

# Does Electoral Proximity Affect Security Policy?\*

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February 25, 2015

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\*Authors' names in alphabetical order, with the first two being the main authors. Support for this research was provided by Macmillan Center for International and Area Studies, Yale University. Data and supporting materials necessary to reproduce the numerical results in the paper are available in the JOP Dataverse (<http://thedata.harvard.edu/dvn/dv/jop>). Supplemental analyses, appendices are available as part of this article at [www.nikolaymarinov.com](http://www.nikolaymarinov.com) and at [www.williamgnomikos.com](http://www.williamgnomikos.com).

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## **Abstract**

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 (“OEF”) and the International Security Assistance Force (“ISAF”) mission in Afghanistan. In that case, we argue 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 the first arguably identified effect of electoral proximity on security policy.

# Introduction

Political-business cycles are studied profusely in economics. For reasons that differ by perspective, manipulating macro-economic policy close to elections is said to benefit the re-election 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 over-invest 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 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 case of peace-keeping 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 (1995)'s seminal application of the political business cycle framework to Presidential uses of force in the U.S. 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 re-election. In peace-keeping operations, where victory is elusive and long-term, domestic publics are highly sensitive to casualties, and often the effort is a coalition one, 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), albeit 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 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 Peace-Keeping 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 based upon *realpolitik*, developments on the battlefield, or international commitments, even as dissenters have called attention to domestic factors and considerations (Mearsheimer 2001; Waltz 1979; Ostrom

and Job 1986; James and Oneal 1991). 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, p. 63).

More recently, scholars have applied political economy models and models of elite-lead decision-making to support the expectation of no variation around elections. Thus, Saunders argues for the U.S. 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 pre-election 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 U.S. 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 where 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.<sup>1</sup> Traditionally, economists argued that incumbent governments could engage in inflationary monetary policy prior to elections in order to lower unemployment (Nordhaus 1975).

Problematically, such an approach assumed that voters are myopic and retrospective; caring only about employment 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 regards 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 incum-

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

bent 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 (that is, lower inflation) than incompetent incumbents. For adverse selection theories, electoral cycles may be a normatively positive phenomenon in so far as they allow competent incumbents to signal their ability.

In this paper, 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 upon 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 non-election years, incumbents have the time to invest in policies that may not yield positive national security benefits immediately. 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 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, on the other hand, risks casualties without offering immediately visible payoffs.

In peace-keeping operations, changes in the forces committed to an operation produces relatively few observables in the short-term. Governments enjoy an informational advantage, voters cannot immediately detect if a policy is successful or not. 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, on the other hand, 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 types is more likely to achieve that

without inviting allegations of compromising the longer-term security objective. In effect, this results in both types of governments to some (and different) degree underinvesting in the type of security policy that pays off only in the long run.<sup>2</sup>

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

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<sup>2</sup>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.

<sup>3</sup>Our argument here captures some of the tensions observed in early work by Quandt (1986, 826-827), 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.”

Our work also speaks to diversionary war theory about the effects of political competition on security policy. These authors suggest that democratic leaders utilize war-making as a diversionary political tool to gain votes before an election.<sup>4</sup> 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 they could deal well with a war should one arise in their next term. We do not see much empirical support in peace-keeping 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 hypotheses, that scholars should apply depending on the structure of incentives inherent in the security policy under investigation:

**Hypothesis 1** *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*

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

*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 electoral business cycles in peace-keeping operations.<sup>5</sup> In the case of troops on a peace-keeping 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 peace-keeping operations - the risks are born by the contributing states but the benefits, if ultimately realized, accrue disproportionately to the target state (Fearon and Laitin 2004). Thus, in peace-keeping 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 of 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

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

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 a testable proposition below:

**Proposition 1** *Elections will not produce variation in troop levels because elites determine troop commitments based on 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 peace-keeping operations in the run-up to elections compared to other periods*

We also formulate two propositions that may confirm our hypotheses indirectly, by testifying to the link between election year troop draw-downs and public opinion/ casualties respectively. We clarify that the effect we posit should apply more strongly to states which 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.<sup>6</sup>

**Proposition 3** *Contributors suffering casualties are more likely to reduce their troop commitments in the run-up to elections than contributors that 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 from October 2001 through October 2011 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 dataset 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 communication 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 contribu-

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

tions. OEF is a United States and United Kingdom operation that began when the first combat operations in Afghanistan were launched on October 7th, 2001. OEF also involved the Northern Alliance in the early stages of the war and the officially recognized government of Afghanistan in the latter 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).<sup>7</sup>

Troop levels 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 datasets 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 heterogenous data to test a model that usually includes a pre-

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

diction 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 time to start a dispute and target of 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.<sup>8</sup> Two patterns emerge from the data: NATO dominates troop contributions to Afghanistan and the

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<sup>8</sup>Tables [A2](#) and [A3](#), also in the Online Appendix, list the descriptive statistics—monthly averages—for NATO and non-NATO contributors respectively.

United States dominates troop contributions among NATO states.<sup>9</sup> 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.

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,<sup>10</sup> allowing us to be more confident that our findings would not be driven by outliers.

## Elections

For information about elections during this period, we utilized the National Elections Across Democracy and Autocracy (NELDA) database.<sup>11</sup> 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.<sup>12</sup> 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

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<sup>9</sup>Non-NATO states account for less than three percent of the total contributions to OEF and ISAF.

<sup>10</sup>See Figures A4 and A5 in the Online Appendix for graphical representations of the distributions.

<sup>11</sup>Hyde and Marinov (2011).

<sup>12</sup>See Table A4 for a listing of each election event.

presidential systems, only presidential elections; and for mixed systems, both legislative as well as presidential elections.<sup>13</sup>

Our argument posits that politicians who are up for re-election 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.<sup>14</sup> In all of our cases, we consistently found constitutionally-mandated ways in which this can happen. In France, a semi-Presidential 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 UK. 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

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<sup>13</sup>We used the Democracy-Dictatorship dataset, to separated elections into those held in parliamentary, mixed (semi-presidential), and presidential systems (Cheibub et al. 2008).

<sup>14</sup>We studied closely the elections in Belgium, Canada, Czech Republic, Denmark, Estonia, France, Germany, Italy, Netherlands, Norway, Poland, Spain, Sweden, Romania, UK.

measures). Governments can and do deviate from the mandate, especially in a downward direction, claiming logistical 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.<sup>15</sup>

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 spans 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 twelve-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 peace-keeping operations as elections near in the contributing countries? We rely on the exogeneity of elections, and we conduct ordinary

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<sup>15</sup>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.

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 Figure A5 in appendix.) We offer three solutions to this issue: (1) we look at absolute troop levels as well as per capita figures, which approximate much better a normal distribution; (2) we conduct analyses on six different subgroups to see how the finding holds across 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 OLS regressions with controls for time trends as well as country and year-month fixed effects.

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.<sup>16</sup>

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<sup>16</sup>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.

We recognize that relatively early elections, even on unrelated issues, may be different. We used the NELDA dataset to econometrically code for such events.<sup>17</sup> We found that 34 out of the 157 elections had occurred early by this measure. We discuss the importance of these elections in the section with regression results.

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 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.

<sup>17</sup>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” (that is, 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.

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 twelve 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 GDP, 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.<sup>18</sup>

For each of the six subpopulations of interest we run five different model specifications on

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<sup>18</sup>Although the large coefficients when we operationalize troops as absolute number of troops are useful for examining the average and total troop deployments to Afghanistan, as we do 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 of the Online Appendix. Results are robust to various model

the outcome variable (i.e., troops per millions of citizens).<sup>19</sup>

$$Troops_{i,t} = \beta_0 + \beta_1 \mathbf{ElectionPeriod}_{i,t} + \epsilon_{i,t} \quad (1)$$

$$Troops_{i,t} = \beta_0 + \beta_1 \mathbf{ElectionPeriod}_{i,t} + \alpha_c + \epsilon_{i,t} \quad (2)$$

$$Troops_{i,t} = \beta_0 + \beta_1 \mathbf{ElectionPeriod}_{i,t} + \alpha_c + \alpha_y + \epsilon_{i,t} \quad (3)$$

$$Troops_{i,t} = \beta_0 + \beta_1 \mathbf{ElectionPeriod}_{i,t} + \alpha_c + \alpha_m + \epsilon_{i,t} \quad (4)$$

$$Troops_{i,t} = \beta_0 + \beta_1 \mathbf{ElectionPeriod}_{i,t} + \beta_2 \mathbf{LogUSTroops}_{i,t} + \alpha_c + \epsilon_{i,t} \quad (5)$$

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 provides a baseline by which to compare the other model specifications.<sup>20</sup> In models 2 through 5, we include country fixed-effects, denoted above by the term  $\alpha_c$  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  $\alpha_y$  in which  $y$  represents year. Second, we add, instead, year-month fixed effects represented by  $\alpha_m$  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 specifications and suggest the same patterns of troop deployments as discussed above.

<sup>19</sup>Because model 5 uses US troops to index a time trend, we do not run it for the subgroups that include the United States.

<sup>20</sup>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 online appendix.

level of US troops may serve as a useful proxy for the troop requirements of the operation.

Figure 1 illustrates graphically the coefficient estimates (we include a table in the online appendix showing results in tabular form). The results speak against Proposition 1 (no pre-election 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 magnitude of the effect is smaller. The regression results reject the null hypotheses of no variation (Proposition 1) in favor of the alternative, looming elections cause a drop in troop in contributions (Proposition 2).

Since our analysis is based upon 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.

[Figure 1 about here]

The large-N analysis discussed above provides evidence in favor of the proposition that contributors that suffered casualties during the mission in Afghanistan are more likely to decrease their troops and to a greater extent than states (Proposition 3). States that suffered casualties committed more than 8 fewer troops per million citizens during election years compared to other periods ( $p < 0.01$ ). By contrast, states that did not suffer casualties committed slightly more troops during election years, though this estimate is statistically indistinguishable from zero at conventional levels. To concretize, the median state which suffered casualties contributed about 55 troops per million citizens. Election years, then, produce a 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 that suffered casualties have their troops stationed in more dangerous areas, at constant risk of more casualties.<sup>21</sup> For states that do not expose their troops in a way that puts their troops in jeopardy, the time inconsistency problem simply

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<sup>21</sup>We also 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.

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 FE allows us to see if our findings are robust through all the months in Afghanistan, in case some were substantially different or not, such as by being more lethal.<sup>22</sup>

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 twelve-month electoral periods preceding early elections, the general magnitude and direction of our findings hold.<sup>23</sup> 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, toward government competence and awareness of the war in predictable ways.<sup>24</sup>

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<sup>22</sup>We also report results that show that election timing and fighting season are not correlated in Table A13 in the Online Appendix.

<sup>23</sup>We report these results in Table A7 of the online appendix.

<sup>24</sup>We should clarify that we do not necessarily disagree with arguments such as those by Feaver and Gelpi (2005) on public support for a war effort in spite of casualties. What we are

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 troops numbers in the field. Table 1 shows two regressions that are consistent with this argument. The first column shows that approaching elections (in 12 months or less) tend to reduce 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. The second column shows that troop presence is in fact a strong predictor of fatalities, but approaching elections on their own are not.

To make the interpretation of this coefficient easier for small contributors, who may have fewer troops altogether, the last column 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 4 troops per million citizens. Per the estimated coefficient, such an increase would result in about a 50 percent chance of 1 soldier lost in a given month. If sustained for the the whole pre-electoral period of 12 months, such an increase would result in about 6 extra battlefield casualties in expectation.

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.<sup>25</sup> If one of the goals of lowering claiming is that in ongoing security operations, elected leaders will aim for fewer casualties as elections approach, relative to at other times.

<sup>25</sup>Both regressions include country and year-month fixed effects to adjust for changing battlefield dynamics and different country casualty levels.

troop levels before voting is to have fewer fatalities in the battlefield, we would expect to find empirical evidence along the lines identified on Table 1.

[Table 1 about here]

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 allows us to think in more 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 1st to December 31. Approximately 1/2 or 75 of them did not claim lives (while arguably intending to). The remaining 76 claimed between 1 and 16 lives, with an average of 3. The victims were contractors, civilians, clerics, government workers, UN employees, NGO 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 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 lingering 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,<sup>26</sup> 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 re-election incentives to 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.

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<sup>26</sup>About 50 % in the Pew survey and 38 % in the Transatlantic Trends one.

[Table 2 about here]

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. The percent 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 Netherlands. One casualty, in the month before or on average for the past six months, increases the percentage of voters reporting knowledge of the operation between approximately 5 and 10 per cent.<sup>27</sup> Furthermore, in the 12 months preceding the election voters in this survey reported knowing more about the operation, by close to 7 percentage point, than outside of it.

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 incumbent's capacity to lead is judged partly based on their ability to minimize the costs of the operation.

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<sup>27</sup>Respondents were not told how many casualties the country has had as part of the polling.

## 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 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 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 again. 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 micro-foundations of the time-inconsistency logics we outline.

\*We thank Peter Aronow, Bernd Beber, Allan Dafoe, Hein Goemans, Magnus Öberg, Andrew Radin, Nicholas Sambanis, and Cyrus Samii for comments on earlier drafts. We also thank participants at the Yale University MacMillan International Relations Workshop (Nov 2012), the Folke Bernadotte Academy Workshop at NYU (Nov 2012), at the seminar series of the Department of Peace and Conflict Research at Uppsala University (Feb 2013), at the Midwest Political Science Conference (April 2013), the 2013 Chicago meeting of the American Political Science Association (Sep 2013), and at the Conflict Network Meeting at the University of Essex, UK (Sep 2013). Our deepest thanks go to our research assistants Vinicus Lindoso, Igor Mitschka and Arjang Navab who were resourceful and professional throughout. The usual disclaimer applies.

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## Tables and Figures

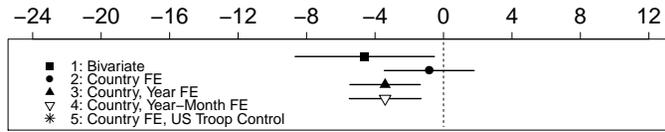
Troop Reductions and Casualties			
Independent Variable	Casualties		
Effect of $\sum_{j=0}^{12} Election_{i,(t+j)}$	-0.423*	0.127	-0.120
(Standard Error)	(0.206)	(0.139)	(0.224)
Effect of $Troops_{i,t}$ (1000's)	0.885***		
(Standard Error)	(0.010)		
Effect of $Troops_{i,t}$ (per million citizens)			0.130***
(Standard Error)			(0.004)
	country year-month FE		
$N$	3930	3084	3084

Table 1: Ordinary least squares estimates of effects of approaching elections and troop levels on country's casualties in Afghanistan. \*\*\* =  $p < .001$ , \*\* =  $p < .01$ , \* =  $p < .05$ .

Public Opinion and Casualties (Monthly Data)				
Dependent Variable	% want withdrawal (Transatl. Trends)	% want withdrawal (Pew)	% believe UK gvt winning (Yougov)	% Dutch aware of Afghan mission (Dutch MoD)
Effect of $Casualties_{i(t-1)}$ (Standard Error)	0.217*** (0.073)	0.132*** (0.043)	-0.075 (0.049)	4.980*** (1.703)
Effect of $\sum_{j=1}^6 \frac{1}{6} Casualties_{i,(t-j)}$ (Standard Error)	0.331*** (0.090)	0.279*** (0.058)	-0.165 (0.064)	9.539 (5.44)
Effect of $\sum_{j=0}^6 Election_{i,(t+j)}$ (Standard Error)				6.846 (3.348)
	country-year FE	country-year FE		
Countries	13	14	UK	Netherlands
Year-Months	62	115	21	48
$N$	62	115	21	48

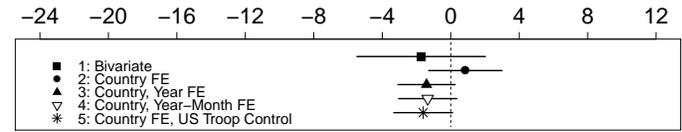
Table 2: Ordinary least squares estimates of effects of country's casualties in Afghanistan on respondents' views of whether country should withdraw troops, on opinion of government winning the war, and on awareness of war. Effect of a country's approaching elections on voter awareness of war also shown. \*\*\* =  $p < .001$ , \*\* =  $p < .01$ , \* =  $p < .05$ .

(a) All contributors



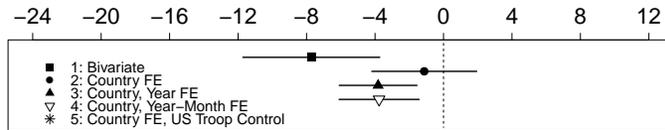
Troop Level Change/Capita during Election Period (12 months)

(b) Non-US contributors



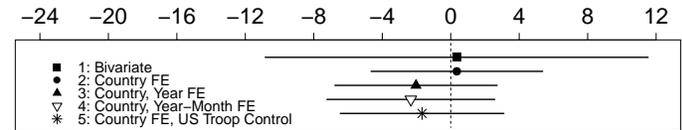
Troop Level Change/Capita during Election Period (12 months)

(c) NATO



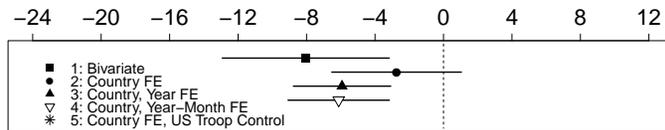
Troop Level Change/Capita during Election Period (12 months)

(d) Non-NATO contributors



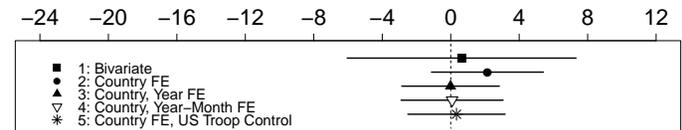
Troop Level Change/Capita during Election Period (12 months)

(e) Experienced casualties



Troop Level Change/Capita during Election Period (12 months)

(f) No casualties



Troop Level Change/Capita during Election Period (12 months)

Figure 1: Coefficient plots of OLS regressions. In each panel, points indicate regression coefficients of approaching elections on troops per capita and line segments 95 % confidence intervals for each group analyzed.

## Appendices for online distribution

### A Tables

Group	Troops	Troops/mil	Casualties/Month	Data Months	Elections
All ISAF/OEF	67031	2380	39	3199	157
Non-US ISAF/OEF	31765	2264	10	3078	155
NATO	63815	1368	39	2415	102
Non-NATO	3215	1012	0	784	55
Casualties	63037	1189	39	1831	79
No Casualties	3994	1191	0	1368	41

Table A1: Descriptive statistics for total contributors to Afghanistan, by group.

State	Troops	Troops/mil	Casualties	Data Months	# Elections
Albania	177	55	0.00	58	2
Belgium	446	41	0.01	58	3
Bulgaria	277	37	0.00	105	5
Canada	2544	76	1.32	58	4
Croatia	171	39	0.00	105	5
Czech Republic	225	22	0.04	114	8
Denmark	659	119	0.34	58	4
Estonia	82	61	0.07	118	3
France	2304	36	0.61	121	6
Germany	3179	39	0.44	119	0
Greece	130	12	0.00	58	3
Hungary	309	31	0.06	58	3
Iceland	7	22	0.00	57	0
Italy	2846	47	0.34	58	2
Latvia	136	60	0.02	105	4
Lithuania	139	42	0.01	99	6
Luxembourg	9	19	0.00	58	0
Netherlands	1404	85	0.21	58	5
Norway	414	87	0.08	121	2
Poland	1058	28	0.23	97	6
Portugal	126	12	0.02	58	6
Romania	1386	64	0.16	118	6
Slovakia	125	23	0.00	109	6
Slovenia	59	29	0.00	92	4
Spain	709	16	0.27	118	2
Turkey	1203	17	0.02	58	3
United Kingdom	8425	136	6.17	58	2
United States of America	35266	116	28.51	121	2

Table A2: Descriptive statistics for NATO contributors to Afghanistan: average number of troops/month, average number of troops/month for every million citizens of the state, average number of casualties/months, number of months for which data is available, and number of election events.

State	Troops	Troops/mil	Casualties	Data Months	# Elections
Armenia	78	25	0.00	20	5
Australia	1163	53	0.00	58	4
Austria	3	0	0.00	58	5
Azerbaijan†	65	7	0.00	58	0
Bosnia and Herzegovina†	23	6	0.00	32	0
El Salvador	24	4	0.00	2	2
Finland	108	20	0.00	56	4
Georgia	393	88	0.00	42	4
Ireland	7	2	0.00	58	4
Jordan†	49	8	0.00	26	0
Macedonia	157	76	0.00	58	7
Malaysia†	35	1	0.00	15	0
Mongolia	80	29	0.00	19	4
Montenegro†	45	70	0.00	20	0
New Zealand	165	38	0.00	58	3
Singapore†	24	5	0.00	35	0
South Korea	315	6	0.00	15	2
Sweden	383	41	0.00	58	3
Switzerland	3	0	0.00	15	0
Tonga†	55	526	0.00	10	0
Ukraine	14	0	0.00	38	8
United Arab Emirates†	28	4	0.00	33	0

Table A3: Descriptive statistics for non-NATO contributors to Afghanistan: average number of troops/month, average number of troops/month for every million citizens of the state, average number of casualties/months, number of months for which data is available, and number of election events. † indicates non-democratic state according to DD dataset.

Albania	2009	Estonia	2003	Lithuania	2004	Romania	2009
Australia	2007	Estonia	2007	Lithuania	2004	Romania	2012
Australia	2010	Estonia	2011	Lithuania	2008	Slovakia	2004
Austria	2008	Finland	2007	Lithuania	2009	Slovakia	2006
Austria	2010	Finland	2011	Lithuania	2012	Slovakia	2009
Belgium	2007	France	2002	Macedonia	2008	Slovakia	2009
Belgium	2010	France	2002	Macedonia	2009	Slovakia	2010
Bulgaria	2005	France	2002	Macedonia	2009	Slovakia	2012
Bulgaria	2009	France	2007	Macedonia	2011	Slovenia	2004
Bulgaria	2011	France	2007	Netherlands	2010	Slovenia	2008
Canada	2008	France	2007	Netherlands	2011	Slovenia	2011
Canada	2011	France	2012	New Zealand	2008	Spain	2004
Croatia	2003	France	2012	Norway	2005	Spain	2008
Croatia	2005	France	2012	Norway	2009	Spain	2011
Croatia	2007	Georgia	2008	Poland	2005	Sweden	2010
Croatia	2009	Germany	2002	Poland	2005	Turkey	2007
Croatia	2010	Germany	2005	Poland	2007	Turkey	2011
Czech Republic	2002	Germany	2009	Poland	2010	Ukraine	2010
Czech Republic	2002	Greece	2007	Poland	2010	Ukraine	2010
Czech Republic	2002	Greece	2009	Poland	2011	United Kingdom	2010
Czech Republic	2004	Hungary	2010	Portugal	2009	USA	2004
Czech Republic	2006	Ireland	2007	Portugal	2011	USA	2008
Czech Republic	2008	Ireland	2011	Portugal	2011		
Czech Republic	2010	Ireland	2011	Romania	2004		
Czech Republic	2010	Italy	2008	Romania	2004		
Czech Republic	2012	Latvia	2006	Romania	2007		
Denmark	2007	Latvia	2010	Romania	2008		
Denmark	2011	Latvia	2011	Romania	2009		

Table A4: Each election event in the dataset.

	<b>Election Period</b>	<b>Out of Election Period</b>	<b>Diff. of Means</b>	<b>p-value</b>
All contributors (troops)	1225	2575	-1350	0.000
All contributors (troops per capita)	42	47	-5	0.008
	(N=2126)	(N=934)		
Non-US contributors (troops)	657	864	-206	0.000
Non-US contributors (troops per capita)	41	43	-2	0.295
	(N=2029)	(N=910)		
NATO (troops)	1477	3508	-2031	0.000
NATO (troops per capita)	44	52	-8	0.000
	(N=1530)	(N=746)		
Non-NATO (troops)	224	180	45	0.116
Non-NATO (troops per capita)	33	33	0	0.929
	(N=596)	(N=188)		
Experienced Casualties (troops)	1947	4232	-2284	0.000
Experienced Casualties (troops per capita)	49	57	-8	0.001
	(N=1259)	(N=548)		
No Casualties (troops)	199	169	31	0.066
No Casualties (troops per capita)	32	32	1	0.808
	(N=867)	(N=386)		

Table A5: Difference between the mean number of troops committed to Afghanistan between 10/2001 and 10/2011 inside of election periods (within twelve months of an election) and outside of election periods.

	1: Bivariate	2: Country FE	3: Country, Year FE	4: Country, Year-Month FE	5: Country FE, US troop control
<b>All contributors</b>					
Election Period (12mo)	-4.6*	-0.83	-3.41**	-3.41**	-3.63***
N=3060	(2.07)	(1.34)	(1.05)	(1.07)	(1.07)
<b>Non-US contributors</b>					
Election Period (12mo)	-1.73	0.85	-1.41	-1.35	-1.61
N=2939	(1.91)	(1.09)	(0.85)	(0.87)	(0.88)
<b>NATO</b>					
Election Period (12mo)	-7.72***	-1.12	-3.82***	-3.76**	-3.95***
N=2276	(2.04)	(1.56)	(1.16)	(1.19)	(1.18)
<b>Non-NATO</b>					
Election Period (12mo)	0.35	0.36	-2.03	-2.33	-1.67
N=784	(5.7)	(2.55)	(2.41)	(2.5)	(2.43)
<b>Experienced Casualties</b>					
Election Period (12mo)	-8.05**	-2.74	-5.93***	-6.12***	-6.1***
N=1807	(2.49)	(1.93)	(1.45)	(1.51)	(1.47)
<b>No Casualties</b>					
Election Period (12mo)	0.64	2.14	-0.01	0.08	0.34
N=1253	(3.41)	(1.67)	(1.45)	(1.52)	(1.45)

Table A6: Regressions of troops per million citizens on indicator variable for whether an election would be held within a year. Effect of elections (coefficient and SE) reported, remaining coefficients suppressed. \*\*\* =  $p < .001$ , \*\* =  $p < .01$ , \* =  $p < .05$ .

	1: Bivariate (Troops)	2: Country, Year-Month FE (Troops)	3: Bivariate (Troops/Capita)	4: Country, Year-Month FE (Troops/Capita)
Early Elections (Within 6 mos) N=2712	-1633.92 (864.38)	-721.93 (607.79)	-4.36 (4.94)	-8.28** (2.54)
Early Elections (Within 12 mos) N=2394	-1916.28** (667.75)	-1045.54* (500.87)	-4.86 (3.76)	-8.91*** (2.08)

Table A7: Regressions of average number of troops and troops per capita on indicator variable for whether an early election would be held within six and twelve months of a given month.

	1: Bivariate	2: Country FE	3: Country, Year FE	4: Country, Year-Month FE	5: Country FE, US troop control
<b>All allies</b>					
Election Period (12mo)	-1349.98***	-505.86	-737.99**	-751.81**	
N=3060	(360.52)	(258.11)	(249.17)	(255.02)	
<b>All allies (no US)</b>					
Election Period (12mo)	-206.31***	17.99	-20.62	-19.56	-25.5
N=2939	(58.4)	(20.66)	(16.92)	(17.25)	(17.11)
<b>NATO</b>					
Election Period (12mo)	-2030.83***	-624.07	-883.35**	-901.1**	
N=2276	(470.87)	(332.9)	(320.94)	(331.57)	
<b>Non-NATO</b>					
Election Period (12mo)	44.67	-19.46	-35.91**	-39.61**	-34.69**
N=784	(27.84)	(13.69)	(12.28)	(12.69)	(12.36)
<b>Experienced Casualties</b>					
Election Period (12mo)	-2284.67***	-834.62	-1169.55**	-1228.48**	
N=1807	(600.67)	(429.21)	(408.72)	(427.07)	
<b>No Casualties</b>					
Election Period (12mo)	30.77	7.01	-7.53	-7.65	-4.35
N=1253	(17.1)	(9.38)	(7.87)	(8.22)	(7.81)

Table A8: Regressions of average number of troops on indicator variable for whether an election would be held within twelve months of a given month. \*\*\* =  $p < .001$ , \*\* =  $p < .01$ , \* =  $p < .05$ .

	1: Bivariate	2: Country FE	3: Country, Year FE	4: Country, Year-Month FE	5: Country FE, US troop control
<b>All allies</b>					
Election Period (6mo)	-1127.48*	-332.64	-492.2	-502.12	-535.8
N=3060	(451.06)	(312.6)	(301.59)	(310.48)	(300.47)
<b>All allies (no US)</b>					
Election Period (6mo)	-189.23**	-1.97	-27.4	-24.07	-36.1
N=2939	(72.87)	(24.94)	(20.4)	(20.92)	(20.62)
<b>NATO</b>					
Election Period (6mo)	-1668.63**	-409.08	-579.8	-585.23	-610.4
N=2276	(583.78)	(402.93)	(388.1)	(403.71)	(385.22)
<b>Non-NATO</b>					
Election Period (6mo)	44.61	-15.82	-37.82*	-38.55*	-32.13*
N=784	(35.81)	(16.64)	(15.01)	(15.46)	(15.02)
<b>Experienced Casualties</b>					
Election Period (6mo)	-1972.44**	-552.65	-838.18	-865.62	-855.15
N=1807	(745.55)	(520.72)	(496.61)	(523.02)	(492.7)
<b>No Casualties</b>					
Election Period (6mo)	30.75	8.04	-2.92	-0.91	-1.61
N=1253	(21.65)	(11.32)	(9.37)	(9.86)	(9.42)

Table A9: Regressions of average number of troops on indicator variable for whether an election would be held within six months of a given month. \*\*\* =  $p < .001$ , \*\* =  $p < .01$ , \* =  $p < .05$ .

	1: Bivariate	2: Country FE	3: Country, Year FE	4: Country, Year-Month FE	5: Country FE, US troop control
<b>All allies</b>					
Election Period (6mo)	-4.6	-1.04	-3.08*	-2.98*	-3.32*
N=3060	(2.59)	(1.62)	(1.27)	(1.3)	(1.29)
<b>All allies (no US)</b>					
Election Period (6mo)	-2.34	0	-1.74	-1.56	-1.93
N=2939	(2.39)	(1.32)	(1.03)	(1.05)	(1.06)
<b>NATO</b>					
Election Period (6mo)	-6.59**	-0.5	-2.33	-2.23	-2.6
N=2276	(2.53)	(1.89)	(1.4)	(1.45)	(1.43)
<b>Non-NATO</b>					
Election Period (6mo)	-2.44	-3.3	-6.37*	-6.34*	-5.51
N=784	(7.32)	(3.1)	(2.94)	(3.03)	(2.95)
<b>Experienced Casualties</b>					
Election Period (6mo)	-7.83*	-1.71	-4.22*	-4.35*	-4.4*
N=1807	(3.08)	(2.34)	(1.76)	(1.85)	(1.79)
<b>No Casualties</b>					
Election Period (6mo)	-0.48	-0.01	-1.58	-1.1	-1.55
N=1253	(4.31)	(2.01)	(1.73)	(1.82)	(1.75)

Table A10: Regressions of average number of troops per million citizens on indicator variable for whether an election would be held within six months of a given month. \*\*\* =  $p < .001$ , \*\* =  $p < .01$ , \* =  $p < .05$ .

Regime Type	Troops	Troops/mil	Casualties	Data Months	# Elections
Democracies	66707	1751	39	2970	157
Non-Democracies	324	628	0	229	0

Table A11: Descriptive statistics for total contributors to Afghanistan, by regime type: average number of troops/month, average number of troops/month for every million citizens of the state, average number of casualties/months, number of months for which data is available, number of election events in the data.

	1: Bivariate	2: Country FE	3: Country, Year FE	4: Country, Year-Month FE
<b>Democracies</b> (Total Troops)				
Election Period (12mo)	-1655.78*** (380.45)	-505.86 (268.14)	-742.23** (258.76)	-755.83** (265.29)
N=2831				
<b>Democracies</b> (Troops/million)				
Election Period (12mo)	-6.12*** (1.81)	-0.83 (1.37)	-3.42** (1.06)	-3.39** (1.08)
N=2831				

Table A12: Regressions of average number of troops and average troops per million citizens on indicator variable for whether an election would be held within a year of a given month only democracies. \*\*\* =  $p < .001$ , \*\* =  $p < .01$ , \* =  $p < .05$ .

<b>Fighting Seasons and Election Timing</b>				
	1: Election	2: Election (Country, Year-Month FE)	3: Election Period (12mos)	4: Election Period (12mos, Country, Year-Month FE)
Winter/Non-Fighting Season (OLS)	-0.0096 (0.0062)	0.025 (0.10)	0.0033 (0.017)	-0.016 (0.26)
Winter/Non-Fighting Season (Logit)	-0.33 (0.21)	-1.18 (1.19)	0.016 (0.078)	-0.11 (1.28)

Table A13: OLS and logistic regressions of indicator variables of an election (Models 1 and 2) and of indicator variables of an election period of 12 months preceding an election (Models 3 and 4) on a variable indicator for the winter (November-April), non-fighting season in Afghanistan.

## B Figures



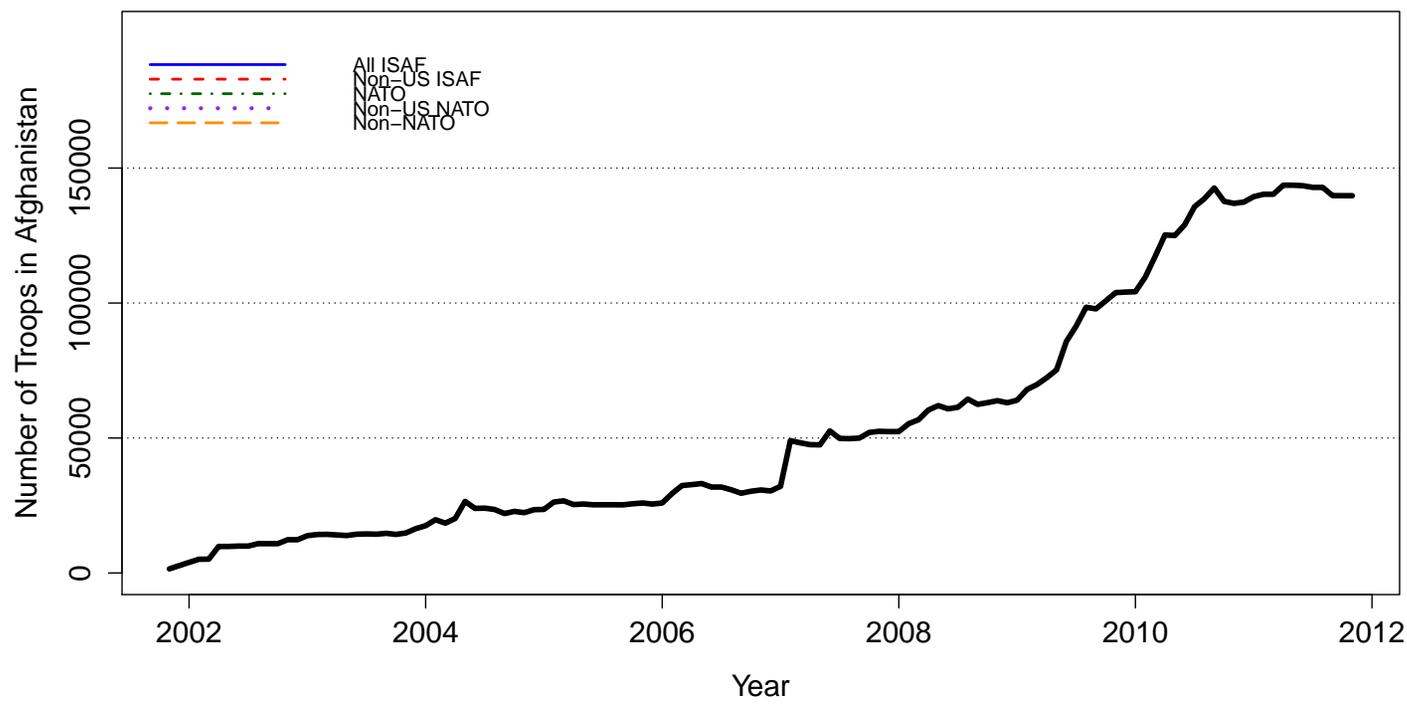
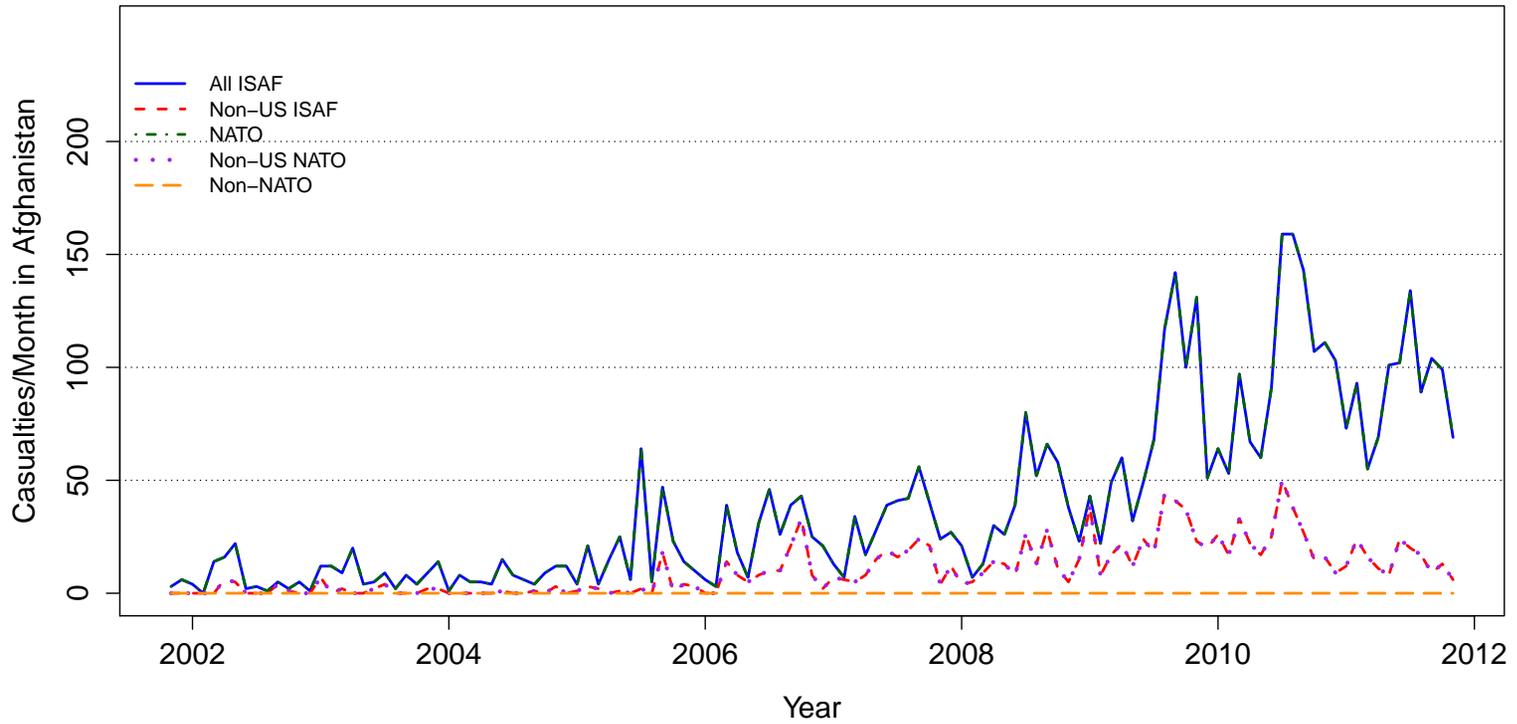


Figure A2: Troops/month in Afghanistan from October 2001 through October 2011.

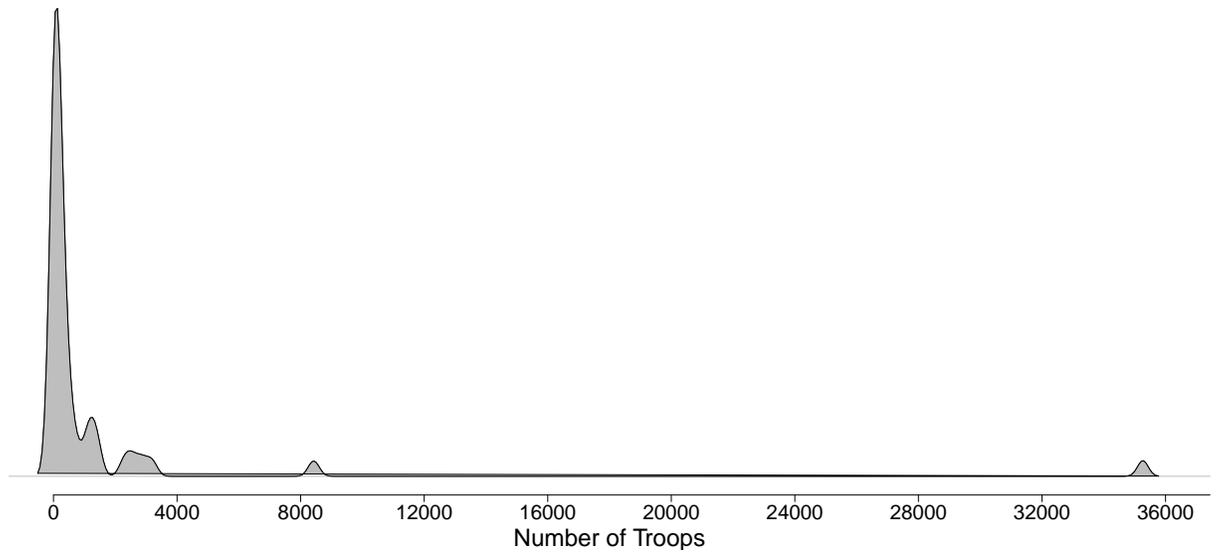
### Casualties/Month in Afghanistan over Time



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Figure A3: Casualties/month in Afghanistan from October 2001 through October 2011. Figure shows total overlap between casualties experienced by NATO and casualties experienced by all of ISAF, meaning NATO suffered all of the casualties, though not all NATO members suffered casualties (see Table A2).

(a) Total Troops



(b) Troops/Capita

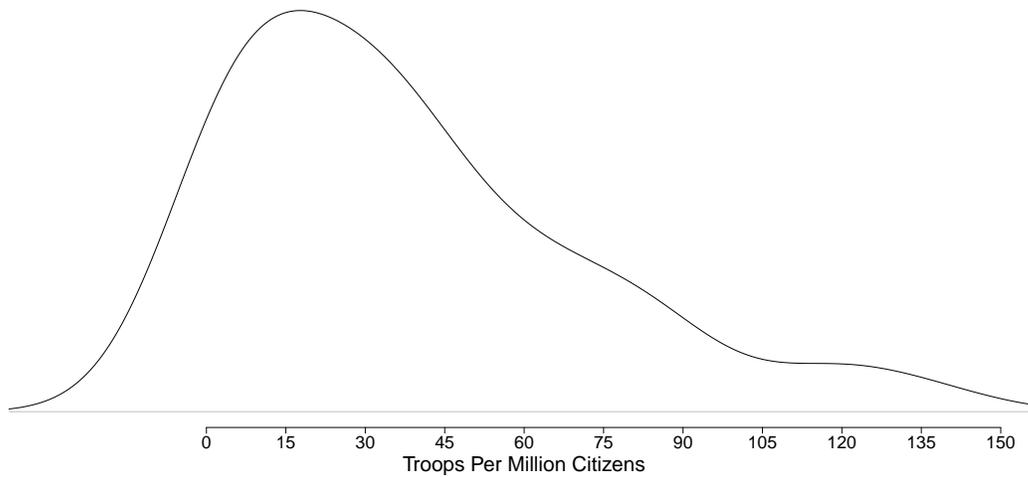
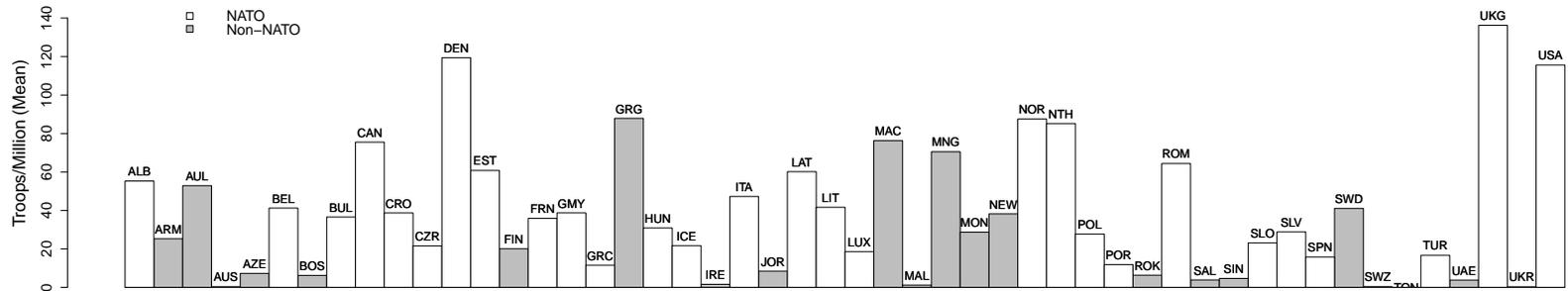


Figure A4: Density plots showing the distribution of troops to Afghanistan, in absolute number form as well as troops/capita. Values were pooled and averaged for each contributor and then displayed. Negative values are projections based on shape of density plot.



(a) Troops



(b) Troops/million

Figure A5: (a) Distribution of average number of troops per month for all contributors to Afghanistan from October 2001 through October 2011. United States contribution not shown to scale. (b) Distribution of average number of troops per month per million citizen for all contributors to Afghanistan from October 2001 through October 2011.