

Research Note

Is the Lipset-Rokkan Hypothesis Testable?*

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This study discusses some of the tests that have been performed on the hypothesis originally advanced by Lipset and Rokkan in 1967 that the modern party systems were frozen along the cleavage lines of the 1920's. It is found that most if not all of the studies of trend/variability character are fraught with problems of both methodology and measurement and hence cannot be used as evidence either for or against the theory. Indeed, it is the final conclusion of the article that Lipset and Rokkan have framed their hypothesis in such a way that it is inherently untestable.

In 1967, Lipset & Rokkan formulated their well-known hypothesis that 'the party systems of the 1960's reflect, with few but significant exceptions, the cleavage structures of the 1920's' (p. 50). The claim has led to a large number of attempts to verify or to refute the hypothesis. It is the purpose of this paper to show that so far no researcher has shown convincingly clear proof for or against the theory. Indeed, I doubt that the Lipset-Rokkan hypothesis *in the form stated by them* is at all testable.

The paper begins with an attempt to determine what the hypothesis actually says and how it has been interpreted by various researchers. The paper continues with an examination of some trends and variability tests, in particular the well-known Rose and Urwin studies, and ends with a summary-and-conclusions section.

Problems of Interpretation

The Lipset-Rokkan hypothesis consists of two parts. The first consists of the statement quoted above. The second part of the hypothesis attempts to identify the cleavage structures that shaped the party systems of the 1920's. Lipset & Rokkan (1967, 47) enumerate four such cleavages and the issues connected with them:

* I wish to thank Bo Särilvik and members of the seminar on Western European party systems for helpful comments on earlier drafts. Needless to say, I alone remain responsible for shortcomings.

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<i>Cleavage</i>	<i>Issues</i>
Center-Periphery	National vs. supra-national religion National language vs. Latin
State-Church	Secular vs. religious control of mass education
Land-Industry	Tariff levels for agricultural products; control vs. freedom for industrial enterprise
Owner-Worker	Integration into national polity vs. commitment to international revolutionary movement

On the surface the two-pronged statement looks very innocuous. Most researchers have also accepted them at face value as simple, logical and testable propositions. But a closer inspection reveals a host of problems of interpretation that must be faced before proceeding to the testing stage.

First, the tests have assumed that it is the cleavages that have created the parties, or rather that it was the cleavages existing at the time when the modern parties were born that created the division between parties. But that is not what Lipset and Rokkan say; their wording is 'reflect' the cleavage structure. Lipset and Rokkan have really not faced up to the question of whether it is the parties that attempt to utilize the existing long-lived cleavages in society or whether the parties try to create different dimensions (or maintain defunct cleavages) in order to motivate their continued existence and maintain their profile.

A question that presents itself is, therefore, whether the parties really can be treated as completely dependent variables or if parties partly act also as independent variables. The use by Lipset and Rokkan of the word 'reflect' may indicate that they did not conceive of the relationship as monocausal. This issue is well treated by Pesonen (1973), who wrote:

The 'political dimension' is a difficult concept to define unambiguously. We ought not to confuse (1) the structural and non-structural social cleavages; (2) the politicized social cleavages which determine the corresponding issue dimensions; ... (5) those dimensions that are concluded from how close to, or distant from their own party the perceivers consider the other parties (i.e. party images) to be. *In the last-mentioned case the parties are the stimuli.* (p. 111, my emphasis).

Secondly, Lipset and Rokkan's discussion gives little or no indication of the order of precedence or the interdependence of the four different cleavages. Can they be taken as completely independent, which would facilitate testing? Can some of the cleavages be regarded as dominant? Which degree of co-variation exists that may create problems of interpretation (multicollinearity)?

The literature reflects completely opposite views concerning this problem. Lipset (1981, 230) writes that 'on a world scale, the principal generalization which can be made is that parties are primarily based on either the lower

classes or the middle and upper classes.’ But this frequently quoted claim is qualified in the next paragraphs by the addition that class is only one of several conceivable origins of party support. Lipset explicitly mentions religion, ethnic or national origin, sex and age. But he gives the impression that he regards class as a superior cause that tends to dominate except when some of the other factors are extremely strong.

Sartori (1969, 76) takes the opposite view and asks if class really is going to exert a dominant influence in a situation where there exists multiple cleavages.

Technically, the problem has tended to be disregarded. However, Lijphart (1980) has solved or at least minimized it by studying only comparable countries where the same cleavages may be assumed to operate. Others, e.g. Inglehart (1977), have shown large differences between coefficients in bivariate and multivariate estimations.

Thirdly, it is far from obvious from Lipset and Rokkan’s formulation how the cleavages are to be defined in practice. In various tests the cleavages have been simplified and modernized: *class* cleavage is operationalized in terms of workers vs. others, *religious* cleavage in terms of the existence of different churches or parties with religious bases or the religiosity of voters, *regional* cleavage in terms of the existence of parties that base their support on specific regions. The countryside-city cleavage dimension is usually not mentioned, but when it is, it is typically operationalized in terms of urbanization.

A closer inspection of Lipset and Rokkan’s definitions immediately reveals that the authors take a historic view of what issues are connected with the different cleavages. For instance, the class issue is interpreted as adherence to the nation or to international revolutionary movements, respectively. Similar highly specific issues are defined for the other cleavages. It is far from obvious that the researcher who finds that ‘class’ in the 1970’s still constitutes an important ground for party choice really has tested the Lipset-Rokkan hypothesis!

Fourthly, it is not easy to find means of testing the hypothesis. One way that is definitely not acceptable shows up in a frequently quoted statement in Rose & Urwin (1969, 20):

Implicit in much sociological discussion of parties is the hypothesis that social groups give birth to political parties. The statement is as implausible empirically as it is metaphorically unnatural. If each permutation of four groups in a society gives birth to a party, then each society would have at least 16 parties. In fact, of the 17 countries surveyed, the average number of parties catalogued is 4.5.

Even if Rose and Urwin make no reference to Lipset and Rokkan here, the choice of ‘four groups’ is indicative of whom they have in mind. And other authors have taken the hint. Zuckerman (1975, 237) writes: ‘In an argument *closely tied* to that of Lipset and Rokkan, Rose and Urwin *demonstrate* the

absence of a direct link between the number and kind of social cleavages and political parties' (italics mine). After this sentence follows the above-quoted statement by Rose and Urwin.

But, as I have indicated with italics in the quote from Zuckerman, this does not make sense. Firstly, Lipset and Rokkan have never claimed that all four cleavages must exist in all countries at all times. Secondly, they themselves qualified their statement by saying that 'with few but significant exceptions' the cleavages of the 1920's have given rise to the party systems of the 1960's. This qualification has been ruthlessly ignored by all testers of the Lipset-Rokkan hypothesis. Thirdly, Rose and Urwin have only used one single year as basis for their claim, namely 1968. The quoted statement is made without further reference or source, but the only data referred to earlier in the paper belong to a table containing material from 1968. One may surmise that also their claim rests on data limited to that year.

However, there is nothing in Lipset and Rokkan to indicate that the situation applies to each and every year. I would rather interpret their statement to mean that there exists some sort of dynamic equilibrium, i.e. a state to which the political party system tends to revert if it is for some reason displaced.

Finally, coalitions among parties may very well make the number of observed parties less than the magic number 16. Indeed, Lipset and Rokkan do have a lengthy discussion on this very issue (p. 32): 'This brings us to a crucial point in our discussion of the translation of cleavage structures into party systems: *the costs and the payoffs of mergers, alliances and coalitions*' (their emphasis). Thereafter follows a discussion of how many parties are likely to be observed in practice.

I find only two ways of testing Lipset and Rokkan's thesis. The first way is to show that the party structure of the 1960's and the 1970's is identical to that of the 1920's. This method is used by Rose & Urwin (1970) and Maguire (1983). I will return in the next section to more detailed critical comments on this approach under the heading of 'Trend and Variability Tests'. But we should notice already here that this test procedure assumes that the parties of the 1920's or, rather, the 1940's since that is when the studies begin, really reflect the cleavages of the 1920's. This requires in turn that we accept *both* that Lipset and Rokkan have convincingly shown that the party structure of the 1920's reflected the four cleavages in question *and* that the party structure of the 1940's directly corresponds to that of the 1920's. I have not been convinced that either of these assumptions holds up.

The other way of testing — and the more common — is to show that the party systems of the 1960's and 1970's are founded upon the four cleavages taken up by Lipset and Rokkan (e.g., Ersson & Lane 1982, Holmberg 1981, Lijphart 1971, Särilvik 1969). This general method is less objectionable although it is far from obvious how the relevant variables should be quantified in view of Lipset and Rokkan's historical definition of issues.

Trend and Variability Test

This type of test attempts to show the continuity in the number, structure and voting support of the political parties in a country.

The case for the null hypothesis — party support is constant — has been stated in general terms by S.M. Lipset and S. Rokkan in their review of the origins of parties and party systems. The analysis, which stops at the end of the First World War, emphasizes the persistence of the same types of parties in the half-century since (Rose & Urwin 1970, 288).

Let us note here that even if Rose and Urwin qualify their statement by the words 'in general terms', there is really nothing in the original Lipset-Rokkan formulation to indicate that they believed the fraction of voting support going to a party should be constant. Lipset and Rokkan formulate, albeit in general terms, a model that is distinctly dynamic in character. Parties and party systems arise, develop and disappear; the issues that parties take up and thrive upon vary and lead to changes in party support. But in the longer run the system tends to revert to the earlier equilibrium, since only the four cleavages mentioned by Lipset-Rokkan can be regarded as permanent. Others are more like 'dimensions' (to use Pesonen's term) and temporary.

There is thus a highly doubtful connection between Rose and Urwin's null hypothesis and the original formulation by Lipset and Rokkan.

Rose and Urwin employ data from 19 countries for the period 1945-69, encompassing a total of 142 elections and 638 observations. In order to be counted as an observation a party must have participated in at least three elections and have attained at least 5 per cent of the votes in a minimum of one election.

The first test is based on trend, measured over at least three elections. If a party's trend, measured by linear regression, amounts to a change of at least 0.25 percentage points in voting share per year, this is regarded as a 'meaningful' change. After this Rose and Urwin note that the number of 'meaningful' trends is very small; 57 per cent of Anglo-American parties, 66 per cent of the continental parties and no less than 80 per cent of the Scandinavian parties lacked trend. Yet it is concluded that 'the electoral strength of most parties in Western nations since the war had changed very little from election to election' (p. 295).

The conclusion must be regarded as meaningless, as anybody who has done any work with linear regression should know. When working with trends it is very easy to be misled because one single observation may have a sharply distorting effect. An example: The Swedish People's Party (Folkpartiet) is one of the few parties with a 'meaningful' trend. This is presumably based on the fact that the first observation is 1948 (the first Swedish parliamentary election after 1945), when the party happened to get 22.8 per cent of the votes. This

contrasts to 12.9 per cent of the votes in the election of 1944. In the case of Sweden the sample ends with the elections of 1968, when the People's Party received 14.3 per cent of the votes. In both 1964 and 1970 the party got a vote 2-3 percentage points higher. Hence the 'meaningful' negative trend for the People's Party clearly depends on the choice of the period of 1948-68, while a choice of 1944-64 would have resulted in no trend or even a 'meaningful' positive one.

It is also unclear why Rose and Urwin choose to regard the figure 0.25 as meaningful. It can hardly be accepted as indicative of a 'significant' change in particular since the number of degrees of freedom in the regressions vary from around 10 to 1!

In a purely statistical sense none of the changes is likely to be significant. One should not, of course, draw the conclusion that there are no changes but only that the trend method as a testing procedure is far from convincing.

The second test is based on the variability of party support: the difference between a party's largest and smallest voting share. Rose and Urwin choose to call this measure 'elasticity'; a better nomenclature would perhaps have been 'amplitude'. They also calculate standard deviations and, finally, measures of significance concerning changes in party support from election to election. Not surprisingly, only 6 per cent of all changes are found to be significant at the 5 per cent level. The average standard deviation is but 2.6 percentage points.

Rose and Urwin then group parties after year of formation into Old parties (formed before WWI), Interwar parties and New parties. They claim that one can formulate the following hypotheses from Lipset and Rokkan:

- H 1.1: 'Old parties are least likely to have their vote trend down'
- H 1.2: 'Old parties are least likely to have their vote show a significant trend'.

The first way of formulating the hypothesis can have no bearing on the Lipset and Rokkan theory, since there is nothing in their statement to indicate that parties building on the four cleavages should become bigger over time. There may, however, be something to the second formulation, namely that old parties should experience random fluctuations without trend. However, this depends very much on the time horizon taken.

It turns out that the data cannot give a clear answer. There is a tendency for old parties to have a positive trend or no trend that is greater than for other parties. (For reasons of caution there is no mentioning here of the word 'significant', but we may recall that 6 per cent was the *total* number of significant trends.) On the other hand, old parties have experienced greater variability, which is contrary to the corollary hypothesis:

- H 1.4: 'The older the party, the steadier its vote should be'.

Rose and Urwin add to this analysis information regarding different types of parties, based on class, religion and so forth. Again the result are 'inconclusive'.

The authors subsequently turn to a more appropriate way of testing Lipset and Rokkan, namely to study the stability of party structure with reference to the following hypothesis:

— H 4.1: 'The greater the age of a party system, the more likely it is to be static'.

Age is measured simply as the age of the two largest parties. Both the method of testing is the same as before. The stability of the party system is measured as the sum of the trend of the individual parties. Again the result is 'inconclusive'. In this case the observation consists of the national party system. The number of observations is thus very small and the results therefore hardly surprising.

It deserves to be pointed out that the paper by Rose and Urwin lacks a conclusion. Only from the first tests do they draw conclusions at all; the later tests are all evaluated in very careful terms by the authors.

For the reasons suggested above, Rose and Urwin have to my mind been taken far too literally by some other students of party structure. Maguire (1983) is rather typical. She writes that

The generally accepted view of European party systems is that they are, on the whole, remarkably stable creatures. The work of ... Lipset and Rokkan ... was the first systematically to draw attention to the fact that most European party systems had altered very little from the time of their consolidation in the early decades of the twentieth century up to the 1960's... This thesis found support in the detailed empirical evidence produced by Rose and Urwin (1970). (p. 67)

Apart from the question whether Rose and Urwin have produced any 'evidence' at all, Maguire's interpretation of their findings is far too strong. The only things that Rose and Urwin did show was that there were very few significant changes in party support or party structure over the period.

What Maguire does is to repeat the Rose and Urwin investigation, but for the period 1948 to 1979 with 1960-79 treated separately. The method and thus the problems of interpretation are identical. As concerns the trend analysis a somewhat larger number of parties now show a 'significant' trend. Note that while Rose and Urwin avoid the term 'significant' and talk of 'meaningful' trends, Maguire writes without any explanation whatsoever that 'as in the Rose and Urwin analysis change at the rate of 0.25 per cent annum is regarded as the minimum meriting description of significant change' (p. 71). A control of what level constitutes significant in proportions would perhaps have been called for before a statement like this one.

The measures of 'elasticity' and the standard deviations give no inkling that the 1970's have changed the previous conclusions. The standard deviation of the median party is almost identical for the two periods 1948-79 and 1960-79, respectively: 2.6 vs. 2.7 percentage points. This is identical to the figure found by Rose and Urwin for the period 1945-69. The conclusion drawn by Maguire (1983, 82) from these data is, however, astonishing:

... we would say that the electoral stability of European parties as demonstrated by Rose and Urwin has given way in recent years to a situation of greater instability ... when the period since 1960 is considered separately it can be seen that a relatively high proportion of parties has experienced significant trends and/or fluctuations in support.

This conclusion must be considered unsupported by the evidence presented. Aside from claiming 'significance' without further testing of significance levels, Maguire claims that the variability has increased while ignoring the fact that the standard deviation is almost identical for the two sub-periods and the elasticity (i.e., the amplitude) smaller for the period 1960-79 than for 1948-79. This latter fact is apparent from her table 3.2, reproduced here in part:

	<i>Elasticity of party support (median)</i>	
	<i>1948-79</i>	<i>1960-79</i>
Continent	7.0	4.6
Britain/Ireland	12.2	8.7
Scandinavia	7.7	7.3
Total	7.8	6.9

These figures show that the amplitude has fallen for all the areas studied. Hence Maguire's conclusion that variability has increased since 1960 is not supported by the evidence that she presents.

Conclusion

I have argued that the studies working with trend and variability tests in the tradition of Rose and Urwin are not capable of resolving the questions raised by the well-known Lipset and Rokkan hypothesis regarding party systems. The evidence is in all cases inconclusive, despite claims made by Maguire that the stability of the party system has diminished.

As for tests of the Lipset and Rokkan hypothesis along lines referred to as 'cleavage tests', they do indicate that the cleavages identified by Lipset and Rokkan remain important for party choice. However, the assertion that only parties that exploit one or several of these cleavages can survive is not adequately demonstrated. In fact, I would go as far as to suggest that because of the many definitional problems inherent in the Lipset-Rokkan hypothesis, it is doubtful that the hypothesis lends itself to tests capable of conclusively proving or disproving the theory.

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