# Are Soldiers Less War-Prone than Statesmen?

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The dominant (though contested) wisdom among international relations scholars is that military officers tend to be more cautious than their civilian counterparts about initiating the use of force. Sobered by the experience of combat, the theory holds, soldiers are hesitant to recommend military action except under the most favorable of circumstances. It might be the case, however, that military conservatism is simply a product of strong civilian oversight. Indeed, scholars have suggested that military officers actually have powerful incentives to promote the use of force, but these predilections may be muted when civilian leaders can punish officers for botched military adventures. In this article, the author details a quantitative, competitive test of these propositions, showing that states with strong civilian control are on average less prone to initiate military action than states without it. The results suggest that civilian control should play a central role in future models of conflict initiation.

*Keywords:* civil-military relations; democratic peace; military conservatism; militarism theory; interstate disputes

**A**re civilian political leaders more prone to use force than military officers? A widespread view among scholars of military affairs holds that professional soldiers are conservative. Because they pay for wars with their own blood, the logic goes, military officers are reluctant to order troops into harm's way. In contrast, civilian politicians, having never experienced the horror of combat, are more likely to engage in military adventures.

This claim appeared repeatedly prior to the 2003 American offensive in Iraq, with opponents of the invasion arguing that the enthusiasm of some civilians for a military campaign reflected a lack of firsthand experience in combat. Remarks by Senator Chuck Hagel were emblematic of this viewpoint:

> Many of those who want to rush this country into war and think it would be so quick and easy don't know anything about war. They come at it from an intellectual perspective, versus having sat in jungles or foxholes and watched their friends get their heads blown off. (Quoted in Hirsh 2002, 25)

Likewise, retired Marine Corps General Anthony Zinni criticized civilians "who have never fired a shot and are hot to go to war" (quoted in Salinero 2002). An experienced soldier, he maintained, would be more cautious.

In political science scholarship, such assertions have been pervasive since Samuel Huntington published his classic treatise *The Soldier and the State* in 1957. Huntington documented military conservatism in the debate preceding U.S intervention in the Korean War, arguing that the Joint Chiefs of Staff under President Truman adopted a "cautious, conservative" attitude toward U.S. involvement (p. 382). Twenty years later, Richard Betts (1991) affirmed Huntington's claims in *Soldiers, Statesmen, and Cold War Crises*, concluding from detailed case analyses that U.S. military officials were typically more timid than their civilian counterparts about recommending military action.[1] More recently, scholars following in Betts's footsteps have drawn similar conclusions from studies of Great Britain, India, African military dictatorships, and the United States since the Vietnam War (Lee 1991; Andreski 1980; Bienen 1980; Petraeus 1989).

I argue that the conventional belief about "military conservatism" is overstated. Numerous studies have confirmed that military officers in the United States display a conservative skepticism about the use of force, but I suggest that it is inappropriate to use this observation to draw inferences about military behavior in general. I show that, on average, states lacking strong civilian control tend to initiate armed conflicts much more frequently than states whose militaries are under tight civilian reins. This finding suggests that the cautious nature of American military officers may be a consequence of strong civilian control and is probably not common to militaries in general. Prevalent assumptions about military preferences thus deserve some reconsideration.

From a scholar's perspective, theories of military conservatism and civilian control play a central role in the study of the causes of war—indeed, Stephen Van Evera's (1984) classic dissertation on the subject devoted no less than 280 pages to the question of military biases. If theories of military conservatism are to be believed, military advisers' checks on civilian leaders play an important role in the prevention of armed conflict. On the other hand, if conservatism is wrong and military leaders are, in fact, prone to favor aggression, then the absence of civilian control could constitute an important predictor of war.

Assessing the effects of civilian control may also aid scholars in locating the sources of the so-called democratic peace. The generally tranquil nature of relationships between democracies is widely recognized—"as close as anything we have to an empirical law in the study of international relations," according to one observer (Levy 1988, 662)—but the reasons for this statistical phenomenon are more contentious. Civilian control of the military is an essential pillar of democratic governance; it may be, then, that peace among democracies is really an effect of strong civilian control. This study thus contributes to the larger effort of testing "institutional" explanations for the liberal-peace thesis (Maoz and Russett 1993; Bueno de Mesquita et al. 1999) by isolating the effects of an important democratic institution.

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1. Betts (1991) maintained, however, that militaries tend to favor "tactical escalation"—that is, once war has begun, officers prefer large-scale, decisive operations (also Wilkenfeld, Brecher, and Moser 1988; Holsti 1998-1999). I do not test this hypothesis here.

For foreign policy practitioners, the implications of the debate about civilian control are equally significant. If one believes that civilian control lessens a state's propensity to initiate armed conflict, then it might strengthen the case for encouraging strong civilian control of the military in developing nations. For example, the belief that autonomous militaries are prone to launch preventive wars has led some to conclude that the West should help civilians rein in the armed forces of emerging nuclear nations. Scott Sagan (2003, 86) writes that encouraging civilian control of the military in states such as Pakistan is "the most important step the United States could take" to minimize the dangers of nuclear proliferation.

This study proceeds in seven parts. The first provides a brief overview of Huntington's (1957) theory of military conservatism. The second describes conservatism's chief theoretical competitor, known as the "militarism" hypothesis. The third explains my choice of research design. The fourth develops competing hypotheses about the effects of civilian control. The fifth describes the data used to test these hypotheses. The sixth presents empirical results in the form of logistic regressions and robustness tests. Finally, I conclude with theoretical implications of my findings.

## THE CONSERVATIVE MILITARY MIND

Huntington's (1957) view of military organizations originally arose in answer to a decades-old stereotype that suggested that military officers are reckless, hawkish, and war-prone. His theory of military conservatism has since achieved widespread acceptance among Western security studies scholars. The conservative theory of military behavior contains three important theoretical precepts, which I review below. First, it highlights military organizational interests that weigh against both the use of force and the adoption of destabilizing military postures. Second, it suggests that civilians' inexperience with armed combat increases the likelihood they will favor using force in crises. Finally, although the theory argues that officers' outlook on international affairs predisposes them to be pessimistic about the inevitability of war, it maintains that civilians are generally more supportive of preventive war options. On the whole, according to the theory, officers advocate military action "only as a last resort in crises, and only when victory is almost certain" (Huntington 1957, 69).

### PAROCHIAL INTERESTS

First, the theory of military conservatism holds that the organizational interests of militaries are not served by the use of force. Put simply, wars kill soldiers. Civilians may pay the financial cost of military interventions, but the military pays for them with the blood of its members. For advocates of conservatism, officers are thus reluctant to risk the lives of friends and colleagues for uncertain or superficial purposes: "The soldier, above all other people, prays for peace," according to General Douglas MacArthur, "for he must suffer and bear the deepest wounds and scars of war" (quoted in Petraeus 1989, 498).

Even high-ranking officers, although rarely killed in battle, have much to lose from military conflicts. An officer who commands victory on the battlefield may earn accolades and promotions, but defeat can mean professional disaster on an equal scale. Rather than risk a career that has taken years to build, the logic goes, the professional officer would rather chase success in the stable environment of peacetime. For this reason, Betts (1991, 202) writes, higher level military officers are cautious when considering military action:

> The irony is that the highest military officials, the Joint Chiefs of Staff, have a vested interest in caution and pessimism. They have already arrived at their career destinations. They do not have reputations to make so much as reputations to *protect*. They have nothing to gain by overoptimism or making promises that cannot be kept.

### CIVILIAN NAÏVETÉ

Second, the conservatism hypothesis contends that civilians are more optimistic than military leaders about the usefulness of military force, leading them to support more "interventionist" military policies (Gelpi and Feaver 2004, 5). Unguided by direct battle experience, the theory argues, civilians often underestimate the costs of military action and overestimate its utility. Ignorance of the technical capabilities and limitations of military combat undermines deterrence and unleashes politicians' ambitions for conquest (Van Evera 1999). This assumption is inherent in General Colin Powell's famous doctrine calling for military planners to be selective, decisive, and distrustful of optimistic projections when planning operations (Powell 1992).

### PESSIMISM AND PREVENTION

Third, advocates of conservatism theory suggest that military caution is acute, even in preventive war debates. Indeed, civilian strategists and leaders—most notably, Winston Churchill and George Kennan—were among the loudest of those calling for preventive war against the Soviet Union in the early cold war (Trachtenberg 1988-1989, 7-9). Moreover, in the 1960s it was President Kennedy's civilian strategists—not military leaders—who drove the administration's interest in keeping China nonnuclear through preventive war (Burr and Richelson 2000-2001, 69).

The central argument of the military conservatism thesis is thus that military leaders are not necessarily more likely to advocate the use of force than their civilian counterparts. In many cases, the theory argues, "the leading hawks have been civilian, not military, advisers" (Petraeus 1989, 491). Consequently, the implication of military conservatism is that civilian autonomy may increase—or at least not reduce—a state's propensity to initiate armed conflict.

> *Hypothesis 1:* As civilian control becomes weaker, states become less likely to initiate militarized disputes with other states.

## MILITARISM THEORY'S CRITIQUE

Scholars of an alternative "militarism" school believe the conservatism thesis is backward. Militarism theory suggests that military officers are, on the whole, more prone to favor policies that are more aggressive than those preferred by civilians. The militarism hypothesis holds that parochial motives, socialized values, and sheer familiarity with the instruments of armed coercion create conditions that may predispose military leaders in favor of using force. It implies that strong civilian control of the military can help avert unnecessary conflicts brought about by hawkish, self-interested military establishments (Brodie 1973).

The militarism hypothesis claims that military leaders share three general biases: first, parochial (or organizational) biases skew the objectives that military leaders pursue; second, perceptual biases influence the way officers interpret information about the world around them; and third, decision-making biases generate aggressive choices by altering the process by which military leaders make them. I discuss each alleged bias and its consequences below.

### PAROCHIAL BIASES

According to militarism theory, if left unchecked, military parochial interests can raise the risk of war in two ways. First, the organizational interests of military officers may lead them to recommend military action more often. Combat not only brings glory and excitement but affirms to the civilian establishment the necessity of high defense spending (Vagts 1937). Moreover, on the level of the individual officer, strong incentives exist for officers to obtain direct battlefield experience. Military leaders are not lionized for service behind a desk; rather, as a former U.S. Marine Corps Commandant wrote, "It takes a war to become a military hero" (Shoup 1969, 54).

Second, the theory holds that military interests favor offensive war-fighting doctrines, which increase the likelihood that force will be used in a crisis (Posen 1984; Snyder 1984). Offensive operations require vast arms expenditures, bolster the prestige of field commanders, and allow a military to fight on its own preplanned terms. The U.S. Army's operations manual declares that "victory requires shifting to the offense as soon as possible" (U.S. Department of the Army 2001)—an avowal eerily similar to that of German General August von Keim prior to the First World War, who declared, "The offensive is the only way of insuring victory" (quoted in Notestein and Stoll 1918, 43).

### PERCEPTUAL BIASES

Advocates of militarism theory claim that officers see the world through unique lenses that impact their choices about the use of force in specific, predictable ways. First, militarism asserts that military officers are socialized to envision national security as a strictly military problem. In this view, officers tend to undervalue economic and diplomatic aspects of international relations while fixating on the balance of military forces, thus exaggerating both external threats and the availability of opportuni-

ties to destroy them (Walt 1987, 159-60). Militaries overstate the advantages of striking first, generate optimistic casualty estimates, and miscalculate the time needed for victory.

For militarism theorists, military leaders also tend to overstate the probability of war. Conservatism agrees that officers view war as an inherent and unavoidable condition of mankind, but militarism theory goes a step further to argue that this pessimism prejudices military officers in favor of preventive war (Sagan 2003). If war is inevitable tomorrow, after all, then it is better to strike today while the enemy can be caught unprepared.

### DECISION-MAKING BIASES

A final contributor to military officers' aggressive proclivities, say militarism theorists, is the process by which militaries make choices about the use of force. This line of argument contends that military leaders are not trained to make careful, introspective decisions. Excessive deliberation on the battlefield can mean capture or death for a soldier; thus, simplicity and directness are the criteria on which officers are trained to base choices (Janowitz 1977).

Moreover, according to militarism theory, officers are accustomed to employing military solutions and are likely to draw from them even when such solutions are poorly suited to the problem at hand. Already seeing violence as "legitimate and effective," officers prefer policy instruments in whose use they have been trained instead of arcane diplomatic or economic options (Brecher 1996, 220). In contrast, the logic goes, "civilian leaders are less subject to organizational biases and have a more 'national' perspective on defense issues" (Desch 1999, 6).

Militarism theory thus boils down to a simple prediction: if left to their own devices, militaries are likely to behave in ways that increase the likelihood of interstate conflict. The desire for prestige, glory, and resources causes officers to advocate provocative offensive doctrines and high levels of military expenditure, whereas a proclivity for speed and finality in decision making privileges military solutions over diplomacy and negotiation.

> *Hypothesis 2:* As civilian control weakens, states become more likely to initiate militarized disputes with other states.

## RESEARCH DESIGN

The debate about military bias has received little scholarly attention since the publication of Betts's landmark study in 1977 (see Betts 1991), in part because conservatism has attained such pervasive acceptance as to leave the debate apparently resolved. But there are good reasons to think that it is not. Studies claiming to validate military conservatism have focused almost exclusively on countries with strong civilian control—predominantly the United States. Unfortunately, the U.S. case offers an incomplete picture of military preferences. It might be the case that tight civilian con-

trol in the United States suppresses military inclinations to recommend the use of force. We thus cannot conclude from these studies that militaries in general are conservative, even if the U.S. military appears to be.

## A STATISTICAL APPROACH

Gaining leverage on the question of the conflict proneness of militaries thus requires data from cases of both weak and strong civilian control. But what kind of data? There are at least three possibilities. First, one could collect survey data about the stated preferences of military officers and civilian elites (e.g., Feaver and Kohn 2001). Second, one could conduct "structured, focused comparisons" (George 1979) of military behavior under varying degrees of civilian control. A third option is to employ quantitative data on international conflicts and civilian control to determine which perspective on military behavior holds up under statistical scrutiny.

Although both of the first two options would conform to prevailing methods in the study of military behavior, I argue that a statistical approach is the better way to proceed with testing these theories, for three reasons. First, statistical analyses permit greater variation of independent variables, allowing researchers to find out if military proclivities change as the degree of civilian control changes. Because case studies and survey projects require immense labor and resources, researchers are typically unable to examine more than a few cases at a time. In the military bias literature, as I have noted, the consequence has been that studies have typically focused on countries in which the degree of civilian control did not change over time. Yet without varying the levels of civilian control among the cases in question, conclusions about military bias remain spurious. A statistical analysis, in contrast, allows us to expand the range of data under scrutiny and decide whether changes in civilian control have any effect on military biases. Employing statistical data collected from a large number of countries over multiple decades, as I do below, produces a less nuanced but more generalizable result.

Second, aggregate statistics permit isolation of the variables we wish to investigate while controlling for factors that might skew inferences. For instance, it could be the case that military officers tend to be aggressive in countries facing serious external threats but are conservative in states living in neighborhoods of relative calm. To confirm or deny this thesis, one would need to examine military behavior under a wide variety of threat conditions—while still ensuring variation in the level of civilian control. This is easily done with quantitative data, but case studies or surveys would impose an unreasonably large research burden on scholars who wish to control for multiple additional variables.

Third, a quantitative approach promises more precise conclusions than existing descriptive work. Description is, of course, an essential component of theory building, and statistical analyses do not help one understand the processes by which military decisions are made. But in the study of military bias, little theory-building work remains to be done. What lacks is research that tests the performance of these theories across a broad set of cases. I aim to build on existing qualitative research by asking not

only *whether* military behavior differs from that of civilians but *when* and by *how much*.

One weakness of the statistical approach is that often it cannot observe the variables of interest to the study, relying instead on proxies to support indirect measurements.[2] I make no claim that military biases can be quantified or directly observed. Ultimately, however, I am concerned with military biases only as they translate into actual behavior. Biases that have no manifestation outside the heads of decision makers contain little importance for policy. Using actual policy choices—which are more easily represented quantitatively—as a proxy for biases is thus defensible. After all, if biases have no effect on actual state behavior, the theoretical debate about them is probably unimportant.

### DEPENDENT VARIABLES AND UNIT OF ANALYSIS

I aim to estimate the probability that, in a randomly chosen year, a state facing a given set of international and domestic conditions will initiate a militarized dispute. I measure this probability using three dependent variables: first, the initiation of *any militarized dispute;* second, the initiation of *military force;* and third, the initiation of *militarized disputes entailing fatalities*. In all cases, I measure the initiation of armed conflict rather than mere participation to distinguish between states that start conflicts and those that are on the receiving end.

Because of the need to identify initiators, the unit of analysis in this study is the directed dyad-year. Like the standard dyadic format, each case consists of a pair of countries in a given year. In the directed-dyad format, however, each dyad-year is listed twice—once with each state designated the potential conflict initiator. With this design, I can identify initiators separately from defenders.

## CONTROL HYPOTHESES

One important advantage of using quantitative models is that such models can hold other things constant, thereby isolating the effect of the variables of interest to the study. What, then, must be held constant to evaluate the preceding hypotheses? This section identifies 16 commonly cited causes of militarized disputes. These hypotheses in turn inform my choice of control variables in the next section.

### NULL HYPOTHESIS

Although conservatism and militarism both predict a relationship between civilian control and conflict-proneness, one must consider the possibility that there are no systematic differences between military and civilian choices regarding the use of force. A

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2. Neither surveys nor case-study approaches, however, would solve this problem. Because surveys cannot simulate genuine decision-making conditions, survey responses are imperfect guides to the actual behavior of civilians and military officers. Case studies offer better representations of reality, but they too lack mind-reading powers and have only behavioral data from which to deduce their conclusions.

structural realist model, for example, would argue that international anarchy forces all statesmen—that is, civilians and military officers alike—to "think strategically" about their state's security needs and construct defense policy accordingly (Mearsheimer 1994-1995, 12). Realism would thus expect civilian control to have little systematic effect on the nature of military policy.

One need not be a strict structural realist, however, to hold this belief. A variety of scholars have challenged the assertion that there are predictable differences between the policy preferences of military and civilian officials (Zisk 1993; Scott 1994; Avant 1994). Some, such as Elizabeth Kier (1997), have argued that "military culture" is a better predictor of military preferences and choices. Others, such as Robert Jervis (1976, 26), suggest that issues of war and peace are so grave that the preferences of policy elites are untainted by their professional and bureaucratic roles. In any case, there is ample scholarship to defend the hypothesis that civilian control cannot help predict armed conflict:

> *Hypothesis 3:* Civilian control is unrelated to the probability that a state will initiate a militarized dispute against another state.

### REGIME TYPE

Whether a state is democratic or autocratic is a crucial control variable in a study of the effects of civilian control. Because civilian control of the military is an essential component of democracy, one would want to avoid misattributing the behavior of states to civilian control if regime type were really responsible. There are three hypotheses to consider: two at the state level and one at the dyad level.

First, it could be the case that democracies, by virtue of their domestic institutions, inherent values, or signaling capabilities, are more likely than other states to reach negotiated settlements that avoid armed conflict—regardless of the nature of the opponent (e.g., Ray 1995; Benoit 1996; Rousseau et al. 1996):

> *Hypothesis 4:* The democratic nature of a state's regime reduces the probability that it will initiate a militarized dispute against another state.

A corollary to this hypothesis is the possibility that highly autocratic states are more likely to initiate conflicts than either anocratic or democratic states:

> *Hypothesis 5:* The autocratic nature of a state's regime increases the probability that it will initiate a militarized dispute against another state.

Even more widely accepted, however, is the argument that democracies are unlikely to go to war with one another. Dubbed the "dyadic" democratic-peace hypothesis, this argument suggests that democratic norms of negotiation, trust, and compromise operate only when democracies deal with one another. Thus, by this logic, democratic dyads (but not necessarily democratic states as a whole) should be more pacific (e.g., Doyle 1986; Russett 1993):

*Hypothesis 6:* Dyads composed of two democratic states are less likely than other types of dyads to experience militarized disputes.

## REGIME TRANSITION

Mansfield and Snyder (1995, 2002) contend that regimes in transition are particularly prone to conflict. They argue that elites in partially democratized and autocratized states adopt aggressive and nationalistic foreign policy stances as a tactic for gaining political allies and maintaining popular support:

*Hypothesis 7:* New but incompletely democratic regimes are more likely to initiate militarized disputes with other states.
*Hypothesis 8:* New but incompletely autocratic regimes are more likely to initiate militarized disputes with other states.

## GEOPOLITICS

Of course, I must account for international conflicts that are driven by clashing interests rather than by individual state or dyad attributes. I do so in three ways. First, one might reasonably argue that states that share alliance ties are less prone to militarized disputes. If alliances are proxy indicators of common interests, then it would follow that dyads containing two allies will be less prone to conflict:

*Hypothesis 9:* Allies are less likely to initiate militarized disputes against one another.

Second, states facing external threats might be particularly likely to launch preemptive attacks against menacing adversaries. Specifically, states that have been targeted by other states in recent years are likely to believe that their neighborhood poses a high degree of threat:

*Hypothesis 10:* States that have been targeted by others in recent international conflicts are more likely to initiate military action.

Finally, the conflict-proneness of an interstate relationship itself might exert an influence on the probability of future disputes. The "temporal dependence" hypothesis suggests that states with a history of relative peace are unlikely to find their interests clashing in the future:

*Hypothesis 11:* Periods of peace between two states reduce the likelihood that one will initiate a militarized dispute against the other.

## POWER AND MATERIAL CAPABILITIES

Much recent scholarship on the effects of power balances concludes that parity in material capabilities exerts a destabilizing influence on state behavior (Bremer 1992).

This camp finds war to be more probable between states of relatively equal power and less likely when one state enjoys a preponderance over the other:

> *Hypothesis 12:* As a state's material advantage increases vis-à-vis a potential opponent, it becomes less likely to initiate militarized disputes against that opponent.

Nevertheless, some statistical results indicate that pairs of minor powers are somewhat less susceptible to militarized disputes than other types of dyads (Oneal and Russett 2001):

> *Hypothesis 13:* Minor powers are more likely to initiate militarized disputes against major powers than minor powers.

Finally, a variety of studies have suggested that major powers are more likely than minor powers to initiate armed conflicts. Major powers not only have greater interests abroad but are also more apt to possess the material capability to protect those interests (Gochman and Maoz 1984; Bremer 1993). Thus,

> *Hypothesis 14:* Major powers are more likely than minor powers to initiate militarized disputes.

### GEOGRAPHY

The vast majority of international conflicts occur between neighbors. Other things being equal, states far apart are unlikely to experience conflicts of interest or to possess the military force projection capabilities necessary for a long-distance fight. This observation generates two hypotheses related to the proximity of states within a dyad:

> *Hypothesis 15:* A shared land border increases the likelihood that two states will engage in a militarized dispute.
> *Hypothesis 16:* As the distance between two states increases, the probability that one will initiate a militarized dispute against the other declines.

### DOMESTIC STRIFE

Some research takes the position that domestic discord motivates leaders to launch military adventures abroad to divert public attention and shore up popular support for the regime (see Levy 1989). In particular, scholars have argued that nonviolent strife encourages diversionary wars because "diversion is not a viable strategy" (Gelpi 1997, 262) once strife has crossed the threshold of violence; instead, repression becomes necessary.

> *Hypothesis 17:* States experiencing high degrees of nonviolent domestic strife are more likely to initiate militarized disputes.

**TRADE DEPENDENCE**

A classical liberal argument holds that trade interdependence between states reduces the likelihood of conflict. Wars disrupt profitable financial flows, destroy resources, and offer few extractable economic gains for the conqueror. Moreover, the expectation of future gains from trade motivates states to resolve their disputes peacefully rather than through violent conflict (Copeland 1996; Mansfield and Pevehouse 2000). Thus, a high degree of trade interdependence may lessen the likelihood of military conflict:

> *Hypothesis 18:* States are less likely to initiate militarized disputes against states upon which they are highly dependent for trade.

## DATA AND OPERATIONALIZATION OF VARIABLES

The population of cases in this study covers 1,550 politically relevant dyads spanning the years 1947 to 1992, for a total of 73,472 directed dyad-year observations.[3] The dyads range in duration from 1 to 46 years, with an average duration of 23.7 years. Below, I describe the variables and the data used to test the preceding hypotheses.[4]

**DEPENDENT VARIABLES: INITIATION OF MILITARIZED DISPUTES**

Although interstate wars are an important component of armed conflicts, they do not constitute the full set of disputes that are of interest to this study. Political scientists typically use the threshold of 1,000 casualties to distinguish wars from minor conflicts, but theories relating to military bias make predictions about military conflicts in general, not just all-out wars. The Correlates of War project has created a broader data set of militarized interstate disputes (MIDs) that is not restricted by a minimum casualty number (Jones, Bremer, and Singer 1996; Ghosn and Palmer 2003). These data avoid the inherent arbitrariness of casualty thresholds and constitute a more legitimate test of the theories in question.

I created three dependent variables using these data. The first variable received a coding of 1 if State A initiated at least one new dispute against its dyad partner that year (thus excluding ongoing MIDs) and 0 otherwise.[5] But some of the disputes in these data set consist only of verbal threats or displays of force. Thus, I created two variables

3. A politically relevant dyad contains at least one major power or two states that share a land border (Lemke and Reed 2001).

4. Data for militarized interstate dispute (MID) initiation, regime type, distance and contiguity, trade dependence, alliances, peace years, major power status, and material capabilities were generated with EUGene (version 3.03), an immensely useful software tool that constructs data sets according to user specification (Bennett and Stam 2000a).

5. For convenience, the first state (and potential initiator) in the directed dyad is labeled State A, and its dyad partner is labeled State B.

for more serious disputes: one variable for actual uses of force[6] and another for MIDs involving fatalities. "Joiners"—that is, states that were not involved in a dispute on the day it began—were not counted as initiators in any of the three variables (Bennett and Stam 2000b).

## CIVILIAN CONTROL OF THE MILITARY

I obtained data on civilian control from Banks's (2002) "Cross National Time Series" data archive. These data define three levels of civilian control based on the bureaucratic roles of civilians and military officials.[7] For easier interpretation in the context of this study, I relabeled these categories *strong civilian*, *weak civilian*, and *military government*.

First, strong civilian regimes include "any government controlled by a nonmilitary component of the nation's population" (Banks 1976). Such cases are not limited to democratic states but may also include dictatorships, as long as civilians exercise full control over military decisions.

Second, *weak civilian* regimes are characterized by "an outwardly civilian government effectively controlled by a military elite" (Banks 1976). In these cases,

> Civilians hold only those posts (up to and including that of Chief of State) for which their services are deemed necessary for successful conduct of government operations. An example would be retention of the Emperor and selected civilian cabinet members during the period of Japanese military hegemony between 1932 and 1945. (Banks 1976)

Although figurehead civilian leaders make up a large share of the cases in this second category, some weak civilian regimes in the data set also include military heads of state. Examples include Iraq under Saddam Hussein, Pakistan under Mohammad Zia ul-Haq, and Taiwan under Chiang Kai-shek. Although the heads of state in these cases were officers in the armed forces, their states received a weak civilian coding because the military elite in general did not play a significant role in the management of state policy.

The civilian bureaucratic nature of weak civilian states contrasts with the third group of cases, military governments, which are characterized by "direct rule by the military, usually (but not necessarily) following a military coup d'etat" (Banks 1976). These cases are distinct in that civilians do not make up the main bureaucratic apparatus of the state:

> The governing structure may vary from utilization of the military chain of command under conditions of martial law to the institution of an ad hoc administrative hierarchy with at least an upper echelon staffed by military personnel. (Banks 1976)

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6. In the language of the MID data set, this means that State A's hostility level was coded as a 4 (use of force) or 5 (war). (See Ghosn and Palmer 2003.)

7. Banks (2002) also constructs a fourth category ("other"), which includes mostly theocratic regimes such as Vatican City and postrevolutionary Iran. I included these cases in the strong civilian category.

Examples of military governments in the data set include Peru under Alvarado and Bermudez (1968-1975 and 1975-1980), Argentina under various military dictators from 1966 to 1982, and Nigeria under Babangida (1985-1993).[8]

### DEMOCRACY AND AUTOCRACY

Consistent with most international relations research that employs regime type as a quantitative variable, I used the most recent version of the Polity III data set (Jaggers and Gurr 1995). In keeping with convention, I created a composite score by subtracting each country's autocracy score from its democracy score. I then employed a simple three-pronged classification system: countries from –10 to –4 were considered autocracies, countries from +4 to +10 were considered democracies, and those in between were catalogued as anocracies.[9] These categories were represented in the data set using dichotomous variables for democracies as well as for autocracies; anocracies were simply assigned a 0 for both variables.[10]

One risk of using democracy and autocracy variables is that if they correlate strongly with civilian control, they might introduce multicollinearity into the regression. But a quick diagnostic shows this not to be the case. Table 1 reports the distribution of democracies, anocracies, and autocracies across the three levels of civilian control. As might be expected, the majority of cases of weak civilian control are found in nondemocratic states. But democracies comprise only half of all cases of strong civilian control. In fact, in none of the nine cells is the correlation coefficient *r* greater than .3, indicating that multicollinearity is unlikely to be a problem.[11] Table 1 also shows that the civilian control variable is not just a proxy for Western industrialized democracies since nearly half the cases of strong civilian control occur in dictatorships.

*Democratization and autocratization*. Using the Polity III data described above, I created four variables to represent democratization and autocratization processes. Following the procedures of Mansfield and Snyder (2002), states were coded "in transition" if their trichotomous regime classification changed during the preceding five years. An autocracy that became an anocracy during this period was coded as an

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8. One other study has utilized Banks's (2002) civilian control data to assess the conflict behavior of military regimes (Lai 2003). It finds preliminary evidence to suggest that some military autocracies are more aggressive than democracies and nonmilitary dictatorships. This study, however, does not consider the behavior of weak civilian states (which need not be autocratic). This is an important limitation, because if weak civilian states are found to be appreciably less conflict-prone than military governments, doubt would be cast on the militarism hypothesis.

9. A number of Polity cases are coded as "an executive-guided transition period during which new institutions are planned, legally constituted, and put into effect" (Jaggers and Gurr 1995). For these cases, I obtained democracy and autocracy scores by averaging the scores from the years immediately before and after the transition period. Regimes in transition during their first or last years of existence, however, had no second score with which to calculate an average, so these observations were coded as missing data. Cases originally coded as periods of interregnum (indicating a collapse of central authority) or interruption (indicating foreign occupation) were recoded as missing data.

10. In the original data set, four cases were simultaneously coded as democratic regimes and military governments—by definition an impossible outcome—so some adjustment was required. These cases and their coding rationales are available from the author.

11. I address this issue in more depth below.

TABLE 1
Case Distribution and Correlation Coefficients,
by Civilian Control and Regime Type

|  | *Democracy* | *Anocracy* | *Autocracy* |
|---|---|---|---|
| Strong civilian | 31,642  $(r = .298)$ | 3,759  $(r = -.046)$ | 27,497  $(r = -.274)$ |
| Weak civilian | 143  $(r = -.246)$ | 476  $(r = .030)$ | 4,735  $(r = .230)$ |
| Military government | 0  $(r = -.156)$ | 233  $(r = .037)$ | 1,780  $(r = .138)$ |

incomplete democratic transition, whereas autocracies or anocracies that became democracies were counted as complete democratic transitions. Democracies that became anocracies were classified as undergoing incomplete autocratic transitions, whereas a change from a democracy or anocracy to an autocracy was coded as a complete autocratic transition.

**GEOPOLITICS**

*Alliances*. States with membership in a common defense pact, neutrality pact, or entente were coded as alliance partners. These data were drawn from the Correlates of War project (Gibler and Sarkees 2004).

*Threat environment*. The vast labor and imprecision associated with constructing a measurement of threat perception from descriptive sources necessitate a simple and objective gauge. I crafted such a gauge by constructing a variable that indicates whether a state was targeted by its dyad partner in a MID during the preceding 5 years—that is, if it participated in an MID that it did not initiate or join.

*Peace-years*. Using the Correlates of War data on MID participation, I created a continuous variable that measures the length of existing periods of peace (years without an MID) between dyad partners. If an MID occurred between the dyad partners in the previous year (whether new or ongoing), the variable was coded as 0. I also employed natural cubic splines to correct for the problem of temporal dependence (Beck, Katz, and Tucker 1998).

**POWER AND MATERIAL CAPABILITIES**

In addition to data about interstate conflicts, the Correlates of War data set on national material capabilities (Singer, Bremer, and Stuckey 1972) contains a composite measure of material resources to distinguish between major and minor powers.[12] I used this measure to generate three variables. First, I used the composite index to calculate the ratio of each state's capabilities to those of its dyad partner. Second, I created

12. These factors include military manpower, military expenditures, energy consumption, iron and steel production, and urban population.

a dummy variable that coded 1 for major powers and 0 otherwise. Third, I created a dummy variable for dyads containing two minor powers.

### GEOGRAPHY

There are two variables related to geographic proximity. One is a dummy variable for states sharing a contiguous land border, to account for the fact that most interstate conflicts in the data set (67.8%) occur between states that border each other. The second variable measures the distance, in miles, between the capital cities of the two states in the dyad (Stinnett et al. 2002), to test the hypothesis that the likelihood of military conflict between two states declines as the geographic distance between them grows. Contiguous states received a coding of 1 to reflect the geographic equivalence of contiguous dyads.

### DOMESTIC STRIFE

To create a measure for domestic unrest, I summed together the number of strikes, protests, demonstrations, and government crises experienced by a country during a single calendar year (from Banks 2002). To eliminate the possibility that incidents of domestic protest were caused by a state's decision to use force (rather than vice-versa), I lagged this variable by using the domestic strife composite from the previous year (Gelpi 1997).

### TRADE DEPENDENCE

The variable for trade dependence represents the proportion of State A's gross domestic product comprised by its trade with State B (Oneal and Russett 2001). Since the hypothesis about the effect of trade on MID initiation is concerned only with the initiator's dependence on the target, State B's dependence level is not included as a variable.

## RESULTS AND ANALYSIS

I assess the effects of civilian control with logistic regressions since the dependent variables represent binary outcomes. To estimate regression coefficients, I use CLARIFY, a software plug-in for Stata that minimizes the bias inherent in estimating rare events (King, Tomz, and Wittenberg 2000; King and Zeng 2001; Tomz, Wittenberg, and King 2003). Below, I discuss the implications of the results for militarism and conservatism theory and evaluate the quality of my models.

### EFFECTS OF CIVILIAN CONTROL ON DISPUTE INITIATION

Coefficients and standard errors for the logistic regression models are presented in Table 2. The militarism hypothesis receives strong confirmation. In all models in

TABLE 2
Logit Coefficients for the Initiation of Militarized Interstate Disputes, 1947-1992

| Independent Variable | All Disputes | | Uses of Force | | Fatal Disputes | |
|---|---|---|---|---|---|---|
| | Model 1A | Model 1B | Model 2A | Model 2B | Model 3A | Model 3B |
| Weak civilian | 0.5555*** | | 0.5998*** | | 0.5828** | |
| | (0.114) | | (0.128) | | (0.197) | |
| Military government | 0.5117** | | 0.5196** | | 0.1090 | |
| | (0.172) | | (0.194) | | (0.363) | |
| Democracy | 0.1889 | 0.0933 | –0.0372 | –0.1520 | 0.2405 | 0.1301 |
| | (0.205) | (0.202) | (0.245) | (0.240) | (0.458) | (0.443) |
| Autocracy | 0.1351 | 0.1600 | 0.2668 | 0.2877 | 0.5241 | 0.5441 |
| | (0.187) | (0.185) | (0.217) | (0.215) | (0.405) | (0.401) |
| Democratic dyad | –0.6127*** | –0.6368*** | –0.3034 | –0.3278 | –0.1301 | –0.1585 |
| | (0.157) | (0.157) | (0.195) | (0.194) | (0.323) | (0.322) |
| Complete democratic transition | 0.1358 | 0.1591 | –0.0047 | 0.0241 | 0.6550* | 0.6957* |
| | (0.189) | (0.189) | (0.248) | (0.248) | (0.324) | (0.325) |
| Complete autocratic transition | –0.0789 | 0.0807 | –0.1399 | 0.0275 | 0.3003 | 0.4109 |
| | (0.161) | (0.161) | (0.183) | (0.182) | (0.260) | (0.252) |
| Incomplete democratic transition | –0.1171 | –0.0965 | –0.0790 | –0.0672 | 0.2362 | 0.2314 |
| | (0.303) | (0.302) | (0.351) | (0.351) | (0.598) | (0.594) |
| Incomplete autocratic transition | 0.1604 | 0.1466 | 0.2341 | 0.2102 | 1.1720 | 1.1827 |
| | (0.519) | (0.519) | (0.610) | (0.614) | (0.786) | (0.790) |
| Nonviolent strife | 0.0370*** | 0.0387*** | 0.0436*** | 0.0457*** | 0.0678*** | 0.0689*** |
| | (0.007) | (0.006) | (0.008) | (0.008) | (0.013) | (0.013) |
| Capability ratio | –0.0046*** | –0.0046*** | –0.0046** | –0.0045** | –0.0038 | –0.0036 |
| | (0.001) | (0.001) | (0.002) | (0.002) | (0.004) | (0.004) |
| Trade dependence | 1.4543 | 1.2614 | 3.4107** | 3.2271* | –1.0571 | –1.1489 |
| | (1.402) | (1.404) | (1.248) | (1.245) | (3.888) | (3.924) |
| Contiguity | 0.7433*** | 0.7054** | 0.7955** | 0.7568** | 1.5230** | 1.4820** |
| | (0.212) | (0.211) | (0.260) | (0.258) | (0.571) | (0.568) |
| Distance | –0.0001** | –0.0001** | –0.0001* | –0.0001* | 0.0000 | 0.0000 |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| Allies | –0.1806 | –0.1376 | –0.3301** | –0.2832* | –0.5962** | –0.5667** |
| | (0.095) | (0.094) | (0.113) | (0.112) | (0.190) | (0.185) |
| Peace years | –0.3594*** | –0.3621*** | –0.3850*** | –0.3890*** | –0.4296*** | –0.4350*** |
| | (0.027) | (0.027) | (0.033) | (0.033) | (0.060) | (0.060) |
| Threat environment | 0.2346* | 0.2240* | 0.0157 | 0.0044 | 0.1029 | 0.0858 |
| | (0.096) | (0.096) | (0.113) | (0.113) | (0.177) | (0.178) |
| Major power | 0.5649*** | 0.4941** | 0.3297 | 0.2423 | –0.1677 | –0.2327 |
| | (0.150) | (0.148) | (0.187) | (0.185) | (0.382) | (0.383) |
| Minor power dyad | 0.3236 | 0.3845* | 0.3212 | 0.3861 | 0.5049 | 0.5700 |
| | (0.179) | (0.178) | (0.209) | (0.208) | (0.364) | (0.362) |
| Spline 1 | –0.0018*** | –0.0018*** | –0.0017*** | –0.0018*** | –0.0023** | –0.0024** |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.001) | (0.001) |

TABLE 2 (continued)

| Independent Variable | All Disputes | | Uses of Force | | Fatal Disputes | |
| --- | --- | --- | --- | --- | --- | --- |
| | Model 1A | Model 1B | Model 2A | Model 2B | Model 3A | Model 3B |
| Spline 2 | 0.0006*** | 0.0006*** | 0.0005* | 0.0005* | 0.0009 | 0.0009 |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| Spline 3 | 0.0000 | 0.0000 | 0.0001 | 0.0001 | 0.0000 | 0.0000 |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| Constant | −3.0824*** | −2.9771*** | −3.3407*** | −3.2185*** | −5.3740*** | −5.2665*** |
| | (0.257) | (0.254) | (0.310) | (0.307) | (0.569) | (0.559) |
| $\chi^2$ | 1,738.78*** | 1,708.21*** | 1,337.15*** | 1,303.94*** | 515.74*** | 506.21*** |
| Log-likelihood | −2,953.51 | −2,967.18 | −2,211.51 | −2,223.74 | −872.74 | −876.98 |
| Pseudo-$R^2$ | .24 | .24 | .25 | .24 | .29 | .28 |
| $n$ | 55,185 | 55,246 | 55,185 | 55,246 | 55,106 | 55,167 |

NOTE: Robust standard errors are in parentheses.
*$p < .05$. **$p < .01$. ***$p < .001$.

which it is present, the weak civilian variable is positive and highly statistically significant. The military government variable is also significant and positive when dispute initiation and the initiation of force are used as dependent variables (models 1A and 2A). These results indicate that states with weak civilian control are considerably more aggressive, holding other things constant.

One can therefore conclude that states with weak civilian control of the military tend to initiate military conflicts at higher rates. But how much more conflict-prone are they? Below I use the coefficients from the logit models to estimate the likelihood that a state under a chosen set of conditions will initiate a militarized dispute against a given dyad partner during a given year. By holding other factors constant and varying the level of civilian control, I can then isolate its effects.[13]

The impact of civilian control is illustrated in Figure 1, which charts the effects of civilian control and material capability ratios on the probability of MID initiation (model 1A). The solid and dashed lines represent, respectively, predicted probabilities for military and weak civilian governments, and the dashed-dotted line represents strong civilian states. This figure shows that military governments and strong civilian states have persistently higher probabilities of MID initiation. This is strong confirmation for the militarism hypothesis, which expects the strong civilian line to lie well below the other two. When capabilities are equal, for example, strong civilian states have a 18.8% probability of initiating a militarized dispute, but weak civilian governments have a 28.8% risk, holding other things constant. Moreover, capability ratio is significant and negative in models 1A, 1B, 2A, and 2B, supporting the hypothesis that power preponderance correlates negatively with dispute initiation (hypothesis 13).

13. In generating the predicted probabilities below, variables are held constant at the following values unless otherwise specified: democracy (0), autocracy (0), all regime transitions (0), nonviolent domestic strife (mean value), capability ratio (1.0), trade dependence (mean value), contiguity (1), distance (1), shared alliance (0), peace years (0), threat environment (1), major power (1), minor power dyad (0), and splines (0).
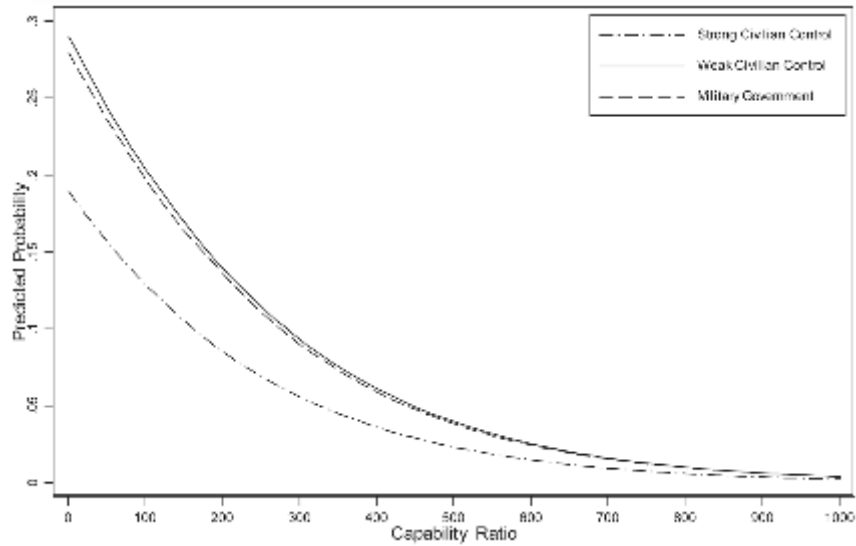
**Figure 1:    Variation in the Probability of Dispute Initiation with Capability Ratio**

My results also provide strong confirmation for diversionary theories of war. The data reveal that recent episodes of domestic strife sharply increase a state's propensity to initiate disputes of all types (hypothesis 17). The effects of unrest are illustrated in Figure 2, which considers uses of force (model 2A). As incidents of strife swell from 0 to their observed maximum of 60, a state's likelihood of initiating military force jumps by more than 50 percentage points (10.3% to 61.1%) for states with strong civilian control and nearly 60 percentage points (17.3% to 74.1%) when civilian control is weak.

**ROBUSTNESS OF THE RESULTS**

The militarism hypothesis holds up well under escalating degrees of dispute severity. When the dependent variable is the use of force (model 2A) rather than simple dispute initiation, civilian control remains significant in both statistical and substantive respects. Changing from strong to weak civilian control amplifies a state's conflict propensity to 153% of its prior value in model 1A, 167% in model 2A, and 176% under the "fatal dispute initiation" model (model 3A).

The lone anomaly for militarism theory is the military government coefficient for fatal disputes (model 3A), which does not achieve statistical significance. This result does not give much help to conservatism theory since weak civilian states are still nearly 1.8 times as likely to initiate fatal disputes as strong civilian states, but it does lend some support to the null hypothesis (hypothesis 3). The insignificance of the military government variable, however, is highly sensitive to the rarity of fatal disputes in
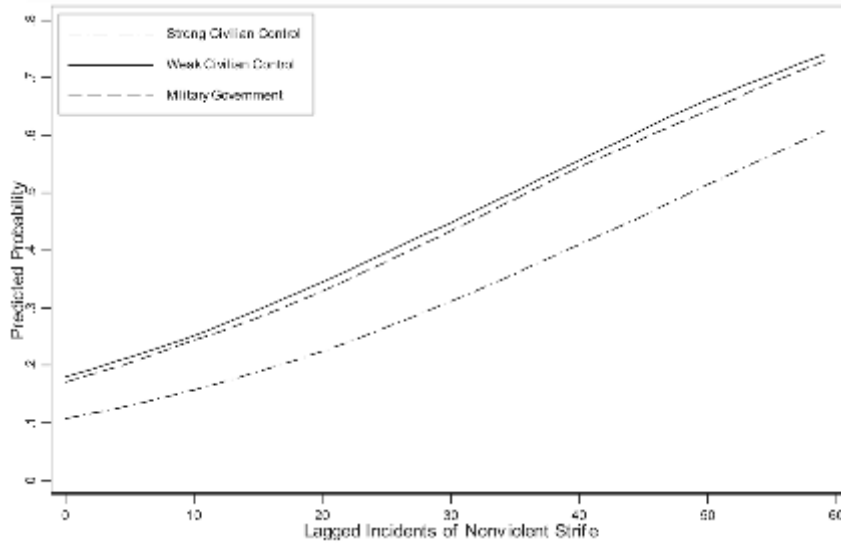
**Figure 2:** **Variation in the Probability of Initiation of Force with Domestic Strife**

general: if military governments had initiated only six more fatal disputes between 1947 and 1992 (0.3% of total opportunities for that type), this variable would be significant at the 95% level.

**PREDICTIVE QUALITY OF THE MODEL**

My analysis shows that civilian control of the military has a significant impact on the likelihood of military conflict. This is important for theory, but it is not immediately clear how well my models predict actual cases of conflict initiation. Below, I gain leverage on the question of predictive accuracy by comparing propensity scores in model 1A with observed outcomes. First, I sorted the cases by propensity score, in order from lowest to highest. Second, I separated the scores into clusters along 2.5% intervals—that is, cases with scores between 0% and 2.5% were grouped together, scores between 2.5% and 5% were grouped together, and so on.[14] Third, I calculated each cluster's average propensity score, which I used to represent its predicted proportion of positive outcomes (MIDs). For instance, if the average score of a particular group is, say, 10%, then approximately 10% of the cases in that group should contain actual MID initiations if the model is good. Figure 3 plots model 1A's predicted pro-

14. The exception is the last cluster, which contains all scores greater than or equal to 27.5%. This expanded interval was necessary to ensure that the cluster was large enough to make comparisons with other clusters meaningful. Nevertheless, this cluster still contains only 16 cases, compared with 48,916 in the first cluster.
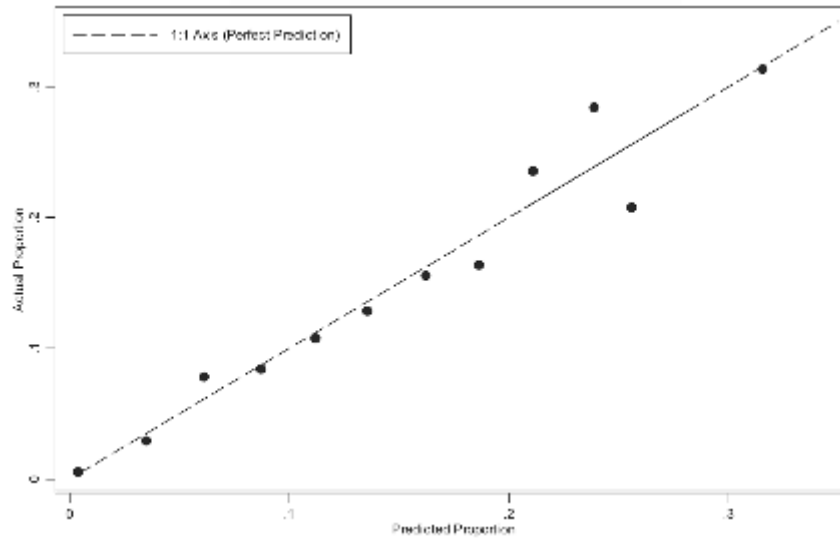
**Figure 3:    Predictive Quality of Model 1A**
NOTE: Proximity to the 45-degree line indicates high predictive quality.

portions against the actual proportion of MID initiations in each cluster. The overlaid 1:1 axis represents perfect predictive accuracy.

How well do the model's predictions fit the data? There are a number of ways to answer this question. One technique compares the total number of MIDs "expected" by the model to the total number actually observed in the data. The expected figure is found by first multiplying each cluster's average propensity score by the number of total cases in that cluster and then summing the totals. Doing this, I find that model 1A predicts 735 MIDs, matching the data perfectly.

A better technique would measure how well the plotted points in Figure 3 fit around the "ideal" 1:1 line. The coefficient of determination, or $R^2$, is conventionally used to evaluate a regression line's fit to surrounding data points, so I can employ it here even though Figure 3 is not technically a regression. The $R^2$ of the plot in Figure 3 comes out to be .984, signifying a near ideal relationship between the predicted and actual proportions of MIDs within the clusters (an $R^2$ equal to 1 would indicate a perfect fit).[15]

The $R^2$ method is potentially misleading, however, because one or two incorrect predictions toward the right-hand side (where the clusters contain fewer cases) could exert an inordinate influence on the proportion of errors in that cluster and, by exten-

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15. A bivariate regression of actual probabilities against expected probabilities yields a slope of .985 with a standard error of .077—almost a perfect correspondence between predictions and actual observations.

16. The calibration index (CI) is calculated as follows: $CI = \dfrac{\sum_j N_j (f_j - x_j)^2}{\sum_j N_j}$, where $j$ indexes the 12 clusters, $N_j$ refers to the number of observations in each cluster, $f_j$ represents the predicted proportion of MIDs in cluster $j$, and $x_j$ is cluster $j$'s actual proportion of MIDs.

sion, the overall quality of fit represented by $R^2$. Yates (1990) suggests correcting for this problem by using a calibration index (CI).[16] This term should ideally be close to zero if the model offers good predictions. Indeed, for model 1A, the CI is very close to zero (CI = $1.77 \times 10^{-5}$).[17]

## CONTRIBUTION OF THE CIVILIAN CONTROL VARIABLE

Do these models of conflict initiation improve on models that do not account for civilian control? This question is important for assessing the value of civilian control as a practical research tool. For reasons of practicality and efficiency, researchers would like to exclude unimportant independent variables from their models whenever possible. If civilian control variables add predictive power to models of conflict initiation, then it might be worthwhile to retain them in later studies. If not, however, then one could safely exclude them even though their coefficients might be statistically significant.

To determine the marginal contribution of civilian control, I estimated a logistic regression model (model 1B) containing all the variables from the first model except those for civilian control. Using these new coefficients (also reported in Table 2) to generate new propensity scores, I again formed clusters along 2.5% intervals and compared the fit of the predictions in this truncated model to those of the full model (model 1A). The $R^2$ of the resulting plot is .958—still a good fit but below that obtained from the full model. Moreover, the CI generated by model 1A turns out to be about 21% better than model 1B's index, which comes out to $2.15 \times 10^{-5}$.

These figures are more meaningful when compared to the marginal value of other variables in model 1A. I estimated a series of truncated models that omitted exactly one other statistically significant variable and used the new regression results (not reported here) to construct plots equivalent to Figure 3. The results are presented in Table 3. The first line in the table contains the $R^2$ and CI terms for the original plot in Figure 3 (using model 1A), and the lines below it report the same terms for the truncated models. Only the peace years variable had more of an impact on the predictive quality chart's $R^2$ than civilian control. Although four variables improved the CI more than civilian control, an equal number of variables did not. For example, when I left the democratic dyad variable out of model 1A and used the results to re-create Figure 3, the CI of this new plot actually decreased by $.08 \times 10^{-5}$, and the $R^2$ of this new plot improved slightly as well. These results suggest that civilian control bolsters predictive quality more than many important variables and should be considered for inclusion in future models of conflict initiation.

17. My clustering procedure differs slightly from Stam (1996) and Scheve and Tomz (1999), who divide their prediction scores into 10 clusters of equal case numbers (rather than equal risk intervals, which I use). But model 1A actually performs better under this alternative method. Using 10 clusters of equal case size, the model predicts 735 MIDs, generates an $R^2$ of .9989 (closer to 1), and yields a calibration index of .09 $\times 10^{-5}$ (closer to 0).

TABLE 3
Effect of Variable Omission on Predictive Quality Chart of Model 1A

| Omitted Variable | Resulting R[2] Term[a] | Change | Resulting Calibration Index (CI) $(\times 10^{-5})$[b] | Change $(\times 10^{-5})$ |
|---|---|---|---|---|
| None (model 1A) | .984 | — | 1.77 | — |
| Peace years | .740 | –.244 | 3.24 | 1.47 |
| Civilian control (model 1B) | .958 | –.026 | 2.15 | 0.38 |
| Nonviolent strife | .966 | –.018 | 3.42 | 1.65 |
| Threat environment | .967 | –.017 | 2.97 | 1.20 |
| Major power | .976 | –.008 | 1.69 | –0.08 |
| Contiguity | .981 | –.003 | 2.21 | 0.44 |
| Distance | .986 | .002 | 1.77 | 0.00 |
| Capability ratio | .991 | .007 | 1.07 | –0.70 |
| Democratic dyad | .992 | .008 | 1.69 | –0.08 |

a. Larger values signify better predictive quality.
b. Smaller values signify better predictive quality.

## IMPLICATIONS AND CONCLUSIONS

At the outset of this article, I noted that isolating the effects of civilian control might help shed light on theories about regime type and international conflict. I conclude here by returning to this claim. In general, my data offer little support for these theories. Neither democratic peace theory nor theories of regime transition and war receive much support from the analysis above.

### DEMOCRATIC PEACE

My results offer strong evidence against the monadic version of democratic peace theory (hypothesis 4). Most important, the monadic democracy and autocracy variables are insignificant in all six regressions, implying that one cannot confidently reject the possibility that regime type has no effect on conflict proneness. Even if they were significant, however, Table 4 shows that the substantive effects of these variables do not correspond to theoretical expectations. This table reports, at each level of civilian control and dispute escalation, initiation propensities for four types of regimes: democracies with democratic dyad partners, democracies with nondemocratic dyad partners, anocracies, and autocracies. These figures show that when facing nondemocratic opponents, democracies are in fact more dispute-prone in general than both autocracies and anocracies and more prone to initiate fatal disputes than anocracies, regardless of the level of civilian control.[18]

A skeptical observer might suspect that a correlation between democracy and civilian control might be responsible for the weak performance of the monadic regime

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18. My analysis challenges prior scholarship that asserts that anocracies are uniquely prone to initiate conflict (Maoz and Abdolalai 1989). At none of the three escalation levels are anocracies more conflict-prone than autocracies, and in two of the models they are less dispute-prone than democracies with nondemocratic dyad partners.

TABLE 4
Predicted Probabilities of Conflict Initiation (in percentages)

| | *Democracy (Democratic Partner)* | *Democracy (Nondemocratic Partner)* | *Anocracy* | *Autocracy* |
|---|---|---|---|---|
| Dispute initiation (model 1A) | | | | |
|    Strong civilian control | 13.2 | 21.9 | 18.8 | 21.0 |
|    Weak civilian control | 20.9 | 32.8 | 28.8 | 31.6 |
|    Military government | — | — | 27.9 | 30.7 |
| Initiation of force (model 2A) | | | | |
|    Strong civilian control | 8.1 | 10.7 | 11.0 | 13.9 |
|    Weak civilian control | 13.8 | 17.9 | 18.4 | 22.8 |
|    Military government | — | — | 17.3 | 21.4 |
| Initiation of fatal dispute (model 3A) | | | | |
|    Strong civilian control | 2.4 | 2.7 | 2.2 | 3.6 |
|    Weak civilian control | 4.2 | 4.8 | 3.8 | 6.3 |
|    Military government | — | — | 2.4 | 4.0 |

indicators. The truncated models (models 1B, 2B, and 3B) offer an opportunity to evaluate this claim. If multicollinearity were to blame for the insignificance of the democracy and autocracy variables, one would expect to see these variables become significant at the 95% level or above when the civilian control variables are omitted. But this is not the case. Even in the three truncated models, one cannot reject the null hypothesis that democracy and autocracy have zero effect on the likelihood of conflict initiation. These observations and low correlation coefficients in Table 1 offer reassurance that multicollinearity is not distorting the results.

The dyadic form of the theory (hypothesis 6) receives only partial support. On one hand, the variable for democratic dyads in model 1A is both highly significant and negative, as the theory expects. Table 4 confirms this observation, showing that democracies with democratic dyad partners are substantially less likely than other states to initiate militarized disputes at every level of escalation: when a democracy's partner is changed from a nondemocracy to a democracy, the probability of MID initiation drops by more than one-third.

On the other hand, however, the democratic dyad variable does not survive statistical significance tests at higher levels of dispute escalation. When the dependent variable is the initiation of force (models 2A and 2B) or the initiation of fatal disputes (models 3A and 3B), the dyadic variable is not significant at the 95% level. The marginal effects of democratic dyads also diminish in these models: when the dyad partner of a strong civilian democracy is changed from a nondemocracy to a democracy, its likelihood of initiating force drops by only 2.6%. If the dependent variable is the initiation of a fatal dispute, the drop in probability is less than half a percentage point. At these dispute levels, the effect of shared democracy is smaller in magnitude than the effects of changes in civilian control.

It is worth noting that these results do not support the view that civilian control is the engine behind the democratic peace, for two reasons. First, civilian control is not an

interactive variable—there is no theory that predicts an independent pacifying effect for partners sharing strong systems of civilian oversight. If anything, one might expect civilian control to drain the monadic democracy variable of its explanatory power, but as described above, this variable is insignificant even without including civilian control in the model.

Second, if the dyadic democratic peace owed its existence to civilian control, then the coefficient for democratic dyads should be more significant in model 1B—where civilian control is omitted—than in model 1A, where civilian control is present. But this is not the case. The coefficient for the democratic dyad variable is nearly the same in both regressions, with identical standard errors. Moreover, the substantive effect of democratic dyads is similar regardless of whether civilian control is included: in model 1A, when the dyad partner of a civilian-controlled democracy is made democratic, MID initiations become 8.7% less likely, whereas in model 1B, when a democracy's dyad partner becomes democratic, the probability of MID initiation by drops by 8.4%. This indicates that the effect of dyadic democracy is probably not driven by civilian control.

### DEMOCRATIZATION AND AUTOCRATIZATION

Equally interesting are the implications of my analysis for theories about regime transition and conflict. It is notable that only one of the four variables for democratization and autocratization is statistically significant in any of the models. In addition, the direction of the incomplete democratization variable—the only significant transition variable in Mansfield and Snyder's (2002, 538) analysis of MID participation—is negative in four of the six models, contrary to hypothesis 7. There are two possible reasons for this. One reason might be that the absence of civilian control in partially democratized states explains their conflict-proneness, so when that absence is accounted for, the destabilizing effects of partial democratization disappear. This is unlikely, however, given that the transition variables are not significant in two of the truncated models (models 1B and 2B). More likely is that the two studies measure different phenomena: unlike the research design presented here, Mansfield and Snyder's study accounts for conflict participation, not initiation. The combination of their result—that incompletely democratized states participate in many conflicts—with my finding—that such states tend not to initiate disputes—suggests that partial democracies may be attractive targets rather than aggressive initiators.

### CONCLUSION

I offer the first quantitative, cross-national evidence in support of the argument that military officers are more likely than civilians to favor the use of force. I find that militarism theory fares well in the data analysis above, but the general form of conservatism theory does not. On average, when military officers have the authority to initiate militarized conflicts, they tend to do so at rates substantially higher than civilians. I also find no substantive behavioral difference between military governments and weak civilian states, indicating that the aggressive biases of military governments are

not subdued by the responsibilities of state management. This evidence points toward a conclusion that, compared to civilians, officers on the whole are more prone to initiate military action.

Why, then, have so many scholars come to opposite conclusions? The likely answer is that most prior studies of military bias have not varied the level of civilian control in their subjects. The above results lead me to suspect that these studies would have found military officers to be more vocal in advocating the use of force when civilian oversight was weak. Indeed, my data show that militaries behave in ways contrary to the observations of Betts (1991), Huntington (1957), and others when they are released from the constraints of civilian control.

My conclusions should not be misinterpreted to mean that military officers in the United States are more aggressive than their civilian counterparts. There is overwhelming evidence elsewhere to suggest that U.S. officers are at least as cautious as civilians about recommending military action and perhaps even more so. I do not dispute this evidence. Rather, I suggest that cautious and conservative militaries are more likely to be found in states with strong civilian control. When civilian authority disappears, some of this caution disappears along with it. The observation that top U.S. officers tend to be wary of military action is therefore entirely consistent with the findings of this study.

I thus consider and reject the idea that conservatism theory is applicable beyond countries with strong civilian control. Instead, the analysis here suggests that civilian control of the military is not merely a means to promote democracy, but it is also a force in favor of peace. This result contains importance not only for academic theoreticians seeking to explain war but for policy practitioners seeking to prevent it.

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