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Article

The deterrent effect of the death penalty? Evidence from British commutations during World War I

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ABSTRACT

During World War I, the British Army relied on the death penalty to enforce strict discipline, handing down over 3000 death sentences for desertion and other offenses. Yet only around 12% of these sentences were actually carried out; the remaining 88% were quietly commuted to lesser punishments. Crucially, soldiers themselves were unaware that most death sentences would be commuted, causing them to perceive the risk of execution as uniformly high. This hidden "lottery" in the application of the death penalty provides a rare opportunity to study deterrence under conditions where the threat of capital punishment was both visible (through executions) and secretly mitigated (through commutations). I show that, overall, executing soldiers did not strongly deter subsequent desertions. However, when the executed soldier was Irish—an ethnic group often marginalized within the British Army—desertion rates in that unit actually rose. This divergence sheds light on the critical role of legitimacy in shaping compliance. Among many Irish soldiers, the British command was perceived as less legitimate, so executing an Irish comrade could breed resentment instead of deterrence. This finding underscores a fundamental argument in the literature on deterrence and compliance: punishment severity alone does not guarantee obedience. When individuals or groups already harbor doubts about the authority's legitimacy, harsh penalties can backfire and spur further defiance. The British-Irish split thus illustrates how perceived legitimacy can magnify or negate deterrent effects—an insight that resonates in contemporary debates about the death penalty and law enforcement (*JEL* N44, K14, K42, P48).

1. INTRODUCTION

The British Army during World War I took drastic measures to deter desertion: it imposed death sentences swiftly, often had them carried out by members of the deserter's own battalion, and publicly proclaimed each execution. Soldiers were reminded that desertion, unlike the spontaneous act of cowardice, was defined as a "premeditated absence" from danger

(Peaty 1999). In principle, this severe approach was intended to instill fear and obedience among the troops, meeting two classic conditions for deterrence: a heightened perception of punishment risk and an offense understood as deliberate (Lochner 2007; Apel and Nagin 2011). Yet the practical reality was more nuanced. Of the 3300-plus soldiers sentenced to death, only about 12% were actually executed; the rest had their sentences commuted to lesser penalties—but unbeknownst to most servicemen, who believed execution was a near-certain fate for desertion (Oram 2003; Putkowski and Sykes 2007).

This study leverages that hidden "lottery" in British military justice to investigate one of the most enduring puzzles in law and economics: Does the death penalty deter crime? Prior research on capital punishment has generated mixed findings (Ehrlich 1975; Donohue and Wolfers 2005; Nagin and Pepper 2012), partly because executions are rarely random and often shrouded in delays. Through new archival data, I show that, in this World War I setting, the risk of capital punishment was widely perceived to be high and immediate, offering a unique opportunity to examine deterrence under seemingly ideal conditions.

A key insight, however, is that deterrence alone cannot explain why some subgroups respond differently to harsh penalties. The British Army's large minority of Irish soldiers highlights how legitimacy—the perceived moral right of an authority to govern—can shape compliance with the law (Tyler 2006). Many Irish troops, already subordinated and geographically distinct, viewed British command with skepticism. This study documents that executing an Irish soldier sometimes increased the likelihood of future desertions in that unit—especially among Irish servicemen—whereas executing British soldiers had no such destabilizing effect. This heterogeneity suggests that punishment can backfire if the punishing authority is seen as illegitimate or unjust (Becker 1968; Donohue 2011; Alesina and La Ferrara 2014).

By focusing on these dual aspects—(1) the British Army's quasi-random application of the death penalty, and (2) the differing loyalty among Irish versus non-Irish soldiers—this study speaks to broader theoretical questions about why deterrence sometimes fails. If soldiers already doubt the legitimacy of their superiors, severe punishments can erode, rather than reinforce, discipline. These themes align with a wave of work emphasizing that people do not obey the law purely because of rational cost-benefit calculations; they also heed internal moral norms and social identity (Becker 1968; Tyler 2006).

The remainder of the study proceeds as follows. Section 2 provides historical background on British Army executions in World War I, explaining why the military believed immediate, public firing-squad executions would secure obedience and why, in practice, only a fraction of death sentences were carried out. Section 3 details the multisource archival data—ranging from court-martial records to war diaries—used to link desertion outcomes with sentencing decisions. Section 4 provides the identification strategy and Section 5 quantitatively demonstrates that executions were indeed decided in a manner that appears close to random, conditional on the soldier receiving a death sentence. Sections 6 and 7 lay out the empirical specifications and present results showing weak overall deterrence but a distinct "backfire" effect when the executed soldier was Irish.

In illuminating how the British Army's draconian threats sometimes undermined its own objectives, this study underscores that maximal severity does not guarantee compliance—especially if the punished group views authority as unjust. These findings offer cautionary lessons for modern discussions about the death penalty and, more broadly, about relying on brute force in complex social contexts where legitimacy is contested.

1.1 Literature on the deterrent effects of the death penalty and punishment

A long-standing debate in economics and criminology concerns whether capital punishment deters crime. Seminal work by Ehrlich (1975) used econometric techniques suggesting a

deterrent effect, but subsequent studies (e.g. Ehrlich 1975; Dezhbakhsh et al. 2003; Mocan and Gittings 2003) have produced mixed or conflicting results. Critics such as Donohue and Wolfers (2005) argue that the evidence is simply too fragile to draw strong conclusions, citing methodological challenges and confounding factors across jurisdictions. Broader studies of punishment, originating with Becker (1968) and expanded upon by Nagin (1998) and Apel and Nagin (2011), emphasize how both the certainty and severity of punishment can shape criminal behavior. Tyler (2006) and Glaeser and Sacerdote (2003) highlight that deterrence is not the sole factor—perceived procedural fairness or legitimacy can also influence whether individuals comply with the law.

In light of this literature, my study's main contribution is to exploit a historical setting where execution decisions were quasi-random, conditional on a death sentence, while soldiers were under unusually high risk from combat itself. By comparing subsequent desertion rates after an actual execution versus a commuted sentence, I am able to identify deterrence effects more cleanly than in many modern contexts, where selection of who receives the death penalty—and knowledge about that selection—can be highly confounded.

1.2 Economic and historical context of World War I

World War I profoundly shaped the political, social, and economic landscapes of participating nations (Broadberry 2005). Large-scale conscription, mass mobilization, and industrial shifts reoriented economies toward military production (Chickering and Förster 2000). In Britain, unprecedented state interventions in labor markets and resource allocation affected millions of soldiers and factory workers alike (Offer 1991; Winter 2003). On the home front, evolving public attitudes toward war, government authority, and social hierarchy created fertile ground for questioning the legitimacy of harsh penalties, including execution for desertion (Sheffield 1996).

Researchers such as Ferguson (2008) and Winter (1998) underscore how trench conditions on the Western Front were uniquely brutal, with extraordinary casualty rates, disease, and psychological stress (what later generations identified as "shell shock"). Such conditions help explain both the willingness of commanders to enforce discipline via extreme measures and the inability of many soldiers to continue fighting. Previous studies of World War I military justice (Babington 1983; Oram 2003; Putkowski and Sykes 2007) suggest officers believed that a harsh, immediate example was necessary to deter desertion in the face of unprecedentedly grim warfare. Yet, until now, most historical research has not been able to formally test the deterrent impact of these measures. My study helps fill that gap, integrating economic theory on deterrence and legitimacy into a specific World War 1 (WWI) context and thereby enriching our understanding of how soldiers respond to capital punishment threats in extreme settings.

2. HISTORICAL BACKGROUND

2.1 The British army's embrace of the death penalty

By the outbreak of World War I, British military officers regarded the death penalty as essential to discipline. Corporal punishments such as branding and flogging had been abolished in the preceding decades, leaving them to believe that execution was the only remaining tool severe enough to deter desertion (Oram 2003: 38). Sir Neville Macready, then Adjutant-General of the British Expeditionary Force (B.E.F.), famously insisted that ending the death penalty would effectively end the army (Jahr 1998: 314). General Horace Smith-Dorrien likewise demanded some executions to stem "a serious prevalence of desertion" (Babington 1983: 19; Oram 2003: 69). Courts martial records further confirm that officers often

invoked "the state of discipline" to justify the harshest sentence (Department of Foreign Affairs 2004: 38).

From the recruits' first day, the possibility of execution was made common knowledge: "It is well known . . . to all soldiers that desertion in the face of the enemy is liable to be punished by death" (Under-Secretary of State for War Harold Tennant, quoted in The Western Gazette, 28 January 1916). Soldiers thus served under the shadow of an apparent "zero tolerance" policy, even though subsequent events would show that few capital sentences were actually carried out.

2.2 Desertion and the battlefield environment

In contrast to cowardice, which generally involved fleeing under fire, desertion was understood as a premeditated absence from the line. The typical soldier's schedule—a recurring rotation of front-line duty, reserve duty, and rest—meant desertion was easiest during a night-time move away from the trenches (Babington 1983; Peaty 1999). Still, the war's logistical apparatus—layered policing, barriers, and roving patrols—rendered successful desertion in forward areas quite difficult (Sheffield 1996: 76). Soldiers who attempted to escape during combat might be summarily shot in the field.

Soldiers were rarely forewarned of the exact timing or scale of major offensives; artillery barrages and movement orders were kept secret (Ferguson 2008). This secrecy prevented many from orchestrating a planned absence well in advance, and the military believed that the extreme penalty would dissuade anyone even considering such a plan.

2.3 Scale and likelihood of apprehension

Records indicate that from 1914 to 1918, roughly 10.26 soldiers per 1000 deserted—translating into an estimated 55,400 overall deserters from an army of 5.4 million (The War Office 1922; Corns and Hughes-Wilson 2007). However, the precise figure remains uncertain due to the fog of war and potential overreporting of absences. Even so, historians generally concur that the actual desertion rate was relatively low—roughly 1%.

Contemporary sources agree that most deserters were arrested within 2 weeks (Jahr 2014). French civilians were reluctant to shelter deserters, and the military police patrolled intensively behind the lines. Official records also show at least 44,395 courts martial for absentees or deserters (The War Office 1922: 667). To put this in perspective, deserters in the US Civil War faced comparatively little risk of capture, and desertion rates there were far higher (Costa and Kahn 2003). During World War I, the British Army's determination to prosecute desertion was thus markedly robust.

2.4. Military justice: Field General Courts Martial

Desertion cases within France and Flanders usually proceeded under Field General Courts Martial (FGCM), composed of three or more officers (Department of Foreign Affairs 2004: 7). Defendants had minimal legal representation—prosecution by the adjutant, defense by a junior regimental officer—and trials were private (Babington 1983: 13). Any soldier absent from the front for ≥ 21 d was typically presumed to have deserted unless mitigating circumstances were evident (Putkowski and Sykes 2007: 14).

Not every conviction for desertion led to a death sentence; approximately 13% of those convicted received capital punishment. Among those sentenced to death, only about 12% were ultimately executed. Soldiers themselves had scant knowledge of these commutations because death-sentence decisions were tightly controlled at multiple levels of command and not widely publicized (Babington 1983; Oram 2003). The official stance of "death for desertion" thus diverged from the reality that most sentences were quietly reduced.

2.5 Secrecy surrounding commutation versus execution

Although over 3300 soldiers received death sentences for desertion, only a fraction were executed—creating what some historians call a "pitiless lottery" (Babington 1983). Official records on commutations remained confidential throughout the war, and public announcements solely noted the final outcome if it involved an execution. This means that from an ordinary soldier's vantage point, every death sentence might as well have been carried out.

If an execution was confirmed, an entire battalion (and sometimes additional units) witnessed the firing squad. These spectacles emphasized the Army's commitment to harsh punishment, reinforcing the notion that desertion meant certain death. Firsthand accounts from diaries and letters show that witnessing executions traumatized both soldiers and the firing squads (Oram 2003; Corns and Hughes-Wilson 2007). In a few rare memoirs, individuals claimed knowledge of commuted sentences, but historians doubt the accuracy or prevalence of such stories (Carrington 1965; Arthur 2002).

2.6 Soldiers' perceptions of punishment risk

One might wonder whether the average soldier truly believed in a high likelihood of execution, given that they rarely witnessed the direct penalty for deserters from other battalions. After all, large-scale "special parades" typically involved soldiers of the same battalion as the condemned, and men from different units mainly heard rumors rather than witnessing the event firsthand. Moreover, if a comrade quietly disappeared, it was easy to suspect he was missing in action, killed, or transferred rather than executed—especially when daily casualty rates were already high (Babington 1983; Oram 2003).

Yet from the vantage of most enlisted men, the risk still appeared high for several reasons. First, any unit's execution was often widely promulgated through orders and rumor networks. Men recalled hearing disturbing tales of firing squads, which amplified the sense that desertion meant certain death. Second, although soldiers did not know that most death sentences were commuted, many were aware that some men had indeed been shot; this reinforced the fear that one's own desertion might easily end in an execution. Third, the official rule that "death is the usual penalty" for desertion (Under-Secretary of State for War, The Western Gazette 1916) was prominently stated and repeated by superiors. Even if no immediate example surfaced within one's own battalion, the Army's unwavering messaging—and rumors of executions in other units—likely sustained a widespread perception of very real danger.

It is nevertheless true that some soldiers recognized the low probability of being summarily shot if they deserted far from the front, or they speculated that enough time might pass without consequence. Such possibilities may have slightly dampened the immediate perceived cost of desertion for a fraction of the rank-and-file. However, the data and firsthand accounts (Babington 1983;Oram 2003) overwhelmingly indicate that fear of execution persisted overall, propelled by repeated promulgations of actual shootings and by the Army's persistent emphasis on "certain death" for deserters on active service. In other words, while plausible alternative explanations existed for missing comrades (e.g. "he was killed in action," "he got transferred"), the publicized cases of proven executions—and the widespread belief that everyone was vulnerable—were potent enough to maintain a climate of fear.

2.7 The Irish minority in the British army

Although Irish regiments had long served under the British Crown, their status as a subordinated minority became especially fraught during World War I. Ireland was then part of the United Kingdom, yet many Irish citizens resented British rule; political tensions rose sharply after the Easter Rising of 1916, in which Irish republicans staged a major armed insurrection

in Dublin (Oram 2003). While the Rising was swiftly suppressed, it left deep scars on the relationship between Irish communities and the British military establishment.

Historical records indicate that Irish enlistments in British regiments—some voluntary, some driven by economic need—still amounted to a substantial minority in the overall force. Some Irish units (e.g. the Royal Irish Rifles, the Royal Munster Fusiliers) bore names reflecting their regional roots, though not every Irish-born soldier served in a nominally Irish regiment. Many historians note that the Army viewed Irish soldiers with a degree of suspicion, particularly those from nationalist areas of Southern Ireland (Denman 1992; Fitzpatrick 1996).

High-ranking officers often worried about the loyalty of Irish troops. This fear may have been exaggerated but was nonetheless pervasive, especially after 1916, when the Army suspected Irish regiments might harbor sympathies for the independence cause (Babington 1983; Oram 2003). Commanders sometimes singled out potential "agitators" for harsh discipline, believing such displays would prevent rebellion from within. At the same time, Irish soldiers faced pressure from home communities that questioned their service under the British flag.

In this tense climate, an Irish soldier convicted of desertion risked being perceived as having betrayed not only the Army, but also, in some sense, the "enemy"—since from the Army's viewpoint, the soldier's loyalty was already suspect. Meanwhile, executing an Irish comrade might trigger resentment among other Irish troops who already doubted the British Army's legitimacy. Unlike British soldiers who might perceive the execution simply as due punishment for desertion, Irish soldiers could view it as further evidence of political and ethnic oppression (Oram 2003). This dual perspective—a harsh penalty administered by an authority regarded as illegitimate—helps explain why executing Irish soldiers could spur, rather than deter, additional desertion.

Although the war in Europe continued until 1918, the seeds of Irish independence were taking root. By 1919, the Irish War of Independence had begun, further eroding any sense of shared British-Irish identity within the forces. Soldiers of Irish descent might have held back from full cooperation with the Army, or might even have seen desertion as a political act. For some, being executed by British officers was not just a judicial punishment but a symbol of colonial rule, thus galvanizing other Irish troops into acts of defiance.

This backdrop underscores the central hypothesis of this study: while the British Army expected the death penalty to reinforce discipline, it may have backfired among the Irish by amplifying grievances about British authority. Hence, the Irish/British heterogeneity is not merely an ethnic distinction but a politically charged fault line, shaping how soldiers perceived—and reacted to—capital punishment.

2.8 Summary and implications

By embedding the threat of immediate execution into its disciplinary code, the British Army hoped to quell desertion under extraordinarily grueling war conditions. In practice, however, only one in eight court-martialed deserters was actually put to death. This gap between threat and reality—coupled with soldiers' near-complete ignorance of commutations—yielded an effective perception of uniform severity. The next sections show how this "lottery" of execution versus commutation, largely invisible to the rank-and-file, enables an empirical design approximating random assignment of capital punishment.

From a broader perspective, the Army's reliance on firing squads underscores a singular faith in deterrence. Yet as we will see, these punishments sometimes carried unintended consequences—particularly among soldiers, such as the Irish minority, who perceived British authority as lacking legitimacy. That divergence in reactions to the same threat undergirds

this study's main argument: punishment alone cannot guarantee compliance unless it is viewed as legitimate in the eyes of those being punished.

3. DATA

This study draws on a broad collection of archival and administrative records compiled from British World War I–era sources. Each data component focuses on a different facet of desertion, discipline, and casualties, and they are meticulously linked at the battalion level—then aggregated to divisions—for subsequent analysis. This section summarizes the data creation process and the main variables that emerge from each source. The following section will detail how these datasets underpin the empirical identification strategy.

3.1 Death sentences and commutations

A central dataset comprises all 3342 death sentences imposed by the British Army from August 1914 to September 1923 (Oram 2003). These records note each soldier's name, rank, unit, date of sentencing, and offense, along with the outcome—execution (and date) or commutation (e.g. imprisonment or reduced rank). For 2724 of these cases, the soldier served under the B.E.F. in France and Flanders. The official sources for this information include War Office registers of trials for executed soldiers (WO 71), FGCM records (WO 213/2–26), and general court martial (GCM) records (WO 90). Although desertion is the most common charge (2005 cases), other offenses range from "sleeping at post" to disobedience, murder, and Irish rebellion. The final sentence in each case reflects the Commander-in-Chief's confirmation; unconfirmed death sentences were "quashed" or converted into other punishments, an outcome often unknown to rank-and-file soldiers.

3.2 Absentee data from war diaries

Additional information on desertion emerges from monthly War Diaries of the Assistant Provost Marshal (APM), preserved in National Archive files (e.g. WO 154/112, WO 154/114, WO 154/8). These diaries record, for each month, names and descriptions of men absent from their units beyond a specific threshold—generally one month after being reported missing in daily roll calls. Typical entries include the soldier's rank, regiment, physical characteristics, and the suspected date of disappearance. While coverage extends through 1914–1918, the most complete records come from mid-1916 to mid-1917. Because the diaries were originally used to coordinate apprehension efforts, they focus on absences considered deliberate or suspicious, providing a partial but highly relevant window into desertions in the field.

3.3 Police Gazette listings of deserters and absentees

A more comprehensive—but broader—source of absence data is found in the weekly Police Gazette's "Deserters and Absentees" (D&A) supplements, covering 1914–1918. Over 126,000 entries list each soldier's name, rank, regiment, age, and physical characteristics, along with their last known post or location of desertion. Because these publications served for both home and overseas absences, only about 3000 references to the B.E.F. were likely subject to the death penalty for desertion. The rest primarily concern men who failed to report back from leave in the United Kingdom or other theaters of war. This Gazette dataset helps verify the War Diaries' coverage and indicates seasonal or temporal patterns in desertion (e.g. spikes around holidays or at the war's end).

3.4 Field General Courts Martial trial records

A fourth source of absence-related data comes from handwritten FGCM registers (WO 213/2–26). Between January 1914 and November 1919, these records document roughly 144,609 trials, with details on each soldier's rank, battalion, regiment, place and date of trial, the charges, sentences, and any notes on acquittals or commutations. Of these, 13,309 concern desertion and 28,754 concern absence without leave (AWOL). Since many of these cases did not escalate to the death-sentence stage, the FGCM registers provide a more granular view of frontline discipline, capturing whether soldiers faced penal servitude, imprisonment, or lesser punishments. Comparing the FGCM listings to the official death-sentence records reveals that only a fraction of desertion convictions culminated in confirmed capital sentences.

3.5 Casualty records

To gauge each unit's exposure to danger, the analysis incorporates casualty data from the Soldiers Died in the Great War database (658,555 entries). These records describe the soldier's name, rank, regiment, date of death, and the cause (killed in action, died of wounds, or illness). Linking casualties by battalion and date yields a measure of real-time combat intensity. Prior research (Costa and Kahn 2003) suggests that higher casualty rates can drive desertions, so controlling for these figures helps isolate the role of execution risk from battlefield conditions.

3.6 Identifying Irish soldiers

Because one objective is to test whether the perceived legitimacy of punishment varied across ethnic groups, the analysis requires a proxy for "Irishness." This study relies on a dictionary of 426 Irish surnames, using matches to flag men of probable Irish descent. Although some inaccuracies occur—for instance, a British-born soldier might inherit an Irish surname—this remains the most feasible large-scale method. When possible, birthplaces from the Police Gazettes or Service Records further refine the identification of Irish versus non-Irish soldiers.

3.7 Service and pension records

Digitized Service (WO 363) and Pension (WO 364) Records supplement the above data with individual characteristics such as age, birthplace, enlistment date, and address. These records are incomplete due to wartime and archival losses (the "Burnt Documents"), but for the subset that survives, they allow checks on whether the outcome of a death sentence was correlated with factors like soldier age—potentially relevant for testing quasi-random assignment of execution versus commutation.

3.8 Unit of analysis and order of battle

All these datasets must be linked at a consistent organizational level. Although many records list a soldier's battalion (about 1000 men), certain decisions—especially the confirmation or commutation of a death sentence—were ultimately reviewed at higher echelons (Oram 2003: 129). Consequently, the division (about 18,000–19,000 soldiers) emerges as the primary unit of analysis here: it balances the salience of an execution within a narrower group of soldiers with sufficient data coverage to observe multiple events and outcomes.

To facilitate these merges, a custom Order of Battle dataset tracks each battalion's movements among brigades and divisions from 1914 to 1918 (based on The Long, Long Trail, Edmonds 1922; James 1978). By matching a soldier's battalion and the relevant date, it is possible to establish which division he served in at any given time. This step is crucial for

linking desertion events, casualty rates, and any local patterns of discipline (e.g. prior executions in that division).

3.9 Summary

Taken together, these records form an unusually rich view of desertion, sentencing, and military conditions in the British Army during World War I. The death-sentence data reveals which offenses triggered capital punishment; the War Diaries, Police Gazettes, and FGCM trial registers catalog broader patterns of absences and discipline; the casualty database measures the shifting intensity of the battlefield; and the surname dictionary and service records distinguish Irish from non-Irish soldiers. After assigning each soldier-event to its proper division via the Order of Battle, the resulting dataset spans thousands of recorded absences, death sentences, and commutations. Additional data documentation appears in Supplementary Appendix A, providing deeper background and exhibits on sources, merges, and variable construction.

In the next section, I detail how the analysis leverages these sources to identify the causal impact of an actual execution (as opposed to a commuted death sentence) on subsequent desertions. That discussion covers the potential-outcomes approach, the randomization checks, and the methods used to account for the complexities of measuring absences in multiple, partially overlapping datasets.

4. IDENTIFICATION STRATEGY

4.1 Overview of the quasi-random assignment

A central challenge in evaluating the deterrent effect of any punishment, especially the death penalty, is that who receives the punishment is often nonrandom. Regions that apply capital punishment differ from those that do not, and even within a single legal system, high-profile offenses may attract harsher sanctions. In the British Army during World War I, however, a unique dynamic emerged: although thousands of soldiers were sentenced to death, only about 12% of these sentences were ultimately confirmed. Archival records suggest that the final decision was left to the Commander-in-Chief, who reviewed each case in a manner that appears uncorrelated with observable soldier or unit characteristics (Babington 1983; Oram 2003). If so, then whether a soldier was actually executed (versus commuted) could be treated as close to random, conditional on receiving a death sentence.

This quasi-randomness is pivotal. It means that two soldiers in the same division, both convicted of desertion around the same time, might have identical observable characteristics—yet one was executed, and the other returned to the trenches. Soldiers and officers at the front were largely unaware of the high commutation rate, so the perceived cost of desertion remained uniformly severe. By contrasting division-level outcomes following an execution with those following a (secretly) commuted death sentence, I can tease out the causal effect of an actual execution on subsequent desertions.

4.2 Potential outcomes and the within-division design

To formalize the intuitive comparison, I adopt a potential-outcomes framework (Rubin 1974). Each division D at time t experiences a "treatment event" when a soldier in that division is sentenced to death. Let: $E_{i,t} = 1$ if the sentence is executed, $E_{i,t} = 0$ if the sentence is commuted.

I want to estimate the impact of $E_{i,t}$ on subsequent desertions or absences in division i at future times $t' > t$. Because the choice of execution or commutation appears random (conditional on the death sentence), I interpret commutation as a counterfactual for what would

have happened if the same division had not executed that soldier. The relevant assumption is that, absent the difference in confirmation decisions, these divisions were on comparable trajectories.

A practical advantage of focusing on within-division variation is that each division serves as its own control over time. By comparing how desertions evolve after an execution event versus how they evolve after a commutation event (for the same division at different points in time), I mitigate confounding factors specific to that division—such as combat intensity, underlying morale, or leadership style.

4.3 Evidence of randomness

A key component of this strategy is to verify that the confirmation decision—execution versus commutation—does not systematically correlate with observed soldier or unit attributes:

Regression checks in the following section show that neither soldier age, rank, nor Irish surname predicts whether a death sentence is confirmed. The probability of execution also does not vary with offense severity or timing of the war, once I condition on the fact that a capital sentence was already imposed.

Additional tests indicate that recent casualty rates, prior executions, or prior desertion levels in a division do not significantly affect the odds of another soldier being executed rather than commuted. I detect no autocorrelation beyond chance levels in the sequence of execution/commutation outcomes within a division.

Contemporary sources (Babington 1983; Oram 2003) and official communications from the Commander-in-Chief describe the final confirmation process as a closed-door review that did not publicize rationales. Historians refer to this practice as a "lottery" due to its apparent arbitrariness, further supporting my assumption that conditional on a death sentence, the outcome is as good as random.

4.4 Treatment variants and timing

Although the commander typically decided within a couple of weeks, the exact day of commutation was not announced, and soldiers learned of an execution only on the eve of the firing squad. My treatment variable, therefore, is dated to the day the final outcome (execution or commutation) was promulgated and made known to the battalion or division. This timing ensures that I capture the moment when soldiers in the division recognized that a deserter was definitively executed, thus reinforcing (or failing to reinforce) the army's threats.

An unresolved question is whether divisions also respond to accumulated prior outcomes (multiple executions or commutations) or merely to the most recent event. In practice, both could matter. I therefore estimate models under two approaches: (1) Strong "SUTVA"—Only the most immediate event influences subsequent absences. (2) Weak "SUTVA"—Past executions retain a decaying effect over time (Sims 2003; Kahneman 2011).

4.5 Empirical approaches

The identification hinges on comparing post-event desertions in a division with the counterfactual scenario (had the event been a commutation). Two complementary frameworks help capture these dynamics: (1) Duration analysis. I treat time until the next recorded absence as the outcome variable. Once a death sentence is handed down in a given division, I measure how quickly another desertion occurs following an execution versus a commutation. (2) Day-by-day probability models. I estimate a discrete hazard or logistic regression where each division on each day either experiences at least one absence or not. Executions (and commutations) enter as "treatment dummies," which fade over time if multiple events

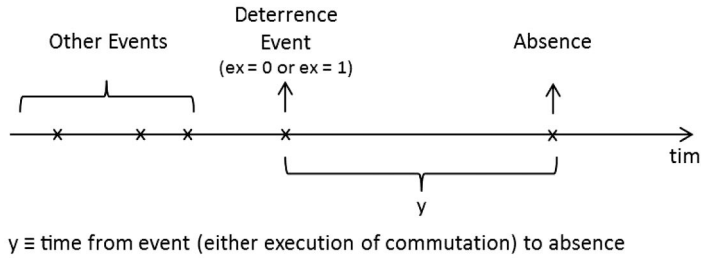

Figure 1. Illustration of duration model.
Notes: This figure visually summarizes the duration model.

accumulate. Figure 1 visually summarizes the duration model and Figure 2 visualizes the day-by-day approach.

In both designs, identification rests on the assumption—supported by randomization checks—that, conditional on receiving a death sentence, executed and commuted soldiers' divisions were ex ante equivalent. Differences in post-event outcomes thus can be interpreted as causal.

4.6 Irish versus non-Irish responses

A further goal is to test whether ethnic identity, specifically Irish versus British, moderates any deterrent effect. If the punishment authority is seen as illegitimate—an arguably stronger sentiment among Irish soldiers—executions might backfire by spurring group solidarity or resentment. Within the identification strategy, I compare post-event desertions specifically among Irish soldiers versus non-Irish in the same division. Because the assignment of an Irish defendant to execution or commutation is equally random, I can similarly interpret any difference in Irish versus non-Irish desertion patterns as driven by variation in perceived legitimacy, rather than by systematically different cases going to execution.[1]

4.7 Summary

In sum, the British Army's court-martial system, combined with a nearly opaque commutation process, offers a rare setting in which soldiers with otherwise similar capital convictions were effectively randomized to execution or lesser punishments. This provides the crux of the identification strategy: within each division, I compare subsequent desertions after an execution event with those after a commuted event. I then refine my estimates by controlling for casualties, seasonality, and potential cumulative effects of prior executions. The next sections present empirical results under these frameworks, demonstrating both a limited overall

[1] A key concern is that using only surnames to identify "Irish" soldiers may mislabel some individuals—for example, British-born soldiers with Gaelic-like names or Irish-born soldiers whose names do not appear on my list. Such measurement error systematically biases any true difference toward zero rather than inflating it. To see why, consider the following steps: (1) Assume a Genuine Difference: Suppose, in reality, soldiers with truly Irish ancestry respond more strongly (i.e. are more likely to desert) after an Irish comrade is executed than do soldiers without Irish ancestry. (2) Mislabeling Soldiers: False Negatives–Some authentically Irish soldiers lack a surname in the dictionary and get coded as "non-Irish." False Positives–Some British-born soldiers have a surname flagged as Irish, so they appear in the "Irish" group. (3) How Misclassification Dampens the Measured Gap: Irish-Labeled Group–Now includes British-born men, who presumably respond less intensely to Irish executions, thereby pulling down the group's average reaction. Non-Irish Group–Gains a few actual Irish soldiers who react strongly, pushing up the average desertion response in the "non-Irish" group. (4) Net Effect: Convergence of Both Groups Because each group absorbs soldiers who "truly" belong to the other, the observed difference between them shrinks. In statistical terms, an explanatory variable measured with random error yields an attenuated coefficient—that is, a smaller estimated gap than the true gap. (5) Implication for Results: If, despite this mixing of groups, I still find a notable divergence (i.e. executing an Irish soldier leads to more desertions), that difference is likely a lower bound. A perfectly accurate measure of Irish ancestry would likely reveal an even larger disparity.

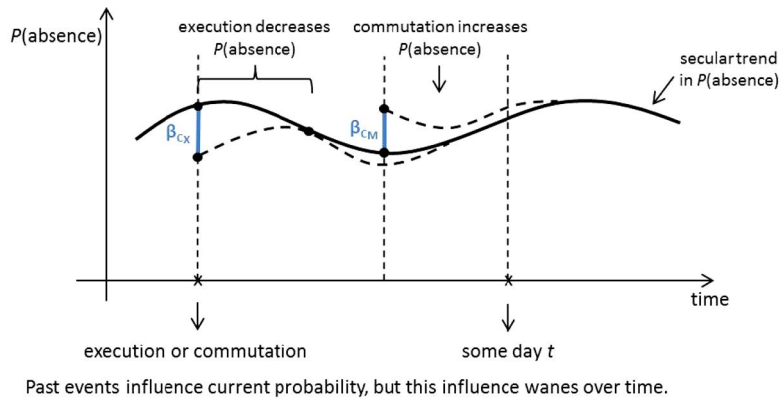

Figure 2. Illustration of day-by-day model.
Notes: This figure visually summarizes the day-by-day model.

deterrent effect and a pronounced ethnic divergence that underscores the role of legitimacy in shaping how soldiers respond to extreme sanctions.

5 ASSESSMENT OF RANDOMNESS

A crucial element of this study's identification strategy is that, conditional on receiving a death sentence, the final confirmation of that sentence—execution versus commutation—appears close to random. Historians have often described it as a "pitiless lottery" (Babington 1983; Oram 2003). Yet I must rule out the possibility that certain types of soldiers or units were systematically more likely to see their sentences confirmed, thereby biasing any subsequent measure of deterrence. This section shows that the few obvious, historically relevant factors—namely ethnicity (Irish versus non-Irish), soldier rank, past discipline problems in the unit, and proximity to high-casualty battles—do not systematically predict who was executed or commuted.

5.1 Historical reasons for concern

Historically, four main considerations stand out:

1) Ethnicity (Irish): Given the tension between Irish soldiers and British command, one might suspect that Irish defendants were more likely to be executed. This concern arises from the historical subordination of the Irish minority and anecdotal claims that authorities "made an example" of soldiers suspected of weak loyalty (Oram 2003).
2) Rank of the soldier: Many WWI-era officers expressed a belief that punishing rank-and-file troops was necessary to maintain discipline, while officers might be spared or tried differently (Moore 1975; Babington 1983). If privates were more vulnerable to execution, the deterrent effect could be confounded by rank differences in morale.
3) Local unit discipline: The British Army sometimes viewed entire units as "bad apples" or lacking discipline (Babington 1983; Putkowski and Sykes 2007). If a recent spike in desertion or other infractions made an execution more likely (to "set an example"), I could not treat the actual execution as random. The same logic holds for strong leadership: a harsh commanding officer might consistently confirm executions.

4) Battle conditions and casualties: Some historians propose that high casualty rates or upcoming offensives might push commanders to commute a death sentence on a useful soldier. Others suggest that heavy losses prompt the Army to enforce stricter discipline. Either way, if these battlefield factors predicted who was executed, randomization would fail.

5.2 Core randomness checks

5.2.1 Ethnicity and rank

In Table 1, Columns 1 and 2 regress the probability of execution on (1) an Irish indicator and (2) soldier rank (private versus officer), restricting the sample to those soldiers already sentenced to death. Neither coefficient is significant at conventional levels. If Irish defendants or low-ranking soldiers had notably higher odds of execution, that would undercut my randomization assumption. Instead, the data show no discernible bias in the final confirmation.

5.2.2 Prior discipline and commanding officers

Next, Table 2 considers whether units with higher rates of desertion or prior executions are more likely to confirm a new death sentence. I also examine whether certain commanding officers (division or corps level) are systematically more "execution-prone." None of these variables significantly predict an execution decision, once I condition on the fact that a death sentence was already imposed. Historians often argue that a commander might "make an example" of a unit with discipline problems. If so, that unit's later desertions might not be comparable to a unit receiving commutations. The data, however, show no robust evidence that discipline troubles or officer identities drive confirmation decisions. Tables 3 and 4 repeat the exercise for all capital sentences regardless of crime, and the results are similarly null.

5.2.3 Battlefield conditions

Because some scholars suggest capital punishment might be used to restore order during high-casualty offensives (Babington 1983), Columns 9–11 of Table 1 test whether execution outcomes correlate with local casualty rates or major battle timing (e.g. the Somme, Passchendaele). I find no systematic relationship: even large spikes in casualties do not predict a higher chance of execution. If divisions in the midst of bloody campaigns were more likely to confirm executions (or to commute them to save manpower), it would bias any subsequent analysis of deterrence. Instead, the data support the notion that final decisions were largely idiosyncratic at the Commander-in-Chief level.

5.3 Autocorrelation tests

If executions were genuinely random, I would not expect them to cluster within particular units or time windows—beyond chance. Table 2, Panel B thus examines whether one execution in a division makes another more or less likely. I observe no significant autocorrelation, even at different echelons (brigade, division, corps). Similarly, no evidence suggests "streaks" of executions or alternating patterns. This finding is consistent with anecdotal accounts describing the final outcome as a "lottery" (Babington 1983).

5.4 Implications for causal identification

These results indicate that conditional on a soldier being sentenced to death, no key observable—Irish ethnicity, rank, local discipline problems, commanding officer identity, or battlefield intensity—meaningfully predicts who is actually executed versus commuted. Because

Table 1. Are observable characteristics correlated with execution decisions? (deserters).

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Irish	−0.00133	−0.00162	0.00750	−0.00261	−1.93e-14	−0.000283	−0.000398	−0.00313	−0.0147	−0.00467	0.0131
	(0.0214)	(0.0214)	(0.0143)	(0.0214)	(0.230)	(0.0215)	(0.0214)	(0.0219)	(0.0244)	(0.0218)	(0.0181)
Private		−0.0577									−0.0265
		(0.0443)									(0.0371)
Age			−0.00464								0.00274
			(0.00316)								(0.00396)
1915				−0.138	−0.135					−0.210*	−0.0495
				(0.0951)	(0.107)					(0.110)	(0.115)
1916				−0.148	−0.150					−0.246**	−0.00752
				(0.0934)	(0.105)					(0.109)	(0.114)
1917				−0.205**	−0.205**					−0.288***	−0.0376
				(0.0930)	(0.104)					(0.109)	(0.114)
1918				−0.222**	−0.219**					−0.320***	−0.0678
				(0.0939)	(0.105)					(0.110)	(0.114)
Irish *x* 1915					−0.0152						
					(0.237)						
Irish *x* 1916					0.00798						
					(0.234)						
Irish *x* 1917					0.00133						
					(0.233)						
Irish *x* 1918					−0.0156						
					(0.235)						
New army									0.0185		
									(0.0214)		
Territorial army									−0.00713		
									(0.0303)		
ΔLog casualties										−0.00134	−0.00493
										(0.00742)	(0.00565)

(continued)

Table 1. (continued)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
ΔLog casualties 30 d ago										0.00327	−0.00292
										(0.00718)	(0.00540)
Distance to coast											−0.000474*
											(0.000268)
Distance to Berlin											0.000240
											(0.000467)
Year fixed effects	N	N	N	Y	Y	N	N	N	N	Y	Y
Month fixed effects	N	N	N	N	N	Y	N	N	N	N	N
Day of week fixed effects	N	N	N	N	N	N	Y	N	N	N	N
Division fixed effects	N	N	N	N	N	N	N	Y	N	Y	Y
Joint test of fixed effects				0.000883	0.00575	0.750	0.258	0.0272	0.590		
Joint test of Irish *x* year FE					0.995						
Joint test of casualties										0.839	
Joint test of distance											0.209
Constant	0.151***	0.206***	0.984***	0.334***	0.333***	0.144***	0.178***	0.188**	0.162***	0.493***	0.757**
	(0.00959)	(0.0436)	(0.0801)	(0.0921)	(0.103)	(0.0306)	(0.0275)	(0.0889)	(0.0154)	(0.140)	(0.379)
N	1741	1741	1741	1741	1741	1741	1741	1741	1418	1741	1190
R^2	0.000	0.001	0.554	0.011	0.011	0.004	0.004	0.055	0.001	0.066	0.609

Notes: All regressions use ordinary least squares on death sentences occurring in France & Flanders before the end of World War I. Death sentences recorded without divisions or from the Labor Corps were removed. Log casualties is calculated as log(1+ casualties). ΔLog casualties is defined as the difference in Log casualties 1–29 d ago versus 30–59 d ago. ΔLog casualties 30 d ago is defined as the difference in Log casualties 30–59 d ago versus 60–89 d ago. Distances are set to missing before the first battle and after the last battle. Territorial/new/regular army status is not assigned for Indian, Australian, Canadian, or New Zealand divisions. Regressions including age also dummy out age when it is missing (i.e. assign a constant and include an indicator for age being missing). Standard errors in parentheses. Results are similar with logit or probit.

*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

Table 2. Are observable characteristics correlated with execution decisions? (deserters).

Panel A	**Joint test of significance**
Brigade unit	0.106
Corp unit	0.230
Army unit	0.242
Brigade commanding officer	0.872
Division commanding officer	0.0211
Division first general staff officer	0.109
Corp commanding officer	0.527
Corp first general staff officer	0.529
Army commanding officer	0.214
Army first general staff officer	0.182
GHQ commanding officer	0.129
GHQ first general staff officer	0.277
Irish soldier *x* Irish officer FE	0.659
Military indiscipline 30–59 and 60–89 d ago	0.482
Death sentences 30–59 and 60–89 d ago	0.139
Execution rate 30–59 and 60–89 d ago	0.415
Panel B	
Aggregation level	**Correlation with lag decision**
Division	0.0495
	(0.0331)
Brigade	0.00376
	(0.0387)
Corp	0.0225
	(0.0330)
Army	0.0282
	(0.0354)
Army type	−0.0343
	(0.0359)
All	0.0354
	(0.0508)

Notes: Data are restricted to death sentences occurring in France & Flanders before the end of World War I. Death sentences recorded without divisions or from the Labor Corps were removed. In Panel A, each row reports a separate ordinary least squares regression and tests of joint significance of the fixed effects or measures of the recent battle environment. Military indiscipline and death sentences are calculated as log(1+ number). Military indiscipline is the average of absentees and trials measured from the War Diaries, Police Gazettes, and FGCM trial registries. Lag execution rates is a set of controls comprising the numbers of executions and commutations within each time window. Units or officers that appeared with less than 10 frequency were categorized in a separate "other" category. All regression models include year, division, and Irish fixed effects. In Panel B, each row reports a separate ordinary least squares stacked autocorrelation regression. The strings of events within each unit were stacked and the first event within each unit was excluded as a dependent variable. If more than one event occurred on a day within a unit, the average outcome was calculated for that day. All regression models include year fixed effects and the leave-one-out mean execution rate of the unit. Standard errors in parentheses. Results are similar with logit or probit.

$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

these are precisely the factors that historical context leads us to worry about, their insignificance strongly supports a quasi-random process.

With the randomness assumption in place, comparing post-execution outcomes to post-commutation outcomes becomes a credible strategy for identifying the causal effect of an actual execution on subsequent desertions. I exploit this in the empirical analysis, focusing on whether executions truly deter absenting soldiers or, in some contexts (particularly for the Irish), might actually spur more desertion.

Table 3. Are observable characteristics correlated with execution decisions? (all death sentences).

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Irish	0.00998	0.00969	0.0136	0.0101	0.0619	0.0106	0.0114	0.0106	0.00105	−0.000867	0.0697	0.0117
	(0.0171)	(0.0170)	(0.0110)	(0.0171)	(0.0961)	(0.0171)	(0.0171)	(0.0173)	(0.0188)	(0.0169)	(0.0951)	(0.0141)
Private		−0.0842**									−0.0768**	−0.0160
		(0.0327)									(0.0329)	(0.0272)
Age			−0.00302									0.00153
			(0.00261)									(0.00327)
New army									−0.00922			
									(0.0161)			
Territorial army									−0.0143			
									(0.0236)			
Desert										0.0970	0.0994	0.0603
										(0.0673)	(0.0674)	(0.0626)
Coward										−0.00968	−0.0128	−0.00141
										(0.0713)	(0.0714)	(0.0652)
Disobedience										−0.0366	−0.0367	−0.000702
										(0.0827)	(0.0828)	(0.0781)
Murder										0.872***	0.861***	0.377***
										(0.115)	(0.115)	(0.0948)
Mutiny										0.186*	0.189*	0.0383
										(0.108)	(0.108)	(0.0953)
Quit										−0.0290	−0.0315	−0.00165
										(0.0781)	(0.0782)	(0.0705)
Sleep										−0.0820	−0.0758	−0.00236
										(0.0696)	(0.0698)	(0.0645)
Striking										0.0466	0.0491	0.113
										(0.0898)	(0.0899)	(0.0822)
Rape										−0.0467	−0.0473	0.0253
										(0.104)	(0.104)	(0.107)
ΔLog casualties											0.000332	−0.00213
											(0.00553)	(0.00437)
ΔLog casualties											0.00487	−0.00140

(continued)

Table 3. (continued)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
30 d ago											(0.00512)	(0.00410)
Distance to coast												−0.000409*
												(0.000211)
Distance to Berlin												0.000262
												(0.000383)
Year fixed effects	N	N	N	Y	Y	N	N	N	N	Y	Y	Y
Month fixed effects	N	N	N	N	N	Y	N	N	N	N	N	N
Day fixed effects	N	N	N	N	N	N	Y	N	N	N	N	N
Division fixed effects	N	N	N	N	N	N	N	Y	N	Y	Y	Y
Joint test of fixed effects				0.0790	0.252	0.486	0.111	0.0554	0.770			
Joint test of Irish *x* year FE					0.864							
Joint test of casualties											0.615	
Joint test of distance												0.152
Constant	0.123***	0.204***	0.956***	0.0824**	0.0714	0.116***	0.157***	−2.90e-13	0.138***	0.113	0.160	0.665**
	(0.00751)	(0.0321)	(0.0668)	(0.0394)	(0.0442)	(0.0250)	(0.0217)	(0.329)	(0.0119)	(0.330)	(0.332)	(0.338)
N	2408	2408	2408	2408	2408	2408	2408	2408	2044	2408	2408	1612
R^2	0.000	0.003	0.583	0.004	0.004	0.005	0.004	0.040	0.000	0.099	0.102	0.637

Notes: All regressions use ordinary least squares on death sentences occurring in France & Flanders before the end of World War I. Death sentences recorded without divisions or from the Labor Corps were removed. Log casualties is calculated as log(1+ casualties). ΔLog casualties is defined as the difference in Log casualties 1–29 d ago versus 30–59 d ago. ΔLog casualties 30 d ago is defined as the difference in Log casualties 30–59 d ago versus 60–89 d ago. Distances are calculated based on the soldier's unit's participation in battles and are interpolated between battles. Distances are set to missing before the first battle and after the last battle. Territorial/new/regular army status is not assigned for Indian, Australian, Canadian, or New Zealand divisions. Regressions including age also dummy out age when it is missing (i.e. assign a constant and include an indicator for age being missing). Standard errors in parentheses. Results are similar with logit or probit.

*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

Table 4. Are observable characteristics correlated with execution decisions? (all death sentences).

Panel A	**Joint test of significance**
Brigade unit	0.277
Corp unit	0.190
Army unit	0.328
Brigade commanding officer	0.670
Division commanding officer	0.185
Division first general staff officer	0.517
Corp commanding officer	0.366
Corp first general staff officer	0.0900
Army commanding officer	0.0688
Army first general staff officer	0.308
GHQ commanding officer	0.369
GHQ first general staff officer	0.455
Irish soldier x Irish officer FE	0.452
Military indiscipline 30–59 and 60–89 d ago	0.325
Death sentences 30–59 and 60–89 d ago	0.109
Execution rate 30–59 and 60–89 d ago	0.324
Panel B	
Aggregation level	**Correlation with lag decision**
Division	0.0195
	(0.0285)
Brigade	0.00486
	(0.0333)
Corp	0.0469
	(0.0288)
Army	−0.00508
	(0.0318)
Army type	−0.00762
	(0.0333)
All	0.0790
	(0.0503)

Notes: Data are restricted to death sentences occurring in France & Flanders before the end of World War I. Death sentences recorded without divisions or from the Labor Corps were removed. In Panel A, each row reports a separate ordinary least squares regression and tests of joint significance of the fixed effects or measures of the recent battle environment. Military indiscipline and death sentences are calculated as log(1+ number). Military indiscipline is the average of absentees and trials measured from the War Diaries, Police Gazettes, and FGCM trial registries. Lag execution rates is a set of controls comprising the numbers of executions and commutations within each time window. Units or officers that appeared with less than 10 frequency were categorized in a separate "other" category. All regression models include year, division, and Irish fixed effects. In Panel B, each row reports a separate ordinary least squares stacked autocorrelation regression. The strings of events within each unit were stacked and the first event within each unit was excluded as a dependent variable. If more than one event occurred on a day within a unit, the average outcome was calculated for that day. All regression models include year fixed effects and the leave-one-out mean execution rate of the unit. Standard errors in parentheses. Results are similar with logit or probit.

$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Extensive robustness tests (including additional variables like seasonality or day of week) along with visual assessments of randomness appear in the Supplementary Appendix, reinforcing these central findings without altering the conclusion of random assignment.

6. EMPIRICAL SPECIFICATIONS

6.1 Duration analysis

My first modeling approach is to assume that the elapsed time from the most recent deterrence event to the next absence *in a particular unit* is a random variable drawn from some

distribution parameterized by unit and time characteristics (i.e. y is drawn from a distribution with a pdf f). For exposition's sake I use an exponential distribution, though other parametric distributions are possible. I assume that the likelihood of observing an elapsed time of y from a given deterrence event to the next absence is given by $f(y) = \lambda \exp(-\lambda y)$.

The hazard rate, λ, depends upon the characteristics of that particular deterrence event. $\lambda = \beta_0 + \beta_{ex} ex_{ij} + \beta_{exd} ex_{ij} \cdot des_{ij} + \beta_{des} \cdot des_{ij} + \gamma^C cas_{it} + \gamma_j^U + \gamma_{year(j)=T}^T$. Military units are indexed by i, observations by j. *ex* is an indicator for an execution, *des* is an indicator that the trial was for desertion, *cas* is the casualty rate and γ^U and γ^T are unit and year fixed-effects, respectively. Collectively, I refer to these parameters as a vector θ. The specification can also be interpreted as follows: *cas* controls for the cost of staying and *ex* captures exogenous variation in perceptions of costs.

It is possible, however, that an execution or commutation occurs at the end of the data frame, in which case the elapsed time y is no longer a realization of the time until an absence, but rather a censored value. I assume that without the intervention I would have eventually observed an absence. In these censored cases, which I indicate with $d = 0$, the likelihood is not $f(y|\theta)$, but rather $1 - F(y|\theta)$. The log-likelihood function consistent with this censoring is given by: $L(\theta) = \sum_{j=1}^{N} d_j \log\Big(f(y_j|\lambda(\theta))\Big) + (1 - d_j)\Big(1 - F(y_j|\lambda(\theta))\Big)$.

When analyzing the impact of the most recent event, the calculations treat desertions and capital sentences that occurred in pairs or groups as one observation since the decisions to execute or commute these soldiers were not independent: almost without exception, they were determined simultaneously and with identical outcomes. The time until the next absence is calculated beginning on the following day. Absences that occurred on the day of an event are considered as having occurred the previous night, so they do not count as the first absence after an event. Multiple absences or events on the same day from different ethnicities are considered as British as they constitute the typical soldier.[2]

6.2 The weak-SUTVA approach

I assume that past events matter, but that they fade out exponentially, according to some parameter k. I test values of k such that $k = -\frac{\log\frac{1}{2}}{\Delta t}$ where Δt takes values of 7, 14, 30, 60 and 90, corresponding to deterrence-effect half-lives of 1 week, 2 weeks, 1 month, 2 months, and 3 months. In the weak-SUTVA approach, I define two sets: $E_{ex}(t^*) \equiv$ times of all executions in the unit prior to t^* and $E_{cm}(t^*) \equiv$ times of all commutions in the unit prior to t^*.

These two terms measure the cumulative effects of past events, one for executions and one for commutations. They also measure idiosyncratic variation in execution rates over time within divisions, since the sequence is also exogenous. Differences in the effects of these two terms characterize the effect of exogenous variation in the application of the death penalty. Neither term by itself has a causal interpretation because the number of death sentences could be endogenous. To be consistent with the strong-SUTVA parameter, multiple events on the same day and division are still treated as one event. $D_{ex}(k) = \sum_{t \in E_{ex}(t^*)} e^{-k(t^* - t)}$ and $D_{cm}(k) = \sum_{t \in E_{cm}(t^*)} e^{-k(t^* - t)}$. The hazard is the original hazard plus two terms for past executions and commutations: $\lambda'(k) = \lambda + \alpha_{ex} D_{ex} + \alpha_{cm} D_{cm}$.

Results of the hazard model are presented in the main tables with standard errors clustered at the division level because the weak-SUTVA parameters are serially correlated within division. The Supplementary Appendix tables present two checks—one set of results

[2] A date with an Irish execution (absence) means that only Irish were executed (absent).

without clustering (to see if the statistical significance is similar), and another set of results where time is run backward and I calculate the time until the previous absence before a treatment event (to see if there is a null result). In the specification check with time run backward, to minimize leakage, where the absence event that led to the death sentence is included by chance as an outcome, the clock begins 90 d into the past.

6.3 Day-by-day probability, maximum likelihood approach

One difficulty of treating each death sentence as an observation, with an indicator for executions as the primary independent variable and absences as an outcome (either a count of absences or duration until the next absence) is that each unit experiences a whole sequence of executions and commutations. These past deterrent effects presumably affect the probability of future absences within that unit, and hence it is hard to see why they can be ignored. My response is to use a framework where the effects of past events are explicitly modeled. I assume that each unit had some probability of experiencing absence on any particular day, and that this probability depends upon military unit and year fixed effects, all past death sentences, including the nature of the crime and the outcome, and their distance in time from the present day and the instantaneous casualty rate.

Military Units: $i = 1 \ldots I$
Time: $t = 1 \ldots T$ Measured from day 0, July 28, 1914.
Absences: $a_i(t)$ is an indicator for whether there was an absence in unit i on day t.
Preceding Events: $K_i(t)$ is the set of past deterrence event dates in a unit i (executions or commutations) before time t; $|K_i(t)|$ is the number of events in the set.
Day: t_k is the day on which the k^{th} element of K occurred.
Execution or Commutation: x_k is an indicator for execution or commutation.
Crime Type: d_k is an indicator for desertion or some other crime.

I use the logit as my link function, so the probability of an absence in unit i on day t is: $p_i(t) = \frac{1}{1+e^{-z(i,t;\theta)}}$, where $z(i,t;\theta)$ is $z(i,t;\theta) = \left(\sum_{k=1}^{|K_i(t)|} e^{-\lambda(t-t_k)} D(k)\right) + X(t)\gamma$,

$$D(k) = \beta \cdot \mathbf{E}(\mathbf{k}) = \begin{pmatrix} \beta_{exd} & \beta_{exo} & \beta_{sd} & \beta_{so} \end{pmatrix} \cdot \begin{pmatrix} x_k d_k \\ x_k \\ d_k \\ 1 \end{pmatrix}, \quad \text{and} \quad X(t)\gamma = \gamma^0 + \gamma^C cas_{it} + \gamma_i^U + \gamma_{year(t)}^T.$$

$\beta_{exd} \equiv$ effect of executing a deserter, $\beta_{exo} \equiv$ effect of executing for any crime, $\beta_{sd} \equiv$ effect of a desertion death sentence, and $\beta_{so} \equiv$ effect of a death sentence for any crime.

I define a vector of parameters: $\theta = (\lambda, \beta_{exd}, \beta_{exo}, \beta_{cd}, \beta_{co}; \gamma^0, \gamma^C, \boldsymbol{\gamma}^{\mathbf{U}}, \boldsymbol{\gamma}^{\mathbf{T}})$. $X(t)$ is a collection of covariates, such as the instantaneous, unit-specific danger rate (computed from casualties) and a unit fixed effect. The effects of past events fade as time progresses. There is one λ for both executions and commutations, that is, events are "forgotten" at the same rate since commutations serve as control for executions.[3] F is the link-function whose range is $[0, 1]$. The log-likelihood is thus: $L = \sum_{i=1}^{I} \sum_{t=1}^{T} a_i(t) \log p_i(t) + [1 - a_i(t)] \log(1 - p_i(t))$. β_{exd} and β_{exo} have causal interpretation. I also introduce terms for Irish executions and Irish death sentences. Results are presented only with standard errors clustered at the division level

[3] An attempt to infer λ from the data did not converge, so I present estimates using different values of λ instead.

since the treatment variable is serially correlated within the division. The Supplementary Appendix tables present a specification check for null results where time is run backward.

7. RESULTS

This section presents three complementary analyses, each offering a different lens on whether executing condemned soldiers deters or spurs subsequent desertion. Section 7.1 provides a visual overview using Kaplan-Meier (survival) plots, highlighting basic patterns without controls. Sections 7.2 and 7.3 then detail two forms of duration analysis (strong versus weak SUTVA). Section 7.4 introduces a day-by-day probability approach to account for the cumulative effects of previous events. Section 7.5 ties these findings together, clarifying how they reinforce—or differ from—one another.

7.1 Graphical overview: Kaplan-Meier survival functions

Figures 3–5 plot univariate survival (time-to-next-absence) curves, comparing post-execution intervals (in red) and post-commutation intervals (in blue). Each panel distinguishes Irish from British executions. War Diaries (Figure 3): A clear separation appears. After Irish executions, the survival curve shifts left, indicating shorter intervals to the next desertion—that is, a "spurring" effect. In contrast, after British executions, the curve shifts right, suggesting a modest deterrent effect. Police Gazettes and FGCM Registries (Figures 4 and 5): Similar patterns emerge, though they are generally less pronounced. Irish executions

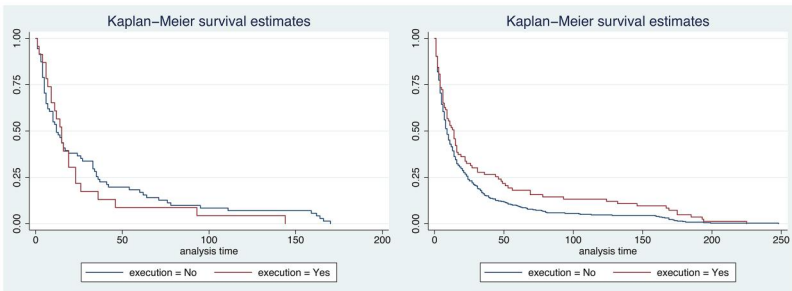

Figure 3. Nonparametric survival distributions (War Diaries).*Notes*: This figure shows the impact of Irish (left) and British (right) executions on time until next absence in the War Diaries dataset.

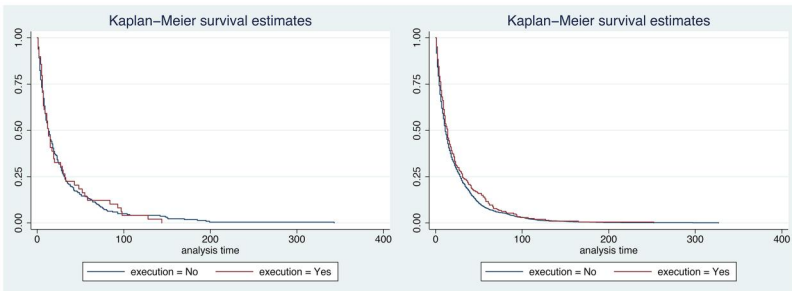

Figure 4. Nonparametric survival distributions (Police Gazettes).*Notes*: This figure shows the impact of Irish (left) and British (right) executions on time until next absence in the Police Gazettes dataset.

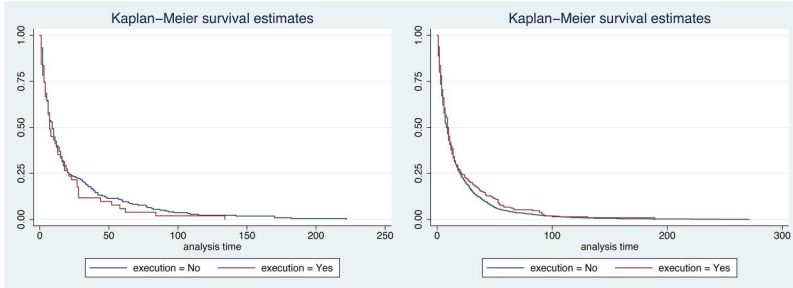

Figure 5. Nonparametric survival distributions (FGCM trial registries)*Notes*: This figure shows the impact of Irish (left) and British (right) executions on time until next absence in the FGCM trial registries dataset.

again show some evidence of spurring, while British executions lean toward deterrence. Notably, this pattern is clearer in the FGCM Registries than in the Police Gazettes. One likely explanation is that FGCM data captures frontline trials more systematically, whereas the Gazette—published in the United Kingdom—may introduce additional noise. In particular, the weekly Gazette only contains absences for soldiers not yet apprehended, leading to incomplete B.E.F. data. Consequently, the War Diaries and FGCM Registries provide a more direct reflection of frontline outcomes, whereas the Gazette's selective coverage can obscure the distinction between execution and commutation intervals.

These raw, uncontrolled plots suggest two main themes: (1) there is limited or no overall deterrence effect for desertion, and (2) the ethnic identity of the executed soldier matters. I now refine these insights with multivariate approaches.

7.2 Duration analysis, strong SUTVA

In a strong SUTVA framework, I assume that only the most recent execution or commutation affects the time to the next desertion. Table 5 presents hazard-model estimates (Exponential, Weibull, Cox) for each of the three datasets (War Diaries, Police Gazettes, FGCM trial registries). I also experiment with different assumptions about the exact date of commutation. Overall deterrence: Columns focusing on "any execution" versus "commutation" yield mixed evidence; in some specifications, there is a weak deterrent effect for British executions. But in most models, the coefficient is small or insignificant, indicating limited or no overall deterrence.

Irish versus British Soldiers: Table 6 then interacts the execution indicator with an "Irish soldier" indicator. I consistently find positive and significant coefficients on the "Irish execution" term, implying shorter time to next absence—a "spurring" effect. By contrast, "British execution" sometimes deters, sometimes shows no effect, but does not significantly spur desertion.

Magnitude: Point estimates suggest that, in the War Diaries sample, an Irish execution can triple the hazard rate (i.e. reduce median time to next absence by about 66%). Effects are somewhat smaller (but still positive) in the Police gazettes and FGCM datasets. These magnitudes often exceed the effect of an increase in casualties, underscoring how illegitimate punishment can overshadow pure battlefield risks.

7.3 Duration analysis, weak SUTVA

One might worry that past events (multiple executions) have lingering effects. Table 7 addresses this by including cumulative measures of prior executions and prior commutations

Table 5. Effects of executions versus commutations on elapsed time until next absence.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: War Diaries	**Exp/+14**	**Wb/+14**	**Cox/+14**	**Exp/NN**	**Wb/NN**	**Cox/NN**	**Exp/C = T**	**Wb/C = T**	**Cox/C = T**
Execution	−0.177	−0.144	−0.158	0.183	0.167	0.129	0.280*	0.250*	0.209
	(0.174)	(0.152)	(0.155)	(0.202)	(0.171)	(0.167)	(0.147)	(0.132)	(0.129)
ΔLog casualties	0.0928	0.0802	0.0648	0.0494	0.0372	0.0159	0.124**	0.110**	0.0992**
	(0.0671)	(0.0579)	(0.0516)	(0.0833)	(0.0715)	(0.0638)	(0.0629)	(0.0562)	(0.0505)
ΔLog casualties	0.151**	0.139***	0.108**	0.140**	0.132**	0.107**	0.208***	0.190***	0.159***
30 d ago	(0.0601)	(0.0506)	(0.0444)	(0.0692)	(0.0573)	(0.0515)	(0.0600)	(0.0519)	(0.0454)
N	536	536	536	536	536	536	536	536	536
Panel B: Police Gazette									
Execution	−0.0770	−0.0715	−0.0662	0.0503	0.0535	0.0567	−0.0179	−0.0133	−0.0114
	(0.0974)	(0.0912)	(0.0838)	(0.0885)	(0.0825)	(0.0764)	(0.102)	(0.0956)	(0.0883)
ΔLog casualties	0.0569*	0.0546*	0.0517*	0.0518*	0.0502*	0.0495*	0.0584*	0.0571*	0.0558*
	(0.0303)	(0.0290)	(0.0271)	(0.0292)	(0.0277)	(0.0264)	(0.0341)	(0.0327)	(0.0310)
ΔLog casualties	0.0620**	0.0601**	0.0584**	0.0685**	0.0664**	0.0646**	0.0719**	0.0706**	0.0695**
30 d ago	(0.0289)	(0.0276)	(0.0256)	(0.0296)	(0.0280)	(0.0265)	(0.0301)	(0.0288)	(0.0272)
N	1640	1640	1640	1638	1638	1638	1640	1640	1640
Panel C: FGCM trial registries (time until next desertion trial)									
Execution	−0.206*	−0.198*	−0.191**	0.135	0.121	0.114	0.0282	0.0283	0.0235
	(0.119)	(0.106)	(0.0948)	(0.112)	(0.100)	(0.0888)	(0.0926)	(0.0879)	(0.0796)
ΔLog casualties	0.0476	0.0387	0.0298	0.0563	0.0472	0.0386	0.0369	0.0339	0.0296
	(0.0420)	(0.0386)	(0.0349)	(0.0409)	(0.0373)	(0.0339)	(0.0444)	(0.0430)	(0.0405)
ΔLog casualties	0.0796**	0.0740**	0.0684**	0.0840**	0.0796**	0.0757**	0.0272	0.0248	0.0227
30 d ago	(0.0377)	(0.0361)	(0.0342)	(0.0378)	(0.0359)	(0.0343)	(0.0387)	(0.0380)	(0.0361)
N	1654	1654	1654	1654	1654	1654	1654	1654	1654

Notes: Outcome is elapsed time from death sentence resolution (execution or commutation) until next absence. "Exp," "Wb," and "Cox" use the exponential, Weibull and Cox models respectively to parameterize the baseline hazard. In columns subtitled "+14," the announcement of the commutation is assumed to occur 14 d after trial. In columns subtitled "NN" the nearest-neighbor method is used, which means the imputed announcement of the commutation is same as the most nearby execution announcement, while in columns labeled "C = T," the trial date is used as the announcement date of the execution and commutation. Log casualties is calculated as log(1+ casualties). ΔLog casualties is defined as the difference in Log casualties 1–29 d ago versus 30–59 d ago. ΔLog casualties 30 d ago is defined as the difference in Log casualties 30–59 d ago versus 60–89 d ago. All specifications include division and year fixed-effects. War Diaries analysis restricts to July 1916–June 1917, which is the time window for the surviving data. Standard errors clustered at the division level in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6. Effects of executions versus commutations on elapsed time until next absence differing by whether case was a desertion trial and whether soldier was Irish.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: War Diaries	**Exp/+14**	**Wb/+14**	**Cox/+14**	**Exp/NN**	**Wb/NN**	**Cox/NN**	**Exp/C = T**	**Wb/C = T**	**Cox/C = T**
Execution	−0.417	−0.394	−0.308	0.219	0.182	0.239	0.723**	0.627**	0.689**
	(0.736)	(0.687)	(0.670)	(0.359)	(0.324)	(0.313)	(0.338)	(0.304)	(0.273)
Desert	−0.0429	−0.0218	−0.00996	0.0470	0.0531	0.0511	0.138	0.146	0.133
	(0.305)	(0.274)	(0.240)	(0.302)	(0.265)	(0.234)	(0.311)	(0.283)	(0.246)
Ex-desert	−0.00330	0.0467	−0.0154	−0.241	−0.161	−0.218	−0.650*	−0.555*	−0.627**
	(0.746)	(0.700)	(0.671)	(0.406)	(0.360)	(0.336)	(0.374)	(0.337)	(0.291)
Irish	−0.727***	−0.629***	−0.464***	−0.646***	−0.541***	−0.391**	−0.475*	−0.407*	−0.263
	(0.179)	(0.176)	(0.147)	(0.207)	(0.194)	(0.173)	(0.252)	(0.237)	(0.208)
Ex-Irish	1.179***	1.003***	0.805***	0.768***	0.579***	0.399**	0.619***	0.537***	0.355*
	(0.285)	(0.256)	(0.248)	(0.222)	(0.195)	(0.190)	(0.202)	(0.201)	(0.195)
N	536	536	536	536	536	536	536	536	536
Panel B: Police Gazette									
Execution	−0.372	−0.355	−0.340	0.0857	0.0890	0.0811	0.206	0.197	0.163
	(0.387)	(0.365)	(0.333)	(0.277)	(0.259)	(0.246)	(0.286)	(0.266)	(0.249)
Desert	−0.0459	−0.0409	−0.0341	−0.0245	−0.0228	−0.0212	−0.0510	−0.0488	−0.0454
	(0.0938)	(0.0888)	(0.0820)	(0.0887)	(0.0828)	(0.0772)	(0.0890)	(0.0849)	(0.0797)
Ex-desert	0.251	0.241	0.235	−0.0773	−0.0747	−0.0611	−0.327	−0.309	−0.267
	(0.422)	(0.399)	(0.365)	(0.316)	(0.295)	(0.279)	(0.318)	(0.298)	(0.280)
Irish	−0.179	−0.172*	−0.164*	−0.187*	−0.175*	−0.169*	−0.119	−0.116	−0.114
	(0.109)	(0.103)	(0.0950)	(0.106)	(0.0990)	(0.0925)	(0.103)	(0.0983)	(0.0937)
Ex-Irish	0.431**	0.410**	0.387**	0.219	0.203	0.196	0.408**	0.392**	0.382**
	(0.196)	(0.181)	(0.161)	(0.199)	(0.185)	(0.169)	(0.207)	(0.193)	(0.177)
N	1640	1640	1640	1638	1638	1638	1640	1640	1640
Panel C: FGCM trial registries (time until next desertion trial)									
Execution	−0.709	−0.648	−0.588	0.0476	0.0296	0.0233	0.0772	0.0703	0.0526
	(0.522)	(0.473)	(0.420)	(0.308)	(0.276)	(0.252)	(0.252)	(0.240)	(0.222)
Desert	0.0535	0.0411	0.0235	0.110	0.0816	0.0482	−0.0590	−0.0656	−0.0855
	(0.136)	(0.121)	(0.108)	(0.135)	(0.121)	(0.111)	(0.133)	(0.127)	(0.118)
Ex-desert	0.442	0.397	0.351	−0.0496	−0.0232	−0.00214	−0.164	−0.148	−0.116

(continued)

Table 6. (continued)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	(0.555)	(0.506)	(0.451)	(0.343)	(0.311)	(0.286)	(0.267)	(0.256)	(0.238)
Irish	−0.353**	−0.326**	−0.297**	−0.221	−0.196	−0.172	−0.252*	−0.243*	−0.218*
	(0.141)	(0.129)	(0.117)	(0.142)	(0.130)	(0.118)	(0.132)	(0.127)	(0.118)
Ex-Irish	0.718***	0.639***	0.560***	0.651***	0.566***	0.480***	0.556**	0.525**	0.465**
	(0.243)	(0.224)	(0.206)	(0.206)	(0.191)	(0.178)	(0.234)	(0.226)	(0.215)
N	1654	1654	1654	1654	1654	1654	1654	1654	1654

Notes: Outcome is elapsed time from death sentence resolution (execution or commutation) until next absence. "Exp," "Wb," and "Cox" use the exponential, Weibull and Cox models respectively to parameterize the baseline hazard. In columns subtitled "+14," the announcement of the commutation is assumed to occur 14 d after trial. In columns subtitled "NN" the nearest-neighbor method is used, which means the imputed announcement of the commutation is same as the most nearby execution announcement, while in columns labeled "C = T," the trial date is used as the announcement date of the execution and commutation. All specifications include division and year fixed-effects and ΔLog casualties and ΔLog casualties 30 d ago. War Diaries analysis restricts to July 1916–June 1917, which is the time window for the surviving data. Standard errors clustered at the division level in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 7. Effects of execution versus commutation on elapsed time until next absence, full sample, weak SUTVA.

	War Diaries						Police Gazettes						FGCM trial registries (desertion trials)					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
Execution	−0.417	−0.390	−0.378	−0.368	−0.374	−0.389	−0.372	−0.544	−0.513	−0.475	−0.432	−0.411	−0.709	−0.919	−0.895	−0.856	−0.791	−0.752
	(0.736)	(0.798)	(0.781)	(0.742)	(0.701)	(0.679)	(0.387)	(0.527)	(0.497)	(0.447)	(0.407)	(0.393)	(0.522)	(0.699)	(0.675)	(0.622)	(0.566)	(0.541)
Desert	−0.0429	−0.0203	−0.0188	−0.0253	−0.0517	−0.0763	−0.0459	−0.0711	−0.0804	−0.0656	−0.0458	−0.0412	0.0535	0.0272	0.0179	0.0317	0.0559	0.0630
	(0.305)	(0.300)	(0.302)	(0.300)	(0.298)	(0.297)	(0.0938)	(0.0879)	(0.0859)	(0.0851)	(0.0882)	(0.0912)	(0.136)	(0.127)	(0.123)	(0.122)	(0.127)	(0.132)
Ex-desert	−0.00330	−0.0249	−0.0306	−0.0251	0.000868	0.0202	0.251	0.312	0.284	0.258	0.234	0.228	0.442	0.518	0.504	0.474	0.430	0.411
	(0.746)	(0.782)	(0.762)	(0.728)	(0.697)	(0.683)	(0.422)	(0.562)	(0.529)	(0.480)	(0.441)	(0.426)	(0.555)	(0.741)	(0.716)	(0.663)	(0.607)	(0.580)
Irish	−0.727***	−0.769***	−0.784***	−0.822***	−0.850***	−0.836***	−0.179	−0.158	−0.172	−0.186*	−0.189*	−0.185*	−0.353***	−0.351**	−0.365**	−0.373***	−0.366**	−0.358*
	(0.179)	(0.181)	(0.190)	(0.212)	(0.226)	(0.220)	(0.109)	(0.110)	(0.109)	(0.107)	(0.108)	(0.108)	(0.141)	(0.143)	(0.144)	(0.145)	(0.144)	(0.143)
Ex-Irish	1.179***	1.258***	1.262***	1.310***	1.359***	1.347***	0.431**	0.432**	0.440**	0.437**	0.424**	0.420**	0.718***	0.726***	0.750***	0.778***	0.775***	0.761***
	(0.285)	(0.323)	(0.308)	(0.282)	(0.270)	(0.268)	(0.196)	(0.210)	(0.202)	(0.196)	(0.196)	(0.196)	(0.243)	(0.260)	(0.255)	(0.253)	(0.251)	(0.249)
ΔLog casualties	0.0870	0.0812	0.0781	0.0721	0.0626	0.0574	0.0537*	0.0738***	0.0774***	0.0748*	0.0682*	0.0629*	0.0422	0.0597	0.0658+	0.0662+	0.0605	0.0543
	(0.0602)	(0.0602)	(0.0599)	(0.0595)	(0.0597)	(0.0603)	(0.0301)	(0.0269)	(0.0281)	(0.0292)	(0.0294)	(0.0293)	(0.0413)	(0.0371)	(0.0380)	(0.0389)	(0.0394)	(0.0398)
ΔLog casualties	0.170***	0.173***	0.170***	0.163***	0.153**	0.148**	0.0652**	0.0619**	0.0681**	0.0719***	0.0709***	0.0689***	0.0856**	0.0826**	0.0924***	0.100***	0.0987***	0.0944***
30 d ago	(0.0569)	(0.0596)	(0.0599)	(0.0613)	(0.0634)	(0.0641)	(0.0286)	(0.0276)	(0.0272)	(0.0275)	(0.0275)	(0.0274)	(0.0370)	(0.0358)	(0.0350)	(0.0344)	(0.0342)	(0.0343)
Ex's—7 d		−0.194						0.214**						0.356***				
		(0.214)						(0.0939)						(0.116)				
Cm's—7 d		−0.0304						0.214***						0.183***				
		(0.158)						(0.0562)						(0.0453)				
Ex's—14 d			−0.146						0.0840						0.172*			
			(0.155)						(0.0788)						(0.0901)			
Cm's—14 d			−0.0439						0.156***						0.140***			
			(0.108)						(0.0383)						(0.0328)			
Ex's—30 d				−0.147						−0.0270						0.0216		
				(0.130)						(0.0703)						(0.0738)		
Cm's—30 d				−0.0653						0.105***						0.0990***		
				(0.0722)						(0.0267)						(0.0255)		

(continued)

Table 7. (continued)

	War Diaries						Police Gazettes						FGCM trial registries (desertion trials)					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
Ex's—60 d					−0.193						−0.0734						−0.0390	
					(0.132)						(0.0643)						(0.0667)	
Cm's—60 d					−0.0856						0.0651***						0.0623***	
					(0.0546)						(0.0211)						(0.0206)	
Ex's—90 d						−0.222*						−0.0857						−0.0569
						(0.132)						(0.0624)						(0.0650)
Cm's—90 d						−0.0887*						0.0455**						0.0425**
						(0.0480)						(0.0190)						(0.0181)
N	536	536	536	536	536	536	1640	1640	1640	1640	1640	1640	1654	1654	1654	1654	1654	1654

Notes: All specifications use the "+14" commutation date imputation method and all specifications use exponential models to parameterize baseline hazard rates. All specifications include division and year fixed-effects. Log casualties is calculated as log(1+ casualties). ΔLog casualties is defined as the difference in Log casualties 1–29 d ago versus 30–59 d ago. ΔLog casualties 30 d ago is defined as the difference in Log casualties 30–59 d ago versus 60–89 d ago. The regressors labeled ex's-Yd or cm's-Yd measure the cumulative effects of previous deterrence events in the unit. Y is the half-life of the effect. War Diaries analysis restricts to July 1916–June 1917, which is the time window for the surviving data. Standard errors clustered at the division level in parentheses.

*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

Table 8. Effects of execution versus commutation on ethnicity of next absence.

	% of next absences that are Irish		
Panel A: +14 imputation	**War Diaries**	**Police Gazettes**	**FGCM (desertion trials)**
All death sentences			
Irish execution	19.2	9.8	21.6
Non-Irish execution	11.1	9.0	15.3
Irish commutation	13.3	16.4	12.0
Non-Irish commutation	13.1	14.4	13.3
Desertion death sentences			
Irish execution	20.0	9.3	23.3
Non-Irish execution	9.5	9.1	15.5
Irish commutation	14.0	17.8	13.2
Non-Irish commutation	12.8	15.8	14.4
Panel B: NN imputation			
All death sentences			
Irish execution	19.2	9.8	21.6
Non-Irish execution	11.1	9.0	15.3
Irish commutation	12.5	16.4	12.4
Non-Irish commutation	12.6	13.7	13.5
Desertion death sentences			
Irish execution	20.0	9.3	23.3
Non-Irish execution	9.5	9.1	15.5
Irish commutation	10.4	17.4	13.2
Non-Irish commutation	12.1	15.8	14.2
Panel C: C = T imputation			
All death sentences			
Irish execution	15.4	7.8	15.7
Non-Irish execution	10.4	10.0	16.2
Irish commutation	9.1	17.6	11.0
Non-Irish commutation	14.0	13.8	12.1
Desertion death sentences			
Irish execution	15.0	7.0	16.3
Non-Irish execution	7.4	10.2	16.5
Irish commutation	9.6	20.6	10.8
Non-Irish commutation	13.8	15.6	13.1

Notes: In panels subtitled "+14," the announcement of the commutation is assumed to occur 14 d after trial. In panels subtitled "NN" the nearest-neighbor method is used, which means the imputed announcement of the commutation is same as the most nearby execution announcement, while in panels labeled "C = T," the trial date is used as the announcement date of the execution and commutation. War Diaries analysis restricts to July 1916–June 1917, which is the time window for the surviving data.

with different "half-lives" (7–90 d). Most recent event: My earlier result—that Irish executions spur desertion—is robust to controlling for prior events. Because the timing of executions is effectively random, including past events does not meaningfully change the estimated effect of the most recent one.

Aggregate execution rate: When I allow for a decaying effect of all executions, I see some modest overall deterrence from a higher local execution rate. In other words, repeated executions in the same division might, over time, discourage desertion for British soldiers. But for Irish soldiers, repeated Irish executions appear consistently correlated with increased desertions.

Hence, relaxing the strong SUTVA assumption does not alter the core conclusion about Irish versus British. It does reveal that, in the long run, a high execution rate may provide

some deterrence—except where the executed soldier belongs to a group whose loyalty is already tenuous.

7.4 Day-by-day probability approach

I next shift to a day-by-day modeling framework (Tables 9 and 10) that treats each division-day as an observation, controlling for the cumulative effect of all prior events. Columns vary the "half-life" of memory (7 d, 1 month, 3 months). The results again reinforce the central theme: (1) When absences are aggregated (Table 9): Evidence of deterrence is sporadic and not always robust, though in certain specifications (e.g. the Police Gazette data) I do see a mild negative effect of an execution. (2) Focusing on Irish versus non-Irish Absences (Table 10): Irish executions systematically increase the probability of an Irish absence on the same or subsequent day, consistent with the "spurring" interpretation. In contrast, British executions either have no effect or appear to deter absences slightly.

The day-by-day approach thus adds further weight to the notion that, unless the executed soldier is Irish, executions seldom produce a major shift in desertion. Even so, some deterrence can arise in certain data sources or under longer half-life assumptions—indicating that cumulative knowledge of repeated executions might dissuade future deserters, at least among non-Irish troops.

7.5 Synthesis of findings

Although each method—strong SUTVA (Section 7.2), weak SUTVA (Section 7.3), and day-by-day (Section 7.4)—implements a different modeling strategy, they converge on three overarching conclusions:

1) Limited overall deterrence: No analysis finds strong, consistent evidence that executing a deserter reliably decreases subsequent desertions across the board. At best, some specifications show a modest deterrent effect for British soldiers under certain conditions.
2) Powerful Irish "backfire": All three approaches indicate that executing an Irish soldier spurs desertion in that division, often more forcefully than the effect of rising casualties. This pattern emerges regardless of how the execution date is imputed or whether cumulative effects are modeled.
3) Role of repeated executions: The weak-SUTVA and day-by-day frameworks highlight that repeated executions may, over time, produce some general deterrence—except for Irish units, where multiple Irish executions reinforce the perception of illegitimacy and lead to more desertions.

The results align with the historical context that harsh punishments are only effective if they are perceived as fair or legitimate. For British soldiers, repeated public executions may have had some chilling effect; for Irish soldiers, each execution reinforced doubts about the British command and spurred further desertion. Table 8 provides direct evidence of this dynamic, showing that Irish executions roughly double the likelihood the next absentee is Irish. A more detailed description of the results appears in the Supplementary Appendix, reinforcing the main findings.

Overall, the analyses underscore the importance of legitimacy in shaping how individuals respond to extreme sanctions. Even a penalty as severe as death—implemented swiftly and publicly—may fail to deter, and can even backfire among groups who perceive the authority as unjust. The final section discusses how this interpretation connects to broader questions

Table 9. Day-by-day framework, all absences.

Half-life	(1) 1 week	(2) 2 weeks	(3) 1 month	(4) 2 months	(5) 3 months
Panel A: War Diaries					
Execution	0.00894	0.00760	0.00804	0.00920	0.00957
	(0.0110)	(0.00925)	(0.00902)	(0.00832)	(0.00742)
Death sentence	0.00170	0.000383	−0.000446	−0.000740	−0.000807
	(0.00202)	(0.00108)	(0.000769)	(0.000766)	(0.000768)
Ex-Irish	−0.0124	−0.00516	−0.00106	−0.00260	−0.00453
	(0.0119)	(0.0108)	(0.00944)	(0.00767)	(0.00688)
Irish	0.00608	0.00486	0.00191	0.000680	0.000635
	(0.00877)	(0.00669)	(0.00490)	(0.00353)	(0.00316)
Ex-desert	−0.0177	−0.0135	−0.0114	−0.0111	−0.0112
	(0.0112)	(0.00890)	(0.00841)	(0.00799)	(0.00736)
Desert	0.000511	0.000805	0.00127	0.00207	0.00271*
	(0.00280)	(0.00169)	(0.00125)	(0.00131)	(0.00146)
N	20,750	20,750	20,750	20,750	20,750
Panel B: Police Gazettes					
Execution	−0.0188***	−0.0174**	−0.0133	−0.00861	−0.00639
	(0.00601)	(0.00752)	(0.00794)	(0.00683)	(0.00578)
Death sentence	0.00340	0.00348	0.00329	0.00274	0.00238*
	(0.00211)	(0.00242)	(0.00230)	(0.00166)	(0.00127)
Ex-Irish	−0.00932	−0.00846	−0.00875	−0.0115**	−0.0128**
	(0.00618)	(0.00557)	(0.00529)	(0.00539)	(0.00527)
Irish	0.00316	0.00327	0.00363	0.00399	0.00396
	(0.00486)	(0.00508)	(0.00452)	(0.00345)	(0.00270)
Ex-desert	0.0115	0.0118	0.0102	0.00751	0.00626
	(0.00791)	(0.00860)	(0.00882)	(0.00723)	(0.00600)
Desert	−0.00385	−0.00438	−0.00419*	−0.00331*	−0.00278**
	(0.00289)	(0.00279)	(0.00247)	(0.00165)	(0.00115)
N	54,605	54,605	54,605	54,605	54,605
Panel C: FGCM desertion trial registries					
Execution	0.0122	0.0146	0.00819	0.000144	−0.00264
	(0.0223)	(0.0205)	(0.0158)	(0.0116)	(0.00992)
Death sentence	0.0106**	0.00628**	0.00356	0.00210	0.00145
	(0.00403)	(0.00311)	(0.00219)	(0.00155)	(0.00134)
Ex-Irish	−0.00844	−0.0143	−0.0111	−0.00676	−0.00469
	(0.0194)	(0.0144)	(0.0102)	(0.00814)	(0.00763)
Irish	−0.000543	0.00317	0.00454	0.00489	0.00484
	(0.00855)	(0.00661)	(0.00468)	(0.00344)	(0.00304)
Ex-desert	−0.0125	−0.0156	−0.0121	−0.00601	−0.00362
	(0.0193)	(0.0179)	(0.0147)	(0.0116)	(0.0102)
Desert	0.00236	0.00320	0.00256	0.00159	0.00108
	(0.00388)	(0.00357)	(0.00277)	(0.00204)	(0.00175)
N	59,355	59,355	59,355	59,355	59,355

Notes: Outcome is whether there was any absence on that day and division. All specifications use the "+14" commutation date imputation method and include division and year fixed-effects, ΔLog casualties, and ΔLog casualties 30 d ago. The half-life row indicates the assumed exponential half-life of the effect of past events. War Diaries analysis restricts to July 1916–June 1917, which is the time window for the surviving data. War Diaries analysis restricts to July 1916–June 1917, which is the time window for the surviving data. Standard errors clustered at the division level in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 10. Day-by-day framework, Irish—non-Irish absence.

Half-life	(1) 1 week	(2) 2 weeks	(3) 1 month	(4) 2 months	(5) 3 months
Panel A: War Diaries					
Execution	−0.0207	−0.0129	−0.00711	−0.00546	−0.00531
	(0.0143)	(0.0124)	(0.00923)	(0.00664)	(0.00554)
Death sentence	−0.00106	−0.000889	−0.000577	−0.000368	−0.000286
	(0.00162)	(0.00137)	(0.00113)	(0.000938)	(0.000830)
Ex-Irish	0.0255**	0.0219**	0.0156*	0.0126*	0.0119*
	(0.0127)	(0.0105)	(0.00839)	(0.00686)	(0.00611)
Irish	0.0000700	−0.000649	−0.000233	0.000565	0.00105
	(0.00781)	(0.00643)	(0.00491)	(0.00356)	(0.00296)
Ex-desert	0.0268*	0.0174	0.00913	0.00542	0.00468
	(0.0153)	(0.0131)	(0.00926)	(0.00665)	(0.00569)
Desert	−0.00595*	−0.00439*	−0.00241	−0.00159	−0.00158
	(0.00331)	(0.00244)	(0.00178)	(0.00149)	(0.00140)
N	20,750	20,750	20,750	20,750	20,750
Panel B: Police Gazettes					
Execution	0.00661*	0.00495	0.000708	−0.00208	−0.00266
	(0.00388)	(0.00448)	(0.00454)	(0.00403)	(0.00363)
Death sentence	−0.000576	−0.00118	−0.00120	−0.00108	−0.00100*
	(0.00143)	(0.00142)	(0.00116)	(0.000747)	(0.000544)
Ex-Irish	0.0121**	0.00937**	0.00784***	0.00760***	0.00736***
	(0.00545)	(0.00412)	(0.00289)	(0.00239)	(0.00233)
Irish	−0.00474	−0.00312	−0.00200	−0.00120	−0.000779
	(0.00314)	(0.00237)	(0.00192)	(0.00144)	(0.00119)
Ex-desert	−0.00778	−0.00491	−0.000327	0.00160	0.00150
	(0.00595)	(0.00547)	(0.00521)	(0.00448)	(0.00388)
Desert	0.000462	0.00136	0.00123	0.000937	0.000832
	(0.00169)	(0.00167)	(0.00136)	(0.000872)	(0.000635)
N	54,605	54,605	54,605	54,605	54,605
Panel C: FGCM desertion trial registries					
Execution	−0.0240	−0.0183	−0.00784	0.00197	0.00559
	(0.0162)	(0.0131)	(0.00972)	(0.00716)	(0.00620)
Death sentence	−0.00425	−0.00241	−0.00147	−0.00104	−0.000829
	(0.00514)	(0.00329)	(0.00193)	(0.00114)	(0.000885)
Ex-Irish	0.00333	0.00267	−0.000828	−0.00408	−0.00540
	(0.0160)	(0.0120)	(0.00782)	(0.00576)	(0.00510)
Irish	0.00498	0.00192	0.0000538	−0.000915	−0.00116
	(0.00575)	(0.00451)	(0.00328)	(0.00236)	(0.00202)
Ex-desert	0.0313*	0.0258**	0.0162*	0.00644	0.00249
	(0.0157)	(0.0120)	(0.00883)	(0.00679)	(0.00618)
Desert	−0.00724	−0.00605*	−0.00400*	−0.00230	−0.00153
	(0.00517)	(0.00350)	(0.00222)	(0.00146)	(0.00120)
N	59,355	59,355	59,355	59,355	59,355

Notes: Outcome is whether there was any Irish absence on that day and division minus whether there was any non-Irish absence on that day and division. All specifications use the "+14" commutation date imputation method and include division and year fixed-effects, ΔLog casualties, and ΔLog casualties 30 d ago. The half-life row indicates the assumed exponential half-life of the effect of past events. War Diaries analysis restricts to July 1916–June 1917, which is the time window for the surviving data. War Diaries analysis restricts to July 1916–June 1917, which is the time window for the surviving data. Standard errors clustered at the division level in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

about capital punishment and law enforcement, both within and beyond the wartime context.

7.6 External validity and generalizability

A natural question is whether these results, drawn from an extreme wartime setting, generalize to civilian contexts where the death penalty is more commonly debated. On one hand, visibility of executions was exceptionally high in the British Army: firing squads performed the sentence in front of the deserter's own battalion, often the same day. Soldiers were constantly reminded that desertion could bring swift capital punishment. Such immediacy might amplify the deterrent effect compared to civilian settings, where legal proceedings are lengthy, less public, and capital punishment is carried out (if at all) years after sentencing.

On the other hand, the alternative to desertion—remaining in some of the most brutal trench combat of World War I—was itself perilous. A soldier facing near-certain danger might be less responsive to even a guaranteed death penalty, meaning the baseline incentive to desert could be extraordinarily high. Under more ordinary peacetime conditions, where the "cost" of not breaking the law is much lower than staying at the front, one might expect a different response.

It is therefore ambiguous whether the net effect of these unique features yields a stronger or weaker deterrent in this context. Soldiers might have been more deterred by highly publicized, immediate executions (leading us to overestimate the deterrent effect relative to a civilian context). Or, given the extreme risk of remaining in combat, deterrence could be weaker than in civilian life, leading us to underestimate the general deterrent impact of the death penalty.

Ultimately, these findings do not claim to resolve the broader societal debate on capital punishment once and for all. They do, however, underscore a key principle with potentially wider relevance: even under conditions seemingly optimal for maximizing deterrence—swift punishment, public application, extreme visibility—the effect can be limited or even backfire if the penalty is perceived as illegitimate by any sizable subgroup. The Supplementary Appendix provides a legitimacy-based model illustrating this mechanism, reinforcing the study's central claim that legitimacy and social context are crucial to the effectiveness of sanctions, whether on the battlefield or in civilian life.

8. CONCLUSION

Many nations grapple with law noncompliance, and sanctions, including the death penalty, often fail to enforce law effectively (Goldsmith and Posner 1999; Posner 2003). Despite its widespread use—applicable to 60% of the global population—empirical evidence on the death penalty's efficacy remains scant. This study's examination of British WWI executions offers a new perspective on this issue. It addresses the fundamental question: Do individuals respond to the heightened perceived risk of criminal sanction, including death (Nagin and Pepper 2012)? This research suggests that even under conditions of maximized deterrence—immediacy, public visibility, and wide promulgation—the death penalty may not be as potent a deterrent as often presumed.

I validate statistically the historical belief that execution or commutation decisions during WWI were essentially random. Leveraging this, along with archival data on desertions, I apply three analytical models: strong SUTVA focusing on immediate post-event effects, weak SUTVA incorporating broader historical context, and a day-by-day nonparametric model assessing cumulative past event impacts. The results reveal limited evidence that executions deterred absences overall. Notably, executing Irish soldiers significantly increased desertion

rates, particularly among the Irish, highlighting the complex interplay of ethnicity and military discipline.

These findings extend beyond historical military contexts, offering insights into contemporary discussions about the legitimacy of legal systems and their impact on compliance. The study presents a rare scenario where similar offenses receive drastically different sanctions, allowing for a nuanced exploration of how state-imposed punishments can inadvertently erode state legitimacy. It underscores the need to consider alternatives to harsh sanctions and emphasizes the importance of legitimacy and other nondeterrent factors in legal compliance, an area that warrants further exploration and research.

SUPPLEMENTARY MATERIAL

Supplementary material is available at *Journal of Law, Economics, & Organization* online.

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