

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

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Abstract

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 electing right-wing candidates increases state aggression. We also find 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. This result is consistent with other studies that find that the likelihood of state aggression increases following major leadership transitions.

Keywords

political leadership, interstate conflict, foreign policy, militarized interstate disputes, domestic politics

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Is the likelihood that a democracy will take military action against other countries largely influenced by which party controls the presidency? Many believe so (Palmer, London, and Regan 2004; Arena and Palmer 2009; Clare 2010). 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; Trager and Vavreck 2011; Gries 2014). Moreover, many believe that Al Gore, had he been elected, would not have invaded Iraq like President George W. Bush did (Jervis 2003; Lieberfeld 2005), and that the foreign policies of Hillary Clinton and Donald Trump would be similarly opposed (Paletta 2016).

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 decrease 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 (RD) 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 are 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.

We run two main analyses. First, we look 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. Second, to increase our statistical power, we examine cases where candidates from incumbent parties barely won or barely lost

to candidates from challenger parties ($n = 36$). Specifically, we test whether countries experienced a larger change in their propensity to engage in 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 involvement. We find statistically significant evidence that electing candidates from challenger parties causes countries to experience a larger change in their propensity to engage in military conflict with other states.

Upon further examination of the data, we find that the results from our second test 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 by acting tough.

This article makes several important contributions to the study of international relations. First, there is a long-standing debate in political science over whether leaders have an important independent impact on interstate conflict or whether their influence is largely constrained by strategic realities (Byman and Pollack 2001; Mearsheimer and Walt 2003; Jones and Olken 2009; Chiozza and Goemans 2011; Saunders 2011; Horowitz, Stam, and Ellis 2015; Croco 2015). This study provides quasi-experimental evidence that leaders do have a meaningful impact on foreign policy. Second, the results presented here suggest 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). The findings presented here support that hypothesis by showing that left-wing leaders do tend to behave more dovishly in international affairs. Third, these results suggest that we should be alert to the potential for interstate conflict when right-wing leaders are in office, as well as after elections where party control of the presidency changes hands.

This study is also notable because it is one of the first in the international relations literature to use a preanalysis plan. Prior to looking at any of the results, we pre-registered the main tests that we planned to conduct in this article. 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 are most “interesting” 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 preanalysis plans is to help ensure that research remains credible.

The article proceeds as follows. We first discuss the theoretical bases for the claim that party control of the presidency influences conflict decisions and review the existing 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, much debate has arisen over whether leaders influence the chances of interstate conflict, and if so, how. A major question in this research program is whether leaders from certain parties are more likely to behave aggressively in foreign affairs or whether the ideology of the leader is largely unrelated to state behavior.

The theory that party control of the presidency influences the chances of interstate conflict can be derived from three premises. The first is that conservatives and liberals hold different views about the legitimacy or efficacy of military force. This assumption is backed by cross-national survey data showing that liberals tend to be more concerned with fairness, duties of care, and preventing harm, while conservatives tend to favor the preservation of social orders, the purity of sanctified objects, and loyalty to in-groups (Graham, Haidt, and Nosek 2009; Boer and Fischer 2013). Several studies have also found that these differences in moral foundations influence foreign policy attitudes (Schwartz 1992; Kertzer et al. 2014; Kertzer and Rathbun 2015). In particular, liberals are more “prosocial” and seek compromise internationally, in contrast to conservatives, who are more “proself” and therefore bargain more aggressively (Schwartz, Caprara, and Vecchione 2010).

The second assumption is that general differences in party attitudes appear at the elite level. There are two ways that these differences could affect the behavior of political elites. First, the political leaders could sincerely hold beliefs and preferences similar to those of their constituents, leading them to have different foreign policy strategies and goals. Alternatively, the leaders could have different beliefs and attitudes than their constituents, but nonetheless recognize that they must carry out their supporters’ agenda if they hope to stay in office.

Although it is difficult to know the extent to which leaders’ true foreign policy preferences reflect those of their constituents, several observational studies show that changes in a leader’s base correlate with changes in their approach to international affairs. First, Mattes, Leeds, and Carroll (2015) find that changes in the supporting coalitions of leaders predict foreign policy change, measured by the policy positions taken by nations in the United Nations General Assembly. Rathbun (2004) and Haas (2005) come to a similar conclusion looking at support for peace-enforcement missions, and Solingen (2009) finds that economic interests and the ideologies of partisan coalitions influence nuclear weapons policy. Therefore, even when a leader has different foreign policy beliefs and goals than the rest of the party, there may still be pressure to toe the party line.

The third assumption is that leaders from different parties can act on their divergent preferences. This means that international and domestic constraints on leaders

cannot be so powerful that they largely limit leaders to a single course of action. For example, some realists argue that there is little room for leaders to have an independent impact on foreign policy because they all need to defend and advance the national interest (Mearsheimer 2001; Mearsheimer and Walt 2003). Regarding domestic constraints, Trager and Vavreck (2011) find that right-wing and left-wing leaders can have incentives to hide their “types.” Liberal leaders may be forced to adopt more hawkish foreign policies because they fear that their moderation will sometimes be interpreted as weakness (Schultz 2005), whereas conservative leaders may have incentives to adopt more moderate policies because the public would likely judge them unduly aggressive if they acted hawkishly. Thus, leader preferences and political incentives could actually push in opposite directions.

Several previous studies have examined whether right-wing leaders tend to behave more aggressively in foreign policy than left-wing leaders. Using logistic regression on panel data covering eighteen parliamentary democracies from 1949 to 1992, Palmer, London and Regan (2004) find that right-wing governments are more likely to be involved in military disputes, while left-wing governments are more likely to see the disputes in which they are involved in escalate. Their explanation is that right-wing parties favor using force more often, so their leaders will engage in military conflict more often. However, when left-wing leaders engage in conflict, they will need to emerge victorious to justify their involvement, so they will be more likely to bargain tough and escalate if necessary. These researchers find that a shift from left to right government increases the chances of dispute initiation by about 50 percent and that left-wing governments are about twice as likely to escalate conditional on being in a dispute. Second, Arena and Palmer (2009) apply a probit model to panel data covering twenty stable democracies from 1960 to 1996 and find that right-wing governments are more likely to initiate disputes. Their theory is based on the finding that right-wing leaders are less likely to be removed from office for using force unwisely than left-wing leaders. This makes right-wing leaders more likely to start international conflicts in the hopes of increasing their domestic support. Third, Clare (2010) applies logistic regression to twenty parliamentary democracies from 1950 to 1998 and finds that parliamentary democracies are about twice as likely to initiate disputes when they are controlled by right-wing parties.

The central limitation of these studies is that their conclusions rest on the results of regression analysis on cross-national panel data. Such an approach is not guaranteed to eliminate bias from omitted variables. In fact, the results from this type of analysis can be badly biased, even when researchers control for a wide range of important covariates (Clarke 2005). In some cases, controlling for potential confounders can even amplify bias (Pearl 2013). Thus, the results from these past studies should be interpreted as a tentative first cut at answering this question rather than the final word on the subject.

The design-based approach that we employ in this article gets around the omitted variable bias problem because the as-if random assignment of leaders to office should create balance across observable and unobservable pretreatment

characteristics. In many other scientific fields, the results of conventional observational analyses have been overturned by design-based studies. For example, the validity of hormone replacement therapy and a variety of theories in development economics, psychology, and elsewhere have been overturned when experimental and quasi-experimental approaches were brought to bear (Women's Health Initiative 2002; Freedman 2009; Dunning 2012). Therefore, the tests that we present in this article provide an important step forward in our understanding of the empirical relationship between party control of the presidency and interstate conflict.

Before moving on to our research design, though, we should first lay out the hypotheses that we want to test. As we detail in our preanalysis 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. Given this prior, we formulated two main 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 question of whether leaders matter in international relations. 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, making it more likely that disagreements with other states will turn into military conflicts (Potter 2007). Second, new 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 (Wolford 2007; Dafoe 2012). Third, new 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. This idea that leaders are more likely to get involved in military disputes when they first arrive in office has received support from cross-national logistic regression analysis on panel data (Gelpi and Grieco 2001) and a mixed-methods analysis that looks at American presidents (Potter 2007).

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, Leeds, and Matsumura 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.

We did not preregister the *challenger aggression hypothesis* prior to looking at the results, but this was the only hypothesis we tested outside of those we preregistered. Thus, the findings do not reflect data mining. Nevertheless, some readers may wish to interpret the test of this particular hypothesis as exploratory.

Research Design

There are several different design-based approaches that could be used to investigate how leaders affect state behavior. One would be to look at all cases of leadership turnover and compare how countries behaved before and after the leadership change. This research design rests 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 probably have to deal with a more complicated political situation in the aftermath of the assassination, making the beginning of their term much different from the end of the previous leader's term.

Another 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 environments. 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 could be 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 an RD design. RD 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 fifty or higher got a scholarship, researchers could assess the effects of getting the scholarship by comparing the students who scored fifty and fifty-one to the students who scored forty-eight and forty-nine. So long as there is no sorting at the cut point, as could happen if the graders had opportunity and motive to nudge some test takers above the cut point, 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 RD 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).¹ Political scientists have used RD 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 RD 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 RD specifically to presidential elections. For our analysis, we followed the procedures that were outlined in our preanalysis plan (which is available at the end of the Online Appendix). We will briefly summarize these procedures in the remainder of this section.

Our Statistical Approach

There are two general ways to analyze an RD. The first, known as the continuity approach, involves plotting two smoothing functions on either side of the cut point and estimating the difference at the cut point (Voeten 2014). This method should be used when the score, or "forcing variable," is continuous. The second method is the local-randomization approach, appropriate when the forcing variable is discrete (Lee and Card 2008; Cattaneo, Frandsen, and Titiunik 2015; Bertoli 2017). It involves

drawing a window around the cut point and treating the units within that window like they were in a randomized experiment.

Since the forcing variable in this study is vote share in a presidential election, which is essentially continuous, we would normally use the continuity approach. However, we discovered in our preanalysis plan that the continuity approach had a type 1 error rate (false-positive rate) of 12 percent for this study, which we believe is due to our small sample size. Since the type 1 error rate should be 5 percent by design, we chose not to use this method, since it was overly likely to give us significant results. Instead, we used the local-randomization approach, which we found had a type 1 error rate of about 4 percent.

Defining Close Elections

We considered elections to be close if the top two candidates were within 2 percent of the cut point (48 percent to 52 percent range). Data on close races were available in the data set constructed by Bertoli, Dafoe, and Trager (2018). This data set includes every democratic election between 1815 and 2010, where democracies are defined as countries with Polity IV Institutionalized Democracy scores above five. The data set provides information on the top two candidates including their names, parties, and vote shares in the election. If there were more than two candidates running in an election, we focused only on the votes for the top two candidates, rescaling their vote shares accordingly. In cases where there were runoffs, we used their vote shares from the runoff rather than the initial election. We also excluded close elections in nondemocracies 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 that arose is that the United States 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 (Bertoli, Dafoe, and Trager 2018). 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 postcommunist 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 the ideology test.

Main Analyses

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 twenty-nine 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 thirty-six of these close elections in our data set. 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.

Although our sample sizes are not large, the power tests that we ran at the beginning of this project indicated that we had a good chance of picking up a medium-sized or large effect. For the test of left- versus right-wing parties, we determined we would correctly detect (at $\alpha = .05$) a medium-sized effect (0.5 standard deviation [*SD*]) 30 percent of the time, a large effect (0.8 *SD*) 54 percent of the time, and a very large effect (1.2 *SD*) 82 percent of the time. In the incumbency power analysis, we found that we would detect a medium-sized effect 55 percent of the time, a large effect 93 percent of the time, and a very large effect over 99 percent of the time. Also, if the effects were small or nonexistent, the power tests indicated that we would be able to establish confidence intervals that were precise enough to rule out very large (± 1.2 *SD*) positive and negative effects.²

Moreover, although the results turn out to be significant at conventional levels, we encourage readers to avoid interpreting *p* values as either significant ($p < .05$) or not while reading this article and to bear the bias-variance trade-off in research design in mind. Almost all quantitative 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 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 actually reflect more evidence against the null than a lower *p* value in a biased one. Small *p* values

(e.g., $p < .2$), even if not significant at conventional standards, also provide important evidence in these contexts.

In addition to our two main tests, we 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. Our motivation for running this test came from reading Wolford (2007), Dafoe (2012), and Wu and Wolford (2016). 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:

$$\text{Absolute change in military aggression} = |\text{MIDs/year during winner's term} \\ - \text{MIDs/year during previous term}|$$

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.³ Following the preanalysis plan, we examine high-level disputes, which constitute actual uses of force, separately because the factors that drive posturing may be different from those that drive actual violence. 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 pretreatment 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 twenty-four covariates, and not a single one is significantly imbalanced. Thus, the data are consistent with the assumption that who won these close elections was as-if random.

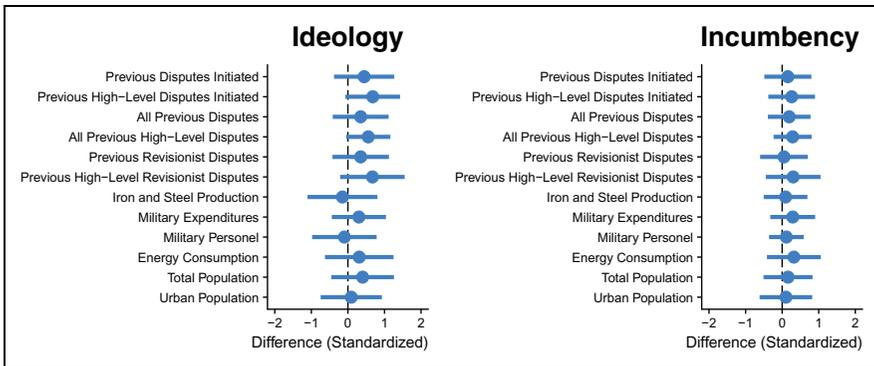


Figure 1. Balance tests.

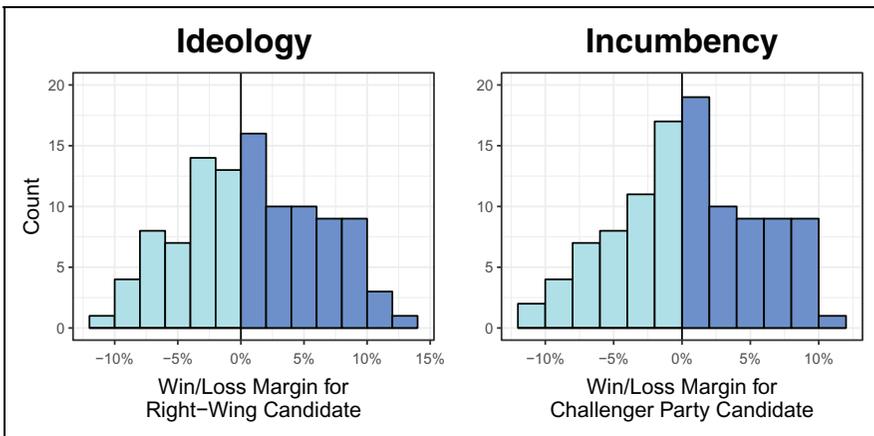


Figure 2. Distribution of the forcing variable.

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 twenty-nine close elections between right-wing and left-wing parties, there were sixteen cases where the right-wing party won and thirteen cases where the left-wing party won ($p = .71$). Similarly, for the thirty-six close elections between incumbent and challenger parties, there were seventeen cases where the incumbent party won and nineteen cases where the challenger party won ($p = .87$). Thus, there is no evidence of sorting in either sample.

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

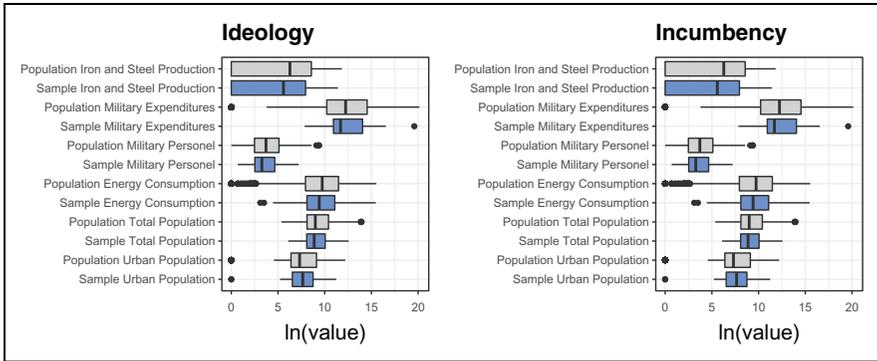


Figure 3. Checking for external validity.

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 democracies from 1815 to 2010. Thus, at least with respect to these covariates, there is little reason to believe that either of our samples consist of an idiosyncratic group of countries that would behave differently than most other democracies. Rather, the representativeness of our samples indicates that our results should be indicative of broader trends in international relations.

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

Our results indicate 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 .06 more disputes per year than countries where left-wing parties barely won. Similarly, they engaged in .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 .32 more disputes initiated and .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. Of course, all of these confidence

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

Cases Where the Left-Wing Party Barely Won				Cases Where the Right-Wing Party Barely Won			
Country	Year	All Military Disputes Initiated	High-level Military Disputes Initiated	Country	Year	All Military Disputes Initiated	High-level Military Disputes Initiated
1. Austria	1957	.33	.00	1. USA	1877	.50	.00
2. Austria	1965	.00	.00	2. Finland	1956	.00	.00
3. Austria	1974	.00	.00	3. Costa Rica	1958	.00	.00
4. Colombia	1978	.25	.00	4. France	1974	.43	.14
5. Portugal	1986	.00	.00	5. Dominican Rep.	1986	.25	.00
6. Cyprus	1988	.00	.00	6. Cyprus	1993	.20	.20
7. Colombia	1994	.00	.00	7. Dominican Rep.	1994	.00	.00
8. Costa Rica	1994	.25	.25	8. Dominican Rep.	1996	.00	.00
9. South Korea	1997	.20	.00	9. Israel	1996	.33	.00
10. Taiwan	2000	.75	.00	10. Madagascar	1996	.00	.00
11. South Korea	2002	.26	.00	11. Colombia	1998	.25	.25
12. Taiwan	2004	.50	.00	12. Costa Rica	1998	.00	.00
13. Finland	2006	.00	.00	13. Cyprus	1998	.00	.00
				14. USA	2001	2.25	1.25
				15. Mexico	2006	.00	.00
				16. Mongolia	2009	.00	.00
		Average = .20	Average = .02			Average = .26	Average = .12

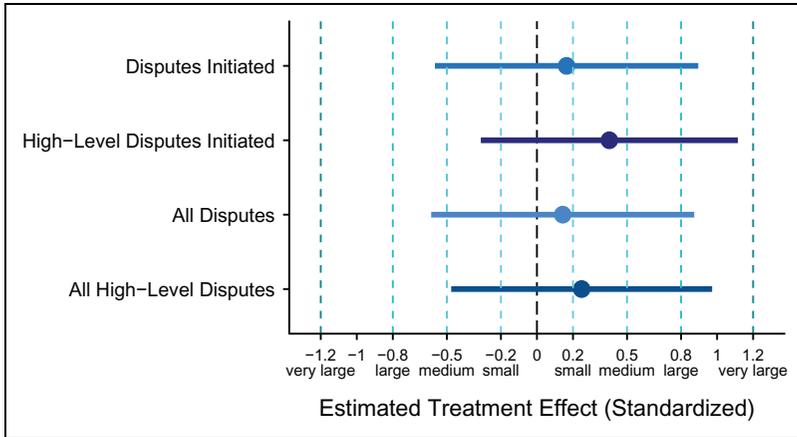


Figure 4. Testing how barely electing right-wing leaders affects military disputes initiated per year.

intervals cover zero, so we cannot rule out zero effect with 95 percent confidence based on this analysis alone. The estimate most different from zero is of high-level disputes initiated ($p = .25$). For disputes initiated, the results appear to be more consistent with no effect ($p = .64$), as do the results for the supplemental tests of all disputes and all high-level disputes.

However, if we look at the specific disputes in more detail, the evidence that electing right-wing leaders increases state aggression grows stronger. While all the high-level disputes that the right-wing leaders engaged in involved unequivocal uses of force, the only high-level dispute that any of the left-wing leaders 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 countries to initiate .12 more high-level disputes per year ($p = .162$).⁴

Moreover, the only reason that these results are not significant is because the United States (2001) is an outlier, which inflates the standard errors. We can address this issue by modifying the outcome to a simple indicator variable for whether countries initiated any high-level disputes ($no = 0, yes = 1$), which makes our test insensitive to outliers. The estimates then suggest that electing right-wing parties increases the chances that countries will initiate high-level military disputes by 25 percent ($p = .041$). Therefore, even though the initial tests were not statistically significant, they become more conclusive after we address some minor issues with the data.

Given the number of democracies in the world today, there may be enough close elections to get much more precise estimates a decade or two from now or maybe even after the next expansion of the MID data set. This design is definitely worth returning to in the near future. However, for the present, we will turn to a second test in the next section on more data that yields increased statistical power. This test provides further evidence that which party controls the presidency does affect the likelihood of state aggression.

Results for Incumbent versus 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 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 .031 greater than when candidates from incumbent parties barely won ($p = .30$; 26 percent increase from baseline). For high-level disputes, the difference is even more notable. The absolute change in high-level disputes initiated per year was .074 greater than when candidates from incumbent parties barely won ($p = .046$, 133 percent increase from baseline). The average length of the presidential terms for these data was 4.42 years, so this adds up to a difference of .33 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 percent (Colgan 2010), arms transfers by about 60 percent (Krause 2004), and neutrality pacts with potential conflict joiners by about 57 percent (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 6 illustrates the effect for high-level disputes across a greater range of margins of victory. As countries move from incumbent party victories (the points on the left) to challenger party victories (the points on the right), there is a large shift in the absolute change in high-level disputes initiated. Countries where the challenger

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 Disputes Initiated	Δ in High-level Disputes Initiated	Country	Year	Δ in Military Disputes Initiated	Δ in High-Level Disputes Initiated
1. USA	1877	.50	.25	1. Germany	1925	.19	.17
2. Austria	1957	.33	.00	2. Finland	1956	.00	.00
3. Austria	1965	.00	.00	3. Costa Rica	1958	.25	.00
4. Ireland	1966	.00	.00	4. Venezuela	1968	.60	.40
5. Ireland	1973	.00	.00	5. Venezuela	1978	.41	.41
6. Colombia	1978	.25	.00	6. Dominican Rep.	1986	.00	.00
7. Portugal	1986	.00	.00	7. Cyprus	1988	.00	.00
8. Dominican Rep.	1990	.00	.25	8. Cyprus	1993	.20	.20
9. Colombia	1994	.00	.00	9. Costa Rica	1994	.25	.25
10. Dominican Rep.	1994	.25	.25	10. Israel	1996	.00	.30
11. Cyprus	1998	.20	.20	11. Madagascar	1996	.00	.00
12. Finland	2000	.00	.00	12. South Korea	1997	.20	.00
13. South Korea	2002	.20	.00	13. Colombia	1998	.25	.25
14. Taiwan	2004	.25	.00	14. Costa Rica	1998	.25	.25
15. Cape Verde	2006	.00	.00	15. Cape Verde	2001	.00	.00
16. Finland	2006	.00	.00	16. USA	2001	.50	.25
17. Mexico	2006	.00	.00	17. Honduras	2005	.00	.00
				18. Ghana	2008	.00	.00
				19. Mongolia	2009	.00	.00
		Average = .12	Average = .056			Average = .15	Average = .13

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 preanalysis plan, the results for this approach are fairly conclusive.

After further inspection of the data, we found that 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 .26 disputes in the first year the new leader was in office, compared to .00 disputes for countries where incumbent parties barely won (two-sided p value = .021). Moreover, the countries where challenger parties barely won initiated an average of .21 high-level military disputes in the first year of the new presidential term, compared to .00 high-level disputes for countries where incumbent parties barely won (two-sided p value = .042). These results are summarized in Figure 7.

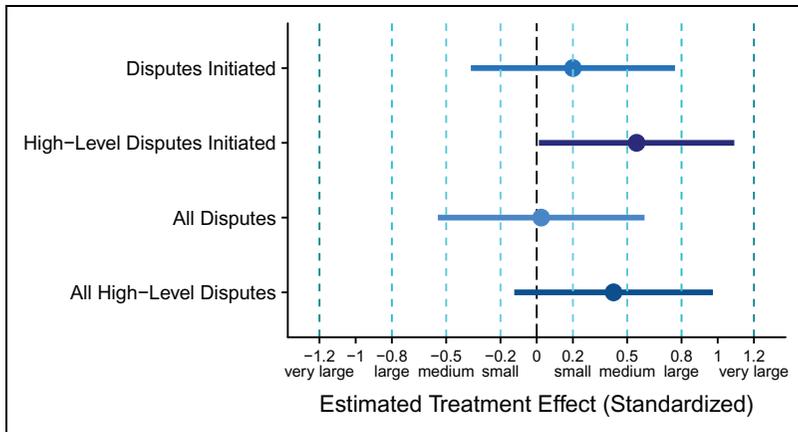


Figure 5. Testing how barely electing challenger parties to the presidency affects the absolute change in military disputes initiated per year.

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

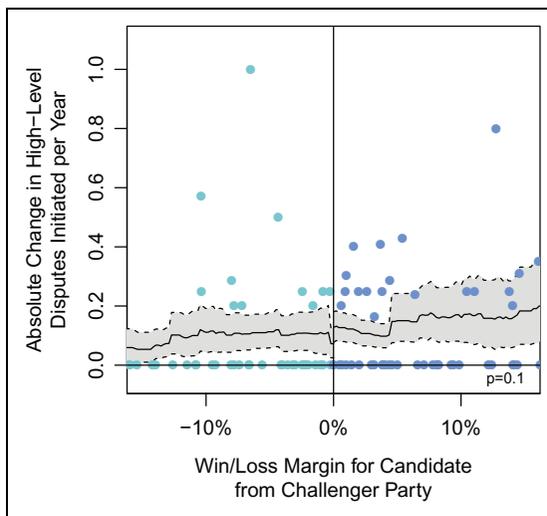


Figure 6. Regression discontinuity graph for incumbency.

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

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

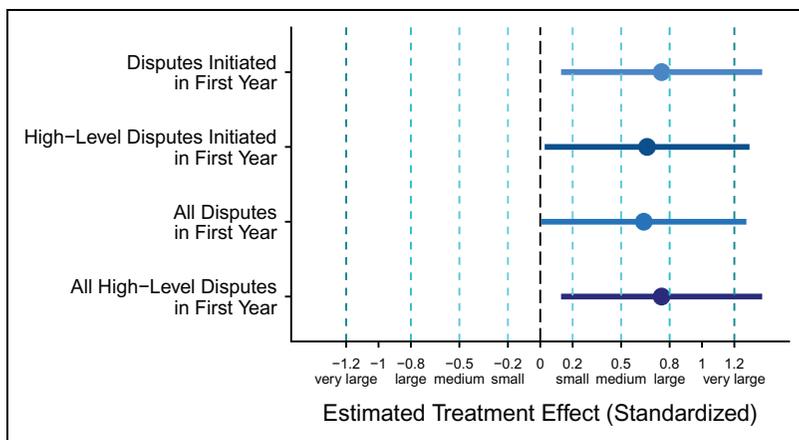


Figure 7. Testing how barely electing challenger parties to the presidency affects state aggression in the first year of the new term.

to domestic and foreign audiences (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 twelve of our thirty-six cases had incumbent candidates running. In the remaining cases, the incumbent party ran a new candidate. Unfortunately, twelve cases are too small a sample size to narrow our focus to cases where incumbent candidates ran against challenger candidates ($p = .39$ 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 article, 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 provide strong support for that theory.

Conclusion

This study used RD 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. Thus, the forces of the international system do not constrain leaders to a single course of action. Rather, leaders seem to have an important independent impact on international affairs.

These findings might appear to contrast with those in Palmer, London, and Regan (2004), who find that right-leaning governments are more likely to initiate disputes while left-leaning governments are more likely to escalate. Our results indicate that which party controls the presidency has little effect when low-level

disputes are included in the analysis, but that right-wing parties are more likely to initiate high-level disputes. One issue here is that Palmer, London, and Regan (2004) define escalation as either more than twenty-five battle deaths or as a case where the target of an initiation takes an action coded at a higher hostility level in the MID data set. Our analysis does not directly speak to the question of escalation in this sense and cannot due to data limitations. We can say only that our findings allow us to rule out more than a small effect of left parties on increased engagement at high escalatory levels.

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 nineteenth century, for instance, disagreed about whether the nation 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 2004; 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. We focus 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 characteristics 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 array of possibilities, produce these persistent differences between parties is an ongoing topic of research in the field.

Thus, an important next step is to investigate which 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 years.

Another possible extension of this project would be to look at how party control of the presidency and leadership turnover affect other outcomes such as alliances, trade, and participation in international institutions. Since some of these outcomes are more fine grained than MID, it is likely that even more precise estimates will be possible. Most countries do not initiate any MID 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.

Authors' Note

The data and replication code for this article are available in the supplemental material and on the authors' websites.

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Supplemental Material

Supplementary material for this article is available online.

Notes

1. Most studies that look at close elections find that it is essentially random who wins and loses. A notable exception is an article by Caughey and Sekhon (2011), which finds that from 1942 to 2008 incumbent candidates in the U.S. House of Representatives tended to win close elections at a disproportionate rate. This finding has not replicated in other settings (Eggers et al. 2015), and it does not hold for our data either. In fact, we find that candidates from challenger parties are more likely to win close elections than candidates from incumbent parties.
2. As 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 ten-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 MIDIs initiated per year in our data (0.23), and then dividing by the *SD* of MIDIs initiated per year in our data.

3. We use the “initiator” variable in the MIDIs data set instead of the “revisionist” variable because the initiator coding, which we preregistered, is less subjective and captures the side that initiates the use of force rather than, as is often the case with the revisionist variable, the side that for many years has arguably been seeking to change the status quo. In 1993, for instance, the United States and South Korea conducted military exercises aimed at North Korea and are therefore coded as initiators of a display of force even though the MIDIs data set codes North Korea as the revisionist state because it is judged to be the more desirous of a change in the *status quo*. The decision to take action by the United States and South Korea, not the long-running stance of North Korea, is what we expect to be influenced by the result of the recent elections. We list all cases and codings in the Online Appendix and present available online the results from a supplemental analysis that recodes one controversial case.
4. 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.

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