No Evidence of Personal Incumbency Disadvantage[1]

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Abstract

Scholars have long studied the advantages of incumbent candidates in the United States and other advanced democracies, but a recent spate of empirical studies claims to have identified incumbency *disadvantages* in other, sometimes less developed, democracies including Brazil, Colombia, India, Japan, Mexico, and Romania. These surprising findings have spurred researchers to develop new theories of incumbency disadvantage. In this paper, we reassess the existing evidence and conclude that there is little compelling evidence of personal incumbency disadvantage—defined as the electoral punishment to an individual candidate by virtue of her incumbency status—in any context so far studied. Some of the incumbency disadvantage results in the literature arise from unusual specifications and are not statistically robust. Others identify interesting phenomena that are conceptually distinct from a personal incumbency disadvantage. For example, there is evidence of an incumbent party disadvantage in some settings where incumbents themselves are not allowed to run for reelection. Of course, our analysis does not preclude the possibility of incumbency disadvantage somewhere, but incumbency disadvantage does not appear to be a prevalent feature of developed or developing democracies.

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Incumbent candidates possess measurable advantages over challengers in many democracies around the world including the U.S. (Ansolabehere and Snyder 2002), the U.K. (Eggers and Spirling 2014), Germany (Haimueller and Kern 2008), Australia (Horiuchi and Leigh 2009), and Canada (Kendall and Rekkas 2012). "Incumbency advantage" has proven to be a fruitful but methodologically fraught topic of study. What incumbents' advantages imply for voter welfare in a normative sense is unclear, but measuring it nonetheless sheds light on a variety of positive, empirical questions about electoral institutions, voter behavior, and the incentives of reelection-minded representatives. In expanding the study of incumbency advantage beyond advanced, western democracies, recent work promises to help researchers elucidate the characteristics of countries' electoral institutions that relate to bigger or smaller incumbency advantages. In this vein, some of the most visible recent work has argued that there are even incumbency *disadvantages*—that is, electoral contexts where running for reelection as the incumbent *decreases* a candidate's chance of winning. Table 1 provides a summary of all previous studies that, to our knowledge, could be interpreted as evidence of incumbency disadvantage, whether at the level of the individual candidate or the candidate's party (an issue we will return to below). These counterintuitive results, primarily arising from less developed countries, have triggered a rush to generate theories that explain incumbency disadvantage (for example, see Eggers 2015; Kartik and Van Weelden 2016; Klašnja 2016) and a search for further negative incumbency estimates in new contexts.

In response to these trends, we reconsider the accumulated evidence for incumbency disadvantages, and we conclude that there is no compelling evidence for incumbency disadvantage in any context thus far studied in the literature, at least in the sense that most scholars writing on the topic would take the phrase *incumbency disadvantage* to mean. Specifically, what we mean is that we find no evidence that *individual* candidates do worse, electorally, because they are incumbents. Although there are a few cases with evidence for *party*-level disadvantages, until very recently the

phrases *incumbency advantage* and *incumbency disadvantage* have unambiguously referred to candidate-specific quantities and, as we will show, subsequent work has repeatedly mistaken the existing negative estimates as evidence of personal incumbency disadvantages even though this interpretation is unwarranted.

We make three main points. First, considering the full scope of electoral settings across the world, the aggregated statistical evidence for incumbency disadvantages, when considered as a whole, is decidedly weak. When we apply a uniform set of data coding and estimation procedures to electoral data from every available and relevant setting, we find negative and statistically significant effects of incumbency in only 2 out of 21 cases. Second, in the few cases where the estimated effects are robustly negative, they appear to be explained by phenomena that are conceptually distinct from what most readers would likely think of as incumbency effects. As we will explain, contrary to the common interpretation in the recent literature, these few negative estimates do not imply that incumbent individuals suffer an electoral penalty because they are incumbents. Although the papers presenting these effects are clear that they reflect disadvantages that accrue to the party, rather than to individual incumbents, we show that subsequent researchers have nevertheless interpreted these results as personal incumbency disadvantages. This is likely because the long literature on incumbency advantage focuses almost exclusively on individual incumbency (for recent discussions of this issue see Erikson and Titiunik 2015 and Fowler and Hall 2014). Third and finally, incumbents enjoy electoral advantages almost everywhere, even in countries that seem quite similar, institutionally, to places claimed to have incumbency disadvantages. While incumbency disadvantage is plausible, it is far too early to declare incumbency disadvantage a pervasive phenomenon requiring theoretical explanation.[2] If anything, the variation in the degree to which incumbents are positively

[2] We do not mean to suggest that *only* pervasive phenomena warrant theoretical investigation. But theoretical work on incumbency disadvantage clearly takes as its starting point that such disadvantages have been documented empirically.

advantaged is the more pressing matter demanding further theoretical explanation and empirical examination.

Beyond what our study says about the phenomenon of incumbency advantage in a comparative perspective, our results are also relevant for the recent literature on research transparency, publication bias, and replication (e.g., Franco, Malhotra, and Simonovits 2014, 2015; Nosek et. al. 2015). We suspect that the growth in incumbency disadvantage papers reflects the need for researchers to produce surprising results in order to overcome the bar for publication. Because incumbency advantages have been well documented, it may be harder to publish additional findings of positive effects compared to counterintuitive estimates of disadvantage. When we take a step back and reevaluate the literature, we find little evidence for such negative effects. In documenting these patterns, we hope that our paper encourages further replication efforts that, in our view, are crucial for accumulating empirical knowledge as a discipline.

Defining and Estimating Incumbency Effects

A long and well-trod literature in political science seeks to study the incumbency advantage, defined as the degree to which an individual candidate is advantaged when she runs as an incumbent compared to the counterfactual world in which she runs in an open-seat race.[3] Identifying the effects of incumbency status on elections is made difficult by obvious issues of omitted variables and reverse causation. Previous estimators for incumbency advantage include the sophomore surge and retirement slump (Erikson 1971) along with panel regressions (Ansolabehere and Snyder 2002; Gelman and King 1990). While innovative and valuable, these strategies are often unable to separate

[3] See Fowler and Hall (2014, pp. 507-508) for a more formal definition of "personal incumbency advantage" as well as a discussion of a hypothetical randomized experiment that could identify this quantity.

the effects of incumbency from other phenomena including mean reversion, strategic retirement, and the selection of high-quality candidates into incumbency.

Regression discontinuity (RD) designs provide one strategy for overcoming some of these challenges. By exploiting close elections where some candidates barely win or lose by virtual chance, we can minimize selection problems because barely winning incumbents should be no more popular than the barely losing candidates they just beat out. One general strategy involves conducting an RD analysis at the candidate level (e.g., De Magalhaes 2015) and estimating the effect of an electoral victory on a candidate's future success. However, not all incumbents seek reelection, and many losing candidates may not run again in the future. If we drop the cases where these individuals do not run again, we risk severely biasing the estimates. This could create a positive bias if the set of incumbents who seek reelection are especially likely to do well, but it could also create a negative bias if the set of challengers who choose to run again are especially likely to win. Alternatively, some authors avoid dropping these cases by estimating the effect of incumbency on the probability a given candidate wins a future election, regardless of whether the candidate runs again. In other words, these papers ask whether winning one election makes an individual candidate more likely to both run and win in the next election. The problem is that we want to estimate the effect of incumbency conditional on running—that's how incumbency advantage is conventionally defined, but this approach estimates a different (although still interesting) quantity. Suppose we obtain a positive (negative) estimate with this design. It could arise because there is a positive (negative) incumbency advantage or if winning simply makes high-quality candidates more (less) likely to run again.

To circumvent these issues, Lee (2008) applies a regression discontinuity design at the party level, estimating the effect of a candidate winning an election on the electoral success of her *party* in the next election, regardless of whether she seeks reelection or not. For the remainder of the paper, we refer to this quantity as the *Lee estimand*. See Erikson and Titiunik (2015) and Fowler and Hall

(2014) for discussions of how one could relate the Lee estimand to the personal incumbency advantage and for more detailed reviews of this broader literature.

The cost of this design is that it estimates a quantity that differs from the goals of the previous literature. As explained by Fowler and Hall (2014), with some assumptions, the Lee estimand can be related to the personal incumbency advantage through the following equation:

(1) Lee Estimand = 2*Personal Incumbency Advantage*Pr(Winner Runs Again) + 2*Partisan Incumbency Advantage.

In words, the Lee estimand equals two times the personal incumbency advantage times the probability that the winners of close elections run again plus two times the partisan incumbency advantage—a conceptually distinct quantity whereby the party of the incumbent might influence future election results even if the incumbent herself retires. We have to account for the probability that winners run again because the Lee estimand will only detect a personal incumbency advantage to the extent that individual incumbents seek reelection. We also have to consider a partisan incumbency advantage, because we could imagine ways in which the incumbent party could systematically influence elections, even if incumbent candidates do not seek reelection. And the number *2* factors into the equation because when one party benefits from personal or partisan incumbency, the opposing party suffers, just as the result of a one-dollar bet has a two-dollar effect on each gambler's wealth.

The equation above holds several important implications for the current literature on incumbency advantage. The Lee estimator is the most common approach used in the literature today, yet it does not cleanly identify the personal incumbency advantage. First, the Lee estimand is potentially contaminated by another phenomenon—the partisan incumbency advantage. Second, if the winners of close elections don't or can't run for reelection, or if they switch parties before they run for reelection, then the Lee estimand tells us *nothing* about the personal incumbency advantage.

However, if the partisan incumbency advantage is negligible and if most incumbents do seek reelection, then the Lee estimand and the personal incumbency advantage will have the same sign. These are the assumptions one would have to make in order to learn about the sign of the personal incumbency advantage from the Lee estimand. They may or may not be defensible in a particular setting, and we are not explicitly making those assumptions. We are simply pointing out that if one were to use the Lee estimand as evidence of incumbency disadvantage, they would have to demonstrate that a meaningful share of incumbents do seek reelection and defend the assumption the partisan incumbency advantage is negligible.

Keeping these issues in mind, in the next section, we will apply the Lee estimator to many different electoral settings in order to assess the evidence of incumbency disadvantage. If incumbency disadvantage is a widespread phenomenon, we might expect the Lee estimand to be negative in a number of different electoral settings. The point of this exercise is not to pretend that the Lee estimator will perfectly capture the personal incumbency advantage in each setting. Rather, the point is to grant, for the moment, the assumptions necessary for the Lee estimator to isolate this quantity, and see if we can find evidence for persistent disadvantages in many contexts. For any context where we obtain negative estimates, we will explore them further to assess whether those estimates are indicative of a personal incumbency disadvantage. And in contexts where there is a discrepancy between our estimates and those in the original paper, we consider in depth what estimator is appropriate and what the data tells us.

RD Estimates of Incumbency Effects across Contexts

To assess the evidence of incumbency disadvantage, we apply the Lee estimator to every setting for which it is appropriate and for which we have available data. Specifically, the Lee estimator requires that a single incumbent is elected by plurality rule, and it requires consistent party

labels such that partisan vote shares can be calculated in each election. In the next section, we will discuss evidence from other settings where the Lee estimator is not applicable.

In order to minimize p-hacking and researcher degrees of freedom (Simmons, Nelson, and Simonsohn 2011), we apply the same specification and coding rules to each setting. Considering the incentives of researchers to obtain surprising findings and the incentives of journals to publish them, we might worry that some of the incumbency disadvantage results in the literature are false positives that arose through unusual specifications, and we mitigate some of these issues by applying the same simple, transparent specification to each setting.

Most of our data for this analysis comes from Eggers et al. (2015) who obtained the data from varied sources (see Eggers et al. for details). Additional data on mayoral elections in Chile, Colombia, and Peru come from Klašnja and Titiunik (2016), data on state legislative elections in India come from Uppal (2009), and data on mayoral elections in Romania come from Klašnja (2015). In total, we analyze 21 different electoral contexts.

The Lee estimator first requires that for each setting we define one particular party as the reference party. When we use their data, we choose the same reference parties as Eggers et al., who typically select the most dominant party in each setting. When we use data from Klašnja and Titiunik in Chile, Colombia, and Peru, we follow their protocol and define the reference party as whichever party previously held office.

The dependent variable in our analyses is a measure of the electoral success of the reference party. We present separate results in each setting using both vote share and an indicator for electoral victory as the dependent variable. Vote share is defined as the proportion of votes received by the reference party among all votes going to the reference party and the top-performing party other than the reference party. We do not have the necessary data to analyze vote shares in Chile, Colombia, and Peru, so we only show the victory results for these countries. The running variable in our

analyses is the vote margin of the reference party in the previous election. When this variable is greater than 0, the candidate from the reference party won and became an incumbent, and when this variable is less than 0, a candidate from another party won and became an incumbent.

The Lee estimator requires that we estimate the expected limit of the dependent variable as the running variable approaches the electoral threshold (0 in this case) from both sides. Following Eggers et al., we do this in a simple, transparent way that attempts to appropriately weight considerations of both bias and efficiency. We select a bandwidth of 10 percentage points and we implement separate linear regressions on each side of the threshold. We do this by estimating the following regression using OLS, only including observations where the previous vote margin fell between −10 and 10 percentage points:

(2) $\text{Victory/Vote Share}(t)_{it} = \alpha + \beta*\text{Victory}(t-1)_{it} + \gamma*\text{VoteMargin}(t-1)_{it} + \delta*\text{VoteMargin}(t-1)_{it}*\text{Victory}(t-1)_{it} + \varepsilon_{it}.$

Each observation represents an election in constituency *i* in period *t*. The Lee estimand is denoted by β, the discontinuity in the outcome of interest (either vote share or probability of victory) at the electoral threshold. In the Appendix, we present the same estimates using the local polynomial RD estimation procedure with optimal bandwidths recommended by Calonico, Cattaneo, and Titiunik (2014), and the results are very similar.

Estimates of β for each setting and each outcome variable are shown in Table 2, and those for win probability are plotted in Figure 1 as well. In total, we have 39 estimates from 21 different electoral contexts. 31 of these 39 estimates (about 4 in 5) have a positive sign, and 21 of these 31 are statistically significant ($p < .05$). Among the 8 negative estimates, only 4 of them are statistically significant, and those are specific to mayoral elections in Brazil and Mexico. The negative estimate in Colombia is substantively large, although not statistically significant, and beyond these three settings, we find virtually no evidence of incumbency disadvantage. On the whole, incumbency advantage, as

measured by the Lee estimator, appears to be positive and substantively important in most settings. And the 2 or 3 settings where we do observe an incumbency disadvantage are mayoral elections in South American countries with term limits, a point that we will return to later. Of course, there could be a personal incumbency disadvantage is some setting that is masked by a larger, positive partisan incumbency advantage—which would also explain the positive Lee estimates—but acknowledging the limitations of the RD design, we detect little evidence of disadvantage. The simplest explanation is that incumbent candidates are rarely disadvantaged by their status as incumbents.

Naturally, our sample is weighted toward advanced, western democracies, and 4 of the 21 rows in Table 2 correspond to the U.S. Nonetheless, we see evidence of positive incumbency advantage in many regions and in some less developed democracies. Some readers may notice that our vote share estimate of 3.8 percentage points for the U.S. House is notably smaller than what other studies, such as Lee (2008), report. This is because we include all elections between 1880 and 2010 in this particular analysis. The estimated incumbency advantage in the U.S. rose significantly over time (Ansolabehere and Snyder 2002), so studies that focus only on a later period will produce larger estimates.

Our results diverge from the existing literature in at least two settings, India and Romania. In India, Linden (2004) and Lee (2016) estimate incumbency disadvantage in the Lok Sabha, the lower chamber of the national parliament. Both studies conduct their analysis at the individual candidate level, so the results are subject to the general concerns with that design described above. Furthermore, in both studies, the negative estimate appears to be sensitive to specification and is obtained in the 1990s but not the 1980s.[4] Employing our specification and including a wider range of years, we find no evidence of incumbency advantage or disadvantage in the Lok Sabha. Uppal

[4] In personal correspondence, Lee reports that he obtains negative and statistically significant results for the 2000s when selecting on candidates who run again.

(2009) estimates incumbency disadvantage in state legislative elections in India, also employing a candidate-level analysis, and these negative estimates only arise from dropping cases where the incumbent chose not to seek reelection. When we employ our design and include all observations, we obtain positive incumbency advantage estimates for Indian state legislatures. To be fair, Indian elections are unusual in several respects, and individual incumbents often change parties between elections, making the Lee estimator less appropriate in these settings. Frequent party switching could lead the Lee estimator to be close to zero, even if there is an incumbency disadvantage in India. However, the Lee estimator is, in fact, positive for Indian state legislatures, and the combination of evidence from multiple designs and multiple settings leads us to conclude that there is no compelling evidence of incumbency disadvantage in India.

For Romania, Klašnja (2015) reports negative estimates, while we obtain estimates that are substantively small and statistically indistinguishable from zero. The reason for this discrepancy is that Klašnja implements an RD design using *pre-runoff* vote shares, while we, following Eggers et. al. (2015) and Hall (2015), use runoff vote shares when applicable. To be specific, for the set of races that go to a runoff, Klašnja compares a party's electoral outcome when the party barely makes the runoff vs. when it barely misses the runoff—i.e., when the party finishes second vs. third. Barely making the runoff makes the party more likely to become the incumbent party, but non-compliance arises from cases where the party loses the runoff. Klašnja (2015, Appendix Table A2) reports that parties barely finishing second rather than third, and making the runoff as a result, go on to win only 20% of the time. The other 80% of the time the "treated" party does not become the incumbent party, making the first stage quite weak. Furthermore, we might worry that participating in a runoff has an additional effect on subsequent election results independent of incumbency. On one hand, there could be reputational benefits associated with coming in second place (Fujiwara 2016). On the other hand, the costs of competing in the runoff could harm a candidate or party (Fouirnaies and

Hall 2016), especially in the marginal cases where a party barely got into the runoff and has a low chance of winning. In either case, the exclusion restriction necessary for Klasnja's main analysis would be violated. For these reasons, Klašnja's negative estimates tell us little about the effects of incumbency itself, while our more straightforward design shows no evidence of incumbency disadvantage in Romania.

Other Evidence of Incumbency Disadvantage

Several other studies claim to have identified incumbency disadvantages in settings where either we do not have the necessary data or where the Lee estimator is not appropriate. Macdonald (2014) finds some modest evidence of incumbency disadvantage in Zambia, but the results are statistically imprecise and largely null. While the results are interesting, future analyses with more data will be needed to obtain a more precise picture of incumbency effects in Zambia.

Ariga (2015) claims to find evidence of incumbency disadvantage in the lower house of Japan's national parliament. In this setting, multiple candidates, often from the same party, compete for multiple seats within each district, so the Lee estimator is not applicable. However, similar to De Magalhaes (2014), Ariga conducts a candidate-level analysis, testing whether winning a seat makes an individual more likely to run in the next election or more likely to win a seat in the next election. Strangely, despite the term *incumbency disadvantage* appearing prominently in the paper's title, Ariga finds no evidence of a disadvantage. Winning a close election appears to slightly increase the chances that a candidate runs again, and it has no statistically detectable effect on the chances that a candidate runs again and wins. Ariga argues that these two results imply that the effect of incumbency on winning, conditional on running, must be negative, but this does not logically follow. Consider the following proof by contradiction. Suppose there is no incumbency (dis)advantage and that winning a close election makes some candidates more likely to run again, but those candidates

motivated to run by incumbency are all low-quality candidates who won't win again. There would be a positive effect of winning on running in the future and no effect on winning in the future—consistent with Ariga's results, although there is no incumbency disadvantage. Suppose further that there is a positive incumbency advantage, that winning a close election decreases the chances that high-quality candidates will run again (perhaps because they move up to higher offices), and that winning a close election increases the chances that low-quality candidates run again. With the right mix of low- and high-quality candidates and with the right magnitude of incumbency advantage, we could produce Ariga's results in a world with a positive incumbency advantage. In short, Ariga's paper presents no compelling evidence of incumbency advantage or disadvantage in Japan.

Eggers and Spirling (2015) present some negative incumbency effects in the U.K. Parliament using the Lee estimator, although these negative estimates are not the primary focus of their paper. Table 2 shows evidence of a large, positive incumbency advantage in the U.K. Parliament, but in a side analysis where Eggers and Spirling estimate the effect in a narrow range of years around 1870, they obtain negative estimates. One concern is that estimates will fluctuate naturally across subsamples, and when many tests are run, a negative estimate could arise by chance even if there is no incumbency disadvantage. This is not a critique of the main findings of Eggers and Spirling. They show compelling evidence that incumbency advantage increased over time in the U.K., but consistent with their interpretation, we believe the negative estimates for a small time period are simply suggestive and could be a false positive.

Incumbency Disadvantage in Brazil, Colombia, and Mexico?

We now turn to discussing the robust, negative effects of incumbency estimated in Brazil and Mexico, as well as the large and negative (though noisy) estimate in Colombia, all reported in

Klašnja and Titiunik (2016). Of the cases we have evaluated, these are the only cases in which clear negative effects are found. However, none of these estimates should be interpreted as evidence of a personal incumbency disadvantage, and to be clear, the authors do not claim to have found evidence of a personal incumbency disadvantage in these setting. Nonetheless, readers appear to have misinterpreted Klašnja and Titiunik's study, so this issue is worth discussing and clarifying. For example, Eggers and Spirling (2014), citing Klašnja and Titiunik, write that in Brazil, "researchers have found evidence that incumbents do poorly *because they are incumbents*" (p. 2). Lee (2016), also citing Klašnja and Titiunik, writes that studies "have found evidence that incumbency may hurt reelection chances of legislators and local officials" (p. 2). Golden and Picci (2015) credit Klašnja and Titiunik for the notion that "local-level incumbents face electoral disadvantages" (p. 512). And Klašnja (2016) cites evidence from Klašnja and Titiunik to motivate a theoretical model of personal incumbency disadvantage.

In Brazil, Mexico, and Colombia, as Klašnja and Titiunik point out, mayors are term limited.[5] For the period under investigation, a mayor can only serve two terms in Brazil and only one term in Mexico and Colombia. This means that the Lee estimator provides no information about personal incumbency advantage in Mexico and Colombia—because incumbents are never allowed to seek reelection—and only limited information about personal incumbency advantage in Brazil. The incumbent party but not the incumbent candidate appears to experience an electoral penalty in these settings.

Consistent with this idea, Klašnja and Titiunik report that in Brazil the Lee estimator is close to zero for the cases when the incumbent can run again—that is, for contexts where each of the competing parties' candidates, if he or she wins, will be eligible to run for reelection next time. On the other hand, the estimate is negative for the cases where at least one of the competing parties'

[5] The authors write on p.38, "there is a strong correlation between incumbent electoral losses and whether incumbent mayors are allowed to run for reelection."

candidates will not be eligible to run for re-election. This makes it clear that the negative effect is driven by the cases where a party runs as the incumbent party but without the benefit of running the individual incumbent. Indeed, within the framework of Equation 1, this result is potentially consistent with a positive personal incumbency advantage and a negative partisan incumbency advantage.[6]

Precisely why incumbent parties can be hurt when they run for reelection without their personal incumbent is itself an interesting question and one that Klašnja and Titiunik take up in some detail, specifically focusing on the role that term limits might play in eroding the accountability of incumbents. However, this phenomenon is entirely separate from an incumbency disadvantage in the common parlance where this phrase refers to the disadvantage of an individual candidate running for her own reelection.

Conclusion

Various papers have reported negative effects of incumbency, creating a sense that incumbency disadvantages are a widespread feature of democratic elections. These claims have spurred attempts to generate theories of incumbency disadvantage, and, in our opinion, have created a new conventional wisdom that incumbents can be disadvantaged in a variety of electoral contexts, especially in developing countries. In this paper, we have reevaluated the evidence for the incumbency disadvantage, and we conclude that there is no evidence in any context thus far

[6] Klašnja and Titiunik report a Lee estimate of −.04 when both candidates would be eligible to seek reelection and −.21 when one candidate would not be eligible. If we suppose that the probability that the winner seeks reelection in each of these cases is .8 and .4, respectively (the authors do not report these numbers, so this is just a guess), we could plug these numbers into Equation 1, leaving a system of two equations (one for each election type) with two unknowns. Solving this system, we would estimate that the personal incumbency advantage is about .21, and the partisan incumbency advantage is about −.19. Of course, the assumptions necessary for this computation may not be applicable in this setting, but this exercise illustrates that Klašnja and Titiunik's results are potentially consistent with a large and positive personal incumbency advantage in Brazil.

observed that individual candidates are disadvantaged by their status as incumbents. Although we do not claim that incumbency disadvantages are impossible or that we will not observe them in any context ever, our meta-analysis suggests that the far more pressing question for the literature is why incumbents tend to be advantaged and why this advantage is larger in some contexts but smaller in others.

Indeed, we find large and persistent effects in most of western Europe and the U.S., while in other settings we find small effects. However, the pattern is not purely regional. Chile appears to have a large incumbency advantage (also see Salas 2016), while local elections in the UK seem to have a relatively small incumbency advantage. Similarly, variation between national and sub-national races explains little variation, as subnational elections in the U.S., India, and elsewhere exhibit large incumbency advantages.[7] Further research is needed to understand why incumbents tend to be advantaged in democratic elections. Historically, the study of the incumbency advantage has been focused on the U.S., but recent scholarship, which we have reviewed and replicated in this paper, has extended this topic to many other contexts. The differences in the size of the estimated effects and the variation in the underlying institutional structures of each country and electoral context should help us generate new hypotheses about the mechanisms driving incumbency advantage.

Finally, in addition to its substantive contributions, our study adds to the recent literature on research transparency and publication bias (e.g., Franco, Malhotra, and Simonovits 2014). The incumbency disadvantage literature is a burgeoning one; to push it forward we must reevaluate it and take stock of the state of our knowledge. We suspect the increasing trend of incumbency disadvantage papers is, in part, the result of the pressure to publish surprising and non-null findings. Because the previous literature identified positive advantages, journals may now be more willing to publish negative estimates than positive or null estimates. Indeed, evidence of incumbency

[7] The estimate on win probability in Indian state legislatures is relatively small (.066), but the effect on vote share is quite large (.141).

advantage in any of the countries where disadvantages have been claimed would, we suspect, lead to boilerplate comments from referees and editors about how the findings were not important enough for a top journal. We hope the accumulated evidence in this paper will make readers of this literature more skeptical of surprising, counterintuitive findings. And we hope authors, referees, and editors will be more willing to publish good research on this topic, regardless of the sign or magnitude of the estimates, so that we will eventually develop a better understanding of incumbency advantage in elections around the world. Although a personal incumbency disadvantage may exist in some contexts, we see no evidence for it in any of the cases studied thus far.

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Table 1. Previous Studies of Incumbency Disadvantage

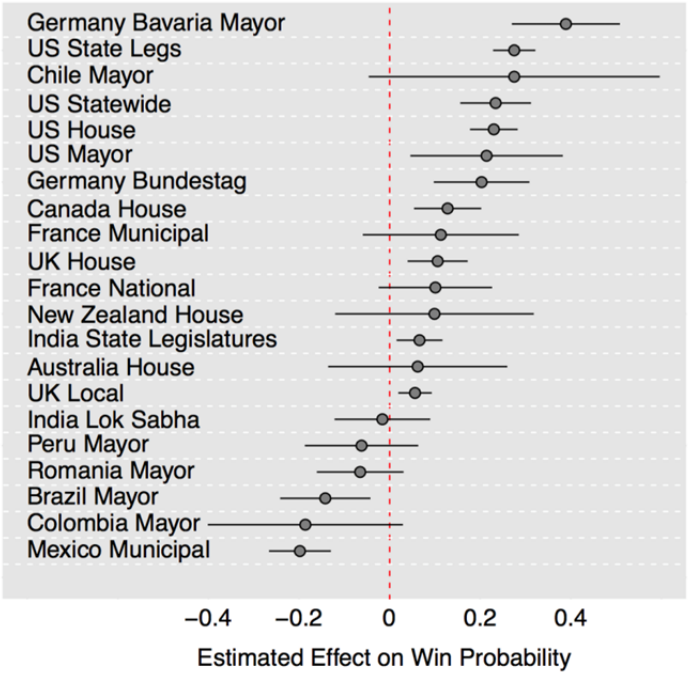
Study	Setting	Method	Finding
Linden (2004)	Indian national parliament, 1951-1999 (Lok Sabha only)	RD at individual level, no selection on re-running in main analysis	Negative effect after 1991; positive before 1991
Uppal (2009)	Indian state legislative elections, 1975-2003	RD at individual level, selection on re-running in main analysis	Negative effect overall and especially post 1991
Lee (2016)	Indian national parliament, 1977-2014	RD at individual level, with and without selection	Negative but close to zero overall; paper focuses on variation across parties
Ariga (2015)	Japan, lower house of National Parliament, 1948-1993	RD at individual level, no selection on running again, unusual fuzzy RD	Null, with argument for why this implies disadvantage
Klašnja and Titiunik (2016)	Brazil mayoral elections, 1996-2012 (also results for Chile, Mexico, Peru, Colombia)	RD at party level	Negative
Macdonald (2014)	Zambia parliament and local elections, 1991-2011	RD at party level	Negative, but largely null results
Eggers and Spirling (2015)	U.K. Parliament, 1802-2010	RD at party level	Mostly positive, but negative estimates for small range of years
Klašnja (2015)	Romania mayoral elections, 2000-2008	RD at party level pre-runoff	Negative

Table 2. Lee Estimates around the World

Country and Office	DV = Vote Share	DV = Victory
Australia, House of Representatives	.007 (.009)	.062 (.100)
Brazil, mayors	−.039 (.015)*	−.142 (.050)**
Canada, House of Commons	.027 (.006)**	.128 (.037)**
Chile, mayors		.275 (.163)
Colombia, mayors		−.186 (.109)
France, municipalities	.046 (.018)*	.113 (.087)
France, National Assembly	.019 (.009)*	.101 (.063)
Germany, Bavarian mayors	.141 (.019)**	.389 (.060)**
Germany, Bundestag	.013 (.005)**	.203 (.053)**
India, Lok Sabha	.003 (.013)	−.016 (.053)
India, state legislatures	.141 (.019)**	.066 (.025)**
Mexico, mayors	−.028 (.006)**	−.198 (.034)**
New Zealand, House of Representatives	.018 (.012)	.099 (.111)
Peru, mayors		−.062 (.063)
Romania, mayors	.002 (.014)	−.065 (.048)
U.K., House of Commons	.012 (.005)**	.106 (.033)**
U.K., local councils	.016 (.004)**	.056 (.018)**
U.S., House of Representatives	.038 (.005)**	.230 (.026)**
U.S., mayors	.058 (.031)	.214 (.085)*
U.S., state legislatures	.068 (.009)**	.275 (.023)**
U.S., statewide	.060 (.008)**	.234 (.039)**

*Robust standard errors in parentheses, * $p < .05$, ** $p < .01$. Rows with no estimate on vote share are those where replication datasets do not contain vote share variable.*

Figure 1. Lee Estimates around the World.



Appendix

Table A1. Local Polynomial Estimation with Optimal Bandwidths

Country and Office	DV = Vote Share	DV = Victory
Australia, House of Representatives	.009 (.011)	.078 (.109)
Brazil, mayors	−.040 (.017)**	−.138 (.054)**
Canada, House of Commons	.028 (.006)**	.138 (.039)**
Chile, mayors		.222 (.144)
Colombia, mayors		−.203 (.109)*
France, municipalities	.055 (.017)**	.172 (.076)*
France, National Assembly	.018 (.011)	.071 (.064)
Germany, Bavarian mayors	.129 (.017)**	.325 (.052)**
Germany, Bundestag	.014 (.006)*	.197 (.070)**
India, Lok Sabha	−.003 (.014)	−.010 (.053)
India, state legislatures	.145 (.019)**	.068 (.027)*
Mexico, mayors	−.032 (.007)**	−.225 (.039)**
New Zealand, House of Representatives	.017 (.012)	.094 (.127)
Peru, mayors		−.028 (.093)
Romania, mayors	−.003 (.016)	−.073 (.050)
U.K., House of Commons	.010 (.005)*	.103 (.037)*
U.K., local councils	.015 (.003)**	.053 (.021)*
U.S., House of Representatives	.040 (.005)**	.232 (.029)**
U.S., mayors	.041 (.027)	.203 (.084)**
U.S., state legislatures	.059 (.011)**	.260 (.028)**
U.S., statewide	.059 (.009)**	.239 (.036)**

*This table replicates Table 2 but using Calonico, Cattaneo, and Titiunik's (2014) recommended procedure of local polynomial estimation with optimal bandwidths. The table reports "conventional" RD estimates with robust standard errors in parentheses, * $p < .05$, ** $p < .01$.*