Musical Chairs

Pocketbook Voting and the Limits of Democratic Accountability

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Draft: 8 September 2004

Abstract

The best current defense of democracy is the theory of retrospective voting. Citizens may not know much about the issues, the argument goes, but they can tell good from bad outcomes, and that allows them to remove incompetent or corrupt incumbents. Moreover, knowing that the voters use that rule, every government will have every incentive to do what they want, thus fulfilling the promise of democratic theory. Some formal analysis and much empirical work during the last several decades, particularly on "pocketbook voting," has seemed to uphold this interpretation. We find, however, that the voters cannot manage the task of competent retrospection. They forget all about most previous experience with the incumbents and vote solely on how they feel about the most recent months. Knowing that, governments pander to the voters near election time, showering them with one-time benefits atypical of their performance in office. Governments are retained or removed, then, not because they drift away from the voters ideologically or because they have performed poorly on average during their term, but most often because of last-minute pork or unexpected misfortunes unrelated to their performance in office—a high-stakes game of musical chairs. We show that even the Roosevelt realignment should be interpreted in this fashion. Thus current defenses of democracy are in intellectual disarray, and much remains to be done before conventional understandings of democratic responsiveness can be responsibly defended.

Musical Chairs

Pocketbook Voting and the Limits of Democratic Accountability[1]

So one may almost say that the theory of universal suffrage assumes that the Average Citizen is an active, instructed, intelligent ruler of his country. The facts contradict this assumption.

— James Bryce (1909, 35)

We endow our presidents with mythical power, holding them responsible for the health of the economy. . . . If the economy is flourishing in the final weeks of a campaign, when the music stops, the incumbent is likely to be re-elected, assuming that other issues—like the Iraq war, in Mr. Bush's case—don't get in the way. But if the economy is heading south in those final weeks, the challenger—Mr. Kerry—is more likely to be our next president.

— Louis Uchitelle (2004)

Of all races in an advanced stage of civilization, the American is the least accessible to long views. . . . Always and everywhere in a hurry to get rich, he does not give a thought to remote consequences; he sees only present advantages. . . . He does not remember, he does not feel, he lives in a materialist dream.

— Moiseide Ostrgorski (1902, 302-303)

The Voter's Limitations

Democratic rhetoric has long celebrated popular wisdom. Actual citizens are another matter.

The distinction was well known to the Founding Fathers, as *The Federalist Papers* repeatedly

demonstrates, but the steady democratization of American politics during the nineteenth century led to

forgetfulness. The Populists and Progressives were particularly prone to romanticism. By the early

twentieth century, Charles Beard (1912, 14) could take note of democracy's failings but nonetheless

write, "Every branch of law that has been recast under the influence of popular will has been touched

with enlightenment and humanity." He argued that greater democratization through the initiative, referendum, and recall would surely produce still better government.

Thoughtful observers of their day were less impressed with ordinary citizens (for example, Bryce 1893; 1909; Dickinson 1930), but they, too, typically felt that the ideal of democracy was appealing even if the reality was not. Wallas (1908, 127) skewered this evasion:

> No doctor would now begin a medical treatise by saying, 'the ideal man requires no food, and is impervious to the action of bacteria, but this ideal is far removed from the actualities of any known population.' No modern treatise on pedagogy begins with the statement that 'the ideal boy knows things without being taught them, and his sole wish is the advancement of science, but no boys at all like this have ever existed.'[2]

More than other commentators, Walter Lippmann (1922; 1927) faced the inevitable limits of human cognitive ability in politics, and he remains the deepest and most thoughtful of the modern democratic critics. He engaged in a life-long struggle to put democracy on an intellectually respectable foundation, but in the era in which he wrote, few could hear. It was all too easy and convenient to dismiss the entire intellectual lineage as elitist and cynical, based on nothing but jaundiced interpretations of personal experience.

By the Fifties and Sixties, this earlier literature was no longer much read. However, new tools had emerged for assessing the voters, notably survey research. A bleak portrait of habitual, socially determined political behavior soon appeared in the work of Berelson, Lazarsfeld, and McPhee (1954), Campbell et al. (1960), and other early analysts of electoral choice. Summing up a decade's work, Converse (1964) was able to revive the case against the democratic citizen, this time substituting random national samples for anecdotes. The article set off a critical discussion of Converse's methodology and historical examples, but few public opinion scholars disputed the central point he

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[2] Wallas is rightly remembered for his early advocacy of psychology as a tool for understanding politics, but his scholarship is sometimes less impressive, and his enthusiasm for eugenics and instant world government (1908, ch. 4) seem no less romantic than the indulgent views of the democratic populace that he criticizes. In the

made: Most voters are woefully uninformed.[3] Several decades later, the case is, if anything, even

stronger (Delli Carpini and Keeter 1996). Scholars continue to express hope in their final chapters that

better political institutions, more education, better mass media, more mobilization of the poor, or

stronger moral exhortation might close the knowledge gap, much as Bryce suggested a century ago,

but in sober moments most acknowledge the repeated failures of all those prescriptions.

The Logic of Retrospective Voting

The development of survey research destroyed simplistic defenses of democracy. No wonder,

then, that when a less demanding version of democratic theory emerged in the 1960s, it was greeted

with acclamation. The influential works of Downs (1957, chap. 3), Key (1966), Kramer (1971),

Jackson (1975), and Fiorina (1981) all portrayed the voters as behaving rationally in spite of limited

information, by focusing on simple judgments regarding the past performance of incumbent officials.

As voters mark their presidential ballots, Key (1966, 9, 61) wrote,

> they have in their minds recollections of their experience of the past four years…. [The
> electorate] judges retrospectively; it commands prospectively only insofar as it expresses either
> approval or disapproval of that which has happened before. Voters may reject what they have
> known; or they may approve what they have known. They are not likely to be attracted in great
> numbers by promises of the novel or unknown.

This interpretation of voting behavior, the theory of "retrospective voting," provides the most

compelling account we have of the relationship between leaders and citizens in democratic political

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quotation given in the text, Wallas is disputing a remark due to Bryce, but taken out of context, and he gets the
title of Bryce's famous *American Commonwealth* wrong as well.
[3] For example, one methodological critic, Achen (1975, 1218, 1231), wrote, "There can be little doubt that the
sophisticated electorates postulated by some of the more enthusiastic democratic theorists do not exist, not even
in the best educated modern societies," and he noted that his re-analysis left open the issue of whether the voters
were "well-informed or sophisticated." However, the distinction between disagreeing with Converse's
inferential logic and disagreeing with his conclusions has sometimes eluded authors. Thus Saris (2004, 18)
recently remarked that "others, following Achen, suppose that people have very detailed opinions." None of the
customary scholarly quotations from Achen or any other author are given in support of this statement, and we
know of no reputable scholar who has taken the view that the voters have "very detailed opinions."

systems. The theory is attractive, in no small part, because it seems to rescue voters from the charge

that they are too uninformed or too disengaged to play a meaningful role in the democratic process.

Thus Key famously put the "perverse and unorthodox argument" of his book in a nutshell: The "voters

are not fools" (1966, 7). Kramer characterized his results as demonstrating "that election outcomes are

in substantial part responsive to objective changes occurring under the incumbent party; they are not

'irrational,' or random, or solely the product of past loyalties and habits, or of campaign rhetoric and

merchandising" (1971, 140). And Fiorina (1981, 3-4) devoted the opening pages of his book to

criticizing "behavioral political scientists" of the 1950s and 1960s who "made sport of civics-book

accounts of American politics" while failing to appreciate the "rough justice" evident (to him) in the

election outcomes of 1932, 1952, 1958, 1968, and 1974.

The theory of retrospective voting dispenses with the unrealistic notion that ordinary citizens vote

on the basis of detailed preferences regarding every issue that might conceivably come before their

future leaders. The depressing survey research evidence about how little voters know becomes, if not

quite irrelevant, at least not fatal to democratic theory. The voters need only remember and evaluate

their own and their fellow citizens' past experiences. As Fiorina (1981, 5) put it,

> [Voters] typically have one comparatively hard bit of data: they know what life has been
> like during the incumbent's administration. They need *not* know the precise economic or
> foreign policies of the incumbent administration in order to see or feel the *results* of those
> policies. And is it not reasonable to base voting decisions on results as well as on
> intentions? In order to ascertain whether the incumbents have performed poorly or well,
> citizens need only calculate the changes in their own welfare. If jobs have been lost in a
> recession, something is wrong. If sons have died in foreign rice paddies, something is
> wrong. If thugs make neighborhoods unsafe, something is wrong. If polluters foul food,
> water, or air, something is wrong.

The most important application of the retrospective theory of voting has been in the realm of

"pocketbook voting." Of course, politicians and journalists have long believed that weak economies

lead to lost elections for incumbents. The contribution of political science has been to provide an

impressive body of systematic evidence that voters reward incumbent politicians for good economic

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times and punish them for bad times (Kramer 1971; Markus 1988; Lewis-Beck 1988; and many others). While details of the causal path are still disputed, a virtual consensus has emerged that the effect of pocketbook voting is real and substantial, thus supporting the retrospective view of elections. In the summer of 2004, one professional survey researcher characterized the coming presidential election this way:

> Ordinary people don't have a good sense of what the president can and cannot do about the economy. Their perception is that he is responsible for how they are faring, and if he does not acknowledge their complaints, they judge him as indifferent and not doing a good job (Andrew Kohut, quoted in Uchitelle 2004).

Evidence of pocketbook voting seems on its face to bolster a rational interpretation of election outcomes. In the post-Keynesian era, competent governments are thought to be able to control the economy, at least to some degree. Thus, punishing incompetents by removing them from office appears to be eminently rational. It is true that one strand of the voluminous literature on economic voting focuses on the possibility that myopic voters can be fooled by irresponsible policies that stimulate the economy around election time, leaving the costs to be borne after the votes are counted (Tufte 1978). But for the most part, the strong tendency of voters to reward incumbents for good economic times and punish them for bad times is viewed as a mark of the rationality of democratic electorates.

Our aim here is to provide a closer look at the "rationality" of pocketbook voting. While we attach great importance to the virtues of simplicity and realism, we believe that the retrospective theory as it stands fails to do justice to the very considerable logical and informational difficulties faced by retrospective voters attempting to assess "changes in their own welfare" and translate those assessments into evaluations of incumbent political officials. For one thing, it is by no means obvious that voters can "ascertain whether the incumbents have performed poorly or well" simply by

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calculating "changes in their own welfare." If jobs have been lost in a recession, something is wrong, but is that the president's fault? If it is not, then voting on the basis of economic results may be no more rational than killing the pharaoh when the Nile does not flood, as some scholars believe occurred in ancient Egypt (Bell 1971; Hassan 1994), or voting against Woodrow Wilson when sharks attack the Jersey shore, as we believe happened in 1916 (Achen and Bartels 2002b).[4]

An even more fundamental problem is that voters may have great difficulty accurately assessing "changes in their own welfare." Analysts have routinely taken this part of the retrospective theory to be unproblematic. Individual voters' economic perceptions may reflect substantial partisan biases, rationalization, and sheer randomness (Kramer 1983; Conover, Feldman, and Knight 1987; Bartels 2002; Erikson 2004), but the electorate as a whole is assumed to respond sensibly and systematically to actual economic experience under the incumbent administration. Our view is that they do no such thing. Rather, they forget most of their previous experience and vote solely on the basis of how they feel about what has happened lately. The result is not a slightly distorted version of rational retrospection. It is something else altogether—a high-stakes game of musical chairs.[5]

Can Retrospective Voters Select Competent Economic Managers?

The most straightforward way to interpret pocketbook voting is as an attempt by voters to select the best available team of economic managers in order to maximize future prosperity. In this view, there are consequential differences between the economic prospects offered by competing candidates

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[4] Fiorina (1981, 202) noted in passing that the normative appeal of the theory of retrospective voting hinges in significant part on the assumption "that the *electorate does a passable job of attributing responsibility to government decision makers*." However, his brief discussion of the problems raised by that assumption focused almost entirely on the issues of divided government and responsible parties, rather than on the more basic questions considered here.

[5] For readers from other cultures, we note that "musical chairs" is a children's game in which a group of children march around a set of chairs in which they have been seated. Music is played while one chair is removed. Then at some sudden moment, the music is stopped and everyone must try to find a seat. The child who fails to do so is out of the game. This sequence is repeated until at the last round, only one child, the winner, has a seat.

in any given election, which may reflect differences in motivation, competence, ideology, or some combination of these and other factors. The voters' problem is to forecast the future course of the economy under each of the competing candidates and choose, other things being equal, whichever candidates offers the most favorable economic prospects.

This interpretation of pocketbook voting portrays voters as rational and forward-looking; but their *prospective* choices are rendered *retrospective* through the auxiliary assumption that the most obvious and reliable way to form rational expectations about the future is by evaluating the parties' actual past performance in office. In the influential formulation of Downs (1957, 106, 39), "rational behavior is impossible without at least some way of forecasting future events. . . . Since one of the competing parties is already in power, its performance in period *t* gives the best possible idea of what it will do in the future, assuming its policies have some continuity." Or, as Kramer (1971, 134) put it, "The past performance of the incumbent party in particular gives some indication of what it would do if returned to office, and of the effectiveness of its policies and personnel."

Certainly this is the way candidates and journalists talk about the electoral relevance of the economy. In July 2004, for example, the *New York Times* reported that a "jump in the unemployment rate gave Democrats a new opening today to attack President Bush's management of the economy and question the effectiveness of his signature tax cuts" (Stevenson 2004). "This president has a failed economic policy on his hands," Democratic critic Richard Gephardt charged, "and he has no inclination to be flexible and change it." Meanwhile, the president "traveled the country, cheerfully telling audiences that 'we've turned the corner' on the economy" (Seelye 2004). His "upbeat assessment of the economy, saying it was getting stronger and lauding the American entrepreneurial spirit . . . prompted cheers of 'four more years.'"

It certainly does not seem unreasonable to imagine that an administration's past economic performance might provide voters with a useful clue about its likely future economic performance. However, in light of the crucial importance of this assumption for the whole notion of retrospective

selection, it is striking that it has never, as far as we know, been subjected to any systematic empirical examination. Proponents of retrospective voting have simply *assumed* that there are real, persistent differences in economic competence between competing teams of political elites, and that voters evaluating the incumbent administration on the basis of its past performance are likely to get more competent government in the future than they otherwise would. In this section, we attempt to put those assumptions to the test.

The data for our analysis consist of quarterly readings of real GDP per capita and real disposable income per capita from the Bureau of Economic Analysis.[6] The quarterly data are available from the first quarter of 1947 through the first quarter of 2004. We focus on annualized quarterly growth rates in real GDP and income, defined as

$$\Delta GDP_t = 400*[\ln(GDP_t) - \ln(GDP_{t-1})]$$

(and similarly for ΔRDI_t).[7]

Some important characteristics of these data are evident in Figure 1, which shows the quarter-by-quarter changes in real disposable income per capita over the entire period covered by our analysis. First, there is a good deal of short-term volatility in growth; quarterly fluctuations of five percentage points or more in the annualized growth rates are fairly common.[8] Second, it is hard to see any striking trends in prevailing growth rates over this period. (More elaborate statistical analysis reveals some drift over time in the average income growth rate, but the magnitudes of these trends are quite modest.) Third, and relatedly, there is surprisingly little temporal continuity in quarter-to-quarter growth rates,

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[6] To some extent these fluctuations presumably reflect measurement error. However, there is no strong pattern of negative association between growth rates in successive quarters, as one would expect if the *level* of income in each quarter were measured with substantial random error.

[7] The natural logs in these standard definitions convert gains and losses to an equivalent scale, so that they count equally. (Without logs, a 25% gain has a ratio of 5:4 to its former value, while a 25% loss has a 3:4 ratio; these disproportions make it harder to satisfy the usual assumptions underlying regression analysis.) Moreover, to a good approximation when changes are not large, the differences in the logs represent fractional changes. Multiplication by 100 turns the fractions into percentage points, and the further multiplication by 4 expresses the quarterly changes in growth as more intuitive annual rates.

even over fairly short periods of time. Thus, current deviations from the long-run average growth rate are unlikely to provide a good basis for forecasting future deviations—a point of some importance for the theory of retrospective voting.[9]

*** **Figure 1** ***

The solid line in Figure 1 represents the average income growth rate for each four-year presidential administration.[10] Here, and throughout our analysis of the quarterly data, we treat each administration's economic responsibility as beginning in the third quarter of the year it took office (Q3)—approximately five months after inauguration—and ending in the quarter following the next inauguration (Q18). We adopt this particular convention for assigning responsibility because it turns out to maximize variability across administrations in economic outcomes, which maximizes the potential basis for pocketbook voting along the lines envisioned by theorists of retrospective voting. We note, however, that plausible alternative definitions (assigning each administration responsibility beginning with Q2, Q4, or Q5) generally produce results quite similar to those reported here.

Changes in the level of long-term income growth from one administration to the next are clearly discernible in Figure 1; but they are also clearly modest by comparison with the short-term fluctuations from one quarter to the next. Regressing the quarterly changes in income on indicator variables for each administration produces an adjusted R^2 statistic of .02. The standard error of the regression, 4.1 percentage points, is larger than the difference in average growth rates between the most and least successful of the 15 administrations covered by our analysis, 3.5 percentage points. (The corresponding statistic for a regression of quarterly changes in GDP on indicator variables for each

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[8] The data are publicly available from the BEA website, http://www.bea.gov.
[9] Regressing income growth in the current quarter on income growth in the four previous quarters produces lag coefficients of .00, .04, .01, and −.16. Regressing GDP growth in the current quarter on GDP growth in the four previous quarters produces lag coefficients of .31, .13, −.08, and −.11. More elaborate time series analyses produce similar results.

administration is .04.) A more elaborate time series analysis including seasonal adjustments and

multiple lagged values of each growth rate as additional explanatory variables produces essentially

similar results. In the case of GDP growth it is not even possible to reject the hypothesis that the fixed

effects for administrations are all equal; the tail probability for an F-test is .34.

These features of the data are relevant for our analysis of retrospective voting because they

suggest that voters may have a good deal of difficulty discerning systematic changes in growth rates

amid constant short-term fluctuations in their economic well-being. Obviously, if voters cannot

reliably *discern* differences in economic outcomes they cannot reliably *respond* to those differences.

The volatility of short-term income growth rates also underscores the potential political

significance of the distinction between pocketbook voting on the basis of long-term retrospections and

pocketbook voting on the basis of how things are going right now. Voters evaluating the economy on

the basis of any one or two quarters are likely to do little better than chance at capturing the potentially

meaningful differences in long-term performance represented by the administration fixed effects

shown in Figure 1.[11] In that case, *any* inference about the differential competence of specific

administrations to produce future economic growth can be little more than a roll of the dice.

Of course, suggesting reasons why successful retrospective selection may be difficult is not the

same thing as establishing that it does not occur. Unfortunately, it is notoriously difficult to provide a

direct test of the prospective efficacy of pocketbook voting, since the logic of the argument hinges on

an inherently unobservable comparison between future economic performance under the incumbent

and future economic performance under the challenger. Inevitably, the voters' choice on Election Day

relegates one of those two outcomes to the realm of historical might-have-beens. Thus, we are reduced

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[10] We ignore the midterm transitions from Kennedy to Johnson and from Nixon to Ford, since in both cases
there seems to have been a good deal of continuity in economic management teams and policies.
[11] The correlation between income growth in any given quarter and the administration's long-term income
growth rate is only .28; for GDP growth the corresponding correlation is .32. Expanding the short-term focus
from one quarter to two helps a bit, but not much—the comparable correlations between short-term and long-
term growth are .34 for income and .33 for GDP.

to testing the plausibility of the retrospective selection hypothesis from a variety of more or less oblique angles.

If we take seriously the notion that reelection hinges on economic competence, we should expect to see more economic growth when the incumbent party is reelected than when it is dismissed by the voters. In the former case the administration has presumably been retained because its past performance makes it a better-than-average bet to provide good economic management in the future.[12] In the latter case the new administration is presumably an unknown quantity—a random draw from some underlying distribution of economic competence. A secondary implication is that future economic performance should be less variable when the incumbent party is retained, since reelected administrations are a truncated subsample of the original distribution of economic competence (the worst economic performers having presumably been weeded out at reelection time).

These implications of the retrospective selection model are examined in Table 1, which compares post-election growth in real GDP per capita and real disposable income per capita in seven recent cases where the incumbent party has been reelected with the corresponding growth rates in six recent cases where the incumbent party has been voted out of office.[13] In both comparisons the retrospective selection perspective fares poorly. Average annual post-election GDP growth was virtually identical in cases of incumbent retention and partisan turnover, while average annual post-election income growth was almost 30 percent *lower* in terms with reelected incumbents (though, given the small number of cases for comparison, this difference is not statistically reliable). Clearly, there is no support here for

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[12] Obviously, election outcomes are influenced by many other factors besides economic management. That fact complicates our proposed comparison, but does not alter the basic logic.

[13] We follow the usual practice in the literature of treating new incumbents of the previous president's party as continuations of the same partisan team. This assumption simplifies both the theory and the empirical analysis. Of course, it may or may not be substantively sensible. For fairly short historical periods of the sort considered here, it is simply not possible to tell. We note, however, that Norpoth's (2001) analysis going back to the 1870s casts some doubt on the notion that voters view successors to retiring incumbents as continuations of the same administration. Nevertheless, we group the one case of partisan succession in our analysis—1988—along with the more straightforward reelection cases of 1948, 1956, 1964, 1972, 1984, and 1996. Our cases of partisan

the notion that incumbents are reelected on the basis of economic competence. Nor is there any evidence that economic performance is any less variable under reelected incumbents than in cases of partisan turnover. At this level, at least, the economy seems steadfastly impervious to voters' decisions to retain or replace the hand at the helm.

*** Table 1 ***

A further, more stringent test of the retrospective selection perspective is to examine directly whether post-election economic performance helps to account for election outcomes. The very possibility sounds paradoxical. But if voters do succeed to some extent in forecasting future economic competence, and vote accordingly—as the logic of the retrospective selection perspective implies— then we should observe more electoral support, other things being equal, for incumbents who turn out to be competent economic managers after the election than for those who turn out to be incompetent. (There is no parallel expectation for challengers, since the usual assumption is that retrospective voters have little or no basis to assess their likely competence.)

Of course, other things never are equal. The context of post-election policy-making may bear little resemblance to what voters imagine on Election Day. The prospect of changes in the global economy, wars and terrorist attacks, unforeseen bouts of inflation or recession, and alterations in the makeup of the incumbent management team all produce a great deal of uncertainty about future economic performance under any incumbent. But that uncertainty is by no means irrelevant to our assessment of the likely efficacy of retrospective selection. If we think the test of actual post-election performance is too stringent, we are in effect conceding that retrospective selection is too difficult for voters to succeed at it.

Table 2 presents the results of a series of regression analyses intended to search for traces of

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turnover are 1952, 1960, 1968, 1976, 1980, and 1992. We exclude the 2000 election from this comparison because complete post-election economic growth data are not yet available.

economic foresight in the outcomes of recent presidential elections. (We limit the analyses to the

seven elections in which the incumbent party was, in fact, reelected, since the logic of retrospective

selection provides no basis for imagining that voters can successfully predict the economic competence

of challengers). The dependent variable in each regression analysis is the incumbent party's popular

vote margin (in percentage points). The key explanatory variables are post-election growth in GDP

and income, measured over the entire four-year period for which the post-election administration is

assumed to be responsible (Q19-Q34).[14]

<center>*** Table 2 ***</center>

The regression analysis reported in the first column of Table 2 tests whether voters' support for

incumbent administrations depends at all upon the *future* success of those administrations in producing

GDP growth. The answer seems to be no; the parameter estimate for future GDP growth is actually

negative, though a good deal smaller than its standard error.

Of course, this regression model would be very hard to defend as a plausible model of voting

behavior, and its fit to the data (with an adjusted R^2 value of $-.16$) does nothing to inspire confidence.

Thus, the analysis presented in the second column of Table 2 adds two powerful control variables,

short-term growth in income in the two quarters preceding the election and the length of time (in years)

that the incumbent party has controlled the White House. We shall have more to say subsequently

about the first of these control variables. The second is intended to capture the strong tendency of

majority coalitions in American electoral politics to erode over time (Stokes and Iverson 1962;

Abramowitz 1988; Bartels 1998).[15]

Not surprisingly, adding these variables to the regression analysis improves the goodness of fit

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[14] As in the analyses presented in Table 1, we assume that each new administration becomes responsible for economic conditions five months after taking office.

substantially. The standard error of the regression is reduced by almost 60 percent, and the adjusted R^2 value has increased from $-.16$ to $.79$. However, the effect on our key parameter estimate, for future long-term GDP growth, is perverse from the perspective of retrospective selection. Once the regression model is made to fit the data on election outcomes reasonably well, the estimated impact of prospective GDP growth is even more strongly negative.

The third and fourth columns of Table 2 present the results of parallel analyses focusing on prospective income growth. They require little by way of exposition, since the results are virtually identical to those for prospective GDP growth. In the third column of the table, without controls, it appears that voters are slightly *less* supportive of incumbents who turn out, upon reelection, to preside over high rates of income growth. Adding control variables produces an even stronger negative estimate for the effect of future income growth on incumbent vote margins. As with the results presented in the first two columns of Table 2—and those presented in Table 1—these results offer no empirical support for the notion that incumbents are reelected, even in part, on the basis of enduring economic competence.

It behooves us to reiterate that the tests of retrospective selection offered here are unhappily indirect, and that their statistical power is unhappily modest. Our results certainly do not *prove* that successful retrospective selection through pocketbook voting is impossible. Nevertheless, it is striking how little empirical trace we have been able to find of the presumed connection between presidential election outcomes and the subsequent economic fortunes of American voters. Better data and more powerful tests may, of course, be forthcoming. In the meantime, the prospective rationality of retrospective pocketbook voting must, we think, be taken as a matter of faith, not as a matter of fact.

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[15] The estimated effect of incumbent tenure is fairly consistent across the range of regression models included

Can Retrospective Voting Provide Incentives for Sustained Economic Growth?

Whether or not voters can reliably identify variation in the competence of incumbents, their efforts to do so may work to their benefit if they reinforce incentives for future incumbents to do whatever they can to maximize voters' welfare. The key idea here is that a rational electorate may punish the incumbent party when times are bad, despite the fact that the past is past, and regardless of *why* times are bad, simply in order to discipline future incumbents. The influential model developed by Ferejohn (1986) highlights this idea by assuming that all politicians are identical, so that retrospective voting cannot possibly be a matter of selecting between more and less competent leaders.[16] Rather, retrospective voting is a simple mechanism for enforcing as much correspondence as possible between the goals of the voters and the goals any set of self-interested politicians will actually pursue once they are in office. In equilibrium, voters have no reason to expect the outcome of any particular election to have any direct impact on the course of the economy; moreover, whichever party wins will, in fact, produce exactly the same post-election economic conditions. Thus, the empirical results presented in Tables 1 and 2 are no embarrassment to an electorate whose sole aim is to establish a reputation for rewarding success and punishing failure.

For this retrospective sanctioning model to work effectively, however, voters must reward (or punish) current incumbents for doing (or failing to do) what they want future incumbents to do. The usual, and quite sensible, assumption is that voters want to maximize the present discounted value of their future income stream. If we add the corollary assumption—well supported by a good deal of empirical evidence—that gains in income cumulate over time, then the increment in voters' long-term economic welfare on any given incumbent's watch can be indexed, to a good approximation, by the

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in Tables 2, 3, and 4, reducing the incumbent party's vote margin by about 5 to 8 percentage points for each additional term in the White House.

[16] Banks and Sundaram (1993), Fearon (1999), and others have developed more general models in which voters are motivated both to select competent incumbents and to minimize moral hazard. On the logic of principal-agent models more generally see Laffont and Martimort (2002).

average rate of real income growth throughout his term.[17] Thus, that is what rational voters

implementing a retrospective sanctioning strategy should reward or punish—"the electorate should

valuate performance over the incumbent's entire term of office, with little or no backward time

discounting of performance outcomes" (Hibbs 2004, 7).

It is striking, then, that the substantial empirical literature on pocketbook voting finds voters

doing no such thing. Indeed, as far as we know, *no* scholar has ever published an empirical analysis

relating presidential election outcomes to a simple average of economic performance over each

incumbent's entire term of office. Some have measured economic growth over the election year

(Kramer 1971; Tufte 1978). Others have focused on even shorter time periods, such as the second

(Campbell 2001) or first and second (Lewis-Beck and Tien 2001) quarters of the election year. Still

others have assumed that most or all of each incumbent's term matters, but with recent performance

weighted more heavily than earlier performance (Hibbs 1987, ch. 6; Erikson 1989; Hibbs 2000; Bartels

and Zaller 2001).

Why so much emphasis on short-term performance, when what should presumably matter most

to voters is total economic growth over the course of an entire administration? We suspect that

analysts have simply given up on what *should* work because it *doesn't* work. With the notable

exception of Hibbs (2004, 14), whose analyses imply "modest (if any) discounting over the term," the

clear consensus in the literature is that recent economic performance is much more relevant at election

time than what happened earlier.[18] However, most analysts have failed to notice the troubling

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[17] A more exact calculation depends on the discount rate voters use to assess the present value of future income.
The average rate of real income growth throughout a president's term is insensitive to differences in the timing
of that income growth, which affect voter's welfare in the short run but are swamped in the long run, assuming
that there *is* a long run and that voters do not discount future income too severely. To the extent that voters are
impatient, they should reward early income growth more than later income growth of similar magnitude. As we
shall see, they do not. In any case, for the sake of simplicity we adopt the customary assumption in the literature
that voters care about long-run average income growth.
[18] Erikson (1989), Bartels and Zaller (2001), and Erikson, Bafumi, and Wilson (2002) all employed versions of
Hibbs's specification with geometrically declining weights applied to earlier economic performance. However,
they all estimated (or, in the case of Erikson (1989), assumed) much more discounting of early-term

implications of this asymmetry for democratic accountability under the retrospective sanctioning model.

Table 3 presents the results of six regression analyses exploring the time horizons of economic voting in presidential elections since 1952. The dependent variable in each analysis is the incumbent party's electoral success as measured by its candidate's national popular vote margin (in percentage points). The incumbent party's tenure (in years) is included as a control variable in each regression model.

<center>*** **Table 3** ***</center>

The regression analysis presented in the first column of Table 3 includes a measure of long-term economic performance, the quarterly growth rate in real GDP per capita in the thirteen quarters leading up to Election Day—that is, over the entire period for which the incumbent administration might reasonably be held accountable. The estimated effect is fairly large, implying that the best long-term GDP growth in the past half-century (Truman's 4.3 in 1952) would raise the incumbent party's popular vote margin by about ten percentage points over the worst long-term GDP growth (Eisenhower's 0.4 in 1956). However, the estimate is not very precise, and the overall fit of the regression model is rather poor, with an average error of almost nine percentage points in the fitted values of the incumbent vote margin. (Obviously, Stevenson's loss in 1952 and Eisenhower's landslide reelection in 1956 both contribute significantly to the poor fit of the model.)

The analysis presented in the second column of Table 3 focuses on short-term GDP growth in the six months leading up to Election Day rather than long-term GDP growth. It is clear that election outcomes are more strongly correlated with short-term GDP growth than with long-term GDP growth. The standard error of the regression is two percentage points smaller, the *t*-statistic for the economic

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performance than Hibbs did. Hibbs's (2000) estimated quarterly discount is .046; Bartels and Zaller's (2001) is .18; Erikson's (1989, following Hibbs (1987)) is .20; and Erikson, Bafumi, and Wilson's (2002) is .47.

variable is increased from 1.3 to 3.1, and the implied effects on incumbent vote margins range from

−11.4 percentage points (in 1980) to +11.4 percentage points (in 1972).

The regression analysis presented in the third column of Table 3 includes both long-term and

short-term GDP growth. Again, it is clear that voters respond much more to short-term growth than to

long-term growth. The estimated effect of GDP gains in the two quarters leading up to Election Day is

reduced by only 7 percent, whereas the estimated effect of long-term growth is reduced by about two-

thirds; moreover, the fit of the model is slightly worse than for the second model, which ignores long-

term growth entirely.

The fourth, fifth, and sixth columns of Table 3 present parallel analyses in which economic

performance is measured by quarterly growth in real disposable income per capita rather than GDP.

These models all fit the data significantly better than the comparable models focusing on GDP

growth—as one might expect, given that real disposable income is a more direct measure of voters'

concrete economic pain or pleasure. Here, too, however, it is clear that the short term rather than the

long term is what matters. Income growth in the two quarters leading up to the election has a very

powerful positive effect on the incumbent party's expected popular vote margin. For example, the

parameter estimates presented in the fifth column of Table 3 suggest that a one-term incumbent

seeking reelection during a recession (Jimmy Carter in 1980) might expect to lose by about four

percentage points, whereas a one-term incumbent seeking reelection in a boom year (Lyndon Johnson

in 1964, Richard Nixon in 1972, or Ronald Reagan in 1984) might expect to win in a landslide, by

from 18 to 23 percentage points. (In fact, Carter lost by ten points, Johnson won by 22, Nixon won by

23, and Reagan won by 18.)

The final column of Table 3 includes both long-term and short-term income growth as

explanatory variables in the same regression model. Here, the apparent impact of short-term income

growth is even greater, while the estimated effect of long-term income growth is actually negative

(albeit only a bit larger than its standard error). As in the previous column, the standard error of the

regression is about five percentage points, which implies that a typical error in fitting the incumbent

party's vote *share* would be on the order of two or three percentage points.[19]

These results suggest that it is possible to account for recent presidential election outcomes with a

fair degree of precision solely on the basis of how long the incumbent party has been in power and

how much real income growth voters have experienced in the six months leading up to Election Day.

Long-term economic growth, whether in real income or GDP, contributes little or nothing to the

incumbent party's electoral prospects.

We probed the robustness of the results presented in Table 3 by examining several additional

regression specifications relating incumbent party vote margins to income growth measured over a

variety of alternative time horizons. For example, we examined all of the time horizons longer than

the short-term (2-quarter) time horizon in Table 3 but shorter than the long-term (13-quarter) time

horizon to see if any produced a better regression fit; none did. Results for the best-fitting of these

intermediate models, with income growth averaged over the last seven quarters before the election

(Q9-Q15), are presented in the first column of Table 4. The standard error of this regression is more

than 20 percent larger than the standard error of the short-term regression reported in the fifth column

of Table 3; the standard errors for the other intermediate models ranged from 23 to 51 percent larger.

<center>*** Table 4 ***</center>

The regression analysis presented in the second column of Table 4 examines whether our

estimate of the electoral impact of short-term income growth is biased by the omission of earlier

growth rates in the fifth column of Table 3. Adding additional variables representing average income

growth in Q9-Q13 and Q3-Q8 leaves the parameter estimate for short-term growth virtually unchanged

(3.11 versus 2.90), while doing nothing to improve the fit of the model (and the estimated effects of

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[19] Eisenhower's landslide reelection in 1956, a year with below-average income growth, produced the biggest
residual in both these analyses, 8.6 percentage points.

<center>19</center>

early-term income growth are small and negative).

The analysis presented in the third column of Table 4 focuses on the possibility of differential effects of income growth in the three quarters leading up to the election, with a single parameter to reflect the impact of average growth before the election year. (The parameter estimate for early-term income growth is, once again, small and negative.) Allowing for differential effects of income growth in the last three quarters before the election seems to improve the fit of the regression model significantly, albeit at the cost of estimating six parameters from 13 observations. Clearly, the resulting parameter estimates cannot be considered entirely trustworthy. Nevertheless, they go some way toward accounting for the appearance of a long time-horizon in Hibbs's (2000) analysis, in which the weights attached to successive quarters looking backward from Election Day are constrained to decline geometrically. The geometric lag specification, despite its theoretical appeal, does not look like a good candidate to fit these data, since the apparent effect of income growth in Q14 is substantially *larger* than in Q15, while the estimate for Q13 is negative.[20]

Finally, the analysis presented in the fourth column of Table 4 relaxes our assumption that only Q3 through Q15 of each administration are potentially relevant to voters. Here we calculate the average rate of income growth over the four quarters of each calendar year, and allow growth in each calendar year to have a distinct effect on incumbent vote margins. (Again, notice that we are estimating six parameters from 13 observations.) The result is a smoother-looking declining pattern of regression weights as we go backward in time. However, the second of these parameter estimates is less than half as large as the first (and the associated *t*-statistic is only 1.4). Meanwhile, the parameter estimate for income growth in the second year of each president's term is close to zero, and the parameter estimate for income growth in the first year of each term is strongly negative (with a *t*-

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[20] Including the last quarter of the election year in the calculation, as Hibbs does, may compound this problem; in most of our analyses the apparent impact of income growth in Q16 is even smaller than in Q15, which in turn is smaller than in Q14—exactly the opposite of what one would expect if the geometric lag specification is appropriate.

statistic of −2.6).

None of the alternative regression analyses presented in Table 4 seem to us to improve upon the simple analysis presented in the fifth column of Table 3, in which voters are assumed to respond to income growth only in the last two quarters before the election. Certainly none provides empirical support for the notion that voters weigh economic performance equally, or even approximately equally, over the entire period for which the current incumbent could plausibly be considered responsible. As Uchitelle (2004) put it, "If the economy is flourishing in the final weeks of a campaign, when the music stops, the incumbent is likely to be re-elected," regardless of what happened earlier on his watch.

Myopic Policies for Myopic Voters

How would we expect reelection-seeking incumbent politicians to respond to electoral incentives of the sort we have described? The obvious-seeming answer is that they should attempt to maximize income growth in the immediate run-up to elections, but care little about what happens to the economy at other times. A president who shirks (or pursues his own ideological agenda) in the months just before the election may be punished, but a president who shirks (or pursues his own ideological agenda) earlier in his term is likely to suffer little or no penalty at the polls; as a result, there is little or no (electoral) reason for him to promote prospective voters' well-being during much of his time in office. Meanwhile, voters' short time horizons magnify incentives for incumbents to manipulate the economy in order to maximize economic performance around election time; in Tufte's words (1978, 143), "There is a bias toward policies with immediate highly visible benefits and deferred hidden costs—myopic policies for myopic voters."

These considerations imply that voters' short time horizons should induce a "political business cycle" in democratic political systems (Nordhaus 1975), with incumbents' efforts to stimulate the

economy producing regular peaks in growth around election times. The first extensive empirical

analysis of political business cycles, by Tufte (1978), turned up statistical evidence of electoral cycles

in transfer payments, income growth, unemployment, and inflation, as well as considerable qualitative

evidence regarding the specific efforts of incumbents to produce those cycles. However, a good deal

of subsequent empirical research has produced less clearcut results. As one observer (Schultz 1995,

79) put it, "while the general logic behind the theory is quite persuasive, the empirical evidence for

electoral-economic cycles is spotty at best."

We believe that a variety of problems contribute to the spotty empirical support for electoral

cycles in economic outcomes. The power of statistical tests to detect pre-election fluctuations in

economic conditions may be quite modest in short times series with small numbers of elections. Some

studies focus on secondary measures of economic performance such as unemployment and inflation

rather than income growth, which is clearly of primary electoral importance. Others are insensitive to

important institutional details, such as whether the timing of elections is fixed or variable. The advent

of "rational expectations" theory in macroeconomics has probably contributed to the skepticism of the

scholars generating the empirical evidence, despite demonstrations that political business cycles of

some form can emerge even in dynamic equilibrium models with fully rational voters and politicians

(Rogoff and Silbert 1988; Rogoff 1990).

We do not pretend to offer a thorough review and assessment of empirical evidence regarding

political business cycles here. Instead, we provide a few very simple tests of whether economic

performance has fluctuated in the run-ups to recent presidential elections in the way one might expect

if incumbent presidents were catering to myopic pocketbook voters. The simplest of these tests is

reported in Table 5, which compares average economic growth rates between presidential election

years and non-election years from 1947 to 2003.

*** Table 5 ***

Table 5 provides comparisons between election and non-election years of real growth rates in both GDP and real disposable income. We have two distinct reasons to expect a stronger electoral cycle in the latter than in the former. First, since we find that voters are much more responsive to income growth than to GDP growth we believe that incumbents have much more to gain from manipulating income growth. Second, as a practical matter, it is probably a good deal easier for incumbents to manipulate income growth—for example, through transfer payments and tax cuts—than to manipulate the pace of real economic output through macroeconomic policy.

As it turns out, the results in Table 5 provide little evidence of an electoral cycle in GDP growth—the average growth rate in presidential election years is only .4 percentage points higher than in other years, a difference readily attributable to chance. However, the corresponding difference in average income growth is 1.5 percentage points, with a *t*-statistic of 2.4. This difference provides strong evidence, precisely where we would expect to find it, of an economic electoral cycle tailored to the myopia of voters. Moreover, the fluctuation in income growth rates over the course of the election cycle is impressively large—average growth in election years is 80 percent higher than in non-election years.

We can carry the logic of the political business cycle one step further by combining it with the largely separate literature on partisan differences in macroeconomic goals and policies (Hibbs 1977; 1987). Our analysis suggests that incumbents of both parties are constrained by the demands of voters for robust income growth around election time, but largely free to pursue their own goals and policies at other times. Thus, we should expect smaller partisan differences in economic performance around election time, but larger differences reflecting the parties' distinct ideologies and core constituencies at other times. That is precisely the pattern evident in the separate comparisons of election years and non-election years for Democrats and Republicans in Table 5. In presidential election years the two parties have produced virtually identical average levels of both GDP growth and income growth; but in non-election years Democrats have produced a good deal more growth, particularly in GDP. (Indeed,

Democratic presidents have produced higher average levels of GDP growth in non-election years than in election years, while still managing to concentrate income growth in election years.) As a result, the contrast in patterns of economic performance between election years, when voters are attentive, and non-election years, when voters are largely inattentive, is especially striking for Republican presidents.

A more detailed picture of the timing of income growth over the course of the presidential election cycle is provided in Figure 2, which depicts average growth rates in real disposable income per capita in each quarter from inauguration (Q1) to reelection (Q16) over the 58 years covered by our quarterly income data. The solid bars represent actual (annualized) growth rates, while the trend line represents the smoothed growth rate for each quarter.[21] The most striking aspect of the figure is the dramatic decline in average income growth from 3.3 percent in Q16—the quarter including Election Day—to −.4 percent in Q1—the first post-election quarter. More generally, there is a noticeable increase in average growth in Q2 through Q5, a lull in Q6 through Q12, and a boom in Q13 through Q16—the year of the election.[22] Maximum income growth does not quite appear in Q14 and Q15— the two quarters that seem to be most salient to voters—but the relative precision of the timing of growth in relation to Election Day is impressive nonetheless, as is the magnitude of the election-year income growth spurt.

*** **Figure 2** ***

Another way to gauge the magnitude of election-related economic manipulation is to compare the rate of income growth in Q14 and Q15 with the rate of GDP growth in Q3 through Q15—arguably the best simple measure available to voters before Election Day of each administration's overall economic

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[21] Our smoothed growth rates are derived from a locally weighted ("lowess") regression with a bandwidth of .25. Each quarter's smoothed value is based in part on the observed values in the two preceding quarters and the two succeeding quarters, with more proximate values weighted more heavily.
[22] The early-term increase in income growth appears to be largely attributable to the expansionary policies of Democratic presidents. The average rate of income growth in Q3-Q6—which we take to be the first year in

success or failure. Across the 13 administrations for which both numbers are available, the former exceeds the latter by an average of almost half a percentage point.[23] Given the estimated impact of short-term income growth on incumbent vote margins in Table 3, this difference suggests that post-war incumbents have typically padded their electoral margins by one or two percentage points through political manipulation of the economy.

Of course, in any particular instance the difference between short-term and long-term economic performance may reflect sheer good or bad luck in the timing of economic growth. We note, however, that Richard Nixon's 1972 reelection bid—Tufte's (1978) prime source of colorful examples of economic manipulation—produced the second-largest difference on record between short-term income growth and long-term GDP growth, boosting Nixon's vote margin by 8 or 9 percentage points. (Ronald Reagan in 1984 made it an even 10 percentage points.) At the opposite extreme, Jimmy Carter's vote margin in 1980 was probably *reduced* by almost 11 percentage points—more than enough to cost him reelection—by the fact that voters judged him on the basis of an election-year recession rather than on the basis of his long-term economic performance.

The real social cost of political manipulation of the economy is very difficult to assess. If incumbents merely shift income gains that would otherwise have occurred earlier or later into the election-year window of voter cognizance, the result is simply to add one more electoral edge to every incumbent's balance sheet.[24] On the other hand, to the extent that political manipulation of the economy generates deadweight losses due to wasteful government spending and other forms of "economic instability and inefficiency" (Tufte 1978, 143), the costs are presumably borne by the same voters whose short-sightedness generates the electoral economic cycle in the first place.

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which the new president may be able to affect economic performance—was 4.2 percent in Democratic administrations but only 1.0 percent in Republican administrations.

[23] This difference is not attributable to any peculiarity in the way the numbers are computed. Outside of election periods, average GDP growth slightly exceeds average income growth.

What, if anything, can be done to mitigate political manipulation of the economy? Nordhaus (1975, 188-189) doubted "the practical possibility" of what he referred to as "the 'classical' political solution"—"improve the information available to voters so they can judge and condemn the partisan nature of myopic economic policies." He argued instead for tinkering with the frequency of elections or assigning greater control over economic policy to "persons who will not be tempted by the Sirens of partisan politics," including unelected central bankers and interest groups. In contrast, Tufte (1978, 154) concluded his analysis by asserting that "sleazier efforts at manipulating economic policy for short-run advantage cannot survive public scrutiny," and hoping that books like his might "improve the level of public understanding so that voters can evaluate and repudiate corrupt economic policies."

Alas, there is little evidence to suggest that developments in the quarter-century since Tufte wrote have done much to improve public understanding or scrutiny of electoral economic cycles. In the period from 1947 through 1977, the average income growth rate was 1.5 percentage points higher in presidential election years than in other years; since 1978 it has been 1.4 percentage points higher in presidential years than in other years.[25] Despite well-meaning educational efforts, voters continue to think and act in much the same way they always have—and politicians behave accordingly.

Myopic Voters and the New Deal Realignment

Running for the presidency in the midst of the Depression, Franklin Delano Roosevelt handily defeated the incumbent, Herbert Hoover, in 1932. He then went on to a crushing victory in 1936, capturing 46 of 48 states and thereby confirming the dramatic policy changes he had brought to Washington. The resulting realignment, with occasional readjustments, has dominated American politics ever since.

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[24] Of course, sensitive democrats might still consider it unfair or distasteful that the most shameless manipulators get the biggest electoral edge. As Tufte (1978, 63) delicately put it, "The extremes of 1972 were special because Richard Nixon was special."

[25] The *t*-statistics for these differences are 1.6 and 1.8, respectively.

The conventional view of this period is that the 1932 election was 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. By 1936, however, the character of his administration had become clear, not least to himself, and he campaigned well to the left of where he had stood the first time. 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." The voters joining the ensuing landslide are said to have been "attracted by the Democratic program and the Rooseveltian personality and leadership" (Sundquist 1983, 208-210, 213-19). Even the authors of *The American Voter*, no friends to intellectualist interpretations of elections, use virtually identical language in accounting for the realignment (Campbell et al. 1960, 534). Indeed, that is how elites responded to the great president at the time.

But did ordinary voters respond the same way? Did their customary myopia disappear, leaving them with a clear vision of Roosevelt's entire first term? The elections of 1932 and 1936 were conducted in dramatic economic circumstances, with states undergoing very large gains and losses in personal income over the period. Thus we are not restricted in our own myopia to squinting at recent years with two or three percentage point changes in income, trying to see how much economics matters. The horrifying miseries of the Thirties present an extraordinary opportunity to further test the findings of this paper.

We confine our analysis to states that were not part of the Confederacy, since the Solid South of this era, with its heavy Democratic leanings, low white turnout, excluded African-American population, and racially based politics, offers little opportunity to assess the effect of economics on the voting behavior of ordinary citizens. Across the northern states, the pattern is clear. There personal income per capita dropped 12% in 1930, another 16% in 1931, and an appalling additional 25% during

1932.[26] In the same states, incumbent president Hoover's two-party vote share took a similar dizzying

plunge in 1932 compared to his 1928 totals, falling from 60% to 43%. The voters wanted a change.[27]

After Roosevelt took office, the economy was slow to improve. It fell another 8% in the

northern states during 1933. By 1934, however, it turned around, gaining 14% just in time for the

midterm elections, which led to dramatic Democratic gains in Congress. The year 1935 brought

another 14% jump, and 1936 showed an additional 12% gain. A steady recovery was underway. As

they went to the polls in 1936, voters were still 25% below their 1929 personal income, but they were

34% above where they had been when Roosevelt took office. They might project a still better future

for themselves if Roosevelt were retained. Certainly, the recent trend was strongly favorable.

Which of these factors was most influential in their vote decisions? The gain since Roosevelt

took power? That *retrospective* view might be taken by voters purely concerned to punish incumbents

for shirking or corruption, and to reward them otherwise. Or were the voters thinking ahead, adjusting

for the economy's path during the last few years and forecasting what their lives would be like under

Roosevelt rather than his opponent? That *prospective* view is presumably the most relevant criterion,

assuming that presidents have any control over the economy at all, since it contributes most directly to

the voters' future welfare. Or finally, were the voters merely *myopic*, noticing how they felt lately and

forgetting the rest? The latter style of voter decisionmaking has no obvious justification at all.

These are the same questions we addressed in the first part of this paper, finding that myopia

was the only successful predictor. However, one might expect that the Depression would be different.

The depth of the crisis may have focused voters' minds. Lost jobs and lost homes, hungry children and

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[26] All income changes reported here are geometric means across the 37 non-southern states, weighted by their populations. The data are taken from the "State Personal Income 1929-2000," a CD-ROM issued by the Bureau of Economic Analysis, U.S. Department of Commerce, Washington, D.C., November, 2001.
[27] We would like to have studied economic effects on the 1932 election, but doing so is not easy. The 1928 results are distorted by Democratic candidate Al Smith's Catholicism, and 1924 is skewed by the Progressive candidacy of Robert LaFollette, so that finding a recent baseline year to control for partisanship is challenging. Even if midterm years were used, the economic data go back only to 1929, so that the vote deviations from

ruined lives should not have been forgotten quickly. Moreover, the continuing intense debate in the country over Roosevelt and his program might have allowed the voters to see the connection between their circumstances and political decisions in Washington.

We can test these alternative specifications decisively because economic conditions varied so dramatically across states in this period. In 1936, for example, Delaware gained 24% in personal income, and Nevada shot up 28%. At the same time, North Dakota lost 14% of its previous year's income, and South Dakota plunged 21%. The states' pasts and futures differed sharply as well. Thus there is no statistical difficulty in assessing the differential impact of retrospective, prospective, and myopic economic evaluations: They are all dramatically different across the states.[28] That is why this period provides such fertile ground for statistical analysis.

The dependent variable in our analyses is FDR's vote gain in 1936 by comparison with 1932. This variable essentially controls for differing partisanship across states. Figure 3 shows why 1932 is a good baseline for the 1936 vote in the 37 non-southern states. Maine and Vermont famously anchor the bottom left, while Midwestern and Western states cluster at the top right. The two elections are closely related, and each reflects the new partisan alignment. Roughly speaking, it is the deviations around this line that the regressions below attempt to explain.[29]

*** Figure 3 ***

Our goal is to assess the effect of economic conditions during the Roosevelt administration on the

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partisanship in baseline years, which were considerable in some mining and farming states, could not be controlled.

[28] If one computes the standard deviations across states of the logged 1936 personal income changes (as defined below), then reconverts to percentages, the results are standard deviations of 10 percentage points for retrospective evaluations, 9 percentage points for prospective evaluations, and 10 percentage points for myopic (one-year) changes. Here the logs compute gains and losses symmetrically and smooth out outliers to get a more representative value for the spread of the distribution.

[29] The corresponding plots for 1936 and 1932 vs. prior presidential election years are very noisy and show little relationship. As one would expect, the realignment mattered. Hence we cannot use an average of several prior elections to proxy for partisanship, as we would otherwise prefer.

vote. Doing so is not an historical misjudgment, imposing current ideas on people of a different era.

To the contrary, political insiders in 1936 were well aware of the connection between voter income and

voter choices at the polls. Indeed, during the campaign James Farley and Harry Hopkins, FDR's

powerful aides, were repeatedly accused of trying to buy the election with relief funds (Sherwood

1948, 85). That there may have been something to these charges is shown in Figure 4, which gives the

distribution across states of per capita government transfer funds in each year from 1933 to 1936. The

jump in 1936 spending is unmistakable.[30]

*** Figure 4 ***

The explanatory variables used in our analysis parallel those used in the previous sections with

minor exceptions, the principal difference being the annual character of the economic observations

here. (Quarterly data do not exist for this period.) We focus on personal income rather than GDP in

light of the statistical results in the first part of this paper.[31] For the reasons set out in note 7, we define

the income change between any two years as the difference in logged income levels (with the number

of years over which the change is calculated appearing in the denominator to convert the results into

comparable annual rates of change):

Myopic personal income change in year t:

100*[ln(year t personal income) – ln(year t−1 personal income)]

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[30] However, no such pattern appears in the corresponding data from 1937-1940. The recovery was much farther along by then, and payments may have settled into bureaucratic routine.

[31] We use nominal values of the economic variables. The Bureau of Labor Statistics (ftp://ftp.bls.gov/pub/special.requests/cpi/cpiai.txt) gives the Consumer Price Index in 1982-1984 dollars for the years 1932-1936 as follows: 13.7, 13.0, 13.4, 13.7, and 13.9, respectively. Thus $13.70 in 1932 would buy what $100 bought in 1982-84. But this index is for city consumers only, and even for them, its accuracy during a period of drastically declining incomes (and thus substantially altered market baskets of purchases) is imperfect. Moreover, the estimated changes in the index are not large in any case. Using nominal values alters the corresponding regression coefficients slightly (a maximum of 7% of the coefficient's value) relative to the true but unknown constant-dollar figures, but of course does not affect *t*-ratios or significance tests.

Retrospective personal income change in 1936:

  100*[ln(1936 personal income) – ln(1932 personal income)]/4

Prospective personal income change in 1936:

  100*[ln(1940 personal income) – ln(1936 personal income)]/4 .

Before we proceed to the results, one more remark is needed: Vote gains in a state in 1936 depend on where the state was in 1932. Consider Kansas, for example. That state actually dropped a percentage point in its backing for Roosevelt in 1936 in spite of relatively good economic conditions that year. The source of the anomaly is not 1936 but 1932, when Kansas incomes fell by a dramatic 34%. This propelled the state into unaccustomed enthusiasm for a Democrat. In consequence, when 1936 arrived, it was nearly impossible for FDR to gain further, and he did not.

What this example shows is that economic conditions in 1932 need to be controlled in explaining vote gains in 1936. The mathematical argument for doing so is straightforward,[32] but the intuition should be clear. Hence the 1932 change in state economic conditions is included as a control variable in all the regressions below. We expect it to acquire a positive coefficient, but we do not interpret that coefficient as a causal impact on the 1936 vote.[33] Instead, it has a causal impact on the baseline from which the vote gain is measured, namely the 1932 election.

With that point of interpretation in mind, the result of regressing the Roosevelt vote gain in 1936 on various combinations of the explanatory variables may be seen in Table 6. Column 1 attempts

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[32] Suppose that (1936 Dem Vote) = Partisanship + β_1(1936 Economic Conditions) + u_1, where u_1 is a disturbance. Suppose similarly that (1932 Dem Vote) = Partisanship - β_2(1932 Economic Conditions) + u_2, where β_2 has a negative sign because a Republican was the incumbent in 1932. Then the vote gain in 1936 is the difference between the two dependent variables on the left, while the correct specification on the right-hand-side is generated by performing the corresponding subtraction. Hence the proper setup omits partisanship as explanatory, but includes the economic conditions in both 1936 and 1932, both of whose coefficients are expected to have a positive sign.

[33] The coefficient is positive because, as in the Kansas example, the more negative economic conditions in 1932 become, the more negative the vote gain in 1936, which is a positive correlation. The same point may also be seen in the logic of the previous footnote, where 1932 economic conditions have a negative sign in predicting

to sort out the relative impact of retrospective, prospective, and myopic economic considerations. The verdict is unambiguous: Only myopic considerations matter. The effects of retrospective and prospective considerations are small, noisy, and uncertain, and the corresponding partial regression plots (not shown) give no cause to imagine a causal impact for either variable.

*** Table 6 ***

Columns 2, 3, and 4 examine various lags of income changes. These specifications essentially allow the retrospective variables to take on any relative weights, not just those that a sensible retrospective voter would choose. Again there is no evidence that anything other than the most recent period matters. Easily the best fit in the entire table uses only the 1936 myopic variable, plus the 1932 control to predict Roosevelt's vote gain. Figure 5 shows the corresponding partial regression plot for the 1936 myopic variable: The effect is clear and pronounced, with the statistical benefit of having so much variation across states very visible in the graph. No other variable in any of these regressions has nearly the same power.

*** Figure 5 ***

We also tried a great many other specifications and estimation methods. We evaluated the use of actual income in each year rather than its change from the previous year. In addition, instead of explaining the gain in FDR's 1936 vote from 1932, we looked at the 1936 vote itself as the dependent variable, using the 1932 vote as a control variable. We also tried measuring long-term prospective and retrospective measures, not as the total change over the interval (for example, 1936 income minus 1932 income), but rather as the average change over the interval (the average of the changes in 1936, 1935, 1934, and 1933, each compared to 1932). And we tried these alterations in various

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the 1932 Democratic vote (because a Republican is the incumbent). After that equation is subtracted, the negative value becomes positive in its effect on predicting the 1936 Democratic vote gain.

combinations. None of them made the slightest difference. Only the myopic 1936 income change mattered (along with the 1932 income change variable, the control for the *previous* round of myopia).

We also did extensive data analysis, examining outliers and deleting those for which there was good substantive justification. For example, Oklahoma was the heart of the Dustbowl in this period, and New Deal policies were often misdirected and mismanaged there (Bryant 1975). Thus its support for FDR dropped more in 1936 than the raw economic figures would seem to justify, and it is an outlier. Conversely, Pennsylvania gained in support for Roosevelt in 1936 more than its economic numbers imply, almost surely because the Republican Philadelphia machine, which held down the Democratic vote in 1932, was no longer able to do so from 1936 on. So the gain was relatively large, and an outlier results. Finally, Montana enjoyed more than 20% growth for two years after Roosevelt officially supported the price of silver in 1934, a victory in a battle dating to the nineteenth century. For that or other reasons, Montana's jump in support for FDR in 1936 was larger than expected given its relatively small economic gains in 1936 itself. Another possibility is that in a state so dependent on mineral extraction and lumbering, Montanans' retrospections were focused differently.[34]

When these three outliers are deleted, the fit improves sharply and standard errors decline, making all the *t*-statistics larger. However, the pattern is again just the same: Only myopic economic considerations matter. Subtracting other mild outliers from the data, or adding various combinations of the three principal outliers back in, made no difference. Robust regression fits, with or without the outliers, relentlessly produced the same answer over and over: Among the various economic conditions that might have influenced the voters in 1936, only the current conditions mattered. That statistical finding is very reliable across alternate specifications and estimation methods.

We also tried to assess the separate impact of transfer payments and "relief" during this period. Both are included in personal income and thus are already counted in our calculations, but we sought to estimate them separately because they are more directly under government control. However, it

proved impossible to do so. Transfers went primarily to the states with large cities, where the greatest need was concentrated. Not until 1936 were the problems of the drought-stricken lower Midwest fully recognized. Payments to those states jumped dramatically from 1935 to 1936 (they tripled in Oklahoma, for example), but it was too little and too late: Those states backed off from Roosevelt in the election. The result is that 1935 transfers predict 1936 vote gains, but probably for the spurious reason that they were concentrated in the cities where FDR ran well. By 1936, we found that their correlation with the vote disappears entirely, suggesting that the effect was so endogenous by 1936 that its impact cannot be determined, and more probably, that the correlation was not causal in 1935. With 37 observations, it is impossible to be sure. This is not to say that the administration did not try, as we saw earlier—only that the other powerful economic factors at work probably overwhelmed the relatively small amounts that the government distributed.

Relief money, too, proved impossible to study. Payments per person within each state were quite steady from month to month, with only a small upward drift until late 1935. The rules seem to have been bureaucratic rather than political. Then at that point, the New Deal turned the problem over to the states, with only partial federal subsidies thereafter. The result is that per-person relief payments dropped dramatically in many states during the election year, and the number of people on relief dropped in most states due to improving economic conditions (Whiting 1942). Less money was spent, and that was partly a bad thing (state penny-pinching) and partly a good thing (fewer people were needy). None of the drop was the fault of the federal government in any case, at least not directly, and the payment differences across states appear to reflect differences in state capacity and enthusiasm. Thus any test of the *federal* effect of relief is of doubtful validity, particularly since the amount of money involved is small relative to total transfers, and greatly smaller than changes in personal income. We therefore regretfully set relief aside as an explanatory variable.

In the end, then, we restricted ourselves to examining changes in personal income. This

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[34] Yet another possibility is measurement error in a small state with many isolated ranchers.

variable includes both relief and total transfer payments, but unlike them it describes economic changes that affect everyone in a given state and in that respect is both more relevant and more valid. Moreover, personal income matches well with the usual retrospective voting story: The voters cannot sort out which aspects of their current life are due to government intervention, and so they vote on the basis of overall satisfaction. However, we recognize that further study of transfer and relief payments themselves would be valuable, perhaps by exploiting the city-level data in Whiting (1942).

We conclude this section by bringing to the reader's attention how dramatically this interpretation of the 1936 election changes the conventional understanding of the realignment. In our interpretation, the voters made no judgment about the content of the New Deal, nor were they grateful for the 14% income gains in each of the two preceding years. All that was water under the bridge. They cared only about 1936 conditions. Roosevelt's re-election—and the realignment—depended solely on that one year. 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. The election turned on a short-term, blinkered retrospective answer to the question, "What have you done for me lately?"

Finally, we hazard a rough guess about the economic conditions that would have led to FDR's defeat in 1936. No exact calculation is possible, since we have statistical results only for the effect of the economic variation across states. Voters undoubtedly attended also to the national economy, but since that does not vary cross-sectionally, we have no way to learn its importance. However, we know that in the non-southern states, Hoover dropped 17 percentage points when personal income fell by 25 points in 1932, or roughly 2/3 of a vote percent for each economic percent lost. This total impact is

approximately double the effect of the state variation alone, which comports with our investigations in the modern era.[35]

Roosevelt received 57% of the vote in 1932. To lose, he would have had to drop by 7 percentage points or more, and this would require, by our estimate, approximately a 10 or 11 percentage point drop in personal income nationwide. Recalling that personal income fell in the North by 8% in 1933, the first year of his administration, one gets a sense of how fortunate the timing of the recovery was. If FDR had been elected a bit sooner—say, right after the stock market crash in 1929— he might well not have won a second term.

Conclusion

We have argued for an understanding of democracy quite different from the usual contemporary views. First, the voters are poorly informed, as so many have noted. But second—and here we part company with the consensus—citizens cannot perform sensible retrospective judgments at election time. They reward and punish for events no administration can control. Moreover, while they know how they feel at the moment, they lose all track of how they have felt over the course of the administration's term in office. Like medical patients recalling colonoscopies, their assessments of past pain and pleasure are significantly biased by "duration neglect" (Kahneman 2000; Redelmeier, Katz, and Kahneman 2003). Thus the voters are often arbitrary and nearly always myopic.

This perspective makes sense of electoral outcomes such as Churchill's loss in 1945. He may well have asked, "What? Saving you from death in the concentration camps wasn't enough?" But once victory in Europe was achieved, the voters lost interest in that topic. What was on their minds at election time seems to have been the housing shortage, which the government had had little time for

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[35] Rough tests in the modern era seemed to show that voters put approximately equal weight on state conditions and the national economy, so that the total impact is double that of the state effect. However, the effect for each percentage point change in the economy is considerably larger now than it was in the Depression period.

during the war (McCallum and Readman 1947, 203-204). They voted for a change.

Thus the "rational" scaffolding for the theory of retrospective voting collapses. Whether the voters are trying to select competent leaders or merely create incentives for honest effort makes no real difference. In either case, the electorate cannot manage the task. Even in times of national crisis like the Depression, the same short-sighted voting decisions prevail. As Mayhew (2002, 161) said of elections that have turned on economic issues:

> 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.

We would add that the pail had to be full and the chicken in the pot only just in time for the election.

The result is that, for the great bulk of voters, the apparent issue content of most elections is largely illusory. As we write, the United States is consumed by "the Swift Boats controversy," a debate over the day in Vietnam when current Democratic candidate John Kerry received his medals as a young soldier. Were there a lot of bullets flying? Only some? Or none at all? A less relevant criterion for choosing presidents in a time of war, threats of terrorism, and looming budget and other domestic crises can scarcely be imagined. In the same way, George W. Bush's quarter-century-old arrest for drunk driving became an issue in the 2000 campaign.

Functioning in everyday life, voters probably recognize that only the incompetent choose people for leadership jobs in middle age on the basis of whether they were heroes or blunderers in their twenties. Moreover, common law has long taught that testimony regarding decades-old events should be set aside instantly as wholly unreliable. Thus these campaign debates are not just irrelevant, but undecidable. But partisan interest group money, seeing an opportunity in voter gullibility during a close election, will always bring such topics to the fore.

Alleging old personal mistakes by one's opponent is not the only kind of irrelevant claim in American presidential campaigns. Running in 1992, Bill Clinton blamed the first president Bush for

the inevitable structural changes in the late twentieth century American economy. The voters bought that, too. In 1916 and 1940, too, both sides promised to keep the country out of wars that desperately needed American intervention. Happily for the nation's welfare, the winners double-crossed the voters shortly thereafter. In every election, Democratic and Republican operatives alike treat the voters as dunces, playing to their gullibility and showering them with as much last-minute pelf as the laws allow. It works.[36]

Democratic government as practiced in the United States, then, is a form of limited, random oligarchy. It is an *oligarchy* because, year to year, the voters are not paying any attention and thus have no say in what the government does. Only at election time do they assess how they feel. At that point, the oligarchy becomes *random*, because the choice between alternative governing teams often comes down to accidental and arbitrary criteria such as droughts and recessions, which no government can affect, and aspects of the candidates' personal histories and personalities with no relevance to the job.

Yet the oligarchy is *limited*, too, because American history, working through a dynamic party balance, has set bounds on what any administration can do. Successive national crises and party realignments have settled some once-bitterly-disputed national questions. The last quarter of the eighteenth century established a sovereign national state with republican governmental institutions. Since then, any administration must guarantee our independence and our basic liberties. In the middle of the nineteenth century, elites came to the settled view that the federal government must also limit the unconstitutional or immoral behavior of state governments, by civil war if necessary. In the twentieth century, an elite consensus developed around the position that any administration must

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[36] Most partisans in both camps are largely unaffected by these debates because they resist messages critical of their candidate. They convince themselves that they have thought the issues through rather than simply viewed the facts through prejudiced lenses. Hence at election time, they will sound as though they have been thinking, and their issue preferences will line up nicely with their vote intentions. But that kind of voter rationality is no more impressive than the poor Independents' choices as they are blown around by issues like the Swift Boat affair.

provide some relief to those whose incomes have been reduced by industrial accidents, unemployment, sickness and death in the family, and natural disasters. World War II taught policymakers we must engage with the countries of the rest of the world rather than hiding from their threats. And during the last fifty years, administrations have learned that they must plan small and medium-sized wars with care, though this seems more difficult to remember.

These major decisions were made because in each case the underlying crisis dragged on for years. In such cases, the voters' myopia suffices: The problem under their nose every year is the same as the longstanding structural crisis. Hence the voters will manage to express their continuing unhappiness at the polls, and political elites will learn the appropriate lesson. That is a great accomplishment.

Within those limits, however, administrations with elite support from their own political party can do pretty much as they please. It is true that we can turn them out at election time. But we the voters make no promise to do so fairly or sensibly. Mostly, we vote our party loyalties, acquired primarily in other circumstances or from our parents. Party identifications may have substantial rational content applicable to the present, of course; we do not regard voters as entirely foolish or incompetent. But party identification can be a hindrance to thought, too, as with the Nebraska Republican woman who didn't know "what the parties have been up to lately," but planned to vote against John Kennedy in 1960 because FDR had ended Prohibition (Converse 1966, 144). Nor is the problem confined to the less educated: Inappropriate partisanship and rigid ideologies often blind the intellectuals and the professoriate as well.[37] Moreover, many voters lack ties to either ideology or party, and so are overwhelmed by the confusing claims of both sides.

Thus when the party balance is close—and there seem to be a "law of the pendulum" working to keep it close most of the time (Lowell 1898; Munro 1924, chap. 3; Stokes and Iverson 1962; Bartels

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[37] The slow updating on the left about Stalinism in the Fifties and about Maoism in the Sixties and Seventies are depressing cases in point (e.g., Crossman 1949).

1998)—election outcomes turn on how independent and swing voters happen to feel at the time. As

Converse (1966, 136) put it, "Not only is the electorate as a whole quite uninformed, but it is the least

informed members within the electorate who hold the critical balance of power, in the sense that

alternations in governing party depend disproportionately on shifts in their sentiment."[38] They kick the

dog if they are in a bad mood, whether the government had responsibility for it or not. In consequence,

there is no reason for governments to listen to us, the voters, about the vast bulk of critical national

decisions that escape our notice, and so they do not. Instead, elites decide, some better informed than

we and some merely more powerful. Either way, they decide, not ourselves.

This raises the issue of how the balance is to be struck between steady, coherent government

and popular control. Popular judgments are often erratic, yet elections are critical lest the government

lose all track of its democratic purpose—serving the needs of the populace. How should we trade off

between these two values?

The issue arises in practical form when constitutions are written. How long should terms of

office be, and how often should elections be held? This is the issue Hamilton addressed in Federalist

71, and it brought him to these considerations:

> The republican principle demands that the deliberate sense of the community should
> govern the conduct of those to whom they intrust the management of their affairs; but it
> does not require an unqualified complaisance to every sudden breeze of passion, or to every
> transient impulse which the people may receive from the arts of men, who flatter their
> prejudices to betray their interests… When occasions present themselves, in which the
> interests of the people are at variance with their inclinations, it is the duty of the persons
> whom they have appointed to be the guardians of those interests to withstand the temporary
> delusion, in order to give them time and opportunity for more sedate reflection. Instances
> might be cited in which a conduct of this kind has saved the people from very fatal
> consequences of their own mistakes. . . .

Many other early American political leaders made similar remarks in the course of debating whether

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[38] Converse went on to show, famously, that swing voters are not the same as the least informed, but rather are
intermediate in information—people who learn a few things about the campaign (Swift Boats?) but perhaps not
enough to grasp where their interests truly lie.

terms of state and federal legislators should be one, two, three, or four years (see, for example, the Massachusetts debate at http://press-pubs.uchicago.edu/founds/documents/a1_2_1s13.html, reproduced from Elliot 1888). In short, the viewpoint of this paper would have come as no surprise to most of the Founding Fathers. Although many Americans have now forgotten, some insulation from popular demands is the price of competent government.

Debates over how much democracy is enough usually founder on disagreements over correct outcomes. Partisan differences becloud the matter as well. But from time to time, an issue emerges which allows an assessment. For example, less institutionalized, more volatile national economies shed particularly clear light on the economic consequences of election-time pork. The director of the Turkish Treasury recently noted that the inflation rate was 17% in the six months preceding the November, 1987 elections, and 48% in the next six months as voters paid the price for pre-election pump-priming. The corresponding rates were 26% and 36% at the October, 1991 elections, and 24% and 46% at the December, 1995 elections. "We, economy bureaucrats, do not like early and frequent elections," he said (Vedat 2004).

Another instance, this time in the United States, was the fluoridation controversy. During the 1950s and 1960s, many cities decided whether to add fluoride compounds to their drinking water. The scientific evidence that fluoride reduced tooth decay was compelling, and cities whose administrators or city councils made the decision without a referendum overwhelmingly adopted fluoridation. However, when the measure went to the voters, 60% of the time the electorate voted it down. Nor were the losses confined to rural or uneducated parts of the country. Cambridge, Massachusetts voted three times, defeating it, then passing it by the narrowest of margins, and then defeating it decisively on the third try (Crain et al. 1969, 4, 48). Around the country, the voters who rejected fluoridation saved themselves a few pennies in taxes per year in return for hundreds or even thousands of dollars in family dental bills. Why the self-defeating choices?

The simple answer is that the voters were confused. Crackpots, rogue doctors, and certain

extreme right-wing interest groups all fought fluoridation, and many voters, including a substantial fraction of those with college educations, could not sort out the difference between the self-appointed gurus and the competent experts. Many political leaders, not caring deeply about the topic, ducked. Only when partisan competition for city offices gave officials a party coalition to back them, or when lengthier terms protected them from the "sudden breezes of passion," was sensible decisionmaking achieved (Crain et al. 1969, 179-183). Figure 6 shows the dramatic difference that longer terms made to mayoral support for fluoridation, a key step in getting it approved without going to a referendum. As the figure shows, longer terms let mayors do what was right. Conversely, the more democracy the voters had, the more likely they harmed their children.

*** Figure 6 ***

What the voters should retain in a democracy is not policymaking power but a veto, with regularly scheduled opportunities to exercise it. Usually, in effect, they toss a coin. No more than once or twice in a lifetime do issues arise which substantially affect the voters over a series of years and thus force elites to deal with it. In those instances, in effect, the voters force a course of action that structures American politics ever after. Those titanic policy choices are the principal reason why national policy decisions in a democracy represent popular preferences better than dictatorship or oligarchy. Without necessarily grasping the issues at hand, the voters nonetheless set broad limits on what can be done. Because the out parties stand ready to replace the incumbents when we tire of current policy for real or imaginary reasons, long-term nagging problems get addressed at the polls. The rest of our talk about popular control of actual policy decisions is largely empty rhetoric and democratic mythology.[39]

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[39] Of course, in legislative elections, constituencies may have a standing preference for one political party, and that party may in broad outline represent their political orientations rather well. Elections do not become meaningless because they fail to give voters control over policy decisions. The point is rather that it is futile to ask "what the voters really want."

We talk Thomas Jefferson's language, albeit in the bowdlerized form that has come down to us through the Populists and Progressives. More democracy and direct popular control are thought to be always good. In fact, sometimes we are foolish enough to take our own oratory seriously. That is why some states have acquired a cheap and manipulable initiative process that allows interest groups to bypass the legislature, and why they have adopted wide-open primaries and easy recall elections that bypass the parties and put country-and-western singers, B-list actors, wrestlers, body-builders, and mountebanks of all persuasions into office. Our self-deceptions about our own wisdom sometimes have real consequences, particularly at the state level, where elite safeguards are likely to be less institutionalized.

In reality, though, we live in the prosperous, industrialized, urbanized, and powerful country that Alexander Hamilton foresaw, and most of the time we operate the national government as he thought we should. Elites run our national affairs. From time to time they make serious, even tragic, errors; that is an inevitable fact of political life. But more often than not, they save us from ourselves. Thus we continue to enjoy the benefits of Hamilton's government, which we cheer and support on the Fourth of July because we see it as Jefferson's yeoman democracy. If we can go on kidding ourselves like that, what combination could possibly be better?

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Table 1: Post-Election Economic Growth Under Old and New Administrations

Average values (with standard errors in parentheses) of real annual growth rates, 1952-2000

	Post-Election GDP Growth			Post-Election Income Growth		
	Incumbent Party Retained	Incumbent Party Replaced	Difference	Incumbent Party Retained	Incumbent Party Replaced	Difference
Average Post-Election Growth	2.20 (.57)	2.24 (.47)	−.04 (.76)	1.93 (.45)	2.70 (.47)	−.77 (.65)
Standard Deviation	1.51	1.16	.35	1.20	1.15	.05
N	7	6	13	7	6	13

Table 2: The Impact of Future Growth on Election Outcomes

Regression parameter estimates (with standard errors in parentheses)
for incumbent party's popular vote margin (%)

	Future GDP Growth		Future Income Growth	
	Without Controls	With Controls	Without Controls	With Controls
Post-Election ΔGDP (Q19-Q34)	−.94 (2.21)	−3.42 (1.91)	---	---
Post-Election ΔRDI (Q19-Q34)	---	---	−.91 (2.79)	−2.49 (1.93)
Pre-Election ΔRDI (Q14-Q15)	---	3.24 (1.01)	---	2.52 (.90)
Incumbent Tenure	---	−1.71 (.38)	---	−1.82 (.44)
Intercept	16.28 (5.74)	17.52 (2.71)	15.97 (6.22)	18.83 (3.31)
Std err of reg	8.13	3.42	8.19	3.95
Adjusted R^2	−.16	.79	−.18	.73
N	7 (Incumbent Party Retained)			

Table 3: Electoral Effects of Long-Term and Short-Term Pre-Election Growth

Regression parameter estimates (with standard errors in parentheses)
for incumbent party's popular vote margin (%)

	Pre-Election GDP Growth			Pre-Election Income Growth		
	Long-Term	**Short-Term**	**Both**	**Long-Term**	**Short-Term**	**Both**
ΔGDP, Q3-Q15	2.62 (1.99)	---	.88 (1.74)	---	---	---
ΔGDP, Q14-Q15	---	2.05 (.66)	1.90 (.74)	---	---	---
ΔRDI, Q3-Q15	---	---	---	3.73 (1.95)	---	−1.87 (1.80)
ΔRDI, Q14-Q15	---	---	---	---	2.90 (.57)	3.56 (.85)
Incumbent Tenure	−1.91 (.59)	−1.47 (.44)	−1.56 (.49)	−1.62 (.52)	−1.76 (.32)	−1.80 (.32)
Intercept	13.78 (5.61)	12.63 (4.05)	11.74 (4.57)	8.46 (6.51)	10.64 (3.08)	13.61 (4.18)
Std err of reg	8.94	6.90	7.17	8.28	5.11	5.08
Adjusted R^2	.42	.65	.62	.50	.81	.81
N	13 (1952 to 2000)					

Table 4: The Time Horizon of Electoral Responses to Income Growth

Regression parameter estimates (with standard errors in parentheses)
for incumbent party's popular vote margin (%)

ΔRDI, Q15	4.63 (1.22)	3.11 (.82)	1.19 (.56)	---
ΔRDI, Q14			2.11 (.37)	---
ΔRDI, Q13		−.12 (1.66)	−1.35 (.55)	---
ΔRDI, Q9-12			−.84 (1.23)	---
ΔRDI, Q3-Q8	---	−.91 (.88)		---
ΔRDI, Q13-Q16	---	---	---	3.49 (.88)
ΔRDI, Q9-Q12	---	---	---	1.71 (1.18)
ΔRDI, Q5-Q8	---	---	---	.29 (.63)
ΔRDI, Q1-Q4	---	---	---	−1.92 (.73)
Incumbent Tenure	−1.09 (.42)	−1.68 (.46)	−2.12 (.31)	−1.65 (.41)
Intercept	1.16 (5.44)	11.68 (6.53)	19.51 (4.42)	5.22 (4.93)
Std err of reg	6.19	5.34	4.33	4.73
Adjusted R^2	.72	.79	.86	.84
N	13 (1952 to 2000)			

Table 5: Electoral Cycles in Economic Growth

Regression parameter estimates (with standard errors in parentheses)
for annualized quarterly growth rates (%)

	Real GDP Growth			Real Income Growth		
	Both Parties	**Dems**	**Reps**	**Both Parties**	**Dems**	**Reps**
Election Years	2.41 (.53)	2.33 (.74)	2.50 (.75)	3.36 (.55)	3.45 (.83)	3.27 (.73)
Non-Election Years	2.01 (.31)	2.91 (.45)	1.27 (.42)	1.86 (.32)	2.17 (.50)	1.60 (.41)
Std err of reg	4.04	3.94	4.03	4.14	4.39	3.94
Adjusted R^2	.00	−.01	.01	.02	.01	.02
N	228	96	123	228	105	123
(Election Effect)	.40 (.62)	−.58 (.87)	1.23 (.86)	1.50 (.63)	1.28 (.97)	1.67 (.84)

Table 6: Effects of Retrospective, Prospective, and Myopic Personal Income Perceptions on FDR's Vote in 1936

Regression parameter estimates (with standard errors in parentheses)
for Roosevelt's vote gain from 1932 to 1936 (%)

	Retrospective, Prospective, and Myopic	Lags Only	Four Lags Only	Myopic Only
Retrospective (1932-1936)	.104 (.303)	---	---	---
Prospective (1937-1940)	.077 (.514)	---	---	---
1936 Myopic (1935-1936)	.353 (.116)	.368 (.134)	.360 (.126)	.347 (.067)
1932 Myopic (1931-1932; control)	.254 (.110)	.246 (.157)	.253 (.109)	.249 (.100)
1935 Myopic (1934-1935)	---	−.035 (.156)	−009 (.093)	---
1934 Myopic (1933-1934)	---	.030 (.139)	.050 (.088)	---
1933 Myopic (1932-1933)	---	−.065 (.191)	−.029 (.152)	---
1931 Myopic (1930-1931)	---	−.019 (.191)	---	---
1930 Myopic (1929-1930)	---	−.063 (.183)	---	---
Intercept	4.98 (4.24)	4.12 (6.18)	5.15 (4.02)	5.96 (2.63)
Std err of reg	3.82	3.95	3.83	3.71
Adjusted R^2	.38	.34	.38	.42
N	37 (non-Confederate states)			

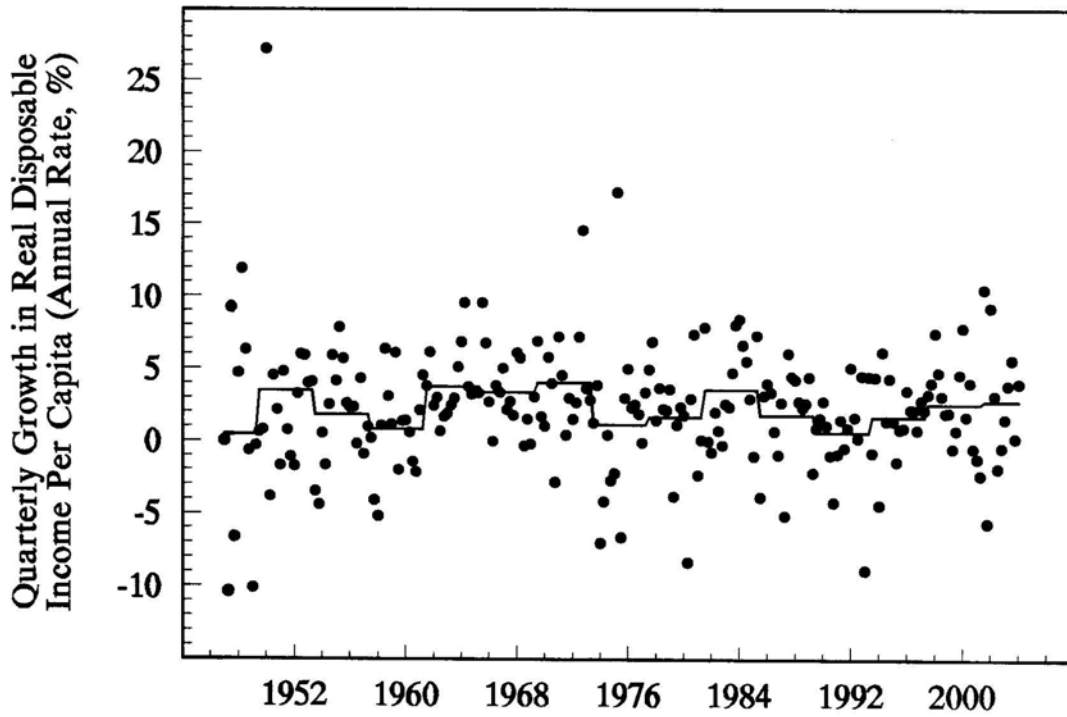## Figure 1: Income Growth by Presidential Administration

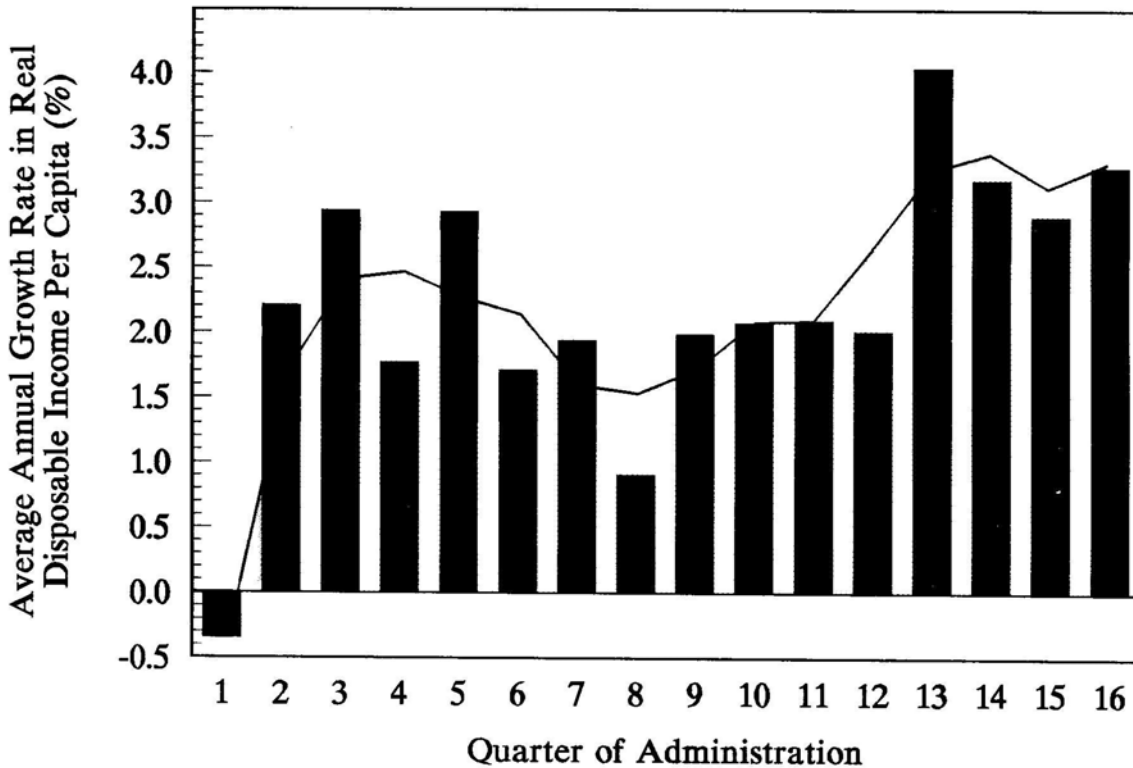## Figure 2: Income Growth through the Electoral Cycle

Figure 3: FDR vote 1936 vs. 1932
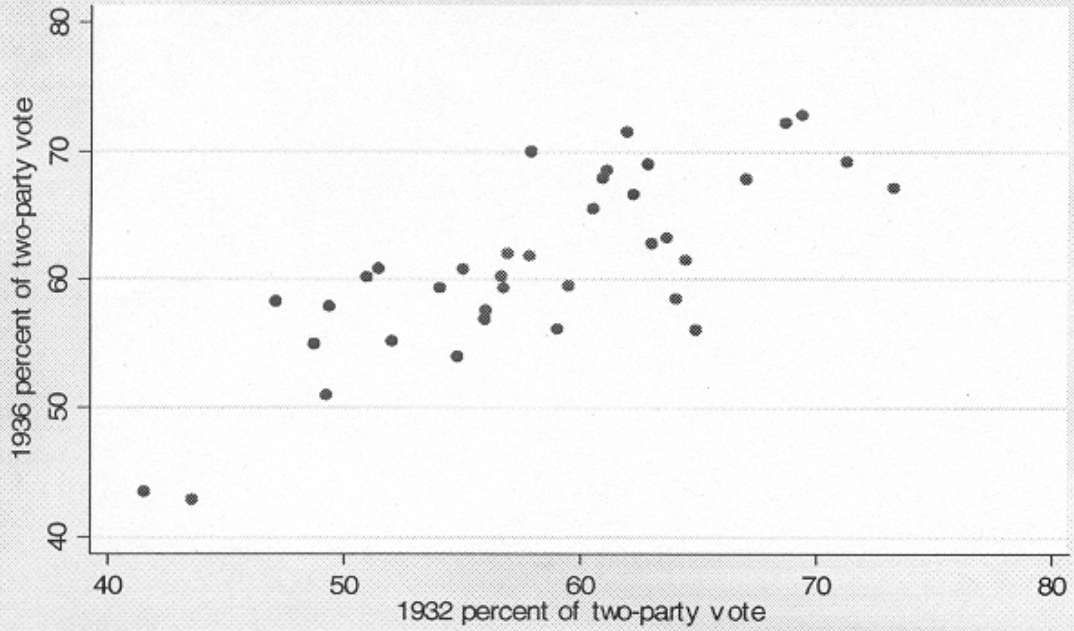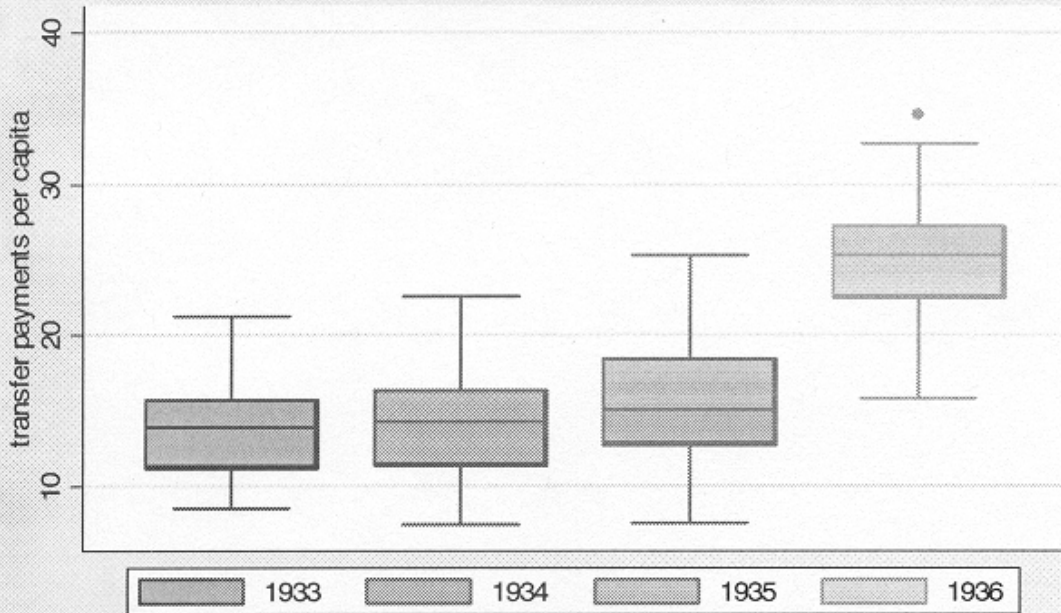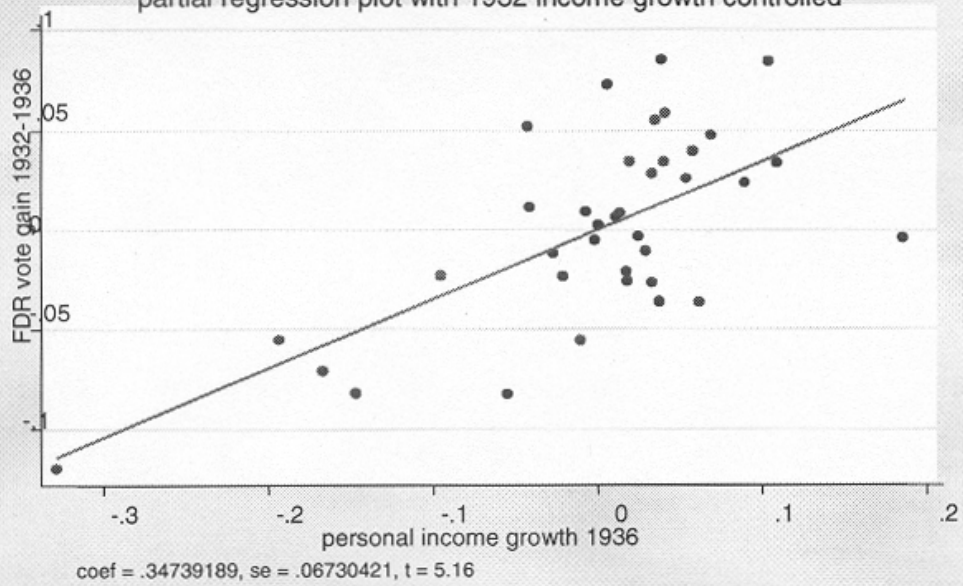### 37 non-southern states

Figure 4: Government transfer payments 1933-1936
37 non-southern states

Fig. 5: FDR Vote and State Income Growth 1936
partial regression plot with 1932 income growth controlled

coef = .34739189, se = .06730421, t = 5.16

Figure 6: Mayoral Terms and Support for Fluoridation