

Military Labor Systems, Domestic Politics, and the Battlefield

Comparative Political Studies
2025, Vol. 0(0) 1–38

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DOI: 10.1177/00104140251328026

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Abstract

Research has demonstrated that decisions around military conflict can significantly impact governments' political survival. One important strand of the extant literature has focused on the impact of military labor systems, with most academic studies distinguishing between two types of systems: conscription, in which citizens are compelled to serve; and market-based enlistment according to which citizens self-select into military service. In this article, we argue that this conceptualization of military labor systems misses substantial theoretical and empirical variation in recruitment of military personnel over time and across cases. We make two contributions. First, we introduce new data that allow us to capture an important and common additional category of military labor acquisition—selective conscription. Second, we use these new data to empirically demonstrate that selective systems afford governments significant opportunities to manipulate how the costs of military conflict are distributed across the population. Specifically, we provide empirical evidence showing that countries with selective conscription are more willing to tolerate battlefield casualties.

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Data Availability Statement included at the end of the article

Keywords

military and politics, security, conflict, war, government, political survival, accountability, domestic politics

The extant literature on military labor acquisition typically focuses on two stylized systems—compulsory enlistment (conscription) and market-based enlistment (voluntary) (Horowitz et al., 2011; Vasquez, 2005). In this article, we argue that the literature's stylization has given rise to the view that military conflict does not afford governments the same opportunities for political and electoral manipulation that are available to them in other policy areas, such as economic or welfare policy. Under a market-based, professional system, anybody who does not want to earn their livelihood in military service is free to opt out. A universal conscription system, on the other hand, requires everybody to serve in the military. The two systems share that neither of them affords governments with any ability to fine-tune their impact, thereby leaving governments without any levers to use these systems to manage their political survival.

We show that in reality many existing military labor systems afford governments far more room to maneuver. Our extensive data collection demonstrates that the literature's stylized categories miss significant empirical variation that occupies the space between the two prototypical systems of military labor acquisition. Historically, many governments have opted for a hybrid military labor acquisition system, which gives them more control over the distribution of costs and benefits of military conflict and, by extension, the ability to control their political survival.¹

Once we account for the rich variation that has existed in military labor acquisition across the world and across time, it is clear that military conflict, just like any other policy choice, is actively and strategically used by governments in furtherance of their own political survival. In what follows, we show that military labor systems determine a government's political exposure to the battlefield. Given an existing military labor system and the broader political context in any particular country, its government is presented with a risk profile that maps the level of engagement in military conflict into likely political outcomes, determining the government's tolerance for battlefield deaths in pursuit of political survival and military victory.

The two stylized systems of military labor acquisition referred to in previous literature have distinct risk profiles, but, by definition, offer few opportunities for governments to manipulate risk exposure. Universal conscription compels all young men/young adults to serve, thereby exposing a maximally politically diverse part of the population to the risks of military conflict. Stated differently, the risks associated with military conflict reach

every part of the political spectrum, thereby heightening the government's risks of political survival. Thus, governments have very limited ability to influence the distribution of costs and benefits from military conflict across the population.

The picture is quite similar under the other extreme—a market-based/professional military. That system is based on self-selection into military service, which is driven by, among other factors, individual economic need and the lucrative nature of military service, relative to alternative employment opportunities. Governments can obviously manipulate the employment benefits for military service to some extent, but it is too crude and too expensive a lever to be an effective tool for building and maintaining political coalitions.

The situation changes significantly once we move away from these stylized extremes to systems of selective conscription, which allow governments to target or exclude specific subgroups of the population for compulsory military service. Governments with selective conscription have significantly greater room to influence their citizens' exposure to the battlefield. Crucially, they can insulate groups of the population that are particularly valuable to their political survival, such as swing voters in electoral democracies or elites' co-ethnics in non-democracies. Further, the regime can remove groups of the population perceived as distrusted or disloyal from military service altogether. By avoiding threats of regime destabilization—whether at the ballot box, through an uprising, or disenchantment among the selectorate—selective conscription offers governments opportunities not afforded under the stylized extremes of military labor acquisition. We articulate two types of selective conscription—shielding and exclusion—that wartime governments may use to mobilize segments of their society.

One of the many observable implications of our argument is that different military labor acquisition systems, because of their different political risk profiles, result in observable variation in battlefield behavior. We articulate a mechanism according to which political leaders must often weigh military approaches that incur battlefield deaths against other approaches that produce fewer deaths (Sirin & Koch, 2015; Sullivan, 2007; Valentino et al., 2010). We hypothesize that selective conscription, because of the accompanying discretion to limit political exposure, grants governments more latitude to pursue the first type. With greater tolerance for casualties, countries with selective conscription will, on average, suffer more battlefield deaths, all else equal.²

We empirically test our theoretical argument using new and innovative data, which we have collected from a variety of primary and secondary sources. Our *Military Labor Acquisition Dataset* (MLAD), which we introduce in this article, provides the broadest available coverage of military labor acquisition policies for each state in the international system from 1800 to 2015, manually coded from archival, historical, and secondary

sources. We demonstrate that countries with selective conscription systems suffer approximately 7000 more battlefield deaths on average—nearly double the baseline rate—compared to countries with market-based or universal conscription policies. Further, consistent with our theoretical argument, we find that countries with selective conscription experience longer conflicts and a higher rate of victory.³

Our article makes two important contributions to the study of military labor acquisition and military conflict. First, while many scholars have examined the differences between market-based and universal conscription systems (Asal et al., 2017; Choulis et al., 2021; Cohn & Toronto, 2017; Horowitz & Levendusky, 2011; D. L. Kriner & Shen, 2016; Pickering, 2011), ours is the first to identify the critical distinction between universal and various forms of *selective* conscription. We show that the latter allows governments to shield politically valuable constituencies from battlefield casualties, which opens the door for military behavior that is less likely to jeopardize a government's political survival.

Second, we build on previous research that has explored the effects of military labor acquisition on battlefield performance (Horowitz et al., 2011; Vasquez, 2005) by drawing a connection between military labor acquisition and domestic political exposure and subsequently, battlefield deaths or war duration. Our findings that selective conscription systems are associated with higher casualty rates or longer wars resonate with an emerging literature on how domestic political incentives affect the ways in which military conflicts are fought (Anderson et al., 2020; Croco, 2011; Davis, 2023; Koch & Sullivan, 2010; Narang & Talmadge, 2018; Sirin & Koch, 2015; Valentino et al., 2010).

In the remainder of this article, we first review the extant literature and lay out our argument about the domestic politics of conscription. We then discuss a number of observable implications of our argument. In particular, we argue that when a government operates under a selective conscription system, they will choose battlefield behavior that leads to a larger number of deaths or longer wars. Next, we explain our theoretical mechanisms and provide historical examples. Subsequently, we present the data and empirical models that we use to test our argument. This is followed by a discussion of our results.

Domestic Politics of Conscription

Governments at war have two primary objectives: winning the war and staying in power. Pursuit of these two goals is complicated by the fact that they are often in conflict with each other (Capella Zielinski, 2016; Croco, 2011; Debs & Goemans, 2010; Weeks, 2012). Winning a war requires governments to spend money and absorb casualties. As these costs add up, they can erode a

government's support (Saunders, 2018), or even result in a government being replaced (Escribà-Folch, 2013; Geys, 2010). Democratic governments that fail to contain these costs may lose re-election, and both democratic and non-democratic governments may be overthrown, with leaders ousted, exiled, or killed (Goemans, 2000).

Governments have some means to mitigate the risk of waning support and replacement. For example, they could try to spread the costs of war strategically in order to limit political fallout. Unfortunately, from the perspective of governments, many costs cannot be targeted very easily: labor dislocations, food shortages, commodity supply disruptions, and other disturbances to daily life are shared burdens affecting most, if not all, of a country's population (Carter, 2017).

Notwithstanding, governments have some tools at their disposal to target the *economic* costs of war to control their political survival. Taxes offer one such means. Governments can also offer targeted incentives for contributing to the war effort, such as vouchers and, for businesses, military contracts (Rockoff, 2012). When public opinion does not allow governments to raise taxes, politicians may instead turn to borrowing funds to pay for war, pushing the cost onto future generations (Capella Zielinski, 2016; Flores-Macias & Kreps, 2017; Kreps, 2018; D. Kriner et al., 2018).

Yet the economic costs are only part of the price countries pay for being at war. Even more consequential than the economic costs are the human costs paid on the battlefield. When soldiers die in combat, the loss of life is compounded, as communities from which they are drawn experience short- and long-term psychological, social, and economic damage (Angrist, 1990; Hansen & Weisbrod, 1967; D. L. Kriner & Shen, 2010). Even citizens who supported the war initially may sour on it, particularly when casualties exceed initial expectations at the outset of the conflict (Gartner, 2008; Koch & Sullivan, 2010). Politicians may pursue military strategies and tactics that risk relatively fewer casualties such as air strikes (Allen & Martinez Machain, 2018, 2019), but not all states have the capacity to use such methods, nor are they feasible in all conflicts.

The political impact of these costs is also not evenly distributed. A broad demographic—for example, all men in a country—is less likely to organize in response to mass mobilization. The lack of an organizing principle around which to oppose conscription policy means the government can conscript from that group with relatively low fear of destabilization. By contrast, politically-critical groups, or groups with a history of opposition to the regime, may quickly organize in opposition to conscription and contribute to the removal of the government from power. Universal conscription schemes and market-based systems do not allow the government to distinguish among groups and therefore impedes the government's ability to effectively manage its political survival.

Interestingly, previous research has found that the experience of psychological, social, and economic damage tends to be quite localized. For example, D. L. Kriner and Shen (2007) find that the death of an American soldier negatively impacts their home county to a much greater extent than it does their state or the country as a whole. (see also, e.g., Althaus et al., 2012; Gartner & Segura, 1998; Gartner, 2008).⁴ These localized effects also have negative short and long-term economic, political and fertility outcomes (Brodeur & Kattan, 2022; De Bruin, 2018) Although this research is America-centric, there is reason to believe these findings generalize to other conflicts. In theory, that opens up opportunities for governments to target the human costs of war in such a way that their political survival is minimally impacted, but it requires a particular type of institution of military labor acquisition—selective conscription.⁵

Institutions of Military Labor Acquisition

Institutions of military labor acquisition—which we define as the set of policies and procedures by which governments raise militaries—determine governments’ ability to localize the costs of war and thereby shield politically important segments of society from exposure to the battlefield. Governments at war face domestic pressures from many sources—varying levels of public support for the regime, selectorates, swing voters and marginal constituencies, etc.—which constrain the ways by which they can staff their militaries. The two stylized systems of military labor acquisition—universal conscription and market-based—do not allow governments to effectively navigate and mitigate the pressures from these sources. Yet the landscape of military labor acquisition systems is significantly more varied than the stylized narrative suggests. Once we acknowledge the rich variation in military labor acquisition systems, we find that many governments are in a much better position than others to mitigate the risks of military conflict.

We argue that military labor acquisition institutions broadly fall into three categories: market-based conscription; universal conscription; and selective conscription. Market-based labor acquisition systems rely on voluntary enlistment, while universal conscription systems require all individuals falling in certain categories to serve. Under selective conscription systems, governments compel only certain groups of citizens to enlist, using arbitrary procedures to include or exclude groups. This offers the government mechanisms to remove some military-eligible groups from service, with the goal of protecting the stability of the regime.

Governments with market-based militaries compete for personnel against other professions in the labor market. This has the consequence of making a market-based force more expensive (Eikenberry, 2013) but also limits a government’s ability to choose who serves. As such, governments in these

systems have only very limited ability to target subsets of the population for enlistment. They can make wages and benefit packages more attractive to increase the supply of applicants, but in the end individuals still self-select into the profession. Political manipulation of enlistment is hypothetically possible, but severely curtailed in practice. For example, recruiters could be sent to a “politically advantageous” area but the civil-military relations literature suggests that whether or not the residents of this area find the military an attractive employer is entirely dependent upon the desires, culture, history, and economic conditions of the individuals at that location (Dempsey, 2009; Lutz, 2008; Urban, 2010). Beyond this, and more importantly, the costs to recruit, train, and replace a soldier in a market based system is very high (Cohn & Toronto, 2017; Horowitz et al., 2011; Mulligan & Shleifer, 2005).

The ability of governments to successfully fight and win wars with market-based schemes varies. Throughout its post-independence history, India’s market-based military has fought several wars against Pakistan and China, as well as internal conflicts (Cohen, 2020; Khalidi, 2001). It has not faced severe shortages in military labor throughout these conflicts. Further, any selective or universal conscription scheme would require large buy-in from across India’s complex federal democratic system, and no government to date has had the political capital to enact such an agenda. This type of system is an obvious choice for countries where labor exists in large supply to staff the military, and where there is no immediate need—or capacity—to change the status quo.

By contrast, the United States’ invasion of Iraq in 2003 was beset with concerns about US capacity and will to subdue Iraqi insurgencies after the collapse of the Hussein regime. The approximately 130,000 US troops in Iraq at the start of the invasion could not pacify the country, and ultimately the US military withdrew in 2011 without completing many military objectives. Throughout, the US military was plagued by recruitment shortfalls and missed targets, in spite of the efforts by recruiters, improved pay, and reduced standards to increase the pool of enlistees (Golding, 2006; Ryan, 2006).

Governments operating under universal conscription are afforded even fewer opportunities for political manipulation of enlistment. By design, universal conscription requires almost all individuals within certain age and—often—gender categories to enlist. As a result, the costs of war are more or less evenly spread across society. Governments, therefore, cannot easily shield political supporters from military service. Of course, no conscription system is truly universal: governments may provide exemptions and deferments, but these tend to be narrowly defined to protect wartime industrial capacity and civilian morale.⁶

For example, Finland maintains a universal conscription scheme wherein up to 80% of eligible male citizens serve in the military for a short time. Annual lotteries dictate military service, and very few exemptions, largely on the grounds of fitness, are granted. Finland’s political history—the threat of

annexation or subjugation from neighboring Russia, and strong ethnic homogenization—permits political leaders to maintain such a scheme (Hadar & Häkkinen, 2020; Kosonen & Mälkki, 2022). Indeed, Finnish conscription may be seen as state-sponsored socialization as well as an effective mechanism for defending a large border. This situation is emblematic of several states across the world facing similar threats. During its war of independence from 1991 to 1995, Croatia employed universal conscription, which permitted the fledgling military to rapidly expand and ultimately defeat Serbian armies (Berdak, 2013). Yet Serbia also employed universal conscription from 1992 to 2011, when it resulted in defeats in 1995 and in 1999 against significantly smaller opponents.

In stark contrast to market-based and universal systems, governments with selective conscription systems can compel enlistment, ensuring an ample supply of soldiers, while at the same time allowing them to limit enlistment to certain groups of the population. Governments face pressures from segments of society, which in turn constrains their capacity to wage war. If the government mobilizes these groups, the government's ability to continue prosecuting the war is threatened. The government needs to be able to both trust that mobilized soldiers will faithfully fight the war and that the communities from which mobilized soldiers are drawn will not retaliate against the government, either at the ballot box or using extrajudicial means. If a government anticipates the potential emergence of such behavior, it has a strong incentive to conscript soldiers from some groups but not from others. Yet if it does, the government opens itself to criticisms of favoritism or unfairness. Therefore, we argue that selective conscription schemes are implemented using a variety of rules, opaque procedures, and public appeals for shared sacrifice. Examples range from military service taxes and replacements, or a multi-tier organizational structure with unclear accountability, to discretionary deferments systems that prevent citizens from examining the scope of who does and does not join the military.⁷

Selective conscription broadly operates through two mechanisms: *shielding*, under which the government protects a segment of society it deems so politically or economically valuable that it would not risk sending citizens from this group into combat; and *exclusion*, under which the government refuses to enlist a segment of society for fear of civil disruption.⁸ If the government were to conscript from either shielded or excluded groups, it risks substantial public backlash. The discriminator is loyalty to the regime: shielding works to protect from enlistment those who are suspected of being loyal but potentially pivotal in maintaining support for the government, while exclusion works to protect from enlistment those who are suspected of being disloyal to both the government and regime.

Governments employ shielding as a means of protecting valuable and loyal groups from enlistment. The government identifies these groups as those that

are essential for the maintenance of the regime—skilled manufacturing laborers, pivotal voters, economic elites—but do not have a strong loyalty to the government and could result in its toppling, either democratically or undemocratically. The government protects these groups from enlistment by creating a series of opaque procedures designed to disguise its true motives from the general public—discretionary control over local draft boards, deferment classes and categories, and targeted exemptions that are driven disproportionately from the shielded group—and over-enlisting from other groups. Then, the government constantly assesses its public to ensure that it has achieved an ‘optimal’ balance between shielding and meeting enlistment targets. The resulting military consists of a large number of soldiers with variable loyalty to the government, but are not pivotal to its stability and therefore the government can prosecute the war with relative flexibility.

Governments employ exclusion as a means of preventing potentially disloyal groups from serving in the military. The rationale is twofold: if enlisted as soldiers, giving weaponry to these groups might destabilize the wartime military; alternately, the civilian portion of these groups might revolt against the regime. Anticipating these concerns, the government designs its conscription scheme, and military composition, to ensure that units in the excluded group do not enlist, and if they do, are not trained in and experience combat. Excluded groups could include, but are not limited to: ethnic minorities with a history of independence or irredentism; pacifist religious groups; and linguistic monolingual minorities where there is little appetite for an official tolerance of many languages. Unlike shielding, the government’s mechanisms are more straightforward using legislation: in many cases, they want to make it clear that the group is being excluded. The resulting military composition is much more loyal to the regime but consists of groups that might be pivotal to its stability, leaving the government much less discretion to prosecute the war.

Examples of selective conscription via shielding include discretionary conscription (i.e., arbitrarily waiving the requirement to serve), as well as outright statutory favoritism. In North Korea, all male citizens are required to serve, except for the children of the political and military elite, as well as those whose political loyalty is considered questionable (Worden, 2008). Elsewhere, policies may superficially look similar to those under universal conscription (e.g., granting deferments), but are routinely and asymmetrically abused to favor particular groups, such that conscription is *de facto* selective. For example, France’s 1793 *levée en masse*, the first universal conscription system, changed to a selective system just seven years later when Napoleon’s Consulate began allowing citizens to buy themselves out of military service, effectively exempting the landed aristocracy (Poutvaara & Wagener, 2009). Similar policies have been embedded in military labor procurement in the

United States during the Civil War, the former Soviet Union, and modern China and Russia.

An example of a country employing exclusion is Cote d'Ivoire; gaining independence in 1960, the country has consistently maintained a small army through a selective conscription scheme (Schiel et al., 2017). During the country's two civil wars (2002–2007; 2010–2011), the government did not change its conscription policy to mobilize large numbers of soldiers to fight the rebels. This is due to the country's ethnic and religious fractionalization, as well as the potential threat of an ethnically-diverse military with dubious loyalty to the civilian government (N'Diaye, 2005). These twin factors prohibited the government from mobilizing a large army, as it could not trust conscripts to loyally fight for the regime. This case is representative of many states, both democratic and authoritarian, that refrain from conscripting large segments of the population for political reasons, primarily because these conscripts pose either an electoral, or even existential, threat to the governing party.

Once a conscription scheme is selected, the government must then prosecute the war. How does each system fare in keeping casualties low, or the war short? In the following section we examine these dynamics.

Observable Implications

Governments employing a market-based system are typically not able to effectively shield politically valuable groups from service. Because the government must compete in the labor market by offering incentives such as attractive pay structures, benefit packages, and other quality of life perks (Bailey, 2007; Levy, 2013), these forces are more expensive to recruit and maintain (Cohn & Toronto, 2017; Mulligan & Shleifer, 2005). Volunteers who die are therefore more costly to replace (compared to conscripts), compared to casualty-intensive military strategies which the government may otherwise pursue (Horowitz et al., 2011).⁹ For example, the American military has been a market-based force since the end of the Vietnam War.¹⁰ Relatively few soldiers died in the war in Afghanistan, but because the military was not able to commit hundreds of thousands of troops to exert control across the remote regions of the country, the Taliban were able to reconstitute and deny the US their ability to complete major objectives. For countries fighting wars like the US in Afghanistan, the cost of recruiting troops constrains the government's ability to take on a large number of deaths.

Based on the discussion in the previous section, it is clear that selective conscription systems fundamentally differ from market-based and universal systems in that they allow politicians to strategically manipulate enlistment in a manner that affords them significant opportunities to protect politically important groups from military service—for example, on grounds of ideology

or social class—and, by extension, from the human cost of war. The ability to target certain groups for enlistment, in turn, provides politicians in these systems more latitude to take on battlefield deaths, continue to fight wars, and achieve military success without jeopardizing their chances of political survival. This leads to our first testable observable implication:

H1: Countries with selective conscription systems, on average, experience more battlefield deaths during war than countries with market-based or universal systems.

We expect lower casualties among belligerent states with market-based forces and universal conscription for different reasons. While incurring casualties under universal conscription risks political replacement because it may cause a loss of support among the public or key elites, recent research has shown that the same mechanism exists, albeit in weaker form, in countries with market-based recruitment. For example, the public backlash against battlefield deaths is more muted when casualties occur among those who consented to deployment (Krebs et al., 2021). Instead, market-based systems tend to experience lower casualties due to the much higher cost of replacing troops (Horowitz et al., 2011).

In addition to our main hypothesis, our theory also suggests a testable auxiliary hypothesis. Since we anticipate that states using selective conscription reduce the political costs of battlefield deaths, these states will also have more will and capacity to pursue their objectives, with little regard for the costs. This will lead states with these types of systems to fight relatively longer wars, on average, than states that do not have the political flexibility to absorb the costs of war as effectively. In contrast, we expect that the relative lack of flexibility will lead states employing universal or market-based systems to fight relatively shorter wars.

H2: Countries with selective conscription systems, on average, experience longer wars than countries with universally-conscripted or market-based militaries.

Before we introduce our Military Labor Acquisition Dataset (MLAD), it is worth highlighting one significant benefit of our theoretical argument, as well as one limitation. Readers will have noticed that we have consistently avoided talking about elections as primary means of holding governments accountable; instead, we have used the language of political survival. The reason for this is that our theoretical argument is agnostic about regime type: we expect our predictions about military labor acquisition institutions to apply to both democracies and non-democracies alike.¹¹

Of course, there are important differences across regime types in how military labor acquisition institutions are designed and implemented (Cowen, 2006; Levi, 1996), how wars are fought (Reiter & Stam, 1998; Talmadge, 2015; Valentino et al., 2010), and how politicians are sanctioned for poor performance on the battlefield (Chiozza & Goemans, 2004; Croco, 2011; Goemans, 2000; Reiter & Stam, 1998). For example, a British Prime Minister may fear only a vote of no confidence or the loss of re-election if battlefield casualties rise beyond what the party or public will tolerate. In contrast, war may leave a North Korean dictator vulnerable to a military coup if casualties erode support among the selectorate. Yet notwithstanding these differences, the mechanism underlying our theoretical argument applies across the full spectrum of regimes. We expect that selective conscription diminishes the threat of political sanctions, relative to universal conscription and market-based institutions, because selective conscription affords governments in any regime the ability to insulate politically valuable groups from exposure to the battlefield.

We note a limitation of our argument: military labor acquisition institutions, of course, do not spontaneously materialize; they are a political choice in themselves (Flynn, 1998; Mulligan & Shleifer, 2005). However, for our purposes here, we are comfortable treating these institutions as “quasi-parameters” (Greif & Laitin, 2004), that is, institutions that, while subject to change over the long run, are typically fixed in the short term. Stated differently, when a government is confronted with the choice to go to war, it rarely goes back to the drawing board first to change its institution of military labor acquisition. This is confirmed by our empirical data: 1 only 1.4% of our cases—just six of 410 observations—include countries that switched conscription systems mid-war. Thus, for the purpose of our cross-national analysis below, we take military labor acquisition institutions as fixed.¹²

Data and Methods

To recap, our two hypotheses are derived from the theoretical argument that selective conscription systems, relative to other military labor acquisition institutions, allow governments to take more risks on the battlefield without significantly increasing the risk of domestic backlash against battlefield deaths. To test this argument, we first needed to collect data on our core independent variable—military labor acquisition institutions. Using archival and historical sources, we coded each country’s military labor acquisition institution as either market-based, universal conscription, or selective conscription for each year from 1800 to 2015. We refer to this new data set as the Military Labor Acquisition Data (MLAD). Because our argument centers on who actually bears the human cost of war and the political implications thereof, we focus on *de facto* military labor acquisition

institutions. This approach also allows us to readily distinguish universal conscription with standardized deferments from selective conscription with policies that look similar but in fact are politically motivated exemptions from service.

Conceptualizing and Coding Military Labor Acquisition Institutions

The initial step is to carefully define the labor acquisition systems we have identified. We first set out to distinguish market-based forces from systems that rely on conscription. *Market-based* recruiting is a system that relies on voluntary military enlistment by a country's residents. In contrast, conscription is defined as any system wherein the state *compels* its residents to serve. We code countries as having market-based forces, if there is either no law on the books requiring military service, or if any previous conscription system is in a dormant, unenforced state.

The next step is to further distinguish systems within the category of conscription. The policies and procedures governing conscription exist on a continuum from completely selective to entirely universal (N. W. Toronto & Cohn, 2020), with no system truly fitting into either of these extremes. In an ideal world, we would operationalize these extremes and the various gradations between them. Unfortunately, coding such fine gradations between states' conscription systems is impossible in practice, given the lack of codified rules around military labor acquisition across context and time, as well as the deterioration of historical evidence on these rules. Given these challenges, we believe that the most feasible and useful way forward is to categorize conscription systems as either "universal," or "selective" (N. W. Toronto & Cohn, 2020).¹³

We define *universal* conscription as a system wherein a government compels military service from members of all segments of society, with the exception of narrow exemptions based on age, gender, medical fitness, and occupations deemed essential to the war effort. Selective conscription systems, in contrast, compel military service only from some segments of society. While exemptions based on age, gender, medical fitness, and occupation may or may not be granted in selective systems, the criteria defining which segments of society are compelled to serve in the military include at least one *ambiguous* (e.g., a provision in the conscription law that excuses certain war related industries but does not clearly define what these are and how they can be determined), *arbitrary* (e.g., a local administrator is tasked with meeting a conscription quota and chooses those that are easily found or that live in a certain densely populated area), and/or *politically motivated* provision.

We start the data collection process by gathering any evidence we can find on military labor acquisition for each state-year. We then determine if there is

any evidence of conscription, and finally, if any of the policies and procedures around conscription exempt segments of society for ambiguous, arbitrary, and/or politically motivated reasons. Exemptions can, for example, be based on membership in a specific religious, ethnic, racial, or linguistic group, or apply to certain economic classes by means of a replacement or a tax. Local draft boards, where arbitrary and opaque decisions may be made, are another prominent means by which exemptions are granted. Since nearly all countries exclude children, the elderly, and the infirm from service, we consider conscription with these exemptions still universal. Similarly, since women were prohibited from military service for the vast majority of states and years in our data, we allow gender-based exemptions under universal conscription. Finally, since most countries exempt skilled workers whose labor is vital to the war effort—e.g., miners, machinists, scientists, and university students—we also allow these exemptions under universal conscription. More generally, the take-away is that universal conscription systems can still include exemptions; however, any exemption must fit at least one of the following criteria for a system to be considered universal:

1. It lays out systematic rules (i.e., does not generate uncertainty or ambiguity about who is to be exempted);
2. It is justified by the war effort (i.e., explicitly tied to a reasonable argument for why conscripting individuals from the exempted category would detract from the war effort);¹⁴
3. It is not politically-motivated (i.e., does not clearly stem from politicians' attempt to distribute military service unequally across society in furtherance of their political survival).

These basic rules allow us to create a series of questions that provide our coders with a clear scheme for coding all state-year observations in our data. The main objective is to categorize exemptions first as either *selective* or not and then to determine which exemptions cause the state-year to fall into one of three selective categories (Figure 1).¹⁵

These rules allow us to take the available historical data on each state's military labor acquisition system and place each observation into the three main categories, as well as to further distinguish within the category of selective systems. The coding rules and procedures we developed yield an inter-coder reliability score of 88.2%. We consulted a wide variety of archival and secondary sources to code our independent variable.¹⁶ Our first step was to consult a standard set of sources: Keegan (1979), N. Toronto (2005), Horeman and Stolwijk (1998), and Central Intelligence Agency (2020). These sources greatly assisted us with the categorization of market-based and conscription systems for much of the second half of the 20th century and to a much more limited extent with observations dating before

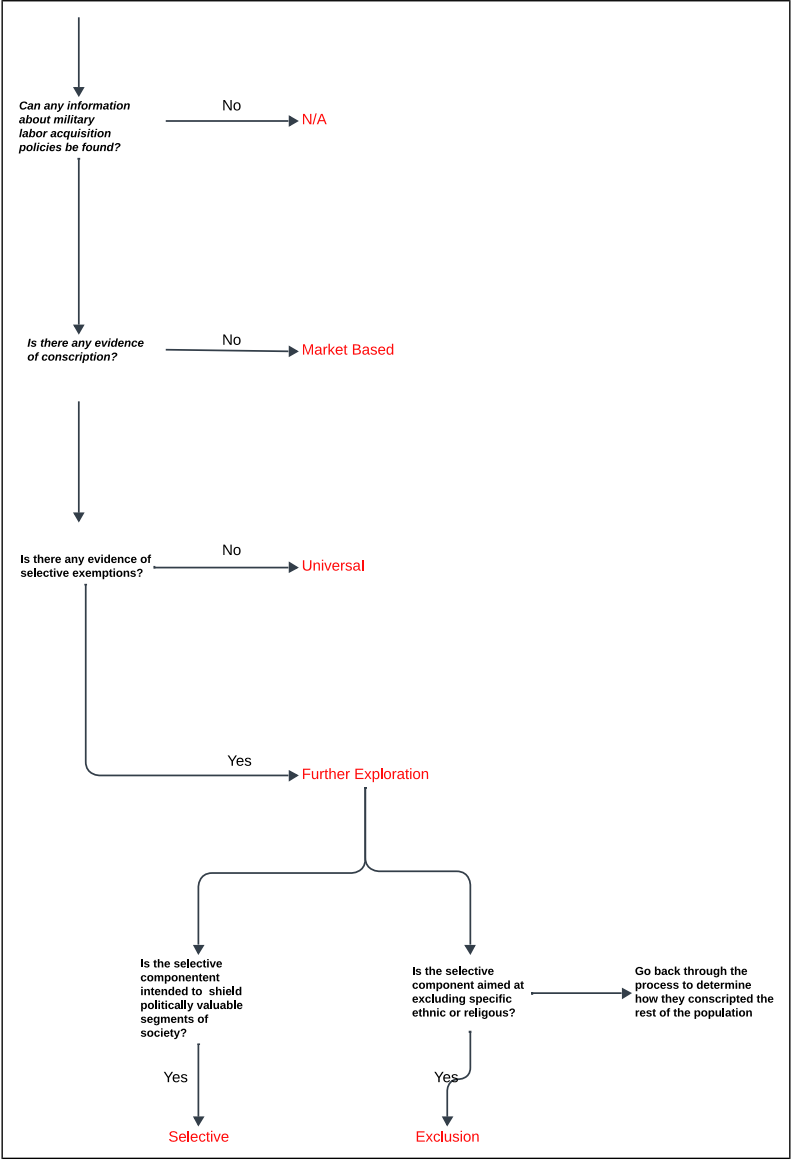


Figure 1. Coding flow chart. Text in red indicates the final value assigned to the state-year.

1945. The data collection for the period before 1945 and the more fine-grained categorization of conscription into universal and selective systems relied on extensive research of primary sources by the authors (see [Appendix 1](#) for detailed information).

Descriptive Statistics

Our final MLAD data set is an unbalanced panel of 15,015 observations with 274 unique states and 216 unique years.¹⁷ The panel is unbalanced because we do not have state-year observations for the period before a state was formed or after it ceased to exist (e.g., Austria-Hungary appears in the data only from 1816 to 1918). In our data, we have 3,998 country-years of universal conscription (22.87% of observations), 6,638 observations of market-based recruitment (38.10%), and 4,378 country-years of selective conscription (25.04.%).

Figure 2 shows a line graph with the percentage of observations across different labor acquisition systems by decade. We note that selective conscription peaked in the 19th century and has steadily declined leading up to the 2010s at which point it accounts for only 19% of observations. The proportion of observations with a market-based force drops in the early 19th century and then increases to about 70% of observations in the 2010s. Universal conscription has followed a different path, making up around 20% of observations in the early 1800s, increasing throughout the mid and late 1800s (hovering around 40%), and then falling throughout the 20th century to the point where it accounted for only about 10% of the observations in the 2010s.¹⁸

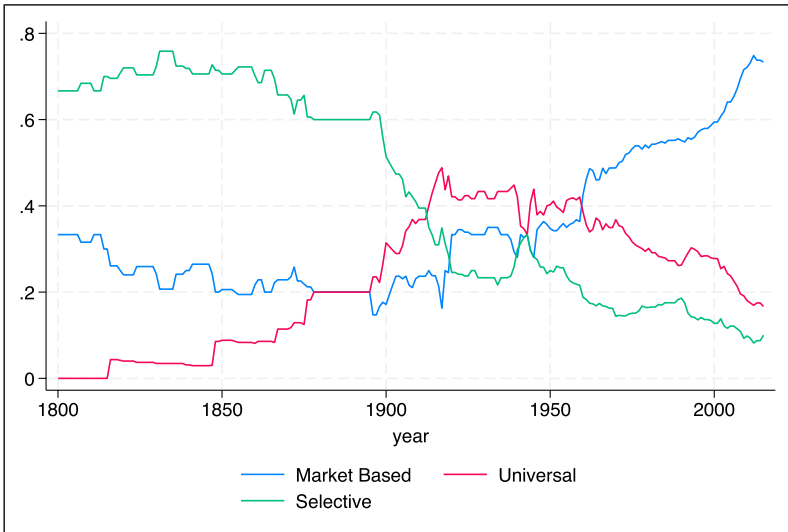


Figure 2. Percent of different types of military labor acquisition systems across decades.

Data Analysis

To test our hypotheses, we merge MLAD with the *Project Mars* data (Lyall, 2020).¹⁹ *Project Mars* data captures all belligerents that fought in a conventional war during the period 1800 to 2011. As MLAD only includes state actors and excludes civil wars, we drop all non-state belligerents from the *Project Mars* data. This results in an initial set of 491 belligerent observations over the span of our data, which reduce to 410 when we remove missing observations.²⁰

For our first dependent variable, associated with our first hypothesis (*H1*)—battlefield deaths—we use *Project Mars* data covering all wars fought between conventional armies, excluding civil wars, from 1800 to 2011 (Lyall, 2020). Deaths are expressed as a count of the number of soldiers killed in action (KIA) by country and war. Only deaths that occurred on the battlefield are included. The mean of this variable is 56,327 with a standard deviation of 530,242 while the minimum is zero and the maximum is 11 million. Soldiers who went missing or died of illness are not counted toward the total. We again note that these data only include combatants recognized as countries.²¹ However, this selection rule does not threaten our analysis because non-state actors rarely have the capacity to organize and implement conscription systems.²²

For the purposes of testing *H1* on battlefield deaths, our unit of analysis is the country-war (not dyads). For example, separate observations capture France in World War I, France in World War II, and Germany in World War II. Since countries can engage in multiple conflicts at the same time, our data are technically speaking cross-sectional.

Many potential mechanisms can impact battlefield deaths, so we include a number of control variables in our models.²³ Because countries that join an already existing war may suffer fewer casualties, we control for whether the country joined the war after its initial commencement.²⁴ Further, a long tradition in democratic peace theory suggests that democracies are less likely to fight wars that result in heavy casualties (Gleditsch, 1992; Reiter & Stam, 2010; Valentino et al., 2010).²⁵ We therefore control for whether the country is a democracy, using the dichotomous measure created by Boix et al. (2013). As rich countries may have the ability to employ technology that limits its own casualties and thus fight short wars with low casualties (Beckley, 2010), we include a measure of GDP per capita (Fariss et al., 2022). Research has also shown that pre-war inequalities, if carried over into the military, can have an effect on military performance and subsequent battlefield deaths. To account for this, we use the military inequality index developed by Lyall (2020). Finally, some militaries may use a conscription system while also taking volunteers. To account for this, we control for whether or not the state

employed a composite military or a military recruited via multiple means (Lyall, 2020).²⁶

Given the direct correlation between the magnitude of a conflict and the number of casualties, we also incorporate number of troops deployed as a proportion of the states population. Further, since a larger population will theoretically open up the possibility of a state being willing to take on more casualties, we control for the total number of a state's citizens (Fariss et al., 2022).²⁷ Following the convention in the literature (e.g., Lacina et al., 2006), we include a dummy variable for whether the country-war is part of either World War. The extreme casualties suffered during each of these conflicts are serious outliers; without these controls, they would exert enormous influence on our estimates and threaten the generalizability of our findings.²⁸ Since we are using a count model to analyze our main hypothesis, it is important to account for time. To this end we introduce a logged duration control in Table 1 Model 2. In line with best practice, we do this by using the logged duration (in days) of the conflict (Hilbe, 2014). Beyond these controls, we control for a country's capacity to wage war relative to their opponent, using a measure devised by Lyall (2020). We employ a measure of whether certain segments of society are excluded from conscription, to account for a small number of states that prohibit these groups from serving in the military. We account for whether or not the observation was in the modern era, as the literature has shown that there are fewer battlefield deaths in the modern era (Fazal, 2014).²⁹ Finally, we control for whether a country joined an ongoing war, as such states may suffer fewer battlefield deaths and are more likely to be on the winning side (Downes, 2009).

We test our main hypothesis by regressing battlefield deaths on our military labor variable and a series of control variables using a standard negative binomial regression. We then turn to assessing our auxiliary hypotheses. To test our first auxiliary hypothesis, we regress our military labor acquisition variable, and a series of controls, on durations using a Cox proportional hazards model.³⁰

Results and Discussion

We present the findings from our analyses of *HI* in Table 1. In our main model, we set the selective category as the baseline. The first column reveals a negative and statistically significant coefficient for universal systems of conscription. This suggests that a state employing a universal system during a war will experience lower battlefield deaths relative to the baseline category, a state with a selective system. The size of this coefficient is rather large. Specifically, there is a 58.1% reduction in the number of expected battlefield deaths for a universal system relative to selective conscription. We further find that the coefficient for the market-based category is negative and statistically

Table 1. Regressions on Battlefield Deaths.

	1	2	3	4	5	6	7
	Main	With duration	Post-1945	No multi-party	No deployment	Initiator	No initiator
Market based	-1.784*** (0.331)	-1.323*** (0.277)	-2.254** (0.965)	-1.632*** (0.305)	-1.767*** (0.355)	-2.481*** (0.367)	-1.193*** (0.400)
Universal	-1.420*** (0.346)	-0.834*** (0.279)	-3.607*** (0.979)	-1.125*** (0.361)	-0.996** (0.396)	-1.095*** (0.390)	-1.489*** (0.327)
World war	1.874*** (0.308)	1.532*** (0.280)		0.301 (0.411)	3.474*** (0.533)	2.136*** (0.329)	1.664*** (0.346)
Modern	0.897*** (0.321)	0.741*** (0.282)		0.945*** (0.329)	1.157*** (0.400)	-0.114 (0.301)	1.468*** (0.362)
Joiner	0.092 (0.338)	-0.210 (0.310)	4.144*** (0.843)		-0.008 (0.487)		0.566 (0.372)
Democracy	-1.222*** (0.252)	-1.123*** (0.332)	-1.029*** (0.393)	-0.834*** (0.320)	-1.231*** (0.333)	-0.622* (0.324)	-1.320*** (0.300)
GDP per capita	-0.095* (0.051)	-0.065 (0.043)	-0.176 (0.127)	-0.138*** (0.067)	-0.107* (0.055)	-0.060 (0.063)	-0.033 (0.047)
Military inequality	-1.499* (0.854)	-1.576** (0.758)	-0.552 (1.402)	0.353 (0.955)	-1.095 (1.112)	0.359 (1.368)	-1.243 (1.012)
Composite	0.471** (0.202)	0.520*** (0.199)	-0.060 (0.527)	0.810*** (0.261)	0.750*** (0.224)	-0.676** (0.292)	1.084*** (0.268)
Pop./Million	0.003*** (0.001)	0.002*** (0.001)	-0.002* (0.001)	0.002* (0.001)	0.002 (0.001)	-0.000 (0.001)	0.002** (0.001)

(continued)

Table 1. (continued)

	1	2	3	4	5	6	7
	Main	With duration	Post-1945	No multi-party	No deployment	Initiator	No initiator
Prop. Pop. Deployed	0.403*** (0.092)	0.285*** (0.062)	0.363*** (0.123)	0.477*** (0.105)		0.500*** (0.127)	0.356*** (0.058)
Relative capabilities	-0.828 (0.555)	-0.618 (0.531)	0.468 (1.212)	1.200** (0.533)	-0.612 (0.779)	-0.750 (0.808)	-0.829 (0.768)
Exclusion	0.248 (0.262)	0.424** (0.213)	-0.583 (0.584)	0.694** (0.295)	0.102 (0.244)	-0.416 (0.395)	0.792*** (0.280)
ln (Duration)		0.419*** (0.085)					
Constant	9.703*** (0.586)	7.078*** (0.684)	12.082*** (1.211)	7.981*** (0.670)	9.955*** (0.702)	10.017*** (0.670)	8.483*** (0.668)
lnalpha	1.018*** (0.057)	0.890*** (0.074)	1.007*** (0.125)	0.802*** (0.062)	1.213*** (0.056)	0.969*** (0.092)	0.883*** (0.060)
Observations	418	418	81	227	418	193	225
Log likelihood	-4064.487	-4023.685	-746.3151	-2040.480	-4128.543	-1800.3056	-2233.5795

* $p \leq .10$ ** $p \leq .05$ *** $p \leq .01$ in a two-tailed test.

significant. This coefficient corresponds with a 65.0% reduction in the number of battlefield deaths relative to the selective category. This suggests, as expected, that, due to the high-costs of replacement and the inability to shield, countries employing market-based systems will suffer fewer battlefield deaths than those that employ selective systems. Overall, these results are in line with our theoretical argument, specifically, that states employing a selective system of conscription will tend to be more tolerant of taking on a higher number of battlefield deaths.

To further assess the relative size of the effects of Model 1, we plot the results in [Figure 3](#). This figure provides the predicted number of battlefield deaths by labor acquisition system. It shows that countries with market based forces and universal conscription are expected to experience approximately 4,229 and 6,740 deaths per war, respectively. In contrast, countries with selective conscription are predicted to experience far more, around 27,964 deaths per war, more than double the rate of deaths under universal and market-based systems. These differences are substantively large and statistically significant.³¹ Consistent with our theoretical argument, selective conscription correlates with more battlefield deaths (relative to other military labor systems).

To ensure that our results are not simply a product of model specification, we present five additional models as robustness checks in [Table 1](#). First, we want to ensure that our findings are robust to the inclusion of duration. As expected, even with the inclusion of the logged duration variable, our results remain largely unchanged. The results of this analysis can be found in Model 2. Next, we want to ensure that our findings are not a result of anachronistic

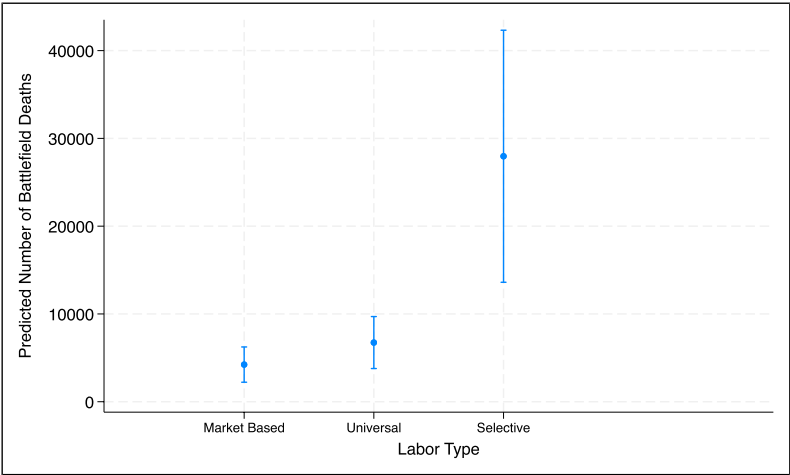


Figure 3. Battlefield deaths by labor type (main model).

fighting practices that were a unique feature of 19th and early 20th century wars. To check that this is not the case, in Model 3, we subset our data so that we are only looking at wars that took place since 1945, the end of World War II. As with our previous robustness checks, our results remain unchanged. In Model 4, we constrain our data to only include cases wherein a state is not fighting a multi-party war. The results for this analysis remain substantively unchanged from our main model. In Model 5, we remove the control for the proportion of the population that was deployed. This allows us to show that our results are robust to excluding a potentially endogenous confounder.

One could assume that since an initiator chooses when to start the war and can more fully dictate the pace of fighting, the number of battlefield deaths the initiator suffers is a better reflection of the political shielding offered to a government by selective conscription. To further investigate *Hypothesis 1*, we re-estimate our main model but first constrain our data to only include initiators, then constrain the data again to only include non-initiators. This is an important test, as it demonstrates that countries with selective conscription—on both offense and defense—suffer more battlefield deaths. The results of this analysis are presented in Model 4 of [Table 1](#).

The table shows that the coefficients for both universal and market-based labor acquisition are negative and statistically significant in Models 6 and 7. This suggests that regardless of whether the state is acting as an initiator or a non-initiator, when they employ a system of selective conscription, they are willing to suffer more battlefield deaths relative to the other two labor types.

As mentioned in our theory, we have reason to suspect that when a state employs a system that relies upon exclusion, it will largely behave in a manner consistent with how it conscripts the rest of the population.³² For example, if a state excluded certain ethnic or religious groups from conscription but selectively chose from the rest of the population, their behavior on the battlefield would be similar to that of a state employing a purely selective system. If anything, the political pressure on the leader to avoid battlefield deaths will be more intense as the state will be forced to draw more heavily upon the rest of the population. The same would hold true for a state that excludes some groups and universally conscripts the rest of the population. Alternatively, it could be expected that when a state excludes its potentially most disloyal soldiers, those that are left over may be willing to fight harder and suffer more, thus leading to a higher number of battlefield deaths. To test these two possibilities, we introduce a model wherein we recode our military labor variable into five categories. The first three categories are consistent with the coding we used in the main analysis. The next two are new. The first of these is universal exclusion. In this labor system, a state excludes a potentially disloyal group and universally conscripts the rest of the population. The second is a system of where most of the population is drawn from selectively, while a

distrusted group is excluded. These systems are categorized as selective-exclusion.³³

We present the results of this analysis in Table 2. The results of his additional analysis are largely in line with our expectations. We find that states that employ a universal-exclusion system largely behave in a manner consistent with universal systems of conscription. Specifically, the coefficients for

Table 2. Regressions on Battlefield Deaths.

	(1)
Market based	-1.702*** (0.379)
Universal	-1.760*** (0.374)
Selective-exclusion	-1.533*** (0.482)
Selective-both	0.510 (0.394)
World war	1.886*** (0.278)
Modern	1.170*** (0.374)
Joiner	0.294 (0.320)
Democracy	-0.942*** (0.265)
GDP per capita	-0.098** (0.048)
Military inequality	-0.946 (0.849)
Composite	0.531** (0.214)
Pop./Million	0.003* (0.001)
Prop. Pop. Deployed	0.388*** (0.077)
Constant	9.176*** (0.488)
Inalpha	1.012*** (0.057)
Observations	395
Log likelihood	-3859.321

* $p \leq .10$ ** $p \leq .05$ *** $p \leq .01$ in a two-tailed test.

Table 3. Cox Model on War Duration.

	(1)
Market based	0.035 (0.120)
Universal	0.373*** (0.129)
World war	−0.240 (0.192)
Democracy	0.002 (0.114)
GDP per capita	0.001 (0.023)
Initiator	−0.022 (0.090)
Joiner	−0.044 (0.232)
Military inequality	−0.285 (0.451)
Modern	0.129 (0.174)
Composite	−0.167 (0.112)
Prop. Pop. Deployed	−0.129*** (0.028)
Population/Million	−0.000 (0.001)
Death rate	0.000*** (0.000)
Relative capabilities	0.312 (0.238)
Exclusion	−0.090 (0.145)
Observations	418
Log likelihood	−2086.372

* $p \leq .10$ ** $p \leq .05$ *** $p \leq .01$ in a two-tailed test.

both variables are both negative and statistically significant as well as nearly identical. The predicted battlefield deaths for these two categories make the similarities between the two even more apparent. Specifically, states that employ a universal-exclusion system are expected to suffer 7,946 battlefield deaths per war while a state employing a universal system is predicted to experience 7,650 deaths. On the other hand, there is not a statistically

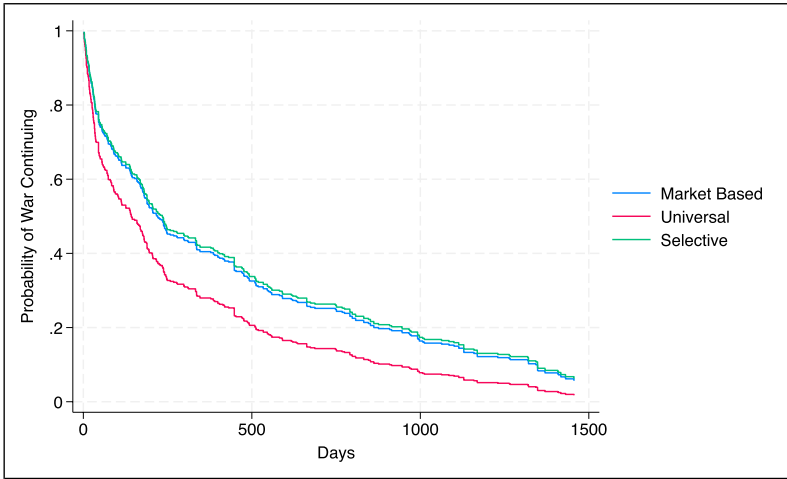


Figure 4. Survival curves by labor type, main model.

significant difference between selective-exclusion systems and purely selective systems.

It is important to note that these findings say very little about the relationship between exclusion and battlefield performance. This is because battlefield deaths, as we argue here, are more likely to be a product of decisions made at a strategic level that are influenced by the institutions implemented to extract military labor. All of this is to say that battlefield deaths are a very imprecise measure of performance. To further probe the relationship between the labor systems and performance, we estimate a logistic regression on a dichotomous mass desertion variable. The results of this analysis can be found in [Appendix 6](#). However, we do find that states that exclude potentially disloyal elements from military service while universally conscripting the rest of their population are much less likely to experience mass desertion than selective conscription or even universal systems of conscription alone. While this is suggestive of a relationship, much more needs to be done to explain the relationship between military labor and battlefield performance.

We now move to our analysis of auxiliary hypothesis (H2). According to this hypothesis, we anticipate that the desire to limit battlefield deaths will lead to shorter wars when states are employing universally-conscripted forces. To perform this analysis, we use a Cox proportional hazards model. In addition to the potential confounders included in the battlefield death analysis, we also include a measure to control for relative capabilities, as an imbalance of capabilities has been shown to lead to shorter wars, as well as the initiator variable, which has been shown to be associated with war duration ([Bennett & Stam, 2009](#)). Finally, we control for the death rate (deaths per day) as the

Table 4. Cox Model on War Duration (Five Category Labor Model).

	(1)
Market based	0.027 (0.125)
Universal	0.407*** (0.134)
Selective-exclusion	0.446** (0.201)
Selective-both	−0.246 (0.232)
World war	−0.269 (0.187)
Democracy	−0.038 (0.117)
GDP per capita	−0.004 (0.024)
Initiator	0.009 (0.089)
Joiner	0.039 (0.204)
Military inequality	−0.235 (0.472)
Modern	0.054 (0.189)
Composite	−0.191 (0.122)
Prop. Pop. Deployed	−0.129*** (0.027)
Pop./Million	−0.000 (0.001)
Death rate	0.000*** (0.000)
Relative capabilities	0.307 (0.246)
Observations	395
Log likelihood	−1947.281

* $p < .10$ ** $p < .05$ *** $p < .01$ in a two-tailed test.

intensity of the war has been shown to lead to shorter wars (Weisiger, 2016). The results of this model are presented as coefficients rather than hazard ratios. A positive coefficient suggests a higher probability of the war ending, or a shorter war, while a negative coefficient suggests a longer war.

Our estimated coefficients in [Table 3](#) show strong support for this hypothesis. Specifically, we find that the coefficient for the universal conscription variable is positive and statistically significant. This suggests that relative to a selective system, states that employ universal conscription experience shorter wars; at any given point in time throughout the duration of war, states employing universal conscription are 35% more likely to have their wars end than states that employ a selective force. We suspect that the implementation of universal labor acquisition systems will make states less willing to take on battlefield deaths and thus lead to shorter wars.³⁴

To further illustrate the effect of various systems of military labor acquisition on war duration, we present the predicted survival curves for each military labor type in [Figure 4](#). This graph shows that for short wars, less than 50 days, there is relatively little difference between the different military labor systems, with each of them having around a 0.8 probability of continuing. At 250 days, there is considerable divergence. Selective systems are predicted to have around a 0.55 probability of continuing and market-based forces around 0.45. However, universal systems have around a 0.30 probability. As wars progress, this difference continues to persist. This offers support for our theory that states which employ a selective system of conscription, due to their ability to shield politically valuable groups, are willing to fight longer, due to their relative willingness to take on battlefield deaths.

Once again, it is crucial to verify that exclusion is not influencing a duration factor that we have not fully considered. To mitigate this possibility, we present the model in [Table 4](#), illustrating the outcomes of the same model analyzed in [Table 3](#). It is evident that, akin to our findings in the analysis of battlefield deaths, the selectively excluded category is exhibiting behavior similar to that of the universally included category. We attribute these resemblances to the similarities between these systems, particularly the practice of excluding potentially problematic groups from conscription and subsequently conscripting the remaining population universally.

Conclusion

We have argued that conscription systems structure the relationship between domestic political incentives and wartime military behavior. Battlefield deaths can threaten incumbents' political survival, but selective conscription gives politicians more flexibility to determine who fights, granting them more latitude to pursue risky, casualty-intensive strategies. Using the new Military Labor Acquisition Data (MLAD), we show that countries with selective conscription policies fight shorter, more deadly wars, and win them more often, compared to countries with alternative military labor acquisition institutions. Taken together, these results provide substantial evidence that

selective conscription shapes wartime behavior by structuring politicians' domestic political incentives.

Our findings point to a number of avenues for future research. For one, because mid-war changes to conscription systems are so rare, we have taken conscription systems as largely fixed in our main empirical analysis. Yet military labor acquisition policies shift over time, often for political reasons. Scholars working on conscription systems and warfare may want to study these relationships by examining their endogenous evolution across changing domestic political landscapes. It may be that focusing on politicians' fear of replacement during one war may shed new light on how they rebuild conscription systems ahead of future wars, and therefore how warfare evolves over time.

Our work also raises important questions about the relationship between military labor acquisition systems and decisions to go to war. Our data indicate that, conditional on fighting a war, countries with selective conscription are no more likely to have been the initiator than countries with alternative conscription systems. However, the unconditional probability of how likely politicians are to fight wars under different conscription systems remains an open question. We show that military labor acquisition policies structure risk preferences, suggesting that otherwise identical risk-reward calculations may produce different tolerances for war under different conscription systems. It may be that governments with selective conscription are more likely to go to war when they have high-risk, high-reward opportunities to achieve their foreign policy objectives through conflict. The joint distribution of wars and casualties may therefore look differently across military labor acquisition systems. However, there is little cross-national evidence on this question, suggesting an important avenue for future work.

Finally, while beyond the scope of this study, the relationship between conscription systems and public opinion during war deserves greater scholarly scrutiny. Military labor acquisition systems and public opinion evolve endogenously over the long run. Countries that anticipate future conflicts might adopt particular conscription systems, and in so doing help avert the conflict by projecting strength. Unlocking this dynamic will be key for future research. Clarifying this relationship could strengthen the case for claims that loopholes in conscription systems undermine their legitimacy (Levi, 1997), potentially explaining why these systems have become less popular. Further, public support for war varies by such contextual factors as international backing, domestic elite consensus, and the objective of military action. How these contextual factors interact with conscription systems is an open question. We hope that our new data set provides scholars with opportunities to further investigate these important questions.

Acknowledgments

We thank Rikhil Bhavnani, Erica Frantz, Brad Gomez, J. Andrew Harris, Kyle Haynes, Raymond Hicks, Gary King, Chris Reenock, Swati Srivastava, Rocío Titunik, Josh Tucker, seminar participants at the 2019 LDS Scholars Workshop at Brigham Young University, the 2019 and 2021 EPSA annual conferences, the University of Salzburg, and the editors and reviewers at *Comparative Political Studies* for their helpful feedback.

Author Contributions

The authors are listed in alphabetical order; all authors contributed equally.

Declaration of Conflicting Interests

The author(s) declared no potential conflicts of interest with respect to the research, authorship, and/or publication of this article.

Funding

The author(s) received no financial support for the research, authorship, and/or publication of this article.

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Data Availability Statement

All replication files are available at <https://dataverse.harvard.edu/dataset.xhtml?persistentId=doi:10.7910/DVN/JSTTOJ> (Atkinson, 2021).

Supplemental Material

Supplemental material for this article is available online.

Notes

1. This is in no way meant to take away from the enormous strides that previous literature has made in advancing the field's understanding of the relationship between military labor acquisition and military conflict, as well as the link between military conflict and domestic political *outcomes*, for example, the effect of battlefield deaths on public opinion (Berinsky, 2009; Gartner & Segura, 1998; Gelpi et al., 2009) and elections (Althaus et al., 2012; Croco, 2011; Grose & Oppenheimer, 2007).

2. Note that our theoretical argument and empirical analysis do not directly speak to questions of institutional choice (endogenous institutions) over military labor acquisition systems, or to a country's initial decision to enter into a military conflict. These questions are beyond the scope of this article. Instead, we argue and demonstrate empirically that countries with selective conscription tend to experience more casualties relative to countries with other military labor acquisition systems.
3. For an version of our replication materials, see here: <https://doi.org/10.7910/DVN/JSTTOJ>.
4. This research investigates whether the localized effects of battle deaths are really a product of the attention that individuals pay to the news. For the attuned public, national casualties might be as important in influencing opinion about the war as local ones. There is also a debate about whether these effects are short-lived or long-lasting (Althaus et al., 2012; Hayes & Myers, 2009; Myers & Hayes, 2010).
5. We note that we are agnostic about whether this strategy protects politicians against the risk of replacement, or whether politicians *perceive* it to do so.
6. See Sections 1 and 2 of the [online appendix](#) for the MLAD code book for reasonable exemptions under universal conscription.
7. See [Appendix 4.7](#) for a discussion of exclusion, which is a strategy employed in conscription policy.
8. Some states may employ both: during its existence, the Ottoman Empire at times excluded religious and ethnic minorities, and also shielded the elite population from military service.
9. The cost difference arises from several factors, such as the labor market competition mentioned earlier, along with additional expenses per soldier for recruitment and training (Williams, 2005). It is plausible to assume that in a market-driven system, where soldiers volunteer to serve, the public response to their deaths might be less severe (Krebs et al., 2021). While this could alleviate some of the political limitations stemming from the inability to fully select recruits, it is expected that cost restrictions will remain substantial. Consequently, these limitations are likely to significantly influence the number of casualties a leader is prepared to accept (Horowitz et al., 2011).
10. The Selective Service System remains in law but is dormant.
11. In [Appendix 3](#), we present an extensive discussion about selective conscription in democracies.
12. It is possible that would-be conscripts from politically important groups who are shielded from service under selective conscription do not believe themselves to be protected, and so place the same constraints on their government that they would under universal conscription. Even if this is the case, it does not meaningfully alter our theoretical expectations. For one, even if all citizens actually do hold the government politically accountable, if governments (wrongly) believe that selective conscription policies sufficiently insulate them from public opinion backlash, then they are still likely to choose the military behaviors we predict.

Further, even if governments could perfectly observe citizens' preferences, every citizen who lets the government off the hook for battlefield casualties because they are unaffected by the war gives the government marginally more strategic freedom.

13. We further disaggregate "selective" conscription in subsequent analyses into "shielding" and "exclusion" categories depending on how.
14. Note that some exemptions which fall into this category are broadly about preserving social order during wartime. For example, many states exempt teachers or single parents of young children from service. We allow for these under universal conscription.
15. For a detailed list of these questions, please refer to [Appendix](#) Section 1.
16. See [Appendix 1](#) for an exhaustive overview of the sources used to code each state.
17. Of 17,420 observations with any data, 2,592 contain substantial missingness on conscription policy. Not surprisingly, missingness is concentrated in the earlier years of the dataset and occurs more frequently for states just after independence from colonial rule.
18. For a figure showing labor types by regime, see [Appendix 5](#).
19. To ensure the robustness of our findings, we also test our argument using COW's Interstate War data. The findings are consistent with those in the main document. Please see [Appendix 4.11](#).
20. For a figure displaying the relative frequencies of each labor type, see [Appendix 5](#).
21. Consistent with [Lyall \(2020\)](#), states-to-be fighting in wars of independence are included.
22. In the main analysis, we choose to use Lyall's low estimate. In [Appendix 4.3](#), we use the high estimate; our results are substantively unchanged.
23. See [Appendix 5](#) for a table of the summary statistics for each of our variables. Because of the potential for confounders for our various outcomes of interest, we do not include the summary statistics for the variables that are only included in the analysis of the auxiliary hypotheses here, but we will provide them in our discussion of the analysis.
24. We aim to ensure the robustness of our results, regardless of whether a state participated in a multi-party war or not. In [Table 1](#), we include a model that drops all multi-party wars.
25. See [Appendix 4.8](#) for robustness checks using V-Dem's liberal democracy index and a model with the full continuous V-Dem variable. We also tested the interaction of democracy, electoral democracy, and liberal democracy with our military labor variable and found that these changes did not significantly impact our results. Further, we acknowledge the complex role of the relationship between civilian and military leaders within a society and the growing body of literature in the civil-military relations literature that explores these links([Cebul & Grewal, 2022](#); [Narang & Talmadge, 2018](#); [Piplani & Talmadge, 2016](#); [Talmadge, 2015](#)). See [Appendix 4.10](#) for more discussion.

26. We also assess whether our results hold controlling for whether the state employed a secondary means of labor acquisition in [Appendix 4.9](#) and [Appendix 5.3](#).
27. As a robustness check, we also estimate coefficients where the DV is the proportion of battlefield deaths of the total population as an outcome (with unchanged results; see [Appendix 4.4](#) for details).
28. Nevertheless, our results are robust to dropping the World War variable, as well as robust to dropping observations belonging to one of the World Wars; see [Appendix 4.2](#).
29. To further ensure that our results are not a product of a particular time period, we introduce decade fixed effects to the main model in [Appendix 4.6](#) as well as constraining our data to only observations that occurred after 1945. To further ensure that our results are robust to model specification, in [Appendix 4.2](#), we introduce our variables in steps. We find that our results are robust to each of these tests.
30. We specify the models to test auxiliary hypothesis *H2*, controlling for many of the same potential confounders. In instances where this is not the case, we will note it in the main text preceding the introduction of the specific analysis.
31. They are also robust to adding country fixed effects to our model, see [Appendix 4.5](#).
32. In [Appendix 5.1](#), we present a table presenting the rest of the initial robustness checks found in [Table 1](#).
33. In [Appendix 3](#), we present a list of all belligerents that used exclusion.
34. In [Appendix 5.1.3](#), we also control for issue salience, as this has been shown to lead to longer wars ([Atkinson, 2021](#); [Slantchev, 2004](#); [Weisiger, 2016](#)). We do not include this in the main document as the salience variable introduces considerable missingness.

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