# THE CONCEPT OF A CRITICAL REALIGNMENT, ELECTORAL BEHAVIOR, AND POLITICAL CHANGE

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*The realignment perspective has exerted an enormous amount of influence on thinking about American politics, but recently it has fallen into disfavor. As a theory of political change, this dissatisfaction is warranted. However, in rejecting the realignment perspective, scholars risk losing a valuable concept, the notion of a critical realignment. My thesis is that, properly defined, the concept of a critical realignment can be a powerful tool in the study of electoral behavior and an important component of a broader theory of political change. This thesis derives from an analysis of presidential elections between 1828 and 1984. This analysis provides dramatic evidence for the proposition that critical realignments are important electoral phenomena. The evidence is equally clear, however, that critical realignments are subnational phenomena that vary considerably in form, not the majestic national movements some believed them to be. The analyses reported here reveal broadly based electoral eruptions of 40 to 50 points that endure for decades.*

In his seminal article on critical elections, V. O. Key (1955) attempted to formulate a partial theory of elections. Intrigued by the broad implications of his formulation, scholars quickly transformed it into a "democratic" theory of political change—the realignment perspective on American politics (Clubb, Flanigan, and Zingale 1990; Kleppner 1987). This perspective has exerted an enormous amount of influence on thinking about American politics, for two reasons. First, it offers important caveats to the "pluralist-incrementalist" paradigm (Almond 1988) by offering hope for nonincremental change in a government structured in the late eighteenth century to block the whims of transient majorities. While its complex system of checks and balances prevents the U.S. government from acting hastily during normal times, critical elections—the realignment perspective asserts—mobilize and integrate cumbersome institutional networks during political crises (Brady 1988; Burnham 1970). Second, this perspective resurrects voters from the shadows to which they have been relegated since the beginning of the behavioral tradition. Voters, not elites, are the dynamic component within the realignment perspective. Responding to crises and broad socioeconomic developments, they generate the political force that precipitates the mobilization and integration of policymaking institutions.

Despite the attractiveness of the realignment perspective and its importance to American democratic thought, it recently has fallen into disfavor. Students of critical realignments can agree on little more than that the empirical findings in this area have yielded less than clear-cut results. While such ambiguity is not uncommon in social science research, it is disturbing in an area concerned with seismic changes in electoral behavior. If these seismic changes are to have ramifications throughout the political system, then social scientists analyzing electoral patterns over time should be able to detect them with a high degree of precision and agreement. Carmines and Stimson capture nicely the frustrations of scholars unable to do so:

> If simplicity is often the first casualty of scientific research, that is even more the case in realignment research: the simple and dramatic model of critical realignment, the one that so readily captures our attention, finds no supporting evidence. . . . The closer we look, the more these "simple" realignments become movements over time, taking decades or multiples of decades to achieve their final form. When precursors and aftershocks are added, the multiple decade processes overlap, and the continuing effects of old movements are still manifested while new and different movements toward a still new alignment are underway. There is no "normal" period left, no time when the reshaping of loyalties and behaviors is absent. . . . [Thus] the realignment literature may be seen as a long string of amendments by typology, as successive scholars have applied the concepts to the raw materials of (usually American) political history and found the empirical materials too complex to be subsumed by the initial concept. (1989, 21–23)

Most objective reviewers of the realignment literature would not disagree too strongly with this characterization, which forms the basis for Carmines and Stimson's classification of the concept of a critical realignment as "prescientific." Defying empirical refutation, they argue that the notion of a critical realignment is too resilient to be useful as a scientific concept. Empirical inconsistencies merely become the basis for provisos and refinements, impairing the concept's utility in guiding empirical research.

Carmines and Stimson are correct in underscoring deficiencies in the prevailing views of a critical realignment. But these deficiencies do not necessarily mean that this concept has no theoretical value. Because the concept of a critical realignment is so beguilingly simple, and its potential ramifications so important, realignment scholars moved quickly—too quickly—to the construction of a broad, encompassing theory of political change, the realignment perspective. This developmental path put critical realign-

ments at the center of the study of American political change but also led to a neglect of the concept itself. It has never received the scholarly scrutiny it deserved, nothing that would compare, for example, with the scrutiny given to the concept of party identification. This is unfortunate because the concept of a critical realignment, like that of party identification, is not as straightforward as it appears—as we shall see shortly.

The failure to develop and refine the concept of a critical realignment produced a weak foundation for a theory of political change, causing a great deal of confusion, dissatisfaction, and frustration. However, rather than leading scholars to reexamine the theory's conceptual base, this consternation led to pleas to abandon the idea of a critical realignment altogether, or to limit it severely (Shafer 1991). These pleas, I intend to demonstrate, are ill founded and would lead to the loss of a valuable *concept*, the concept of a critical realignment. My thesis here is that if properly defined, the concept of a critical realignment is a powerful tool in the study of electoral behavior. More importantly, it can be an important component of a broader theory of political change.

To develop this thesis, I first offer a restatement of the concept of a critical realignment and briefly describe a normal vote approach to detecting critical realignments. Next, I examine this reconceptualization by applying the normal vote approach to presidential electoral patterns. The data generated by this analysis reveal if, when, and where critical realignments in presidential voting patterns occurred in the United States since 1828. They also provide measures of the impact of these realignments on prevailing electoral patterns. I close by developing the implications of this restatement for the study of political change.

## CRITICAL REALIGNMENTS: CONCEPTUALIZATION AND METHODOLOGICAL APPROACH

The notion of a critical realignment is an aggregate-level concept that refers to an abrupt, large, and enduring form of change in prevailing electoral patterns, one that is initiated by a critical election and results in a significantly different partisan balance in the electorate. This much is clear from the literature on critical realignments. What is less clear is that in order to understand the role of critical realignments in structuring electoral patterns, scholars must be sensitive to considerations of both time and space. In order to detect and gauge enduring changes in electoral patterns accurately, scholars must examine long time frames and be sensitive to the fact that enduring critical change can assume a variety of forms. Equally important for the detection and measurement of realignments are spatial considerations. It is unrealistic to expect the entire U.S. electorate to respond simultaneously and uniformly to the type of stimuli that will generate a critical realignment in electoral patterns. There is simply too much geopolitical diversity in the United States to justify such an expectation.[1]

These observations lead us to expect considerable diversity in the impact of critical realignments on prevailing electoral patterns over time and space. These expectations can be examined by analyzing results from a study of presidential electoral patterns based on data from every county and most major cities (3,136 counties and cities and 107,974 unit-elections) in the continental United States for every presidential election between 1828 and 1984. The units of analysis in this study are 215 substate regions. These regions are geographically contiguous counties that share similar electoral patterns over time, or they are major cities that manifest distinctive electoral patterns within a state. I report the procedures used to construct these regions elsewhere (Nardulli 1994) and summarize them in the Appendix.

The analyses of electoral patterns I shall report are based on normal vote analyses of electoral change in each of these 215 regions. Thus, before the results of these regional analyses can be presented, a brief overview of the normal vote approach to electoral change will be useful. I provide more details elsewhere (Nardulli 1994) and in the Appendix.

### The Normal Vote Approach to Electoral Change

The normal vote approach I employ here uses interrupted time-series analysis to map the flow of electoral change over time across subnational regions. This technique requires the analyst to specify the point at which major, enduring interruptions in long-term electoral trends (critical elections) are hypothesized to begin, as well as the form of those interruptions. It also tests whether each year hypothesized as a critical election in fact marks the beginning of a statistically significant interruption in electoral patterns. The forecast from a time-series analysis reflects the deterministic component of a time series, which is derived by filtering short-term disturbances (noise) from the raw data. In the case of the electoral time series being analyzed here, these forecasts can be interpreted as normal vote estimates that are useful in depicting prevailing electoral patterns.

These normal vote estimates also can be used, in conjunction with the data on critical elections, to gauge the impact of a critical realignment on prevailing electoral patterns. Although calculating the overall electoral impact of a critical realignment is quite involved (and will be addressed later), measuring its *initial impact* is rather straightforward and nicely illustrates the utility of the normal vote approach in the study of critical realignments. A realignment's initial critical movement is measured by subtracting the value of the normal vote estimate in the year preceding a critical election from its value in the critical election year.[2] I used a margin-of-victory variable (Democratic % of total vote − Republican % of total vote) to conduct the interrupted time-series

analyses. Thus the normal vote estimates are expressed in terms of the margin of victory normally enjoyed by a party in a region. A value of .10 would indicate a 10-percentage-point cushion normally enjoyed by the Democrats, while a value of −.10 would suggest the same for the Republicans. If the value of the normal vote estimate for a region was −.10 in 1928 but jumped to .30 after a critical election in 1932, the magnitude of the initial impact would be .40, in favor of the Democrats. Correspondingly, a movement from −.05 to −.25 after the critical election of 1896 would suggest an initial impact of −.20, in favor of the Republicans.

One form of realigning change that will not be detected by this approach is a coalitional shift among important electoral subgroups that does not result in a significant change in the partisan balance of the regional electorates analyzed here. Consider, for example, a situation in which industrial workers shifted in large numbers to the Republicans while farmers shifted to the Democrats in equally large numbers, resulting in a very different coalitional structure within each party. *If the shift occurred at the same election and if the magnitude of the shift within each of the 215 electoral regions was roughly equal,* then this important change would not be picked up by the normal vote approach developed here. If, however, the geographic distribution of these groups were different and changes in regional partisan balances occurred, then the coalition shift would be captured by this approach—even if the overall shift at the national level is equivalent and no overall shift in the national partisan balance is generated. The change would be undetected only in regions where the coalitional shift produced no change in a region's partisan balance.

In sum, for realigning change that produces a shift in the partisan balance within regional electorates, this approach permits a precise specification of *whether* a critical realignment occurred as well as *when* it occurred. Because this approach was implemented at the subnational level, it also provides a precise specification of *where* the interruptions occurred. Finally, the normal vote approach permits us to gauge the *impact* of these interruptions on prevailing electoral patterns. We can now examine the results from the normal vote approach to illustrate each of these points.

## THE INCIDENCE OF CRITICAL ELECTIONS OVER TIME

If, as some critics claim, the concept of a critical realignment lacks an empirical referent, the normal vote approach just described would yield largely null results. Null results would obtain if (1) there are no marked and enduring breaks in prevailing electoral patterns or (2) there are no prevailing electoral patterns (i.e., vote outcomes are determined largely by short-term forces resulting in highly volatile time series). As suggested earlier, however, these null results do not obtain. Most regions with vote returns for the entire 156-year period experienced three to four critical realignments; a few had as many as five, while some had only one.

The mere rejection of the null hypotheses in the 215 analyses conducted here is not very telling for present purposes. It is insufficient to argue that critical realignments are important political phenomena merely because they are prominent in regional electoral patterns. Unpatterned interruptions detected at the substate level, no matter how large, could be little more than random eruptions. To be truly consequential political phenomena, the regionally detected realignments uncovered here must have some temporal ordering when viewed in the aggregate. That is, the critical elections detected here should cluster around a handful of elections. If they did not, it would be difficult to argue that critical realignments reflected a widespread electoral response to important political stimuli.

We can determine whether this temporal ordering exists simply by examining the incidence of critical elections by year. If the realignments identified here are merely random eruptions, then no elections would stand out as prominent electoral watersheds. Figure 1 reports the weighted proportion (for the nation as a whole) of all regions that registered a critical election, by year and party.[3] It reveals, for example, that over 40% of the national electorate lived in regions registering a critical election in 1856. Most of these shifts favored the newly formed Republican party. In 1932, over half of the national electorate lived in regions that registered a critical election; all of these shifts favored the Democrats. No critical realignments at all were initiated in 1844, 1908, 1956, and so on.

Figure 1 provides strong evidence that the regional realignments identified here have a discernible temporal structure. Most of the critical elections that occur (88%) fall in or around six elections—1836, 1856, 1896, 1932, 1948, and 1960—or in the South between 1876 and 1904.

The realignment eras that emerge from this analysis of critical elections can be categorized readily. I label the first realignment the Whig realignment, as it roughly coincides with the emergence of the Whigs on the national scene.[4] A minor set of Democratic antebellum critical elections began in 1852 and continued through 1856.[5] The Republican antebellum realignment began in 1856. The period between 1864 and 1904 looks unstructured. But most of the critical elections during that era occurred in the South and are part of what I will refer to as the Jim Crow realignment.[6] The pro-Democratic critical elections of 1896 began the Democratic populist realignment, while the pro-Republican critical elections of 1896–1900 started the Republican industrial realignment. I will refer to the 1928-1932-1936 Democratic realignments as the New Deal realignment, the 1948-1952 Republican sequence as the post–World War II realignment, and the 1960 Democratic surge as the New

Frontier realignment. These labels will be useful in the graphic presentations that follow.

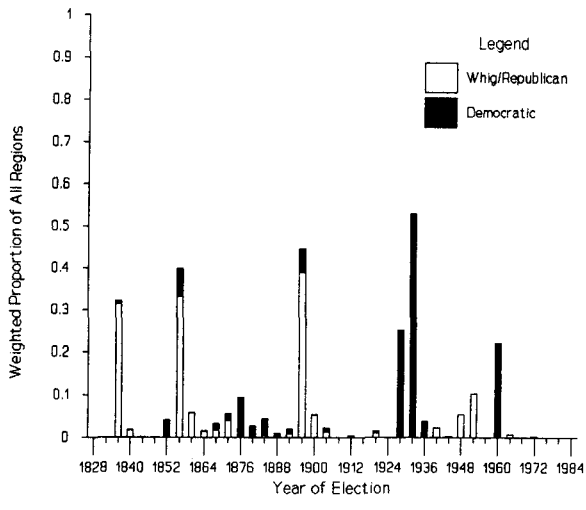
## THE DISTRIBUTION OF CRITICAL REALIGNMENTS OVER SPACE

The data in Figure 1 strongly support the utility of a subnational approach to understanding critical realignments. There has never been a truly national realignment. Indeed, with the exception of 1932, no critical election involved as much as half the nation. Using national-level data to detect critical realignments would have led to misleading results about both the incidence and the electoral impact of critical realignments in U.S. electoral history. However, contending that critical realignments are subnational phenomena requires more than establishing that they are not national phenomena. Just as we expected some temporal order to the incidence of critical realignments, there should be some spatial order.

The existence of this spatial ordering can be assessed simply by examining maps of the regions that realigned during the various eras identified earlier. Figure 2 reports those regions and provides strong empirical evidence for the type of spatial ordering we expect. Consider the Whig realignment (map a). It shows that most of the South and much of the West realigned in the period between 1836 and 1840. The only part of the Northeast that realigned was western Pennsylvania. Map b reveals a similarly clear but very different pattern. The Republican antebellum realignment at the presidential level was heavily concentrated in New England, upstate New York, and the upper West. Wisconsin is not really an exception here because its time series begins in 1856; while it voted strongly Republican in 1856 it had no base from which to realign. Interestingly, there are a smattering of Missouri and Southern regions (northern Alabama, central North Carolina, and northwest Arkansas) that register a Republican realignment, but very few in the central part of the country (southern Indiana, Ohio, Pennsylvania, etc.).

FIGURE 1

**Distribution of Critical Elections, 1828–1984**




Because the Democratic antebellum realignment affected so few regions (see Figure 1), it is not displayed here. The more broadly based and enduring shift in Southern voting patterns generated by the events that precipitated the Civil War occurred during the Jim Crow era,[7] and its distribution is displayed in map c. Almost the entire South (east of Texas) experienced critical shifts during this era. The primary exceptions are some regions in the deep South that experienced a Democratic realignment in the antebellum period.

The Democratic populist realignment was both short-lived and narrowly based and it too is not displayed here. It was confined to three contiguous states: Kansas, Nebraska, and Colorado, plus northern Washington state. In contrast, the Republican industrial realignment (map d) was concentrated in the Northeast. There were a smattering of realignments in the South and the border states, as well as on the West Coast (northern California and the Seattle metropolitan area). This realignment looks very similar, in spatial terms, to the Republican antebellum realignment (map b).

The New Deal realignment (map e) is unmatched in its geographic scope. Most of the nation outside the deep South realigned. The exceptions here are most of Virginia and Ohio, upstate New York and southern Indiana, as well as parts of the plains states (most of the two Dakotas and Nebraska), and some regions in southern Missouri and Arkansas. Although the New Deal movement is in a class by itself, it cannot be interpreted properly without examining it in light of the Republican post–World War II realignment. If map f is compared with map e, it can be seen that one is almost the mirror image of the other. Most of the deep South realigned after World War II, along with some of the regions in Indiana, Ohio, Virginia, Missouri, Arkansas, and Nebraska that did not realign in the New Deal era.

I have not presented a map for the New Frontier realignment because, with some exceptions,[8] the regions that realign in 1960 are major urban areas: New York, Philadelphia, Boston, Chicago, Detroit, Cleveland, Indianapolis, Kansas City, Louisville, Baltimore, and Seattle. But while these cities are barely visible on a map of the United States, they constituted nearly 20% of the electorate in 1960, making this an important shift in American electoral history. Moreover, this realignment is important for reasons that go beyond its numerical scope. It is undoubtedly part of a political dynamic that began in 1928. The New Deal realignment broke the South's grip on the Democratic party, leading it to move in policy directions that led to the South's estrangement in 1948 and 1952 (see map f). The subsequent shoring up of Democratic strength in 1960 among urban constituencies is probably a consequence of the Kennedy

candidacy and the party's reorientation that Carmines and Stimson (1989) document so nicely.

## THE IMPACT OF CRITICAL REALIGNMENTS ON PREVAILING ELECTORAL PATTERNS

Figure 2 illustrates the diverse impact of political stimuli across different regions of the United States. We expect the same type of diverse impact over time. When viewed throughout U.S. history, it is clear that the critical realignments that have occurred have been generated by a different mix of political, moral, cultural, and economic forces. These different mixes cannot be expected to generate similar patterns of change over time. Nor can we expect the patterns generated within a realignment era to be uniform across space. These differences will be reflected in the magnitude of a realignment's initial critical movement, the course of the realignment after its initial movement, and its duration. These three factors combine to give a critical realignment's impact on electoral patterns a distinctive contour. The shape of these contours, along with differences in geographic scope (see Figure 2), means that some of these quintessentially democratic eruptions will register much higher on the electoral Richter scale than others. These differences must be considered when thinking about the political and policy consequences of the realignment eras identified earlier. To ignore the magnitude of the shock to the electoral status quo would be to overlook what must be a key factor in any attempt to understand the impact of popular sentiment upon political processes, especially for those interested in whether that impact has changed over time.

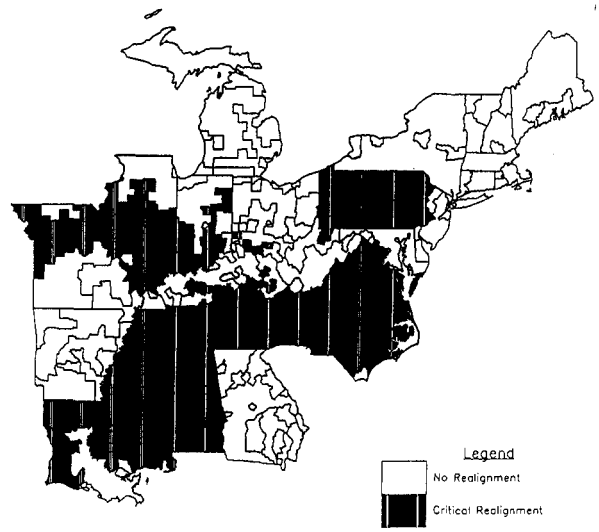
### Measuring Electoral Impact

These considerations make it essential to capture the impact of critical realignments on prevailing electoral patterns. This requires a set of electoral impact measures and a multistage process to develop them. This process must begin with a *regional impact measure*. Such a measure must capture the cumulative electoral change generated by a realignment, beginning with the critical election that initiates it and ending with the point at which the realignment's force is spent (i.e., when it is interrupted by a subsequent realignment). Also important is an *aggregated regional impact measure*—a summary measure that aggregates the electoral impact of regional critical movements for an entire realignment era. Finally, a *national impact measure* is needed, one that captures the effect of a realignment era on national vote patterns. This measure must take into account the proportion of the national vote that was involved in the realigning regions.

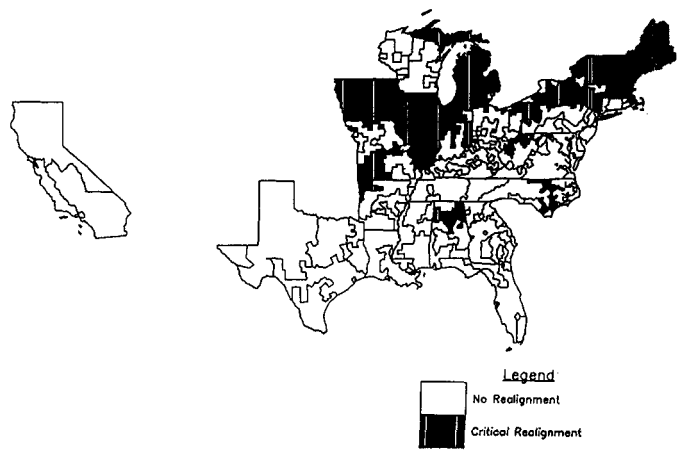
Capturing the cumulative changes from the electoral status quo requires that some attention be given

FIGURE 2

**a. Whig Critical Realignments, 1836–40**




**b. Antebellum Republican Critical Realignments, 1856–60**




**c. Jim Crow Critical Realignments, 1876–1904**

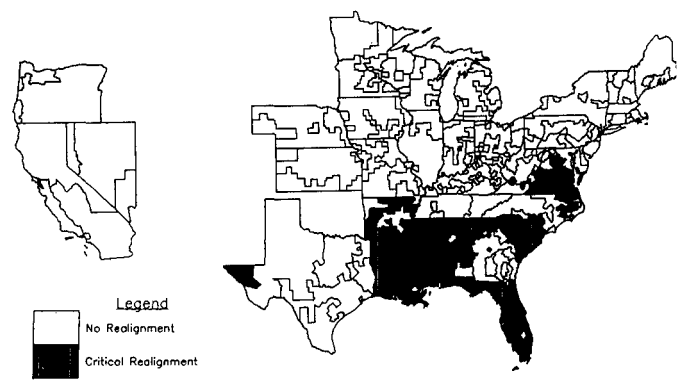
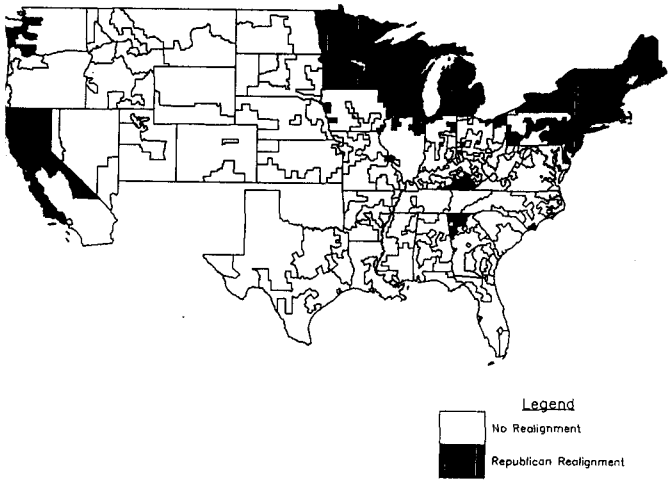
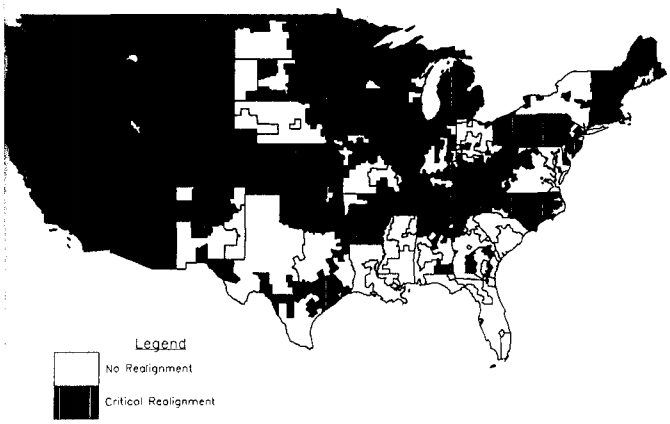


FIGURE 2 continued

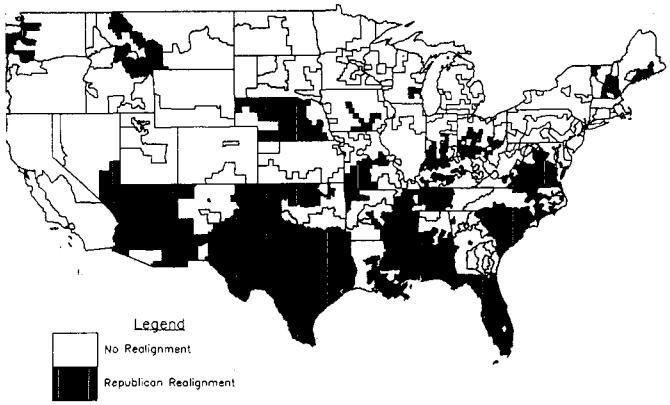
**d. Republican Industrial Critical Realignments, 1896–1904**




**e. New Deal Democratic Critical Realignments, 1928–36**




**f. Post–World War II Republican Critical Realignments, 1948–52**




to the various forms that critical realignments can assume. Although Carmines and Stimson (1989, 139–45) introduce four models of temporal change (the impulse-decay model, the step model, the dynamic growth model, the secular model) that emerge from the literature on interrupted time-series analysis (Box and Tiao 1975; McCleary and Hay 1980), they associate only the step model with a critical realignment. While this is consistent with most work in the field since Key, he had a much broader conception of a critical realignment.

The literature on time-series analysis was not developed when Key did his work, but he clearly understood that some decay of the initial critical movement occurred in at least some locales (Key 1955, 6–8). Indeed, this is almost a defining characteristic of the New Deal realignment, which served as his model. This decay was not troubling to Key because of the scale upon which these changes were taking place. An initial shift of 40 points that deteriorates by half over a twenty-year period is best fit by an impulse-decay model, in the parlance of interrupted time-series analysts. But to political observers such as Key, such a disruption in the electoral status quo is a major political happening! Had Key examined the Republican industrial realignment he would have noticed a different form of critical change, one that began with a 20-point shift but then grew over the course of several decades. A dynamic growth model would be required to capture such a change accurately.

Given these historical realities, it would be shortsighted to exclude the impulse-decay and the dynamic growth models as variants of critical electoral change.[9] Along with the step model, they fit comfortably within the definition of a critical realignment. Each model refers to an abrupt, enduring change in electoral patterns. The course of change after the initial critical movement is simply different for each.[10]

These differences are important to note because they have important implications for measuring regional electoral impact. Regional impact cannot be computed simply by combining the size of the initial critical movement (e.g., 40 points) with the realignment's duration (e.g., 28 years). Rather, the procedures used to measure the regional impact variable must be sensitive to the fact that the realignment's force may pick up momentum in subsequent elections or lose steam. This was done by computing the difference between the status quo ante (the normal vote estimate in the year before the critical election that initiated the realignment) and each of the equilibrium points that reflect the new status quo (the normal vote estimates for every election during which the realignment endured). The regional impact measure for a region was defined only for the duration of the realignment in that region.

A hypothetical example will illustrate how the *regional impact* variable was constructed. Consider a region that experienced a critical election in 1896 as part of the Republican industrial realignment. Assume that the realignment initiated by this critical

election endured in this region until it was interrupted by the New Deal realignment in 1932. The normal vote estimate for the 1892 election is .05, suggesting that it was a marginally Democratic region. Due to the events that precipitated the critical election, the normal vote estimate moved to −.15 in 1896, making it safely Republican. If that realignment was captured with a dynamic growth model, then the distribution of values for the regional impact variable could be, for example, −.20 in 1896, −.25 in 1900, −.30 in 1904, and so on. An impulse-decay model would produce the opposite type of distribution—a large initial value followed by decaying values. A step model would produce constant change throughout the 1896–1928 period. The regional impact variable for the Republican industrial realignment in this region would be defined only between 1896 and 1928.

The *aggregated regional impact* measure was calculated for each realignment era by computing a weighted average of the regional impact measures, using only regions that experienced a realignment during that era. These regional impact measures had to be weighted in order to compute the aggregated impact measure because of differences in vote totals across realigning regions. The time frame for the aggregated regional impact measure begins with the earliest critical election in the realignment era and ends when the last of the realigning regions experiences a subsequent realignment. A region's contribution to the aggregated impact measure was considered 0 if it experienced a subsequent realignment before the realignment era in question had run its course. Thus the aggregated impact measure can evidence a major decay if half of its constituent regions experience a subsequent realignment while the others do not.

The *national impact* measure was computed by weighting the aggregated regional impact measure by the proportion of the nation's vote that realigned. Thus, if the aggregated regional impact measure for a year is .40 and the realigning regions constitute half the nation's electorate, the national impact measure's value would be .20.

### Electoral Impacts and Realignment Eras

The aggregated regional and national impact measures for most of the realignment eras identified earlier are displayed in Figure 3 a–g. These graphs depict the type of sharp, enduring changes in presidential electoral patterns that have captured the imagination of scholars for four decades. There is little ambiguity as to when or if a realignment begins. Moreover, the interruptions involved in critical realignments are major political happenings in the locales where they occur. While the average short-term fluctuation around a regional normal vote line is about 10 percentage points (Nardulli 1994), critical realignments result in changes that can be four to five times as great and endure for long periods of time, even if at reduced levels.

Consider the Whig realignment. It begins with an initial shift of over 55 points. This impact deteriorates gradually due to secular changes in some regions and then moves abruptly to a lower plateau in 1856, when many of the regions that realigned in 1836 realign again. The same thing happens in 1896. While the Whig realignment loses much of its influence at the regional level after 1896, many of its constituent regions do not realign again until 1932. At the national level the Whig realignment had a major impact until 1856; after 1896 its national impact was almost nonexistent.

The Republican antebellum realignment (Figure 3b) was not as sharp or as enduring as the Whig realignment and had a relatively minor impact on national electoral patterns—although not on the political life of the nation! The Democratic antebellum realignment (not displayed here) represented a sharper break than the Republican movement, but it decayed rapidly. Its electoral impact at the national level was almost undetectable. The effect of the Jim Crow realignment, reported in Figure 3c was far more enduring. It shaped voting patterns in the South for almost three-quarters of a century and forged the "solid South." In contrast to the other realignments, the Jim Crow realignment approximates the conventional, steplike, critical change so prevalent in thinking about critical realignments. While the impact of the Jim Crow realignment on national vote patterns was not great, its real national impact was felt in the electoral college. Thus its real significance is understated in Figure 3c.

The Democratic populist realignment (not displayed here) revealed a sharp break from prevailing normal vote patterns, but it decayed quickly. It never had much of a national impact because of its narrow geographic scope. The Republican industrial realignment (Figure 3d), on the other hand, began with a relatively modest 20-point shift. Its greater scope, however, translated into a significant national impact. The national impact of this interruption was all the more significant because it broke a deadlock that had gripped presidential politics since the end of Reconstruction. The industrial realignment also is important because it reveals a distinctive form. While relatively short-lived (its effects are essentially eliminated with the onset of the New Deal realignment), its impact actually picks up momentum after 1896, because of secular change. Thus it reflects a dynamic growth pattern.

The New Deal realignment (Figure 3e) also picks up some momentum after a sharp initial break. Overall, however, the New Deal era generated an electoral impact that deteriorated rather steadily after its peak. At the same time, however, its effects are felt even today. Because its geographic scope is virtually coterminal with most of the nation outside of the South, the New Deal realignment's national impact is almost equivalent to its aggregated regional impact.

Viewed more broadly, the New Deal's impact on normal vote patterns should take into account its likely role in precipitating the post–World War II realignment in the South (Figure 3f), as well as the

New Frontier realignment (Figure 3g) that followed. The initial magnitude of the post–World War II realignment was quite large, over 40 points. This movement, like the industrial realignment of 1896, evidences a dynamic growth pattern, picking up strength over the course of three decades. While its national impact upon vote patterns does not compare to the New Deal's, its real effect is felt in the electoral college, as recent presidential elections have shown. The New Frontier realignment does not reveal nearly as marked a shift as the earlier Republican movement (less than 30 points). Moreover, it deteriorates almost as soon as it peaks.

## CRITICAL REALIGNMENTS AND THE STUDY OF AMERICAN POLITICAL CHANGE

The data presented in Figures 1–3 provide fairly conclusive evidence that pleas to abandon the concept of a critical realignment are ill founded. As developed and operationalized here, critical realignments are temporally structured and geographically concentrated phenomena that represent marked and enduring breaks in regional electoral patterns. At the same time, the regional realignments identified here are not the majestic national movements that many believed them to be. Moreover, as an electoral *concept*, the notion of a critical realignment does not have the sweep or appeal of the realignment *perspective* on American political change.

Does this mean that the reconceptualization proposed here has trivialized the concept? A brief review of the data presented here would suggest the contrary. The restatement offered here has simply provided a more defensible view of an extremely important electoral phenomenon. In doing so, this reconceptualization provides a much stronger foundation for a more encompassing theory of American political change. This is important because, while critics of the realignment perspective have generated important insights into the complexities of political change in the American context (Carmines and Stimson 1989; Shafer 1991), a satisfactory alternative to the realignment perspective has not emerged.

Two of the most important insights generated by these critics concern the catalytic agents that spark political change and the receptors of the transmissions that flow from disturbances created by catalytic agents. Succinctly stated, these critics contend that more catalytic agents are involved in political change than voters and that American government has become almost impenetrable by external agents such as voters. Taken on their face, these criticisms would suggest that even a resurrected conception of a critical realignment would contribute little to a broader theory of political change. This extreme interpretation notwithstanding, it is worthwhile to integrate what has been learned about critical realignments here with some of these insights concerning catalysts and receptors. Although a comprehensive theory of political change will require the development of a far more elaborate set of concepts than these two, this exercise can shed some light on the broad outlines of such a theory. It also can lead to a better understanding of the role of critical realignments in political change.

### Catalytic Agents

A comprehensive theory of political change would have to incorporate a *variety* of catalytic agents. It also would have to recognize that a *hierarchy* exists among these catalysts, as well as a certain amount of *specialization*. Finally, this theory would have to recognize differences in the *contours* of the disturbances generated by these agents over time and across incidents. If a theory of political change failed to recognize these points, it would be unable to disentangle the shifting effects of the multiple forces involved in American political development.

Consider the agents of change. A disturbance can be generated by government elites (as in the neoinstitutionalist model), by social and economic leaders (as in the power elite model), by party activists (as in the issue evolution model), by organized interest groups (as in the pluralistic model), or by mass publics (as in the traditional realignment model). It cannot be expected that each will respond similarly to the same stimuli; some catalysts are more likely to be activated by certain types of issues or incidents than others. For example, party elites will be less likely to act on issues that would divide their core constituencies, or alienate important swing groups. Interest groups may be passive on important issues or developments deemed irrelevant to their raison de être.

We cannot expect that, once activated, the catalytic agents will carry the same force. At a given point in time, some are simply more powerful than others. Moreover, the hierarchy of catalytic agents has changed considerably over time. For example, in what might be called the deferential period in American politics (roughly the pre-Jacksonian period), formal governmental elites and the gentry undoubtedly were the primary movers (Heale 1977; Shallope 1990). What they felt and did mattered far more than what the masses felt or did, and there were no well established party elites or organized interest groups. Party elites undoubtedly enjoyed the upper hand during the latter part of the nineteenth century (Silbey 1991). As parties declined, party elites lost ground to better organized interest groups, more highly institutionalized governmental officials, and a more independent electorate. Currently national political parties seem to be emerging as an important force.

In theorizing about the impact of catalytic agents, it is important to differentiate between the place of a catalytic agent in the political hierarchy and the contours of the disturbance generated by a catalyst. Disturbances are complex, multifaceted phenomena. Differences in the contours of these disturbances would affect their impact on policies and institutions

independent of the agent generating them. Important differences exist in mass demonstrations, lobbying efforts, and letter-writing campaigns, even when they are generated by the same agent. One need only contemplate the variance in the magnitude, scope, and duration of the realignments reported earlier to realize that not all electoral disturbances generated by mass publics are similar. But to say that realignments vary is different from saying that comparable realignments at different times produced different effects due to the electorate's placement within the constellation of forces that generate political change. Thus we must be sensitive to the contours of disturbances if we are to disentangle the effects of the multiple forces involved in the production of political change.

## Receptors

In formulating a broad, encompassing theory of political change we also must pay attention to the receptors of the pulses generated by political catalysts—both representative institutions that receive the pulses and transform them into institutional and policy changes and the institutions and policy structures that absorb those transmissions. The insightful work of Chubb and Peterson (1985) stresses that these receptors have undergone important modifications over the course of American political development and that these modifications have implications for understanding political change. Consider governmental institutions:

> As the Republic has matured, its government has become further established and resistant to change. Citizens and groups have become increasingly attached to established practices, and officeholders have been ever more successful at insulating themselves—and their organizations—from external political pressures. . . . Political officials with a stake in maintaining their institutions were finding ways to insulate them from voters. Government at all levels and in all branches was employing more people divided into more departments, agencies, and committees; following more rules; and adhering more closely to established norms of behavior. Restrained internally by extensive commitments to the status quo, the political system, though responding to the rising demands of a country experiencing rapid growth and modernization, was containing these demands within increasingly well-entrenched organizations. The system was, in other words, becoming institutionalized. (Chubb and Peterson 1985, 6, 8)

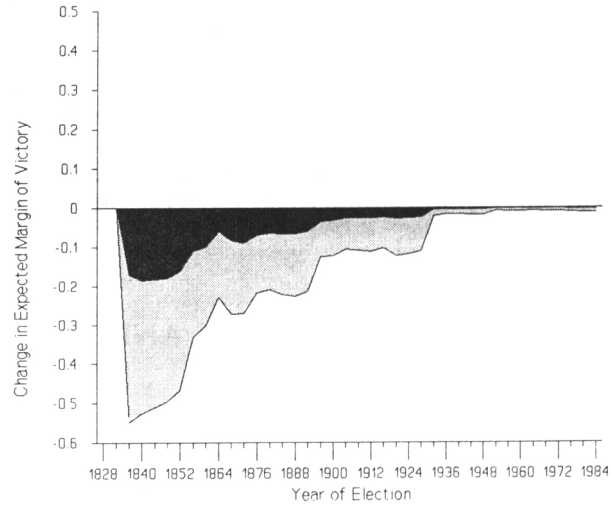
The authors go on to note that rather modest electoral shifts in 1896 led to major institutional shifts, while the major shifts in 1932 took far longer to effect institutional changes. By the 1980s, Chubb and Peterson see the engines of change within government being limited to the executive branch, national political parties, and, to some extent, the U.S. Senate. The House of Representatives and state governments largely have become conservators of past practices.

Also important are changes in the structure of certain public policy domains. In some policy domains, "programs are so complex, associated interests are so entrenched, and procedural protections

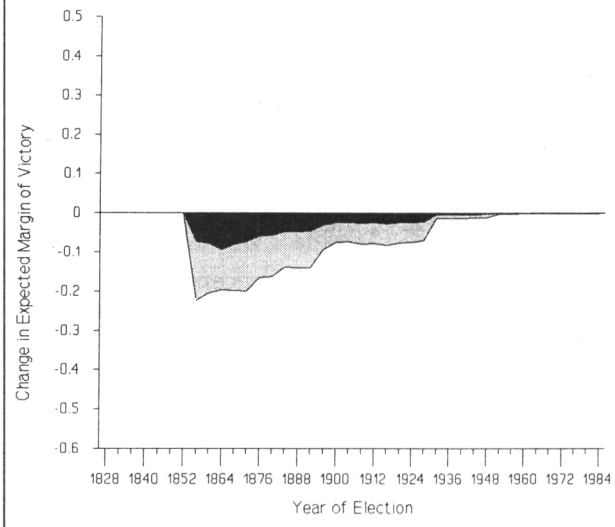
FIGURE 3

**Electoral Impact of Critical Realignments**

**a. Whig Realignments**




**b. Antebellum Republican Realignments**




**c. Jim Crow Realignments**

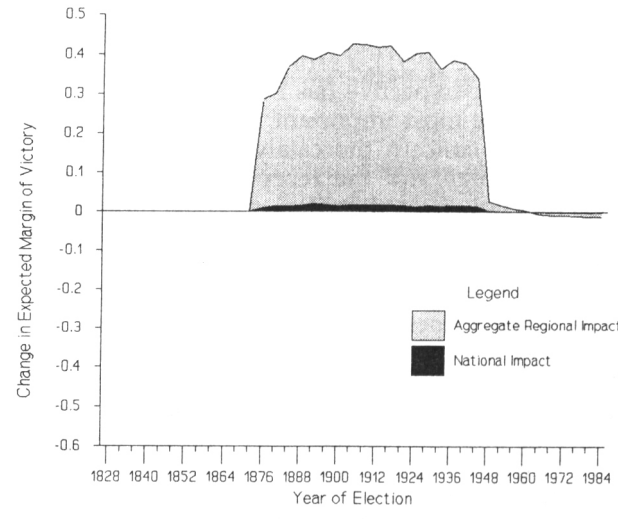
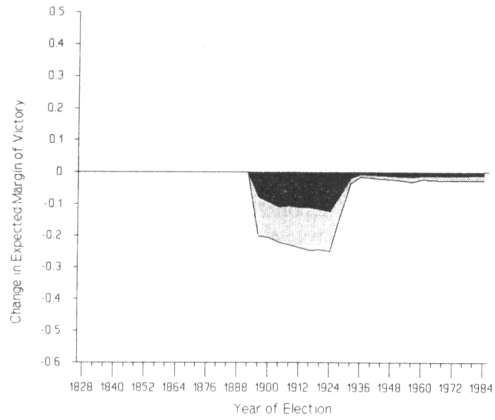
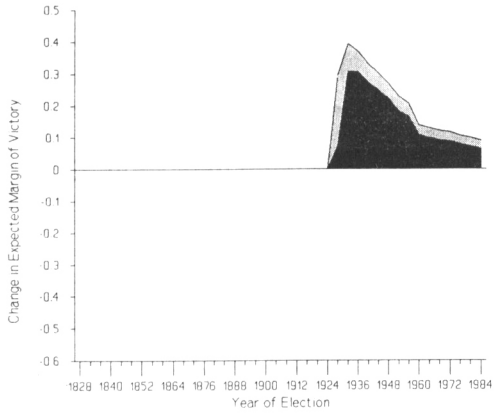


FIGURE 3 continued

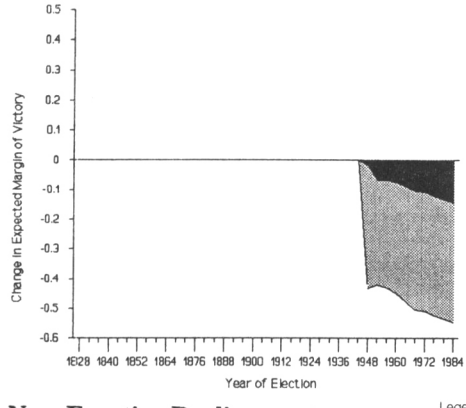
d. Industrial Realignments



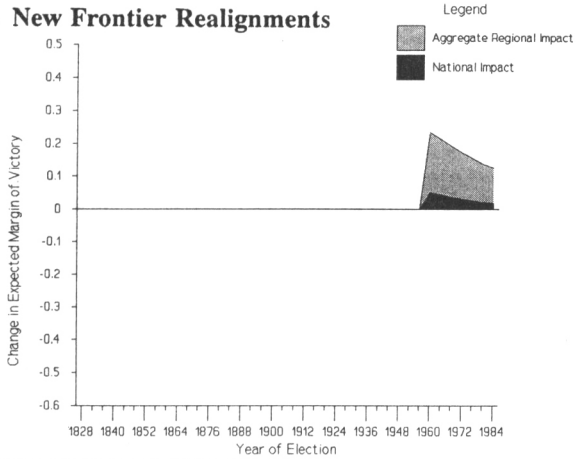

e. New Deal Realignments




f. World War II Realignments




g. New Frontier Realignments




are so extensive that the rate of change is inevitably slowed" (Chubb and Peterson 1985, 9). The fabric of policy in certain areas (those involving commitments to the elderly, the poor, national security, etc.) has become so tightly woven that it resists tears. Thus, while the creation of an economic recovery plan during the 1930s generated considerable resistance, that effort cannot compare to what will be required to create a new health policy.

While these observations suggest how involved a broad, encompassing theory of political change would have to be, they also make clear the role and centrality of the concept of a critical realignment. It is a disturbance generated by one catalytic agent, the electorate, among several. The significance of this disturbance to the receptors of political change will depend on its magnitude, duration, and geographic scope, as well as its reach across elective offices. Also important here, however, is the electorate's relative standing among catalytic agents, as well as whether the disturbances generated by other agents were reinforcing, undercutting, or nonexistent. How the disturbances generated by the electorate are transformed into political changes depends on the strength and clarity of the underlying message but also on the degree of institutionalization among receptors.

While the concept of a critical realignment relates to the behavior of only one political catalyst, no theory that purported to explain political change over the course of the United States experience would be complete without it. This article makes a small contribution to the development of a comprehensive theory of political change by developing and implementing an approach to capturing the disturbances generated by the electorate over the past century-and-a-half. The work reported here needs to be supplemented by the extension of this approach to other elective offices. Only then will we have a truly comprehensive measure of the disturbances generated by the electorate, a measure that will allow us to compare these disturbances across American political history. This will provide us with a good base for beginning to build and elaborate on the other components of a more comprehensive model of political change.

## APPENDIX

The quantitative data reported in this article derive from a six-year study of electoral patterns in U.S. presidential elections. The first stage of this study involved the construction and refinement of a large set of electoral data—all U.S. counties and most major cities for all presidential elections between 1828 and 1984. The second phase implemented a set of procedures that grouped the counties and cities into geographically contiguous substate regions based on the similarity of their voting patterns over time. The third stage of the project derived normal vote estimates for each of these regions. The resulting data set, then, contains macro level normal vote measures for every region in the continental United States since 1828 or statehood.

The procedures used at every stage of this research were developed after a great deal of thought and pretesting, were quite

involved, and were documented carefully. A detailed appendix is available from the author upon request. A less detailed overview is contained in Nardulli 1994. This appendix provides an even briefer summary of the procedures used to (1) derive the regions and (2) construct the normal vote measures. These two procedures constitute the methodological footings for the findings reported in the article.

## The Derivation of Political Regions

The argument for using substate political regions as the unit of analysis for this project is developed in Nardulli 1994 and cannot be reiterated here. It rests, however, on a long line of research that focuses on the centrality of these regions in American politics since the colonial era. Despite their centrality, there has been little systematic research on the specification of these substate regions, and for good reason. To identify substate regions in a precise, reliable, and meaningful manner is a challenging task. After a great deal of preliminary analysis and experimentation, however, a set of procedures was developed to group the 3,136 counties and cities into 215 electorally homogeneous, substate political regions.

These regional groupings were identified, on a state-by-state basis, by entering the results of a cluster analysis into a mapping program that displayed the location of geopolitical units that clustered together. In the cluster analysis, counties and cities were used as the unit of analysis. The grouping variables used were a set of margin of victory (MARGIN) variables, one for each of the 40 or so elections in the 1828–1984 time period. MARGIN was defined as the Democratic percent of the total vote minus (−) the Republican percent of the total vote. The groupings of four different cluster solutions were displayed using a mapping program. Each solution was examined for reliability, geographic contiguity, and electoral distinctiveness. From these analyses one solution was chosen to represent the state's regional structure. In most states, some minor refinements had to be made to produce a geographically contiguous regional structure (i.e., 2–3 counties had to be assigned to a different grouping, about 4% of all counties).

The problem with using cluster analysis in this grouping procedure is that it will produce $n - 1$ groupings for every set of $n$ cases on any set of $m$ variables (Aldenderfer and Blashfield 1984; Lorr 1987). Thus, the possibility exists that the substate regions identified here are merely artifacts of the method used to identify them. That is, there may not be phenomena in the "real world" that correspond to the notion of an electorally homogeneous substate region. If there are, the methods used here may not have identified them accurately. Finally, even if the regional groupings produced here are valid and reliable, an examination of their utility in the study of electoral change must be conducted. Each of these concerns was addressed.

Face validity for the proposition that the regions identified here are "real" is found in the geographic contiguity of the counties grouped by the clustering procedure (see Nardulli 1994 for maps of electoral regions for selected states). While cluster analysis will *always* yield groupings, there is no assurance that the groupings produced would be geographically contiguous. Maps of the cluster solutions could well have appeared more like checkerboards than cohesive regional structures, but they did not.

The reliability of the procedures used here had to be checked to ensure that the results of the cluster analysis were not highly dependent upon how counties voted in a small handful of "deviant" outlier elections (e.g., 1856, 1912, 1964). To examine this possibility, a "split-half" reliability test was developed. The results of the cluster analysis for all elections were compared with the results for two other analyses that were conducted on alternating sets of elections (one group included 1828, 1836, 1844, etc.; the other group included 1832, 1840, 1848, etc.). A reliability coefficient was then computed by determining the number of counties that were assigned to the initial grouping. Overall, 88% of counties were assigned to their original grouping, a result that further suggests that the groupings are empirically well grounded.

While the results reported thus far suggest that substate regions are real and that the procedures used here do a reliable job of identifying them, we also must examine the internal cohesiveness of these groupings over time. If the counties and cities that comprise the electoral regions analyzed here do not maintain a fairly stable relationship over the forty elections that are the subject of this study, then their utility for the study of electoral change is not great. It is not reasonable to expect any group of political units to march lock-step across the panorama of American politics since the Jacksonian era, nor does the conceptual definition of a homogeneous political region require it (see Nardulli 1994). What is required is that the electoral responses of the constituent counties and cities ebb and flow together over time, in some sort of loose harmony. Stated in operational terms, we expect that the value of the MARGIN variable for constituent units will form a fairly stable band over time around the regional mean of MARGIN.

To check this expectation, we calculated the standard deviation of the MARGIN variable, by region, for each election. Thus, for each region we had up to 40 standard deviations (one for each election since 1828 or statehood) measuring the spread of the margin of victory variable for the counties and cities in the region. If these constituent units do in fact form a stable band around the regional mean across time, we would expect a fairly small mean standard deviation for the 40 or so standard deviations we have for each region. Also, we would expect those standard deviations to evidence no trend. This latter expectation is important because if, for example, the size of the standard deviations *increases* monotonically over time, it would suggest that the constituent units of the region were becoming less cohesive. Both of these expectations were met (see Nardulli 1994). Cohesiveness was especially high for regions outside the South and for elections after 1872.

## Time-Series Analysis and Normal Vote Estimates

The second methodological cornerstone of this approach is the use of time-series analysis to estimate the normal vote in the regions generated by our grouping procedures. Time-series analysis is an analytic technique that is designed to facilitate the analysis of time-ordered data, which often cannot be analyzed legitimately using ordinary regression techniques (Gottman 1984; McCleary and Hay 1980). Unlike ordinary least squares, ARIMA techniques (the subset of time-series techniques that are used here) utilize a dynamic procedure that gives equal weight to all of the time-ordered observations in estimating an empirically based model of the underlying process generating the series. The ARIMA procedure is used to estimate a "noise" model that if properly constructed (stationary, homoscedastic, parameters significant and within bounds, etc.), acts as a prism in that it decomposes that data into different spectra—a deterministic component (forecast) and a noise component (residual). The decomposition of time-series data into these two spectra is ideal for our purposes because they conform nicely to Converse's (1966) conception of the normal vote.

If estimating the normal vote entailed only the construction of an adequate noise model for a given time series, it would be a relatively straightforward process. But there is more. The long-term component in electoral series does not always change slowly and short-term factors sometimes have long-term consequences. This is, of course, the thrust of the realignment perspective as developed by Key (1955) and Schattschneider (1956, 1960) and elaborated upon by a long list of distinguished scholars such as Burnham (1970), Sundquist (1983), and Clubb, Flanigan, and Zingale (1990). The existence of marked discontinuities, or interruptions, in electoral trends causes significant problems for an ARIMA analysis. If the interruptions are too large, it would be difficult, if not impossible, to construct an adequate noise model. If an adequate noise model could be constructed, its forecasts would misrepresent the origin and sharpness of the interruptions; they would appear at a later point in time. Also, they would be muted because the model would be attempting to estimate them based upon a misconception of the process that was generating them. Thus the long-term component of the process generating the time series would be misspecified, which would adversely affect estimates of the residuals.

This is wholly unacceptable because the precise identification of these interruptions is a necessary component of any attempt to

operationalize the normal vote at the macro level and it is the central concern of the present analysis. To resolve this problem we must turn to a variant of time-series analysis called interrupted time-series analysis (Box and Tiao 1975; McCleary and Hay 1980; McDowall et al. 1980). Interrupted time-series analysis allows one to include in an ARIMA model a set of intervention components. The procedure tests for the statistical significance of the interruptions, to see if observations following it are likely to have been generated by a different process. Interrupted time-series analysis requires the analyst to specify the point at which the interruption occurs and the form of the change that is posited to occur. There are normally three models of temporal change that can be posited: the impulse-decay model, the step model, and the dynamic growth model. We also had occasion to fashion a fourth type that is relevant to some partisan realignments: a modified impulse-decay model.

The application of interrupted time-series analysis to the study of critical realignments presented several problems. First, realignment theory is not as precise as would be preferred in specifying when to test for realignments. For the time period under consideration, most observers would agree that, at the presidential level, major realignments occurred around the Civil War, around 1896, and around 1932. Fortunately, this ambiguity is not a significant barrier to the analysis. Not knowing whether a Civil War realignment occurred in a region in 1856 or 1860 is not as important as knowing that it occurred around that time frame. An integral part of an interrupted time-series analysis is an examination of a plot of the variable being analyzed over the time frame in question. That plot usually provides decisive information on the year a discontinuity began as well as the type of change it precipitated (impulse-decay model, step model, etc.).

Unfortunately, ambiguity concerning the hypothesized beginning of critical change is not the most serious deficiency of the realignment literature. It became clear early in the preliminary stages of the time series analysis that the literature on realignments was simply inadequate to deal with the rich complexity of electoral patterns embodied in American electoral history. This posed a serious dilemma. We did not want to begin "fishing" for realignments in a haphazard manner. Structuring a set of intervention components to fit the unique distribution of data embodied in a region's electoral history would do little to further a generalized understanding of electoral change. At the same time we did not want our efforts handicapped by conceptual blinders. It would have been irresponsible to ignore major and enduring interruptions that recurred in a significant number of regions simply because prior researchers failed to detect them. To do so would have led to serious biases in our normal vote estimates.

To deal with this problem in a methodologically responsible manner, a three-phase procedure was adopted. In phase 1, about half of the regions (130 of the 215) were analyzed. In the initial stages of this phase, traditional conceptions of when realignments occurred served as the primary guide in the specification and testing of intervention components. As insights were gained from dealing with the electoral patterns of additional regions, we experimented with additional sets of intervention components. The results of the first 130 analyses were inventoried to differentiate between idiosyncratic interruptions and those that characterized a significant number of regions, even if those regions were all in one section of the country. In phase 2, armed with this empirically grounded sense of the pattern of realignments in U.S. electoral history, the time-series analyses were reconducted for all of the initial 130 regions, as well as the remaining 85. Once all 215 analyses were completed, a graduate student who was conversant with the literature on partisan realignments and time-analytic techniques blindly examined the raw time plots of all 215 regions and was asked to specify when realignments occurred and what form they took. These "blind" specifications were compared with the results of the interrupted time-series analyses. This comparison led to about 20 discrepancies. The time series for those regions were reconducted, specifying different models of change, and the results were compared with the original analyses. This procedure led to changes in seven models.

Examples of the forecasts from this procedure are presented in Nardulli 1994, along with more details on how other problems, such as "outlier" elections (1912, 1916, 1964, etc.), were handled.

## Notes

I am enormously indebted to James H. Kuklinski for many unselfish years of insightful comments on this project and this paper. My colleagues Paul J. Quirk, Michael Krassa, and Michael McBurnett provided me with important comments on earlier versions of this article, and Jeffrey M. Stonecash and W. Phillips Shively were kind enough to review the final version. This effort would not have been possible without the incredible efforts of a team of talented and devoted research assistants that included Michael Frank, Jon Dalager, Norman Hurley, and Ellen Riggle. It also could not have been done without financial support from the Research Board, the College of Liberal Arts and Sciences, the Department of Political Science, and the Institute of Government and Public Affairs at the University of Illinois.

1. Political observers have been aware of the geophysical heterogeneity of the nation and its political implications since colonial days. Turner provided the first influential scholarly treatment of the electoral and political implications of this heterogeneity (1932, 38, 45, 287–314); his pioneering work was supplemented by the later work of Key (1949) and his students (Fenton 1957, 1966; Lockard 1959). See Nardulli 1989 for a review of some key works on regionalism and politics. It must be recognized that the same type of factors that contribute to differences in partisan allegiances across regions will lead to varying reactions to the type of stimuli that lead to critical realignments. It also should be realized that while geopolitical diversity leads us to expect *different* responses to stimuli across regions, the more recent work of Huckfeldt and Sprague (1993) provide a strong theoretical foundation for the proposition that cohesive spatial units are likely to react *similarly* to stimuli that could precipitate a critical realignment. This is a key premise to the normal vote approach used here to study critical realignments.

2. The advantage of using normal vote estimates to measure the magnitude of a realignment, rather than actual vote margins, is that short-term fluctuations in the vote (noise) are removed by the time-series routine. Because a normal vote estimate is a composite of all elections in an electoral era (a set of elections demarcated by critical realignments), it provides a stable means of measuring the impact of a realignment.

3. The proportion in Figure 1 is weighted by that region's proportion of the total national vote. This was necessary because the size of the electorate varies considerably across regions. The data in Figure 1, then, show the proportion of the nation that realigned during each year. The number of regions and states will vary across years because states entered the union at different times.

4. The election of 1836 actually had three regional National Republican candidates: Hugh White, Daniel Webster, and William Henry Harrison. While the Whigs did not run a candidate under their party banner until 1840, the real break in the electoral patterns comes in 1836.

5. The inconsequential nature of the Democratic antebellum realignment depicted in Figure 1 is surprising at first glance. We would have expected more Southern regions to manifest clear, sharp breaks in their electoral patterns in the years before the Civil War. However, the combination of a critical realignment's definition and historical circumstances in the South makes this expectation unwarranted. An examination of electoral patterns in the South reveals sharp movements toward the Democrats in the early and mid-1850s, as well as a good deal of Democratic secular change beginning in the mid-1840s. However, none of the South voted in 1864, and much of it did not vote in 1868. This, of course, interfered with the emergence of stable patterns. More importantly, when the South did begin to vote in 1872 and 1876, the electorate was vastly different from the pre-Civil War period. Many regions voted heavily Republican. Because an *enduring* interruption did not emerge, many of the antebellum Democratic shifts in the South do not qualify as realignments. Instead, the entire period around the Civil War simply reveals a great deal of instability in the South.

6. This period presents special problems because a number of things are happening. First, there are a series of short-lived Republican realignments that occurred during Reconstruction. These will not be dealt with here because they are few in number, largely due to the disenfranchisement of Confederates, and because they began to deteriorate very quickly after they emerge. Second, a series of Democratic realignments emerged in New England in 1876, presumably due to the recession that began in 1873. These realignments were of some magnitude, but as they are highly localized (Massachusetts, and parts of Vermont, Maine, and Rhode Island), they will not be dealt with here. Finally, the election of 1884 saw a series of realignments in Iowa and rural Wisconsin. They also are of too little scope to merit further analysis here.

7. See n. 5.

8. The exceptions are parts of Connecticut, New Jersey, Maryland, and Massachusetts, New Mexico, and the Upper Peninsula in Michigan.

9. In addition to these three models of change, I found it was necessary to fashion a fourth—a modified impulse-decay model. This model fit electoral changes that required a series of pulses (usually two) before reaching a peak and then decaying. This adaption was necessary in order to model accurately the structure of change in urban areas beginning in 1928. Unlike most regions, most large cities registered a major shift in 1928 (immigrants reacting to the candidacy of Al Smith), followed by another in 1932. Then, like most regions that realigned in 1932, the initial interruption deteriorated. This situation was captured by measuring the 1928 jump with a "pulse" function and building it into the impulse-decay model.

10. For a graphical depiction of these various models of change using presidential elections data from the regions used in from this study, see Nardulli 1994.

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