

Is There a War Party?
Party Change, the Left-Right Divide
and International Conflict

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Are leaders from certain parties particularly likely to engage in military conflict? This question is difficult to answer because of selection bias. For example, countries may be more likely to elect right-wing leaders if their publics are more hawkish or if the international system is particularly dangerous. Put simply, who comes to power is not random, which makes causal inference difficult. We overcome this problem by using a regression discontinuity design. Specifically, we look at close presidential elections that were essentially “tossups” between two candidates. We find that parties do behave differently in office, along with suggestive evidence that electing right-wing candidates increases state aggression. In support of theories of reputational signaling, we also show that electing candidates from challenger parties makes countries much more likely to initiate military disputes, particularly in the first year of the new leader’s term.

Is the likelihood that a democracy will take military action against other countries largely influenced by which party controls the presidency? Many believe so. In modern American politics, one party is consistently identified as more hawkish than the other. Surveys have revealed that Republican voters consistently prefer more aggressive policies (Eundak 2006, 71; Trager and Vavreck 2011; Gries 2014, 9). Moreover, many believe that Al Gore, had he been elected, would not have fought the wars that U.S. President George W. Bush did, and that the foreign policies of Hillary Clinton and Donald Trump would be similarly opposed.

Nevertheless, it is very difficult to determine whether the party in control of the presidency really has an important impact on foreign policy due to the selection of parties into particular domestic and international contexts. Put simply, which party controls the presidency is not random. For example, the victory of George W. Bush in 2004 can be attributed to a number of domestic and international factors at the time, including the American public's heightened concerns over national security following September 11. Similarly, Barack Obama's success in 2008 was influenced by problems at home and a shift in public willingness to engage in military adventurism. Therefore, an observational analysis would likely be biased by such selection processes. Thus, even if countries behave differently when certain parties control the presidency, it would be very difficult to know if that difference is explained by the parties or by the environments into which the parties are selected.

In principle, we could overcome this problem by running an experiment in which we randomly assigned countries to be ruled by leaders from different parties. Such an ideal research design would avoid the confounding problem, making it possible to test whether countries tend to be more or less aggressive when certain parties control the presidency. Experiments are unmatched in their ability to identify causal effects, so this type of study could greatly improve our understanding of how electing candidates from different parties influences foreign policy.

We approximate this ideal experiment by using a regression discontinuity design. Specifically,

we look at close presidential elections where a candidate from one party barely defeated a candidate from a different party. Such a design works if it is close to random which party won in these cases, a premise which is plausible given the inherent randomness in large national elections. Thus, we use close elections to get data that is similar to what would result from a real experiment. Such natural experimental designs are extremely rare in the study of war, and thus warrant attention in the exceptional instances when they do occur.

The first analysis that we carry out looks at whether countries tend to be more (or less) aggressive when presidential candidates from right-wing parties barely defeat candidates from left-wing parties. This quasi-experimental comparison involves a small sample size ($n = 29$), but we still find noteworthy evidence that electing right-wing candidates increases the likelihood that countries will initiate high-level military disputes against other states. Also, despite the limited sample size, we can confidently reject that right-wing parties have very large ($1.2 \cdot \text{SD}$ of the outcome) positive effects on dispute initiation ($p < 0.05$). We can also reject that they have negative effects that are medium-sized ($0.5 \cdot \text{SD}$; $p < 0.05$), large ($0.8 \cdot \text{SD}$; $p < 0.01$), or very large ($1.2 \cdot \text{SD}$; $p < 0.001$).

To increase our statistical power, our second analysis focuses on cases where candidates from incumbent parties barely won and barely lost to candidates from challenger parties ($n = 36$). Specifically, we look at whether countries experienced a larger change in their propensity to initiate military disputes when the candidate from the challenger party barely won. Thus, our key outcome of interest here is how much countries deviated from their prior levels of dispute initiation. We find statistically significant evidence that electing candidates from challenger parties increases the extent to which countries deviate from their previous levels of aggression. Thus, this test gives us conclusive evidence that which party controls the presidency does affect the likelihood of interstate conflict.

This finding seems to suggest that some parties are more inclined to use military force abroad than others, and that these preferences are only partly captured by the left-right divide. However,

we find instead that our results are largely explained by a tendency for candidates from challenger parties to initiate military disputes in their first year in office. Thus, these findings support the theory that major leadership transitions tend to increase the chances of state aggression, either because new leaders lack the experience to manage international crises effectively or because they need to prove their resolve in foreign affairs by acting tough.

The main limitation of our research design is that there are not that many close elections to analyze. Without a large amount of data, it is difficult to detect small effects and get narrow confidence intervals. However, in the power tests that we ran at the beginning of this project, we found that this design could pick up medium-sized and large effects more reliably. For the test of left versus right-wing parties, we determined we would correctly detect (at $\alpha = 0.05$) a medium-sized effect ($0.5 \cdot \text{SD}$) 30% of the time, a large effect ($0.8 \cdot \text{SD}$) 54% of the time, and a very large effect ($1.2 \cdot \text{SD}$) 82% of the time. In the incumbency power analysis, we found that we would detect a medium-sized effect 55% of the time, a large effect 93% of the time, and a very large effect over 99% of the time. Also, if the effects were small or non-existent, we would be able to establish confidence intervals that were precise enough to rule out very large ($\pm 1.2 \cdot \text{SD}$) positive and negative effects.¹

Moreover, although the results for our second test are significant at conventional levels, we encourage readers to avoid interpreting p -values as either significant ($p < 0.05$) or not while reading this paper and to bear the bias-variance trade-off in research design in mind. Almost all research in international relations lacks any claim to strong causal identification, being based on observational data and linear adjustment of largely ad-hoc covariate sets. By contrast, the design presented here has a strong claim to causal identification and unbiasedness, providing a crucial complement to the

¹As a basis for comparison we can convert the estimated associations of some standard conflict variables into these standardized effects: the existence of an alliance has a small effect (0.2 SD), a 10 point increase in the Polity score of an authoritarian country has a medium sized effect (0.53 SD), and whether a country is a major power has a very large effect (1.35 SD). These calculations were done by taking the estimated associations from a statistical model (G1 in Dafoe 2011), expressed in log-odds, applying them to the baseline probability of MIDs initiated per year in our data (0.23), and then dividing by the standard deviation of MIDs initiated per year in our data.

vast majority of the literature which does not. Thus, since p -values provide a continuous measure of how inconsistent the evidence is with the null hypothesis, a higher p -value in an unbiased design may be more informative than a lower p -value in a biased one. Small p -values (eg $p < 0.2$), even if not significant at conventional standards, also provide important evidence in these contexts.

This paper makes several important contributions to the study of international relations. First, there is a longstanding debate in political science over whether leaders have an important independent impact on interstate conflict or whether their influence is largely constrained by domestic and strategic realities (Waltz 1979; Byman and Pollack 2001; Olken and Jones 2009; Chiozza and Goemans 2011; Saunders 2011; Horowitz, Stam, and Ellis 2015; Croco 2015). This study provides quasi-experimental evidence that leaders do matter. Second, this study provides evidence that domestic political ideology can spill over into the international realm. One of the main explanations for the democratic peace is that democracies act in accordance with their domestic norms when it comes to foreign policy (Morgan and Campbell 1991). While this study does not conclusively show that left-wing leaders are more dovish than others, the findings presented here are consistent with that hypothesis. Third, these results suggest that we should be alert to the potential for interstate conflict when new leaders take office, especially when party control of the presidency changes hands. These leaders will be particularly likely to initiate military disputes during their first year in office.

This study is also notable because it is one of the first in the international relations literature to use a pre-analysis plan. Prior to looking at any of the results, we pre-registered the main tests that we planned to conduct in this paper. Our motivation here was to tie our hands so that there could be no question of sifting through the data to find the statistical tests that produced the most interesting or significant results. The temptation for scholars to run many tests and then report the ones that they like can lead to misleading findings. This danger has attracted a great deal of attention across scientific fields over the last decade, and it is seen by many as a major problem for

quantitative research (Nosek et al. 2015). The purpose of pre-analysis plans is to help ensure that research remains credible.

The article proceeds as follows. We first review the existing theoretical and empirical work on this subject. We then outline the research design in more detail. Next, we conduct design checks to verify that the research design is appropriate. We then present the results for party ideology. After that, we test whether party turnover leads to changes in the likelihood of state aggression. We then discuss the findings and conclude.

Leaders, Parties, and International Conflict

In recent years, there has been a great deal of interest in the longstanding question of whether leaders influence the chances of interstate conflict, and if so, how. Researchers have looked at the extent to which leaders' backgrounds affect their countries' foreign policies (Paper #1; Paper #4; Paper #8) and how other countries respond to them (Paper #7; Paper #8). They have also examined the ways that leaders can carry out their personal agendas by shifting public opinion (Paper #2; Paper #3; Paper #5). A major question in this research program is whether which party the leader comes from matters for interstate conflict. Are leaders from certain parties more likely to behave aggressively in foreign affairs, or is the ideology of the leader largely unrelated to state behavior?

In the contemporary United States, liberals and conservatives respond to different sets of fundamental moral values. Graham, Haidt and Nosek (2009) show that liberals are most concerned with fairness and the duties of care and preventing harm. Conservatives have a greater concern than liberals for the preservation of social orders, the purity of sanctified objects, and loyalty to an in-group. The authors refer to these conservative values together as the “binding foundations,” which bind identity groups together and preserve orders within and between them.² This study builds on a long

²This is consistent with the view that individuals represent moral types as a result of inherent nature and socialization, with moral reason-giving following rather than directing moral intuitions (Haidt 2001).

tradition linking personal values to political orientation (e.g. Caprara and Zimbardo 2004; Feldman 1988; Franklin, Mackie and Valen 2009; Sniderman, Brody and Tetlock 1993).

Kertzer et al. (2014) show that these differences in moral foundations predict differences in foreign policy attitudes. The values associated with conservatives predict a more militant approach to international affairs in the United States. Kertzer and Rathbun (2015) argue further that these distinctions apply cross nationally and determine distinct international negotiating styles for liberals and conservatives. The values these parties and their leaders hold “are thought to be universal in nature by those who hold them, meaning that they should apply at home and abroad” (Kertzer and Rathbun 2015; Schwartz 1992). In particular, liberals, they argue, are more “pro-social” and seek compromise internationally, in contrast to conservatives, who are more “pro-self” and therefore bargain more aggressively (Schwartz, Caprara and Vecchione 2010). They apply this idea to Anglo-French and Franco-German negotiations in the 1920s and argue that the conservative governments were indeed less amenable to compromise.³

A variety of other conceptualizations of the roles of parties and leader characteristics in international politics also imply systematic cross-national differences in the conflict behavior of political parties. According to the venerable Hobson-Lenin thesis, the interests of capitalists imply the need to export capital and then the need to protect it, and therefore the imperialist policies of the parties that represent those interests.⁴ In contemporary scholarship, Narizny (2007, 28) argues that “partisan coalitions tend to choose leaders whose policy positions correspond to their aggregated interests.” Thus, “the dichotomy between the study of international security and the rest of political science is conceptually bankrupt... [and] instead foreign policy follows the same logic as domestic

³For a related approach, see Rathbun, Kertzer and Paradis (2015).

⁴Hobson (1965/1902); Lenin (1964/1916). According to Kirshner (2007), financial interests push for peace, which implies a partisan logic if some parties are more responsive to the finance sector. On this topic, see also Gartzke (2007) and McDonald (2009). On the influence of the economic character of districts on foreign policy voting in the U.S. Senate, see Fordham (1998).

politics” (Narizny 2007, 1). Left-leaning governments strengthen their countries’ militaries because the costs are disproportionately born by wealthy interests (Narizny 2003). Grand strategy is the product of coalitions of business sectors that capture political parties. Similarly, Trubowitz (1998) argues that regional interests embodied in party divides explain foreign policy differences. For both authors, these domestic coalitions are relatively stable and thus imply persistent differences in the foreign policy orientations of parties from election to election.⁵

Mattes, Leeds and Carroll (2015) argue that changes in the supporting coalitions of leaders predict foreign policy change better than changes in the leadership itself. They further consider changes of the party in power to be a primary determinant of shifts in supporting coalitions. Their analysis of changes in the foreign policy positions of nations in the United Nations General Assembly (UNGA) confirms that shifts in the party in power do indeed correlate with UNGA voting differences.⁶

Other scholars argue that partisan coalitions influence specific aspects of security policies. Rathbun (2004) and Haas (2005) find this influence on support for peace-enforcement missions. Solingen (2009, 40-7) argues that economic interests and the ideologies of partisan coalitions influence nuclear weapons policy. Leeds, Mattes and Vogel (2009) argue that changes in domestic coalitions influence changes in alliance commitments in autocracies but not in democracies. Haas (2003) argues that ideological similarity leads to alliance (cf. Walt 1987).⁷

But despite the widespread belief that electing leaders from different parties can influence the

⁵Brooks (2013) argues by contrast that in the globalized world, powerful economic actors on both sides of the left-right divide favor peace.

⁶UNGA ideal points are estimated using voting data through the method applied in Bailey, Strezhnev and Voeten (2015).

⁷A substantial portion of the literature on ideology in international relations deals with the influence of foreign ideology on perceptions of threat. See, for instance, Risse-Kappen (1996), Russett (1994) and Miller (2002). Some works in this literature, such as Owen (2006), also consider how different political parties behave internationally. Constructivist literatures consider how identity and ideology influence foreign policy, although most works in this tradition investigate identity at the level of the nation-state rather than the party. For an overview of this literature, see Checkel (1998) and Adler (2013).

likelihood of international conflict, a number of scholars have challenged this view. Trager and Vavreck (2011) examine the political incentives of partisan politicians in international crises. They find that while the voters of one U.S. party are more hawkish than the voters of the other, leaders of both parties can have incentive to hide their “types.” Democratic presidents have incentives to adopt more hawkish foreign policies because the Democratic reputation for dovishness implies that moderation will sometimes be interpreted as weakness (Schultz 2005). Republican presidents, by contrast, have incentive to adopt more moderate policies because the Republican reputation for hawkishness implies that the president can easily be judged unduly aggressive. Thus, “party elites have incentives to behave according to type in Congress and contrary to type in the Oval Office” (Trager and Vavreck 2011, 526). On this account, therefore, preference and political incentive may push in opposite directions; there is no clear prediction about the actual foreign policy behavior of the parties and these dynamics may serve as an explanation for foreign policy consistency from one administration to another in spite of the divergent preferences of leaders of opposing parties and their core constituencies.

Other scholars argue that the role of leaders is largely constrained by domestic and international factors. This idea was powerfully expressed by Waltz (1959), who classified theories of war into three categories: (1) 1st image theories that explain war by pointing to individual traits, like human nature, (2) 2nd image theories that focus on state-level variables, like regime type, and (3) 3rd image theories that look at the structure of the international system, like the balance of power. Waltz dismissed 1st image theories in part because an important one at the time was the idea that human nature explains war (Morgenthau 1948), and Waltz was pessimistic about the malleability of human nature. Thus, he maintained that the variable record of human conflict could not be explained by human nature, which is relatively constant. This simple critique and others led many international relations theorists to ignore individual leaders, focusing instead on factors like the offense-defense

balance, regime type, domestic instability, and the distribution of power.⁸ Recently, however, more scholars have turned their attention back to the 1st image. These scholars assert that there is a large gap in the international relations literature when it comes to explaining how different types of leaders affect foreign policy (Byman and Pollack 2001; Horowitz, McDermott and Stam 2005). Nevertheless, this new emphasis on leaders has its critics, who maintain that state behavior is primarily driven by the distribution of military power or national identities (Wendt 1999; Mearsheimer 2001).

As we detail in our pre-analysis plan, we started this project with the belief that leaders do matter and that electing leaders from different parties does affect the likelihood of state aggression. From the vast literatures that relate to the international effects of political parties, we drew two hypotheses. The first is the *Party Ideology Hypothesis*, which predicts that electing leaders from right-wing parties will increase the likelihood of state aggression. The second hypothesis is highly general and speaks directly to the value of 1st image theory. It posits that electing a leader from the incumbent party will lead to less change in international dispute behavior than electing a leader from a challenger party. We refer to this as the *Incumbent/Challenger Hypothesis*.

Party Ideology Hypothesis: Electing presidential candidates from right-wing parties will make countries more aggressive than electing candidates from left-wing parties.

Incumbent/Challenger Hypothesis: Electing candidates from challenger parties will lead to a greater change in state aggression than electing candidates from incumbent parties (the absolute difference in aggression between presidential terms will be greater when there is party turnover).

One issue that is related to the Incumbent/Challenger hypothesis is that new leaders may be particularly likely to act aggressively early in their terms. There are several reasons why this might be the case. First, new leaders may lack the experience to manage international crises effectively,

⁸Waltz also argued that the distribution of power “shapes and shoves” but does not determine events in the international system, leaving room for 1st image theory (Waltz 2003, 53).

making it more likely that disagreements with other states will turn into military conflicts (Potter 2007). Second, these leaders may be more likely to want to show the international community that they are willing to use force abroad, which could strengthen their bargaining leverage in future international negotiations (Paper # 1; Wolford 2007; Dafoe 2012, Ch 4). Third, these leaders may want to send a signal to their domestic audiences that they are tough when it comes to foreign affairs, which could increase their popularity at home.

While most of the existing theory and research on leadership transitions has focused on cases where new leaders come to office, a similar logic might be applied to party control of the presidency, particularly when it comes to the reputational mechanisms. New leaders who are from the same party as the old one should be able to associate themselves with the previous leader's reputation, giving them less of a need to signal their resolve. On the other hand, when leaders from challenger parties come to power, there should be less certainty that the new leader will have an approach to foreign policy that is similar to the old one's. In short, when party control of the presidency changes hands, it marks a more significant leadership transition (Mattes et al. 2016). Thus, even if parties tend to behave pretty similarly across ideologies, we might still find that leaders from challenger parties might be much more aggressive early in their tenures.

Challenger Aggression Hypothesis: Electing candidates from challenger parties will lead to an increase in state aggression when the new leader takes office.

The central challenge in testing these hypotheses is endogeneity. The independent effects of parties and leaders are difficult to identify because of the likely selection of parties and leaders into particular international and domestic contexts. In the German election of 1925, for instance, the electorate was narrowly divided between two candidates of the political right, Paul von Hindenburg and Wilhelm Marx. Many of his supporters hoped that Hindenburg would abolish the Weimar

democracy, and Marx's party later voted in favor of the Enabling Act, giving Adolf Hitler dictatorial powers. The fact that the German people were presented with this choice of candidates who were both from the political right, at this moment, and with Adolph Hitler a few years later, has its roots in the context of the time. Therefore, an observational analysis of the effects of political party and leader characteristics would likely be strongly biased by such selection processes. The factors that led to the restricted ideological range of the candidates may also have contributed to the coming of the conflicts that followed (Tooze 2006, Chapter 1).

Thus, evaluating how electing leaders from different parties influences the likelihood of state aggression requires a strategy to overcome the endogeneity problem. In the next section, we present a research design that can do so. Specifically, it takes advantage of the as-if random outcomes of close presidential races to shed light on how political parties and their leaderships affect interstate conflict.

Research Design

There are several different approaches that could be used to address the omitted variable bias problem. One would be to control for a range of domestic and international factors using a linear model or a matching technique. However, a major disadvantage with this approach is that it is not guaranteed to eliminate, or even reduce, bias from omitted variables. The results from conventional regression or matching can be badly biased, even when researchers control for a wide range of important covariates (Clarke 2005). In fact, these methods can even make bias worse (Pearl 2013). Thus, we should remain cautious when interpreting analyses for which confounding or selection bias are suspected to exist.

An alternative approach would be to look at all cases of leadership turnover and compare how countries behaved before and after the leadership change. This research design is based on the

idea that countries are comparable before and after leadership transitions. This assumption may be plausible in some cases, but in others it is clearly invalid. For example, the periods before and after normal electoral leader transitions are usually not comparable. Many countries elect the leader and members of the legislature at the same time, making it difficult to determine the effect of leadership change by itself. Similarly, looking at cases when leaders were forcibly removed from office also has its limitations, since leaders are usually removed at times of extreme political tension. Likewise, leadership changes that are caused by assassinations are not likely to provide valid comparisons. The new leader will have to deal with a heightened political tension following the assassination, making the beginning of their term much different from the end of the previous leader's term.

One potentially promising approach would be to focus on changes in leadership that resulted from the natural deaths of leaders. The timing of natural leader deaths should be fairly unrelated to the domestic and international environment. Moreover, the legislature will typically not change following the natural death of a leader, making it much easier to isolate the independent effect of leaders on foreign policy. However, the natural death approach is not well-suited for this particular study. The reason is that the new leader almost always comes from the same party as the old leader. Thus, this exogenous change in leadership does not provide much leverage in determining how party control of the presidency affects interstate conflict. This research design would be much more useful in looking at other types of variation in leaders, such as age, military experience, and occupational background. However, it is not a promising design for this study.

The approach that we take instead is to use a regression discontinuity design. Regression discontinuity is a type of natural experiment that involves comparing units that barely surpassed and barely fell short of an important cut-point that influenced treatment assignment. For example, if there was a test where everyone who scored a 50 or higher got a scholarship, researchers could assess the effects of getting the scholarship by comparing the students who scored 50 and 51 to the students

who scored 48 and 49. So long as there isn't strategic sorting at the cut-point, as could happen if the graders had opportunity and motive to nudge some test takers above the cutpoint, it should be close to random which of these students won the scholarship, since they were all on the verge of getting it (Lee 2008).

Close elections provide an excellent opportunity to use regression discontinuity analysis. Given the inherent randomness in the electoral process, whether candidates barely win or barely lose in close elections is plausibly as-if random (Eggers et al. 2015). Political scientists have used regression discontinuity to study questions like how winning an election influences a party's likelihood of winning the next election (Lee 2008) and how winning an election affects a candidate's wealth later in life (Eggers and Hainmueller 2009). Scholars have also used regression discontinuity to test how economic and political outcomes differ when Republican candidates for mayor barely defeat or barely lose to Democratic candidates (Pettersson-Lidbom 2008; Gerber and Hopkins 2011; Beland 2015; de Benedictis-Kessner and Warshaw 2016).

In this article, we look at close presidential elections. To our knowledge, this study is the first to apply regression discontinuity specifically to presidential elections. For our analysis, we followed the procedures that were outlined in our pre-analysis plan. We will briefly summarize these procedures in the remainder of this section.

Defining Close Elections

We considered elections to be close if the top two candidates were within 2% of the cut-point (48%-52% range). Data on close races was available in the dataset constructed by (AUTHORS). This dataset includes every democratic election between 1815 and 2010. It provides information on the top-two candidates, including their names, parties, and vote share in the election. In cases where there were runoffs between two candidates, we used their vote shares from the runoff rather than

the initial election. We also excluded close elections in non-democracies because we were concerned about fraud in these cases. Given the possibility of fraud, we did not feel confident in assuming that the outcomes of these elections were as-if random.

One complication was the United States, which elects presidents through the electoral college. This system makes it possible for candidates to lose the popular vote but still win the election if they defeat their rival in the electoral college. To deal with this issue, we counted the electoral college vote rather than the popular vote when looking at the United States. This decision is consistent with other similar studies (AUTHORS). For every other country, we used the popular vote.

Measuring Party Ideology

To identify parties as left or right-wing, we evaluated the parties against each other according to their positions at the time of the election on social questions associated with liberalism and conservatism. Parties were judged further to the right when they expressed support for “traditional values,” national, religious, racial or ethnic in-groups, or the benefits of authority and traditional sources of authority such as a monarchy. Parties were judged further to the left when they expressed inclusive sentiments, a duty of care for vulnerable groups, and support for democratic principles. Secondly, we evaluated parties as left or right on economic policy preferences. Advocacy for wealthier interests placed a candidate further to the right and advocacy for the less well-off is associated with the left. These two social and economic dimensions are highly correlated, with the principal exceptions coming from communist and post-communist countries. In these cases, the primary social dimension determined the left-right coding. When parties could not be easily classified as left or right according to these metrics, we excluded the election from this test.

Main Tests

We looked at two different types of close elections. The first were close elections between right-wing and left-wing parties, where it was essentially random whether the presidency was controlled by a leader with a right-wing or left-wing ideology. In total, we have 29 close elections between right-wing and left-wing parties. The second set of close elections that we analyzed was narrow races between an incumbent and challenger party. In these cases, it was as-if random whether the country experienced party continuity or change in the executive branch. We have 36 of these close elections in our dataset. For this group of cases, we were particularly interested in testing whether a change in party control of the presidency increased the likelihood of a change in state aggression.

We also examined whether candidates from challenger parties are more likely to initiate military disputes at the beginning of their terms than candidates from incumbent parties, which would be consistent with the theory that major leadership transitions make state aggression more likely. However, we did not discuss this test in our pre-analysis plan, so the results from this analysis should be regarded as exploratory. Our motivation for running this test came from reading Wolford (2007), Dafoe (2012), and Paper #1 in this special issue. These articles advance a compelling theory and intriguing empirical evidence that new leaders have reputational incentives to act tough when they first come to office. We find strong evidence consistent with this hypothesis.

Outcomes

We measured aggression using the number of Militarized Interstate Disputes (MIDs) that a country initiates. These disputes are cases where countries explicitly threatened, displayed, or used force against other states (Ghosn, Palmer and Bremer 2004). Specifically, we look at the number of these disputes that a state initiated starting from when the leader took office and ending at the date that the winner of the next election was scheduled to start. In cases where leaders were replaced

part of the way through their term, we used the day that they left office instead. Since the length of time that candidates held office varied, we divided the total number of disputes by the duration of the time period. Thus, the unit of measurement is military disputes initiated per year in office.

We use slightly different versions of the outcome variables for our different tests. For the ideology test, we use military disputes initiated per year, as described in the previous paragraph. For the main incumbency test, we use the absolute change in military disputes initiated per year from the previous term. We use this variable because we are interested in evaluating whether there was a larger absolute change in military aggression when the challenger party barely won. Thus, the measure is:

$$\text{Absolute Change in Military Aggression} = \left| \text{MIDs/Year During Winner's Term} - \text{MIDs/Year During Previous Term} \right|$$

In other words, we are testing whether challenger parties gaining control of the presidency makes countries with high levels of prior aggression more likely to experience a decrease in dispute initiation and countries with low levels of prior aggression more likely to experience an increase in dispute initiation. We conduct a one-sided test for this analysis, since we expect that the absolute change will be larger for countries where the challenger party barely wins. Lastly, for the exploratory test about whether challenger candidates tend to be more aggressive when they first take office, we look at the number of disputes that each country initiated in the first year of the new presidential term.

Across these tests, our main outcomes are (1) military disputes initiated and (2) high-level military disputes initiated. High-level disputes are cases where countries used force against other states or entered into international wars. As secondary outcomes, we look at (3) all disputes that countries engaged in and (4) all high-level disputes that countries engaged in. These cases include disputes that countries did not start, but participated in nonetheless.

Estimation

We employ two estimation strategies. Our primary statistical analysis involves t-tests. This is a simple approach, recommended for its parsimony and robustness, which is appropriate given the assumption that close elections were as-if random (Dunning 2012). As a secondary test, we plot the outcome as a function of the electoral result and estimate how the expected value of the outcome changes at the cut-point using local linear regression, as is often done for RD designs. An advantage with using this approach is that it makes it possible to visualize how outcomes change at the cut-point.

Design Checks

Our research design rests on one main assumption, necessary for internally valid estimates: the outcomes of the close elections considered in this study are as-if random. For example, the design would be invalid if any candidates could precisely manipulate their vote shares around the cut-point, such as by counting the votes and adding just enough to win. This assumption should be valid for democracies provided that elections are fair (Eggers et al. 2015).

A second “representativeness” assumption facilitates generalizing from our results, and this is that the democracy-years experiencing close elections are not dissimilar to democracy-years in which elections are not close. If this assumption is reasonable, then we can generalize from our results to all democracy years. However, if the countries that had close elections are not representative of other democracies, then the causal estimates that we find may not reflect broader patterns in international relations.

We can test the as-if randomness assumption in two ways. First, we can check that the samples are balanced on important pre-treatment characteristics. Figure 1 plots the balance using two-sided t-tests. The graph on the left shows that countries where right-wing parties barely won were very

similar to countries where left-wing parties barely won, and the graph on the right shows that countries where incumbent parties barely won were similar to countries where challenger parties barely won. In Figure 1, we look at 24 covariates, and not a single one is significantly imbalanced. Thus, the data is consistent with the assumption that who won these close elections was as-if random.

[Figure 1 about here.]

Second, we can test whether there is balance in the number of cases on either side of the cut-point. Figure 2 shows how close right-wing and incumbent parties were to winning the presidency. For the 29 close elections between right-wing and left-wing parties, there were 16 cases where the right-wing party won and 13 cases where the left-wing party won ($p=0.71$). Similarly, for the 36 close elections between incumbent and challenger parties, there were 17 cases where the incumbent party won and 19 cases where the challenger party won ($p=0.87$). Thus, there is no evidence of sorting in either sample.

[Figure 2 about here.]

We can also evaluate the external validity assumption by comparing the two samples to the broader population of all democracies since 1815. Figure 3 uses box-plots to compare our samples to the broader population with respect to covariates related to military power. The comparisons show that our samples are very similar to the broader population of countries from 1815-2010. Thus, at least with respect to these covariates, there is little reason to believe that our samples consist of an idiosyncratic group of countries that would behave differently than most other democracies. Rather, the representativeness of our sample indicates that our results should be indicative of broader trends in international relations.

[Figure 3 about here.]

In sum, the outcomes of the close elections appear to be random, and the countries where the close elections happened are fairly representative of all other democracies. Therefore, the design appears to have worked very well. In the next two sections, we will look at how electing presidential candidates from different parties affects state aggression using this new empirical approach.

Results for Party Ideology

[Table 1 about here.]

Our results suggest that right-wing parties tend to be more aggressive than left-wing parties. Table 1 shows the aggression levels of the countries that had close elections between right-wing and left-wing candidates. On average, the countries where right-wing parties barely won started 0.06 more disputes per year than countries where left-wing parties barely won. Similarly, they engaged in 0.10 more high-level disputes per year than countries where left-wing parties barely won. Given that the average duration of a presidential term for these countries is 4 years and 169 days, this adds up to 0.32 more disputes initiated and 0.43 more high-level disputes initiated over an average presidential term.

Figure 4 plots the estimates for the two main outcome variables, along with the two other indicators of aggression. The confidence intervals are based on two-tailed t-tests. They suggest that electing right-wing parties does increase state aggression, particularly when it comes to high-level disputes. However, all of these confidence intervals cover zero, so we cannot rule out zero effect with 95% confidence. The estimate most different from zero is of high-level disputes initiated ($p = 0.25$). For disputes initiated, the results are more consistent with no effect ($p = 0.64$), as are the results for the supplemental tests of all disputes and all high-level disputes.

[Figure 4 about here.]

Although we cannot confidently conclude that electing leaders from right-wing parties increases state aggression, we can rule out some possible effect sizes. That is, we can confidently reject the hypothesis that electing candidates from right-wing parties leads to a very large increase in high-level dispute initiation ($1.2 \cdot \text{SD}$; $p = 0.030$). We can also confidently reject the hypothesis that electing left wing parties leads to an increase in high-level dispute initiation that is very large ($1.2 \cdot \text{SD}$; $p = 0.0002$), large ($0.8 \cdot \text{SD}$; $p = 0.002$), or medium-sized ($0.5 \cdot \text{SD}$; $p = 0.016$). Thus, this analysis does allow us to draw some strong conclusions about how electing parties with right-wing or left-wing ideologies affects state aggression.

Looking at countries where right-wing parties barely won, one potential concern is that the U.S. (2001) is an outlier, and this might be driving the results for high-level disputes. However, the estimates are very similar if we look at whether countries initiated any high-level disputes (No=0, Yes=1). This test is insensitive to outliers. Electing right wing parties appears to increase the chances that countries will initiate at least one high-level military disputes by 17% ($8\% \rightarrow 25\%$, $p = 0.21$).

Moreover, if we turn our attention to the countries where left-wing parties barely won, the only high-level dispute that any of these countries initiated is questionable and should probably be excluded. This dispute was between Costa Rica and Nicaragua in 1995, and it did not involve any military action by either country. Costa Rican police crossed the Nicaraguan border in pursuit of suspects and were arrested. Two days later, the Costa Rican police force retaliated by arresting two Nicaraguan police officers who had crossed the border “to get a drink of water.” The two sides made a prisoner swap on the following day. If this case is dropped, then electing right-wing parties appears to lead to a 25% increase in dispute initiation ($0\% \rightarrow 25\%$, $p = 0.041$).⁹

Given the number of democracies in the world today, there may be enough close elections to get

⁹All of the cases of high-level military disputes initiated by right-wing governments involved unequivocal uses of military force. A list of all Militarized Disputes included in the data, alongside their codings and descriptions of the recent cases, including the Costa Rica / Nicaraguan border crossing, can be found in the Online Appendix.

much more precise estimates a decade or two from now, or maybe even after the next expansion of the Militarized Interstate Dispute dataset. This design is definitely worth returning to in the near future. However, to make progress in the present, we turn to a second test in the next section on more data, yielding increased statistical power. This test provides more conclusive evidence that which party controls the presidency does affect the likelihood of state aggression.

Results for Incumbent vs. Challenger Parties

The second test that we run compares cases where challenger parties barely defeated incumbent parties to cases where they barely lost to incumbent parties. In these cases, it was as-if random whether the incumbent or challenger party won. Thus, we can test how much military aggression changes when the party that controls the executive branch changes. The outcomes that we use for this test are the absolute changes in the military indicators between the term when the incumbent or challenger party barely won and the previous term. For this analysis, we use one-sided tests that assume that there will tend to be a larger change in military aggression when the challenger party barely wins.

[Table 2 about here.]

Table 2 shows the absolute change in aggression levels for the countries that had close elections between candidates from incumbent and challenger parties. When the candidates from challenger parties barely won, the absolute change in disputes initiated per year was 0.031 greater than when candidates from incumbent parties barely won ($p=0.30$; 26% increase from baseline). For high-level disputes, the difference is even more notable. The absolute change in high-level disputes initiated per year was 0.074 greater than when candidates from incumbent parties barely won ($p=0.046$, 133% increase from baseline). The average length of the presidential terms for this data was 4.42 years, so this adds up to a difference of 0.14 disputes initiated and 0.33 additional high-level disputes initiated

per presidential term.

Figure 5 plots the confidence intervals for the aggression indicators. This estimated effect is substantively large relative to other determinants of conflict that international relations scholars have analyzed. For example, past studies have found that revolutions increase the likelihood that countries will initiate military disputes by about 74% (Colgan 2010), arms transfers by about 60% (Krause 2004), and neutrality pacts with potential conflict joiners by about 57% (Leeds 2003). The effect of challenger parties winning appears to be in the ballpark of these estimates, although it is hard to nail down this effect very precisely because of the relatively small sample size.

[Figure 5 about here.]

Figure 6 illustrates the effect for high-level disputes across a greater range of margins of victory. As countries move from incumbent candidate victories (the blue points on the left) to challenger party victories (the red points on the right), there is a large shift in the absolute change in high-level disputes initiated. Countries where the challenger party barely won experienced a much larger change than countries where the incumbent party barely won. Although this method of estimating the treatment effect was not the primary method that we discussed in our pre-analysis plan, the results for this approach are fairly conclusive.

[Figure 6 about here.]

Moreover, this effect is primarily explained by a tendency for countries to become more aggressive following close wins by candidates from challenger parties. On average, countries where challenger parties barely won initiated 0.26 disputes in the first year that the new leader was in office, compared to 0.00 disputes for countries where incumbent parties barely won (two-sided p-value=0.021). Moreover, the countries where challenger parties barely won initiated an average of 0.21 high-level military disputes in the first year of the new presidential term, compared to 0.00 high-level disputes

for countries where incumbent parties barely won (two-sided p-value =0.042). These results are summarized in Figure 7.

[Figure 7 about here.]

This evidence is consistent with the theory that major leadership transitions increase the likelihood of state aggression, since new leaders will be less experienced at managing crises and may have more incentive to signal their toughness to domestic and foreign audiences (Paper # 1; Potter 2007; Wolford 2007; Dafoe 2012). The main differences between our results and past studies are that (1) we focus on a small subset of cases (n=36) where who became the leader was plausibly random and (2) we look at incumbent parties rather than just incumbent candidates. Only 12 of our 36 cases had incumbent candidates running. In the remaining cases, the incumbent party ran a new candidate. Unfortunately, 12 cases is too small a sample size to narrow our focus to cases where incumbent candidates ran against challenger candidates (p=0.39 for both all disputes initiated and high-level disputes initiated for this very small sample). However, as we discuss in the theory section of this paper, it is reasonable to think that leaders from challenger parties will have more to prove when they first come to office than new leaders from incumbent parties. Our data provides very strong support for that theory.

Conclusion

This study used regression discontinuity to investigate how electing leaders from different parties affects state aggression. We find that what party a leader comes from does influence the likelihood that countries will initiate high-level military disputes against other states. These results may be partly predicted by the left-right divide, although we currently lack enough data to say so conclusively. What we can say, however, is that party-control of the presidency does influence state aggression.

Specifically, leaders from challenger parties tend to be much more likely to initiate military disputes when they first come to office. Thus, the forces of the international system do not constrain leaders to a single course of action, and leaders that appear different on the campaign trail probably will behave differently once they are in office.

In one sense, however, the results presented here also understate the influence of leaders and parties on international policy. Parties often disagree as much on the appropriate targets of force as on the appropriate levels and instances to employ it. The leading political parties in the United States in the early part of the 19th century, for instance, disagreed about whether the country was most threatened by France or by Great Britain when these countries were engaged in the Napoleonic wars. One U.S. party fought the Quasi-War against France, but when the other party came into power, it prosecuted the War of 1812 against Britain (Hickey 1989; Levy and Mabe 1999; Trager 2004). These differences in foreign policy approach were passionately expressed in the party politics of the day, but such dynamics are not captured in this study, which focuses only on differences in levels of aggression, not on differences of foreign policy approach.

One major question that our research design does not answer is why right-wing leaders appear to behave more aggressively than left-wing leaders. It may be due to differences in party ideology or parties' constituencies or the characters of their leaderships at particular times and places. Some constituencies may be more vengeful than others (Stein 2015) or participate to a greater degree in an honor culture (Dafoe and Caughey 2016). Understanding which processes, among the vast arrays of possibilities, produce these persistent differences between parties is an ongoing topic of research in the field.

Thus, if these results on ideology are confirmed, an important next step is to investigate what factors explain why right-wing parties are more aggressive internationally. Domestic political ideology is a strong possibility, but other factors are worth considering. Conducting these tests will be easier

in the future because there will be more close presidential elections to look at. The large number of democracies in the world today make it very likely that this design will lead to other important empirical findings in the coming decades.

Another possible extension of this project would be to look at how party control of the presidency and leadership turnover affect other outcomes, like alliances, trade, and participation in international institutions. Since some of these outcomes are more fine-grained than militarized interstate disputes, it is likely that even more precise estimates will be possible. Most countries do not initiate any militarized interstate disputes during a given presidential term, so our outcome variable had many zeros, which decreases statistical power. The fact that we obtained informative results should encourage optimism about the promise of using close presidential elections to shed light on other important questions about international relations in the future.

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Figure 1. Balance Tests

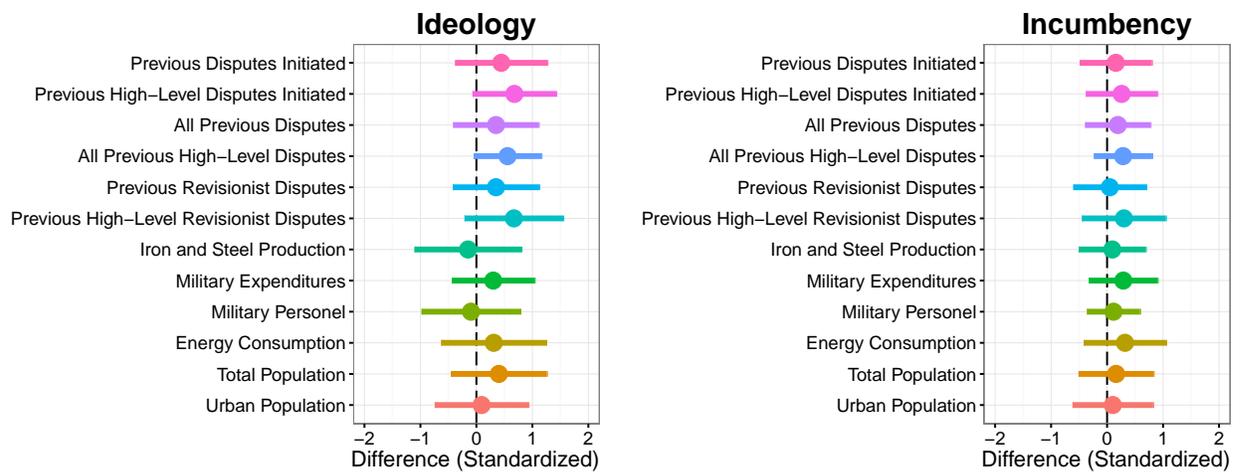


Figure 2. Distribution of the Forcing Variable

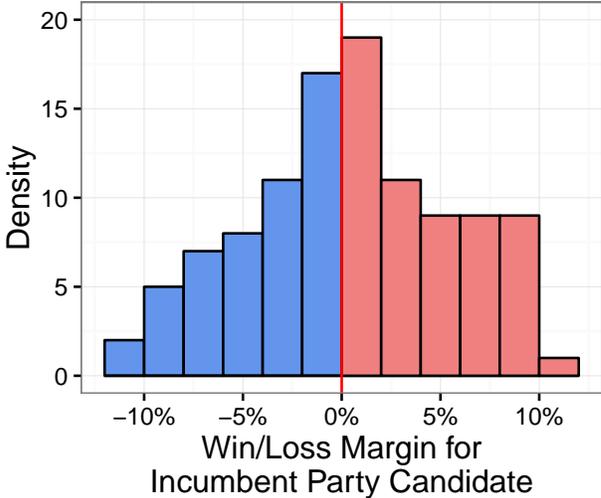
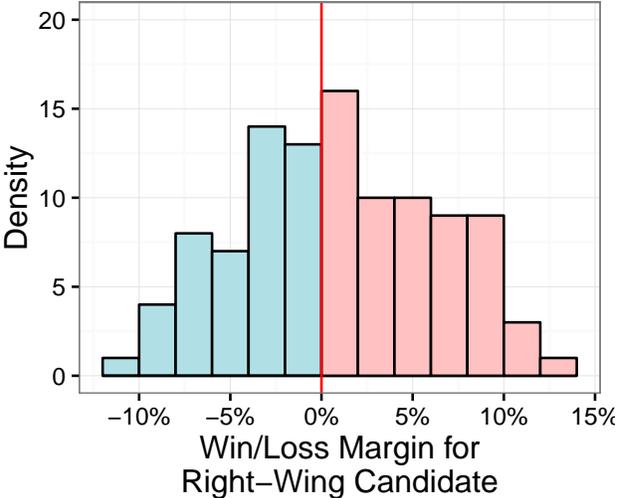


Figure 3. Checking for External Validity

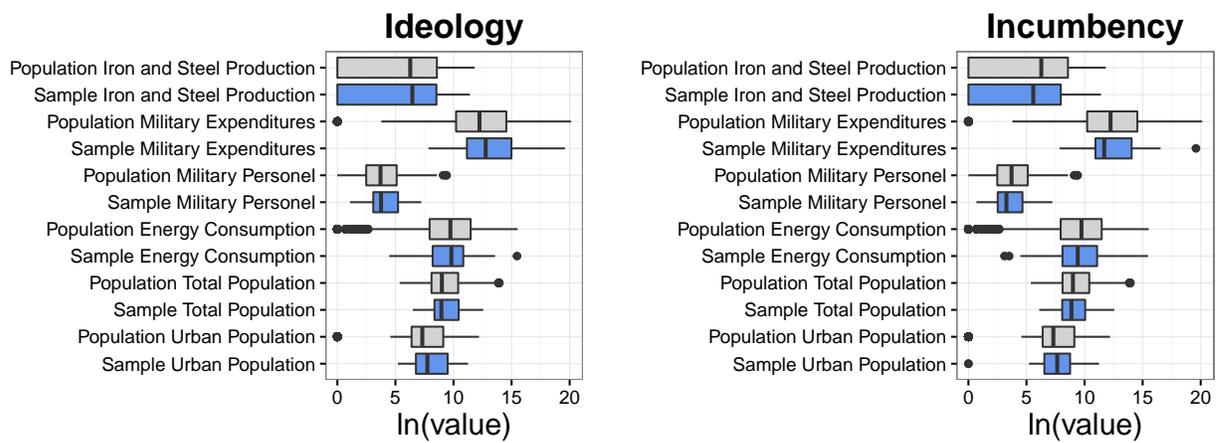


Table 1. Disputes per Year for Countries with Close Elections Between Candidates from Right-Wing and Left-Wing Parties

<u>Cases Where the Left-Wing Party Barely Won</u>				<u>Cases Where the Right-Wing Party Barely Won</u>			
<u>Country</u>	<u>Year</u>	<u>All Military</u>	<u>High-Level</u>	<u>Country</u>	<u>Year</u>	<u>All Military</u>	<u>High-Level</u>
		<u>Disputes</u>	<u>Military Disputes</u>			<u>Disputes</u>	<u>Military Disputes</u>
		<u>Initiated</u>	<u>Disputes</u>			<u>Initiated</u>	<u>Disputes</u>
1. Austria	1957	0.33	0.00	1. USA	1877	0.50	0.00
2. Austria	1965	0.00	0.00	2. Finland	1956	0.00	0.00
3. Austria	1974	0.00	0.00	3. Costa Rica	1958	0.00	0.00
4. Colombia	1978	0.25	0.00	4. France	1974	0.43	0.14
5. Portugal	1986	0.00	0.00	5. Dominican Rep.	1986	0.25	0.00
6. Cyprus	1988	0.00	0.00	6. Cyprus	1993	0.20	0.20
7. Colombia	1994	0.00	0.00	7. Dominican Rep.	1994	0.00	0.00
8. Costa Rica	1994	0.25	0.25	8. Dominican Rep.	1996	0.00	0.00
9. South Korea	1997	0.20	0.00	9. Israel	1996	0.33	0.00
10. Taiwan	2000	0.75	0.00	10. Madagascar	1996	0.00	0.00
11. South Korea	2002	0.26	0.00	11. Colombia	1998	0.25	0.25
12. Taiwan	2004	0.50	0.00	12. Costa Rica	1998	0.00	0.00
13. Finland	2006	0.00	0.00	13. Cyprus	1998	0.00	0.00
				14. USA	2001	2.25	1.25
				15. Mexico	2006	0.00	0.00
				16. Mongolia	2009	0.00	0.00
		<u>Avg=0.20</u>	<u>Avg=0.02</u>			<u>Avg=0.26</u>	<u>Avg=0.12</u>

Figure 4. Testing How Barely Electing Right-Wing Leaders Affects Military Disputes Initiated per Year

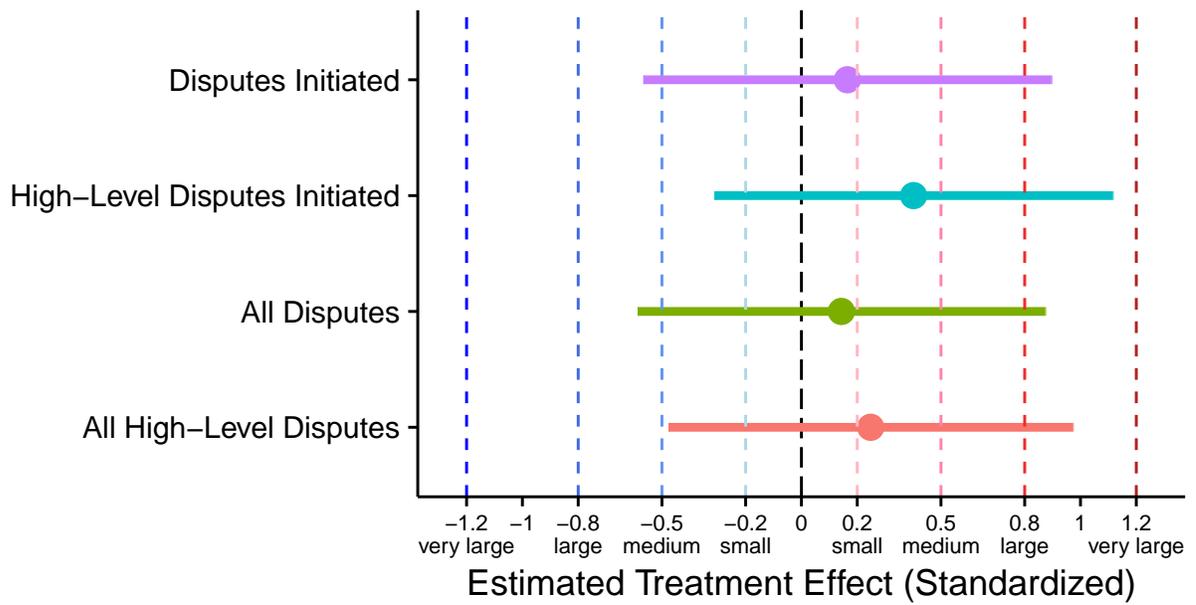
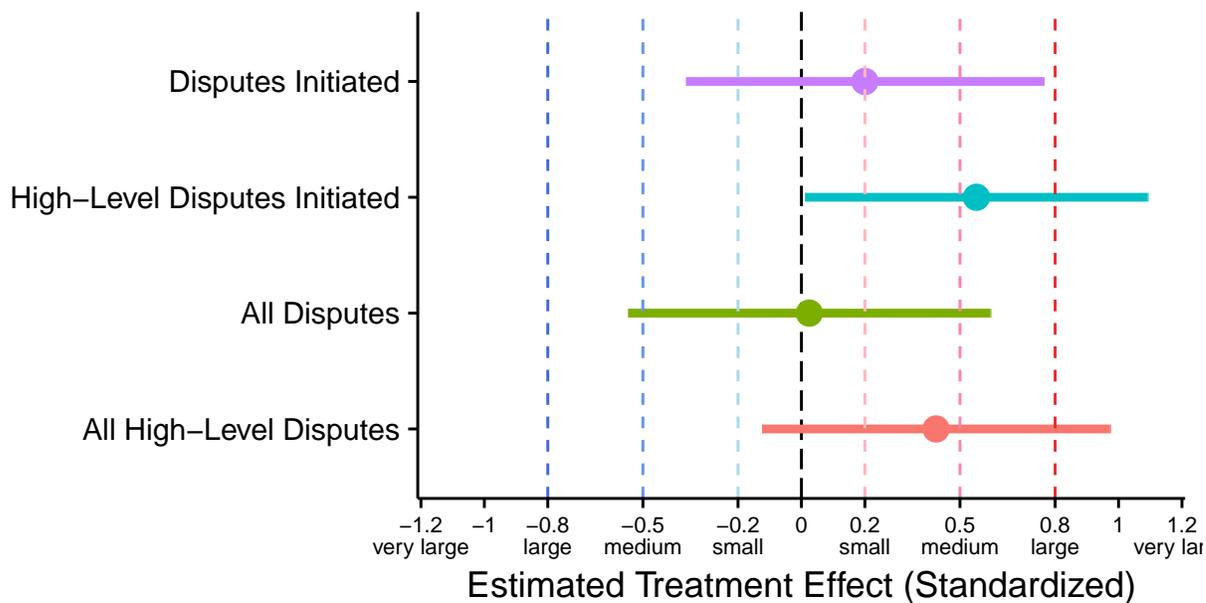


Table 2. Disputes Per Year for Countries with Close Elections Between Candidates from Incumbent and Challenger Parties

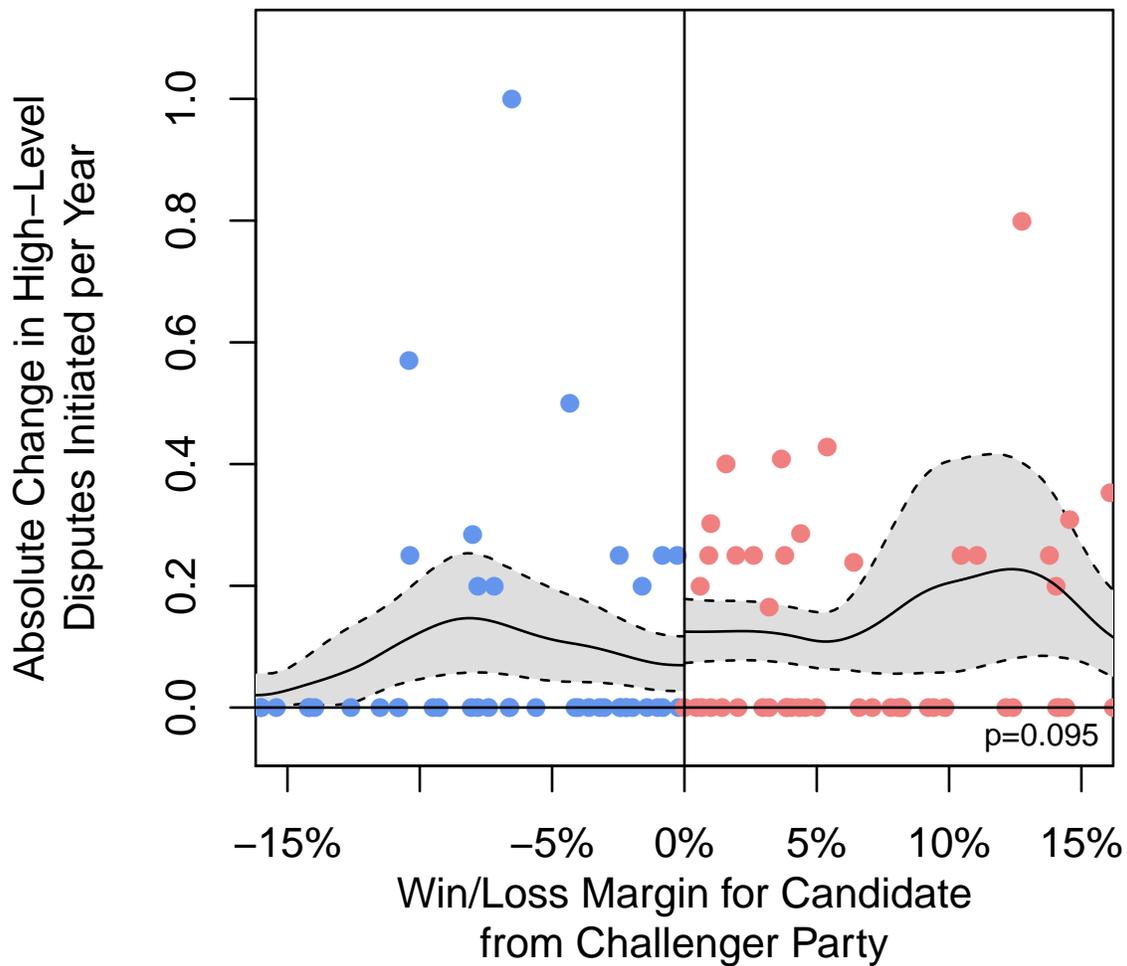
Cases Where Incumbent Parties Barely Won				Cases Where Challenger Parties Barely Won			
Country	Year	Δ in Military	Δ in High-	Country	Year	Δ in Military	Δ in High-
		Disputes Initiated	Level Disputes Initiated			Disputes Initiated	Level Disputes Initiated
1. USA	1877	0.50	0.25	1. Germany	1925	0.19	0.17
2. Austria	1957	0.33	0.00	2. Finland	1956	0.00	0.00
3. Austria	1965	0.00	0.00	3. Costa Rica	1958	0.25	0.00
4. Ireland	1966	0.00	0.00	4. Venezuela	1968	0.60	0.40
5. Ireland	1973	0.00	0.00	5. Venezuela	1978	0.41	0.41
6. Colombia	1978	0.25	0.00	6. Dominican Rep.	1986	0.00	0.00
7. Portugal	1986	0.00	0.00	7. Cyprus	1988	0.00	0.00
8. Dominican Rep.	1990	0.00	0.25	8. Cyprus	1993	0.20	0.20
9. Colombia	1994	0.00	0.00	9. Costa Rica	1994	0.25	0.25
10. Dominican Rep.	1994	0.25	0.25	10. Israel	1996	0.00	0.30
11. Cyprus	1998	0.20	0.20	11. Madagascar	1996	0.00	0.00
12. Finland	2000	0.00	0.00	12. South Korea	1997	0.20	0.00
13. South Korea	2002	0.20	0.00	13. Colombia	1998	0.25	0.25
14. Taiwan	2004	0.25	0.00	14. Costa Rica	1998	0.25	0.25
15. Cape Verde	2006	0.00	0.00	15. Cape Verde	2001	0.00	0.00
16. Finland	2006	0.00	0.00	16. USA	2001	0.50	0.25
17. Mexico	2006	0.00	0.00	17. Honduras	2005	0.00	0.00
				18. Ghana	2008	0.00	0.00
				19. Mongolia	2009	0.00	0.00
		<u>Avg=0.12</u>	<u>Avg=0.056</u>			<u>Avg=0.15</u>	<u>Avg=0.13</u>

Figure 5. Testing How Barely Electing Challenger Parties to the Presidency Affects the Absolute Change in Military Disputes Initiated per Year



Note: Since our test for this section are one-sided, we use 90% confidence intervals for this graph.

Figure 6. Regression Discontinuity Graph for Incumbency



Note: The confidence intervals were computed using non-parametric bootstrapping. The bandwidth for this graph ($h=6.2\%$) was selected using the algorithm provided by Calonico, Cattaneo, and Titiunik (2014).

Figure 7. Testing How Barely Electing Challenger Parties to the Presidency Affects State Aggression in the First Year of the New Term

