Mad CoW: A Reply to Gibler and Miller
Full Version*

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In *Divided Armies*, I argue that inequality within armies ("military inequality") has shaped their battlefield performance in conventional wars since 1800. Gibler and Miller (2021) are unpersuaded. They raise a flurry of concerns about the crossnational evidence and one statistical analysis in the book's Chapter 4. In particular, they maintain that Project Mars, the book's dataset, offers nothing new compared to the Correlates of War (CoW). I find their criticisms misplaced. I use their own statistical models for reanalyzing Project Mars to demonstrate that military inequality is an important driver of battlefield performance across six different measures in all types of CoW wars over the past 200 years. We should build, not bury, a research program that further explores the relationship between inequality and political violence.

*For helpful feedback, I thank Jeff Friedman, Kosuke Imai, Ben Valentino, Yang-Yang Zhou, and Yuri Zhukov. The latest version of Project Mars (v.1.1), codebook (Lyall, 2020*b*), replication files, and supporting documentation for these analyses have been archived on the Project Mars Dataverse site: `https://dataverse.harvard.edu/dataset.xhtml?persistentId=doi:10.7910/DVN/DUO7IE`.

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1 Introduction

Divided Armies argues that we have overlooked a key determinant of battlefield performance in conventional wars since 1800: inequality (Lyall, 2020*a*). Specifically, prewar ethnic inequalities within armies help shape wartime performance by undercutting combat motivation, sowing seeds of distrust within units, and forcing commanders to adopt repressive countermeasures that further erode combat power. The argument is tested using a mixed-methods research design that incorporates a natural experiment, crossnational evidence from 250 wars (1800-2011), and three paired historical comparisons spanning 150 years. A microlevel study of four Soviet Rifle Divisions on the Eastern Front in 1941 drawn from declassified archival and personnel records rounds out the evidence. Military inequality, it turns out, sabotages battlefield performance. As inequality rises, so too does the likelihood of experiencing lopsided casualties, mass desertion and defection, and the use of violence to force one's own soldiers to fight.

Ignoring five of six empirical chapters, Gibler and Miller issue a scattershot review of the book's crossnational evidence in Chapter 4 (Gibler and Miller, 2021). They contend that Project Mars, the book's new dataset of conventional wars, adds little that isn't already in the Correlates of War (CoW) universe; that Project Mars misses an additional 355-450 conventional wars in CoW; that the book's independent variable, the military inequality coefficient (MIC), is flawed; and that MIC explains fractional loss-exchange ratios (FLERs) — a measure I don't actually use — in only two of four CoW war types. They advocate discarding inequality in favor of more traditional explanations of military effectiveness like regime type.

These criticisms are misguided and, to an alarming degree, littered with factual errors. In this short reply, I demonstrate that they have mischaracterized Project Mars' intent, construction, and contributions. Hundreds of relevant wars are not missing from Project Mars. Given tight word limits, I make extensive use of an online appendix, including to rebut their incorrect claims about my independent variable, military inequality. Most importantly, I use the same statistical models, data partition, and periodization from their own reanalysis of Project Mars to demonstrate that military inequality outperforms traditional variables when explaining six different aspects of battlefield performance in all four types of CoW wars since 1800.

2 Project Mars and its Contributions

To test my claims about military inequality, we first needed a dataset of conventional wars. Unfortunately, no off-the-shelf solution existed. For decades, nearly every quantitative study of military effectiveness (and war) has relied on CoW's Inter-State War dataset.[1] It is not, however, an exhaustive list of conventional wars. Indeed, CoW's own coding rules excluded dozens of relevant belligerents — mostly non-Western — and their wars from the Inter-State War dataset. As a result, we were forced to construct a new dataset to capture these missing belligerents and wars. We consulted a wide range of war lists, datasets, and specialized histories to construct our new sample. These datasets included: the CoW Inter-, Intra-, Extra-, and Non-State War datasets; Clodfelter's *Warfare and Armed Conflicts* (2008); Wimmer and Min's war list (2009); Reiter and Stam's (2002) modification to the CoW Inter-State War dataset; the CDB90 list of battles; the UCDP/PRIO Armed Conflict Dataset; and Kalyvas and Balcell's 2010 list of civil wars. Table 1 summarizes how many wars from each source are found in Project Mars. Our "cross-walk" spreadsheet, created in June 2010, details the source(s) for every single Project Mars war; it is archived on Dataverse.[2]

Built by the tireless efforts of 134 coders, Project Mars represents a new dataset of *conventional* wars fought between 1800 and 2011. Nowhere do I claim that Project Mars is a "completely new" dataset of 250 undiscovered wars, as Gibler and Miller charge. Tellingly, they do not provide a single quote from the book making this claim. Yet they devote one-third of their review to a quixotic quest to reverse-engineer Project Mars to show that is "passing off" CoW wars "as its own discoveries." This is, frankly speaking,

[1] See, for example, Singer and Small 1972; Stam 1996; Reiter and Stam 2002; Biddle 2004; Downes 2008; Weeks 2014; Lehmann and Zhukov 2019; Min 2021.

[2] I have updated it by adding the new Interstate War dataset (v.1.1), which appeared after Project Mars was finished. I have also brought the UCDP/PRIO ACD up to its latest version (ver.20.1).

Table 1: Project Mars Cross-Walk With 11 Existing Datasets and War Lists

Source	N in Project Mars	% of Project Mars
COW Datasets		
Inter-State War (v.4.0)	115	34.9%
Extra-State War (v.4.0)	62	18.8%
Intra-State War (v.4.0)	58	17.6%
Non-State War (v.4.0)	24	7.3%
Not in COW	70	21.3%
Additional Datasets		
Clodfelter (2008)	263	79.9%
Wimmer and Min (2009)	208	63.2%
Interstate War Data (v.1.1)	111	33.7%
Reiter and Stam (2002)	77	23.4%
CDB90	48	14.6%
Post-1945 Wars Only		
UCDP/PRIO Armed Conflict Dataset (v.20.1)	58	17.6%
Kalyvas and Balcells (2010)	29	8.8%

Note: There are 329 wars/campaigns and 825 belligerent observations in Project Mars (v.1.1).

bizarre. In addition to the "cross-walk," *Divided Armies* took pains to acknowledge the importance of the CoW universe and, in particular, the dominant Inter-State War dataset. To cite just a few examples, the book's introduction (pp.23-26) and codebook (pp.4-12, "How does Project Mars compare with CoW?") discuss coding differences between the two datasets. All statistical models in Chapter 4 include indicators for CoW "non-states," civil wars (drawn mostly from the Intra-State War dataset), CoW Great Powers, and CoW country codes. Appendices detail which wars are included in the Inter-State War dataset and the 124 belligerents in Project Mars not considered states by CoW. Robustness checks reestimate all models using CoW-only belligerents and Inter-State War-only subsets. Even their own quote from my codebook — "Project Mars considerably expands our coverage of wars and combatants *compared to COW's Inter-State 4.0 dataset"* — accurately reflects its contribution.

Worse, Gibler and Miller make serious mistakes when assigning CoW ids to wars in Project Mars. In particular, they overstate CoW's coverage of Project Mars by engaging in questionable coding decisions. These include: (re)assigning the same CoW war id to multiple wars or to wars not in CoW's own historical summaries; assigning war ids despite the fact that the belligerents involved are different in CoW and Project Mars wars; adding conflicts from the Militarized Interstate Dispute dataset; disregarding nearly all pre-1816 wars; and simply assigning CoW war ids to the wrong war.

Space does not permit a war-by-war discussion of their efforts. Instead, I have archived our "cross-walk" document, which cross-references every Project Mars war with 11 different datasets (including CoW), on our Dataverse page.[3]

Here I simply provide a few examples of different errors made. These are important for both the historical record and for assessing the reliability of their war type coding used in the "reanalysis" of Project Mars.

First, they recycle the same CoW war id to cover separate war/conflicts. For example, they apply CoW war id 576 ("Tungan Rebellion of 1862-1873") to five separate conflicts in Project Mars despite the fact that these wars are separated by years and involved different actors than provided in Sarkees and Wayman (2010). They apply war id 1531 ("First Haiti-Santo Domingo War of 1844-45") to three distinct wars, including the Third Dominican War which was fought in 1849. Similarly, they assign CoW war id 303 ("First Bolivar Expedition of 1817-19") to three separate Project Mars wars, all of which occurred between 1810 and 1815.

Second, they assign CoW war ids to the wrong Project Mars war. For example, they assign war id 359 ("Russian-Kokand War of 1864-65") to Project Mars war id 95, which involved a war between China and Kokand in 1865. Similarly, they assign CoW war id 550 ("Viennese Revolt of 1848") to Project Mars war id 63 ("Austro-Venetian War of

[3] `https://dataverse.harvard.edu/dataset.xhtml?persistentId=doi:10.7910/DVN/DUO7IE`

1849"), which covers the April-October 1849 encirclement, siege, and subsequent fall of the Republic of Venice.

Third, Gibler and Miller assign 12 Militarized Interstate Dispute ids to Project Mars wars. This is an odd move if the point is to demonstrate that these wars are already in one of four CoW datasets. Fourth, they drop 29 wars/campaigns in the pre-1816 as outside CoW's domain. For Gibler and Miller, these cannot be considered "outside" CoW because of its 1816 start date, and are apparently dropped from their reanalysis. Confusingly, they also allow 7 pre-1816 wars to count as "in" one of the four CoW datasets.

Finally, CoW still does not recognize the multi-front treatment of World Wars I and II (as first proposed by Reiter and Stam 1998). I therefore coded all World I and II campaigns and belligerents in Project Mars as also found in CoW even if they lacked a relevant belligerent observation.[4] This somewhat inflates the degree of overlap between Project Mars and CoW.

In fact, Project Mars contains 70 wars without a close (or any) CoW match. Even apparent agreement between the two datasets masks important differences. For example, only 10 of 250 wars in Project Mars had the same start and end years as CoW. A full 75 wars had different start years between the two datasets; 84 had different end dates, with discrepancies in start/end years of ± 5 years common.

They conclude that Project Mars is merely a pale shadow of CoW that "does not constitute data creation." Left unsaid, however, is anything about the data we collected for dozens of new variables. We built new measures for regime type, for army size and recruitment, for the distribution of forces deployed, distance to the battlefield, and at least ten aspects of battlefield performance. Their own "reanalysis" (see below) is impossible without these new data. By contrast, CoW's own Intra-, Extra-, and Non-State War datasets possess no information about the "non-state" belligerents that fought these wars. They are glorified lists of war unsuitable, and unused, for statistical analysis since they lack even basic data about belligerents' political systems, armies, or military power. Despite a fifty year head-start, CoW still hasn't even managed to assign unique ids to these non-

[4]There are 33 belligerent observations over 9 different campaigns in WWI. A further 61 belligerent observations in 20 campaigns comprise Project Mars' coding of WWII.

states.[5] A fair review would at least acknowledge how Project Mars represents a costly investment in data collection that greatly improves existing coverage of conventional wars.

3 Missing Wars?

After chastising Project Mars for borrowing too much from CoW, Gibler and Miller reverse course, arguing that Project Mars didn't take enough. They conduct a random audit of 20 (unnamed) Intra-State Wars and conclude that 17, and possibly all 20, were conventional wars. Extrapolating from this estimate to the entire CoW universe, they maintain that Project Mars is missing a staggering 350-455 additional CoW wars. In effect, they argue that (nearly) *all* wars in CoW are conventional in nature.

This claim simply isn't credible. CoW's own founders rejected organizing CoW by type of warfare.[6] As a result, the Intra-State War dataset, much like its Extra- and Non-State cousins, is a kaleidoscope of political violence. It contains conflicts as diverse as street violence (Sparticist Uprising, 1919), guerrilla wars (Ukrainian Partisans War of 1945-47), coups (Overthrow of Abd el-Aziz, 1907-08) and counter-coups (Young Turks Counter-Coup of 1908), one-sided government violence (the Janissary Revolt of 1826), revolutionary warfare (Cultural Revolution Phase I of 1967), peasant rebellion (Green Rebellion, 1920-21), and low-tech "symmetrical non-conventional"[7] wars (First Congo-Brazzaville War of 1997).

More systematically, we excluded 313 of the remaining 335 wars in the Intra-State War dataset (ver.5.1) because these conflicts did not met our definition of conventional war.[8] Project Mars defines *conventional wars* as armed combat between the military organizations of two or more belligerents engaged in direct battle that caused ≥500 battlefield fatalities over the duration of hostilities. Armies fought using combined arms; wore uni-

[5] All non-states are designated by a "-8" code.

[6] Sarkees and Wayman 2010, 67.

[7] Kalyvas and Baicells 2010.

[8] We identified six possible reasons for excluding a war; multiple reasons for exclusion are possible. Our original exclusion list was drawn from Version 4.0 of the Intra-State War dataset. I have updated it here to reflect the latest version.

forms; had functional specialization (infantry-artillery-cavalry, or historical equivalent); and were under central direction of the belligerent's political authorities. A *belligerent* is defined as a political entity that claims control over, and authority within, a definite territory and populace, and that can field a conventional army (pp.23-26).[9] We only included wars that were fought predominantly along conventional lines. By contrast, many of these excluded cases represent guerrilla wars. Again, this is unsurprising: leading datasets of irregular war build on the Intra-State War dataset precisely because it contains so many cases of insurgency.[10] I have posted our Intra-State War exclusion list to Dataverse.

Similarly, 214 of these 335 conflicts involved a belligerent that did not meet our definition of a state. CoW's own historical summaries detail wars against bandits (China-Pai-Ling War of 1914), protestors (Romanian War, 1989), drug lords (Eighth Colombian War of 1989—Present), political parties (Agrarian Uprising of 1923), cults (Third Brazil-Canudas War, 1896-97), and miners (Spanish Miners War of 1934). As a whole, belligerents in the Intra-State War dataset run the gamut from "factions," "rebels," "Communists," "leftists," "guerrillas," "anti-imam coalitions," "warlords," and more. We excluded actors that did not have a territorial base, that lacked a political system, or that functioned as private militia acting on behalf of a political, tribal, or ideological faction. These non-state actors are simply outside Project Mars' original remit.[11]

Of course, scholars might disagree over what constitutes a conventional war, just as they still do for civil wars. That's why we archived a subset of 111 wars that we excluded from Project Mars, along with our reasons, on Dataverse ("ExcludedCases"). This subset represents edge cases that other scholars had coded as conventional wars but that did not meet our stricter inclusion criteria. In some cases, we did not have the confidence in existing sources to code the independent or dependent variables. As the preceding discussion shows, this subset is not, as they maintain, a catalogue of all the wars we considered.

[9]Unlike CoW, Project Mars did not require that a belligerent have diplomatic recognition by Great Britain or France or, later, the United Nations, to be included. Civil wars, if fought conventionally, are also included in Project Mars.

[10]Fearon and Laitin 2003; Kalyvas and Baicells 2010.

[11]Dixon and Sarkees 2016.

4 Military Inequality as an Independent Variable

Since Gibler and Miller's two-sentence summary of the military inequality coefficient (MIC) contains multiple errors, it makes sense to restate how it was constructed. Briefly, the MIC calculates an army's level of inequality across its constituent ethnic groups. It consists of two components. First, I calculated the relative share that each group represented of an army's prewar personnel. Second, I assigned each ethnic group a numeric value based on its prewar treatment by the state. Specifically, I recorded whether the group enjoyed full citizenship (a "0"), faced state-organized discrimination (a "0.5"), or suffered state-orchestrated repression (a "1"). These two components are then combined to generate a value between 0 (perfect equality) and 1 (perfect inequality). Formally, we have:

$$MI = \sum_{i=1}^{n} pt_i$$

Here, p is the proportion of a belligerent's army that an ethnic group represents, t is the nature of the state's prewar treatment of that ethnic group, and n is the total number of ethnic groups in the army. The military inequality coefficient has several desirable properties. It is easily interpreted; higher values indicate greater inequality within the army. Both components are measured before war commences — not during battle, as Gibler and Miller claim — helping to avoid confounding with wartime processes. Finally, it is flexible; scholars can apply it to entire armies, specific divisions, or even small detachments. In Chapter 4, I simply calculated one prewar MIC per belligerent per war using the mean of high and low estimates.

Gibler and Miller remain unimpressed by the MIC variable, however, and submit a barrage of complaints about its measurement. It is data-greedy, they contend, requiring granular data on armies that "are not usually feasible to collect." This is an odd charge. Data collection difficulties do not justify excluding potentially important explanatory variables. To be sure, our coders spent years collecting high and low MIC estimates for each belligerent; this was easily the most time-consuming aspect of Project Mars. Yet to suggest that this task is infeasible is to ignore the recent explosion of similar efforts to collect crossnational, time-series, data on the ethnic composition of armies and security forces.[12] Similarly, multiple large-N datasets record the state's treatment of ethnic minorities over time, whether in terms of mass violence, physical security, or political rights.[13] Scholars harnessing the power of record linkage have now constructed panel data on the ethnic and racial composition of armed formations from millions of personnel records in conflicts as diverse as the American Civil War, the British Commonwealth armies in the Second World War, and the Korean War.[14] In one notable case, 100 million personnel records were used to construct a *monthly* panel-dataset on the ethnic composition of 609 Soviet Rifle Divisions on the Eastern Front.[15] Gibler and Miller have simply missed the sea-change in sources and methods available to scholars interested in these questions.

There's no doubt that the quality and quantity of evidence available for estimating MIC values varied across conflicts, time, and belligerents. That's precisely why we created quality codes for our assessments; made them publicly available in Project Mars; and reestimated our results dropping observations with low confidence codes. Gibler and Miller, however, contend that these quality assessments are problematic. After scraping the online 1,200-page Project Mars bibliography, they conclude that "systematic bias" exists in the MIC value because our quality codes are inversely correlated with the number of sources per war. For Gibler and Miller, the quality of information should increase with the number of citations; here, however, the opposite appears true.

Unfortunately, there are two basic problems with their approach. First, the public Project Mars bibliography was designed as a starting guide for new coders to familiarize themselves with the wars; it is literally called the "starter kit" in the codebook (p.2). It is not a comprehensive accounting of the sources used to code MIC values (or, indeed, other variables). As noted in the book (p.154), the codebook (pp14-16), and in all of the historical cases (e.g., pp.208,210,262,265), the MIC was derived from diverse sources, including military tables of organization, large-N datasets, wartime correspondents, war diaries, censuses, official histories, and regimental narratives. The Project Mars bibliogra-

[12]E.g., Harkness 2018; Johnson and Thurber 2020; De Bruin 2020; Carey, Mitchell and Paula 2022.
[13]E.g., Gurr 1993; Wucherpfennig et al. 2011; Eck and Hultman 2007.
[14]Dippel and Heblich 2021; Fennell 2019; Huff and Schub 2021.
[15]Rozenas, Talibova and Zhukov 2020. See also Talibova 2021.

phy reflects general histories, not these more specialized sources, which were recorded in a separate database. Second, the number of sources per war is simply not a credible measure for their quality. A single citation might indicate the absence of a robust historiography; it might also indicate the presence of a statistical encyclopedia with a wealth of information. Similarly, five sources might indicate a high volume of information, sharp disagreement over its true nature, or fragmentary data scattered over multiple sources. Given that they scraped the wrong sources and their own measure of quality makes little sense, their claim of systematic bias is unwarranted.

Next, Gibler and Miller claim that the MIC isn't especially useful. Armies, they contend, don't appear to be that divided; the mean MIC for belligerents is "only" 0.205. Yet this is a *high* value: it signifies that 20.5% of an army is comprised of soldiers from violently repressed ethnic groups or 41% from groups experiencing state discrimination. They also suggest that there are few high and medium inequality belligerents. This, too, is incorrect. Project Mars records 142 belligerent observations at high/extreme levels of inequality and a further 216 at medium levels. Six of the book's case studies involve belligerents with MIC values at these levels. They suggest something is amiss by noting that MIC's "effective range" only extends to 0.75. Yet, as I noted in the book (p.7), the MIC has a ceiling because it is impossible for armies to be fielded without some core of reliable soldiers.[16] The MIC performs exactly like its cousin, the Gini coefficient: while perfect inequality is theoretically possible, it is typically not achieved in reality. Both measures top out around 0.80.[17] Finally, they argue that MIC is "meaningless" because some values for some belligerents straddle the entire continuum of possible values. But there are no such belligerents in Project Mars.

[16]It is possible for some individual units to exceed 0.80 (see Chapter 8).

[17]Scheidel 2017, 445-448.

5 Inequality, Loss-Exchange Ratios, and Battlefield Performance Across CoW War Types

Gibler and Miller's cavalcade of criticism culminates in a final claim: that Project Mars "naively" pools the four types of CoW wars together. They insist on two interventions: (1) Project Mars must be partitioned by CoW war type to prevent exaggerating its explanatory effects on battlefield performance and (2) these tests should use their preferred measure of military effectiveness, not the ones actually used in *Divided Armies*.

To review briefly, I used four variables to test the association between military inequality and battlefield performance. These are: (1) a binary indicator of whether the belligerent (or coalition) suffered higher casualties than its enemy (*LER below parity*); (2) an indicator of whether $\geq$10 percent of a belligerent's army deserted (*Mass desertion*) or (3) defected (*Mass defection*) during the war; and (4) whether the belligerent fielded specialized units authorized to kill one's own soldiers to enforce discipline (*Blocking units*). These measures were combined into a summary index to facilitate crossnational comparison (*BP Index*).[18]

Gibler and Miller ignore these new measures. Instead, they propose a return to the venerable loss-exchange ratio (LER) as the "clearest example" of battlefield performance. To be clear, Project Mars does contain the raw loss-exchange ratios used to construct *LER below parity*. But they contend that I used an outmoded calculation (enemy killed in action/friendly KIA). Instead, they prefer a fractional approach (enemy casualties/enemy casualties + friendly casualties), which they claim reflects the prevailing standard in the literature. They also allege that errors were made when calculating loss-exchange ratios, noting that 36% of "low" LER estimates are actually higher than the supposed "high" estimates.

These issues can be dismissed quickly. It's true that the chapter did not directly test how military inequality affected the magnitude of relative casualties. I did, however, conduct robustness checks in the book's supplemental appendix (Table 27) using actual loss-exchange ratios. These tests confirmed a negative association between military inequality

[18]The index runs from 0 to 1, where 0 indicates poor performance, 1 denotes excellent performance, and a 0.25 penalty to the belligerent's score is assigned for the presence of each of these four problems.

and logged LER values; the higher the belligerent's prewar inequality, the worse its relative losses. Moreover, my calculation of LER follows leading quantitative (e.g. Biddle, 2004; Pilster and Böhmelt, 2011) and qualitative studies (e.g. Talmadge, 2015; McNerney et al., 2018) of military effectiveness as well as war- and battle-level datasets (e.g. Dupuy, 1984; Cochran and Long, 2017). As one recent study concluded, "dividing enemy casualties by friendly casualties is a standard measure of relative attrition."[19] Fractional LERs, by contrast, have received little attention in the literature. If we care about comparing our findings to earlier influential work, we need to use the same LER calculation. Finally, there are no errors in my calculation of LER; they simply misread the codebook. "Low" estimates are simply the lowest credible estimate of soldiers killed for each side (low/low); "high" estimates are the highest credible assessments of soldiers killed on each side (high/high). It is therefore possible for "low" LER estimates to exceed "high" ones depending on the range of casualties recorded. This is precisely why I used the *mean* of LER estimates for *LER below parity* and associated robustness checks.

Armed with their preferred measure of casualties, Gibler and Miller turn to a "reanalysis" of the relationship between military inequality and LER. In doing so, they drop all coalitional wars from Project Mars (44% of all observations); use a "corrected" version of MIC; drop six control variables from my original models, including measures drawn from CoW itself; pool observations from the early (1800-1917) and modern (1918–) periods of combined arms rather than splitting the sample to reflect changes in military technology; and then partition the remaining observations by CoW war types, despite their serious mistakes (noted above) in assigning CoW wars to Project Mars.

Few would consider this a fair test. Yet despite dictating the relevant sample, model specification, dependent variable, and data subsets, Gibler and Miller nonetheless conclude that military inequality is *still* associated with poor FLERs in the two most frequent forms of war, Inter- and Extra-State wars. It also narrowly misses conventional significance for Non-State wars. Military inequality outperforms all other explanations — even their favorite, regime type[20] — across multiple types of war while using an idiosyncratic

[19]Lehmann and Zhukov 2019, 145.

[20]Gibler and Miller are encouraged that *Regime Type* is significant in three of four war types. But

dependent variable and scarcely half of the relevant observations. At worst, their findings suggest scope conditions for my argument, not its wholesale rejection.

But what happens if we use the standard LER measure (enemy/friendly KIA)? One empirical contribution of Project Mars is that we are able to conduct these cross-war type comparisons for the first time. I therefore use their preferred statistical model, periodization, and CoW typology to reestimate the relationship between military inequality and LER using the entire Project Mars sample. As Table 8 illustrates, rising military inequality is associated with increasingly poor LERs in *every* CoW war type, including wars not in CoW, though only at the p=0.089 level for Non-State Wars (the least frequent category).

regime type is not a standalone variable. Following standard practice, it was interacted with *Initiator* status to create a joint *RegimeType*Initiator* variable. We must calculate the joint significance of these three variables, not *Regime Type* alone. When we do so, regime type appears to have no effect on loss-exchange ratios in their own models.

Table 2: Military Inequality and Loss-Exchange Ratios, by CoW War Types

	Inter-State War	Intra-State War	Extra-State War	Non-State War	Not in COW
Military Inequality	-3.884^{***}	-2.173^{*}	-4.330^{***}	$-2.538^{\dagger}$	-2.740^{***}
	(0.861)	(1.028)	(1.039)	(1.462)	(0.650)
Regime Type	0.020	0.074	0.101^{*}	0.163	0.092^{**}
	(0.026)	(0.050)	(0.042)	(0.131)	(0.028)
Regime Type*Initiator	0.033	−0.013	−0.010	−0.054	−0.010
	(0.038)	(0.050)	(0.042)	(0.143)	(0.038)
Democratic Opponent	-1.391^{***}	−0.147	$-1.022^{\dagger}$	−2.167	-2.030^{***}
	(0.288)	(0.495)	(0.609)	(3.159)	(0.458)
Initiator	0.117	−0.406	−0.156	−0.758	−0.075
	(0.261)	(0.381)	(0.238)	(1.092)	(0.303)
Distance to Battle	0.120	0.188^{**}	0.221	0.134	0.231^{**}
	(0.098)	(0.059)	(0.090)	(0.140)	(0.070)
Standing Army	0.938	0.258	0.969	−0.516	0.619
	(0.621)	(0.359)	(0.646)	(0.568)	(0.396)
Volunteer Army	0.134	0.127	0.173	0.076	0.386
	(0.354)	(0.256)	(0.360)	(0.589)	(0.280)
Composite Army	$-0.388^{\dagger}$	-0.870^{**}	$-0.845^{\dagger}$	0.384	0.341
	(0.228)	(0.293)	(0.477)	(0.509)	(0.291)
Relative Forces	0.197	−1.435	-2.364^{**}	$-2.020^{\dagger}$	0.895
	(0.693)	(0.891)	(0.802)	(1.074)	(0.799)
Constant	0.400	$1.568^{\dagger}$	1.177	$2.822^{\dagger}$	$-1.166^{\dagger}$
	(0.639)	(0.792)	(0.477)	(1.411)	(0.629)
F Score	9.923^{***}	3.53^{***}	18.39^{***}	1.319	8.812^{***}
r^2	0.220	0.265	0.492	0.272	0.378
N	329	154	133	56	153

Note: Robust standard errors clustered on individual belligerents. *** $p < 0.001$ ** $p < 0.01$ * $p < 0.05$ † $p < 0.10$.

I plot the average marginal effects of military inequality on LERs in Figure 1 for ease of interpretation.

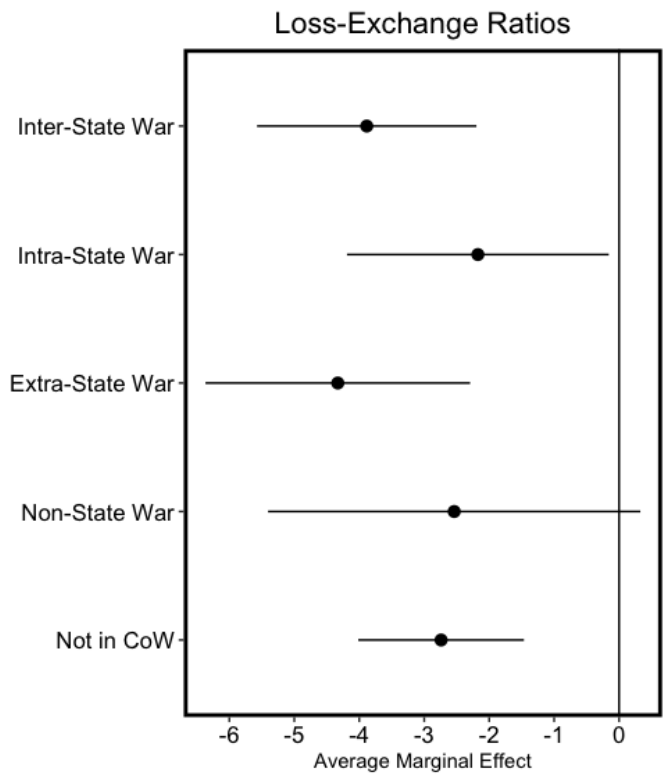

Figure 1: Average Marginal Effects of Military Inequality on Loss-Exchange Ratios, by CoW War Type, 1800-2011

But why stop there? We can repeat this analysis for all five measures of battlefield performance actually used in the *Divided Armies*. In Figure 2, I plot the average marginal effects of military inequality on each measure for each war type using their preferred models. Military inequality is associated with decreased battlefield performance in 23 of 25 models (see Tables 4-8 in the Appendix).

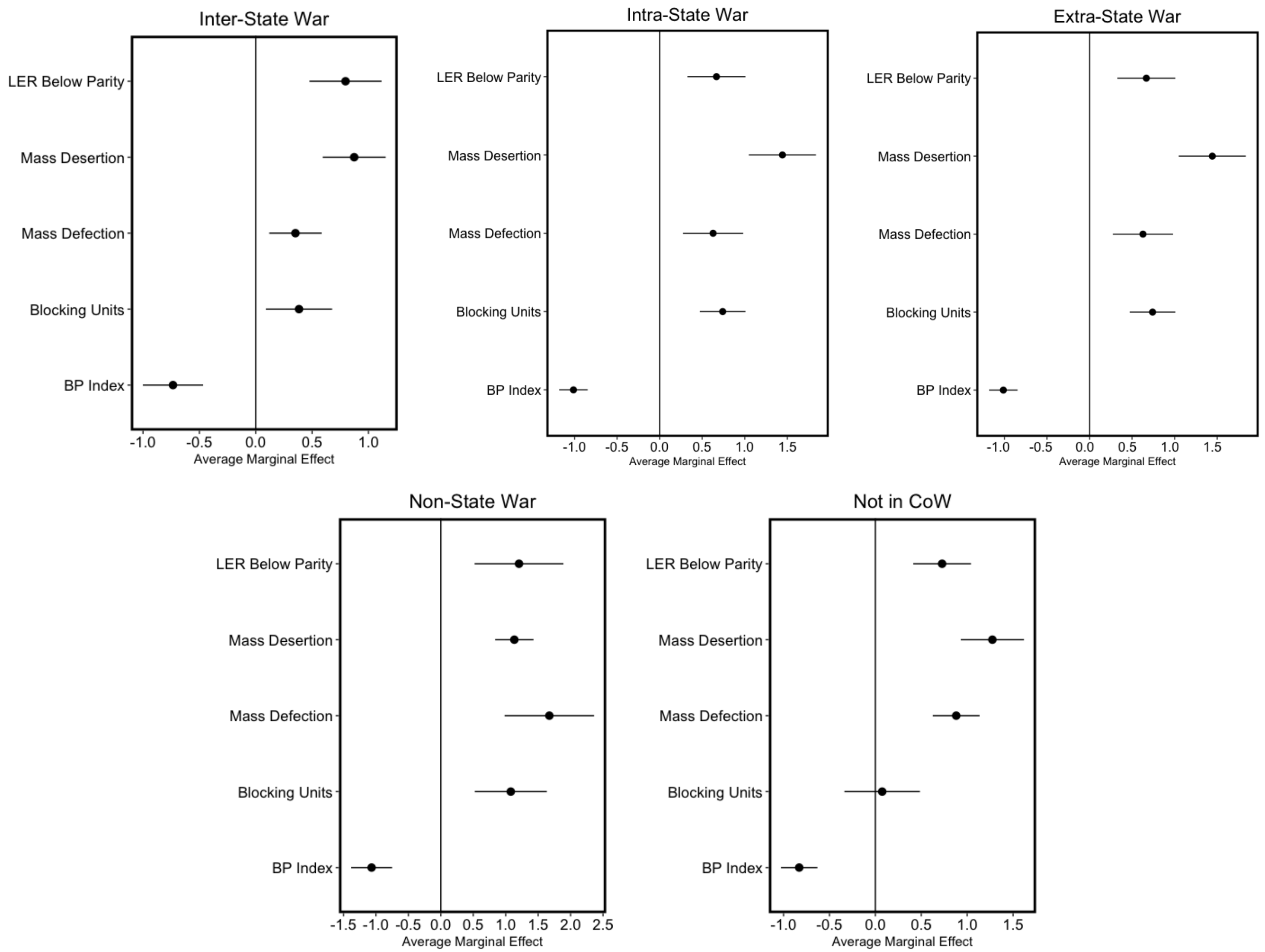

Figure 2: Average Marginal Effects of Military Inequality on Battlefield Performance, by CoW War Type, 1800-2011

6 Robustness Check: *Bands of Inequality* and Five Measures of Battlefield Performance, by CoW War Type, 1800-2011

As an additional robustness check, I reestimated the models above with an alternative measure of military inequality, *bands of inequality*, which was also used in *Divided Armies* (pp.179-83). *Bands* assigns belligerents to one of four "bands" based on their military inequality coefficients. These bands are: Low (0-0.20), Medium (0.21-0.40), High (0.41-0.60) and Extreme ($\geq$0.61). These bands help reduce sensitivity to measurement error while also providing a grammar for speaking about the magnitude of inequality across belligerents.

Figure 3 plots the average marginal effects of *Bands* across five measures of battlefield performance for each type of CoW war. As above, a fifth category of non-CoW wars is included. Once again, Gibler and Miller's own preferred specification and data partitioning illustrate the importance of military inequality for explaining battlefield performance. In short, 23 of 25 models return favorable results for this alternative measure of inequality, increasing our confidence in the association between inequality and a wide range of battlefield outcomes.

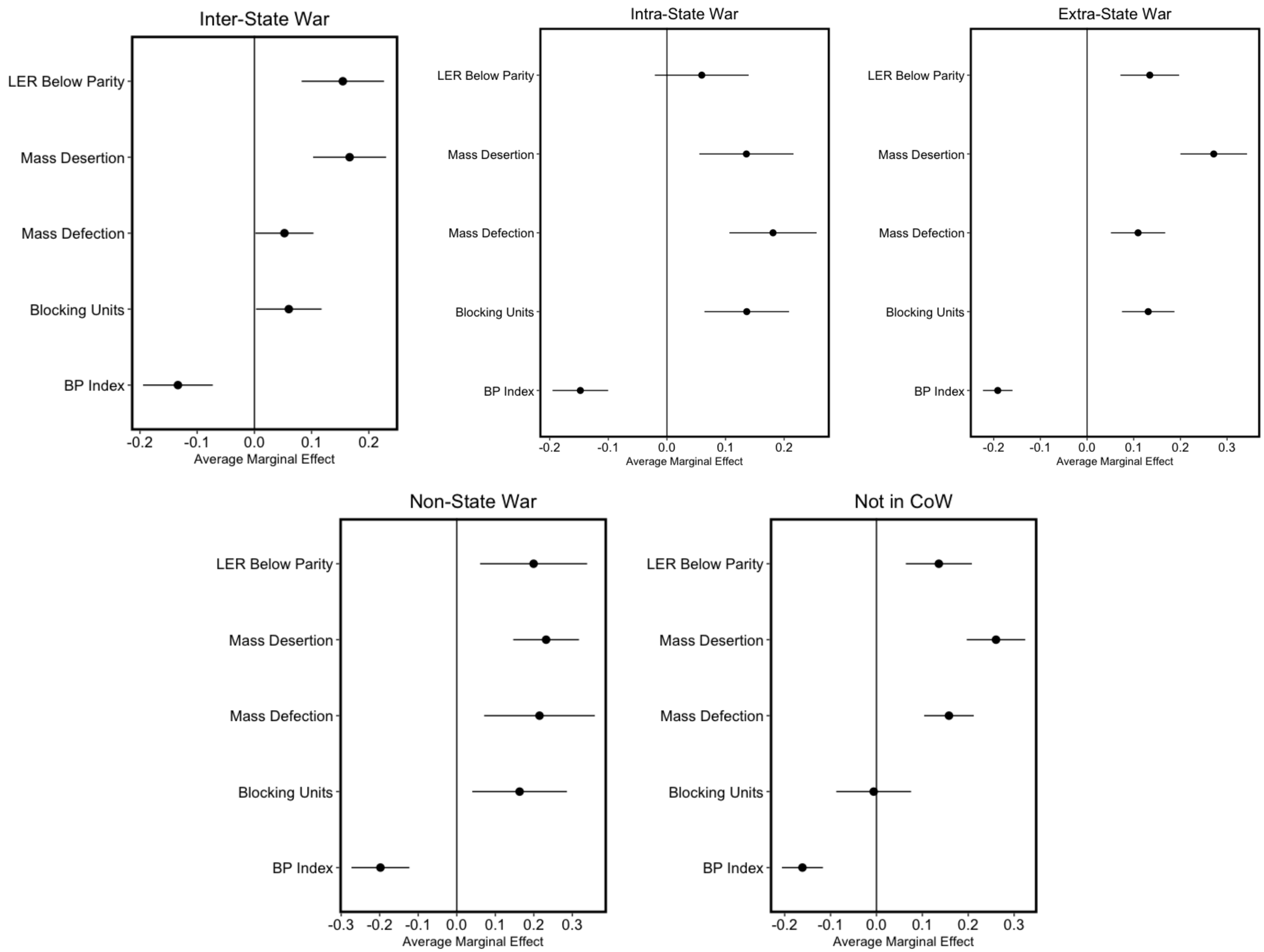

Figure 3: Average Marginal Effects of Military Inequality on Battlefield Performance, by CoW War Typology, 1800-2011, Using *Bands*

7 Additional Robustness Check: Reestimating Models Using Clodfelter (2008)-only Wars

Since CoW is not the only well-known dataset of wars, I reestimate Gibler and Miller's preferred models using only the wars recorded in Clodfelter (2008) to examine how military inequality performs in a non-CoW sample (Table 3). Our confidence in the military inequality argument should increase if MIC performs consistently across the full Project Mars dataset, the CoW-partitioned subsets, and an influential non-CoW sample. As expected, MIC is again associated with diminished battlefield performance across all measures (see Figure 4).

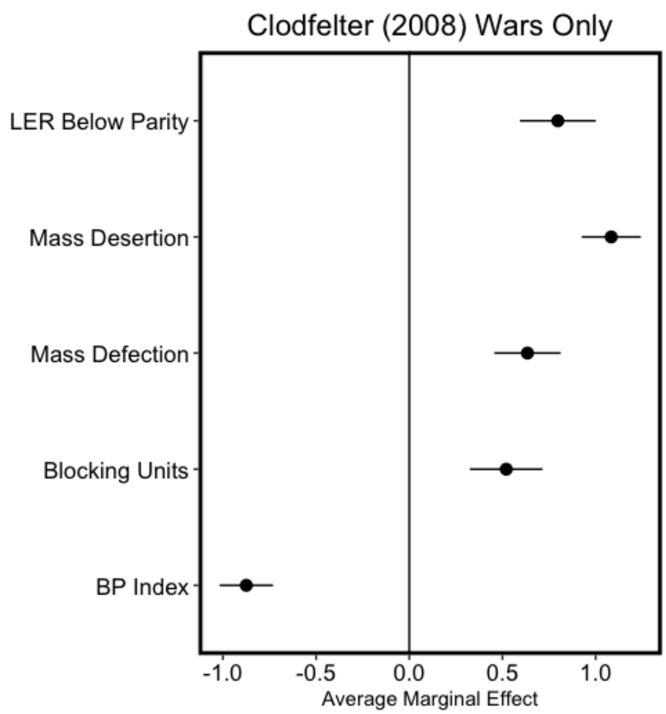

Figure 4: Average Marginal Effects of Military Inequality on Battlefield Performance, Clodfelter (2008)-only Wars, 1800-2011

Table 3: Military Inequality and Battlefield Performance in Clodfelter (2008)-only Wars, 1800-2011

	LER Below Parity	Mass Desertion	Mass Defection	Blocking Units	BP Index
Military Inequality	4.207^{***}	5.763^{***}	4.919^{***}	4.050^{***}	-0.874^{***}
	(0.700)	(0.591)	(0.765)	(0.653)	(0.073)
Regime Type	-0.025	-0.002	-0.026	0.006	0.001
	(0.019)	(0.021)	(0.027)	(0.019)	(0.001)
Regime Type*Initiator	-0.020	-0.003	0.028	-0.000	0.000
	(0.027)	(0.033)	(0.031)	(0.032)	(0.002)
Democratic Opponent	1.004^{***}	-0.261	-0.311	0.071	-0.030
	(0.236)	(0.298)	(0.378)	(0.316)	(0.031)
Initiator	-0.240	-0.158	0.096	-0.098	0.020
	(0.195)	(0.250)	(0.218)	(0.241)	(0.015)
Distance to Battle	-0.154^{**}	-0.067	-0.004	0.041	0.009^{*}
	(0.049)	(0.047)	(0.057)	(0.072)	(0.004)
Standing Army	0.066	-0.219	-0.539^{**}	-1.112^{**}	0.069^{*}
	(0.326)	(0.329)	(0.335)	(0.348)	(0.031)
Volunteer Army	-0.198	0.057	0.316	-0.759^{*}	0.020
	(0.213)	(0.179)	(0.276)	(0.315)	(0.019)
Composite Army	0.222	0.147	0.463	$0.652^{\dagger}$	-0.047^{**}
	(0.202)	(0.193)	(0.230)	(0.341)	(0.017)
Relative Forces	0.984^{*}	-0.478	0.258	-0.138	-0.032
	(0.416)	(0.370)	(0.532)	(0.577)	(0.040)
Constant	-1.192^{*}	-0.905^{*}	-2.778^{***}	-1.861^{**}	0.815^{***}
	(0.489)	(0.438)	(0.604)	(0.567)	(0.044)
Wald χ^2	104.60^{***}	136.13^{***}	82.06^{***}	120.35^{***}	
F Score					29.84^{***}
(Pseudo) r^2	0.173	0.173	0.154	0.160	0.3798
N	667	667	667	667	667

Note: Robust standard errors clustered on individual belligerents. *** $p < 0.001$ ** $p < 0.01$ * $p < 0.05$ † $p < 0.10$.

8 Conclusion

Taken together, Gibler and Miller misrepresent Project Mars; botch their discussion of the book's independent variable; and conduct a reanalysis that only confirms the disastrous effects of military inequality on battlefield performance across six different measures in four types of CoW wars over the past 200 years. Yet they conclude with a warning: "novel" theories like mine are dangerous because they lead us to "ignore other factors that could help militaries defend their countries." I disagree. The evidence amassed in *Divided Armies* and here suggest that we need more, not less, research on inequality and military effectiveness since many existing theories struggle to explain patterns of battlefield outcomes. Other inequalities — class, gender, income, religion, ideological — surely matter as well. We need additional theorizing about how these inequalities intersect to shape patterns of wartime and postwar violence for both armies and rebels alike. We also need to harness all our methodological tools to test, revise, and extend these theories. And we need to invest in data collection to reset our empirical work on a more global, less Western, foundation. Collaborative in spirit and execution, this research agenda promises to break new ground if we are willing to build on, and move beyond, a status quo that continues to dismiss inequality and its effects on violence.

9 Appendix

Table 4: Military Inequality and Battlefield Performance in CoW Inter-State Wars, 1800-2011

	LER Below Parity	Mass Desertion	Mass Defection	Blocking Units	BP Index
Military Inequality	4.567***	4.951***	3.437**	3.750***	−0.734***
	(0.911)	(0.901)	(1.097)	(1.077)	(0.136)
Regime Type	−0.035	−0.037	−0.081†	−0.095*	0.007***
	(0.027)	(0.026)	(0.041)	(0.043)	(0.002)
Regime Type*Initiator	−0.015	−0.008	0.063	0.070	−0.003
	(0.041)	(0.040)	(0.055)	(0.062)	(0.002)
Democratic Opponent	1.332***	−0.490	−0.266	−0.687	−0.016
	(0.315)	(0.341)	(0.449)	(0.452)	(0.035)
Initiator	−0.325	−0.495†	0.210	−0.275	0.045*
	(0.297)	(0.291)	(0.405)	(0.444)	(0.021)
Distance to Battle	−0.119	0.030	0.086	0.175	−0.003
	(0.074)	(0.076)	(0.106)	(0.129)	(0.007)
Standing Army	−0.766	−0.389	−1.186	15.085	0.076
	(0.780)	(0.823)	(0.923)	(1127.265)	(0.077)
Volunteer Army	0.208	−0.140	0.155	−2.365*	0.028
	(0.346)	(0.347)	(0.474)	(1.045)	(0.026)
Composite Army	−0.091	0.018	0.470	0.094	−0.009
	(0.284)	(0.278)	(0.381)	(0.375)	(0.037)
Relative Forces	−0.870	−0.131	0.109	−1.088	0.065
	(0.726)	(0.716)	(0.958)	(0.971)	(0.076)
Constant	0.149	−1.229	−2.594*	−17.968	0.799***
	(0.893)	(0.935)	(1.144)	(1127.265)	(0.078)
Wald χ^2	70.65***	59.62***	38.58***	60.40***	
F Score					10.97***
(Pseudo) r^2	0.173	0.137	0.093	0.190	0.287
N	329	329	329	329	329

Note: Robust standard errors clustered on individual belligerents. *** $p < 0.001$ ** $p < 0.01$ * $p < 0.05$ † $p < 0.10$.

Table 5: Military Inequality and Battlefield Performance in CoW Intra-State Wars, 1800-2011

	LER Below Parity	Mass Desertion	Mass Defection	Blocking Units	BP Index
Military Inequality	1.878	4.092**	5.857***	6.367***	−0.829***
	(1.194)	(1.296)	(1.391)	(1.551)	(0.121)
Regime Type	0.002	0.094*	−0.063	0.029	−0.003
	(0.044)	(0.046)	(0.053)	(0.059)	(0.004)
Regime Type*Initiator	−0.096	−0.089	0.017	−0.095	0.010*
	(0.076)	(0.065)	(0.073)	(0.083)	(0.005)
Democratic Opponent	0.654	1.005	0.425	1.715**	−0.174***
	(0.537)	(0.614)	(0.588)	(0.636)	(0.046)
Initiator	−0.553	0.621	0.556	0.115	−0.040
	(0.550)	(0.476)	(0.541)	(0.613)	(0.043)
Distance to Battle	−0.162†	0.089	0.084	0.123	−0.004
	(0.089)	(0.088)	(0.096)	(0.109)	(0.007)
Standing Army	0.015	−1.296*	−0.302	−0.794	0.102†
	(0.593)	(0.579)	(0.618)	(0.681)	(0.053)
Volunteer Army	−0.164	−1.115**	0.702†	0.186	0.032
	(0.394)	(0.421)	(0.423)	(0.489)	(0.051)
Composite Army	0.645	1.051*	0.299	0.999*	−0.126***
	(0.406)	(0.414)	(0.419)	(0.509)	(0.036)
Relative Forces	1.891†	−0.132	−0.795	−1.088	−0.016
	(0.990)	(0.963)	(1.020)	(1.234)	(0.085)
Constant	−1.609†	−0.824	−2.967**	−3.293**	0.895***
	(0.890)	(0.861)	(0.995)	(1.164)	(0.080)
Wald χ^2	19.86*	30.28***	38.94***	38.97***	
F Score					11.24***
r^2	0.132	0.214	0.175	0.263	0.443
N	154	154	154	154	154

Note: Robust standard errors clustered on individual belligerents. *** $p < 0.001$ ** $p < 0.01$ * $p < 0.05$ † $p < 0.10$.

Table 6: Military Inequality and Battlefield Performance in CoW Extra-State Wars, 1800-2011

	LER Below Parity	Mass Desertion	Mass Defection	Blocking Units	BP Index
Military Inequality	5.414^{**}	9.519^{***}	6.815^{***}	6.746^{***}	-1.012^{***}
	(1.793)	(1.924)	(1.898)	(1.772)	(0.085)
Regime Type	-0.153^{*}	0.047	0.157^{*}	0.07	-0.001
	(0.068)	(0.054)	(0.068)	(0.061)	(0.003)
Regime Type*Initiator	-0.021	0.028	-0.106	0.085	0.000
	(0.089)	(0.075)	(0.103)	(0.090)	(0.004)
Democratic Opponent	1.226	-0.809	-0.759	-0.566	0.016
	(0.858)	(0.835)	(1.177)	(0.929)	(0.059)
Initiator	0.790	0.089	-0.754	-0.182	-0.007
	(0.566)	(0.491)	(0.667)	(0.572)	(0.025)
Distance to Battle	-0.113	$-0.185^{\dagger}$	-0.088	-0.068	0.017^{*}
	(0.121)	(0.108)	(0.158)	(0.133)	(0.007)
Standing Army	0.245	0.358	-0.736	-1.460^{*}	0.038
	(0.760)	(0.746)	(1.006)	(0.819)	(0.050)
Volunteer Army	-0.277	-0.248	-0.184	-0.759	$0.056^{\dagger}$
	(0.535)	(0.501)	(0.661)	(0.6406)	(0.028)
Composite Army	0.739	-0.582	0.495	0.000	-0.017
	(0.626)	(0.584)	(0.804)	(0.715)	(0.032)
Relative Forces	4.619^{***}	-1.172	0.365	-0.676	-0.094
	(1.216)	(0.912)	(1.155)	(1.100)	(0.060)
Constant	-4.568^{***}	-0.728	-2.564^{*}	-0.768^{***}	0.835^{***}
	(1.364)	(1.072)	(1.265)	(1.142)	(0.096)
Wald χ^2	52.42^{***}	38.97^{***}	15.35	33.95^{***}	
F Score					26.15^{***}
r^2	0.430	0.287	0.226	0.228	0.598
N	133	133	133	133	133

Note: Robust standard errors clustered on individual belligerents. *** $p < 0.001$ ** $p < 0.01$ * $p < 0.05$ † $p < 0.10$.

Table 7: Military Inequality and Battlefield Performance in CoW Non-State Wars, 1800-2011

	LER Below Parity	Mass Desertion	Mass Defection	Blocking Units	BP Index
Military Inequality	8.834**	8.212***	19.802*	8.459*	−1.066***
	(3.064)	(2.389)	(8.253)	(3.455)	(0.161)
Regime Type	−0.276	−0.077	0.774†	−0.109	0.004
	(0.199)	(0.171)	(0.431)	(0.215)	(0.012)
Regime Type*Initiator	0.181	0.032	−0.287	0.259	−0.010
	(0.209)	(0.192)	(0.302)	(0.240)	(0.013)
Democratic Opponent	0.207	−0.102	−17.248	−19.025	0.220
	(4.034)	(1.858)	(2494.659)	(2453.817)	(0.187)
Initiator	1.233	−0.872	−4.288	0.355	0.034
	(1.627)	(1.461)	(2.965)	(1.723)	(0.097)
Distance to Battle	0.022	−0.308	−0.976†	0.188	0.018
	(0.213)	(0.219)	(0.508)	(0.283)	(0.015)
Standing Army	−0.280	−0.539	−2.709	−2.803*	0.102
	(0.949)	(0.882)	(1.986)	(1.284)	(0.068)
Volunteer Army	0.600	1.014	−2.104	−0.595	−0.054
	(0.868)	(0.834)	(1.621)	(0.909)	(0.071)
Composite Army	−1.573	−0.444	4.362†	1.758	−0.053
	(0.984)	(0.904)	(2.503)	(1.246)	(0.062)
Relative Forces	6.040**	−1.076	−4.087	2.456	−0.125
	(2.110)	(1.677)	(2.574)	(1.971)	(0.127)
Constant	−7.406*	−0.868	3.236	−5.968†	0.940***
	(2.881)	(2.005)	(3.343)	(3.274)	(0.123)
Wald χ^2	12.62	15.20	23.81**	34.17***	
F Score					8.93***
r^2	0.378	0.335	0.515	0.325	0.565
N	56	56	56	56	56

Note: Robust standard errors clustered on individual belligerents. Note that Democratic Opponent returns large standard errors in several specifications due to the relative sparsity of observations of democratic opponents and the small number of overall conflict observations. *** $p < 0.001$ ** $p < 0.01$ * $p < 0.05$ † $p < 0.10$.

Table 8: Military Inequality and Battlefield Performance in Non-CoW Wars, 1800-2011

	LER Below Parity	Mass Desertion	Mass Defection	Blocking Units	BP Index
Military Inequality	3.746**	7.359***	8.436***	0.669	−0.829***
	(1.185)	(1.407)	(1.841)	(1.505)	(0.101)
Regime Type	−0.101†	0.023	0.065	0.096	0.001
	(0.059)	(0.054)	(0.096)	(0.062)	(0.003)
Regime Type*Initiator	0.087	−0.016	−0.047	0.031	−0.003
	(0.072)	(0.072)	(0.119)	(0.080)	(0.006)
Democratic Opponent	1.613*	−0.769	−0.333	−0.132	−0.031
	(0.735)	(0.750)	(0.901)	(0.914)	(0.037)
Initiator	0.895	−0.123	−0.467	0.782	−0.043
	(0.579)	(0.554)	(0.921)	(0.616)	(0.049)
Distance to Battle	−0.275**	0.095†	0.263†	−0.160	0.008
	(0.103)	(0.096)	(0.145)	(0.116)	(0.006)
Standing Army	0.000	−1.249	−0.689	−0.267	0.099†
	(0.652)	(0.753)	(0.792)	(0.851)	(0.059)
Volunteer Army	−0.671	0.520†	1.120†	−0.909	−0.000
	(0.446)	(0.465)	(0.601)	(0.630)	(0.039)
Composite Army	−0.271	−0.034	0.784	0.626	−0.018
	(0.396)	(0.429)	(0.600)	(0.577)	(0.037)
Relative Forces	−1.593†	−0.286	−0.016	0.925	0.058
	(0.911)	(0.915)	(1.148)	(1.146)	(0.083)
Constant	0.766	−5.946***	−5.946***	−1.300	0.757***
	(1.008)	(1.746)	(1.746)	(1.170)	(0.081)
Wald χ^2	34.08***	24.66**	39.19***	28.85**	
F Score					11.16***
r^2	0.168	0.223	0.302	0.102	0.383
N	153	153	153	153	153

Note: Robust standard errors clustered on individual belligerents. *** $p < 0.001$ ** $p < 0.01$ * $p < 0.05$ † $p < 0.10$.

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