Using Term Limits to Estimate Incumbency Advantages When Officeholders Retire Strategically[1]

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Abstract

Gelman and King (1990) and Levitt and Wolfram (1997) propose methods of measuring the incumbency advantage that have become widely used in the literature. Recently, however, critics have argued that these measures, as well as less sophisticated measures of incumbency advantage that preceded them, suffer from a failure to consider strategic retirements, and this may lead to as much as a 100in state executive and legislative elections as a natural experiment to correct for strategic retirment. We find that as an empirical matter strategic retirement is not substantively important. Estimates of incumbency advantages that take account of strategic retirement actually are marginally *larger* than those that do not.

Incumbents' vote margins in all U.S. state and federal elections have risen dramatically over the last fifty years. The estimated incumbency advantage has risen to fully 10 percentage points in the 1980s and 1990s, up from only 1-2 percentage points in the 1940s and 1950s.[1]

One line of scholarship argues that the level and increase in the incumbency advantage are much smaller than they appear. All estimates of the incumbency advantage contrast vote margins in incumbent contested seats and vote margins in open seats. Politicians' strategic career decisions can produce bias in these statistical estimates. Incumbents choose to retire when their electoral prospects look especially dim and staying put when they are assured of reelection.[2] Bias in conventional estimates of the incumbency advantage may result because researchers do not observe the vote that vulnerable incumbents would have received had they in fact stood for reelection.[3] Cox and Katz state the argument most forcefully, and claim that conventional estimates overstate the true value of the incumbency advantage by more than 100% (Cox and Katz, 2002, chapters 8-10).

Correcting for strategic retirements poses a thorny statistical problem. Regression models commonly used to estimate the incumbency advantage allow researchers to hold many factors constant, most importantly the normal vote and partisan tides. However, if incumbents are more likely to run for reelection when their electoral prospects are good, there will be a simultaneous relationship between incumbency and the *expected* vote. As a result, any factor, such as scandal, that strongly affects the vote but is not included in the regression analysis will produce bias in estimates based on differences of means or regression.[4]

[1]Most research has focused on U.S. House elections. See, for example, Erikson (1971, 1972), Cover (1977), Cover and Mayhew (1977), Alford and Brady (1989), Gelman and King (1990), Cox and Katz (1996), and Levitt and Wolfram (1997). Most recent studies of U.S. Senate and state executive and legislative elections show similar patterns of growth below the federal level. See, for example, Holbrooke and Tidmarch (1991), King (1991); Cox and Morgenstern, (1993, 1995), Erikson, Wright, and McIver (1993), and Ansolabehere and Snyder (2001).

[2]See Jacobson and Kernell (1983), Brace (1984), Kiewiet and Zeng (1993), Hall and van Houweling (1995)

[3]Gelman and King (1990, page 1152) discuss this point, but, they assert, controlling for the normal vote and party tides corrects this problem. For this to be a problem retirement decisions must depend on short-term factors such as scandals that the researcher does not observe but the candidates do.

[4]The direction of potential bias due to strategic retirement is in fact ambiguous. The OLS estimate will be too small if the factor that predicts retirement is positively correlated with incumbency but negatively

In this paper, we present estimates of the incumbency advantage that correct for strategic retirement (and possibly other problems) using the natural experiments that arise from term limits imposed on statewide executives and state legislators. Term limits create ideal conditions for a natural experiment because term limits are exogenous to the particular factors that make individual incumbents safe or vulnerable in a particular year. We use term limits to construct an instrumental variable estimator for the incumbency effect. We also apply this method to correct for endogenous challenger entry.

We study state elections from 1978 to 2000. Over this period, the use of term limits varies considerably across states, across offices, and over time. Term limits strongly predict retirement rates, and the variation across offices and over time allows us to assess the validity of the natural experiment. Importantly, because term limits act as valid instruments for the incidence of incumbent contested races, our approach potentially corrects for problems beyond strategic retirement, including measurement errors and ommitted variables. In addition, our analysis exploits the panel structure of state executive and legislative elections to control for the normal vote and national and state tides in elections.

The results of the analysis indicate that strategic retirement is less of a problem than is sometimes thought. We contrast the results of the natural experiment with conventional ordinary least squares estimates and find statistically significant evidence of bias in the regression estimates. However, the magnitude of the bias in regression analyses is substantively small—about one-percentage point of the vote. Importantly, the analysis contradicts the strategic retirement hypothesis directly: instrumenting for incumbency produces effects that are somewhat *higher* than the simple OLS estimators.

Section 2 of this paper presents the logic of the statistical design and the data used in the analysis. Section 3 presents the factors that predict retirement rates and analysis of the quality of the instrumental variables. Section 4 presents the estimated incumbency advantages. Section 5 applies our approach to challenger quality. Section 6 considers the

correlated with the vote. Scandals might be such a variable.

implications for our understanding of elections and methods for studying voting patterns.

2. Data and Methods

We study all partisan, statewide elections from 1978 to 2000 and state legislative elections from 1994 to 2000. We extend the specifications developed by Gelman and King (1990) and Levitt and Wolfram (1997) to incorporate instrumental variables for incumbency, indicators of challenger political experience, and alternative measures of the normal vote. A companion paper (Ansolabehere and Snyder, 2001) discusses the incumbency advantages and party normal votes produced by the Gelman-King and Levitt-Wolfram models applied to state executives and legislative races. Our focus here is on the hypothesis that strategic retirement produces substantial biases in regression models of the incumbency effect.

A. Statistical Model of the Incumbency Effect

All estimates of the incumbency advantage contrast the average vote in seats where incumbents are running for reelection with the average vote in seats where no incumbent is running. Retirement slump and sophomore surge, two early estimation approaches in this literature, compute the difference in the vote for a given seat between one election when an incumbent is running and another election when that individual is no longer an incumbent (*e.g.*, Erikson 1971, 1972; Alford and Brady, 1989). Regression models, such as Gelman and King (1990) and Levitt and Wolfram (1997), perform a similar contrast, but introduce statistical controls for the normal vote in the district and partisan tides.

The statistical model of the incumbency effect that we use divides the two-party vote into several components: normal party vote in a state or district, annual tides or swings, incumbency effects, and local idiosyncratic variation.

Let i index offices, j index states, and t index years. Let V_{ijt} be the share of the two-party vote received by the Democratic candidate running for office i in state j in year t. There are two incumbency variables: $D_{ijt}=1$ if the Democratic candidate running for office i in state

j in year t is an incumbent, and 0 otherwise; $R_{ijt} = 1$ if the Republican candidate running for office i in state j in year t is an incumbent, and 0 otherwise. Assuming the advantages are symmetricy, we can define $I_{ijt} = D_{ijt} - R_{ijt}$.[5] The specification relating Vote Shares to Incumbency is as follows:

$$V_{ijt} = \alpha_j + \theta_t + \beta I_{ijt} + \epsilon_{ijt} \quad (1)$$

The state fixed-effects (α_j) capture the underlying partisanship (normal vote) in each state or district, and the year fixed-effects (θ_t) capture national tides.

Existing research uses this framework to address the effects of partisan tides and the normal vote. Omitting these factors from the model can bias estimates of the incumbency effect (Gelman and King, 1990, pages 1143-1149). We include measures of tides, quality and normal vote in the data analysis, and discuss the measurement of these factors below.

No analysis has addressed directly the endogeneity of incumbency, which arises from strategic retirements and omitted factors. That is the focus of our research design, to which we now turn.

B. Design of the Natural Experiment

To correct for the endogeneity of incumbency we use the natural experiment created by term limits. The dependent variable in this natural experiment is the vote share receive by the incumbent party's candidate. The "treatment group" consists of those races in which an incumbent runs for reelection. The "control group" consists of those races in which no incumbent runs, open seats. This is analogous to a clinical trial in which some participants take a treatment (say a drug) and some do not.

Control in any experiment (true or natural) derives not from the taking of the treatment but from the "assignment" of the treatment: some people are assigned to a control group

[5]We allowed Democrats and Republicans to have different incumbency advantages, but we found no statistically significant differences between them. In elections immediately following each decennial redistricting, there are a few U.S. House races and state legislative races in which both parties' candidates are incumbents. We drop the year just after each redistricting, so these never appear in our analysis.

and others to a treatment group. Only those who are eligible to participate can; however, some who are assigned to the treatment do not participate or comply (e.g., do not take the drug). In this respect, term limits are an ideal "assignment" for a natural experiment. Only those politicians eligible to run for election can. Any incumbents who faces term limits must retire, regardless of the chances that they would win for reelection. Some of those who do not have to retire run for reelection, but some retire voluntarily.

Angrist, Imbens, and Rubin (1996) show that assignment variables can be used to correct for the biases created by voluntary compliance with the treatment. Specifically, term limits can be used to construct an unbiased instrumental variables estimator of the effect of incumbency on the vote.[6] One important condition must hold. The assignment variable—in our case, term limits—must not affect the dependent variable (the vote) directly. This "excludability" assumption seems plausible, especially with state fixed-effects in the analysis. Term limits are imposed prior to a given race, sometimes decades earlier, and likely do not affect specific races directly outside of whether an incumbent can run. A central concern with excludability is whether the offices or states that term limits represent an unusual group. Below we will consider these objections.

The analysis has two stages. In the first stage we predict retirements using measures of the normal vote and national tides, as well as an indicator of whether the incumbent is term-limited or can run again. Let CR_{ijt} and CD_{ijt} indicate whether a Republican incumbent or a Democratic incumbent, respectively, can run again. Further, let $C_{ijt} = CD_{ijt} - CR_{ijt}$.[7] The first stage reduced form equations are:

$$I_{ijt} \;=\; \delta_j + \delta_t + \delta C_{ijt} + \mu_{ijt} \tag{2}$$

[6]Angrist, Imbens, and Rubin derive the linear instrumental variable estimator for discrete endogenous variables as a semi-parametric estimator. An alternative approach is to employ a selection model such as the Heckman model (1979). This model does not require the monotonicity assumption but it is heavily parametric and depends more on distributional assumptions. We estimated the models using Heckman's method and found very similar results. This is not surprising because the first stage is so strong. For simplicity, we present only the simpler instrumental variables estimates.

[7]The effects of CR and CD on I are not statistically different from one another in absolute value, so it is reasonable to combine the two variables.

In the second stage, the predicted value of I is, then, used instead of the observed incumbency to predict the vote in equation (1).

Generally, instrumental variables estimators are unintuitive. In this particular case, because those seats where an incumbent cannot run for reelection must be open, the IV estimate of β has a relatively straightforward interpretation. Elections involving seats open because of term limits represent a pure control group. As a result, the instrumental variables estimator is the ordinary least squares estimator adjusting for the discrepancy between the pure control group and all open seats (including those open due to strategic retirement).

Consider the simple bivariate case, in which only incumbency is used to predict the vote and only term limits are used to predict incumbency. Let n be the total number of cases; k is number of incumbents eligible to run for election (the number of seats not subject to term limit); and m is the number of incumbents running. Recall that V is the vote, C indicates whether the incumbent *can* run, and I indicates whether the incumbent *is* running.

$$\hat{\beta}_{IV} \;=\; [\bar{V}(I=1) - \bar{V}(I=0)] + \frac{n}{k}[\bar{V}(I=0) - \bar{V}(C=0)] \tag{3}$$

The instrumental variables estimator equals the simple differences of means estimator plus the difference between the control group used (i.e., all open seats) and the true control group (i.e., the seats made open through term limits). In the limit where k = n, that is where only those who are term-limited retire, the estimator is simply the difference between the pure control group (C=0) and the treatment group (I=0). [8]

C. Data

We study statewide elections from 1978 to 2000. The 1970s, 1980s, and 1990s are the decades when the incumbency advantage is largest, and when strategic retirement is expected to have a large effect on conventional estimates of the incumbency advantage. Also, survey

[8]The derivation follows from algebraic manipulation of the simple IV estimator: $\hat{\beta}_{IV} = \frac{\sum_i (C_i - \bar{C})(V_i - \bar{V})}{\sum_i (C_i - \bar{C})(I_i - \bar{I})} = [\bar{V}(C=1) - \bar{V}(C=0)]$. See Angrist, Imbens, and Rubin (1996) for details.

data on state-level partisanship, one measure of the normal vote, is available for this period. We also examine state legislative elections from 1994 to 2000 in those states where term limits have taken effect.

The broader project within which this paper is written encompasses all statewide and state legislative elections from 1942 to the present. One contribution of our general project is that we have assembled a comprehensive data base on all statewide elected offices, including governors, lieutenant governors, attorneys general, secretaries of state, treasurers, auditors and controllers, judges, and various commissioners (agriculture, education, insurance, public utilities, etc.), as well as U.S. Senators, U.S. House members, and state legislators. Appendix Table A.1 provides information on the offices covered and data sources. Ansolabehere and Snyder (2001) provides other information about the data.[9].

Following the main current of the incumbency advantage literature, the dependent variable in our analysis is vote-shares. Alternatively, we could study re-election rates. Study of re-election rates involves redefinition of several concepts, such as normal vote, and presents several methodological problems, such as heterogeneity in the standard deviations, which are best estimated using the votes. In this respect, studying the votes is the first step to understanding reelection probabilities.[10]

Partisan tides—national swings toward one of the parties—are readily incorporated into the regression model using indicator variables for each year.

Measuring the normal vote presents a greater challenge. Previous research has captured the normal vote with three alternative measures: fixed-effects for states and districts (Levitt

[9]In this paper we do not included judicial offices, and we also drop offices that are elected in fewer than five states (*e.g.*, Arizona is the only state with an elected commissioner of mines, and Arizona, New Mexico, and Oklahoma are the only states with elected corporation commissioners.)

[10]There is some confusion in the literature between vote margin and reelection probabilities. The belief that governors, U.S. senators, and U.S. House members have differing incumbency advantages emerges from the study of reelection rates. The observation that incumbency advantages have grown dramatically emanates from the study of vote margins. Reelection rates have not changed as much as vote margins, owing in part to the non-linear relationship between them (*e.g.*, Kendall and Stuart, 1950). Other issues to resolve in the study of re-election rates involve the difference between survival rates and reelection rates (*e.g.*, Glazer and Grofman, 1987).

and Wolfram, 1997), survey-based measures of state party identification (Erikson, Wright, and McIver, 1993; Wright, et al., n.d.), and lagged vote corrected with lagged party control (Gelman and King, 1990). For statewide races, all three measures are available, and we can compare the estimates with these alternative models directly. For statewide races, the fixed-effects and survey measures produce statistically indistinguishable estimates of the incumbency advantage. There appears to be a slight bias in the use of lagged vote, of about 1 percentage point. This likely emerges because lagged vote loses all cases that were uncontested in the previous election, and this can create selection bias. We use separate specifications for state legislatures. In one specification we use district-specific fixed-effects, as in Levitt and Wolfram (1997). The other employs lagged vote and lagged party control.

In the analysis of the incumbency effect, we distinguish the types of offices. We allow for heterogeneity in incumbency advantages by grouping the statewide elected officers into "High" (HI) and "Low" (LO) officeholders. The HI offices are governor, lieutenant governor, attorney general, secretary of state, and U.S. Senator. The LO offices are auditor, treasurer, and various commissioners. We do this mainly to gain efficiency. Analysis of each office separately shows that the clustering is appropriate.

The natural experiment uses term limits to predict which seats will be open. Data on term limits come from a review of each state's statutes and constitutions and from reports in the Book of the States. Table A2. lists all elected state offices subject to term limitations. For those offices for which the term limit was imposed after 1978, the table shows the year in which the limits went into effect and the year in which limit was first binding. The table shows the extent to which term limits vary across states, across offices, and over time.

3. Term Limits As Natural Experiments

To correct for potential biases due to strategic retirement, term limits must be a very strong predictor of retirements and term limits must satisfy the excludability assumption.

The first stage regressions show the strength of the assignment variable. Table 1 presents

the results of the regressions predicting the incidence of incumbent-contested races. For this analysis, the dependent variable is a trichotomy that equals +1 if a Democratic incumbent runs for reelection, 0 if a seat is open, and -1 if a Republican incumbent runs for reelection. The regression includes indicators of term-limited Democratic incumbents and term-limited Republican incumbents. These are the excluded exogenous variables. The regression also includes year effects (to capture national tides), and a measure of the normal vote—either state fixed-effects, the survey-based measure of partisanship, or lagged vote plus lagged party control. The table displays the results for three different models, corresponding to three different measures of the normal vote.

[Table 1]

The full sample in Table 1 consists of all states. The restricted sample consists of only those offices subject to term limits. As discussed below this is an important model check.

The specifications explain most of the variation in retirements. For statewide elections, the adjusted R^2 for all three models is quite high: term limits, party tides, and normal vote explain 67 to 72 percent of the variation in the retirement variable. For state legislative elections, the adjusted R^2 is higher still, accounting for 80 percent of the variation.

Almost all of this is accounted for by the term limit variable. For example, for Model 2 the partial R-squared of the term limit variable is .65 using the whole sample and the overall R-squared is .67; for the restricted sample the partial R-squared of the term limit variable is .69 and the overall R-squared is .72. So, party tides and the normal vote combined only account for about 3 percent of the variation in retirements.

Our findings about the normal vote and party tides are consistent with research on the US House. Kiewiet and Zeng (1993) find that age is by far the strongest predictor of voluntary retirement from the U.S. House. Scandals are a distant second in predicting House retirements. The normal vote and party tides have no significant effects.

The bottom line seems to be this. People run when they can, and retire when they must – because of age, shame, or term limits.

Term limits prove to be an extremely strong predictor of retirements, even in the face of year and state fixed-effects. The coefficient on whether an incumbent can run again in statewide elections ranges from +.64 to +.71, depending on the model. The t-statistic on this effect ranges from 35 to 60, depending on the model. In state legislative elections, the effects are greater still. These effects need not have proved so significant if it were the case that many incumbents would have retired voluntarily before the term limit went into effect. The predictive power of term limits indicates that the natural experiment has power.

In addition to their statistical strength, the term limit variables also appear to satisfy the exclusion restriction. Most of the term limits in our analysis come from laws passed long before the period of study. In our sample, seventy percent of the state executives that are term-limited occur in states that imposed the restriction on their executives before the 1970s. The states that limit legislative terms adopt those restrictions in the early 1990s; those limits become binding in the late 1990s. The state legislative analysis is consistent with the state executive analysis, so we are confident that the term limit is "causally prior."

We see two further objections to the use of term limits as an instrument. First, the very presence of term limits may affect the behavior of officeholders. We assume that statewide officeholders run equally hard for reelection whether or not they are in offices subject to term limits. We also assume that lame-duck officeholders do not act in ways that hurt their party's chances in the upcoming open-seat race to replace them. Second, the states that adopt term limits might themselves be unique, and less hospitable to incumbents.

Table 2 provides one test of the distinctiveness of states that limit terms of office. If states with and without term limits are distinctive, then we expect different retirement behavior in the two sets of states. The first stage regressions for statewide elections are very similar for the full and restricted samples (only those states with term limits).

Two further comparisons suggest that the behavior of politicians is the same and that the set of states with term limits is not unusual. First, incumbents' average vote margins are the same in states with and without term limts. If there is something unique about the states

that have term limits or if incumbents' behavior changes then the average incumbent's vote margin should differ between states with term limits and states without term limits. The top panel in Table 2 shows that the average Democratic percent of the two party vote was 59.6 in the states with term limits and 60.3 in the states without term limits, a statistically and substantively trivial difference.

[Table 2]

Second, in states that have term limits, incumbents' average vote margins are the same for offices that have term limits and for offices that have no term limit, such as the U.S. Senate. If term limits themselves change the behavior of politicians, then the officers on which there is a term limit should have different vote margins when running for reelection than the officers on which there is not a term limit. This also is false. The average Democratic vote percent of the 259 term-limited politicians was 59.6. The average Democratic vote share of the Senators in these states equalled 60.3. The difference is, again, statistically insignificant.

To push this point further, we compute the retirement slump for executives and legislators in states that have term limits and states that do not. The incumbency effects in these two types of states are very similar—identical for state legislators—and the differences are statistically not significant. Incumbents in states with term limits do just as well electorally as incumbents in states that do not have term limits. This suggests that states that have term limits do not harbor more "anti-incumbent" sentiments than states without term limits, and the incumbents in states with term limits work just has strongly for their reelection as incumbents in states without limits.

A final contrast of interest is between the vote in seats made open because of term-limits and the vote in seats made open through voluntary retirement. The difference is slight and statistically insignificant. This suggests that there is nothing unusual about the states that have term limits.

4. Estimates of Incumbency Advantages

To assess the effects of strategic retirement on incumbency advantage estimates, we compare the ordinary least squares and instrumental variables estimates.

Table 3 displays the estimated incumbency effects using conventional methods and using term limits to adjust for possible biases. Complete regression results are contained in Table A3. Results from a range of specifications are displayed. Models (1), (2), and (3) use different measures of the normal vote. For each model we present the results from different methods: ordinary least squares (OLS) and instrumental variables (IV). The different models and methods are presented in the rows. Reading down each column, the table shows the estimated incumbency effects for different sorts of political offices: all state executives (pooled), High and Low statewide offices separately, and state legislators.

[Table 3]

First consider the conventional estimators. Looking at statewide offices, the ordinary least squares regressions produce estimates of the incumbency advantage ranging from 7.7 percentage points to 8.1 percentage points, depending on the model. In state legislative elections, model 1 yields an estimated advantage of 5.2 percentage points and Model 3 produces an estimate of 5.9 percentage points.

Using term limits to correct for strategic retirements yields somewhat different estimates. Looking at all offices, the IV estimates of the incumbency advantage are 8.6 percentage points in all three models. The IV estimates, then, are one-half to one-percentage point higher than in the conventional OLS estimates.

The difference between these methods is statistically significant. Below each pair of estimates is the Hausman test for the equality of the coefficients. Assuming term limits are a valid instrument, then the IV estimates will be unbiased but less efficent than OLS, and OLS may be biased. This test assesses whether there is a statistically significant change in the coefficient from the more efficient but possibly biased OLS estimates to the less efficient

but less biased IV estimate. For Models 1 and 2, the difference between the OLS and IV estimates are larger than one would expect to observe by chance (at the .05 level).

Particularly striking, though, are the similarities among the estimates in Table 3. We have used very different normal vote measures and very different estimation methods. OLS is subject to obvious objections, but, the first stage regressions show that we have found an extremely powerful natural experiment to correct for those potential biases. All of the statistical analyses produce very similar estimates. Statewide officeholders have incumbency advantages in the range of 8.5 percentage points and state legislators have incumbency advantages slightly higher than 5 percentage points. The OLS estimates for statewide offices are off the mark by only 5 and 10 percent (not percentage points, percent). For state legislative elections the bias is less than 5 percent.

The estimates contradict the strategic retirement hypothesis in one other respect. The strategic retirement hypothesis predicts that the estimated incumbency effect should fall after the adjusting for strategic retirement. In fact, the coefficients rise, slightly.

There are many reasons why this could occur. First, strategic retirement on net appears to have little effect on the estimates. Second, the instrumental variables estimator corrects for other possible problems. Third, the strategic retirement hypothesis infact has ambiguous predictions: the direction of the bias depends on the relationship between omitted factors and incumbency and on the relationship between omitted factors and the vote.

5. What About Challenger Quality?

Who runs against incumbents also matters for election outcomes. There is considerable disagreement within the elections literature about how to handle the quality of the opposing candidates when estimating the incumbency advantage. The disagreement runs along two lines: (1) whether challenger quality has a separate effect or is part of the incumbency advantage, and (2) whether challenger quality is endogenous or exogenous.

Previous research has taken three different approaches to challenger quality in predicting

the vote. First, some analyses simply omit any measures of challenger experience or ability from the model, because challenger quality is part of the incumbency advantage.[11] Second, following Jacobson (1978), much of the literature includes indicators previous offices held as a measure of challenger quality. Third, some researchers try to subtract out challenger effects using multiple observations of the same challenger and incumbent involved in an election (Levitt and Wolfram 1997; Ansolabehere, Snyder, and Stewart 2000). The latter two approaches tacitly assume that challenger quality has a direct effect on the vote, beyond what is captured by incumbency, and that challenger quality is not correlated with the regression error.

The analysis in Table 3 follows the thinking expressed by Gelman and King and others. Rather than parse the incumbency advantage into its different sources, the analyses in Table 3 estimate the overall magnitude, correcting for the bias in the estimates created by strategic retirement.

We can also incorporate conventional measures of challenger quality in the model, as Jacobson and others do. The biographical information in the database allows us to measure whether a general election candidate has previously held state-level or federal office. We use three indicators of challenger quality: (1) candidates who held some other statewide office (including U.S. Senate), (2) candidates who held U.S. House seats in small states, and (3) candidates who had previously won U.S. House seats in large states.[12]

When we include these indicators of experience in the analyses represented in Table 3, we find that challenger quality indeed matters, but it has little effect on the estimated incumbency effect. The new estimates are shown in Table 4. Candidates who previously held a statewide office won about 2 percentage points more of the vote than other candidates. In

[11] Gelman and King (1990, page 1153) justify this specification decision as follows: "including a measure of the quality of the opposition candidate would be tempting but inappropriate because the quality of the opposition candidate is largely dependent on the incumbent's decision about whether to run for reelection."

[12] We define small states as those with four or fewer U.S. House districts. We experimented with dummy variables for state legislators, but found that they do not have higher vote margins than other candidates running for statewide office. We do not include these variables in any of the regressions reported in the paper.

small states, U.S. House members won nearly 7 percentage points more in the vote than other challengers, but in larger states, U.S. House members drew only 2 percentage points more than other challengers. The incumbency advantage estimates in the OLS and IV versions of Models 1, 2, and 3 grew by about one-half of one percentage point with the inclusion of the challenger quality indicators.

[Table 4]

These results suggest that even though challenger quality matters, it is not an important explanation of the incumbency advantage. An experienced challenger who has won office from the jurisdiction, in our case statewide, brings considerable electoral advantages, but those are unique to that candidate. The estimated incumbency advantage, but the estimated advantage is not attenuated by the inclusion of challenger quality. The best evidence of this is found in interactions between incumbency and challenger quality indicators. When we included such interactions in Models 1, 2, and 3 we found no substantively important interactions, and all but two of the interactions were statistically insignificant. The two significant interactions had opposite signs, indicating the instability of the interactions.

Behind this analysis lies the assumption that challengers affect vote shares and that entry is exogenous. Behind the analysis of Table 3 lies the assumptions either that challenger effects are independent from incumbency effects or that challenger entry is entirely endogenous. Both perspectives may be partly right: Challengers may directly affect the vote and entry may be partly endogenous.[13] If so, then quality should be included in the analysis, but some correction for the resulting simultaneity would be required.

The fact that including challenger quality in the analysis had little effect on the incumbency estimates suggests that challenger quality is orthogonal to incumbency and may even be exogenous.

To test this further we attempted to instrument for challenger quality using the number of other statewide officers who were term-limited. Specifically, when a state-wide officer, such

[13]Banks and Kiewiet (1989) present a theoretical analyses on this point, and some empirical evidence.

as an attorney general, is term-limited he or she may be more likely to run for governor, so the governor is more likely to face an experienced challenger. This instrumental variable gives us some leverage, but it is weak. The R-square for the regression explaining whether an experienced opponent runs is .06, but the t-statistic on number of other statewide officers who are term-limited is 3.5.

The incumbency advantage estimates change little, after correcting for strategic entry. The estimated incumbency effects when we instrument for challenger quality differ by about one-half of one percentage point from the IV estimates in Table 3. Moreover, the Hausman tests easily reject the hypothesis that instrumenting for challenger quality improves on the analyses in Table 4. In other words, we have no evidence to support the claim that challenger entry is endogenous. These results may be due to the relative weakness of the instruments, but term limits do offer some leverage over entry. More likely, the effects of entry on incumbency are independent from the incumbency advantage, as suggested by Table 4, and challenger entry is by-and-large exogenous.

We view the analysis of the endogeneity of challenger quality as preliminary, but promising. Stronger instruments are desired, and will require careful and tedious research on the term lengths, term structure and district geographies of state elected officials. Also, as more state officeholders are term-limited in the coming election cycles we will gain more leverage. These preliminary results suggest that eventually scholars will be able to use term limits to untangle the endogeneity of challenger entry. Our preliminary estimates, though, suggest that the challenger entry may in fact be exogenous.

6. Discussion

Strategic retirement has long been considered a leading factor contributing to the magnitude and growth of the incumbency advantage in American elections. Tests of this conjecture have been difficult because of the simultaneity between retirements and the expected vote. Previous studies offer indirect evidence in support for the conjecture (*e.g.*, Cox and Katz,

2002).

The states are an excellent laboratory for a direct test. Using the natural experiment created by term limits, we find direct and conclusive evidence against the strategic retirement hypothesis. The estimated incumbency effects in the IV and OLS models are very similar, differing by one-half to one-percentage point. Moreover, using term limits as an instrument, the estimated incumbency effects are slightly higher than in the OLS models. This runs contrary to expectations of those who would argue that more conventional estimates of the incumbency advantage are inflated by strategic retirement.

We also find direct evidence against the claim that strategic entry of challengers explains the incumbency advantage. Including challenger experience in the analysis does not change substantially the estimated effect of incumbency on vote margins in state executive and legislative elections. We offer the first attempt to correct for the endogeneity of challenger entry. The results of the instrumental variables estimates for challenger quality are admittedly preliminary provide, but they no support for the conjecture that challenger quality is highly endogenous.

Do our conclusions apply to federal elections as well as the states? We suspect that they do. First, the incumbency advantages in federal and state elections are of similar magnitudes and grew at the same rate and at the same times (Ansolabehere and Snyder, 2001). It seems unlikely that such similar phenomena would have substantially different causes. Second, we compare behavior in U.S. Senate and other statewide elections directly. Electoral results in U.S. Senate elections closely resemble the results of elections for governors and other higher statewide officers. Third, our sample of state legislative elections includes several highly professional state assemblies, such as California, Michigan, and Ohio, the members of which had very long tenures prior to term limits. In their tenures and professionalism, these assemblies resemble the U.S. House. When we isolate just these state legislatures, we find no evidence that strategic retirement biased the estimated incumbency advantage estimates.

The method and results presented here can be used to test the validity of potential

instruments for strategic retirement at the federal level. We have found a very powerful instrument–term limts. Researchers may now use this instrument to test the validity of other potential instruments for retirement, such as incumbent's age.[14] If other factors prove valid and powerful instruments in state elections, it is reasonable to suppose that they could be used to correct for strategic retirement in U.S. House elections.

We do not deny that strategic retirement and entry occur. Politicians are career oriented, and party tides do affect the differential retirement rates of Democrats and Republicans and the quality of Democratic and Republican challengers.[15] The extent of strategic behavior may be overstated. The normal vote has no discernable effect on the retirement rate. Those in very safe seats are just as likely to retire voluntarily as those in unsafe seats. The professional orientation of politicians may, in fact, work against the strategic retirement hypothesis: politicians abandon office when they must, because of law or personal circumstances.

However much it occurs, strategic retirement cannot explain either the magnitude of the incumbency advantage in state elections today or the growth of the incumbency advantage over the last 50 years.

[14]Standard tests for instrument validity based on overidentification, such as Hausman's test, require at least on valid instrument.

[15]Further evidence of strategic behavior is the jump in U.S. House and state legislative retirements during redistricting years.

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Table 1: First Stage Estimates

Dep. Var. = Incumbency Status

	Full Sample			Restricted	
Statewide Executive Elections, 1978-2000					
	Model 1	Model 2	Model 3	Model 1	Model 3
Non-Term-Limited Incumbent	0.68** (0.01)	0.69** (0.01)	0.64** (0.03)	0.71** (0.02)	0.70** (0.04)
Democratic Party Strength	−−	−0.001 (0.001)	−−	0.001 (0.002	−−
Lagged Democratic Vote Share	−−	−−	−0.002 (0.001)	−−	−0.007* (0.003)
Lagged Party Control	−−	−−	0.07 (0.04)	−−	0.07 (0.04)
Adjusted R^2	.67	.67	.68	.70	.72
# Obs.	1759	1759	1589	435	399
State Legislative Elections, 1994-2000					
	Model 1		Model 3		
Non-Term-Limited Incumbent	0.82** (0.02)		0.85** (0.02)		
Lagged Democratic Vote Share	−−		−0.001 (0.001)		
Lagged Party Control	−−		−0.03 (0.02)		
Adjusted R^2	.77		.81		
# Obs.	2250		1900		

Standard errors in parentheses.

Model 1 includes state fixed-effects and year fixed-effects (for statewide races), or district fixed-effects and year fixed-effects (for state legislative races). Models 2 and 3 include year fixed-effects.

Restricted sample = set of states/offices with term limits in effect.

* statistically significant at the .05 level

** statistically significant at the .01 level

Table 2: Model Checks

Statewide Offices

All States and Offices:

	Offices Subject to Term Limits	Offices not Subject to Term Limits	Difference
Incumbent's Vote Share	58.2 (196)	59.8 (900)	-1.4

States with Term-Limited Offices:

	Open Seat Race Due to Term Limited Incumbent	Open Seat Race Due to Voluntary Exit	Difference
Incumbent Party's Vote Share	53.8 (159)	53.0 (266)	0.8
Slump	9.8 (78)	8.9 (129)	0.9

State Legislatures

All States:

	States Subject to Term Limits	States not Subject to Term Limits	Difference
Incumbent's Vote Share			
1986-1992	65.1 (1222)	64.0 (2764)	1.1
1994-2000	64.0 (1527)	63.9 (4178)	0.1

States with Term-Limits in Effect:

	Open Seat Race Due to Term Limited Incumbent	Open Seat Race Due to Voluntary Exit	Difference
Incumbent Party's Vote Share (1994-2000)	61.2 (275)	58.4 (462)	2.8
Slump	5.1 (210)	5.1 (272)	0.0

Cell entries are averages.

Number of observations in parentheses.

Table 3: OLS and IV Estimates of Incumbency Effects in Statewide Executive Elections, 1978-2000 and State Legislative Elections, 1994-2000

Dep. Var. = Democratic Share of Two-Party Vote

	Statewide Offices			**Legislators**
Model & Method	All	HI[(a)]	LO[(b)]	Lower House
Model 1				
(Normal Vote = Fixed Effects)				
OLS	**7.97** (.25)	**8.78** (.30)	**6.49** (.40)	**5.22** (.21)
IV (Using Term Limits)	**8.53** (.31)	**9.19** (.40)	**7.09** (.52)	**5.40** (.27)
Hausman Test (p-value)	2.99 (.01)	1.92 (.07)	2.58 (.025)	1.06 (.23)
Model 2				
(Normal Vote = Survey)				
OLS	**8.14** (.24)	**8.96** (.29)	**6.49** (.40)	–
IV (Using Term Limits)	**8.68** (.30)	**9.37** (.36)	**7.26** (.49)	–
Hausman Test (p-value)	3.06 (.01)	2.00 (.05)	2.75 (.01)	–
Model 3				
(Normal Vote = Lagged Vote)				
OLS	**7.70** (.41)	–	–	**5.90** (.33)
IV (Using Term Limits)	**8.61** (1.01)	–	–	**5.96** (.48)
Hausman Test (p-value)	1.01 (.24)	–	–	0.17 (.39)

Estimated incumbency effects in bold font. Standard errors in parentheses.

See Appendix Tables A.2-A.5 for complete results.

[(a)] HI offices are Governor, Lieutenant Governor, Attorney General, Secretary of State, and U.S. Senator.

[(b)] LO offices are Treasurer, Auditor, and the commissioners of Agriculture, Education, Insurance, Public Lands, and Public Utilities.

Table 4: Estimates of the Incumbency Advantage Statewide Races, 1978-2000

Dep. Var. = Democratic Share of Two-Party Vote

OLS Estimates

	Model 1	Model 2	Model 3
Incumbent	8.24**	8.42**	8.13**
	(0.26)	(0.24)	(0.41)
Democratic Party Strength	−−	0.41**	−−
		(0.02)	
Lagged Democratic Vote Share	−−	−−	0.27**
			(0.03)
Lagged Party Control	−−	−−	−0.91*
			(0.40)
Candidate is Other Statewide Officer	1.43*	1.78**	1.92**
	(0.58)	(0.57)	(0.61)
Candidate is U.S. House Member in Small State	6.89**	7.00**	8.90**
	(1.35)	(1.33)	(1.44)
Candidate is U.S. House Member in Large State	1.81*	1.92**	2.66**
	(0.92)	(0.91)	(0.95)
Adjusted R^2	.58	.57	.53
# Obs.	1759	1759	1589

Standard errors in parentheses.

Model 1 includes state fixed-effects and year fixed-effects. Models 2 and 3 include year fixed-effects.

* statistically significant at the .05 level

** statistically significant at the .01 level

Table A.1: Elected Offices and Data Sources for Each State		
	Elected Offices	Sources
All	Gov., Sen., House Rep.	Dubin (1998), ICPSR #7757,
All	1999, 2000 data, all offices	various state web pages
AL	LG, SS, AG, Tr, Au, E, Ag, PU, J	*Official and Statistical Register*
AZ	SS, AG, Tr, Au, E, Co, M, Tx	*Year Book*; *Official Canvass*
AR	LG, SS, AG, Au, Ld	*Official Register*
CA	LG, SS, AG, Tr, Au, I	*Statement of Vote*
CO	LG, SS, AG, Tr, E, Rg, J	*Abstract of Votes Cast*
CT	LG, SS, AG, Tr, Au	*Statement of Vote*
DE	LG, AG, Tr, Au, I	*State Manual*
FL	SS, AG, Tr, Au, E, Ag, RR	*Report of Secretary of State*
GA	LG, SS, AG, Tr, Au, E, Ag, PU, Lb, I	*Official and Statistical Register*
ID	LG, SS, AG, Tr, Au, E, M	*Abstract of Votes*
IL	LG, SS, AG, Tr, Au, E, Ck	*Official Vote of the State of Illinois*
IN	LG, SS, AG, Tr, Au, E, Ck, J	*Report of Secretary of State*
IA	LG, SS, AG, Tr, Au, E, Ag, Cm, J	*Official Register*; *Canvass of the Vote*
KS	LG, SS, AG, Tr, Au, E, I, Pr	*Official Statement of Vote Cast*
KY	LG, SS, AG, Tr, Au, E, Ag	*Statement of Official Vote*
LA	LG, SS, AG, Tr, Au, xx	*Biennial Report of Secretary of State*
MD	AG, Au	*Complilation of Election Returns*
MA	LG, SS, AG, Tr, Au	*Election Statistics*
MI	LG, SS, AG, Tr, Au	*State Manual*
MN	LG, SS, AG, Tr, Au, RR	*Legislative Manual*
MS	LG, SS, AG, Tr, Au, E, Ag, Ld, I, Ck, Tx	*Official and Statistical Register*
MO	LG, SS, AG, Tr, Au	*Official Vote of the State of Missouri*
MT	LG, SS, AG, Tr, Au, E, RR, Ck	*Official General Election Returns*
NE	LG, SS, AG, Tr, Au, RR	*Official Report of the State Canvassing Board*
NV	LG, SS, AG, Tr, Au, M, Ld, Ck, Pr	*Political History of Nevada*
NM	LG, SS, AG, Tr, Au, E, Ld, Co, J	*Blue Book*
NY	LG, AG, Au	*Legislative Manual*
NC	LG, SS, AG, Tr, Au, E, Ag, Lb, I, J	*State Manual*
ND	LG, SS, AG, Tr, Au, PU, Lb, I, Tx	*Official Abstract of Vote Cast*; *Compliation of Election Returns, 1976-1987*
OH	LG, SS, AG, Tr, Au, J	*Election Statistics*
OK	LG, SS, AG, Tr, Au, E, Co, Lb, I, M, CC, J	*Directory of the State of Oklahoma*
OR	SS, AG, Tr, Lb	*Blue Book*; *Official Abstract of Votes*
PA	LG, SS, AG, Tr, Au, J	*State Manual; Official Results*

Table A.1, continued
Elected Offices and Data Sources for Each State

	Elected Offices	Sources
RI	LG, SS, AG, Tr	*Official Count of the Ballots Cast*
SC	LG, SS, AG, Tr, Au, E, Ag, Ad	*Supplemental Report of Sec. of State*
SD	LG, SS, AG, Tr, Au, PU, Ld	*Offical Election Returns*
TN	RR	*Directory and Official Vote*
TX	LG, AG, Tr, Au, Ag, RR, Ld, J	*Texas Almanac*
UT	LG, SS, AG, Tr, Au, J	*Abstract of Vote*
VT	LG, SS, AG, Tr, Au	*Legislative Directory and State Manual*
VA	LG, AG	*Report of Secretary of State*
WA	LG, SS, AG, Tr, Au, Ld, I	*Abstract of Votes*
WV	SS, AG, Tr, Au, E, Ag, J	*Official Returns*
WI	LG, SS, AG, Tr	*Blue Book*
WY	SS, Tr, Au, E	*Official Directory*

In AK, HI, ME, NH, and NJ there are no statewide races other than Senate and Governor.

LG = Lieutenant Governor
SS = Secretary of State
AG = Attorney General
Tr = Treasurer
Au = Auditor, Controller, Comptroller, Examiner
Ag = Commissioner of Agriculture, Agriculture and Industry, etc.
E = Commissioner of Education, Superintendent of Schools, etc.
Rg = Regent
PU = Public Utility Commissioner, Public Service Commissioner, etc.
RR = Railroad Commissioner, Railroad & Public Utility Commissioner, etc.
Co = Corporation Commissioner
Cm = Commerce Commissioner
I = Insurance Commissioner
Lb = Commissioner of Labor
Ld = Land Commissioner, Surveyor
M = Commissioner of Mines, Mine Inspector
Tx = Tax Commissioner, Tax Collector
CC = Charities and Corrections Commissioner
Pr = Printer
Ck = Court Clerk, Court Reporter
Ad = Adjutant General
J = Supreme Court Justice, Appeals Court Judge

Table A.2: Term Limited Elective Offices for Each State	
Governor	AK, AL, AR (92, 02), AZ (92, 02), CA (90, 98), CO (90, 98), DE, FL, GA, HI (78, 86), ID (94, 02), IN, KS, KY (92, 03)[1], LA, MD, ME, MI (92, 02), MO, MS (86, 91)[2], MT (92, 00), NC, NE, NJ, NM (86, 94)[3], NV, OH, OK, OR, PA, RI (92, 02), SC (81, 92)[4], SD, TN, UT (94, 06), WV, WY (92, 02)
Lieutenant Governor	AK, AL, AR (92, 98), CA (90, 98), CO (90, 98), DE, FL (92, 02), HI (78, 86), ID (94, 02), KS, KY (92, 03), MI (92, 02), MS (92, 99) MT (92, 00), NC, NE (92, 02), NM (86, 94), NV (96, 02), OH, PA, RI (92, 02), SD
Attorney General	AL, AR (92, 02), AZ (92, 02), CA (90, 98), CO (90, 98), FL (92, 02), ID (94, 02), IN (96, xx), KY (92, 03), MI (92, 02), MT (92, 00), NE (92, 02), NM (86, 94), NV (96,02), OH (92, 02), OR (92, 00), PA, RI (92, 02), SD (92, 02)
Secretary of State	AL, AR (92, 02), AZ (92, 02), CA (90, 98), CO (90, 98), FL (92, 02), ID (94, 02), IN, KY (92, 03), MI (92, 02), MT (92, 00), NE (92, 02), NM (86, 94), NV (96, 02), OH (92,02), OR, RI (92, 02), SD (92, 02), WY (92, 02)
Treasurer	AL, AR (92, 02), AZ, CA (90, 98), CO (90, 98), FL (92, 02), ID (94,02), IN, KY (92, 03), MO, ND, NE, NM (86, 94), NV (96, 02), OH (92, 02), OR, PA, RI (92, 02), SD (92,02), WY (92, 02)
Auditor, Comptroller	AL, AR (92, 02), CA (90, 98), FL (92, 02), ID (94, 02), IN, KY (92, 03), MT (92, 00), NE (92, 02), NM (86, 94), NV (96, 02), OH (92, 02), PA, SD (92, 02), WY (92, 02)
State Commissioner	AR (92, 02), AZ (92, 02), CA (90, 98), CO (95, xx), FL(92, 02), ID(94, 02), IN(00, xx), KY (92, 03), MT(92, 00), SD(92, 02), NM(86, 94), WY(92, 02)
State Legislator	AR (92, 98), AZ (92, 00)[5], CA (90, 96), CO (90, 98), FL (92, 00), ID (94, 02), LA (95, 07), MA (94,-)[6], ME (93, 96), MI (92, 98), MO (92, 00), MT (92, 00), NV (94, 06), OH (92, 00), OK (90, 02), OR (92, 98), SD (92, 00)[5], UT (94, 06), WA (92,-)[6], WY(92, 06)

Year enacted and first year of impact in parentheses, if after 1978.

[1] Prior to 1992, all KY elected statewide officers were limited to 1 term.

[2] Prior to 1986, the MS Governor was limited to 1 term.

[3] Prior to 1986, all NM elected statewide officers were limited to 1 term.

[4] Prior to 1981, the SC Governor was limited to 1 term.

The Virginia the Governor is limited to 1 term.

After 1996, all UT elected statewide officers other than governor are limited to 3 terms.

[5] AZ and SD use multi-member districts.

[6] In 1997 the state supreme courts in MA and WA declared the term-limit laws unconstitutional.

Table A.3: Estimates of the Incumbency Advantage Statewide Races, 1978-2000

Dep. Var. = Democratic Share of Two-Party Vote

Model 1

	OLS	IV	OLS	IV
Incumbent	7.97**	8.53**	−−	−−
	(0.25)	(0.31)		
HI Office Incumbent	−−	−−	8.78**	9.19**
			(0.30)	(0.37)
LO Office Incumbent	−−	−−	6.49**	7.09**
			(0.40)	(0.52)
Adjusted R^2	.57		.58	
# Obs.	1759	1759	1759	1759

Standard errors in parentheses.

Model 1 includes state fixed-effects and year fixed-effects.

* statistically significant at the .05 level

** statistically significant at the .01 level

Table A.4: Estimates of the Incumbency Advantage Statewide Races, 1978-2000

Dep. Var. = Democratic Share of Two-Party Vote

Model 2

	Full Sample				Restricted	
	OLS	IV	OLS	IV	OLS	IV
Incumbent	8.14** (0.24)	8.68** (0.30)	−−	−−	7.46** (0.54)	7.60** (0.66)
HI Office Incumbent	−−	−−	8.96** (0.29)	9.37** (0.36)	−−	−−
LO Office Incumbent	−−	−−	6.49** (0.40)	7.26** (0.49)	−−	−−
Democratic Party Strength	.42** (0.02)	.41** (0.02)	.42** (0.02)	.41** (0.02)	.47** (0.05)	.47** (0.05)
Adjusted R^2	.55		.56		.55	
# Obs.	1759	1759	1759	1759	435	435

Standard errors in parentheses.

Model 2 includes year fixed-effects.

Restricted sample = set of states/offices with term limits in effect.

* statistically significant at the .05 level

** statistically significant at the .01 level

Table A.5: Estimates of the Incumbency Advantage Statewide Races, 1978-2000

Dep. Var. = Democratic Share of Two-Party Vote

Model 3

	Full Sample		Restricted	
	OLS	IV	OLS	IV
Incumbent	7.70**	8.61**	8.42**	8.31**
	(0.41)	(1.01)	(0.82)	(1.27)
Lagged Democratic	.28**	.28**	.31**	.31**
Vote Share	(0.03)	(0.03)	(0.06)	(0.06)
Lagged Party Control	−.83*	−1.42*	−1.84*	−1.78
	(0.41)	(0.72)	(0.79)	(0.98)
Adjusted R^2	.51		.50	
# Obs.	1589	1589	399	399

Standard errors in parentheses.

Model 3 includes year fixed-effects.

Restricted sample = set of states/offices with term limits in effect.

* statistically significant at the .05 level

** statistically significant at the .01 level

Table A.6: Estimates of the Incumbency Advantage State Legislative Races, 1994-2000

Dep. Var. = Democratic Share of Two-Party Vote

	Model 1		**Model 3**	
	OLS	IV	OLS	IV
Incumbent	5.22**	5.40**	5.90**	5.96**
	(0.21)	(0.27)	(0.33)	(0.48)
Lagged Democratic Vote Share	−−	−−	.74**	.74**
			(0.02)	(0.02)
Lagged Party Control	−−	−−	−.71	−.75
			(0.37)	(0.44)
Adjusted R^2	.90		.81	
# Obs.	2250	2250	1900	1900

Standard errors in parentheses.

Model 1 includes district fixed-effects and year fixed-effects.

* statistically significant at the .05 level

** statistically significant at the .01 level