

# Does Electoral Proximity Affect Security Policy?\*

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February 18, 2014

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## Abstract

How do approaching elections affect the security policy states conduct? While international relations has paid some attention to this issue, existing theoretical work is scattered among many disparate arguments and the evidence does not allow researchers to identify causal relationships. We improve on both points. We argue that one problem likely faced by democratic policy-makers near elections is that of time-inconsistency, which arises when the costs and benefits of policy are not realized at the same time. Just as it does in economic policy, time-inconsistency should produce electoral cycles in security policy. We apply our argument to the case of allied troop contributions to War in Afghanistan. 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. We specify the role of election-related incentives in eliciting second-best security policy, and discuss the broad implications for foreign policy constrained by desirable but suboptimal accountability.

**Keywords:** democracy, elections, electoral proximity, political business cycles, international security, peace-keeping, Afghanistan

**Word Count:** 6164

# Introduction

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. Realists may argue that security policy is above the usual partisan fray of elections. Yet, time and again leaders and op-eds bemoan the difficulties electoral cycles create in the way of pursuing a coherent, consistent security policy in democracies. Does electoral proximity affect security policy?

Our goal in this paper is to develop theoretical mechanisms that unpack the relationship between elections and policymaking and to test these on an empirically relevant case. Specifically, we suggest that certain security policies may present a time inconsistency problem for elites: costs concentrate in the short-term but benefits accrue only over time. Because of governments' informational advantage, voters cannot immediately detect if a policy is successful or not but must, instead, rely on observable proxies such as casualties figures. Competent incumbents can achieve national security goals at lower cost and will want to signal their ability by keeping casualties low. Less competent incumbents, on the other hand, will also want to keep casualty figures low before elections but are less likely to be able to achieve that while leaving the ultimate objective uncompromised. Thus, as elections near, on average, decision-makers will underinvest in security policies with short-term costs and long-term benefits.

To test our argument, we examine a case in which a large panel of observational data offers us a rare opportunity to identify econometrically the effects of approaching elections. 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. Peacekeeping and peacemaking missions as in Afghanistan are especially vulnerable to time inconsistency problems. The costs of successful peacekeeping may bring rewards in

the long run, but are bound to increase casualties in the short run. 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, we develop systematic arguments about how proximity to voting day can affect the security choices of democratic elites. Second, we test what we find to be the most coherent and persuasive of these arguments, one centered around time-inconsistency. Third, we also spell out some of the implications of accountability through the ballot box for the consistency and coherence of the security policies democracies pursue.

## Security Commitments in the Shadow of Approaching Elections

When it comes to security policy, scholarship has often focused on international-level factors. Traditionally, structural theories often ignored almost entirely the effect of domestic politics. 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. The international system provides states different incentives and disincentives to pursue certain kinds of foreign policy, including troop commitments abroad. Since elections do not alter these incentives in one way or another, we should not expect them to affect security policy in a consistent pattern (Mearsheimer 2001; Waltz 1979).<sup>1</sup> Moreover, states have long-term obligations to

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<sup>1</sup>These scholars may offer two caveats to this logic. First, the forces of the international system may not apply to smaller powers. This objection would be particularly salient in the case of Afghanistan, in which only one contributing nation—the United States—can be considered a “great power.” Second, the

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 it, “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).

As the logic goes, these factors 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). While it may be difficult to predict how elites will react to every setback or success, we can surmise, based upon this logic, that elections will not factor into the troop commitment calculus significantly. This leads us to the following hypothesis:

**Hypothesis 1** *Elections will not produce variation in security policy because elites make security policy based on factors independent of the timing of elections*

## **Time Inconsistency of Security Policy, Competency Signaling, and Political Business Cycles**

Recent scholarship has moved beyond the realist emphasis on structural factors inherent in the international system. Emerging primarily from a crisis bargaining perspective, the audience cost literature hints at how electoral fortunes affect the making of security policy. Smith (1998), building upon Fearon’s (1994) seminal game, explicitly ties electoral vulnerability in democracies to the credibility of foreign policy commitments.<sup>2</sup> Audience cost theory

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conflict itself may be considered too insignificant (i.e., non-existential) to affect foreign policy-making in a substantial way.

<sup>2</sup>Schultz’s important work on opposition parties and foreign policy emphasizes how the opposition can help reveal information about a state’s preferences (Schultz 1999, 2001). The experimental turn has provided an even more finely-grained understanding of these dynamics (Tomz 2007). Recent work has also explored how party politics affect the conditions under which leaders (i.e., presidents) make policy (Trager and Vavreck 2011; Levendusky and Horowitz 2012), and the electoral costs in the U.S. that accrue from different policy choices (Karol and Miguel 2007).

incorporates elections as hypothetical mechanisms of accountability,<sup>3</sup> but provides no explicit treatment of the electoral time-horizon for elites in office. We can distinguish democracies from other states (Schultz 1998), but we do not learn how the proximity of elections matters for security policy.

Adherents to diversionary war theory also offer some promising hypotheses regarding the effect 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 (Smith 1996). However, the tradeoff between the costs and benefits of war and their impact on electoral politics remains underexplored. Moreover, diversionary war theorists cannot explain why the incentives facing politicians change as elections approach. That is, it is not clear why an incumbent would not engage in diversionary policies throughout their term. After all, public support is *always* important for democratic politicians.<sup>5</sup>

Our point of contention with existing approaches is not that we argue that elections do not matter in democracies. We are concerned with identifying the causal links between the shadow of elections drawing near and security commitments in democratic states that have regular elections. We argue that incumbents will make pre-election policy decisions that they believe will give them an electoral boost. 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

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<sup>3</sup>As Weeks (2008) and Kinne and Marinov (2012) point out, there is nothing necessarily unique about democratic elections in the generation of audience costs. Theoretically, leaders could be held accountable by other institutions as well.

<sup>4</sup>Morrow (1991) finds only mixed evidence in favor of the main hypothesis. More recent studies have alternatively found evidence both in favor as well as against and, in general, empirical findings are inconclusive. Gaubatz (1991) finds that democracies tend to fight wars more often early, rather than late, in their electoral cycles. For an extensive literature review and explanation for this uneven pattern, see Tarar (2006).

<sup>5</sup>See Bueno de Mesquita and Smith (2012) for a recent, much more detailed review of domestic explanations of international outcomes.

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>6</sup> Traditionally, economists argued that incumbent governments could engage in inflationary monetary policy prior to elections in order to lower unemployment (Nordhaus 1975; MacRae 1977).

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.<sup>7</sup> 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 incumbent engaging in inflationary monetary policy designed to lower unemployment in the short run (Holmstrom 1982; Lohmann 1998; Persson and Tabellini 2002).<sup>8</sup> On the other hand, adverse selection models (also called rational opportunistic models) posit instead that

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<sup>6</sup>Empirical evidence remains mixed about whether this phenomenon occurs at all, in the United States (Tuftes 1978; Grier 2008), developed countries (Alesina and Roubini 1992; Alesina et al. 1993; Canes-Wrone and Park 2012), developing countries (Schuknecht 1996), or not at all. For a review, see Drazen (2001).

<sup>7</sup>This brief review omits a discussion of the work done on partisan political competition and electoral cycles, including work that incorporates the rational approach adopted here (Alesina 1997).

<sup>8</sup>According to these scholars, this is the normatively undesirable moral hazard posed by electoral cycles.

political business/budget cycles are the result of informational asymmetries that exist between governments and voters (Rogoff and Sibert 1988; Rogoff 1990). 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. Thus, these authors argue that a separating equilibrium emerges where competent incumbents engage in cyclical policy-making but incompetent incumbents do not.<sup>9</sup>

In this paper, we adapt these rationalist perspectives to security policy. However, we suggest that security policy differs from monetary and fiscal policy in three important 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.<sup>10</sup> 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.

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. In peacekeeping, this means making fewer commitments to missions abroad, risking fewer lives. Because of governments' informational advantage,

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<sup>9</sup>For adverse selection theories, electoral cycles may be a normatively positive phenomenon in so far as they allow competent incumbents to signal their ability.

<sup>10</sup>This is an important objection raised to the work of Rogoff and Sibert (1988) and Rogoff (1990). These critics argue that the magnitude of informational asymmetries required for political budget cycles to exist do not apply to modern, developed democracies with transparent policymaking.



voters cannot immediately detect if a policy is successful or not. Instead, in the short term, they 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.

The structure of these incentives can explain different conjectures or theories in the literature on security and elections. For example, office-minded leaders can resort to war-mongering while, in a different context, being reticent to send troops to peacekeeping mission. Threatening war might be beneficial in the short run, with the benefits of sabre-rattling realized immediately while the costs accrue primarily in the future (i.e., post-election). Manning an ongoing peacekeeping mission, on the other hand, risks casualties – an unpopular outcome and a potential ground for allegations of incompetence – without offering immediate national security rewards. Moreover, our argument explains why leaders engage in those peacekeeping missions to begin with or, conversely, why they may be less likely to launch a war early in the electoral cycle. During non-election periods, investment in security policy that will pay off in the long run is worthwhile and will, come election time, be rewarded. After all, voters care about national security *as well as* the costs of achieving it. Conversely, the electoral benefits of a wars launched too early in the electoral cycle might not be harvested in time for elections. That is, the electoral benefits of launching a war may be stronger in election years, a prediction sometimes made in the literature.<sup>11</sup>

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<sup>11</sup>Our theory also provides a rationale for why an office-minded government might prefer to start larger

We formulate the following general hypothesis as an alternative to the base-line hypothesis of no variation around election time:

**Hypothesis 2** *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.*

## **An Application: Troop Contributions to Peace-Keeping Operations**

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. In the case of troops on a peace-keeping or peace-making mission, governments see the long-term value of such policies but, as elections near, they also become sensitive to the costs of sending troops abroad. Achieving peace is a long-term objective, and more troops now can make it more likely but only at the cost of potential casualties in the short term. Thus, in peace-keeping operations, where casualties are possible, we would expect to see evidence of the first part of Hypothesis 2.

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

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scale wars earlier in the electoral cycle. 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. Thus, if governments fear that wars might entail heavy casualties and last a long time before national security benefits begin to accrue, they would have an incentive to launch such wars during non-election periods to avoid signaling incompetence.

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 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. Once the immediate cost of troop deployments is reduced, we do not expect to see as much sensitivity in election years. We also formulate in testable form the proposition that public opinion toward sending troops should sour in the wake of incurring casualties.<sup>12</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.*

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<sup>12</sup>See Kreps (2010) on the relationship between public opinion and the war in Afghanistan in general.

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

We test these propositions next.

## 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”).<sup>13</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. 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.<sup>14</sup>

Table 1 summarizes the totals for six different and potentially theoretically-relevant groups of contributing states: all allies, non-US contributors, NATO, non-NATO, states that experienced casualties, and states that did not.<sup>15</sup>

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<sup>13</sup>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. A list of available communications is available from the authors upon request. This was particularly important for the US and the UK, for which the ISAF record lists only incomplete information since it excludes OEF contributions. In total, we gathered data on 50 different states.

<sup>14</sup>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). For more details on the ISAF mission, see <http://www.isaf.nato.int>.

<sup>15</sup>Tables 7 and 8 in online appendix list the descriptive statistics—monthly averages—for NATO and non-NATO contributors respectively.

[Table 1 about here]

Two patterns emerge from the data: NATO dominates troop contributions to Afghanistan and the United States dominates troop contributions among NATO states (see Figure 1).<sup>16</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.<sup>17</sup> The subsetting and per capita transformations effectively normalize the distribution of troops commitments, allowing us to be more confident that our findings would not be driven by outliers.

[Figure 1 about here]

For information about elections during this period, we utilized the National Elections Across Democracy and Autocracy (NELDA) database.<sup>18</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>19</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>16</sup>Non-NATO states account for less than three percent of the total contributions to OEF and ISAF and the vast majority of non-NATO states contribute fewer troops on average than the number by which NATO states *lower* their contribution in the run-up to elections. Moreover, not only are these states fewer in number (22 to NATO's 28) but they have also provided troops in Afghanistan for a shorter time, resulting in about third of the number of country-year-month observations for these states. Moreover, non-NATO contributors tend to undertake the missions with the fewest risks and lowest short-term costs, something to which the null casualties *total* for all such states can attest. This is affirmed by the similar finding for states that did not experience casualties, which includes minor NATO contributors.

<sup>17</sup>See Appendix for further graphical representations of this data, specifically Figures 7, 8, 9, and 10.

<sup>18</sup>Hyde and Marinov (2011).

<sup>19</sup>Figure 2, in the appendix, displays all of these elections. See also Table 9 for a listing of each election event.

presidential systems, only presidential elections; and for mixed systems, both legislative as well as presidential elections.<sup>20</sup>

In total, there were 157 leadership election events across all contributors. Our unit of observation is the country-year-month, (troops, elections) in a country for a particular month, 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.

What happens to troop contributions for peace-keeping operations as elections near in the contributing countries? To illustrate our design, Figure 2 shows all of the contributing countries’ elections, which are included in our analysis. Elections follow the electoral calendar and not the progress of the security operation. 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 a collapse or unrelated issue. The one exception is the Netherlands’ 2010 election, which we excluded from our analysis for this reason. Looking at the effect of elections on troop commitments provides us with design-based control observed confounders and unobserved confounders.

To draw an analogy from the experimental literature, the idea is that a 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 period”). The reason for this is simply that since election timing occurs exogenously to the war in Afghanistan, we can say that the treatment—whether a state finds itself in an election period or not—is

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<sup>20</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).

as-if randomly assigned. This allows us to identify the effect of elections in our data.

[Figure 2 about here]

## Findings

We leverage the exogeneity of elections in two ways: first, we utilize basic difference-of-means t-tests to compare states inside and outside of the run-up to elections and second, we conduct ordinary least squares regressions with minimal controls to correct for possible weaknesses of a basic comparison of means. While difference-of-means t-tests are transparent, they are vulnerable to skewed distributions, a particular concern with the case of troop commitments to Afghanistan. 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 to demonstrate that our findings hold even if we relax strict exogeneity assumptions.

If our exogeneity assumption holds, a basic comparison of means between treatment groups should be sufficient for causal inference. The as-if random treatment assignment assures us that covariates are balanced between groups. For this reason, we do not need control for observed differences parametrically using a host of familiar control variables such as GDP, population, and others.<sup>21</sup>

However, 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 exam-

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<sup>21</sup>See Sekhon (2009); Dunning (2010).

ple, 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. Contrary to the conventional wisdom, fixed effects in these models should actually increase efficiency. Assuming a large enough number of observations and that time periods are large enough, adding both country- and year-month fixed effects will not cause the standard errors to balloon.

## Comparison of Means

We begin by conducting a basic t-test comparing the troop commitments by governments in the pre-election period. More informally, we compare the mean number of troops deployed in each pre-election month to those outside of the pre-election the period entirely. For each subgroup we compare a year-long pre-election period with non-election periods.<sup>22</sup> While comparisons of average absolute troop level can tell us much about how elections affect the capabilities of security forces on the ground, they can provide some misleading insight into how elites make policy in the run-up to elections. In particular, there are two areas of major concern: effects might be driven by large troop contributing states (i.e., outliers) and effects might be driven by large population states that change troop commitments at such high levels as to bias the average causal effect. To address these concerns, we run the same t-tests as before but operationalizing the outcome variable as troops per million citizens instead of an absolute value. As Figure 1b shows, this produces a near-normal distribution across all allies.

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<sup>22</sup>We also conduct robustness checks with a six-month election period. Results change only slightly for these two measures of election period length, with the twelve month period exhibiting a slightly larger effect. This makes intuitive sense: if one accepts our argument that in the full year leading up to elections, governments scale down the number of troops they have committed to Afghanistan, then the six month measure will miss half of this period.



[Table 2 about here]

The results of the t-tests 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. Using both absolute troop numbers as well as troops per capita, we find a statistically and substantively significant negative effect of approaching elections for all allies, NATO states, and contributors that experienced casualties. That is, all allies tend to have, on average, about 1,300 troops fewer troops per month committed during the election period. The effect is the largest for contributors with casualties, which decrease their troops by an average of more than 2,200 troops during election periods. We can say with some confidence that this effect is not driven solely by the United States since non-American states lowered their contributions by about 200 troops during a given election period and NATO member states, as a group, lowered their contributions by about 2,000 troops. The effects hold across the different subgroups and t-tests using the troops per capita measure. That is, states commit fewer troops per capita to Afghanistan in the run-up to elections.

This effect does not apply to non-NATO states and states without casualties, for which we find, instead, a statistically insignificant or borderline significant positive effect. The findings for non-NATO states remain noisy.<sup>23</sup> The results of the t-tests support, to a degree, Proposition 3, security cycles apply more strongly to cases where policy-makers run non-trivial risks. When the troops are going to be safe, there is little reason to mind the approaching ballot.

We hasten to add that, since most of the period we study features rising troop levels, what that means in practice is that close to elections, countries tend to increase their troop levels less. This makes sense: faced with a demand for more troops necessitated by the operation, policy-makers respond by postponing the real increases for when elections are not

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<sup>23</sup>Moreover, electoral proximity in non-NATO states is also negatively associated with troops committed to Afghanistan per million citizens in all but two of ten models. However, it is difficult to draw further conclusions from these results because the confidence intervals are large and the p-values are quite high.

around the corner.

## Regression Results

In addition to difference-of-means t-tests, we also conduct a series of regressions of our outcomes of interest—number of troops and troops per million citizens—on whether a country is within twelve months of an election. For each of the six subpopulations of interest we run five different model specifications on each of the two outcomes (i.e., absolute numbers of troops and troops per millions of citizens).<sup>24</sup>

$$Troops_{i,t} = \beta_0 + \beta_1 ElectionPeriod_{i,t} + \epsilon_{i,t} \tag{1}$$

$$Troops_{i,t} = \beta_0 + \beta_1 ElectionPeriod_{i,t} + \alpha_c + \epsilon_{i,t} \tag{2}$$

$$Troops_{i,t} = \beta_0 + \beta_1 ElectionPeriod_{i,t} + \alpha_c + \alpha_y + \epsilon_{i,t} \tag{3}$$

$$Troops_{i,t} = \beta_0 + \beta_1 ElectionPeriod_{i,t} + \alpha_c + \alpha_m + \epsilon_{i,t} \tag{4}$$

$$Troops_{i,t} = \beta_0 + \beta_1 ElectionPeriod_{i,t} + \beta_2 LogUSTroops_{i,t} + \alpha_c + \epsilon_{i,t} \tag{5}$$

We write out each model specification using scalar notion instead of matrix notation because of the limited number of terms on the right-hand-side of the equations and to illustrate the differences between the five. The first model is simply a bivariate regression of the outcome measures on the election-period indicator, producing the same results as the t-test finding but are worth repeating here to provide a baseline by which to compare the other model specifications. 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

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<sup>24</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.

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 level of US troops may serve as a useful proxy for the troop requirements of the operation.<sup>25</sup>

The regression results mirror the findings of the t-tests. 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. Tables 3 and 4 show the results in tabular form. These results hold when we operationalize the dependent variable as troops per million citizens. Again, 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 as well troops per million citizens. Compared to the full sample of states, the magnitude of the effect is much smaller for the number of troops and slightly smaller in terms of troops per capita.

[Tables 3 and 4 about here]

As in the case of the basic t-tests, 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). Figures 3 and 4 illustrate graphically the coefficient estimates.

[Figures 3 and 4 about here]

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<sup>25</sup>We take the natural log because US troop commitments are not normally distributed.

The large-N analysis discussed above provides some initial 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 almost 2,300 troops less during election years compared to other periods ( $p < 0.001$ ). 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. The intuition is that contributors that suffered casualties have their troops stationed in more dangerous areas, at constant risk of more casualties. The leaders of these states will be especially concerned about signaling incompetency through increased casualties. For states that do not expose their troops in a way that puts their troops in jeopardy, the war in Afghanistan is a far less costly enterprise. That is, the time inconsistency problem that fuels cyclical behavior in our theory simply does not apply.

We find support for the main proposition derived from the theory, that incumbent governments lower their troop contributions as elections approach. Our research design leaves us relatively confident that we can identify a causal effect with a basic comparison of means. Nonetheless, the principal finding holds across models that control for unique features of contributors and time trends. As our theory would predict, the effect is particularly strong for larger contributors to the war effort (i.e., NATO states).

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.

## Discussion

Next, we explore these troop variations further. In particular, we demonstrate that the large declines in troop commitments as elections approach can be attributed to the time inconsistent costs and benefits of peacekeeping. 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.

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 5 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  $-0.42$ , a notable decrease given the average of 1.4 casualties per month in the overall sample.

This 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. 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. Both regressions include country and year-month fixed effects to adjust for changing battlefield dynamics and different country casualty levels. 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 on Table 5.

[Table 5 about here]

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.

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 6 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 the per cent attitudes favoring withdrawal. Given that about half of the domestic publics, on average, want withdrawal to begin within 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.

[Table 6 about here]

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

The third column of Table 6 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 per cent 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 per centage 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 per centage point, than outside of it. This would be expected if domestic political debate is especially sensitive to casualties as elections near, and the competence of the government becomes an issue of special concern.

Thus, local awareness of the war effort, and assessment of the operation, changes with casualties. To the extent that these judgements 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.

The arguments we outline suggests further testable propositions. For example, since the withdrawal effect is particularly pronounced in states that experience casualties, we would expect these elevated findings to hold for individual contributors and, aggregating down, units that fight in the deadliest provinces of Afghanistan, like Helmand or Kandahar. Geo-coded data on troop levels would also show whether countries are rotating troops to various risk areas and, if so, how such rotations relate to elections. This would tend to depress our findings. In another possible extension of this work, it is often noted that voters trust the credentials of right-wing leaders on national security than those of leaders from the left. This

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

may allow some leaders to get away with the short-term costs of policy, and may dampen their incentive to prove competence through foreign policy adventures as elections near. We would expect the election-nearing effect to be more pronounced for governments on the left.

## Conclusion

Our findings suggest that a traditional realist story in which elites determine security policy independent of electoral results may not explain the fluctuations in policy for cases like the war in Afghanistan. Our theory holds that competent incumbents, interested in re-election, can signal competency by achieving security goals while keeping casualties low by lowering troop levels. Incompetent incumbent, facing the same electoral incentives, will also reduce their troop commitments. While we would expect elections to produce similar behavior in both types of incumbents, an implication of this formulation is that incompetent incumbents, in expectation, achieve fewer national security goals than competent ones.

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. At present, we cannot adjudicate between an argument suggesting that voters are myopic and one that suggests incumbents are aiming to convince skeptical publics of their skill by seeking to minimize foreign policy costs around election time. What we seek to accomplish with this paper is to take a step further, while inviting further debate on this issue. We believe that it is important to connect different behavior around election time to time-inconsistency in the incentives faced by leaders. As to whether this is due to a rational public reacting or to a myopic domestic public opinion, is a matter of further debate.<sup>28</sup>

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<sup>28</sup>We can imagine survey-experiments, in the lab or in the field, as promising ways to arbitrate among competing mirco-foundations of the time-inconsistency logics we outline.



Different institutional features of democratic states may produce variation in the time-inconsistent behavior we would expect. It is possible the makeup of the incumbent government might affect the negative association we suppose holds for governments incurring losses. For instance, does a coalition government mitigate the effect? Similarly, what type of mandate and independence does the executive have in making troop commitment decisions? Is he or she restrained by a mandate or legislative oversight?

Ultimately, the best security policy is probably not far from the realist “ideal” when it comes to variation around elections. This is also not to say that the citizens of democracies do not benefit from the electoral accountability imposed on incumbents. In some ways, our argument reinforces this theme: voters may be able to select better leaders, at a cost, by observing policy choices. We show one way in which this could potentially happen in the area of security policy.

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# Tables

<b>Group</b>	<b>Troops/month</b>	<b>Troops/mil</b>	<b>Casualties/month</b>	<b>Data Months</b>	<b># Elections</b>
All contributors	67031	2380	39	3199	157
All non-US contributors	31765	2264	10	3078	155
NATO member states	63815	1368	39	2415	102
Non-NATO states	3215	1012	0	784	55
Experienced casualties	63037	1189	39	1831	79
No casualties	3994	1191	0	1368	41

Table 1: Descriptive statistics for total contributors to Afghanistan, by group: 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.

	<b>Election Period</b>	<b>Out of Election Period</b>	<b>Diff. of Means</b>	<b>p-value</b>
All contributors (troops)	1234	2570	-1336	0.000
All contributors (troops per capita)	43	47	-4	0.021
	(N=2138)	(N=946)		
Non-US contributors (troops)	674	869	-195	0.000
Non-US contributors (troops per capita)	42	43	-1	0.501
	(N=2041)	(N=922)		
NATO (troops)	1484	3494	-2010	0.000
NATO (troops per capita)	45	52	-7	0.000
	(N=1542)	(N=758)		
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	4209	-2262	0.000
Experienced Casualties (troops per capita)	51	58	-7	0.001
	(N=1271)	(N=560)		
No Casualties (troops)	199	169	31	0.066
No Casualties (troops per capita)	32	32	1	0.808
	(N=867)	(N=386)		

Table 2: 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 for all contributors, non-US contributors, NATO member states, non-NATO states, states that experienced casualties, and states that did not experience casualties.



	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) N=3084	-1336.36*** (357.23)	-495.84† (255.7)	-734.93** (246.87)	-750.92** (252.71)	
<b>Non-US contributors</b>					
Election Period (12mo) N=2963	-194.91*** (57.99)	22.47 (20.59)	-17.37 (16.92)	-16.51 (17.25)	-23.42 (17.11)
<b>NATO</b>					
Election Period (12mo) N=2300	-2009.94*** (465.35)	-610.06† (328.92)	-882.27** (317.15)	-904.19** (327.6)	
<b>Non-NATO</b>					
Election Period (12mo) N=784	44.67 (27.84)	-19.46 (13.69)	-35.91** (12.28)	-39.61** (12.69)	-34.69** (12.36)
<b>Experienced Casualties</b>					
Election Period (12mo) N=1831	-2261.53*** (591.47)	-812.48† (422.6)	-1166.7** (402.69)	-1232.07** (420.82)	-
<b>No Casualties</b>					
Election Period (12mo) N=1253	30.77† (17.1)	7.01 (9.38)	-7.53 (7.87)	-7.65 (8.22)	-4.35 (7.81)

Table 3: Regressions of average number of troops on indicator variable for whether an election would be held within a year of a given month. \*\*\* =  $p < .001$ , \*\* =  $p < .01$ , \* =  $p < .05$ , † =  $p < .1$ .

	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) N=3084	-3.99† (2.07)	-0.56 (1.33)	-3.21** (1.05)	-3.21** (1.07)	
<b>Non-US contributors</b>					
Election Period (12mo) N=2963	-1.12 (1.91)	1.12 (1.09)	-1.2 (0.86)	-1.13 (0.87)	-1.48† (0.88)
<b>NATO</b>					
Election Period (12mo) N=2300	-7*** (2.04)	-0.78 (1.55)	-3.61** (1.16)	-3.59** (1.19)	
<b>Non-NATO</b>					
Election Period (12mo) N=784	0.35 (5.7)	0.36 (2.55)	-2.03 (2.41)	-2.33 (2.5)	-1.67 (2.43)
<b>Experienced Casualties</b>					
Election Period (12mo) N=1831	-7.06** (2.47)	-2.26 (1.91)	-5.61*** (1.44)	-5.86*** (1.5)	
<b>No Casualties</b>					
Election Period (12mo) N=1253	0.64 (3.41)	2.14 (1.67)	-0.01 (1.45)	0.08 (1.52)	0.34 (1.45)

Table 4: Regressions of average number of troops per million citizens on indicator variable for whether an election would be held within a year of a given month. \*\*\* =  $p < .001$ , \*\* =  $p < .01$ , \* =  $p < .05$ , † =  $p < .1$ .

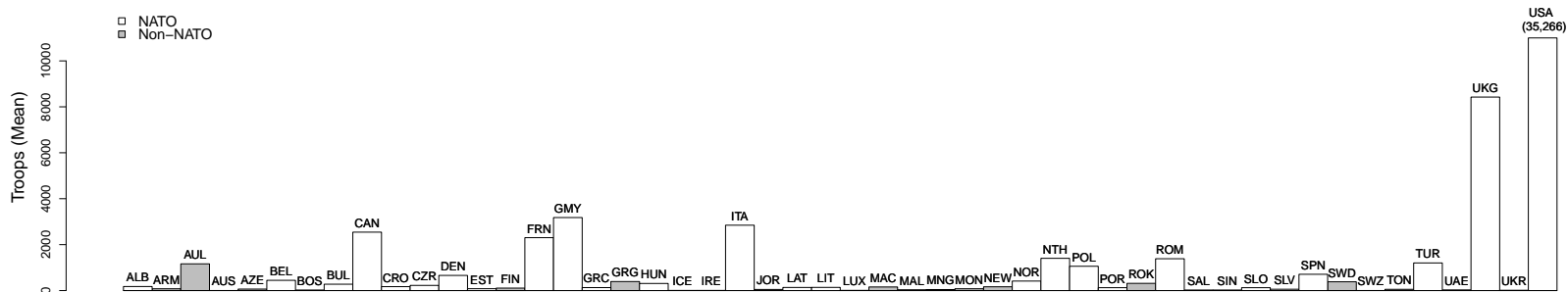
<b>Troop Reductions and Casualties</b>		
Dependent Variable	Casualties	
Effect of $\sum_{j=0}^{12} Election_{i,(t+j)}$ (Standard Error)	-0.423** (0.206)	0.127 (0.139)
Effect of $Troops_{i,t}$ (1000's) (Standard Error)		0.885*** (0.010)
	country year-month FE	
$N$	3930	3084

Table 5: Ordinary least squares estimates of effects of approaching elections and troop levels on country's casualties in Afghanistan. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

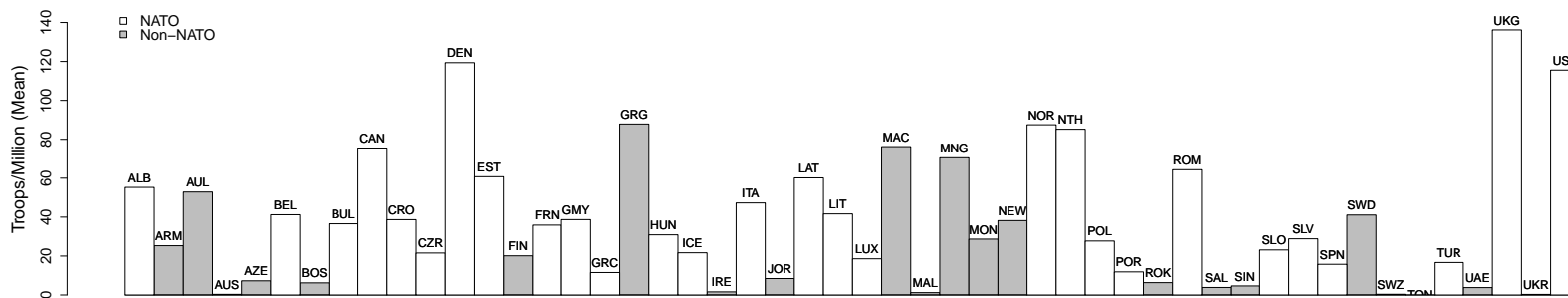
Public Opinion and Casualties (Monthly Data)				
Dependent Variable	% want withdrawal (Transatl. Trends)	% want withdrawal (Pew)	% believe UK gvt winning (Yougov)	% Dutch aware of Afghan mission (Dutch MoD)
Effect of $Casualties_{i(t-1)}$ (Standard Error)	0.217*** (0.073)	0.132*** (0.043)	-0.075 (0.049)	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 6: 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. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

# Figures



(a) Troops



(b) Troops/million

Figure 1: (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.

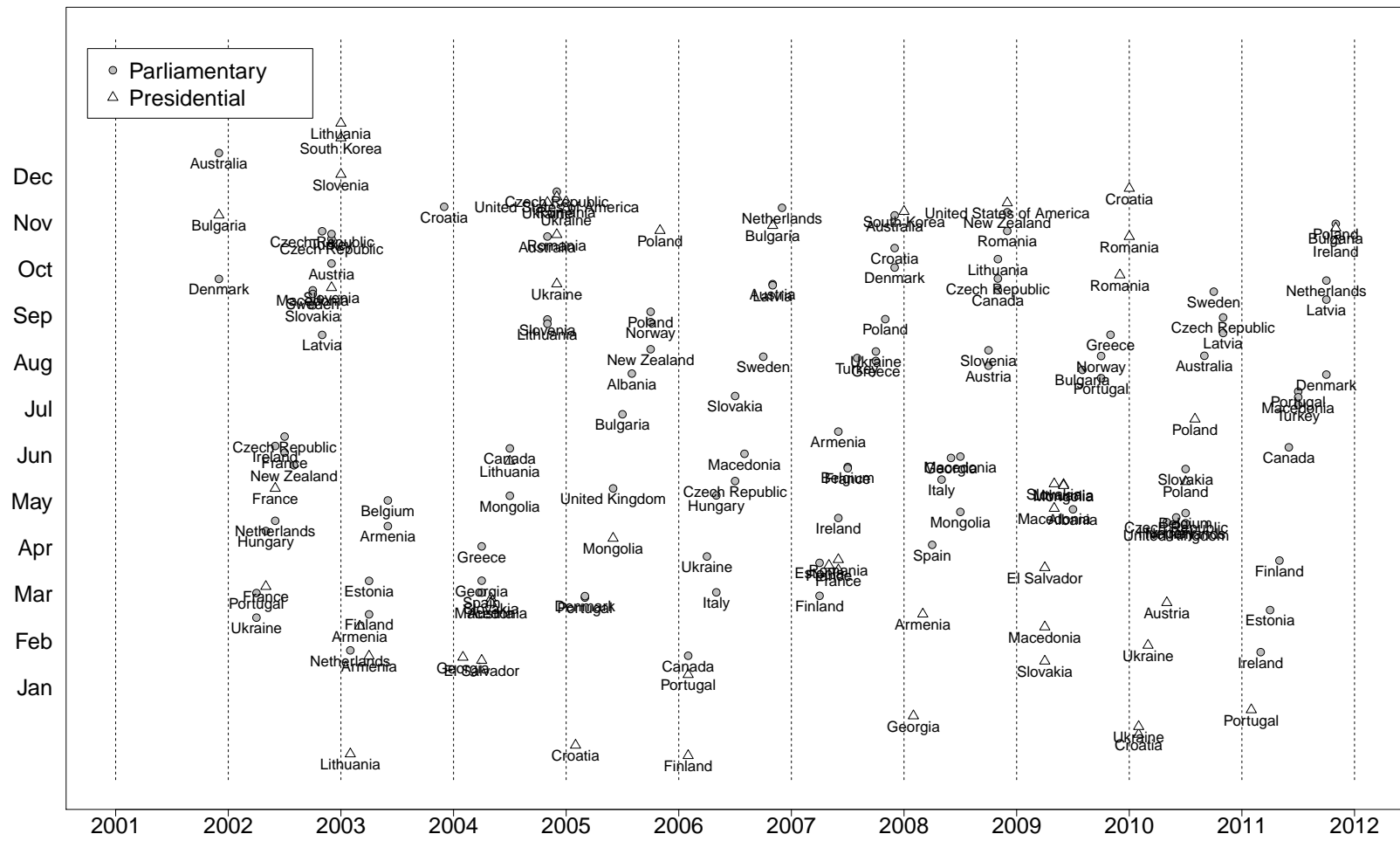
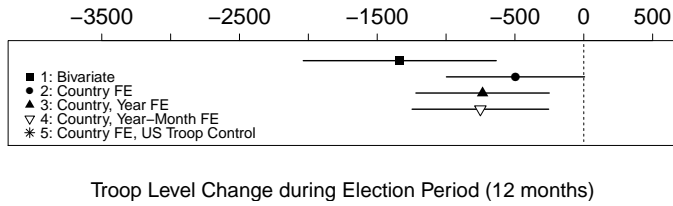
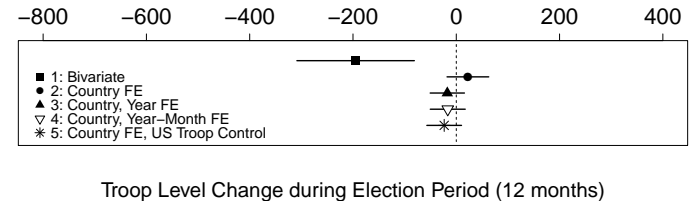


Figure 2: Elections for the October 2001 through October 2011 period.

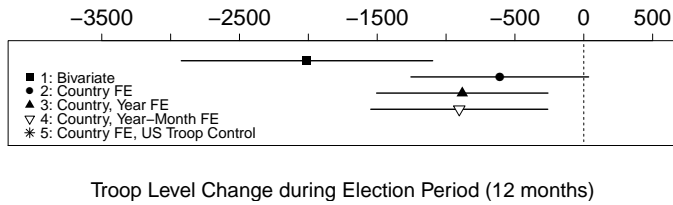
(a) All contributors



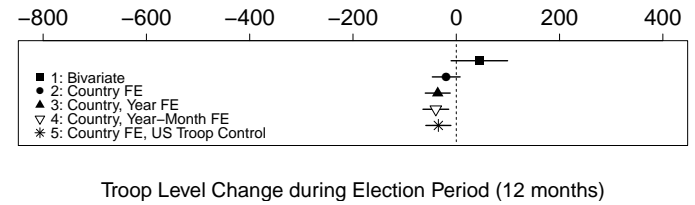
(b) Non-US contributors



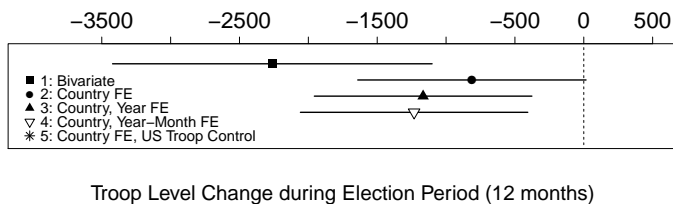
(c) NATO



(d) Non-NATO contributors



(e) Experienced casualties



(f) No casualties

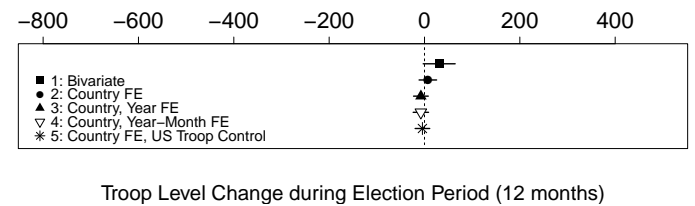
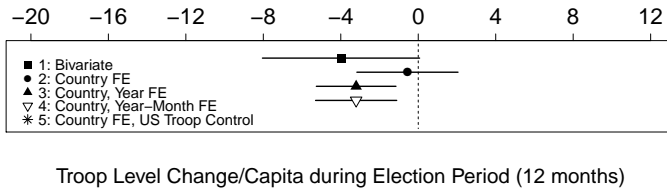


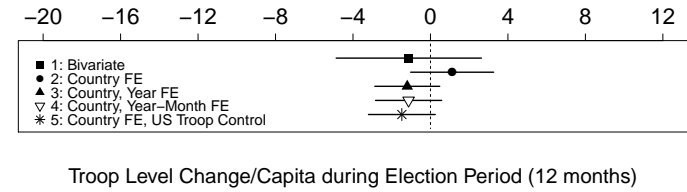
Figure 3: Coefficient plots of OLS regressions. In each panel, points indicate regression coefficients and line segments 95% confidence intervals for each group analyzed.



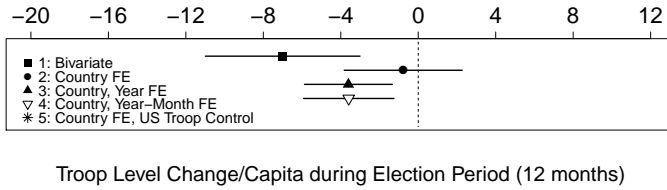
(a) All contributors



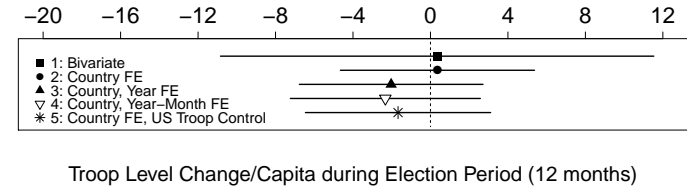
(b) Non-US contributors



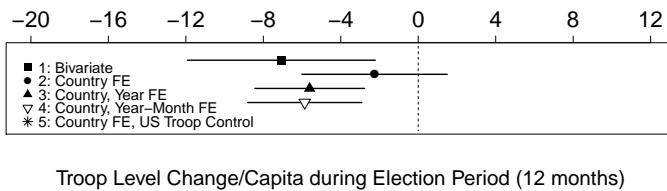
(c) NATO



(d) Non-NATO contributors



(e) Experienced casualties



(f) No casualties

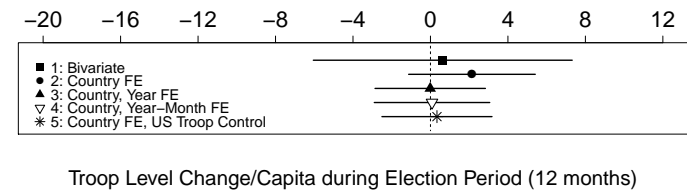


Figure 4: Coefficient plots of OLS regressions. In each panel, points indicate regression coefficients and line segments 95% confidence intervals for each group analyzed.

## Appendices for online distribution

# A Tables

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 7: 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 8: 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.

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	United States of America	2004
Czech Republic	2006	Ireland	2007	Portugal	2011	United States of America	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 9: Each election event in the dataset.

## B Figures

## Number of Troops in Afghanistan over Time

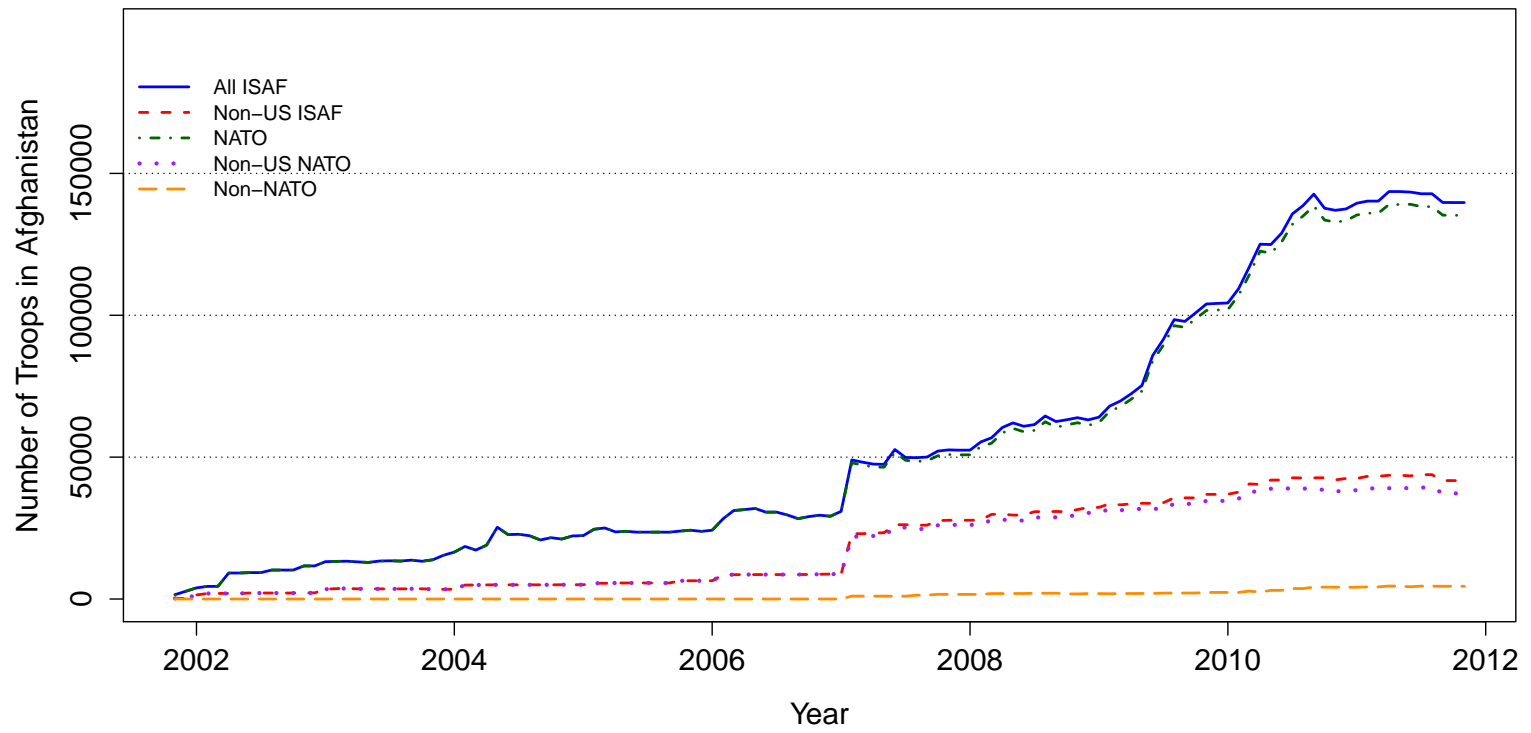


Figure 5: Troops/month in Afghanistan from October 2001 through October 2011.



### Casualties/Month in Afghanistan over Time

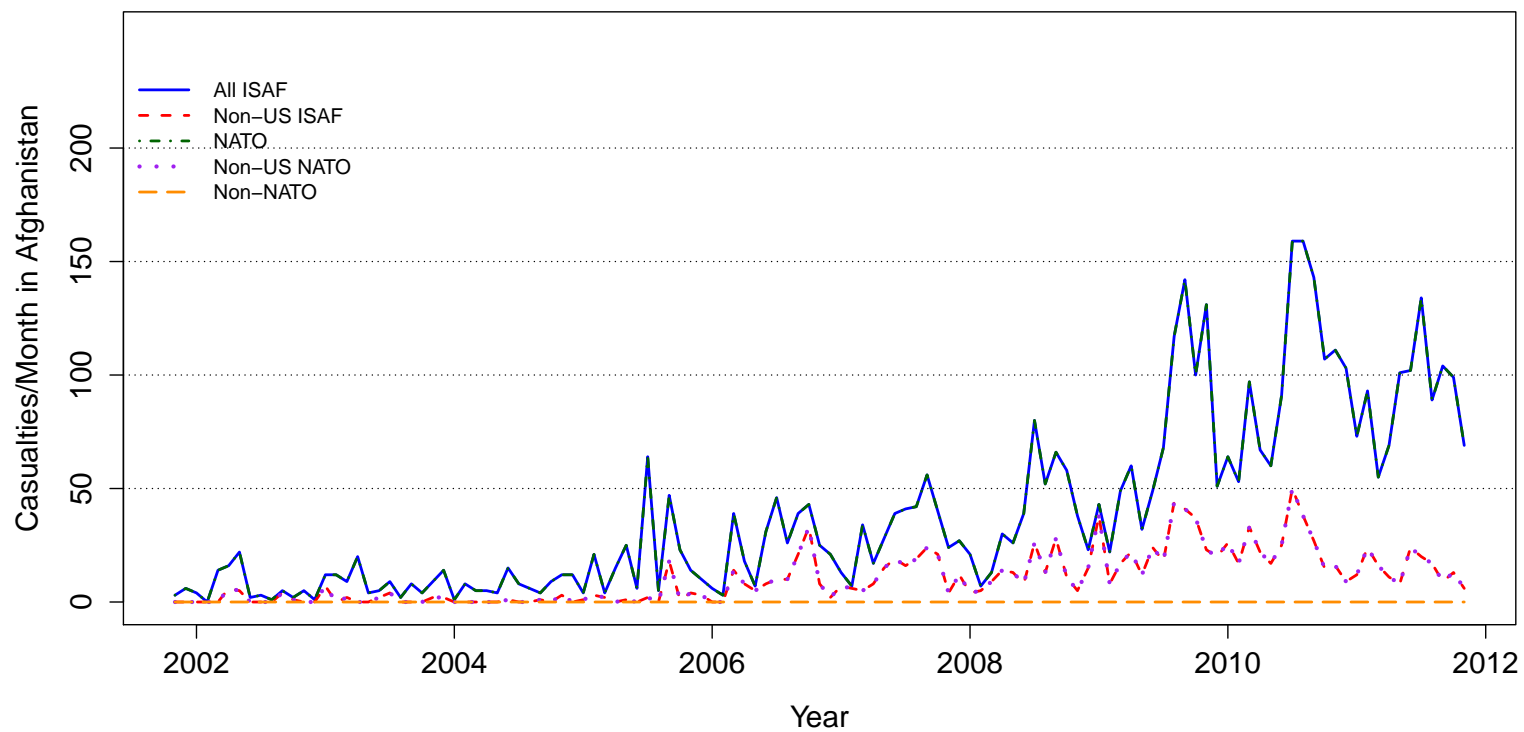


Figure 6: Casualties/month in Afghanistan from October 2001 through October 2011.

### Distribution of Troop Levels among non-US NATO Member States

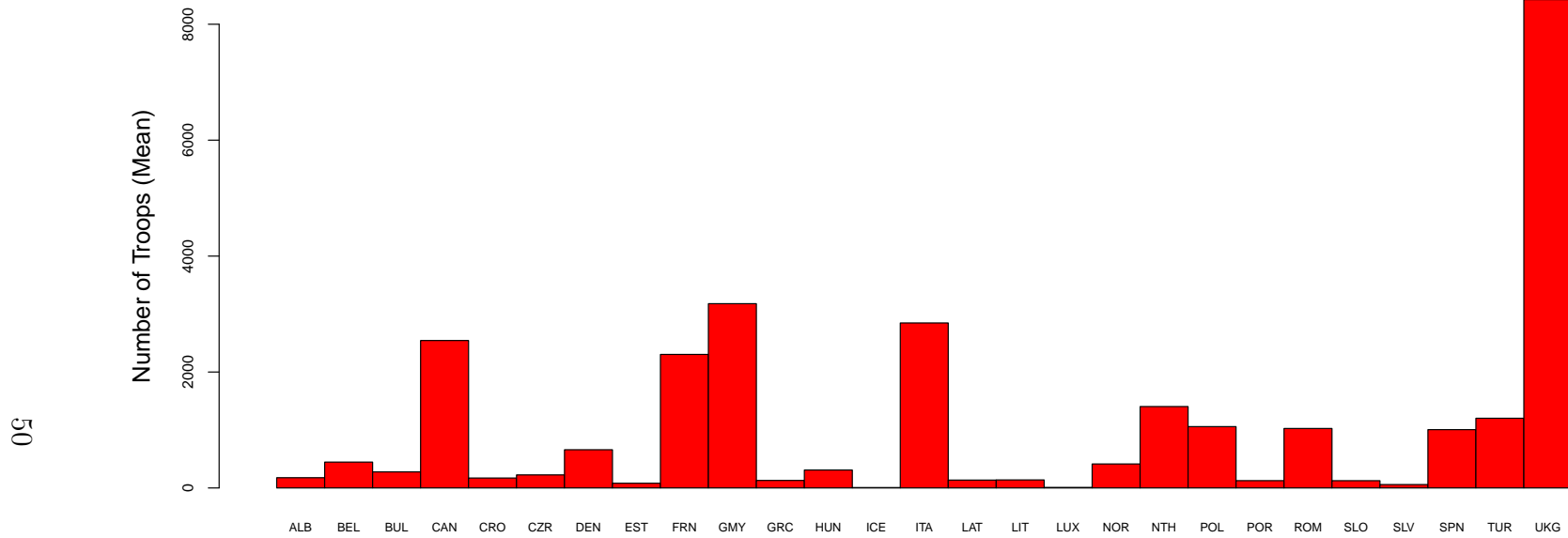


Figure 7: Distribution of average number of troops per month for all non-US NATO contributors to Afghanistan from October 2001 through October 2011 .

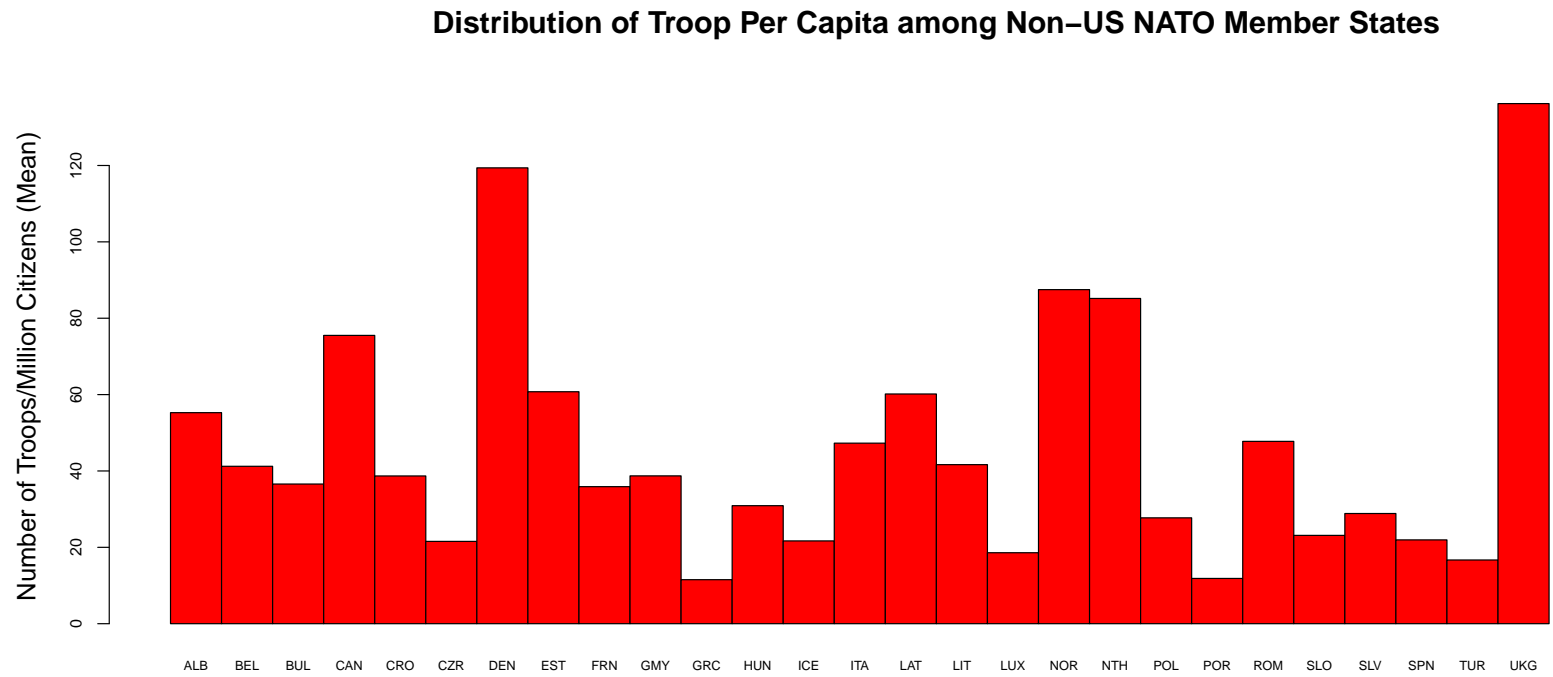
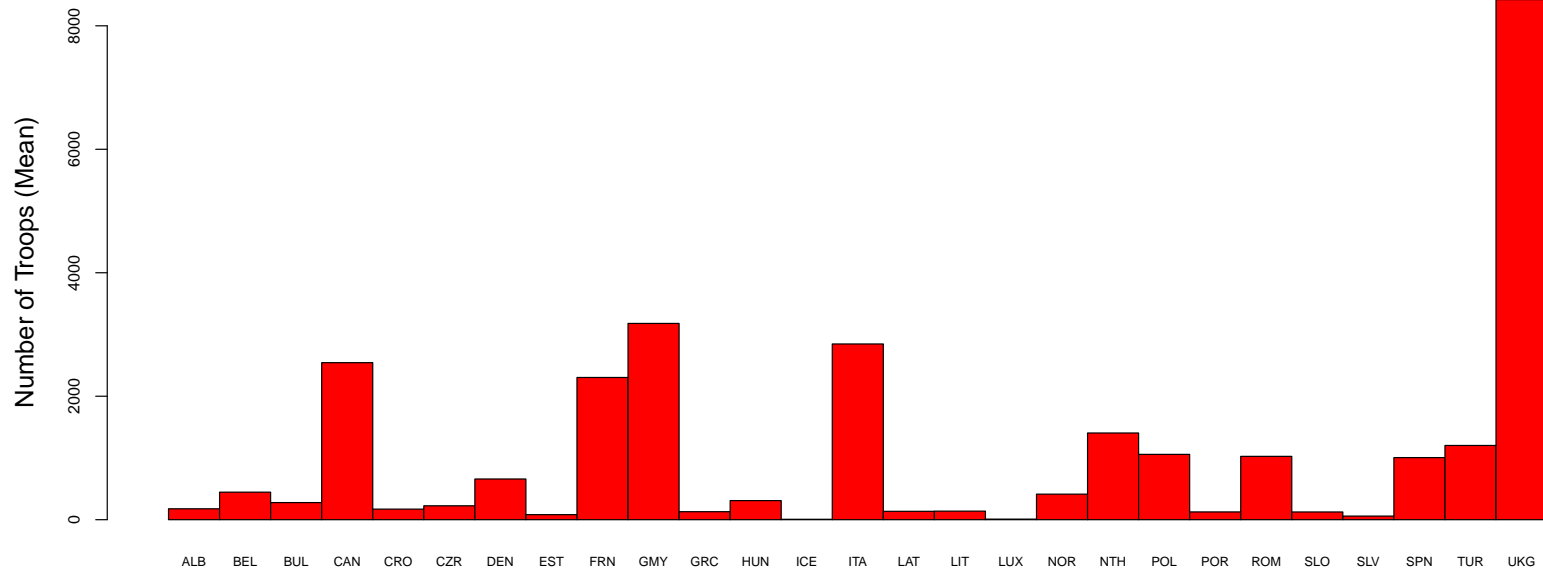


Figure 8: Distribution of average number of troops per month per million citizen for all non-US NATO contributors to Afghanistan from October 2001 through October 2011.

### Distribution of Troop Levels among non-US NATO Member States



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Figure 9: Distribution of average number of troops per month for all non-NATO contributors to Afghanistan from October 2001 through October 2011 .

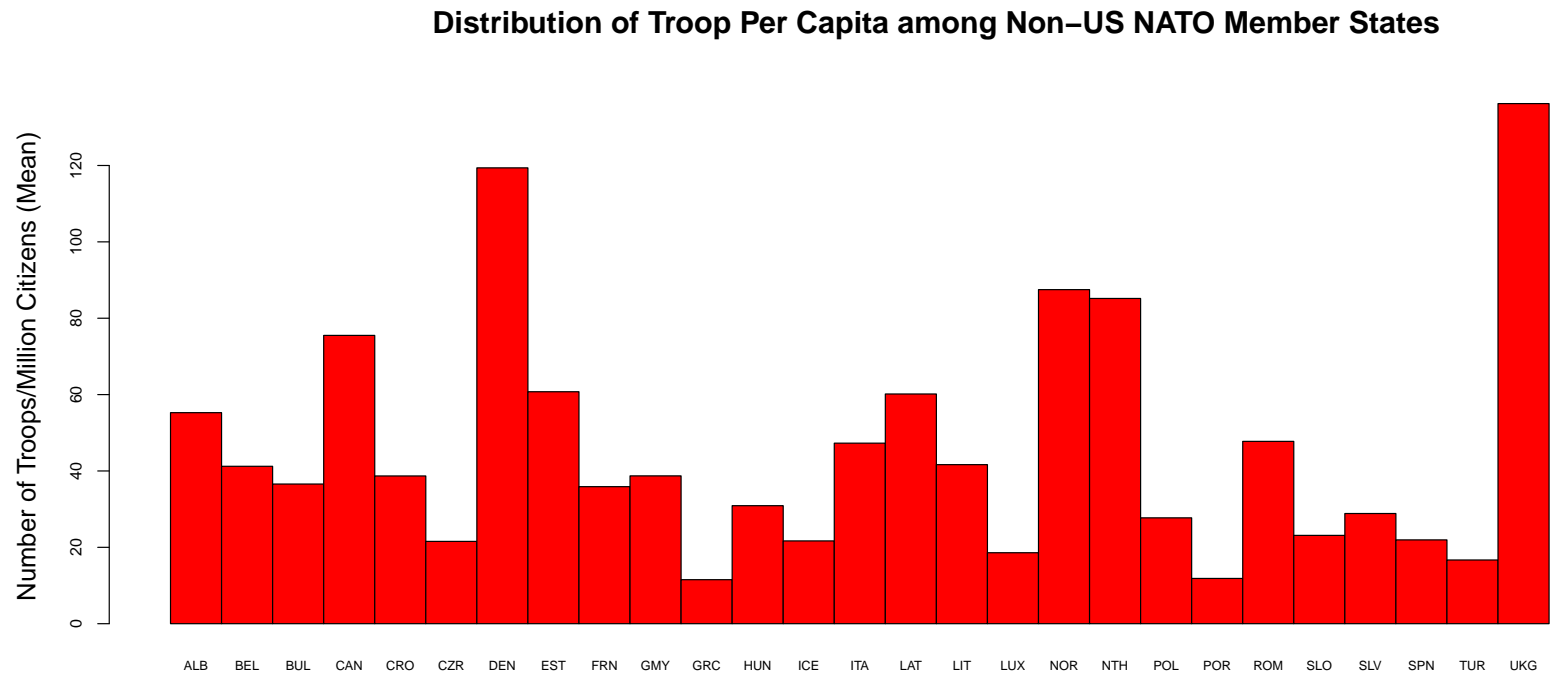


Figure 10: Distribution of average number of troops per month per million citizen for all non-NATO contributors to Afghanistan from October 2001 through October 2011.

## C Data Sources

*The following is a list of the data sources used to construct the dataset elections\_troops.dta.*

### C.1 ISAF Placemats

*Description* Data on troop contributions of each ISAF state downloaded from official ISAF website. Data exists on a monthly basis from January 2007 through October 2011.

*Source files* placemats.csv

*Data Management/Modification* We inputted troop numbers for each state from the website into a year-month dataset (placemats.csv). Placemats do not exist for every month so we imputed values: we assumed that for missing months, troops did not change (an assumption supported in our communications with various defense ministries). We saved the data, once imputed in this manner, in a state-year-month format as “isaf\_troops.dta.”

### C.2 Troop Information from Governments

*Description* We contacted the appropriate ministries and departments for each state in order to collect troop data. We noted whether the troop figures reflected troops on the ground or merely a mandate to send those troops to Afghanistan (it was always one or the other). When the governments did not distinguish, we placed the troop numbers into a catchall “trpsmod” column.

*Source files* marinov\_nomikos\_mod.csv

### C.3 Public Opinion

*Description* We looked at answers to the following four questions from a Transatlantic Trends survey on ‘ conducted in 2009-2012 in Germany, France, Italy, the Netherlands, Portugal, Spain, United Kingdom, Poland, Slovakia, Turkey, Bulgaria, Romania, the United States, Sweden, and Russia (1000 respondents for each and 1,500 for Russia):

1. “As you may know, [COUNTRY] currently has troops stationed in Afghanistan. In your view, should [COUNTRY] increase the number of troops in Afghanistan, keep its troops at its current level, reduce the number of its troops or should it withdraw all troops from Afghanistan?”
2. “As you may know [COUNTRY] troops are currently stationed in Afghanistan. Do you approve or disapprove of the presence of [COUNTRY] troops in Afghanistan?”
3. “As you may know, some countries have troops currently engaged in different military operations around the world. To what extent, would you approve or

disapprove of the deployment of [NATIONALITY] troops for the following operations? [READ OUT - RANDOMLY ROTATE - ONE ANSWER PER ITEM]: To conduct combat operations against the Taliban in Afghanistan”

4. “As you may know, many countries are engaged in different ways to stabilize Afghanistan. To what extent would you approve or disapprove of [COUNTRYS] participation in the following efforts in Afghanistan:[READ OUT RANDOMLY ROTATE ONE ANSWER ONLY. INTERVIEWER: REPEAT STATEMENT: Please tell me to what extent.]”

We also looked at the following two questions from Pew:

1. “Do you think the U.S. and NATO should keep military troops in Afghanistan until the situation has stabilized, or do you think the U.S. and NATO should remove their troops as soon as possible?”
2. “As I read some specific US policies tell me if you approve or disapprove of them...the US led military campaign against the Taliban and Al Qaeda in Afghanistan.”

For the month and year that the survey was fielded, we coded the percentage of respondents from a given country that entered a certain response. For more on the variables, see descriptions in the description of the variables below.

*Source files* Public opinion polls data - Sheet1.csv, stateabb2stateabb2.dta

*Data Management/Modification* The .csv file is already in country-year-month format but we merged it with the .dta in order to produce the necessary state abbreviations and country codes. This produced a country-year-month dataset saved as “marinov\_nomikos\_public\_opinion.dta,” which we merge into the master dataset.

## C.4 World Bank Population

*Description* Annual population data from 2001 through 2011 downloaded from World Bank.

*Source files* Population.Data.World.Bank.csv

## C.5 NELDA

*Description* The National Elections across Democracy and Autocracy (NELDA) dataset provides detailed information on all election events from 1960-2006.

*Source files* id & q-wide.dta

## C.6 Casualty Data

*Description* Data on casualties gathered from icasualties.org. Website lists each casualty by state. We combined all of this information into one spreadsheet, denoting by COW ccode each state.

*Source files* iCasualties\_data\_Afghanistan.csv

*Data Management/Modification* We transformed the event-based data into state-year-month format where we added up all of the casualties each state had for each month and saved the data as icasualties.dta.

## D List of Variables

*The following is a list of variables used in dataset elections\_troops.dta.*

**ccode** Correlates of War country code designation.

**stateabb** Abbreviations for each state (from COW).

**statenme** The name of each state (from COW).

**year** Year; ranges from 2001 through 2011.

**month** Month; ranges from 1 to 12.

**regime** Political system indicator from DD dataset. State is coded as a 0 if it is a parliamentary democracy; as a 1 if it is a mixed democracy; as a 2 if it is a presidential democracy. States are coded on a yearly basis from 2001 to 2008. We allow for variation within a state from 2001 to 2008 (i.e., when a state changes from one political system to another) but, due to data limitations, assume that from 2008 through 2011 all states maintained the same type of political system, a reasonable assumption for the states that contributed troops to Afghanistan (mostly consolidated, liberal democracies).

**execelec** Binary indicator for whether state had an election for an executive in a given month. State-year-month is coded as 1 if a parliamentary system had a legislative election; if a presidential system had a presidential election; or if a mixed system had either a legislative or a presidential election. Every state-year-month that did not have an election or had a non-executive election was coded as a 0.

**electionapproach6** Binary indicator for whether an executive election will occur within six months of the given month. Month of election is not included in the calculation.

**electionapproach12** Binary indicator for whether an executive election will occur within twelve months of the given month. Month of election is not included in the calculation.



**electionrecede6** Binary indicator for whether an executive election occurred in the past six months from the given month. Month of election is included in the calculation.

**electionrecede12** Binary indicator for whether an executive election occurred in the past twelve months from the given month. Month of election is included in the calculation.

**electype** Uses NELDA 20 to code whether a given election in a state-year-month was for the office inhabited by the incumbent at the time of the election. A month is coded as a "1" if a presidential election of this nature was held and as a "0" if a legislative election for the executive was held. Non-executive elections and non-election months are coded as missing. This variable does not distinguish mixed regimes from parliamentary and presidential systems and so, may miss a set of important elections for our analysis. For this reason, we recommend against its usage for extended analysis.

**electionapproach6v2** Uses NELDA20 to code whether the office of the leader would be contested in the next months. A country-year-month observation is coded as 0 if the office would not be contested within the next six months and as a 1 if it would be. May be used in robustness checks instead of electionapproach6; we recommend against this because of the lack of mixed systems in the NELDA20 data.

**electionapproach12v2** Uses NELDA20 to code whether the office of the leader would be contested in the next months. A country-year-month observation is coded as 0 if the office would not be contested within the next six months and as a 1 if it would be. May be used in robustness checks instead of electionapproach6; we recommend against this because of the lack of mixed systems in the NELDA20 data.

**trpsISAF** Measure for the number of troops that a state committed to ISAF in a given month. Data is taken from ISAF placemat archives. Some data imputed (see Data Source appendix for more details).

**trpsoef** Measure for the number of troops that a state committed to OEF in a given month. Data taken from communication with ministries of defense. Data mostly missing; recommend against its use for data analysis.

**trpsmod** Measure for the total number of troops that a state committed to Afghanistan (ISAF or Operation Enduring Freedom) in a given month. Data taken exclusively from ministry of defense communication. Does not distinguish between whether number reflects troops on the ground or not.

**trpsmand** Measure for the number of troops permitted to be sent to Afghanistan by parliamentary or presidential mandate. Data taken from communication with ministries of defense. Data mostly missing; recommend against its use for data analysis.

**trpsgrnd** Measure for the number of troops that a state currently has stationed in Afghanistan. Data taken from communication with ministries of defense. Data mostly missing; recommend against its use for data analysis.

**trpsavg** Average of values in *trpsmod* variable and *trpsISAF* variable.

**ntroops** Measure for the number of troops that a state had stationed in Afghanistan. Takes on value of *trpsmod* if *trpsISAF* is missing; takes on value of *trpsISAF* if *trpsmod* is missing; and takes the value of *trpsavg* if there exist values for both.

**lnthroops** Natural log of number of troops stationed in Afghanistan. Values derived by taking the natural log of values in *ntroops* variable.

**totalmonths** Total months for which we have data for *ntroops* variable. Values recorded by state. Maximum was 121 (10/01 through 10/11).

**population** Population of a state according to World Bank estimates. Data is gathered on an annual basis so we assume that population changes do not occur every month. That is, we keep the value of this variable constant through an entire year.

**pop2** Population in millions of citizens. Figures taken from *population* variable divided by 1,000,000.

**lnpop** Natural log of population of a state. Values derived by taking the natural log of values in *population* variable.

**trpspcap** Number of troops stationed in Afghanistan per million of citizens. Values derived by dividing values in *ntroops* variable by *pop2* variable.

**casualties** Total number of casualties suffered by a state in a given month taken from icasualties.org. We assumed that a state that contributed troops to ISAF but is not listed in the icasualties database has suffered 0 casualties as opposed to merely being “missing.”

**lncasualties** Natural log of number of casualties. Values for this variable taken by adding 1 to the values in the *casualties* variable and taking the natural log. We add 1 in order to distinguish between states that suffered no casualties and those that did not contribute troops at all.

**cas\_state** Binary indicator for whether a state suffered a casualty in Afghanistan at any point in the conflict. 1 indicates that it did, 0 that it did not. States that did not contribute troops to Afghanistan are coded as missing.

**NATO** Binary indicator for whether a state was a member of NATO or not. A state is coded as 1 if it was at any point during the conflict or 0 if it was not. States that became members during the conflict (such as Croatia) are coded as NATO members throughout the period because we assume that they faced similar restrictions on their foreign policy as NATO members in practice, even if they did not by international commitment.

**lgcontr** Binary indicator for whether a state was a large contributor or not. A state is coded as 1 if it contributed 1,000 or more troops in a given month and as 0 if it contributed more than 0 troops but less than 1,000. A state is coded as missing if it did not contribute troops during this period.

**transatlantic\_increase** Transatlantic Trends question 1, denotes percentage who want increase of troops.

**transatlantic\_keep** Transatlantic Trends question 1, denotes percentage who want to keep troops at current level.

**transatlantic\_reduce** Transatlantic Trends question 1, denotes percentage who want to reduce troops

**transatlantic\_withdraw** Transatlantic Trends question 1, denotes percentage who want total withdrawal

**transatlantic\_refused** Transatlantic Trends question 1, denotes percentage refused to answer

**transatlantic2\_approve** Transatlantic Trends question 2, denotes percentage who approve of troops

**transatlantic2\_disapprove** Transatlantic Trends question 2, denotes percentage who disapprove of troops

**transatlantic2\_dk** Transatlantic Trends question 2, denotes percentage who dont know

**transatlantic3\_very\_much\_approve** Transatlantic Trends question 3, denotes percentage that approves of troops

**transatlantic3\_somewhat\_approve** Transatlantic Trends question 3, denotes percentage somewhat approve of troops

**transatlantic3\_somewhat\_disappro** Transatlantic Trends question 3, denotes percentage somewhat disapprove of troops

**transatlantic3\_very\_much\_disappr** Transatlantic Trends question 3, denotes percentage disapprove very much of troops

**transatlantic3\_dk** Transatlantic Trends question 3, denotes percentage who dont know.

**transatlantic3\_refused** Transatlantic Trends question 3, denotes percentage who refused to answer

**transatlantic4\_very\_much\_approve** Transatlantic Trends question 4, denotes percentage who approve of troops.

**transatlantic4\_somewhat\_approve** Transatlantic Trends question 4, denotes percentage who somewhat approve of troops.

**transatlantic4\_somewhat\_disappro** Transatlantic Trends question 4, denotes percentage who somewhat disapprove of troops.

**transatlantic4\_very\_much\_disappr** Transatlantic Trends question 4, denotes percentage who disapprove of troops.

**transatlantic4\_dk** Transatlantic Trends question 4, denotes percentage who dont know.

**pew1\_keep\_troops** Pew question 1, denotes percentage who want to keep troops in Afghanistan.

**pew1\_remove\_troops** Pew question 1, denotes percentage who want to remove troops from Afghanistan.

**pew1\_dk** Pew question 1, denotes percentage who dont know.

**pew2\_approve** Pew question 2, denotes percentage who approve of war in Afghanistan.

**pew2\_disapprove** Pew question 2, denotes percentage who disapprove of war in Afghanistan.

**pew2\_dk** Pew question 2, denotes who dont know.