COMMUNICATIONS

The Nationalization of State Policy

TO THE EDITOR:

In an earlier piece in this Review, Douglas Rose has argued that comparative state analyses which are confined to just the state level are not useful or meaningful because states do not act as policy systems.1 In addition, he argues that such analyses are of little value because state level policy does not differ much since policy has become nationalized and similar.2 His work represents a potentially general critique of subnational unit analyses, and a possible set of techniques that might be applied to other subnational units of analysis. Given Rose’s conclusions, his original work deserves some careful scrutiny. In addition, his work has drawn some critical reactions, which also deserve consideration.3

The point of this comment is that both Rose and his critics are arguing about techniques and evidence which are only weakly related to his stated theoretical problem. The issue of how the variance of a single indicator might be calculated, decomposed, and interpreted does not deal with whether particular areal units act as policy systems, and barely touches the complex issue of “sameness” of policy and its causes. These points will be elaborated upon, but the important matter is that Rose’s analytics do not deal with his theoretical question. His empirical work and technical arguments are largely irrelevant because he has not formulated specific hypotheses, presented relevant evidence, or dealt with the necessity of linking evidence to hypotheses. His failure to execute this basic aspect of research results in his relying on irrelevant evidence, and leaves us with a series of unsubstantiated assertions.

Rose’s stated theses are: (1) that states in a multi-level policy system do not constitute closed policy systems (p. 1162) and (2) that the predominant factor in state policy determination is national policy norms (p. 1169) which creates high policy similarity among states. As I interpret his piece, his focus is therefore: (1) the extent of “systemness” (the degree to which open/closed systems exist) of the areal units of states, and (2) the extent of nationalization of state policy. These concerns can be stated formally. Let $Y_i = \text{state policy (e.g., type of tax system, presence or absence of consumer protection legislation)}; X_1 \ldots X_n = \text{state factors (degree of political competition, urban representation)}; \text{and } X_{n+1} \ldots X_{n+k} = \text{external or other level factors (federal legislation, degree of enforcement of federal laws).}$ Using systemness in the sense of some degree of covariation among the bounded set $Y_1, X_1 \ldots X_n$, the question of state systemness has to do with whether $Y_i = f(X_1 \ldots X_n), Y_i = f(X_{n+1} \ldots X_{n+k})$, or $Y_i = f(\text{some combination of } X_1 \ldots X_n, X_{n+1} \ldots X_{n+k})$. To the extent that $Y_i = f(X_1 \ldots X_n)$, states are closed, autonomous policy systems. To the extent that $Y_i = f(X_{n+1} \ldots X_{n+k})$, states are open policy systems.

The second problem deals with the extent of nationalization of state policy. What this refers to can also be formally stated, though there is some ambiguity because of the various ways in which the term nationalization has been used. The general potential policy relationships of relevance can be expressed as:

$$Y_i = a + b_1 X_1 + \ldots + b_n X_n + b_{n+1} X_{n+1} + \ldots + b_{n+k} X_{n+k}$$

Nationalization of policy has been used in the sense of declining subnational unit autonomy and/or as meaning things are becoming homogenized, standardized, or “the same.” In terms of the previous equation, declining autonomy can be represented as the increasing relative importance of $X_{n+1} \ldots X_{n+k}$ as determinants of $Y_i$.4 Standardization can refer, at a

2Ibid., p. 1168.
4There are also likely to be interactive relationships between $X_1 \ldots X_n$ and $X_{n+1} \ldots X_{n+k}$, but for purposes of simplicity, only the additive model of relationships will be discussed here.
minimum, to the tendency toward equivalence across all units of analysis of: (a) \( Y_i \) values; (b) \( b_1 \) \( \cdots \) \( b_n \); (c) \( b_{n+1} \) \( \cdots \) \( b_{n+k} \); (d) the distribution of \( X_1 \) \( \cdots \) \( X_n \); (e) the distribution of \( X_{n+1} \) \( \cdots \) \( X_{n+k} \); or (f) some combination of these.

Rose outlines in a general way that these two problems are his focus. He then argues that states do not constitute systems, and that nationalization, in the sense of low state autonomy and "similarity," predominates. The essential point to be made here is that, while these statements might be true, the evidence which is presented is not relevant for assessing these statements. To examine state systemness Rose would have to determine the relative significance of \( X_1 \) \( \cdots \) \( X_n \) as determinants of \( Y_i \) as compared to the relative significance of \( X_{n+1} \) \( \cdots \) \( X_{n+k} \) as determinants, either cross-sectionally or longitudinally. He does not present such evidence. When speaking of nationalization, he does not specify which meaning he is using. Regardless, the evidence he does present is only for the notion of nationalization defined as (a) above, and the data he uses are aggregated individual attributes, which seem to have very little relationship to state policy. No evidence is presented on the other types of nationalization.

In general, the logic of analysis and the evidence offered are not related to the stated theoretical focus. The evidence involves an odd assortment of per capita measures analyzed by an odd analysis of variance technique. Though some comments are in order on what empirical analysis he has done, the essential point here is that the exchange over the multiple means of calculating analysis of variance, while interesting and perceptive, skips over the prior questions of what is a relevant test, and what is appropriate evidence.

The analysis is inappropriate and misplaced, and suffers from several specific flaws. First, the indicators used appear to only dubiously represent state policy; states do differ a great deal on some policies, such as aid to local governments, or the distributional mix of their tax systems, and these policy differences are not examined. Second, one of Rose's main points appears to be that individual-level data aggregated to the state level show little variation, so that, in a case of guilt by association, units of that level of analysis become undifferentiated, and comparative state analyses become meaningless. It is rather that individual-level indicators aggregated to that level are of little value. Rose has confused the utility of aggregating certain indicators to higher "levels" with the utility of higher levels of analysis. Third—and this is particularly important—in using variance components analyses, equivalence or nonequivalence of group means, or finding low within or between group variances should be interpreted with great caution. Any result could be explained by multiple combinations of (b) through (e), as discussed above, and only multivariate analyses will tell what similarities or dissimilarities of (b) through (e) are associated with patterns of variations in \( Y_i \). The point is that the behavior of \( Y_i \) cannot be used with any certainty to infer patterns of (b) through (e), which is what Rose seems to be doing. Fourth, the evaluation of the significance of within-state variations is confusing. Rose regards them as the important policy analysis focus, but he seems to do his empirical analysis as if within state distributions of benefits are not associated with state action. That is, in decomposing variance, he regards the between state variance as reflective of state impact, and the within state variance becomes "other" or "individual level," apparently regarded as not being associated with state policy. But if states differ in their efforts toward equalization of benefits, then differences in within state distributions may certainly be due to state policy. For example, in an ANOVA of five states with cities as the unit of analysis, and per capita intergovernmental aid as the variable, differences in within state aid distributions (or violations of the equal standard deviations

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5 I have considerable sympathy for the statements, and have argued for the importance of their examination in Jeff Stonecash, "Urban Policy Analysis, Systems Assumptions, and Multiple Levels of Influence," *Publius*, Vol. 7, No. 1 (Winter, 1977).

6 I am indebted to Heywood Sanders, The Institute of Government and Public Affairs, The University of Illinois, for contributing to my understanding of the statistical analysis in Rose's work.

7 In regard to this point, and the first, it may be that I am in error in criticizing his analysis for it may be that he is purposefully doing this to demonstrate that prior analyses taking this approach using such aggregated individual level data are incorrect. While that is possible (see p. 1166), it does not seem to be the case on the basis of the data and analysis used on pp. 1164–1165. I am unable to determine what is finally meant regarding the use of such data.

assumptions) may reflect state policy differences; therefore it may be incorrect to regard only between-group variances as reflecting state differences.

The result of all this is that Rose, while producing a piece with some very interesting ideas, and some intuitively appealing statements (p. 1171) in a research area which is highly complex, has not examined, or supported his basic hypotheses. We still need sophisticated analyses to determine state systemness and the nature and extent of nationalization.

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Party Leadership Change in the House of Representatives: 1910—1976

TO THE EDITOR:

One of the problems faced by the profession today is the frequency with which calls for further research go unanswered. This problem, coupled with a hesitancy to report negative or inconclusive findings, creates the possibility that a number of questions of importance may not be answered. The purpose of this note is to answer a call for research by Robert Peabody on a question of party leadership selection. It also coincidentally reports a negative finding.

The impact of elected congressional leaders is generally recognized by political scientists.1 However efforts to study leadership change in the House have been constrained by the extreme stability of the elected leadership. Peabody has suggested a number of interesting factors for explaining periods of instability in congressional leadership selection during the period 1955—1966.2 Professor Peabody has called for research over a broader period of time in order to explore these factors in greater detail.3 The findings which will concern us here are those which relate to the Republican minority of 1955—1966.4

For the purposes of this analysis Peabody's concept of "revolt" will be combined with his next most violent category of leadership change, challenge to the heir apparent to form the category "deviant election." Since there is a well-established pattern of succession in which the majority or minority leader assumes the office of Speaker when it becomes vacant because of the retirement of the incumbent or a change in electoral fortunes which does not apply to any other office over time, only the offices of Speaker and majority and minority leader will be considered. A Speaker will be expected to become minority leader if his party loses the majority. If the Speaker is not re-elected the majority leader will be expected to fill the spot. Only successful revolts and challenges will be considered.

Using the above criteria one finds only four deviant elections during the period 1911—1975, all occurring in the Republican Party.5 Although the low number of cases makes it impossible to establish a statistical correlation between the suggested independent variables and deviant elections, there is an approach which may give a rough estimate of the importance of the variables. Instead of emphasizing conditions in the rare deviant elections, one should examine the variation in the factors during the entire period. We can hypothesize that the greater the amount of fluctuation in a variable during periods of leadership stability, the less the effect of the variable on instability.

I have examined two of the factors Peabody suggests may affect leadership stability, minority or majority status and the gain or loss of seats, from the 1910 election to the present.6 Peabody's first conclusion regarding the Republican minority of 1955—1966 is that it is more prone to intraparty leadership change through contests. A look at the data from 1910 to 1976 suggests that this statement may be expanded into the generalization that the Republican party is more prone to leadership change through contests regardless of minority majority status. There were four Republican


