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Does Electoral Proximity Affect Security Policy?

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How do approaching elections affect the security policy states conduct? We build on classic political economy arguments and theorize that one problem likely faced by democratic policy makers near elections is that of time inconsistency. The time-inconsistency problem arises when the costs and benefits of policy are not realized at the same time. We develop an application of the argument to the case of allied troop contributions to Operation Enduring Freedom and the International Security Assistance Force mission in Afghanistan. In that case, we argue that the expectation should be one of fewer troops committed close to elections. The exogenous timing of elections allows us to identify the effects of approaching elections on troop levels. Our finding of significantly lower troop contributions near elections is arguably the first identified effect of electoral proximity on security policy.

Political business cycles are studied profusely in economics. For reasons that differ by perspective, manipulating macroeconomic policy close to elections is said to benefit the reelection prospects of incumbents. We build on some of the arguments advanced in the literature for the case of security policy. Specifically, we suggest that many security policies may present a time-inconsistency problem for elites: optimal policies may not be pursued when proximity to elections creates an incentive to show, in a costly manner, the benefits of retaining a particular incumbent leader.

While such arguments have been applied before to security policy, our contribution is to suggest that election incentives differ by the type of security policy. When the benefits of the policy can be realized and revealed to voters quickly, leaders would seek to overinvest in such policies in the run-up to elections. This type of argument is relatively well known. A stylized example is a leader initiating a crisis, or undertaking a quickly winnable war, close to elections. When, however, the benefits of a security policy are only

realized in the long-term, whereas the costs are realized in the short-term, we argue that leaders would have an incentive to underinvest in such policies. Competent incumbents will have an incentive to signal their ability by keeping costs low, while still meeting the larger national security goals. We argue that the case of peacekeeping operations fits the second logic we describe. Democratic leaders have an incentive to underinvest in troop contributions close to elections.

To test our argument, we examine the changes in troops committed to Afghanistan from October 2001 through October 2011, exploiting the leverage provided by a total of 157 elections in 50 contributing states. The exogenous timing of elections in the data allows us to identify the impact of electoral proximity: a fairly substantial, 10%, average drop in the pace of troop contributions in the year preceding the polls.

Our contribution to the literature is threefold. First, our argument is close to the signaling argument developed by Hess and Orphanides's (1995) seminal application of the political business cycle framework to presidential uses of force

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in the US context. But our main prediction is diametrically opposed. While we agree that incumbents seek to demonstrate their ability to conduct security policies more competently than their challengers, it does not need to generally follow that incumbents will always escalate the use of force close to elections. In some cases, we would expect uses of force to communicate precisely the opposite information.

The reason for that has to do with what is observable to voters in the short run. This varies by the type of security operation. In a war of choice, for example, a quick victory can conceivably communicate higher competence in time for voters to decide on reelection. In peacekeeping operations, where victory is elusive and long-term, domestic publics are highly sensitive to casualties, and often the effort is undertaken by a coalition. A troop build up close to elections does not dramatically affect winning the war. It does, however, lead to casualties if violence is expected. Our findings echo earlier work (Gaubatz 1999), although the theoretical motivation, rooted in a signaling argument and a novel distinction, differs.

Second, the evidence we use to identify the effects of approaching elections improves in a number of ways on the current standard. We do not use a disparate group of events, such as militarized interstate disputes (which may be heterogeneous, poorly measured, and strategically timed), but monthly data on troop deployments to the same war theater.

Third, the larger picture of a security policy responding to time-inconsistent incentives, even if it did not ultimately undermine the operation we study, remains a worry. While the existence of elections generates a welcome mean shift toward greater accountability in democratic states, the periodicity of voting creates a secondary problem.

TIME INCONSISTENCY OF SECURITY POLICY AND TROOP CONTRIBUTIONS TO PEACEKEEPING OPERATIONS

The notion that political parties compete over foreign policy and that elections, as a result, may be accompanied by shifts in the security policy of democracies is sometimes taken for granted and, at other times, disputed. Realism, with its minimalist emphasis on fundamentals, remains a good starting point when it comes to setting out some theoretical expectations on the security policy states pursue close to elections. For realist scholars, elites make security policy on the basis of *realpolitik*, developments on the battlefield, or international commitments, even as dissenters have called attention to domestic factors and considerations (James and Oneal 1991; Mearsheimer 2001; Ostrom and Job 1986; Waltz 1979). Moreover, states have long-term obligations to international organizations and alliances that

they feel they must maintain regardless of what domestic public opinion dictates in the short-term. As Sherard Cowper-Coles, the former British ambassador to Afghanistan put the “realist” view, “How would you explain [troop reductions] to our NATO partners? We would do severe, perhaps fatal, damage to the international alliance. No responsible British prime minister could support such a policy” (Stewart and Knaus 2011, 63).

More recently, scholars have applied political economy models and models of elite-led decision making to support the expectation of no variation around elections. Thus, Saunders argues for the US case that commitments to the same fundamental foreign policy interests lead elites from both sides of the aisle to forge a consensus that makes security policy less vulnerable to shifts in public opinion (Saunders 2013).

We argue that incumbents will make preelection policy decisions that they believe will give them an electoral boost. We do not disagree that it is possible in some cases, such as the US case, to achieve a durable pact or a model of foreign policy formation that places security beyond the water’s edge (Gowa 1998). However, we question the degree to which such an outcome may obtain in different areas of security policy and in different cases.

We argue that if the costs and benefits of a policy are realized at the same time, then no reason exists for engaging in opportunistic behavior (adjusting policy strategically before an election). However, when the costs (or benefits) of a policy are paid at time t (i.e., before an election) and the benefits (or costs) occur at time $t + 1$ (i.e., after an election), incumbent politicians have a reason to refrain from (or engage in) the policy in the run-up to the election. This type of time inconsistency is emblematic of the issues facing accountable elites making policy when the costs and benefits of policies do not occur contemporaneously.

The intuition behind our theorizing derives from insights on political business cycles in the political economy literature. According to this logic, incumbent politicians will recognize that proximity to elections influences the optimal policy they should set.¹ Traditionally, economists argued that incumbent governments could engage in inflationary monetary policy before elections in order to lower unemployment (Nordhaus 1975).

Problematically, such an approach assumed that voters are myopic and retrospective, caring only about employ-

1. Empirical evidence remains mixed about whether this phenomenon occurs in the United States (Tufte 1978), developed countries (Alesina and Roubini 1992; Canes-Wrone and Park 2012), or not at all.

ment in the present and not about the deleterious nature of inflation in the long-term. In recent years, scholars have offered two types of rationalist alternatives. On one hand, the “moral hazard” approach suggests that rational audiences might still vote retrospectively for two reasons. First, high growth and low unemployment reflect competence, which voters assume to be lasting and, with regard to monetary policy, fairly static. Second, voters only observe employment and other output during election years; they do not actually observe inflation or policy. Thus, during an election period, voters cannot distinguish between a competent incumbent producing growth and an incompetent incumbent engaging in inflationary monetary policy designed to lower unemployment in the short run (Persson and Tabellini 2002). On the other hand, adverse selection models (also called rational opportunistic models) posit instead that political business/budget cycles are the result of informational asymmetries that exist between governments and voters (Rogoff and Sibert 1988). Incumbents want to signal high competence, defined as the ability to enact policies with the minimal revenue necessary, through monetary policy. However, competent incumbents can send such signals at a lower cost (i.e., lower inflation) than incompetent incumbents. For adverse selection theories, electoral cycles may be a normatively positive phenomenon insofar as they allow competent incumbents to signal their ability.

In this article, we build on these rationalist perspectives to study security policy. We suggest that security policy differs from monetary and fiscal policy in the following ways. First, there exist greater informational asymmetries in security policy than in fiscal policy. As such, elites will be especially concerned with signaling their competence in matters of national security. Critically, even as governments have become more transparent in their dealings in general, national security policy remains tightly guarded. Second, there can be two types of security policies, based on the time-inconsistent distribution of their rewards: one provides benefits in the long-term and costs in the short-term, the other vice versa. Since our signaling argument presents an application of already available political economy formal models such as those cited, we do not offer a formalization here.

We assume that voters, all else equal, prefer a government competent in dealing with matters of national security. To this end, we recognize that governments vary in their degree of competence but argue that incumbents face similar incentives regardless of type. During nonelection years, incumbents have the time to invest in policies that may not yield positive national security benefits immedi-

ately. During election years, however, incentives change and incumbents focus on policies the benefits of which accrue immediately with costs that occur after elections.

The structure of these incentives can explain different predictions in the literature on security and elections and may depend on the security policy in question. For example, office-minded leaders can resort to war-mongering (Smith 1998), while, in a different context, being reticent to send troops to a peacekeeping mission. Winning a war of choice might be beneficial in the short run, with the benefits of saber-rattling realized immediately and outweighing the immediate costs. Increasing contributions to an ongoing peacekeeping mission, however, risks casualties without offering immediately visible payoffs.

In peacekeeping operations, changes in the forces committed to an operation produce relatively few observables in the short-term. Governments enjoy an informational advantage: voters cannot immediately detect whether a policy is successful. Instead, in the short-term, voters must rely on casualties figures. Competent incumbents can largely achieve national security goals with lower numbers and will want to signal their ability by keeping casualties low. Incompetent incumbents, however, will try to imitate this but are unlikely to achieve lower casualties without pushing troop levels to a degree that jeopardizes the operation and invites accusations of mismanagement. While both types will aim to keep casualty figures low before elections, the competent type is more likely to achieve that without inviting allegations of compromising the longer-term security objective. In effect, this results in both types of government to some (and different) degree underinvesting in the type of security policy that pays off only in the long run.²

We agree that it is precisely the elitist or unobservable nature of the benefits of key security policies that can motivate incumbents to signal their ability to deal with them, and we would see the implied tendency to adopt “risk-free” policies as one manifestation of underinvestment in costly policies close to elections. Our work is also in broad agreement with Gaubatz’s pioneering work on electoral proximity and war (Gaubatz 1999). We agree on the fundamentals that war is costly and electorates will try to discourage it. We provide an explanation rooted in rational

2. Our theory also provides a rationale for why an office-minded government might prefer to start larger-scale wars earlier in the electoral cycle (Stoll 1984). Wars that drag on become very costly, electorally and resource-wise, in the long run, even if they had been popular initially. If wars linger, the structure of their electoral payoffs begins to resemble that of peacekeeping missions.

expectations and signaling and, so, not one relying on the short memory of the electorate.³

Our work also speaks to diversionary war theorists about the effects of political competition on security policy. These authors suggest that democratic leaders use war-making as a diversionary political tool to gain votes before an election.⁴ Formal approaches to diversionary war have argued that incumbents may use war to signal foreign policy competence in the run-up to elections (Bueno de Mesquita and Smith 2012). In a recent application, Zeigler, Pierskalla, and Mazumder (2014) focus on term limits to argue that term-limited leaders will be free of the need to appease the median voter and will pursue more wars in their last term in office. We argue that incumbents face different incentives with respect to different security operations and that models should scrutinize these differences.

Finally, our work is closest to the political economy model in Hess and Orphanides (1995), in which voters need to choose whether to retain an incumbent for another period, and the incumbent sometimes pursues (successful) wars of choice to inform voters that he or she could deal well with a war should one arise in his or her next term. We do not see much empirical support in peacekeeping operations for a key stylized fact in that model: increased use of force as an observable signal of the quality of a leader. We thus formulate the following more general hypothesis, which scholars should apply depending on the structure of incentives inherent in the security policy under investigation:

H1. Elections will produce variation in security policy when the costs and benefits of a policy do not accrue at the same time: (i) as elections near, decision makers will underinvest in security policy with short-term costs and long-term benefits; (ii) as elections near, decision makers will overinvest in security policy with short-term benefits and long-term costs.

The problem of election security cycles may be different depending on the specific type of security policy. We focus on one type below, which, for us, is the general problem of

3. Our argument here captures some of the tensions observed in early work by Quandt (1986, 826–27), who wrote: “But there is still a constitutionally rooted problem that seriously affects the conduct of foreign policy. It derives from the structure of the electoral cycle. . . . The price we pay is a foreign policy excessively geared to short-term calculations, in which narrow domestic political considerations often outweighs sound strategic thinking.”

4. Morrow (1991) finds only mixed evidence in favor of the main hypothesis. For a recent treatment, see Tarar (2006).

electoral business cycles in peacekeeping operations.⁵ In the case of troops on a peacekeeping mission, achieving peace is a long-term objective. Individual increases in troop contributions are unlikely to make success dramatically more likely, especially in multinational operations with many contributing states. Domestic publics are highly sensitive to the risk of casualties, given the asymmetric public goods nature problem in peacekeeping operations—the risks are borne by the contributing states, but the benefits, if ultimately realized, accrue disproportionately to the target state (Fearon and Laitin 2004). Thus, in peacekeeping operations, we would expect to see evidence of the first part of hypothesis 1.

In the case of Afghanistan, for example, outside decision makers have long considered the stability and relative peace of Afghanistan to be of critical importance to the long-term interests of countries everywhere. For this reason, Western governments and their allies have committed a significant number of troops. However, while more troops would tend to build trust with the allies and facilitate the success of the mission, these effects are only felt in the long run. Improvements in the security situation in Afghanistan have been patchy and have followed troop build ups with a long delay. Up until 2011, coalition casualties climbed each year. Only in 2012 did they come down. In the short-term, troop commitments may result in losses. By reducing (or failing to meet a needed increase in) troops during an election year, incumbents may hope to avoid the negative signal conveyed by more casualties.

Our null hypothesis in the case of contributing troops to peacekeeping operations is formulated as follows:

Proposition 1. Elections will not produce variation in troop levels because elites determine troop commitments on the basis of factors independent of elections.

The alternative hypothesis, based on the logic of political business cycles in security policy, gives rise to the following proposition:

Proposition 2. Incumbents will commit fewer troops to peacekeeping operations in the run-up to elections compared to other periods.

We also formulate two propositions that may confirm our hypothesis indirectly, by testifying to the link between elec-

5. We follow the peacekeeping literature and use the term “peacekeeping” to encompass various types of international operations designed to maintain peace after a conflict, including both more traditional, consent-based peacekeeping as well as enforcement operations.

tion year troop draw downs and public opinion/casualties respectively. We clarify that the effect we posit should apply more strongly to states that could plausibly incur casualties but not to other states. We also formulate in testable form the proposition that public opinion toward sending troops should sour in the wake of incurring casualties.⁶

Proposition 3. Contributors suffering casualties are more likely to reduce their troop commitments in the run-up to elections than contributors who do not suffer casualties.

Proposition 4. Public support for the war is negatively associated with the number of casualties a state suffers.

RESEARCH DESIGN

We look at troop commitments to the war in Afghanistan made through two mechanisms: Operation Enduring Freedom (OEF) and the International Security Assistance Force (ISAF). We collected monthly data on troops in Afghanistan in two ways, producing an original data set of troop commitments to Afghanistan from October 2001 through October 2011. First, we scraped the monthly contributions to ISAF from the official ISAF archive for January 2007 through October 2011. Second, we complemented these numbers with data from individual communications with foreign and defense ministries in each of the contributing states. In total, we gathered data on 50 different states. The ISAF mission is distinct from OEF, and not all the contributing countries to Afghanistan are NATO members. Our study explores both ISAF and OEF contributions. OEF is a US and UK operation that began when the first combat operations in Afghanistan were launched on October 7, 2001. OEF also involved the Northern Alliance in the early stages of the war and the officially recognized government of Afghanistan in the later stages. Outside of Afghanistan, OEF also includes a variety of other countries, mostly NATO member states, engaged in other theaters of the war on terror. The ISAF mission has existed since December 2001, with NATO assuming full control on August 11, 2003. Shortly thereafter, ISAF's UN mandate was expanded to include all of Afghanistan. Since then, ISAF has proceeded outward from Kabul in four stages: to the north, to the west, to the south, and since October 2006, to the east (and the entire country).⁷

6. See Kreps (2010) on the relationship between public opinion and the war in Afghanistan in general.

7. For more details on the ISAF mission, see <http://www.rs.nato.int/>.

Troop levels sent to a single conflict area by multiple contributors provide a concrete operationalization of security policy in a way that allows us to more precisely test the implications of our theoretical framework. This also highlights our empirical contribution. The existing literature has looked, almost exclusively, at wars or militarized interstate disputes (Gochman and Maoz 1984). As an empirical illustration, those data sets have at least the following limitations. Wars are, fortunately, relatively sparse in the empirical record, but this does limit substantially the power of any test involving war initiation or escalation. Militarized interstate disputes are a very disparate aggregation of disputes—different contexts, different initiators, different expectations by the domestic public on appropriate response by their leaders. Working with such heterogeneous data to test a model that usually includes a prediction based on a very well-defined crisis context introduces noise, which, with a limited dependent variable, is always a concern. The binary nature of the dependent variable (war initiation) limits or makes more problematic the use of techniques such as country-level fixed effects or instrumental variables. Finally, the strategic selection of the time to start a dispute and the target of a dispute threatens the exogeneity assumptions behind typical regression specifications.

The strength of the design is augmented by the as-if-random assignment of elections to the progression of the war. To draw an analogy from the experimental literature, the idea is that states in election periods (the treatment group) possess, on average, the same observed and unobserved characteristics as those states outside of the election periods (the control group).

Troops

Table A1 in the online appendix summarizes the totals for six different and potentially theoretically relevant groups of contributing states: all contributors, non-US contributors, NATO, non-NATO, states that experienced casualties, and states that did not.⁸ Two patterns emerge from the data: NATO dominates troop contributions to Afghanistan, and the United States dominates troop contributions among NATO states.⁹ For these reasons, we complement our analysis of the contributions of all allies with a separate analysis of non-US NATO contributions and non-NATO contributions.

8. Tables A2 and A3, also in the appendix, list the descriptive statistics—monthly averages—for NATO and non-NATO contributors respectively.

9. Non-NATO states account for less than 3% of the total contributions to OEF and ISAF.

We use population data from the World Bank to calculate the per million citizens troop contributions of each state. The subsetting and per capita transformations effectively normalize the distribution of troops commitments,¹⁰ allowing us to be more confident that our findings would not be driven by outliers.

Elections

For information about elections during this period, we used the National Elections across Democracy and Autocracy (NELDA) database (Hyde and Marinov 2011). This provided us with data on elections as well as the conditions under which elections were held. This was critical for establishing the exogeneity of the call for elections to the commitment of troops in Afghanistan.¹¹ Because of the unilateral ability of governing elites to send troops and withdraw to Afghanistan in presidential systems, we only considered leadership (i.e., executive) elections in the contributing states. For parliamentary systems, we looked at national legislative elections; for presidential systems, only presidential elections; and for mixed systems, both legislative as well as presidential elections.¹²

Our argument posits that politicians who are up for reelection can manipulate the number of troops on the ground. While this statement is plausible, to strengthen our confidence that this is the case we conducted case studies of 15 of our troop-contributing countries, selected for the variety of institutions they feature.¹³ In all of our cases, we consistently found constitutionally mandated ways in which this can happen. In France, a semipresidential system in which the presidency's power has expanded in recent years, the presidency has a wide mandate to reduce or increase troops. The executive's mandate is somewhat curtailed in parliamentary systems and in mixed systems (e.g., Poland) but still present. It is not uncommon in both presidential and parliamentary systems for the legislature to play some role in the process. Often, when it does play a role, it has to approve a mission in the first place, approve the maximum number of soldiers deployed, or both. Troop deployment authority rests significantly with the executive in Belgium, Denmark, Canada, the Netherlands, Spain, and the United Kingdom. Parliamentary

approval is officially needed in Germany, Hungary (up to 2003), and Italy. In Norway, the number of troops is left to be set by Parliament. Yet, even in such cases, there is a distinction between mandates, or what a legislature authorizes, and the actual number of troops (which we are after conceptually and in terms of our measures). Governments can and do deviate from the mandate, especially in a downward direction, claiming logistical constraints or other grounds. Finally, even in the case of relatively stronger parliamentary control, we should emphasize that governments represent parties in control of Parliament. Thus, they can ask the parliament to pass resolutions, and they can refrain from asking for troop increases close to elections.¹⁴

In total, there were 157 leadership election events across all contributors. Our unit of observation is the country-year-month, with troops and elections measured at that level, and the data span October 2001 through October 2011. According to the conceptualization described in the previous section, we note whether a country had a leadership election by assigning a value of 1 to an indicator variable, *electionapproach12*, if a country-year-month belongs in the 12-month period leading up to a country's election ("the election year"). For robustness checks, we also code whether a country is in the six-month period leading up to an election (*electionapproach6*), with few changes for the results.

Estimation strategy

What happens to troop contributions for peacekeeping operations as elections near in the contributing countries? We rely on the exogeneity of elections, and we conduct ordinary least squares regressions with minimal controls to correct for possible weaknesses of a basic comparison of means. Any specification is vulnerable to skewed distributions, a particular concern with the case of troop commitments to Afghanistan (see fig. A5 in the appendix). We offer three solutions to this issue: (1) we look at absolute troop levels as well as per capita figures, which better approximate a normal distribution, (2) we conduct analyses on six different subgroups to see how the finding holds across a theoretically and empirically relevant distribution of states (all allies, non-US contributors, NATO states, non-NATO states, states that experienced casualties, and states that did not experience casualties), and (3) we conduct ordinary least squares regressions with controls for time trends as well as country and year-month fixed effects.

10. See figs. A4 and A5 in the appendix for graphical representations of the distributions.

11. See table A4 in the appendix for a listing of each election event.

12. We used the Democracy-Dictatorship data set, to separate elections into those held in parliamentary, mixed (semipresidential), and presidential systems (Cheibub, Gandhi, and Vreeland 2010).

13. We studied closely the elections in Belgium, Canada, Czech Republic, Denmark, Estonia, France, Germany, Italy, Netherlands, Norway, Poland, Spain, Sweden, Romania, and the United Kingdom.

14. While in principle it may be desirable to measure the effects of different democratic institutions on political cycles, our sample is not large enough to allow us to do so econometrically.

Figure A1 in the appendix provides a quick validity check of the assumption that elections are not systematically related to the progress of the security operation. Some countries have a fixed electoral calendar; in other countries the timing of elections can vary somewhat. For example, a government may fall or call for snap elections. We checked our cases for the general prevalence of early elections and for the war in Afghanistan as an issue affecting timing. With the exception of one election, our case study work suggests that the timing of elections is independent of the conflict in Afghanistan. All the election dates are either fixed or triggered by the government because of an unrelated issue.¹⁵

We recognize that relatively early elections, even on unrelated issues, may be different. We used the NELDA data set to econometrically code for such events.¹⁶ We found that 34 out of the 157 elections had occurred early by this measure. We discuss the importance of these elections in the next section.

It might be the case that our exogeneity assumption does not apply due to a set of country- or time-period-specific characteristic that would invalidate our inference. For example, NATO contributors might hold more frequent elections than non-NATO contributors. Or, contributors might face stronger incentives to withdraw toward the latter half of the war. Country and time fixed effects allow us to make valid inferences under a weaker set of assumptions. That is, as-if randomness is conditional on country- or year-month-specific covariates.

FINDINGS

We conduct a series of regressions of our outcome of interest—troops per million citizens—on whether a country is

within 12 months of an election. The as-if-random treatment assignment assures us that observed as well as unobserved covariates are balanced between groups. For this reason, we do not need to control for observed differences parametrically using a host of familiar control variables such as gross domestic product, population, and others. We hone in on troops per million citizens here in order to focus our interpretation of the coefficients on each individual contributor, something that is made easier with a measure of the dependent variable that is readily applicable to all states, as is the case with troops per million citizens.¹⁷ For each of the six subpopulations of interest, we run five different model specifications on the outcome variable (i.e., troops per million citizens).¹⁸

$$\text{Troops}_{i,t} = \beta_0 + \beta_1 \text{ElectionPeriod}_{i,t} + \varepsilon_{i,t}. \quad (1)$$

$$\text{Troops}_{i,t} = \beta_0 + \beta_1 \text{ElectionPeriod}_{i,t} + \alpha_c + \varepsilon_{i,t}. \quad (2)$$

$$\text{Troops}_{i,t} = \beta_0 + \beta_1 \text{ElectionPeriod}_{i,t} + \alpha_c + \alpha_y + \varepsilon_{i,t}. \quad (3)$$

$$\text{Troops}_{i,t} = \beta_0 + \beta_1 \text{ElectionPeriod}_{i,t} + \alpha_c + \alpha_m + \varepsilon_{i,t}. \quad (4)$$

$$\begin{aligned} \text{Troops}_{i,t} = \beta_0 + \beta_1 \text{ElectionPeriod}_{i,t} \\ + \beta_2 \text{LogUSTroops}_{i,t} + \alpha_c + \varepsilon_{i,t}. \end{aligned} \quad (5)$$

The first model is simply a bivariate regression of the outcome measures on the election period indicator, producing the same results as a *t*-test finding and providing a baseline by which to compare the other model specifications.¹⁹ In models 2–5, we include country fixed effects, denoted above

15. One notable exception is the Netherlands' 2010 election, which we excluded from our analysis for this reason. In this election, NATO requested that the Dutch increase and extend their troop deployment to the more dangerous southern provinces of Afghanistan. The ruling coalition fell apart when Deputy Prime Minister Wouter Bos, the leader of the second largest party (the Labour Party), withdrew from the coalition government led by then-prime minister Jan Peter Balkenende. The Labour Party's withdrawal forced early elections.

16. Variable *nelda6* codes the presence of such elections. While this variable measures with some imprecision what we care about (it codes elections that were either early or late relative to when they were supposed to take place), in most cases it actually picks cases of early/unexpected elections. We coded two new variables, *electionapproach6v6* and *electionapproach12v6*, which take a value of 1 when *nelda6* is coded as "yes" (i.e., elections are early) and a given country-year-month is approaching an election within 6 or 12 months, respectively. We further created two more variables—*early6* and *early12*—that are essentially interactive terms, equal to 1 for electoral periods with elections for the de facto executive that are also early.

17. Although the large coefficients obtained when we operationalize troops as the absolute number of troops are useful for examining the average and total troop deployments to Afghanistan, as in our discussion of the difference of means, these coefficients can be difficult to interpret when applied to individual states. The reason for this is that these specifications pool together all contributors, large and small, and average out their effects. The large decreases during the electoral periods of larger contributors bring up the average for all states, which is what these large coefficients are capturing. Nonetheless, we report these regression results in table A8 in the appendix. Results are robust to various model specifications and suggest the same patterns of troop deployments as discussed above.

18. Because model 5 uses US troops to index a time trend, we do not run it for the subgroups that include the United States.

19. If our exogeneity assumption holds, a basic comparison of means between treatment groups should be sufficient for causal inference. Using both absolute troop numbers as well as troops per capita, we also conducted basic *t*-tests to check our results. Results are similar or identical and are reported in the appendix.

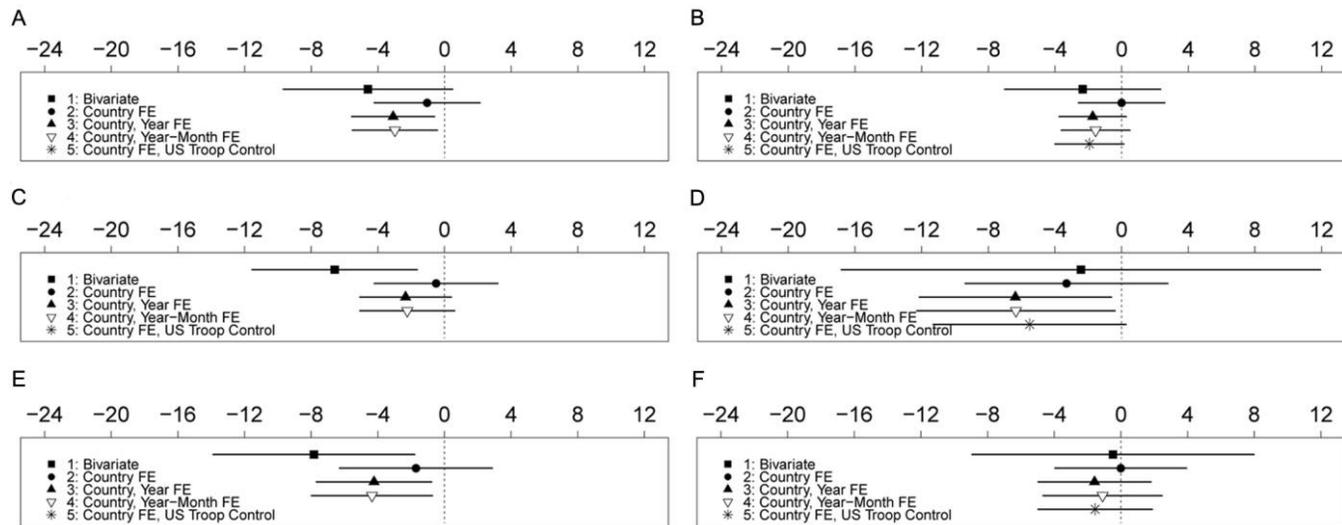


Figure 1. Ordinary least squares regression coefficients of effect of approaching elections on troop level change per capita during election period (12 months); line segments are 95% confidence intervals. A, All contributors; B, non-US contributors; C, NATO; D, non-NATO contributors; E, experienced casualties; F, no casualties.

by the term α_c in which c stands for every country analyzed in the given subpopulation. Country fixed effects let us account for country-specific decisions to contribute troops at a certain level that are constant over time.

We control for time trends in three different ways. First, we include year fixed effects in model 3, denoted by the term α_y in which y represents year. Second, we add, instead, year-month fixed effects represented by α_m in model 4. Third, we index each country's troop commitment by the number of US troops in Afghanistan in model 5. The idea is that the level of US troops may serve as a useful proxy for the troop requirements of the operation.

Figure 1 illustrates the coefficient estimates (see the appendix for results in tabular form). The results speak against proposition 1 (no preelection variation) and in favor of the alternative proposition 2, approaching elections tend to induce significantly lower troop commitments. For all contributors, NATO states, and states with casualties, the lead up to an election year leads to a statistically and substantively significant decrease in troop levels, robust to the addition of country fixed effects and time-trend controls.

Results hold across specifications. The confidence intervals increase, and we become less confident in the precision of the estimates of the effect of the run-up to elections for the fixed effects model. Nonetheless, all coefficients remain negative. For non-NATO states and states without casualties, the election period is associated with a small or insignificant decrease in the number of troops per million citizens. Compared to the full sample of states, the mag-

nitude of the effect is smaller. The regression results reject the null hypothesis of no variation (proposition 1) in favor of the alternative: looming elections cause a drop in troop contributions (proposition 2).

Since our analysis is based on troop mandates—the maximum number of troops a government is allowed to commit abroad by domestic policy—rather than actual boots on the ground, it is likely that this is a conservative estimate. Governments may actually be sending even fewer troops to the battlefield than they are allowed to commit in order to avoid casualties.

The large- N analysis discussed above provides evidence in favor of the proposition that contributors who suffered casualties during the mission in Afghanistan are more likely to decrease their troops (proposition 3). States that suffered casualties committed (more than) 8 fewer troops per million citizens during election years compared to other periods ($p < .01$). By contrast, states that did not suffer casualties committed slightly more troops during election years, although this estimate is statistically indistinguishable from zero at conventional levels. The median state that suffered casualties contributed about 55 troops per million citizens. Election years, then, produce a troop decrease of about 15% on average for these states. This suggests large electoral decreases for both large and small contributors. Even for the United States, the largest contributor in absolute terms, this represents a substantial predicted decrease during electoral periods: from 116 to 108 troops per million citizens, or a 7% decrease.

The intuition is that contributors who suffered casualties have their troops stationed in more dangerous areas, at constant risk of more casualties.²⁰ For states that do not put their troops in jeopardy, the time-inconsistency problem simply does not apply.

Our findings are robust against the “fighting season” in Afghanistan. It is sometimes argued that the months of November through April see little fighting. We point out that models 3 and 4 in table A6 include controls for year-month fixed effects. Controlling for year-month fixed effects allows us to see whether our findings are robust through all the months in Afghanistan, in case some were substantially different, such as by being more lethal.²¹

In another check, we turned to the importance of early elections. We looked at troop contributions during election periods preceding early or unexpected elections. We find that whether we look at six- or 12-month electoral periods preceding early elections, the general magnitude and direction of our findings hold.²² This suggests that even when elections occur early, elites attempt to decrease troop deployments abroad.

DISCUSSION

Next, we explore these troop variations further. In particular, we show that casualties decline in the run-up to elections and that fatalities help drive public attitudes toward the military operation, government competence, and awareness of the war in predictable ways.²³

If our argument is correct, we would expect in the run-up to elections to see fewer casualties, and we would expect this effect to be attributable to lower troop numbers in the field. Table 1 shows two regressions that are consistent with this argument. Column 1 shows that approaching elections (in 12 months or less) tend to change casualties in the full sample by -0.42 per month. The statistically significant effect, however, washes out when we control for the number of troops in the battlefield. Column 2 shows that troop presence is in fact a strong predictor of fatalities, but approaching elections on their own are not.

20. We find no evidence that states relocate their troops to safer areas or change the orders troops are given close to elections. If these alternative avenues for lowering casualty rates existed, they would tend to depress the strength of the findings we expect.

21. We also report results that show that election timing and fighting season are not correlated in table A13 in the appendix.

22. We report these results in table A7 in the appendix.

23. We should clarify that we do not necessarily disagree with arguments such as those by Feaver and Gelpi (2005) of public support for a war effort in spite of casualties. What we are claiming is that in ongoing security operations, elected leaders will aim for fewer casualties as elections approach, relative to other times.

Table 1. Troop Reductions and Casualties

Independent Variable	Casualties		
	(1)	(2)	(3)
Effect of $\sum_{j=0}^{12} \text{Election}_{i,(t+j)}$	-.423*	.127	-.120
	(.206)	(.139)	(.224)
Effect of Troops _{<i>i,t</i>} (1,000s)		.885***	
		(.010)	
Effect of Troops _{<i>i,t</i>} (per million citizens)			.130***
			(.004)
Country-year-month fixed effects	Yes	Yes	Yes
<i>N</i>	3,930	3,084	3,084

Note. Ordinary least squares estimates of effects of approaching elections and troop levels on country's casualties in Afghanistan. Standard errors in parentheses.

* $p < .05$.

*** $p < .001$.

To make the interpretation of this coefficient easier for small contributors, who may have fewer troops altogether, table 1 column 3 includes a regression of casualties on troops per million citizens and approaching elections. A 10% increase in a country's contribution on this variable (not uncommon in smaller or greater contributors) implies, for the median of 40 in the data, an increase by of four troops per million citizens. Per the estimated coefficient, such an increase would result in about a 50% chance of one soldier lost in a given month. If sustained for the whole preelectoral period of 12 months, such an increase would result in about six extra battlefield casualties.

The two sets of results, in combination, suggest that the casualty-mitigating effect of elections works via the reduction of troop numbers as balloting nears.²⁴ If one of the goals of lowering troop levels before voting is to have fewer fatalities in the battlefield, we would expect to find empirical evidence along the lines identified in table 1.

We also add that for our argument to work, it is not necessary that all countries lose soldiers all the time. What counts, in a climate of extreme aversion to casualties, is whether there is a perceived risk that this may happen. The data on lethality from Afghanistan allow us to think in more

24. Both regressions include country and year-month fixed effects to adjust for changing battlefield dynamics and different country casualty levels.

Table 2. Public Opinion and Casualties (Monthly Data)

Dependent Variable	% Want Withdrawal (Transatlantic Trends)	% Want Withdrawal (Pew)	% Believe UK Government Winning (Yougov)	% Dutch Aware of Afghan Mission (Dutch MoD)
Effect of Casualties _{i(t-1)}	.217*** (.073)	.132*** (.043)	-.075 (.049)	4.980*** (1.703)
Effect of $\sum_{j=1}^6 \frac{1}{6} \text{Casualties}_{i(t-j)}$.331*** (.090)	.279*** (.058)	-.165 (.064)	9.539 (5.44)
Effect of $\sum_{j=0}^6 \text{Election}_{i(t+j)}$				6.846 (3.348)
Country-year fixed effects	Yes	Yes		
Country	13	14	United Kingdom	Netherlands
Year-months	62	115	21	48
N	62	115	21	48

Note. Ordinary least squares estimates of effects of country's casualties in Afghanistan on respondents' views of whether country should withdraw troops, on opinion of government winning the war, and on awareness of war. Effect of a country's approaching elections on voter awareness of war also shown. Standard errors in parentheses.

*** $p < .001$.

informed terms about this risk (Bove and Gavrilova 2014). Taking 2004, a not particularly deadly year, we see the following patterns. There were 151 attacks in 2004, from January 1 to December 31. Approximately 1/2, or 75, of them did not claim lives (while arguably intending to). The remaining 76 claimed between one and 16 lives, with an average of three. The victims were contractors, civilians, clerics, government workers, UN employees, nongovernmental workers, and coalition soldiers. Thus, it is reasonable to assume that even if a particular coalition country did not suffer casualties at a particular point of time, the overall level of violence kept that possibility vivid.

Furthermore, we would expect public attitudes toward the war effort, the government, and general awareness of the operation to follow specific patterns if our argument is correct. Specifically, we would expect to find that casualties tend to depress support for the war and to increase calls to bring the troops back home.

We have data from two surveys, the Pew Global Attitudes Survey and the Transatlantic Trends survey, that give us a reasonable cross-sectional, over-time variation in a score of troop-contributing countries, including the main contributors and covering mostly countries that suffer casualties. The first two columns of table 2 show a regression of respondents' desire to withdraw a country's troops on the basis of a country's casualties in the operation. We run two types of regressions, one on a simple one-period (one month) lag of fatalities and one on a lagged six-month average number of troops lost. The more complicated lag probes for the lin-

gering effect of casualties. We control for country and year fixed effects (the data coverage is too sparse to allow for year-month fixed effects). As the table illustrates, casualties are a significant predictor of public attitudes to withdraw. The effect is stronger when casualties are averaged over a longer period of time: one additional casualty, on average, for the six preceding months tends to increase by about a third of a percent attitudes favoring withdrawal. Given that about half of the domestic publics, on average, want withdrawal to begin with in the sample,²⁵ this tends to weigh measurably on an already sensitive public mood.

Thus, if domestic publics use casualties as a litmus test for successful policy, pursuing lower casualties, whenever possible, can be one way in which incumbents can win domestic support for the operation. In election years, domestic publics may use this measure of how the operation is succeeding to judge the competence of a leader, yielding reelection incentives for lower costs. Being able to lower casualties without bringing in accusations of placing the operation's success at risk is a capacity some incumbents possess to a greater degree than others.

The third column of table 2 shows how respondents' perception of whether the government is winning the war in Afghanistan changes with casualties for the one country for which we were able to identify such polling data, the United Kingdom. We use the British survey YouGov for

25. This is about 50% in the Pew survey and 38% in Transatlantic Trends.

these data. The percentage of respondents who believe the war is being won declines with casualties. The decline is statistically significant in the case of the averaged casualties lag.

The last column identifies the set of voters reporting some knowledge of the peacekeeping operation using polling data from the Ministry of Defence (MoD) Netherlands. One casualty, in the month prior or on average for the past six months, increases the percentage of voters reporting knowledge of the operation between approximately 5% and 10%.²⁶ Furthermore, in the 12 months preceding the election, voters in this survey reported knowing more about the operation, by close to 7 percentage points, than outside of this period.

Thus, local awareness of the war effort, and assessment of the operation, changes with casualties. To the extent that these judgments are affected by battlefield fatalities, it is more likely that incumbents' capacity to lead is judged partly on the basis of their ability to minimize the costs of the operation.

CONCLUSION

A traditional realist account in which elites determine security policy independent of electoral results may not explain the fluctuations in policy for cases like the war in Afghanistan. Incumbents lower troops levels close to elections. While we argue that this is due to incumbents trying to signal competency by achieving security goals at a lower cost, it is possible that alternative mechanisms may also be able to explain some of our results. For example, it might be the case that voters are myopic—that what type of an incumbent is making policy does not matter since voters do not foresee that an incumbent may lower troop levels before an election only to raise them after the election. To adjudicate between these competing logics, we can imagine survey experiments, in the lab or in the field, as promising ways to examine further the microfoundations of the time-inconsistency logics we outline.

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26. Respondents were not told how many casualties the country had as part of the polling.

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