Political Institutions and the Causes of Military Spending

by

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For my parents, and my grandmother.

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Chapter 2: Military Spending, Democracy, and the Likelihood of Conflict (Paper 1)

Chapter 3: Domestic Politics and Arming in Autocracies: Military Regimes and Military Spending (Paper 2)

Abstract

Why do states invest the amount they do in their militaries? I identify three sets of causes. First, political institutions that create public accountability shape how states respond to the likelihood of conflict. Publically accountable leaders shift how much they spend as the likelihood of conflict shifts. This relationship between democratic institutions and threat response explains both why democracies spend less on average than non-democracies, and why some democracies, some of the time, invest heavily in their military. It also helps explain why democracies win wars more often than non-democracies, even when targeted: democracies invest in their military in order to build military capacity. Autocracies invest in their military for other reasons. Specifically, they use military spending as a side payment to high-ranking members of the military. In autocracies, military spending is the cost of military support. This leads to the counterintuitive finding that when the military runs the government, the state invests less in the military. Finally, I examine the foreign origins of military spending, and find that state military spending is affected by military spending in other states through several pathways. State military spending is positively interdependent with military spending in states with which it is likely to fight. It is negatively interdependent with

states with which it is allied. However, it is positively associated with the aggregate likelihood of conflict of its allies, making alliances a source of both less and more military spending. In contrast to the conventional wisdom, I do not find evidence of military spending in response to rival spending when the likelihood of conflict and ally spending are accounted for. The three papers in this dissertation contribute to the literatures on military spending and arms races, alliances, enduring rivalries, and political institutions and foreign policy.

Chapter 1: Introduction

The question of who arms, to what extent, and why, is central to the study of politics. Arming is the acquisition of the tools of violence. The ability to commit violence is proportional to the possession of the tools of violence, and some actors have more than others. The ability to commit violence shapes how actors relate to one another. Actors who can commit violence can exploit those who do not. Understanding who can use violence, and when and how they use it, is necessary to understand a wide range of political activities, especially in the international sphere. Those who lack the tools of violence can acquire them, but doing so is costly. Why do some actors pay those costs, while others do not?

I address the question of who arms, and how much, by looking at variation in the military spending of states. Military spending is only one type of arming, much as states are only one type of political actor. But just as states are the most important political actor in the international system, military spending is the most important instance of arming. Militaries make up the bulk of the violent capacity of many, if not most, states. Despite that, there is enormous variation in the degree to which states invest their resources in their military. I provide a series of partial explanations for that

variation. Those explanations partially answer the broader question of who arms, and how much, for at least the subset of political actors known as states.

What explains variation in military spending? Two broad answers exist in the literature on international relations. The first is foreign threat. Governments increase their military spending when they are threatened, and decrease it when they are not (Nordhaus et. al. 2012). The second is political institutions. Democratic states invest less in their military than non-democratic states (controlling for threat) (Fordham and Walker 2005, Goldsmith 2003, Goldsmith 2007). I explore these two causes of military spending in three papers, and find that their importance varies across different states and circumstances. I find that democratic states are more responsive to a particular kind of foreign threat: the likelihood of conflict. Variation in autocratic states, on the other hand, is driven by domestic conditions, particularly the role of the military in government. In different types of state, variation in military spending is driven by different factors.

Each paper examines how political institutions shape military spending. Political institutions create and describe the relationships between political actors. The term institution captures a wide range of power relationships, with varying levels of formality, precision, and scope. Each paper explores a different set of institutions. Paper 1 focuses on democratic institutions, specifically those that create public accountability. Paper 2 examines the role of the military capture of the government. Paper 3 examines

the role of military alliances, which provide an alternative to military. Together they look at political institutions at three different levels – broad regime distinctions (democracy/autocracy), within category variation (military/civilian autocracy), and international institutions (alliances).

Paper 1 makes several contributions. First, it shows that the effect of the likelihood of conflict is conditional on the institutions of the state. Democracies invest in their military in order to build war fighting capacity, leading democratic states to respond more to changes in the likelihood of war than non-democratic states. This amends the existing literature, which assumes that states respond equally to similar threats to their security (Waltz 1979, Nordhaus et. al. 2012, Fordham and Walker 2005, Goldsmith 2003). It also provides a partial explanation for democratic success in wartime, especially when they are the targeted state. Democratic spending increases the capacity of the state more than autocratic spending. Paper 1 also shows that the existing work, which claims that democratic states invest less in their militaries than non-democratic states, all else equal, only holds when conflict is unlikely. When conflict is likely, democracies spend as much or more than non-democracies in similar situations. The current story, that democratic states are more peaceful, is incorrect. Democracies spend less on average because they are more efficient in their military spending, not because they are inherently more pacific. Finally, paper 1 shows that the specific democratic

institutions that matter are those that make leaders accountable to the public, mass suffrage and political competition.

Paper 1 shows that autocracies respond less than democracies to changes in the likelihood of war. Paper 2 demonstrates that autocracies use military spending as a side payment to high-ranking members of the military. In civilian autocracies, military spending is the cost of military support. In military autocracies, where military elites can extract rents directly, military spending is less necessary. This leads to the counterintuitive finding that when the military runs the government, the state invests less in the military. This finding contradicts existing work, which finds that military involvement in government increases military spending (Jackman 1976, Nordlinger 1970) or has no effect (Conrad et. al. 2013).

Paper 3 examines the foreign origins of military spending. I find that state military spending is affected by military spending in other states through several pathways. State military spending is positively interdependent with military spending in states with which it is likely to fight, which is consonant with existing work and paper 1. Military spending is negatively interdependent with ally military spending. However, it is positively associated with the aggregate likelihood of conflict of its allies, making alliances a source of both more and less military spending. Both findings support theories of alliances that suggest substitution effects between allies (e.g. Morrow 1993). Allied states exchange arming for allying, but acquire some of their ally's threat as a

result. The finding of negative interdependence between allies supports those theories that warn of the risk of free-riding in alliances. In contrast to the conventional wisdom, I do not find evidence of military spending in response to rival spending when the likelihood of conflict and ally spending are accounted for. This finding calls into question the importance of enduring rivalries, separate from their use as a proxy for likelihood of conflict.

Each paper of this dissertation can be read as an independent article or as part of a broader project on political institutions and military spending. Taken separately, they improve on the existing knowledge in numerous ways, including those described above. Taken together, they show that the effects of political institutions go beyond independent, linear effects on phenomena of interest. Military spending provides an excellent window into the various ways that political institutions matter, because it is, itself, multi-use and multi-dimensional. Political institutions shape how states respond to other important variables, such as foreign threat. They shape who gets paid, and in what manner. They provide possible solutions to problems, and help create the environment in which the state exists.

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Chapter 2: Military Spending, Democracy, and the Likelihood of Conflict (Paper 1)

Abstract

Different types of states respond differently to their circumstances. Democratic states, with leaders who are accountable to a broad public through institutions of competitive elections and mass suffrage, invest in their militaries in response to changes in the likelihood of conflict. Autocratic states respond less, or not at all, to shifts in the probability of conflict, suggesting that the purpose of military spending differs in states with and without public accountability. Because democratic military spending is directed towards deterring and winning wars, while autocratic spending serves other purposes, democratic targets become less likely to lose wars as they spend more.

How do political institutions shape arming decisions? Military spending is commonly understood as a response to foreign threats. Evidence suggests that belief is broadly correct (Goldsmith 2007, Nordhaus et. al. 2012, Dunne et. al.). Existing work also demonstrates that different kinds of states spend different amounts on their military. Specifically, they find that more democratic states spend less on their military (Fordham and Walker 2005). Both findings, while broadly true, mask an important source of variation: different types of states respond differently to their circumstances. Little or no attention is given to how diverse states perceive and respond to identical threats differently.

Many leaders answer to a domestic audience. The leader can be replaced if she chooses too many unsuccessful policies, or deviates too far from the preferred policies of the domestic audience. Public accountability, created by institutions that allow a large domestic audience to replace a leader, motivates leaders to try harder during conflicts, because losing can cost them their job (Bueno de Mesquita et. al. 2003, Croco 2011). Conflict can be politically costly for leaders even if the outcome is favorable, particularly in longer conflicts (Gardner and Segura 1998, Bennett and Stam 1998, Valentino et. al. 2010). Leaders subject to public oversight, then, should seek to do two things – prepare to win conflicts they think are likely, and try to deter challenges that could lead to politically costly conflicts. One way of doing both is to invest in military power, which can deter challenges and promote a more favorable outcome should

conflict occur. Both goals result in more military spending in response to perceived increases in the likelihood of war. I argue that public accountability, created by mass suffrage and political competition, motivates democratic leaders to respond to foreign threats in proportion to their likelihood of manifesting in a manner visible to the public. Foreign threats are most visible when they are militarized and fatal. I find that democracies spend less on their military when conflict is unlikely, but respond more to increases in the likelihood of conflict. However, they do not respond more to shifts in the capabilities of the strategic rivals or the aggregate military spending of states with whom they do not share strategic interests. My argument implies that democratic military spending is more geared toward success in foreign wars than autocratic military spending. My findings also support that claim, as democratic targets are disproportionately less likely to lose when their military spending is higher.

Review of the Literature

Military spending is a response to foreign threats. That contention exists in the classic theoretical literature (Waltz 1979, Walt 1990), is the basis for the considerable body of work on arms races (Richardson 1960, Glaser 2000, Morrow 1993), and has been subject to recent empirical evaluation (Nordhaus et. al. 2012, Goldsmith 2003, Dunne et. al. 2007, Rosh 1988). The military provides security by increasing the capacity of the

state to use violence to inflict costs, and preventing costs from violence, either through deterrence or direct prevention. What is considered a threat varies widely across the literature. Answers include:

- any state capable of inflicting damage (Waltz 1979)
- any state capable and with intent (Walt 1990)
- strategic rivals (Colaresi et. al. 2008)
- beliefs about the likelihood of war in the future (Bueno de Mesquita 1981, Nordhaus et. al. 2012)

The common understanding of threat, across most of the conceptualizations listed above, is that an actor who is a threat possesses the ability and intention to harm the interests of the state. These different understandings of threat vary across other dimensions, however. Some are directly tied to conflict (Bueno de Mesquita 1981, Nordhaus et. al. 2012), while others focus more on both hot and cold strategic competition (Colaresi et. al. 2008). Waltz, and other classic big thinkers in the field, focused primarily on the causes and effects of power. An important amendment to Waltz's argument came from Walt, who argued that states do not balance (through alliances) against just power. They do it against power that is perceived as having hostile intent (Walt 1990). Rosh introduced the idea of a security web on which state military spending – and foreign policies more generally – might depend. The security web, or security environment, is a familiar concept in international relations, which

spans multiple levels of analysis, including both the systemic and dyadic levels. When defining the foreign security environment, I focus on the concept of threat, with an eye to these to central formulations. Threat is some combination of the capacity to harm, and the desire, willingness, and likelihood of doing so. The relative importance of each, however – the emphasis of capacity versus intent – may vary across state types.

Institutions and Arming

Democratic institutions affect a variety of international phenomena and foreign policies, including conflict occurrence, behavior, and outcomes. Work on that subject intersects with the arming literature when it dwells on how political institutions affect military spending, in and out of wartime. One consistent finding is that democracies spend less on their military (Goldsmith 2003). In many empirical evaluations, that manifests as increases to polity or a related measure being associated with decreases to a measure of aggregate spending (absolute or relative to GDP). Fordham and Walker (2005) directly investigate the pacifying effect of democracy, and find, in concordance with Kantian liberal theory, democracy leads to less military spending. In their investigation of external security environment and military spending, Nordhaus, Oneal, and Russett (2012) find that external threat does increase military spending. In the process, they also find that, consistent with Fordham and Walker, democracy decreases

military spending. Further, they find that democracy has the secondary effect of reducing threat by making conflict less likely with some states, which in turn reduces military spending.

Bueno de Mesquita et. al.'s (2003) selectorate theory emphasizes the importance of the size of the winning coalition, and the body of actors who could help form a winning coalition (the selectorate). Leaders with large winning coalitions have two incentives to provide national security. First, national security is a public good (Dunne et al. 2007, Smith 1995, Sandler and Hartley 1995) – it is not exclusive or rival among the domestic population. Large winning coalition leaders provide more public goods because, with a large coalition, providing private goods to buy support is inefficient. As a result, large winning coalition leaders should be more likely to provide national security. Importantly, those same governments are likely to provide more of other public goods as well, and there may be substitution effects. In times of low threat, national security is already provided, and investments in defense have small returns. Large winning coalition leaders invest in national security when the country is otherwise insecure, and invest in other public goods when it is relatively secure, because they are subject to public accountability.

Scholars like Lake and others argue that democracies should try harder during war (Lake 1992, Bueno de Mesquita et. al. 2003) for a variety of reasons. However, Reiter and Stam (2002) find no evidence that democracies are better at extracting resources for

war. Goldsmith (2007) finds evidence that democracies do try harder. There are a variety of explanations for the observed correspondence between lower military spending and democracy, and more spending by democracies during wartime. For example, Goldsmith tries to arbitrate between these three: executive constraints, large winning coalitions or political participation, and political competition. He argues that political competition is the primary reason that democracies spend less during peacetime but more during times of war. In particular, competition leads to flexibility in defense effort. His principle foil is the selectorate theory, which argues that as the group which is required to maintain the winning coalition necessary to stay in power increases in size, so does the incentive of the leader to provide public goods, rather than private goods (Bueno de Mesquita et. al. 2003). Goldsmith agrees with the finding, but disagrees with the mechanism (2007).

These, and related works, address how democracies respond to *war* rather than more general responses to threat – arms races and similar dynamics are outside of the scope of their work. A substantial body of work on war treats war as a bargaining process, with actual military action being part of a more general process, rather than a wholly distinct enterprise (Wagner 2000, Blainey 1976). Arming is also a part of that process – the provision of security through policy, or the acquisition of goods through bargaining with other states. This is consistent with formal models that include arming or arms races as part of a game, in which one possible outcome is war (Powell 1999).

Theory

The literature on military spending and political institutions neglects the interaction of threat and institutions. Democracy is a cluster of political institutions associated with political competition, widespread suffrage, the protection of minority rights, and rule of law. I examine the role of public accountability on arming. Arming is a policy choice made by the leader(s) of the central government. One challenge to modeling aggregate military spending effectively is that money can be spent on the military for many reasons, but looks the same when aggregated. Military spending can serve many purposes: security from foreign threats, domestic security for the regime and/or leader, side payments to the military, and non-security related benefits for the public (distributive politics). For ease of discussion, I treat these as if they are wholly separate categories, but in fact each dollar spent may serve multiple purposes to some extent.

National security, as a non-excludable, non-rival commodity is a public good. While domestic actors, be they voters, leaders, or political elites may place different values on national security, they cannot easily be excluded from it. Domestic or regime security, on the other hand, favors some members of society more than others. In particular, it favors those that benefit from the status quo. Most of all, it favors members

of the regime. It is therefore more like a club good. Side payments to the military may increase security, but their primary purpose is to enrich (please) military elites. Distributions to members of society outside of the regime vary from focused private goods (private contracts, for example) to widespread public and club goods, such as disaster relief or jobs programs. In this article I focus primarily on the implications of military spending as a public good, but each purpose warrants further exploration.

The value of military spending as a public good is commensurate to the security it provides, and the necessity of security provision. The perception of threat, then, is a central motivation for investing in the military. As perceived threat increases, the need to spend in response increases. One implication is that the more a state values public goods, the more it responds to changes in perceived threat. The state cares about public goods, security is a public good, and military spending is partially a way of providing security. Military spending increases when the public's security, absent military spending, goes down. States with political institutions that incentivize leaders to provide public goods will respond more to external threat.

The above argument suggests greater responsiveness to threat in democracies. As such, I expect them to respond more to threats to their security than other states. Democracy has already been widely shown to lower military spending on average. There are many possible explanations for this. I expect democracies to spend less in times of low threat for two reasons. They spend less on their military when threat is low

because security is already relatively high, making marginal improvements through arming less valuable relative to other public goods. However, when threat increases, they shift more resources into the provision of security. As such, both the effect of democracy and the effect of threat are conditional. First, democracies spend less in times of low threat, but respond more to increases in threat, negating or reversing the effect in times of high threat. Second, in consolidated democracies, the ability of the regime to use repression to remain in power is both restricted by court systems and civil liberties, and less necessary, because the leader has the consent of the public. Unlike in autocratic states, military spending does not provide domestic security for the regime. Variation in military spending in democracies is a product of their external security environment.

Who makes policy varies somewhat from state to state, but typically the head of state is the principal decision-maker, sometimes acting with or on behalf of a legislature. Leaders make their decisions according to their own preferences, and the circumstances in which they find themselves and their country in. Because one preference of leaders is to remain in power, they also consider the preferences of those who could remove them from power – their constituents. Political institutions intervene in that process in multiple ways. They determine how leaders are selected and remain in power. They determine what constraints are on the leader's ability to make policy, as well as how and to what degree they are held accountable for the success of policies.

Every leader answers to both elites and the public, but the degree to which they do varies. I assume that leaders desire to remain in office. The larger the number of people required to acquire and stay in office, the more efficient public goods will become relative to private goods for earning support. Leaders who answer to large groups – the public – will distribute more public goods, and favor policies whose benefits are widely dispersed (Bueno de Mesquita et al. 2003).

I assume that the public wishes to be both secure from foreign threats and wealthy, but there is a trade-off between them. Their preferred policy (in this case, aggregate military spending) will alter with their perception of the inherent insecurity of their circumstances. If they are secure already, spending on the military will be inefficient. It would not make sense for an official to provide a public good – military spending, in this case – for which there is little demand. Their incentive to provide it is contingent on the circumstances of the state – whether it is under threat or not. Rather than just exerting upward force on military spending (as a public good), public accountability shapes how the government responds to its security situation, which can increase or diminish demand for that particular public good. Such an argument is at the heart of the literature on democracies and war – even under identical circumstances, states with different regimes respond differently. I argue that this dynamic is at work even in times of peace – democracies that perceive conflict as likely ex ante increase spending more than non-democracies. If they are not secure, they will spend on their military until the

optimal balance of security and wealth is achieved. While there may be an independent effect of institutions (general pacification, for example), they also shape how governments respond to their environment.

Widespread suffrage and political competition create public accountability. The general public can replace the leader. This shifts the preferences of the leader towards policies that visibly provide public goods. The credible threat of replacement incentivizes the leader to perform better in office across several dimensions. First, it means that she will enact policies that will please the people whose support is required to stay in power. Otherwise, a leader with a policy platform preferred by more members of the relevant selectorate will replace her. Second, it requires leaders to enact those policies as efficiently and competently as possible. Otherwise, a more competent leader with a similar policy platform would replace him or her. Finally, leaders will favor policies that they can easily claim credit for, and avoid those that are either invisible to their relevant audience, or will expose them to electoral risk without equivalent gain.

This last aspect is crucial – credit claiming is difficult for unobserved public goods. Security from foreign threats is a public good, but is difficult to gauge. Leaders of democratic states should invest in security when they think the security provided by their investment is likely to be observed and rewarded, or, importantly, the insecurity created by their lack of investment is likely to be observed and punished. As a result,

democratic leaders respond more to threats to the security of their state, but only when those threats are likely to manifest in a manner visible to the public. This can lead to the overprovision of visible security policies, at the expense of more effective but less visible alternatives (Bueno de Mesquita 2005).

The most visible manifestation of insecurity is open conflict. While the public may not be aware of subtle shifts in the geopolitical circumstances of the state, it will notice if war breaks out. Politicians subject to political accountability, then, are particularly sensitive to changes in the likelihood of conflict. Democratic leaders will invest more in preparation for conflict when the likelihood of conflict goes up. Democratic leaders may also be responsive to other kinds of threats, but no strategic competition has as significant and well-documented political costs as those associated with military conflict. Nor, generally, are other types of threats as visible to the public. Arms races may receive some political attention, but nothing receives the same attention as, and as a result, threatens political elites like, fatal militarized disputes. The likelihood of conflict instigates military spending on the part of leaders subject to public accountability because when conflict breaks out, the public knows it.

Research Design

Evaluating the relationship between regime type and threat response requires variation in spending and threat across countries with different regime types, and variation in spending and threat within countries, over time. Dunne and Smith have an excellent review of the specification challenges posed by arms race dynamics (Dunne and Smith 2007). While not, by any means, alleviating all of the various difficulties, time series cross sectional (TSCS) data is used in most recent empirical work on aggregate military spending (Collier and Hoeffler 2007, Fordham and Walker 2005, Nordhaus et al 2012). I follow those scholars, and use country year data from 1950-2000 to estimate a series of multivariate regression models evaluating my hypotheses. I provide estimates from a variety of alternative specifications to demonstrate the robustness of my findings. Following that, I evaluate potential alternative explanations for democracy's effect on responsiveness to threat, including elite accountability and a more general public goods story, using data on autocratic regime characteristics and alternative measures of international threat environment. Finally, I test whether military spending in democracies has a larger effect on war outcomes than military spending in non-democracies, using data on war-dyad-years, outcomes, and military spending.

Measurement and Variable Selection

Military Spending

I use TSCS data of logged military expenditures from 1950-2000. While military spending as a percentage of GDP is easy to understand, and reflects how much of itself the state dedicates to military power (Fordham and Walker 2005), absolute changes in spending are important when considering the influence of international threats – 1% of GDP is not the same everywhere. Each dollar spent is also not equal. A reasonable assumption is that there are diminishing marginal returns per dollar, so larger numbers of dollars are required to improve security as total spending increases. I approximate that dynamic by using the natural log of military spending. Nordhaus et. al. also use the natural log of military spending in their investigation of the relationship between external threat and military spending (2012). I use their data, which they construct using data from the Correlates of War project and SIPRI[1].

Likelihood of Conflict

I use a variable created by Nordhaus et. al. as a measure of foreign threat (2012). They estimate the ex ante likelihood of conflict, between dyads using a well-established model of conflict, and aggregate those likelihoods for each state. That aggregated

[1] See their paper for a discussion of where and why they substitute SIPRI estimates for COW estimates, and vise versa.

likelihood is an estimate of what the state in question believes about its chances of conflict, given what it knows at the time. Because it is constructed to represent the ex ante belief of leaders about the likelihood of conflict, it is an excellent fit for my theory.

Regime Type

I use Boix et al's dichotomous coding of democracy (2013). They evaluate countries based on their suffrage and political competition, coding states with high levels of both as democracies, and the remainder as non-democracies. Among the variety of possible measures of regime type, theirs most closely captures the institutions I argue drive public accountability.

Two standard measures are Polity (Marshall, Jaggers & Gurr 2009), which is often used to represent variation in regime type, and W, from Bueno de Mesquita et. al. (2003), which measures the size of the winning coalition needed by the leader to remain in power. I argue that both mass suffrage and high levels of political competition are necessary for public accountability to affect threat responsiveness, making a continuous measure unsuitable for my test. Polity is a scale from -10 to 10, and is constructed from measures of various institutional characteristics associated with democracy and autocracy. Polity captures a collection of institutions beyond mass suffrage and political competition that do not apply to my theory.

W adopts 5 values, from 0 to 1, and is closer theoretically to my causal story than Polity. However, it too does not fit the dichotomous structure of my theory. That said, the results presented below do not change substantially if polity or W is used in lieu of Boix et. al.'s measure (2013).

Control Variables

The models presented include few control variables, but the findings are robust to the inclusion of many others. Each model includes the natural log of real GDP,[2] and a variable that counts battle deaths as a percentage of pre-war population, used by Fordham and Walker (2005) to control for wartime spending. On each of these, the coefficients appear in line with existing work. The inclusion of other plausible control variables yields similar results.

Estimation Strategy

Times series panel data can exhibit numerous problems to effective estimation, including unit heterogeneity, temporal autocorrelation, spatial autocorrelation, and heteroskedasticity. I use a variety of different models to address these concerns in turn, though no model individually addresses all possible problems.

[2] I again use Nordhaus et. al.'s data (2012), and refer the reader to their article for a discussion of its construction.

Unit Heterogeneity

Fixed effects may attenuate coefficient estimates on variables that change slowly over time but vary considerably across units. Excluding them risks omitted variable bias from unmodeled unit heterogeneity. I estimate two models with fixed effects, with Newey West (Newey and West 1987) and Driscoll Kraay standard errors (Driscoll and Kraay 1998).

Temporal Autocorrelation

Military spending is sticky over time. What a country spent last year is a good predictor of what they will spend this year. That temporal dependence could be a result of slow moving independent variables, both within-unit and external: balance of power and international threat dynamics change slowly. It could be part of the nature of military spending – investment in weapons systems takes place over years, so the actual process is not yearly, even if the data is. Finally, it could be a product of bureaucratic dynamics that make changing the budget difficult. Institutional characteristics may make deviations from status quo spending difficult. Most likely, the observed temporal dependence is a product of all of these. Addressing which and to what degree is outside of the scope of this article.

One common solution for temporal autocorrelation is to use a lagged dependent variable (LDV). The use of a lagged dependent variable, however, can also cause problems, particularly in data with slow moving independent variables, and can lead to erroneous null findings. (Achen 2000) Estimated coefficients on variables like foreign threat, which are highly correlated over time, are likely to be attenuated when lagged military spending is included in the model[3]. However, there are also substantive reasons to include a lagged dependent variable in models of military spending, such as the bureaucratic argument mentioned above. Temporal autocorrelation might be a result of a causal process between military spending and the previous years military spending, as well as being a result of processes that are, in this case, nuisances. I address temporal autocorrelation in three ways: with standard errors that are robust to temporal autocorrelation in the disturbances (Newey West and Driscoll-Kraay); with the direct inclusion of a lagged dependent variable in a Newey-West model; and with a model that instruments for the lagged dependent variable, as used by Nordhaus, Oneal, and Russett (2012) and Conrad, Kim, and Souva (2013).

Heteroskedasticity and Spatial Disturbances

I use regression with Newey West standard errors to account for heteroskedasticity and temporal autocorrelation in the disturbances. To accommodate

[3] Achen discusses work on arms races as an exemplar of this danger.

the possibility of further time dynamics, I include a lagged dependent variable in one model. I specific a third model with Newey West standard errors and fixed effects, to account for possible omitted variable bias from unobserved unit heterogeneity. Finally, I use models with Driscoll-Kraay standard errors, with and without fixed effects, to account for general temporal and spatial autocorrelation, heteroskedasticity, and unit-level heterogeneity (Hoechle 2007).

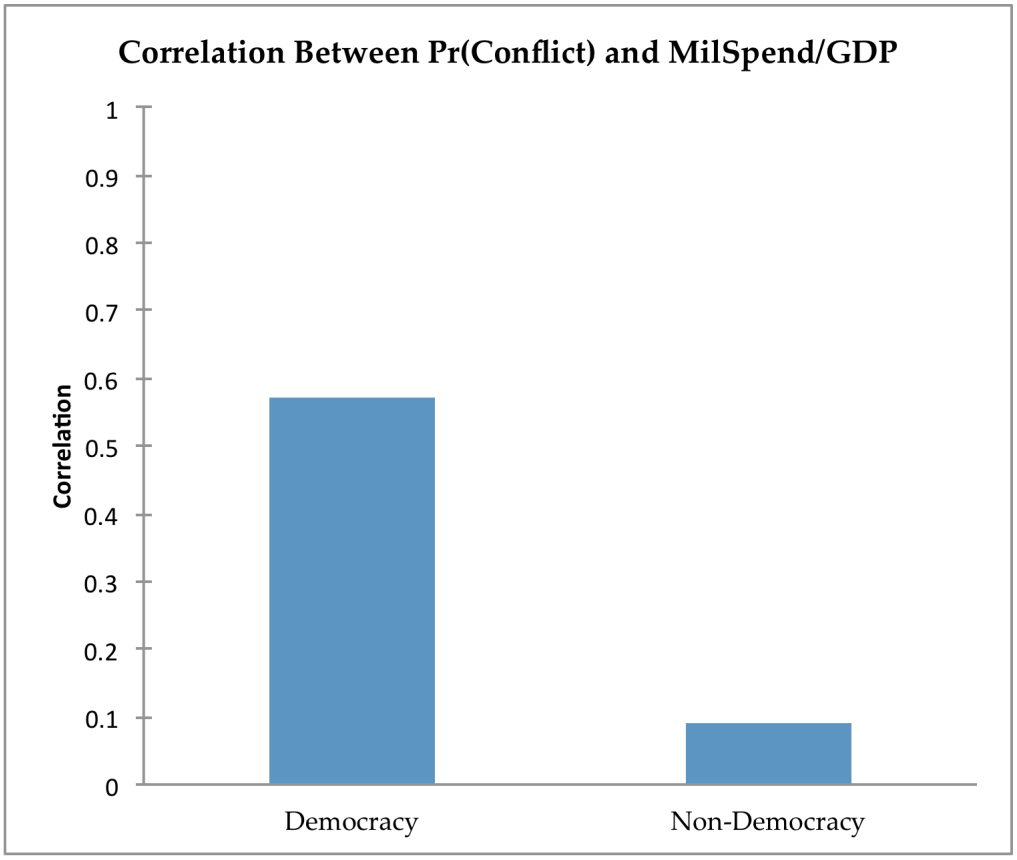
Analysis

Figure 1 presents the correlation between military spending and the likelihood of conflict in democracies and non-democracies, as well as some summary statistics of military spending as a percentage of GDP. In democracies, there is a strong association (.57) between how much they invest in their militaries, and how likely conflict is, according to Nordhaus et. al.'s model of ex ante likelihood (2012). In non-democracies, the association is quite small by comparison (.09)[4]. While not rigorous, these

[4] If you use the natural log of military spending instead of military spending as a percentage of GDP, the correlations become somewhat closer, but still very far apart, with .41 for democracies and .16 for non-democracies. I use military spending as a percentage of GDP for Figure 1 because it allows me to control for GDP. I use the natural log of military spending in all of the remaining analysis because I can control for GDP directly, and still allow absolutely numbers to matter, and each additional dollar to matter less than the previous one.

correlations are consistent with my argument – military spending in democracies is more responsive to the likelihood of conflict than in non-democracies[5].

Figure 2.1



	N	Mean	St. Dev.	Corr
Democracy	2515	1.69	2.23	0.57
Non-Democracy	3676	3.03	5.57	0.09

[5] A similar chart using W in lieu of Boix et al.'s (2013) democracy measure supports my contention that this is a dichotomous effect. When W takes values of 0, .25, and .5, the correlations between military spending as a percentage of GDP and the likelihood of conflict is .009, .038, and .089 respectively. When W takes values of .75 or 1 those correlations jump to .556 and .562, respectively.

Table 1 provides the estimates of six models that test my theory with different assumptions. Models 1-3 use linear regression with Newey West standard errors. Model 1 is the simplest specification, with subsequent models including additional components, or a different set of assumptions. Model 2 includes a lagged dependent variable as well as the autocorrelation in the disturbances. Model 3 includes fixed effects. Model 4 presents the estimates from the time dynamic model that instruments for the lagged dependent variable. Models 5-6 use Driscoll-Kraay standard errors (Hoechle 2007), with a maximum of two lags for temporal autocorrelation in the disturbances. Using Driscoll-Kraay standard errors also adjusts for general spatial autocorrelation. Model 6 also includes fixed effects.

Table 2.1: Models of Ln(Military Spending)

	Model 1	**Model 2**	**Model 3**	**Model 4**	**Model 5**	**Model 6**
Dependent Variable	*Newey*	*Newey*	*Newey*	*Instrumented LDV*	*Driscoll-Kraay*	*DK*
Natural Log of Military Spending		*Lagged DV*	*Fixed Effects*			*Fixed Effects*
Independent Variables	b/se	b/se	b/se	b/se	b/se	b/se
Natural Log of GDP	1.066***	0.071***	0.951***	0.337***	1.066***	0.951***
	(0.02)	(0.01)	(0.03)	(0.00)	(0.01)	(0.04)
Battle deaths as a percentage of prewar population	6.106***	0.445*	4.205***	1.997***	6.106***	4.205***
	(0.72)	(0.19)	(0.61)	(0.00)	(0.32)	(0.55)
Likelihood of Conflict	1.513***	0.094	-0.062	0.487***	1.513***	-0.062
	(0.27)	(0.05)	(0.34)	(0.00)	(0.20)	(0.26)
Democracy	-0.732***	-0.091***	-0.363***	-0.262***	-0.732***	-0.363***
	(0.09)	(0.02)	(0.08)	(0.00)	(0.11)	(0.07)
Democracy X Likelihood of Conflict	0.969**	0.162**	0.723*	0.380***	0.969**	0.723**
	(0.34)	(0.06)	(0.35)	(0.00)	(0.35)	(0.25)
Lagged Natural Log of Military Spending		0.930***		0.68***		
		(0.01)		(0.00)		
constant	-4.481***	-0.226***	-2.261***	-1.352***	-4.481***	-3.064***
	(0.17)	(0.05)	(0.61)	(0.37)	(0.20)	(0.41)
N	5866	5648	5866	5503	5866	5866

* p<0.05, ** p<0.01, *** p<0.001

Table 2 presents the marginal effect of a change in the regime type variable, dependent on the likelihood of conflict, and vice versa. The bolded coefficients are statistically significant at the p<.05 level. Across all of the models, democracy has a negative and statistically significant effect when conflict is unlikely. Across all of the models, the likelihood of conflict has a positive and statistically significant effect when the state is democratic. The estimated effect of democracy when conflict is likely is positive in all of the models, but only statistically significantly distinct from zero in Model 4. The effect of the likelihood of conflict is statistically significant and positive in Models 1 and 5.

Table 2.2: Marginal Effects

		Model 1		Model 2		Model 3		Model 4		Model 5		Model 6	
Marginal Effects		*Newey*		*Newey Lagged DV*		*Newey Fixed Effects*		*Instrumented DV*		*Driscoll-Kraay*		*Driscoll-Kraay Fixed Effects*	
		Coeff	*p-value*	*Coeff*	*p-value*	*Coeff*	*p-value*	*Coeff*	*p-value*	*Coeff*	*p-value*	*Coeff*	*p-value*
Democracy	Conflict Unlikely	**-0.73*****	0.00	**-.09*****	0.00	**-.36*****	0.00	**-.26****	0.00	**-.73*****	0.00	**-.36*****	0.00
	Conflict Likely	.24	.36	.07	.13	.36	.23	.12	.07	.237	.338	.36	0.092
Likelihood of Conflict	Democracy	**2.48*****	0.00	**.26*****	0.00	**.66***	.04	**.87*****	0.00	**2.48*****	0.00	**.66****	0.01
	Non-Democracy	**1.51*****	0.00	.09	.08	-.06	.85	**.49*****	0.00	**1.51*****	0.00	-.06	0.81

* p<0.05, ** p<0.01, *** p<0.001

Figures 2 and 3 show the estimated marginal effects visually. Figure 2 presents the marginal effect of democracy over different levels of threat. The effect of changing from a non-democracy to a democracy is negative and significantly different from zero *when conflict is unlikely*. As conflict becomes more likely, the effect of democracy diminishes to zero. While it is above zero on the far right, it is not statistically significant. The increased responsiveness of democracies means that regime type during times of high threat does not affect military spending. Changing the regime of a state when conflict is

very likely is unlikely to shift its aggregate military spending. Similarly, Figure 3 shows the average marginal effect of the likelihood of conflict in democracies and non-democracies. It is positive in democracies, but smaller and not significantly different than zero in non-democracies. In short, as Figure 1 suggested, the likelihood of conflict matters more to the level of military spending of democratic states than non-democratic ones.

Figures 2.2 and 2.3

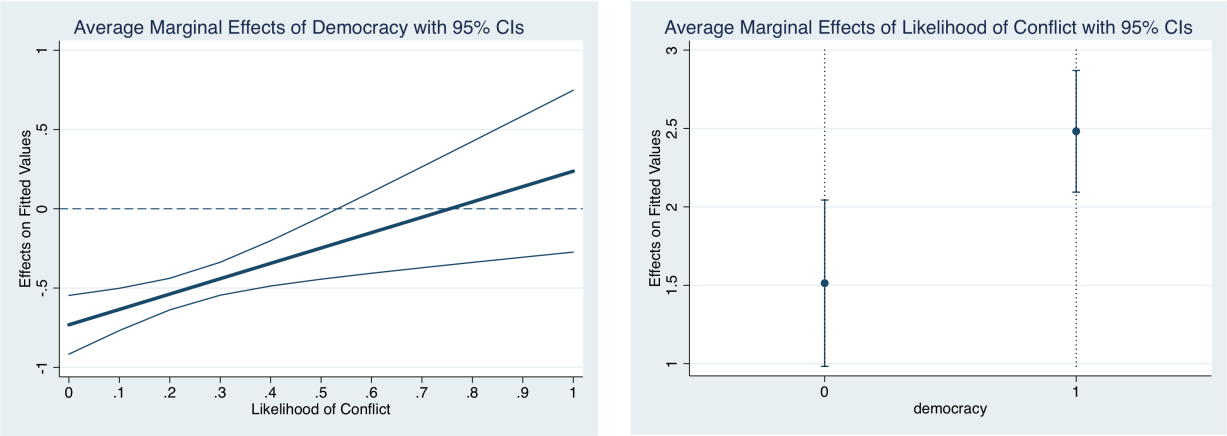
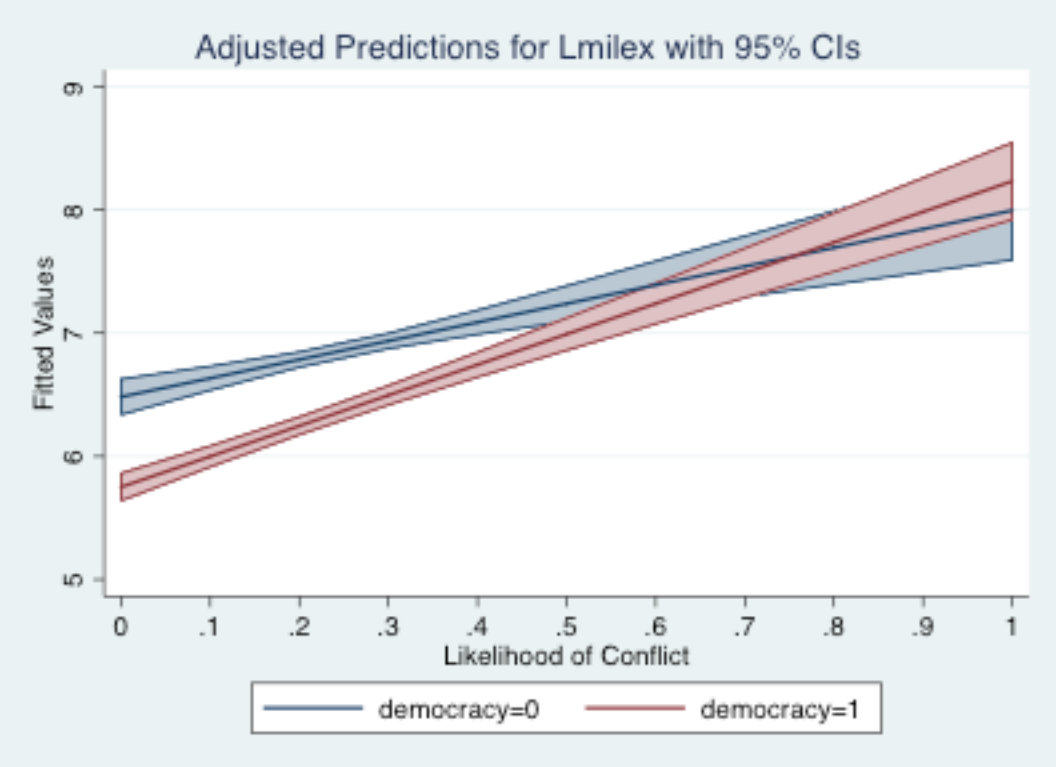


Figure 4 shows the average predicted natural log of military spending for democracies and non-democracies, over the likelihood of conflict, with all other variables held at their means. As the marginal effects charts suggest, democracies spend less in times of low threat, but increase their spending more as the likelihood of conflict increases. In times of high threat, an average state would spend more if it is a democracy than if it is a non-democracy, but not by a statistically significant amount.

Figure 2.4



Alternative Explanations

I evaluate two possible alternative explanations for the findings above by replicating my tests with alternative independent variables. By showing that plausible alternative explanations for the evidence above do not wash out the effect of my measure of democracy on responsiveness, I strengthen my empirical results, and provide additional support for my argument,

Public and Elite Accountability

One possible alternative explanation is that the variation between regime types is actually a product of differences between types of autocratic regimes, rather than between democracies and non-democracies. I argue that public accountability causes democratic regimes to respond more than non-democratic regimes to the probability of conflict. It is possible that accountability, elite or public, is the cause of different responsiveness. In that case, democracies would still appear to be more responsive than autocracies, when the autocracies are grouped together. If the relevant divide is between regimes with accountable leaders and those without, and all democratic leaders are accountable, while only some autocrats are accountable, the autocratic states are going to have on average lower levels of responsiveness, even if the accountable autocracies are identical in responsiveness to democracies. This argument follows work by Weeks (2012). She argues that the selectorate/winning coalition model by Bueno de Mesquita et. al. (2003) underestimates the durability of elites in many autocratic regimes. Elites in some regimes have less stake in the particular leader than selectorate theory assumes. In such cases, the elite domestic audience can replace the leader for poor performance. She further argues that, in many such regimes, there is little reason to believe that the small, elite domestic audience will be less conflict averse than a broad audience of voters. As a result, some autocracies will be no more likely to initiate conflicts than democracies (Weeks 2012).

I examine variation in autocratic regimes for evidence of accountability versus public accountability using data from Weeks (2012). Her personalist/non-personalist dimension captures whether the leader of the regime is subject to an elite audience. Personalist leaders are unrestricted, while others have to answer to either civilian or military elites. In non-personalist regimes, there is little reason to expect behavior which is much different than democracies, she argues[6]. If Weeks is correct, the predicted effect of democracy – negative in times of low threat, but magnifying the effect of external threat – should be observed, but only in contrast to personalist autocracies. Regimes that have elite domestic audiences that can hold the leader accountable should act similarly. The provision of national security because it is a public good cannot account for such an effect in small winning coalition regimes. An examination of autocratic variation, then, can help distinguish between the effect of political institutions incentivizing public good provision from their effect on incentivizing good governance. In the former, democracies behave differently than non-personalist autocracies. In the latter, they may not. If Weeks' argument is correct, the effect of public accountability I observe in Models 1-6 is really driven by the effect of accountability that separates democratic and non-personalist regimes from personalist regimes.

[6] In military regimes, you might see more conflict propensity (as she finds), but it is not a result of the domestic audience being attached to the leader. Rather, it is a product of a military audience being more acceptant of the use of military force as a policy tool than a non-military audience.

Security as a Public Good and Specific Consequences of Conflict

My argument is closely tied to the literature on conflict as costly for democratic leaders. Part of that story is the role of security as a public good. However, there is more to my argument than the statement that democratic leaders provide more public goods. Public accountability requires that the public can observe the performance of the leader. Public accountability makes leaders more sensitive to publically visible policy failures. As a result, democratic leaders are more responsive to threats that are observable to the public. While both the probability of manifestation and the severity of the threat are important, public accountability makes leaders particularly sensitive to the former. While any conception of threat ought, as Walt argues, to include both the intent and capacity to harm, democratic leaders are disproportionately concerned with intent, as intent is more representative of the likelihood of a threat manifesting, while capacity is more associated with severity.

Militarized conflict is the most visible instance of the manifestation of foreign threat, and how the state performs in that conflict is important for leader tenure (see, for example, Croco 2011). If there are security concerns that the public is unlikely to observe, I do not expect public accountability to lead to greater responsiveness to those concerns. Conflict is not the only security policy visible to the public, but it is the most

important. I find that the coefficient on the likelihood of conflict yields is larger for democratic states than non-democratic states. That result conforms my argument. However, the test provided by Models 1-6 does not distinguish between an argument about the general provision of public goods, and the narrower claim I make, about responsiveness to the possibility of conflict specifically.

Democratic leaders respond more because international security is a public good, but only do so when the threat is likely to be observable. A similar result could be found for other reasons. Perhaps the visibility is not crucial, and democracies respond more to all types of threats, either because international security is a public good, independent of observability, or for some other reason not discussed here. To evaluate that, I include an alternative measure of foreign threat in a series of tests, and interact it with regime type. I use Fordham and Walker's (2005) measure of the total capabilities of strategic rivals for one alternative measure of threat environment, and Nordhaus et. al.'s (2012) measure of the aggregated military spending of the state's potential foes (states with dissimilar alliance portfolios) as another. Neither measure focuses on intent like Nordhaus et. al.'s measure of the probability of conflict, though both capture aspects of the general strategic environment of the state. Aggregating the CINC scores of the rivals allows for variation between states with just one major threat, those with many, and the relative power of those threats. The aggregate measure of military spending of states with dissimilar alliance portfolios captures threat environment in a general manner,

without focusing on particular enemies. Most importantly, members of the public are unlikely to be aware of shifts in global aggregate military spending or rival material capacity. The public is more likely to know about the outbreak and outcome of war.

If democracies respond more to threats only because security is a public good, regardless of whether it is observable to the public or not, then the strategic rivals and aggregated military spending of foes models ought to exhibit similar findings to the likelihood of conflict models. On the other hand, null or contrasting results suggest that the effect of public accountability on threat response is particular to the probability of conflict, and not to other types of foreign threat.

Results

Table 3 summarizes the results of three models similar to Models 1, 4 and 5. Each model includes the variable personalist/non-personalist and its interaction with the likelihood of conflict. The aggregated capability of strategic rivals and the natural log of aggregated foe military spending, and their interactions with democracy, are also included. All of the results remain substantively the same when each alternative independent variable and its interaction is included in separate models.

Table 2.3: Models of Ln(Military Spending)

	Model 7	**Model 8**	**Model 9**
Dependent Variable *Logged Military Spending*	*Newey*	*Instrumented LDV*	*Driscoll-Kraay*
Independent Variables	b/se	b/se	b/se
Natural Log of GDP	1.063***	0.316***	1.063***
	(0.02)	(0.06)	(0.01)
Battle deaths as a percentage of prewar population	5.665***	1.758***	5.665***
	(0.66)	(0.36)	(0.29)
Likelihood of Conflict	1.508***	0.443***	1.508***
	(0.29)	(0.11)	(0.30)
Democracy	4.956**	0.844	4.956***
	(1.51)	(0.51)	(0.97)
Democracy X Likelihood of Conflict	1.521***	0.571***	1.521***
	(0.38)	(0.12)	(0.40)
Natural Log of Aggregate Foe Military Spending	0.404***	0.106**	0.404***
	(0.08)	(0.03)	(0.09)
Personalist Regime	0.144	0.038	0.144
	(0.16)	(0.04)	(0.13)
Personalist X Likelihood of Conflict	-0.252	-0.079	-0.252
	(0.63)	(0.16)	(0.47)
Democracy X ln(foes)	-0.427***	-0.082*	-0.427***
	(0.11)	(0.04)	(0.07)
Total CINC score of strategic rivals	2.725***	0.910***	2.725***
	(0.52)	(0.22)	(0.34)
Democracy X Total CINC score of strategic rivals	-6.025***	-2.065***	-6.025***
	(0.89)	(0.40)	(0.81)
Lagged Natural Log of Military Spending		0.699***	
		(0.05)	
constant	-9.922***	-2.705***	-9.922***
	(1.10)	(0.65)	(1.28)
N	5866	5503	5866

* p<0.05, ** p<0.01, *** p<0.001

The negative effect of democracy and the magnifying effect of democracy on the effect of likelihood of conflict are found in all of the models. Personalist regimes do not appear to respond differently than other regimes, and their inclusion does not wipe out the effect of democracy. The evidence demonstrates that there is an effect of public accountability, rather than elite accountability.

Democracies appear to be less responsive to other measures of threat, not more. It is only the possibility of conflict, with all its potential costs, to which democracies are particularly responsive. This suggests that democracies are, as I argue, particularly sensitive to threatening intent, rather than capacity, and primarily concerned with the potential outbreak and outcome of conflict.

Democratic Military Spending and Victory

I argue that military spending in democracies is driven by leader concerns about the political costs of conflict and conflict outcomes. Accordingly, I expect military spending by democracies to have a greater effect on conflict outcomes than military spending in non-democracies, where military spending serves other purposes. To evaluate this claim, I use data from Bennett and Stam (1998) on wars between 1823 and 1990, coded for outcome, military spending, and regime type. The military spending data comes from Nordhaus, Oneal and Russett (Nordhaus et. al. 2012), while the regime type data is the same as above, from Boix et al. (2013). There are 105 dyad-war-years that meet all the criteria. I am interested in the effect target military spending has on the likelihood of initiator victory. Initiators know how much of their spending is focused towards foreign conflict, and make their decisions with that knowledge. As a result, the effect of their military spending on victory may reside primarily in the initiator

advantage[7]. Target states, on the other hand, have less agency in selecting into the war (though not none). Accordingly, the effect of military spending on victory is better represented in target states than initiator states. If democratic military spending is, as I argue and the above evidence suggests, more directed towards the building of military capacity than autocratic military spending, military spending in democratic target states ought to have a greater impact on the likelihood of conflict than military spending in autocratic target states.

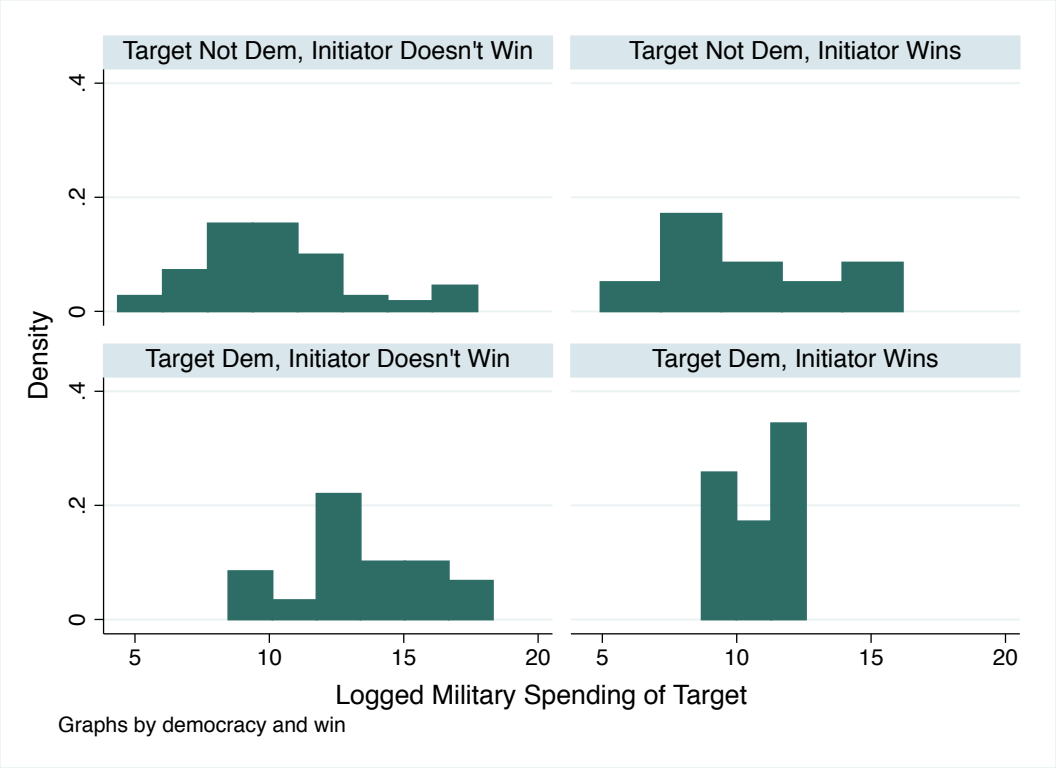
Hypothesis: *Military spending in democratic target states will suppress the likelihood of initiator victory more than military spending in autocratic target states.*

Figure 5 provides a first look at my hypothesis, as well as a summary of the data. Each histogram has the distribution of target logged military spending. The military spending of democratic targets who lose is in the lower right corner. Note that it is more narrow, and clustered on the left, compared to democratic targets who do not lose, indicating that democratic targets who lose typically spend less. There are no observations of democratic targets that lose and spend (comparatively) a lot on their

[7] One possible implication of this is that the better a state selects which conflicts to initiate, the less independent effect its own military spending ought to have.

military. In contrast, non-democratic targets look roughly the same whether the initiator wins or not.

Figure 2.5



For a more rigorous evaluation, I estimate three models, using initiator victory for the dependent variable. The first model is a logit with regime type and the logged military spending of each participant. The second is a logit model with the interaction of each state's regime type and logged military spending. The third is a heteroscedastic probit, which I include to account directly for heterscedasticity that might otherwise create inconsistent and inefficient estimates (Green 2003). The substantive implications of the third model are equivalent to the first, so I present marginal effects and predicted probabilities from the simpler logit model, with interactions (Model 2a).

Table 2.4: Models of Conflict Outcome

	Model 1a	**Model 2a**	**Model 3a**
Dependent Variable *Initiator Victory*	*Logit*	*Logit*	*HetProbit*
Independent Variables	b/se	b/se	b/se
Initiator democracy	1.892**	8.986**	2.06
	(0.63)	(2.79)	(1.11)
Target democracy	0.6	8.341*	2.138
	(0.67)	(3.59)	(1.15)
Initiator Military Spending (logged)	0.460***	0.475***	0.172***
	(0.12)	(0.13)	(0.05)
Target Military Spending (logged)	-0.475***	-0.146	-0.115**
	(0.13)	(0.17)	(0.04)
Initiatior Democracy X Initiator Military Spending		-0.614**	-0.132
		(0.23)	(0.08)
Target Democracy X Target Military Spending		-0.668*	-0.171
		(0.30)	-0.1
constant	-1.828	-5.719**	-1.303*
	(1.05)	(1.91)	(0.62)
LnSigma2: *Military Spending Ratio*			-1.359**
			(0.49)
N	105	105	105

* p<0.05, ** p<0.01, *** p<0.001

Figure 6 shows the estimated marginal effect of the target state's (logged) military spending on the probability of initiator victory for democracies and non-democracies. As predicted, the effect for democracies is negative and statistically significant, and of a greater magnitude than the effect in non-democracies, which is not statistically significant to the .05 level.

Figure 2.6

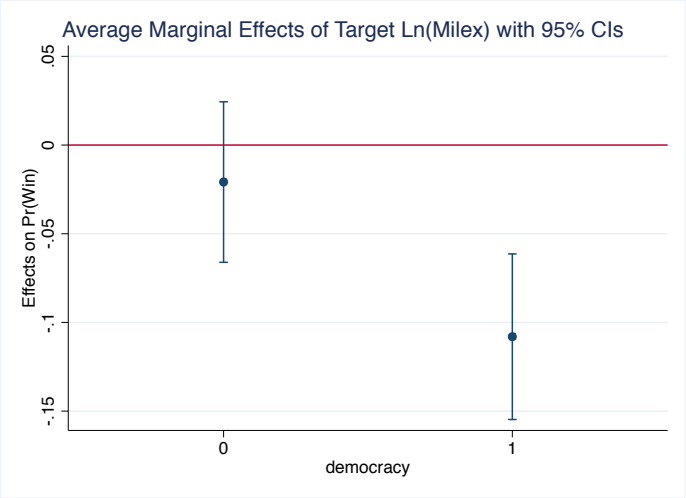


Figure 2.7

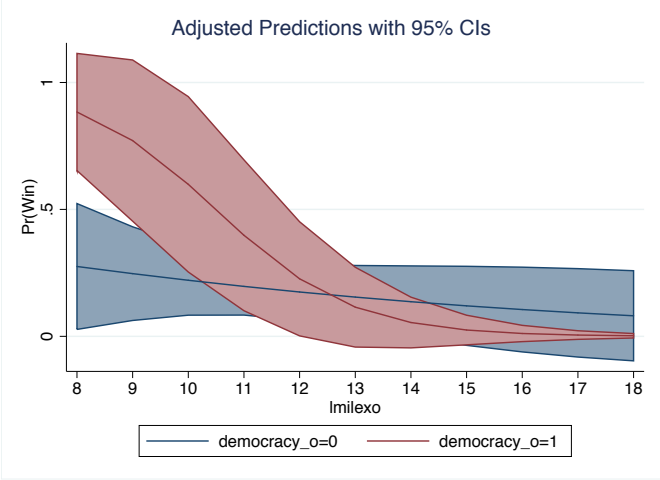


Figure 7 provides estimates of predicted probabilities for initiation victory across target spending levels for democracies and non-democracies. There is a considerable decrease in the likelihood of initiator victory as democratic target spending increases. Non-democratic targets are less likely to lose initially, but see little change in that probability as their spending increases.

Discussion

In democracies, domestic politics do not, in and of themselves, determine foreign policy. They shape how governments respond to their security environment. Democratic states invest more in their militaries in response to likely conflict than non-democracies. These findings update the literature in a number of ways. First, I show that democracy only has a dampening effect on aggregate military spending when conflict is unlikely. The previous argument that democracy is pacifying requires, at least in the context of military spending, a significant caveat. Far from being naturally more pacific, democracies arm more when conflict appears likely, even as they spend less when not threatened. They adapt more to the probability of conflict than other states. While some literature suggests that democracies do try harder during war, as argued by Bueno de Mesquita et al., (2003) and Goldsmith (2007), I find that their increased effort by democracies extends to likelihood of conflict, even in times of peace. This is in keeping with the insights of Clauswitz, and more recently Wagner (2000) and others. War is an extension of politics, rather than a wholly distinct process. I argue that it is specifically public accountability that drives democracy's effect on military spending. When the likelihood of conflict increases, leaders subject to public accountability increase their military spending. When the likelihood of conflict decreases, they cut it. Autocratic military spending, even when the leader can be held accountable, is a result of some other process. Both type of states may appear to be spending similar amounts

in a high threat environment, but as Figure 4 shows, they got to that level of spending in different ways.

Democratic spending is driven by a political need for success in, or avoidance of, international conflict. Because democratic military spending is directed towards deterring and winning wars, democratic targets become less likely to lose wars as they spend more. That helps explain why democracies appear to fight better, despite mixed evidence of trying harder (Reiter and Stam 2002). Autocratic military spending serves purposes other than war fighting, and so has a limited effect on war outcomes.

My findings show that, as suggested by Figure 1, democratic military spending is closely related to the security environment, particularly the possibility of war. Public accountability causes democratic leaders to be more sensitive to the costs of war, and therefore more responsive to the likelihood of war. To understand the importance of regime characteristics to foreign policy, future work needs to account for the interaction of the institutions of the state with the environment of the state.

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Chapter 3: Domestic Politics and Arming in Autocracies: Military Regimes and Military Spending (Paper 2)

Abstract

I argue that autocratic leaders use military spending as a way of buying support. Institutional differences between regimes affect the degree to which they do so, and so explain some variation in military spending across states. Military elites can extract rents directly when involved in non-military aspects of the government, making side payments through the military budget less necessary. As a result, military participation in government reduces military spending, with one exception. Immediately after a transition to a military regime from a civilian one, military leaders pay off their supporters in lower levels of the military. This results in a one-time increase in spending. The negative effect of military regime on military spending increases in magnitude as the regime ages. These findings contradict the existing literature empirically, and suggests that constituency and bureaucratic "you stand where you sit" arguments warrant reconsideration, particularly in light of the variety of ways in which a supporter can be rewarded.

How does military involvement in governance affect military spending? The first paper of this dissertation shows that democracies invest in their militaries differently than autocracies, spending less on average, but responding more to foreign threat. I argue that autocratic leaders use military spending as a way of buying support. Institutional differences between regimes affect the degree to which they do so, and so explain some variation in military spending across autocratic states. Military elites can extract rents directly when involved in non-military aspects of the government, making side payments through the military budget less necessary. As a result, military participation in government reduces military spending.

Review of the Literature

Variation in military spending has numerous causes. In much of the literature on military spending and international relations, military spending is shown to be a response to foreign threats, particularly from other countries. That contention (Waltz 1979, Walt 1990), is the basis for the considerable body of work on arms races (Richardson, Glaser 2000, Morrow 1993), and has been subject to recent empirical evaluation (Nordhaus et. al. 2012, Goldsmith 2003, Dunne et. al. 2007, Rosh 1988). The military provides security by increasing the capacity of the state to inflict costs through violence, and by preventing costs from violence, either through deterrence or direct prevention.

Recent literature on military spending in autocratic states focuses primarily on domestic causes of military spending. Kim, Kim, and Lee (2013), for example, find that military regimes that come to power as a result of coups spend more on their military than other autocratic regimes. They argue that military spending is a way of rewarding core supporters. Bueno de Mesquita and Siverson (2009), when discussing leader tenure and institutional change, echo that sentiment:

> "Hence in democratic systems, while some, such as defense contractors, benefit privately from the provision of security, the focus of defense spending is to protect the nation from a foreign threat. In contrast, in small winning-coalition systems, the policy focus is skewed toward private goods-bloated procurement contracts for cronies, and luxuries for offices are more important than an effective fighting force." (Bueno de Mesquita and Siverson, 2009. pg 937)

Conrad, Kim and Souva (2013) similarly argue that political participation influences variation in military spending across autocracies. According to their argument, variation in military spending is driven by the degree to which the leader is answerable to narrow interests, and the age of the regime. As the interests of the regime get narrower, military spending increases. As a regime gets older, the leader becomes more secure in her authority, which also increases spending (because the military is no longer seen as a significant threat to her power).

Powell, in his discussion of coup proofing, summarizes the work on coups and military spending thusly:

> "Short-term increases in material or financial incentives send a clear signal to the armed forces that their interests are being taken into account. This should greatly reduce the expected pay off from a coup, rendering one less likely." (Powell 2012)

Powell goes on to argue that sharing spoils with the military is one of two general strategies for avoiding coups. The other is changing the structure of the state's security apparatus, which increases the cost of staging a coup and decreases the likelihood of success. Roessler (2011) makes a similar point – governments exclude ethnic groups because exclusion decreases coup risk, even though it increases the risk of civil war. Fractionalization and exclusion, along structural or ethnic lines, of the military has both costs and benefits.

Recent work on autocratic variation and foreign policy is also relevant. For example, Weeks argues that the selectorate/winning coalition model by Bueno de Mesquita et. al. (2003) underestimates the durability of elites in many autocratic regimes. Elites in some regimes have less stake in the particular leader than selectorate theory assumes. In such cases, the elite domestic audience can replace the leader without the elites losing their positions. She further argues that, in many such regimes, there is little reason to believe that the small, elite domestic audience will be less conflict averse than a broad audience of voters. As a result, some autocracies will be no more likely to initiate conflicts than democracies (Weeks 2012).

Finally, my argument is informed by recent work on autocratic variation that emphasizes the different tools leaders use to remain in power. Work on autocratic institutions and leader tenure identifies two threats leaders contend with – those from elites within the government, and mass revolution from outside the government. Gandhi and Przeworski put it bluntly: "Autocrats face two types of threats to their rule: those that emerge from within the ruling elite and those that come from outsiders within society." (2007) Svolik calls these challenges "the problem of authoritarian power-sharing" and the "problem of authoritarian control." (Svolik 2012) These dual threats closely resemble Rossler's trade-off between coups and civil war threats, mentioned above.

Authoritarian leaders' use of legislatures and party systems to co-opt possible opponents (Svolik 2012), mitigate commitment and monitoring problems (Magaloni 2008, Boix and Svolik, Svolik 2012), and distribute private goods (Wright) occupy much of the recent work. Wright, for example, discusses how the use of legislatures differs across regime types. Personalist regimes use them to distribute private benefits, while in non-personalist regimes, legislatures are a mechanism for constraining the leader (Wright 2008).

Theory

Autocratic leaders have two primary goals – to remain in power and to extract rents. They face two threats to remaining in power, public uprisings and elite overthrow (revolts and coups). To deal with these threats, they use a combination of sticks, in the form of repression, and carrots, in the form of spoils and policy concessions. The relative use of these tools is shaped by the circumstances of the leader, and the institutions around them, be they inherited or constructed by the leader and her allies to solve a particular problem.

Political actors in autocratic regimes face a potential commitment problem when making power and spoil-sharing agreements. Once a leader wields some of the levers of power, she can use them to solidify her position, at the expense of others. Many elites have no recourse against abuse from the leader, though in some governments, legislatures can create trust and facilitate bargaining between the leader and potential rivals (Svolik 2012). Military elites, on the other hand, always have the option of intervening violently. Interventions are costly, and vary in their success rate, so members of the military are not eager to use them, but the potential for intervention diminishes their risk of exploitation by the leader. That allows them to form more credible agreements with the leader, and extract payments, even from otherwise unconstrained leaders. It also may increase the incentive of the leader to organize her government in ways that diminish the likelihood of successful coups (coup proofing).

Paying off the military

Militaries are multifaceted organizations and often feature numerous competing factions. I assume a relatively simple structure that distinguishes between military elites and rank and file members. These elites and rank and file members differ in the manner in which they can be bought. Elite actors care about policy concessions and political authority, as well as side payments, because they can use both to extract rents for themselves, while rank and file members care primarily about direct income, which comes in the form of military spending.

Military elites, like other elites, want primarily two things: to remain in power, and to extract rents. Unlike other elites, they have particular authority within the apparatus of violence within the state. As a result, even when they are not part of the formal governing apparatus, they possess leverage over the leader. The military functions as a coercive tool against political and civilian elites, and is a source of protection against public uprising. It is also capable of intervening violently against the leader. They use that leverage to demand a share of the profits of governing. When they are outside of the policy-making process, that side payment manifests as military spending. Military spending, then, is a form of distributive politics in autocratic regimes. This has several implications that may not be immediately apparent.

Military Spending in Civilian Regimes

Civilian leaders and elites want to avoid military intervention in the government. Military interventions often result in the replacement of the leader, and diminish the power of non-military elites. Militaries that are not already politicized typically wish to avoid intervention, which is costly and can reveal political fissures within the military itself (Svolik 2012). At the same time, military actors wish to reap the rewards of office, and civilian leadership depends on the military as the repressive apparatus of last resort. Accordingly, the civilian government funnels money to the military in sufficient quantity to offset any gains the military might receive from intervention, and to guarantee the support of the military in case of domestic opposition. Military spending, then, functions as coup proofing through distributive politics.

Military Spending in Military Regimes

Once the military has intervened in politics, continued intervention is likely, as some of the costs have already been paid, and the wall between the military and the rest of the policy apparatus has broken down. Holding office allows military elites to extract rents directly, granting them independent revenue streams and political power. Military spending becomes less important as a side payment – the gains of governing

are available to military elites directly. The more involved the military is in the governance of the state beyond military matters, the more it is able to directly extract rents from that process, just like other political elites. Military involvement also grants military elites control over public policy. In a military regime, military elites have acquired two new forms of reimbursement: policy concessions, and direct rent seeking. These make side-payments through military spending less important. As a result, military regimes will spend less on the military than civilian regimes.

H1. Military spending will be negatively associated with military regimes.

Hypothesis 1 is at odds with existing literature, which expects that military interventions will be associated with increases in spending (Kim et al. 2013), or unrelated to military spending (Conrad et al 2013). I believe these conflicting findings are a result of conflating the different effects of regime change and regime type.

Regime Change: Becoming a Military Regime

Shifts to and from military regimes have separate effects from the general effect of being a military regime. When military leaders first come to power, either through a coup, or in lieu of a coup, they typically do so with the support of the rank and file members of the military. This leads to an increase in military spending. One reason the military would stage a coup is because it is not being sufficiently paid off. So, post-coup

rents for military elites ought to increase. On its own, that may not be associated with an increase in spending, because the elites can extract rents directly once they are participants in the government. However, staging a coup also requires the support of the rank and file of the military, which requires paying them as well. Accordingly, immediate post-coup politics demand increased funds directed specifically toward the military. At moment right before and after a coup, the rank and file members of the military are at the height of their leverage – they can decide whether to back a military leader and which military leader to support. As a result, the leader needs to reward the rank and file shortly after coming to power, and typically does so financially[8]. Over time, because military elites can extract increased rents directly and command the loyalty of the rank and file, a military autocratic regime will spend less on its military than a civilian autocratic regime. In the short term, post coup, military elites need to funnel money directly to their broader base of support – the military public.[9] I expect this to be particularly true when the military ceases direct control over the government.

H2. *Transitioning from a civilian to a military regime will increase military spending.*

[8] Roman Emperors made one time payments to soldiers called donatives or donativums, often immediately upon coming to power. (Ng 2012)

[9] A similar, but reversed, dynamic takes place in transitions from military regimes to civilian ones. Such transitions require diminishing the military as a meaningful political institution. A period of defunding after a transition from a military regime is likely.

Regime Change: Becoming a Civilian Autocratic Regime

Civilian autocratic leaders, like military leaders, wish to consolidate power and extract rents. While some support from the military is always necessary, leaders in civilian autocratic regimes draw their support from civilian elites and publics. Once they are in power, leaders will want to replace current military leaders with leaders who are loyal to them, and purge the military of potential rivals. When a military regime transitions to a civilian regime, it will be associated with a one-time decrease in military spending as potential rivals are eliminated. Over time in a civilian autocratic regime, military spending may again increase, as the new military leaders solidify their position, and the civilian leader and elites increase military spending as a side payment and in response to risk[10].

H3. Transitioning from a military to a civilian regime will decrease military spending.

Regime Age

Conrad et. al. (2013) argue that as regimes age, and the leader solidifies her position, the interests she serves narrow, and this leads to an increase in military spending. I agree that military influence increases relative to other interests in regimes as they age. In civilian authoritarian regimes, that means increased military spending as

[10] Civilian leaders cut spending initially because they distrust the previous military elites. While new military elites may consolidate their positions over time (leading to increased side payments), they are still perceived by the leader as more loyal to the regime, allowing other factors (such as foreign threat) to lead to increased spending over time as well.

a side payment to military elites. In civilian autocratic regimes, regime age will be positively related to military spending, as Conrad et. al. argue (2013).

H4: As regime age increases in civilian regimes, so will military spending.

I argue that those side payments are unnecessary in military regimes, because the military elites can extract benefits directly. As military leaders consolidate their position, their need to pay the rank and file decreases, as they consolidate authority within the military and build support from civilian elites. Because military spending is primarily a side payment, the effect of regime age predicted by Conrad et. al. (2013) is negated by the military's involvement in politics, and the negative effect on military spending of being a military regime is magnified by regime age.

H5: The size of the negative effect of military regime will increase with regime age.

Varieties of Military Intervention

There are a variety of ways and degrees to which the military can involve itself in politics. Svolik breaks regimes into four categories based on the role of the military: civilian, indirect involvement by the military, corporate military involvement, and personalist military involvement. The latter two categories are forms of direct involvement, and require that the leader herself be a professional soldier, who came to power through military means (or was elected by a military junta) (Svolik 2012, pg 33).

There are two important considerations when evaluating the manner in which military involvement affects military spending: who is getting paid, and in what coin? In indirect regimes, the leader is a civilian, but the military is involved in politics. In such cases, I assume it is primarily military elites whose loyalty is being purchased who are being paid. Because they are elites, the leader can use both policy concessions and share spoils. In indirect military regimes, the military elites cannot extract rents directly from civilian portions of the military (though the military itself may have its own revenue streams). Instead, the military is able to exert some control over policy. In contrast, in a purely civilian regime, the military may have leverage over the leader, but it does not have influence over policy making; hence, the military elites are reimbursed financially, rather than politically.

H6: Indirect military regimes will spend less on their militaries than civilian regimes.

In direct military regimes, the leader is a professional soldier, who came to power with the support of the military. In personalist military regimes, the military is not institutionally integrated into the policy apparatus. The top political posts, including the head of state, are held by military elites, but otherwise the government is civilian. Military elites, then, are rewarded directly through control over policy and direct rent-seeking. As a result, side payments through military spending are unnecessary for elite buy-in – they serve primarily as income for middle and lower level soldiers. I expect

that, on average, direct, personalist military control of the regime will drive military spending down.

H7: Personalist military regimes will be associated with lower levels of military spending.

In corporate military regimes, the effect is not as clear. In corporate military regimes, the military are institutionally integrated into traditional civilian fields of governance[11]. In that case, those government roles could result in less military spending, because a significant portion of the military is reimbursed in other ways; or increased military spending, because the military has taken on responsibilities outside of its area.

Table 3.1: Hypotheses

H1. Military spending will be negatively associated with military regimes.
H2. *Transitioning from a civilian to a military regime will increase military spending.* H3. *Transitioning from a military to a civilian regime will decrease military spending.*
H4: As regime age increases in civilian regimes, so will military spending. *H5: The size of the negative effect of military regime will increase with regime age.*
H6: Indirect military regimes will spend less on their militaries than civilian regimes. *H7: Personalist military regimes will be associated with lower levels of military spending.* *H8: Personalist regimes (strongmen and bosses) invest more in their military than non-personalist regimes (juntas and machines).*
H9. Military spending will be negatively associated with political competition. *H10. Political competition reduces the effect of military regime*

Executive Accountability

Jessica Weeks argues that leaders who are accountable to elite audiences act similarly to leaders who are accountable to public audiences (i.e. democratic leaders). She distinguishes between regimes in which the leader answers to an independent

[11] This integration can be achieved in a variety of ways, including both membership in the legislature and responsibility for policy execution.

domestic audience, and those in which the leader does not. She codes the latter as personalist dictatorships. Combined with her conceptualization of military and non-military regimes, this yields four types of autocracies: strongmen, juntas, bosses, and machines. Strongmen are leaders with a military background who are surrounded by military advisors, but do not answer to military elites – they are unconstrained. Juntas feature military leaders who are constrained by military elites. Bosses are unconstrained civilian leaders, and machines are civilian regimes with accountable leaders. Weeks expects leaders to be more aggressive internationally when they cannot be held accountable to elites (2012). If she is right, personalist dictatorships will spend more on their militaries, because they are more likely to use military power.

H8: Personalist regimes (strongmen and bosses) invest more in their military than non-personalist regimes (juntas and machines).

Her argument does not negate mine, so as stated in hypothesis 1, I expect juntas and strongmen to invest less in their military than do machines and bosses (respectively).

Political Competition and Institutions

Conrad, Kim, and Souva (2013) argue that political competition expands the interests the leader serves, and as a result, decreases military spending. Political competition is related to accountability, but is often treated as a distinct concept and measure (see Goldsmith 2007, for example). I include Conrad et. al.'s hypothesis, and expand on it. Political competition decreases the ability of the leader and ruling elites to

engage in exclusive rent seeking and forces policy compromise. As a result, military elites, even when in control of the government, cannot pay themselves as much as they could in the absence of political competition. Political competition diminishes the effect of military regime on military spending.

H9. Military spending will be negatively associated with political competition.

H10. Political competition reduces the effect of military regime.

Methodology

Times series panel data can exhibit numerous problems to effective estimation. I follow Conrad, Kim and Souva (2013), and use established models from Fordham and Walker (2005) and Nordhaus, Oneal, and Russett (2012). Both sets of models address temporal autocorrelation. Fordham and Walker use Prais-Winsten estimation, while Nordhaus et. al. use a two-step least squares design that instruments for the lagged dependent variable. Because the models include different sets of control variables, and different dependent variables (discussed below), using both tests the robustness of my findings. The findings below are robust to a wide range of alternative specifications, including Newey-West and Driscoll-Kraay standard errors.

Measurement of the Dependent Variable

I use TSCS data of logged military expenditures from 1950-2000. Military spending as a percentage of GDP is easy to understand, and reflects how much of itself the state

dedicates to military power (Fordham and Walker 2005). I use Fordham and Walker's measure of military spending as a percentage of GDP for one measure of military spending. However, absolute changes in spending are important – 1% of GDP is not the same everywhere. However, each dollar spent is also not equal. There are diminishing marginal returns per dollar, so larger numbers of dollars are required to improve security as total spending increases. I use the natural log of military spending to approximate that dynamic. I use data from Nordhaus et. al. (2012)[12], which they construct using data from the Correlates of War project and SIPRI.

Measurement of Military Regimes

I use two measures of political institutions in autocracies, coded by Svolik and Weeks. Svolik breaks regimes into four categories based on the role of the military: civilian, indirect involvement by the military, corporate military involvement, and personalist military involvement. The latter two categories are forms of direct involvement, and require that the leader herself be a professional soldier, who came to power through military means (or was elected by a military junta) (Svolik 2012, pg 33).

Weeks codes regimes as personalist and non-personalist and military and non-military. The latter coding is based on the military background of the leader and her

[12] See their paper for a discussion of where and why they substitute SIPRI estimates for COW estimates, and vise versa.

immediate advisors. Personalist regimes are regimes where the leader does not answer to an elite audience. In non-personalist regimes, the leader can be removed and replaced by a cadre of political elites. Weeks uses these two categories to define four types of autocracies: strongmen, juntas, bosses, and machines. Strongmen and bosses are personalist, while juntas and machines are non-personalist. Strongmen and juntas are military regimes, while bosses and machines are civilian.

I use both sets of measurement to show that my findings are robust to different measurements of military involvement in governance. While Svolik's measure is conceptually closer to my argument, Weeks' measure provides an important robustness check. Weeks measure also has been explicitly linked to foreign policy decisions, making it well suited to models of military spending. (Svolik 2012, Weeks 2012). Their measures of military regime overlap significantly, but not entirely.

Table 3.2

	Military - Weeks		
Military - Svolik	0	1	
0	8,248	78	8,326
1	375	946	1,321
	8,623	1,024	9,647

Control Variables

The models presented include control variables from Fordham and Walker (2005), Nordhaus et. al. (2012), or Conrad et. al. (2013), but the findings are robust to the

inclusion of many others. Every model includes a measure of GDP, as well as variables that control for foreign security environment, a measure of regime age, and a measure of political competition (the latter two based on Conrad et. al 2013). With each of these, the estimated coefficients are in line with existing work.

Analysis

Table 1 provides summary statistics of my main independent and dependent variable, breaking down military spending as a percentage of GDP by each category of military regime, using Svolik's coding.

Table 3.3: Military Regime Types by Svolik

Military Involvement in Politics	Military Spending as a Percentage of GDP		
	Mean	St. Dev.	Frequency
Civilian	3.89	6.60	2349
Corporate	1.97	3.53	492
Indirect	1.18	1.06	71
Personal	2.11	3.47	642
Total	3.24	5.79	3565

The summary statistics are only suggestive, but they are consonant with hypothesis 1. On average, civilian governments spend considerably more on their military than any of the three types of military regimes. The same pattern holds if Weeks' definition of military regime is used.

Table 3.4: Regime Type by Weeks

Military Involvement in Politics	Military Spending as a Percentage of GDP		
	Mean	St. Dev.	Frequency
Strongman	1.89	1.91	588
Junta	1.22	1.13	403
Boss	4.78	8.98	617
Machine	4.26	5.21	658
Total	3.25	5.76	2266

Military Involvement: Hypotheses 1-3

Tables 2a and 2b include tests of hypotheses 1, 2 and 3. Hypothesis 1 is tested in Models 1a, 1b, 2a, and 2b. "A" models are based on Fordham and Walker (2005), and use military spending as a percentage of GDP for the dependent variable. "B" models are based on Nordhaus et. al's (2012) favored model, and use the natural log of military spending as the dependent variable. The models each include Fordham and Walker (2005) and Nordhaus et. al.'s (2012) control variables, respectively, as well as the main variables of interest from Conrad et. al., political competition and regime age (2013). Models 1 and 2 include measures of military regime from Svolik (2012) and Weeks (2012), respectively. Hypothesis 1 is supported in all four specifications. In all four models, military regime has a negative and significant effect on military spending.

Models 3 and 4 include everything in Models 1 and 2 as well as dummy variables for transitions to and from military regimes. Hypothesis 2 receives moderate support, as

there is a positive and statistically significant coefficient on transitions to a military regime in 3 of the 4 models (Model 3a is the exception), but there is no evidence in support of Hypothesis 3.

Regime Age and Type: Hypotheses 4 and 5

Table 3 presents results from Models 5 and 6, a and b. They are similar to the models 3 and 4, with two differences. Most importantly, regime age is interacted with military regime, allowing a more dynamic relationship between military regimes coming to power and becoming entrenched, and regime age. These models provide a test of hypotheses 4 and 5. The second difference is that, because a measure of military regime "newness" is included implicitly in the interaction of regime type and age, I excluded the dummy for transition to military regime.

Figure 1 shows the effect of regime age on military and non-military regimes. As Conrad et. al. (2013) predict, regime age increases military spending in civilian regimes. Contrary to their expectation, but consonant with mine, it has no effect in military regimes. In civilian autocratic regimes, as the civilian leaders consolidate their support, the number of actors they have to answer to decreases. However, the military always remains one of the crucial members of their coalition. As a result, they increasingly distribute funds to the military as they age.

Figure 2 shows the estimated effect of military regime over regime age in each of the four models. In all four, hypothesis 5 is supported. The negative effect of military regime increases in magnitude with regime age. As military regimes consolidate, the elites extract more benefits directly, requiring less military spending for side payments. Figure 2 also provides a secondary test of hypothesis 2. Only in model 5b, using the natural log of military spending and Svolik's measure of military regime intervention, is there a statistically significant evidence of a positive effect of transitioning to a military regime, though.

Varieties of Military Regimes and Executive Responsibility: Hypotheses 6-9

Table 4 shows the results of tests of hypotheses 6-8. The models are identical to models 3 and 4, with disaggregated measures of autocratic regime type instead of the dichotomous measures of military regime. Models 7a and 7b test hypotheses 6 and 7. In both models, personalist and indirect military regime have negative and statistically significant coefficients, as my theory predicts. Corporate military regimes, where military spending may increase to support the rank and file who do government work beyond traditional military responsibilities, or decrease because of military involvement in the government, show no statistically significant relationship. Models 8a and 8b test hypothesis 9, which predicts that Weeks' definition of personalist regimes – ones with

leaders who do not answer to elite audiences – will spend more on average that non-personalist regimes.

Table 3.5a: Models of Military Spending as a Percentage of GDP

	model1a	**model2a**	**model3a**	**model4a**
DV: Military Spending as a Percentage of GDP	b/se	b/se	b/se	b/se
Political Competition	-0.413***	-0.455***	-0.396***	-0.450***
	(0.08)	(0.09)	(0.08)	(0.09)
LN(Regime Age)	0.240*	0.268*	0.265*	0.297*
	(0.11)	(0.11)	(0.13)	(0.12)
Military Regime - Svolik	-0.414*		-0.601*	
	(0.21)		(0.27)	
Battle deaths as a percentage of prewar population	12.167*	12.451*	11.977*	12.304*
	(4.86)	(4.87)	(4.84)	(4.86)
Civil war deaths as a percentage of prewar population	0.066	0.066	0.069	0.026
	(0.05)	(0.05)	(0.05)	(0.05)
Total CINC score of strategic rivals	8.052***	8.237***	7.813***	8.069***
	(2.00)	(1.95)	(2.03)	(1.96)
Gross domestic product	-0.000**	-0.000***	-0.000***	-0.000***
	0.00	0.00	0.00	0.00
Population of empire	-0.000***	-0.000***	-0.000***	-0.000***
	0.00	0.00	0.00	0.00
Military Regime - Weeks		-0.577**		-0.880***
		(0.19)		(0.25)
Military Transition - Svolik			0.319	
			(0.18)	
Civilian Transition - Svolik			-0.203	
			(0.26)	
Military Transition - Weeks				0.431**
				(0.15)
Civilian Transition - Weeks				-0.301
				(0.22)
constant	3.302***	3.325***	3.272***	3.320***
	(0.46)	(0.46)	(0.49)	(0.46)
R-sqr	0.015	0.017	0.015	0.017

* p<0.05, ** p<0.01, *** p<0.001

Table 3.5b: Models of Military Spending as a Percentage of GDP

	model1b	**model2b**	**model3b**	**model4b**
DV: Logged Military Spending	b/se	b/se	b/se	b/se
LMILEX(-1)	0.609***	0.594***	0.608***	0.592***
	(0.13)	(0.14)	(0.13)	(0.14)
Political Competition	-0.116**	-0.125**	-0.116**	-0.125**
	(0.04)	(0.04)	(0.04)	(0.04)
LN(Regime Age)	0.072*	0.072*	0.083**	0.078*
	(0.03)	(0.03)	(0.03)	(0.03)
Military Regime - Svolik	-0.075*		-0.081*	
	(0.03)		(0.03)	
Probability of Conflict (Nordhaus et. al. 2012)	0.318*	0.349*	0.309*	0.349*
	(0.15)	(0.16)	(0.14)	(0.16)
LN(Real GDP)	0.374**	0.389**	0.374**	0.391**
	(0.12)	(0.13)	(0.13)	(0.14)
LN(FOES)	0.1	0.104	0.098	0.104
	(0.05)	(0.06)	(0.05)	(0.06)
LN(FRIENDS)	0.012	0.014	0.01	0.014
	(0.01)	(0.01)	(0.01)	(0.01)
Military Regime - Weeks		-0.120**		-0.129**
		(0.05)		(0.05)
Military Transition - Svolik			0.166*	
			(0.07)	
Civilian Transition - Svolik			0.006	
			(0.08)	
Military Transition - Weeks				0.128*
				(0.06)
Civilian Transition - Weeks				-0.046
				(0.07)
constant	-2.671*	-2.793*	-2.651*	-2.816*
	(1.17)	(1.25)	(1.17)	(1.27)
R-sqr	0.943	0.941	0.943	0.941

* p<0.05, ** p<0.01, *** p<0.001

Table 3.6: Models of Military Spending

	model5a	model6a	model5b	model6b
DVa: MilGDP; DVb: Ln(milex)	b/se	b/se	b/se	b/se
Political Competition	-0.416***	-0.445***	-0.107**	-0.115**
	(0.08)	(0.09)	(0.03)	(0.04)
LN(Regime Age)	0.364*	0.357*	0.113**	0.094*
	(0.16)	(0.14)	(0.04)	(0.04)
Civilian Transition - Svolik	-0.271		0.046	
	(0.26)		(0.09)	
Military Regime (Svolik)	0.049		0.164*	
	(0.34)		(0.07)	
Military Regime (Svolik) * Ln(Regime Age)	-0.376*		-0.101**	
	(0.19)		(0.04)	
Battle deaths as a percentage of prewar population	12.087*	12.320*		
	(4.85)	(4.86)		
Civil war deaths as a percentage of prewar population	0.071	0.073		
	(0.05)	(0.05)		
Total CINC score of strategic rivals	7.796***	7.975***		
	(2.02)	(1.97)		
Gross domestic product	-0.000***	-0.000***		
	0.00	0.00		
Population of empire	-0.000***	-0.000***		
	0.00	0.00		
Civilian Transition - Weeks		-0.37		-0.03
		(0.22)		(0.08)
Military Regime - Weeks		-0.066		0.063
		(0.26)		(0.05)
Military Regime (Weeks) * Ln(Regime Age)		-0.417**		-0.078*
		(0.16)		(0.03)
LMILEX(Lagged)			0.620***	0.607***
			(0.13)	(0.13)
Probability of Conflict (Nordhaus et. al. 2012)			0.313*	0.337*
			(0.14)	(0.15)
LN(Real GDP)			0.361**	0.375**
			(0.12)	(0.13)
LN(FOES)			0.096	0.102
			(0.05)	(0.05)
LN(FRIENDS)			0.008	0.013
			(0.01)	(0.01)
constant	3.135***	3.204***	-2.641*	-2.772*
	(0.50)	(0.47)	(1.15)	(1.22)
R-sqr	0.016	0.018	0.946	0.944

* p<0.05, ** p<0.01, *** p<0.001

Figure 3.1

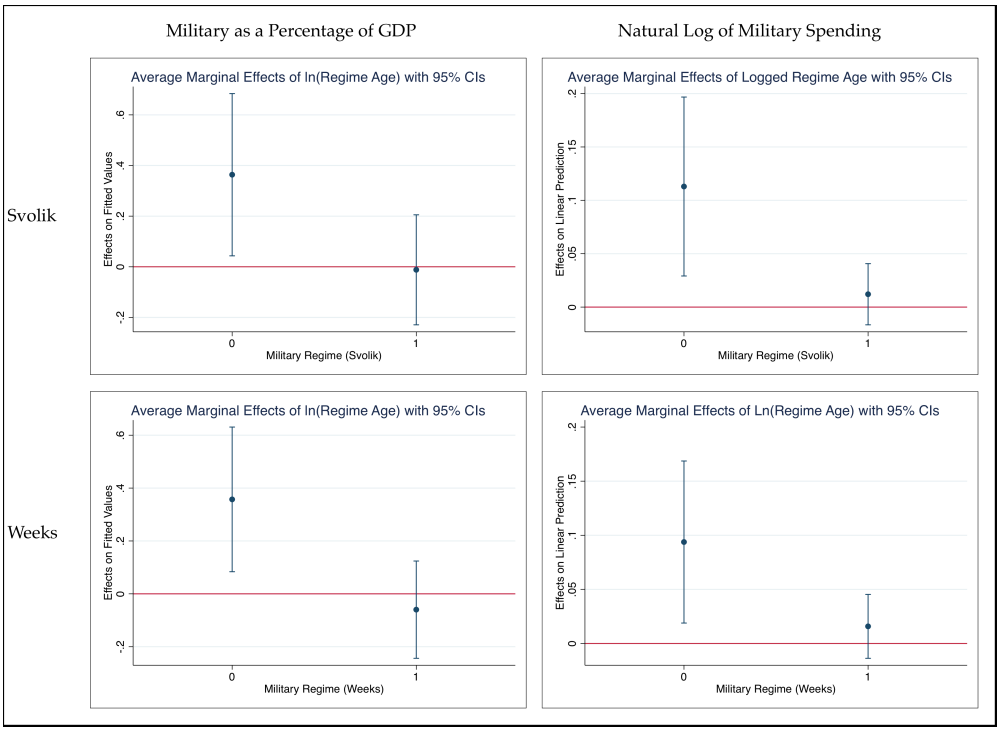
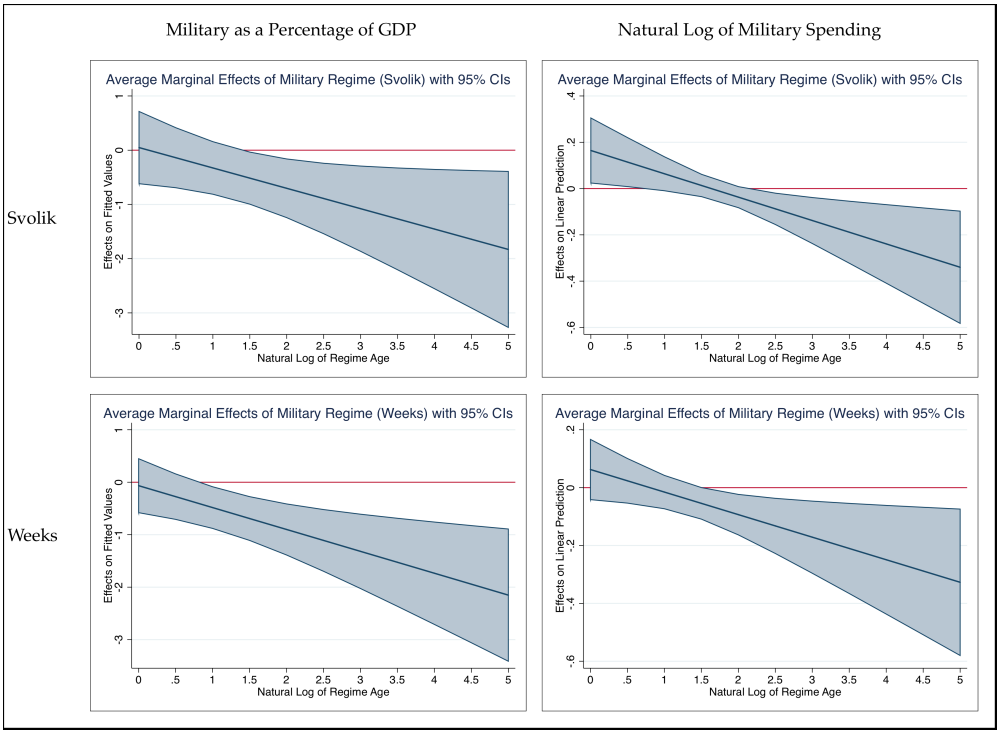


Figure 3.2



To evaluate the claim that personalist regimes spend more, the important comparisons are boss regimes to machines, and strongman regimes to juntas. To evaluate whether, controlling for personalist regime, military regimes still spend less, the relevant comparisons are boss to strongman and machine to junta. In both models the order of effect of regime type, from most negative to most positive, is junta, strongman, machine, boss. Table 5 contains the p-values of a chi-squared test comparing their coefficients. In the 4 relevant comparisons (2 comparisons over 2 tests), personalist regimes spend more than non-personalist regimes with a .1 level of significance (machine-boss is $p<.056$ for Model 7a). Model 7b shows no difference between bosses and machines. So the evidence in favor of hypothesis 9 is mixed, as personalist regimes appear to spend more than non-personalist regimes when that regime is military (strongmen spend more than juntas), but not necessarily more when the regime is civilian. That finding fits with my central argument that spending decreases in military regimes because members of the military can extract rewards directly, because in juntas a larger cadre of military elites hold authority. On the other hand, breaking the regimes out into different types of military regimes, either by Svolik's measure of military involvement or Weeks' measure of personalist/non-personalist regimes, does not diminish the negative effect of military regime on spending. Hypothesis 1 remains supported.

Table 3.7: Models of Military Spending

	model7a	model8a	model7b	model8b
DVa: MilGDP; DVb: Ln(milex)	b/se	b/se	b/se	b/se
Political Competition	-0.401***	-0.415***	-0.121**	-0.128**
	(0.08)	(0.09)	(0.04)	(0.04)
LN(Regime Age)	0.275*	0.279*	0.101**	0.090**
	(0.13)	(0.12)	(0.04)	(0.03)
Military Transition - Svolik	0.327		0.189*	
	(0.18)		(0.08)	
Civilian Transition - Svolik	-0.208		0.023	
	(0.26)		(0.09)	
Military Personalist - Svolik	-0.724*		-0.089*	
	(0.28)		(0.04)	
Military Indirect - Svolik	-0.863*		-0.179*	
	(0.44)		(0.08)	
Military Corporate - Svolik	-0.463		-0.041	
	(0.31)		(0.03)	
Battle deaths as a percentage of prewar population	12.052*	12.187*	3.096**	3.174**
	(4.85)	(4.85)	(0.96)	(1.05)
Civil war deaths as a percentage of prewar population	0.07	0.033		
	(0.05)	(0.05)		
Total CINC score of strategic rivals	7.905***	6.935***		
	(2.02)	(1.94)		
Gross domestic product	-0.000***	-0.000**		
	0.00	0.00		
Population of empire	-0.000***	-0.000***		
	0.00	0.00		
Military Transition - Weeks		0.384*		0.144*
		(0.15)		(0.07)
Civilian Transition - Weeks		-0.343		-0.04
		(0.23)		(0.08)
Junta		-1.094***		-0.161**
		(0.32)		(0.06)
Strongman		-0.359		-0.072*
		(0.33)		(0.03)
Machine		0.039		0.046
		(0.35)		(0.04)
Boss		0.887		0.048
		(0.46)		(0.04)
Probability of Conflict (Nordhaus et. al. 2012)			0.326*	0.373*
			(0.15)	(0.17)
LN(Real GDP)			0.394**	0.423**
			(0.13)	(0.15)
LN(FOES)			0.081	0.098
			(0.05)	(0.06)
LN(FRIENDS)			0.01	0.013
			(0.01)	(0.01)
LMILEX Lag			0.581***	0.553***
			(0.14)	(0.16)
constant	3.265***	3.109***	-2.503*	-2.857*
	(0.49)	(0.49)	(1.11)	(1.31)
R-sqr	0.016	0.018	0.94	0.936

* p<0.05, ** p<0.01, *** p<0.001

Table 3.8: T-test p-values for Regime Types on Military Spending

	DV: MILGDP				DV: LN(MILEX)			
	Junta	*Strongman*	*Machine*	*Boss*	*Junta*	*Strongman*	*Machine*	*Boss*
Junta		0.001	0.000	0.000		0.045	0.015	0.014
Strongman	0.001		0.158	0.002	0.045		0.026	0.026
Machine	0.000	0.158		0.056	0.015	0.026		0.949
Boss	0.000	0.002	0.056		0.014	0.026	0.949	

Other Political Institutions

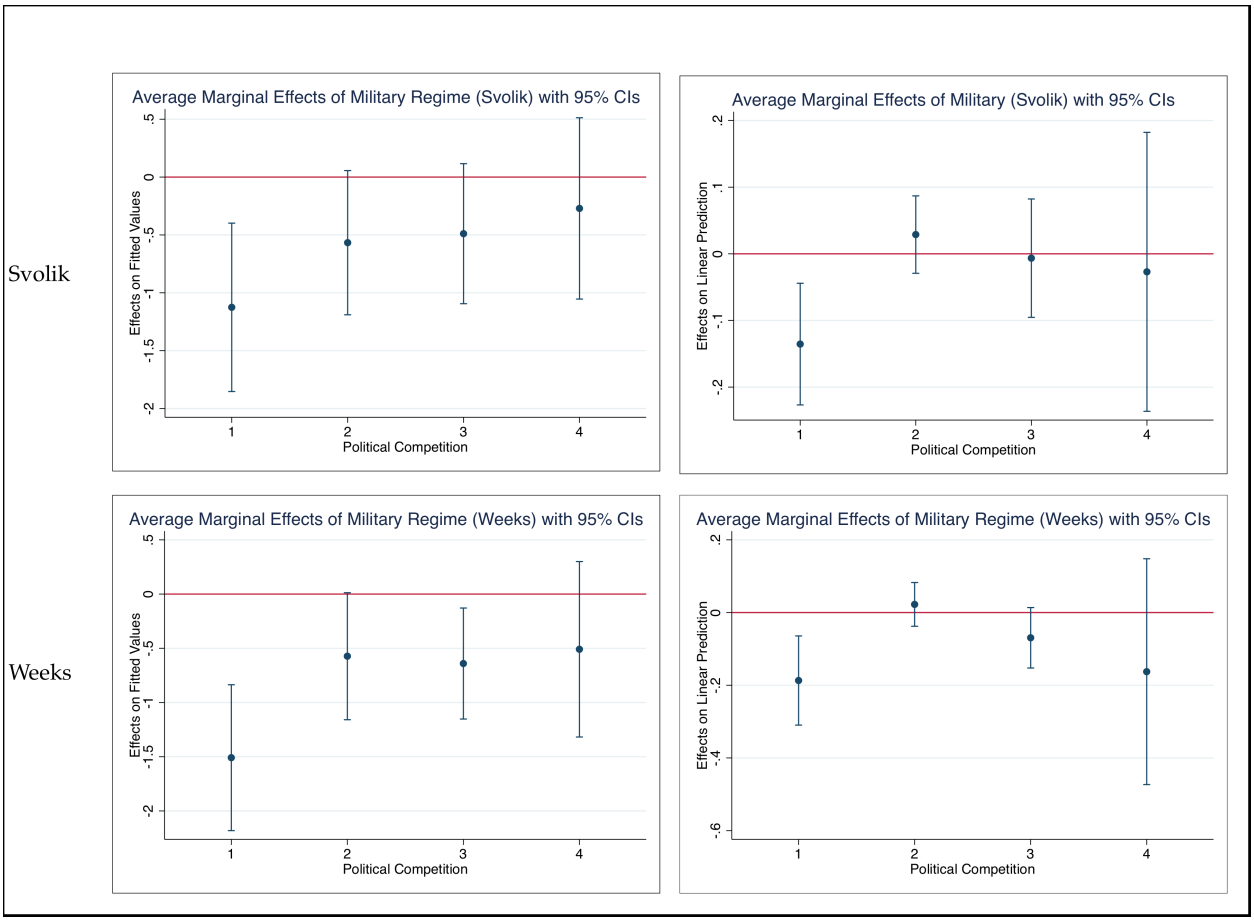
Hypothesis 9 does not require a separate test, because political competition is included in all of the above models. In each of them, it has a negative and statistically significant effect. Hypothesis 10 is tested by models 9 and 10 (a and b), which are presented in table 6. These models build on models 5 and 6, adding an interaction between political competition and military regime. Figure 3 presents the results of those interactions. When political competition increases, the negative effect of military regime diminishes. Interestingly, that effect appears to take place even at relatively low levels of political competition – it is only in the cases of very low political competition that there is a statistically significant effect of military regime. However, most military regimes reside in that category. While political competition ranges from 1 to 5, there are very few or no instances of military regimes with political competition values of 5. Accordingly, the graphs only present the marginal effect of military regime for political competition for values ranging from 1 to 4.

Table 3.9

	model9a	model10a	model9b	model10b
DVa: MilGDP; DVb: Ln(milex)	b/se	b/se	b/se	b/se
Political Competition	-0.514***	-0.546***	-0.119**	-0.123**
	(0.11)	(0.11)	(0.04)	(0.04)
LN(Regime Age)	0.357*	0.358*	0.107**	0.090*
	(0.16)	(0.14)	(0.04)	(0.04)
Civilian Transition - Svolik	-0.212		0.05	
	(0.26)		(0.09)	
Military Regime (Svolik)	-0.44		0.073	
	(0.45)		(0.06)	
Military Regime (Svolik) * Ln(Regime Age)	-0.355		-0.097**	
	(0.19)		(0.04)	
Military Regime (Svolik) * Political Competition	0.275*		0.055*	
	(0.13)		(0.02)	
Battle deaths as a percentage of prewar population	12.117*	12.364*		
	(4.85)	(4.86)		
Civil war deaths as a percentage of prewar population	0.072	0.076		
	(0.05)	(0.05)		
Total CINC score of strategic rivals	7.674***	7.846***		
	(2.04)	(1.98)		
Gross domestic product	-0.000***	-0.000***		
	0.00	0.00		
Population of empire	-0.000***	-0.000***		
	0.00	0.00		
Civilian Transition - Weeks		-0.334		-0.029
		(0.22)		(0.07)
Military Regime - Weeks		-0.681*		-0.013
		(0.34)		(0.05)
Military Regime (Weeks) * Ln(Regime Age)		-0.423**		-0.079*
		(0.16)		(0.03)
Military Regime (Weeks) * Political Competition		0.396**		0.054*
		(0.13)		(0.02)
LMILEX lag			0.630***	0.613***
			(0.12)	(0.13)
Probability of Conflict (Nordhaus et. al. 2012)			0.317*	0.344*
			(0.14)	(0.15)
LN(Real GDP)			0.351**	0.369**
			(0.12)	(0.12)
LN(FOES)			0.098	0.104
			(0.05)	(0.05)
LN(FRIENDS)			0.007	0.012
			(0.01)	(0.01)
constant	3.327***	3.389***	-2.601*	-2.755*
	(0.55)	(0.50)	(1.12)	(1.20)
R-sqr	0.017	0.018	0.948	0.945

* p<0.05, ** p<0.01, *** p<0.001

Figure 3.3



Discussion

Military spending in authoritarian regimes is an important side payment used by the leader to purchase the support of the military. Military involvement in governance creates substitutes for military spending as side payment, either by giving military elites the ability to extract rents directly or make policy. As a result, authoritarian military regimes of all stripes spend less on average on their military than do civilian authoritarian regimes. This finding has important implications for academic scholarship

on this topic and related policy questions. It shows that not all military spending is created equal. Literature that treats military spending as equivalent across states with regards to conflict is mistaken – much of the military spending in autocratic regimes is not intended for the creation of military power. Second, it shows that the manner in which leaders reward their supporters varies, and it is not sufficient to argue that a military leader will reward military supporters and a civilian will reward civilian supporters. Arguments about how leaders reward their constituents need to consider the variety of ways those constituents can be rewarded, and what involvement in (or exclusion from) politics means for those rewards. It may be true that military leaders depend more on members of the military for support than civilian leaders. This does not imply that they will budget more for the military, however. The evidence revealed in this paper shows that it is, in fact, the opposite. Perhaps counter intuitively, but sensibly in light of my argument, military participation in government reduces military spending. This finding similarly casts doubt on naïve versions of the bureaucratic "you stand where you sit" argument.

For policy-makers, my findings suggest that military spending in autocratic regimes should not be necessarily seen as indicating foreign aggressiveness. Rather, it may reflect domestic shifts in power within the state. The desirability of those shifts will depend on the observer – I make no claims about the relative superiority of civilian or military autocracies, but the assessment of their meaning should be made with the

knowledge that military spending functions significantly, perhaps even primarily, as a side payment in autocratic regimes. Further, these findings ought to assist policy-makers in predicting the likely outcome of certain types of regime changes. For example, military stewardship as part of a planned (though often not fulfilled) transition to democracy may be associated with a drop in military spending. That does not mean, however, that the military elites in charge of governance are not lining their pockets.

Finally, this article calls attention to how much work remains to be done on both military spending and autocratic variation. Military spending serves many purposes, and varies in purpose across contexts. Autocracies vary widely in their institutional make-up and security environment. I address political competition and executive accountability, leaving the role of institutions like legislatures, political parties, or independent courts for later work. The importance of the military to domestic security will vary across contexts, with active police and other security organizations providing some, but not all, domestic control. Those variations, too, will affect how militaries are funded, both in amount and in kind (what they spend it on). There is much on both topics left to explore, and studies of military spending can provide a useful window into policy making across regime types.

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Chapter 4: The Foreign Origins of Military Spending: Threat and Alliances as Sources of Interstate Contagion (Paper 3)

Abstract

I use a spatial lag and error model to examine three possible pathways of interdependence between military budgets: probability of conflict, rivalry, and alliances. I show that military budgets affect each other spatially through conflict probability and military alliances. States arm competitively when the likelihood of conflict is high. Allies, on the other hand, treat military spending by their allies as a substitute for military spending on their own. However, allies increase their spending in response to increases in their allies' likelihood of conflict. With two sets of models, I also show that modeling these processes separately can lead to false positive results, as rivalry creates statistically significant interdependence only when the likelihood of conflict is excluded from the analysis.

How does the military spending of one state affect the military spending of another? I examine how alliances and threat influence military spending in order to better understand why states invest the amounts they do in their militaries, and how the state policies are interdependent. I use a spatial lag and error model to examine three possible pathways of interdependence between military budgets: probability of conflict, rivalry, and alliances.

The empirical literature on interdependence between state foreign policies and military spending is lengthy. Early work focused on a small number of states, particularly superpower rivalries, and used a variety of methodological techniques. The most sophisticated of them found dependencies between the budgets and crisis behavior of competing states (e.g. Freeman 1983), but rarely, if ever, dealt with more than three actors. Some recent work has begun to include spatial lag techniques. Flores finds that a state's military spending is positively correlated with its allies' military spending (Flores 2011). Goldsmith also uses a spatial lag model, and finds a positive correlation between a state's military spending and its neighbors, as well as a regional effect on spending (Goldsmith 2007). I build on, and in some cases, contradict, those investigations.

Competitive Arming: Foreign Threat and the Security Dilemma

In the classic theoretical literature (Waltz 1979, Walt 1990) and work on arms races (Richardson 1960, Glaser 2000, Morrow 1993), military spending is considered a response to foreign threats. Recent empirical work supports that contention (Nordhaus et. al. 2012, Goldsmith 2003, Dunne et. al. 2007, Rosh 1988). A useful starting point for understanding threat and military buildups is the security dilemma. The security dilemma describes a situation where security is relative and competitive. The idea was first proposed in modern scholarship by Herz, and is a distillation of ideas from Hobbes, Rousseau and others (Herz 1950, Wagner 2007). Herz, and later scholars such as Walt and Wendt, also identify important ways in which the severity of the security dilemma can vary between pairs of actors. The security dilemma is described by Herz:

> Groups or individuals living in such a constellation must be, and usually are, concerned about their security from being attacked, subjected, dominated, or annihilated by other groups and individuals. Striving to attain security from such attack, they are driven to acquire more and more power in order to escape the impact of the power of others. This, in turn, renders the others more insecure and compels them to prepare for the worst. Since none can ever feel entirely secure in such a world of competing units, power competition ensues, and the vicious circle of security and power accumulation is on (Herz 1950).

The dominance of the security dilemma as the defining characteristic of the international system varies across different schools of thought in international relations theory. "Realists" argue that international anarchy means that all states are subject to

the security dilemma, and that therefore relative capabilities are the foremost concern of states foreign policy. Within the realist paradigm, there is ample variation, with Mearshimer (2001) arguing that all great powers must therefore be power maximizers, and Walt (1990) amending the concerns of states to include perceived intentions. "Liberals" generally believe that the security dilemma, while it exists, is not the defining characteristic of international relations. They are therefore more optimistic about the possibility of cooperation. Between those two paradigms exists a wide swath of assumptions, beliefs, and arguments about the severity of the security dilemma. Jervis, following Herz, discusses how cooperation is even with the security dilemma (Jervis 1978). Wagner argues that the security dilemma is not, itself, sufficient for war, and points to work by Fearon and Powell that explores competitive security through the related concept of the commitment problem (Wagner 2007). In perhaps the most broad-based argument about the role of the security dilemma in international politics, Wendt argues that the relationship between states is a product of the interaction of those states, their perception of each other, and their beliefs about the nature of the international system (1986).

Pairs of states may regard each other as threatening, and therefore find themselves in a security dilemma, but they may also view each other as friendly and non-competitive (at least in the realm of security). The literature that most directly tackles the question of why some states are more acrimonious than others is the enduring

rivalry literature (Colaresi et. al. 2008, Thompson 2001, Goertz and Diehl 2000). Enduring or strategic rivals view each other as geopolitical competitors.

Arms races do not necessarily emerge wherever there is a security dilemma. Rather, the security dilemma is a necessary condition for arms races. States can act to mitigate the security dilemma through international agreements or institutions. They can choose other options for increasing their security. They may choose to settle for the status quo rather than continuously ratcheting up military spending. However, in each of those scenarios, the amount spent by one state still depends on the amount spent by the other state. Put another way, if there is a security dilemma between states, military spending between states is interdependent. Their spending will move together. Dramatic, simultaneous increases, or arms races, are a subset of that.

Alliances and Arming

Alliances aggregate the capabilities of their participants, often against a common threat. Waltz referred to that process as external balancing, which he contrasted with internal balancing. Existing work on alliances often asserts that arms and allies are substitutes, but rarely tests that contention. More often, work on alliances focuses on a particular kind of substitution, free-riding. I make no effort to distinguish between

substitution that creates efficiencies, and substitution that results in the under-provision of security.

Interestingly, some investigations of alliances and arming even find that alliances increase spending across allies (e.g. Flores 2011). I argue that this is because they inadequately control for threat, and do not directly model the interdependence created by alliances between states' military budgets. Allies' spending will often increase together, but in response to omitted variables such as shared threat.

Theory

Foreign Threats and Competitive Arming

Countries engage in two types of competitive arming. Most directly, competitive arming is a result of the specter of armed conflict between the states. Potential conflict creates a security dilemma. Spending in state *i* makes state *j* less safe, and vice versa. That leads each state to increase spending when the other does, because if conflict occurs, neither wishes to be at a disadvantage. Dependence between the military budgets of two states is a product of the likelihood of conflict between them. As the probability of conflict increases, the effect of state j's military spending on state i's security increases. As conflict grows more likely, so does the dependence between the states.

H1. Positive interdependence between military budgets will correspond to the likelihood of conflict between states.

States may also engage in competitive arming independent of the likelihood of conflict. Arming can be a form of signaling or non-violent competition. It is a way two states may engage in a game of attrition, without engaging in the actual use of force. Arming in this manner is a result of a particular type of security dilemma, where states consider their power relative to one another, independent of the probability of conflict. Strategic rivals will be particularly prone to this kind of arming. Rivals regard each other as competitors. They emphasize relative gains more than other dyads. Arming between rivals is more than preparation for armed conflict, though it is that as well. It is a competition in of itself. Rivalry creates dependence between states beyond its effect on conflict likelihood.

H2. Strategic rivalry causes positive interdependence between state military budgets.

Alliances and Arming Substitution

States can contract for security through military alliances (Morrow 1991, Olsen and Zeckhauser 1966, Poast et. al. 2014). Alliances aggregate capabilities and threat. When state *i* and state *j* form an alliance, they agree to come to each other's defense (often

under particular circumstances)[13]. This leads to multiple paths of contagion and substitution.

Arming and alliances are substitutes (Morrow 1993). Alliances are formed because at least one state cannot or prefers not to increase their military spending to meet their security needs. When both (or all) allies have a common enemy, this can lead to more efficient spending. Consider a simple scenario, with three states, A, B, and their threat, T. If X dollars are required to balance against T, and A and B have to do so independently; each spends X, leading to 2X in military spending. If they ally and combine their resources, neither needs to spend the full X amount. Some amount between X and 2X is spent between them, leading to a more efficient distribution of resources. Resources devoted to defense by A reduce the quantity B needs to devote to defense, and vice versa. An alliance creates negative interdependence (substitution) between state military budgets.

H3: Alliances will be associated with negative interdependence between military budgets.

Alliances where there is not a common threat also create negative interdependence between military budgets. To amend the above scenario, consider if B is threatened by T but A is not. State A's spending, if A and B have an alliance, still contributes to B's

[13] This is a simplification, as alliances serve different purposes, and often more than one purpose. However, virtually all those purposes include a component of arming substitution and exchange. For example, offensive alliances are similar to defensive ones, with a different focus. They feature a target opponent, and while the goal is not to deter that opponent, but rather to conquer or coerce, less spending is required on the part of each than it would were they individually to pursue the same aims.

security. Similarly, A does not need to spend as much to provide security for B, if B spends more. However, A does have to spend more than it would were it not in an alliance, because now it is concerned with the threat to B, as well as to itself. It spends in respond to T, where otherwise it would not. When only one state has a threat in an alliance, the other state acquires some of that threat through the alliance. Accordingly, alliances introduce a positive dependence between a state and threats to its allies.

H4: States will increase their spending in response to increases in threat to their allies.

Research Approach

I use time series cross sectional data of state military spending, state characteristics, and dyadic relationships between the states to evaluate hypotheses 1-4. Dyadic relationships between states (pathways of possible interdependence) are grouped in matrices called W matrices. Each W matrix describes one set of connections. For example, the W matrix of alliances is an NTxNT matrix with a 1 where two cases share a military alliance, and a 0 otherwise.

Spatial Relationships and Interdependence

Theories about the strategic relations between states necessitate the treatment of states as interdependent actors. The actions of one state often affect the actions of

another state. This is particularly true in the field of security studies, which includes the prominent concepts of arming and allying. Empirical evaluations of such theories should therefore incorporate this interdependence into their statistical models. This can be achieved in two ways. One is by treating relationships as the unit of observation. Dyadic analysis is the most common example of this (though most analysis still does not account for interdependence between related dyads) (Neumayer 2010). The second way is by modeling the interdependence directly in the statistical model, as advocated by Franzese and Hays and others. (Franzese and Hays 2007, 2008)

The use of spatial models in political science has ballooned in recent years, with good cause. Many phenomena of interest, across virtually all fields of political science, involve some type of interdependence between the units of analysis. The use of *spatial* to describe this interdependence is convenient, and in many cases distance or geography is a relevant source of interdependence, but it should not be taken to mean that all interdependence is a result of proximity in space. There are many ways in which units in a model can be linked in a manner that impacts the outcome being studied, whatever it may be. The appropriate method for modeling such interdependence is spatial regression.

Models that fail to specify interdependence when it is present risk inflating the estimated coefficients on unit level variables. Variables that correlate with the true terms of connectivity are typically the most inflated. To the extent that one portion of

the model is misspecified, typically leading to coefficients that underestimate the magnitude of the relationship, the coefficients for other portions will be overestimated. The exclusion or poor specification of one portion of a data-generating process (often, but not always, the interdependence), introduces, in essence, omitted variable bias, which can lead to poor estimations (exaggerated typically) for even the well-specified portion of the model.

Models of interdependence face a number of methodological challenges, including Galton's problem and simultaneity bias. Galton's problem refers to the difficulty of distinguishing between common shocks (or other mechanisms that can affect a group of units similarly) and true interdependence. As with other difficulties, misspecification in either the common shock or interdependence portion of the model exacerbates this problem. Simultaneity bias[14] is a result of endogenity between the y variables – specifically, if y_i is dependent on y_j and y_j is dependent on y_i, then, in the case of positive interdependence, the estimated coefficient on W may be inflated[15]. This can, in turn,

[14] Kays, Kachi, and Franzese (2010) state that the inclusion of only a time-lagged spatial lag removes the simultaneity bias that exists in estimation with spatial OLS rather that ML. However, as they go on to argue, that particular scenario does not seem to reflect the reality of many social phenomena, especially given the large units of time typical for TSCS data at the international level.

[15] In situations of negative interdependence this may be reversed. In both cases, the effect of inflation or deflation on other portions of the model also depends on the direction of the correlation.

lead to the coefficients on unit level variables being underestimated, particularly those variables positively correlated with variables of interdependence[16].

A generic spatial-autoregressive model with spatial-autoregression in the disturbances can be written[17]:

$$\boldsymbol{y} = \lambda \mathbf{W}\boldsymbol{y} + \mathbf{X}\boldsymbol{\beta} + \mathbf{u}$$

$$\mathbf{u} = \rho \mathbf{M}\mathbf{u} + \varepsilon$$

The above includes unit-level explanatory variables $\mathbf{X}$, with coefficients $\boldsymbol{\beta}$. $\mathbf{X}$ represents NT observations with *k* independent variables (presumed exogenous) and β is their corresponding *k x 1* and vector of coefficients. The dependent variable is represented by the NT x 1 vector $\boldsymbol{y}$. $\mathbf{W}\mathbf{y}$ defines the spatial lag, with the NT x NT matrix $\mathbf{W}$ as a spatial weighting matrix that defines the relative weights of connection between one unit in $\mathbf{y}$ with the other units in $\mathbf{y}$. $\mathbf{W}$ describes the relationship between the units, with λ the coefficient on $\mathbf{W}\mathbf{y}$[18]. $\mathbf{M}$ is often identical to $\mathbf{W}$, and describes the relationship between the disturbances. In a model without spatial autoregressive disturbances, $\mathbf{u} = \varepsilon$. ε is an NT x 1 stochastic term and is frequently assumed to be i.i.d. In the

[16] In time series cross-sectional data, a time lag can introduce one further complication. Specifically, the first period (however defined) is treated as fixed, and the rest of the estimation is conditional on the y value of that first period. In low t contexts that can lead to biased results. However, that bias shrinks as the number of time periods expands.

[17] I follow Drukker et. al. (2013) notationally, but an identical model could be written differently (see, for example, Franzese and Hays 2007 and 2008).

[18] $\lambda\mathbf{W}\boldsymbol{y}$ can be rewritten as $\lambda \sum w_{ij} y_{jt}$. Note that w_{ij} denotes the connections between the observations, while λ is the strength of those connections.

estimated models below, I assume the innovations are independent but heterosketastically distributed.

Data

Dependent Variable: Military Spending

I use TSCS data of logged military expenditures from 1952-1999. I use the natural log of military spending in lieu of military spending as a percentage of GDP because of the enormous variation in the size of states. 1% of a small state's GDP cannot substitute for 1% of a large state's GDP in an alliance. I get my data from Nordhaus et. al.'s article on relationship between threat environment and military spending (2012). They use a combination of data available from the Correlates of War project and SIPRI[19].

W Matrices

I construct three matrices describing possible lines of dependence between states. These matrices of connectivity, or W matrices, correspond to hypotheses 1-3. The first is a predicted probability that a fatal MID will occur between states *i* and *j* that year. The

[19] See their paper for a discussion of where and why they substitute SIPRI estimates for COW estimates, and vice versa.

second is whether *i* and *j* are strategic rivals. The third is whether *i* and *j* are members of a formal alliance together.

Row Normalization

Row normalization makes interpreting coefficients easier, but makes assumptions about how states respond to threats. With row normalization, adding an additional rival will only lead to an increase in threat experienced by the state if the new rival spends more on its military than the previous rival(s). A row normalized W measures the relative importance of each other state as a source of contagion, but not the net exposure to contagion. I do not normalize my W matrices, in order to account for net exposure as well as the relative influence of each y_j.

Predicted Probability of Fatal MIDs

I used a logit model to generate predicted probability $\hat{p}$ of a fatal mid between *i* and *j* for each year, based loosely on Hegre, Oneal and Russett (2010)[20]. To avoid predicting military spending with military spending, I used GDP in lieu of COW material capabilities scores or military spending as a measure of power. I also included distance between states and contiguity, polity scores for each state (Marshall et. al.

[20] I used their replication data, but simplified the model considerably.

2009), alliance portfolio similarity (Signorino and Ritter 1999), the natural log of trade between states, the presence of preferential trade agreements, year dummies, and a one year lag of the dependent variable. Because I want to distinguish between the effect of rivalry and of conflict, I excluded strategic rivalry from my measure. Similarly, while I included Signorino and Ritter's measure of alliance portfolio similarity (1999) (as Hegre et. al. does), I did not include whether or not the two states are allies. All of the variables used are knowable by the participant states, so $\hat{p}$ can be thought of as an estimate of states' belief about the likelihood of conflict.

Strategic Rivals

I use Colaresi et. al.'s measure of strategic rivalries (Colaresi et. al. 2008). They examine diplomatic histories to determine which states mutually viewed each other as rivals. Many other measures of rivalry are available, but most rely on some measure of conflict history. Colaresi et. al.'s focus on the perceived competition between the states is a better fit for my conception of perceived threat, particularly as a complement to likelihood of conflict. With the likelihood of conflict included, the strategic rivals measure captures the effect of a rivals spending, controlling for probability of conflict. That allows me to distinguish between arming due to non-violent strategic competition and arming driven by the expectation of fighting.

Military Alliances

I use ATOPs data on military alliances to build this matrix (Leeds et. al. 2002). I use all alliances, not just dyadic ones, but because my unit of analysis is country year, each connection is dyadic. In a dyadic analysis, breaking multilateral agreements into dyadic agreements would lead to biased estimates (Poast 2010). In this case, using multilateral agreements assumes that the substitution effect between states is the same in multilateral and dyadic agreements[21]. Because I do not row normalize, **Wy** strictly increases as the number of allies increase.

Construction of Spatial Lags and Instruments

I use the *spmat* function in the *sppack* stata package to create spatial lag variables with each of the **W** matrices. I generate two types of lags: spatial lags of *y*, Wy, and spatial lags of RHS variables, x, Wx. I use the Wys in models 4-6. I use one Wx, the spatial lag of ally likelihood of conflict (from Nordhaus et al. 2012), to test hypothesis 4. The Wx variables are instruments for the Wys in models 4-6.

[21] Though I do not evaluate them here, one can imagine a variety of alternative possible specifications that fit with the general argument of substitution, but designate more complicated alliance relationships. For example, rather than a W contiguity matrix, populated by 0s and 1s, with 1s being an alliance between states, one could design a matrix that divides the distance measure by the number of allies in that alliance (i.e. a dyadic alliance would yield a score of .5, and triadic alliance .33, and so forth). That would suggest that while there is a substitution effect, each ally added to the alliance also waters down the benefit (in capabilities) received by state *i*.

Unit-Level Control Variables

I use a variable generated by Nordhaus et. al. as a unit-level measure of foreign threat (2012). They estimate the aggregated likelihood of conflict, given what the state knows at the time. The result is a measure of threat environment based on expected likelihood of conflict[22]. The model they use is similar but not identical to the one I use to generate phat for the likelihood of conflict matrix. Following Von Hagen-Jamar (Paper 1), I use Boix et. al.'s recently dichotomous coding of democracy (Boix et. al. 2012). They evaluate countries based on their suffrage and political competition, coding states with high levels of both as democracies, and the remainder as non-democracies. I include the natural log of real GDP[23] to control for available resources. I follow Fordham and Walker (2005) and include variables that count battle deaths as a percentage of pre-war population for interstate and intrastate wars; this controls for wartime spending. Finally, I include a lag of the dependent variable, to account for temporal dependence[24].

[22]Nordhaus et. al. argue that the ex ante quality of their measure alleviates concerns about endogeniety between military spending and the likelihood of war. In models where I additionally instrument for their measure, or leave it out entirely, I do not find results that are substantively different than those presented below.

[23] I again use Nordhaus et. al.'s data (2012), and refer the reader to their article for a discussion of its construction.

[24] In models 1-3 I mimic Nordhaus et. al's technique and instrument for the lagged term using lags of GDP and the likelihood of conflict. In models 4-6, however, I include the variable as is, and focus on instrumenting the spatial lags.

Estimation

There are four common estimation strategies that are relevant to estimating spatial lag models: OLS, Spatial OLS (S-OLS), Spatial 2SLS, and Spatial Maximum Likelihood. OLS suffers from (potentially severe) omitted variable bias. Nevertheless, it remains the most typical approach in political science for estimating models, including those that purport to be about interdependent systems. Spatial OLS specifies the interdependence theorized (and so is an improvement over OLS), but it suffers from the aforementioned simultaneity bias. Spatial maximum likelihood estimation is less prone to simultaneity bias and outperformed OLS based spatial regression in simulations (Franzese and Hays 2007, 2008). Spatial 2SLS deals explicitly with that simultaneity bias by using exogenous covariates in the model as instruments in order to resolve the endogeniety between the vector $\boldsymbol{y}$ on the right side and on the left side.

I use a spatial two-step least squares approach, estimated using the stata function *spivreg* from the package *sppack* to estimate 6 models (Drukker et. al. 2013). The first three address hypotheses 1-3 in turn. Models 4-6 each evaluate hypotheses 1-3, with minor changes in model specification. All of the models test hypothesis 4. Spivreg is designed estimate spatial 2sls models with additional endogenous variables. It accepts one W matrix for the dependent variable, and allows spatial autoregression in the disturbances as well. Including the possibility of spatial error helps to distinguish between shocks that are jointly experienced by connected states and actual dependence

between y_i and y_j. In each of the models, I use the same W matrix for both spatial processes. In models 4-6, I include spatial lags generated using spmat, and instrument them using the corresponding Wxs[25]. Therefore, each model contains all three pathways of interdependence, but because my instrumentation is slightly different than that done by *spivreg*, and each only has spatial autoregression in the disturbances along one W matrix, there are small variations in the estimates each model produces. In all models, I allow for heterogeneity in the disturbances.

Results

Table 1 shows the estimates for models 1 through 3. Note that coefficients do not equate to full effects, because y is determined simultaneously. However, the direction and significance of the coefficients remain meaningful. Note also that because the W matrices are not row standardized, the spatial variables are not on the same scale, so their coefficients are not directly comparable.

Model 1 uses the likelihood of conflict as the spatial variable. The coefficient on it is positive and significant, supporting hypothesis 1. It also features a positive and significant spatial relationship in the disturbances. Nordhaus et. al.'s (2012) unit level measure of threat environment is also positive and significant. Hypothesis 4 is not

[25] I exclude W*pr(conflict), because I use the spatially lagged probability of conflict, with the alliance W, as a RHS variable in the model. I also exclude the W*lagged DV.

supported, as there is a positive but insignificant coefficient on allies' likelihood of conflict (from the Nordhaus et. al. 2012 measure). Model 2 uses strategic rivalry as the spatial variable (Colaresi et. al. 2008). As in model 1, the spatial variable is positive and significant in both the spatial lag and error coefficients. Hypothesis 4 is again unsupported. Model 3 uses military alliances as the spatial variable to test hypothesis 3. The coefficient is, as hypothesis 3 predicts, negative and statistically significant. Hypothesis 4 finds weak statistical support (significance level $p<.1$). Interestingly, the coefficient on the spatial error is positive and statistically significant, suggesting that, while allies have negative interdependence in their military spending, they are subject to common disturbances. That may explain why previous work has found ally military spending to be positively correlated.

Models 1-3 all find support for their respective hypotheses, suggesting that there may be multiple pathways of interdependence between states. Unfortunately, that also means all 3 models suffer from omitted variable bias of unknown severity or direction. If networks of rivals and likely conflict participants overlap (and they do), a false positive on either of those spatial lags could result from excluding the other. Unit level variables related to threat or allies may also be biased by the exclusion of relevant spatial lags. Accordingly, models 4-6 include all three spatial lags. Each model has a different spatial lag in the disturbance. Table 2 presents the results of models 4-6.

Table 4.1

DV: Natural Log of Military Spending	**Model 1**	**Model 2**	**Model 3**
Unit Level Variables	b/se	b/se	b/se
Natural Log of Real GDP	0.1219**	0.1155***	0.1103**
	0.0430	0.0302	0.4283
Democratic Regime	-0.0868**	-0.0834***	-0.0478*
	0.0284	0.0215	0.0191
Military Regime	-0.0278	-0.0310*	-0.0137
	0.0173	0.0156	0.0140
Aggregate Likelihood of Conflict	0.2279**	0.2220***	0.1985*
from Nordhaus, Oneal, & Russett 2012	0.0838	0.0587	0.0818
Aggregate Likelihood of Conflict of Allies	0.0022	0.0021	0.0341+
	0.0033	0.0027	0.0201
Battle deaths as a percentage of prewar population	0.6943*	0.5102*	0.5625*
	0.2960	0.2078	0.2600
Civil war deaths as a percentage of prewar population	-0.0351	-0.0374	-0.0335
	0.0500	0.0478	0.0530
Lagged DV	0.8811***	0.8867***	0.8950***
instrumented with lagged ln(rGDP) & Pr(Conflict)	0.0418	0.0295	0.0412
Constant	-0.4628*	-0.4293**	-0.4320*
	0.1830	0.1279	0.1887
Spatial Variables			
Likelihood of Fatal MID	0.0153**		
	0.0057		
Strategic Rivals		0.0017*	
		0.0008	
Allies			-0.0011*
			0.0005
Spatial Autocorrelation in the Disturbance	0.4099***	0.0461***	0.0528***
(Same spatial relationship as above)	0.1081	0.0138	0.0034

+ p<0.10, * p<0.05, ** p<0.01, *** p<0.001

All estimates using spivreg (gs2sls) in Stata 13

Table 4.2

DV: Natural Log of Military Spending	**Model 4**	**Model 5**	**Model 6**
Unit Level Variables	b/se	b/se	b/se
Natural Log of Real GDP	0.077***	0.078***	.0817***
	0.0115	0.0116	0.0118
Democratic Regime	-0.046***	-0.046***	-.0356**
	0.0124	0.0123	0.0124
Military Regime	-0.015	-0.016	-0.0090
	0.0123	0.0123	0.0120
Aggregate Likelihood of Conflict	0.085*	0.089*	.1176**
from Nordhaus, Oneal, & Russett 2012	0.0389	0.0384	0.0389
Aggregate Likelihood of Conflict of Allies	0.030***	.0325***	.0239**
	0.0059	0.0060	0.0079
Battle deaths as a percentage of prewar population	0.355+	.3612*	0.3902*
	0.1874	0.1824	0.1866
Civil war deaths as a percentage of prewar population	-0.052	-0.053	-0.0423
	0.0389	0.0385	0.0466
Lagged DV	0.925***	0.925***	.9217***
	0.0110	0.0110	0.0114
Constant	-0.2734***	-0.2761***	-.3013***
	0.0521	0.0523	0.0531
Spatial Lags (instrumented with Wx)			
Likelihood of Fatal MID		0.0105**	0.0080*
		0.0035	0.0039
Strategic Rivals	0.0009		0.0004
	0.0006		0.0006
Allies	-0.0009***	-0.0010***	
	0.0002	0.0002	
Spatial Variables			
Likelihood of Fatal MID	0.0125**		
	0.0041		
Strategic Rivals		0.0009	
		0.0006	
Allies			-.0008***
			0.0002
Spatial Autocorrelation in the Disturbance	0.3399***	0.0423**	.0195***
(Same spatial relationship as above)	0.0903	0.0140	0.0023

+ p<0.10, * p<0.05, ** p<0.01, *** p<0.001

All estimates using spivreg (gs2sls) in Stata 13

The results in models 4-6 differ from models 1-3 in several important ways. In all three models, hypothesis 4 is supported. States do spend more in response to increases in their allies' likelihood of conflict. In all three models, alliances create a substitution effect between a state and its allies – hypothesis 3 is supported. Hypothesis 1 is supported in all 3 models as well, including model 4, where the spatial error W matrix is likelihood of conflict. Hypothesis 2, however, is not supported in any of the models. Models 4-6 do not provide statistically significant evidence that strategic rivals engage in competitive spending, when the likelihood of conflict is included in the model. That suggests that the positive finding in Model 2 is a product of likelihood of conflict being omitted.

Discussion

I find strong evidence that military spending in one state affects others, and that it occurs through multiple pathways. This is, firstly, an addition to the literature that evaluates whether and when arms races occur. Countries engage in competitive arming when they believe war is likely. By separating two aspects of the security dilemma, the likeliness of conflict, and the perception of the importance of relative material strength, I reveal more about who engages in competitive arming, and why. There is strong evidence that the probability of conflict creates interdependence between military

budgets. Evidence supporting strategic rivalry and interdependence is much weaker. This suggests that competitive arming is likely to occur where conflict is likely, but not necessarily so where strategic rivalry exists. Policy makers looking to decrease the militarization of pairs of states should, then, focus on decreasing the likelihood of war, rather than diminishing the acrimony between states (except insofar as the latter does the former).

I also contribute to the literature on the causes of military spending, which has recently focused on the role of state characteristics, particularly regime type (Goldsmith 2003, Fordham and Walker 2005, Conrad et. al. 2013) with some work examining the international causes of arming. In particular, I build on the findings of Nordhaus et. al. (2012) and Goldsmith (2007), by distinguishing between types of threats, and by modeling interdependence directly. Increases in the likelihood of conflict lead to increased military spending, as Nordhaus et. al. find, but not only in the manner that they describe. Rather, increases in the likelihood of conflict increase the dependence of a state's military spending on its likely opponent's military spending. The relationship between likelihood of conflict is spatial, as well as linear. The evidence in favor of a similar relationship between rivals, however, is limited. The implication is, then, that security dilemmas lead to competitive arming, but only when the security risk is real, and not simply perceived. The perception of competition is not, in and of itself, sufficient to lead to interdependence. This contrasts with findings by Rider, Findley,

and Diehl (2011), who find a relationship between rivalries and arms races. It does not necessarily contradict their findings, as they note that this relationship only exists in the later stages of rivalry. My findings on the effect of the likelihood of conflict provide a possible explanation for that conditionality. If the likelihood of conflict increases in the later stages of rivalry, but only then, controlling for the likelihood of conflict may make rivalry appear causally unimportant. Further untangling the role of rivalry on, and beyond, the likelihood of conflict is an avenue for future work.

My findings contribute to the alliance literature by demonstrating the substitution effect theorized by Morrow and others (Morrow 1993). This is in contrast to recent work on interdependence in military spending through allies, which finds the opposite (Flores 2011). I also show that alliances have multiple effects on military spending. Specifically, money spent by an ally decreases military spending, but increased threat to an ally increases military spending. Allies also experience strong interdependence in their disturbances: exogenous shocks in ally *j* are likely to have a significant effect on military spending in ally *i*. When forming an alliance, a state acquires a partner in security, which can lead to efficiencies in military spending, but the state also acquires some of the security concerns of their ally. Both affect the military spending of states. It may be that different types of alliances affect military spending in different ways. Understanding those differences could help us understand more about alliance dynamics, and is another worthy avenue of future work.

That military spending has foreign origins is not surprising. I show that military budgets affect each other spatially through two particular pathways, the likelihood of conflict and military alliances. These findings have important implications for grand theories of international security. Threats associated with the likelihood of conflict matter. It is not clear that other types of foreign threats do, including rivalry, outside of their impact on the risk of war. Alliances can create efficiencies in the provision of security, but they also increase the exposure of member states to the conflict risk endured by other members. Perhaps most importantly, examining these processes separately, as Table 1 does, yields different substantive results than does examining them together. International relations are dynamic spatial processes across multiple pathways of connection, and modeling them otherwise can lead to incorrect or misleading conclusions.

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Chapter 5: Conclusion

The three papers in this dissertation contribute to the literatures on military spending and arms races, alliances, enduring rivalries, and political institutions and foreign policy. Arming is one of the fundamental characteristics of states. Variation in military spending, despite extensive examination over several decades, was not explained well by the existing literature. Different types of states respond differently to their surroundings, as illustrated by paper 1. Different types of states face different domestic incentives to use military spending to buy support, as shown in paper 2. And states respond to some types of threats and not others, as found in paper 3. Alliances, another fundamental subject of the field of international security, are closely related to arming. They allow states to substitute ally spending for their own, but expose states to threats against their allies. In all three papers, political institutions, in a variety of forms, play a crucial role in explaining variation in military spending. The institutions of and around the state shape how much the state invests in its military.

These contributions come with a number of important caveats. The theoretical arguments are specific to military spending and particular independent variables. I do not conduct any analysis here on alternative measures of arming. It may be that the

insights above only apply to military spending itself, and not to arming or foreign policy in a broader sense.

The empirical analysis requires several caveats. In every analysis, other than paper 1's examination of conflict outcomes, the data is limited to the second half of the 20th century[26]. It is possible that the processes described in this dissertation function differently outside of that time period. The data also suffers from possible measurement error. Military spending numbers are frequently estimates, combining the released numbers from the states themselves with the opinions of experts. Governments may have an incentive to provide incorrect numbers (too high or too low), and outside experts make their judgments on limited information. My military spending data comes directly from Nordhaus et. al. 2012, who use a combination of Correlates of War and SIPRI data to construct their dataset. I am relying on their work to minimize the measurement error in the data. Similarly, in some models, particularly the models in Paper 1, I rely on measures of threat that may be endogenous to military spending. I again rely on Nordhuas et. al. (2012) for one such measure. They claim their measure of ex ante probability of conflict is not endogenous to military spending. In Paper 3, I

[26] This is due to some limitations of data availability, as GDP and military spending data prior to that time period is increasingly guesswork.

instrument for my own measure of the likelihood of war in some models. In all cases, I have avoiding using measures constructed with military spending, but cannot get around the risk that, in international relations at least, everything is related to everything else.

Some of the weaknesses described above can be, or have been, addressed through further modeling. I use both the natural log of military spending and military spending as a percentage of GDP, and have estimated most models using just data from the Correlates of War. I use instruments where possible to address possible endogeniety. Some of the caveats point to directions for future work. For example, paper 3 has numerous potential follow-ups. One possible approach would be to model the linkages between states as co-evolutionary with the state level variables themselves. That approach will require other assumptions, but directly addresses some concerns about connections between states that are endogenous to military spending. Other concerns can be partially dealt with by looking at small and medium N data. A close look at a small number of countries, particularly if they are clustered, may help alleviate concerns about data measurement. More nuanced data, such as arms imports, could be used in smaller N analysis as a complement to aggregate military spending data.

The findings in this dissertation suggest a number of important future investigations. An examination of conditional interdependence between military spending, one that combines the insights from papers 1 and 3, would help illuminate how states react differently to other states. That, in turn, would help us describe how changes in one state affect other states. It may be that the characteristics of leaders play an important role in military spending decisions. Their inclusion, and their interaction with institutions, may shed further light on when and how institutions shape policy, and introduces an additional level of analysis (the individual leader). Finally, the arguments in this dissertation may apply to phenomena beyond military spending. For example, democracies may adjust a variety of policies more in response to changes in their threat environment. The findings in this dissertation, both in their individual contributions and taken as a whole, point the way to future work on political institutions and military spending, both jointly and separately.