Leveraging an October Surprise to Estimate Coattail Effects

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Abstract

Does a presidential candidate's popularity impact her party's down-ballot success? So-called presidential "coattail effects" can be challenging to identify: it is difficult to distinguish between voters' opinion of a prominent candidate and her political party. In this paper, I exploit a shock to candidate popularity created by late-election information in the 2016 presidential election to estimate coattail effects. Using a difference-in-differences design and variation in the availability of early voting, I find that counties only able to cast their ballot after the release of FBI Director James Comey's letter to Congress on October 28, 2016, saw an increase of 3.2 percentage points in Republican presidential vote share. Using an instrumented difference-in-differences model, I find that a one percentage point increase in Republican presidential vote share led to a 0.86 percentage point increase in down-ballot Republican vote share. This suggests that late-election information not only affects the election of the candidate in question, but can lead to spillover effects for others in her party as well.

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1 Introduction

Political parties act as a coordination mechanism through which voters are mobilized, resources raised, and policies passed in the name of advancing common ideological goals. They also lower informational costs to voters: rather than taking the time to evaluate every candidate each election, a voter can interpret a party affiliation as a signal of a candidate's values and objectives. This generates the possibility for electoral spillovers between candidates, especially from ones with high visibility to those with less. These spillovers are known as "coattail effects": a candidate has coattail influence if down-ballot candidates in her same party receive votes they otherwise would not have received, save for her popularity [Miller, 1955]. Coattail effects, though widely believed to exist, can be challenging to quantify: the popularity of a single candidate is difficult to parse from the public's sentiment toward her political party. In this paper, I provide the first causally identified estimates of presidential coattail effects.

I estimate presidential coattail effects using a natural experiment created by late-election information in the 2016 U.S. presidential election. On October 28, 2016, then-FBI Director James Comey sent a letter to Congress regarding Democratic presidential candidate Hillary Clinton. Comey announced the discovery of additional emails which pertained to an investigation into Clinton's use of a private email server while serving as Secretary of State – an investigation he previously testified was complete. The letter reflected negatively on Clinton, but variation in early voting laws across states meant voters had differing abilities to react to the information. Many people in states with early voting had already cast their ballots. In my central identification strategy, I compare election outcomes in counties with no-excuse early voting available before 10/28 to those in counties where no-excuse early voting was either unavailable or began after 10/28, relative to election outcomes in 2012.

First, I show that the Comey letter impacted the 2016 presidential election: exposure to the letter decreased the Democrat presidential vote share by 2 percentage points and increased the Republican presidential vote share by 3.2 percentage points. This increase is larger than the vote margins of eight states in 2016: of these eight, four did not have early voting ahead of October 28$^{\text{th}}$, and three of those four (Florida, Michigan, and Pennsylvania) broke in favor of Republican candidate Donald Trump.[1] Using this as a first stage, I then use an instrumented difference-in-differences model to estimate coattail effects. I find that a one percentage point increase in the Republican presidential vote share led to a 0.86 percentage point increase in the down-ballot Republican vote share.

These findings could be the result of changes along the extensive margin (voter turnout) or intensive margin (vote-switching). To assess the underlying mechanism, I make use of partisan turnout data and the timing of early voting in Nevada. In 2016, Nevada had two weeks of early voting: October 22-28 and October 29-November 4. This provides for a natural comparison of partisan turnout by "week one" and "week two" early voters. I find that the letter increased early turnout by 0.4 percentage points, but that registered Democrats' share of early turnout decreased by 0.9 percentage points, suggesting the Comey letter affected the presidential election and down-ballot races by affecting voter turnout in both parties.

The theory of coattails is not a novel one; scholars have long hypothesized about the possibility of electoral spillovers between different levels of government in a federalist system. Formal theoretical treatments include Zudenkova [2011] and Halberstam and Montagnes [2015]. Zudenkova [2011] documents that coattail effects arise if we assume incumbents prefer their co-partisans to win races in different levels of government.[2] Halberstam and Montagnes [2015] document that senators first elected in presidential years are more ideologically extreme than those first elected in midterms. To explain this observed pattern, they devise a model which assumes the existence of presidential coattail effects and shows

[1] Of the four that did have early voting ahead of the letter's release, three (Minnesota, Nevada, and Maine) broke in favor of Clinton.

[2] Knowing this, retrospective voters (meaning they vote based on an incumbent's performance) jointly evaluate co-partisans, providing for two-sided coattails (that is, spillovers both up and down the ballot).

how such effects can lead to ideological differences in senators depending on when they run.[3]

However, it is difficult to validate this theory: there are inherent complications in separating a particular candidate's popularity from that of her political party.[4] How might we distinguish coattail effects from party politics? Coattail effects are defined as the impact of a candidate's personal popularity on her co-partisans' success elsewhere on the ballot. Thus, proper identification of such an effect requires some exogenous shock to a candidate's popularity – importantly, separate from the favorability of her party at large – and variation in voters' experience of that shock.

Meredith [2013] exploits the excess support granted by (geographically) nearby voters to estimate gubernatorial coattail effects, finding that a one percentage point increase in gubernatorial vote share leads to 0.1-0.2 percentage point increases in vote shares for partisan allies running for secretary of state and attorney general. Many recent papers use close-election regression discontinuity designs to estimate the effect of a co-partisan winning a prior election in a different level of government. These have yielded mixed results on the existence and direction of such "incumbency spillovers," depending on setting, the levels of government, and the direction[5] [Hainmueller and Kern, 2008, Broockman, 2009, Folke and Snyder, 2012, Ade and Freier, 2013, Feierherd, 2020, Ventura, 2021, Savu, 2024].

However, it is possible that each of these designs could be identifying not just an increase in the personal popularity of a candidate, but also an amplified opportunity for party rhetoric.

[3]It should be noted that this pattern of differing senator ideologies may have other explanations. For example, it could be that parties run more extreme Senate candidates to generate additional excitement for the party in presidential years. The documented correlation is between senator ideology and the *presence* of a presidential election, not co-partisan presidential success, and thus could be a result of strategic choices by both parties in presidential years, independent of their presidential candidates' popularity.

[4]Most prior work consists of observational studies which attempt to control for determinants of down-ballot vote shares [Campbell and Sumners, 1990, Mondak, 1993, Flemming, 1995, Mattei and Glasgow, 2005] or structural models [Kramer, 1971, Ferejohn and Calvert, 1984] which have proven to be sensitive to model specification [Fair, 2009]. See Meredith [2013] for a detailed discussion of the difficulties of identifying coattail effects.

[5]Several of these papers estimate "reverse coattails," which refer to spillovers from down-ballot candidates to their fellow party members higher up on the ticket; for example, from a Democratic mayoral candidate to a Democratic U.S. House candidate.

Geographically proximate candidates and incumbents alike should have increased opportunities to positively advertise their own political party, complicating the distinction between coattail effects and party politics. Instead, the Comey letter (or more broadly, late-election information) provides a plausibly exogenous shock to candidate popularity alone.

Late-election information offers the popularity shock needed to estimate presidential coattails, and disparate state-level early voting laws supply the necessary variation. Key to my empirical design is the notion that convenience voters and Election Day voters face fundamentally different information environments [Meredith and Malhotra, 2011]. My identification strategy is similar to ones used in Montalvo [2011], which shows that a terrorist bombing in Spain negatively impacted the performance of the incumbent party in the 2004 general election which took place three days later, and Graham and Svolik [2020], which shows that a Montana U.S. House candidate's assault of a journalist the night before an election hurt his performance with moderate voters on Election Day. However, this paper is the first to use late-election information to 1) analyze its impact in a U.S. presidential race and 2) identify coattail effects.

The paper is also related to work on the impact of information and media in elections [Couttenier et al., 2024, Ash and Galletta, 2023, Wang, 2021, Morton et al., 2015]. Some work has been done on the impact of the Comey letter, specifically: there is some evidence that the letter had impacts in both polling and electoral prediction markets [Halcoussis et al., 2020, Silver, 2017]. McKee et al. [2019] points out that in each of the seven swing states that Trump won, he lost the sum of the votes cast early in-person or by mail; this paper builds on this by using variation in early voting to provide the first quasi-experimental analysis of the Comey letter.

Identifying the presence and magnitude of coattail effects furthers our understanding of voter behavior and has significant strategic implications for politicians. While coattail effects are likely not a determinant of down-ballot candidates' party affiliation,[6] they may influence how closely such candidates choose to align themselves with up-ballot ones. Presidential coattail effects may even influence *when* down-ballot candidates run: they may wish to obtain an electoral boost from a popular co-partisan or avoid being dragged down by an unpopular one. Coattails have important ramifications for presidential candidates, too: strong spillover effects into state-level races should increase a president's legislative efficacy once in office. Finally, this paper's finding that late-election information can impact presidential and down-ballot races suggests that the incentives for the strategic release of late-election information may be even larger than previously thought [Gratton et al., 2018].

2 Setting

In this section, I provide background information on convenience voting in the United States – key to my identification strategy – and on James Comey's letter to Congress.

2.1 Convenience Voting in the United States

Thirty-five states began convenience voting before October 28^{th}, 2016, the day of the Comey letter's publication. A total of thirty-seven states practiced no-excuse convenience voting at the time: see Figure 1 for statewide variation (note that Florida and Oklahoma had no-excuse convenience voting, but their voting periods did not open until *after* October 28^{th}). Of those thirty-seven, three states used universal vote-by-mail, meaning ballots were mailed to all registered voters.[7] The rest of the thirty-seven had early in-person voting options, where polls were open for citizens to cast their ballot in-person up to more than six weeks

[6] Though there is a recent uptick in party-switching amongst state lawmakers in the U.S. [Crampton, 2023].

[7] These three states were Colorado, Oregon, and Washington; nearly 80% of counties in Utah were also using universal vote-by-mail in 2016 [Thompson et al., 2020].

ahead of Election Day.[8]

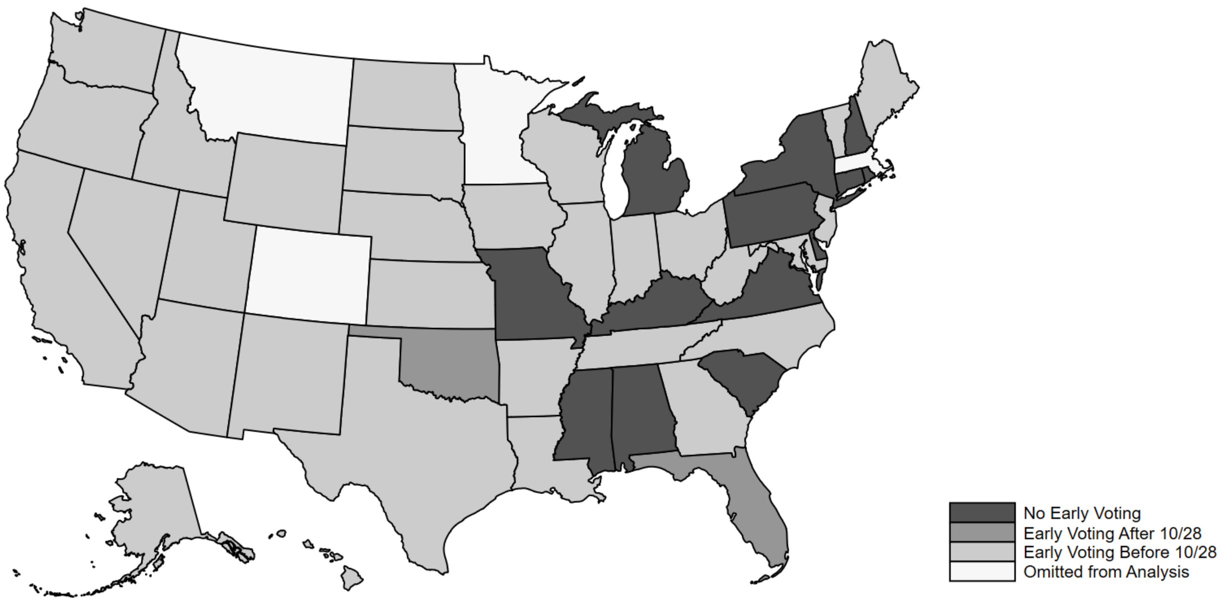

Figure 1: This map displays variation in convenience voting laws for all states in the U.S. in 2016. Four states (Colorado, Massachusetts, Minnesota, and Montana) made changes to their convenience voting laws between 2012 and 2016; I omit them from my analysis.

See Table 1 for summary statistics on counties in states with convenience voting and states without convenience voting. All variables, with the exception of the Democrat/Republican U.S. House margin, are taken in 2012, i.e. the "pre-treatment" election of interest. The Democrat/Republican U.S. House margin is an average calculated over elections from 2008, 2010, and 2014. The two groups appear quite similar along most dimensions; the largest difference is in the unemployment rate (there, the groups differ by 0.424 standard deviations).

A potential concern with comparing the two groups is that there may be underlying qualities which differ between the two and are somehow correlated with treatment. That

[8]The earliest early voting start date in 2016 was September 21st in Wisconsin.

is, if one group was more or less responsive to the Comey letter due to differential baseline characteristics. However, as Table 1 shows, the two groups possess comparable baseline demographic, economic, and political attributes. To further address this concern, I estimate a second difference-in-differences model where I compare early voters in Nevada who voted before 10/28/16 to early voters in Nevada who voted in the week after the letter's release.

Table 1: This table displays summary statistics in 2012 for counties in the United States, broken into states with convenience voting and counties without convenience voting.

	Convenience Voting	No Convenience Voting	Difference (Std. Devs.)
18+ Population	78,171.43	77,744.84	0.002
Unemployment Rate	7.554	8.74	0.424
Population Density	136.341	410.587	0.184
Median Age	40.258	40.279	0.004
# Men Per 100 Women	100.823	98.334	0.221
% Non-white	15.434	18.461	0.183
Median Income	45,540.93	44,108.41	0.122
% with Bach. or Higher	19.024	19.148	0.015
Dem/Rep. House Margin*	-0.234	-0.158	0.241
Dem. Vote Share in Pres. Race	0.368	0.417	0.331
Turnout	0.549	0.568	0.2
Count	2090	801	

Notes: The means of the convenience voting and no convenience voting groups are reported, along with the difference in terms of the standard deviation of the entire sample, for reference. For example, the difference in 18+ population between the groups is 0.002 of one standard deviation (measured from the entire sample). The variable denoted with an asterisk is averaged over three U.S. House election cycles: 2008, 2010, and 2014. All other variables are taken from 2012. Colorado, Massachusetts, Minnesota, and Montana are not included, as they changed convenience voting laws between 2012 and 2016; Alaska is not included, as it does not report election results by county. Population density is calculated by dividing a county's 18+ population by its land area in square miles (as reported by the 2010 Census). The % with Bach. or Higher variable refers to the percent of those aged 25+ with a bachelor's or more advanced degree.

2.2 The Comey Letter as Late-Election Information

Late-election information, broadly speaking, is revealed to voters in the weeks or days preceding Election Day and has the potential to change voters' minds. This information can take the form of something already structured within the political system, such as presidential debates or campaign stops, or a spontaneous revelation, known as the "October surprise" in American politics. Interested parties certainly have incentives to release information with the intent to impact election outcomes as Election Day looms.[9] However, this late-election information, whether spontaneous or strategically released, can nevertheless "surprise" the public and might induce changes in voter behavior.

There are many recent examples of late-election information in the United States: in 2020, news of an extramarital affair and a subsequent investigation by the U.S. Army Reserve of Cal Cunningham, a Democrat running for a U.S. Senate seat in North Carolina, occurred less than one month away from Election Day [Robertson, 2020]. In the 2003 California gubernatorial recall election, Republican Arnold Schwarzenegger replaced incumbent Democratic Governor Gray Davis, winning by a margin of 968,491 votes [Ballotpedia, b]. The *Los Angeles Times* reported on the Friday before the election that six women had accused Schwarzenegger of sexual misconduct [Cohn et al., 2003] after "more than three million of ten million voters had already cast their ballots" [Gronke et al., 2008]. In 2000, news of George W. Bush's 1976 DUI arrest was reported on by media outlets four days before Election Day [Balz, 2000].

In this paper, I focus on a particular example of late-election information: then-FBI Director James Comey's October 28, 2016 letter to Congress. Comey announced in the letter that the FBI had discovered more emails which seemed pertinent to the investigation of Hillary Clinton's use of a private email server during her time as Secretary of State (an investigation Comey had previously testified was complete). The letter was sent to Congress just eleven days before the presidential election between Republican candidate Donald Trump and Democratic candidate Hillary Clinton [Perez and Brown, 2016].

The last month or so of the campaign leading up to Election Day on November 8$^{\text{th}}$ was

[9] Gratton et al. [2018] present this as a tradeoff between credibility and scrutiny: good signals of candidate quality are released earlier, as this conveys credibility of the information, but poor signals will be released later, since they would not hold up to the public's scrutiny if examined for too long.

quite dynamic in terms of late-election information. On October 7th, *The Washington Post* obtained a video in which "Donald Trump bragged in vulgar terms about kissing, groping and trying to have sex with women during a 2005 conversation caught on a hot microphone" [Fahrenthold, 2016]. This prompted several prominent Republicans to condemn Trump; some even suggested he withdraw his candidacy [Wellford, 2016]. On that same day, the Office of the Director of National Intelligence and the Department of Homeland Security released a statement accusing Russia of interfering with the election, and media organization WikiLeaks began releasing emails belonging to Clinton campaign chairman John Podesta, many of them "embarrassing for Clinton" [Cohen, 2017]. These releases continued on a near-daily basis throughout the rest of the campaign, but none seemed to generate a nation-wide shock in the way that 1) the aforementioned tape, 2) the beginning of the WikiLeaks releases, or 3) the Comey letter did.

I choose to focus on the Comey letter as the late-election information of interest in the election. The events of October 7th negatively impacted both candidates, prompting muddled behavioral implications for voters. In comparison, the Comey letter (and thus October 28th) clearly represented negative information for Clinton alone. Additionally, not as many people had yet had the opportunity to vote by October 7th, making for a smaller control group: just ten states began convenience voting before October 7th.[10]

As mentioned above, thirty-five states began convenience voting before October 28th. Although concerns over Clinton's use of the email server had already been raised and an investigation completed, this letter and its implication of a re-opened investigation may have legitimized some voter's concerns about the candidate. It certainly represented an information shock of interest to many voters; see Figure 2 in the appendix for Google Trends data, which shows a large spike in searches for "Comey" on 10/28/16. The election was still eleven days away, yet many had already cast their ballot before they learned of this

[10]In a robustness check, I drop these ten states from my control group to ensure that all counties, both treated and control, had knowledge of the events of October 7th prior to voting: see Section A.3.

investigation. Their hands were now tied; would they have voted differently had they waited until Election Day, or perhaps abstained altogether?

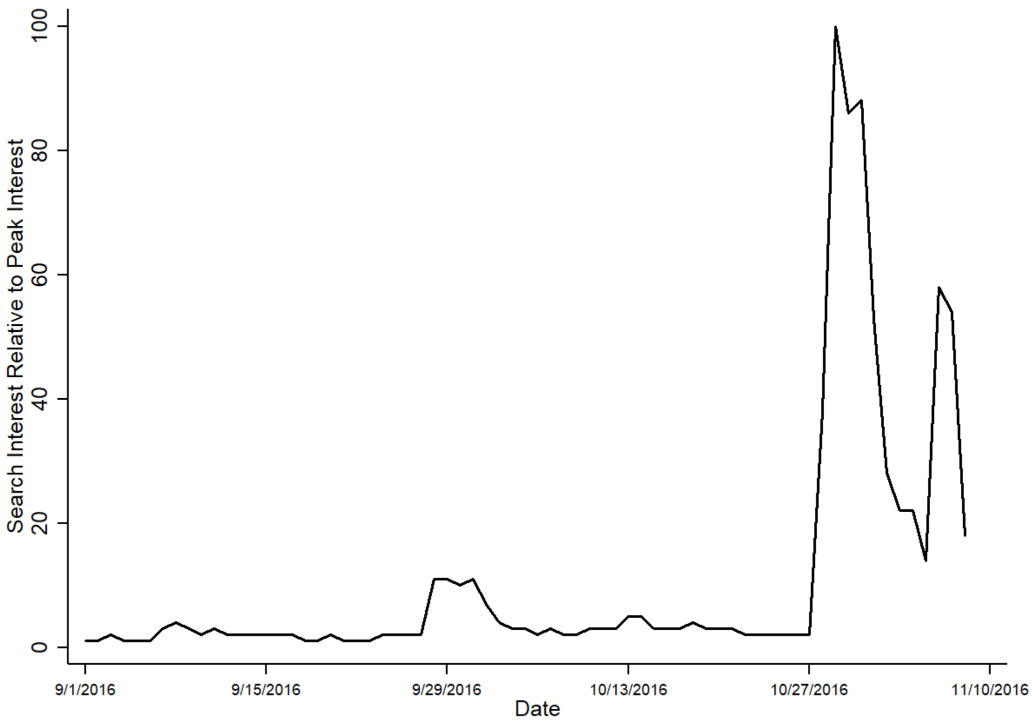

Figure 2: This figure shows Google Trends data for searches for "Comey" in the United States between 9/1/2016-11/8/2016. A value of 100 is peak popularity for the term.

3 Data & Methodology

In this section, I present the data and methodology used in my analysis. I make use of two complementary empirical strategies in this paper. The first, which relies on cross-state variation in the availability of early voting, allows me to examine how the Comey letter impacted both the extensive margin (turnout) and the intensive margin (presidential vote shares). In addition, I study the performance of state-level candidates to determine if the letter had spillover effects based on party affiliation. The second identification strategy leverages a feature of a singular U.S. state (Nevada)'s convenience voting system and allows me to shed further light on the letter's extensive margin effects.

3.1 Data

I obtain data on convenience voting laws and timing from the Elections & Voting Information Center for the 2012 election [Hicks, 2012] and Ballotpedia for the 2016 election [Ballotpedia, a] (Accessed: Sep. 2021) and use it to define treatment in the central difference-in-differences design: states with early voting windows which open before 10/28/16 are control states and those without early voting windows open before 10/28/16 are treatment states (this includes states which did not have early voting at all). Treatment is therefore defined by the *lack* of ability to vote early without an excuse before the letter's release, as voters in these states would have been less constrained in their means to respond to the letter in the ballot box.

I collect county-level presidential, congressional, and gubernatorial election returns (vote totals for each candidate) for 2012 and 2016 from Dave Leip's Atlas of Elections [Leip] (Accessed: Apr. 2022 and Nov. 2023) and counts of 18+ population for each county in 2012 and 2016 from the Survey of Epidemiology and End Results (SEER). I use these counts as voting-age population for calculating turnout rates. For the Nevada design, I collect county-level early voting returns for 2012 and 2016 (turnout rates and turnout rates by party registration, broken out by week) from the Nevada Secretary of State's website. Finally, I obtain county-level control variables from the U.S. Census Bureau's American Community Survey 5-year estimates, the U.S. Bureau of Labor Statistics, and the U.S. Census Bureau's Census of Population and Housing.

3.2 State-Level Variation in Convenience Voting

In the main empirical specification, I categorize two types of counties as treated: counties in states without early voting and counties in states where early voting did not open until after October 28$^{\text{th}}$, 2016. The treatment turns on with the Comey letter (only in 2016 and not in 2012). Counties in states with early voting which opened before October 28$^{\text{th}}$, 2016 are controls, as voters in these states would have been least able to respond to the information. Here, the variation of interest comes from the availability of early voting, something set independently by each state. In order to avoid any confounding trends at the state level, I exclude any state which changed its convenience voting laws between 2012 and 2016 from my analysis (these states are Colorado, Massachusetts, Minnesota, and Michigan).

The identifying assumption here is that, absent the Comey letter, treatment and control counties would have trended similarly in the outcomes of interest (turnout rate and partisan vote shares).[11] This design also requires that any late-election information in 2012 did not affect political behavior.[12] I estimate

$$y_{ct} = \alpha + \delta T_c + \gamma P_t + \beta(T_c \times P_t) + \epsilon_{ct}, \tag{1}$$

where T_c takes a value of 1 for counties in states without early voting and states where early voting did not open until after October 28$^{\text{th}}$, 2016 and 0 otherwise, and P_t takes a value of

[11] It may be problematic to compare voters in states with early voting to voters in states without early voting if these two types of states are on different political *trends* – this would mean a violation of the parallel trends assumption. To this end, counties in Florida and Oklahoma may be a superior "treated" group, as both Florida and Oklahoma had no-excuse early voting periods that did not open until *after* 10/28/16, meaning both treated and control voters live in states with early voting (the difference is just in when early voting is offered). See Section A.1 in the appendix for a robustness check; results are robust to a refinement of the design which compares counties in Florida and Oklahoma to counties with early voting open before 10/28/16.

[12] Some may view Hurricane Sandy, which made landfall in the U.S. on October 29$^{\text{th}}$, 2016, as late-election information benefiting President Obama (since his handling of the storm was positively regarded by both parties). See Section A.2 in the appendix for a robustness check; results are robust to leaving out counties in Connecticut, New Jersey, and New York, which had the largest populations in storm surge zones during Hurricane Sandy [Center for International Earth Science Information Network, 2012].

1 in 2016 and 0 otherwise.[13]

Outcomes of interest y_{ct} for county c in year t include turnout rate, vote shares for Democratic and Republican presidential candidates, and average vote shares for Democratic and Republican state-level candidates. I calculate turnout rate as a proportion of the voting-age population (i.e., the 18+ population); presidential vote shares are proportions of total ballots cast for the president. To determine the average partisan vote shares for state-level candidates, I average together the Democrat (or Republican) vote shares in U.S. House, Senate, and gubernatorial elections.[14] I weight by voting-age population, since 1) I expect more accurate measurement of rates in counties with larger populations and 2) treatment effects could be correlated with population (insofar as how population might be related to political ideology, etc.).

3.3 Nevada

A potential issue with the above design is comparing voters across states – several other statewide races coincide with presidential elections, which could lead to confounding influences on turnout and partisan vote shares. Thus, I focus solely on the state of Nevada, which reports its early vote turnout by party and by week. Nevada has a two-week early voting period which was divided into October 22–28 and October 29–November 4 in 2016. Since the Comey letter was released on October 28$^{\text{th}}$, those who voted in Week 2 were able to incorporate information in their vote which those who voted in Week 1 were not.

The unique timing of early voting and system of reporting early voting turnout in Nevada buys me a few things: I am able to compare within state, ruling out confounding cross-state differences, and I am able to compare early voters to other early voters. Although it comes

[13]This definition of treatment means that some control counties had early voting available prior to October 7$^{\text{th}}$, another important day in the election cycle. In Section A.3 in the appendix, I show results are robust to the exclusion of all counties which had early voting available prior to 10/7.

[14]I exclude unopposed elections, as vote shares would not reflect any spillover effects in these cases.

at the cost of a much smaller sample size, these are valuable improvements over the first design.

Using week-county-year observations, I estimate a difference-in-differences specification and compare Week 2 and Week 1 voters in 2016 and in 2012. The identifying assumption here is that, absent the Comey letter, Week 1 and Week 2 voters would have trended similarly in terms of the outcomes of interest. Again, this design also requires that any late-election information in 2012 did not affect political behavior in Nevada. I estimate

$$y_{wct} = \alpha + \delta T_t + \gamma P_w + \beta(T_t \times P_w) + \epsilon_{wct}, \tag{2}$$

where w indexes week, c indexes county, and t indexes year/election (it may be helpful to think of the 2016 election as treated and 2012 as control, where Week 2 is the post-period and Week 1 is the pre-period). Outcomes of interest include early turnout as a proportion of 18+ population and turnout by party registrants (i.e., voters registered as Democrats, etc.) as a share of total early votes. Unfortunately, I am unable to see the outcomes (i.e., ballots cast for the Democrat, etc.) broken up into weeks. However, turnout by party registration is helpful in determining how different "types" of voters are reacting on the extensive margin, something I am unable to determine by just looking at election outcomes. I weight by voting-age population and report bootstrapped standard errors (clustered at the county level).

4 Results

Table 2 shows results from the main empirical design. First, note that there is no change in turnout. Since there is not a significant change on the *extensive* margin, I look for changes on the *intensive* margin by examining the effect of the Comey letter on presidential vote shares (total ballots cast for a candidate, divided by total presidential ballots cast). The results in Table 2 show an increase in Republican presidential vote share of 3.2 percentage points, in combination with a decrease in Democrat presidential vote share of 2 percentage points.

Table 2 also displays an increase in average Republican down-ballot vote share of 3.2 percentage points and a complementary decrease in average Democrat down-ballot vote share of 3.3 percentage points. As mentioned above, I drop uncontested races from my analysis, explaining the difference in the number of observations across columns. In Section 5, I discuss interpretation of these magnitudes.

Results are robust to the inclusion of time-varying county economic and demographic controls including median income, unemployment rate, population density, median age, number of men per 100 women, percent non-white, and percent of those 25 and up with a Bachelor's degree or higher (right-hand columns under each outcome). Additionally, results are robust to a refinement considering only counties in Florida and Oklahoma (which had early voting, but not before 10/28) as treated units (Section A.1), dropping counties in states most affected by Hurricane Sandy in 2012 (Section A.2), and excluding counties in states with early voting open before 10/7 (Section A.3).

It is possible that the results shown in Table 2 reflect larger dynamics within the election, rather than voters' response to the singular shock of the Comey letter. Perhaps Clinton and her fellow Democrats would have faced a downturn late in the election regardless of the letter's release. If this were the case, we might expect to see these same results when treatment is defined around the final presidential debate, October 19$^{\text{th}}$. However, as shown in Section A.4, the results do *not* hold when I redefine treatment in this way. This suggests that the results presented in this section are indeed attributable to the Comey letter.

Table 2: Voters most able to respond to the Comey letter were more likely to vote for Republican candidates in presidential and down-ballot races.

	Turnout Rate		Dem. Presidential Vote Share		Rep. Presidential Vote Share		Dem. Down-Ballot Vote Share		Rep. Down-Ballot Vote Share	
Treat×Post	0.009	0.009	-0.02*	-0.024	0.032***	0.036**	-0.033**	-0.035*	0.032*	0.033*
	(0.006)	(0.007)	(0.011)	(0.015)	(0.01)	(0.015)	(0.015)	(0.018)	(0.017)	(0.018)
N	5782		5782		5782		5400		5400	
Mean	0.56		0.347		0.618		0.358		0.608	
Controls		✓		✓		✓		✓		

Standard errors are clustered at the state level and listed in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table shows results from a county-level difference-in-differences specification, weighted by 18+ population: $y_{ct} = \alpha + \delta T_c + \gamma P_t + \beta(T_c \times P_t) + \epsilon_{ct}$, where T_c takes a value of 1 in counties *without* early voting open before 10/28/16 and 0 otherwise, and P_t takes a value of 1 in 2016 and 0 in 2012. I include time-varying controls in the right-hand column under each outcome: unemployment rate, median income, population density, median age, number of men per 100 women, percent non-white, and percent of those 25 and up with a Bachelor's degree or higher. I calculate turnout rate as a proportion of 18+ population; presidential vote shares are proportions of total presidential votes cast. I calculate U.S. House, Senate, and gubernatorial vote shares as proportions of total ballots cast in the respective races before averaging the three together to calculate the down-ballot vote share. I drop observations with uncontested races, leading to the different number of observations for the down-ballot outcomes.

Table 3 shows results from the complementary Nevada design. The results show an increase in early turnout rate of 0.4 percentage points: Week 2 voters were more likely to turn out upon learning of the Comey letter. Additionally, there is a decrease of 0.9 percentage points in the share of early ballots cast by registered Democrats. This may serve as informative of *who* is reacting to the Comey letter: it is plausible that the letter mobilized right-leaning and independent voters in Nevada and discouraged left-leaning ones.[15]

[15] One might be concerned that this result does not represent the "true" impact of the Comey letter on registered Democrats in Nevada; for instance, it could be that registered Democrats are discouraged in Week 2, but come around to vote by Election Day. However, this is not the case. Section A.5 shows results from a similar difference-in-differences specification which compares Election Day voters to Week 1 voters; the decrease in registered Democrat vote share actually becomes *stronger*.

Table 3: Early voters in Nevada were more likely to cast a ballot after the Comey letter was sent; however, registered Democrats were less likely to vote.

	Early Turnout Rate	Registered Dem. Early Turnout Share	Registered Rep. Early Turnout Share	Registered Ind. Early Turnout Share
Treat×Post	0.004**	-0.009*	0.001	0.009
	(0.002)	(0.006)	(0.008)	(0.008)
N	68	68	68	68
Mean	0.135	0.277	0.532	0.191

Bootstrapped standard errors are clustered at the county level and listed in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table shows results from a county-week level difference-in-differences specification, weighted by 18+ population: $y_{wct} = \alpha + \delta T_t + \gamma P_w + \beta(T_t \times P_w) + \epsilon_{wct}$, where T_t takes a value of 1 in 2016 and 0 in 2012, and P_w takes a value of 1 for "Week 2" voters and 0 for "Week 1" voters. The early turnout rate is the number of early ballots cast divided by 18+ population. Registered Democrat early turnout share is the number of registered Democrats who cast a ballot early divided by the number of early ballots cast; registered Republican and Independent early turnout shares are defined similarly for registered Republicans and voters not registered as Republicans or Democrats, respectively.

5 Interpreting Magnitudes: An Instrumented Difference-in-Differences Approach

In Table 2, I find that exposure to the Comey letter led to significant decreases in down-ballot Democrat vote shares (and increases in their Republican counterparts). These findings are suggestive of presidential coattail effects: the popularity of a presidential candidate affected the performance of her party's candidates in concurrent races. Scholars have long sought to estimate presidential coattail effects. However, this is often a difficult task: rarely can the popularity of a presidential candidate be separated from the general attitudes toward her political party.

In order to estimate presidential coattail effects in my setting, I turn to an instrumented difference-in-differences (DDIV) model [Hudson et al., 2017, Duflo, 2001]. This model scales the difference-in-differences effect of the Comey letter on down-ballot partisan vote shares by its effect on partisan presidential vote shares. In other words, it allows me to obtain the effect of a one percentage point increase in partisan presidential vote share, rather than the effect of exposure to the Comey letter itself. The DDIV coefficient β is estimated via the following instrumental variables system:

$$y_{ct} = \alpha + \delta T_c + \gamma P_t + \beta \widehat{Y_{ct}} + \epsilon_{ct} \tag{3}$$

$$Y_{ct} = \theta + \rho T_c + \pi P_t + \eta(T_c \times P_t) + \mu_{ct}, \tag{4}$$

where T_c and P_t are defined as in equation (1). Here, exposure to the Comey letter instruments for Y_{ct}, the Republican presidential vote share in county c in year t.[16,17] In the second-stage equation, outcomes of interest y_{ct} include average vote shares for Republican and Democrat state-level candidates. I weight by 18+ population in both estimations.

Note that the first-stage parameter, η from equation (4), is equivalent to the coefficient estimated by equation (1) when regressing on presidential vote shares.[18] The DDIV coefficient β can be written as the ratio of the reduced-form and first-stage parameters. The reduced-form parameter can be found by estimating:

$$y_{ct} = \nu + \tau T_c + \theta P_t + \phi(T_c \times P_t) + \omega_{ct}, \tag{5}$$

i.e. equation (1) when regressing on state-level vote shares.

As discussed in Hudson et al. [2017], the DDIV design requires several assumptions. Just as in the difference-in-differences model estimated in equation (1), the assumption of parallel

[16] I instrument for Republican presidential vote share rather than Democrat for two reasons: a stronger first-stage and ease of interpretation (exposure to the Comey letter increased the Republican – not Democrat – presidential vote share).

[17] In the formal treatment of DDIV provided in Hudson et al. [2017], Y_{ct} is assumed to be "a discretely- and positively-valued treatment." However, the authors note that it is straightforward to extend to a setting with continuous treatment – such as a presidential vote share.

[18] For the IV exercise, I drop all counties which only had one major party run in a state-level race. This results in a sample of 5400, rather than 5782, observations, and means that η from equation (4) and β from equation (1) differ slightly.

trends must hold (see Section 3.2 for discussion). Similar to traditional instrumental variables models, monotonicity and the exclusion restriction must hold. In my context, this requires assuming 1) exposure to the Comey letter did not cause an increase in Democrat presidential vote share for some subpopulations and 2) knowledge of the Comey letter did not affect partisan down-ballot vote shares except through its effect on the popularity of each party's presidential candidates.

Table 4 presents the results from equations (3) and (4) where the outcomes of interest are down-ballot partisan vote shares (averaged over U.S. House, Senate, and gubernatorial elections).[19] Results indicate that a one percentage point increase in the Republican presidential vote share leads to a 0.86 percentage point increase in the down-ballot Republican vote share.

[19] Note that much of the coattails literature is dedicated to estimating the extent of spillovers between presidential and U.S. House races. In Table A8, I re-estimate equations (3) and (4) using partisan U.S. House vote shares as the sole outcomes of interest.

Table 4: An increase in the Republican presidential vote share leads to concurrent increases (decreases, resp.) in Republican (Democrat, resp.) down-ballot vote shares.

	(1)	(2)
Rep. Down-Ballot Vote Share	0.863***	0.861***
	(0.332)	(0.32)
Dem. Down-Ballot Vote Share	-0.892***	-0.899***
	(0.3)	(0.297)
First-Stage (Rep. Pres. Vote Share)	0.037***	0.038**
	(0.014)	(0.016)
N	5400	5400
Controls		✓

Standard errors are clustered at the state level and listed in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table shows results from an instrumental variables system. In the first-stage, I estimate $Y_{ct} = \theta + \rho T_c + \pi P_t + \eta(T_c \times P_t) + \mu_{ct}$, where T_c takes a value of 1 in counties *without* early voting open before 10/28/16 and 0 otherwise, and P_t takes a value of 1 in 2016 and 0 in 2012. Here, Y_{ct} is the Republican presidential vote share (as a proportion of total presidential votes cast) in county c in year t. Then, I estimate $y_{ct} = \alpha + \delta T_c + \gamma P_t + \beta \widehat{Y_{ct}} + \epsilon_{ct}$, where y_{ct} is the average of Republican or Democrat U.S. House, Senate, and gubernatorial vote shares as proportions of total ballots cast in their respective races. All specifications are weighted by 18+ population. In column (2), I include time-varying controls: unemployment rate, median income, population density, median age, number of men per 100 women, percent non-white, and percent of those 25 and up with a Bachelor's degree or higher. I drop observations where only one major party competed in all relevant races.

6 Conclusion

This paper is the first to make use of an October surprise to estimate presidential coattail effects: using an instrumented difference-in-differences design, I find that a one percentage point increase in Republican presidential vote share leads to a 0.86 percentage point increase in Republican down-ballot vote share. Results in this paper indicate that this increase in Republican presidential vote share is the result of James Comey's letter to Congress, released just eleven days ahead of the 2016 presidential election. Using cross-state variation in the availability of early voting (generating variation in ability to respond to the letter), I find that exposure to the letter led to an increase in the Republican presidential vote share of

3.2 percentage points – larger than the eight closest state margins that election. Evidence suggests that at least part of these effects are owed to changes in partisan turnout: I find that exposure to the letter led to a decrease in registered Democrats' early turnout share of 0.9 percentage points in Nevada.

The findings of this paper indicate that the popularity or electoral strength of a party's presidential candidate has meaningful downstream effects. This raises numerous strategic considerations for down-ballot political candidates: for example, how closely to align with prominent co-partisan candidates or even when to run for office. These spillovers have important implications for a party's agenda, too: presidential coattails imply that the more popular a newly-elected president is, the more co-partisans she can expect to be seated in her new Congress. The finding that late-election information regarding a presidential candidate has down-ballot effects suggests that there are strong incentives for the strategic release of information during presidential campaigns beyond first-order effects on the presidential race.

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A Appendix

This appendix contains robustness checks for the identification designs presented in the paper and additional results from the DDIV model. Sections A.1, A.2, A.3, and A.4 present robustness checks for the main research design, introduced in Section 3.2. Section A.5 presents a robustness check for the complementary research design, introduced in Section 3.3. Section A.6 gives results from the DDIV specification using only partisan U.S. House vote shares as the outcome.

A.1 Florida and Oklahoma

In equation (1), I leverage cross-state variation in preexisting convenience voting laws at the time of the Comey letter. Counties in states with early voting open prior to 10/28 are controls, and counties in states 1) with early voting open only *after* 10/28 and 2) with no early voting at all are treated. The identifying assumption is the canonical parallel trends assumption: absent the Comey letter, treatment and control counties should have trended similarly in the outcomes of interest. However, one might be concerned that states with and without early voting might be on different political trends, violating this assumption. I exclude any state which changes its convenience voting laws between 2012 and 2016 to help alleviate this concern. In this section, I make a refinement to the design to further address it: I limit treatment counties to those in group (1): with early voting open only *after* 10/28 (all counties in Florida and Oklahoma). Then, I re-estimate (1) with the refined treatment group and same control group (all counties in states with early voting open prior to 10/28).

Results are in Table A1. Here, there *is* a change on the extensive margin: voters in Florida and Oklahoma, who were more able to respond to the Comey letter, saw an increase in turnout rate of 1.5 percentage points. This is a bit puzzling: if the control group already has a number of voters "locked in" and the rest (who did not vote early) are free to respond, then an increase in turnout does not make much sense. This finding persists when limiting the control group to counties in neighboring states (Arkansas, Georgia, Kansas, New Mexico, and Texas; see Table A2). However, looking at disaggregated turnout of a subset of these neighbors in Table A3, I show that the Comey letter did not lead to a significant change in early turnout rate or Election Day turnout rate. This result appears noisy and is perhaps due to measurement error in the denominator.[20]

Returning to Table A1, the results show an increase in Republican presidential and state-level vote shares. These results hold (and strengthen) when comparing Florida and Oklahoma only to their neighboring states (see Table A2). Broadly, the results remain consistent with those found in my main specification and reported in Table 2.

Table A1: Voters in Florida and Oklahoma were more likely to vote for Republican presidential and down-ballot candidates than voters in states with convenience voting open before 10/28/16.

	Turnout Rate		Dem. Presidential Vote Share		Rep. Presidential Vote Share		Dem. Down-Ballot Vote Share		Rep. Down-Ballot Vote Share	
Treat×Post	0.015***	0.012*	-0.006	-0.007	0.017*	0.017	-0.045***	-0.035*	0.052***	0.033*
	(0.004)	(0.006)	(0.01)	(0.017)	(0.01)	(0.015)	(0.013)	(0.018)	(0.015)	(0.018)
N	4180		4180		4180		3814		3814	
Mean	0.555		0.334		0.629		0.341		0.621	
Controls		✓		✓		✓		✓		✓

Standard errors are clustered at the state level and listed in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table re-estimates the results in Table 2 using only counties in Florida and Oklahoma as treatment units.

[20] If 18+ population were growing more quickly in Florida than documented by the SEER measures, this could lead to a false finding of increased turnout rate.

Table A2: Voters in Florida and Oklahoma were more likely to vote for Republican presidential and down-ballot candidates than voters in neighboring states with convenience voting open before 10/28/16.

	Turnout Rate		Dem. Presidential Vote Share		Rep. Presidential Vote Share		Dem. Down-Ballot Vote Share		Rep. Down-Ballot Vote Share	
Treat×Post	0.012**	0.01	-0.03*	-0.01	0.031***	0.011	-0.05	-0.008	0.076***	0.045
	(0.005)	(0.009)	(0.017)	(0.039)	(0.012)	(0.03)	(0.04)	(0.05)	(0.041)	(0.034)
N	1288		1288		1288		1288		1288	
Mean	0.507		0.298		0.674		0.292		0.665	
Controls		✓		✓		✓		✓		✓

Standard errors are clustered at the state level and listed in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table re-estimates the results in Table 2 using only counties in Florida and Oklahoma as treatment units and counties in their neighboring states as controls.

Table A3: The Comey letter led to no significant change in the Election Day or early turnout rates in Florida and Oklahoma counties, as compared to counties in their neighboring states: Arkansas, Georgia, and New Mexico.

	Turnout Rate		Early Turnout Rate		Election Day Turnout Rate	
Treat×Post	0.017***	0.018**	0.015	0.017	-0.008	-0.01
	(0.006)	(0.008)	(0.026)	(0.034)	(0.027)	(0.039)
N	816		816		816	
Mean	0.512		0.2		0.266	
Controls		✓		✓		✓

Standard errors are clustered at the state level and listed in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table disaggregates results from Table A2, examining the impact of the letter on turnout by method. Note that the control sample differs from that in Table A2: Kansas and Texas do not provide turnout by voting method and are thus dropped from the estimation.

A.2 Hurricane Sandy

The first empirical design, summarized in equation (1), takes 2012 as a base year for the difference-in-differences specification. Thus, I assume that any late-election information in 2012 did not affect election outcomes. One potential violation of this assumption comes from Hurricane Sandy, a tropical cyclone which made landfall in the United States around October 29, 2012. Many at the time posited the storm would benefit incumbent Democrat President Obama, who was widely praised for his response. However, as found in Hart [2014], the effect of Hurricane Sandy on the election was "variable and small in magnitude."

To account for any possible impact of Hurricane Sandy, I re-estimate equation (1) after omitting counties in Connecticut, New Jersey, and New York. These three states had the largest populations in storm surge zones during Hurricane Sandy (Center for International Earth Science Information Network [2012]).[21] As such, it is probable that any effect Hurricane Sandy had on political outcomes was largest there. Results are robust to this omission; see Table A4.

Table A4: Results are robust to the omission of Connecticut, New Jersey, and New York.

	Turnout Rate		Dem. Presidential Vote Share		Rep. Presidential Vote Share		Dem. Down-Ballot Vote Share		Rep. Down-Ballot Vote Share	
Treat×Post	0.005	0.005	-0.02*	-0.025	0.032***	0.036**	-0.043***	-0.046***	0.043**	0.045**
	(0.007)	(0.007)	(0.012)	(0.017)	(0.011)	(0.017)	(0.014)	(0.017)	(0.016)	(0.017)
N	5600		5600		5600		5218		5218	
Mean	0.561		0.342		0.623		0.351		0.614	
Controls		✓		✓		✓		✓		✓

Standard errors are clustered at the state level and listed in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table re-estimates the results in Table 2 after omitting counties in Connecticut, New Jersey, and New York from the analysis.

A.3 October 7th, 2016

As described in Section 2.2, October 7th was an important day in the 2016 presidential election. Three major events unfolded: *The Washington Post* reported on the 2005 "Access Hollywood" tape, the U.S. government accused Russia of meddling in the election, and WikiLeaks began releasing emails of John Podesta. By 10/7, ten states had already begun early voting: Idaho, Iowa, Maine, Nebraska, New Jersey, Ohio, South Dakota, Vermont,

[21] Along with Massachusetts, which is already excluded from my analysis.

Virginia, and Wyoming. In order to ensure the results from (1) are not driven by comparing pre-10/7 voters to post-10/7 voters, I omit these ten states from my analysis below. Results are robust to this omission; see Table A5.

Table A5: Results are robust to the omission of counties which had access to early voting prior to October 7th, 2016.

	Turnout Rate		Dem. Presidential Vote Share		Rep. Presidential Vote Share		Dem. Down-Ballot Vote Share		Rep. Down-Ballot Vote Share	
Treat×Post	0.009	0.009	-0.031***	-0.035**	0.043***	0.047***	-0.043***	-0.045**	0.046***	0.048***
	(0.006)	(0.007)	(0.01)	(0.016)	(0.009)	(0.016)	(0.014)	(0.018)	(0.016)	(0.017)
N	4588		4588		4588		4281		4281	
Mean	0.546		0.344		0.622		0.356		0.607	
Controls		✓		✓		✓		✓		✓

Standard errors are clustered at the state level and listed in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table re-estimates the results in Table 2 after excluding states with early voting open before 10/7/16 from the analysis.

A.4 October 19th, 2016

It is possible that the central empirical design simply captures changing dynamics throughout the campaign leading up to the election. It could be the case that Clinton and the Democratic party would have performed worse with "later" versus "earlier" voters, even in the absence of the letter. To address this concern, I redefine treatment around October 19th, 2016, the date of the last presidential debate. If I find that voters reacted to the October 19th "treatment", then it is likely that the results in Table 2 are reflective of voter preferences shifting over time in response to a number of factors rather than the singular Comey letter "shock." As can be seen in Table A6, I do *not* find that voters responded to this false treatment, increasing confidence in the attribution of my results to the Comey letter.

Table A6: The results shown in Table 2 do not hold when treatment is redefined around October 19th, 2016, suggesting the results were due to the Comey letter and not simply the larger dynamics of the election.

	Turnout Rate		Dem. Presidential Vote Share		Rep. Presidential Vote Share		Dem. Down-Ballot Vote Share		Rep. Down-Ballot Vote Share	
Treat×Post	0.001	0.005	-0.006	-0.015	0.011	0.02	-0.005	-0.016	0.002	0.011
	(0.006)	(0.007)	(0.016)	(0.021)	(0.016)	(0.021)	(0.02)	(0.023)	(0.021)	(0.022)
N	5782		5782		5782		5400		5400	
Mean	0.56		0.347		0.618		0.358		0.608	
Controls		✓		✓		✓		✓		✓

Standard errors are clustered at the state level and listed in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table re-estimates the results in Table 2 after re-defining treatment: counties *without* early voting open before 10/19/16 – the date of the last presidential debate – are considered treated; those with early voting open ahead of 10/19/16 are controls.

A.5 Nevada: Comparing Week 1 early voters to Election Day voters

In the second empirical design, summarized in equation (2), I compare "Week 2" early voters to "Week 1" early voters in Nevada, where week 2 occurred post-10/28 and week 1 occurred pre-10/28. The results, displayed in Table 3, show a significant decrease in registered Democrat early turnout share (the number of early ballots cast by registered Democrats as a proportion of total early ballots cast). This seems to imply that the Comey letter discouraged registered Democrats in Nevada. However, one might wonder if that discouragement was only temporary: did Democrats come around by Election Day and turn out?

To answer this question, I compare "week 1" early voters to Election Day voters in Nevada, using the same difference-in-differences framework as in equation (2). The idea behind the design remains the same: those voting in week 1 did so pre-Comey letter; those voting on Election Day did so post-Comey letter. The result from Table 3 persists and even strengthens:

the Comey letter led to a decrease in registered Democrat turnout share of 1.43 percentage points.

Table A7: Registered Democrats in Nevada were less likely to vote after learning of the Comey letter.

	(1)	(2)	(3)
	Registered Dem. Turnout Share	Registered Rep. Turnout Share	Registered Ind. Turnout Share
Treat×Post	-0.0143***	0.0043	0.01*
	(0.0043)	(0.0081)	(0.0057)
N	68	68	68
Mean	0.272	0.52	0.208

Bootstrapped standard errors are clustered at the county level and listed in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table re-estimates the results in Table 3 using Election Day voters as the treatment group, rather than "week 2" early voters.

A.6 Instrumented Difference-in-Differences Results on U.S. House Vote Shares

The results in Table 4 demonstrate the impact of a one percentage point increase in Republican presidential vote share on partisan down-ballot vote shares. I calculate these down-ballot vote shares by calculating the average of Republican or Democrat U.S. House, Senate, and gubernatorial vote shares. However, much of the presidential coattails literature uses only U.S. House vote shares as outcomes – likely because, unlike senators and governors, U.S. House representatives face re-election every two years.

Table A8 presents results from the DDIV model where the outcomes of interest are partisan U.S. House vote shares. I find that a one percentage point increase in Republican presidential vote share leads to a 0.51 percentage point increase in Republican U.S. House vote share.

Table A8: An increase in Republican presidential vote share leads to concurrent increases (decreases, resp.) in Republican (Democrat, resp.) U.S. House vote shares.

	(1)	(2)
Rep. U.S. House Vote Share	0.514**	0.549**
	(0.254)	(0.223)
Dem. U.S. House Vote Share	-0.49*	-0.527*
	(0.291)	(0.303)
First-Stage (Rep. Pres. Vote Share)	0.035***	0.037**
	(0.013)	(0.016)
N	4910	4910
Controls		✓

Standard errors are clustered at the state level and listed in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table re-estimates results from Table 4 using only U.S. House vote shares as the outcome of interest. I drop observations where only one major party competed.