The Rising Incumbent Reelection Rate: What's Gerrymandering Got to Do With It?*

John N. Friedman University of California at Berkeley
Richard T. Holden MIT Sloan School of Management

The probability that an incumbent in the U.S. House of Representatives is reelected has risen dramatically over the last half-century; it now stands at nearly 95%. A number of authors and commentators claim that this rise is due to an increase in bipartisan gerrymandering in favor of incumbents. Using a regression discontinuity approach, we find evidence of the opposite *effect. All else equal, changes in redistricting have reduced the probability of incumbent reelection over time. The timing of this effect is consistent with the hypothesis that legal constraints on gerrymandering, such as the Voting Rights Act, have become tighter over time. Incumbent gerrymandering may well be a contributor to incumbent reelection rates, but it is less so than in the past.*

In each of the four Congressional elections up to 2004, more than 97.9% of incumbents who ran again were reelected. Indeed, there has been a noticeable upward trend in incumbent reelection rates over the last half century (see Figure 1). Many have seen this as a worrying trend; for instance, in one article these facts led *The Economist* to compare the state of democracy in America to that in North Korea.[1] Of course, the increasing rate of incumbent success is not necessarily problematic. Tocqueville, for instance, in his epic work *Democracy in America*, noted that

> ... preventing the re-election of the chief magistrate would deprive the citizens of the surest pledge of the prosperity and the security of the commonwealth; and, by a singular inconsistency, a man would be excluded from the government at the very time when he had shown his ability in conducting its affairs.[2]

Regardless of one's stance on the desirability of the rising incumbent reelection rate, it is natural to ask what has caused this trend. Legal scholars and public intellectuals seem to have little doubt that redistricting—specifically incumbent-protecting gerrymandering—is the culprit. They argue that technological improvements, bearing on the redistricting process, have effectively allowed representatives to choose their voters, rather than the converse. The following quotations are instructive.

> "Although elections may be uncompetitive for many reasons – including money in politics and the declining prestige of political service – the role of incumbent protection through the redistricting process is undeniable thanks to the wizardry of computer programs that draw incumbent-safe districts with ease." Common Cause.[3]

*We wish to thank John Geer (the editor), three anonymous referees, Alberto Alesina, David Cutler, Rosalind Dixon, Edward Glaeser, Larry Katz, Gary King, Ilyana Kuziemko, Emily Oster, participants in seminars at Harvard University for helpful discussions and suggestions, and Gary Jacobson for providing us with Congressional election data.

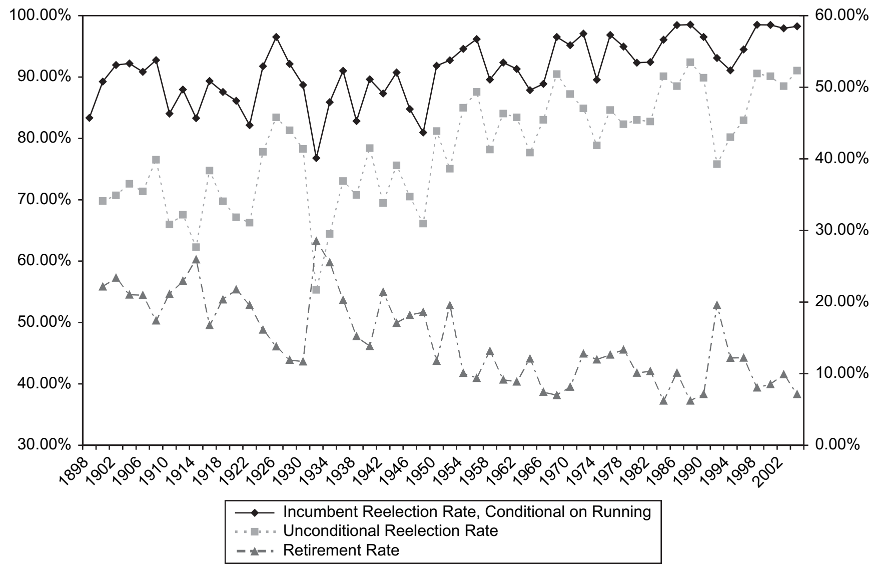
[1]"Pyongyang on the Potomac? ; The congressional elections," The Economist, September 18, 2004.

[2]It must be noted that, on balance, Tocqueville had a negative view of the possibility of reelection on the President. He states "But by introducing the principle of re-election they partly destroyed their work; and they rendered the President but little inclined to exert the great power they had vested in his hands. If ineligible a second time, the President would be far from independent of the people, for his responsibility would not be lessened; but the favor of the people would not be so necessary to him as to induce him to court it by humoring its desires. If re- eligible (and this is more especially true at the present day, when political morality is relaxed, and when great men are rare), the President of the United States becomes an easy tool in the hands of the majority. He adopts its likings and its animosities, he hastens to anticipate its wishes, he forestalls its complaints, he yields to its idlest cravings, and instead of guiding it, as the legislature intended that he should do, he is ever ready to follow its bidding. Thus, in order not to deprive the State of the talents of an individual, those talents have been rendered almost useless;and to reserve an expedient for extraordinary perils, the country has been exposed to daily dangers." ([1835] 2004, 137).

[3]"Democracy on its head" by Pamela Wilmot, Executive Director, Common Cause Massachusetts.

The Journal of Politics, Vol. 71, No. 2, April 2009, Pp. 593–611 doi:10.1017/S0022381609090483
 ISSN 0022-3816

FIGURE 1 Incumbent Reelection Rates



> "Bipartisan gerrymandering is emerging as a new, equally serious but different kind of threat to American democracy. Congressional elections in the wake of the 2000 round of redistricting were the least competitive of any general elections in United States history, with redistricting a central reason . . . Bipartisan gerrymanders increasingly make election day for representative bodies an empty ritual." Pildes (2002).

Recently three states—Florida, Ohio, and California—held referenda on whether to place redistricting in the hands of bipartisan panels of retired judges. In the widespread press coverage of the issue a popular wisdom has emerged that gerrymandering is killing political competition in America and rendering intractable problems which require bipartisan support. Thomas Friedman of the New York Times put it this way:

> "And it is the yawning gap between the huge problems our country faces today—Social Security reform, health care, education, climate change, energy—and the tiny, fragile mandates that our democracy seems able to generate to address these problems that is really worrying. Why is this happening? Clearly, the way voting districts have been gerrymandered in America . . . is a big part of the problem."[4]

The evidence presented to support these claims appears to be that, over the past two decades, technology available to redistricters has become more sophisticated and, over the same time frame, the incumbent reelection rate has risen. *Post hoc ergo propter hoc*.[5]

In contrast, the political science literature could not be clearer that gerrymandering is not the culprit. A series of papers have carefully investigated the components of the incumbent advantage. Ansolabehere and snyder (2002) use the change in districts after census years to distinguish between the incumbent advantage for "old" voters (those voters previously in a Representative's district) and "new" (recently added) voters. They find that two-thirds of the incumbent advantage is concentrated among "old" voters. Levitt and Snyder (1997) find that pork barrel spending in a district helps incumbents, while Levitt (1994) suggests that campaign spending has little impact on the outcomes of Congressional races.

The literature has had modest success isolating the causes of the rising incumbent reelection rate, but has been uniform in its dismissal of redistricting as a cause of the change. Ansolabehere, Snowberg, and Snyder (2004) argue that the introduction and proliferation of television cannot explain the rising incumbent reelection rate. Levitt and Wolfram (1997) argue that decreasing challenger quality has been the primary driver of the rise. Cox and Katz (1996, 2002) claim that the cause is the interaction between gerrymandering and challenger quality. Ansolabehere and Snyder (2002) and others show that the incumbency advantage in non-redistricting offices, such as U.S. Senators and

[4]Thomas L. Friedman, "Thou Shalt Not Destroy the Center", *New York Times*, November 11, 2005.

[5]From the Latin meaning "After this therefore because of this."

state governors, grew at roughly the same rate and at the same time as for redistricted offices. Burnham (1970) and Gross and Garand (1984), among others, show that the proportion of ''marginal districts'' has declined over time in ways that are inconsistent with redistricting as an explanation. Similarly, Gelman and King (1994) estimate changes in the ''responsiveness'' and ''bias'' of districting plans as affected by redistricting and find that redistricting tends to increase electoral responsiveness, implying that each party's share of seats in the legislature is more sensitive to changes in its underlying vote share.

In this paper we bring a more complete dataset and newer techniques to the question. We exploit the fact that, until 2004, redistricting (that was not court ordered) took place only once each decade. On the other hand, secular trends in such matters as campaign finance, voter polarization, and the media evolve in a continuous fashion. Thus, following van der Klaauw (2002), we are to able identify the impact of gerrymandering based on this discontinuous treatment. As in van der Klaauw's original application, we separate the changes in incumbent reelection rates into smooth, continuous changes and jumps between discrete buckets, in our case redistrictings. Our analysis follows in the spirit of Ferejohn (1977) and more recently Abramowitz, Alexander, and Gunning (2006) who compare reelection rates in years immediately before redistrictings with those immediately after and find little difference. Because we use all years of data, though, we can distinguish between the discreet impact of redistrictings and more gradual changes in the reelection rates, such as those which might follow an increase in polarization, for instance. We can also allow for different short run and long run effects of redistricting, as well as controlling for other covariates.

We find that a smooth function in time explains more than 100% of the increase in the incumbent reelection rate, while the decennial discontinuities are negative. This runs counter to the popular sentiment about the impact of gerrymandering. It implies that gerrymandering has become *less* incumbent-friendly over time.[6] We also test for differences in incumbent reelection rates between redistrictings that occurred during partisan or bipartisan governments, and we can find no significant difference. As an independent check, we also examine how retirement rates vary over time with redistricting and find that incumbents retire in increased numbers at the time of redistricting and especially when redistricting makes reelection more difficult.

Although technology available to gerrymanderers has unquestionably improved over time (see, e.g., Brace 2004), so have the constraints placed on them by statute and Supreme Court rulings. Our results suggest that the latter force has been the more powerful one. Furthermore, we find a large and statistically significant negative discontinuity before the 1992 round of redistricting. A natural interpretation of this is that the Voting Rights Act 1982 (Amended) significantly constrained gerrymanderers. This legislation came into force *after* the 1982 round of redistricting but prior to the 1992 round.

The focus of this paper is to assess the impact of changes in redistricting on the incumbent reelection rate. Since we conclude that redistricting is not to blame, one naturally wonders what is. This is an important and interesting question, but one which is beyond the scope of our present inquiry.

The rest of this paper is organized as follows: first we present and discuss the relevant legal and political background. The next section describes our empirical methodology and data sources, and then we present the main empirical results in this paper and some robustness checks. Finally, we conclude and discuss the broader implications of our work.

Background

In this section we make three points. First, we ground the basic ideas by distinguishing between partisan and incumbent gerrymandering. Second, we describe some of the technological advancements which have made gerrymandering (of both types) more effect over time. Third, we describe the legal backdrop against which gerrymandering takes place—and argue that the entry of the United States Supreme Court in the ''political thicket'' in the early 1960s, the Voting Rights Act of 1965, and then the amendments to it in 1982 have increasingly constrained gerrymanderers.

The literature on gerrymandering distinguishes between partisan and incumbent gerrymandering. Partisan gerrymandering is the redrawing of political lines to favor a particular political party. Incumbent gerrymandering is the redrawing of boundaries in a bipartisan manner, in order to benefit incumbents on both sides of the aisle.

The advent of sophisticated map and computer technology means that legislators can draw districts more finely than ever before. In the 1970s, districting

[6]While other factors that evolve more smoothly over time are the real culprits.

plans were extremely labor intensive to create and difficult to change. Constructing a plan literally required hours of drawing on large floor-maps using dry-erase markers. Now lawmakers use Census TIGER-Line files to create and analyze many alternative districting schemes both quickly and accurately. This allows very granular analysis and fine tuning of districts. For instance, the Florida 22nd congressional district comprises a coastal strip not more than several hundred meters wide in some places but ninety miles long. The Illinois 4th is drawn to include large Hispanic neighborhoods in the North and South of Chicago but not much in between; in some places the district is no more than one city block wide, and such necks are often narrower than 50 meters. Other similar examples abound.

Article I, §4 of the United States Constitution leaves election law to the states, subject to regulation by Congress. For a long time this meant that incumbent gerrymandering was constrained only by state election law and state constitutions. In 1962, however, the Supreme Court ruled in *Baker v. Carr* 369 U.S. 1862 (1962) that violations of one-person-one-vote violated the Equal Protection Clause of the Fourteenth Amendment. In *Wesberry v. Sanders* 376 U.S. 1 (1964) the Court further held that Congressional districts must contain populations which are "as nearly equal as possible"—and that Federal Courts were empowered to impose their own district plan as part of their remedial powers. The Court subsequently applied a similar standard for state legislative districts in *Reynolds v. Sims* 377 U.S. 533 (1964) and for local government districts in *Avery v. Midland County* 390 U.S. 474 (1968). As a consequence of these decisions, incumbent gerrymandering is constrained so as not to violate the Equal Protection Clause.

A second set of additional constraints were imposed on incumbent gerrymandering by the 1982 amendments to the Voting Rights Act ("VRA"). The original (1965) VRA had mixed success in curbing various practices of racial vote dilution. The *constitutional* prohibition (established in *Baker v. Carr*) of vote dilution was subject to the so-called "discriminatory purpose test" which the Court delineated in *Washington v. Davis* 426 U.S. 229 (1976). This made it extremely difficult for plaintiffs, since they had to show that a particular practice was *intentionally* discriminatory. This high burden was, in practice, almost never met—see for instance *Nevett v. Sides* 571 F.2d 209 (5th Cir. 1978), *cert. denied* 446 U.S. 951 (1980) and *City of Mobile v. Bolden* 446 U.S. 55 (1980). As Issacharoff, Karlan, and Pildes (2002) note, "After the Supreme Court decided *Bolden*, vote dilution litigation virtually shut down." The 1982 amendments to section 2 of the VRA were important because they removed the requirement that plaintiffs show a discriminatory *purpose*. The test which the Court adopted in interpreting the amended VRA, in *Thornburg v. Gingles* 478 U.S. 30 (1986), made the plaintiff's burden in vote dilution litigation substantially lighter. This constrained incumbent gerrymandering since, if racial vote dilution was a by-product of such gerrymandering, the districting plan may be rejected.

Empirical Strategy and Data

Empirical Methodology

There are many potential drivers of an incumbent's reelection chances. If we could accurately measure each of these variables, we could control for them to recover the effect of gerrymandering in those years when redistricting took place. Some of these variables we can measure: for instance, we control directly for economic variables, seniority, and the political cycle of midterm and presidential election years. Unfortunately, though, most of these variables are difficult, if not impossible, to measure. For instance, even the most well-designed measure of general public confidence in government would be unlikely to remove concerns about omitted variable bias.

Instead, this paper identifies the effect of gerrymandering by separating smooth changes from discrete jumps in the probability of incumbent reelection. Before 2004, states only redistricted (with a few exceptions) preceding Congressional elections that followed Census years.[7] Thus, the primary impact of gerrymandering should appear as a jump in the probability of reelection in the election immediately following the decadal redistricting. In contrast, most of the other variables that may affect the reelection probability vary more smoothly over time. By including both a flexible continuous function and a

[7]There are two classes of exceptions to this pattern. First, Maine, Hawaii (in 1982), and Montana (in 1984) conducted the Constitutionally mandated decadal reredistricting in off-years. We take account of these issues of timing in our empirical specifications. Second, federal courts (after *Wesberry*) occasionally declared particular districts unconstitutional in the middle of a decade, resulting in states being required to redraw boundaries. Such changes were always directed at precisely the problems identified by the courts, and, as such, did not much affect the composition of districts.

step-function, with its jumps at redistricting years, in a regression, we can separate the impact of redistricting while controlling for the other important variables which may also have changed over time.[8] This technique mirrors that applied by Lee, Moretti, and Butler (2004) to the relationship between candidates, public support, and political positioning. The key identifying assumption is that all factors unrelated to gerrymandering either change smoothly over time, in which case they are "picked up" by our function in time, or are generally uncorrelated with the timing of redistricting over the sample, in which case they enter the model in the random shock ε_{rst}.[9]

Though much of the redistricting literature uses incumbent voteshare as the dependent variable, we instead focus on the outcome of the election, a dummy variable equal to 1 if the incumbent was reelected. We do so for two reasons. First, an incumbent's goal is to gain reelection, not necessarily to maximize voteshare. Thus, a simple dummy variable for reelection is a more direct measure of an incumbent's electoral success than voteshare. Second, because incumbents may not care about voteshare per se, it is less clear how it will respond to a favorable redistricting. For instance, if an incumbent appears unbeatable, voters may feel more able to vote nonstrategically, perhaps supporting a favored minor candidate or not turning out to vote at all. In this case, an incumbent's voteshare might actually decrease, though her probability of reelection would have increased. The major drawback to our measure is the inherent noise in a binary outcome variable relative to a continuous underlying measure. But this weakness biases the analysis against us, since the less precise estimation yields higher standard errors.[10]

Our primary specification is

$$Y_{rst} = \alpha + \beta X_{rst} + g_t + \gamma_t Gerry_{st} + \eta_s + \varepsilon_{rst}$$

where Y_{rst} is a dummy variable, equaling one if representative r in state s in year t is reelected to Congress, η_s denotes a vector of dummy variables for each state, g_t is the smooth flexible function in time discussed above, and X_{rst} is a vector of control variables, including: U.S. aggregate-level economic growth, a dummy variable for a midterm election, a dummy variable for being in the same party as the president, an interaction between these last two, and whether the incumbent is a first-time Congressman. We estimate a linear probability model here for simplicity, though we have replicated all our results using logit and probit models and the results are substantively unchanged. Finally, $Gerry_t$ denotes a vector of dummy variables that picks up the effect of each redistricting "scheme" within a state. In a state s that redistricted only in years 1972, 1982, etc . . . , a piece of this regressor would be

$$\begin{array}{cc} t = & \begin{array}{cccccccc} 1972 & \cdots & 1980 & 1982 & \cdots & 1990 & 1992 & \cdots \end{array} \\ \begin{pmatrix} Gerry_{s,1970} \\ Gerry_{s,1980} \\ Gerry_{s,1990} \end{pmatrix} = & \begin{pmatrix} 1 & \cdots & 1 & 1 & \cdots & 1 & 1 & \cdots \\ 0 & \cdots & 0 & 1 & \cdots & 1 & 1 & \cdots \\ 0 & \cdots & 0 & 0 & \cdots & 0 & 1 & \cdots \end{pmatrix} \end{array}$$

The coefficients measured on this system of dummies variables, denoted above by γ_t, estimate the marginal impact of each round of gerrymandering on the incumbent reelection probability. For instance, γ_{1970} measures the marginal impact of the 1970s round of redistricting relative to that in the 1960s, and so on.

We begin the empirical analysis by assuming that the "jump" from redistricting varies by decade but not with state-level political conditions. We then explore less restrictive assumptions, allowing the effect of gerrymandering to vary across different state political arrangements at the time of redistricting (that is, bipartisan vs. partisan vs. court ordered gerrymandering). It is important to note that γ_t can only measure *changes* in the impact of redistricting across states and time. Any constant or base effect is indistinguishable from the regression constant. Thus our results cannot speak to the outcome of a reform in which redistricting occurred solely though independent commissions.

Data

Our data primarily comprise historical records of Congressional elections. We construct a panel from 1898 through 2004 by combining a dataset compiled by Cox and Katz (ICPSR Study 6311) with one graciously provided to us by Gary Jacobson. These datasets provide information on the winner of each Congressional election, whether an incumbent (or more) was involved, and the party of the incumbent. These datasets also indicate whether the incumbent was a freshman.

[8] In particular, we use smooth cubic splines (following van der Klaauw 2002) to control for smooth changes.

[9] To control for the realistic possibility that these random omitted factors are correlated across space within a given year, we cluster by year in all our results.

[10] For readers interested in the estimates from our specifications using instead the incumbent vote share as the dependent variable, please view the unpublished web appendix at either of the authors' websites. There we present Figure 1A and Tables 3A and 4A, which are analogous to Figure 1 and Tables 3 and 4 in this paper.

We augment these data with a number of covariates. We gather data on real U.S. aggregate-level GNP growth from the Economic Report of the President (2005) (Table B-2, computed from Column 11: Real GNP).[11] We only have data on economic growth from 1914 to 2004; this becomes the "long" window in our dataset.

We also classify each redistricting since 1970 as Bipartisan, Court Ordered, Partisan Democrat, or Partisan Republican. To do so we researched the political situation in each state and the outcome of the redistricting process using a number of different sources.[12] If one party controlled all relevant branches of state government at the time of redistricting, we classify it as Partisan. If neither party controlled all relevant branches, then we classify it as Bipartisan. If a federal court actually implemented its own redistricting plan after the state government failed to do so, we classify it as Court Ordered. Some authors have similarly classified redistrictings, though have done so based on the actual outcomes of the political negotiations surrounding the process. But these judgements may not only be endogenous to the process, but tainted in hindsight by the actual outcomes of the elections that followed. We prefer our measure, as it relies solely on objective and preexisting political conditions.

The Impact of Gerrymandering

Summary Statistics and Basic Determinants of Incumbent Reelection Rate

Figure 1 displays the reelection rates of incumbents over the last century. The solid line represents the proportion of Representatives who won reelection, conditional on standing again for election and receiving the party nomination in the primaries.[13] The upward trend, especially over the past 50 years, is pronounced. The reelection rate, already quite high in 1950 at 91.82%, was 98.25% in the 2004 Congressional elections. Though the incumbent reelection rate in 2004 is not the maximum in the data set, the last decades have been, on average, the least hospitable times for challengers in the history of the nation.

This high reelection rate reflects more than merely an artifact of strategic retirement in the face of a tough election challenge. The lower, dotted line in Figure 1 recalculates the reelection probabilities for incumbents under more conservative assumptions: For this series, if an incumbent Representative retires before an election and a member of the opposite party ends up filling the seat, then we count the action as though the incumbent had stood for reelection and lost. Though still not accounting for losses in the primaries, this measure should *overestimate* the correction for strategic retirements, and thus provide a lower bound for the "true" reelection rates. This series tracks the first quite closely, though. Other factors must be driving this increase.[14]

One of the possibilities is that redistricting has changed over time to become more incumbent-friendly. Unlike other explanations, though, such shifts in gerrymandering have to have occurred before election years that follow the decadal census and not before other elections. Table 1 shows the timing of redistrictings since 1970. (Before 1964, many states did not redistrict to adjust for population changes—but when they did so, it occurred with a similar timing. Nearly all states were forced by federal courts to do so in the late 1960s. The standard redistricting cycle as we know it today begin after the census in 1970). While nearly all 50 states redistricted in 1972, 1982, 1992, and 2002, no more than 10 redistrictings occurred out of phase in any decade. Furthermore, these mid-decade boundary adjustments were often either scheduled off-cycle changes (as in Maine or Montana) or small district-specific adjustments in response to court decisions. We correct for these slight timing anomalies in our specifications.

In order to identify the impact of gerrymandering as precisely as possible, we include a number of control variables that could affect the incumbent reelection rate. Summary statistics for these variables, along with the main dependent variable, appear in Table 2. Since we also run regressions using only our data for 1972–2004, we provide summary statistics for this subperiod as well. Incumbents who run for reelection win just under 92% of the time in the full sample, and more than 95% of elections since 1972.

[11]Data from earlier years can be found in Alesina and Rosenthal (1995).

[12]These sources include a very helpful online state-by-state index of gerrymandering at www.fairvote.com, contemporary news articles from national and local sources, and Hardy et al. (1981).

[13]In practice, incumbent Representatives are challenged successfully in primaries so infrequently that this limitation is insignificant. This is a greater problem in the precivil rights South, where the real elections often occurred not even in the Democratic primaries (since a Democrat would always win) but even in a racially segregated Democratic party booster club.

[14]We have also replicated our regression results below using this alternative measure of incumbent defeat; the coefficients are substantively unchanged.

Table 1 Redistrictings Since 1970

Time Period	Bipartisan	Court Imposed	Partisan Democrat	Partisan Republican	No Redistricting
1972	17	8	10	8	6
Other 1970's	2	1	1	0	0
1982	11	11	13	7	6
Other 1980's	2	2	3	1	0
1992	15	13	12	2	7
Other 1990's	4	6	0	0	0
2002	18	5	13	6	7
Other 2000's	1	0	0	2	0
Total	70	46	52	26	26

Data compiled by the authors from www.fairvote.com, contemporary articles from LexisNexis, and Hardy et al. (1981). See Table A1 for details on this classification proceedure.

Our first covariate is Real GDP growth, measured in percentage points, for the election year. The economy grew at an average of 2.6% per year in our sample and at the faster rate of 3.05% since 1972. The variability of economic growth is also much lower in the more recent part of the sample period, since the past 30 years have not seen economic conditions as extreme as those during the Great Depression or World War II.[15] Indeed there is a substantial literature exploring why this is the case (see, for example, Blanchard and Simon 2001).

Nearly 7% of incumbents in our sample are freshman, meaning that they have served, at most, one full term prior to standing for reelection. The number of new incumbents increases in the later years of our sample to more than 17%. Exactly one half of the observations in our sample come from midterm years, or those Congressional election years without a presidential election, though slightly fewer of our incumbents stand for reelection in midterm years, relative to presidential years after 1972. Approximately 52% of our incumbents are in the same party as the sitting president, but this number falls to 47% in the last several decades of our sample. Finally, though we do not include this characteristic as a covariate, approximately 56% of incumbents in our sample are Democrats.

Table 3 displays the results of regressions of incumbent reelection outcomes on our set of control variables. Column 1 simply regresses Y_{rst} on a linear time trend (in Congressional elections). The probability that incumbents are reelected to the House of Representatives has, on average, increased by 0.262 percentage points per election over the last 80 years. All coefficients have been multiplied by 100, so that they represent percentage points.[16] Column 2 shows that real economic growth (during the year of the election) is also a powerful predictor of movement over time, as noted by Kramer (1971); an additional 1 percentage point of economic growth increases the reelection rate by 0.221 percentage points. Column 3 adds a number of other variables to the regressions. "Freshman" incumbents are significantly more likely to suffer defeat than more senior incumbents, a crude measure of the more generally positive effect of tenure explored in the literature (i.e., Alford and Hibbing 1981 and Dawes and Bacot 1998). There is also a pronounced political cycle, as the literature has well established. In nonpresidential election years—that is, midterm elections—incumbents in the party of the president are 11.263 percentage points less likely to be reelected than those in the opposition who are 5.579 percentage points more likely to win than an average incumbent in a presidential election year. The difference in presidential election years in less pronounced. As predicted by Alesina and Rosenthal (1989, 1995), economic conditions have less predictive power when controlling for the political cycle. In the later years of our sample, though, economic growth does have an impact on the incumbent reelection rates. Finally, we allow for an additional impact of economic growth when growth is negative.

We do not control for challenger quality in our specifications. Many studies have found this has much

[15]In other specifications not reported here, we included national economic growth in the year preceding the election, as well as state-specific economic conditions since 1960. Neither variable added much explanatory power or materially affected the coefficients of interest.

[16]The literature has commented on this trend at least since Erikson (1971), though it has focused more on the "incumbency advantage," traditionally defined the additional vote share garnered by an incumbent relative to an otherwise similar nonincumbent.

Table 2 Summary Statistics

	Mean	Std Dev
Period: 1914–2004		
Incumbent Wins	0.918	0.275
Real GDP Growth	2.601	5.949
Freshman?	0.068	0.252
Party in Power?	0.523	0.499
Midterm?	0.500	0.500
*Midterm * In Power*	0.279	0.448
Incumbent is Democrat?	0.557	0.497
N	17143	
Period: 1972–2004		
Incumbent Wins	0.956	0.205
Real GDP Growth	3.049	2.252
Freshman?	0.177	0.382
Party in Power?	0.468	0.499
Midterm?	0.471	0.499
*Midterm * In Power*	0.230	0.421
Incumbent is Democrat?	0.567	0.496
N	6601	

The incumbent data come from Gelman and King (1994) (ICPSR Study #6311) and, for more recent years, from Gary Jacobson. The national growth data are from Alesina and Rosenthal (1995) and the Economic Report of the President (2005).

predictive power on electoral outcomes, and even that movements in this variable over time have contributed towards the increase in the reelection rate (Cox and Katz 1996, 2002, Levitt and Wolfram 1997). These effects from the quality of challengers may not be an alternative explanation, though, but rather a channel through which gerrymandering affects elections. Thus, we do not include challenger quality as a control, so as to capture the full impact of redistricting.

In the primary specifications below, we use more complicated functions in time rather than a simple linear trend. Column 4 of Table 3 uses a smooth cubic spline to control for shifts over time, and the other coefficients are not substantively different. The same is true in Column 5 with the addition of state-specific dummy variables. Columns 6 and 7 repeat these final two specifications in the "short" window. Economic growth has a much larger impact in these later years, while the political cycle is less pronounced, though still significant.

Main Results

The primary results in this paper appear in Table 4.

Column 1 implements our base empirical strategy, including both a time trend and decadal jumps for redistricting as explanatory variables. We also include all of the control variables from Table 2, so this regression mirrors that in Table 3, Column 3. If changes in the incumbent bias of gerrymandering were responsible for the entire increase in reelection rates, then the decadal jumps would be positive, on average, and the coefficient on the time trend would be zero. We find just the opposite; the time trend is positive and statistically significant, while the decadal jumps are, on average, negative. Three decades show statistically significant shifts against incumbent reelection, while none in favor of it.

Column 2 replaces the time trend with a three-part smooth cubic spline. To do so, we divide the sample into three equally sized time periods. We then estimate a separate cubic function in time over each period, requiring only that the aggregate function be continuous and that it have a continuous first derivative at both knots. Thus, we estimate a linear term for the entire sample and independent quadratic and cubic terms for each sub-period, so that our smooth function in time is seven-dimensional. The coefficients measuring the size of the discontinuous jumps associated with redistricting became slightly more negative, on average.

Columns 4 and 5 present the specifications from Columns 1 and 2 in the "short" window, beginning in 1972. Because there are fewer election years in the sample, the smooth cubic spline now includes only a single knot. The results, however, are remarkably similar to those for the "long" window. The time trend or smooth cubic spline still accounts for more than all of increase in the incumbent reelection rates, while the discontinuous jumps associated with redistricting are, on average, negative.

Figure 2 provides a graphical representation of the results from Column 2 in Table 4. The thick dark line represents the actual reelection rate for incumbent Representatives since 1914, taken from Figure 1. The lighter lines represent the predicted probabilities from Column 4, adjusted to begin from the same point in 1914. The upper-most line plots the smooth cubic spline. It far outpaces the actual reelection probability, suggesting that the many factors which likely changed continuously over time, such as money in politics, confidence of the electorate in politicians, and the quality of representative-to-district matching, account for more than all of the increase in incumbent reelection rates. The lower-most step function represents the impact of changes in redistricting, as captured through discontinuous jumps after the decadal census, which is negative in all decades except the 1920s and 1950s. The lighter line in the middle is the combination of the smooth cubic spline and the step function, not including state fixed effects or

TABLE 3 Basic Determinants of Incumbent Reelection Rates

	Dependent Variable: Prob(Incumbent Reelection)						
	Period: 1914–2004					Period: 1972–2004	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Time Trend	0.262**	0.254**	0.230**				
	(0.040)	(0.037)	(0.033)				
Growth		0.221**	0.040	0.184	0.181	0.615*	0.614*
		(0.093)	(0.119)	(0.149)	(0.150)	(0.267)	(0.268)
Growth < 0			0.355	0.116	0.118	0.585	0.589
			(0.225)	(0.282)	(0.282)	(0.679)	(0.679)
Freshman?			−2.854**	−3.167**	−3.041**	−3.424**	−3.224**
			(1.270)	(1.202)	(1.286)	(1.115)	(1.072)
Party In Power?			1.474	1.533	1.324	−0.683	−0.651
			(2.882)	(2.864)	(2.855)	(1.316)	(1.309)
Midterm Election?			5.579**	5.684**	5.216**	3.572*	3.522*
			(1.824)	(1.731)	(1.656)	(1.259)	(1.227)
*In Power * Midterm*			−11.263**	−11.252**	−10.435**	−6.500*	−6.426*
			(3.374)	(3.349)	(3.288)	(3.018)	(2.977)
Smooth Cubic Spline?	*No*	*No*	*No*	*Yes*	*Yes*	*Yes*	*Yes*
State Fixed Effects?	*No*	*No*	*No*	*No*	*Yes*	*No*	*Yes*
N	17143	17143	17143	17143	17143	6601	6601

All Standard Errors are clustered by year. * and ** denote statistical significance at the 5% and 1% level, respectively. Election data are from ICPSR Study #6311 (Gelman and King) and Gary Jacobson. Growth data are taken from Alesina and Rosenthal (1995) and the Economic Report of the President (2005). The "Smooth Cubic Spline" has two evenly spaced breakpoints (one breakpoint for the shorter period) and is continuous with a continuous first derivative. "Party in Power?" equals one if a member of the incumbent's party is President at the time of the election.

Table 3 covariates. When combined, the great increases in the smooth cubic spline and the large decreases from redistricting balance out and account for 100% of the actual increase in the reelection rate over the past 80 years. But the implication of this breakdown is clear: The direct effect of changes in incumbent gerrymandering, captured by discontinuous jumps after redistrictings, cannot account for the rise in incumbent successes.

Though the point estimates of the effect of redistricting are negative, perhaps more important is the size of effect that we can reject. Columns 3 and 6 reconfigure the redistricting effect to measure the cumulative effect of the discontinuous jumps rather than the marginal impact in each decade. Column 3 shows that we can easily reject an effect in 2000 (relative to that in 1910) greater than zero, and, with 95% confidence, we can reject an effect greater than −5.85. Of course, measuring the relative impact of redistricting over such a long horizon may be less informative than concentrating on the past 30 years. Column 6 displays the aggregate changes in the impact of gerrymandering relative to that in the 1970s, now rejecting an effect greater than −0.892 with 95% confidence. The implication of this breakdown is clear: changes in redistricting cannot explain the increase in the incumbent reelection probability over the past several decades or even the twentieth century.

Though we directly control in our regressions for many of the drivers of the time-series volatility (such as economic conditions and the political cycle), there are surely other unobservables which affect the incumbent reelection rate. Since the variation in the incidence of redistricting is mostly year-to-year, we cannot control for "year effects." For instance, our step function might reflect political scandals that randomly occurred in years ending in "2" rather than the true effect of redistricting. Furthermore, there may be serial correlation not accounted for by the parametric clustering procedure we used for our standard errors. In order to account for these possibilities, we perform a nonparametric permutation test that randomly selects "treatment" years. Since there were nine episodes of redistricting since the beginning of sample, we randomly select nine Congressional election years between 1916 and 2004 as "placebo" redistrictings.[17] We then estimate our full specification, as in Column 3 of

[17]We cannot identify the impact of a redistricting in the first year of the sample, since the effect would be measured in the regression constant. Since our full sample begins in 1914, the allowable range begins in 1916.

Table 4 Gerrymandering and Incumbent Reelection Rates

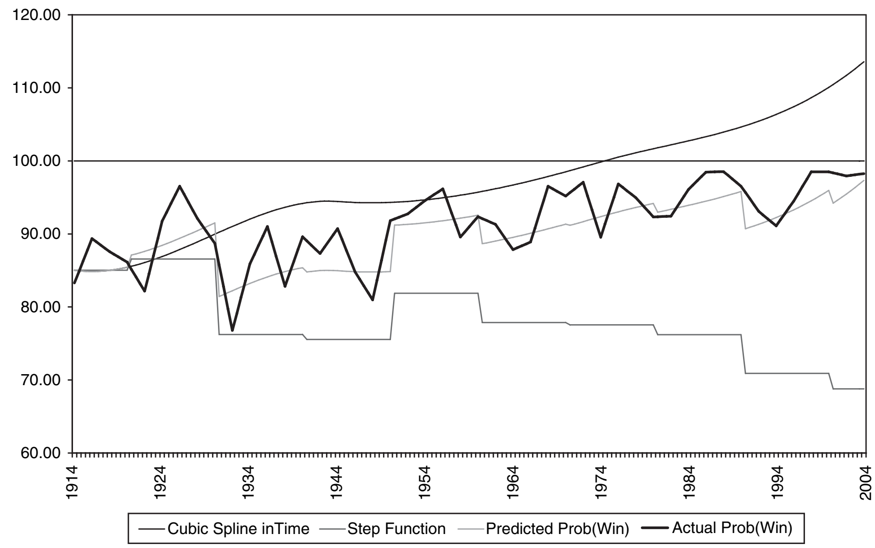
	Period: 1914–2004			*Period: 1972–2004*		
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Time Trend	0.589**			0.741*		
	(0.122)			(0.297)		
Redistricting:						
1920s	1.749	1.537	1.537			
	(1.929)	(3.556)	(3.556)			
1930s	−8.662**	−10.339**	−8.803			
	(1.937)	(2.972)	(5.510)			
1940s	−2.173	−0.675	−9.478			
	(2.046)	(3.330)	(5.288)			
1950s	4.507	6.335	−3.143			
	(3.111)	(3.992)	(5.759)			
1960s	−4.780*	−4.021	−7.164			
	(2.058)	(3.105)	(5.357)			
1970s	−0.040	−0.321	−7.485	*(omitted)*	*(omitted)*	*(omitted)*
	(1.957)	(3.527)	(5.239)			
1980s	−1.401	−1.343	−8.829	−1.911	−0.647	−0.647
	(1.276)	(1.832)	(4.924)	(1.651)	(2.672)	(2.672)
1990s	−3.999**	−5.289**	−14.118**	−5.242**	−7.826**	−8.473*
	(1.203)	(1.822)	(4.863)	(1.656)	(2.288)	(3.952)
2000s	0.870	−2.124	−16.241**	0.862	0.881	−7.592*
	(1.084)	(2.059)	(5.213)	(1.698)	(2.487)	(3.350)
Smooth Cubic Spline?	*No*	*Yes*	*Yes*	*No*	*Yes*	*Yes*
Table 3 Controls?	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
State Fixed Effects?	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
N	17092	17092	17092	6601	6601	6601

All Standard Errors are clustered by year. * and ** denote statistical significance at the 5% and 1% level, respectively. Election data are from ICPSR Study #6311 (Gelman and King) and Gary Jacobson. Growth data are taken from Alesina and Rosenthal (1995) and the Economic Report of the President (2005). The "Smooth Cubic Spline" has two evenly spaced breakpoints (one breakpoint for the shorter period) and is continuous with a continuous first derivative. "Table 2 Controls" include all non-time explanatory variables from Table 2, including economic growth, freshman indicators, and political cycle variables. "Redistricting Effect" measures the discontinuous jump in the probability that an incumbant wins reelection each time a state redistricts. Columns 3 and 6 measure the aggregate impact of redistricting in all previous decades rather the marginal effect for a particular decade. For ease of interpretation, the dependent variable has been multiplied by 100, and so a coefficient of 3, for instance, would indicate a 3 percentage point effect.

Table 4, including a three-part smooth cubic spline in time and the control variables from Table 3, and using the placebo treatment years. The coefficient of interest in these regressions is the cumulative effect of redistricting changes. The placement of our actual parameter estimate in the distribution of the placebo estimates, over many random draws of years, is a nonparametric p-value for this coefficient.

We execute this procedure and find that the distribution of the cumulative effect of redistricting is roughly symmetric, with a mean and mode slightly greater than zero. The actual estimate ($\beta = -16.241$) lies at the 17th percentile of this distribution. Thus, our estimated impact of redistricting lies below the vast majority of effects generated randomly. As one might expect, redistricting years alone do not make up the worst years in the past century for incumbents. For instance, the 1974 election (following Watergate and the OPEC Crisis) was very bad for incumbents; if one pretended that the 1970s redistricting occurred just before this election, rather than in 1971, then the cumulative effect of redistricting would instead lie at the 7th percentile of this distribution. Thus, a statistically significant cumulative effect of gerrymandering need not translate into an effect that lies below the 5th or 2.5th percentile of this distribution. But even if we take this overestimate of the true standard error, we can still rule out a large positive coefficient. Viewed from any perspective, these results cannot support the hypothesis that increases in gerrymandering caused the increase in the incumbent reelection rate.

Figure 2 Predicted Incumbent Reelection Rates



Robustness Check: Bipartisan Versus Partisan Gerrymandering

One factor for which we have not, as yet, controlled is the variation in priorities with which state governments redistrict based on the political circumstances. For instance, if no single party controls all relevant branches of the state government, then a compromise is usually in order. Such a case would generate a "bipartisan" gerrymander and might benefit all incumbents, regardless of party. Many popular writings blame the increase in incumbent reelection probabilities on this particular type of gerrymandering. On the other hand, if one party controls all involved parties of government, then that party may attempt a "partisan" gerrymander, in which that party attempts to oust a number of the opposing party's incumbents. Such an objective may even lower the probability of reelection for the majority party's incumbents in exchange for increasing the number of seats held by the majority in expectation. It could be that "bipartisan" gerrymandering has increased the advantage to incumbents over time, but a similar increase in the efficacy of "partisan" gerrymandering has offset that increase, on aggregate. It might also be the case that each type of gerrymandering has increased the incumbent reelection rates associated with that mode, but more states are conducting less incumbent-friendly "Partisan" gerrymanderings, creating a negative aggregate effect of redistricting.

Table 5 explores these possibilities. We could only classify the motivations of states in redistrictings since 1970, and so our regressions therefore focus on the "short" window.[18] For easy comparison, Column 1 replicates the results from the main specification in Column 5 of Table 4.

Column 2 of Table 5 includes fixed effects for the different types of redistricting discussed above (the omitted category is "No Redistricting"). This specification allows all states with "bipartisan" gerrymanders to have higher average incumbent reelection rates than other states, for instance, but keeps the decadal jumps constant across all states. Since the mode of redistricting does not change frequently within a state, we do not include state fixed effects in this specification. The coefficient estimates for the decadal jumps are not changed substantively, nor is there any indication of a significant difference in incumbent reelection probabilities across types of redistricting. Column 3 repeats the specification from Column 2, but includes state fixed effects, so that the "Redistricting Type" effects are identified solely from changes in the mode of gerrymandering within a state.

[18]One potential objection to this procedure is that we count each new redistricting as equivalent. For instance, a court drew districts for New York in 1992, but the legislature was forced to slightly modify the plan in 1998 by another federal court ruling. Since the bipartisan government accomplished this redrawing itself, for the purposes of Table 6, we count this as New York shifting from "Court Ordered" to "Bipartisan" redistrciting. Such mid-decade court-mandated redistrictings may not provide the same opportunity for gerrymandering as those at the beginning of each decade. To see if this issue affects our results, we reran the regressions in Table 6 eliminating all "minor" mid-decade redistrictings. Our intention was to retain those redistrictings which were simply a belated resolution of the initial decadal process (as in New Jersey in 1984) but remove more minor changes (such as New York in 1998). Results were substantively unchanged.

TABLE 5 Gerrymandering Types and Incumbent Reelection Rates

	Dependent Variable: Prob(Incumbent Reelection)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Redistricting:							
1980s	−0.647	−1.238	−2.007	−0.993	−2.073	−0.759	−1.777
	(2.672)	(2.459)	(2.536)	(2.620)	(2.706)	(2.838)	(2.872)
1990s	−7.826**	−7.572**	−7.608**	−7.238**	−7.701**	−7.190**	−7.782**
	(2.288)	(2.092)	(2.126)	(2.013)	(2.088)	(1.892)	(2.000)
2000s	0.881	1.132	0.759	1.659	0.583	−0.555	0.126
	(2.487)	(2.392)	(2.370)	(2.378)	(2.313)	(2.471)	(2.470)
Redistricting Type Fixed Effects:							
Bipartisan		2.744	12.255	3.335	12.223	3.190	11.891
		(2.859)	(6.217)	(2.770)	(5.964)	(2.748)	(5.960)
Court Imposed		3.730	12.407*	3.351	12.477	3.195	12.018
		(3.067)	(5.840)	(3.355)	(6.074)	(3.424)	(6.120)
Partisan		3.815	13.673*	3.752	13.446*	4.218	13.531*
		(2.823)	(6.031)	(2.706)	(5.896)	(2.850)	(6.184)
Decadal Jump Differentials:							
Bipartisan				−0.793	0.069		
				(0.952)	(0.839)		
Partisan				−0.258	0.230		
				(0.745)	(0.696)		
Redistricting Interactions:							
1980s × Bipartisan						0.318	1.274
						(1.549)	(1.954)
1980s × Partisan						−1.623	−1.128
						(1.495)	(1.627)
1990s × Bipartisan						−3.348	−2.138
						(2.096)	(2.570)
1990s × Partisan						1.547	2.154
						(1.300)	(1.578)
2000s × Bipartisan						3.641	1.843
						(2.918)	(3.336)
2000s × Partisan						0.553	−0.684
						(1.799)	(2.287)
Smooth Cubic Spline?	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Control Variables?	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
State Fixed Effects?	*Yes*	*No*	*Yes*	*No*	*Yes*	*No*	*Yes*
N	6601	6601	6601	6601	6601	6601	6601

All Standard Errors are clustered by year. * and ** denote statistical significance at the 5% and 1% level, respectively. Election data are from ICPSR Study #6311 (Gelman and King) and Gary Jacobson. Growth data are taken from Alesina and Rosenthal (1995). Redistricting data compiled by the authors from www.fairvote.com, articles from LexisNexis, and Reapportionment Politics, ed. Hardy, Heslop, and Anderson (1981). All regressions include a two-part smooth cubic spline. The omitted decade is the 1970s; the omitted redistricting type is "No Redistricting." For ease of interpretation, the dependent variable has been multiplied by 100, and so a coefficient of 3, for instance, would indicate a 3 percentage point effect.

The estimated sizes of the discontinuities do not change much, but the results suggest that states with only one "At-Large" Congressional district may be less favorable towards incumbents. Though this is an intuitively plausible effect, since these states have no opportunity to conduct gerrymandering of any kind, the large estimated difference relies on just three states which have moved into or out of this state since 1970 (Nevada, Montana, and South Dakota) and so is not estimated precisely. All other types of redistricting appear equal in average reelection rate, as in Column 2.

Columns 4 and 5 allow for differently sized decadal jumps for partisan, bipartisan, and court-ordered redistrictings. For instance, the coefficient of –0.793 on "Bipartisan Decadal Jump Differentials" can be interpreted that, at the beginning of each decade, the jump associated with redistricting is 0.793 percentage points more negative for states conducting "bipartisan" gerrymanderings, as compared to those whose new districts were imposed by a court. (Since states without redistricting, by definition, have no decadal jumps, the omitted category here is a Court-Ordered redistricting). There is no evidence that the size of the decadal discontinuities varies across modes of gerrymandering in this systematic way.

Columns 6 and 7 allow further variation in the size of the decadal jumps, estimating an independent coefficient for each type of redistricting in each decade. Thus, relative to the average effect of redistrictings in the 1980s (measured by the 1980s fixed effect), and relative to the average effect in bipartisan gerrymanderings (measured by the bipartisan fixed effect), the bipartisan redistricting in the 1980s was 0.318 percentage points more favorable to incumbents. (The omitted category is a Court-Ordered redistricting in a given decade). None of these estimated coefficients here are statistically different from zero. Of course, the standard error bands in these regressions are quite wide, since we are attempting to estimate quite flexible models using somewhat limited data. But despite the low power of these tests, these results offer no evidence that differences in the reelection rates associated with the several modes of redistricting are important explanatory factors for the puzzle at hand. Like the other regressions, Table 5 suggests that we must look elsewhere to explain the general increase in incumbent reelection rates since 1970.

Tables 6 and 7 explore further specifications using our classification system for the motivations behind redistricting. Table 6 focuses on the possibility that an election immediately after redistricting is different for incumbents than later years under the same districting regime. For instance, one incumbent often must face another incumbent in a new district created by redistricting. Partisan gerrymanders may also cause incumbents to lose in the year of redistricting but face diminished competition thereafter. To distinguish between these one-time effects and longer term changes in the incumbent reelection rate, we include in these regressions a dummy variable for a "Redistricting Year," which is an election immediately after a redistricting.[19] Column 1 of Table 6 shows the main estimates over the full sample from Table 4, for ease of comparison. Column 2 includes a "redistricting year" fixed effect; the estimate is both statistically and economically insignificant. Columns 3 and 4 repeat this procedure over the shorter sample. Though the estimated effect of a redistricting year is larger than before, it remains insignificant at the 5% level. Furthermore, the point estimate is greater than zero, a finding which runs counter to the intuition that reelection rates fall in redistricting years, as parties attempt to oust opposing incumbents, but then increase afterwards; our results suggest that, if anything, just the opposite occurs.

In columns 2 and 4, we constrained the effect of a "redistricting year" to be constant across all types of gerrymandering. One might not expect this to be the case, though; elections immediately following bipartisan gerrymanders might be more favorable to incumbents, while those after a partisan redistricting might be worse for incumbents. Columns 5 and 6 allow the "redistricting year" effect to vary across different types of redistricting, but there are no significant differences. It does not appear that the years immediately following gerrymanderings are significantly different from other years.

Table 7 further investigates the dynamics which might occur around partisan gerrymanders. In particular, such a redistricting will likely have a different effect on the reelection probabilities of politicians in the majority party (which conducts the gerrymandering) than on the hopes of those in the minority party. Column 1 reproduces the results from Column 2 of Table 5, with redistricting type fixed effects, for ease of comparison. (As before, the omitted category throughout this table is "No Redistricting"). Column 2 splits the fixed effect for a partisan redistricting into separate coefficients for the majority and minority party; there is no significant difference between these estimates. Column 3 replicates the results from Column 2, including state fixed effects. To recall, this specification estimates the fixed effects for redistricting types solely from within state changes. As in Table 6, the overall effect of any redistricting (relative to "At-Large districts") on incumbents' reelection chances is much larger in this specification. But there is no significant difference between the effect for the majority and minority party.

Even though there is no average difference between majority and minority success rates under partisan redistricting regimes, an effect may still exist only in the first year after such a gerrymander (after which few minority representatives may remain). Thus, in the remaining columns, we include both a "redistricting year" effect, as in Table 6, in the regression, as well as

[19]In this specification, a "redistricting year" is the election after *any* redistricting, including both the primary decadal process and any mid-decade corrections required by courts. In results not reported in the paper, we have run these regressions with an alternative definition of a "redistricting year" as only the year following the primary decadal redistricting. The results are substantively unchanged.

Table 6 **Gerrymandering Year Effects and Incumbent Reelection Rates**

	Dependent Variable: Prob(Incumbent Reelection)					
	Period: 1914–2004		*Period: 1972–2004*			
	(1)	(2)	(3)	(4)	(5)	(6)
Redistricting:						
1920s	1.537	1.596				
	(3.556)	(3.591)				
1930s	−10.339**	−10.276**				
	(2.972)	(2.961)				
1940s	−0.675	−0.667				
	(3.330)	(3.344)				
1950s	6.335	6.493				
	(3.992)	(3.995)				
1960s	−4.021	−3.906				
	(3.105)	(3.047)				
1970s	−0.321	−0.676	*(omitted)*	*(omitted)*	*(omitted)*	*(omitted)*
	(3.527)	(3.415)				
1980s	−1.343	−1.634	−0.647	0.477	−1.097	−1.897
	(1.832)	(1.857)	(2.672)	(1.924)	(2.437)	(2.532)
1990s	−5.289**	−5.642**	−7.826**	−7.967**	−8.380**	−8.525**
	(1.822)	(2.019)	(2.288)	(1.642)	(1.845)	(1.885)
2000s	−2.124	−2.673	0.881	−1.837	−1.754	−2.436
	(2.059)	(2.236)	(2.487)	(2.792)	(2.732)	(2.468)
Redistricting Year Fixed Effect		0.522		1.790		
		(1.123)		(1.234)		
Redistricting Type × Redistricting Year						
Bipartisan						
					1.796	1.961
					(1.929)	(1.667)
Court Imposed					1.718	1.958
					(1.129)	(0.972)
Partisan					1.830	2.015
					(1.164)	(1.036)
Smooth Cubic Spline?	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Control Variables?	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
State Fixed Effects?	*Yes*	*Yes*	*Yes*	*Yes*	*No*	*Yes*
Redistricting Type FE?	*No*	*No*	*No*	*No*	*Yes*	*Yes*
N	17143	17143	6601	6601	6601	6601

All Standard Errors are clustered by year. * and ** denote statistical significance at the 5% and 1% level, respectively. Election data are from ICPSR Study #6311 (Gelman and King) and Gary Jacobson. Growth data are taken from Alesina and Rosenthal (1995). Redistricting data compiled by the authors from www.fairvote.com, articles from LexisNexis, and Reapportionment Politics, ed. Hardy, Heslop, and Anderson (1981). All regressions include a smooth cubic spline. The regressions with Regression Type *Redistricting Year fixed effects include the basic Redistricting Type fixed effects, which are omitted for ease of interpretation. For ease of interpretation, the dependent variable has been multiplied by 100, and so a coefficient of 3, for instance, would indicate a 3 percentage point effect.

a dummy variable indicating that a new partisan redistricting, directed against an incumbent's party, has just been instituted where none existed before. We denote this effect the ''Against'' effect. Note that this dummy variable measures only *new* partisan attempts; thus, the variable would equal 1 for Democrats in Texas in 2004, since the redistricting had been a court-ordered effort in 2002, but it would equal 0 for Democrats in Texas in 2012 if another republican gerrymander is put in place. Though an effect may be present in the latter situation, theory predicts it should be stronger in the former case, and so we wish to concentrate our measure of the effectiveness of partisan gerrymanders as much as possible. Approximately 4%

TABLE 7 Partisan Gerrymandering and Incumbent Reelection Rates

	Dependent Variable: Prob(Incumbent Reelection)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Redistricting:							
1980s	−1.238	−1.241	−2.013	−1.067	−1.874	−1.178	−2.034
	(2.459)	(2.459)	(2.530)	(2.407)	(2.518)	(2.410)	(2.530)
1990s	−7.572**	−7.588**	−7.621**	−8.409**	−8.542**	−8.404**	−8.545**
	(2.092)	(2.098)	(2.130)	(1.840)	(1.881)	(1.822)	(1.867)
2000s	1.132	1.139	0.767	−1.656	−2.374	−1.772	−2.480
	(2.392)	(2.394)	(2.371)	(2.699)	(2.434)	(2.537)	(2.306)
Redistricting Type Fixed Effects:							
Bipartisan	2.744	2.751	12.274	2.768	12.720	2.870	12.816
	(2.859)	(2.861)	(6.215)	(2.871)	(6.400)	(2.835)	(6.344)
Court Imposed	3.730	3.739	12.433*	3.737	12.826*	3.831	12.946*
	(3.067)	(3.069)	(5.836)	(3.066)	(6.043)	(3.034)	(5.985)
Partisan	3.815			3.906	14.187*	3.968	14.253*
	(2.823)			(2.897)	(6.229)	(2.867)	(6.182)
Partisan Redistricting For		4.004	13.866*				
		(2.852)	(6.014)				
Partisan Redistricting Against		3.544	13.447*				
		(2.896)	(6.097)				
Redistricting Year Effect				1.871	2.046	1.858	2.033
				(1.173)	(0.970)	(1.153)	(0.960)
"Against" Effect				−0.774	−0.574		
				(1.418)	(1.311)		
"Against" Effect, Republican						0.844	1.024
						(1.976)	(2.098)
"Against" Effect, Democrat						−4.125	−3.856
						(2.650)	(3.038)
Smooth Cubic Spline?	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Control Variables?	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
State Fixed Effects?	*No*	*No*	*Yes*	*No*	*Yes*	*No*	*Yes*
N	6601	6601	6601	6601	6601	6601	6601

All Standard Errors are clustered by year. * and ** denote statistical significance at the 5% and 1% level, respectively. Election data are from ICPSR Study #6311 (Gelman and King) and Gary Jacobson. Growth data are taken from Alesina and Rosenthal (1995). Redistricting data compiled by the authors from www.fairvote.com, articles from LexisNexis, and reapportionment Politics, ed. Hardy, Heslop, and Anderson (1981). All regression of include a two-part smooth cubic spline. The omitted decade is the 1970s; the omitted redistricting type is "No Redistricting." The "Against" effect represents elections in a redistricting year in which there has been a partisan redistricting against that particular incumbent. The "Against Effect," when broken out by party, denotes the party against whom districts were gerrymandered. For ease of interpretation, the dependent variable has been multiplied by 100, and so a coefficient of 3, for instance, would indicate a 3 percentage point effect.

of incumbents who have run for reelection since 1972 have done so under such circumstances.

Columns 4 and 5 include this "Against" effect, as well as a "Redistricting Year" effect. Neither coefficient is significantly different from 0, though the redistricting year effect is nearly so, as in Table 6. Columns 6 and 7 allow the "Against" effect to vary across parties; in both specifications, it appears more negative when Republicans gerrymander against Democrats, but this difference is not statistically significant. This result suggest that, on average, partisan redistricting may not be as effective as popularly thought.

Redistricting and Retirement Rates

All of our regressions thus far have examined the *outcome* of elections. But we can also analyze the choice to stand for reelection. For instance, if redistricting becomes more incumbent-friendly, we might expect incumbents to retire at an decreased rate in the following decade due to a decreased probability of losing. Alternatively, if redistricting becomes less favorable for incumbents, we may observe a sharp spike in retirements immediately after redistricting or a higher propensity to retire throughout the decade. We now look for each of these patterns in the data. We define the variable $retire_{rst}$ as a dummy variable equaling one if representative r in state s in year t does not stand for reelection in year t.[20] We first run the specification

$$retire_{rst} = \alpha + \beta X_{rst} + g_t + \gamma gerry_{st} + \eta_s + \varepsilon_{rst} \quad (1)$$

which mirrors the regressions above in identifying permanent shifts in the retirement rate at the time of a redistricting. We also run regressions of the form

$$retire_{rst} = \alpha + \beta X_{rst} + g_t + \delta R_{st} + \eta_t + \varepsilon_{rst} \quad (2)$$

where R_{st} is a dummy variable for redistricting occurring in a given state and year to measure "spikes" in the retirement rate during redistricting years. We also interact a time trend with R_{st} to see if the effect has changed over time. Figure 1 shows the retirement rate in each year of our sample (measured on the right-hand axis). The retirement rate begins around 20% at the very beginning of our sample and declines to just over 10% at the end, though there is considerable variation along the way. The mean of the retirement rate over the entire period is 16.2%. There are also noticeable spikes in the retirement rate in 1932, 1942, 1952, and 1992, each redistricting years. The year 1992 is especially notable as the final year in which certain incumbents could convert their campaign war chests into personal wealth; Figure 1 shows it as the highest retirement rate since before WWII.[21]

Results on strategic retirement appear in Table 8. Columns 1 displays results from equation 1 and investigates permanent changes to retirement rates at the times of redistricting. Retirements increased significantly at the time of redistricting in the 1930s, as well as in the 1990s. These findings are broadly consistent with our results on incumbent reelections in Table 4; redistricting hurt incumbents in these decades as well. In fact, the correlation between the coefficients in Column 1 of Table 8 and those in Column 4 of Table 4 is –0.68, which is significant at the 5% level.[22] Incumbents appear to retire in increased numbers precisely when redistricting makes reelection more difficult.

We next investigate whether retirement rates spiked in redistricting years. Column 2 estimates equation (2) and shows that, all else equal, the retirement rate increases by 4.311 percentage points in redistricting years. Column 3 allows this effect to vary linearly over time; the effect is decreasing, though not significantly. Column 4 combines the permanent effects from Column 1 and the temporary spikes from Column 2, and the results are qualitatively unchanged. It appears these two effects are independent of each other. Columns 5 through 8 run the same specifications in the years 1972–2004. The redistricting year "spike" is somewhat larger in this later period, and the increase in the retirement rate after 1990 is more pronounced as well. Column 8 estimates that the 2000s round of redistricting decreased the ongoing retirement rate, though this effect is estimated off of only two elections, and so the coefficient is quite noisy across specifications.

Taken together, these results support our findings above that redistrictings have not helped incumbents. Incumbents generally dislike running for reelection in redistricting years, as evidenced by the spike in retirements in those years, and the redistrictings in 1930 and 1990 have significantly and permanently increased the retirement rates. In addition, in regressions not shown, we investigated whether the type of redistricting in a given state, in a given decade, affected the results. We found no significant changes when we controlled for the type of redistricting, nor did these effects interact in any way with the type of redistricting. Though perhaps counterintuitive, these results further support our findings in Tables 5 through 7 where we found no significant evidence that the type of redistricting in a state had differential effects on the incumbent reelection rates.

Discussion and Conclusion

In this paper, we use a regression discontinuity design to measure the impact of changes in redistricting on the probability that incumbents are reelected. Though many factors contribute to the successes of

[20]Our data does not include information on primary elections, and this this variable will equal one both for incumbents who retire and for those who are defeated in the primary. Incumbents are rarely challenged in the primary, though, and even more rarely beaten, so we do not believe that this affects the analysis in a quantitatively meaningful way.

[21]In 1982, Congress barred incumbents from taking past campaign contributions as personal wealth, as has been the common practice upon retirement. The law exempted existing incumbents, though. In 1989, Congress voted to end this grandfather clause effective January 1993.

[22]The nonparametric exact p-value is 0.039.

Table 8 Redistricting and Incumbent Retirement

	Dependent Variable: Prob(Incumbent Retirement)							
	Period: 1914–2004				Period: 1972–2004			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Redistricting:								
1920s	−3.442			−4.232*				
	(2.507)			(1.855)				
1930s	13.847**			12.689**				
	(2.875)			(2.616)				
1940s	3.771			3.127				
	(2.857)			(2.473)				
1950s	−0.861			−2.625				
	(4.496)			(2.994)				
1960s	−3.542			−4.459				
	(3.142)			(3.127)				
1970s	0.913			−1.326	*(omitted)*	*(omitted)*	*(omitted)*	*(omitted)*
	(2.454)			(1.936)				
1980s	−4.980			−6.160**	−3.577			−2.856
	(3.075)			(2.190)	(2.178)			(1.795)
1990s	8.431*			6.170**	10.599**			7.757**
	(3.348)			(2.001)	(3.468)			(1.808)
2000s	3.555			−0.900	2.357			−6.960*
	(2.950)			(2.878)	(1.957)			(3.111)
Redistricting Year Fixed Effect		4.311**	6.158*	4.170**		5.877**	9.179**	5.866**
		(1.091)	(2.595)	(0.822)		(1.516)	(3.480)	(1.193)
Redistricting Year × Time Trend			−0.293				−1.578	
			0.410				(1.442)	
Smooth Cubic Spline?	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Control Variables?	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
State Fixed Effects?	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Redistricting Type FE?	*No*	*No*	*No*	*No*	*No*	*No*	*No*	*No*
N	19692	19692	19692	19692	7386	7386	7386	7386

All Standard Errors are clustered by year. * and ** denote statistical significance at the 5% and 1% level, respectively. Election data are from ICPSR Study #6311 (Gelman and King) and Gary Jacobson. Growth data are taken from Alesina and Rosenthal (1995). Redistricting data compiled by the authors from www.fairvote.com, articles from LexisNexis, and Reapportionment Politics, ed. Hardy, Heslop, and Anderson (1981). All regressions include a three-part (two-part for post-1972) smooth cubic spline. Control variables include those in Table 3 except those dummy variables indicating that the representative is a freshman, shares the party of the president, and the interaction of this second variable with a dummy variable indicating a midterm election. For ease of interpretation, the dependent variable has been multiplied by 100, and so a coefficient of 3, for instance, would indicate a 3 percentage point effect.

incumbents, most of these effects change continuously over time. Redistricting, on the other hand, occurs only sporadically, and usually after decadal Census years. Thus, we identify the effect of redistricting as a discontinuous jump when states redraw boundaries, while using a flexible but smooth function in time to control for the many potentially confounding effects.

Our results show that changes in redistricting have actually *reduced* the success rate of incumbents quite steadily over the past century. While the smooth function in time that we estimate increases somewhat faster than the raw time series, the discontinuous jumps from redistricting are usually negative and sometimes significantly so (as shown in Figure 2). These results are robust to the use of a number of different functional forms for the smooth function in time and actually get stronger when controlling for economic conditions, political cycles, and seniority. Furthermore, there is no evidence that this impact of redistricting can be explained by controlling for the difference between bipartisan, court-imposed, and partisan modes of gerrymandering.

How is this possible? We know that the technology involved in redistricting, as well as professionals' understanding of political demographics, has improved overtime. With ever faster computers and Census TIGERLine files, state legislatures can now draw finer districts than before, and redistricting consultants can give politicians a better idea of which citizens to group together to maximize political advantage. The effect of redistricting on incumbents' reelection chances has not increased over time, though.

One potential explanation of this seeming paradox is bounded rationality. Perhaps those charged with the responsibility of redistricting still do not understand this very complex problem well enough for new technology to have made a major difference. On the other hand, there are huge stakes on the line in each redistricting. The redistricting process in many states produces much rancor among legislators, and many parties seem willing to go to great lengths to prevent or force through redistrictings. The spectacle in Texas in 2003, including Democrat state representatives fleeing to Oklahoma to prevent a quorum and Congressman DeLay's use of the FAA to bring them back, is only the most recent and colorful of these stunts. Though politicians may not all understand the many nuances of redistricting, it seems that at least some know enough to make a serious difference. Hence, bounded rationality is not a compelling explanation for our findings.

Another, perhaps more appealing explanation is that the judicial restraints on gerrymandering have increased alongside technological capabilities. Courts have continually updated their practical interpretation of "equal population" for districts; while districts differing by as much as 2% in size passed the test in 1972, districts whose populations differed by as few as 17 people were invalidated by federal courts in 2002. Furthermore, the amendment to the Voting Rights Act, passed in 1982 but first affecting the redistrictings in the 1990s, restrained states even more in their treatment of minorities. It is always difficult to draw causal inference from what is, in essence, a single datum, but it seems no accident that this decade, the most active for redistricting litigation (as shown in Table 1), was also the least favorable to incumbents since 1972. More recently, though, federal courts have taken a more restrained approach to redistricting, opting, for instance, not to strike down the Pennsylvania partisan redistricting in *Vieth v. Jubelirer*.

Paradoxically, these legal constraints may also have interacted with the greater accuracy with which redistricting plans can be analyzed to further bind potential gerrymanderers.[23] The tightening constraint of "equal population" is one salient example of this phenomenon, but there are others. For instance, groups can now more easily challenge a proposed redistricting plan on some legal grounds (such as the elimination of majority-minority districts) by themselves designing an alternative plan. Increased computing power has also made it easier to compute indices of district compactness that have desirable properties (see Fryer and Holden 2007), and this may further increase the constraints on gerrymanderers.

So if not changes in gerrymandering, then what accounts for the observed increase in incumbent success at being reelected? Incumbents might have more access to the professional class of political operatives and campaign knowledge, decreasing the chance that a challenger could force her way into office. The change in the role of money—both legal and illegal—in politics over the past decades may have affected the fortunes of incumbents, as might have the increasing 24-hour coverage of political events in Washington, D.C., which provides free exposure for incumbents. On the other hand, Congressmen may simply be more suited to their jobs now than before, in which case citizens could be more satisfied with their Representatives. More work needs to be done to determine which factors have caused the rising incumbent premium—and to address the key question of whether it is a good or a bad thing.

Manuscript submitted 26 June 2007
Manuscript accepted for publication 16 August 2008

[23]We thank an anonymous referee for this point.

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John N. Friedman is Robert Wood Johnson Scholar of Economics, University of California, Berkeley, Berkeley, CA 94720.

Richard T. Holden is assistant professor of management, Massachusetts Institute of Technology, Cambridge, MA 02142.