Civil-military Pathologies and Defeat in War: Tests Using New Data

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Abstract

This article uses an original data set, the Wartime Civil-military Relations Data Set, to test arguments about the causes of victory and defeat in war. Our analysis provides strong initial support for the notion that civil-military relations powerfully shape state prospects for victory and defeat. Specifically, states whose militaries have a significant internal role or whose regimes engage in coup-proofing appear to have a substantially lower probability of winning interstate wars, even when we account for the role of other important variables, including regime type and material capabilities. Crucially, our measures of civil-military relations include coup incidence but also move beyond it to detect more subtle indicators of civil-military relations. The resulting analysis should give us confidence in acknowledging the importance of nonmaterial variables in explaining war outcomes, while also paving the way for further research that can utilize and extend the data set.

Keywords

war outcomes, war, military power, interstate conflict, international security, internal-armed conflict, domestic politics

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War is the ultimate contest in international relations, responsible at times for reshaping the entire international system. Yet our understanding of why countries win or lose these contests still contains significant shortcomings. Past research has heavily emphasized the role of material factors in war outcomes, perhaps taking its cue from Napoleon's rumored remark that "God favors the big battalions." This work has focused on variables such as wealth and economic development in explaining military performance (Lake 1992; Desch 2008; Beckley 2010).

A second wave of literature has questioned this materially focused approach (Brooks and Stanley 2007). In an effort to explain why even countries with material advantages sometimes lose wars and even countries with material disadvantages sometimes win, this literature points to the importance of factors such as regime type and political institutions (Reiter and Stam 2002; Avant 2007), societal structure and identity (Castillo 2014; Rosen 1996; Lyall 2015), military organizational culture (Kier 1999; Long 2016) and capacity (Horowitz 2010), global norms (Farrell 2007), and civil-military relations (Brooks 1998, 2006; Quinlivan 1999; Biddle and Zirkle 1996; Talmadge 2013, 2015; Nielsen 2005; McMahon and Slantchev 2015). Notably, this emphasis on civil-military relations—that is, on the relationships connecting political leaders, military officers, and society in a given regime—is consistent with a long-standing body of qualitative work suggesting that such factors are broadly important for military performance (Huntington 1957; Janowitz 1960). Yet efforts to probe the generalizability of the findings in this qualitative literature have foundered on a lack of good cross-national measures of civil-military relations that can be used to explore the concepts of interest quantitatively.

For example, one of the most commonly used measures, recent coup incidence (Biddle and Long 2004; Grauer and Horowitz 2012), has been problematic because the absence of coups in a given country can result from diametrically opposed civil-military relations. Consider the fact that neither the United States nor Iraq had experienced recent coups when the two states fought each other in 1991 and thus are coded as equivalent under this approach. The two states' civil-military relations, however, were starkly different, and in ways that had critical implications for military performance (Biddle and Zirkle 1996). The United States had not experienced a recent coup because it has a strong democratic norm against military intervention in politics, whereas Iraq had not experienced a recent coup because Saddam Hussein systematically purged his army of anyone who might have been capable of unseating him and packed the officer corps with men chosen for their loyalty rather than their competence. In short, past coups are not irrelevant to civil-military relations (Powell and Thyne 2011), but the context in which a country has become coup-free is critical to accurately assessing the likely implications for military performance.

Indeed, even scholars who employ this measure have noted a wide range of consequential civil-military patterns besides coup incidence (Biddle and Long 2004, 533, fn 10) and called for more refined measures of civil-military relations that can capture this important variation (Grauer and Horowitz 2012, 109, fn 77). Recent scholarship offers useful progress in this direction (Weeks 2008, 2012, 2014;

Geddes 2003; Belkin and Schofer 2003; Pilster and Bohmelt 2011, 2012; Powell 2012; De Bruin 2016; Miller and Elgun 2011) but still has some shortcomings in producing quantitative analysis that directly captures the civil-military distinctions consistently found to be the most important in the qualitative literature (Downes 2016).

In this article, we seek to remedy these deficits by using a new original data set, the Wartime Civil-military Relations Data Set, to test arguments about the causes of victory and defeat in war. Consistent with the existing qualitative literature, we argue that civil-military relations characterized by an internal role for the military or coup fears on the part of the regime should lower states' prospects of winning interstate wars. Crucially, however, we probe these claims with new data that code the civil-military relations of all states that have fought interstate wars starting with World War II (WWII). Our indicators consist of thirteen civil-military characteristics that we draw largely from qualitative literature on civil-military relations, not from the existing data sets. Although not without its limits, our analysis provides strong initial support for the notion that civil-military relations, when coded properly, powerfully shape state prospects for victory and defeat in war across a broad range of circumstances. States whose militaries have a significant internal role or whose regimes engage in coup-proofing appear to have a substantially lower probability of winning interstate wars, even when we account for other crucial variables traditionally associated with war outcomes.

Importantly, as we explain below, our novel data enable us to make this claim with a higher degree of internal validity than often has been achieved in previous quantitative studies of civil-military relations, and a higher degree of external validity than is sometimes apparent in qualitative studies. We also disaggregate the broad concept of civil-military relations into key components and dimensions that could separately or in various combinations help explain war outcomes. The subsequent analysis should give scholars confidence in acknowledging the importance of non-material variables in explaining war outcomes while also paving the way for further research using the new data.

Our article proceeds in five parts. First, we review the existing work on the connection between civil-military relations and state performance in war, noting both the contributions and limitations of this literature. Second, we present our argument, generate testable hypotheses from it, and discuss our approach to gathering original data to test these hypotheses. Third, we present the Wartime Civil-military Relations Data Set in detail and discuss our methodology. Fourth, we conduct regression analysis highlighting the novel applications of the new data and discuss the results. The article's final section concludes with an assessment of the broader implications for research and policy.

The Contributions and Limitations of Existing Approaches

While not ignoring the importance of material variables such as power, scholars and practitioners of international relations have long posited a general connection between states' civil-military relations and their performance in war. Thucydides

(1982), for example, paid close attention to the differing civil-military relations of Athens and Sparta, noting the starkly different relationships each military had to its political leadership and society, and evaluating the implications for the type and degree of fighting power that each regime was able to generate.

Sun Tzu (1971) similarly noted that victory and defeat hinged at least in part on what today we would think of as civil-military relations. As he explained in his emphasis on the importance of military autonomy from politics, "He whose generals are able and not interfered with by the sovereign will be victorious" (83). Centuries later, Clausewitz (1984) also discussed the relationship between political leaders and generals, though he took a rather different view, arguing that military action was inherently political and that political involvement in military affairs was both inevitable and essential. "War is only a branch of political activity," he explained. "It is in no sense autonomous" (605).

Modern observers have continued this debate about how best to structure the relationships among political leaders, military officers, and society in order to ensure victory in war. Huntington's classic *The Soldier and the State* (1957) emphasized the value of what he called "objective control" of the military: keeping the military separated from politics and endowing it with substantial autonomy over operational and tactical matters in order to encourage professionalism. He argued that such professionalism would lead both to healthy civil-military relations as well as to robust military preparation for warfighting. Huntington contrasted this model of civil-military relations with what he called "subjective control" of the military, that is, civilians controlling the military by politicizing it and becoming closely involved in areas best left to the discretion of military officers. In his view, subjective control of the military stunted the development of professionalism, inviting military intervention into politics and distracting the military from its core task of warfighting.

Subsequent scholarship has questioned Huntington's case for military autonomy from civilian leadership. For example, Posen (1984) has shown that civilian intervention in the military can be critical to aligning military doctrine with broader grand strategic goals. Cohen (2002) has demonstrated the value of active civilian involvement in military decision-making during war, even in tactical and operational matters. Avant (1994) has argued that properly assertive institutions of civilian oversight are crucial to the conduct of effective military operations. Feaver (2003), too, has shown the importance of what might be considered intrusive forms of civilian monitoring, demonstrating that they are critical to ensuring that civilians obtain both "protection *by* the military and . . . protection *from* the military"—that is, a military that can successfully defeat external adversaries while still remaining subordinate to political authority at home (6), or overcome what McMahon and Slantchev (2015) call the "guardianship dilemma."

Other work has shown that some forms of civilian intervention in the military do not adequately resolve this dilemma. In regimes concerned about coups, for example, political intervention in the military can reach problematic extremes, preventing the military from performing effectively in war and potentially leading to the state's

defeat on the battlefield. In an influential qualitative study, Quinlivan (1999) termed the forms of political intervention adopted by such regimes "coup-proofing" and argued in his analysis of Syria, Iraq, and Saudi Arabia that such measures reduce the generation of military power. Coup-proofing, in his account, involves "reliance on groups with special loyalties to the regime and the creation of parallel military organizations and multiple internal security agencies" (131). Although they did not use the term "coup-proofing," Brooks (1998, 2006) and Biddle and Zirkle (1996) similarly used qualitative research to show that the measures Arab regimes used to protect themselves from coups harmed military performance: purges and executions in the officer corps, the politicization of the officer promotion process, political surveillance of the officer corps, frequent rotations of officers, development of overlapping lines of command, and politicization of training, among others. Their work is consistent with other work on Arab armies by Pollack (1996).

In more recent research, Talmadge (2013, 2015) has argued that regimes concerned about internal threats, especially coups, adopt practices related to promotions, training, command structure, and information management that reduce the internal threat posed by the military but also severely hamper the state's ability to generate conventional military power. These measures include selection against competence in the officer corps, restrictions on training, the development of divided and heavily centralized chains of command, and deliberate restrictions on both horizontal and vertical information sharing in the military. Talmadge examined the causes and consequences of these practices in case studies of Iran, Iraq, and South Vietnam.

The strength of these qualitative studies of coup-proofing, which generally rely on some combination of interviews, archival research, and secondary sources, is the high degree of fidelity they provide regarding knowledge of the causal mechanisms at work in particular cases, especially over time. When properly done, case studies enable the examination of a wide range of heterogeneous, contextually appropriate, case-specific evidence (George and Bennett 2004). Nevertheless, the heavy focus in the qualitative coup-proofing literature on individual countries raises questions about the external validity of the arguments. Is coup-proofing a widespread phenomenon, and, if so, does it have similar effects on military performance wherever it is adopted?

A second group of studies has shed light on this question through quantitative analysis of a wider range of states. Results from these studies have been mixed, however, with some finding civil-military relations to be statistically significant (Biddle and Long 2004) and others not (Grauer and Horowitz 2012). These divergent findings may stem in part from the way these studies have measured civil-military relations. Their approach measures recent coups, drawing on data from the Arthur Banks data set (Banks 2005) that codes states according to how many coups they have had in the last five years. To be sure, frequent coups are an indicator of problematic civil-military relations, as the Pakistani experience among others suggests (Cohen 2004). Military takeovers are without question still prevalent and highly consequential for civil-military relations, military performance, and many other

outcomes of interest (Svolik 2009; Miller 2012; Powell and Thyne 2011; Piplani and Talmadge 2016), so certainly coup incidence is an important indicator.

Nevertheless, a variety of civil-military arrangements can result in an absence of coups (Belkin and Schofer 2003). For example, some countries might not experience coups because the military is genuinely out of the business of contesting civilian political power. Other countries might evince a similar record of noncoups, but only because the regime in power takes a series of active measures to prevent military overthrow. These measures both reflect, and are likely to engender, a diverging pattern of civil-military relations from those in a state where no such measures are necessary in the first place. The contrasting reasons for the coup-free records of Iraq and the United States in 1991 discussed earlier are informative in this regard.

Furthermore, even in states that do not experience coups due to the military's genuine refusal to contest the regime's political power, a wide range of civil-military patterns is still possible with potentially very different implications for military performance. Consider, for example, the differences between the United States and the post-Stalin Soviet Union. The militaries in both states accepted their subordination to civilian control, and coups were unthinkable in both. Yet the relationships between political leaders and military officers were structured quite distinctly in the two countries, one a single-party state and the other a consolidated democracy. Among many other differences, for example, the Soviet Union required party membership for entry into the officer corps and political commissars were embedded throughout the Soviet command structure—both practices that the United States firmly rejected (Kolkowicz 1985; Reese 2005). It is likely that these differing choices would have had important implications for the fighting capabilities of the two states had the Cold War ever turned hot, and substantively meaningful measures of civil-military relations should capture these distinctions.

Some work has moved in this direction, although not with the goal of examining the implications of civil-military relations for performance in war. For example, Murdie (2012) uses events data to track the role of civil-military conflict, rather than coups alone, in states' crisis bargaining outcomes. Sechser (2004) draws on an earlier version of the Banks data set (2002) to classify states according to whether they have "strong civilian control," "weak civilian control," or "military dictatorship." Even with this measure, however, there remains the problem that two states (again, such as Iraq and the United States) can both have strong civilian control over the military—in the sense that coups are very unlikely—even though the ways in which this control is achieved can vary greatly and reflect different underlying patterns of civil-military relations.

Even among autocracies, political leaders can achieve tight control of the military through very distinct mechanisms that have potentially powerful implications for states' broader conflict behavior (Brooks 2006). For example, leaders in North and South Vietnam both maintained a firm grip on their militaries, but the system of communist control practiced in Hanoi contrasted sharply with the coup-proofing mechanisms adopted in Saigon, which may in turn have accounted for the radically

different military performances of the two regimes (Talmadge 2015). As such, it is important to identify measures that provide more context about the particular ways in which political control is maintained.

In addition, some of the Banks codings seem to be substantively incorrect. For example, Iraq under Saddam is coded as a military dictatorship, which it was not. Saddam actually owed his political rise under President Ahmed Hassan al-Bakr in the 1970s to the fact that he did not have a military background and, therefore, posed little risk of conspiring with the officer corps to overthrow Bakr (Karsh and Rautsi 1991, 15, 35). Even if Saddam's regime were correctly characterized as a military dictatorship, however, this coding still would not necessarily capture the most important aspects of its civil-military relations. For example, many of the most problematic aspects of Iraqi civil-military relations as well as Iraqi military performance were also evident in the civil-military relations and military performance of true military dictatorships in the region, such as Qaddafi's Libya, so the key distinctions might have less to do with being a military dictatorship per se and more to do with other traits not captured in the existing measures.

Another approach to measuring civil-military relations tries to solve some of the aforementioned problems by tracking the actual steps leaders take to prevent coups (Belkin and Schoefer 2003; Pilster and Bohmelt 2011, 2012; Powell 2012; De Bruin 2016). Specifically, these studies examine the extent to which leaders divide their militaries in order to counterbalance any particular faction that might attempt a coup. By counting the number of military branches (such as paramilitaries or special presidential forces) in a given state, this approach seeks to detect leaders engaged in the aforementioned coup-proofing behavior.

These measures are a step in the right direction. Unfortunately, with the exception of De Bruin's promising new collection effort (2016), most of the counterbalancing data are sharply time restricted, beginning only in 1967 when the International Institute for Strategic Studies (IISS) *Military Balance* first began publication. De Bruin also has shown that the *Military Balance* data are plagued with numerous inaccuracies, such as not listing important security forces for some countries while including security forces for other countries years after the forces have been disbanded. The data on developing countries are especially poor prior to the 1990s, given that *Military Balance* initially focused on NATO and Warsaw Pact members (De Bruin 2016, 3-4).

In addition, *Military Balance* data were not created with the purpose of tracking civil-military relations and are missing some context. So, for example, the U.S. military may look as though it has been designed for counterbalancing according to this measure, simply because the military is large and complex, with many branches and suborganizations. This ambiguity may not be a problem for studies seeking to explain coup propensity or outcomes, because such organizational complexity could pose a coordination obstacle to coup attempts regardless of whether this was the structure's intended function or not. For our purposes, however, the distinction is important, because organizational complexity could reflect military

specialization rather than an attempt at counterbalancing, and the implications could be very different for military performance. In short, it is important to know not only the structure of a given military, but the intended purpose of that structure, yet existing data do not provide that context.

Weeks' (2008, 2012, 2014) work moves in this direction. She has developed one of the most accurate and substantively useful measures of civil-military relations, drawing in part on data gathered by Geddes (2003). Among other indicators, Weeks codes states according to whether the leader is a current or former high-ranking military officer; whether military officers hold cabinet positions not related to the armed forces; whether the military high command is consulted primarily about security (as opposed to political) matters; and whether most members of the cabinet or Politburo-equivalent are civilians.

Weeks' measure avoids many of the problems of the past measures described above. That said, Weeks' focus is on the broader variable of regime accountability, not on civil-military relations per se, and she codes only authoritarian regimes. This makes sense in light of her research focus but does leave unanswered the question of whether and when democracies might share some of these same civil-military traits. Indeed, there is substantial debate about whether "pathological" civil-military relations can arise only in authoritarian regimes. Earlier research tended to treat civil-military relations as nearly synonymous with regime type, assuming that democracies had "normal," generally good civil-military relations, while autocracies had significant civil-military pathologies (Reiter and Stam 2002). However, recent scholarship by Weeks and others has suggested that some autocracies have quite "harmonious" or at least functional civil-military relations (Biddle and Zirkle 1996; Brooks 2006; Weeks 2014; Talmadge 2013, 2015; Reiter 2016).

By the same token, democracies do not appear to be immune from civil-military dysfunction (Avant 1994; Posen 1984; Snyder 1984). In particular, nascent democracies, such as Republican Spain and Nehru's India, seem to have adopted some elements of coup-proofing, with consequences similar to those seen among their autocratic brethren (Graham 2002; Howson 1998; Raghavan 2009; Cohen 1990; Longer 1974; Wilkinson 2015). As a result, it would be useful to have data on civil-military traits across regime types. White's recent work (2016) makes an important move in this direction with its coding of levels of military participation in politics across all states since 1964, though coding additional facets of civil-military relations beyond this one would be useful as well. In the next section, we discuss our approach to doing so and present the broader arguments we seek to test by examining the resulting data.

Our Argument and Approach

As discussed, recent research provides good general reasons to challenge the notion that material factors are the primary determinant of military performance. More specifically, a growing body of research drawing on both qualitative and quantitative

evidence suggests that states with civil-military pathologies should perform worse in war compared to states with harmonious civil-military relations, though disagreements remain about what "pathological" and "harmonious" actually look like. Based on that general intuition, we argue here that regimes characterized by coup concerns are less likely to win their wars against other states, as are militaries distracted by internal tasks such as governance.

First, regimes characterized by coup concerns are likely to take a variety of measures that reduce the state's ability to generate military power. This is mainly because many of the same steps that would lead to the generation of conventional military power also create the potential for the military to threaten the regime at home. In order to hobble this power, leaders concerned about coups may attempt to stack the officer corps with those likely to be loyal to the regime, even at the expense of military proficiency. They may choose to periodically purge the officer corps of those they deem disloyal. They are likely to frequently shuffle the assignments of officers, in order to prevent officers from developing independent bases of support in the armed forces. They are likely to restrict training in the military, given that it could provide opportunities for plotting against the regime. Restrictions on officer communication, the imposition of an internally directed intelligence apparatus to monitor the military, and the deliberate creation of redundancies in the command structure are also likely, in order to inhibit the information sharing that officers might need to foment a coup and to provide the regime with the information needed to fend off any plots.

All of these measures should indeed help the regime stay in power in the face of potential military challenges, but they also run the risk of reducing the military's ability to perform well in conventional wars against external adversaries. Artificial restrictions on the composition of the officer corps are likely to reduce the quality of human capital among military leaders, lowering the level of competence with which campaigns are planned and executed. Purges and frequent shuffling of command assignments are likely to fracture bonds of trust between officers and soldiers and prevent the development of the solid working relationships needed for a well-functioning military. Restrictions on training and communication are likely to further prevent the military from adequately preparing for conflict as well as adapting to battlefield challenges once conflict has begun.

For all of these reasons, we should expect regimes concerned about coups to generate less military power. Certainly, this logic is consistent with the connection between coup concerns and poor military performance generally observed in states such as Argentina under junta rule (Stewart 1991; Kon 1983), Saddam Hussein during most of his rule in Iraq (Al-Marashi and Salama 2008; Talmadge 2013), the Soviet Union in the early days of WWII (Glantz 1998; Watt 1990; Ziemke 1988), and China shortly after the Cultural Revolution (MacFarquhar and Schoenhals 2006; Scobell 1995; Joffe 1982). This logic and evidence lead to our first hypothesis:

Hypothesis 1: Regimes characterized by coup concerns are less likely to win interstate wars.

Coup fears are one form of civil-military pathology. Even in regimes that may not be explicitly concerned about coups, however, the military can have an outsized role in governance and internal politics that diminishes its capacity to focus on fighting conventional wars. In some states, the military is the most functional national institution, leading it to assume substantial domestic responsibilities. These could include the running of commercial enterprises, government administration, policing, conducting counterinsurgency operations, or repressing other forms of internal dissent.

Although these activities may help stabilize the regime, they also impose substantial costs in terms of the military's ability to develop externally oriented combat power for the prosecution of conventional wars. Military officers who spend their time on matters of administration, business, local governance, or policing are unlikely to simultaneously have time to properly gather intelligence about external adversaries, plan appropriate conventional campaigns, or conduct the training and other preparations needed to effectively execute such campaigns. Even the skills involved in policing or counterinsurgency operations can be quite different from those required in conventional, external wars and lead to different forms of military organization and force structure (Sechser and Saunders 2010). Combating lightly armed criminals or insurgents on familiar terrain is often a far cry from engaging in battles against regular, professional military forces from an opposing national army. Certainly, the military fortunes of states such as Pakistan again attest to this reality (S. Cohen 1998; Staniland 2008). For all of these reasons, we would expect that militaries with an internal focus are likely to fare poorly in such contests. This logic leads to our second hypothesis:

Hypothesis 2: Militaries focused on internal tasks are less likely to win interstate wars.

Thus far scholars have lacked the precise data needed to test these hypotheses cross-nationally. We seek to test them more rigorously through the collection of high-quality, comparable cross-national data on the relevant independent variables. We do this by developing an original data set of the civil-military traits of all war participants starting with WWII. In the next section, we describe these data in more detail, along with our methodology for testing the relative strength of these variables versus plausible alternatives in explaining war outcomes.

Data and Methods

We test our hypotheses using the Wartime Civil-military Relations Data Set, an original data set of civil-military characteristics of war participants, as designated by the Correlates of War (COW) participant data (Sarkees and Wayman 2010).[1]

Participant-level data allow us to investigate whether war participants with particular civil-military pathologies are more or less likely to be associated with various outcomes of interest such as war outcomes, war duration, and casualties. In order to ensure some comparability among regimes, civil-military organs, and wars, we restrict our timeframe to war participants starting with WWII. This choice results in 129 total observations, with the unit of analysis being the war participant.[2]

We acknowledge that there may be selection effects into the data set due to our focus on war participants. Our data do not code all states in the international system in a given year, only those states that find themselves in interstate wars. If some set of observed or unobserved variables systematically determines which states enter the data set and then also influence our dependent variable, the coefficients that we estimate for the independent variables in our model, including civil-military traits, may be biased.

We believe that our existing data are still quite useful for several reasons. First, wars have many causes (Van Evera 2001), only some of which seem even potentially correlated with our independent variable of interest. As a result, it is unlikely that any effects we find are purely a function of selection effects, such that the independent variables of interest have no independent weight at all. In fact, other prominent studies of war outcomes have followed a similar research design (Lyall and Wilson 2009; Reiter and Stam 1998), examining prewar traits of war participants as a means of explaining war or battlefield outcomes, even though it is plausible that those prewar traits might have had something to do with why the state ended up in the war in the first place. Such studies are widely cited and their findings considered by many to be robust, generating confidence in our research design.

Second, precisely because wars have many causes and civil-military relations can assume many different forms, it is unlikely that by examining war participants alone we are biasing the analysis in favor of our chosen independent variables. Indeed, ex ante, it is hard to know which way particular civil-military traits should cut when it comes to entry into wars. A large and complex literature suggests a variety of possible answers (Brooks 2008; Powell 2014; Reiter and Stam 2002; Snyder 1991; Sechser 2004; Van Evera 2001; Weeks 2012, 2014). In fact, some of these answers seem directly in tension with one another. Weeks, for example, finds that personalist regimes are more likely to be reckless in their initiation of conflict, while Powell, using different data, finds that coup-proofed regimes actually are not as likely to initiate disputes as often assumed.

Investigation of our own data shows that states with key civil-military pathologies are as likely to be targets as they are to be initiators, with the distribution of initiation across most of the indicators being almost 50/50. This suggests that there is no reason to think that our variables of interest are skewing entry into the data set in any particular direction. Our inclusion of both targets and initiators of wars in our analysis thus helps to guard against the possibility that our data systematically truncate important variation and to ensure that the deck at least is not stacked in favor of our hypotheses. We also include a control variable for initiation, as discussed below,

which has not always been done in past studies utilizing this design and which we believe helps further hedge against the possibility that some set of factors correlated with initiating or being a target of wars is entirely driving outcomes.

Third, we do not overstate the inferences one can make from the data we provide. Our goal is to examine the plausibility of nonmaterial explanations of war through initial tests using new data that better capture the underlying concepts of theoretical interest. Pending confirmation of that possibility, our framework provides a clear pathway for the construction of a more expansive dyad-year data set that would include information on nonwar countries across all years. In fact, other studies with our same basic research design (Lyall and Wilson 2009; Reiter and Stam 1998) have paved the way for similar expansions after the publication of the initial findings (Sechser and Saunders 2010; Reiter and Stam 2002). As a result, we believe the steps we take here are both valuable and necessary before moving on to any further data collection effort. Indeed, if there are no initial findings, then there is little reason to undertake such efforts.

With that objective in mind, we coded the independent variables of interest related to civil-military relations as binary measures based on the questions listed below. As mentioned, these questions spring largely from the findings of the qualitative literature on civil-military relations. They generate thirteen candidate independent variables that might be associated with outcomes of interest:

1. Did the current regime come to power in a military coup? (*coupRegime*)
2. Has the country ever experienced a military coup? (*coupHistory*)
3. Is the country's top leader a former military officer? (*milExperience*)
4. Are ethnic, sectarian, or racial criteria used to exclude segments of the population from the officer corps? (*officerExcl*)
5. Are there strict ideological requirements for entry into the senior officer corps? (*ideolExcl*)
6. Is party membership required for entry into the senior officer corps? (*partyExcl*)[3]
7. Does military training involve extensive political education or ideological indoctrination? (*indoctrination*)
8. Has the military been used to repress internal dissent in the last five years? (*milRepression*)
9. Has the military been used to govern the country in the last five years? (*milGovern*)
10. Is there a paramilitary organization separate from the regular military, used to provide regime or leader security? (*paramil*)
11. Is there an internal intelligence apparatus dedicated to watching the regular military? (*intelSplit*)
12. Has a purge of the officer corps occurred in the last five years? (*purge*)
13. Is there an institutionalized forum through which civilian leaders and military officers regularly exchange information? (*infoExchange*)

These variables are original, and each was coded through in-depth qualitative research on the country in question for the year prior to war participation.[4] In most cases, coders relied on secondary sources, though in some cases primary documents and interviews with subject matter experts were necessary. Each coding resulted in a yes/no answer to the given question, as well as longer prose explanations to discuss any further details, caveats, or coding decisions. Certainly, future research could explore more gradated values on these independent variables. For the purposes of initial data collection and replicability, however, we chose to code the variables in binary fashion. Our goal was to answer each question in the context of a given time and place and then to explain that context as necessary. All coding choices were justified with specific references to documentation and specific data points that supported the coder's conclusions. For intercoder reliability, all codings were then double-checked by at least one other coder to ensure consistency, clarity, and accuracy.[5]

We also used several aggregate measures to capture increasing civil-military "badness" or pathologies. In particular, *sumBad* is the sum of the following variables: *coupRegime*, *coupHistory*, *officerExcl*, *intelSplit*, and *purge*. All of these variables are subsets of the same conceptual family, so we constructed this aggregate variable to more accurately capture coup concerns and coup proofing. It therefore ranges from 0 to 5. Another aggregate measure, *sumInternal*, is the sum of *milRepression* and *milGovern*. It thus ranges from 0 to 2. This aggregate variable captures the two subsets of internal focus, that is, those militaries that have internal burdens. These aggregate variables test whether "badness" on certain clustered dimensions is additive and can have cumulative effects on a state's propensity to win or lose wars. The detail of these civil-military measures allows us to probe what is correlated with outcomes of interest and, equally importantly, to probe which civil-military dimensions are *not* necessarily related to them.[6]

Our dependent variable is whether the war participant was victorious in a war taken from the COW data set (Sarkees and Wayman 2010). We focus on war outcomes as our dependent variable for two reasons. First, war outcomes are the most consequential implication of our argument for international politics. Simply put, if our data can help explain the results of the most violent and significant contests in international politics, we believe our claims will have passed one of the broadest and most demanding possible tests. Second, we believe that the testable implications of our argument for war outcomes are more straightforward than they are for other dependent variables commonly examined in the conflict literature. Regarding loss-exchange ratios, for example, states endowed with an ample supply of military recruits could make strategic choices to incur more casualties, which would deflate their effectiveness according to this metric, even though states in fact do win wars with attrition strategies (as the Soviet Union managed to do in WWII). States can also be militarily ineffective while suffering relatively few battle casualties if their armies flee the battlefield (as many South Korean units did in 1950 after North Korea invaded). Hence, although our argument and data could implicate multiple

metrics of military performance, we limit ourselves to war outcomes as an initial test here, because the logic of our argument provides such clear predictions about what we should observe if the argument is correct.

Because this measure has outcomes besides win/loss, including transformation and stalemate, we use two separate measures derived from the standard COW outcome. The first simply drops all outcomes that were not clearly win/loss, *War-Outcome*. This results in the attrition of 42 observations (or roughly a third of the data). The second measure follows standard practice and codes stalemates as "non-wins" and aggregates these with losses, *WarOutcomeStale*. This results in a loss of twenty-five observations. There is no obvious way to treat the remaining observations, most of which involved "war transformation." But the latter measure captures one of our primary outcomes of interest, which is whether particular civil-military pathologies inhibit a state's chance of winning a war.[7]

Because this is an initial effort and we wanted to explore all possible implications of our data, we also code outcomes of interest derived from COW, including war duration (*warDuration*) in months, a binary war duration variable measuring whether the war lasted longer than six months (*warDuration6*), and casualties (*casualties*). The casualties variable is also updated using the data from Grauer and Horowitz (2012).

Control variables generally associated with outcomes of interest are also included, many from COW. First, we include a capabilities variable (*convCapabilities*) using the Composite Index of National Capabilities (CINC) measure from COW, to capture the material capabilities of the war participant. Since these vary cross-nationally, their inclusion helps control for the relationship between additional material power and war outcomes. In addition, we include a control for whether the war participant had a defense pact that would externalize the conflict and potentially increase chances of victory (*pact*). Third, because it is known that initiators are often more likely to win the war, we include a control variable for war initiation (*initiation*). Fourth, we add a control for regime type using the *Polity* IV score (*polity*) from the *Polity* data sets (Marshall, Gurr, and Jaggers 2013). We use the full range of the variable (-10 to 10) to capture regime type characteristics.[8] Finally, we include various measures of whether a war participant was simultaneously experiencing civil conflict, which might inhibit its ability to win an interstate war (*CivWar*).

Because we use a binary measure for *warOutcome*/*warOutcomeStale* and *warDuration6*, a logit model is most appropriate for the analysis.[9] Because the data structure is not time series, and most of the independent variables are time invariant over the course of a war, we cannot employ a hazard model. For the raw *warDuration* and *casualties* variables, we use a variety of models including ordinary least squares, but the skewed distribution of these variables makes them very difficult to model appropriately. Hence, our most robust results come from the logit models using the three dependent variables noted above. In each case, we use country-clustered robust standard errors to account for spatial dependencies.

Table 1. Crosstabs Indicating That Some Civil-military Relations Variables Take War Outcome from Being a Flip of a Coin to Heavily Skewing Them Toward Loss.

		OfficerExcl	
		No	Yes
WarOutcome	Lose	34	27
	Win	34	8

		Purge	
		No	Yes
WarOutcome	Lose	36	26
	Win	33	9

		milGovern	
		No	Yes
WarOutcome	Lose	35	27
	Win	32	10

Note: Corr(*purge, OfficerExcl*) = 0.048; Corr(*purge, milGovern*) = 0.27; Corr(*OfficerExcl, milGovern*) = 0.19.

Analysis and Results

We now report the results for several dependent variables of interest, focusing on war outcomes, which is where we found primary support for the relationship between several civil-military dimensions and conflict outcomes of interest, consistent with the hypotheses that we presented earlier. Since each of the dependent variables is binary, we report the results of logit analysis in each of the following tables. In each case, we report the variables that were shown to be significantly associated with the particular outcome of interest. This means that many of the thirteen civil-military variables above were *not* significantly associated with the particular dependent variable. We return to the implications of those nonfindings later.

As a simple illustration of how some of these variables affect war outcomes, the cross-tabs above show that the presence of variables such as *officerExcl*, *milGovern*, and *purge* essentially take state prospects of winning a war from a 50/50 gamble toward a heavily skewed chance of losing. Table 1 depicts the crosstabs for war outcome and each of the civil-military relations variables (see Appendix B for the list of war losers we code as possessing each civil-military dimension).

For a more complete and rigorous assessment, we now turn to our multivariate regression analyses to measure the substantive and statistical impact of these particular civil-military relations variables. The first set of results we report focuses on the war outcome variable, that is, win/loss. Table 2 depicts the results using the original COW coding of win/loss and dropping any other war outcome (e.g., transformation and stalemate).

Table 2 indicates which of the civil-military dimensions are significantly correlated with war outcomes, when stalemates are dropped. The critical civil-

Table 2. Relationship between Civil-military Measures and War Outcomes (Dropping Stalemates).

Variable	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
coupHistory	−1.27**					
	(0.72)					
officerExcl		−1.40**				
		(0.69)				
milGovern			−1.24**			
			(0.58)			
purge				−1.19*		
				(0.66)		
sumBad					−0.80***	
					(0.31)	
sumInternal						−0.86*
						(0.46)
initiation	1.36**	1.31**	1.47**	1.22**	1.42**	1.45**
	(0.64)	(0.63)	(0.62)	(0.61)	(0.61)	(0.64)
convCapabilities	−1.01	−0.49*	−0.50	−0.55**	−0.73	−0.48
	(4.43)	(0.28)	(0.32)	(0.20)	(1.52)	(0.33)
pact	0.72	0.15	0.33	0.63	0.47	0.41
	(0.76)	(0.71)	(0.70)	(0.65)	(0.65)	(0.67)
polity	0.12***	0.12**	0.12**	0.10**	0.07*	0.11**
	(0.05)	(0.04)	(0.05)	(0.05)	(0.05)	(0.05)
CivWar	0.61	0.44	0.56	0.44	0.75	1.05
	(1.22)	(1.20)	(1.21)	(1.32)	(1.29)	(1.35)
Constant	−0.52	−0.30	−0.48	−0.63	0.30	−0.26
	(0.68)	(0.67)	(0.70)	(0.59)	(0.61)	(0.68)
N	76	76	76	76	76	76

*$p < .10$.
**$p < .05$.
***$p < .01$.

military relations variables that appear significant when controlling for a host of covariates considered to be related to war victory are as follows: *officerExcl*, indicating that the officer corps excludes candidates using some non-merit based criteria; *milGovern*, indicating those militaries that have had to govern at some point in the past five years; *purge*, which reflects the militaries that have experienced an officer corps purge in the past five years; and *sumBad* and *sumInternal* as defined above, which are summation variables of constituent "bad" civil-military practices.

These findings provide support for our hypotheses. Officer exclusion, purges, and the other civil-military pathologies are all symptoms of the coup concerns we theorized should lead to poor military performance and a reduced likelihood of victory in war. Likewise, the military government variable and other variables related to the

military's internal role also are negatively associated with victory, again consistent with our hypotheses.

As for the controls, *polity*, or the regime-type measure, is also unsurprisingly correlated with war outcomes, with a higher degree of democracy imparting increased chances of winning wars. Even with the inclusion of *polity*, however, the various civil-military variables are significant and, as we report below, still exert significant influence on the probability of winning wars. Interestingly, however, once controlling for war initiation, conventional capability as measured by CINC is sometimes not significantly related to war outcomes and its direction is almost always negative, or actually correlated with loss. These findings are relatively robustly significant to various specifications and using alternative measures of controls.[10] Table 3 presents the results for war outcomes with stalemates coded as nonwins, which results in lower data attrition.

The only major change between using the two definitions of war outcome is that *coupHistory* is no longer significant, though the sign is in the expected direction. The other five civil-military measures are still robustly significantly associated with reducing the probability of winning wars. The general trends identified in Table 2 largely hold with respect to *polity* and the virtual irrelevance of capabilities, particularly once initiation is included. To indicate just how important the civil-military variables are in reducing the probability of war victory, we calculate first differences (using Clarify) for each of the significant civil-military relations (CMR) variables in Table 4, holding all other variables at their median.

The first differences suggest that, holding all other variables at their median, a state's probability of winning a war falls by 18 percent (the 95 percent confidence interval does not include zero, and the first difference is significant at the $p < .10$ level)[11] if there is an exclusion criterion applied to the officer corps as part of coup-proofing. Similar reductions are experienced for *milGovern* and *purge*, all significant at the $p < .10$ level. If a state has two of these pathologies, as expressed by *sumBad* equals 2, the probability of winning a war falls by 28 percent. These are rather large substantive effects.

To test whether any of the civil-military variables are associated with war duration, we also constructed a dummy variable that partitioned war duration at six months. We chose a six-month cutoff for two reasons: (1) most wars are actually grouped in this category so there is a substantial number of observations and (2) long wars enable states to reorient their institutions and potentially generate new sources of military power during the war, so short wars best capture the effect of the prewar civil-military relationship.[12] The only civil-military variable that was significant was *officerExcl*, as shown in Table 5. This suggests that states that exclude officers on nonmeritocratic grounds are more likely to not only lose wars, but to lose them *quickly*. That is, if a state decides to shape its officer corps using sectarian, ethnic, or some other exclusionary criteria, it sacrifices a substantial amount of warfighting capability, leading not only to loss, but quick loss. None of the other civil-military variables were robustly significantly related to war

Table 3. Relationship between Civil-military Measures and War Outcomes (Including Stalemates as Losses).

Variable	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
coupHistory	−0.69					
	(0.55)					
officerExcl		−1.21**				
		(0.55)				
milGovern			−1.09**			
			(0.45)			
purge				−1.00*		
				(0.53)		
sumBad					−0.65***	
					(0.24)	
sumInternal						−0.88**
						(0.44)
initiation	0.58	0.69	0.79	0.60	0.74	0.88
	(0.58)	(0.52)	(0.54)	(0.58)	(0.52)	(0.53)
convCapabilities	−0.91	−0.64	−0.63	−0.55**	−2.24	−1.54
	(3.05)	(0.93)	(1.11)	(0.27)	(3.72)	(4.66)
pact	0.35	−0.07	−0.003	0.33	0.21	0.02
	(0.54)	(0.51)	(0.51)	(0.44)	(0.49)	(0.46)
polity	0.11***	0.12***	0.11***	0.09***	0.07**	0.10**
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
CivWar	−0.22	−0.32	−0.24	−0.21	0.02	0.27
	(0.99)	(0.99)	(0.97)	(1.04)	(1.08)	(1.13)
Constant	−0.66	−0.41	−0.47	−0.65	0.11	−0.15
	(0.48)	(0.51)	(0.56)	(0.45)	(0.50)	(0.61)
N	93	93	93	93	93	93

*$p < .10$.
**$p < .05$.
***$p < .01$.

Table 4. First Differences for War Outcomes (Including Stalemates as Losses), Using Clarify.

Variable	First difference
officerExcl (0–1)	−18%*
milGovern (0–1)	−17%*
purge (0–1)	−18%*
sumBad (0–2)	−28%*
sumInternal (0–2)	−28%*

Note: Percentage depicts reduction in odds of winning if a war participant is coded as having the relevant civil-military dimension.
*$p < .10$.

Table 5. Relationship between Ethnic Exclusion and War Duration (Wars Shorter than Six Months).

Variable	War duration < six months
officerExcl	−0.85*
	(.45)
initiation	−0.24
	(.40)
convCapabilities	−0.10
	(.13)
pact	.06
	(.37)
polity	−0.05*
	(.03)
CivWar	−0.77
	(.82)
Constant	−0.26
	(.40)
N	115

*$p < .10$.
**$p < .05$.
***$p < .01$.

duration. Which states possessed *officerExcl*, lost wars, and lost them quickly? This is, in fact, a surprisingly common mechanism for losing wars quickly, as Table 6 illustrates.

Conclusions and Implications

In this article, we have shown that civil-military relations, when coded properly across various dimensions, do appear to exert a substantial influence on states' probability of winning interstate wars. The effect is large and consistent across a number of indicators. It is also interesting to note that we find little support for many dimensions of civil-military relations hypothesized in the literature to be related to war wins/losses. For example, we do not find that the presence of paramilitary organizations affects the likelihood of victory, even though that is one of the key civil-military indicators examined in the counterbalancing literature. Similarly, we do not find that ideological training in the military has an effect on war outcomes, even though one might imagine that such training could be both beneficial and harmful to military effectiveness in different ways. Our findings thus shore up the importance of examining specific civil-military mechanisms, particularly those associated with coup proofing and internal governance tasks, rather than merely characterizing civil-military environments as generally "conflictual" or "harmonious."

Table 6. States That Excluded Officers for Nonmerit-based Reasons, Lost Wars, and Lost Them in Less than Six Months.

War	State	Year
Franco-Thai	France	1939
Arab–Israeli	Egypt	1947
Arab–Israeli	Syria	1947
Arab–Israeli	Lebanon	1947
Arab–Israeli	Jordan	1947
Soviet invasion of Hungary	Hungary	1955
Ifni War	Morocco	1956
Six-Day War	Jordan	1966
Six-Day War	Syria	1966
Bangladesh	Pakistan	1970
Yom Kippur War	Saudi Arabia	1972
Yom Kippur War	Jordan	1972
Yom Kippur War	Syria	1972
Yom Kippur War	Iraq	1972
Turco-Cypriot	Cyprus	1973
Ugandan–Tanzanian	Uganda	1977
War over Lebanon	Israel	1981
War over Lebanon	Syria	1981
Azeri-Armenian	Azerbaijan	1992
War for Kosovo	Yugoslavia	1998
Kargil War	Pakistan	1998

This is a provocative finding for those who see war outcomes as strongly dependent on the balance of material power. Our work provides support to those who argue that nonmaterial, political variables are quite important in explaining victory and defeat. In fact, once the analysis appropriately accounts for regime type and civil-military relations, along with other relevant covariates, we find that material capabilities are not very powerful predictors of war outcomes at all.

For those already convinced of the general importance of nonmaterial variables, this work enables scholars to have more confidence in exactly which nonmaterial variables matter, how much, and why. Our analysis shows that although regime type remains very important in explaining war outcomes, civil-military relations still exert a major independent influence. Most crucially, our data set draws on the insights of qualitative research on civil-military relations in order to enable much more precise yet generalizable quantitative tests of this variable than have often been used in past studies. As such, our findings should give researchers confidence that civil-military pathologies, particularly stemming from coup-proofing or an internal role for the military, are indeed a wide-ranging phenomenon and quite significant in explaining war outcomes across time and space. Future research should explore these results further by using alternate specifications, additional controls, and expanded original data, especially data that capture any changes over time in

civil-military relations in particular countries, either within or between wars. Such research could use our data to examine postwar outcomes, such as the long-term, postconflict fate of leaders.

These findings also have significant implications for foreign policy. For example, U.S. efforts to assess and train foreign militaries, both adversaries and allies, often seem to depend heavily on the assumption that military power is mostly about material capabilities—men under arms, weapons purchased, ammunition stockpiled, and so on. Indeed, the United States has poured billions of dollars into training the Iraqi and Afghan militaries over the last decade based largely on this premise ("U.S. Military Operations" 2015). The findings here instead suggest that the conversion of resources into military power is highly dependent on civil-military relations, which have an independent ability to erode even significant material advantages. Notably, contemporary Iraq and Afghanistan both exhibit many of the civil-military pathologies coded in the Wartime Civil-military Relations Data Set (Talmadge and Long 2015), which may be why the Iraqi military, for example, collapsed rapidly in the face of Islamic State attacks in 2014, despite significant material advantages (Fraiman, Long, and Talmadge 2014). These recent disappointments, along with the research provided here, strongly suggest the need for closer attention to dynamics between the political leaders and military organizations that are actually responsible for utilizing external military assistance.

Authors' Note

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Supplemental Material

Notes

1. The full data set is available online at caitlintalmadge.com.
2. For wars involving large coalitions, we drop minor participants whose military contributions were unlikely to have affected the outcome of the war. For example, we do not count the Philippines as a separate war participant in the Vietnam War, even though it did send a small contingent of forces to the war to assist its ally the United States (mostly with noncombat tasks).
3. We include separate measures for ideology (question 5) and party membership (question 6) because some highly ideological states may not have party systems, and not all militaries that require party membership are highly ideological. Also, the existing literature makes predictions about the role of both ideology and party systems in military performance, so we wanted to disaggregate the two measures in our initial data gathering efforts. As it turns out, the overlap between the two variables is relatively high, .66, but not total. Ultimately, neither variable was significant in our models, and combining the two measures into a single variable, with party as a subset of ideological requirements, did not change this.
4. Our data do not speak to the possibility of intrawar changes in civil-military relations that could then affect war outcomes. Although such changes do occur (as in the Iran-Iraq war, see Talmadge 2013, 2015), most wars are too short for these to come into play (as we discuss further under "Analysis and Results"). Extensive, in-depth case research would also be needed to detect the emergence and impact of these shifts and hence was beyond the scope of our initial coding effort. However, this question is a fruitful topic for further research.
5. The full coding sheets for each observation are available online at www.caitlintalmadge.com so that all of our coding decisions are completely transparent and our dataset easily accessible. We include a randomly chosen example in Online Appendix A, also available online, to give the reader a sense of our data.
6. Although some of the indicators are obviously correlated with each other, such as *coupRegime* and *coupHistory*, which occur together roughly half the time, most of the other indicators in the aggregate indexes are independent of one another. In the *sumInternal* index, for example, the correlation between *milGovern* and *milRepression* is less than one-third.
7. We also employed different measures for war outcomes besides Correlates of War (COW), including Horowitz, Reiter, and Stam (forthcoming), which codes outcomes for individual battles rather than aggregate war outcomes. Unfortunately, the lower overlap between COW participants and the battles they code results in significant missingness in our data set. We therefore report results using COW outcomes here.
8. We also included the Geddes/Weeks personalist, military, and single-party rule variables in our data set as controls (Weeks 2008), but because they are restricted to autocracies and

have substantial missingness prior to the Cold War, as well as in some regions during the Cold War, especially the Middle East, these variables are included only as robustness checks. The results were robust to their inclusion.

9. There is no obvious study using this data structure with up-to-date and high-quality data that we could easily replicate using our Wartime Civil-military Relations Data Set. This is, therefore, an originally constructed data set which we believe has higher quality data than any off-the-shelf data set we could have employed.

10. These results are also substantively robust to the inclusion of an *Arab* dummy, which we included in robustness checks because a large number of wars post-WWII involve Arab losses. These results are robust to various cutoffs and measures of *polity*, and a multitude of permutations with respect to specification. Additional robustness checks are available upon request.

11. First difference plots available upon request, but since these are all ordinal variables, they convey the same substantive information as Table 4.

12. Again, these are reasons we chose not to examine intrawar shifts in civil-military relations. See also footnote 4.

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