

Myopic Retrospection and Party Realignment in the Great Depression

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Abstract

We have argued elsewhere that retrospective voting is often causally unsophisticated, ideologically confused, and highly myopic (Achen and Bartels 2002; 2004). Here, we extend those assertions to party realignments, arguing that they, too, depend far less on ideological shifts than on the simple cumulation of myopic retrospections in election years. We examine voters' responses to the most far-reaching economic disaster in the history of democratic politics, the Great Depression. In contrast to Key (1958) and others who have interpreted the New Deal realignment in the U.S. as "a popular ratification of the broad features of new public policy," we show that Democratic gains in the 1930s were based primarily on short-term income gains and losses, which cumulated willy-nilly into a durable Democratic majority in the electorate.

Introduction¹

Americans are accustomed to thinking of the New Deal realignment as a triumph of both democratic responsiveness and Democratic ideology. In the face of an unprecedented economic catastrophe, a rigidly conservative government resisted public pressure to provide energetic relief and institutional reforms. Voters responded with a historic repudiation of the incumbent president, Herbert Hoover, in 1932. Franklin Roosevelt swept into office with 57 percent of the popular vote, and the Democrats—a minority party for most of the preceding 70 years—won 313 of the 435 seats in the House of Representatives.

Roosevelt's first hundred days in the White House brought a flurry of innovative policies. A robust economic recovery followed in short order. Real per capita income increased by one-third between 1933 and 1936 and unemployment declined by one-third. Voters rewarded Roosevelt with a landslide reelection in 1936; he won more than 60 percent of the popular vote and carried 46 of 48 states. He went on to win an unprecedented four terms in the White House, and the Democratic Party enjoyed a durable reservoir of popular support that allowed it to dominate congressional elections for most of the next 60 years. As V. O. Key, Jr. (1958, 589) summarized these events, "The election of 1936 ratified a sharp turn in public policy and successive Democratic victories clinched the reforms of the New Deal."

Our aim in this paper is to challenge this conventional interpretation of the New Deal era. We do so by analyzing American voters' reactions to the Depression, using aggregate electoral and economic data to document the importance of myopic economic retrospections in accounting for the Democratic Party's success in presidential and congressional elections throughout the 1930s, and its continuing advantage in partisan identification in subsequent years. Thus, what looks to the eye like a triumph of both democratic responsiveness and Democratic ideology may instead be an illusion produced by a specific configuration of election dates, partisan alterations, and economic vicissitudes in a world where policies are, in fact, largely irrelevant and voters are blindly and myopically retrospective.

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The New Deal Realignment

For their part, academics have tended to dismiss campaign slogans of the past like ‘the full dinner pail’ and ‘a chicken in every pot’ on the grounds that something deeper must have been going on in these elections. But perhaps it wasn’t.

– David R. Mayhew (2002, 161)

The conventional account of FDR’s triumph in 1932 is that it represented a protest vote, a cry for help, with the electorate taking a chance on a largely unknown and cautious moderate whose principal recommendation was that he was not Herbert Hoover. As Key (1947, 268) put it,

The campaign gave to the public no clear-cut alternatives of policy, except with respect to prohibition. The Democrats were thoroughly wet. But no other issue of a major nature presented itself sharply and dramatically in the campaign. The times called for a great debate on measures to lift the American economy out of the morass, but a stranger might have presumed that all the fighting was about when and whether one could get a glass of legal beer. . . . [I]t is doubtful that the rational appeals of either candidate had much to do with the election results. All types and classes of people had suffered deprivations; all of them were anxious for a change. Poor men, rich men, middle-class men, farmers, workers, all moved over into the Democratic ranks in sufficient number to give Roosevelt a resounding victory. All these classes could identify themselves with the ‘forgotten man,’ and they could equally feel themselves deserving of a ‘new deal’ without necessarily insisting on exactitude in the definition of what the ‘new deal’ was to be.²

By 1936, however, the character of Roosevelt’s administration had become much clearer, not least to himself, and he campaigned well to the left of where he had stood in 1932. In his famous speech at Madison Square Garden two nights before the election, he attacked “organized money” for their hatred of him, and proclaimed to a thunderous ovation, “I welcome their hatred.” A new political barometer, the Gallup poll, found a striking degree of partisan polarization in the general public: 45% of the poll respondents, and 83% of

²Lest Key’s emphasis on the political significance of repealing Prohibition strike modern readers as exaggerated, we note that a highly laudatory account of Roosevelt’s campaign published in the early months of the new administration (Guilfoyle 1933, 218-219) suggested that the “return of beer in less than a month after the new Administration took office did more than anything else to inspire the people with confidence in the President. . . . Seldom, if ever before in the history of the country, has there been such a major accomplishment in such a short time. . . . If there was any turning point in the attitude of the people toward this depression it came simultaneously with beer.”

Republicans, agreed that “the acts and policies of the Roosevelt Administration may lead to dictatorship” (Key 1961, 246).

Despite the breadth and intensity of opposition to Roosevelt, the election result was a historic landslide for the incumbent. The voters joining in that landslide are said to have been “attracted by the Democratic program and the Rooseveltian personality and leadership” (Sundquist 1983, 214). Even the authors of *The American Voter*, no friends to intellectualist interpretations of elections, used virtually identical language, writing that “The program of welfare legislation of the New Deal and the extraordinary personality of its major exponent, Franklin D. Roosevelt, brought about a profound realignment of party strength, which has endured in large part up to the present time” (Campbell et al. 1960, 534).

Key, who had disparaged the importance of policy issues in the 1932 election, interpreted the 1936 election in a very different light. “The return of a party to power under circumstances [like those] of the 1936 campaign,” he wrote (Key 1958, 578-579),³

gives such an election a special significance. Drastic innovations in public policy aroused the most bitter denunciation by the outs; the ins had to stand on their record. The electorate had before it the question whether to ratify these innovations, few of which had been clearly foreshadowed in the 1932 campaign. The result could only be interpreted as a popular ratification of the broad features of new public policy.

This interpretation of the 1936 election has persisted down to the present, making it a textbook example of a policy-based realigning election. For example, Hershey (2005, 294) wrote that

At critical times in American history, the parties have divided in ways that were, if not truly ideological, at least determinedly policy oriented. In the 1936 presidential election, for example, the Democrats and the Republicans offered dramatically different solutions to a nation devastated by the Great Depression. The hardships of that economic collapse probably focused voter attention to an unusual degree on the possible remedies that government could provide. This, combined with a campaign centered on the pros and cons of the Roosevelt program for social and economic change, may well have produced something close to a mandate in the election for both the president and Congress.

³The interpolated words are from a subsequent (1958) edition of Key’s textbook, which repeats the quoted passage with only minor alteration.

Similarly, in a book-length study of party identification, Green, Palmquist, and Schickler (2002, 106-107) emphasized the importance of enduring social identities in the development and maintenance of partisan attachments but cited the New Deal as a salient exception:

Although we are in general skeptical of ideology-based explanations of party identification, the New Deal represents an instance in which such explanations work. Here was an unusually clear ideological divide between the parties, dramatized again and again as the Republicans denounced relief programs enacted by the Democratic executive and legislature. In the formative moment when the new party system emerged, and issues such as the scope of government replaced the tariff, it is quite possible that ideological affinity shaped party attachments to an unusual extent.

The left panel of Figure 1 charts the course of the Great Depression in the U.S. as measured by changes from year to year in real personal income per capita.⁴ The right panel charts electoral support for the incumbent president's party (Republicans from 1928 through 1932, Democrats from 1932 through 1940) in presidential and congressional elections.⁵

*** Figure 1 ***

It should be evident that there is a good deal of correspondence between the economic and electoral patterns in Figure 1. In 1930, the first year of widespread economic distress, the Republican Party lost 3.6% of the two-party House vote (and 52 seats, plus 8 in the Senate) in a midterm election. Two more years of accelerating depression triggered a thoroughgoing repudiation of Hoover and the Republicans in 1932. Roosevelt and the Democrats took power in early 1933, at what turned out to be almost precisely the low point of the Depression. Three years of steady improvement saw real incomes return almost to their 1929 level in 1936; the Democrats made modest gains in the midterm election of 1934 and again in the presidential and congressional elections of 1936, adding a total of 2.6% and 3.2%, respectively, to their 1932 shares of the two-party House and presidential votes. The economy continued to improve in 1937 but took a marked turn for the worse in 1938, with incomes falling below their 1936 level and unemployment rising back to 19 percent; the Democrats lost 7.3% of the two-party House vote (and 73 seats, plus 6 in the

⁴Data on real per capita personal income are from Table 7.1 of the National Income and Product Accounts available from the website of the Bureau of Economic Analysis, U.S. Department of Commerce (<http://www.bea.doc.gov/bea/dn/nipaweb>). Unemployment figures tell much the same story as the real income figures, except that unemployment remained well above its pre-Depression level throughout the 1930s. According to the United Nations Statistical Yearbook (Statistical Office of the United Nations 1949), the unemployment rate increased from 3.2% in 1929 to 23.6% in 1932, peaked at 24.9% in 1933, declined to 14.3% in 1937 before spiking at 19.0% in 1938, then declined back to 14.6% by 1940.

⁵The popular vote shares shown in the right panel of Figure 1 are taken from Rusk (2001), as are the vote and seat shifts reported in the text.

Senate), leaving them well behind where they had been in 1932. In 1939 and 1940 the recovery resumed, and the Democrats regained some of their lost ground, but they were still less popular than they had been in 1932—and less popular than the Republicans had been in 1928.

The conventional, ideological interpretation of the voting patterns in the right panel of Figure 1 is that voters punished Hoover for his conservative ideological orthodoxy in 1930 and 1932, rewarded Roosevelt for adopting more appropriate, progressive policies in the early years of the New Deal, and tapped the ideological brakes in 1938 when Roosevelt’s court-packing scheme and the “second New Deal” raised concerns that policy might be drifting too far to the left.⁶ (“By 1940,” it is said—somewhat arbitrarily—“the New Deal had run its course” (Key 1958, 209).)

Although this period predates the survey-based measure of “Policy Mood” developed by Stimson (1991), it is not hard to imagine shifts in public opinion that were smoothly responsive to the policies adopted in Washington, and that led in turn to both the electoral shifts evident in Figure 1 and short-term policy adjustments by the Democrats during the course of the New Deal. This is the logic of “dynamic representation” outlined by Erikson, MacKuen and Stimson in *The Macro Polity* (2002), which seems to provide a general theoretical framework quite consistent with the historical accounts of Key, Sundquist, and other analysts of the New Deal realignment.

Our alternative interpretation is that the correspondence between income changes and electoral shifts in Figure 1 can be accounted for in the simpler terms suggested by Mayhew (2002, 161): when voters got a chicken in every pot at election time they liked the incumbent party’s ideology just fine, whatever it happened to be; but when incomes eroded and unemployment escalated they became ripe for defection to anyone who would promise to make things better.

The authors of *The Macro Polity* allowed in principle for the possibility that “open-minded voters” might “say yes to whatever works,” so that “the degree of prosperity and well-being influences ideological choices by ordinary voters, depending on who is in power and how the country is going” (Erikson, MacKuen, and Stimson 2002, 440). However, they reported finding “little in the way of convincing statistical support” for this sort of interaction between economic conditions and liberal or conservative policy moods in the post-war period (Erikson, MacKuen, and Stimson 2002, 441).

Our own analysis of economic voting in the post-war period (Achen and Bartels 2002, 2004) suggests that economic conditions have a substantial direct effect on election outcomes—but only economic conditions at the time of the election. Of course, one might expect that

⁶For example, Sundquist (1983, 226) supposed that “independent voters were by now rebelling against Democratic excesses and swinging to a Republican party that in many states had acquired new progressive leadership, and deviant Republicans, having chastised their party sufficiently, were returning home.”

the Depression would be different. The depth of the crisis may have focused voters' minds. Lost jobs and lost homes, hungry children and ruined lives should not have been forgotten quickly. By the same token, the continuing intense debate in the country over Roosevelt and his program might have allowed voters to see the connection between their circumstances and political decisions in Washington, making them more ideological than usual as well as less myopic than usual.

The elections of the Depression era were conducted in dramatic economic circumstances, with states undergoing very large gains and losses in real income over the period. In 1936, for example, real personal income per capita increased by 19% in Colorado, 20% in Delaware, and 24% in Nevada.⁷ At the same time, real income plunged by 16% in North Dakota and by 25% in South Dakota. Figures for some of the other Depression years are even more variable. Thus there is no statistical difficulty in assessing the impact of state-level economic conditions on the vote. That is why this period provides such fertile ground for statistical analysis of retrospective voting.

In previous work (Achen and Bartels 2005), we found that voters' reactions to Roosevelt's first term rested primarily on simple short-term retrospection, just as they have in the postwar period. There was a strong and fairly consistent tendency for Roosevelt to gain support in states that experienced significant income growth in 1936, and to lose support in states that experienced declines in real income. This support was myopic. In 1934 and 1935, Roosevelt presided over an increase in real personal income per capita of more than 17 percentage points, recouping half of the total income lost through the preceding four years of depression. Our analysis suggests that he got little or no electoral credit for doing so. Rather, 1936 voters asked "What have you done for us lately?" They voted on that basis. We found similar results in studying congressional elections.

We also compared Roosevelt's actual performance in the 1936 election with his hypothetical performance under the economic conditions prevailing in 1938, suggesting that under the economic conditions prevailing in 1938 Roosevelt would have received only about 48 percent of the two-party vote outside the South in 1936. More importantly, he would have lost 17 of the 46 states he actually carried, including New York, Pennsylvania, Illinois, Ohio, and Michigan. Even with a lock on the Solid South, he would have fallen just short of an Electoral College majority, bringing the New Deal realignment to an abrupt and (from

⁷All of our state-level data on real per capita income are from "State Personal Income 1929-2000," a CD-ROM issued by the Bureau of Economic Analysis, U.S. Department of Commerce, Washington, DC, November, 2001. We converted the published figures to 1929 dollars using the ratio of current to constant dollars in the BEA's Disposable Personal Income series in Table 7.1 of the National Income and Product Accounts (<http://www.bea.doc.gov/bea/dn/nipaweb>). We define real income change in each year as the difference in logged real income levels. Taking natural logs converts gains and losses to an equivalent scale, so that they count equally (and are more likely to satisfy the usual assumptions underlying regression analysis.) To a good approximation when changes are not large, the differences in the logs represent fractional changes. We multiply them by 100 to express them as percentage changes.

the perspective of hindsight) very premature conclusion. These results provide a dramatic indication of voters' myopia, even in a situation where the economic stakes were much larger than any observed in subsequent eras of American politics.

We reiterate how dramatically our interpretation of the elections of the 1930s changes the conventional understanding of the realignment. In our interpretation, the voters made no judgment about the ideological appropriateness of New Deal policies. Nor did they provide any cumulative assessment of the economic performance of the Roosevelt administration. Their reactions were decidedly myopic. In 1936, for example, they cared only about 1936 conditions; the substantial gains in real income in the preceding two years were water under the bridge. Roosevelt's reelection—and the realignment—depended solely on that one year, and if 1936 conditions had approximated those prevailing in 1938 he probably would have been defeated. Judgments about the role of the government in economic life, the value of laissez-faire economics, or specific aspects of the New Deal program were irrelevant—or, at least, unnecessary to account for the outcome.

The sequence of election outcomes in this period manifested a sharp structural break, followed by a good deal of persistence—the realignment. We argued that the new partisan balance stemmed in significant part from the extent to which voters developed partisan attachments consistent with their myopic short-term assessments of economic conditions in each successive election year. The fact that times were good in 1936 had a significant impact on the 1938 congressional vote because much of the heightened Democratic support stemming from good times in 1936 carried over to 1938. Of course, the fact that times were also good in 1935 had no such effect, since 1935 was not an election year and thus was not incorporated in voters' party identifications or voting behavior in 1936 or, as best we can tell, thereafter. *The great partisan realignment of this period was largely due to a cumulation of myopic retrospections.*

This finding, so at odds with contemporary understandings of the great partisan realignment of the Thirties, is consistent with the election returns in other parts of the world during this period, in which incumbent governments of widely varying ideological coloration were all driven willy-nilly from office. The new incumbents often became long-term majority or plurality parties, just as our argument would suggest. However, in the previous paper, the historical evidence was merely suggestion, and the supporting statistical analysis was given only modest theoretical underpinning. In this paper, therefore, we hope to fill in the blanks.

We begin by asking how one might verify that short-term retrospections have been incorporated into long-term party ID. For example, just because economic retrospections influence the current vote, and just because current votes change in a semi-permanent way to create a realignment, that does not prove that economic retrospections were responsible

for the realignment. How should one proceed?

Should partisanship be controlled? What if the data consist only of votes? How do votes relate to partisanship? Should some measure of partisanship be lagged, or should differences be used? Should only current short-term forces be controlled, or do we need to control for past ones as well? This collection of inferential problems is subtle enough that even Harold Gosnell, the giant for whom the annual Society for Political Methodology prize is named, vacillated about how to proceed (Gosnell & Gill 1935; Gosnell & Pearson 1939). For such problems, garbage-can regressions are unlikely to be either successful or persuasive. The next three sections build on Gosnell's ideas, especially their further development in Gosnell & Coleman (1940), to argue for a particular statistical alternative. Our derivation goes on a bit and may strike some as using a blunderbuss to kill a fly. But in our view, previous efforts have too often used a flyswatter to hunt this quarry, with predictable consequences when the tiger arrived.

Deriving the Statistical Model

In this discrete-time model of voter decisionmaking, a time period is a two- or four-year interval. Within each period, a campaign first takes place, during which the voter receives some new information about the next period from the campaign. She then votes. In the second part of the period, the winning party produces her party-differential benefit, which she observes. Then the next campaign begins.

Consider first a two-party system between realignments. Then during each presidential term t , voter i receives a payoff u_{it} , the utility difference between the first and second parties. Assume that

$$u_{it} = \delta_i + a_{it} \tag{1}$$

where δ_i is the mean over-time difference in party benefits for voter i , and a_{it} (for “administration”) is the deviation from that mean in presidential term t . Denoting the expectation over time for individual i by $E_i(\cdot)$, we assume $E_i(a_{it}) = 0$. That is, payoffs to voters across presidential terms deviate randomly around a constant mean. For present purposes, this assumption defines “a period between realignments.”

Next define “the party identification of individual i at time t ” as the expected future utility difference between the parties conditional on information known at the time of the campaign in period t . That is, party identification (PID) is a rational expectation—a best estimate of δ_i —based on experience with the parties in presidential terms prior to period t . Denote this optimal estimate by $\hat{\delta}_{it}$.

It should be stressed that “party identification” here is not an answer to a survey question nor a feeling of emotional attachment. Instead, it is a behavioral disposition,

a tendency to make certain choices. Party registration might tap it, as might survey questions, but actual votes are the best indicator. It is *behavior* that is fundamental to the definition. Other measures are subject to observational error.

During the campaign at period t , new information c_{it} arrives about the administration effect a_{it} that will be observed later in the period.⁸ This information is defined as an innovation or “news”: It is the part of next period’s payoff that is not the voter’s customary payoff from her partisanship.⁹ Thus c_{it} may consist of reactions to the current candidates’ personalities and demographic types, somewhat credible campaign promises different from customary party positions, and so on. The information is treated as a rational expectation and thus arrives with zero-mean, uncorrelated with a_{it} , additive noise e_{it} . That is, perceptual errors during the campaign are uncorrelated with true benefits in the coming administration¹⁰:

$$c_{it} = a_{it} + e_{it} \tag{2}$$

where e_{it} has error variance τ^2 , assumed constant across individuals.¹¹ During the campaign, neither a_{it} nor e_{it} is known. The voter observes only c_{it} .

For the over-time behavior of the components of the random variable c_{it} , assume a bivariate normal distribution for analytic convenience, with known mean vector and covariance matrix Σ :

$$\begin{bmatrix} a_{it} \\ e_{it} \end{bmatrix} \sim N(0, \Sigma) \tag{3}$$

and

$$\Sigma = \begin{bmatrix} \omega^2 & 0 \\ 0 & \tau^2 \end{bmatrix} \tag{4}$$

Note that for simplicity, the variance matrix Σ is taken to be common to all voters, meaning that party benefits and campaign information vary over time to the same degree for all.

Next, again for simplicity, we take a_{it} and e_{it} to be uncorrelated over time for each

⁸It is probably more plausible to assume that c_{it} is an estimate of $u_{it} = \delta_i + a_{it}$, as in Achen (1992). That is, campaign information can be used to update both the next period’s deviation a_{it} and true PID δ_i , an effect seen in survey data. However, in aggregate data, no such updating is observable, and assuming it away simplifies the mathematics with no real loss.

⁹Of course, the parties may have to work hard to provide the customary payoffs each period. But because they are customary, they are rationally expected, and thus are included in the partisanship estimate $\hat{\delta}_{it}$.

¹⁰That is, if a_{it} does not have zero mean, then administration effects consistently favor one party, and the voter should adjust her PID. Similarly, if e_{it} does not have zero mean, then she should adjust her prediction of the next period’s administration effect.

¹¹Of course, τ^2 varies across individuals in practice, but this variation is not observable in aggregate data and thus is ignored here for simplicity.

individual i . Thus for $s \neq t$:

$$E \begin{bmatrix} a_{it} \\ c_{it} \end{bmatrix} \begin{bmatrix} a_{is} \\ c_{is} \end{bmatrix}^T = \begin{bmatrix} 0 & 0 \\ 0 & 0 \end{bmatrix} \quad (5)$$

Again, this assumption is known to the voter.

Under these assumptions, consider first the voter's problem of estimating her PID δ_i during the campaign at time T . For this purpose, the assumptions imply that past and current c_{it} are useless: They reflect only deviations from PID. Furthermore, u_{iT} has not yet been observed. Hence the only relevant information is previous u_{it} ($t = 1, \dots, T - 1$). But these are independent draws from a univariate normal distribution whose mean is the voter's PID δ_i and whose known variance is ω^2 . If the prior is uniform for δ_i , then by the usual Bayesian updating formula for the estimation of a mean given independent normally distributed observations with known variances¹², the voter's best estimate of her PID at time T is:

$$\hat{\delta}_{iT} = \frac{1}{T-1} \sum_{t=1}^{T-1} u_{it} \quad (6)$$

Thus the estimated PID is just a sum of prior experiences with the parties, a "running tally." It will be helpful below to express this simple result as an updating formula from the PID at the previous period:

$$\begin{aligned} \hat{\delta}_{iT} &= \frac{h_{1T}\hat{\delta}_{i,T-1} + h_{2T}u_{i,T-1}}{h_{1t} + h_{2t}} \\ &= \frac{(T-2)\hat{\delta}_{i,T-1} + u_{i,T-1}}{T-1} \end{aligned} \quad (7)$$

where $h_{1T} = (T-2)/\omega^2$ and $h_{2T} = 1/\omega^2$, the inverses of the variances around δ_i .

To vote, the citizen wishes to estimate the expected value of the candidate difference she will experience this period, $u_{iT} = \delta_i + a_{iT}$. With uniform priors for δ_i and a_{iT} , and with $\hat{\delta}_{i,T}$ and c_{iT} independent random variables with means δ_i and a_{iT} respectively, the best estimate is simply:

$$\hat{u}_{iT} = \hat{\delta}_{iT} + c_{iT} \quad (8)$$

That is, the voter's utility of voting for Party 1 is the sum of her PID plus the short-term forces, in parallel with the usual working assumption in voting studies. The latter two quantities are known to the voter, but not to the analyst. Note that the differing quality of

¹² Alternately, a conjugate normal prior for δ_i , representing parental socialization, might be added, as in Achen (2002). In that case, one may regard the number of prior periods $T-1$ as including a certain number of periods due to socialization rather than personal experience, as in the usual Bayesian normal distribution updating formulas.

the PID estimates for young and older voters make no difference; under these assumptions, everyone just adds.

In the usual survey sample case, the analyst knows exogenous covariates X_{iT} that influence the short-term force c_{iT} and has a measure of PID $\hat{\delta}_{i,T}$ from the survey responses (assumed uncontaminated by the short-term forces). For example, it is often assumed that $c_{iT} = X_{iT}\beta_{1T} + \epsilon_{it}$, where β_{1T} is an unknown coefficient vector specific to time T , and ϵ_{it} is a normally distributed, mean-zero disturbance term independent of X_{iT} , also unknown to the analyst (though not to the voter).¹³

If we make the customary assumption that the voter chooses Party 1 at time T if $\hat{u}_{iT} \geq 0$, while otherwise she votes for Party 2, then under these conditions, Equation (8) with c_{iT} expressed as a function of its covariates has the form of a probit equation linear in $\hat{\delta}_{iT}$ and X_{iT} . That is, denoting the vote choice by v_{it} , with $v_{it} = 1$ if the voter chooses Party 1, and $v_{it} = 0$ otherwise, then:

$$\Pr(v_{it} = 1 | \hat{\delta}_{iT}, X_{iT}) = \Phi(\hat{\delta}_{iT} + X_{iT}\beta_{1T}) \quad (9)$$

where $\Phi(\cdot)$ is the standard normal (Gaussian) cdf.

Estimation with Aggregate Data

Now move to the aggregate level. Assume that within each geographic unit (“district”) under study, $\hat{\delta}_{it}$ and X_{it} are multivariate normally distributed across individuals at each time period t . This is an assumption about cross-sections, and thus distinct from the over-time assumptions for each individual already made. Obviously, it can only be an approximation for the arbitrarily distributed covariates used in practice.

The next step is to integrate (take the average) over $\hat{\delta}_{iT}$ and X_{iT} within each district to derive the aggregate relationship. (In what follows, bar superscripts are used to denote simple arithmetic means, and asterisk superscripts to denote aggregate coefficients and random variables.) If the cross-sectional normality assumptions hold, then we conjecture that the observed mean vote in each district is a linear probit function of the mean PID and the mean new information. That is, if let v_{jT} be the vote for Party 1 at time T and v_{jT}^* its transformation to a probit scale, so that:

$$v_{jT}^* = \Phi^{-1}(v_{jT}) \quad (10)$$

¹³The assumption that c_{iT} has over-time mean zero implies that the covariate vector X_{iT} should be scaled to have over-time mean zero as well. In practice, the scaling constants are unknown, and so it is assumed here and in all other similar regression equations throughout this appendix that a constant term is included among the covariates to mop up scaling differences.

with $\Phi^{-1}(\cdot)$ the inverse cdf of the standard normal distribution. Then the conjecture is that, if $\phi(\hat{\delta}_{iT}, X_{iT})$ is the multivariate normal joint distribution of $\hat{\delta}_{iT}$ and X_{iT} , then¹⁴:

$$\begin{aligned} E(v_{jT}^*) &= \int \int \Phi(\hat{\delta}_{iT} + X_{iT}\beta_{1T}^*) \phi(\hat{\delta}_{iT}, X_{iT}) d\hat{\delta}_{iT} dX_{iT} \\ &= \frac{\bar{\delta}_{jT} + \bar{X}_{jT}\beta_{1T}^*}{q_T} \end{aligned} \quad (11)$$

where $\bar{\delta}_{jT}$ and \bar{X}_{jT} are the within-district means of $\hat{\delta}_{iT}$ and X_{iT} , and $q_T = [1 + \text{var}(\hat{\delta}_{iT} + X_{iT}\beta_{1T}^*)]^{1/2}$, with the latter variance computed cross-sectionally within each district and assumed constant across them. Equation (11), if correct, is a small extension of the standard result when only one variable is aggregated within a probit setup. For example, if the $X_{iT}\beta_{1T}^*$ term is absent, so that only $\hat{\delta}_{iT}$ is aggregated, then the result is well known (see Lord and Novick (1968, 376-377)).

If true, Equation (11) implies that:

$$v_{jT}^* = \gamma_T^* \bar{\delta}_{jT} + \bar{X}_{jT}\beta_{1T}^* + \epsilon_{1jT}^* \quad (12)$$

with $\gamma_T^* = 1/q_T$, $\beta_{1T}^* = \beta_{1T}/q_T$, and ϵ_{1jT}^* the disturbance term arising from aggregation of the unmeasured short-term forces ϵ_{iT} in district j . Typically, this equation cannot be estimated since we have no good measure of contemporary PID at the aggregate level.¹⁵ However, if a measure of district PID at the previous time period is available from the vote totals $v_{j,T-1}$ in one or more sleepy, underfinanced down-ballot, open-seat races without short-term forces (converted to a probit scale $v_{j,T-1}^*$), then proceeding as in Equation (7), Equation (12) may be written as:

$$v_{jT}^* = \gamma_T^* \bar{u}_{j,T-1}/(T-1) + (T-2)\gamma_T^* \bar{\delta}_{j,T-1}/(T-1) + \bar{X}_{jT}\beta_{1T}^* + \epsilon_{1jT}^* \quad (13)$$

Express the term with the mean of the most recent retrospections $\bar{u}_{j,T-1}$ as a deviation from expectations $\bar{u}_{j,T-1} - \bar{\delta}_{j,T-1}$, so that it represents new information, and then write it as a function of covariates, so that $\gamma_T^*(\bar{u}_{j,T-1} - \bar{\delta}_{j,T-1})/(T-1) = \bar{X}_{jT}\beta_{2T}^* + \epsilon_{2jT}^*$.¹⁶ Next, substitute for $\bar{\delta}_{j,T-1}$ from the lagged relationship paralleling Equation (12), namely

¹⁴The conjecture treats the pure binomial sampling error in the votes as having mean zero after transformation by the nonlinear function Φ^{-1} . This is only trivially inaccurate if districts have at least a few hundred votes and if vote percentages are bounded between 20 and 80 (since the inverse normal cdf is nearly linear in that range).

¹⁵Down-ballot races in period T itself too often reflect coattail and other effects of the current presidential campaign to be good measures of current PID.

¹⁶The number of prior terms of political experience T varies across individuals, of course, but we are implicitly assuming here that it does not vary on average across districts, or else that its effect is picked up by the covariates.

$v_{j,T-1}^* = \gamma_{T-1}^* \bar{\delta}_{j,T-1} + \epsilon_{1jT}^*$. Finally, assume $q_T = q_{T-1}$ (constant cross-district variance of the short-term forces in the two elections), so that $\gamma_T^* = \gamma_{T-1}^*$. That gives an aggregate-level regression equation in which all variables are now observable by the analyst¹⁷:

$$v_{jT}^* = v_{j,T-1}^* + \bar{X}_{jT}(\beta_{1T}^* + \beta_{2T}^*) + \epsilon_{3jT}^* \quad (14)$$

or equivalently,

$$v_{jT}^* - v_{j,T-1}^* = \bar{X}_{jT}(\beta_{1T}^* + \beta_{2T}^*) + \epsilon_{3jT}^* \quad (15)$$

where $\epsilon_{3jT}^* = \epsilon_{1jT}^* + \epsilon_{2jT}^*$. Thus at the aggregate level, the difference of current vote from a recent prior year's down-ballot race is a function of current retrospections and the current short-term forces. This is the most straightforward equation suitable for assessing the total impact of short-term forces at the current period. It corresponds to Gosnell & Coleman's (940) methodology, and goes by the modern name of "differences in differences." Note, however, that since $q_T > 1$, aggregate coefficients such as $\beta_{1T}^* = \beta_{1T}/q_T$ are smaller than the true individual-level effects β_{1T} .

Equations (14) and (15) imply that to estimate short-term effects, one cannot simply regress current votes on them. That gives the answer to a different, more descriptive question: What kinds of people favored Party 1 at the current election? To assess the impact of short-term forces such as economic retrospections requires that partisanship be controlled.

Equations (14) and (15) also imply that either a lagged regression or a differenced equation regression estimate would work well.¹⁸ The differenced version is safer, however, since even down-ballot races have small short-term effects (intense local races that change the PID balance at the polls in a particular county, for instance). Assuming that these small effects are uncorrelated with the covariates or can be turned into covariates themselves, then the lagged vote is randomly noisy. Putting it on the left-hand side eliminates any bias.

If the prior race were a prominent one with substantial short-term forces of its own, both measured and unmeasured, then the measured ones would enter when $v_{j,T-1}^*$ was substituted for $\bar{\delta}_{j,T-1}$. Hence they would need to be controlled on the right-hand side. They enter with a negative sign since they influence $-v_{j,T-1}^*$ on the left:

$$v_{jT}^* - v_{j,T-1}^* = \bar{X}_{jT}(\beta_{1T}^* + \beta_{2T}^*) - \bar{X}_{j,T-1}(\beta_{1,T-1}^* + \beta_{2,T-1}^*) + \epsilon_{3jT}^*$$

¹⁷Note that there is no loss of generality in having both u_{jT} and c_{iT} depend on the same covariates. A coefficient can be set to zero when a particular covariate has no impact.

¹⁸Note that, in spite of having lag coefficients equal to unity, Equations (14) and (15) are *not* unit root equations. Under the assumptions, the disturbances do not cumulate over time, and thus the dynamic system is stationary. Hence unlike the unit root case, ordinary regression analysis will estimate this lag coefficient without bias.

Now the larger unmeasured short-term forces strengthen the case for the differenced form, Equation (16) rather than a lag specification. This is not a new conclusion. In his work on the Depression period comparing elections where short-term forces were often very powerful, Gosnell and his co-authors (Gosnell & Gill 1935; Gosnell & Pearson 1939) found empirically that differenced equations tended to work better than lags.¹⁹

Incorporation of Short-Term Forces into PID

Now $\beta_{2,T-1}^*$ in Equation (16) is the coefficient vector of interest where incorporation of short-term effects into long-term partisanship is concerned. Some such incorporations may have sensible interpretations, as when economic hardships convince voters that the welfare state needs strengthening, or when government over-regulation puts small businesses into bankruptcy. However, if $\beta_{2,T-1}^*$ puts weight on short-term forces that no government can control (drought, shark attacks), or if only myopic economic retrospections are represented in $\beta_{2,T-1}^*$, then are being incorporated into PID in a manner less easily defended. Unfortunately, in Equations (14) and (15), $\beta_{1,T-1}^*$ and $\beta_{2,T-1}^*$ are not individually identifiable; we can estimate only their sum. This not entirely obvious result means that no two-period analysis can separate retrospections incorporated into estimates of long-term PID from those that are merely of the moment and are discarded after the election.

To identify incorporation of short-term forces into PID, we need an election whose temporary short-term effects do not affect the dependent variable, but whose incorporated effects do. For that purpose, extend Equation (13) backward one more period, maintaining the assumption of constant γ^* , and as before, take $\bar{\delta}_{j,T-2}$ to be a down-ballot race with no large or systematic short-term forces, but now from two prior periods ago:

$$\begin{aligned} v_{jT}^* &= \gamma^*(\bar{u}_{jT} - \bar{\delta}_{j,T-2})/(T-1) + \gamma^*(\bar{u}_{j,T-1} - \bar{\delta}_{j,T-2})/(T-1) \\ &\quad + (T-3)\gamma^*\bar{\delta}_{j,T-2}/(T-1) + \bar{X}_{jT}\beta_{1T} + \epsilon_{1jT}^* \end{aligned} \quad (16)$$

Substituting in the same fashion as before gives:

$$v_{jT}^* - v_{j,T-2}^* = \bar{X}_{jT}\beta_{3T}^* + \bar{X}_{j,T-1}\beta_{2,T-1}^* + \epsilon_{jT}^* \quad (17)$$

where $\beta_{3T}^* = \beta_{1T}^* + \beta_{2T}^*$, $v_{j,T-2}^* = \gamma^*\bar{\delta}_{j,T-2} + \epsilon_{3jT}^*$, and $\epsilon_{jT}^* = \epsilon_{1jT}^* + \epsilon_{3jT}^*$.

Since $\beta_{2,T-1}^*$ is the coefficient vector for the incorporation into PID of retrospections from the campaign at time $T-1$, this last equation gives a proper form for carrying out

¹⁹Simulations and tests with our data seemed to confirm that the bias in the lagged form is often very small for parameter values typical of ordinary elections, but that it can grow larger when short-term forces become stronger, are correlated with PID, and are not all measured. Those also seem to be the conditions under which Gosnell found that differencing was better.

the relevant test. Note that $\beta_{1,T-1}^*$ is missing from this equation, since it is the coefficient vector for the effects from the prior period that do *not* get incorporated into PID.

In practice, some variables in \bar{X}_{jT} and $\bar{X}_{j,T-1}$ may be identical: For instance, demographic features of the districts may be measured only decennially by the Census, so that the same estimate must be used at succeeding elections. To avoid collinearity, such covariates must be entered into the regression only once, meaning that only their total effects (the corresponding elements of $\beta_{3T}^* + \beta_{2,T-1}^*$) are identifiable. A test of incorporation of short-term forces into PID therefore requires lagged covariates that are known to have no effect on the current election, conditional on the other covariates. Their coefficients appear only in $\beta_{2,T-1}^*$.

So with a good lagged proxy for partisanship, the recipe is as follows:

1. Find a down-ballot race or party registration figures two periods back with small, unsystematic short-term forces. (An average of such races, occurring simultaneously or in a short period of time, might also be used if partisanship is stable across them.)
2. Convert the current election outcomes to a probit scale, and do the same with the lagged down-ballot outcomes.²⁰ Take their difference. That is the dependent variable.
3. Enter covariates for the important short-term forces at the current election and at last period's election. At least one of the lagged variables must be different from (i.e., not collinear with) the current-period covariates. Moreover, all the covariates have to work well enough that remaining unmeasured short-term forces are uncorrelated with the measured forces at both time periods and also uncorrelated with the lagged down-ballot vote shares. (Note that the current election might also be down-ballot, in which case the current short-term forces may be omitted. However, the one-period lag election must have substantial short-term forces for the test to have a chance.)
4. Run the resulting regression, assessing the coefficients $\beta_{2,T-1}^*$ on the one-period lagged covariates for substantial effects. For our purposes, to make the case that short-term forces are incorporated into PID in a way that cannot plausibly be interpreted as sensible ideological updating, these coefficients must show dubious logic, for example, too much emphasis on the most recent economic conditions and a neglect of others during which the incumbents were in office.

²⁰The probit transformations are nearly linear in the middle ranges, and thus can typically be omitted if all vote percentages are between 20 and 80, and certainly so if they are all between 30 and 70. Interpretation is then somewhat easier. But it is important to maintain the transformations when studying smaller parties in multi-party systems. For them, electoral swings in raw percentage terms are approximately additive in the districts where they do very well, but more nearly multiplicative where they do poorly, complicating the analysis dramatically. Probit (or logit) transformations restore additivity (Thomsen 1987, chap. 2).

If $\beta_{2,T-1}^*$ seems to embody substantial effects with no sensible ideological interpretation, then statistically, the counter-hypothesis is that these lagged effects are just picking up unmeasured short-term forces from the current time period or deviations from actual PID in the lagged down-ballot race. The lagged covariates are one time period removed from both the current election and from the lagged down-ballot election, so that the time difference will help reduce concerns about specification error, but the case has to be made. Thus the more random and unsystematic the one-period lagged short-term forces, the better.

If no down-ballot race is available two periods back, then, more delicately, a two-period lagged race with short-term forces can be used, so long as those forces are measured well enough to be statistically controlled, too, with residual effects uncorrelated with the measured forces at all three time periods:

$$v_{jT}^* - v_{j,T-2}^* = \bar{X}_{jT}\beta_{3T}^* + \bar{X}_{j,T-1}\beta_{2,T-1}^* - \bar{X}_{j,T-1}\beta_{3,T-2}^* + \epsilon_{jT}^* \quad (18)$$

where $\beta_{3,T-2}^* = \beta_{1,T-2}^* + \beta_{2,T-2}^*$ includes the effects of both the two-period lagged short-term forces that are incorporated into PID and those that are not. This modifies the recipe only in the use of a volatile election two periods back rather than a quiet one, and in the need to control for the systematic short-term forces at that two-period lagged election. As before, it is the coefficients $\beta_{2,T-1}^*$ on the one-period lagged variable that measure incorporation into PID.

Lastly, note that everything here assumes that PID is a simple running tally of previous retrospections, with all past periods weighted equally. We know very little as a profession about the true time path of voter benefits, a critical issue if the citizenry is to be modeled accurately. More complex lag structures may be needed, possibly including those with a structural break if voters decide that a realignment may have occurred, making older experience irrelevant (Jackson and Kollman 2008). If so, accurate modeling will require stronger assumptions and more elaborate estimation routines. Neither differencing nor lagging votes will be successful under those conditions. Similarly, working with vote returns alone without covariates is not possible without additional postulates. In such cases, achieving good estimates requires a procedure like Kalman filtering. (PID would be the state variable and election outcomes the observation equation: see, e.g., Jacobs 1993, chap. 13.) We lay those interesting possibilities aside for the present, and take up an example with a good measure of prior partisanship, Montana elections in the Thirties.

Volatile Wheat Crops and Party Identification

Agrarian discontent and radical farmers' parties were an intermittent feature of American politics from the 1890s through the early 1930s. Roosevelt's broad coalition incorporated

the farmers and made them far more successful nationally in their demands than they had ever been before. Montana is an excellent example. Wheat prices declined dramatically in the early 1930s (Figure 2). Drought conditions in 1931 and again from 1934 to 1937 ensured that production dropped considerably as well. The double whammy of low prices and small harvests shattered the family finances of wheat-growing farmers.²¹ Solidly Republican through the Twenties but with a long history of flirting with Populists, Progressives, and Socialists, Montana shifted dramatically toward the Democrats as economic crises arose in the first part of the decade. By 1936, Roosevelt received nearly 70% of the state-wide vote. Some wheat-growing areas gave him more than 75%, and in one case (Valley county) more than 80%. Eight years earlier, Al Smith had received 30-40% in these same counties. In most of Montana, as in other volatile farming states (Bean 1948), the realignment was a political earthquake. Roosevelt won all his four contests in Montana, and Truman coasted to a ten-point victory margin in 1948 as well.

Figure 2

Roosevelt's success translated to the rest of his party, not just in presidential elections but in congressional years as well. Figure 3 shows the vote percentage for Democrats running for the down-ballot statewide office of Public Service Commissioner. Almost no incumbents ran during this period, so that the state-wide figures are a good measure of partisanship.²² As the figure shows, from 1920 to 1930 the Democrats never won. From 1932 on, they almost never lost, not even in the heavy GOP tide in 1946. Thus short-term forces were dramatic in the Thirties, the political changes were tectonic, and they persisted. Hence the state presents a particularly good opportunity for studying large short-term forces and their incorporation into PID.

Figure 3

Montana has other attractive features for the purposes of this analysis. Some scholars (for example, Anderson 1979) have argued that the realignment was primarily due to incorporation of immigrants and other new voters, not to conversion of their party identification by prior voters. In the East, surges of new voters and dramatic electoral shifts occur simultaneously in an era with opinion surveys, making disentanglement difficult. But in Montana, the electorate is nearly stable over the relevant period. The state had 549 thousand citizens in 1920, 538 thousand in 1930, and 560 thousand in 1940. Some rural counties lost population to the cities, but many did not. Yet their partisan shifts were dra-

²¹The first author's mother grew up on a wheat farm in Idaho. Fond of books, she had to leave school at age 15 because her family had no money to send her after wheat prices hit rock bottom in 1932. In later years, she taught her Montana-born son that politics matters.

²²By contrast, Montana congressional votes are not always as useful as they would be in most states. Senator Burton Wheeler's growing conservatism and antagonism to Roosevelt after the 1937 court-packing fight resulted in odd cross-party factional alliances in some congressional races, with Republicans representing Democratic areas and Republicans voting for conservative Democrats (Waldron & Wilson 1978, *passim*).

matic. For example, a typically stable, formerly Republican Montana agricultural county, ironically named Roosevelt, had 10,347 citizens in 1920, and varied fewer than 550 citizens from that number in the 1930 and 1940 censuses. When such a county shifts its two-party vote 21 percentage points from 1928 to 1932, with only 5% more citizens at the polls, as Roosevelt county did, immigration and new voters cannot be the explanation.²³ The same county was still voting more than 58% for Truman in 1948. The previous voters must have changed partisanship. Many more such examples could be given.

Finally, studies of similar counties within individual states eliminate much of the unobserved heterogeneity that threatens inferences from cross-state samples. For example, are Ogburn and Talbot (1929) right that Prohibition sentiment and Catholicism (in that order) were the key factors in Democratic Party presidential candidate Al Smith's defeat in 1928, with the foreign born largely irrelevant, or is Silva's (1962) finding correct that the foreign born favored Smith, but not enough, in what was otherwise a routine Republican victory in that period, with Prohibition and Catholicism playing only minor roles? Ogburn and Talbot used a sample of counties in eight states; Silva studied the entire country with state-level data. With nearly half a century's perspective on this debate, it is not hard to see that within-state studies could have reinforced the inferences and helped minimize disagreements, as Gosnell and his co-authors did a decade after Ogburn and Talbot (Gosnell & Pearson 1939; Gosnell and Cohen 1940; Gosnell and Colman 1940; Gosnell and Coombs 1940).

In what follows, we take Gosnell's advice and follow the stable Montana population and its partisan shifts, using the statistical methods set out in the previous sections. That is, we ask why Montana voters changed their partisanship. Was it ideological rethinking, persisting over a series of elections? Or was it myopic responses to the state of the wheat crop in a particular year? We combine the official election figures originally published by the Montana Secretary of State's office with the wheat prices and crop production ("all wheat") from the National Agricultural Statistics Service of the United States Department of Agriculture (http://www.nass.usda.gov/Statistics_by_State/Montana/index.asp). Census population figures by county are used to create per capita numbers, and the Consumer Price Index is employed to convert to real income with a 1982-84 base.²⁴ The count of Catholics is from the 1926 U.S. Census of religious bodies, and the "wet" (anti-Prohibition) county percentages are taken from Montana's initiative vote in 1926 on that topic. We focus on the Republican vote to avoid dealing with Socialist and Farmer-Labor candidates, who

²³More substantial turnout changes do occur in some wheat-growing counties, usually upward in the Roosevelt years, but not infrequently downward as people fled unproductive land in eastern Montana. We have more to do to take account of these population shifts in our analysis.

²⁴The CPI is based on urban households and prices, with obvious difficulties when applied to rural areas and farms. Unfortunately, there does not seem to be a better alternative. In any case, prices are relatively stable in this period. For example, from 1932 to 1938, the range is 13.0 to 14.1, where 1982-1984 = 100.

were eventually largely absorbed into the Democratic Party but who received respectable portions of the vote in earlier years, particularly in certain counties.

In this first round of analyses, we focused on Public Service Commissioner elections, since they appear to have been lower visibility affairs than the presidential, gubernatorial, and congressional races, and since the state-wide returns seem to tell a straightforward story. But the votes for this position turned out to have erratic features at the county level for reasons we do not yet understand. For example, the Republican vote for Public Service Commissioner in 1930 correlates at only .48 with the vote in 1934 and at a miserable .35 with the 1938 vote for the same office. Even the two post-realignment elections in 1934 and 1938 correlate at just .51, giving little sign of measuring stable partisanship. By contrast, the GOP presidential vote in 1928 correlates with the 1932 vote at .69 and at .61 with the 1936 vote (about twice the variance explained in the Public Service races), in spite of the very different character of those three presidential contests and the more powerful short-term forces. We therefore treat just the presidential elections in what follows.

From 1926 onward, Montana has had 56 counties. We eliminate those with fewer than 45 bushels production per capita in the good crop year of 1927. This removes from the sample most of the mountainous counties in the Rockies or to the west, where mining and timbering are more important occupations than wheat growing, and thus would not be expected to strongly respond politically to wheat prices. It also removes the counties with the “urban” areas of Anaconda, Butte, Helena, and Missoula (home to the state’s major university, the University of Montana).²⁵ The remaining sample has 43 observations.

“Major” wheat counties are defined as those with more than 100 bushels per capita in 1927. There are 31 such counties. This sample definition also removes the two other consequential towns, Great Falls and Billings, from the sample. The towns remaining in the sample had fewer than 10,000 citizens in 1930, and most were hamlets.

The key question is whether short-term retrospections about wheat crops accumulate in partisanship, and whether they do so myopically. That is, we assess whether successful and failed crops in presidential years not only affect the vote that year, but also have an impact on subsequent elections. And we assess whether the same crop successes and failures have little or no impact when they occur in off-years. The argument then is that no such pattern can be explained by ideological updating in the minds of the voters.

We omit presidential years, due to their powerful coattail effects on down-ballot races visible in the Montana returns. We use raw (logged) income levels per capita from wheat sales as explanatory variables. It is customary to use percent change in income rather than income itself, but that makes little sense in a place and time when crops were bountiful

²⁵No city in Montana had as many as 40,000 residents in 1930, and all but Butte and Great Falls were under 20,000 (Waldron & Wilson 1978, 8). Small cities in rural areas are heavily dependent economically on the farmers and ranchers around them.

some years and then almost nothing in other years due to drought and hail. The usual measure works particularly poorly when a horrible year is succeeded by a merely awful year. If one was in danger of losing the farm last year and the children were hungry, while this year the kids are fed but the farm is being sold, one does not swell with enthusiasm about the incumbents because times have gotten better.²⁶

In many statistical tests, one-period lagged economic conditions were virtually never statistically significant, and they usually had small coefficients as well, in accord with the usual finding that only the economic conditions of current election year become consequential short-term forces. Thus in what follows we present just the current year's economic conditions, plus controls for the relevant prior years as suggested by the "recipe" given in the previous section of this paper.

Table 1 shows the results for the presidential years of 1936, respectively, presented as a change from 1928. That is, the explanatory variable is (GOP vote 1936 - GOP vote 1928). We control for the economic conditions in the same manner as in Equation (18). The key issue is the coefficient on economic conditions in 1932, which should be positive: With Republican president Hoover in office, "better off" (that is, less economically downtrodden) counties in 1932 will acquire less Democratic partisanship, and thus will have higher GOP votes in 1936.

For the coefficients on other control variables, Catholics and wets should have a positive effect (they reduce the GOP vote in 1928, and thus increase the difference between 1936 and 1928), while good economic conditions in 1928 should have a negative effect (they increase the GOP vote in 1928 and thus reduce the difference between 1936 and 1928). Lastly, with Roosevelt in office in 1936, favorable wheat incomes that year should have negative effects, since they decrease the GOP vote.

Table 1 shows that 1932 wheat income became part of the partisanship of Montana voters. Under these and variety of other statistical specifications we tried, the (log of real) 1932 wheat income had the expected positive impact on the GOP vote in 1936, and its coefficient was statistically significant at the conventional .05 level. The impact of 1928 economic conditions and of the Catholic proportion of the county also had the right signs and were statistically significant. Interestingly, neither the wet percent of the county nor the economic conditions of 1936 mattered. (The 1935 conditions were also tried and made no difference, neither did the increase from 1935 to 1936.) Apparently the key increment to PID occurred in the dramatic circumstances of the 1932 election. Lastly, if all these

²⁶Substantial income shifts also occur at the state level in this period, as we noted earlier, but they are dwarfed by the county-level changes in agricultural states like Montana. Thus we adhere to the traditional approach in the following section, where the sample consists of states. We leave to further theoretical and empirical investigation whether the two different computations of voter welfare are somehow aspects of approximately the same voter calculus.

interpretations were correct, we would expect all the effects to be stronger in the major wheat counties, and that is precisely what Table 1 shows.

Table 1

Table 2 carries out the same analysis for the 1940 election, but now with tests for the impact of both 1936 and 1932 economic conditions. (Again the proportion wet made no difference, and those results are omitted.) Again we find that current economic conditions in 1940 made little difference, and there is no impact of 1936 either. And again it is 1932 economic circumstances that seem to have been carried over into party identification. The 1932 coefficient, the coefficient for 1928, and the coefficient for the proportion of Catholics again have the right signs and are significant or nearly so in both columns. And once again, the effects are stronger when major wheat counties are used as the sample, though in this case the differences are not dramatic. The strong correlations in wheat income per capita across years creates some collinearity, leaving coefficients unbiased but less well pinned down than we would like, and the fits are less precise here than in Table 1. But the same story emerges.

Table 2

This analysis of Thirties Montana voters demonstrates two features of their behavior. First, the voters rely largely on the current year's economic conditions, if they pay attention to economic conditions at all. By and large, last year's crops and prices make no difference. This is the usual retrospective myopia, and not a surprise. But second, on-year wheat crops and prices sometimes make a difference not only in the current election, but in future ones as well. They cumulate into party identification, affecting the voter and her children into subsequent decades. And that *is* a surprise to conventional thinking. Behavioral party identification in Montana during the Thirties changed dramatically, and a key influence was the local county crop—but just in 1932 when Roosevelt ran for office only as a vague agent of change. That pattern seems to us very hard to square with the usual intellectualist accounts of the voters in the Roosevelt realignment.

Partisan Updating in Congressional Elections

We have focused on wheat farmers in Montana because their stable population, desperate economic circumstances, and dramatic political shifts seemed likely to provide unusually clear insights into the nature of partisan updating in the Depression era. However, local studies are always subject to the concern that the behavior they study may not be typical. Hence it is well worth asking whether an analogous partisan evolution can be discerned in the behavior of Depression-era voters more generally. Perhaps the greatest political legacy of the New Deal was the establishment of durable Democratic majorities in Congress. Indeed,

the House of Representatives remained in Democratic hands for most of the next sixty years. Thus, the dynamics of congressional voting in the Depression era are crucial to understanding the nature and significance of the New Deal realignment.

Table 3 reports the results of regression analyses relating congressional election outcomes to changing economic conditions through the early New Deal period. The first three columns of the table present separate results for the 1936, 1938, and 1940 congressional elections, while the fourth column presents pooled results based on election outcomes from all three years.²⁷ Our dependent variable is the change in the Democratic share of the two-party vote in each district from four years earlier, expressed in percentage points.

*** Table 3 ***

Comparing Democratic vote shares over four-year periods allows us to explore the extent to which intervening economic experiences are incorporated into partisan dispositions while minimizing the risk of strong correlations between short-term forces in the current and baseline elections. In addition, the fact that the resulting comparisons involve only presidential years (1936 versus 1932 and 1940 versus 1936) or only midterm years (1938 versus 1934) minimizes the risk of mistaking differences in the composition of the electorate between presidential and midterm years for changes in the partisan sentiments of voters. To further guard against confounding changes in the composition of the electorate and changes in partisanship, we include the change in turnout between the current and baseline years as a control variable in our regressions. We also control for the change (if any) in incumbency status between the current and baseline years, which is likely to be the most salient local factor in most congressional elections (albeit less salient in the 1930s than in the contemporary era).

Our primary explanatory variables include real income changes in the election year and the three preceding years. The data on income changes are only available for states, not for individual congressional districts; for this reason, among others, we cluster the observations within states and report robust standard errors that allow for the possibility of correlated disturbances among the districts within each state. In addition, because there was substantial variation in the population of congressional districts in this era, we weight the districts by turnout to provide a more representative portrait of congressional voting behavior.

For each election year, our analysis includes all non-southern congressional districts contested by both major parties in both the current and baseline election cycles.²⁸ Our

²⁷The 1940 election provides a natural stopping point for our analysis, since thereafter politicians and voters alike were increasingly distracted from economic concerns and domestic policy debates by the coming of war. Limiting our analysis to the period from 1932 to 1940 also allows us to avoid complications attendant on congressional redistricting.

²⁸We include cases in which either or both of the major party candidates also ran on minor party lines; but we exclude several cases in California in which the same candidate ran on both the Democratic and

analysis is confined to districts in states that were not part of the Confederacy because voters in the Solid South of the 1930s—with its heavy Democratic leanings, low white turnout, excluded African-American population, and racially based politics—may well have had a rather different political response to the events of the Depression era than voters in the rest of the country did.

The regression results presented in Table 3 provide moderately strong statistical support for our model of partisan updating.²⁹ The results for individual election years are imprecisely estimated and, as a result, quite variable. However, the pooled results presented in the final column of the table present a clearer picture of how successive economic experiences affected congressional voting behavior in this period. Income growth in the election year seems to have had considerably more political effect than income growth in the preceding off-year, just as our account of myopic retrospective voting would suggest. Income growth in the preceding election year (two years before the current election) also seems to have had a stronger effect than off-year income growth, despite being more remote in time.³⁰ This pattern is hard to square with any standard account of partisan updating, but follows logically from the idea that voters incorporate myopic economic retrospections into their long-term assessments of partisan performance.

The most plausible objection to this inference is that, simply by chance, economic conditions two years before each election may have been positively correlated with unmeasured short-term forces in the current election, or negatively correlated with unmeasured short-term forces in the baseline election four years earlier. To address that potential objection, we probed the robustness of our results by repeating the regression analyses reported in Table 3 including a variety of additional control variables. Table 4 presents results from the most comprehensive of these alternative specifications, which includes as additional explanatory variables the real income level of each state, the rural/farm population, the percentage of foreign-born whites, and the percentage of blacks.³¹ Insofar as rich, poor, rural, foreign-born, or black voters responded distinctively to issues arising in any specific election year, those responses should be captured in the parameter estimates for the corresponding control variables, leaving us with cleaner and more convincing estimates of the impact of short-term income changes.

*** Table 4 ***

Republican lines.

²⁹For reasons we do not understand, the minus signs in the original Tables 3 and 4 are turned into question marks by LATEX. We will fix this in a subsequent version.

³⁰The t-statistic for this parameter estimate is 1.9. The t-statistic for the difference between the estimated effects of prior election-year income growth (two years before the current election) and off-year income growth (one year before the current election) is 0.8, suggesting that the true difference is almost 80% likely to be positive.

³¹As with our measures of year-by-year income changes, these data on economic and social characteristics are only available for states, not for specific congressional districts.

As in Table 3, the first three columns in Table 4 report the regression results for 1936, 1938, and 1940, respectively, while the final column reports the results of a regression pooling the data from all three election years. The results suggest that Democratic congressional candidates lost ground among rural voters over the course of the 1930s, while districts with large numbers of black voters became less Democratic in 1936 but substantially more Democratic in 1938. The former effect may reflect either economic conditions (since overall state income growth may not accurately reflect income growth in rural areas) or ideological forces (if rural voters objected to specific aspects of Democratic agricultural policy or perceived New Deal programs as overly oriented toward cities and industrial workers at the expense of rural areas). The latter effect seems more likely to reflect ideological concerns and symbolic politics than group-specific economic retrospections. However, there is little in these results to suggest that foreign-born whites or rich or poor people responded distinctively to Democratic candidates in any of these three election years.

As in Table 3, the apparent effects of income growth in individual election years are both imprecisely estimated and quite variable. However, the pooled results once again provide a clearer picture of the pattern of economic retrospections in congressional voting in this era. The largest parameter estimate, not surprisingly, is for income changes in the current election year. It suggests that, other things being equal, congressional voters in a state with 12% income growth in the election year (about one standard deviation above the average growth rate in this era) would cast about 4% more of their votes for Democratic congressional candidates than voters in a state with a 6% decline in real income in the election year (about one standard deviation below the average growth rate). The estimated effect of income changes in the preceding off-year was only about half as large. However, income changes in the preceding election year again appear to have had a somewhat larger effect, with the electoral pay-off from higher-than-average income growth by comparison with lower-than-average income growth amounting to about 3 percentage points. Finally, the apparent effect of prior off-year income changes was again about half as large as the corresponding effect of prior election-year income changes.

These results provide fairly solid support for our hypothesis that economic conditions in election years have important political effects not only in the short run, by affecting current voting behavior, but also in the longer run, through their incorporation into durable partisan dispositions. Thus, retrospective voting contributes significantly to the shifting balance of the party system and to the changing contours of the political battlefield on which future campaigns will be fought.

Conclusion

We have argued for an understanding of partisan alignments and realignments quite different from the usual contemporary views. Most voters, we argue, are overwhelmed by the complexities of politics in the best of times, and doubly so when they are under great economic pressure in recessions and depressions. Not knowing what the best policies are, they content themselves with asking at election time whether events have gone well or badly lately. Then they vote that myopic judgment.

When the state of the economy during the election season is particularly dreadful, as it surely was during the Depression, the voters feel more strongly about their retrospections. Then they form strong partisan aversions to the incumbents, and strong attachments to the parties that replace them if times improve. The resulting preferences endure in less stressful periods and are passed on to their children, imposing a long-term stamp on the party system. The voters will adopt respectable ideological explanations for their behavior, the policies of the lucky party will be enacted, and a country may be substantially remade. The realignment period may then appear to political scientists and historians as an intellectual decision by the electorate. But we find in the cases we have examined that the evidence for all such interpretations is weak. Elites debate policy alternatives, but the electorate as a whole votes on another basis.

This view of democracy is less cheery than the romantic alternatives, and thus less appealing. Its skepticism raises a host of new questions. And it may impose an obligation on those who propound it to make normative sense of the largely successful workings of contemporary democratic governments, an obligation we are not yet ready to fulfill. Nevertheless, just as a critical step toward democracy occurred when intellectuals lost faith that the king had been appointed by God, so also a similar step needs to be taken in shaking off blind obeisance to the divine right of voters.

An empirically honest defense of democracy is not yet in sight. But the first step is to get the history right. Realignments have very large policy consequences. But are they a success of democratic responsiveness, or a kind of accident? Answering that question requires a deeper understanding than we now have about how voters decide to switch their partisan allegiances. We have attempted to take a step toward that understanding.

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Figure 1: U.S. Income and Incumbent Votes, 1928-1940

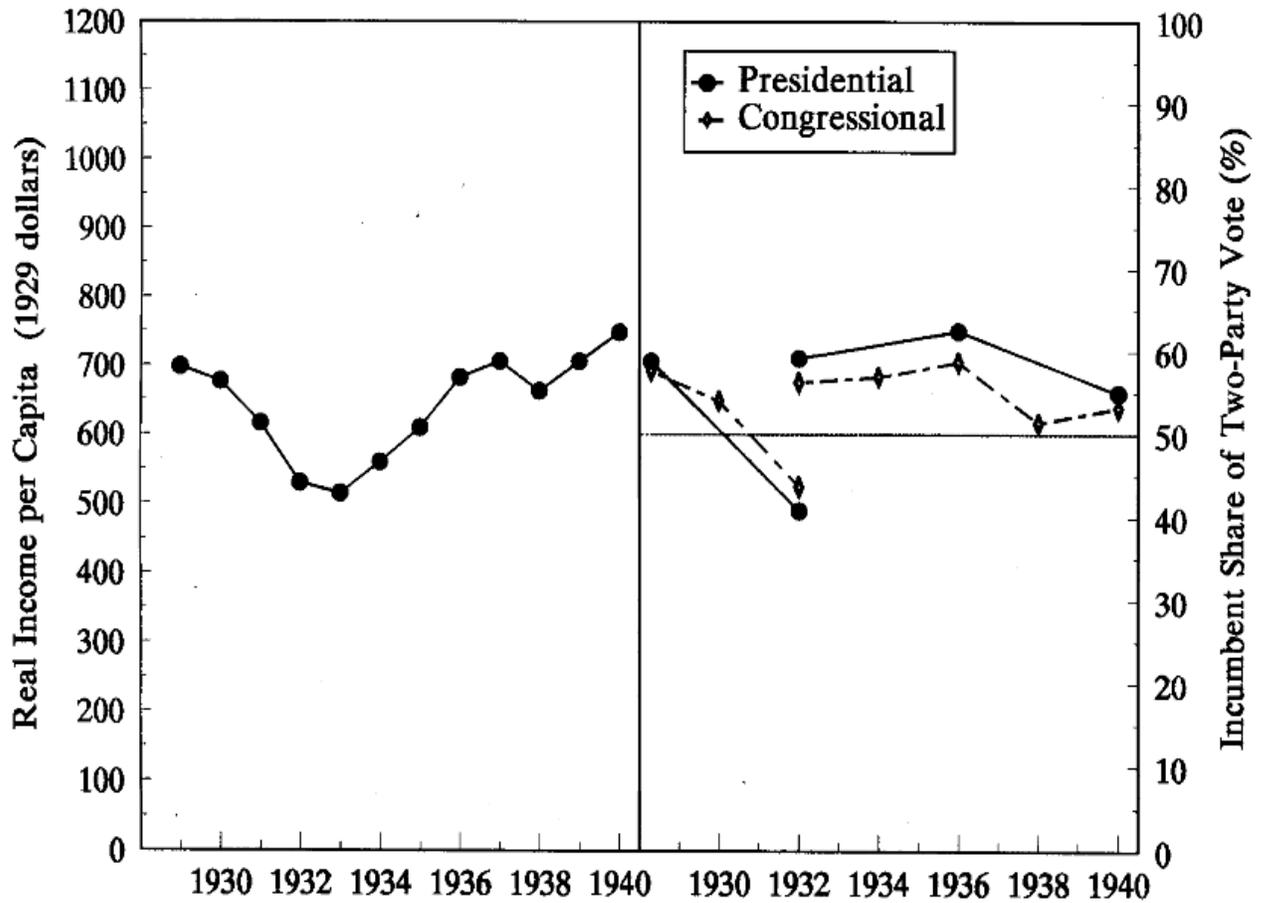


Figure 2: Montana September Wheat Prices 1927-1948

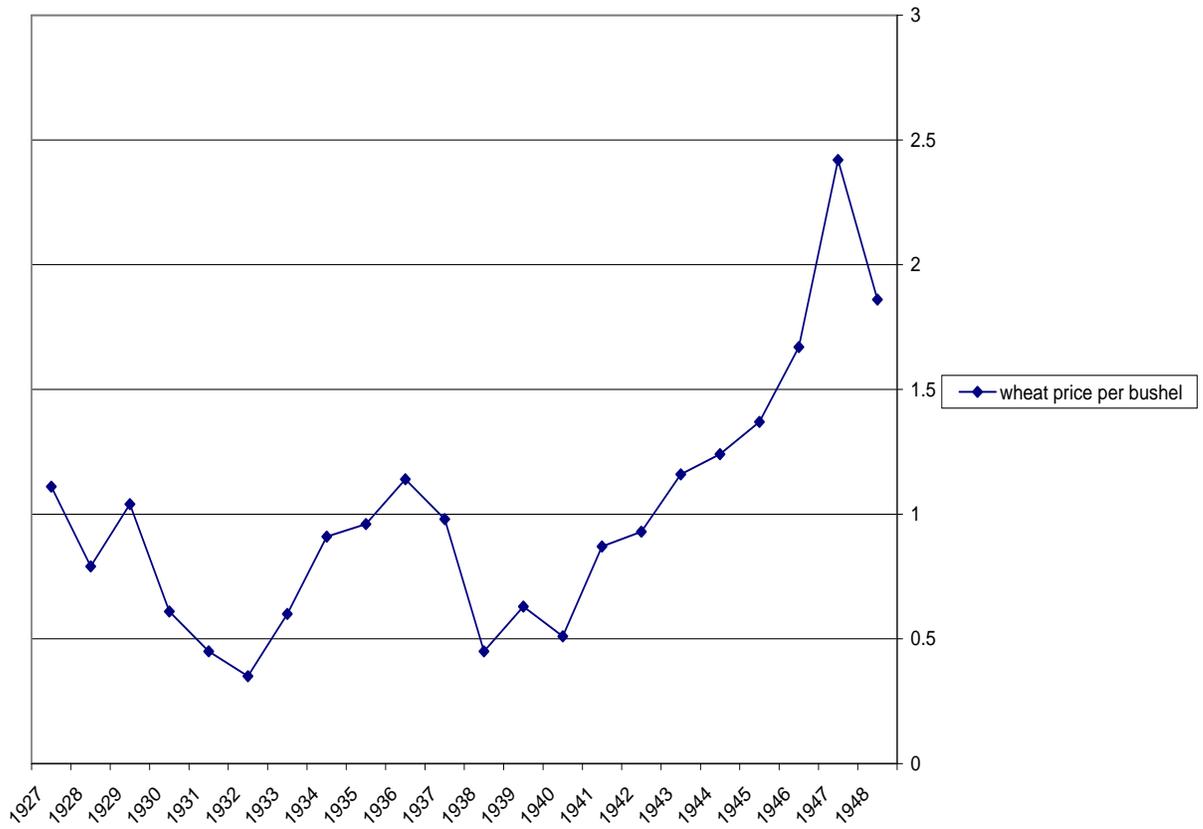


Figure 3: Democratic Share of Montana Public Service Commissioner Vote

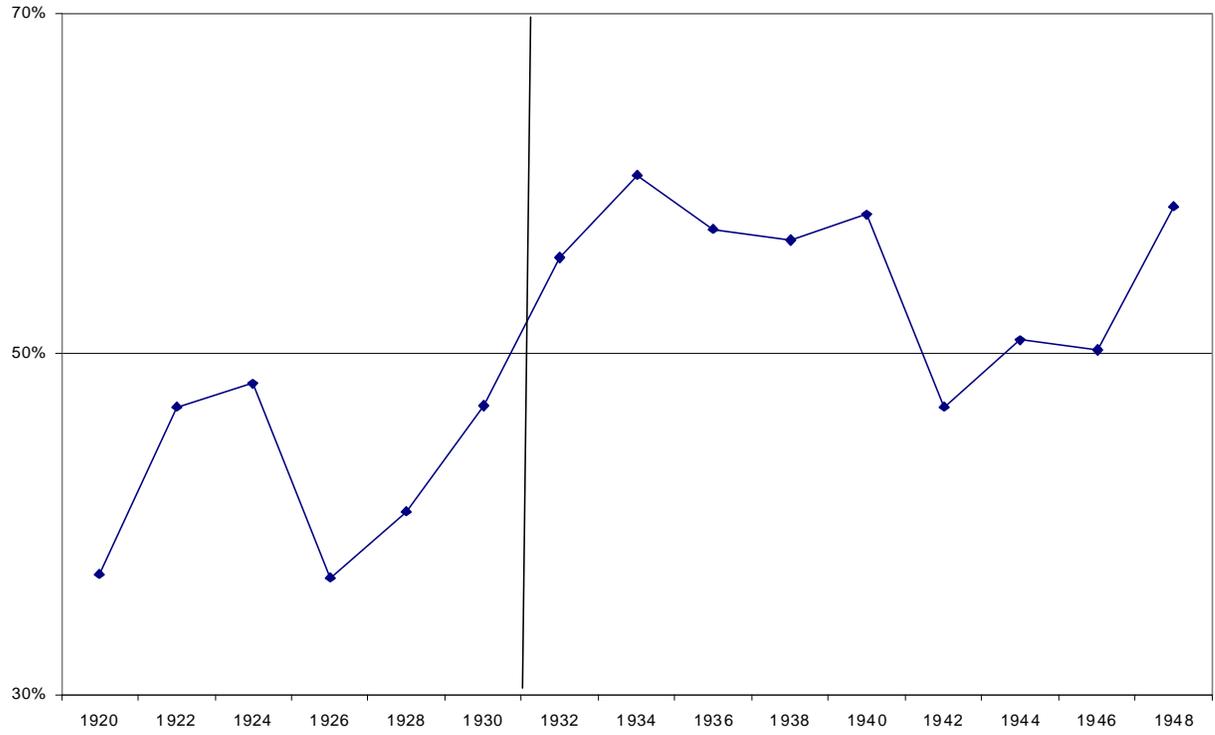


Table 1. Coefficients (standard errors) from ordinary regression analysis of the Montana county difference in percentage points between the GOP presidential votes in 1936 and 1928. (N = 43 for wheat counties; n = 31 for major wheat counties)

	<u>wheat counties</u>		<u>major wheat counties</u>	
Catholic pct.	.245 (.099)	.231 (.092)	.408 (.161)	.410 (.157)
wet pct.	-.046 (.107)		.017 (.130)	
wheat income '36	.348 (.739)	.273 (.710)	.551 (.989)	.609 (.869)
wheat income '32	6.36 (2.65)	6.62 (2.55)	7.89 (2.78)	7.89 (2.72)
wheat income '28	-8.81 (2.58)	-8.79 (2.57)	-11.7 (2.70)	-11.8 (2.59)
constant	-11.8 (9.88)	-15.1 (6.23)	-6.29 (11.6)	-5.30 (8.62)
R ² ; stnd. error	.40 4.89	.40 4.84	.62 4.38	.62 4.29

Table 2. Coefficients (standard errors) from ordinary regression analysis of the Montana county difference in percentage points between the GOP presidential votes in 1940 and 1928. (N = 43 for wheat counties; n = 31 for major wheat counties)

	<u>wheat counties</u>	<u>major wheat counties</u>
Catholic pct.	.249 (.108)	.318 (.198)
wheat income '40	.206 (2.18)	.903 (2.78)
wheat income '36	-.456 (.846)	-.864 (1.09)
wheat income '32	5.76 (2.98)	5.96 (3.63)
wheat income '28	-6.09 (3.60)	-7.77 (3.99)
constant	-13.1 (7.78)	-5.34 (12.4)
R ² ; stnd. error	.23 5.65	.34 5.39

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Table 3: Retrospective Voting in Congressional Elections, 1936- 1940

Dependent variable is the change in the Democratic share of the two-party vote from four-years earlier. Observations include non-southern congressional districts contested by both major parties in both years. Districts are weighted by turnout. Ordinary least squares regression parameter estimates (with standard errors in parentheses); observations clustered by state.

	1936	1938	1940	Pooled
Election- Year Income Change (%)	.240 (.224)	.326 (.209)	.021 (.195)	.167 (.119)
Off- Year Income Change (%)	.069 (.096)	.475 (.242)	? .092 (.225)	.046 (.074)
Prior Election- Year Income Change (%)	.123 (.091)	.513 (.209)	? .279 (.127)	.103 (.054)
Prior Off- Year Income Change (%)	.022 (.162)	? .074 (.139)	? .065 (.065)	? .032 (.052)
Incumbency Change (?1/ 0/ + 1)	1.58 (.77)	2.13 (.38)	1.44 (.48)	1.71 (.36)
Turnout Change (%)	.141 (.025)	? .038 (.031)	.050 (.049)	Varying by year
Intercept	? 4.78 (3.06)	? 9.35 (2.76)	? 7.31 (1.82)	Varying by year
Standard error of regression	6.50	6.60	5.24	6.19
R ²	.24	.18	.09	.36
N (non-southern districts)	273	300	305	878

Table 4: Congressional Voting with Demographic Controls, 1936- 1940

Dependent variable is the change in the Democratic share of the two-party vote from four-years earlier. Observations include non-southern congressional districts contested by both major parties in both years. Districts are weighted by turnout. Ordinary least squares regression parameter estimates (with standard errors in parentheses); observations clustered by state.

	1936	1938	1940	Pooled
Election- Year Income Change (%)	.132 (.207)	.410 (.172)	.099 (.204)	.231 (.112)
Off- Year Income Change (%)	.290 (.158)	.360 (.189)	? .095 (.196)	.112 (.101)
Prior Election- Year Income Change (%)	.352 (.143)	.451 (.241)	? .074 (.143)	.168 (.067)
Prior Off- Year Income Change (%)	.419 (.227)	.009 (.135)	.115 (.087)	.074 (.064)
Incumbency Change (?1/ 0/ + 1)	1.55 (.79)	2.09 (.35)	1.30 (.45)	1.59 (.36)
Turnout Change (%)	.146 (.031)	? .019 (.029)	.095 (.051)	Varying by year
Income Level (1929 \$100s)	? .004 (.013)	? .011 (.011)	? .013 (.008)	Varying by year
Rural/ Farm (%)	? .252 (.153)	? .102 (.167)	? .272 (.115)	Varying by year
White Foreign Born (%)	.153 (.384)	.019 (.308)	? .015 (.149)	Varying by year
Black (%)	? .332 (.292)	.607 (.323)	.288 (.200)	Varying by year
Intercept	.29 (9.89)	? 1.79 (10.88)	7.32 (7.71)	Varying by year
Standard error of regression	6.36	6.45	5.12	5.98
R ²	.28	.22	.15	.41
N (non-southern districts)	273	300	305	878