Comparative Cross-Regional Analysis

The purpose of this chapter is twofold. In the first half of the chapter, I introduce the method of analysis that I use to assess the empirical support for the models and hypotheses presented in the previous chapter. Although no specific component of the analysis represents a new analytic technique or statistical tool, the manner in which they are combined here – and applied to the substantive questions at hand – is novel, and thus warrants a systematic explication of the method. In this vein, the second half of the chapter considers in greater detail the advantages and disadvantages of the method of analysis.

The aims of the method are fourfold. First, it is intended to facilitate a comparative analysis of the effect of economic conditions on election results across multiple cases; in other words, it is explicitly designed for comparative study. Second, the method of comparison is intended to be as transparent and easily replicable as possible; faced with the same data and statistical results, others should be able to come to similar conclusions about the relative empirical support for different hypotheses, or at the very least understand exactly why I have drawn the conclusions that I did. Third, the method is explicitly designed to treat seriously issues of comparability of data, an especially important issue in new democracies. Finally, the method is intended to link the empirical tests of the data – and the conclusions drawn on the basis of these tests – as closely as possible to the actual hypotheses proposed in the previous chapter.

The method of comparison involves the following components. Separate data sets of regional level data are utilized to conduct statistical analyses of the effect of cross-regional variation in economic conditions on cross-regional variation in election results for each of the twenty elections in the dataset. Using these separate datasets, separate estimates for the effect of
Comparative Cross-Regional Analysis

economic conditions on every party in which we have a theoretical interest – in other words all incumbent, New Regime, and Old Regime parties – are calculated. These effects are estimated using a compositional data model that is appropriate for working with multiparty election data. In all cases, the effects are calculated while controlling for a constant set of demographic variables in an effort to control for the other dominant explanation for cross-regional support for political parties, which is the existence of social cleavages. In order to compare the effect of the economy across the different elections, I focus not on individual coefficients and standard errors but, rather, on our overall level of confidence that the party in question either enjoyed more or less (depending on the hypothesis) electoral support in areas of the country where the economy was performing better. To measure this overall level of confidence by party, I use stochastic simulation to estimate a probability distribution of the substantive effect of a commensurate economic shock on each party in the study, and then calculate our confidence level that the effect of this shock on the vote for each party is in fact in the direction predicted by the relevant standard economic voting hypothesis. Support for the different hypotheses is then measured in two ways, first through an inspection of election by election results from the four sets of case studies and then in a more broadly comparative framework that looks at the results across all twenty elections simultaneously. Finally, a series of robustness tests (presented in Chapter 8) are also conducted that involve changing the specifications of the original statistical analyses, recalculating all of the measures of the effects of economic conditions on each party, and then assessing the comparative results in the same manner as in the original analysis.

In the following section, I present the method of analysis, detailing the variables included in the regressions, the statistical model used to analyze the data, the process by which I calculate my quantity of interest, and the manner in which I compare this quantity of interest across different parties and elections. In order to more fully illustrate the method of analysis, I use the 1996 Czech Republic election as a heuristic example. I chose this election because of the variety and clarity of results, as well as the fact that it is not included as one of the paired case studies. The election was the first in the Czech Republic following the dissolution of Czechoslovakia and resulted in the incumbent government of the Civic Democratic Party (ODS), the Civic Democratic Alliance (ODA), and the Christian Democratic Union (KDU) being returned to power with a higher proportion of votes than in the previous election but with a lower share of seats. While
Regional Economic Voting

Table 3.1. 1996 Czech Republic Parliamentary Election

<table>
<thead>
<tr>
<th>Party</th>
<th>Percentage of Party List Vote</th>
<th>Total Seats</th>
</tr>
</thead>
<tbody>
<tr>
<td>Civic Democratic Party (ODS)</td>
<td>29.62</td>
<td>68</td>
</tr>
<tr>
<td>Czechoslovak Social Democratic Party (CSSD)</td>
<td>26.44</td>
<td>61</td>
</tr>
<tr>
<td>Communist Party of Bohemia and Moravia (KSČM)</td>
<td>10.33</td>
<td>22</td>
</tr>
<tr>
<td>Christian Democratic Union (KDU)</td>
<td>8.08</td>
<td>18</td>
</tr>
<tr>
<td>Republican Party (SR)</td>
<td>8.01</td>
<td>18</td>
</tr>
<tr>
<td>Civic Democratic Alliance (ODA)</td>
<td>6.36</td>
<td>13</td>
</tr>
<tr>
<td>Others</td>
<td>11.16</td>
<td>0</td>
</tr>
<tr>
<td><strong>Total</strong></td>
<td><strong>100</strong></td>
<td><strong>200</strong></td>
</tr>
</tbody>
</table>


the government had enjoyed a majority in the parliament from 1992 to 1996, it received only 99 out of 201 seats following the 1996 election.1 As individual party coding decisions are not presented until the following chapters, please note that two of the incumbent parties that competed in the election, the ODS and the ODA, are also coded as New Regime parties. The KDU is the only other incumbent party, and is neither an Old Regime nor a New Regime party. The election also featured one Old Regime party, the Communist Party of Bohemia and Moravia (KSČM). The results of this election can be found in Table 3.1.

Method of Empirical Analysis

The goal of the empirical analysis is to as closely as possible test the standard economic voting hypotheses presented in the preceding chapter in an effort to explore the relative empirical support for the Referendum and Transitional Identity Models.2 All three of the hypotheses generated from these models present predictions about the effects of variation in economic conditions on variation in election results. To assess these hypotheses accurately, therefore, we want to measure the effect of economic conditions while controlling for other potential explanations for distributions

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1 For more on this election, see Fitzmaurice 1996; Fule 1997; Turnovec 1997; Mateju and Vlachova 1998.
2 As described in the previous chapter, the conditional hypotheses are assessed by looking for variation in the degree of empirical support for the standard hypotheses.
Comparative Cross-Regional Analysis

of votes at the regional level. It is important to note, however, that the point of this study is not to attempt to assess the relative importance of economic conditions as opposed to other factors, but merely to control for these factors while measuring the empirical support for the economic based hypotheses.

Two sets of factors appear paramount in this regard. The first are contemporary political developments that might affect the degree of support for different types of political parties, especially those currently in power. These could include, for example, foreign policy decisions, political scandals, or international developments. In time-series analyses of economic voting, such concerns have led scholars to include dummy variables for periods of war or, in the American case, the effect of Watergate. Here, however, I draw on one of the first advantages of the comparative cross-regional approach, which is that by only including election results from one country at one point in time in any given regression analysis, I am able to control for all election-specific effects (and country-specific effects for that matter) in the statistical analysis. Thus, if the election is taking place in the immediate aftermath of the collapse of a communist region, that factor is constant across all of the regions in a country at the time of the election. The same holds for the distraction of a military conflict (e.g., Chechnya in Russia) or the presence of a political scandal (e.g., the Czech Republic in 1998).

The second important explanation for variation in patterns of support for political parties is societal cleavages. There is, of course, a large literature on the topic from studies of Western European countries that points to the importance of labor patterns, rural-urban splits, center-periphery divides, and ethnic cleavages. In addition, studies of voting behavior in postcommunist countries also have identified politically relevant socioeconomic cleavages, in particular highlighting the propensity of older voters

3 See, for example, Chappell and Keech 1985; for a critique of this approach, see Fiorina 1981, Chapter 6.
4 It is important to emphasize again that this study does not in any way argue that political factors cannot influence election results; to do so would clearly be problematic. But as the goal of my analysis is to produce as accurate an estimate of the effect of economic conditions as possible, limiting the pooling of data to a single election is valuable in so far as it allows us to estimate effects for economic conditions while controlling for national political developments. At the same time, the comparative framework of the study then allows us to assess politically based hypotheses like the supply-side conditional hypotheses by comparing the empirical support for the different standard hypotheses in different political contexts.
5 See, for example, Lipset and Rokkan 1967 and the voluminous work that followed it.
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to support Old Regime parties and urban voters to support New Regime parties.  
As opposed to national political developments, there is clearly variation in the presence of different demographic groups in different regions. Thus to control for the effects of societal cleavages on the distribution of vote shares, I include a constant set of control variables in all of the analyses, which, somewhat remarkably, were available at the regional level in all five countries for almost the entire period of the study.  
These variables are the percentage of the workforce employed in agriculture, the percentage of the work force employed in industry, the percentage of the population living in urban areas, the percentage of elderly residents, and finally a control for the size of the region itself.  
While these variables may not capture every possible societal cleavage, they do tap into most of the cleavages described in the preceding paragraph.  

6 For a summary of the role of cleavages in the voting literature in postcommunist countries, see Tucker 2002, 292. For specific examples, see Kopstein 1992; Clem and Craumer 1995c; Wyman, White et al. 1995; Clem and Craumer 1997; Szelenyi, Fodor, and Hanley 1997; Moser 1999 and especially Tworzecki 2003 and Colton 2000b. For a thorough review of the work on cleavages and postcommunist politics, see Whitefield 2002.  
7 In a small number of instances, it was necessary to use demographic data from a different year, although in almost all cases the data were off by no more than a year or two. However, as would be expected, all of the demographic variables are highly correlated across years because of the slow rate at which the demographic composition of a region changes. See Appendix for the actual dates for all variables.  
8 The actual coding of each of these variables for each election can be found in Appendix II. Following convention, regional population is logged to take account of the sharp disparity between the size of capital cities (and occasionally other large cities) and other regions. In addition, the percentage of ethnic Hungarians is also included as a control variable in the Slovak analyses, as this is the only country in the study where an ethnically based party received a significant proportion of the national vote, with the main Hungarian party entering the parliament in all three elections. And in an early version of the analysis, robustness tests revealed different results when including the Hungarian party as a separate party in the analysis or in the base “other” category for analysis (see discussion later in this chapter of this robustness test); this disparity disappeared when the percentage of ethnic Hungarians was included in the analysis as a control variable. In two of the other countries – the Czech Republic and Hungary – I also tested whether including the percentage of ethnic minorities as a control variable had any effect on the assessment of the impact of economic condition. In both cases, I found no meaningful effects.  
9 Moreover, as will be described in greater detail below, this method also allows me to measure the effect of economic conditions on election results directly, as opposed to imputing economic conditions on the basis of beliefs about economic disparities across demographic groups. For example, it is easy to claim that rural residents have been more disadvantaged by the transition than urban residents, and that therefore a link between the percentage of rural residents in a region and support for a political party is evidence that this party
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any way meant to deny that there are demographic patterns to the vote for parties in my cases – and, indeed, a quick glance at the regression results will confirm this fact – but rather that the effects of economic conditions that I identify are not merely functioning as proxies for underlying demographic distinctions between regions.10

Economic Variables

In contrast to the demographic variables, different countries reported different economic variables at the regional level; moreover, there was also variation over time within countries as to what variables were reported at the regional level.11 This situation presents two options for analysis. One option is to exclude all statistics that are not available for all twenty elections from all of the analyses, which would involve ignoring a great deal of information. The other option is to include the best available measure of economic conditions in each case – which necessitates comparing results from regressions with different sets of independent variables – and enjoys more support where economic conditions are worse. While I do not doubt that in many cases economic conditions are worse in rural areas than urban areas, such a claim relies on an assumption about the economic implications of demographic characteristics of a region. In contrast, the analysis I employ relies only on measures of economic conditions to judge the impact of the economy and relegates demographic variables to control variables, thus allowing us to directly measure the impact of economic conditions and skipping the intermediary step of imputing economic attributes to demographic characteristics.

10 One limitation of relying on aggregate-level sociodemographic measures is that they do not directly tap into possible cultural cleavages between voters on issues such as clericalism versus anticlericalism (see, in particular, Tworzecki (2003) in this regard). However, if such cleavages do not vary regionally, they will not introduce bias into the study of the effects of economic conditions. And as this study is not focused on testing the relative importance of economic versus cultural explanations for voting behavior, this should not prove to be much of a limitation. Moreover, to the extent that cultural cleavages may vary regionally, the complete collection of demographic control variables – and in particular the measures for the percentage of urban citizens, elderly citizens, and workers employed in agriculture – should function as a reasonable proxy for these types of differences. Finally, the fact that the study encompasses multiple countries also provides a bulwark against these types of concerns; see note 66 in this chapter for more in this regard.

11 Although it is possible that I may have missed some variables, I collected the data for this project in a series of trips to the national statistical offices of all five countries included in the study. In addition to searching publications available for sale and materials in the libraries of the statistical offices, I also met with officials of the statistical offices and statistical office librarians. While I cannot exclude the possibility that additional information may still be available, the effort extended to track down these data was constant across all of the countries and all of the elections.
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to utilize a standardized method of comparison across the different set of results.

My response to this dilemma was to choose the latter option but to be extremely systematic in the manner in which I did so. Rather than eschew the use of available data, I included all available economic indicators in each individual statistical analysis. However, the decision of which economic indicators to include was guided by a fixed set of *a priori* rules – presented later – that were not in any way affected by the results of any analyses. So I did not in any circumstances drop economic variables because they did not seem to have a statistically significant effect on the outcome.\(^{12}\) Finally, as a robustness test, I reran every analysis using only the three independent variables – unemployment, growth, and change in income – that were available for almost all of the cases. The results of this robustness test are presented in Chapter 8, but they clearly lead to the same overall conclusion regarding the relative strength of the Referendum and Transitional Identity Models as the original analyses. So although the analyses presented in Chapters 5–7 will be based on the use of the best available economic indicators, readers uncomfortable with this approach can rest assured that the conclusions reached by the study would be very similar had I relied only on the three variables noted here. Moreover, I employ a method of comparison across cases that focuses on a standardized measure of our overall confidence that the economy is helping or hurting particular parties – as will be explained shortly – and is therefore not dependent on a direct comparison of any particular coefficient from one regression with another.

My motivation for this solution came from the nature of the hypotheses presented in the preceding chapter. Were the hypotheses seeking to test the effect of particular economic conditions with one another – for example, to compare the effect of unemployment with the effect of growth – then it would be have been crucial to make sure that the analyses contained the same economic variables. However, the hypotheses simply predict that certain parties should enjoy more economic success when economic conditions are either better or worse. Therefore, the most prudent course seemed to be to make the best available estimate in each case of where economic conditions were better and where economic conditions were worse by taking advantage of the available data. This was especially so in the case of wage arrears, which were a major economic concern in Russia but were virtually

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\(^{12}\) For an example of this approach, see Wade, Groth, and Lavelle (1994).

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Comparative Cross-Regional Analysis

nonexistent – and certainly were not reported regionally – in the Central-European cases.\(^{13}\)

Turning to the question of which economic variables to include in the analysis, the existing literature on economic voting in established democracies clearly points to four important economic indicators: unemployment,\(^{14}\) change in income,\(^{15}\) growth,\(^{16}\) and inflation.\(^{17}\) In an ideal world, therefore, we would include all four of these variables in any economic voting

\(^{13}\) Again, the method of comparative cross-regional analysis is important in this regard. By limiting the individual statistical analyses to data from only one election, one can in fact include all available data without introducing problems of missing data to the analysis; in almost all cases, data reported in country X at time T was reported for all regions. Moreover, for a dataset of this size, there are remarkably few instances of missing data. In the majority of the twenty elections there are no missing observations, and in only one election – the 1991 Polish parliamentary election – are there more than two missing observations. In this one case, I am missing data on the vote for the Agarian Alliance (PL) in 1991 in five regions. To address this point I ran every model twice, once including the PL and excluding the missing regions, and once excluding the PL and including the missing regions. The results presented in the text for the PL are taken from the former set of analyses, while the results for the remaining parties contesting the elections are taken from the latter. There are two elections in which a single observation is excluded because of missing data in the dependent variable: Poland in 1991 and Hungary in 1990. In the 1999 Slovak presidential election analysis, there are two missing observations of one of the independent variables (change in growth), so the analysis is run using seventy instead of seventy-two observations. Finally, the number of observations in the analyses of the Russian elections varies from seventy-five to seventy-seven due to the availability of data from the breakaway republic of Chechnya and neighboring Ingushetia. It also should be noted that the Czech Republic added a region (Jesenik) following the dissolution of Czechoslovakia, so the regressions using the 1992 data have one fewer observation than the regressions using the 1996 or 1998 data.

\(^{14}\) From studies of American elections, see Kramer 1971; Kinder and Kiewiet 1981; MacKuen, Erikson, and Stimson 1992; Goidel and Langley 1994; from OECD countries, see Whiteley 1980; Lewis-Beck and Bellucci 1982; Bellucci 1984; Powell and Whitten 1993; Feld and Kirchgassner 2000; and, for a cross-national study extending beyond the OECD, see Wilkin, Haller, and Norpoth 1997. Unemployment also has figured prominently in other cross-regional studies of postcommunist countries; see Pacek 1994; Wade, Groth, and Lavelle 1994; Gibson and Cielecka 1995; Bell 1997; Fidrmuc 2000c.

\(^{15}\) Change in income has been especially prominent in U.S.-based studies; see, for example, Tufte 1975; Kramer 1981; Markus 1988; Erikson 1989, 1990. Other studies have relied on actual income levels as opposed to change in income; see, for example, Lewis-Beck and Bellucci 1982; Bellucci 1984 in the OECD context and Wade, Groth, and Lavelle 1994; Bell 1997; Fidrmuc 2000c in the postcommunist context.

\(^{16}\) From studies of American elections, see Chappell and Keech 1985; Kiewiet and Rivers 1985; Alesina and Rosenthal 1995; from OECD countries, see Powell and Whitten 1993; Whitten and Palmer 1999; from comparative studies of Latin American countries, see Remmer 1991; Roberts and Wibbels 1999.

\(^{17}\) From studies of American elections, see Kramer 1971; Kinder and Kiewiet 1981; Chappell and Keech 1985; MacKuen, Erikson, and Stimson 1992; from OECD countries, see Whiteley 1980; Lewis-Beck and Bellucci 1982; Bellucci 1984; Powell and Whitten 1993;
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analysis. Inflation, however, does not lend itself to a cross-regional study, as the most substantial variation in inflation is usually across countries or over time within a single country as opposed to cross-regionally within a single country in a single time period, largely because of the importance of central bank policy in dictating national inflation levels. Moreover, it is very difficult to find inflation figures disaggregated to the regional level; indeed, I could only locate such data for four of the twenty elections included in the study. For these reasons, I use change in wages, growth, and unemployment as the three base economic variables in every regression analysis.18

Of the three, unemployment rates are relatively self-explanatory. The higher the unemployment rate, the more likely there are to be more people who are upset about the state of the economy for the many reasons discussed in the previous chapter.19

Change in income is calculated as the change in average salaries in the region since the previous year.20 Note that this is not intended to function as a traditional retrospective measure whereby citizens are judging the competence of the government by whether or not they have a larger salary than when the government came to power; this would require measuring the change in salary since the previous election. Instead, it is intended to function as a measure of where the economy is performing better, on the basis of the assumption that all things being equal, people in areas of the country where average wages are rising are likely to be more satisfied with

Chappell and Veiga 2000; Feld and Kirchgassner 2000 from Latin America, see Remmer 1991; Roberts and Wibbels 1999.

18 The one exception is the 1991 Russian presidential election, for which I was unable to locate any unemployment data.
19 As discussed in the previous chapter, the “free pass” scenario could predict a different effect for unemployment early in the decade, although see the discussion of this implication at the regional level in note 48 in Chapter 2.
20 I did not convert these figures to real income because of the lack of inflation figures disaggregated to the regional level. However, if we assume that inflation is relatively constant across the country because of the leading role of a centralized institution (the central bank) in dictating inflation levels, then changes in average income should be a relatively good proxy for changes in real income as well as the period of the change is constant across all observations. It is worth noting that this would not be the case if data were pooled from more than one election. Moreover, although it is clear that, for example, a 100% increase in wages means radically different things in a low inflation and high inflation context, the fact that the analysis only involves pooling data from one country at one point in time means that the only assumption at work here is that people in the part of the country where salaries have risen by 100 percent are likely to be happier than those in areas where salaries have fallen by 50 percent in any overall inflation context.
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the state of the economy than those living in areas of the country where average wages are falling. One also could argue that average salaries growing at a faster rate might also signify the presence of new job opportunities at higher paying salaries, which would compliment the original claim that people ought to be more satisfied with the state of the economy in areas of the country where salaries are rising at a higher rate.21

In some ways, economic growth represents the quintessential measure for establishing areas of the country where the economy is performing better and areas where it is performing worse. And for this reason, I have included a measure of growth in all the regression analyses. The ideal measure to use for regional growth is clearly change in regional GDP. Unfortunately, the situation is complicated by the fact that it proved very difficult to find measures of GDP disaggregated to the regional level in these countries, especially earlier in the decade.22 As it turned out, I could only calculate figures for GDP growth for six of the twenty elections. For the remaining elections, I use change in industrial output as my measure of growth. While problematic, it is the best available proxy I could find, and the alternative was to have no measure of growth in a majority of the elections.23

In addition to these three standard variables, I also include measures of three other economic indicators when available that are more transition specific, two of which focus on positive benefits of the transition and one of which focuses on a negative consequence. The negative variable, mentioned earlier, is wage arrears, or the nonpayment of wages, which was a significant problem throughout the 1990s in Russia; these data are only included in the analyses of Russian elections. The two “positive” indicators are foreign direct investment and foreign trade, both of which were expected to be positive benefits of transitioning from a centrally planned to a market economy. The assumption here is that the economic contact with international actors,

21 One might object that in a transitional context, a rise in average salaries could also result from the closing of unprofitable enterprises and the firing of large numbers of lower income workers. Fortunately, the multivariate nature of the analysis ensures that the effect of change in income is estimated while controlling for unemployment rates.
22 In fact, my attempts to secure such data led to one instance where an official of a statistical office actually laughed at me for suggesting that such data might be available.
23 And keeping within the guideline of using the best available data, I always use change in GDP growth when it is available, and I always revert to change in industrial output when it is not; in no case where GDP growth was available was industrial output used instead.
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both in the form of trade and especially direct investment, would be additional indicators of better economic conditions. And in the few instances in which regional measures of inflation are available, they are included in the analyses as well.

Table 3.2 lists the variables included in each regression. The exact coding of each variable included in each regression can be found in Appendix II.24

Thus, to estimate the effect of economic conditions on election results for incumbents, New Regime, and Old Regime parties, I run a separate regression for each of these parties in each election in which the unit of analysis is the subnational region, the dependent variable is the share of the vote received by the party in question, the independent variables are regional measures of macroeconomic conditions, and regional demographic variables function as control variables, thus resulting in a separate set of regression results for each party in each election.25 Regarding the unit of analysis, one would ideally disaggregate our data to the smallest possible observation, such as individual election districts. However, I was limited in this regard by the availability of commensurate economic data. Therefore, I use as my unit of analysis the smallest region for which I could obtain both electoral and economic data. Thus, the units of analysis in this study are the seventy-seven Russian oblasts, krai, and republics,26 the seventy-six

24 The decision of which dates to use for the variables also was guided by a constant set of a priori rules. For demographic variables, I used the date at which the data were available that was closest to the election. I followed the same rule for unemployment, which was often available at quarterly or even monthly intervals, and wage arrears, which was available monthly. For change in income and growth, I used the year of the election as the later year and the prior year's figure as the reference point; when data was available quarterly, I used the quarter closest to the election and the data from the same quarter of the previous year as the reference point. For inflation, foreign direct investment, and foreign trade, I used the data from the year of the election, on the assumption that this would do a better job measuring economic conditions at the time of the election than data from the previous year. One transition specific variable that was unfortunately not included in the study is private sector job creation, which Jackson, Klich, and Poznanśka (2003a, 2003b, 2005) have now found to be an important determinant of electoral success for Polish reformist parties in cross-regional analyses of the 1993, 1995, and 1997 Polish elections. As there as is a good deal of overlap between their classification of reformist parties and my New Regime party category, my best guess is that including job creation data in the analyses would therefore have only strengthened support for the New Regime hypothesis, especially in Poland.

25 So, for example, because the ODS competed in three separate elections, there is a separate result for the ODS in the 1992, 1996, and 1998 Czech elections.

26 The Russian federation has three different types of subnational units, oblasts, krai, and republics, all of which are mutually exclusive. Moscow and St. Petersburg are also their own regions. Autonomous oblasts and autonomous krais – subsubnational units contained within some of these regions – are omitted from the analysis.
Table 3.2. Independent Variables by Election

<table>
<thead>
<tr>
<th>Country</th>
<th>Year</th>
<th>% Unemp</th>
<th>Δ Income</th>
<th>Δ GDP</th>
<th>Δ Growth</th>
<th>Δ Industrial Growth</th>
<th>FDI</th>
<th>Foreign Trade</th>
<th>Wage Arrears</th>
<th>Inflation</th>
</tr>
</thead>
<tbody>
<tr>
<td>Czech Rep</td>
<td>1992</td>
<td>X</td>
<td>X</td>
<td></td>
<td>X</td>
<td>X</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Czech Rep</td>
<td>1996</td>
<td>X</td>
<td></td>
<td></td>
<td>X</td>
<td>X</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Czech Rep</td>
<td>1998</td>
<td>X</td>
<td>X</td>
<td></td>
<td>X</td>
<td>X</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Hungary</td>
<td>1990</td>
<td>X</td>
<td></td>
<td></td>
<td>X</td>
<td>X</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Hungary</td>
<td>1994</td>
<td>X</td>
<td></td>
<td></td>
<td>X</td>
<td>X</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Hungary</td>
<td>1998</td>
<td>X</td>
<td></td>
<td></td>
<td>X</td>
<td>X</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Poland</td>
<td>1990</td>
<td>X</td>
<td></td>
<td></td>
<td>X</td>
<td>X</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Poland</td>
<td>1993</td>
<td>X</td>
<td>X</td>
<td></td>
<td>X</td>
<td>X</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Poland</td>
<td>1995</td>
<td>X</td>
<td>X</td>
<td></td>
<td>X</td>
<td>X</td>
<td></td>
<td></td>
<td></td>
<td></td>
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<tr>
<td>Poland</td>
<td>1997</td>
<td>X</td>
<td>X</td>
<td></td>
<td>X</td>
<td>X</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Poland</td>
<td>1999</td>
<td>X</td>
<td>X</td>
<td></td>
<td>X</td>
<td>X</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Russia</td>
<td>1993</td>
<td>X</td>
<td>X</td>
<td></td>
<td>X</td>
<td>X</td>
<td></td>
<td></td>
<td></td>
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<tr>
<td>Russia</td>
<td>1995</td>
<td>X</td>
<td>X</td>
<td></td>
<td>X</td>
<td>X</td>
<td></td>
<td></td>
<td></td>
<td></td>
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<tr>
<td>Russia</td>
<td>1996</td>
<td>X</td>
<td>X</td>
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<td>X</td>
<td>X</td>
<td></td>
<td></td>
<td></td>
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<tr>
<td>Russia</td>
<td>1999</td>
<td>X</td>
<td>X</td>
<td></td>
<td>X</td>
<td>X</td>
<td></td>
<td></td>
<td></td>
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</tr>
<tr>
<td>Slovakia</td>
<td>1994</td>
<td>X</td>
<td>X</td>
<td></td>
<td>X</td>
<td>X</td>
<td></td>
<td></td>
<td></td>
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<tr>
<td>Slovakia</td>
<td>1998</td>
<td>X</td>
<td>X</td>
<td></td>
<td>X</td>
<td>X</td>
<td></td>
<td></td>
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<tr>
<td>Slovakia</td>
<td>1999</td>
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<td>X</td>
<td></td>
<td>X</td>
<td>X</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

FDI = Foreign Direct Investment; GDP = Gross Domestic Product
Regional Economic Voting

Czech okresy, the thirty-eight (1992–1994) and seventy-two (1998–1999) Slovak okresy,27 the forty-nine Polish województa,28 and the twenty Hungarian megye. Finally, for reasons detailed in Chapter 5, only parties receiving greater than 2% of the overall vote are included in the analyses.29

The Statistical Model

Many previous aggregate level analyses of election results have relied on some form of standard least squares (LS) regression analysis.30 There are many reasons why LS regression analysis has long been used to analyze election results, including the fact that electoral data are continuous and there is often no reason to expect a nonlinear relationship between any of the variables. However, there are methodological problems associated with using LS regression analysis in models that attempt to explain vote outcomes.

To assess these problems, consider first the nature of electoral data. If we think of each party’s share of the vote as a proportion of the total vote, then there are two important characteristics of voting data. First, each party’s share of the vote must be between 0 and 1. Second, the share of the vote earned by all parties must sum to 1. Stated more formally, the proportion of the Vote ($V$) for each party $j (j = 1, \ldots, J)$ in district $i (i = 1, \ldots, I)$ must meet the following criteria:

\begin{align}
V_{ij} &\in [0, 1] \quad \text{for all } i \text{ and } j \\
\sum_{j=1}^{J} V_{ij} &= 1 \quad \text{for all } i
\end{align}

27 Slovakia changed its administrative regions in 1996; for information, see Krause 2000, 37.

28 For the 1991 Polish parliamentary election, I rely on a breakdown of the vote by województwo provided in Gebethner (1995). For the 1993 and 1997 parliamentary elections, the national election commission reported the results for fifty-two electoral districts, forty-seven of which overlapped exactly with their corresponding województwo. Of the remaining five, three electoral districts were found in Katowice wojewodztwo (Sosnowiec, Katowice, and Gliwice) and two in Warsaw (m. st. Warszawa and warszaskie z wyłączaniem m. st. Warszawy). The votes from these districts were summed by the author to come up with vote totals for the entire województwo in both cases.

29 The one exception to this rule, the Association of Slovak Workers (ZRS) in the 1998 Slovak parliamentary election, is also explained in Chapter 5.

30 See, for example, Tuft 1978; Lewis-Beck and Bellucci 1982; Paldam 1991; Powell and Whitten 1993.

31 I adopt the notation of Katz and King (1999).
Comparative Cross-Regional Analysis

LS regression models, which generate continuous, unbounded predictions, do not guarantee that either of these conditions will be met. Predicted vote proportions for individual parties can be below 0 and above 1, and predictions for all parties are not constrained to sum to 1.\textsuperscript{32}

For this reason, I use a statistical model to conduct the regression analyses in this study that is more appropriate for multiparty electoral data than LS analysis.\textsuperscript{33} A full explication of this model can be found in Tomz, Tucker, and Wittenberg (2002), but the basic distinction from LS analysis is that the dependent variable is modeled as the log of the ratio of the vote for the party in question relative to a base category (the vote for one or more of the other parties) and that all of the equations for any given election are estimated simultaneously using a seemingly unrelated regression (SUR) model. As a result, all predicted vote proportions are bounded between 0 and 1, and the sum of all predicted vote proportions must equal 1; estimating the equations simultaneously using the SUR also allows the analysis to take advantage of the likely covariance across equations from the same election.\textsuperscript{34}

More technically:

- $V_i = (V_{i1}, \ldots, V_{i(J-1)})$ is a vector of vote proportions of party $j = (1, \ldots, J - 1)$ for each district $i$ ($i = 1, \ldots, n$)

\textsuperscript{32} See Brandt, Monroe, and Williams (1999) for more on the topic.

\textsuperscript{33} A widely noted attempt to address these issues in the political science literature was made in Katz and King (1999). In the following years, a number of alternative models building on similar insights but adding additional features were published; in addition to Tomz, Tucker, and Wittenberg 2002, see Honaker, Katz, and King 2002; Jackson 2002; and Mikhailov, Niemi, and Weimer 2002. Having worked with the model presented in the text since the beginning of this project, I chose to continue to do so, but this does not reflect my belief that it is in any way better than any of the available alternatives, and all are undoubtedly more appropriate techniques to use in modeling multiparty data than LS analysis. One strong advantage of the model I employ from the perspective of replication is that it is very easy to implement, as all of my analyses can be conducted using just three lines of code in STATA that have been automated into the (freely available) CLARIFY software package (Tomz, Wittenberg, and King 2003). A new approach to estimating multiparty data based on techniques of robust estimation utilizing a hyperbolic tangent (tanh) estimator is presented in Mebane and Sekhon (2004). Although early assessments of the method appear promising, it appeared too late in the life cycle of this current project for me to incorporate it into this book.

\textsuperscript{34} As is noted in Tomz, Tucker, and Wittenberg 2002, “one could estimate the $\beta_*$ via $J - 1$ separate linear regressions, but the SUR technique is more convenient, since it allows users to estimate the entire system of equations with one simple command. Moreover, the SUR is potentially more efficient, because it takes advantage of interesting information about covariance of the equations. . . . At a minimum, though, the SUR will be more convenient and no less efficient than equation-by-equation OLS” (p. 68).
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- $Y_i$ is a vector of $J - 1$ log ratios where $Y_{ij} = \ln(V_{ij}/V_{iJ})$ for party $j$ ($j = 1, \ldots, J - 1$) relative to party $J$.
- $Y_i$ is multivariate normal with a mean $\mu$ and a variance of $\Sigma$.
- Means can be estimated as a linear function of explanatory variables, whereby:
  $$\mu_{ij} = X_{ij}\beta_j$$
- Equations are estimated simultaneously for all parties $j = 1, \ldots, J - 1$ using a seemingly unrelated regression

Thus to return to the illustrative example, for the 1996 Czech Republic elections, the actual statistical model estimated is:

$$\ln(\%Vote_{ij}/\%Vote_{iJ}) = \alpha_j + \beta_{1j}UNEMPLOYMENT_i + \beta_{2j}\Delta WAGES_i + \beta_{3j}\%INDUSTRIAL.GROWTH_i + \beta_{4j}\%INDUSTRY_i + \beta_{5j}\%ELDERLY_i + \beta_{6j}\%URBAN_i + \beta_{7j}\%AGRICULTURE_i + \beta_{8j}\ln[POPULATION_i] + \epsilon_{ij}$$  \hspace{1cm} (3.3)

where
- $i = \text{region} (1, \ldots, I)$
- $j = \text{party} (1, \ldots, J-1)$ (ODS, ODA, KDU, KSČM)
- $J = \text{total of all other parties}$

Note that this equation estimates the effect of economic conditions on the vote for all of the parties for which the Incumbency (ODS, ODA, KDU), New Regime (ODS, ODA), and Old Regime (KSČM) hypotheses make predictions; the remaining parties that contested the election are included in the other (“J”) category and function as the base category for the estimation of the model. All five control variables are included in the regression, and the independent economic variables are the three core variables: unemployment, change in wages, and growth. Table 3.3 presents the results of estimating Equation 3.3 for the 1996 Czech Republic elections.

These regression results presented in Table 3.3 reveal interesting findings about the effect of the economy on the vote for these four parties. But it is unclear how useful they are for the larger task of assessing whether or not each case provides support for the standard economic voting hypotheses presented in the previous chapter, or how we would compare these results with those found from another election. This would be a difficult enough task if we employed a model such as LS analysis, which would generate
Comparative Cross-Regional Analysis

Table 3.3. *Estimated Coefficients (Standard Errors) of Effect on Party Vote for the 1996 Czech Parliamentary Elections*

<table>
<thead>
<tr>
<th></th>
<th>ODS</th>
<th>ODA</th>
<th>KDU</th>
<th>KSCM</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Unemployment Rate</strong></td>
<td>-.087</td>
<td>-.113</td>
<td>-.087</td>
<td>.029</td>
</tr>
<tr>
<td></td>
<td>(.013)</td>
<td>(.021)</td>
<td>(.033)</td>
<td>(.013)</td>
</tr>
<tr>
<td><strong>Change in Income</strong></td>
<td>.001</td>
<td>.008</td>
<td>.004</td>
<td>.000</td>
</tr>
<tr>
<td></td>
<td>(.007)</td>
<td>(.011)</td>
<td>(.018)</td>
<td>(.007)</td>
</tr>
<tr>
<td><strong>Industrial Growth</strong></td>
<td>-.001</td>
<td>.002</td>
<td>-.006</td>
<td>.001</td>
</tr>
<tr>
<td></td>
<td>(.001)</td>
<td>(.002)</td>
<td>(.003)</td>
<td>(.001)</td>
</tr>
<tr>
<td><strong>Percent Agriculture</strong></td>
<td>-.016</td>
<td>-.016</td>
<td>.082</td>
<td>.003</td>
</tr>
<tr>
<td></td>
<td>(.006)</td>
<td>(.009)</td>
<td>(.015)</td>
<td>(.006)</td>
</tr>
<tr>
<td><strong>Percent Industry</strong></td>
<td>-.005</td>
<td>-.005</td>
<td>.018</td>
<td>-.009</td>
</tr>
<tr>
<td></td>
<td>(.003)</td>
<td>(.005)</td>
<td>(.007)</td>
<td>(.003)</td>
</tr>
<tr>
<td><strong>Percentage Elderly</strong></td>
<td>.036</td>
<td>.052</td>
<td>.000</td>
<td>.007</td>
</tr>
<tr>
<td></td>
<td>(.013)</td>
<td>(.020)</td>
<td>(.031)</td>
<td>(.012)</td>
</tr>
<tr>
<td><strong>Percent Urban</strong></td>
<td>.000</td>
<td>-.000</td>
<td>-.012</td>
<td>-.003</td>
</tr>
<tr>
<td></td>
<td>(.002)</td>
<td>(.002)</td>
<td>(.004)</td>
<td>(.001)</td>
</tr>
<tr>
<td><strong>Log Population</strong></td>
<td>.050</td>
<td>.099</td>
<td>.806</td>
<td>-.069</td>
</tr>
<tr>
<td></td>
<td>(.047)</td>
<td>(.073)</td>
<td>(.115)</td>
<td>(.045)</td>
</tr>
<tr>
<td><strong>Constant</strong></td>
<td>-1.266</td>
<td>-4.675</td>
<td>-11.418</td>
<td>-.531</td>
</tr>
<tr>
<td></td>
<td>(1.038)</td>
<td>(1.628)</td>
<td>(2.564)</td>
<td>(1.008)</td>
</tr>
<tr>
<td>R-Squared</td>
<td>0.61</td>
<td>0.51</td>
<td>0.60</td>
<td>0.36</td>
</tr>
<tr>
<td>N</td>
<td>76</td>
<td>76</td>
<td>76</td>
<td>76</td>
</tr>
</tbody>
</table>

* Models estimated using seemingly unrelated regression (SUR) with logistic transformation of dependent variable.


... one intuitively logical set of coefficients and standard errors for each of the eighty-eight parties being analyzed. But an important feature of this model – and any multiparty election model that uses a logistic transformation of the dependent variable relative to a base category – is that coefficients need to be interpreted in the same manner as one would interpret a coefficient from a multinomial logit model, namely, as a predictor of the effect of the variable in question on the relationship between the vote for the party in question relative to vote for the base party. Thus, rotating the base party (e.g., including ODS as the base party and the “Other” category as one of the four dependent variables) will produce a different set of coefficients, standard errors, and r-squared values in exactly the same way as a multinomial logit analysis. Thus, the correct way to interpret the coefficient for...
unemployment on the ODS in Table 3.3 is not as the effect of unemployment on the overall vote for the ODS but, rather, as the effect of unemployment on the vote for the ODS relative to the composite “other” category (the vote for all parties besides the ODS, ODA, KDU, and KSČM).

Mathematically, however, the results are equivalent no matter what party is included as the base party. Thus, switching to a regression in which ODS is the base party will produce the exact same coefficients (with the opposite signs) and standard errors for the “Other” category regression from what is found for ODS with “Other” as the base category in Table 3.3. The same holds for all combinations of parties with one party in the set of estimated equations and one party included as the base category. What this means is that if we want to assess the empirical support for the different hypotheses on the basis of coefficients and standard errors, examining the results presented in Table 3.3 only presents one part of the picture. For a full sense of the effects of economic conditions, one would actually need to rotate through every party except one as the base party in turn in order to get a complete set of coefficients and standard errors.\(^{35}\) This is complicated enough in the case of one election, but with an average of four economic variables per analysis and four and a half parties per election over twenty elections, any overall assessments of the empirical support offered from these different cases for the various standard hypotheses would have to be based on comparing close to two thousand coefficients and standard errors, which would quickly become an extremely subjective and very tedious enterprise. Moreover, with a log-transformed dependent variable, none of these coefficients would have any real intuitive meaning. Thus, in order to analyze the hypotheses comparatively, a much more compact estimate is needed of the effect of economic conditions on election results for each party that can be compared across cases.

An appropriate measure to use in this case is a first difference that calculates the change in the predicted share of the vote for each party when demographic variables are held constant and economic conditions are varied to simulate a standardized shift from a “bad” economic situation to a “good” economic situation.\(^{36}\) To do this in practice, I calculate two estimates

\(^{35}\) We would not need to rotate every party through because we can calculate the final party from the other regressions. For an example of a presentation of results from a multinomial logit analysis that reflects the complexity of interpreting results by rotating base parties, see Brader and Tucker 2001.

\(^{36}\) There are other options for solving this problem. One approach is to pick only one variable to be representative of the overall state of the economy. So, for example, Aguilar
for the expected vote of the party in question: one with economic variables at their 10th percentile level and another with economic variables at their 90th percentile level. In both cases, demographic variables are set at their means. Negative indicators, such as unemployment, are shifted from the higher value to the lower value, and vice versa for positive indicators, such as change in income. When the expected vote in the “bad” economic situation (e.g., high unemployment, low wage growth) is subtracted from the expected vote in the “good” situation (e.g., low unemployment, high wage growth), the result is a single measure of the overall impact of better economic conditions on the vote for that particular party, which is exactly what is needed to analyze the Incumbency, New Regime, and Old Regime hypotheses. Moreover, not only do first differences have the advantage of producing a single, intuitive measure of the effect of economic conditions on the electoral fortunes of the party in question, they also are unaffected

and Pacek (2000) use only change in GDP, Pacek (1994), Rattinger (1991), and Gibson and Cielecka (1995) all rely on unemployment to measure the state of the economy, and Posner and Simon (2002) utilize poverty. An alternative approach – although not widely employed – is to craft an index that attempts to capture multiple measures of economic conditions in a single variable; one example of this approach using individual level data is Nadeau, Niemi, and Yoshinaka (2002). Both approaches, however, require the analyst to make a priori decision about the relative importance of different economic variables, either through excluding some variables to privilege others or in deciding on the relative weights of variables in the composition of an index. Another type of approach is to include multiple variables in different regressions, and then for the analyst to highlight particularly interesting coefficients and standard errors (see, for example, Bell 1997; Fidrmuc 2000c). While this method is clearly appropriate for providing thick description of individual election results, it is less clear how useful it is in assessing general support for the types of standard economic voting hypotheses presented in the previous chapter. Would two out of three variables in the correct direction provide support for the hypothesis? What about three out of five? Or two in the correct direction with small substantive effects, but one in the wrong direction with a large substantive effect? Ultimately, such a method rests on the subjective insights of the particular analyst for comparison across cases in a way that the method I utilize here does not require. Furthermore, comparing the significance of coefficients in this manner becomes increasingly difficult as the number of cases increases. Indeed, Bell (1997) takes pages to describe her findings, and she is only concerned with estimating the effect of two variables (unemployment and income) across four Polish elections. To adopt such an approach with twenty elections – especially if one hoped to conduct robustness tests such as the ones I describe later in this chapter and present in Chapter 8 – would be extremely, if not prohibitively, cumbersome.

As is described later in this section, one of the robustness tests I employ involves changing the percentiles employed in the economic shock.

For example, if there were one hundred regions, we would use the unemployment level from the region with the 10th highest rate in the first case and from the 10th lowest rate in the second case.
by mathematic definition by the choice of which party is included as the base party for the purpose of the regression analysis.

There is, however, one important shortcoming in comparing first differences. In moving from a set of betas with standard errors to a single point estimate of a first difference, however, we run the risk of losing information regarding our uncertainty in the quantity of interest being reported. Fortunately, this can be avoided by using stochastic simulation to simulate an entire distribution of first differences.\(^{39}\) Such an approach has the added benefit of allowing us to quantify not only our best estimate of the first difference (e.g., the mean value of the simulations) but also our level of uncertainty surrounding that estimate (e.g., the standard deviation of the simulations). Moreover, we can easily generate confidence intervals surrounding substantively meaningful quantities of interest. For example, in the empirical analyses to follow, the crucial concern is whether we can be confident that the party in question either benefits from better economic conditions (incumbent and New Regime parties) or is hurt by better economic conditions (Old Regime parties). To assess this claim, we will want to know how confident we are that the first difference is greater than zero (in other words, that better economic conditions increase the expected vote for the party), or, in the case of Old Regime parties, less than zero. If 90 percent of the simulated first differences are greater than zero, then we can claim with 90 percent confidence that the party in question is helped by stronger economic conditions; the same holds in the reverse direction if 90 percent of the simulated first differences are less than zero. In this manner, calculating estimated probability distributions of first differences allows us to measure exactly what we need to test our hypotheses: how confident we should be that better economic conditions translated into better (or worse) election results for the party in question.

For ease of interpretation, these simulations can be plotted graphically, resulting in an estimated probability distribution of the first difference.

\(^{39}\) More technically, I draw one thousand betas from the sampling distribution of the parameter estimates. From each set of betas, a separate first difference is calculated. The simulations were performed using Clarify 2.1 (Tomz, Wittenberg, and King 2003). For a full treatment of the approach, see King, Tomz, and Wittenberg 2000. Thus in no way does relying on statistical simulation ignore the information contained in coefficients and standard errors; instead, it provides a substantively meaningful way for concisely summarizing this information.
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Figure 3.1  Estimated Probability Distribution of Effect of Economic Conditions on New Regime, Old Regime, and Incumbent Parties in 1996 Czech Republic Parliamentary Elections

Figure 3.1 plots these estimated distributions for the illustrative example, the 1996 Czech parliamentary elections.

From observing Figure 3.1, we find that this particular election provides clear empirical support for the New Regime and Old Regime hypotheses; and mixed empirical support for the Incumbent hypothesis. We are very confident that both New Regime parties, the Civic Democratic Party (ODS) and the Civic Democratic Alliance (ODA), performed better in areas of the country where economic conditions were better, and that the one Old Regime party, the Communist Party of Bohemia and Moravia (KSCM), performed better where economic conditions were worse (as approximately 99 percent of the simulations predict a decrease in the vote for the party in the “good” economic region as compared to the “bad” economic region). The evidence in support of the Incumbent hypothesis is not quite as consistent.
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As noted above, we are confident that the incumbent ODA and ODS enjoyed more electoral support where economic conditions were better. The evidence provided by our analysis of the vote for the Christian Democratic Union (KDU), the third incumbent party, by comparison, is very weak. Although our best guess at the effect of the economic shock (the mean of the simulations) is positive, almost 40% of the simulations predict that the party's fortunes would decline as economic conditions improved. Thus, we have very little confidence that this particular party provides empirical support for the Incumbency hypothesis, as we are almost equally confident that the party was hurt by better economic condition as we are that it was helped by better economic conditions.

The primary quantity of interest – our level of confidence that the empirical data supports the hypothesis in question for the party in question – also can be concisely summarized in a table that lists the percentage of simulations for each party that are in the direction predicted by the particular hypothesis being analyzed. This is exactly how the results are presented for the paired case studies in Chapters 5–7 (see Tables 5.2, 6.2, and 7.2); full graphical presentations of all simulations can be found in Appendix III.

Using stochastic simulation to calculate our confidence that each party either performed better or worse under better economic circumstances has three important advantages as a method for assessing the degree of empirical support for the hypotheses presented in the previous chapter. First, it produces a concise and objective measure of the degree of support provided by each party for each hypothesis that can easily be compared across different elections and different countries. Moreover, as the measure is calculated according to an a priori set of rules, the assessment of whether or not the case provides support for the hypothesis in question can be made in an entirely transparent manner. (For example, it is not difficult to justify a claim that more support is provided for a hypothesis by Party A than Party B if Party A has 99% of the simulations in the correct direction and Party B has 55% in the correct direction. Compare this to an assessment of results based on looking at all of the coefficients and standard errors where Party A has two small statistically significant coefficients in the correct direction, one large but statistically insignificant coefficient in the correct direction, and one large statistically significant coefficient in the wrong direction and Party B has three small and one large statistically insignificant coefficients in the wrong direction. It may very well be the case that there is more support for the hypothesis provided by Party A in both cases, but the former
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method allows for that conclusion to be drawn in much more objective and transparent manner. This is evident even in the case of two parties, but clearly becomes much important when comparing large numbers of parties.) Second, stochastic simulation of first differences produces unbiased estimates of the first differences regardless of which party is modeled as the base party in the analysis, thus eliminating the problem of needing to run multiple versions of each analysis that would be necessitated by direct comparisons of coefficients and standard errors.40 Finally, the method produces a quantitative measure that directly assesses the degree of empirical support for the standard economic voting hypotheses presented in the previous chapter. These hypotheses speak to whether or not particular types of parties perform better or worse in areas of the country where the economy is stronger than in areas of the country where the economy is weaker, and this method measures the empirical support for exactly that proposition: based on our best measures of the strength of the economy after controlling for demographic indicators, what exactly is our confidence that a particular party did in fact perform better or worse where economic conditions were stronger?

Before concluding this section, it is important to reiterate that the quantitative analysis described here is not designed to provide inferences about the motivation for individual level behavior. Instead, the analysis assesses the degree to which regional variation in economic conditions affected regional variation in election results. This is an appropriate approach because, as noted in the previous paragraph, this is exactly what the standard economic voting hypotheses predict: whether particular types of parties enjoy more electoral success in parts of the country with different economic conditions. So although the motivation for these hypotheses presented in the previous chapter contained references to assumptions about how individuals would likely behave, it is important to note again that the empirical analyses included in the study test only the implications of these assumption – the aggregate level relationships between economic conditions and election results – and not the assumptions about individual level behavior themselves.

40 Although the estimated distribution may vary across repeated runs, all variance will be a result of the unbiased variance in the simulation procedure and not a result of the choice of base party. Put another way, repeatedly running simulations with the same base party will generate no less variation across sets of simulations than repeatedly running simulations while rotating the base party.
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Robustness Tests

The final component of the method involves running a series of robustness tests on the overall findings produced by the statistical analyses. As anyone familiar with quantitative analysis is aware, changing variables in estimated equations can produce different results. Although it can be complicated enough to assess the degree to which results change when working with one regression from one dataset, the question of the robustness of findings is considerably more complex when working with findings from eighty-eight different regression using twenty different datasets. Thus, in order to assess the robustness of my findings, I adopt the following strategy. Rather than focus on the effect of various specifications of the model for any one particular party or election, I run the entire analysis from scratch on every single party using a standardized change from the original model and then compare whether or not the overall findings regarding the relative empirical support for the different standard economic voting hypotheses change in any appreciable manner. I do this four different times, using four different types of standardized changes.

In light of the concerns mentioned in the previous section, the first test is probably the most important, and involves running each of the regressions using only the five control variables and the three base independent variables that are available in (almost) all cases: unemployment, change in income, and growth.\textsuperscript{41} Conversely, in the second robustness test I add an additional independent variable to each regression, which is a measure of poverty.\textsuperscript{42}

The third robustness test focuses instead on the dependent variable, and involves increasing the number of parties for which the effects of economic

\textsuperscript{41} As mentioned previously, unemployment rates were not available at the regional level for the 1991 Russian presidential election. To deal with the peculiar case of wage arrears – important in the Russian context, and a non issue in the other four countries – I actually run this test twice for the Russian cases for which arrears were available, once with just the three base variables and once with the three base variables plus wage arrears.

\textsuperscript{42} Ideally, one would use the percentage of people living below the poverty line – or some similarly low standard of living – to measure poverty. Unfortunately, such measures were only available in Poland and Russia, and even in these countries only in the second half of the decade. In the other cases, I have substituted either a per-capita measure of welfare spending, the percentage of people receiving social assistance, or the infant mortality rate. Thus, it would not be a particularly appropriate test for comparing the effects of poverty across different countries or elections. However, as the key point here is to see if the overall results change with an extra economic variable in the model, and not to make any particular assessments about the effects of poverty on election results, the fact that these measures are getting at different aspects of poverty is less of a concern in this particular instance.
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conditions are being estimated in each election. Recall that because of the statistical model, all parties that are neither incumbent, New Regime, nor Old Regime parties are included in the “Other” category that serves as the base party for the logistic transformation of the vote for the parties included in the analysis. Therefore, as we estimate the effect of the vote on more parties, the composition of this base category changes. The purpose of this robustness test, therefore, is to see whether the results for the parties in which we are particularly interested – incumbent, New Regime, and Old Regime parties – change when the composition of the base category changes.43

The final robustness test involves the size of the economic shock. As noted earlier, the primary shock employed is one that shifts economic variables from the 10th percentile to the 90th percentile. As a robustness test, therefore, the size of this shock was decreased to a shock from the 20th to the 80th percentile.

The details and results of these different tests are all presented in Chapter 8. The net result of all these robustness tests, however, is exactly the same: in no way do any of these changes affect the overall conclusion of stronger empirical support for the Transition Model than the Referendum Model. In fact, given the number of cases involved, the aggregate findings remain remarkably similar across all of the robustness tests.

In addition to these specification based robustness tests, recall that in the previous chapter I noted that one way of thinking about the conditional economic voting hypotheses was as theoretically oriented robustness tests of the analyses of the standard economic voting hypotheses. These robustness tests do not change the specification of models, but rather the cases included in the overall assessments of the standard hypotheses. So readers interested in more robustness tests can turn to the analyses of the conditional hypotheses to examine what the overall findings of the book would have been had I changed the case selection to any of the subsets

43 The extra parties included in the analysis were chosen based on substantive interest. This includes important parties that were neither New Regime, Old Regime, or incumbent parties (e.g., the Czech Social Democratic Party or Fatherland-All Russia), the borderline New Regime parties discussed in the appendix to Chapter 5, and the nationalist parties analyzed in Chapter 6. When possible, agrarian, ethnic, and religious parties were also selected as well. As with the standard analyses, parties receiving less than 2 percent of the vote were not selected. At an earlier stage of the project, I also conducted a similar robustness test by decreasing the number of parties in each analysis and again found practically no effect on the remaining parties.
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of categories in the conditional hypotheses (e.g., had I included only parliamentary elections, only high responsibility elections, only late decade elections, etc.).

Finally, in the chapters that follow I adopt a level of 90% of the simulations in the correctly specified direction as a threshold for identifying cases in which there appears to be “strong” empirical support for the hypotheses as a means of concisely summarizing my findings across cases. I use this level both because of the history of using 90% certainty in the discipline, and because it produced a fairly clean split in the data; the closest cases that did not reach the threshold were at least 5% away on both the positive and negative sides.44 But the choice of 90% is of course arbitrary, so as the very first robustness test in Chapter 8, I present assessments of the standard hypotheses using different thresholds for “strong” empirical support.

To sum up, the method of comparative cross-regional analysis involves the following components. Cross-regional datasets from a single election are used to estimate the effect of economic conditions on the election results of each party individually for which one of the standard economic voting hypotheses makes a prediction. These regional economic voting effects are estimated by a compositional data model, and stochastic simulation is used to estimate the probability distribution of standardized economic shocks (first differences) on the vote for each party individually. On the basis of these estimates, the confidence that each party presents empirical support for the relevant hypothesis(es) is calculated, and these confidence levels are then compared across cases. Comparisons are conducted using both case studies of individual pairs of elections (which focus on the actual confidence level for each relevant party) and general comparisons across all twenty elections (which focus on the number of cases in which a threshold of confidence is reached). Finally, these overall assessments are checked using a series of robustness tests that involve changing the specification of each equation in a standard format and then rerunning all of these analyses again from the start.

44 Noting whether 90% of the simulations are in the correctly predicted direction approximates a 90% confidence level because it is essentially a one-tailed test: all three of the standard hypotheses have a predicted direction in which the effect is expected to be. I thank Garret Glasgow for highlighting this point. Nevertheless, as is illustrated in Table 8.2, choosing a 95% threshold for claiming strong empirical evidence would have yielded the exact same results regarding the relative support for the Old Regime, New Regime, and Incumbency hypotheses.
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Advantages and Disadvantages of Comparative Cross-Regional Analysis

All methods of analysis in political science – be they quantitative or qualitative – have their advantages and disadvantages. Most books that are not explicitly focused on methodological topics usually do not take too much time to dwell on these concerns, in part because most studies rely on methodology that has been used before and because many excellent treatments of methodological questions exist in works devoted precisely to these tasks. However, as the method of comparison I employ in this book is novel, it is important to devote at least some attention to the tasks of highlighting the advantages and disadvantages of the method. Before doing so, I want to reiterate the point made in the introduction of this chapter, which is that none of the individual components of the method are new: there are other studies of the effects of economic conditions on election results that utilize regional level data in a comparative framework; the statistical model has been analyzed in an article in Political Analysis; and an emphasis in quantitative work on substantively meaningful quantities of interest as opposed to simply coefficients and standard errors has long been popular in certain quarters of political science. However, with the exception of my own previous work on this project, the three have never before been combined in this manner, and so I devote the rest of this chapter to addressing some of the advantages and disadvantages of the method I have chosen to employ. Readers who are not particularly interested in methodological considerations are invited to skim or even skip this section entirely; there are no empirical results presented here, and the theoretical arguments are not advanced in any way beyond what was presented in the previous chapter.

I begin with the assumption that there is no silver bullet research strategy that is perfect for studying the effect of economic conditions on election results in a comparative context. And although this section is largely focused on the method employed in this book, I do want to take just a

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45 See note 25 in Chapter 1.
47 And has certainly seen a renewed emphasis since the publication of “Making the Most of Statistical Analysis: Improving Interpretation and Presentation” (King, Tomz and Wittenberg 2000) in the American Journal of Political Science and its accompanying Clarify software.
48 For a striking example of this, see the criticism of aggregate time-series models in Fiorina 1981, Chapter 6, as compared to the criticism of survey-based approaches leveled in Kramer 1983.
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moment to mention that other methods have concerns as well. Single country time-series analysis, which has played a crucial role in the development of the field of economic voting in the American context, faces the serious challenge of accounting for other over-time developments that are not included in the model, which is why we have seen the use of “war” and “Watergate” dummies and corresponding criticisms of this approach. Moreover, single country time-series analysis requires the presence of enough competitive elections to actually construct a large enough sample of observations, which is extremely problematic in countries with short histories of competitive elections. Cross-national time-series analysis, although increasing the number of observations, has the same time related problems as single-country time series analyses while also introducing questions of country-specific effects, comparability of data, and the question of how to correctly model cross-sectional time-series data. Comparative survey analysis, in addition to all of the standard methodological issues confronting single country survey analysis such as selection bias, question wording, and ordering of response items, also has to contend with the question of whether or not similar questions are interpreted differently in different countries, a topic that has recently attracted growing interest in the political science literature, and is especially consequential for surveys that are not explicitly designed to be compared. And as will be discussed later in this section, pooling cross-regional data across time or countries introduces serious questions about the appropriate comparability of data, in addition to the previously mentioned concerns about the fact that different countries report different economic statistics at different periods of time.

This is not meant to imply that we can not learn useful information about the relationship between economic conditions and election results in the postcommunist context by using one or more of these methods, but only that no approach is without its drawbacks. I have elected to use the comparative cross-regional approach because of its strength as a methodology; I leave it to others who choose to adopt alternative methodological strategies to make the case in this manner for their approaches as well. In the rest

49 See, for example, King, Murray et al. 2004. On selection bias, see Berinsky 2004; in the postcommunist context, see Berinsky and Tucker 2003. For an interesting recent methodological approach to comparative survey analysis, see Long 2004. Recent efforts to design surveys that are explicitly designed for cross-national analysis may help to alleviate these types of problems. See, for example, the Comparative Study of Election Systems (http://www.umich.edu/~ces/) and Huber, Kernell, and Leoni 2003 for a study utilizing these data.
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of this section, I highlight what I think are the most important benefits of
the method that I use and then discuss what I think are its most serious
drawbacks.

Advantages of Comparative Cross-Regional Analysis

Comparative Emphasis  First and foremost, the comparative cross-
regional approach is characterized by the fact that it is explicitly compara-
tive: it is designed to compare findings both over time and across coun-
tries. Although this seems an almost trivial point to highlight at this point
in the book, it is worth noting both the importance of comparative anal-
ysis and its relative paucity in the postcommunist electoral landscape. In
the review I conducted of the 101 articles on elections and voting in post-
communist countries described in Chapter 1, fewer than half (40) compared
more than one election, and of these only thirteen explicitly compared elec-
tions from different countries.50 And although studies of single elections
are of course extremely valuable, most country-level experts armed with
reliable survey data can always produce a multitude of compelling explana-
tions for why the vote turned out the way it did in that particular election.
Comparative analysis, by contrast, allows us to examine support for gen-
eral hypotheses across different contexts and to present empirical analyses
that move beyond a particular circumstance. Although one can of course
test general hypotheses using data from just a single election, the possibil-
ity always remains that the results may in some way be a function of the
peculiar circumstances surrounding that particular election. A comparative
study – and a methodological approach designed to facilitate such a study –
serves an important role in insulating one’s findings from such a critique.

At the same time, most prior comparative studies of the relationship
between economic conditions and election results reduce each election to
but a single observation, whether it is a single country time-series analysis,51
cross-national analysis,52 or cross-national time-series analysis.53 Such a

50 For books that compare postcommunist election results across multiple countries, see
51 See, for example, Tufte 1975; Rosa and Amson 1976; Lewis-Beck and Bellucci 1982; Bellucci
52 See, for example, Wilkin, Haller, and Norpoth 1997.
53 See, for example, Remmer 1991; Powell and Whitten 1993; Palmer and Whitten 1999;
Roberts and Wibbels 1999; Whitten and Palmer 1999; Chappell and Veiga 2000; Royed,
Leyden, and Borrelli 2000.
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framework must contend with the possibility that election results are being influenced by election specific events that are not captured by the model. Moreover, as long as each national election is only one observation in the analysis, it is very difficult to learn anything about the effect of economic conditions on different parties within each election. Although less important in the two-party context of the United States, this is an issue worth considering in multiparty democracies. For example, if there are three incumbent parties competing in an election, any study that uses national election results as the unit of analysis must either code the incumbent vote from that election as all three parties, one of the three as the primary incumbent party (if there is an emphasis on a “primary” incumbent party) or some combination thereof. Moreover, whatever results are returned from the regression analysis will speak to the findings across all of the elections, but will tell us little about the effects of economic conditions on the vote for the three political parties from the election in question.

Comparative cross-regional analysis, by contrast, addresses both of these issues. By only estimating equations from one election at one period of time, all election specific effects are controlled for when equations are estimated, as was explained in the previous section. Thus, there is no need for a “war” or “scandal” control in the statistical analysis, because these factors are shared contexts across the entire electorate. At the same time, comparative cross-regional analysis produces party by party estimates of the effect of economic conditions on each party included in the analysis. To return to the example of the 1996 Czech Republic election, we can learn from this method that the Incumbency Hypothesis is supported by empirical data for two incumbent parties, the ODS and the ODA, but not in the case of the third incumbent party, the KDU. With this information in hand, one can then begin to ask questions about what differentiates the ODS and the ODA from the KDU, such as, for example, that the ODS and ODA are both also New Regime parties, whereas the KDU is not a New Regime party. The ability to make this kind of distinction is even more illuminating when considering the 1998 Hungarian election. This election feature two incumbent parties, one that was a New Regime party and one that was an Old Regime party. By investigating the effect of economic conditions on each party

54 For an interesting take on this problem, see Wilkin, Haller, and Norpoth 1997. In this article, the authors estimate their model twice, once using all incumbent parties and once using just the primary incumbent party.
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individually, I find strong support for the Incumbency hypothesis in the case of the incumbent–New Regime party, but no support for the Incumbency hypothesis in the case of the incumbent–Old Regime party (see Chapter 5 for details). Without the ability to generate party by party estimates of the effect of economic conditions, this important distinction between the two incumbent parties would be obscured.

Indeed, generating party by party estimates addresses what is one of the more vexing problems in studying the effect of economic conditions on election results even in established democracies, which is the continued empirical evidence generated from these studies that the relationship between economic conditions and voting behavior varies over time and across contexts. To note just a few examples: Lin (1999) argues that economic voting is dependent on electoral context by examining American elections stretching back into the nineteenth century; Palmer and Whitten (1999) argue that the relationship between economic conditions and the vote for incumbent governments in OECD countries has become more volatile over time; and Alvarez, Nagler, and Willette (2000) report that economic conditions take on different levels of importance in the United States, Canada, and the United Kingdom. By generating party by party level estimates of the effect of economic conditions, I am able to examine the extent to which these concerns hold in my particular cases.

And although it is of course possible to build control variables into a cross-national model to control for variation in the relationship between economic conditions and election results, each new attempt to do so involves a new specification of the primary model for assessing the affect of economic conditions on election results. Comparative cross-regional analysis, by contrast, allows for one original specification of the statistical model for each election – which controls for any and all of these election specific effects – to generate the party by party estimates of the effect of economic conditions. Once generated, these results can be compared in a multitude of ways (e.g., all of the conditional economic voting hypotheses presented in the previous chapter), but without ever needing to respecify the original statistical model. We can therefore use the same set of original analyses to examine whether there is more support for the standard economic voting hypotheses earlier in the decade, in high responsibility election, or in East-Central European elections, for example. Moreover, as noted previously, we also can examine the degree of support generated for any hypothesis by any particular party, something that is not possible using a pooled cross-national model.
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**Availability and Appropriate Use of Data**  Another advantage of comparative cross-regional analysis is that it vastly increases the amount of data that one can bring to bear upon the task of studying the effect of economic conditions on election results. A cross-national data set composed of these twenty elections would have only twenty observations. Moving to the regional level from the national level therefore allows us to drastically increase the number of observations on which we can base our conclusions. Of course, survey data also allows us to radically increase our number of observations from any aggregate study, but comparative analysis of survey data introduces all of the issues discussed previously about the availability and applicability of comparative survey analysis. Furthermore, it also moves us out of the realm of studying the effect of economic conditions on actual election results – in other words, the hypotheses presented in the previous chapter – and into the domain of individual voting behavior.

The question of the comparability of data raises another important advantage of the comparative cross-regional approach. When working with regional level data from the postcommunist context, the appropriateness of comparing regional data collected by different countries at different times is a legitimate question that warrants important consideration. Were I to pool all of my data into a single analysis, I would be making the assumption that a statistic from country A at time T in region X (e.g., unemployment in Bardejov, Slovakia, in 1992) measures the exact same economic reality as a statistic from country B at time T + 1 in region Y (e.g., unemployment in St. Petersburg, Russia, in 1999). This is a high level of assumption to put on regional data from established democracies, let alone data from countries undergoing the kinds of transitions found in the postcommunist world in the 1990s.

Conversely, the method that I have presented in this chapter only requires pooling data for statistical analysis within a single election. This type of analysis therefore relies only on the assumption that a statistic from country A at time T in region X (e.g., unemployment in Bardejov, Slovakia, in 1992) measures the same economic reality as a statistic from the same country A at the same time T in region Y (e.g., unemployment in Bratislava, Slovakia, in 1992). Thus, we need only assume that statistics collected at the same time under the direction of the same statistical agency are measured in the same manner. Although it is likely that even this assumption is violated in certain instances – especially earlier in the decade – it is undoubtedly a much, much lower threshold for comparability of data than to assume that regional data collected under the direction of different
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statistical agencies at different periods of time are measured in the same manner.

A slightly different way of thinking about this issue is that it makes measurement error much less likely to introduce bias into the analysis. As is illustrated in King, Keohane, and Verba (1994, Chapter 5), systematic measurement error can introduce bias into analysis, whereas measurement error that is randomly distributed does not bias results.55 If we believe that measurement error is likely to be present in economic statistics reported in transition countries, then the issue of measurement error has serious consequences for how we pool our data. If we think that measurement error is likely to vary either by country (e.g., Russia might systematically understate unemployment as compared to Poland) or within countries across time (e.g., Slovakia might have overstated growth earlier in the decade), then pooling data across these divides could introduce bias into the analysis. Pooling data within countries for a single time period, however, will only introduce bias if there is systematic variation in measurement error within a single country in a single time period (e.g., unemployment is systematically overstated in Western Hungary but understated in Eastern Hungary in 1994). Although we cannot dismiss out of hand the possibility that such patterns might exist, we certainly have no a priori reason to assume that measurement error will vary within individual countries in a single period; conversely, there are many reasons to think that we might find variation in measurement error across different countries and time periods. Or, to put it the same way as in the previous paragraph, the threshold necessary to assume that measurement error is randomly distributed within a single country at a single point in time is much lower than the threshold necessary to assume that measurement error is randomly distributed across countries.

Although the issue of comparability of data is serious when economic statistics are intended to measure the same phenomenon, such as unemployment rates, the issue is even more blatantly a problem when countries choose not to even report the same economic statistics at the regional level. Were all of the data to be pooled in a single analysis, we would either need to ignore all data that were not reported by all countries or else rely on some form of statistical procedure to impute entire series of missing data.56

55 Randomly distributed measurement error can, however, influence the efficiency of one’s results, and thus decrease the level of confidence we have in our results. This point is addressed in greater detail in the following section.
56 See, for example, King, Honaker et al. 2001.
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Moreover, even when countries choose to report the same statistic (e.g., the percentage of older citizens in a region) they may define this statistic differently (e.g., men over sixty and women over fifty-five in one country or everyone over sixty-five in another country). The comparative cross-regional approach, by contrast, both allows for the use of all available data in each individual statistical analysis without the need to impute nonreported data, and at the same time guarantees that the same definition of each statistic is being employed in each individual statistical analysis. With the data necessary for the current study, this would simply not be the case if the data were pooled across countries and elections.

For these reasons, comparative cross-regional analysis appears to be an especially appropriate method of analysis for studies of new democracies when different countries report different economic statistics and when there are questions about the comparability of data across elections. It allows us to greatly increase the data we can use to make our estimates of the effect of the economy on election results at a time when countries have had few elections while simultaneously leaning on these data only to the extent that is warranted. In doing so, it opens up a means of examining empirical support for general hypotheses about the relationship between economic conditions—as well as other factors that vary cross-regionally—and election results in areas of the world where it might not otherwise be possible to conduct comparative analyses.

Strategic Voting

An additional benefit of comparative cross-regional analysis is that it largely insulates one’s study from problems associated with the phenomenon of strategic voting. Although there are a number of different ways to think about strategic voting, the most common involves voters who shy away from their preferred party when it looks likely that the party (or candidate) is not likely to cross a minimum threshold necessary for the party (or candidate) to gain representation in the parliament (or win the contested seat, or make it into a second round of balloting). In an effort not to “waste” one’s vote, the strategic voter is then posited to cast her ballot for another party (or candidate) with a more likely chance of actually making it into the parliament.57 Had I chosen a cross-national research design in which each election represented only one observation, then the fact that certain parties (e.g., those close to a minimum threshold for representation

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57 See, in particular, Cox 1997. For a concise summary of different approaches to strategic voting, see Meirowitz and Tucker 2004.
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in the parliament) might see a drop off in their vote totals because of strategic voting whereas other parties (such as those safely above the threshold) would see an increase in vote totals could bias my results.

In a cross-regional framework, however, this problem disappears. Let us assume that an analysis of the vote for a particular party would show support for one of my hypotheses in the absence of strategic voting. For simplification, imagine a country with five regions, and this party would receive 10%, 10%, 8%, 6%, and 6% of the vote in each of these regions respectively, for an overall total of 8% of the national vote. Now assume that because of strategic voting, 3% of the electorate deserts the party. As long as the strategic voting occurs uniformly across regions – and as the strategic concern in question is a national threshold, there is no reason to assume it should not – the party will now receive 7%, 7%, 5%, 3%, and 3% of the vote in each of the regions, respectively. My analysis, which looks at the covariance between regional distribution of economic conditions and regional election results, will show the same result in either case. If the economy is performing best in the first two regions and worse in the last two regions, than either set of results will confirm that this particular party has performed better where the economy is better.58

Bridge between Methods Finally, the comparative cross-regional approach has the potential to function as a bridge between the two most popular means of studying the relationship between economic conditions and election results. Like survey analysis, cross-regional analysis can identify party specific effects for economic conditions within individual elections. And like cross-national analysis, comparative cross-regional analysis provides a method for testing the level of empirical support for general hypotheses across different contexts, including time and countries. At the same time, it allows us to avoid problems with both of these methods. By relying on aggregate data, we avoid the need to compare results from different surveys in different countries. But conducting this aggregate analysis at the regional level allows us to avoid treating each election as only a single observation,

58 If seats are distributed in particularly small districts – as is the case in parts of Poland – then the opportunity exists for strategic voting to have different effects in different regions. However, this requires that citizens know not only how popular a party was nationally, but also how close the party might be to passing the threshold necessary to gain a seat in an individual's home region. While it is not impossible to imagine some citizens in transition countries possessing this kind of information, it is difficult to see how such information would be broadly known.
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and, consequently, to estimate the effect of economic conditions on multiple parties from a given election. As a result of these party-by-party estimates of the effect of economic conditions, we can then ask the interesting question of why we find different results for different parties, which I do throughout this book with the conditional hypotheses. This blending of focused analysis on individual elections and the assessment of the general level of support for hypotheses is further accentuated by the use of both paired cases studies and comparisons from the entire dataset to assess the empirical support for hypotheses. So while comparative cross-regional analysis is especially appropriate for new democracies, in the future it also may open up new avenues of research for established democracies as well, and especially in multiparty democracies.59

Drawbacks of Comparative Cross-Regional Analysis

As was noted in the beginning of this section, no method of studying the relationship between economic conditions and election results is without its drawbacks, and comparative cross-regional analysis is not distinct in that regard. The purpose of this section, therefore, is to highlight and address three potential drawbacks of the method: the potential for misattributing other effects to economic conditions; concerns over comparing confidence in estimates from datasets of different sizes; and the difficulty in assessing traditional models of retrospective voting using cross-regional approaches.

Correctly Attributing Economic Effects

One of the defining features of the comparative cross-regional analysis approach is that all of the actual statistical analyses are conducted on aggregate-level datasets. As with any aggregate analysis of voting, we cannot identify the precise causal mechanism through which economic conditions affect the individual vote calculus, which in turn then aggregate up to the vote totals that we observe. Indeed, all our statistical analyses can confirm is whether or not there is a connection between patterns of votes for particular parties and patterns of variation in macroeconomic conditions. Without going into too much of a digression into the philosophy of scientific inquiry, this is the reason we need the types of theoretical arguments presented in the previous chapter to predict exactly what types of patterns we should expect to find. With these

59 See, for example, Rattinger 1991, who uses cross-regional analysis to study elections in Germany.
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*a priori* arguments in hand, we can then make the claim that the empirical evidence presented is either consistent with the observable implications of the theoretical arguments or suggests evidence to falsify these theoretical propositions.60

However, the possibility always exists that one’s independent variable of interest – here cross-regional variation in macroeconomic conditions – could indeed be correlated with a true causal variable that is not measured by the model. This is, of course, the dreaded “omitted variable” problem faced by most quantitative analyses in political science.61 What makes the problem particularly worrisome in the context of the relationship between economic conditions and election results at the regional level is that it is possible to produce an historical example that illustrates the problem. Imagine a comparative cross-regional study set up like the one in this book that used data from a number of elections in the United States from 1870 to 1920 to test a hypothesis that predicted that the Republican Party would perform better in areas of the country where economic conditions were better. If, as one might expect, economic condition were worse in the South in the half century following the Civil War, then a cross-regional analysis of the relationship between economic conditions and election results would find strong evidence to support the proposition that the Republican Party was performing better where economic conditions were better, when in fact the true cause of this relationship would be dislike of the Republican Party in the South due to fall out from the Civil War.62

Although it is impossible to ever control for all potential omitted variable bias, I would like to suggest a number of factors that should assuage concerns in this regard for this particular study.

First, as explained in detail earlier in this chapter, I have included a series of demographic control variables in every regression to control for the effect that societal cleavages might have on the distribution of votes. This includes both the types of cleavages associated with the long-standing development of political parties in Western Europe, such as labor patterns, rural-urban splits, and center-periphery relations, and those that have been privileged in discussions of postcommunist politics, such as the popularity of Old Regime parties among older voters and in areas with more industrial

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60 For more, see King, Keohane, and Verba 1994.
61 See, for example, King, Keohane, and Verba 1994, 168–82; Greene 1993, 246–7.
62 I thank Donald Kinder for highlighting this example specifically and this issue more generally.
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development and the appeal of New Regime parties in more urban areas. All of these factors are controlled for in the original statistical analyses, and therefore cannot be the source of omitted variable bias in my results.

In addition to the demographic control variables within individual elections, the presence of multiple countries in the study also serves as an important control for the effect of larger macrohistorical factors. For example, if one were to argue that voting behavior in the postcommunist era were to some extent a function of the history of which empire the country had been a part of, the sample includes countries from the Russian empire (Russia) the Austro-Hungarian empire (Hungary, Slovakia, and the Czech Republic) and parts of multiple empires (Poland). Similarly, if one felt the true story was one of religious orientation, the study includes an Eastern Orthodox Country (Russia), an almost entirely Catholic country (Poland), two majority Catholic countries (Hungary and Slovakia), and a country with a plurality of atheists (Czech Republic).

Another factor besides the presence of both within country and across country control variables is the centrality of the economic transition that was occurring in these countries during the 1990s. As mentioned in the previous chapter, it is difficult to overstate the degree to which economic life changed across all of these countries during the transition period. Economies that were characterized by fixed prices, massive shortages of goods, full employment, and cradle to grave social entitlement programs were replaced, often in exceedingly short periods of time, by economies featuring wide-scale unemployment, serious inflation, and loss of entitlement programs, but also all sorts of new opportunities for employment, travel, and purchasing goods. In short, if anything was going to be the postcommunist version of the Civil War – in other words, a salient focal point around which we might expect political behavior to revolve – the economic transformation would be as good a guess as any. Thus, far from being likely that there might be some underlying causal factor that is independent of economic factors affecting the vote, any such alternative explanation would require a

63 They are also controlled for in the estimates of the effects of the economic shocks; recall that demographic variables are held at their means when estimates of the vote for parties in regions with “bad” and “good” economic conditions are calculated.

64 Indeed, to return to the example of the United States and the Civil War, the percentage of workers employed in agriculture control variable might actually have picked up some of the distinctions between the South and other parts of the country.

65 For information on religious adherents by country, see the CIA World Factbook (http://www.cia.gov/cia/publications/factbook/).
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theoretical argument for why economic factors would not prove equally or more salient in the context of the postcommunist transitional period.

Another factor mitigating against the likelihood that some omitted variable can explain long-standing patterns of support for parties is the de novo status of most of the political parties competing in these elections. With the exception of the Old Regime parties, almost all of the other parties analyzed in this study were formed during the transition era. Moreover, even if some of these parties claimed to trace their lineage back to the precommunist era, the single-party communist era had lasted over forty years in East Central Europe and over seventy years in Russia. So for most political parties, their popular support was developing concurrently with the transition itself. The one major exception here, the existence of Old Regime parties, is built directly into the theoretical argument of the Transitional Identity Model, as it is precisely the fact that we expect voters to have more information on and a certain set of beliefs about these parties that produces the Old Regime hypothesis.

Taking these three points together, in order to have a Civil War–type omitted variable problem in the analysis, we would need the following conditions to hold. Strong support for political parties would have had to have developed almost immediately in the life span of these parties that was independent of the social-demographic control variables included in the study and was independent of the economic developments during the transition as measured by the explanatory variables in the different models, but at the same time was highly correlated with patterns of variation in these same macroeconomic variables.

Again, this is not to say that other factors do not matter, but, rather, that because of the structure of the analysis in this book, the effects that I am reporting are due to economic conditions are unlikely to be serving merely as a proxy for another underlying variable. Moreover, the fact that multiple countries are included in this study again functions as a relevant buffer against this type of problem. Even if all of these conditions did hold in one particular country, it is that much more unlikely that they would be replicated across all of the countries in the study in the same manner.

66 Although for an analysis of town-by-town variation in the transmission of political identities from the pre to postcommunist eras in parts of Hungary, see Wittenberg 2006.

67 The most common suggestion I have heard in this regard concerns the Polish case and the possibility that the legacy of the different imperial empires that ruled different parts of the country before World War I could have affected both economic developments and the political proclivities of citizens differently in different parts of the country (e.g., Jackson,
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**Different Sized Datasets** A second issue of concern with comparative cross-regional analysis stems from the method of comparison of results across elections. As explained in the chapter, my primary quantity of interest for the purpose of comparison in this study is the degree of confidence that each party either performed better or worse in parts of the country where economic conditions were stronger. This measure is in a large part a function of two factors: the size and direction of the effects for each of the independent variables, and our underlying uncertainty in these effects. All things being equal, more data allows us to reduce uncertainty, which means that to a certain extent the quantity of interest is a function of the number of observations in the dataset. In other words – again all things being equal – the parties from countries with a larger number of regions should be more likely to generate estimated first differences with higher confidence levels than parties from countries with a smaller number of regions.\(^{68}\)

As this concern is a mathematical fact, there are two ways of addressing it in a study. The first is an *a priori* question of research design; the second is to *ex post* examine one’s results to see if there seems to be any correlation between the number of observation in each dataset and the results generated for parties in that country.

From an *a priori* perspective, there are two features that need to be considered: the number of observations in each data set and the research design. One could imagine that there would be more serious concerns in this regard if one were comparing results from a dataset with twenty observations with results from a dataset with two thousand observations. For my study, however, the variation in the size of the datasets is not that great. Indeed, fifteen of the twenty elections are from datasets with between

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\(^{68}\) I thank Neil Beck for raising this point.
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forty-nine and seventy-seven observations. The remaining five elections did involve smaller datasets, with two elections (the 1992 and 1994 Slovak parliamentary elections) having thirty-eight observations and three elections (all three Hungarian parliamentary elections) having only twenty observations.

Fortunately, ex-post observations show no relationship between the number of observations in each country and the prevalence of strong confidence in empirical results. Hungary, with the smallest number of observations, did not generate noticeably fewer cases of empirical support for any of the three standard hypotheses, whereas parties from Russia, which had the largest number of observations, actually generated the least empirical support for the Old Regime hypothesis (a point that is discussed in great detail in Chapter 7). Moreover, as is discussed in Chapter 8, there were two elections in the sample that generated especially poor empirical support for all three of the hypotheses. Neither was a Hungarian election; one was a Russian election with seventy-six observations and one a Slovak election with thirty-eight observations. Finally, although all of the regressions with an N < 40 were for parliamentary elections, there is actually more empirical evidence to support all three standard hypotheses in the parliamentary elections than in the presidential elections (as is discussed in detail in Chapters 5–8). So while there certainly is some discrepancy in the size of the datasets, in no way do the overall findings of the book seem to be a function of this variation in size of dataset.

Moreover, the a priori research design of the study mitigates against such a concern, especially for the standard hypotheses. Indeed, the comparative design of the study requires that each standard hypothesis be tested using each different dataset. So even if there were some meaningful “advantage” to returning empirical results with a higher confidence level to be gained from analyses in countries with a larger number of regions, each hypothesis would still have the opportunity to perform exceptionally well in each larger N context and face the same challenge of generating empirical support in each smaller N context. So although a direct comparison of results from Hungary and Russia might be of more concern in this regard, the

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69 This includes all the Polish, Czech, and Russian elections, and the Slovak elections that occurred after the 1996 administrative reorganization.

70 The one exception to this rule is the 1999 Slovak presidential election, which did not have an Old Regime candidate.
assessments of the three standard hypotheses is less likely to be biased in any meaningful way because each hypothesis was tested across all of the different datasets.\(^7\)

Variation in measurement error also can lead to a similar problem. While randomly distributed measurement error does not bias results, it can introduce inefficiency into the analysis (King, Keohane, and Verba 1994, 157–67). If we accept the proposition from the that measurement error might vary by country, then the possibility exists that countries with less measurement error in their statistical measures might be more likely to yield analyses with – all things being equal – greater confidence in the findings of empirical support for the standard economic voting hypotheses. Thus, we have a similar concern as with the number of regions: that some countries might have an unfair advantage over others in producing results with greater confidence in the findings.\(^2\) Fortunately, both the ex-ante research design and ex-post findings discussed here mitigate against this type of factor having unduly influenced the conclusions of the study as well.\(^3\)

The conditional hypotheses, however, are not necessarily insulated from such a concern in this manner, as many of the conditional hypotheses do not include at least one election from each country in each category. As specified in this study, though, there is no example of conditional hypothesis that groups the low N countries (Hungary, pre-1996 Slovakia) in one category and the high N countries (Czech Republic, Russia, post-1996 Slovakia) in another category. In fact, almost all of the categories across all of the conditional hypotheses include cases from multiple countries; the only exception from the theoretically motivated conditional hypotheses is the institutional responsibility hypothesis, which includes only Polish elections in one category. But that being said, Poland, with forty-nine regions, is not at either extreme in terms of the number of regions.

Although it should be noted that if we are most suspicious of measurement error in Russia, then this concern yields the opposite prediction from the number of regions concern. Measurement error might lead us to anticipate the least confidence in the Russian analyses, but the fact that Russia (along with the Czech Republic) has the largest number of regions in the study would lead us to expect comparatively more confidence in the Russian results.

The one exception to this claim concerns the time-based conditional hypotheses. If we think that measurement error was in general decreasing across all five countries as time passed – which is an intuitively plausible claim – then there is no way to reject the claim that variation in measurement error could be accountable for findings of more support for the standard economic voting hypotheses later in the decade. As was noted in the previous chapter, there are also theoretical reasons why we might expect this to be the case, but we cannot rule out the methodological explanations either. Accordingly, I return to this point in Chapter 8 when assessing the overall support for the time-based conditional hypotheses. Moreover, it should be noted that this does not affect our assessment of the standard economic voting hypotheses, as these are tested using elections from all three periods.
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It is also important to note that this issue of comparing results from datasets of different sizes or with different levels of measurement error is not in any way a function of the decision to use simulated economic shocks (first differences) to generate the quantity of interest; any attempt to compare the statistical significance of individual coefficients would be subject to the same criticism. 74 This does, however, raise the question of why I chose to focus on the confidence that the estimated effect of economic conditions is either positive or negative as opposed to the magnitude of the predicted effect (e.g., the size of the mean or median predicted effect from the simulated distribution).

The simplest answer to the question is that the confidence that the effect of the economy is either positive or negative is the most appropriate measure to test the standard hypotheses, which predict exactly that: the types of parties that ought to do better where economic conditions are better or worse. By focusing on our confidence that the party either did do better or did do worse, I am able to present evidence that precisely speaks to the hypothesis.

However, there is also a large methodological concern in comparing the predicted size of the first difference across different parties because it is highly correlated with the size of the overall national vote received by the party. Indeed, across the eighty-eight incumbent, New Regime, and Old Regime parties, the correlation between the absolute value of the mean predicted effect for economic conditions on parties (e.g., a predicted improvement of 5.1% in the “good” economic region as opposed to the “bad” economic region, as was the case for the ODS in the illustrative example) and the proportion of the vote they received nationally in the election (e.g., 29.62% of the vote for the ODS, see Table 3.1) is +0.46. Therefore, any analysis comparing the size of the predicted economic effect across parties would first have to account for the difference in the overall percentage of the vote received by the different parties. This is a step that is not necessary when comparing confidence levels, as there is almost no relationship between the degree of confidence that we have that a party either performs better or worse following the economic shock and the percentage of the vote received nationally in the election (correlation = +0.05). 75 However,

74 Nor would pooling the data across countries solve the problem, as this would still generate results on the basis of an unequal number of observations from each country.

75 Confidence here is calculated as |positive simulations - 50|, (where positive simulations = the percentage of simulated effects that are greater than zero) to take account of the fact that we can have high certainty in either a positive or negative direction.
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there is a strong correlation between the degree of confidence we have that the party has performed better or worse following the economic shock and the size of the predicted effect (correlation = +0.52). This makes sense, because as our level of confidence that an effect is either positive or negative approaches its lowest level (50 percent of positive simulations), it becomes more and more likely that the distribution straddles 0, and thus that the absolute value of the mean predicted effect also will be close to 0. So one way to think about the size of the predicted effect for economic conditions is that it is a function of both the overall popularity of the party and our confidence that the party is indeed performing better or worse where economic conditions are better. In this sense, by just focusing on the confidence level, which is what I do in this book, I able to tap into the size of the economic effect (recall the +0.52 correlation) but only after having stripped out the component of the measure that is a result of the overall popularity of the party. And this is exactly what is needed to assess the degree of empirical support for the standard hypotheses: a measure of the effect of the economy on election results that is independent of the overall popularity of the party.

Appropriateness for Measuring Traditional Retrospective Voting

Although this is a study of the relationship between regional economic conditions and election results in postcommunist countries, I have attempted as much as possible to link the work to the larger literature on the relationship between economic conditions and election results. As noted in the preceding chapters, this literature has been dominated by work on elections in established democracies, has relied almost entirely on national level election results as the unit of analysis, and for the most part has focused primarily on the question of the relationship between economic conditions and the vote for the incumbent party. The theoretical framework adopted to motivate this empirical question in the existing literature has largely been one of retrospective voting: did voters react to economic conditions by punishing or rewarding incumbents for how the economy had performed on their watch? My study includes a hypothesis with a similar empirical prediction, the Incumbency hypothesis, which predicts that incumbent parties should perform better in areas of the country where economic conditions are stronger than in areas of the country where economic conditions are worse. I did not, however, use a retrospective voting model to motivate this hypothesis; instead, I used the information-based model presented in the previous chapter that relied on predictions about how concentrations of people who
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were more or less satisfied with the state of the economy at the time of the election might be likely to behave. So while the hypothesis is very similar to the standard one found in the literature, its theoretical implications are different.

This highlights a final shortcoming of the comparative cross-regional approach, which is that it is not particularly well suited for assessing theories of retrospective voting, especially in a transitional context. The primary reason for this conclusion is because it is unclear whether we ought to expect residents of regions where economic conditions have changed more in the period of time since the previous election to be more upset or happy with the current government than residents of regions where economic conditions have remained at a more extreme level throughout the period. Consider as an example unemployment in two regions. In region A, unemployment rises from 2% to 4% between two elections. In region B, unemployment remains at 15% between both elections. By any retrospective measure, unemployment has gotten worse in region A while staying the same in Region B. Therefore, any analysis that attempts to build changes in economic conditions under the watch of the current government into its model would end up predicting that citizens of region A ought to “punish” the incumbent government more than citizens of region B.\(^{76}\) And as retrospective voting models are used to assess whether citizens are holding the government accountable for their behavior, any finding that citizens in region B had “punished” the government more than citizens in region A would need to be interpreted as evidence that voters were not holding the government accountable for economic behavior, when in reality it is easy to imagine citizens in region A being thrilled that the unemployment rate in their region – even though it had doubled – was still incredibly low whereas citizens in region B would be furious that after X number of years in office, the government had done nothing to lower unemployment in their region.

As a result, it is difficult to conclude anything other than that cross-regional data are not particularly suitable for assessing classic arguments about whether or not voters use elections to hold the government

\(^{76}\) Punishment here could either be interpreted as citizens of region A giving a lower proportion of the vote to the incumbent government than citizens of region B or as decreasing the proportion of the vote given in region A compared to the previous election by a larger amount than in region B; either way, the points that follow about the difficulty in interpreting this result hold.
accountable for economic developments under its watch. But this is not to say that one can not test whether or not incumbent parties perform better in parts of the country where economic conditions are better, but only that it is important to think about this in terms of the effects of having more people in certain areas that are likely to be satisfied with the state of economy generally than in other areas, as I have done in the previous chapter. Moreover, this is not to say that such a test does not have a retrospective feel to it, but only that it is crucial to realize that, to the extent that I test a model of accountability, it is the degree to which voters hold incumbents accountable for the state of the economy at the time of the election, as opposed to in terms of how it has changed since the previous election.

Finally, I want to conclude by addressing the question of why I have not chosen to include control variables for the previous vote in my regression equations, as is often done in cross-national time-series analyses. The simplest answer to the question is that the hypotheses presented in the previous chapter do not make predictions about the change in vote from previous elections, but instead make predictions about where in the country parties are expected to enjoy more or less electoral support. Therefore, to include a control variable for vote in the previous election would mean that the analysis was not directly testing the hypotheses.

But there are also other logistical, theoretical, and methodological concerns with including controls for the previous vote in the analysis. Logistically, the presence of so many parties that appear and disappear and move in and out of different electoral coalitions between different elections raises the very serious question of how one would in fact determine the correct vote for the “previous” vote total. For starters, one would almost certainly need to eliminate the 1991 Russian presidential election, the 1990 Hungarian

77 There is one potential exception to this claim, although it involves a reinterpretation of the traditional definition of incumbency; this point is discussed in detail in Chapter 9.
78 The question of where microlevel studies of economic voting fall in this regard is an interesting one. Although such studies are usually designed to test a traditional retrospective approach, the question most often used in actual statistical analyses – especially in the American context – is some variant on the degree to which the respondent thinks either the country’s or their own economic situation has changed over the past year, as opposed to since the previous election. Moreover, it would be interesting to know the extent to which answers to this type of question reflect an assessment of the respondent’s view of the economy (or her own economic situation) at the time of the survey, or an assessment of how the economy (or her own situation) has changed under the entire period of rule of the current government. To the extent that it is the former, the similarities with cross-regional analysis may be stronger than at first glance.
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parliamentary election, and the 1993 Russian parliamentary election from the study because of a lack of any appropriate “previous” vote total. Similarly, the 1992 Slovak, 1991 Polish, and 1992 Czech parliamentary elections would face the challenge of the appropriate “previous” vote for umbrella movements that had subsequently disintegrated. The 1992 and 1994 Slovak elections and the 1998 Czech elections would need to face the question of controlling for the “previous” vote for new parties that were formed when they split from existing parties against which they were then explicitly competing in the new election. The 1997 Polish parliamentary election would raise the question of which parties to control for when newly merged and newly formed electoral coalitions play a prominent role in the election. The 1995 and 1999 Russian parliamentary elections both featured prominent parties that had not participated in the previous election. Moreover, none of these points even touch on the question of whether presidential elections can reference parliamentary elections for “previous” vote totals or vice versa. Thus, regardless of the theoretical desirability of testing hypotheses that include controls for previous vote totals as part of the analysis, the reality of doing so in the postcommunist context would most likely result in (1) the dropping of numerous elections from the analysis and (2) the introduction of a tremendous number of ad hoc coding rules to figure out how to do this in each individual case. The first point would lead to the arbitrary discarding of important and useful data, whereas the second point would have a severe impact on the robustness of results, as it is impossible to imagine a set of coding decisions that could not be exchanged for multiple other plausible ways of coding the “previous” vote.79

Moreover, it is far from certain that it would be desirable to test cross-regional models by controlling for the previous vote for theoretical reasons. The decision to control for previous vote totals in these types of analyses is based on the presence of three assumptions: that voters have partisan loyalty, that this partisan loyalty affects their voting calculus, and that the prior vote totals received by the party represent a good proxy for estimating this unobservable regional party loyalty. The final assumption is based on the idea that the previous vote total represents a draw from a distribution of all possible prior vote totals, and that the draw itself is a good approximation

79 Not surprisingly, the precedent in cross-regional studies of the effect of economic conditions on election results in postcommunist countries has been to do exactly what I have done in this book, which is to focus on the distribution of votes in the current election as opposed to the change in the distribution of votes since the previous election, although see Fidrmuc 2000a and Pacek 1994.
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of that distribution. This is likely to be the case only if the distribution is normally distributed with a small level of variance. Even within stable democracies, scholars often doubt whether one single election represents a suitable proxy for this distribution, and instead they use a running average from multiple previous elections.\footnote{Erikson 1990. Of course, given the difficulties of correctly identifying even an appropriate single “previous” vote total in the postcommunist context, trying to use a running tally of this nature with the data currently available is practically impossible.} In transition countries, there is no reason to assume that any of these propositions are true. We do not know the degree to which partisan loyalty is present from the start in transition; and the idea that there could be full partisan loyalty of the Western variety seems unlikely given the proliferation of new parties, especially in the earlier elections. In most cases, there are at best one or two elections on which to base a “proxy” measurement of this loyalty, and these are elections that have occurred under rather extraordinary circumstances. In many cases – as has been discussed earlier – there is not even a single appropriate prior election. Moreover, given the nature of the postcommunist transitional experience, it is very likely that the state of the economy had an important effect on the vote for whatever party is being analyzed in the previous election. Thus, it would seem theoretically problematic to claim that this measure captures party loyalty that is somehow independent of concern over economic conditions.

Additionally, at least one recent paper raises the point that there may be methodological concerns with including lagged dependent variables in time-series analyses – especially when there are theoretical reasons to suspect whether the value of the dependent variable in the previous period has actually had a causal effect on the value of the dependent variable in the current period – noting that doing so can result in statistically significant coefficients appearing in regression results with signs in the wrong direction.\footnote{Achen 2000. The author uses budget expenditures as an example of a case in which the dependent variable may look similar from year to year, but which in no way implies that legislators simply adopt the same figure as the previous year because of inertia. Instead, it may be that the causal factors that actually determine budget allocations themselves do not change much over time. In this case, including a lagged version of the dependent variable may simply obscure the real causal variables, and, as noted above, the results can even be more pernicious. This seems a useful analogy to election results in the postcommunist context, whereby economic conditions may have had a very large impact on why a party performed better in particular areas of the country in an election at time $T$, and then continued to have a similar effect in time $T + 1$. Such a pattern does not mean that voters in the region have simply become blind followers of that party oblivious to...}
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Taken together, controlling for the previous vote in the types of regressions included in this study is logistically problematic, theoretically questionable, and may be methodologically suspect. And, most importantly from the point of view of this study, it would not result in an appropriate test of the hypotheses presented in the previous chapter.\(^82\)

economic concerns but may instead just reflect the importance of economic factors across both elections.

\(^82\) That is not to say that hypotheses that make predictions about the change in vote since the previous election would not be interesting to consider. Nevertheless, testing such hypotheses in a comparative framework using regional level data from the countries and time periods included in this study would be an inherently problematic task for all of the reasons outlined in the previous paragraphs.