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Do Congresspeople Have Reverse Coattails? Evidence From A Regression Discontinuity Design*

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Abstract

While the Presidential coattail effect has been an object of frequently study, the question of whether popular Congressional candidates boost vote shares in return for their parties' Presidential candidates remains unexplored. This paper investigates whether so-called reverse coattails exist using a Regression Discontinuity Design (RDD) with Congressional-district level data from Presidential elections between 1952 and 2004. Considering incumbency as pseudo-randomly distributed in cases where Congressional candidates have just won or lost their previous elections following from Lee (2008), I find that the substantial advantages of Congressional incumbency have no effect on Presidential returns for these incumbents' parties. This null finding underscores my claim that the existing theoretical and empirical literature on Presidential coattails deserves greater scrutiny and also has ramifications for the coattail literature as well as the literature on voter mobilization and both major American political parties' recent attempts to employ so-called "50-state strategies."

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1. Introduction

During his preparations for the Presidential election of 1956, James Finnegan, Adlai Stevenson's campaign manager, 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 an article in Time magazine during the run-up to the 1956 election recorded, 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." A strategy the Stevenson campaign dubbed "Operation Reverse Coattails" was thus 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 fifty 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 (Baraback 2008). While the strategy was met with harsh criticism early on, most notably from now-White House Chief of Staff Rahm Emanuel, after the 2006 midterms brought Democrats back into control of the US 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 anticipated Democrats success in the

2008 Presidential campaign ("ASDC Applauds 50 State Strategy"). Having witnessed Democratic success nationwide in recent elections, 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 intuitions of leaders of both parties today and historically, from the perspective of American Political Science the existence of reverse coattails may seem counterintuitive when Presidential coattails dominate the coattail literature – 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; Ferejohn 1983; Campbell 1986; Campbell and Sumners 1990; Flemming 1995; Cohen et al. 2000; Mattei and Glasgow 2005; Koch 2008; etc.). Whereas the traditional coattail effect as originally defined by Miller (1955) occurs when "the Congressional vote decision is motivated by the appeal of the Presidential candidate," reverse coattails occur when the Presidential vote decision is motivated by the appeal of the Congressional candidate.

As I will discuss in more detail, empirical investigation of the traditional coattail has proven extremely difficult because the counterfactual of what would have happened in specific Congressional elections without the Presidential influence is difficult to observe; furthermore, there are few good candidates for instrumental variables that could conceivably influence only President vote share. The same challenges apply equally to reverse coattails in principle – there is little at first glance that causes Congressional vote share to increase that would not also conceivably directly influence Presidential vote share as well. However, this paper describes how

a Regression Discontinuity Design (RDD) locates just such an instrument for Congressional candidate popularity in the electoral system itself.

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 pseudo-experimental counterfactuals for observations in which the same party has just lost the previous election. In brief, these districts can be considered to provide observable counterfactuals 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. As the only variable that should systematically differ right on either side of the 50% threshold in two-party vote (i.e. at the limit) 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 itself has shown that the causal effect of winning an election – in other words, the incumbency effect – is an approximately 10 percentage point increase in vote share in the next. However, since Lee's method allows incumbency to be considered as essentially experimentally introduced in these close districts, we can also consider the associated benefits of incumbency as having been randomly introduced as well. In other words, Lee thus establishes not only a rigorous measure of the magnitude of the incumbency effect but also an exogenous instrument for Congressional popularity (i.e. incumbency in close districts) that could not conceivably directly affect Presidential results except through a coattail mechanism. In this paper, I accordingly test whether these benefits of Congressional incumbency, which include an approximately 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, what has been called reverse coattails.

However, RDD finds 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 matters and is interesting, especially given that American political elites have claimed that it exists for more than half a century. More significantly, I argue that the current theoretical logic of the traditional Presidential coattail literature together with the widely accepted conclusions of the incumbency literature about the personal vote bonus do predict the existence of reverse coattails. I thus argue that my findings cast doubt on the Presidential coattail literature's existing theoretical model as they demonstrate that one of the existing literature's theoretical predictions does not hold. I also show how bias introduced by this literature's existing methodology led to existing theoretical explanations for the Presidential coattail effect; in fact, applying the current coattail literature's methodology to reverse coattails registers a highly significant positive result despite RDD's null finding. Finally, this result may some ramifications for other literatures in comparative and American politics.

The rest of the paper proceeds as follows: In the next section, I review and critique the existing theoretical and empirical approaches to the study of coattails; In Section 3, I describe the data, this paper's Regression Discontinuity Design, and the results as well as applying the existing literature's methods; In Section 4 I review several remaining objections to my theoretical analysis, conclude, and discuss possible implications of my results for other topics in the study of politics.

2. Existing Presidential Coattail Literature

While there are no studies of reverse coattails in the American context to my knowledge, as noted the literature is rich with studies of Presidential coattails. In this section, I review and critique the existing Presidential coattail literature's empirical methods and explain why an exploration of reverse coattails is important to deepening our understanding of Presidential coattails.

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 originally identified highlights the inability of researchers to observe the counterfactual in traditional observational studies of coattails, which at bottom require an attempt to isolate what Congressional vote shares would have been without the Presidential influence. As there is no such counterfactual world and factors that influence Presidential vote shares are likely to influence Congressional vote shares as well, researchers over the past several decades attempting to recover the coattail counterfactual have primarily employed two strategies: a) controlling for variables such as Congressional candidate spending, quality, and incumbency status, thus claiming to isolate the remaining effect of increased vote share attributable to the Presidential candidate's appeal as reflected by the Presidential

candidate's vote share, and b) narrowing the focus of analyses to open seat races or comparing across open seat and non-open-seat races to minimize bias.

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 output might read 'the unobservable factors that influence gubernatorial vote share for a party also influence that party's state legislative vote share'. Likewise, much of the existing Presidential literature on coattails rests on the assumption that researchers can employ models capable of capturing all the factors that may contribute to both Congressional and Presidential candidates' success except for Presidential candidate effects in such a way that allows researchers to establish a direct causal link between candidates' vote shares.

Faced with a similar pattern of strictly observational research in the incumbency literature, Gelman and King (1990) opened their influential piece on the Congressional incumbency advantage with a broad critique of such methods, arguing instead for a focus on the difference between vote shares by candidates running for open seats and incumbents running for reelection, a technique that has been employed by most contributions to the incumbency literature for the past two decades. Inspired by Gelman and King, recent contributions to the coattail literature have also partitioned data by open seat status, arguing 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, despite that this new approach has features that resemble a natural experiment, as Cox and Katz (2002) argue open seats are not likely to occur randomly but in large part due to the strategic decisions made by incumbents. Using evidence from a natural experiment on term limited legislators, Engstrom and Monroe (2006) corroborate this expectation empirically and find that seats vacated voluntarily by incumbents, perhaps because these incumbents are more likely to be those that expect to lose, strongly and systematically differ from seats where incumbents decide to run again. Both of the coattail literature's existing strategies for reducing bias thus remain critically vulnerable to bias.

Existing Theoretical Model

Not only are existing empirical methods vulnerable to the bias outlined in the previous subsection, but since incumbents are more likely to retire when the 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 Gelman and King's method, recent literature on Presidential coattails has thus reached the conclusion that Presidential coattail effects are much stronger in open seat contests than in districts where Congressional incumbents are running for re-election. 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 finding, a potentially spurious result of flawed methodology, as evidence that the Presidential

coattail effect is a result of the availability heuristic, which leads voters to incorporate their attitudes towards candidates for whom they hold well-defined and more cognitively accessible attitudes into their attitudes of 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 towards 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.

This explanation is entirely plausible, yet, as outlined previously, persistent omitted variable bias has made it prove extremely difficult to empirically investigate this claim that the availability heuristic leads to spillover effects in the American political system. Since as I have argued it has been difficult to submit this theoretical claim to a rigorous empirical test that would be capable of falsifying it directly, another of evaluating the coattail literature's theoretical model is by investigating another of its 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 – i.e., the literature's theoretical model 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, with the average Congressperson having served their district for around 10 years, even given the high salience of Presidential campaigns it is imprudent 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, much of the literature claims that attitudes towards Congressional incumbents are strong enough to overwhelm the heuristic entirely even after incumbents have served just one term in Congress.

Since this paper uses Congressional incumbency as an instrument for Congressional popularity, it is 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 the coattail literature argues generate 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 voters form towards incumbents are not associated with a reorientation of voters' general political attitudes but 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.

If the powerful Congressional incumbency effect, responsible for an approximately ten percentage-point difference in vote shares and driven by the 'personal vote' could inform voters' Presidential choices, the incumbency literature taken together with recent coattail literature following Mondak and McCurley (1994) thus leaves us with strong reasons to think that if the personal vote can spillover in the American context 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 there is thus cause to place our theoretical understanding of Presidential coattails under even closer scrutiny.

3. Method, Data, and Results

In this section, I describe in more detail how a Regression Discontinuity Design (RDD) can be applied to the model of reverse coattails, my data, my method, and the results both using RDD and employing the literature's traditional methods.

As discussed, the coattail literature's traditional methods rest on the implicit assumption that researchers are capable of capturing all of the factors that might cause variation in both Congressional and Presidential vote shares. To escape this persistent omitted variable bias and test the proposition that all else equal a more popular Congressperson helps their party's Presidential candidates, researchers would most ideally be able to dole out popularity to members of Congress in a randomized experiment. While this is of course impossible, the natural

world does something similar to random assignment of Congressional candidate popularity through the pseudo-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 re-election at *Time 2* – in other words, the incumbency effect – by calculating the magnitude of the discontinuity at the theoretical limit.

However, since Lee's RDD method allows us to consider incumbency as randomly assigned, it thus establishes not only a rigorous measure of the magnitude of the incumbency effect but also an exogenous instrument[1] for large changes in Congressional popularity (i.e. the incumbency advantage in close districts) that could not conceivably directly affect Presidential results except through a coattail mechanism.[2] Accordingly, I examine the effect of Congressional incumbency's attendant 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

[1] Note that even though this paper essentially employs an instrumental variables approach, it would not be appropriate to use 2SLS. First, the variation induced in the Congressional incumbency is associated with no uncertainty – either a candidate is an incumbent or she is not. Second, the variability in the strength of the incumbency advantage across districts is neither rigorously measurable nor of interest; the only relevant assumption is that *on average* winning grants advantages.

[2] The relevant conditional independence assumption in this case is that Congressional incumbency status itself does not directly affect Presidential results except through the advantages of incumbency. In this case the CIA is met somewhat tautologically since any conferred advantage would be classified as part of the instrument by definition. In other applications of RDD as an instrument, however, the CIA might deserve much greater attention.

performance and popularity is exogenous because of the exogeneity of the incumbency assignment mechanism in these districts, this analysis thus 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 dataset near the treatment threshold.

Data

I use Congressional-district-level data[3] from 1950 to 2006 that includes Democratic two-party vote share for Congress[4] and President. Matching district-years together in the original dataset, each observation in the resulting dataset 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,[5]

4) a special election occurred between *Time 1* and *Time 2*, rendering *Democratic Congressional Vote Share 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 my observations. The redistricting criterion omits a further 1,404 observations, reducing the sample

[3] 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 filled in with data 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.

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

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

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 amount of observations because decennial reapportionment prompted redistricting nationwide between *Time 1* and *Time 2* in these periods in almost all states.

[Insert Table 1 About Here]

Model

A common method for operationalizing RDDs is to model the results on either side of the threshold with fourth-order polynomials, capturing the discontinuity between the two polynomials 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, of just electing a Democratic Congressperson in the previous election on Democratic Presidential performance while (due to the quasi-experimental nature of the design) holding all other relevant characteristics not affected by having an Democratic Congressional incumbent naturally constant. My regression model for reverse coattails thus takes the following form:[6]

(1)

where P_i is *Democratic*[7] *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

[6] Following Imbens and Lemiuex (2008), I use robust standard for all RD regressions.
[7] 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.

the Democrat wins) and 0 otherwise, and ε_i is the error term.[8] γ is the coefficient of interest that captures the causal effect of Congressional incumbency on P_i.

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

(2)

where all the variables are the same as in the previous model except that the dependent variable 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 *Democratic Congressional Vote Share 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 and control specifications.

Results

As this paper considers incumbency as an instrument, I first replicate Lee's (2008) RDD

[8] 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 D to identify the local average treatment effect without interference from the polynomials. I.e., in the regression V_i = *Democratic Two-Party Vote Share* - 0.5.

incumbency model for this dataset to verify that Congresspeople in the districts in my dataset did themselves benefit from incumbency. As predicted, regressing *Democratic Congressional Vote Share Time 2* on the fourth-order polynomials for *Democratic Congressional Vote Share Time 1* and the victory dummy variable for all years (both midterm and Presidential) yields a highly significant result for the effect of incumbency ($t = 9.31$, $p < 0.001$), 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), $t = 7.00$, $p < 0.001$. 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,[9] this estimate of the incumbency effect in Presidential years falls only slightly to 9.47 percentage points, remaining consistent with the other estimates ($t = 6.06$, $p < 0.001$).[10] The incumbency effect remains strong for the years my dataset describes.

However, applying the Regression Discontinuity Design to the problem of reverse coattails by regressing *Democratic Presidential Vote Share Time 2* on the fourth-order polynomials for *Democratic Congressional Vote Share Time 1* and the dummy variable yields a markedly different result. The causal effect of electing a Democratic Congressional incumbent on Democratic Presidential vote share in the next election is insignificant and near zero ($t = 0.39$, $p = 0.698$) while with dummies for state-year this estimate remains insignificant ($t = -0.43$, $p = 0.668$) and becomes negative, with a 95% confidence interval of [-2.37, 1.52] centered at -0.43

[9] That is, separate fixed effects for each state within each year; e.g., Minnesota 1960, Minnesota 1964, Kentucky 1964.

[10] 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 (1946-1998 instead of 1952-2004) and estimate the Congressional incumbency effect at about 8 percentage points, well within the 95% confidence interval of my results.

percentage points. The full results of all regressions described in this section are presented in Table 2 while, as noted, Table 1A in the Appendix details 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.

[Insert Table 2 About Here]

These results of these regressions are especially striking when presented visually.[11] To make the effect of the discontinuity on the data more visually apparent, I display the observations as binned averages in 0.5 percentagAKE Ie-point-wide bins rather than individually together with trend lines representing the expected values and the 95% confidence interval of the model. Graph 1 depicts the incumbency effect in Presidential years while Graph 2 shows the reverse coattail effect. While the incumbency effect is clear from Graph 1, crossing the treatment threshold (displayed as a vertical line) does not have any apparent effect on the Presidential results. While in districts where Democratic Congressional candidates just win the first election they can expect much higher vote shares, Democratic Presidential candidates do not appear to receive any spillover benefits in the next election from their Congressional counterparts' previous victories.

[Insert Graphs 1 and 2 About Here]

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

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

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 most of the time. 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.[12]

Results with Previous Method

In this subsection, I will employ the methods of the traditional coattail literature to this data to underscore these methods' vulnerability to bias. Mattei and Glasgow (2005)'s recent contribution is broadly representative of the existing literature's empirical strategies, regressing *Democratic*[13] *Congressional Vote Share Time 2* on *Democratic Congressional Vote Share Time 1*, *Democratic Presidential Vote Share Time 2*, and dummy variables representing whether or not (i) Democrats won the seat, (ii) there is a Democratic Freshman incumbent, (iii) there is a Republican Freshman incumbent, (iv) there is an open seat held by a Democrat, (v) there is an open seat held by a Republican, and finally an interaction term between a dummy variable representing open seat status and the Presidential two-party vote share. Since in this paper I search for reverse coattails, I instead regress *Democratic Presidential Vote Share Time 2* on *Democratic Congressional Vote Share Time 2* and also change the interaction term to *Open Seat*

[12] In the context of this paper, 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 non-marginal districts where they are nearly guaranteed to win re-election, any reverse coattail effect may in fact change statewide outcomes.

[13] Mattei and Glasgow (2005) actually regress on Republican two-party vote shares, but since my data thus far used Democratic returns I continue to do so for consistency. In a two-party system this subtlety is irrelevant.

x *Congressional Vote Share Time 2* instead of *Open Seat* x *Presidential Vote Share Time 2*.[14] As discussed, however, there is good reason to think short-term factors that cause Congressional vote share to vary may also cause similar variation in Presidential vote share. Strictly observational models will thus attach causality to spurious correlations between Congressional and Presidential vote share generated by uncaptured factors that affect both.

[Insert Table 3 About Here]

The result of a strictly observational regression is presented in Table 3. As predicted, had this paper employed only the current methods of the Presidential coattail literature, it would have concluded that reverse coattails exist; the coefficient for Democratic Congressional vote is highly significant ($t = 18.62$, $p < .0001$) and centered at .302, indicating that the strictly observational model predicts an increase of .302 percentage points in *Democratic Presidential Vote Share Time 2* for each percentage point increase in *Democratic Congressional Vote Share Time 2*. With an incumbency effect of around 10 percentage points, the model would thus predict that an exogenous increase of this size should lead to a reverse coattail effect of about 3 percentage points in Presidential vote share, far above any of the estimates of the RDD reverse coattail model.

The extraordinarily high statistical significance of this result also underscores the claim made by Gerber, Green, and Kaplan's (2004) that strictly observational methods not only supply biased estimates but fail to report uncertainty about the method itself; the error embedded in observational coattail models is far higher than the statistics report. Of course, this aberrant result does not 'disprove' the validity of the Presidential coattails' prevailing models per se. However, it does illustrate how applying a strictly observational model to the problem of coattails is very

[14] Because the existing literature does not restrict its analysis to cases near the 50% threshold, I only exclude uncontested races from this regression. However, the results do not meaningfully change when different bandwidths are used.

likely to induce unreported bias when any of the many factors that might lead both Congressional and Presidential results to vary are not taken into account.

4. Discussion, Potential Criticism, and Conclusion

In this paper I demonstrated that large, exogenous increases in Congressional popularity granted near the 50% threshold in the two-party vote by the incumbency advantage fail to translate into any systematic improvement in performance for Presidential candidates who share the ticket with these popular incumbents. I also explained and demonstrated that RDD is superior to existing empirical methods for measuring coattail effects, which remain vulnerable to omitted variables bias. Macro events such as broad shifts in partisanship or even economic conditions (e.g. Fair 1978; Bartels 2008, Ch. 4; etc.) may be mistaken for spillover effects by traditional observational techniques; indeed, applying traditional techniques to my data revealed a markedly different result.

Furthermore, I reviewed the traditional coattail's theoretical model that Presidential candidates with strong personal appeal transfer the benefits of this appeal to Congressional candidates of their party because of the availability heuristic that it argues is especially pronounced when not "muted" by the strong Congressional incumbency advantage. However, as I argued, there are compelling reasons to believe that if the availability heuristic does lead to spillover effects in American politics, the large personal appeal granted by Congressional incumbency should also inform voters' attitudes towards Presidential candidates. Yet, though the fact that this theoretical prediction does not hold cannot reject the hypothesis that Presidential candidates can generate coattails in a way Congressional candidates do not, it does prompt a more thorough examination of the claim that spillover effects exist in the American context.

Before concluding, I will address two potential objections to this theoretical analysis.

Alternative Explanations for Incumbency

The traditional incumbency literature leaves little doubt that the mechanisms of the personal vote mentioned previously such as the franking privilege and television exposure, which generate strong, positive attitudes for incumbents, 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, two recent challenges to this conventional understanding of the incumbency effect merit mention.

The first has stressed the ability of incumbents to deter high quality challengers as responsible for part of the observed incumbency effect (Cox and Katz 1996; Carson, Engstrom and Roberts 2007; Butler 2009). With strong empirical support, this literature has shown that high quality challengers are likely to seek more favorable electoral circumstances or delay their ambitions given common knowledge about the power of the incumbency effect. 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, not just electoral fortune. Reassuringly, empirical analyses have found that while assuredly accounting for some of the incumbency effect, the challenger deterrent effect does not account for nearly all of the incumbency effect. Using a Regression Discontinuity Design, 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 pseudo-natural

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," i.e., the very districts that came under the closest study in this paper. 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, even as this advantage does not help Presidential candidates from their party.

Another challenge to the conventional wisdom on incumbency (Cox and Katz 2002; Engstrom and Monroe 2006) proposes, as discussed earlier in the context of the flaws in current coattail methods, that some of the incumbency advantage is due to selection bias and the strategic decisions of incumbents. Since incumbents are less likely to run for reelection if they expect to lose, the incumbency advantage will appear greater than it actually is. However, as my research design does not partition based on open seat status, this is not an issue for my results.

Opportunities for Future Research

First, from a methodological perspective this paper illustrates how RDD can allow incumbency to serve as an exogenous instrument for Congressional performance. While several caveats are attached to this exogenous performance increase, scholars across American politics may find the natural experiment that results from Congressional incumbency in highly contest districts useful for the investigation of other problems in the study of Americans politics. More broadly, this paper also demonstrates how RDD can be used to mimic traditional instrumental variables approaches. While the precise 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.[15]

Second, on the topic of coattails more specifically, while a Regression Discontinuity Design 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 (for example, 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, Krasno and Green 2008) and theoretical work based in such analyses should guide future coattail research since strictly observational techniques have proven inadequate in their efforts to recover the counterfactual.

Not only should my findings prompt a reexamination of our knowledge about the Presidential coattail effect, but the finding that reverse coattails do not exist in the United States can also plausibly speak to several other questions related to the study of politics.

First, one question raised by this paper's result is what leads American political institutions to not be conducive to reverse coattails given the recent analysis of Hainmueller and Kern (2008) (who also employ a Regression Discontinuity Design) 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 paper to speculate on how or why modern Americans conceive of their political parties or candidates differently than Germans – the differences are clearly

[15] For example, consider Pettersson-Lidbom (2008), which demonstrates using RDD that left-wing governments spend and tax 2-3% more than right-wing governments. A macroeconomist might find this exogenous systematic change in government behavior invaluable for recovering the causal effect of the sorts of behaviors left-wing governments are more likely to exhibit; of course, however, such governments also likely to do many other things, so the full context of first-order effects of a discontinuity (i.e. left-wing governments) must be considered when attempting to isolate any of their second-order effects (i.e. the effect of tax rates on economic growth). The advantage of RDD in this example, however, is that the larger circumstances that tend to lead to the election of left-wing governments do not need to be taken into account.

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.

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 2004) and of close US House races as fertile ground for voter mobilization (e.g. Cox and Munger 1989; Rosenstone and Hansen 1993). Work on these subjects would likely predict some spillover effect to result from incumbents' manifold organizational and financial[16] 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 one element of the contemporary strategy undertaken by political parties to expand the number of states in which they compete electorally by first making inroads through the election of local officials in an attempt to build a base of support. 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 sold to Party donors and leaders on the premise that in order to compete effectively in 2008 and

[16]Applying the RDD to Congressional spending data in my dataset 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 ($t = 7.16$, $p < 0.001$). 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 non-incumbent 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. This statistic has been reported in no previous study of a not strictly observational nature.

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, (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 making such claims. While 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), there is 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 Graphs 1 and 2 only include observations within 25 percentage points of the discontinuity on either side. This is because while the discontinuity itself is the object of interest, since no data exists directly on the discontinuity accurately modeling the effect of the discontinuity requires the use of some data for which the pseudo-random assignment assumption is unlikely to hold. This 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 (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 Graph 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 Graph 2 might cause a naïve analysis that considers only these observations to register a spurious result despite the fact that the null trend remains clear from Graph 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 thus 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

 < .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.

[Insert Table 1A About Here]

For completeness, Table 1A thus 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 for this historical period Democrats enjoyed greater success in US 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.[17] 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

[17] 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 ticket-splitting behavior, see Butler and Butler (2006) for a refutation of this claim.

ticket to any meaningful extent.

Balance Check

While RDD's econometric assumptions require that observations be randomly sorted into the Democrat win and loss conditions only at the theoretical limit , sufficiently large systematic differences might exist across the discontinuity in a finite dataset to leave the resulting estimate of 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 was truly observed. More plausibly, balance between the observations 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 this paper's RDD is sufficiently randomly sorting 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*.

[Insert Table 2A About Here]

Table 2A presents the results of this pseudo-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 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 dataset.

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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%**

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**	.1079**	.0947**	.0056	-.0043
	(.0185)	(.0154)	(.0156)	(.0144)	(.0099)
DCVS Time 1	-.1645	-.5050	-.3199	-.4275	-.2799
	(.4580)	(.5945)	(.6340)	(.5478)	(.3655)
DCVS Time 1^2	-14.25	-21.10*	-19.29	-13.74	-13.78
	(8.006)	(10.26)	(10.77)	(9.210)	(6.301)
DCVS Time 1^3	-91.45	-133.5	-124.7	-89.15	-89.92*
	(51.37)	(65.65)	(68.57)	(57.28)	(39.85)
DCVS Time 1^4	-198.3	-278.5*	-263.8	-179.4	-179.5*
	(108.0)	(138.2)	(144.2)	(117.2)	(82.71)
Victory X DCVS Time 1	1.877**	1.486	1.974*	1.352	1.044
	(.6779)	(.9245)	(.9465)	(.8625)	(.6232)
Victory X DCVS Time 1^2	.9482	18.61	7.779	6.100	9.047
	(11.95)	(16.07)	(16.83)	(14.69)	(10.56)
Victory X DCVS Time 1^3	173.4*	155.0	184.7	138.0	115.7
	(76.06)	(102.0)	(104.6)	(91.64)	(66.55)
Victory X DCVS Time 1^4	25.46	221.7	158.7	72.40	132.4
	(157.8)	(221.7)		(187.7)	(137.2)
Constant	-.0623**	-.0700**	-	-.0456**	-
	(.0077)	(.0103)		(.0097)	
State-Year FEs Included?	N	N	Y	N	Y
N†	6736	3303	3303	3336	3336
R^2	.6370	.6674	.7643	.2152	.7315

† Ns differ slightly between incumbency and reverse coattails regressions due to sporadic missing data, concentrated in the 1950s, as discussed.

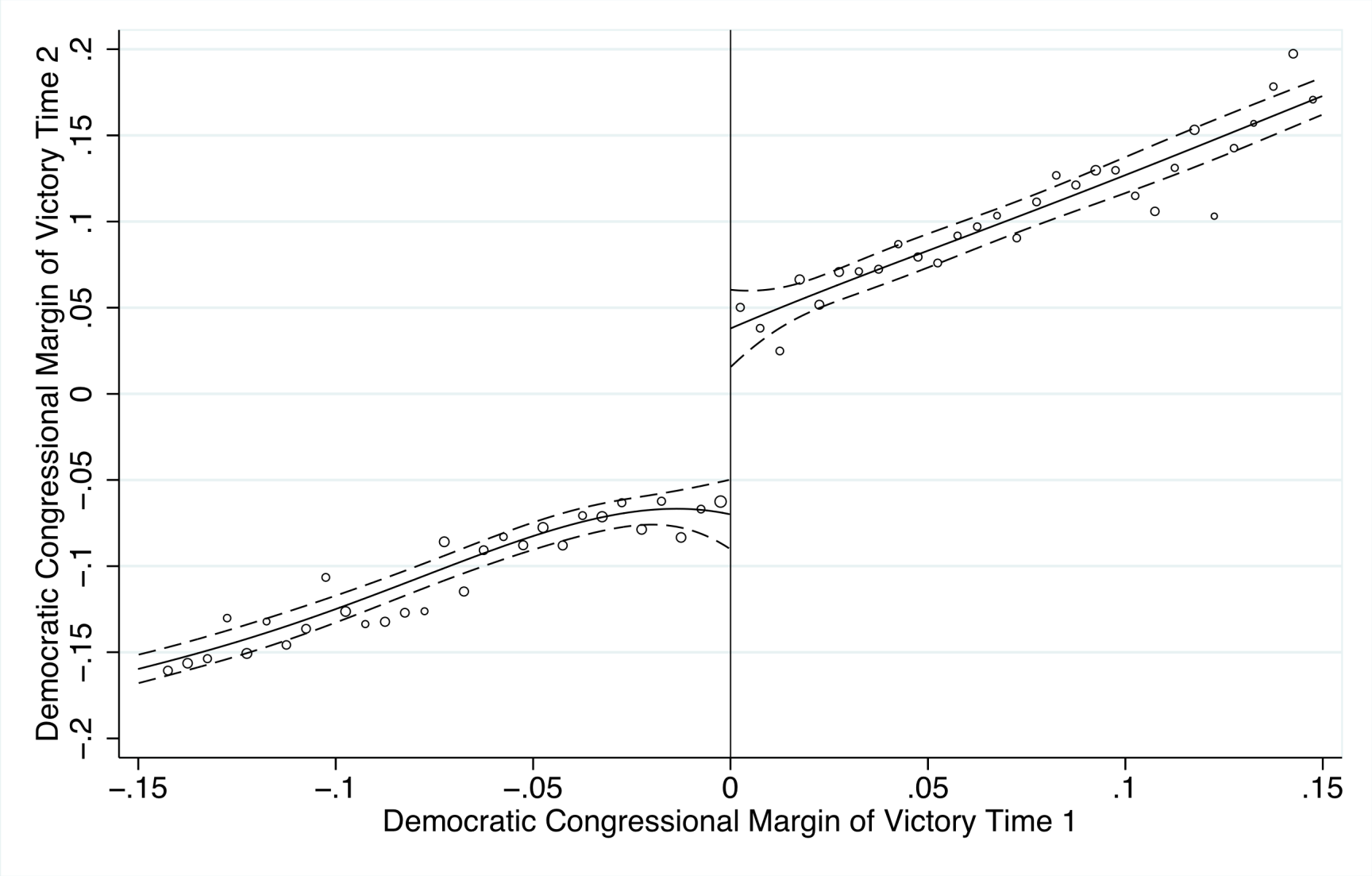
Notes: * = Significant at the 0.05 level. ** = Significant at the 0.01 level. DCVS = Democratic Congressional Vote Share. DPVS = Democratic Presidential Vote Share. The Democratic Cong. Victory, Time 1 variables indicates the effect, ceteris paribus, of crossing the treatment threshold on the dependent variables. While 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 standard errors, following Imbens and Lemieux (2008).

Table 3. Observational Model Applied to Reverse Coattails

Dependent Variable: Democratic Presidential Vote Share Time 2	
Democratic Congressional Vote Share Time 2	.3022**
	(.0162)
Time 2 – Republican Freshman Incumbent	-.0076
	(.0060)
Time 2 – Democratic Freshman Incumbent	.0010
	(.0061)
Time 1 – Democrat Won	-.0059**
	(.0064)
Time 2 – Open Seat Held By Republicans	-.0969**
	(.0218)
Time 2 – Open Seat Held By Democrats	-.0750*
	(.0297)
Democratic Congressional Vote Share Time 1	.2474**
	(.0223)
Democratic Congressional Vote Share Time 2 X Open Seat	.1557**
	(.0467)
Constant	.2297**
	(.0075)
N	3,779
R^2	.4059

Notes: * = Significant at the 0.05 level. ** = Significant at the 0.01 level. As discussed in the text, while the observational model predicts reverse coattails very strongly, the RDD model shows no such pattern.

Graph 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 towards the extremes because of the lower number of observations occurring in this range. As discussed, this is of little concern because the LATE of interest and captured by the regression model applies only near the treatment threshold.



Graph 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. The model becomes less predictive of the data towards the extremes because of the lower number of observations occurring in this range. As discussed, this is of little concern because the LATE of interest and captured by the regression model applies only near the treatment threshold.

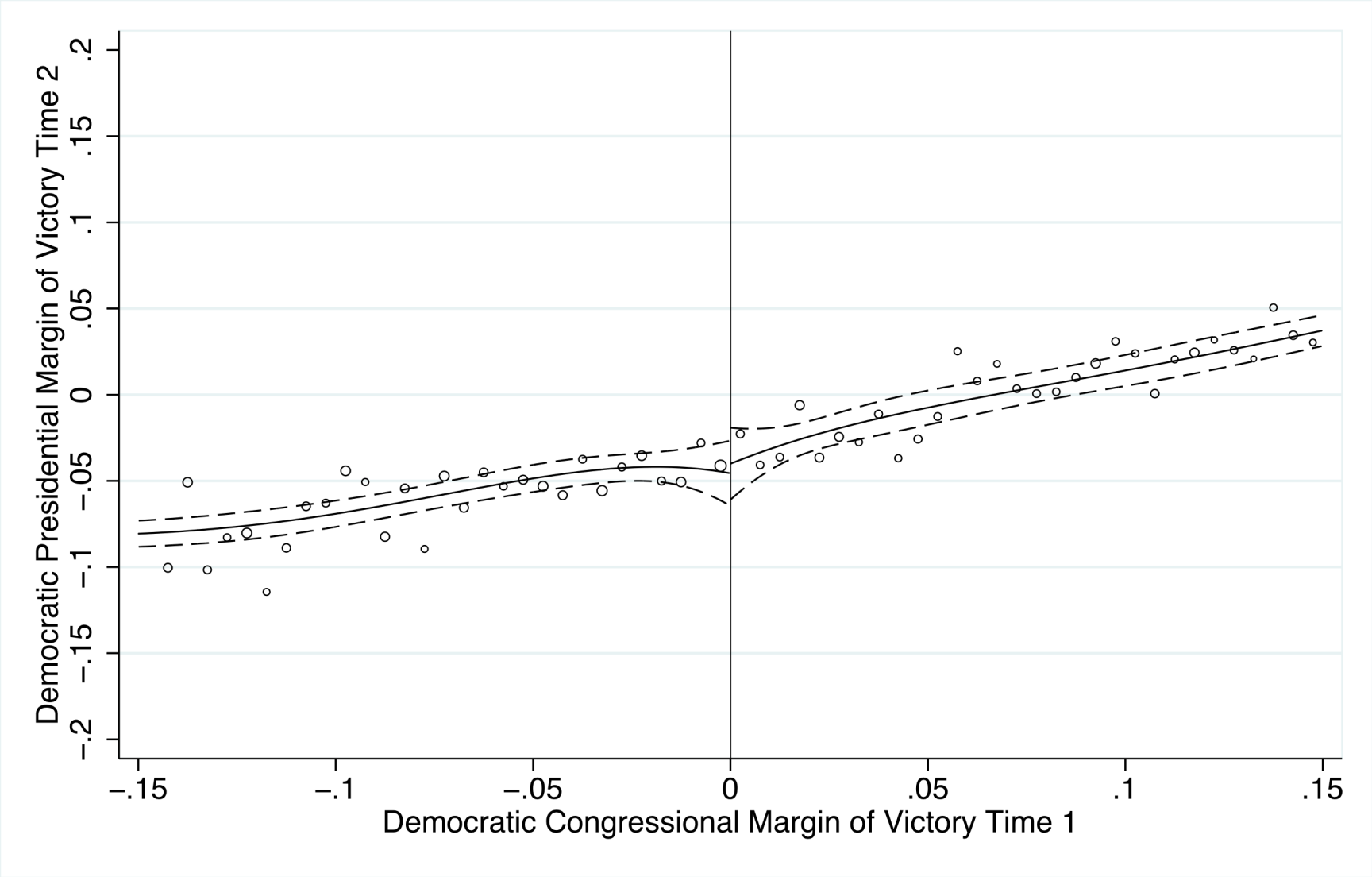


Table 1A. RDD Estimates with Alternate Bandwidths and Controls

Dependent Variables		Specifications			
DCVS, Time 2 *Incumbency*	DPVS, Time 2 *Reverse Coattails*	Bandwidth	Year FE?	State X Year FE?	N
.0916**	-.0103	±.50	-	-	3815
(.0124)	(.0119)				
.0928**	-.0169	±.50	Y	-	
(.0121)	(.0095)				
.0890**	-.0178*	±.50	-	Y	
(.0124)	(.0083)				
.1079**	.0056	±.25	-	-	3336
(.0154)	(.0144)				
.1110**	-.0055	±.25	Y	-	
(.0155)	(.0112)				
.0947**	-.0043	±.25	-	Y	
(.0156)	(.0099)				
.0954**	.0174	-.123/+.196	-	-	2257
(.0195)	(.0177)				
.0956**	-.0045	-.123/+.196	Y	-	
(.0198)	(.0139)				
.0799**	-.0041	-.123/+.196	-	Y	
(.0212)	(.0127)				
.1016**	.0289	±.10	-	-	1490
(.0255)	(.0225)				
.1002**	.0056	±.10	Y	-	
(.0260)	(.0177)				
.0579*	-.0108	±.10	-	Y	
(.0231)	(.0178)				
.1334**	.0488	±.05	-	-	764
(.0388)	(.0323)				
.1273**	.0090	±.05	Y	-	
(.0400)	(.0243)				
.0457	-.0087	±.05	-	Y	
(.0373)	(.0304)				

Notes: * = Significant at the 0.05 level, ** = Significant at the 0.01 level.
All reported coefficients represented 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 for every category.

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

Notes: * = Significant at the 0.05 level.