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Do Congressional Candidates Have Reverse Coattails? Evidence from a Regression Discontinuity Design

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Although the presidential coattail effect has been an object of frequent study, the question of whether popular congressional candidates boost vote shares in return for their parties' presidential candidates remains unexplored. This article investigates whether so-called ''reverse coattails'' exist using a regression discontinuity design with congressional district-level data from presidential elections between 1952 and 2004. Taking incumbency to be near-randomly distributed in cases where congressional candidates have just won or lost their previous elections, I find that the numerous substantial advantages of congressional incumbency have no effect on presidential returns for these incumbents' parties. This null finding underscores my claim that the existing coattail literature deserves greater scrutiny. My results also prompt a rethinking of the nature of the advantages that incumbents bring to their campaigns and may help deepen our understanding of partisanship in the United States.

1 Introduction

During his preparations for the presidential election of 1956, Adlai Stevenson's campaign manager James Finnegan noticed that during Stevenson's previous presidential bid in 1952, his performance had consistently fallen short of that of incumbent Democratic House candidates throughout the country. As a *Time* magazine article recorded two months before the 1956 election, to Finnegan the ''answer was obvious: Stevenson must associate his campaign more closely with those of the state candidates and attract voters to himself through their local popularity.'' Thus, a strategy the Stevenson campaign dubbed ''Operation Reverse Coattails'' was born, designed to increase Stevenson's presidential vote share through direct investment in the persuasion and mobilization efforts of Democratic congressional candidates across the country (Operation Reverse Coattails 1956).

Nearly 50 years after Stevenson's loss, the argument that local party candidates can serve as a springboard for national party candidates, what Stevenson's managers once called ''reverse coattails,'' is returning to American politics. After his election as Democratic Party Chairman, Howard Dean spent the first months of his chairmanship selling party donors and leaders on his similar ''50-state strategy,'' a controversial plan to funnel national party resources to local candidates and parties in historically Republican states and locales with the hope of establishing footholds there (Barabak 2008). Although the

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strategy met with harsh criticism early on, most notably from then Chair of the Democratic Congressional Campaign Committee Rahm Emanuel, once the 2006 midterms brought Democrats back into control of the U.S. House for the first time since 1994, the Association of State Democratic Party Chairs credited the strategy for Democrats' victory. In a joint public statement, the group of Democratic leaders wrote that the strategy had ''la[id] the groundwork'' for both the historic midterm victory and the much-anticipated 2008 presidential campaign (ASDC 2006). Having witnessed Democratic victories nationwide in subsequent elections, some Republican leaders have now called for a similar strategy, hoping to generate their own reverse coattails for gubernatorial, senatorial, and presidential candidates by establishing footholds in lower offices (see Nehring 2009).

Is the key to politics really local? Despite the current bipartisan investment in local elections, traditional presidential coattails dominate the coattail literature whereas reverse coattails have all but eluded empirical investigation—indeed, to my knowledge, scholars of American politics have never before directed their attention to the phenomenon of congressional or reverse coattails. The traditional coattail effect has, of course, been a perennial topic of interest in the study of American presidential and congressional elections (e.g., Miller 1955; Press 1958; Kaplowitz 1971; Calvert and Ferejohn 1983; Campbell 1986; Campbell and Sumners 1990; Flemming 1995; Cohen et al. 2000; Mattei and Glasgow 2005; Koch 2008, etc.). As originally defined by Miller (1955), the traditional coattail effect occurs when ''the congressional vote decision is motivated by the appeal of the presidential candidate,'' whereas reverse coattails occur when the Presidential vote decision is motivated by the appeal of the congressional candidate.

Just as with the study of traditional coattails, approximating the counterfactual in the study of reverse coattails is difficult because numerous unobservable factors influence both presidential and congressional performance. This article describes how a regression discontinuity design (RDD) can estimate the reverse coattail counterfactual in congressional elections near experimentally and with less potential for bias than would be present by applying the existing, strictly observational methodologies that have been used to study coattails.

As Lee (2008) describes, in the context of congressional elections, RDD allows the congressional districts where parties have just barely won previous elections to serve as observable near-experimental counterfactuals for observations in which the same party has just lost the previous election. In brief, these two sets of districts—those in which a party has just won and lost—can be considered as an estimate of the counterfactual for each other because due to slight random variation in congressional vote shares from weather and other such factors, winning or losing the previous election can be considered exogenous very near the 50% threshold in two-party vote (i.e., at the limit). As the only variable that should systematically differ right on either side of the 50% threshold in two-party vote is the incumbency status of one of the candidates, the resulting natural experiment, thus, allows the causal effect of congressional incumbency to be identified.

Lee (2008)'s application of this technique to the problem of measuring the direct effects of congressional incumbency has shown that the causal effect of winning an election—in other words, the incumbency effect—is an approximately 10 percentage point increase in two-party vote share in the next. However, since Lee's method allows incumbency to be considered as near-randomly introduced in these close districts, we can also consider the associated benefits of incumbency as having been near-randomly introduced as well. In this article, I test whether the assemblage of benefits that accompany congressional incumbency, which include both the approximate 10 percentage point increase in the two-party vote and a large spending advantage, cause an increase in vote shares for the presidential candidates of the same party through a reverse coattail effect.

Even when employing a variety of different specifications and controls, RDD provides no evidence that presidential candidates perform better when they appear on the ballot with more popular congressional candidates. This null result is substantively important for several reasons. First, in and of itself, the lack of a reverse coattail effect is interesting given that American political elites have claimed that it exists for more than half a century. More significantly, I also argue that the current theoretical logic of the traditional presidential coattail literature, linked to the widely accepted conclusions of the incumbency literature about the personal vote bonus, does predict the existence of reverse coattails. Thus, I argue that my findings cast doubt on the presidential coattail literature's existing theoretical model as they demonstrate that one of the theoretical predictions of the existing literature does not hold. I also show how bias introduced by this literature's existing methodology led to the current theoretical explanations for the presidential coattail effect. Finally, I review some ramifications my results may have for other literatures in comparative and American politics. In the next section, I review and critique the existing presidential coattail literature's empirical methods and develop my argument that an exploration of reverse coattails is important to deepening our understanding of presidential coattails.

2 Existing Coattail Literature

2.1 *Existing Empirical Strategies*

Early in the coattail literature, Miller (1955) and Press (1958) point out a problem that continues to plague studies of presidential coattails:

> If a presidential candidate leads his party by a wide margin, he is an effective candidate for the party ticket. The greater the margin, presumably, the stronger are the presidential coattails. But large margins, on the contrary, must indicate that the presidential candidate has not been able to transfer all of his votes to the congressional ticket; and so it would seem that the attraction of his coattails is minimal if this margin is great.

This "paradox of margins" Miller that originally identified highlights the inability of researchers to rigorously approximate the counterfactual in traditional observational studies of coattails, which at bottom require an attempt to estimate what congressional vote shares would have been in the world in which there were no presidential elections in the years under study, holding all other factors constant. Researchers over the past several decades attempting to recover this counterfactual have primarily employed two strategies: (1) controlling for factors that influence both congressional and presidential vote shares to isolate the amount of congressional vote share attributable to the presidential candidate's appeal and (2) either narrowing the focus of analysis to open-seat races or comparing across open-seat and non–open-seat races to minimize bias.

In a recent example of the first strategy, Hogan (2005) searches for gubernatorial coattails by controlling for a large number of variables including demographics, past candidate performance, turnout, campaign spending, district characteristics, and other candidate characteristics. Noting a statistically significant relationship between gubernatorial performance and state legislative candidate performance despite these controls, Hogan concludes, "for every one percentage point increase in a Democratic gubernatorial candidate's vote percentage, the Democratic legislative candidate's vote in the district increases by 0.43 percent[age points]." Yet, Hogan's assertion of a causal link is premature; an interpretation at least as plausible of Hogan's regression might read 'the unobservable factors that influence gubernatorial vote share for a party also influence that party's state legislative vote share.' For example, macro events such as broad shifts in partisanship or even economic conditions (e.g., Fair 1978; Bartels 2008, chap. 4; etc.) would be mistaken

for spillover effects by Hogan's method. Likewise, so as to allow researchers to establish a direct causal link between candidates' vote shares, much of the existing presidential literature on coattails rests on the strong assumption that researchers can employ models capable of capturing all the factors that may contribute to both congressional and Presidential candidates' success other than presidential candidate effects.

Faced with a similar pattern of strictly observational research in the incumbency literature, Gelman and King (1990) opened their influential article on the congressional incumbency advantage with a broad critique of such methods, arguing instead for a focus on the difference between the vote shares of candidates running for open seats and incumbents running for reelection, a technique that has been employed by most contributions to the incumbency literature in the past two decades. Inspired by the contribution of Gelman and King, recent contributions to the coattail literature have also partitioned data by open-seat status, arguing either that the coattail effect should reveal itself with less bias in districts without an incumbent or that the coattail effect actually exists most strongly in such districts (e.g., Flemming 1995, Mattei and Glasgow 2005, Koch 2008).

Yet, although the literature's new seat-partitioning approach has features that resemble a natural experiment, as Cox and Katz (2002) and Engstrom and Monroe (2006) argue, open seats do not occur randomly but in large part due to incumbents' strategic decisions. As a result, seats vacated voluntarily by incumbents systematically differ from seats where incumbents decide to run again, perhaps because incumbents that expect to lose are more likely to retire. Both of the coattail literature's most widely-used empirical strategies, thus, remain critically vulnerable to bias.

2.2 *Existing Theoretical Model*

Not only does this bias add a large amount of uncertainty to all of our current estimates of coattail effects, but since incumbents may be more likely to retire when they expect to lose, methods that control for open-seat status and its interaction with presidential vote share are likely to credit the result of any systematic differences in congressional performance in such districts to the presidential candidate's vote share within them. By applying the method of Gelman and King, recent literature on presidential coattails has consequently reached the conclusion that presidential coattail effects are much stronger in open-seat contests than in districts where congressional incumbents are running for reelection. Models of the psychology behind coattail voting, following from Mondak and McCurley (1994) together with other older contributions to the literature (Flemming 1995) and recent empirical analyses (Mattei and Glasgow 2005; Koch 2008), have interpreted this potentially spurious finding as evidence that the presidential coattail effect is a result of the availability heuristic, which leads voters to incorporate their attitudes toward candidates for whom they hold well defined and more cognitively accessible attitudes into their attitudes about political figures about whom they are more uncertain or unaware. That is, the literature holds that the amount of cognitively available information available about congressional incumbents leaves voters with no need to rely upon their evaluations of presidential candidates when forming attitudes toward congressional candidates, significantly attenuating the coattail effect (Koch 2008) or masking it entirely (Mattei and Glasgow 2005); however, the literature still maintains that a strong coattail effect exists in open-seat elections where voters engage the availability heuristic as they make their congressional vote decisions.

Since, as I have argued, it has proven difficult to submit this theoretical claim to a rigorous empirical test that would be capable of falsifying it directly, another way of evaluating the coattail literature's theoretical model is to investigate another one of its other theoretical implications, namely what I argue to be the literature's logical conclusion that if

American voters employ the availability heuristic when forming their attitudes they should do so when they form their attitudes about presidential candidates as well. In other words, the literature's theoretical model necessarily predicts that reverse coattails should exist.

Why does the literature's model predict reverse coattails? Note that there is nothing about the availability heuristic conceptually that indicates only presidential evaluations should tend to inform Congressional evaluations rather than the reverse, too; the only catalyst identified in the literature for the heuristic's application is the amount of cognitively available information about each candidate and voters' confidence in this information. Yet even given the high salience of presidential campaigns, it may not be safe to assume that voters (especially those who would be marginally turned out or persuaded) always have a stronger prior or possess more readily accessible cognitive information concerning presidential candidates than their congressional incumbent. In fact, as noted, the coattail literature claims that attitudes toward congressional incumbents are strong enough to overwhelm the presidential coattail heuristic entirely.

It is also important to note, moreover, that the existing literature does not draw any distinction between the *type* of personal appeal that the congressional incumbency advantage grants (which this analysis uses as an instrument) and the appeal possessed by the sorts of presidential candidates that purportedly engages the availability heuristic and, thus, generates coattails. Following from Miller's (1955) original formulation, contemporary coattail literature has borrowed the vocabulary of the well-established 'personal vote' explanation in the congressional incumbency literature and discusses presidential coattails as flowing from the ''personal appeal'' (Kaplowitz 1971) of the presidential candidate; most recently, for example, Mattei and Glasgow (2005) credit coattails to the ''*attraction* emanating'' from presidential candidates (emphasis added). Likewise, the majority of incumbency literature (e.g., Erikson 1971; Fenno 1978; Erikson and Palfrey 1998; Ansolabehere, Snyder, and Stewart 2000; Prior 2006) similarly associates the incumbency advantage with an incumbent's unique ability to increase their personal appeal to voters—positive attitudes that voters form toward incumbents are not associated with a reorientation of voters' general political attitudes but, instead, are associated with incumbents' ability to forge personal connections with and increase their personal appeal among their constituencies through the use of casework, office hours, franking privileges, pork, and increased television coverage.[1]

[1]The traditional incumbency literature leaves little doubt that the mechanisms of the personal vote mentioned previously such as the franking privilege and television exposure generate strong, positive attitudes for incumbents and, thus, are responsible for the lion's share of the congressional incumbency effect. However, since my critique of the theoretical claims of the presidential coattail literature is predicated on this assumption that congressional incumbency grants the same kind of personal appeal to candidates that the coattail literature claims coattail-generating Presidential possess, one recent challenge to this conventional understanding of the incumbency effect merits mention. Incumbents also achieve high rates of reelection in part through their ability to deter high-quality challengers, who are likely to seek more favorable electoral circumstances or delay their ambitions given common knowledge about the power of the incumbency effect (Cox and Katz 1996; Carson, Engstrom, and Roberts 2007; Butler 2009). This is potentially troubling for my argument since my analysis that congressional candidate popularity has no effect on presidential vote shares relies upon the assumption that incumbency grants popularity to the incumbents themselves, not just the privilege of running against less qualified opponents. Reassuringly, empirical analyses have found that although assuredly accounting for some of the incumbency effect, this challenger deterrent effect does not account for nearly all the incumbency effect. Using an RDD, Butler (2009) estimates that this effect is responsible for about 2.3 percentage points or only about a quarter of the incumbency effect in close districts. Likewise, using the pseudonatural experiment of congressional redistricting, Ansolabehere, Snyder, and Stewart (2000) also find that the personal vote rather than challenger quality is responsible for much of the incumbency effect, especially in areas where incumbents are ''most electorally vulnerable,'' that is, the very districts that came under the closest study in this article. These results, thus, confirm the premise of my argument that the lion's share of the incumbency effect near the treatment threshold is, indeed, due to the personal advantages incumbents cultivate with their constituents and use to their advantage on Election Day against equally qualified competitors.

If the personal vote can spillover in the American context, the incumbency and coattail literatures, thus, leave us with strong reasons to think that it should work both ways—boosting vote shares for congressional candidates when they appear on the ballot with popular presidential candidates and helping presidential candidates when they share the ballot with congressional incumbents with strong personal appeal. If the reverse coattail effect does not exist in the American context, we have cause to place our theoretical understanding of presidential coattails under even closer scrutiny.

3 Method, Data, and Results

As discussed, the coattail literature's traditional methods rest on the implicit assumption that researchers are capable of capturing all the factors that might cause variation in both congressional and presidential vote shares. To escape omitted variable bias and rigorously test the proposition that all else being equal, more popular congresspeople help their party's presidential candidates, researchers would most ideally be able to dole out popularity to members of Congress in a randomized experiment. Although this is of course impossible, the natural world achieves something similar to random assignment of congressional candidate popularity through the near-random assignment of incumbency near the 50% threshold in two-party vote.

As reviewed in the Introduction, due to exogenous factors like weather and other random events that induce small variation in vote shares, districts very near the 50% threshold in two-party vote in an election at *Time 1* are essentially randomly sorted into the 'Democratic incumbent' and 'Republican incumbent' conditions at *Time 2*. Lee (2008) uses RDD to exploit this natural experiment near the 50% level of two-party vote share to determine, ceteris paribus, the effects of just winning an election at Time 1 on a party's prospects for reelection at Time 2—in other words, the incumbency effect—by calculating the magnitude of the discontinuity at the theoretical limit.

Since Lee's RDD method allows incumbency to be considered as near-randomly assigned, I examine the causal effect of congressional incumbency's many advantages on presidential returns in cases that are otherwise equivalent by comparing the presidential results at Time 2 between cases where a Democratic congressional candidate just won and just lost the election at Time 1. As long as the random assignment assumption holds near the threshold and the difference in average congressional performance and popularity is also essentially exogenous because of the near exogeneity of the incumbency assignment mechanism in these very close districts, this analysis allows the claim that popular congressional candidates increase presidential vote share in their districts to be tested without bias. In the Appendix, I verify that the random assignment assumption holds for my data set near the treatment threshold.

3.1 *Data*

I use congressional district-level data[2] from 1950 to 2006 that includes Democratic two-party vote share for Congress[3] and President. Matching district-years together in the

[2]I would like to thank Gary Jacobson for making his data on congressional election returns available and Daniel Butler for his assistance with this data. There was some sporadic missing congressional data in the data set filled from congressional Quarterly's *America Votes* series. A very small amount ($N = 50$) of presidential data was also missing from several elections in some districts from the 1950s.

[3]Following Lee (2008), uncontested races are coded as if the only participant's party had won 100% of the two-party vote.

original data set, each observation in the resulting data set for the reverse coattails regression represents a pair of district-years where Time 1 is a midterm election and Time 2 is a presidential election. From this pool of 6,091 observations, I discard from my analysis anomalous districts in which:

1) a third party won at Time 1,
2) an incumbent switched political parties between Time 1 and Time 2,
3) a state has multiple at-large Congressional seats,[4]
4) a special election occurred between Time 1 and Time 2, rendering *Democratic Congressional Vote Share* (DCVS) *Time 1* moot, or
5) congressional redistricting occurred between Time 1 and Time 2.

The first four criteria exclude only 124 observations out of 6,091 or about 2% of the observations. The redistricting criterion omits a further 1,404 observations, reducing the sample to 4,563 district-year pairings covering every presidential election from 1952 until 2004. Table 1 details the number of paired observations included for every presidential election year. Note that the years 1952, 1972, and 1992 have a particularly small number of observations because decennial reapportionment prompted redistricting nationwide between Time 1 and Time 2 in these periods in almost all states.

3.2 *Model*

A common method for operationalizing RDDs is to model the results on either side of the treatment threshold with fourth-order polynomials, capturing the discontinuity between the two curves at the treatment threshold with a simple dummy variable, a strategy identical to using local linear regression but less sensitive to noise (Imbens and Lemieux 2008). The distance between the values of the two polynomials at the discontinuity captures the causal effect of the discontinuity in the independent variable on the dependent variable; in this case, it captures the causal effect of just electing a Democratic congressperson in the previous election on Democratic presidential performance while (due to the near-experimental nature of the design) holding all other relevant characteristics not affected by having a Democratic congressional incumbent naturally constant. My regression model for reverse coattails, therefore takes the following form:[5]

$$P_i = \alpha + \beta_1 V_i + \beta_2 V_i^2 + \beta_3 V_i^3 + \beta_4 V_i^4 + \gamma D_i + \beta_5 V_i D_i + \beta_6 V_i^2 D_i + \beta_7 V_i^3 D_i + \beta_8 V_i^4 D_i + \epsilon_i, \quad (1)$$

where P_i is *Democratic*[6] *Presidential (Two-Party) Vote Share Time 2* within congressional district i, α is a constant, V_i is the *Democratic Congressional (Two-Party) Vote Share Time 1* within congressional district i, D_i is a dummy variable set to 1 if $V_i \geq ½$ (that is, if the Democrat wins) and 0 otherwise, and ϵ_i is the error term.[7] γ is the coefficient of interest that captures the causal effect of congressional incumbency on P_i.

[4] In North Dakota and New Mexico in the 1950s, for example, the entire state was represented by two congresspeople who ran statewide.

[5] Following Imbens and Lemieux (2008), I use robust standard for all RD regressions.

[6] I use Democratic vote share following Lee (2008) and much of the coattail literature. In a two-party system, Republican results are essentially the exact mirror image. All vote share numbers are two-party vote share.

[7] For actual regressions, Democratic margin of victory in the two-party vote is used instead of two-party vote share such that V_i is equal to 0 at the discontinuity, allowing Y to identify the local average treatment effect without interference from the polynomials. That is, in the regression V_i = *Democratic Two-Party Vote Share* − 0.5.

Table 1 Number of observations included in reverse coattails analysis by year

Time 2 year	*N*	*Percent of all observations*
1952	262	5.74
1956	424	9.29
1960	422	9.25
1964	362	7.93
1968	229	5.02
1972	23	0.50
1976	425	9.31
1980	429	9.40
1984	318	6.97
1988	429	9.40
1992	26	0.57
1996	387	8.48
2000	430	9.42
2004	397	8.70
Total	4,563	100

As a premise of this analysis is the incumbency advantage, I also regress DCVS Time 2 on the fourth-order polynomial for DCVS Time 1 using a nearly identical model:

$$V_{i,t=2} = \alpha + \beta_1 V_i + \beta_2 V_i^2 + \beta_3 V_i^3 + \beta_4 V_i^4 + \gamma D_i + \beta_5 V_i D_i + \beta_6 V_i^2 D_i + \beta_7 V_i^3 D_i + \beta_8 V_i^4 D_i + \epsilon_i \quad (2)$$

where all the variables are the same as in the previous model except that the dependent variable $V_{i,t=2}$ is Democratic Congressional (Two-Party) Vote Share Time 2.

Finally, though RDD naturally places the greatest statistical weight on observations near the threshold, because observations far away from the threshold in the independent variable may be almost entirely unrepresentative of the environment at the threshold, it is common practice to exclude some of the observations furthest away from the threshold. Accordingly, note that the results I describe in the next section are from regressions that only use observations where DCVS Time 1 is within 25 percentage points of the threshold. This restriction excludes only about 13% of the observations that describe contested races. Interested readers are directed to the Appendix, where I discuss the problem of bandwidth selection at greater length and include the results of the main regressions with different bandwidth specifications and controls. The Appendix also shows that the following results are not substantively sensitive to a wide variety of different bandwidth specifications and controls.

3.3 *Results*

As this article is premised on the large effects of congressional incumbency, I first replicate Lee's (2008) RDD incumbency model for my data set to verify that congresspeople in the districts that come under study in this article did themselves benefit from incumbency. As expected, regressing DCVS Time 2 on the fourth-order polynomials for DCVS Time 1 and the victory dummy variable for all years (both midterm and presidential) yields a highly significant result for the effect of incumbency ($t = 9.31$, $p < .001$), estimated at 10.10 percentage points and with a 95% confidence interval of [7.97, 12.22] percentage points.

More importantly, the incumbency effect remains strong in cases where Time 2 is a presidential year (the only years for which the reverse coattails regression can be run), $t = 7.00$, $p < .001$. The 95% confidence interval for this narrower set of cases is [7.78, 13.82] centered at 10.79, indistinguishable from the broader case. With dummies for state-year,[8] this estimate of the incumbency effect in presidential years falls only slightly to 9.47 percentage points, remaining consistent with the other estimates ($t = 6.06$, $p < .001$).[9] The incumbency effect remains strong for the years my data set describes.

However, applying the RDD to the problem of reverse coattails by regressing *Democratic Presidential Vote Share* (DPVS) *Time 2* on the fourth-order polynomials for DCVS Time 1 and the dummy variable yields a markedly different result. The causal effect of electing a Democratic congressional incumbent on DPVS in the next election is insignificant and near zero ($t = 0.39$, $p = .698$); with dummies for state-year, this estimate remains insignificant ($t = -0.43$, $p = .668$) and becomes negative, with a 95% confidence interval of [−2.37, 1.52] centered at −0.43 percentage points. The full results of all regressions described in this section are presented in Table 2. As noted, Table 1A in the Appendix illustrates the robustness of these results to many different specifications and controls. No matter which observations or controls are included, congressional incumbency does not have a significant positive effect on presidential results in the next election.

These results of these regressions are especially striking when presented visually.[10] To make the effect of the discontinuity on the data more visually apparent, I display the observations as binned averages in 0.5 percentage point wide bins with fitted lines and the 95% confidence intervals. Figure 1 depicts the incumbency effect in presidential years, whereas Fig. 2 shows the reverse coattail effect. Although the incumbency effect is clear from Fig. 1, crossing the treatment threshold (displayed as a vertical line) does not have any apparent effect on the presidential results. In districts where Democratic congressional candidates just win the first election, they can expect much higher vote shares, yet Democratic presidential candidates do not appear to receive any spillover benefits in the next election from their congressional counterparts' previous victories.

3.4 *External Validity*

Before proceeding, it is important to note that the counterfactual RDD observes is not necessarily applicable to all congressional districts. Strictly speaking, RDD only captures the local average treatment effect for districts near the threshold—that is, in so-called marginal districts; the result that congressional popularity does not directly increase presidential vote shares, thus, might not be generalizeable to districts where one of the parties predictably wins decisively. However, the close races to which RDD can speak should be most of interest to scholars and practitioners of politics alike; the effect of coattails in districts and states where one party is virtually guaranteed to win is of less interest than coattails' potential roles in changing election outcomes.[11]

[8] That is, separate fixed effects for each state within each year; for example, Minnesota 1960, Minnesota 1964, Kentucky 1964.

[9] These results are consistent with other estimates of the incumbency effect and nearly identical to Lee (2008)'s results, which cover a slightly different historical period (1946–98 instead of 1952–2004) and estimate the congressional incumbency effect at about 8 percentage points, well within the 95% confidence interval of my results.

[10] Thank you to Daniel Butler for his help with constructing these visualizations.

[11] In the context of this article, however, it is of course not necessarily true that congressional districts where one party is favored are of no interest. Because electoral votes are assigned on a statewide basis, if congresspeople have reverse coattails in nonmarginal districts where they are nearly guaranteed to win reelection, any reverse coattail effect may in fact change statewide outcomes.

Table 2 Regression discontinuity design regression results

Dependent variable	*DCVS, Time 2—all years, incumbency*	*DCVS, Time 2—presidential years, incumbency*	*DCVS, Time 2—presidential years, incumbency*	*DPVS, Time 2—presidential years, reverse coattails*	*DPVS, Time 2—presidential years, reverse coattails*
Democratic Cong. Victory, Time 1	.1010** (.0185)	.1079** (.0154)	.0947** (.0156)	.0056 (.0144)	−.0043 (.0099)
DCVS Time 1	−.1645 (.4580)	−.5050 (.5945)	−.3199 (.6340)	−.4275 (.5478)	−.2799 (.3655)
DCVS Time 1^2	−14.25 (8.006)	−21.10* (10.26)	−19.29 (10.77)	−13.74 (9.210)	−13.78 (6.301)
DCVS Time 1^3	−91.45 (51.37)	−133.5 (65.65)	−124.7 (68.57)	−89.15 (57.28)	−89.92* (39.85)
DCVS Time 1^4	−198.3 (108.0)	−278.5* (138.2)	−263.8 (144.2)	−179.4 (117.2)	−179.5* (82.71)
Victory × DCVS Time 1	1.877** (.6779)	1.486 (.9245)	1.974* (.9465)	1.352 (.8625)	1.044 (.6232)
Victory × DCVS Time 1^2	.9482 (11.95)	18.61 (16.07)	7.779 (16.83)	6.100 (14.69)	9.047 (10.56)
Victory × DCVS Time 1^3	173.4* (76.06)	155.0 (102.0)	184.7 (104.6)	138.0 (91.64)	115.7 (66.55)
Victory × DCVS Time 1^4	25.46 (157.8)	221.7 (221.7)	158.7	72.40 (187.7)	132.4 (137.2)
Constant	−.0623** (.0077)	−.0700** (.0103)	—	−.0456** (.0097)	—
State-year fixed effects included?	N	N	Y	N	Y
N[a]	6,736	3,303	3,303	3,336	3,336
R^2	.6370	.6674	.7643	.2152	.7315

Note. The Democratic Congress Victory, Time 1 variables indicates the effect, ceteris paribus, of crossing the treatment threshold on the dependent variables. Although there is a large effect of incumbency overall and in presidential years, with Congressional Victory at Time 1 significantly increasing Congressional Vote Share at Time 2, there is no discernable effect of Congressional Victory at Time 1 on Presidential Vote Share at Time 2. As noted, all regressions use robust SEs, following Imbens and Lemieux (2008).

[a]*N*s differ slightly between incumbency and reverse coattails regressions due to sporadic missing data, concentrated in the 1950s, as discussed.

*Significant at the .05 level; **significant at the .01 level.

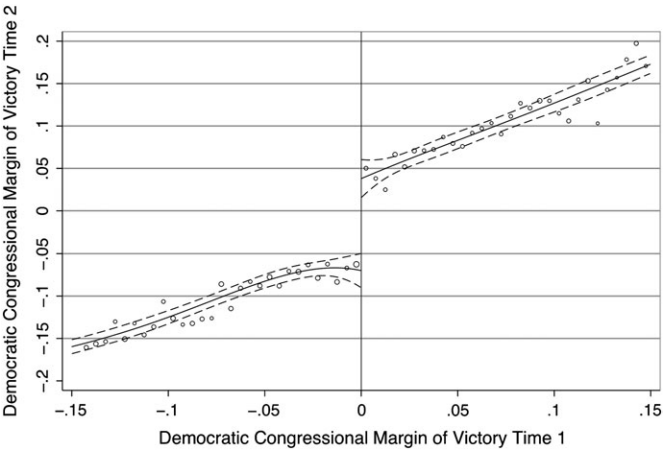

Fig. 1 Incumbency effect with observations in 0.5 percentage point wide bins. Lines represent fitted values from the model and the 95% confidence interval of the model. Sizes of circles correspond to the number of observations in a given bin. The model becomes less predictive of the data toward the extremes because of the lower number of observations occurring in this range. As discussed, this is of little concern because the local average treatment effect of interest and captured by the regression model applies only near the treatment threshold.

4 Conclusion

The results in the previous section demonstrated that large, exogenous changes in congressional popularity introduced by the incumbency advantage fail to lead to any detectable increase in performance for the presidential candidates with whom these incumbents share the ballot. Although this result does not directly contradict the explicit claims of the existing presidential coattail literature, it does run contrary to the literature's logical expectation—namely, that if voters engage the availability heuristic in American elections, there should be

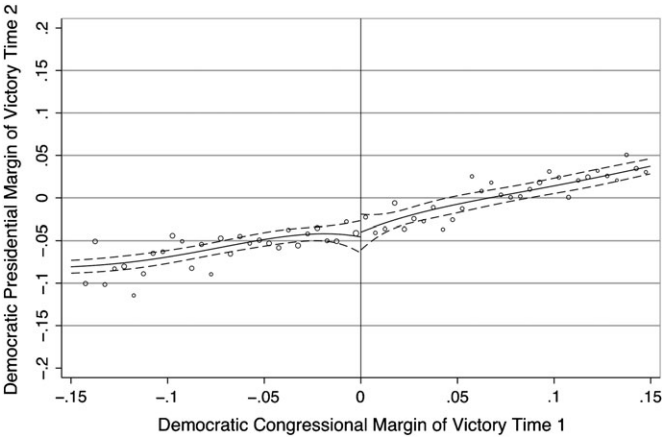

Fig. 2 Reverse coattails effect with observations in 0.5 percentage point wide bins. Lines represent fitted values from the model and the 95% confidence interval of the model. Sizes of circles correspond to the number of observations in a given bin.

spillover effects both up and down the ballot. Given the vulnerability of the existing presidential coattail literature's empirical strategies to bias, these findings further prompt a reexamination of our understanding of the presidential coattail effect.

Although an RDD fits the purposes of this study, it would be difficult to apply this method to the presidential coattail literature given the extremely small sample size and the electoral college (e.g., on which side of the treatment threshold would Al Gore's popular vote victory in 2000 be placed?). However, natural experiments that induce exogenous variation in presidential support such as the contours of media markets (e.g., Huber and Arceneaux 2007) and theoretical work based in such analyses should guide future coattail research since strictly observational techniques are unlikely to rigorously recover the counterfactual.

4.1 *Opportunities for Future Research*

My results can also plausibly speak to several other questions related to the study of politics. From a methodological perspective, although this article does not employ instrumental variables per se, it does demonstrate how RDD may be used as an instrument in a sense. Although many caveats are attached to the exogenous performance increase this article identified, scholars across American politics may find the natural experiment that results from congressional incumbency in highly contested districts useful for the investigation of other problems in the study of American politics. Although the estimated causal effects that emerge on a first order from a particular discontinuity should be considered very carefully, a discontinuity's indirect effects may prove even more interesting than its immediate implications (e.g., Green and Gerber 2002).[12]

One question raised by this article's result is what leads American political institutions to fail to produce reverse coattails given the recent analysis of Hainmueller and Kern (2008) (who also employ an RDD) that significant up-the-ballot spillover effects do exist in the German mixed-member proportional system, where voters simultaneously vote on both locally based and nationally selected representatives to Parliament. It is beyond the scope of this article to speculate on how or why modern Americans conceive of their political parties or candidates differently than Germans—the differences are clearly enormous. However, scholars of American and comparative politics alike may find the contrast between these results helpful to efforts to enrich our understanding of partisanship. Perhaps, for example, voters' partisan identities in the United States remain more fragmented between the local and national levels than we have realized.

Furthermore, a notable feature of this study is that it gauges the aggregate effect of incumbents' local party mobilization and persuasion efforts. Many political science literatures have stressed the importance of campaign spending to getting out the vote (e.g., Green and Gerber 2008) and of close races as fertile ground for voter mobilization (e.g., Cox and Munger 1989; Rosenstone and Hansen 1993). Work on these subjects would

[12]For one example of how a discontinuity might produce interesting results but to which strong caveats are also attached, consider Pettersson-Lidbom (2008), which finds using RDD that left-wing local governments in Sweden spend and tax 2%–3% more than right-wing local governments. Economists could use Pettersson-Lidbom's findings to investigate the causal effects of a larger local government without the confounding influence of constituent preferences since which party is in control—and, thus, which fiscal policy is implemented—is near-randomly introduced. However, although constituent preferences would remain naturally constant just across the discontinuity, left-wing governments also implement a whole host of different policies besides spending more; the causal effect of one action of such a government would, thus, remain highly contextual to its other policies (but, perhaps helpfully, not its electorate).

likely predict some spillover effect to result from incumbents' manifold organizational and financial[13] advantages over their challengers by virtue of increased turnout alone; yet, the structural advantages of incumbency do not appear to lead to any meaningful increase in partisan turnout nor do incumbents during the period studied appear to have used their advantages to successfully mobilize reliably partisan voters. My findings may, thus, prove useful to our understanding of congressional incumbency itself and of how incumbents employ partisan mobilization tactics.

Finally, my results raise questions about the efficacy of making inroads in areas that reliably elect members of the opposite party through the election of local officials. For example, Howard Dean's ''50-state strategy,'' which famously diverted millions of dollars in the 2006 elections to State Democratic parties in places such as Utah, was promoted to Party donors and leaders on the premise that in order to compete effectively in 2008 and beyond the Party needed to ''lay the groundwork'' by working to first elect Democrats down the ticket in 2006 (Gilgoff 2006).

With the Democratic Party continuing to invest in and praise the 50-state strategy well into 2008 (see Rosenberg 2008), the chairman of the California Republican Party, Ron Nehring, has also called for a Republican counterstrategy (2009). In his plea for funding and attention from the national Republican party, Nehring wrote, ''expanding the ranks of congressional, state and local officials from our party . . . makes it more likely a state will be competitive in a presidential election down the road.'' My analysis suggests that Nehring and his counterparts in both parties should be cautious about such claims. Although building party get-out-the-vote infrastructure is no doubt important, especially in light of recent results indicating that door-to-door canvassing can remain effective in getting out the vote even in high-salience elections (Middleton and Green 2008), I find no evidence that the presence of well-funded and well-liked local candidates has any benefits for these candidates' fellow partisans further up the ticket. For presidential hopefuls, the key to victory is unlikely to be found tied to coattails.

Appendix

Bandwidth Selection

As noted earlier, the regressions described in Table 2 and depicted in Figs 1 and 2 only include observations within 25 percentage points of the discontinuity on either side. This is because although the discontinuity itself is the object of interest since no data exist directly on the discontinuity, accurately modeling the effect of the discontinuity requires the use of some data for which the near-random assignment assumption is unlikely to hold. Excluding some observations is standard practice in the use of RDDs for several reasons. First, the sample averages of the dependent variable very close to the discontinuity in the independent variable will generally be biased estimates of the true conditional expectation function when that function has a non-zero slope (Lee 2008). Second, as Imbens and Lemieux

[13]Applying the RDD to congressional spending data in my data set bears out the claim that incumbents enjoy a large spending advantage. Regressing *Democratic Congressional Spending Advantage Time 2*, defined as the amount of *Democratic Spending Time 2—Republican Spending Time 2* unsurprisingly yields a highly significant result for the difference in spending advantages between Democrats on either side of the treatment threshold ($t = 7.16$, $p < .001$). The 95% confidence interval is [$65,084, $114,155], centered at $89,620 in 2008 (inflation adjusted) dollars. In highly contested races where the probability of winning for the nonincumbent party is highest, the incumbent party still enjoys a spending advantage tens of thousands of dollars larger than the disadvantage that party would have faced had they just lost the previous election instead.

(2008) argue, since there is likely to be some noise in any estimate given the small number of observations that fall very close to the threshold, relaxing the bandwidth can help improve the signal-to-noise ratio. An inspection of the observations very near the discontinuity in Fig. 2 illustrates this argument about why it is important to employ a sufficiently wide bandwidth and not only consider cases close to the discontinuity. Natural variation in the dependent variable evident near the discontinuity in Fig. 2 might cause a naive analysis that considers only these observations to register a spurious result despite the fact that the null trend remains clear from Fig. 2. Too wide a bandwidth, however, might place too much weight on observations far away from the threshold and bias the results unpredictably.

The choice of a bandwidth, therefore represents a tradeoff, and, as Imbens and Lemieux (2008) note, there is no one correct answer for which bandwidth should be used. Lee (2008), for example, uses a very wide bandwidth, only restricting his analysis to those observations where $|V_{\mathrm{i}} - .5| < .5$; that is, all races contested at Time 1. Imbens and Lemieux recommend that in practice simply including half of the data on each side of the discontinuity may balance considerations of internal and external validity with the desire to decrease noise and measurement error.

Therefore, for completeness Table 1A represents the RDD estimates of the incumbency and reverse coattail effects with the use of many different bandwidths, including the bandwidth that includes half of the data on each side of the discontinuity as Imbens and Lemieux recommend. Note that because during this historical period Democrats enjoyed greater success in U.S. House elections, the bandwidth for the regression that includes half of the data on either side extends further on the Republican side of the threshold. Each of the estimates in Table 1A are analogous to the first coefficient reported in each column of the regressions in Table 2 or γ in the model. Table 1A also displays the results when state fixed effects and state-year fixed effects are included for each bandwidth. Due to space considerations, the full list of covariate values for the fourth-order polynomials are not presented for each regression but are available from the author upon request.

The robustness of both results to different bandwidth specifications is clear from Table 1A. The incumbency estimate remains significant in all cases except one, where a very narrow bandwidth is paired with a large amount of fixed effects. Conversely, the reverse coattails estimates remain insignificant given every specification except one where the estimate reaches statistical significance in the negative direction.[14] The choice of bandwidth does not have any substantive impact on the claim that despite the strong effect of congressional incumbency on congresspeople's own reelection prospects, these advantages do not appear to transfer down the ticket to any meaningful extent.

Balance Check

Although RDD's econometric assumptions require that observations be randomly sorted into the Democrat win and loss conditions only at the theoretical limit $\lim_{V_i \to \frac{1}{2}}$, sufficiently large systematic differences might exist across the discontinuity in a finite data set to leave the resulting estimate of γ vulnerable to bias. For example, if there were no observations within 5 percentage points of the discontinuity, it would be difficult to argue that the counterfactual could truly be extrapolated. More plausibly, balance between the observations

[14] Interpreting one significant finding out of many insignificant findings would be to fall prey to multiple testing bias. However, for those interested in the implication of this finding that voters may engage in purposeful ticket-splitting behavior, see D. M. Butler and M. J. Butler (2006) for a refutation of this claim.

Table 1A RDD estimates with alternate bandwidths and controls

Dependent variables		*Specifications*			
DCVS, Time 2 incumbency	*DPVS, Time 2 reverse coattails*	*Bandwidth*	*Year fixed effects?*	*State × year fixed effects?*	*N*
.0916** (.0124)	−.0103 (.0119)	±.50	–	–	3,815
.0928** (.0121)	−.0169 (.0095)	±.50	Y	–	
.0890** (.0124)	−.0178* (.0083)	±.50	–	Y	
.1079** (.0154)	.0056 (.0144)	±.25	–	–	3,336
.1110** (.0155)	−.0055 (.0112)	±.25	Y	–	
.0947** (.0156)	−.0043 (.0099)	±.25	–	Y	
.0954** (.0195)	.0174 (.0177)	−.123 to .196	–	–	2,257
.0956** (.0198)	−.0045 (.0139)	−.123 to .196	Y	–	
.0799** (.0212)	−.0041 (.0127)	−.123 to .196	–	Y	
.1016** (.0255)	.0289 (.0225)	±.10	–	–	1,490
.1002** (.0260)	.0056 (.0177)	±.10	Y	–	
.0579* (.0231)	−.0108 (.0178)	±.10	–	Y	
.1334** (.0388)	.0488 (.0323)	±.05	–	–	764
.1273** (.0400)	.0090 (.0243)	±.05	Y	–	
.0457 (.0373)	−.0087 (.0304)	±.05	–	Y	

Note. All reported coefficients represent the estimated causal effect of *Democratic Two-Party Vote Share* Time 1 just passing the 50% two-party vote share threshold on Democratic Congressional and Presidential Two-Party Vote Shares, respectively, at Time 2. Reported sample sizes are for reverse coattail regressions, but the incumbency regressions differ by less than 1% in sample size.
*Significant at the .05 level; **significant at the .01 level.

may be of concern if congressional candidates or those funding them (e.g. party Congressional Campaign Committees) were able to anticipate their vote shares precisely enough spend enough to ''just win'' the most winnable races on purpose or if candidates who barely win are for some other reason systematically different than candidates who just miss winning.

To help ensure that the discontinuity at the 50% threshold in two-party vote does sufficiently randomly assign districts on either side of the threshold, I measure balance across the discontinuity at Time 1 using dummy variables for whether the state is in the American political south and whether the Democrat won the election at Time 0.

Table 2A presents the results of this randomization check across the discontinuity within different margins. Within 1 and 1.5 percentage points of the discontinuity, the assignment of cases to the Democrat loss and win conditions is indistinguishable from chance, with p values of .453 and .227, respectively. It is not surprising, however, that comparing electoral circumstances where Democrats won 48% of the two-party vote and 52% in the previous election would be significantly differences on the observables, with the differences between all cases 2 and 2.5 percentage points away from the discontinuity significant at $p = .044$ and $p = .014$, respectively. Note again that in order for the random assignment assumption to sufficiently hold only cases very near the threshold must be indistinguishable, not all cases included in the regression; from this analysis, it appears that this assumption does, indeed, hold. Readers interested in a more lengthy discussion of the econometric issues surrounding applying RDD in this context are directed to Lee (2008), who more formally defends the use of RDD in congressional races and performs even more extensive randomization and balance checks on a very similar data set.

Table 2A Test for balance on observables across the discontinuity

Time 1 margin	*.01*	*.015*	*.02*	*.025*
F	0.80	1.49	3.17	4.35
df_1	2	2	2	2
df_2	134	208	288	360
p	.453	.227	.044*	.014*
N	137	211	291	363

*Significant at the .05 level.

Although further away from the discontinuity, the randomization assumption does not appear to be satisfied, within 1.5 percentage points of the discontinuity, there are no significant differences between districts on the observables. This indicates that RDD's more lenient assumption that randomization occurs at least at the discontinuity (i.e., at the limit) is met.

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