FINAL

Democracy and Military Effectiveness

A DEEPER LOOK

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The democratic effectiveness thesis is tested by examining traits that lead to the success of democracies in war. Findings show that superior human capital, harmonious civil-military relations, and Western cultural background are largely responsible and that these traits correlate positively with democracy and account for democracy's apparent effectiveness. The results show a powerful effect for unit-level variables in military performance, in the process sharpening understanding of military effectiveness in general and the relationship between military performance and regime type in particular.

A growing literature argues that democracies are unusually successful in war. Empirical researchers, including David Lake, D. Scott Bennett, Dan Reiter, Allan Stam, and others have observed a strong correlation between democracy and victory and have argued that unique properties of democratic decision making, leadership styles, economic performance, or popular commitment to state policy are responsible for this.

This study explores these claims by considering a variety of other potential unit-level contributors to military effectiveness. Some unit-level factor seems responsible for a distinctive pattern of military performance by democracies, but which one? "Democracy," after all, is a complex basket of traits, some direct (such as exposure to

electoral sanction, checks and balances from a loyal opposition, or a free press) and some indirect (such as a culture of individual autonomy, a pattern of superior economic performance, or typically more harmonious civil-military relations). Moreover, the relationship between the direct features of democratic political organization and its correlates in second-order attributes, such as wealth or civil-military relations, is disputed. Some say democracy gives rise to wealth and civil-military comity; others reverse the causal arrow and contend that wealth or civil-military comity gives rise to democracy. The correlations could simply be spurious. And, of course, one could imagine codetermination, in which democracy and traits such as economic performance are tied together in a more complex way, with spirals of wealth leading to political liberalization leading to greater wealth.

Either way, it matters which of these unit-level traits are most important for military performance. If one sees democratic political organization as a causal tap root leading to a variety of militarily useful second-order consequences, then by studying these consequences in more detail, we can explicate the mechanism by which democracy promotes military success, thus enriching the theoretical foundation of the democratic effectiveness thesis. By contrast, if one sees democracy as effect rather than as cause, or as a coincidental correlate of other militarily relevant unit-level traits, then democratic political organization and other unit-level traits offer competing explanations of military effectiveness. And if so, then establishing the relative causal importance of political organization, wealth, culture, education, or civil-military relations can make the difference between accurate and inaccurate prediction of any given state's military potential.

The study thus considers several such unit-level variables explicitly and finds that human capital, civil-military relations, and culture account for much of the effectiveness bonus nominally associated with regime type. This finding has several important consequences. First, it sharpens our understanding of military effectiveness. Focusing on more direct causes rather than on partial correlates, such as regime type, enables more of the variance in military outcomes to be explained. It also offers more accurate predictions for the military power of particular states—especially for states where democracy, strong human capital, harmonious civil-military relations, and Western culture do not all coincide (such as India, Brazil, or interwar France, to cite just a few examples) or, conversely, where autocracy, weak human capital, conflictual civil-military relations, and non-Western culture do not all coincide (such as North Korea, China, or Nazi Germany, for example).

Second, these findings sharpen our understanding of the relationship between regime type and military performance. Many of the variables we consider here correlate positively with democratic political organization—indeed, this correlation is largely responsible for democracy's apparent military effectiveness bonus in the empirical literature.[1] To the extent that democracy causes culture, superior human capital, and harmonious civil-military relations, then the findings here suggest that democracy's effectiveness bonus works via regime type's effect on the intervening and

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1. Human capital, for example, has a simple correlation of .35 with democracy in the full data set. For the observations included in model 2, the correlation is .48.

more causally proximate variables of human capital, civil-military relations, and culture.

Alternatively, if one sees these unit-level traits as causally unrelated, or if one sees democracy as effect rather than cause, then our findings constitute an important challenge to the democratic effectiveness thesis per se. In fact, if democratic political organization is not an underlying cause of strong human capital, harmonious civil-military relations, and Western cultural practices, then our findings show its marginal influence on military effectiveness to be negative, not positive as the democratic effectiveness literature asserts. The difference turns on questions of democracy theory that are beyond our scope here, but either way, the results deepen our understanding of regime type's effects on military performance.

Either way, we agree with the democratic effectiveness school that unit-level traits are central for understanding real military capability—whether regime type ultimately proves to be the most important unit-level variable or not. We thus share the democratic effectiveness critique of system-level theories that rest on capability as an important causal driver: military effectiveness is powerfully shaped by the varying internal characteristics of states. Hence, international political theories turning on military capability cannot soundly be specified solely at the system level.

We present this case in five steps. First, we briefly recap the literature on cause and effect in battle outcomes and derive from this review a series of hypotheses for testing. We then discuss our data set and its limitations. Next, we operationalize our variables. We then present the statistical results. We conclude with a series of implications from these results for scholarship and policy.

EXPLAINING MILITARY OUTCOMES

War's causes have attracted an enormous scholarly literature. By contrast, war's outcomes have been little studied by social scientists. Outcomes of battles, as distinct from wars, have attracted even less attention.[2]

Yet battle outcomes offer some potentially important advantages as instruments for exploring regime type's influence on military effectiveness.[3] Because there are vastly more battles than wars, battle outcomes thus offer a much larger *n*, with the corresponding statistical advantages. Battles also speak more directly to effectiveness in combat per se: whereas wars can be won by militarily ineffective but highly resolute states, success in battle requires military effectiveness.

We thus focus here on battle outcomes. The relative inattention to battles as such in the political science literature, however, means that many of the most important theories of battle outcomes are implicit rather than explicit in that literature. At least six

2. For exceptions, see Reiter and Stam (1998, 2002); Biddle (1998, forthcoming).
3. "Military effectiveness" can be defined in a variety of ways: see, for example Brooks (2003); Biddle (forthcoming, chap. 1). This definitional complexity stems in part from the many tasks militaries are asked to perform (ranging from invasion and territorial defense to peacekeeping or maintenance of domestic order) and in part from the complicated relationship between military action and the larger goals of the state.

broad groups of such theories can nevertheless be discerned: material quantity, material quality, regime type, human capital, civil-military comity, and national culture.[4]

MATERIAL QUANTITY

Realists, for example, emphasize the relative quantity of material resources—troops, defense expenditures, GDP—as the main determinant of the balance of power, which they see as the chief arbiter of victory or defeat in war (Mearsheimer 2001; Copeland 2000; Waltz 1979). The empirical literature in international politics also uses relative material wherewithal as its primary measure of military capability or the balance of power (Gibbs and Singer 1993; Merritt and Zinnes 1989). This in turn implies a powerful role for material preponderance in battle outcomes, because battlefield victory (as opposed to superior resolve or domestic unity) is the implicit

Similarly, ex post measurement of "effectiveness" in practice can be approached in a variety of ways. Some seek to cut through the inherent complexity of military activity by adopting simplified subjective codings of aggregate "victory" or "defeat." This approach provides a single index that might, in principle, account for a broad range of diverse considerations, but the very diversity of the considerations being spanned requires the coder to resolve difficult trade-offs among them to yield a single "winner" and "loser." In the 1968 Tet offensive, for example, the Vietcong failed in their intended objective of inducing an uprising to overthrow the Thieu regime and suffered crippling losses that essentially destroyed them as a military force—thereafter, the war in the south was waged increasingly by North Vietnamese regulars. Yet the battle's unintended effect on American domestic politics surely aided the Vietcong cause in the longer run (Herring 1986, chap. 6). Both sides gained something and lost something, but the gains and the losses were in very different coin, making coding validity heavily dependent on the purpose to which the coded values are ultimately to be put. The Egyptian crossing of the Suez in 1973 resulted ultimately in the encirclement of their Third Army but an enhancement of Sadat's standing in the Arab world; the Israelis defeated an invasion, but Sadat profited politically—both sides "won" in different senses (Baker 1978, chap. 6). Whose military was more "effective"? The American defense of St. Vith in 1944 was ultimately overrun, but it delayed the German advance and bought time; again, both sides "won" in at least some respects (Cole 1965). Such cases are not uncommon. Assessing effectiveness via dichotomous coding of "victory" and "defeat" can thus be much more ambiguous in practice than the scheme's apparent simplicity suggests; whereas CDB90 provides such a subjective coding, its criteria and rationale are opaque, making the effects of these ambiguities on empirical results difficult to assess. By contrast, one can operationalize "military effectiveness" by focusing on subsets of especially important military functions and measuring their achievement objectively via direct observation. This sacrifices the holistic quality of subjective "win/lose" codings but offers in exchange objectivity and transparency. We adopt the latter approach here, focusing on the critical military function of destroying hostile forces while preserving one's own. Other observable dimensions of battlefield performance, such as the ability to take and hold territory, are related to this but are logically separable (see Biddle forthcoming); none of the more prominent choices, however (for example, net territorial gain or advance rate), produce materially different empirical results. We thus focus here on force destruction and preservation: an effective military is one that kills more opponents per friendly loss; an ineffective one is the opposite (see the discussion under "operationalizations" below).

4. There is strong reason to believe that military doctrine and tactics (or "force employment") are powerful determinants of battle outcomes (Biddle forthcoming, 2001, 1998, 1996; Stam 1996; Bennett and Stam 1996; Mearsheimer 1983). Force employment, however, is not treated in standard large-n data sets. Allan Stam has coded a data set using a "maneuver-attrition-punishment" characterization of force employment, but these codings are subjective and unavailable for analyses at the battle level of analysis (Stam 1996; cf. Reiter and Stam 1998). It is likely, however, that variance in force employment is attributable largely to underlying variance in unit-level attributes, such as, potentially, regime type, human capital, civil-military relations, or culture (inter alia)—for which data are more readily available (Biddle 2001). We thus focus on the latter here, but this is not meant to imply that force employment is unimportant. Rather, it reflects our assumption that the variables we consider here are deeper causes of the force employment variations discussed in Biddle (forthcoming, 2001) and that for now, these deeper causes are more amenable to statistical analysis than are their fruits in realized doctrine and tactics.

mechanism by which the balance of power would determine war outcomes in such analyses. As a rule, the numerically preponderant side is expected to prevail; the greater the preponderance, the greater the margin of victory.[5]

Hypothesis 1: Battle outcomes should improve with increasing numerical preponderance.

MATERIAL QUALITY

Intuitively, the quality as well as the quantity of materiel ought to matter. In fact, U.S. defense policy has long emphasized technological sophistication over numerical superiority, and official combat models include elaborate calculations of the quality, as well as the quantity, of the two sides' weapons (Brown 1983, 225-33; Perry 1989, 28-29; Candan, Dewald, and Speight 1987; Davis and Blumenthal 1991; Taylor 1983; Biddle 1988).

In the political science literature, the chief treatment of material quality is offense-defense theory.[6] Offense-defense theorists hold that prevailing weapon technology determines the relative ease of attack and defense, which in turn affects a host of political outcomes, ranging from the incidence of war to the formation of alliances, the severity of arms races, the salience of relative gains from international cooperation, or the structure of the international system (Andreski 1968; Cederman 1997; Christensen and Snyder 1990; Fearon 1995; Glaser 1994-45; Gilpin 1981; Hopf 1991; Jervis 1978; Nalebuff 1986; Powell 1999; Quester 1977; Van Evera 1998, 1999; Walt 1987; Waltz 1989). For warfare since about 1900, the key technologies are usually held to be tanks, artillery, and ground-attack aircraft: the more tank- and ground-attack-aircraft-prevalent the confrontation, the more offense-conducive; the more artillery-prevalent the confrontation, the more defense-conducive.[7]

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5. Some preponderance theorists offer elaborations, typically via thresholds for imbalances sufficient to permit successful attack or defense. A minimum attacker to defender materiel balance of 3:1, for example, is often said to be necessary for offensive success; a minimum "force to space ratio" of perhaps a division per 25 to 50 kilometers is often held to be necessary for defensive success (Thompson and Gantz 1987, 12; Mearsheimer 1983, 178n; 1989; Liddell Hart 1939, 54-55; 1960). Most, however, are vague on the intended unit of account. "Combat power" rather than raw troop strength, for example, is often given as the intended unit of "force," yet without operational specification. The determinants of "combat power" are, in fact, disputed: see below. For a more detailed review and critique, see Biddle (1988, forthcoming).

6. Empirical researchers occasionally use military expenditure per soldier as a measure of military quality, training, or technological sophistication (see, e.g., Stam 1996, 94-95; Huth 1996, 258), though the large-*n* literature as a whole focuses mainly on material quantity per se. Expenditure per soldier also poses a number of potential biases, as it can misrepresent as technical sophistication the effects of differing service systems (conscription militaries, for example, cost less per soldier than professional militaries), differing grand strategies (naval and air powers field more capital-intensive militaries than land powers and thus spend more per soldier regardless of the sophistication of their particular equipment), differing geopolitics (the United States must invest heavily in strategic mobility to reach likely theaters of war and thus spends more per soldier than Continental powers regardless of technology), and differing cultural predilections toward military service (Americans, for example, tend to provide more creature comforts for soldiers in the field than, say, Soviets, producing higher expenditures that do not reflect weapon technology per se). We thus focus on the offense-defense theoretic treatment of technology below.

7. Some offense-defense theorists include nontechnological variables, such as terrain or troop density; many consider other weapon types in addition to tanks, artillery, and aircraft. For a more detailed review and critique, see Biddle (2001). The primary focus in this literature, however, is on technology per se and on

> *Hypothesis 2a:* Battle outcomes for attackers should improve with increasing prevalence of tanks.
> *Hypothesis 2b:* Battle outcomes for attackers should improve with increasing prevalence of ground-attack aircraft.
> *Hypothesis 2c:* Battle outcomes for defenders should improve with increasing prevalence of artillery.

REGIME TYPE

Material quantity and quality are overwhelmingly the most common understanding of battle outcomes in the international relations (IR) literature. By contrast, with these orthodox materialist views, however, there is growing interest in nonmaterial determinants of military outcomes. The most studied nonmaterial determinant to date is regime type. In particular, a growing political science literature now argues that democratic states are more successful in war than nondemocracies.

The claim of superior democratic effectiveness originated in an empirical observation by David Lake (1992) that democracies win the wars they fight more often than do nondemocracies. Two classes of explanation have emerged to account for this observation.

The first credits a selection effect stemming from a combination of the openness of democratic governance and the political vulnerability of democratic leaders. Openness permits a freer airing of arguments and thus superior decision making; the domestic political vulnerability of democratic leaders means they fear electoral sanction in the event of policy failure. Taken together, these make democracies less prone to start wars they cannot win: superior counsel offers leaders sound advice on their military prospects; fear of electoral sanction discourages military risk taking when that advice indicates weak prospects. The net result is held to be that democracies are more selective in the wars they fight than are nondemocracies, explaining their higher winning percentage (Reiter and Stam 2002; Bueno de Mesquita and Siverson 1995; Downs and Rocke 1994; Snyder 1991; Van Evera 1994; Reiter 1995; Goemans 2000).[8]

The second explanation holds that democracies fight more effectively in any given war. A number of reasons have been advanced in defense of this claim. Many, for example, argue that democratic governance promotes economic growth, enabling democracies to out produce nondemocratic opponents and overwhelm them by force of numbers. The openness of democratic decision making is said to promote not just better choices for war or peace but better strategic direction for the conduct of war once engaged. Democracies are also held to promote individual initiative and leadership

the particular technologies noted here: see, for example, Lynn Jones (1995, 666). (On the relationship between regime type and weapon holdings, see Reiter and Stam 1998.)

8. This argument is usually focused on the outcomes of wars rather than battles. Yet the same selection logic would apply to battlefield performance: given variance in likely battlefield outcomes across potential contests, one would expect democracies to participate disproportionately in wars wherein they would enjoy superior equipment, greater numbers, superior tactics, or any other combination of factors conducive to victory in battle. This might be especially true of wars with democratic initiators, but even democratic defenders would still enjoy sufficient freedom of choice for selection bias to operate. Targets of aggression must still choose between concession and violent resistance, and the selection argument's logic would imply that democracies would choose unsuccessful resistance less often than nondemocracies.

skills. These traits conduce to battlefield effectiveness in themselves and enable democracies to employ militarily superior maneuver strategies unavailable to more rigid nondemocratic soldiers. Governing by the will of the governed, democracies enjoy popular allegiance that nondemocracies do not; soldiers with a greater personal stake in the conflict are said to be likely to fight harder. Democracies tend to treat captives leniently and to observe less punitive policies in occupied territories, making their opponents more willing to surrender without fighting to the last cartridge (Reiter and Stam 1998, 1997, 2002; Stam 1996).[9] Taken together, these arguments imply that democracies should field larger militaries; make better strategic choices; and employ their forces with greater flexibility, initiative, and combat motivation; and that their nondemocratic opponents should give up more quickly. Ceteris paribus, these effects should cumulate into superior military performance in battle.

> *Hypothesis 3:* Battle outcomes for democracies should be superior to battle outcomes for nondemocracies.

HUMAN CAPITAL

Although regime type has been the most discussed nonmaterial contributor to date, many other possibilities exist. Human capital, for example, refers to the skills, health, literacy rates, and educational attainment of the civil population from which soldiers are drawn. It is often assumed that states with limited human capital will have difficulty fielding militaries that can operate sophisticated weaponry or implement complex tactics (Biddle and Zirkle 1996). By this logic, illiterate mechanics will have great difficulty maintaining high-performance engines; troops with no meaningful formal education will find it harder to draft or carry out instructions for moving thousands of soldiers over multiple routes to converge on a distant point at the same moment. Whereas developed states such as Britain or the United States will enjoy a pool of educated, able-bodied people large enough to satisfy such demands for both the military and the civil economy, such skills are far scarcer in developing countries, which may have greater problems in finding enough talent to fill all pressing needs. In states such as Liberia, for example, there may simply not be enough literate, trainable people to field a mass military capable of absorbing complicated technology effectively. Hence, one might expect to see stronger military performance from states with better human capital and vice versa.

> *Hypothesis 4:* Battle outcomes for states with strong human capital should be superior to those for states with weak human capital.

9. See also Levi's (1997) discussion of consent in democracies, Schultz and Weingast's (2003) argument about democracies' ability to finance long wars, and Reiter and Stam's (1998) hypothesis that democracies will be more likely to use new technologies.

CIVIL-MILITARY COMITY

Conflictual civil-military relations could interfere with battlefield effectiveness in a variety of ways. At the highest levels, civil-military conflict can interfere with the smooth functioning of senior policy-making councils and thereby undermine national strategy. Sound grand strategy requires that military considerations be integrated with nonmilitary concerns involving diplomacy, economic policy, and domestic politics (Kennedy 1991; Liddell Hart 1954). To bring such disparate elements together requires close collaboration and frank, honest exchanges between civilian and military leaders. Friction, distrust, dislike, or simply unfamiliarity between the civil leadership and senior officers can impede such collaboration and result in poorly formulated strategy and military policy (Kennedy 1988; Feaver 2003; Feaver and Kohn 2001).[10] This in turn can undermine battlefield performance: strategic or policy choices that leave an army with outdated or insufficient arms, commit it to battle in an unpopular cause, or compel it to fight at prohibitive odds against a coalition of enemies obviously make battlefield success less likely.

Civil-military conflict can also interfere with the officer corps' military proficiency per se. In states where the military poses a threat of political violence against the regime, for example, civilian leaders often adopt self-defensive measures that interfere with the effective conduct of war. Such interventions can include frequent rotation of commanders and purges of the officer corps, restriction of enlisted service time, suppression of horizontal communications within the military hierarchy, divided lines of command, isolation from foreign sources of expertise or training, exploitation of ethnic divisions in officer selection or combat unit organization, surveillance of military personnel, promotion based on political loyalty rather than military ability, or execution of suspected dissident officers (Huntington 1957, 82; Perlmutter and Bennett 1980, 205-8; Cohen 1986, 168; Kier 1997). Such techniques can be effective barriers to coups d'état, but they systematically discourage soldiers from focusing on disinterested technical expertise, and they make such expertise hard to obtain for those few who seek it anyway (Biddle and Zirkle 1996; Brooks 1998; Kier 1997; Pollack 1996). One might thus expect highly conflictual civil-military relations to reduce a state's military effectiveness.

> *Hypothesis 5:* Battle outcomes for states with radically conflictual civil-military relations should be inferior to those for states with more harmonious civil-military relations.

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10. Some level of civil-military friction is inevitable, given the inherently very different nature of political and military careers (Cohen 2001, 429-33). Moreover, comity obtained via either side's abdication to the other's preferences is hardly conducive to effectiveness. The best case is probably creative tension within what Eliot Cohen calls an "unequal dialogue" between politicians and engaged, but subordinate, officers rather than harmony through self-censorship on either side (Cohen 2001, 429-33; Cohen 2002). Radical conflict, on the other hand, is crippling, producing decisions neither civilians nor officers would choose if left to their own devices. Below, we focus on coups d'état as our measure of civil-military comity to restrict ourselves to this radically conflictual civil-military case as distinct from the normal tensions that arguably enhance, rather than detract from, effectiveness.

CULTURE

It may also be that some cultural traits affect military performance. Many have argued that Arab cultures, for example, encourage rigidly hierarchical organizational structures and extreme deference to authority. Status is associated with distance from one's subordinates, and hands-on mastery of technical detail by superior officers is discouraged. This tends to interfere with honest assessment of problems and promotes artificiality in training, because mistakes are too rarely acknowledged and thus too rarely rectified. It limits officers' knowledge of the technical requirements for maintaining and employing their equipment and constrains flexibility and small-unit initiative (Pollack 1996; Hinkle et al. 1999).[11] More broadly, it is plausible to suppose that variations in national culture could contribute to differences in battlefield effectiveness across states.[12]

Hypothesis 6: Battle outcomes should vary systematically with cultural characteristics.

DATA

The analyses below use three primary data sources to assess these hypotheses: the Banks (1976), Polity III (Jaggers and Gurr 1996), and CDB90 (Dupuy 1984) data sets. The Banks and Polity III data are widely used in empirical analyses of international politics. CDB90, by contrast, although it has appeared in the democratic effectiveness literature (Reiter and Stam 1998, 2002), is both less familiar and subject to some important shortcomings that required modification before its use below and warrant a brief prefatory discussion.

CDB90 was compiled for the U.S. Army by the private Historical Evaluation and Research Organization (HERO). The modified version used here covers 381 battles fought since 1900 and provides a variety of information on troop strength, weapon counts, and casualties, together with subjective assessments of variables such as "surprise," "morale," and "logistics."[13] Each observation represents a battle, with attacker and defender attributes as variables.

Two primary modifications were undertaken. First, the data set contained a significant number of double counts, where single battles were recorded once for the largest formations engaged and then again for each of the component subunits, with the result that the same combat experience appeared multiple times in the data set. A 1997 review by the Institute for Defense Analyses (IDA) (Hamner 1997) found that 48 of the 419 20th-century data points examined (that is, about 11%) were double counted.

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11. This is just one of several approaches that suggest that culture could influence military effectiveness. Granato, Inglehart, and Leblang's (1996) findings linking cultural traits to economic growth suggest, for example, an indirect link between culture and military effectiveness.

12. For an alternative view, see Desch (1999).

13. For a more detailed description, see Helmbold (1990). Wars represented in CDB90 include the Russo-Japanese War, the Balkan Wars, the two World Wars, the Russo-Polish War, the Spanish Civil War, the 1948, 1956, 1967, 1973, and 1982 Arab-Israeli wars, the Korean War, and the Vietnam War. For a discussion of the selection of battles in CDB90 and the implications of this for findings using these data, see Biddle (forthcoming, chap. 8).

The authors therefore removed the redundant double counts from the original, Army-supplied version of the data set.[14] Because only the 20th-century component of the data set had been reviewed by IDA, only the 20th-century data were used in the analyses below.

Second, the authors conducted spot checks of individual data items to assess the reliability of the non-double-counted data. Where the checks revealed significant inaccuracies, these were corrected. In all, 33 CDB90 codings were checked against accounts in the pertinent official histories; of these, 6 were found to differ from the values in the official record by margins of 20% or more and were corrected to reflect the official values.[15]

The resulting data are not perfect—coding errors doubtless remain, HERO's selection rationale for the battles included is opaque, and the data set provides little information on the technical sophistication of the weapons it treats (weapons data are limited to the numbers of systems of each major type but not their particular makes, models, or performance). On the other hand, CDB90 offers the only meaningful data available on the outcomes of battles, as distinct from wars; it has already played a role in the democratic effectiveness literature; and the great majority of the spot-checked values were in reasonable consistency with the official historical record.

Moreover, and perhaps most important, there is no reason to expect any remaining errors to correlate with any of the variables in the analyses below. Measurement error, although surely present, will thus tend to increase estimated standard errors and reduce the odds of finding statistically significant results—but there is no reason to expect it to bias the estimated coefficients. This in turn increases our likelihood of missing true relationships but does not increase the odds of finding spurious relationships. The evidentiary importance of null findings is thus lower, but the substantive importance of any statistically significant relationships that may emerge in spite of the noise is unaffected. All large-n data sets are subject to measurement error; CDB90 may (or may not) be noisier than most data in common use in international relations, but if so, this does not make positive findings derived from these data unsound. It makes them less likely, but it is not a challenge to their validity if found, absent evidence of correlation between the measurement errors and the variables considered. Claims that measurement error per se undermines any findings from CDB90 data (e.g., Desch 2002) are

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14. Ten of the 48 were retained as the single data points for the respective battles, leaving an n of 381. Missing data elements reduce the usable n for the regression analyses below to 223.

15. CDB90 has been extensively reviewed and extensively modified to remove errors identified in those reviews. The initial version was reviewed in 1984 by the U.S. Army War College Military History Institute, the U.S. Army Center for Military History, the U.S. Army Combat Studies Institute, and the U.S. Military Academy's Department of History; eight randomly selected battles with 159 codings were checked. Of these 159 values, 67% were found to be in error and 18% were judged "questionable." The Army subsequently revised the data set in 1986 and again in 1987 to correct known errors, but the extent of remaining mistakes cannot be known. The data set's size makes exhaustive review prohibitive. Moreover, historians are rarely explicit with respect to their counting rules or variable operationalizations; hence, different accounts often provide different values for reasons no more profound than tacit differences in definitions. The authors' spot checks described above thus looked for gross inconsistencies rather than minor discrepancies of a kind easily attributable to differences in counting rules. On the reviews of CDB90, see CHASE (1986); Dupuy (1984); Mearsheimer (1989, 65-67); Epstein (1989, 104-6); Desch (2002). References used in the authors' spot checks were Edmonds (1935); Ellis (1962); Blumenson (1961); Playfair (1960); Glantz (1991).

thus unsustainable.[16] Although—like all large-*n* IR datasets—CDB90 is imperfect, it is thus a reasonable point of departure for empirical analysis.

OPERATIONALIZATIONS

For the hypotheses above, the key variables are battle outcomes; numerical preponderance; tank, ground-attack aircraft, and artillery prevalence; democracy (that is, regime type); human capital; civil-military relations; and culture. We operationalize these as follows.

- *Battle outcomes:* We use the loss exchange ratio (LER)—attacker casualties divided by defender casualties—as our dependent variable. LER provides an objectively measurable, continuous-variable index of outcomes that is neither sensitive to the scale of the action fought nor dependent on subjective codings of "victory" or "defeat."[17] LER, by contrast, with "win/lose" outcome characterizations, also permits the magnitude of the victory or defeat to be distinguished: a categorical "win/lose" or "win/lose/draw" characterization lumps together close calls and routs as undifferentiated "wins," in the process losing much of the real information in the data set. LER preserves the distinction and thus retains much more of the actual variance in the data. The higher the LER, the more the outcome favored the defender and vice versa.
- *Numerical preponderance:* attacker's fraction of the two sides' total troop strength.[18]
- *Tank prevalence:* total tanks engaged (on both sides) divided by total troops engaged (on both sides).

16. In other words, the relative inefficiency of statistical models based on noisy data does not invalidate statistically significant findings. Bias would; inefficiency does not. Inefficiency deriving from measurement error reduces the odds of finding true relationships—it does not increase the odds of finding spurious relationships (in fact, it reduces them). Inefficiency could thus be a sound basis for reducing confidence in null findings drawn from CDB90 (or any other data set with meaningful measurement error), but it is not a sound basis for rejecting findings that attain customary levels of statistical significance—nor is it a sound basis for rejecting findings of relative importance based on the comparison of null with statistically significant results (cf. Desch 2002).

17. CDB90 lacks a consistent unit of analysis: entries range from minor tactical actions at the company and battalion level to theater offensives by multiple army groups. This poses several potential hazards. In particular, many military outcome measures vary with the size of the units involved regardless of technology, preponderance, or force employment. Consider casualty rates. Companies and battalions consist almost entirely of combat soldiers and are often exposed to fire in their entirety in the course of a single action; if caught in the open, they can be annihilated to the last soldier. Army groups, by contrast, contain large numbers of support personnel not normally exposed to hostile fire. Moreover, few of their component battalions are typically in intense combat at the same time; many are either in reserve or located away from the critical point at any given moment. Daily casualty rates as a fraction of total troops, thus, rarely exceed a few percent a day for large formations even in major battles, whereas they can easily exceed 50% for small subunits, due to nothing more profound than the size of the unit and its relative proportion of active shooters to other soldiers. (For a detailed discussion, see Kuhn 1991.) Rates of advance and total ground gain or loss also vary with the scale of the action: even a radically successful battalion-scale action will rarely involve an advance of more than a few tens of kilometers, because such battalions will ordinarily be replaced with others to continue the advance. Army groups, on the other hand, can sustain advances of hundreds to thousands of kilometers without replacement. Measures of changing territorial control can thus vary enormously as a function of nothing more than the size of the units observed. For such measures (casualty rates or territorial change), statistical analyses must therefore control explicitly for the level of analysis. Loss exchange ratio (LER), by contrast, controls naturally for the level of analysis and thus requires no additional control variables.

18. The CDB90 variables used to calculate this are "amilpert" and "dmilpert," which are the total military personnel, not the initial (which is labeled "amilperi" and "dmilperi").

- *Ground-attack aircraft prevalence:* total ground attack aircraft sorties (on both sides) divided by total troops engaged (on both sides).
- *Artillery prevalence:* total artillery tubes engaged (on both sides) divided by total troops engaged (on both sides).
- *Democracy:* attacker's fraction of the total democracy score for the combatants, measured by the respective Polity III "DEMOC" variable values in the year prior to the outbreak of war.[19] The higher the value, the more democratic was the attacker relative to the defender, and vice versa.
- *Human capital:* attacker's fraction of the sum of attacker and defender states' years of primary and secondary education per capita in the year prior to the outbreak of war as reported in the Banks data. The higher the value, the greater the attacker's relative edge in human capital.
- *Civil-military relations:* two dummy variables are employed. The first, "civmil favoring attacker," takes a value of 1 if the defender had at least one more coup d'état in the 5 years prior to the war than the attacker as reported in the Banks data. The second, "civmil favoring defender," takes a value of 1 if the attacker had at least one more coup d'état in the 5 years prior to the war than the defender.[20] The "no advantage to either" category is dropped to avoid collinearity.
- *Culture:* a series of dummy variables representing combatant states' primary religious affiliations as rough cultural indicators are employed. "PC" denotes a state in which the most common religious affiliation is Protestant or Catholic; "BU," Buddhist, Confucian, Shintoist, or a combination thereof; "MU," Muslim; "JE," Jewish; and "OR," Orthodox (Eastern Orthodox, Russian Orthodox, etc.). Each dummy represents a pair of states—the first two letters identify the attacker's culture, the second give the defender's.[21] Data were drawn from the CIA (2001) *World Factbook*.

19. The Banks data lack annual values for the World Wars, stopping in the year prior to their outbreak and resuming the year after their conclusions. Because the causal logic for the role of regime type, human capital, civil-military relations, and culture rests on long-term effects rather than wartime fluctuation, we thus use the values for the respective states immediately prior to the war. We retain thereby much more of the available data without doing violence to the respective hypotheses' logic. The King et al. (2001) procedure for multiple random imputation was considered as an alternative but rejected because the missing data are not randomly absent (the largest wars have no data).

20. Coups are infrequent events, even for coup-prone states. By considering a 5-year interval, we thus capture more of the relevant variance than would a single-year measure, which would tend to report many coup-prone states as harmonious because of the artifact of their having been calm in the particular year prior to the war's outbreak. The interval cannot be so long, however, as to conceal potential long-term change in a state's civil-military relationship between wars—hence the value of 5 years, as opposed to 10 or 20. A dummy specification is used instead of the "attacker's fraction of total" form used for other variables because, in most cases, neither attacker nor defender suffered coups in the reference interval, yielding mathematically undefined expressions.

21. Of course, primary religious affiliation is an extremely crude proxy for culture, which is a subtle, complex, and multidimensional concept incorporating a great deal more than just religious practice. At the same time, religious affiliation is clearly associated with common intuitive understandings of culture; it is likely to correlate (albeit imperfectly) with more holistic formulations; and it is transparent, objectively measurable, and hence free of subjective coding bias. Because the literature has not yet produced a more satisfactory measure, we thus use it here as a point of departure, but its shortcomings must be taken carefully into account (see, e.g., the discussion in Desch 1999 and in the Data Analysis section below). We hope our findings will inspire closer attention to this variable in the future and development of better measures. Note that the regressions below exclude culture dummies for which fewer than two data points were available in the CDB90 data, or for which the obverse pair is included (PCBU, for example, is included but BUPC is not). Models including both obverse pairs were tested but provided weaker fits.

DATA ANALYSIS

Table 1 presents the statistical results, using log(LER) as the dependent variable.[22] Three models are presented.

The first considers variables addressed in the democratic effectiveness literature to date: democracy and a series of controls for numerical preponderance and technology. The results are very similar to those reported in this literature to date. Greater democracy is associated with greater effectiveness (the more democratic the attacker relative to the defender, the lower the LER and thus the fewer attackers killed per defender killed), and the effect is significant at the .05 level. Democracy is also more important than the balance of military materiel: none of the controls for preponderance or technology are significant at any customary level.[23]

The second model provides a more detailed analysis of unit-level effects by adding human capital, civil-military relations, and culture. The result is a much stronger fit overall: the *F* statistic increases by an order of magnitude from 1.87 to 18.6, and r^2 climbs from .04 to .42. The effects of the new unit-level variables are uniformly as expected: attackers with superior human capital and less conflictual civil-military relations enjoy major effectiveness increases (that is, lower LERs and thus fewer attackers killed per defender killed); cultural variations can induce statistically significant and substantively important effects (the difference between a culturally Protestant/Catholic and a Buddhist/Confucian/Shinto defender in model 2, for example, has more than 5 times the effect on LER than the difference between a democratic and an autocratic attacker has in model 1).[24]

The democracy variable's effect, however, reverses when other unit-level variables are considered: the coefficient switches from negative in model 1 to positive in model 2. That is, whereas democracy enhances effectiveness when considered alone in model 1, it degrades effectiveness when considered in context in model 2. (In model 2, the more democratic the attacker relative to the defender, the more attackers killed per defender killed.) This effect is highly significant: the *t* statistic for the democracy coefficient rises from 2.23 in model 1 to 2.59 in model 2, implying significance at the .05 level for the negative coefficient in model 1 but the .01 level for the positive coefficient in model 2.

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22. A logarithmic transformation is used to reduce the skew in LER and produce a normally distributed dependent variable. Ordinary least squares (OLS) regression requires normally distributed variables, and although the LER is a strictly positive ratio and thus not normally distributed, log(LER) is normally distributed with a zero mean.

23. Compare Desch (2002) and Rotte and Schmidt (2003), who find a greater role for material preponderance but who use a dichotomous "win/lose" dependent variable to assess effectiveness. Desch and Rotte and Schmidt also use CDB90's subjective codings for intangibles, such as "morale" or "surprise" (which we exclude in favor of the objectively measured values in the dataset, as do most official Army analyses using these data). They also include more than 200 pre-20th-century data points absent from the data set used here (which incorporates a variety of improvements in the original CDB90 data—see discussion above—but whose coverage is limited to the 20th century). On the disadvantages of "win/lose" outcome characterizations relative to the continuous-variable LER dependent variable used here, see the discussion in the Operationalizations section above.

24. Assuming a purely autocratic defender in model 1 and a culturally Protestant/Catholic attacker in model 2.

TABLE 1
Ordinary Least Squares (OLS) Regression Results for log(LER)

	Model 1	Model 2	Model 3
Intercept	–0.126	0.191	0.017
	(0.229)	(0.412)	(0.308)
Num. preponderance	0.357	0.514	0.152
	(0.294)	(0.333)	(0.308)
Tank prevalence	–6.356	1.683	–8.734
	(6.961)	(5.655)	(7.029)
Aircraft prevalence	–4.673	3.223	–4.368
	(6.414)	(5.065)	(5.671)
Artillery prevalence	0.782	10.702*	1.918
	(6.764)	(4.721)	(5.576)
Democracy	–0.224*	0.362**	–0.107
	(0.101)	(0.140)	(0.123)
Human capital		–1.126*	–0.142
		(0.492)	(0.411)
Civmil favoring defender		0.399*	0.531**
		(0.173)	(0.155)
Civmil favoring attacker		–0.148	–0.294*
		(0.115)	(0.132)
PCPC		–0.912	
		(0.161)	
PCBU		–1.184**	
		(0.174)	
PCMU		0.701	
		(0.601)	
ORMU		0.415	
		(0.258)	
ORBU		0.223	
		(0.334)	
JEMU		–0.779**	
		(0.206)	
ORPC		0.157	
		(0.232)	
r^2	.041	.421	.078
F	1.87	18.63**	6.68**
n	223	223	223

NOTE: Entries are OLS regression coefficients with standard errors in parenthesis below the estimates. All results employ robust standard errors; hence, adjusted r^2 is unavailable.
*$p < .05$. **$p < .01$.

In these results, the apparently beneficial effect of democracy, when considered alone, is thus attributable chiefly to a series of other unit-level variables that correlate positively with democracy. In the CDB90/Banks/Polity data, democracies tend to have stronger human capital, less conflictual civil-military relations, and predominantly Western cultural characteristics relative to nondemocracies. Each of these traits has

been hypothesized to have military advantages in the effectiveness literature. When their effects are controlled explicitly, the apparent effectiveness advantage of democracy is seen to reside chiefly in these underlying determinants. In fact, the marginal, ceteris paribus, battlefield effect of democratic political organization per se (outside its possible influence on human capital, civil-military relations, and culture) is adverse for any given state with any given combination of domestic social and cultural traits.[25]

Model 3 adds perspective on the relative importance of culture, civil-military relations, and human capital. The importance of culture per se is hard to assess in model 2, because it is represented by multiple variables, some of which are significant and others not. Model 3 thus drops the entire group from the specification in model 2. The result is a substantially inferior fit: r^2 drops from .421 to .078; the F statistic falls from 18.63 to 6.68.[26] Civil-military relations remain significant (more so, in fact, than in model 2), but artillery prevalence, human capital, and democracy all lose significance. Although insignificant here, the democracy variable also reverts to the negative sign seen in model 1, when culture is dropped. These results suggest that although culture is not the only important unit-level variable, it is a particularly important one for these data. Moreover, it is an essential consideration for assessing democracy's effects on battlefield outcomes. If ignored, the apparent effect of democracy reverses.

Although the culture dummies are thus very influential here, caution must be exercised in interpreting them. In the CDB90 data, the two most important dummies, PCBU and JEMU, each covers a restricted range of states: PCBU includes only two Asian states (Japan and North Korea) and a single Western power, America, in two wars; JEMU includes four Arab states (Egypt, Syria, Jordan, and Iraq) and an Arab substate actor (the Palestinian Liberation Army), but only a single Jewish state (Israel). JEMU covers a span of 34 years (1948-82), but PCBU spans only 8 (1943-51). Although it is clear that something about the particular identity of these states in these wars—beyond just their military materiel, regime type, human capital, or civil-military relations—is driving a major difference in battlefield effectiveness, it is less certain that culture per se is the best characterization of the underlying causal agent. There may be other common traits linking these states, to which culture or majority religious affiliation is epiphenomenal. Pending further analysis to consider a wider range of states with Western, Asian, and Arab cultures, perhaps the safest conclusion here is that some unique unit-level trait (or traits) of the states grouped under the PCBU and JEMU dummies here seems very important for battlefield success, and it is clear

25. Note, however, that not all the correlates of democracy included in model 2 would necessarily promote increased effectiveness. Many have argued that democracies underspend on defense and thus field smaller militaries than would be optimal given their international position (Reiter and Stam 2002). By including explicit measures of the military wherewithal fielded by the respective sides, we thus control both for effectiveness-enhancing and for effectiveness-reducing correlates of democracy. Of course, like most of the correlates of democracy modeled here, analysts could differ on the effects of democracy on military wherewithal. One could also argue that democracy, by promoting wealth, would thereby promote greater fielded military wherewithal by enabling democracies to afford this (Reiter and Stam 2002). Here, too, the deductive logic on democracy's effects for other unit-level traits is indeterminate, and conclusive empirical analysis would be required to resolve the question.

26. Although our use of robust standard errors precludes adjusted r^2, analogous models computed without robust standard errors produce adjusted r^2 values of .019, .38, and .043 for models 1, 2, and 3, respectively.

that this trait is not among those controlled for explicitly among the nonculture variables included here. Culture is an intuitively plausible candidate that is consistent with the results reported above, but we cannot conclusively identify it as the key causal agent per se without evaluating a wider range of states with these cultural attributes. For now, it is a promising candidate but not a conclusive finding.

These results are thus inconsistent with the materialist theories embodied in hypotheses 1 and 2. In none of the three models considered were the materiel variables statistically significant (with the sole exception of artillery prevalence in model 2). In fact, the signs were opposite the predicted values for numerical preponderance in all three models and for tank and artillery prevalence in model 2. The unit-level theories in hypotheses 4, 5, and 6, on the other hand, are generally supported. In model 2, human capital and civil-military relations are both statistically significant and take the expected signs (with the exception of "civmil favoring attacker," which takes the predicted sign, but falls short of significance). The block of culture dummies contains two highly significant and substantively important variables in PCBU and JEMU, and the block as a whole accounts for much of the model's explanatory power overall.

The implications for the democratic effectiveness thesis embodied in hypothesis 3, by contrast, depend on one's view of the causal relationship between democracy and other unit-level variables. Some argue that democracy promotes wealth and accumulation of human capital (for example, Lake and Baum 2001). Others see democracy as conducive to nonviolent civil-military relations (for example, Feng 1997). If so, then our findings can be seen as supportive of democracy as an effectiveness enhancer: democracy would promote several militarily critical unit-level traits and thus would promote military effectiveness in turn.[27] Its residual effects are effectiveness-degrading, as implied by its positive coefficient in model 2, but the aggregate of its helpful effects (via the intervening variables of human capital, civil-military relations, and culture) and its unhelpful residual influence are clearly helpful on balance, as implied by its negative coefficient in model 1.

On the other hand, critics have argued that causation, if present at all, runs in the opposite direction, with wealth and civil-military harmony enabling democracy rather than the other way around (for example, Desch 2002). The literature on democracy and culture, although thin, mostly considers the causal effects of culture for democracy rather than the opposite (Bollen 1979; Bollen and Jackman 1985). These views would cast our findings in a very different light. When these other unit-level variables are considered, democracy's effects are at once opposite the democratic effectiveness school's prediction and highly significant. If these other unit-level variables are not in turn caused by democracy, then the statistical results here are strongly at odds with the democratic effectiveness thesis.

The larger question of the causal interrelationships between wealth, civil-military relations, culture, and regime type are thus central to an accurate understanding of democracy's role in military power. To resolve these issues, however, will require a program of empirical analysis well beyond our scope here—deductive logic alone can support either one of two diametrically opposed conclusions. This in turn, however,

27. See also Stam (1996) on this point.

means that the validity of the democratic effectiveness thesis per se must be considered unresolved in light of our findings. Either way, however, our results shed additional light on the thesis, and they highlight the interrelationship between the democratic effectiveness thesis and a series of related debates on the role of democracy in other political and social phenomena.

CONCLUSIONS AND IMPLICATIONS

Our central finding is that the battlefield effectiveness bonus previously attributed to democracy is a product of many democracies' superior human capital, civil-military relations, and cultural background. When we control for these other unit-level traits, the marginal effect of democratic political organization per se is actually to reduce effectiveness, not increase it.

The implications of this depend on larger questions of democracy theory. If the other unit-level traits considered here are effects of democratic governance, then our findings strengthen the democratic effectiveness thesis by explicating its causal mechanism. If not, then our findings challenge it. The latter conclusion would be more consistent with an older literature that considered democracy normatively precious but militarily disadvantageous (Morgenthau 1948/1978; Huntington 1957).

Of course, democratic effectiveness theorists posit two potential causal mechanisms—selection and strength. Even if the independent variables considered here are causally unrelated, the analysis above would challenge directly only the strength mechanism: ceteris paribus, democratic political organization per se does not increase battlefield prowess in these data.[28] The same results could, in principle, be consistent with the selection mechanism. Although democracy might not conduce to superior per capita proficiency, perhaps democracies select for wars in which other advantages (resolve, perhaps, or gross material preponderance) outweigh their battlefield disadvantages.[29] But it is also possible that the entire relationship between democracy and victory could be epiphenomenal. If democracies' battlefield success is attributable not to democracy but instead to other unit-level traits, then perhaps the same could be true of democracies' overall success in war, rather than just in battles. Democracies do in fact win wars more often, but is this because they are democracies? Or is it because this particular collection of states also happens, more often than not, to enjoy stronger human capital, better civil-military relations, or cultural traits that conduce to superior war fighting?

28. Note, however, that many of the intermediate variables brought to light by the democratic effectiveness literature—such as initiative or superior doctrine and tactics—are very likely to be critical determinants of battlefield performance. Moreover, these variables clearly correlate positively with democracy. Democracy, however, may not be their unique wellspring, and democratic political organization per se is thus unlikely to be a sufficient predictor of their incidence. That said, the democratic effectiveness literature has done great service in focusing analytical attention on these phenomena.

29. The weak performance of material preponderance here suggests that the scope for decisive selection effects might be more limited than some would suppose: selecting for wars in which one is numerically superior need not provide decisive military advantage if the effects of numerical superiority on battlefield outcomes is itself weak. Gross numerical superiority could still allow one to outlast an opponent that one could not defeat on the battlefield, although this is an expensive approach to victory in war.

Either way—and whether our independent variables are causally interrelated or not—our findings pose important implications for assessing states whose internal attributes differ from the usual pattern of rich, developed, stable, Western democracies as opposed to poor, underdeveloped, unstable, non-Western autocracies. What, for example, should one make of states, such as India, Brazil, Ivory Coast, Ethiopia, postwar Japan, or interwar France, all democracies but none following the entire Western pattern? How should one assess states, such as Nazi Germany, Fascist Italy, North Vietnam, or contemporary China or Singapore, none of which are democratic but all of which enjoy one or more of the other unit-level attributes of military effectiveness identified above? Such states are not unimportant, either statistically or strategically. Statistically, the correlations between key independent variables here, although positive, are far from perfect: the correlation coefficients for democracy and human capital, for example, are .52 and .50 for attackers and defenders, respectively. So there are clearly many exceptions to the standard pattern. Strategically, many of the states making up these exceptions are of special interest for U.S. defense policy and modern military history. Nazi Germany was hardly an unimportant historical case. North Vietnam and interwar France played major roles in 20th-century international politics. Contemporary China is considered by many to be a rising power with the potential to challenge important American military and foreign policy interests. Many see India and Brazil as great powers of the future. A theory of military effectiveness that mischaracterizes such states by overemphasizing their regime type per se is a theory with important shortcomings.

The analysis above has a variety of other implications that reach beyond the democratic effectiveness thesis per se. First, it reinforces a number of recent findings on the importance of nonmaterial, unit-level variables for military effectiveness.[30] An important contribution of the democratic effectiveness literature has been to help redirect attention away from the systemic, materialist variables that have dominated both IR scholarship and defense policy analysis. Whatever one's position on the causal role of democracy, we clearly share with democratic effectiveness theorists the finding that military capability rests centrally on the internal, nonmaterial characteristics of states. The results above suggest that orthodox systemic, materialist analyses thus court major error—with potentially serious consequences both for IR theory and for U.S. defense policy and national strategy.

Second, our findings suggest a number of important directions for further research. In general, the role of nonmaterial, unit-level variables in military effectiveness has been understudied. Although a start has been made on a more satisfactory approach, much remains to be done.

More specifically, the results here suggest that the role of culture in military performance is a particularly important candidate for theoretical attention. The significance of the cultural variables in our regression results is suggestive of an important relationship, but the restricted range of national variance in the CDB90 data counsels caution

30. See, for example, Stam (1996); Cohen (2002); Rosen (1995, 1996); Johnson (1995a, 1995b); Pollack (1996); Scobell (2002); Biddle (1996, 1998, 2001, forthcoming); Biddle and Zirkle (1996); Biddle, Hinkle, and Fischerkeller (2002).

in interpreting the results. Are these findings attributable to culture per se or to other unit-level properties of the particular states grouped under the PCBU and JEMU dummies? And if other examples of these cultural groupings do perform similarly on the battlefield, then why? What specific causal mechanisms link cultural attributes to military performance? What aspects of culture are most important? The analysis above uses primary religious affiliation as a simple indicator of gross cultural differences that extend beyond religious practice per se, but a much more discriminating measure of this inherently multidimensional variable is needed.

Assessment of culture, in turn, is central for accurate projections of the military potential of specific states. China, for example, is rapidly improving its military equipment and may improve its human capital base substantially as it develops economically. On the other hand, its military leadership has been deeply involved in civil governance—especially through its engagement in the civil economy—and its military and civil cultures are very different from America's (Johnson 1995a, 1995b; Scobell 2002). North Korea, also, is culturally very different from the West. Neither is a democracy, but both differ in important ways from other nondemocratic states. If democratic political organization is less important than other unit-level traits, then the net consequences for Chinese or North Korean military potential are still opaque pending a deeper understanding of the role of culture. It is clear that such nonmaterial factors matter centrally for military capability; what is less clear is how to bring variables such as culture into systematic analysis. Like the literature on unit-level determinants of capability generally, our understanding of culture and war is underdeveloped. The results here suggest that this gap is important and warrants priority attention.

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