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Magnitude and durability of electoral change: Identifying critical elections in the U.S. Congress 1854–2010

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ABSTRACT

The two hallmarks of a critical election and, hence, of a critical realignment are the magnitude of the observed change and the durability of that change. In addition to offering a new approach to measuring durable change in national party dominance, and providing a non-parametric criterion to identify unusual changes in seat/vote shares, we provide fresh insights via a unifying statistical approach that reflects both of these factors simultaneously. Furthermore, we assess the robustness of critical election determinations in two ways. First, we compare the magnitude of inter-election shifts with both average volatility over the entire time period and volatility relative to a particular time period. Second, as an alternative to the usual perspective, we consider critical elections not as a one-time cataclysm, but rather as a pair (or perhaps even triple) of consecutive substantial shifts, generated by the same underlying factors. Overall, we distinguish six elections that marginally or provisionally meet our criteria to be critical elections. But focusing on pairs of elections, 1858–60 and 1930–32 stand out as critical among all elections since the 1850s.

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1. Introduction

The thesis of (regularly spaced) realignments tied to critical elections, i.e., elections that reflect a “dramatic and durable” change from previous patterns, and that thus reshape the landscape of U.S. political competition, has been a staple in political science since the idea was given its classic formulation by V. O. Key in the 1950s.¹ Yet, today, while it is conventional wisdom to recognize the existence of a realignment that has ended the traditional New Deal coalition and turned some elements of it, such as

Democratic dominance in the South, on its head, virtually all political scientists who study realignment would reject the idea that there have been any critical elections since 1932. Although there have been a handful of dissenters (e.g., Norpoth and Rusk, 2007; Hopkins, 2010), we view the current consensus (see e.g., Brunell and Grofman, 1998; Stonecash, 2006, 2010; Brewer and Stonecash, 2009) as claiming that, since the New Deal, we have experienced what Key called a *secular realignment*, i.e., a pattern of gradual change. This view replaces an older consensus that could perhaps best be characterized as realignment skepticism: for example, in academic work in the 1980s and 1990s, the period from 1932 on was characterized in terms such as *dealignment* (see, e.g., Shafer, 1991).

Mayhew (2002: 15 and pages following), has gone even further in challenging the conventional wisdom about the importance of critical elections for U.S. realignment by calling into question the classification of the five elections

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¹ See V. O. Key (1955, 1959), Lubell (1952), Sundquist (1983), Burnham (1967, 1970), and Beck (1974) among many others, as well as the work of numerous historians and political geographers (see e.g., Shelley and Archer, 1994; Shin and Agnew, 2002, 2007).

usually designated as “critical” (1800, 1828, 1860, 1896, and 1932).² Recent statistically oriented work by Norpoth and Rusk (2007) has also cast doubt on the standard listing of 1896 and even 1860 as critical elections, and they suggest that some other historic elections, e.g., 1874, might be on a par with 1860 for that distinction.

We follow Key’s classic definition of critical alignment as a change in partisan strength – in Congressional seat share or in presidential support – that is “sharp and durable” (Key, 1955). Using that definition, the goal of this paper is to offer a variety of methodological and theoretical innovations to better permit a quantitative reassessment of the conflicting viewpoints about the existence (and identification) of critical elections.

We begin by investigating the distribution of inter-election shifts in partisan seat shares in the chambers of Congress and the shifts in the popular votes for president at the national level. If seat and vote shares are relatively stable over substantial periods of time, but with a few large shifts, we would expect that most shifts would be relatively small, but a few (brought about by “critical elections”) would be very large; the more numerous small shifts would likely follow a normal or related distribution with a small variance while the abrupt shifts would constitute pronounced outliers to this distribution. On the other hand, if political change is gradual, then we would expect that the distribution of shifts would be roughly normal, without notable outliers. Hence, a search for outliers is the first key to whether a pattern of critical realignments, as opposed to secular realignments, is characteristic of the data.

But the first of Key’s two criteria of “sharp and durable” is only half the story. Our next analyses look at the second criterion, durability. If there are sharp shifts uncovered by our analysis of outliers, are these shifts durable? To answer this question, we will offer a simple but new criterion for “durability” of change in terms of changes in seat share that we think better expresses the spirit of Key’s original work than the standard way of specifying durability simply in terms of length of continuous partisan control.

Next, we will combine our two analyses to provide a new statistical model that allows us to simultaneously test for magnitude and durability of electoral effects.³ We offer this as the most significant methodological contribution of our paper. And it allows us to offer fresh (and surprising) empirical perspectives on which elections should count as critical, in the sense that V.O. Key meant that term.

We also offer three other important clarifications to the literature on realignment.

First, we observe that whether an election is critical in the sense of sharp change must be determined with respect

to some baseline level of volatility in inter-election swing. Thus, on the one hand, an election might manifest a very large swing, but that swing need not appear all that unusual relative to other elections in that time period while, on the other hand, an election with a lower swing might actually be more sharp and unusual when judged relative to the volatility in seats or votes during its time period. Whereas 1874, 1894 initially appear as critical, they do so only marginally when we compare them not to the overall time series but to the high volatility of elections within their own electoral era. On the other hand, 1994 and 2010 might actually be characterized as critical elections, but only in comparison to the much lower level of mean seat changes in recent decades and, of course, the durability of the 2010 shift remains to be seen.

Second, we repeat the obvious, but all too easily neglected point, that an appearance of stability at the national level in party seat share may mask movements in opposite directions at the regional level. In particular, there have been dramatic (but slow and partly compensating) changes in party dominance over the post-WWII period in the South and New England states.⁴

Third, applying our approach to U.S. elections over the period 1854–2010, we come away with skepticism about even traditional identifications of 1860 and 1932 as critical elections *per se*. While an enormous political change occurred in 1860, it did so largely through the change in the composition of the Congress as a result of southern secession, not as the result of election *per se*, and the evidence for dramatic change in 1932 is strongest for the presidential outcome. However, as we will later show, sometimes we have changes that can be seen as sharp, but yet not limited to a single election, e.g., three successive “shocks”, such as occurred in 1930, 1932 and 1934 in terms of congressional seat share, and between 1928 and 1932 in terms of presidential vote share, or dramatic legislative change in the period 1858–60. Thus, we suggest that we might think about realignment via critical elections not necessarily as a single election shock, but rather as a series of shocks, rather like an earthquake with both preliminary rumbles and an aftershock, with pairs of elections, e.g., 1858–60, 1930–32, being a natural way to conduct an empirical analysis.

In Section 2 we lay out statistical tests for identifying critical elections. Using national election data, we (a) define the magnitude of seat change relative to the average interelection volatility over the entire period from 1854 to 2010, (b) offer a new way to measure durability of election effects, and (c) provide a new statistical test that jointly examines the magnitude and durability of effects. In Section 3 we consider what happens when we “relativize” the notion of magnitude of seat change by comparing shifts to what is typical of the historical epoch in which they occur. In Section 4 we integrate our combined statistical approaches with the historical evidence on the wellsprings of political changes in different political eras, discussing the evidence for each of six different election years (1860, 1874,

² Indeed, Mayhew (2002), has called into question the basic elements of the standard model of electoral realignment, and he is particularly scolding about the empirical evidence for periodicity in critical elections.

³ Norpoth and Rusk (2007) indicate criteria for specifying critical elections (a minimal shift in the vote and the absence of an immediate reversal of the shift), and control for midterm election effects. However the significance levels they report are for a statistical test that is not completely specified either in the paper or in the cited data source (Rusk, 2001).

⁴ A similar long term shift toward the Democrats is observed in the states bordering on the Pacific Ocean.

1894, 1932, 1994, and 2010) being regarded as critical elections. Then we look at the notion of viewing *pairs* of succeeding elections, such as 1858–60 and 1930–32 as critical, and find considerable support for that idea. Finally, we point out some key differences between national patterns of realignment and patterns at the regional level.

In this paper we come neither to praise nor bury realignment theory, but to show how its two aspects – magnitude and durability – fit together. However, the support that our findings provide for the occurrence of critical elections is only marginal, and qualified by reservations such as the need to sometimes look at pairs of elections rather than single elections as critical.

2. A statistical test for identifying critical elections at the national level

There are multiple notions of realignment in the literature (or demographic shifts in party support groups, or changes in which issues structure party cleavages). Here, however, like the vast bulk of the realignment literature, our operationalization of the concept of realignment and the associated concept of critical election is tied to pronounced and durable changes in party strength, involving changes in party dominance. And our focus will be at the national level, although we also have some remarks later in the paper about the relationship between national and regional patterns.

Norpoth and Rusk (2007) seek evidence of critical elections defined in terms of a change in national partisan dominance by examining changes in vote share; other authors such as Merrill et al. (2008) focus only on partisan seat shares. Here, though our principal focus is on seat share change, we look at both indicators.

There are reasonable arguments for using either seats or aggregated popular votes as a measure of partisan strength for purposes of identifying critical elections. Although aggregate vote share for legislative members is not skewed as much as seat share by redistricting effects, the effects of redistricting on the occurrence of durable changes of partisan strength and control appear small.⁵ Likewise, vote share is less affected by “natural” skewness in the shape of the partisan vote-share distribution due to regional effects involving areas of partisan concentration, or by changes in inter-election volatility related to the nationalization of

elections. On the other hand, as a result of non-competitive districts, in which turnout is reduced, often because of incumbent strength – the extreme case of which are districts that go uncontested – vote share can be misleading. Lastly, we rely on the fact that, ultimately, it is winning seats that really counts.

2.1. Statistical methodology to search for outliers in magnitude of inter-election shifts

In this paper, we focus primarily on elections for the U.S. House of Representatives and the U.S. Senate, although we also consider presidential elections. As noted earlier, we follow the classic definition of critical elections in terms of (a) their magnitude and (b) their durability (Key, 1955). Our first test for critical elections involves a search for outliers in terms of magnitude of inter-election shift in seat share (for the Congress) and popular vote share (for the presidency). We begin with the raw data and present, in Fig. 1, time-series plots of seat share change (expressed as a proportion of all seats) over the period 1854–2010. This period can be thought of as the “modern era” of politics dominated by the Democratic and Republican parties.⁶ Fig. 1A plots the inter-election shifts in (Democratic) seat share in each chamber of the U.S. Congress every two years.

If critical elections are characterized by huge swings in seat share, such shifts would be expected to be extreme outliers in terms of inter-election shifts in seats, as judged relative to the entire time period. Thus, a test of normality of the overall series might be rejected. Although we report a conventional test for normality, the presence of multiple outliers significantly inflates the sample variance, rendering outliers difficult to detect by conventional normality tests. Given that outliers may occur, our main focus is on a nonparametric search for outliers and on a test for whether the frequency of such outliers exceeds expectations. The nonparametric approach we use is desirable because our criterion for specifying outliers – which depends on the locations of the quartiles rather than the sample variance – is independent of the extremity of the outliers in the sample and thus avoids circular reasoning. We do the search for outliers separately for each chamber of Congress, for the House and Senate combined, and for the presidency.

We follow Tukey (1977; see also Sheskin, 2007) for the nonparametric designation of which shifts should be classified as outliers, which we interpret, in turn, as possible critical elections. Based on the interquartile range, the Tukey classification specifies *upper* and *lower inner fences* and *upper* and *lower outer fences*.⁷ Note that, if these

⁵ Engstrom (2006) compares partisan electoral strength for House elections for the period 1870–1900 between old and revised district configurations in each state to estimate the likely net effect of redistricting on the national partisan seat share in the House. He finds that differences between the projected (counter-factual) and actual seat share can be as high as four percentage points and, as Engstrom points out, appear to have reversed control of the House in two elections (1878, to the advantage of the Democrats; and 1888, to the advantage of the Republicans). These reversals, however, do not appear to have had much long-lasting effect. In the 16 congressional elections from 1870 to 1900, the Democrats appear to have been significantly advantaged five times, the Republicans three times. In most cases, the party that dominated the House is estimated to have increased its seat share by redistricting, probably because it also dominated a majority of state legislatures. This effect tends to slightly exaggerate the strength of the majority party at any one time, but has had very little effect on long-term major swings in partisan strength.

⁶ Seat share data were obtained from Dubin (1998) and collected by the authors from various electronic sources like www.polidata.org (2004 data) and www.cqpolitics.com (2006, 2008, and 2010 data). Independent members of Congress caucusing with a major party were counted with the major party.

⁷ Denoting the first and third quartiles as Q_1 and Q_3 and the inter-quartile range ($Q_3 - Q_1$) by IQR, the *upper inner fence* is defined as $Q_3 + 1.5 \times \text{IQR}$ and the *lower inner fence* as $Q_1 - 1.5 \times \text{IQR}$. Similarly, the *upper outer* and *lower outer fences* are defined as $Q_3 + 3 \times \text{IQR}$ and $Q_1 - 3 \times \text{IQR}$, respectively.

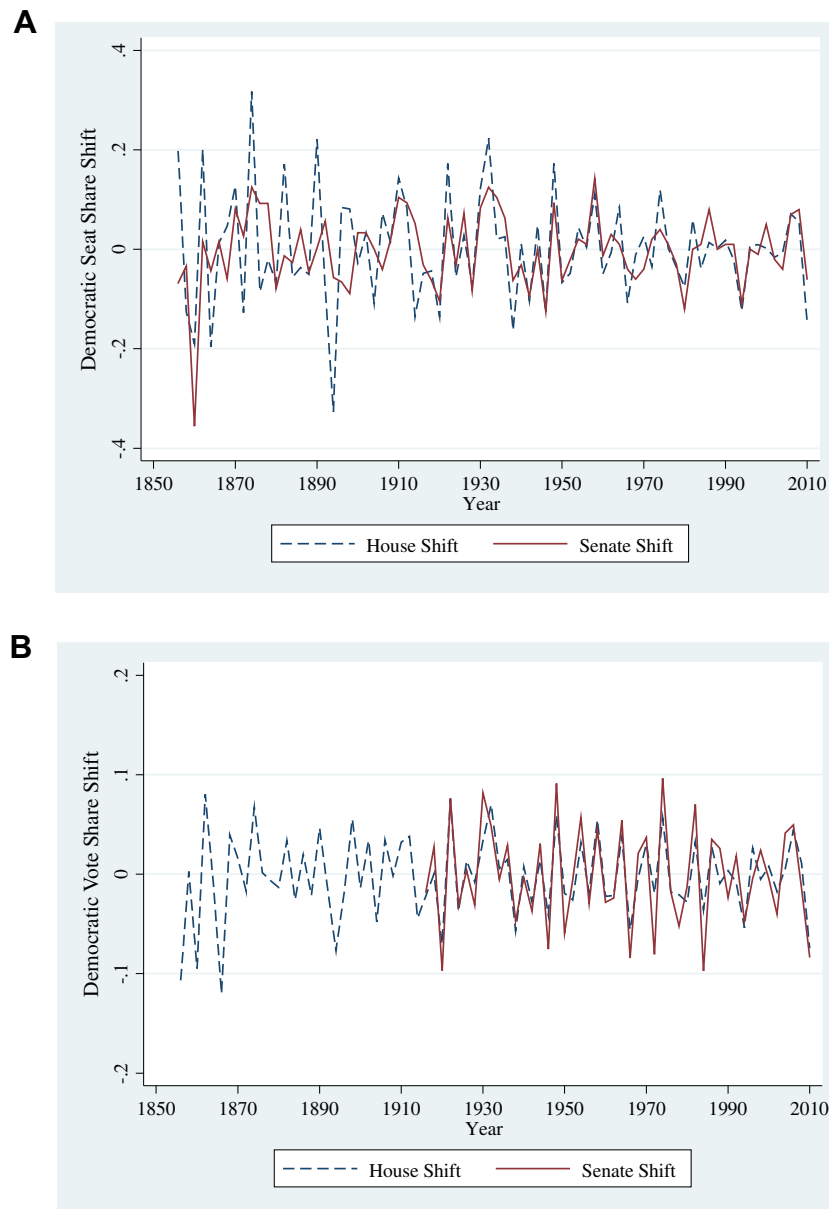


Fig. 1. A. Inter-election shifts in congressional seat share proportions. NOTE: Data series are based on shift in Democratic seat share (as a proportion of major party seats relative to seat share in the previous election). B. Inter-Election shifts in congressional vote share proportions. NOTE: Data are based on changes in the average share of the Democratic vote across all districts in a given election year. Uncontested elections were recoded to 75 percent of the vote for the winning party.

concepts are applied to normal data, the inner fences are 2.7 standard deviations from the mean so that less than 1% of the data points can be expected to lie outside the inner fences (and less than 0.001% outside the outer fences). Data points outside the inner fences are termed *outliers*, and are candidates for designation as critical elections; values outside the outer fences are termed *severe outliers*, and either clearly represent critical elections or else have some other highly unusual explanation.

Because our interest is on the presence of extreme election shifts rather than other possible deviations from normality, we also perform a more focused test, namely a simple binomial test to see whether the frequency of outliers is greater than would be expected if inter-election shifts were normally distributed. If the latter were the case, the number of inter-election shifts beyond the inner fences should be binomially distributed with parameters $p = 0.007$ and $n = 77$ for the Congressional time series.

Table 1
Identification of major shifts: outliers and tests for normality.

Year	Deviations in interquartile units						
	Raw shifts				Detrended shifts		
	House	Senate	H&S	President	House	Senate	H&S
1860	−0.95	−3.30††	−2.50†	−1.02	−0.46	−2.34†	−1.75†
1874	1.96†	0.79	1.90†	NA	1.20	0.53	1.38
1894	−2.00†	−0.14	−1.61†	NA	−1.49	−0.02	−1.32
1932	1.24	0.79	1.39	1.24	1.28	0.80	1.42
1994	−0.44	−0.70	−0.79	NA	−1.59†	−1.08	−1.47
2010	−0.62	−0.17	−0.65	NA	−2.95†	−0.46	−1.48
P-value for Shapiro–Wilk test of normality	0.47	<0.001***	0.33	0.95	0.02*	0.09#	0.49
P-value for binominal test of normality	0.10#	0.42	0.02*	1.00	0.42	0.42	0.10#
P-value for composite test of normality	1.00	0.16	<0.001***	1.00	0.01**	0.16	0.16

NOTES: Data is based on Congressional seat shares for 1854–2010 and presidential popular vote share for 1856–2008. Values in the table in election-year rows are the quantities (in units of the inter-quartile range) by which the inter-election shift fell below the first or above the third quartile. The symbol (†) indicates that the inter-election shift in that year is an outlier, beyond the “inner fence”, i.e., beyond either the first or third quartile by more than 1.5 times the inter-quartile range. The symbol (††) indicates that the shift in that year is a severe outlier, beyond the “outer fence”, i.e., beyond either the first or third quartile by more than 3 times the inter-quartile range. We use outlier status as an indicator of “major” shifts. For the raw shifts, the only elections for which the inner-fence outlier criterion is met for any of the categories House, Senate, House and Senate, or president are 1860, 1874, and 1894. For the detrended shifts, the only elections for which the inner-fence outlier criterion is met for any of the categories House, Senate, House and Senate are 1860, 1994, and 2010. The election of 1932 comes close to meeting the inner-fence criterion for both raw and detrended data; when combined with the elections of 1930, the pair 1930–32 stands out along with 1858–60 as the only paired elections for which the inner-fence criterion is met for either raw or detrended shifts (see Table 3). For the normality tests, the symbol (#) indicates significance at the 0.10 level; the symbol (*) indicates significance at the 0.05 level; the symbol (**) indicates significance at the 0.01 level, and the symbol (***) indicates significance at the 0.001 level.

In what may be the most innovative part of our analyses, we test the frequency of inter-election shifts that are *critical* in being both dramatic (beyond the inner-fences) and durable (for which we use the criterion that the durability of the shift is at least five elections – see discussion of the durability criterion below). We determine a sampling distribution for this frequency (i.e., of the test statistic) by computer simulation.⁸

2.2. Empirical evidence on magnitude of inter-election changes in seats

Table 1 summarizes the results of the analyses (plots showing outliers are presented in Fig. A1 in the Appendix). Values in Table 1 in election-year rows are the quantities (in units of the inter-quartile range) by which the inter-election shift fell below the first or above the third quartile. Hence values above 1.5 or below −1.5 are outliers; those above 3 or below −3 are severe outliers. We will initially analyze the shifts based on raw data; shifts based on “detrended” data will be considered later.

We would emphasize that a sample need not exhibit an outlier at all; the most extreme data point is not necessarily

an outlier. Because the number of outliers identified by the Tukey criteria depends on the sample size, we introduce later a statistical test of whether the frequency of data points designated as outliers exceeds what would be expected if the data were normal.⁹

Initially, applying the omnibus Shapiro–Wilk test of normality and the Tukey outlier criteria to the data series of inter-election shifts, we find that normality is not rejected for the House ($p = 0.47$), but there are two outliers, in the years 1874 and 1894. The Democrats made big gains in 1874, as many Democratic representatives replaced Republicans throughout most of the nation,¹⁰ and suffered large losses throughout most of the country¹¹ in 1894 (following the financial panic of 1893). In the Senate, normality is rejected ($p < 0.001$), and 1860 is a severe outlier; the latter is largely the result of the Democratic loss of many members of Congress as Southern states prepared for secession (see the discussion below). For the House and

⁸ For this test we assume that each inter-election shift x_n after the first is generated by the relation: $x_n = N(0,1)/1.15 - 0.5s_{n-1}$, where s_n denotes the (Democratic) seat share in the n th election. The factor, -0.5 , is suggested by using the historical data set to regress the House seat share shift (x_n) on the House seat share in the previous election (s_{n-1}). A negative sign is expected for this factor because the larger a seat share majority a party holds, the more on average it expects to lose in the next election; in fact this negative sign is significant at the 0.0001 level. The divisor 1.15 is an adjustment so that the resulting sequence of simulated shifts will have variance = 1.

⁹ Jones et al. (2010) present a Markov switching method to model partisan patterns, from which they argue that the evidence from U.S. congressional elections supports a discrete-state model rather than one based on gradual change. Their model, however, makes assumptions that preclude testing the distinction between gradual transitions, i.e., secular change, on the one hand and abrupt transitions signaling critical elections on the other. In particular, Jones et al. assume an underlying parameter, termed party regime, that can take on only one of two or possibly three values (a Republican partisan regime, a Democratic one, and possibly a weak partisan regime). Having assumed that there are only two or three such states, they cannot test whether transitions are gradual or abrupt.

¹⁰ In the 1874 House election, Democrats gained 33 seats in the South and Border states, 25 in the Midwest, and 33 in the Northeast, while the Republicans suffered comparable losses in the same areas (Rusk, 2001).

¹¹ In the 1894 House election, Democrats lost 20 seats in the South and Border states, 55 in the Midwest, and 34 in the Northeast (Rusk, 2001).

Senate combined, normality is not rejected ($p = 0.33$), but the same three dates identified above – 1860, 1874, and 1894 – are the only outliers. The only additional election for which the deviation of the seat share beyond its proximate quartile exceeds 1.0 interquartile units for two or more categories is that of 1932, for which the deviation (1.39) is close to the inner-fence criterion.¹² Finally, using popular vote for the presidency, normality is not rejected ($p = 0.95$) and there are no outliers, but the largest vote share deviation (1.24) occurs for 1932.

In our second more focused test, namely a simple binomial test to see whether the frequency of outliers is greater than would be expected if inter-election shifts were normally distributed, we would reject this null hypothesis if the number of outliers (i.e., inter-election shifts beyond the inner fences) is sufficiently high. The p -values (significance levels) for this test are given in Table 1 and indicate that the frequency of outliers for the combined House and Senate is significantly high at conventional levels to reject the null hypothesis of normality ($p = 0.02$); that for the House alone is marginally significant ($p = 0.10$). Thus, whereas strong shifts in the two chambers may have occurred in different years, taken together the Congress has experienced large shifts more often, but not greatly more often, than would have been expected were these shifts drawn from a normal distribution.¹³

2.3. Empirical evidence on magnitude of inter-election changes in votes

Because our initial focus has been on seats, a natural question is whether similar patterns apply if we look at vote share. We present, in Fig. 1B, simply for comparison purposes, time-series plots of vote share change over the period 1854–2010. Fig. 1B plots the inter-election shifts in (Democratic) vote share in each chamber of the U.S. Congress every two years (the Senate time series starts in 1914 after the advent of popular elections to that body). The correlation between the two series for the House in Fig. 1A and B is 0.70, suggesting a substantial relationship between changes in vote shares and changes in seat shares.

2.4. A new definition of durability of electoral change following a major inter-election shift

Because the evidence is marginal that extreme shifts in seat share have occurred with a frequency greater than might be expected by chance alone, we turn to the second criterion highlighted by V. O. Key – that changes be durable, i.e., not immediately reversing. The standard way to deal with durability has been in terms of some specified continuous period of party control of a legislature or legislative chamber (or of the presidency). But there can be changes in party control even without dramatic changes in seat share and dramatic changes in seat share even without

changes in party control. So, instead of using party control as our key indicator, as a way to quickly distinguish reversible versus persistent changes, we propose the following simple criterion to assess durability following an abrupt change in seat share:

Definition. The *durability of a shift in seat share* is the number of elections (including that in which the shift occurs) before 50 percent of the initial shift has been reversed.¹⁴

Thus, an election contributes to durability if the seat share it generates is closer to the share after the initial shock than to the seat share before that shock. As long as this continues to be the case, we say that the initial shock is sustained. For example, if the Democrats shift from a 45 percent to a 65 percent share in an initial election, and then maintain more than 55 percent in each of five elections including the first, we say that the durability of the shift is sustained for five elections (we could also create a similar measure for vote share, but as noted earlier, we will focus on seats).

2.5. Evidence on durability of electoral change

For the House, the Senate, and for the House and Senate combined, the elections exhibiting sharp change sustained by significant durability are given in Table 2. The same three dates identified above – 1860, 1874, and 1894 – show clear evidence *vis-a-vis* durability. Two other dates – 1932 and 1994 – suggest durable changes as well and – for the presidency, 1932 has a durability of five elections, i.e., 20 years.¹⁵ We will return to discuss the interpretation of this data below, after we consider statistical tests combining more than one criterion.

2.6. Joint statistical test of dramatic and durable shifts

Next, we examine the frequency of inter-election shifts that are *critical* in being both dramatic (beyond the inner-fences) and durable (for which we use the criterion that the durability of the shift is at least five elections) at the national level. Significance levels (p -values) for determining according to this test whether the observed frequencies of durable outliers were higher than expected are given in the last row of Table 1 (where the test is labeled “composite test of normality”). Two of the p -values are lower (more significant) than those for the simple binomial test, but significant at conventional levels only for the House and Senate combined.

In sum, based on our initial analysis of data in which we compare magnitude of inter-election shifts to the data for the entire time period, we would conclude that only 1860, 1874, and 1894 clearly qualify for critical election status based on both magnitude and durability of shift. Note that

¹² The deviation exceeds one IQR unit for only the House in 1856, 1862, 1864, and 1890.

¹³ For the presidential time series, the null hypothesis is, of course, not rejected, as there are no outliers.

¹⁴ For presidential elections, seat share is replaced by popular vote share.

¹⁵ The presidential elections of 1860 and 1920 barely meet the criterion that the deviation exceed one interquartile range; their durabilities are four and three elections, respectively.

Table 2
Durability of electoral gains in the U.S. Congress and presidency.

Year	Initial (absolute) shift in %				Durability (number of elections sustained)			
	House	Senate	H&S	President	House	Senate	H&S	President
1860	19.0	35.6	27.3	15.2	1	7	7	4
1874	31.7	12.5	22.1	NA	3*	10	10	NA
1894	32.7	5.7	19.2	NA	2*	9	8	NA
1932	22.3	12.5	17.4	17.9	3*	7	5	5
1994	12.4	11.0	11.7	NA	6	7	6	NA
2010	14.7	6.0	10.4	NA	–	–	–	NA

NOTE: Durability is the number of elections (including that in which a shift occurs) before 50 percent of the initial shift has been reversed. For the three elections marked with an asterisk (*), the initial shift in the House would be sustained for 10, 8, and 5 elections, respectively, if the 50% criterion were replaced with a 60% criterion.

1932 does not qualify! Why that is so is a question to which we will return when we complete our analyses by looking also at data that judges whether an election is critical relative to the degree of inter-election volatility in the historical period in which the election occurred and by assessing whether two consecutive elections may form a critical pair.

3. Normalizing the magnitude of inter-election shifts in terms of what is typical for a given historical epoch

It is useful to note the obvious fact that standard statistical tests require assumptions of a null hypothesis against which statistical significance is to be estimated. For the tests we have conducted up till now, the baseline has been the characteristics of the time series from 1854 to 2010, taken as a whole. But we can readily imagine that the magnitude of inter-election shifts may not have been even roughly constant over this entire time period, but rather may have increased or decreased significantly over time. If – as we will show – it has not been constant – we would like to know how our results would differ if we were to, say, compare magnitudes of inter-election shifts not to the 150+ year average but to what was happening in elections closer to the ones whose criticality we are seeking to establish or reject.¹⁶

We now investigate possible time-dependent patterns in the amplitude of seat or vote shifts. The simplest possible such pattern would be a linear trend in the magnitude of inter-election shifts.¹⁷ To look for such a pattern, we regressed the absolute value of the seat share shift against the year, separately for the House, the Senate, and the House and Senate combined.¹⁸ The predicted values from each of these regressions constitute an

estimate of the typical size of the shifts as they vary over time. These predicted values display a significantly downward slope for the House (two-sided $p < 0.001$). The same is true for the Senate ($p = 0.05$) and for the combined House and Senate ($p = 0.02$).¹⁹ The regression plots are given in Fig. 2.²⁰

One way to deal with this more or less linear time trend since the beginning of the modern era of two-party competition after the Civil War, is to control for time-specific factors. In this section, we look for evidence of critical elections in terms of shifts that would be counted as extreme relative to the underlying time trend, and thus, in effect, with comparison to the specific period that we are looking at. Looking for critical elections in terms of inter-election shifts normalized by what was common in a given political era is, in effect, working with data whose variance has been de-trended.

Because, as noted above, the raw data on inter-election seat share change has a strong trend, we normalized the House, Senate, and combined House and Senate shifts for each year by dividing by the predicted absolute value of the shift for that year, with the latter being the y -coordinate of the regression line in the respective House, Senate, and combined House and Senate plots. Thus, for each chamber, the detrended shift in a given year is the size of the shift relative to the magnitude of shifts in the era in which it occurs.²¹ We would note, however, that a trend in the volatility of vote share is not nearly as clear as that for seat share, and so our normalization results are most important for seat share and less needed (or not needed at all) for vote share.

Time series plots of the detrended shifts for the House and Senate appear in Fig. 3. The stabilization of variability in shifts is visually clear when these plots are compared with those for the raw shifts in Fig. 1.

¹⁶ Norpoth and Rusk (2007: note 7) suggest a similar context-specific approach, but do not develop that idea in detail.

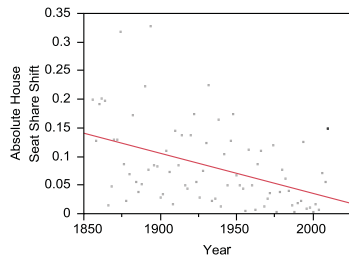
¹⁷ Carson et al. (2007) observe that more House contests were competitive in the latter half of the nineteenth century than there are today (they find that nearly 40 percent of House elections were decided by ten points or less during the period 1870–1900). Competitive elections would lead to a more volatile House and are likely one explanation for the greater variance observed for the partisan division during the earlier period. Whatever the reasons for volatility, a measure of criticality for elections relative to the era in which an election occurs appears useful.

¹⁸ To combine the House and the Senate we averaged the proportions across both chambers, weighted equally.

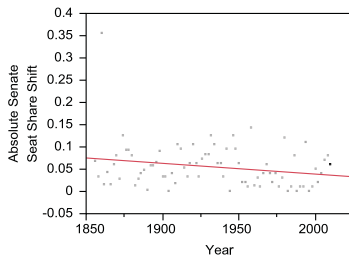
¹⁹ We would argue that it is very hard to have the dramatic level of change in congressional seat share that occurred in the latter part of the 19th century, since there are strong forces mitigating against major seat change, such as a decline in the number of marginal seats, as well as an increase in incumbency advantage, but 2010 demonstrates that sharp change is still possible.

²⁰ Investigation of quadratic regression found that the quadratic term was not significant for either House or Senate.

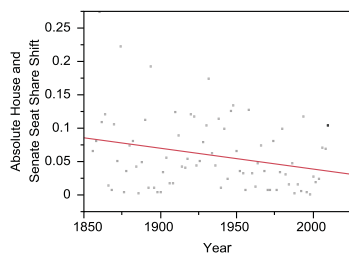
²¹ For presidential elections, detrending was not employed, as no statistically significant time-dependent trend was found.

A Bivariate Fit of House Absolute Seat Share Shift By Year

Linear Fit: House Absolute Seat Share Shift = $1.438 - 0.000701 \cdot \text{Year}$

B Bivariate Fit of Senate Absolute Seat Share Shift By Year

Linear Fit: Senate Absolute Seat Share Shift = $0.522 - 0.000242 \cdot \text{Year}$

C Bivariate Fit of House and Senate Absolute Seat Share Shift By Year

Linear Fit: House and Senate Absolute Seat Share Shift = $0.663 - 0.000312 \cdot \text{Year}$

Fig. 2. Analysis of time-dependent variability in shifts of seat share Proportions.

Normality tests and the multiples of the inter-quartile range by which each shift exceeded the first or third quartile are presented for detrended shifts in the last three columns of Table 1. Among detrended shifts for the House, normality is rejected by the Shapiro–Wilk test ($p = 0.02$), but not by the more focused binomial test for frequency of outliers ($p = 0.42$). There are two outliers (1994 and 2010) and 1894 almost meets the criterion.

For detrended Senate shifts, normality is marginally rejected by the Shapiro–Wilk test ($p = 0.09$) and not by the binomial test ($p = 0.42$), but 1860 is an outlier. Because the outliers that lead to rejection of normality in the separate chambers occur in different years, deviations for the House and Senate combined are dampened so that normality is not rejected by the Shapiro–Wilk test ($p = 0.49$) and only marginally rejected; ($p = 0.10$) by the binomial test. For the House and Senate combined, 1860 is an outlier and the other five elections listed come fairly close to being outliers.

4. Rethinking the evidence for critical elections

We are now in a position to revisit the question of which elections in the period 1854–2010 are critical among six possible suspects: 1860, 1874, 1894, 1932, 1994, and 2010.

4.1. 1860

Using either raw or detrended data, the election of 1860 does qualify as an outlier in terms of both magnitude (Table 1) and durability (Table 2).²² This may be consistent with what everyone seems to believe, but therefore also not surprising. The election of 1860, taken as the start of the Civil War Realignment, is always identified as a critical election in the realignment literature.

We would express a note of caution, however, about this conventional wisdom. What happened in 1860 was a cataclysmic institutional upheaval that clearly can be labeled a kind of realignment, that is, a realignment of a substantial portion of the electorate and its elected officers to another polity, the Confederacy. But the change in the composition of Congress was not a direct result of what happened at the ballot box, although the final trigger that led to secession and hence the loss of mostly Democrats in Congress was in part anticipation of the election of Lincoln. These Democratic losses were not matched by equivalent Republican gains.²³ Because of the qualitative nature of this upheaval, however, we cannot expect that the change in the composition of Congress was a faithful measure of partisan strength that can be compared numerically in a meaningful way with shifts in party strength recorded in other years. Furthermore, the fact that the Senate shift in 1860 is far more out of line than the House shift is an artifact. Senate shifts in other years – due to changes brought about at the ballot box – are muted because, unlike the House, only one third of Senators are chosen at one time. Hence the Senate shift in 1860 – which was driven by factors outside the election – stands out starkly in comparison with Senate shifts in other years in which only one third of the Senate was up for selection. In contrast, the shift in the House, is not that much greater than electoral shifts in other years.

The success in 1860 – at least in the Senate and the presidency – of the recently formed Republican Party initiated a period of general dominance of that party for the next seventy years. Still, the Democrats won the presidency in four of the eighteen elections in that period (and the popular vote for president in six), eleven of the 36 House elections, and four in the Senate (one additional election led to a tie). In fact, during the period of 1876–1892, the

²² Predicted values from the regressions used data from all elections in the study period to detrend shifts. If, however, the severe outlier in 1860 is omitted for the Senate, the downward slope is not significant ($p = 0.27$). If 1860 is omitted for the House and Senate combined, the downward shift is marginally significant ($p = 0.08$). Using the revised regression equations (based on omitting 1860) to detrend the data has only small effects on the detrended shifts; most shifts detrended without 1860 are slightly less extreme.

²³ In 1860 in the House the Democrats lost 47 seats and the Republicans – instead of gaining seats – actually lost five; in the Senate the Democrats lost 23 seats while the Republicans gained only eight (Rusk, 2001).

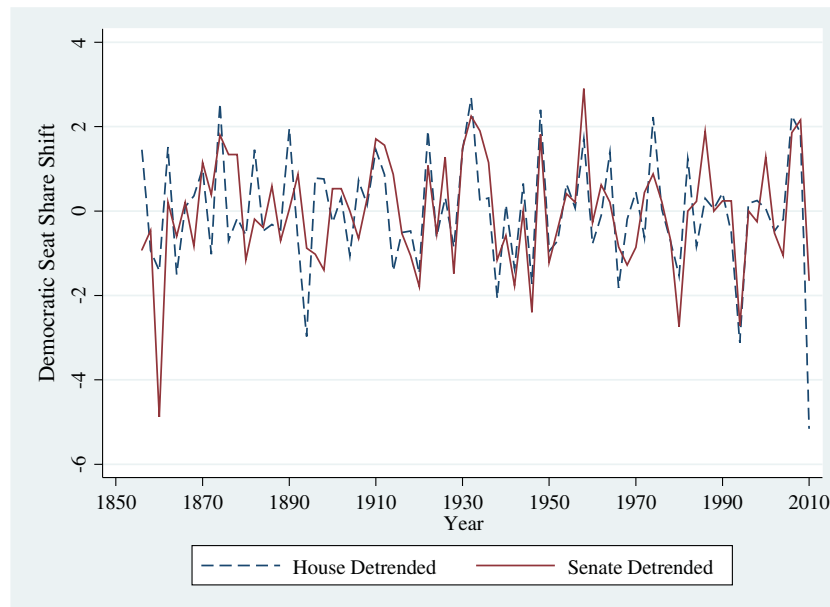


Fig. 3. Detrended Inter-election shifts in congressional seat Shares. NOTE: Shifts are detrended by dividing by the absolute shift predicted from the regression for any given date.

national partisan division was extremely close, with cleavages centered on issues of religion and ethnicity and on conflicts between industrial and agrarian interests, not to mention the continued sectional division between the parties (Brewer and Stonecash, 2009). But if ever there was an example of a build up of political tensions until they “escalate to a flash point” (in the words of Burnham, 1970), it was 1860. That said, it is difficult to disentangle the effects of the election of 1860 itself from the consequences of the Civil War that followed.

4.2. 1874 and 1894

Sharp electoral reversals occurred in the mid 1870s (in the direction of the Democrats) and the mid 1890s (in the direction of the Republicans), the latter precipitated by a severe economic downturn. That of the 1870s was also associated with the end of Reconstruction and the return of white ascendancy in the South under the banner of the Democratic Party, which solidified the solid South for that party for decades to come. But as we have seen, the period from the mid 1870s to the early 1890s was a time of close political division and electoral uncertainty with neither party gaining national dominance; it did not represent a time of stable realignment. The 1890s saw the rise of a populist revolt both in the South and in the West. The Democratic Party, beset with internal conflicts and the unpopularity of President Grover Cleveland and its Eastern establishment wing, put its eggs in the basket of populism and agrarianism at a time of rising urban populations – a strategy that led to the nomination, but defeat, in 1896 of William Jennings Bryan, permitting continued insulation of business elites from the have-nots (Burnham, 1967).

For both the House alone and the House and Senate combined (see Table 1), the magnitude of shifts for 1874 and 1894 are outliers when compared with shifts for the entire time period and both years satisfy the durability criterion as well for the Senate and for the House and Senate combined (see Table 2). With detrended data, however, we reach somewhat different results, since 1874 and 1894 are in an era when inter-election shifts were commonly quite large. Indeed, in the detrended data, neither of these 19th century shifts is classified as an outlier, although 1894 in the House just misses qualifying as such by the inner fence criterion and both 1874 and 1894 come moderately close for the House and Senate combined (see Table 1).

4.3. 1932

The rise to ascendancy of the New Deal Democrats in 1930–32 established the general dominance of the Democratic Party in Congress for the ensuing fifty years, with only two Congresses during that period going to the Republicans (and there was one tie in the Senate), although the latter party won four of the twelve presidential elections. The New Deal realignment was brought on by the sudden appearance of the nation’s deepest financial crisis – a shock that released a long term build up of political tensions while government had catered to the short term interests of business at the expense of industrial workers. The political reversal of the early 1930s inaugurated a majority coalition of the less affluent, organized labor, urban dwellers, Roman Catholics, white ethnics, Northern Blacks, women, and younger voters (Brewer and Stonecash, 2009) in support of the Democratic Party and set the stage for sweeping – and largely permanent – changes in

economic and social policy involving Social Security, unemployment insurance, financial regulation, and the right to collective bargaining.

The 1932 election gives rise to an important puzzle. If, as everyone apparently “knows”, this election was the start of the New Deal Realignment, why, as shown in Table 1 (with or without detrending), does it not meet the criteria for a critical election? Here there are two relevant observations. First and foremost, much of why we think of 1932 as a critical election is the level of support for Franklin Roosevelt for president. Nonetheless, using either raw or detrended data, the election of 1932 comes fairly close to meeting the criteria as an outlier for both the combined House and Senate and for the presidency and it meets the durability criterion in both cases. Second, however, even the major turnover of party dominance in the 1930s was initiated not all at once but rather over at least two and perhaps three congressional elections (certainly 1930 as well as 1932, and arguably 1934 as well).²⁴

4.4. 1994 and 2010

The Congressional election of 1994 completed the long secular movement of the South from solidly Democratic to solidly Republican as well as the consolidation of a new coalition of Republicans emphasizing white men, married persons, cultural and religious conservatives (see Abramowitz, 2010), and those who opposed the social and economic agenda of the New Deal and the Great Society. This new coalition established a decade and a half of Congressional dominance by the Republican Party. However, while 1994 could not have reversed Democratic dominance without the long secular time trend in Democratic loss in the South, it is also true that the Democratic losses that particular election year were spread throughout the country.

Even more than 1874 and 1894 – once we control for the magnitude of inter-election shifts in the period in which these elections occur – 1994, and especially 2010 now stand out for their notable shifts (outliers) in the detrended data, since the shifts occurring in these elections are large relative to other shifts in their era,²⁵ even though the shifts in 1994 and 2010 are modest in absolute terms when judged by the entire 150+ year time period. Of course, the durability of the reversal in 2010 of the short-lived Democratic ascendancy of 2006–2008 cannot yet be judged.

4.5. Thinking of critical elections not as a single event but as a succession of temporally contiguous shocks

The electoral reaction to the stock-market crash of 1929 in the off-year election of 1930, followed by the coming of the New Deal in 1932, suggest that a realignment need not

be completed in a single election but rather in a succession of two or more congressional elections, typically an off-year election followed by an election in a presidential year.

To investigate such a modification of the critical election concept, we paired each off-year election with the following presidential-year election. Under this approach, we find that – for raw data – two pairs of elections stand out: 1858–60, for which both the Senate and the House and Senate combined meet the inner-fence criterion, and 1930–32, for which the inner-fence criterion is met by the House and the combined House and Senate (see Table 3).²⁶ For detrended data, again 1858–60 and 1930–32 are the election pairs that stand out, although 1858–60 meets the inner-fence criterion only for the Senate and 1930–32 only for the House.

Table 3 evaluates *paired shifts*, i.e., combined shifts over two successive elections, that constitute outliers. We note that – whether raw or detrended data is employed – only two election pairs, those of 1858–60 and 1930–32, qualify for outlier status.

This is an important modification to the earlier literature on critical elections, which tended to look for a single cataclysmic shock to the structure of partisan dominance.

4.6. Shifts in the regional support bases of the parties

Like Norpoth and Rusk (2007), and the vast bulk of other authors studying realignment,²⁷ in the discussion above we have focused on elections that changed the national party balance, rather than dramatic shifts (or long term trends) that strengthened a party’s support in one geographical or demographic sector while, perhaps, weakening that party’s support in other sectors. We believe that this is a sensible strategy since, in Samuel Lubell’s apt metaphor (Lubell, 1952), it focuses on a central meaning of realignment as a change in which party is the “sun” and which the “moon” of national politics.²⁸

But we also recognize that such a national approach has important limitations. If we were to take change in the regional support bases of the political parties as our indicator of realignment, then the evidence for very long run (secular) realignment is overwhelming. In particular, over the past roughly 100 year time period, the South has shifted, at both the congressional and presidential level, from being the most Democratic of regions to being among the most Republican, and the New England (and Pacific Coast) states have moved in the opposite direction, with the bulk of this change in the post-WWII period. Because the regional realignment of most districts in the South from solidly Democratic to majority Republican has been largely balanced by movement in the opposite direction in the Northeast and Pacific Coast states, what we now find is

²⁴ Over these three elections, Democrats increased their seat share in the House by 35 percentage points and in the Senate by 31 points.

²⁵ Norpoth and Rusk (2007: 400) note “...a sharp decline in the variation of the aggregate House vote over time”, particularly in the half-century since 1950. Using a smaller variance for the House vote during this period, they identify 1994 as a significantly critical election.

²⁶ The only other paired elections for which two or more of the three measures – House, Senate, and combined House and Senate – deviate even one interquartile range from the nearest quartile are 1874–76 and 1910–12.

²⁷ See, however, esp. Petrocik (1981) and Nardulli (1995) for a geographically contextualized approach.

²⁸ Scholars who take this perspective use changes in party vote or seat shares as the main signal of realignment.

Table 3
Identification of major paired shifts.

Year	Deviations in interquartile units					
	Raw shifts			Detrended shifts		
	House	Senate	H&S	House	Senate	H&S
1858–60	–1.26	–2.74†	–1.99†	–0.83	–1.68†	–1.37
1874–76	0.86	1.39	1.16	0.57	0.93	0.80
1894–96	–0.84	–0.48	–0.76	–0.70	–0.19	–0.57
1910–12	0.83	1.23	1.07	0.75	0.96	0.88
1930–32	1.50†	1.32	1.53†	1.61†	1.13	1.42

NOTES: Data is based on Congressional seat shares for 1854–2010. Values in the table in election-year rows are the quantities (in units of the inter-quartile range) by which the inter-election shift fell below the first or above the third quartile. The symbol (†) indicates that the inter-election shift in that year is an *outlier*, beyond the “inner fence”, i.e., beyond either the first or third quartile by more than 1.5 times the inter-quartile range. The symbol (††) indicates that the shift in that year is a *severe outlier*, beyond the “outer fence”, i.e., beyond either the first or third quartile by more than 3 times the inter-quartile range. We use outlier status as an indicator of “major” shifts.

a pattern of competition that appears highly competitive at the national level, but which actually is quite noncompetitive in some regions. In particular, in both the South and the Northeast there has been a clear change in which party is the regional sun and which is the regional moon.²⁹ However, this change has by and large not occurred in terms of critical elections, but as a result of a long-term pattern of secular change, with some short-term ups and downs.

5. Discussion

We see the main methodological and theoretical contributions of this essay as fourfold: (1) to offer a new way to think about the durability of electoral change, (2) to suggest a statistical framework to jointly examine the existence of critical elections in terms of both magnitude and durability of change, (3) to show how to view criticality in contextualized terms that recognize the time trend in inter-election seat volatility over the course of our nation's history, and (4) to suggest a possible reconceptualization of critical elections not as single elections but as pairs of adjacent elections responding to similar forces. Using a statistical test that assesses the joint frequency of the dramatic and durable nature of shifts, we conclude that, since the 1850s, there have been more sharp, sustained changes in partisan strength in the U.S. Congress than might be expected due to chance, but only barely more.

Our data analyses suggest that there have been six instances since the current two-party system was established in America in the 1850s for which there is at least marginal or provisional statistical evidence of a critical realignment, but none that clearly and robustly meet all the criteria. The year 1860 at the beginning of the Civil War represented a sharp and durable shift – at least in the Senate – either compared to the entire time period or to its

²⁹ For a more nuanced description of regional time trends see Nardulli (1995).

own era. Yet, as we have noted, the partisan shift was only partly due to the election itself. The shift at the beginning of the New Deal was strong and durable, but clearly met the statistical criteria only if two or more elections are combined, such as those of 1930–32.

While there is also evidence for 1874 and 1894 each being treated as dramatic shifts, they come up a bit short when we consider them relative to the typical large size of seat-share shifts around that time period. The durability of these two shifts is, however, unmatched in the period under study. On the other hand, 1994 (and possibly 2010 as well, though the evidence on durability is, of course, still missing) does appear to be a critical election if we judge its seat shift not with respect to the overall time series average, but with respect to the magnitude of inter-election swings typical in more recent decades.

Still, if we think about critical election eras not in terms of a single election but rather in terms of pairs of election, 1858–60 and 1930–32 stand out from all other pairs. These pairs of elections are critical both with respect to statistical tests and in common sense terms of having electoral tides that we can link to dramatic shocks to the political system. This finding about paired shifts is robust to a reexamination of elections in terms of detrended data that allows us to control for differences in the mean level of inter-election seat volatility across different time periods.³⁰

These two critical elections/election pairs have been seminal in shaping the structure of American political competition – e.g., the regional alignment in party strength triggered by the Civil War and an urban-rural split facilitated by New Deal programs. However, secular trends in both party strength and political attitudes have operated to undermine many electoral features of these critical elections, such as the “Solid South” of the old Democratic party.³¹ Relatedly, although there certainly are long lasting and transformative policy changes tied to post-Civil War realignment and the New Deal realignment – e.g., the Civil War Amendments and the New Deal social legislation – secular changes have been operating to erode each. For example, by 1900, Jim Crow had become a new form of racial oppression in the South that lasted more than sixty years. Similarly, the New Deal and its Great Society successor have been steadily, although still only very partially, eroded by political developments. Beginning in the 1940s, intellectual foundations of a conservative movement set the stage for opposition to an activist government and the development of issues that would draw voters to a new conservative coalition – an effort that finally after several decades attained the electoral strength to win both the presidency and the Senate in 1980 and to

³⁰ It is important to emphasize that we are not trying to “salvage critical realignment theory” a la Burnham. When looking for evidence for critical elections combining the criteria for sharp and durable changes most of the singleton elections we examine just barely meet tests for statistical significance.

³¹ Democratic Party strength in Congress – following a rapid rise in 1930–32 – reached its electoral peak in 1936 then tailed off somewhat through the 1950s, only to return beginning in 1958 and peak again in mid 1960s.

institute a new and conservative agenda.³² Moreover, changes in policy direction can occur not just due to critical elections but also due to long lasting secular shifts in partisan strength, especially when they reach a threshold that allows for change in partisan control of the legislature (as in 1994).³³

Given our findings that critical elections are relatively rare, that some elections traditionally classified as critical are only “barely” such, and that both partisan and policy effects of critical elections can be at least partially eroded or reversed by secular change even for cataclysmic changes such as those triggered by the Civil War and the Great Depression, one might ask “Why should we care about classifying elections as critical or not?” Our answer is two-fold. First, one often hears from leaders of the U.S. Congress or the President about how we ought to interpret election outcomes, especially those in which one party does very well. Claims of “mandates for change” are quite typical, and so the question of how to distinguish “ordinary” shifts from extraordinary and durable political tsunamis seems very much worth asking. Second, while secular trends may lead to change in partisan control and to policy reversals, these may not be as durable as what we see following truly critical elections, because secular time trends historically have not generated the same level of durable reversed (or enhanced) partisan dominance as we saw (artificially) in 1860 and again after 1932. Thus, “undoing” a critical election (and reversing/modifying) its policy impacts is likely to take time.

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Appendix

Figure A.1 presents histograms, box plots, and normal quartile plots for Congressional seat share shifts and presidential vote share shifts.

³² In the words of Brewer and Stonecash (2009: 104), “[This effort] is ... a perfect example of the model of gradual realignment as a party searches for a majority”.

³³ Overall, secular realignment has involved ebbing and flowing, not random noise. Merrill et al. (2008) argue that roughly cyclical behavior is to be expected due to the gradual effects of voter reaction against the party in power (see also Stimson, 2004).

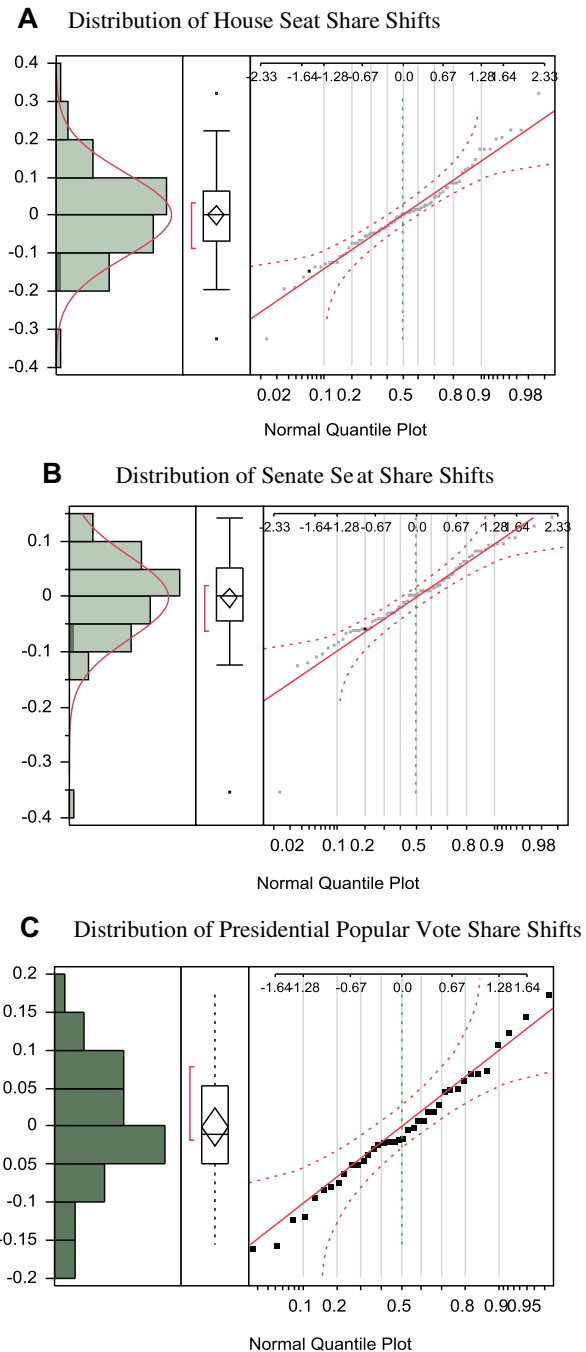


Fig. A1. Distributional Analysis for Shifts in Seat/Vote Shares. A. Distribution of House Seat Share Shifts. B. Distribution of Senate Seat Share Shifts. C. Distribution of Presidential Popular Vote Share Shifts. NOTE: The figures above show histograms, box plots (horizontal lines in the box represent the quartiles while the dotted lines extend to the most extreme data values within the inner fences), and normal quantile plots (which plot the observed order statistics versus order statistics expected for a normal distribution with the same mean and variance). Outliers are apparent as isolated dots beyond the inner fences and as points significantly off the diagonal line depicted in the quantile plot.

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