures that are the result of legislative activity under the reapportioned districts form the data base of the after period.

In order to undertake appropriate quantitative analysis, there must be a sufficient number of observations of policy outcomes during both periods. This requirement eliminates most states from being classified as reapportioned because many did not hold an election under a reapportionment plan until 1968. After the election of 1968, there were, at the time that this study was completed, data for only two fiscal years. Thus, all of the states classified as reapportioned underwent the treatment of reapportionment in a state election before 1966. In spite of this common feature, not all of these states received the same dosage of reapportionment. Some states experienced major shifts in district boundaries and the number of urban legislators increased significantly while other states experienced only minor rearrangments.[7] In future studies of the effects of reapportionment, it will be important to estimate the association between the degree of structural modification and the variation in governmental outlays. However, in this exploratory study, attention is not given to this problem. The states in which the legislatures were reapportioned to some degree are listed with the fiscal years for the respective time periods.[8]

	Before	After
New York	1958-66	1967-69
Massachusetts	1958-63	1964-68
Oregon	1958-63	1964-68
South Carolina	1958-63	1964-68
Kentucky	1959-64	1965-68
Delaware	1959-64	1965-68
Georgia	1958-63	1964-68
Mississippi	1959-64	1965-68
Virginia	1959-66	1967-69
Kansas	1958-66	1967-69
West Virginia	1958-65	1966-69
Wisconsin	1958-66	1967-69
Michigan	1958-65	1966-69
Wyoming	1958-65	1966-69
Oklahoma	1958-66	1967-69

The non-reapportioned states are defined as those states in which a reapportioned legislature was not elected during the first five years of the 1960's. This time dimension is imposed because any state legislature elected after 1965 under a reapportionment plan would make allocations beginning with the fiscal year 1968. Since fiscal year 1969 is the last year for which data are available, the limited number of observations obviate classifying such a state legislature as "reapportioned." Instead, those states that did not experience the election of a reappor-

tioned legislature before 1965 are utilized as a control group. Since the states are not randomly assigned to the control group, it is not possible to assume that all features of state political systems other than the dates of reapportionment are randomly distributed across all the states. The lack of random assignment of states into the control group brings impurity into an assumption about the variables that are not controlled statistically. Nevertheless, the policy outcomes of the states in the control group are analyzed in order to determine if the patterns of policy outcomes without the intervention of reapportionment are similar to, or different from, the patterns exhibited in reapportioned states. The nonreapportioned states are divided artificially into before and after periods. The control group can be examined for evidence of policy changes between the two time periods. If factors other than reapportionment are the foundation for policy changes, then such events would be equally probable in both reapportioned and nonreapportioned states. (The assumption of equal probability can not be made because of the nonrandom selection of units for the control group.) Since factors other than reapportionment, such as a sudden influx of federal aid or a social commitment by a governor to a new program area, can occur in between the before and after period, the existence of any policy changes in a reapportioned state cannot be immediately attributed to reapportionment.

The states that are included in the nonreapportioned category include Alabama, Arizona, Arkansas, California, Colorado, Connecticut, Florida, Idaho, Illinois, Indiana, Iowa, Louisiana, Maine, Maryland, Minnesota, Missouri, Montana, Nebraska, Nevada, New Hampshire, New Jersey, New Mexico, North Dakota, North Carolina, Ohio, Pennsylvania, Rhode Island, South Dakota, Tennessee, Texas, Utah, Vermont, and Washington.[9] The before period for all of these states is 1958-63, and the after period is 1964-67. By using 1967 as the end point, there is no state in which a fiscal year expenditure is the product of a legislature reapportioned after 1965.

The canons of inference of before and after tests, applied to the current research, require that the possible policy impact of social and economic processes not be attributed to legislative reapportionment. Yet, it is reasonable to expect that economic growth and cultural transformations are the foundation of changes in a state's policy outcome. Consequently, the control group of nonreapportioned states functions as a means of observing possible policy changes that are produced by variables

other than reapportionment. While it cannot be assumed that there is perfect randomization of social and economic attributes across all 48 states, if increases in urbanization, industrialization, or wealth cause changes in policy outcomes such results are apt to occur in both reapportioned and nonreapportioned states. If that is the case, a policy change in a nonreapportioned state would indicate that it would be invalid to infer that reapportionment is *the* basis of policy changes. Although this design incorporates no explicit controls on intra-unit variation in social and economic conditions, it is possible to determine systematic differences in such conditions between the two groups of states. The similarity between the conditions of the reapportioned states with the nonreapportioned states is measureable. A test of the differences between the levels of the conditions of the two groups of states is relevant to the assumption of near randomization of social and economic variables. If the differences between the two groups of states are not statistically significant, the assumption is strengthened, while the existence of significant differences weakens the assumption.

The conditions that are examined include items used in many other studies of state policy outcomes; they are: urbanization, wealth, and industrialization. They are operationalized as the percentage of residents living in urban areas, per capita income, and the labor force in manufacturing, respectively.[10] Data collected for the years 1960 and 1966 are analyzed according to a *difference of means* test. The null hypothesis is that the average numerical value (mean) for each of the two groups of states at a given date is the same. Results of the tests indicate that there are no significant differences in these variables in either of the two time periods. The t-values (which are measures of the comparison of the two separate mean scores) are −.690 and −.673 (urbanization), .958 and 1.325 (wealth), and −0.872 and −.646 (industrialization). None of them is significant at the .05 or .01 level of significance with 46 degrees of freedom. Since the null hypothesis is not rejected, the assumption of near randomization is supported.

Some explanation is in order about the confirmatory-bases for the hypotheses. There are two sets of data to be used as the bases for tests of verification of the hypotheses. The first set consists of state governmental expenditures for various areas of policy outcomes. These data represent the total amount of state expenditures in a given fiscal year for specified functional areas. The areas selected are the following per capita spending items:

higher education, inter-governmental expenditures for education (local schools), highways, public welfare, and hospitals.[11]

There are at least two reasons for operationalizing policy outcomes as state governmental expenditures. First, some advocates of reapportionment argued that reapportionment would result in more extensive social welfare programs (Baker, 1955: 23-26, 32-39). Since some of these programs would require state governmental financing, the level of state expenditures is a measure of the decisional outcomes to allocate resources to these programs. Second, the conclusions drawn from existing analyses of state governmental expenditures are that levels of expenditures do not readily change over time. Incremental budgeting is inferred to be a method of decision-making which results generally in only marginal increases in expenditure levels (Sharkansky, 1968). Hence, if there are unexpected changes in expenditures after reapportionment, the finding will be an indication that reapportionment is sufficient to overcome the established pattern of incremental changes. The justification for selecting these particular expenditure items is twofold. First, if reapportionment has an impact on policy outcomes similar to the type that the advocates of reapportionment stated it would have, then these items would indicate such an impact. Second, to allow for some comparability of the current findings with prior research, expenditure items are used. There is a limitation on duplicating all of the measures used in preceding studies, however, because some of the previously used indicators either are not available on an annual basis or are measures of both local and state expenditures.

The second set of data is the amount of money that state governments allocate to municipal corporations. This set of data provides a more valid measure of the policy impact of reapportionment than the first set. Since the alleged effect of reapportionment is to make the legislature more responsive to urban needs and demands, it is imperative to measure the level of state expenditures committed to their major cities. Prior studies that use expenditures as measures of policy outcomes do not include any indicators of state allocations to cities. To fill that gap, the total expenditures allocated to municipal corporations over 100,000 in population by state governments are used.[12] Specific functional areas such as welfare and education are not used since there is no one area funded by all of the states for all of the cities. The periods of the allocations to municipal corporations in the nonreapportioned states are

1959-64 for the before period and 1965-68 as the after period. For the reapportioned states, the time periods are the same as in the first set of data (1960; 1966).

There are limitations in the use of either of these databases. First, it is not certain that either state expenditures or state allocations to cities is a valid measure of the legislature's policy outcomes for urban areas. These indicators do not measure directly where the money is being spent. An increase in the level of expenditures in either set of bases does not necessarily mean that urban problems of housing, welfare, transportation, or education are being tackled with better financed programs. The second limitation is the small number of observations. There are very few observable fiscal years for either group of states in either period. This problem is aggravated by the fact that some legislatures do not hold annual sessions. If the legislatures were to make their allocations for two fiscal years as part of one decision, the number of observations would be reduced even further. However, this problem is minimized because the legislatures of 11 of the 15 reapportioned states meet annually, as do 16 of 33 nonreapportioned states. Moreover, on the basis of our observation of legislatures which meet only biennially, the budgets for each fiscal year do not appear to be determined by the same decision. Finally, from a statistical perspective, in spite of the small number of data points, there is evidence for the validity of the regression equations since the amount of variance explained is generally .8 or higher for every regression equation. The reliability of the measures can be inferred by looking at the pattern of the results. As an inspection of the data indicates, the pattern of policy changes is quite similar within each of the two respective groups of states.

Some mention needs to be made about the meaning of the term "policy change." For both sets of data, if the trend of yearly governmental expenditures increases, a policy change occurs. Generally, the level of expenditures rises in an absolute amount year after year. Given that basic fact, important changes in expenditure patterns are evidenced by increases in the rate of change in levels of expenditures. Since the levels before and after a particular point in time are being compared, it is necessary to compute the rate of change of expenditures for the fiscal years in each period. It is not appropriate to compare rates of change for each year because there is only one breaking point (reapportionment).

MEASUREMENT TECHNIQUE

The measurement technique used to determine the presence of before and after policy changes is the comparison of unstandardized regression coefficients. To ascertain the intrastate policy impact of reapportionment, regression equations are computed for each state. For every state the independent variable is the set of fiscal years included within the respective period. As an illustration, with a 6 year time period, the values of the observations of the independent variable are 1,2,3,4,5,6. The expenditure items are the dependent variables. The regression coefficients are in this instance measures of the average rate of change in the dependent variable as a function of time. They can be interpreted as trends in expenditures because the regression coefficient determines the trend line's slope which is the most important aspect of a trend line. The particular trend in expenditures for each state's before and after period is found by computing regression coefficients for each period. As an illustration, see Table 1. For Massachusetts there are two comparable regression coefficients each time a different dependent variable is regressed on fiscal years. In the case of the first set of data there are five dependent variables. Looking at one dependent variable, higher education expenditures, the rate of change in expenditure levels before reapportionment is .492 and after reapportionment it is 3.647. In order to determine whether or not this difference between the regression coefficients is significant, a test is made of the statistical null hypothesis that $b_1 = b_2$, where b_1 is the regression coefficient of the before period and b_2 is the regression coefficient of the after period. This procedure generates a t-value, which is a measure of the statistical difference between the two coefficients.[13]

Applied to the substantive problem of the current research, the null hypothesis (*H.O.*) is as follows: *H.O.* for every state, the regression coefficient in the before period is equal to the regression coefficient in the after period. This hypothesis is similar to the claim that only minimal policy changes will accompany reapportionment. If the t-value generated by the analysis is not statistically significant, *H.O.* cannot be rejected. In that case, the forecasts of Dye, Hoffebert, Jacob, Brady and Edmonds are confirmed. On the other hand, those individuals who maintain that reapportionment can produce increases in expenditure levels suggest an alternative hypothesis. Their hypothesis (*H.1.*) is as follows: *H.1.* for every state, the regression coefficient in

TABLE 1: TRENDS IN GOVERNMENTAL EXPENDITURES BEFORE AND AFTER REAPPORTIONMENT

State / Policy area	Higher Education b_1	Higher Education b_2	Local Schools b_1	Local Schools b_2	Highways b_1	Highways b_2	Public Welfare b_1	Public Welfare b_2	Hospitals b_1	Hospitals b_2	t-value at .01 level	Number of Policy Changes
Delaware	4.113	8.391	10.450	-.676	1.380	5.243	.382	1.705	-1.50	15.665	d.f. = 6	
t =	-2.788		1.134		-3.304*		-2.971		-.687		3.143	1
Georgia	1.285	5.947	3.671	7.930	3.049	3.371	.925	1.759	3.986	15.981	d.f. = 7	
t =	-9.091*		-2.542		-4.141*		-1.623		-10.168*		2.896	3
Kansas	3.099	5.215	2.885	1.05	1.031	6.465	.759	1.345	6.155	7.21	d.f. = 8	
t =	-1.227		.407		-10.417*		-1.965		-.193		2.896	1
Kentucky	3.062	7.896	3.715	8.303	5.657	11.592	9.006	5.740	7.306	17.695	d.f. = 7	
t =	-4.916		-3.391*		-6.063*		1.211		-6.419*		2.998	4
Massachusetts	.492	3.647	.630	5.175	.371	5.194	.389	1.548	2.303	9.678	d.f. = 6	
t =	-15.626*		-3.027		-6.543*		-3.737*		-5.277*		3.143	4
Michigan	2.709	5.314	2.23	7.459	1.155	8.764	6.28	2.603	5.095	15.095	d.f. = 8	
t =	-2.024		-5.378*		-15.787*		1.462		-4.876*		2.896	3
Mississippi	1.496	2.921	.098	7.949	1.683	1.391	.754	1.147	1.737	12.695	d.f. = 6	
t =	-2.906		-9.211*		-2.783		-.861		-9.643*		3.143	2
New York	1.851	.94	5.242	13.25	.2832	4.15	2.868	27.675	5.23	4.56	d.f. = 8	
t =	.79		-3.3*		-21.948*		-12.619*		-5.919*		2.896	4
Oklahoma	3.243	5.865	1.366	2.177	1.827	4.023	2.748	6.229	.612	1.722	d.f. = 8	
t =	-1.424		-1.138		-658		-2.177		-4.742*		2.896	1

TABLE 1 (CONTINUED)

State	Policy area	Higher Education b_1	Higher Education b_2	Local Schools b_1	Local Schools b_2	Highways b_1	Highways b_2	Public Welfare b_1	Public Welfare b_2	Hospitals b_1	Hospitals b_2	t-value of .01 level	Number of Policy Changes
Oregon		3.605	8.440	2.023	5.291	3.205	1.044	.716	.877	5.741	14.580	d.f. = 6	
	t =	-7.507*		-2.650		-.716		-.764		-5.957*		3.143	2
South Carolina		1.085	4.583	1.285	7.804	1.026	.135	.883	1.382	2.897	17.659	d.f. = 6	
	t =	-6.977*		-6.115*		.980		-2.207		-9.859*		3.143	3
Virginia		1.90	6.01	2.071	11.748	.445	1.436	.329	1.799	3.481	20.559	d.f. = 8	
	t =	-15.725*		-10.089*		-5.141*		-5.936*		-18.304*		2.896	5
West Virginia		1.66	9.082	1.894	5.962	3.065	2	.588	1.789	3.903	16.998	d f. = 8	
	t =	-6.596		-7.191*		.754		-11.469*		-9.465*		2.896	4
Wisconsin		5.001	5.68	2.621	3.14	1.323	9.86	1.182	2.365	7.859	9.825	d.f. = 8	
	t =	-.202		-.516		-9.787*		-1.906		-.459		2.896	1
Wyoming		3.139	5.876	2.195	2.936	.208	1.622	.266	.061	5.466	10.711	d.f. =8	
	t =	-1.153		-.500		-3.917*		.325		-1.583		2.896	1

* The results of the regression analysis for each expenditure variable are listed for each individual state. The regression coefficient in the before period is the set of numbers in the upper left hand side of a cell. In the upper right hand side is the regression coefficient in the after period. Below the two regression coefficients is the t-value. Significant t-values at the .01 level of significance are asterisked.

the after period is a positive increase over the previous regression coefficient. With this hypothesis, the regression coefficients are predicted to be different and the difference is in a particular direction. If the t-value obtained from the calculations is *negative* and statistically significant with a one-tailed test level of significance, then *H.1.* can be accepted.[14] Let us examine briefly the findings in order to illustrate the utilization of these hypotheses. In the case of Massachusetts, a comparison of the regression coefficients .492 and 3.647 yields a t-value of −15.626, which is significant at the .01 level of significance. On the basis of the high negative t-value, we infer the existence of an important policy change.[15]

There is a reason for employing this technique rather than others. Since the level of expenditures is likely to increase over time in every state, it is vital to work with a measure that will take this factor into account while still measuring the difference in the rate of change in expenditure levels. A difference of means test applied to these data would not consider the natural increase of expenditures. It would only indicate that over time, there are significant absolute differences in expenditure levels. With the comparison of regression coefficients, however, the absolute level of spending in the before period can be lower than the level in the after period, but such a difference does not affect the comparison of the average rates of change between the two periods. Hence, the existence of significant positive increases in the average rate of change after reapportionment can be established independent of the absolute levels of expenditures.[16]

FINDINGS

A blunt manner of interpreting the results of the regression analyses is to calculate the relative frequency of policy changes across all of the units within each of the two groups of states. The operational meaning of the term "policy change" refers to a negative t-value that is statistically significant. Such a t-value which is predicted by *H.1.* indicates that there is an upswing in the trend of state expenditures when compared to the trend in expenditures during a preceding period of time. These quantitative measures of policy changes are presented in Tables 1, 2, and 3.

Tables I and II list the regression coefficients and t-values from operations performed on the five state expenditure variables for reapportioned and nonreapportioned states, respectively. *H.1.* is supported in 39 of the 75 possible instances for

TABLE 2: TRENDS IN GOVERNMENTAL EXPENDITURES FOR NON-REAPPORTIONED STATES

State	Policy Area	Higher Education b_1	Higher Education b_2	Local Schools b_1	Local Schools b_2	Highways b_1	Highways b_2	Public Welfare b_1	Public Welfare b_2	Hospitals b_1	Hospitals b_2	t-value at .01 level	Number of Policy Changes
Alabama		1.844	4.8	.041	7.363	1.305	1.383	.862	.128	.670	-.022	3.143	
	t =	-1.100		-6.404*		-.032		.639		1.953			1
Arizona		2.361	9.503	3.065	8.577	1.912	12.917	.503	.603	.114	.355	3.143	
	t =	-7.092*		-2.746		-6.081*		-.353		-.769			2
Arkansas		.999	4.267	.672	8.934	3.785	4.793	1.739	3.462	.655	.058	3.143	
	t =	-3.914*		-3.791*		-.576		-2.063		2.353			2
California		1.647	-670	.340	5.987	285	3.605	1.167	15.07	2.49	.679	3.143	
	t =	.880		-1.317		-1.752		-7.240*		-1.190			1
Colorado		3.661	8.154	1.714	7.043	-.998	3.567	.605	1.793	1.318	.758	3.143	
	t =	-2.860		-5.385*		-3.219*		-1.305		1.331			2
Connecticut		.126	1.403	-9.662	1.996	-8.171	-5.959	1.559	-.442	1.539	-.11	3.143	
	t =	-3.286*		-.959		-.226		1.989		.657			1
Florida		.499	3.757	.257	10.908	.129	-1.225	-.146	1.155	.067	.737	2.896	
	t =	-15.142*		-3.938*		1.011		-5.246*		-9.066*			4
Idaho		1.999	5.174	1.907	8.044	2.838	2.411	.980	1.8	.673	.435	3.143	
	t =	-2.870		-4.307*		.142		-.791		.571			1
Illinois		2.002	3.48	2.194	6.384	-1.804	-.761	3.5466	2.011	.371	1.671	3.143	
	t =	-2.155		-2.887		-.640		1.463		-2.568			0
Indiana		2.872	5.513	1.538	6.646	3.347	.469	.484	.806	.401	1.591	3.143	
	t =	-2.386		-6.078*		1.650		-1.900		-3.571*			2
Iowa		2.885	5.889	.882	6.496	-.132	6.188	.957	1.567	.717	.993	3.143	
	t =	-5.221*		-6.395*		-2.904		-1.437		-1.489			2
Louisiana		1.317	5.994	2.038	8.178	.289	2.92	1.526	.675	7.64	1.783	3.143	
	t =	-3.214*		-5.285*		-.863		2.126		-2.267			2

TABLE 2 (CONTINUED)

State	Policy Area	Higher Education b_1 b_2	Local Schools b_1 b_2	Highways b_1 b_2	Public Welfare b_1 b_2	Hospitals b_1 b_2	t-value at .01 level	Number of Policy Changes
Maine		2.282 5.268	1.305 4.501	.867 2.418	1.618 1.672	.399 .958	3.143	
	t =	-2.100	-5.392*	-.863	.107	-3.367*		2
Maryland		1.432 3.378	3.440 7.265	.251 -.276	.912 5.214	1.167 -.178	3.143	
	t =	-5.419*	-1.347	.222	-4.118*	2.337		2
Minnesota		1.447 7.99	2.697 5.988	.296 5.183	1.394 3.792	.404 .699	3.143	
	t =	-15.048*	-3.901*	-2.025	-4.760*	-1.218		3
Missouri		.912 5.261	2.355 6.576	2.430 -.621	.464 .957	.517 1.12	3.143	
	t =	-6.815*	-1.469	1.971	-.841	-3.880*		2
Montana		1.861 5.789	1.749 5.79	3.737 -.704	-.435 1.986	-.18 .578	3.143	
	t =	-3.823*	-12.314*	1.533	-4.962*	-3.819*		4
Nebraska		1.751 6.392	.260 2.231	3.807 3.359	.733 3.142	.363 2.02	3.143	
	t =	-4.187*	-2.826	.183	-5.311*	-5.323*		3
Nevada		4.175 9.032	4.044 5.441	.761 -5.057	.968 2.34	.290 -.089	3.143	
	t =	-2.124	-.747	.719	-1.355	.507		0
New Hampshire		.392 7.84	.419 -.32	.172 .979	1.078 2.161	.165 1.439	3.143	
	t =	-3.912*	.188	-.406	-2.037	-6.201*		2
New Jersey		1.513 1.893	.452 6.688	1.903 2.486	.84 1.774	.575 1.087	3.143	
	t =	.491	-4.448*	-.671	-9.253*	-1.185		2
New Mexico		3.433 12.87	2.614 11.255	-4.097 6.422	4.305 2.59	.010 11.063	3.143	
	t =	-5.657*	-3.246*	-2.089	.538	-9.303*		3
North Carolina		1.569 4.904	11.502 10.053	.924 4.787	1.047 1.13	.661 .838	3.143	
	t =	-8.248*	.304	-3.165*	-.481	-1.315		2
North Dakota		1.088 4.4	-.156 3.497	1.193 -1.925	3.193 -1.925	1.148 1.358	3.143	
	t =	-.689	-.982	.596	.604	-.342		0

TABLE 2 (CONTINUED)

State	Policy Area	Higher Education b_1	Higher Education b_2	Local Schools b_1	Local Schools b_2	Highways b_1	Highways b_2	Public Welfare b_1	Public Welfare b_2	Hospitals b_1	Hospitals b_2	t-value at .01 level	Number of Policy Changes
Ohio		.965	6.754	.657	5.562	.218	3.752	1.630	1.143	-.028	.621	3.143	
	t =	-4.659*		-4.043*		-2.452		.274		-2.538			2
Pennsylvania		.879	2.091	1.639	5.140	2.020	3.962	2.053	1.78	.616	.828	3.143	
	t =	-3.482*		-3.394*		-1.107		.983		-.433			2
Rhode Island		1.264	6.325	2.471	5.312	.673	8.084	1.623	1.226	.839	2.16	3.143	
	t =	-3.482*		-3.235*		-3.219*		6.783*		-2.908			3
South Dakota		1.567	8.34	.44	6.422	2.165	11.231	.718	5.871	.367	.725	3.143	
	t =	-6.375*		-3.354*		-1.237		3.945		-2.243			3
Tennessee		1.215	7.35	1.573	8.4	3.342	1.986	.375	2.727	-.048	1.326	3.143	
	t =	-6.906*		-1.072		.748		-7.461*		-1.975			2
Texas		1.09	.169	2.325	6.434	.012	1.558	.707	.185	.210	.059	3.143	
	t =	.386		-.829		-.741		.829		.428			0
Utah		3.772	13.202	4.374	7.258	3.835	2.113	1.059	2.557	.1	2.1	3.143	
	t =	-6.884*		-1.488		.433		-3.610*		-3.552*			3
Vermont		4.077	7.837	.133	5.2	7.014	15.45	1.212	2.457	.850	.472	3.143	
	t =	-1.172		-1.920		-2.064		-.877		.306			0
Washington		4.066	6.543	4.825	9.105	1.293	9.105	1.648	1.675	.677	.673	3.143	
	t =	-3.418*		-.970		-2.347		-.952		.007			1

* The results of the regression analysis for each expenditure variable are listed for each individual state. The regression coefficient in the before period is the set of numbers in the upper left hand side of a cell. In the upper right hand side is the regression coefficient in the after period. Below the two regression coefficients is the t-value. Significant t-values at the .01 level of significance are asterisked.

the reapportioned group. The 52 percent level of corroboration of *H.1.* suggests that reapportionment accounts for changes in the direction of expenditures. The occurrence of policy changes in the nonreapportioned states is not as frequent. On the basis of the figures displayed in Table 2, policy changes happened in 62 of the 165 possible instances. The relative frequency of policy changes among all of the nonreapportioned states is 37 percent. A comparison of the two groups of states reveals that the percentage of changes in the reapportioned states is nearly one and a half times greater than in the nonreapportioned states. While a higher proportion of policy changes exist in the reapportioned states, it is clear that reapportionment is not a necessary condition for increases in the trend of state expenditures. Policy changes, as they are defined in this research, are produced in the absence of legislative reapportionment. The fact that a higher proportion of policy changes takes place in states that are reapportioned than occur in nonreapportioned states offers the possibility that reapportionment is a sufficient condition for increases in the trend of expenditures.

TABLE 3: TRENDS IN STATE GOVERNMENTAL EXPENDITURES TO MUNICIPAL CORPORATIONS IN REAPPORTIONED AND NONREAPPORTIONED STATES*

Reapportioned States	b_1	b_2	t-value	Degrees of Freedom
Wilmington, Delaware	-409.828	441.000	-.1757	7
Atlanta, Georgia	-821.285	1416.0	-6.402*	8
Savannah, Georgia	-24.571	173.190	-5.518*	8
Kansas City, Kansas	184.666	134.000	.531	7
Wichita, Kansas	62.399	696.00	-4.157*	7
Louisville, Kentucky	-5.857	852.20	-3.173*	7
Boston, Massachusetts	4069.88	7041.94	-.622	8
Fall River, Massachusetts	6557.22	1224.09	611	8
New Bedford, Massachusetts	537.057	1431.62	-3.504*	8
Springfield, Massachusetts	866.371	1630.37	-1.018	8
Somerville, Massachusetts	300.057	8.666	-2.851	8
Cambridge, Massachusetts	458.2	855.0	-.865	8
Worcester, Massachusetts	756.314	2411.94	-3.121*	8
Detroit, Michigan	656.856	5564.50	-1.551	7
Flint, Michigan	125.023	838.00	-4.819*	7
Grand Rapids, Michigan	174.309	840.000	-3.715*	7
Albany, New York	597.833	1596.00	2.457	8
Buffalo, New York	1049.967	4265.0	-.530	8
New York, New York	87040.867	381272.33	.196	8
Rochester, New York	1625.3	4453.0	-.375	8
Syracuse, New York	981.65	-2368.0	3.764	8
Utica, New York	116.167	451.5	-1.116	8
Yonkers, New York	1062.117	1600.00	-.548	8
Oklahoma City, Oklahoma	70.833	261.7	-1.040	8
Tulsa, Oklahoma	39.393	-43.2	.519	8
Portland, Oregon	129.857	818.57	-5.619*	8
Norfolk, Virginia	1373.85	10118.00	-3.582*	6
Richmond, Virginia	1591.32	4684.00	-1.413	6
Milwaukee, Wisconsin	2893.23	4679.50	-.509	7

Non-Reapportioned States	b_1	b_2	t-value	Degrees of Freedom
Birmingham, Alabama	-6.543	910.799	-6.492*	6
Montgomery, Alabama	3.829	162.500	-3.023	6
Mobile, Alabama	499.542	95.699	.831	6
Phoenix, Arizona	1413.08	1526.00	-.066	6
Little Rock, Arkansas	71.314	265.899	-4.036*	6
Berkeley, California	23.514	3090.09	-4.244*	6
Long Beach, California	174.828	3466.99	-.811	6
Los Angeles, California	2274.08	7808.00	22.296	6
Oakland, California	232.742	23.099	-.727	6
Pasadena, California	99.000	333.299	-1.591	6
Sacramento, California	138.799	1183.59	-2.143	6
San Diego, California	482.83	1280.89	-2.824	6
San Francisco, California	6219.22	9508.90	-2.057	6
Denver, Colorado	6.769	3733.00	-8.857*	6
Bridgeport, Connecticut	318.03	749.299	-.735	6
Hartford, Connecticut	322.142	2822.00	-4.434	6
New Haven, Connecticut	483.114	158.50	-.3869	6
Waterbury, Connecticut	81.571	193.00	-1.756	6
Jacksonville, Florida	73.543	-79.000	1.720	6
Miami, Florida	1.829	8.560	-.310	6
Tampa, Florida	-19.086	74.900	-1.926	6
Chicago, Illinois	-1181.57	-2527.75	-.366	6
Peoria, Illinois	4.971	95.500	-6.922*	6
Evansville, Indiana	-15.942	-27.000	-.162	6
Fort Wayne, Indiana	-12.514	-98.700	-.934	6
Gary, Indiana	30.914	75.100	-.876	6
Indianapolis, Indiana	76.914	-418.099	1.794	6
South Bend, Indiana	18.286	-73.000	1.342	6
Des Moines, Iowa	249.771	187.50	-.951	6
Baton Rouge, Louisiana	379.685	355.199	-.157	6
New Orleans, Louisiana	-155.314	483.899	-2.728	6
Shreveport, Louisiana	-26.171	-12.300	-.458	6
Baltimore, Maryland	7311.37	31457.1	-5.807*	6
Duluth, Minnesota	7.114	399.799	-4.024*	6
Minneapolis, Minnesota	-3.943	3583.79	-2.642	6
St. Paul, Minnesota	766.285	2990.59	-3.050	6
Omaha, Nebraska	27.686	17.400	-.259	6
Camden, New Jersey	17.475	54.900	-1.371	6
Elizabeth, New Jersey	35.771	1029.89	-11.003*	6
Jersey City, New Jersey	-157.285	5565.09	-21.052*	6
Newark, New Jersey	246.371	5511.59	-5.167*	6
Patterson, New Jersey	89.000	1772.39	-8.718*	6
Trenton, New Jersey	53.286	1542.00	-7.714*	6
Charlotte, North Carolina	203.543	152.099	-1.242	6
Akron, Ohio	377.057	-1339.00	1.020	6
Canton, Ohio	75.886	-165.699	4.914	6
Cincinnati, Ohio	-47.029	5880.50	-3.950*	6
Cleveland, Ohio	212.199	-1382.00	4.622	6
Columbus, Ohio	543.228	748.099	-23.748*	6
Dayton, Ohio	422.856	-316.599	1.950	6
Toledo, Ohio	94.457	-390.599	1.348	6
Youngstown, Ohio	-35.486	39.700	-1.648	6
Allentown, Pennsylvania	-23.143	22.500	-.696	6
Erie, Pennsylvania	87.629	143.099	-368	6
Philadelphia, Pennsylvania	2344.17	6401.09	-1.648	6
Pittsburgh, Pennsylvania	504.342	1018.29	-.678	6
Reading, Pennsylvania	24.400	-56.600	-.676	6
Scranton, Pennsylvania	48.34	134.899	-.268	6
Providence, Rhode Island	336.314	362.299	-.128	6
Chattanoga, Tennessee	-514.885	198.199	-1.441	6
Memphis, Tennessee	813.00	3293.79	-4.415*	6
Knoxville, Tennessee	892.028	227.699	1.237	6
Austin, Texas	32.028	166.599	2.438	6
Corpus Christi, Texas	55.4	-57.5	1.318	6
Dallas, Texas	96.200	35.000	-2.438	6
El Paso, Texas	1512.771	478.8	-1.298	6
Fort Worth, Texas	15.943	41.400	-1.088	6
San Antonio, Texas	63.2	671 8	-2.594	6

Non-Reapportioned States	b_1	b_0	t-value	Degrees of Freedom
Houston, Texas	6.143	95.60	-2.462	6
Salt Lake City, Utah	23.914	225.500	-2.372	6
Seattle, Washington	477.885	2496.19	-6.137*	6
Spokane, Washington	174.171	452.500	-1.875	6
Tacoma, Washington	147.199	712.099	-5.644*	6

* The t-values at the .01 level of significance for 6, 7, and 8 degrees of freedom are 3.143, 2.998, and 2.896, respectively. An asterisk denotes a significant t-value.

Evidence outlined in Table 3 supports the observed relationship above, but it indicates that reapportionment is a slightly weaker sufficient condition for policy changes. Table 3 is a compilation of the regression coefficients and t-values based on the data analysis of state allocations to municipal corporations. The proportion of cities experiencing an increase in allocations from the states which fall into the reapportioned category is 10/29, or 34 percent. The incidence of policy changes in the non-reapportioned group of states and respective cities is 18/73 or 23 percent. The margin of difference between the percentage of policy changes in the two states is somewhat discouraging, perhaps, for the individuals who expect reapportionment to produce major policy changes. They might expect, presumably, the relative frequency of changes in the reapportioned states to be greater and the margin of difference between the two groups to be greater. While reapportionment is associated with policy changes, the lack of a stronger relationship may be interpreted as the result of reapportionment strengthening the representation of suburban areas primarily and central cities secondarily. Because inner cities are losing population while the suburbs are growing more rapidly than any other part of the states, reapportionment shifts most of the legislative seats from meager sized districts to suburban based constituencies. Hence, even if a state is reapportioned, there will be no decisive combination of central city based legislators who control policy outcomes. Under these circumstances, any increase in state governmental expenditures to municipal corporations will reflect in significant ways the desires of the citizens who live outside the immediate boundaries of the city's "core." The validity of the measure of state allocations to municipal corporations does not allow us to pursue this line of inquiry because it fails to specify the functional areas to which the state expenditures are directed. It is possible, however, to observe which areas of policy outcomes, as measured by direct state expenditures, are subject to the most change. This brings the focus of the current research back to Tables 1 and 2. By collapsing all of the states

into the general categories of reapportioned and nonreapportioned, the percentage of policy changes by functional area can be determined. The information about such proportions is presented in Table 4.

TABLE 4: PERCENTAGE OF POLICY CHANGES BY FUNCTIONAL AREA

	Higher Education	Local Schools	Highways	Welfare	Hospitals	
Reapportioned States	.18	.18	.26	.10	.28	n = 39
Nonreapportioned States	.34	.27	.06	.18	.15	n = 62

An interesting aspect of the tabulations is the dissimilar percentages of policy changes that are included in the areas of highways and public welfare. In the reapportioned states, the functional area of highways absorbs a quarter of the total number of policy changes (.26) and the smallest proportion is contained in the category of welfare (.10). A contrasting picture develops in the nonreapportioned states where a meager amount of the policy changes occurs in the field of highway spending (.06) and the percentage of change in welfare assistance (.18) is considerably higher. There is no obvious empirical explanation for these comparative findings. Nevertheless, these general results raise the question of the extent to which "urban oriented programs" are defined in terms of the wants of noncentral city dwellers. Problems of inconvenience for residents living outside of central cities are often given high priority as matters receiving social amelioration. This point of view is an essential thesis of Banfield (1968). Thus, when governmental action is taken to satisfy citizen demands for solutions to the urban crisis, the decisional outcomes are likely to improve the transportation linkage across urban communities to the advantage of suburbanites. Fundamental problems of inner city poverty which require expensive and imaginative policies are least likely to receive attention. As a result, ongoing welfare programs are expanded in increments and the likelihood of policy change is not very great. *If* this is the case, *and* reapportionment enlarges the number of metropolitan representatives and not just central city legislators, *then* increases in financial commitments for urban centers would be more frequent in areas such as highways than in welfare. Although this is an intriguing line of inquiry, the absence of finer measures of policy impact prevent further discussion in this research. An obvious need in future studies is a set of indicators which more acutely reflect the distribution of policy benefits.

CONCLUSIONS

The foregoing research has provided empirical support for the hypothesis that legislative reapportionment is related to changes in state public policy. The nature of the data examined and the test utilized allows us to speak with some confidence to one of the major questions posed by political scientists: "How does governmental organization (and reorganization) affect governmental policy?"

The specific nature and the strength of the relationship remains unclear. Legislative reapportionment is not a necessary condition of policy change. There is a possibility that it is a sufficient condition. It clearly has different effects in different policy areas. However, there is a relationship. Future research may want to focus on the variables which intervene between the act of reapportionment and public policy change. When this line of research is pursued, we will perhaps be in a better position to explain the additional questions raised by the present research.

FOOTNOTES

[1] In this research, legislative apportionment is defined as the set of rules which stipulate the essential conditions for some land area to be designated as a representational district and which specify the manner of the distribution of legislative seats among established representational districts.

[2] This argument is somewhat imprecise because it implicity assumes that the numbers of voters in the districts with identical populations are equal. Yet, it does not appear that factors that affect the number of individuals eligible to vote, such as the age distribution of the population, the number of aliens, and the residency requirements for voting, are randomly distributed across all districts. Hence, even if population is used to establish equal sized districts, it does not necessarily follow that resulting values of the votes will be aqual across all districts.

[3] The U.S. Supreme Court initially chose not to adjudicate a dispute over Illinois' allocation of congressional seats because it involved "political, nonjustifiable questions" (*see Colegrove v. Green,* 1946). It subsequently dismissed for want of a federal question challenges to Illinois' legislative apportionment (*see Colegrove v. Barrett,* 1947). With these background decisions, the Court dismissed subsequent cases in which individuals objected to legislative apportionment arrangements (*see Remmy v. Smith,* 1952; *Kidd v. McCanless,* 1956). In another instance, it affirmed a lower court's refusal to hear such petitions from individuals (*see Radford v. Gary,* 1957).

[4] For related literature in political science consult the following: Clem, 1963; Schubert and Press, 1964.

[5] The proposed linkage between vote values and the selection cf policy outcomes is somewhat unconvincing because no consideration is explicitly given to the effects of other determinants of the selection of proposals for allocating resources. Within legislative bodies, factors such as seniority, committee assignments, and the control of house leadership positions affect the ability of a legislator or group of legislators to effect policy outccmes. Thus, unless a bloc of legislators who share policy preferences occupy positions of control over agenda setting and other legislative procedures, it is unlikely that the block constitutes the decisive combination of legislators that makes key policy decisions.

[6] For a description of methodological requisites of quasi-experimental designs applied to longitudinal data, *see* Campbell and Ross, 1968.

[7] In the 1960's not all state legislatures were malapportioned to the same extent. Unique features of state apportionment mechanisms resulted in a wide range of inequality in size among state legislative districts across state legislatures. As an illustration Georgia's county unit system resulted in its House of Representatives being more malapportioned than other lower state houses. Second, the federal courts did not maintain that "one man, one vote" meant exact mathematical equality in the size of districts. Moreover, they also did not employ a common standard in specifying the admissible deviations on variance from perfect equality. Tight restrictions were imposed in the reapportionment of Michigan, for example, while other states had considerably greater latitude in departures from equal sized districts. Consequently, reapportionment did not bring every state to a uniform level approximating equality. Third, both houses of state legislatures were not necessarily reapportioned simultaneously. In Georgia, the Senate was reapportioned in 1962 and the House reapportioned in 1965. On the other hand, Virginia's two houses were reapportioned in 1964. For these three reasons, the degree of structural change is not identical across all of the states.

[8] This category of reapportioned states is obtained from a listing of states that allegedly have reapportionment plans in operation. This listing is found in *Congressional Quarterly,* Vol. LIV (June 17, 1966), at 1285-1306. Letters were then written to the Secretary of State in those enumerated legislatures which had been elected under a recent reapportionment plan in order to verify the listing in the *C.Q.*

[9] Florida is classified as a nonreapportioned state even though a reapportioned legislature was elected in 1964. Florida was excluded because it remained very malapportioned after the plan was adopted and only meager changes could be expected. Second, Florida was reapportioned again for the 1966 election and retained very little malapportionment. Thus, it seems reasonable to look for the effects of reapportionment in Florida after the 1966 election.

[10] The data sources for urbanization, wealth, and industrialization are U.S. Bureau of the Census (1967) and U.S. Bureau of the Census (1961, 1967b).

[11] The data source is the U.S. Bureau of Census Compendium of State Government Finances.

[12] The data source is the U.S. Bureau of the Census Compendium of City Government Finances. Cities included in this data source must have at least 100,000 in population as of 1950.

[13] The procedure for comparing regression coefficients is outlined in Dixon and Massey, 1969: 193-210.

[14] The alternative hypothesis (*H.1.*) dictates the nature of the test of significance used to determine the existence of policy changes. Because we are interested in ascertaining changes in one direction only, *i.e.*, increases in the trends of expenditures, the appropriate test of significance is a one tailed test, where the t-value is a significant negative value. A one tailed test specifies the probabilities that the calculated t-value could have occurred by chance alone. In this research, we consider policy change to occur only when the probability of the calculated t-value could have happened only one out of a hundred times by chance alone. For further information on this point, *see* Blalock, 1972: 159-165.

[15] The explanatory payoffs of tests of significance are minimal when the number of observations becomes very large. Statistically, significant results can be produced by enlarging the number of data points. This limitation does not apply to the current research because the degrees of freedom is very small for all of the states. Any significant t-value is not likely to be a function of the number of observations. Moreover, the fact that the degrees of freedom is slightly greater for the reapportioned states poses no problem of inference. Significant t-values for the nonreapportioned states could not be generated by the addition of two or three degrees of freedom.

[16] Unfortunately, given the limited number of observations, it is not possible to employ a test of significance which overcomes the problems of proximal autocorrelation among data points. It is possible that the test used in this research indicates significant t-values because of the bias

introduced with autocorrelation. The only means of minimizing this problem is to look for unexpected significant t-values in the nonreapportioned states. If many do not appear, then perhaps there is only slight autocorrelation among the observations in both the reapportioned and the nonreapportioned units. For a description of a test of significance that circumvents the effects of autocorrelation, *see* Glass, 1968.

CASES

Baker v. Carr, 369 U.S. 186 (1962).
Colegrove v. Barrett, 330 U.S. 804 (1947).
Colegrove v. Green, 328 U.S. 549 (1946).
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