# 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 military condemned over 3,000 soldiers to death, but executed only approximately 12% of these soldiers; the others received commuted sentences. Many historians believe that the military command confirmed or commuted sentences for reasons unrelated to the circumstances of a particular case and that the application of the death penalty was essentially a random, "pitiless lottery." Using a dataset on all capital cases during WWI, I statically investigate this claim and find that the data are consistent with an essentially random process. Using this result, I exploit variation in commutations and executions within military units to identify the deterrent effect of the death penalty, with deterrence measured by the elapsed time within a unit between the resolution of a death sentence (i.e., a commutation or execution) and subsequent absences within that unit. Absences are measured via "wanted" lists prepared by British military police units searching for deserters. I find limited evidence that executing deserters deterred absences, while executing non-deserters and Irish soldiers, regardless of the crime, spurred absences. This finding is potentially explicable as an iatrogenic effect where minorities react negatively to state-imposed violence.

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# 1 Introduction

## 1.1 Motivation

After decades of empirical research (Ehrlich 1975), there is little convincing evidence that the death penalty deters any form of misbehavior (Donohue and Wolfers 2006). What makes this absence of evidence so intriguing is that economic theory makes an unambiguous prediction: raising the cost of some activity will cause a decrease in its incidence, be it illegal parking, homicide, or military desertion. The great econometric challenge of death penalty research is that the death penalty is applied in way that makes definitive conclusions hard. In the US, states that allow the death penalty differ from states that do not in important ways that probably have independent effect on levels of crime. Further, assessing the effects of the death penalty requires the examination of crime rates in the future, but since crime has multiple causes, disentangling the effect of the death penalty from other confounding socio-economic or cultural factors is challenging.

Despite these empirical difficulties, whether the death penalty deters crime seems in principle to be an answerable question. In an interview with the New York Times[1] regarding the state of empirical death penalty research, Professor Justin Wolfers, a skeptic of existing empirical death penalty research, said, "If I was allowed 1,000 executions and 1,000 exonerations, and I was allowed to do it in a random, focused way ... I could probably give you an answer." Mr. Wolfers' scenario is (thankfully) unlikely to come to pass, but the British Army experience during WWI may be an approximation: a large number of soldiers were executed or commuted for seemingly arbitrary reasons despite having committed essentially identical crimes. In this paper, using the quasi-random application of the death penalty during WWI, I test whether the death penalty deterred desertion. Although this paper answers a question different from that addressed in the usual death penalty research, it has the advantage of a relatively clear source variation that allows identification of any effects. Further, this study focuses on the more basic and timeless question of whether the threat of death by execution influences individual decision-making, albeit in a very particular setting.

## 1.2 Historical Context

British Commanders of the era were convinced of the deterrent power of the death penalty. An inspection of the time-series of death sentences and major British offensives suggests they acted on these beliefs. Over 3,000 soldiers received a death sentence, but British Expeditionary Force (BEF) commanders confirmed the sentences of only a fraction of condemned soldiers, with those not executed receiving commuted sentences. Historians believe there were two reasons for this restraint: (a) commanders were sensitive to political pressure and were concerned about popular anger back home, and (b) commanders were reluctant to execute soldiers who might still make some contribution to the war effort (Oram 2003). These two concerns, balanced against the desire to deter desertions, led to a fairly constant execution rate of around 12% (see Figure 2) — an almost literal decimation — with most soldiers being executed by a firing squad of their fellow soldiers,

[1] *Does Death Penalty Deter? A New Debate*, November 18th, 2007.

usually from that soldier's same unit. Soldiers whose lives were spared normally returned to the trenches and received prison terms or hard labor to be served after the war.[2] Figure 1 shows a plot of the distribution of death sentences and their resolution over the course of the war.

### 1.3 Basic Empirical Framework

To examine whether executions deterred desertions, I adopt the language of potential outcomes: I observe what happened in a particular Army division following an execution — I would like to know what *would* have happened if that same unit had instead experienced a commutation (Rubin 1974). Because I cannot observe the alternate history in which the soldier's life was spared, I must make an inference. If I believed that the execution and commutation decision was truly random at all times for all Army units, then the logic of the controlled experiment would allow me simply to compare some metric (such as a count of absences in some specified time period or the duration until some number of absences) in the execution cases with a similar metric in the commutation cases. While some historians do believe this strong randomization occurred, describing the process as a "pitiless lottery," others are doubtful.

If the commutation decisions were non-random, the non-randomness is likely due to the military commanders' consideration of several factors: the reputation of the condemned soldier's unit, the past sequence of executions and commutations within that unit, and the condemned soldier's individual characteristics. Military historians such as Julian Putowski and Anthony Babington (Putkowski 1989, Babington 1983) have argued that the command targeted certain units for execution for their perceived indiscipline but that individual characteristics were irrelevant, while Gerard Oram, a historian of WWI military justice on both the Allies and Central Powers sides, argues that both unit and individual soldier factors mattered. In particular, he argues that Irish soldiers, non-commissioned officers, and those seen as physically weak or otherwise undesirable were more likely to be executed.

My response to this possibility of non-randomness has three parts. First, I examine whether the information I have about individual condemned soldiers can predict the commutation decision. Second, I restrict my analysis to comparing how executions and commutations *within* a division influenced outcomes. Third, I try to detect non-randomness in the sequence of commutation decisions within a division by using a variety of statistical tests.

A second empirical challenge beyond non-randomness is that my within-unit design means that each division is essentially serving as its own control. This method is problematic if I think that past events in a unit's history can continue to affect outcomes in later time periods. In other words, the stable unit treatment value assumption (SUTVA) is potentially violated since the "treatment assignment" (i.e., execution or commutation) in one unit can affect the outcomes in another unit. I address this problem in two ways: first, I assume a strong form of SUTVA in which I posit that only the most recent event matters and, second, I parametrically model the effects of previous events and explore whether or not my results are robust to the inclusion of prior events in the model

[2] A soldier could not get a safe jail sentence that would have allowed him to leave the trenches.

specification.

## 1.4 Literature

In addition to a large empirical death penalty literature summarized critically by Donohue and Wolfers (2006), my paper follows a literature examining determinants of desertion (Costa and Kahn 2003) and one using commuted prison sentences (Kuziemko 2007) and abortions (which can be thought of as commuted birth sentences) (Levitt and Donohue 2001) to identify causal effects on subsequent outcomes.

The difference between the situation I study and contemporary criminal justice scenarios is so vast that drawing policy lessons from the WWI experience would be an ultimate example of trying to plant cut flowers. To make this point more strongly, I consider the ways in which the WWI experience differs from the classic question of whether the death penalty deters crimes like murder or rape.

- I am comparing units that applied the death penalty with different intensities. In my example, deterrence is not defined in terms of levels, but in changes in duration until the next "crime." In contrast, the contemporary debate is not really about how intensely the death penalty is applied, but whether it is applied at all and the outcome of interest is aggregate levels of crimes for which the death penalty can be applied, not individual instances of particular crimes.

- While my data includes some criminal cases (e.g., murder, rape, assault), it also includes crimes that have no civilian counterpart and are in some sense "victimless" (e.g., desertion, cowardice, and even sleeping while on duty). The purpose of the death penalty in WWI was almost solely to deter desertion; it is not clear if this kind of deterrence is really comparable to the kind of deterrence intended to prevent violent crimes with obvious victims.

- The WWI death penalty was designed for maximum deterrence: executions were immediate, brutal, and public, with particulars of the situation promulgated widely. In contrast, the modern application of the death penalty seems to be more about retribution — the trend is toward more "humane" forms of execution, exacted after lengthy appeals, conducted basically in private.

- Perhaps the most substantive distinction between application of the death penalty today and during WWI was that soldiers faced a large, independent probability of death from combat. Although I would expect that, on the margin, executions would still deter, it is not intuitively obvious that the independent probability of death does not swamp whatever effects the execution policy had on the margins.

Despite these differences, this study, beyond exploring an interesting historical question, offers some insights with potentially capable of greater generalization. The granularity and richness of the data begets questions that are sometimes ignored in the standard time-series crime rate studies. For example, the basic deterrence idea is that as the cost of some activity increases, you see less of that activity. If the sentence is applied non-randomly, however, then "deterrence" becomes a game where the targets of deterrence must weigh their likelihood of being executed, conditional upon their individual characteristics. As a result, a rational punisher must consider this reaction when setting his decision rules, and deterrence becomes intimately tied to beliefs about how rules are applied and how knowledge and beliefs evolve over time.

The remainder of the paper is organized as follows. Section 2 provides historical background for my data on desertions, courts martial, and executions. Section 3 presents a model of how soldiers may behave in response to executions. Section 4 describes my data. Section 5 conducts a number of tests for randomization of the commutation vs. execution decision. Section 6 presents a potential outcomes framework for analyzing the court martial data. Section 7 presents my results. The final section concludes.

## 2 Background

This section describes the processes in which absences are converted into trials for absence or desertion; in which trials for desertion are converted into conviction; in which convictions lead to different kinds of sentences, including death sentences; and in which death sentences lead to executions or commutations (see Figure 3 for a flowchart illustrating the criminal justice procedure). Except where otherwise cited, all of the information presented in this section comes from discussion with British military historian Julian Putkowski. The information presented here motivates the theoretical model as well as the empirical analysis and provides context for the datasets. We focus particularly on the randomness of the confirmation (execution) or commutation decision. We also focus on the salience of execution to soldiers in order to motivate the appropriate unit of analysis.

### 2.1 Desertions and Apprehensions

Deserters in France were typically arrested within two weeks. The prevalence of British and French military police in forward areas, in addition to French civilians' general unwillingness to risk helping a deserter, rendered a deserter's discovery a virtual certainty. Most British soldiers only had a rudimentary knowledge of French and civilians would rarely risk knowingly helping a deserter because it was an offence for which they could be jailed or severely punished. Deserters were viewed as being, if not dangerous, a nuisance because they were compelled to live off the country, scavenging and stealing food, money or clothing. Of those deserters who evaded detection for more than a month, most either enjoyed assistance from civilians or holed up in one of the larger Army bases. This latter strategy, however, was only successful at the beginning of the war when bases suffered from greater disorganization. That deserters would almost invariably be caught suggests that the costs of deserting and factors contributing to the probability of being caught (and ultimately executed) remained roughly the same across treatment and control groups in our analysis. [3]

### 2.2 Trials

Our analysis focuses on desertions that occurred overseas in France or Flanders. Of these desertions, most were dealt with by the Field General Courts Martial (FGCM), which were less formal and easier to convene than a full General Court Martial (GCM).[4] Indeed, the GCM was generally reserved for officers, while the vast majority of deserters were regular or volunteer infantrymen. In addition to the GCM and FGCM, there was also the District Court Martial (DCM), which handled some desertions and AWOLs (absentees without leave) for draft dodgers as well as those on furlough from the front or returning after convalescence in the UK. Unlike the FGCM and GCM, both of which could impose the death penalty, the DCM could impose a maximum sentence of two years of imprisonment.

### 2.3 Affirm or Commute the Death Sentence?

A soldier's guilty conviction did not seal his ultimate fate, as each of that soldier's commanding officers, brigade division, corps, and army commanders were responsible for submitting their own opinion as to whether the death sentence should be confirmed or commuted. Per an official memorandum issued by the British War Office, a soldier's commanding officers were to base their recommendations on three factors: 1) a soldier's character from a fighting point of view as well as with respect to general behavior, 2) the state of discipline within his unit, and 3) whether the crime had been intentional, a necessary ingredient to a desertion conviction.

Once that paperwork, complete with all the recommendations of the soldier's superiors, was submitted, the file was placed before the Commander-in-Chief for his ultimate decision. In reaching his determination, the Commander-in-Chief likely put greatest emphasis on the second factor, the unit's discipline, paying little regard to the deserter's personal circumstances.[5] That said, this claim does not have consensus among historians, though my analysis tends to support the "pitiless lottery" hypothesis (Babington 1983).

Indeed, records indicate that decisions could be arbitrary, with identical extenuating circumstances apparently accepted in some cases and rejected in others. Commanders-in-Chief, Generals Haig and French, could not possibly have had time to exercise individual scrutiny of each dossier, if only because during major offensives, there would not have been time to examine each case (Oram

[3] In contrast, 14% of Union army soldiers deserted during the American Civil War, but only 40% of deserters were caught and deserters faced a negligible risk of death if arrested (Costa and Kahn 2003).

[4] The Field General Court Martial was comprised of at least three officers, the president holding the rank of major or above. It could only pass a death sentence if all members agreed. Prosecution was handled by the accused soldier's adjutant and defense handled by a junior regimental officer. The usual defense was merely a plea of extenuating circumstances. Courts martial in the field invariably took place in private, even though they were theoretically open to the public. The Field General Court Martial was intended to replace the 19th century "drumhead" (summary) court martial.

[5] E.g., age, domestic responsibilities, prospects, civilian character, peacetime occupation, and whether he was a regular, territorial, volunteer, or conscript.

2003). For this reason, each dossier had a one-page typed summary, outlining the salient features of the offence(s) with comments about the soldier's character, fighting qualities, disciplinary record, unit performance, and the lower-level confirming officers' concurrences. In some cases (Putkowski and Sykes 1989), Corps and Army level concurrences would seal a man's fate, while lower-level officer recommendations (division and below), who did not have the incentive to report indiscipline because of career concerns, were basically ignored.[6]

While the historical evidence does suggest that unit-level factors affected decision-making and that some units were targeted, it seems unlikely that any particular case was carefully considered or that case outcomes were strongly dependent upon an individual soldier's characteristics. While I investigate this randomness assumption, the historical evidence is enough to suggest that at least within a particular military unit, which exact soldier received a death sentence — at least from the perspective of the other soldiers in that unit—was essentially random.

### 2.4 Commutations

After the trial, soldiers found guilty may have been detained (Babington 1983) or thrown back into the trenches (Oram 2003). Since the convicted soldier would continue to be held in custody, at least on a casual basis, it would have been informally known that he had been found guilty of a capital offence. The decision to confirm or commute occurred within two weeks of the original Field General Court Martial death sentence, though the exact date that a subsequent commutation was promulgated is unknown. For my analysis, I had to impute the commutation dates: I use the sentence-to-execution date as a benchmark and estimate my model parameters with both fixed durations (14 days) and nearest-neighbor methods. (For a more detailed discussion of promulgation, see Appendix A.)

### 2.5 Executions

After confirmation of a death sentence, there would be a special parade of the condemned man's unit on the evening before the soldier's execution, during which officers from the unit read extracts from the evidence at his trial, the findings and sentence of the court, and the order of confirmation by the Commander-in-Chief. Promulgation was to take place in front of as many men as could be made available (Babington 1983), though enforced audiences may have been rare (Putkowski and Sykes 1989). In some places, executions were carried out by a squad from the victim's battalion, witnessed by the entire battalion or whatever companies were at hand.[7] Executions typically occurred within a few days after a confirmation, so if confirmed, (normally two and a half weeks after the original

[6] For reference, the sequence of military units listed from lowest to highest is: Battalion → Regiment → Brigade → Division→ Corps → Army → Army Group. Each higher level of organization contains three or four subordinate units. Our unit of analysis is the Division.

[7] By mid-1916, public spectacles like this declined for a number of reasons and, in some Army areas (e.g., the Ypres Salient and the Somme), a prison or detention center was used for the execution of men from many units, and the firing squads were not always composed of men from their own battalions. While this presumably weakens the treatment effect, the condemned soldier's fellow soldiers would learn about the execution, even if they did not personally witness it.

death sentence), a firing squad would execute the guilty soldier. If the soldier did not die in the initial volley, an officer was on hand with a pistol to provide the coup de grâce.

The historical record suggests that public executions served their purpose in making soldiers aware of the consequences of desertion. Hearsay, rumor, and newspapers (Sellers 2003) spread the word, once the shocked members of a firing squad shared their feelings with comrades (Corn and Hughes-Wilson 2001). More formally, news about all executions was circulated via Part 2 of Army Orders, so that the name, unit, offence, nature, time and date of punishment was circulated throughout the theatre of operations. The details were read aloud on parade and were pinned up on notice boards (Sellers 2003).

Executions, and in some cases, commutations, were salient to the individual soldier. The number of references to executions in diaries, letters and memoirs is testament to the nature of their impact. For many soldiers, the experience of witnessing an execution and the fear generated by the rumors circulating in the trenches were a profound part of the wartime experience (Oram 2003). One soldier wrote about shooting his comrades, "It's the only thing I look back on in my military career with shame." One witness wrote, "I witnessed a shooting. . . . It shook me a bit" (Sellers 2003).[8] Eyewitness testimony suggests that even if they did not always impress soldiers in the way the army intended, executions were still salient. In some cases, eyewitnesses felt sorry for both the victim and the firing squad. Witnesses might remember the age of the condemned but in our preliminary examination of the historical record, we have not come across letters that indicate soldiers remembering any other characteristics, i.e., "covariates," about the executed.

## 3 Theory

I hypothesize that soldiers use execution-commutation observations to update prior distributions on the probability of being executed if they desert. As long as executions increase soldier's subjective probability of being executed for desertion, regardless of the probability of death in battle, I would expect execution-commutation decisions to change soldier behavior. The facts that witnesses were affected by executions and that military leaders endeavored to promulgate executions to as many people as possible suggest that soldiers likely updated their prior probabilities of whether they would be executed if they deserted. I hypothesize that soldiers learn and forget (i.e., the posterior slackens back to the uninformative prior over time). A rational soldier weighs the benefits of desertion (being reunited with family, avoiding at least some time in the trenches, etc.) and the costs of desertion (social shame and probability of death). While I examine the assumption that commutation decisions were random, I remain agnostic as to whether the soldiers perceived the decisions as random. Even if soldiers perceived the decisions as random, their behavior would still be affected. If, on the other hand, soldiers perceived a predictable, non-random element to the commutation decisions, such as whether they observed the egregiousness of the charge, they may

[8]Since I do not have data on who was on the firing squad nor who was a witness, I will be unable to distinguish between the specific effect of execution on members of the firing squad and eyewitnesses from the general effect of execution on members of the division.

have calibrated their own desertion decisions accordingly. For example, if soldiers perceived that commutations occur for Australians, then only non-Australian soldiers would be affected by executions. Soldiers would need to include themselves in the category of observable characteristics that they believed to predict executions in order for those soldiers to have been deterred. Nevertheless, to measure an unbiased effect of execution, I merely need the distribution of soldier perceptions of non-randomness and soldier response to this perception to be distributed equally across treatment and control groups.

Here I present a model where soldiers have uninformative priors and are using execution-commutation observations to update those priors. The model incorporates a dynamic Bayesian updating scheme in which soldiers learn and forget (i.e. the posterior slackens back to the uninformative prior over time); more precisely, I build a model where soldiers exaggerate how likely it is that a small sample resembles the population from which it is drawn (Rabin 2002). Soldiers learn and forget by overinferring from short, recent sequences. A rational model of individual soldier decision-making incorporating this effect and assigning weights to desertion's benefits—namely reunification with family—and to its costs—e.g., social shame and the possibility of execution—predicts that recent executions would deter soldiers from deserting.

Consider an infinite sequence of signals, execute ($e$) or commute ($c$), $\{s_t\}$. Let $Pr(\{s_t\}=e\}=\vartheta$. Let $\pi(\vartheta)$ be the prior probability of $\vartheta$. The key innovation that leads to soldiers overinferring is that instead of believing the signals are randomly generated from an i.i.d. process, the soldiers instead believe the signals are generated by random draws without replacement from an urn of N signals (Rabin 2002). This captures belief in the law of small numbers (Tversky and Kahneman 1981), since soldiers believe signals must balance out to the population rate before N signals are observed. As N goes to infinity, soldiers become Bayesian. The smaller is N, the more they believe in the law of small numbers. The urn contains $N\vartheta$ executions and $N(1-\vartheta)$ commutations. To capture local representativeness, the urn is renewed every 2 draws. So pairs of signals are i.i.d.

To illustrate with a concrete example, suppose $N = 4$ and $\vartheta = 1/2$. The urn must contain 2 executions and 2 commutations. After observing an execution, the second signal only has a 1/3 chance of being another execution. This is the gambler's fallacy, where people are inclined to believe in a kind of mean reversion. Soldiers underestimate the likelihood of repetition and so their surprise is greater, the rarer the signal. This surprise, and the subsequent posterior updating, captures the hot hand effect, where people overinfer from streaks of identical signals. Let $\{\pi_t{}^N(h_t)\}$ be the N-urn posterior after the $t^{th}$ signal. Suppose the prior $\pi(\vartheta) = 1$. We can observe that $\{\pi_t{}^N(s_t=e|s_{t-1}=c, h_{t-2})=(N\vartheta)/(N-1) > \vartheta\}$ and that $\{\pi_t{}^N(s_t=e|s_{t-1}=e, h_{t-2})=(N\vartheta-1)/(N-1) < \vartheta\}$ for even signals. For odd signals, the posterior is $\vartheta$. The likelihood ratio of getting two e signals in a row (one odd, one even) for $\vartheta$ and $\vartheta$' is: $((N\vartheta-1)\vartheta)/((N\vartheta'-1)\vartheta') > \vartheta 2/\vartheta' 2$, the Bayesian likelihood ratio, if and only if $\vartheta > \vartheta'$. The left-hand side captures the relative surprise when seeing two executions and that this surprise leads to overupdating relative to the Bayesian case. Note that the rarer the signal, $N\vartheta' - 1$ is small, so the greater the surprise the rarer the signal.

Switching back to the illustrative example, consider 3 possible beliefs about the execution rate, 1/4, 1/2, and 3/4. A Bayesian would have the following probabilities for 2 executions in a row: 1/4 * 1/4 = 1/16, 1/2 * 1/2 = 4/16, 3/4 * 3/4 = 9/16. However, for a believer in the law of small numbers, the following probabilities for 2 executions would be: 1/4 * 0/3 = 0, 2/4 * 1/3 = 2/12, 3/4 * 2/3 = 6/12. So soldiers subject to the gambler's fallacy and believing a lower probability of a second execution are then subject to hot hand fallacy and skewing towards believing a higher execution rate. Note that with over 2 death sentences per day and an unknown number of days before being caught, a soldier is assumed to respond to his posterior belief about the execution rate rather than the probability of the next execution within the N-urn.

This over-inference is exacerbated if executions have higher salience. Consider a situation in which soldiers know about a trial-execution with probability 1, but know about a trial-commutation with probability 1/2. Even if they start with a belief that the execution-commutation "bag" has two executions and two commutations, when they start observing more executions, they change their beliefs even more quickly towards a world of all executions, while a rational Bayesian probability updater would make no such radical change. Such a Bayesian would not be any more or less likely to desert after an execution because he realizes that the decisions are just random variables with the memoryless property.

A rational model of soldiers' decision-making incorporating over-inference and assigning weights to the costs and benefits of desertion predicts that recent executions would deter soldiers from deserting. This model must be balanced with a consideration for iatrogenic effects, namely, the negative response by minorities to state-imposed violence (Fagan and Meares 2008). An execution of an oppressed minority might reduce social shame from desertion.

# 4 Data

I employ four datasets: one on court martial death sentences, executions, and commutations; two on absenteeism; and one on casualties. I also use a list of Irish surnames, which I use to identify soldiers of probable Irish ethnicity.[9]

## 4.1 Court Martial Death Sentences & Commutation Data

My death sentence data includes all 3,342 sentences, complete with name, unit, rank, date, offense, final sentence, reference number in national archives, age (if soldier was executed), theater of war, and other information, from August 1914 to September 1923 (Oram 2005). The date refers to date of death sentence, which occasionally differs from date of trial or conviction but invariably is different from date of execution, which is listed separately. The categories of offenses with the highest number of sentences are: desertion (2,005), sleeping at post (449), cowardice (213), disobedience (120), and murder (118).[10] Final sentences in the dataset are those punishments (if any) ultimately confirmed

[9] http://www.last_names.net/origincat.asp?origincat=Irish

[10] The other offenses are: Irish rebellion, quitting post, striking senior officer, mutiny, offense against inhabitant, espionage, treason, hostile act, violence, insubordination, absence, sedition, aiding the enemy, casting away arms, possessing firearms, armed robbery, plundering, drunkenness, threatening senior officer, offense against martial law,

by the Commander-in-Chief. If the soldier's original death sentence was not confirmed, then the soldier was either given a reduced sentence (hard labor, penal servitude, imprisonment, tied to fixed object, or reduced in rank) or the sentence was sometimes "quashed," i.e., vacated. Tables 1 and 2 and Figures 6 and 7 display these general statistics.

### 4.2 Absence Data

In my first analysis, the absentee data come from monthly war diaries of the Assistant Provost Marshal (APM) that have been preserved for the four-year period from 1914 to 1918 (National Archive File: a) WO 154 Series — WO 154/112: Monthly War Diary APM, September 1915 - May 1917; b) WO 154/114: Monthly War Diary APM, August 1914 - November 1916; c) WO 154/8: Monthly War Diary APM 9th Army Corps, December 1916 - May 1918). Lists and descriptions of absentees were printed and circulated with ID number, rank, name, unit, date of absence, physical description (usually including age and height, and sometimes also hair color, build, lips, mouth, complexion, eyes, teeth, mustache, cleanshaven, and accent).

According to conversations with British military historian Julian Putkowski, the absentee list was generated in the following manner. The APM was responsible for the military police and the oversight of general military discipline and order. They maintained war dairies and sent reports to the Provost-Marshall at General Headquarters in France. Amongst his duties for the area of his particular jurisdiction, the APM noted the number of absentees from regiments broadly on a weekly basis. Military units took roll call and attendance every morning (or more frequently). Those not present had to be categorized: killed in action, wounded, missing (prisoner-of-war or wounded), sick or straggler (lost or awaiting return from a "stragglers post" or "battle stop," where they had been gathered up by either regimental or Military Police). After a month, the names of those who were still absent and not accounted for were forwarded to the Provost Marshall at headquarters where the information was collated with other APM reports. The Provost Marshall would aggregate the material and circulate a printed updated list of the names of men absent for a month by unit for the armies at the front. The APM could then match names/descriptions to any soldier arrested. On occasion, three-month lists seemed to have appeared. These lists revised known absentees making earlier lists redundant.

One advantage of comparing post-execution outcomes to post-commutation outcomes within a particular unit is that I minimize potential bias that might result from error in measuring outcomes — to the extent my desertion and absentee lists include those who were killed, were prisoner of war by accident or by design, or were stragglers, this measurement error would affect both treatment and control groups equally.

In my second analysis, I have obtained a list of absences and desertions from a more complete source, the Deserters and Absentees (D&A) supplement to the (weekly) Police Gazette. The details

conspiracy, robbery, theft, attempted assassination, attempted murder, attempted desertion, housebreaking, losing army property, rape, pillaging, aiding enemy whilst POW, and unspecified/other, for a total of over 30 types of offenses.

of everyone who deserted or went absent were recorded in alphabetical order and published: name, rank, serial number; distinguishing characteristics; unit/formation; civilian occupation; home address and place from whence an individual absented himself. Information from soldiers' attestation papers completed at joining the Army were merged, which is why the Police Gazette data contains more information, such as date and place of enlistment, parish and county of birth, trade, and place of desertion (if at Home), than the military war diaries. The D&A supplement records all absentees and distinguishes between Home (where it was much easier to desert) and Abroad; the absentees at Home (soldiers on furlough) provide a potential control experiment to test for latent variables, such as danger, driving both execution and desertion decisions.

### 4.3 Casualties Data

To proxy for danger, I have a database containing roughly 672,000 casualties recorded by regiment, battalion, surname, Christian name, initial, born (town), born (county), enlisted (town), enlisted (county), regimental number, rank, killed in action, died of wounds, died, theatre of war of death, date of death and supplementary notes. Thus I can match this data to desertion dates by military unit in order to control for high frequency changes in perceived danger. This casualty data is used to control for differences in the danger level within units.

## 5 Conditions for Causal Inference

Without certain baseline assumptions necessary for causal inference satisfied, no econometrics technique, however sophisticated, will allow me to estimate the relative deterrence effects of execution and commutation. In particular, I need to know whether the assignment of subjects (in my case, military units) to treatment and control groups is *ignorable* and whether the treatment assignment of one unit affects the potential outcomes of some other unit.

### 5.1 Ignorable Treatment Assignment

If commutations were truly random, then the ignorable treatment assignment condition is met trivially. However, randomness is stronger than what is needed, especially given my within-unit analysis. By comparing outcomes only within units, targeting units with bad discipline is still consistent with ignorability, so long as the particular soldier selected for execution within that unit is random. Even this conditional randomness is not strictly necessary, since a commander could have executed certain soldiers for substantive reasons, but so long as these reasons were not salient to the decision-making of the individual soldier, then this non-random treatment assignment is irrelevant for the outcome I am trying to measure.

It is of course impossible to say definitively what was salient to the individual soldier, never mind to characterize fully his decision-making process, but I can take two steps that justify my approach and inference.[11] First, I can see if the soldier selected for execution within a unit depended upon observable characteristics, such as the soldier's age, national origin, and rank. Second, I can see if the sequence of executions and commutations exhibit statistically improbable regularities. While I admit that I will never be able to prove ignorability of treatment assignment, my findings that a) observable characteristics did not affect commutations or executions, b) the sequence of decisions is consistent with a random process, and c) the dominant thinking among historians that the decision was in fact a "pitiless lottery" makes a causal interpretation justifiable, if not fully justified.

### 5.1.1 Are Decisions Correlated With Observable Characteristics?

In the context of the BEF death sentences, some historians have argued that the decision to execute or commute was not nearly as random as previously thought. They have suggested that the execution-commutation decision was affected by one or more of the following factors: number of casualties, location, timing of offenses, physique and physical hardiness of the condemned soldier, and the soldier's ethnic background. These other factors are in addition to the possibility that a commander might want to signal to his superiors that he was a tough disciplinarian. This challenge to the naive randomization hypothesis suggests I check whether observable characteristics are in fact correlated with the confirmation decision. Because I am comparing execution and commutation decisions within a military unit, I focus on examining the influence of individual-level characteristics on the execution decision.

Table 3 shows the results of several regressions of characteristics on observable characteristics. I do not find any relationship between Irish ethnicity and probability of execution. Figure 8 illustrates that Irish are not disproportionately executed, conditional on the death sentence, and that the Irish are not disproportionately sentenced to death relative to the proportion of Irish absences according to the data preserved in the war diaries on the field. There is also no relationship between rank and probability of execution. Although the rank coefficients are significant, they are all similar in magnitude (within one standard deviation). The reason why rank is significant in these regressions is that the full capital sentence data includes some non-military personnel (such as POW's, spies, camp followers and laborers) who were much more likely to be executed when convicted of a capital crime. I find no relationship between the day, month, or year and the probability of execution. I do find that deserters and murderers are more likely to be executed. While both results are statistically significant, it is important to note that murder increases the probability, ceteris paribus, of execution by 58%; the increase for desertion is a little less than 7%, suggesting that executions for desertions were more common than for other cases. Even so, the difference was small and likely to be imperceptible to the average soldier or even low-level unit commander.

[11] Even a gold-standard random process — the roll of a die — has a deterministic element. If known with precision, the force and torque applied to the die, the subtle air currents, the hardness of the surface, etc., might allow me (or a physicist) to determine with certainty the outcome of these "random" rolls. Despite this obvious non-randomness, I would still have faith in the outcome of a trial with treatment assignments based on die rolls because I am certain that the factors affecting the assignment have no impact on the outcome of interest and hence are ignorable.

### 5.1.2 Is the Sequence of Decisions Within a Unit Non-Random?

The general approach to assessing randomness is analogous to a Fisher exact test, except that I use simulations instead of an analytical approach. The methodology I follow is:

1. Propose a statistic that can be computed from the sequence of 1's and 0's (i.e., executions and commutations) within a unit $i$
2. Compute the statistic for the actual sequence, $s^*$
3. Compute the statistic for each of 1,000 bootstrap samples from the actual sequence, i.e., $\hat{s}_1, \hat{s}_2, \hat{s}_3 \ldots \hat{s}_n$
4. Compute the empirical p-value, $p_i$ by determining where $s^*$ fits into $\hat{s}_1, \hat{s}_2, \hat{s}_3 \ldots \hat{s}_n$
5. Repeat the steps 1-4 and calculate $p_i$ for each unit

The statistics I use are:

**Autocorrelation** I see if the decision made in the $j$th cases depends on the outcome in the $j-1$th case. This statistic can detect whether executions are "clustered," meaning a higher than expected number of back-to-back executions. This test tells me whether commanders executed soldiers in pairs, for example, in the cases of two friends deserting together.

**Mean-Reversion** I test whether there is any form of mean reversion in the sequence, meaning that the execution in the $n$th case is correlated with the execution *rate* in previous $n-1$ cases. This test tells me whether commanders were attempting to equilibrate their decisions, considering whether a unit was "due" for an execution.

**Longest-Run** I test whether there are abnormally long "runs" without any executions. This test tells me whether certain units may have been favored with commutations during certain time periods, for example, if a unit's commanding officer always decided to commute a death sentence and the Commander-in-Chief made the decision to commute if at least one commanding officer decided to commute.

While this process generates a collection of p-values, it is not intuitively obvious what should be the rejection criteria. Since p-values from a truly random process with a sufficient number of possible states is uniformly distributed, even with just 10 units and 3 statistics, the probability of not having even one p-value less than .025 or greater than .975 is only about 21%. With a truly random process, I would expect that collection of all unit p-values to be uniformly distributed. (Imagine that you generate summary statistics for 1000 random strings. The 1001th random string should have a summary statistic that is equally likely to be anywhere from 1 to 1000.) I use Kolmogorov-Smirnov Test to test whether the empirical distribution of p-values approaches the CDF of a uniform distribution using the one-sided critical value with n = 46. Figure 4 plots the empirical distribution for my three test statistics and the corresponding table in that figure confirms the visual intuition that the p-values are uniformly distributed for all tests.

### 5.2 Stable Unit Treatment Value Assumption

Even if treatment assignment is ignorable, valid causal inference is not necessarily possible: I have to be certain that the outcome in one unit is not affected by the treatment assignment in another unit, i.e., that the stable unit treatment value assumption (SUTVA) is satisfied. As noted earlier, my within-unit design helps with ignorability but creates a SUTVA problem because each unit is essentially serving as its own control.

SUTVA is often embedded in panel data and event study models but sometimes does not receive careful attention. To illustrate the problem, consider that each Army unit had a sequence of commutations and executions — if on the $n$th execution, a soldier's decision-making is still being affected by what occurred in the previous $n-1$ cases, then SUTVA is clearly violated. A rapid sequence of commutations and executions before the next absence would appear as an intervening cause and consequently bias the estimated deterrent effect to zero. Furthermore, even if the effects of executions and commutations quickly died out, making within unit SUTVA plausible, it is possible that executions and commutations in neighboring units affect outcomes, which also violates SUTVA if results are aggregated. I address this unit "bleed over" by using the division, which was the largest organic organization with sharply defined, relatively unchanging boundaries.

For the more serious problem of past events affecting future events, one possibility is to select for inclusion only those events between which there is some sufficient amount of elapsed time. Unfortunately, requiring a greater amount of space between events helps SUTVA but hurts the ignorability of treatment since treatment assignment is most likely to be ignorable when comparing capital cases that appeared before the commander at roughly similar times. The approach I use is to make a strong assumption, which is that past events are irrelevant. I then weaken this assumption by assuming a parametric model for deterrence and condition out the past effects of previous events. With this approach, the effect of past treatment assignments on future outcomes is modeled explicitly rather than assumed to be zero.

## 6 Empirical Strategy

The basic empirical strategy is to exploit the ignorability of executions and commutations *within* units to identify the deterrence effect of an execution compared with a commutation as measured by the duration of elapsed time until the next absence. The first approach I take to address the SUTVA issue is to assume that only the most recent deterrence event (i.e., execution or commutation) within a unit matters. Under this assumption, which I call strong-SUTVA, units are in one of two states: they either are in a last-event-was-commutation state or a last-event-was-execution state. My second approach, or weak-SUTVA, is to assume that past events matter, but that the effect of past events decreases over time. In particular, I assume that past events fade away according to an exponential decay process.

With strong-SUTVA, there is the problem that following an execution or a commutation, there might be another execution or commutation before the unit experiences an absence. To deal with this possibility, I assume that the appearance of another deterrence event right-censors the observed time until next absence. My 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: rather they were determined simultaneously and with identical outcome.

### 6.1 Duration Analysis

My first modeling approach is to assume that only the most recent event matters and 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 will 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 Equation 1. In this equation, military units are indexed by $i$, while observations are indexed by $j$.

$$f(y) = \lambda \exp(-\lambda y) \tag{1}$$

The hazard rate in Equation 1, $\lambda$, depends upon the characteristics of that particular deterrence event, as in Equation 2.

$$\lambda = \beta_0 + \beta_{ex} ex_{ij} + \beta_{exd} ex_{ij} \cdot des_{ij} + \beta_{des} \cdot des_{ij} + \gamma^C cas_{it} + \gamma_j^U + \gamma_{year(j)=T}^T \tag{2}$$

In Equation 2, $ex$ is an indicator for an execution, $des$ is an indicator that the trial was for desertion, $cas$ is the casualty rate and $\gamma^{\mathbf{U}}$ and $\gamma^{\mathbf{T}}$ are unit and year fixed-effects, respectively. Collectively, I refer to these parameters as a vector $\theta$. It is possible, however, that the next event following an execution or commutation is another execution or commutation, 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 but for the intervening execution or commutation, 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 Equation 3.

$$L(\theta) = \sum_{j=1}^{N} d_j \log\left(f(y_j|\lambda(\theta)) + (1 - d_j)\left(1 - F(y_j|\lambda(\theta)\right)\right) \tag{3}$$

**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 $\Delta t$ takes values of 7, 14, 30, 60 and 90, corresponding to deterrence-effect half-lives of one week, two weeks, one month, two months, and three months. In the weak-SUTVA approach, I define two sets:

$$E_{ex}(t^*) \equiv \text{times of all executions in the unit prior to } t^*$$

$$E_{cm}(t^*) \equiv \text{times of all commutions in the unit prior to } t^*$$

And hence two cumulative effects of past events, one for executions and one for commutations:

$$D_{ex}(k) = \sum_{t \in E_{ex}(t^*)} e^{-k(t^*-t)}$$

$$D_{cm}(k) = \sum_{t \in E_{cm}(t^*)} e^{-k(t^*-t)}$$

The hazard rate is now the strong-SUTVA hazard rate plus the two terms for past executions and commutations.

$$\lambda'(k) = \lambda + \alpha_{ex} D_{ex} + \alpha_{cm} D_{cm}$$

### 6.2 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. To give a concrete example, suppose that up to time $T$, Unit A's sequence of executions and commutations is $(1, 1, 1, 0)$ while Unit B's is $(0, 0, 0, 1)$. For argument's sake, assume all events in both units fell on the same days. In the period of time $T$ through $T + \Delta T$, if I find fewer absences from Unit A compared to B, should I conclude that executions do not deter desertions, simply because the last event in B was an execution while A has a commutation?

To put the issue in the framework of the Rubin causal model, the problem is that each death sentence is serving as a unit, and the treatment assignment of some units (i.e., execution or commutation) can affect the potential outcomes in other units (i.e., other death sentences that occur later in the same unit). In other words, not accounting for the effects of previous death sentences leads to a clear violation of SUTVA.

My approach to this problem is to use a structural 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 outcomes, 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 0-day, June 28th, 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.

$t_k$ is the day on which the $k$th element of $K$ occurred.

**Execution** or Commutation: $x_k$ is an indicator for whether an element in $K$ was an execution of commutation

**Crime** Type: $d_k$ is an indicator for whether an element in $K$ was a desertion or some other crime

Using the logit as my link function, I assume that the probability of an absence in unit $i$ on day $t$ is given by:

$$p_i(t) = \frac{1}{1 + e^{-z(i,t;\theta)}} \tag{4}$$

where $z(i,t;\theta)$ is

$$z(i,t;\theta) = \left( \sum_{k=1}^{|K_i(t)|} e^{-\lambda(t-t_k)} D(k) \right) + X(t)\gamma \tag{5}$$

where

$$D(k) = \beta \cdot \mathbf{E}(\mathbf{k}) = \begin{pmatrix} \beta_{exd} & \beta_{exo} & \beta_{cd} & \beta_{co} \end{pmatrix} \cdot \begin{pmatrix} x_k d_k \\ x_k(1-d_k) \\ (1-x_k)d_k \\ (1-x_k)(1-d_k) \end{pmatrix}$$

and

$$X(t)\gamma = \gamma^0 + \gamma^C cas_{it} + \gamma_i^U + \gamma_{year(t)}^T \tag{6}$$

$\beta_{exd} \equiv$ Effect of executing a deserter

$\beta_{exo} \equiv$ Effect of executing someone for some other crime

$\beta_{cd} \equiv$ Effect of commuting the death sentence of a deserter

$\beta_{co} \equiv$ Effect of commuting the death sentence of someone convicted of some other crime

I define a vector parameters:

$$\theta = \left(\lambda, \beta_{exd}, \beta_{exo}, \beta_{cd}, \beta_{co}, ; \gamma^0, \gamma^C, \gamma^{\mathbf{U}}, \gamma^{\mathbf{T}}\right)$$

where $\beta_{cm}$ is a measure of the deterrent effect of a commutation, while $\beta_{ex}$ is a measure of the deterrent effect of an execution. $X(t)$ is a collection of covariates, such as the instantaneous, unit-specific danger rate (computed from casualties) and a unit fixed-effect. Note that the effects of past deterrence events fade as time progresses and that there is one $\lambda$ for both executions and commutations — i.e., events are "forgotten" at the same rate, though different kinds of events can have different levels of influence based on the values for $\beta$. $F$ is the link-function whose range is

$[0, 1]$.[12] From this measure, I can compute the log-likelihood:

$$L = \sum_{i=1}^{I} \sum_{t=1}^{T} a_i(t) \log p_i(t) + [1 - a_i(t)] \log(1 - p_i(t)) \tag{7}$$

and hence estimate $\beta$ and $\gamma$ using Newton-Raphson or another suitable algorithm.

# 7 Results

## 7.1 Duration Framework

Table 4 shows the results of the duration framework estimation using different duration distributions and commutation imputation methods: columns 1, 4, and 7 use the exponential distribution, while 2, 5, and 8 use Weilbull and 3, 6, and 9 use the Cox model; columns 1-3 use the +14 days imputation method, 4-6 use nearest neighbor, and 7-8 use the trial date as the commutation date.

I cannot detect a deterrence effect, nor can I rule out such an effect. Table 4 indicates that when looking at the entire sample of death sentences, executions do not lead to an increase in time to subsequent absence, no matter the definition of commutation date. The three variations correspond to three different definitions of commutation dates: commutation announcements occurring 14 days after the trial; commutation announcements occurring as many days after the trial as the time it took for the nearest trial that led to execution to result in execution; and both commutation and execution dates set to their trial dates. Assuming that commutation dates occur on the upper end of the time range, 14 days after the trial date, would tend to magnify the estimated deterrent effect since time between commutation and subsequent absence is minimized. Assuming that only the original trial date is relevant is akin to using an instrumental variables strategy where the execution-commutation decision is my instrument. I do not find an effect no matter what duration model that is used, exponential in Columns 1, 4, and 9.[13]

I find limited evidence, however, that executing deserters deters absence while executing non-deserters and Irish soldiers, regardless of the crime, spurs absence. Table 5 examines how execution of different types of soldiers may have lead to different deterrence effects. The most striking finding is that the coefficient on the interaction term of execution and Irish indicates that executing Irish soldiers leads to faster absences. Figure 5 corroborates this visually in a univariate analysis. In the top half of the figure, execution of Irish leads to shorter duration times until next absence, whereas in the bottom half of the figure, execution and commutations lead to virtually the same time until next absence. This coefficient remains positive and statistically significant across all definitions of commutation dates and whether controls for officer rank are included. Here, I only run exponential

[12] We can allow event-specific values of $\lambda$

[13] Perhaps the easiest way to interpret the coefficients is to consider how a change in a particular covariate affects the mean time until next absence. In the exponential distribution, the mean duration is $\frac{1}{\lambda}$, and since the survival model treats $\lambda$ as a linear function of the independent variables, the marginal effect of a coefficient $\hat{\beta}$ is $-\frac{\beta}{\hat{\lambda}^2}$, where $\hat{\lambda}$ is the average duration. Note that a negative coefficient implies a positive effect on time until next absence i.e., a negative coefficient suggests deterrence.

models. I am in the process of linking absentee data to the Irish names database to uncover who it was that deserted in response to Irish executions. Divisions were not segregated by ethnicity. Of over 2,100 absences, 340 were Irish. My results may be the first causal evidence of minorities reacting badly to state violence.

As noted earlier, I use the division as my level of analysis since there is some evidence that divisions were targeted for execution due to their perceived indiscipline. If I make a stronger assumption that there was no unit-targeting, I can leave out division fixed effects. When I do this, I am no longer comparing executions and commutations within a unit — I am comparing commutations and executions across the entire Army (note that the term "Army" in the British WWI context does not mean the entire universe of military units, as there were several Armies, including the Regular, Territorial and New). When I expand the pool of comparable treatment and control observations, I find a strong deterrence effect of executing deserters (Columns 10-12 of Table 5). However, this result is strongly caveated by the fact that most historical evidence suggests that divisions were targeted and thus only within division comparisons are credible. Analysis at aggregation levels smaller than the division level is not feasible due to the infrequency of absences and capital cases occurring within the same smaller unit. At each level of analysis, I only include units with at least one absence, one commutation, and one execution.

Table 6 shows the results of several regressions under different assumptions about the half-life of the deterrence (or spurring) effects of previous events, each using the "+14" imputation method and an exponential distribution. The purpose of these aggregations of past events is to explore how my results change when I relax the strong-SUTVA assumption that events prior to the most recent death sentence are irrelevant. The earlier main finding—that executing Irish spurs absences—is robust to various controls in Columns 2-6. Moreover, the finding that executing deserters deters absence with army-level fixed effects also remains robust to controls in Columns 8-12. Another interesting finding is that the execution coefficients increase in absolute value as the half-life is extended, suggesting that the effect of execution has a longer half-life than the effect of commutation, which displays monotonically decreasing coefficients as the half-life of the effect is extended. This finding does not hold, however, for the army fixed effect results. Since there is great variation in death sentences across units, the SUTVA coefficients may just be picking up on the number of death sentences in a given unit.

### 7.2 Day-by-Day Framework

Tables 7-9 show the results of the day-by-day approach using different half-lives and clustering of standard errors. All columns use the +14 commutation imputation method. Columns 1-3 assume the effects of executions and commutations fade with a half-life of 1 week; columns 4-6 assume the effects fade with a half-life of 1 month; and columns 7-9 assume the effects fade with a half-life of 3 months. Columns 1, 4, and 7 do not cluster standard errors. Columns 2, 5, and 8 cluster standard errors at the division level. Columns 3, 6, and 9 cluster standard errors at the army level. Table 7 uses all absences as outcome variable, Table 8 uses Irish absences as outcome, and Table 9 uses non-Irish absences as outcome. Unlike the tables for the duration framework, these tables also restrict the sample from day 700 to day 1105 (the assassination of Archduke Ferdinand on June 28th, 1914 is our 0-day). I also use time and time squared (days from the assassination) instead of year fixed effects.

When aggregating Irish and non-Irish absences together, I cannot detect a deterrent or spurring effect as I found in the duration framework. Only in Column 3 of Table 7 do I see that the execution of deserters deters absences. However, when I examine only Irish absences, in Columns 3-7 of Table 8, I find that executing Irish soldiers spurs Irish absences. This effect is not found for non-Irish absences (Table 9). In contrast, for non-Irish absences, in Columns 1-3 and 5-6 of Table 9, I find that executing deserters deters non-Irish absences. This effect is not found for Irish absences in Table 8. What is also interesting to observe is that the executions of deserters appear to have the strongest deterrence effect for specifications assuming a half-life of 1 weeks, whereas the execution of Irish soldiers appear to have the strongest anti-deterrent effect for specifications assuming a half-life of 1 month. I cluster all standard errors at the division level, since my exploratory data analysis found that some covariates are correlated within a division. For example, divisions vary in their proportion of Irish soldiers and thus a clustering correction is needed.

## 8 Conclusion

The British experience provides an extremely low-bar test for the death penalty. Finding a deterrence effect in the context of WWI would certainly not be a strong argument, leaving aside moral issues, that the death penalty is good policy. However, a negative result showing no deterrent effect might have more policy salience since if we ever expected to find an effect, it would be in the WWI context: executions took place almost immediately in a manner purposefully designed to maximize their deterrent effect and death sentences were given out very frequently and quite arbitrarily for victimless, rational "crimes." On the other hand, desertion is certainly not analogous to murder, and criminals weighing the pros and cons of some potential homicidal undertaking are certainly different from terrified, shell-shocked soldiers facing high probabilities of death no matter what course they chose. However, we would still expect that on the margin more executions should deter absences and if we find this not to be the case, it would suggest that the threat of future death applied semi-randomly is not as strong a disincentive as we might imagine.

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# B Historical Background

## B.1 Commander's Beliefs about Execution and Time-Series Inspection

Most British military officers from the WWI-era viewed the death penalty as essential to military discipline. As far as is known from historical records, senior officers were, without exception, death penalty advocates, viewing it as their only recourse for maintaining discipline as corporal punishment had been outlawed as inhumane in the previous half-century. Sir Neville Macready, a former Adjutant-General, stated "if you abolish the death penalty you might as well abolish the army," and Brigadier General Douglas-Smith said "[the] death penalty is the only means by which desertion can be stopped" (Putkowski 1989). Indeed, that Australian forces were by law not subject to the death penalty but also displayed the highest rate of absences is consistent with this view. Military commanders not only believed the death penalty deterred desertion, but also appeared to use the death penalty in a manner they hoped would forestall desertions. As shown in Figure 1, a time series of courts martial and casualties indicate that death sentences peaked shortly before the start of British offensives, though without further analysis this feature of the data could simply reflect the greater volume of desertions (and hence potential for executions) preceding a military action. Prior to Central Power offensives (i.e. British defensives), there was apparently no increase in executions (Oram 2003), but if German offensives were not foreseeable to individual soldiers and their officers, then this finding is consistent with either active or reactive approaches to absences by commanders.

### B.2 The Commutation Decision Process

The final decision to confirm or commute a death sentence was made by the Commander-in-Chief of all British forces, yet the sheer volume of death sentences (almost 2 per day on average) would have precluded the Commander-in-Chief from reading in detail and pondering over each and every case. After a soldier was sentenced to death, paperwork would be passed along to collect the views of each of his commanding officers as to whether the sentence should be confirmed or commuted. Officers were instructed to consider the soldier's character from a fighting point of view as well as general behavior, state of discipline within their unit, and whether the crime had been intentional. Paper work presented to the Commander-in-Chief consisted of a typed one-page summary, outlining the salient features of the offence(s); comment about the man's character, fighting qualities, disciplinary record, and unit performance (i.e., performance of battalion, brigade, and division); and the confirming officers' concurrences. Executions typically occurred within a few days after a confirmation and the morning after the decision reached the soldier, within two weeks of the original death sentence (discussions with Putkowski, 2008).

Some generals never executed anyone[14] while others[15] were instrumental in the deaths of many condemned men. There were also more bureaucratic figures: Brigadier James Wroughton, the head of BEF Personal Services Branch (part of the Army Group's command), and Gilbert Mellor, the Judge Advocate General, were primarily responsible for drawing up the short list that was picked over later by Haig.

The one-page summary indicated the performance of battalion, brigade, and division, so I focus my analyses at these levels of disaggregation (though where able, I check for randomization within other levels of hierarchy as well, Platoon → Company → Battalion → Regiment → Brigade → Division→ Corps → Army → Army Group, because the confirmation process explicitly requested reviewing officers to consider the state of discipline of their units). Brigadier General Sir Anthony Farrar Hockley reckoned that the decision of the divisional commander was pivotal in the confirmation process.[16]

[14]e.g. Ivor Maxse

[15]e.g. Allenby, Haldane and French and Haig

[16]Most of the fighting was conducted by battalions, brigades, and divisions. Battalions did not fight solo in the trenches; they were always attached to a brigade, and there were three brigades to a division. When a division took over part of a front, it was usually one of a pair, both of whom were supported by an administrative body, called a Corps (divisions came and went but the Corps remained in charge of the same sector of the front). Corps were essentially administrative rather than fighting organizations, but they always had additional heavy artillery,

### B.3 Timing of the Commutations

It is possible that the court martial registers of the JAG featured the dates of the announcement of a commutation, but so far, the exact date is unknown. There were times when delays occurred, due to operational imperatives, but the exact frequency is unknown. Commutation decisions would have occurred in batches as death sentences from different units accumulated and awaited review, not on a rolling basis, though the exact batch size or delay is unknown. Commutation goals, if there were any, do not appear to have been preserved in the historical record (no written or explicit statements); however, since there does not appear to be sufficient evidence of coordination of commutation fractions across all theaters of operation or across time, it is unlikely there were explicit commutation goals.

### B.4 Death in the trenches

The intensity of World War I trench warfare meant about 10% of the fighting soldiers were killed. In comparison, only 5% were killed during the Second Boer War and 4.5% killed during World War II. For British and Dominion troops serving on the Western Front, the 12.5% of troops were killed, while an additional 43.5% became wounded. Considering that for every front-line infantryman there were about three soldiers in support (artillery, supply, medical, and so on), almost all fighting soldiers sustained some form of injury. Indeed many received more than one injury during the course of their service. Medical services were primitive and there were no antibiotics. As in many other wars, disease was World War I's greatest killer. Poor sanitary conditions in the trenches led to dysentery, typhus, and cholera. Poor hygiene also led to fungal conditions in the mouth and foot.

### B.5 Irish Backlash

In 1918, the British Government decided to extend conscription to Ireland as additional reservoir of manpower for the front through a new Military Service Bill that linked conscription to home rule. This alienated both Nationalist and Unionists in Ireland. During the vote, Irish parliamentary members walked out in protest and returned to Ireland to organize opposition.

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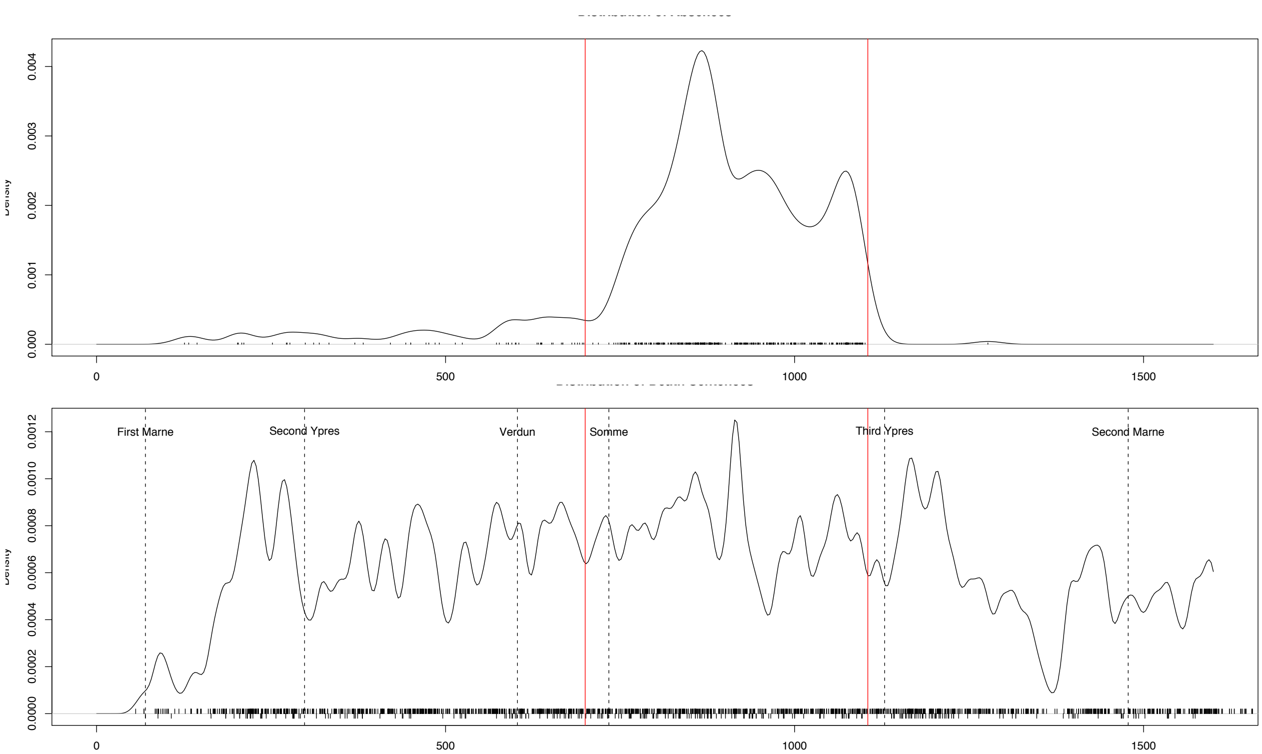
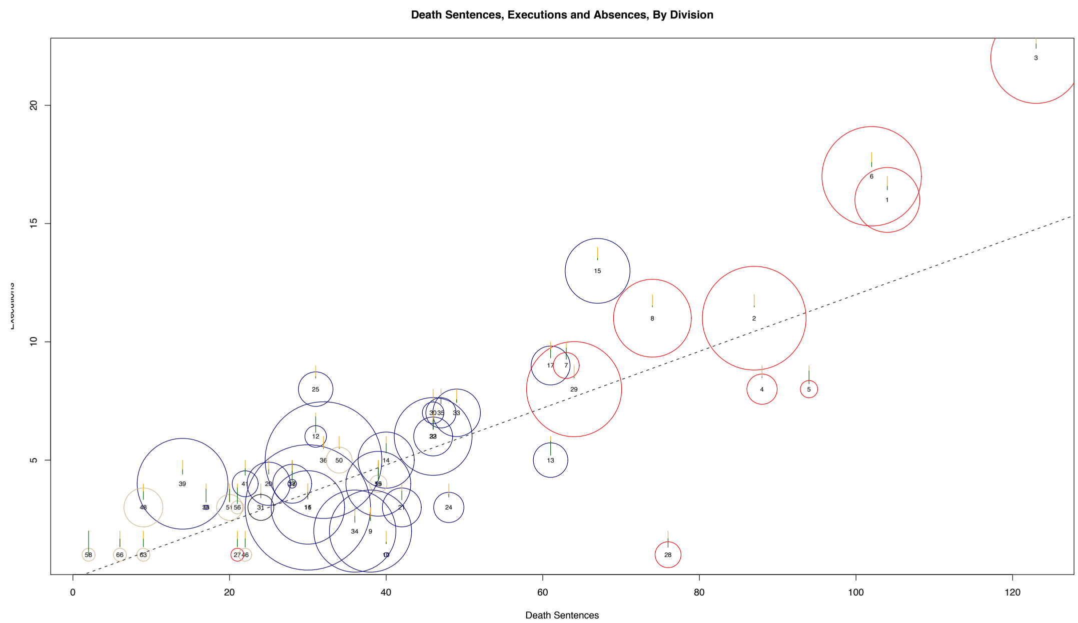
specialists, and supply units. A division commander would typically allocate three brigades along a front, one brigade to each of three sectors. The brigades would rotate their battalions in and out of the line, typically ten days in the firing line, five days out, and a battalion in the line would rotate companies between two lines of reserve or support trenches and the forward (firing) line. This is why I focus on using battalions, brigades, and divisions as my main units of analysis.

Figure 1: Death Sentences and Outcomes for BEF Units



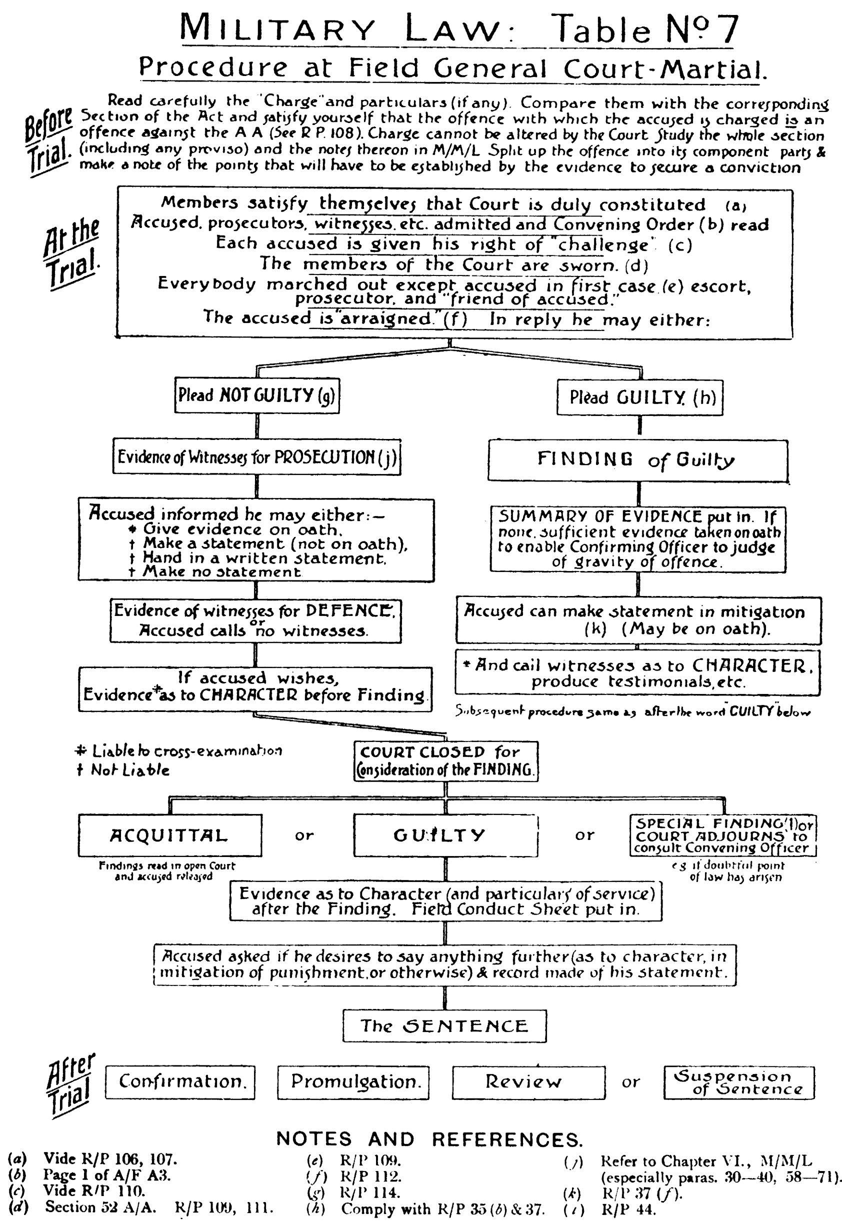

Note: This figure shows the distribution of death sentences during the course of the war. The dotted vertical lines indicate the start of major British offensives. The sequence of tick marks along the bottom axis represent each death sentence, with upward-pointing ticks indicating a commutation and downward-pointing ticks indicating an execution.

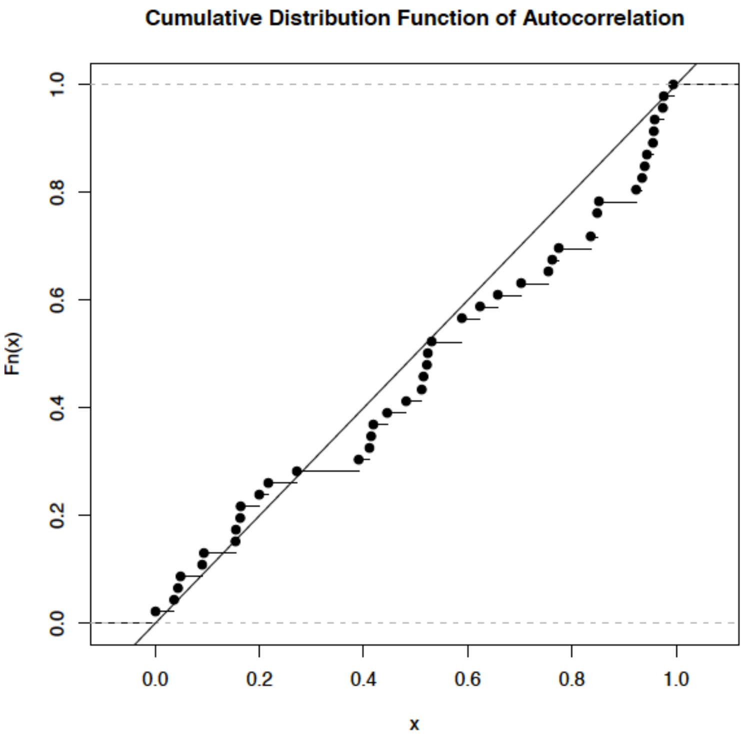
## Figure 2: British Army Divisions

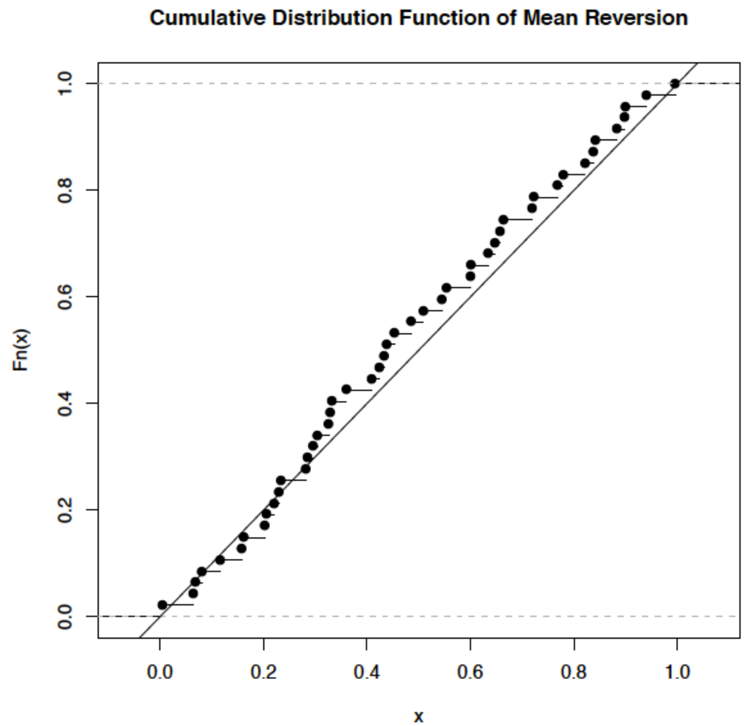


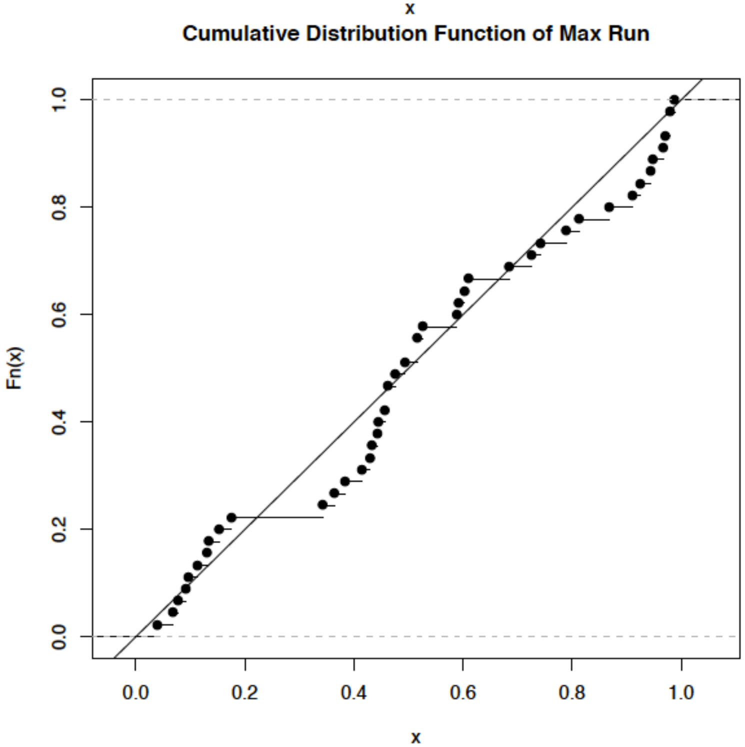

Note: This figure summarizes death sentences, executions and absences by British Army division. The x-axis the number of death sentences passed in a division, while the y-axis is the count of executions. Each division is labeled with its actual divisional number. The diameter of the circle around each division is proportional to the number of absences recorded for that unit, though the exact size of the circle is not directly interpretable in terms of the axes. Regular army divisions are indicated with red circles, new army divisions (Kirchner's Army) are indicated with navy circles and territorial divisions by tan circles. The upward sloping dashed line indicates an execution rate of 12%. For each division, there is a tick above the division name indicating the estimated fraction of absences and death sentences of Irish soldiers in that division. The tick full tick represents 50% of the division, with the green portion indicating the proportion of that 1/2 that was Irish e.g., a solid green tick would indicate that 50% of the death sentences and absences were passed on / committed by Irish soldiers.

Figure 3: British Courts Martial Procedure





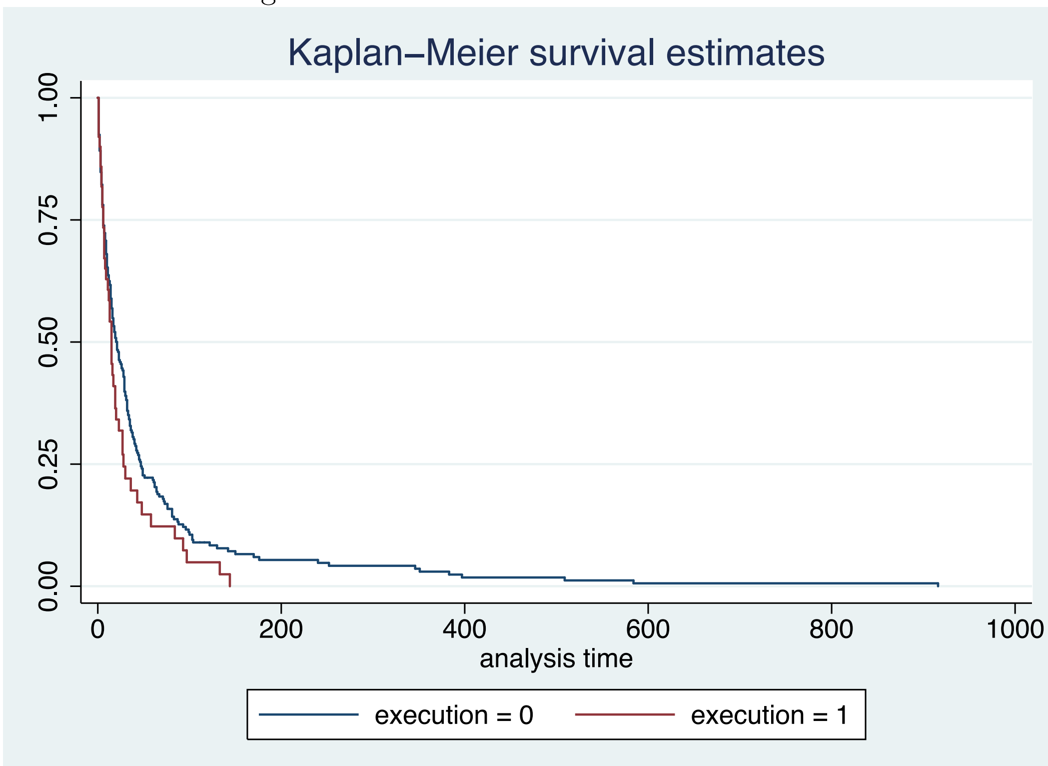




Kolmogorov-Smirnov Test - Randomness Check of P-Values

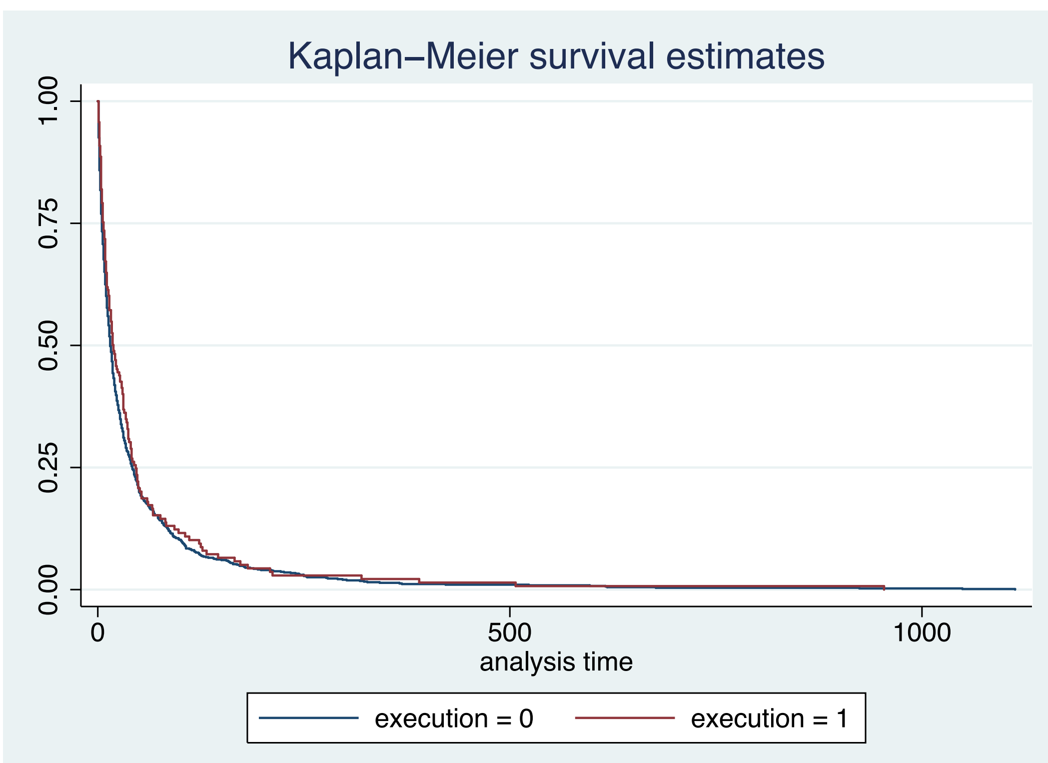
| | Distance | Size | 90% | 95% | 99% |
|---|---|---|---|---|---|
| Autocorrelation | 0.1403913 | 46 | 0.1545 | 0.1767 | 0.2195 |
| Mean Reversion | 0.08068085 | 47 | 0.153 | 0.1748 | 0.2171 |
| Longest Run | 0.12611111 | 45 | 0.1562 | 0.1786 | 0.2219 |

Figure 4

Figure 5: Non-Parametric Survival Distributions




(a) Anti-Deterrence Effect, Irish Executions Only




(b) No Deterrence Effect, Non-Irish Executions Only

Figure 6

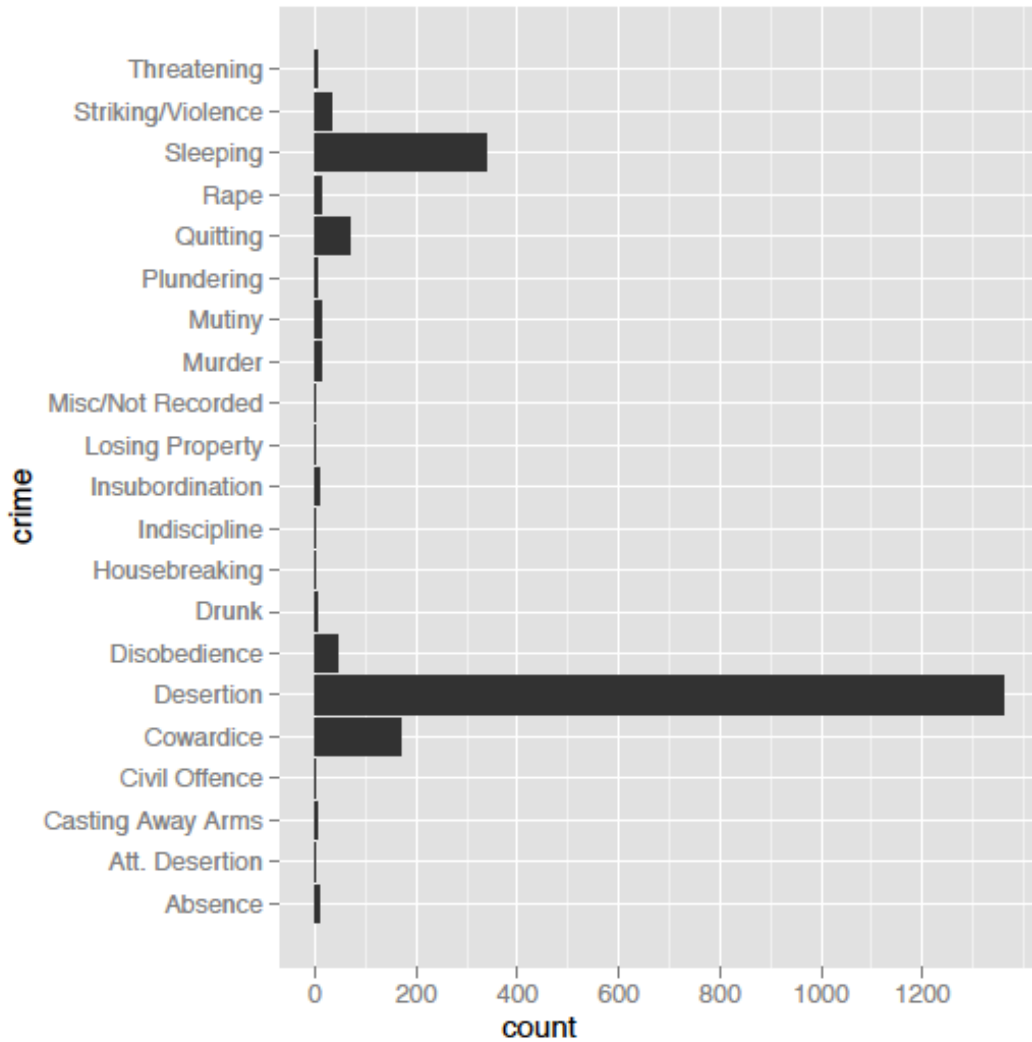

Threatening
Striking/Violence
Sleeping
Rape
Quitting
Plundering
Mutiny
Murder
Misc/Not Recorded
Losing Property
Insubordination
Indiscipline
Housebreaking
Drunk
Disobedience
Desertion
Cowardice
Civil Offence
Casting Away Arms
Att. Desertion
Absence
crime
0
200
400
600
800
1000
1200
count

Figure 7

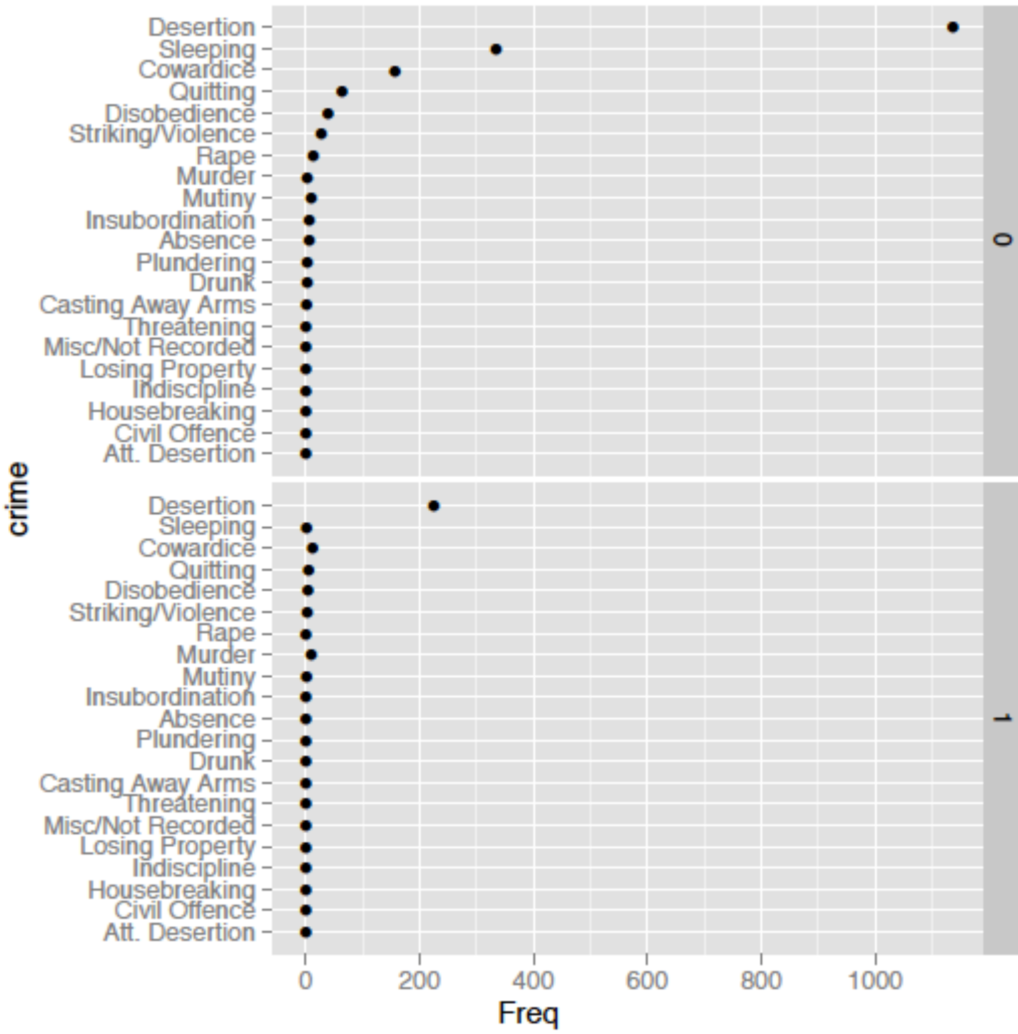

Desertion
Sleeping
Cowardice
Quitting
Disobedience
Striking/Violence
Rape
Murder
Mutiny
Insubordination
Absence
Plundering
Drunk
Casting Away Arms
Threatening
Misc/Not Recorded
Losing Property
Indiscipline
Housebreaking
Civil Offence
Att. Desertion
0
1
crime
0
200
400
600
800
1000
Freq

Figure 8

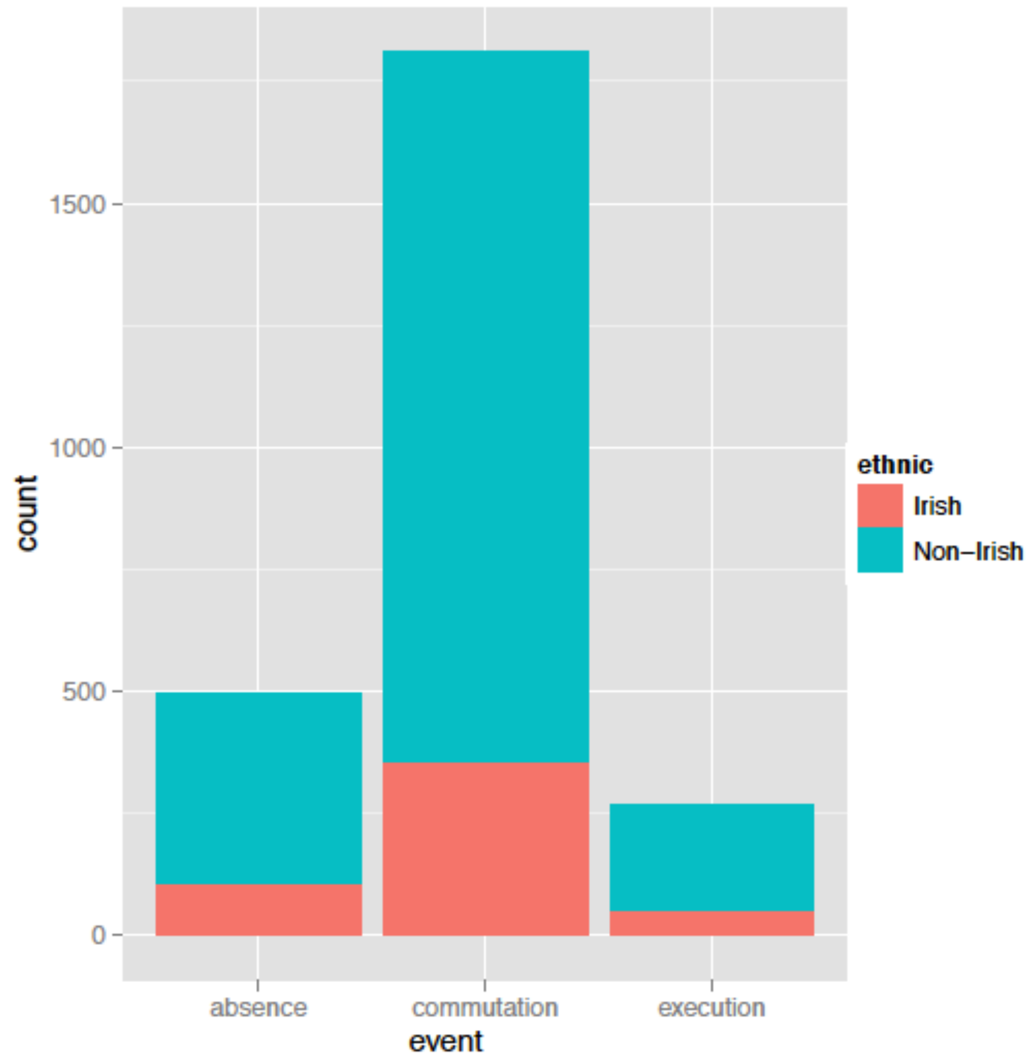

1500
1000
500
0
count
absence
commutation
execution
event
ethnic
Irish
Non–Irish

Table 1: Crimes and Death Sentence Outcomes

| | Commuted | Executed |
|---|---|---|
| Absence | 6 | 0 |
| Att. Desertion | 1 | 0 |
| Casting Away Arms | 2 | 1 |
| Civil Offence | 1 | 0 |
| Cowardice | 157 | 12 |
| Desertion | 1136 | 225 |
| Disobedience | 39 | 4 |
| Drunk | 3 | 0 |
| Housebreaking | 1 | 0 |
| Indiscipline | 1 | 0 |
| Insubordination | 6 | 0 |
| Losing Property | 1 | 0 |
| Misc/Not Recorded | 1 | 0 |
| Murder | 3 | 10 |
| Mutiny | 10 | 2 |
| Plundering | 3 | 0 |
| Quitting | 64 | 5 |
| Rape | 13 | 0 |
| Sleeping | 335 | 2 |
| Striking/Violence | 27 | 3 |
| Threatening | 1 | 1 |

Table 2: Ranks And Death Sentence Outcomes

| | Commuted | Executed |
|---|---|---|
| Corporal | 50 | 14 |
| Lieutenant | 0 | 3 |
| Private | 1735 | 243 |
| Sergeant | 26 | 5 |

Table 3: Are Observable Characteristics Correlated with Executions?

| | (1) execution | (2) execution | (3) execution | (4) execution | (5) execution | (6) execution | (7) execution | (8) execution |
|---|---|---|---|---|---|---|---|---|
| irish | 0.00790 | 0.00824 | 0.00643 | 0.00923 | 0.00470 | -0.000725 | | |
| | (0.0157) | (0.0157) | (0.0157) | (0.0158) | (0.0157) | (0.0153) | | |
| year | | 0.00373 | | | | | | |
| | | (0.00523) | | | | | | |
| month | | -0.00185 | -0.00209 | -0.00188 | -0.00126 | -0.000719 | | |
| | | (0.00182) | (0.00187) | (0.00195) | (0.00194) | (0.00190) | | |
| day | | -0.000394 | -0.000430 | -0.000715 | -0.000824 | -0.000947 | | |
| | | (0.000692) | (0.000694) | (0.000706) | (0.000699) | (0.000685) | | |
| Pte | | | | | -0.299** | -0.230** | -0.322** | |
| | | | | | (0.0423) | (0.0425) | (0.0368) | |
| Sgt | | | | | -0.228** | -0.169* | -0.246** | |
| | | | | | (0.0670) | (0.0665) | (0.0636) | |
| Rfm | | | | | -0.311** | -0.244** | -0.305** | |
| | | | | | (0.0517) | (0.0516) | (0.0439) | |
| Cpl | | | | | -0.225** | -0.163** | -0.247** | |
| | | | | | (0.0552) | (0.0549) | (0.0508) | |
| Desert | | | | | | 0.0883* | | 0.0650+ |
| | | | | | | (0.0419) | | (0.0392) |
| Coward | | | | | | -0.0349 | | -0.0140 |
| | | | | | | (0.0462) | | (0.0443) |
| Disobedience | | | | | | 0.00159 | | -0.0282 |
| | | | | | | (0.0557) | | (0.0531) |
| Murder | | | | | | 0.534** | | 0.584** |
| | | | | | | (0.0743) | | (0.0620) |
| Mutiny | | | | | | 0.111 | | -0.00225 |
| | | | | | | (0.0689) | | (0.0589) |
| Quit | | | | | | -0.0306 | | -0.00247 |
| | | | | | | (0.0519) | | (0.0496) |
| Sleep | | | | | | -0.0827+ | | -0.0773+ |
| | | | | | | (0.0435) | | (0.0409) |
| Striking | | | | | | 0.00765 | | 0.0289 |
| | | | | | | (0.0667) | | (0.0651) |
| AgainstInhab | | | | | | 0.0708 | | 0.0844 |
| | | | | | | (0.0876) | | (0.0837) |
| Year Fixed-Effects | N | N | N | Y | Y | Y | Y | N |
| Division Fixed-Effects | N | N | Y | Y | Y | Y | N | N |
| $N$ | 2814 | 2814 | 2814 | 2814 | 2814 | 2814 | 2814 | 2814 |
| $R^2$ | 0.000 | 0.001 | 0.005 | 0.074 | 0.092 | 0.137 | 0.028 | 0.069 |

Standard errors in parentheses
+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

Notes: All regressions are performed using ordinary least squares; standard errors are conventional (i.e., non-robust).

Table 4: Effects of Executions vs. Commutations on Elapsed Time Until Next Absence

| | (1)<br>Exp/+14 | (2)<br>Wb/+14 | (3)<br>Cox/+14 | (4)<br>Exp/NN | (5)<br>Wb/NN | (6)<br>Cox/NN | (7)<br>Exp/C=T | (8)<br>Wb/C=T | (9)<br>Cox/C=T |
|---|---|---|---|---|---|---|---|---|---|
| main | | | | | | | | | |
| execution | -0.00174 | -0.00625 | -0.00883 | 0.127 | 0.110 | 0.0993 | 0.149+ | 0.127 | 0.119 |
| | (0.0804) | (0.0795) | (0.0792) | (0.0823) | (0.0815) | (0.0811) | (0.0809) | (0.0800) | (0.0796) |
| year1915 | 0.0124 | 0.0190 | 0.0265 | 0.260 | 0.242 | 0.242 | 0.0216 | 0.0265 | 0.0290 |
| | (0.155) | (0.154) | (0.155) | (0.169) | (0.168) | (0.168) | (0.155) | (0.154) | (0.155) |
| year1916 | -0.160 | -0.178 | -0.185 | -0.0213 | -0.0450 | -0.0500 | -0.157 | -0.180 | -0.191 |
| | (0.157) | (0.157) | (0.157) | (0.170) | (0.170) | (0.171) | (0.157) | (0.157) | (0.157) |
| year1917 | -0.171 | -0.162 | -0.155 | -0.0863 | -0.0800 | -0.0655 | -0.210 | -0.197 | -0.189 |
| | (0.156) | (0.157) | (0.158) | (0.170) | (0.171) | (0.171) | (0.156) | (0.157) | (0.157) |
| Division FE | Y | Y | Y | Y | Y | Y | Y | Y | Y |
| $N$ | 1722 | 1722 | 1722 | 1540 | 1540 | 1540 | 1725 | 1725 | 1725 |

Standard errors in parentheses
+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

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 sub-titled "+14", the announcement of the commutation is assumed to occur 14 days 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 fixed-effects, which are not shown. A positive coefficient $\implies$ lower "hazard" of having and absence i.e., more time until the next absence, which can be interpreted as deterrence.

Table 5: Effects of Execution vs. Commutation on Elapsed Time Until Next Absence, Differing by whether Case was a Desertion Trial

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | +14 | +14 | +14 | NN | NN | NN | T=C | T=C | T=C | +14 | +14 | +14 |
| execution | 0.0790 | 0.0195 | 0.202 | 0.138 | 0.0918 | 0.382 | -0.0867 | -0.154 | -0.261 | 0.652** | 0.586* | 0.905+ |
| | (0.224) | (0.224) | (0.411) | (0.222) | (0.221) | (0.424) | (0.201) | (0.200) | (0.383) | (0.229) | (0.233) | (0.509) |
| desert | 0.182* | 0.182* | 0.173* | 0.167* | 0.173* | 0.163* | 0.128+ | 0.132+ | 0.123+ | 0.227** | 0.230** | 0.230** |
| | (0.0719) | (0.0719) | (0.0724) | (0.0737) | (0.0737) | (0.0741) | (0.0711) | (0.0712) | (0.0717) | (0.0693) | (0.0694) | (0.0698) |
| ex·desert | -0.141 | -0.159 | -0.102 | -0.0456 | -0.0775 | -0.101 | 0.219 | 0.160 | 0.153 | -0.619* | -0.622* | -0.515+ |
| | (0.241) | (0.239) | (0.263) | (0.240) | (0.240) | (0.253) | (0.222) | (0.220) | (0.224) | (0.246) | (0.246) | (0.265) |
| irish | | -0.0511 | -0.0312 | | -0.169* | -0.145+ | | -0.0789 | -0.0633 | | -0.122 | -0.110 |
| | | (0.0776) | (0.0786) | | (0.0804) | (0.0814) | | (0.0754) | (0.0761) | | (0.0811) | (0.0824) |
| ex·irish | | 0.395* | 0.392* | | 0.411* | 0.380+ | | 0.684** | 0.659** | | 0.364+ | 0.348+ |
| | | (0.195) | (0.196) | | (0.208) | (0.210) | | (0.200) | (0.202) | | (0.204) | (0.206) |
| Pte | | | 0.315 | | | 0.364 | | | 0.254 | | | 0.255 |
| | | | (0.226) | | | (0.242) | | | (0.218) | | | (0.339) |
| ex·Pte | | | -0.264 | | | -0.297 | | | 0.0763 | | | -0.428 |
| | | | (0.404) | | | (0.419) | | | (0.373) | | | (0.472) |
| Sgt | | | 0.302 | | | 0.384 | | | 0.186 | | | 0.201 |
| | | | (0.357) | | | (0.375) | | | (0.344) | | | (0.437) |
| ex·Sgt | | | -0.753 | | | -0.191 | | | 0.624 | | | -1.168 |
| | | | (0.698) | | | (0.686) | | | (0.622) | | | (0.790) |
| Rfm | | | 0.224 | | | 0.173 | | | 0.121 | | | 0.256 |
| | | | (0.258) | | | (0.273) | | | (0.250) | | | (0.355) |
| ex·Rfm | | | 0.144 | | | 0.0650 | | | 0.372 | | | -0.328 |
| | | | (0.480) | | | (0.495) | | | (0.456) | | | (0.531) |
| Cpl | | | 0.332 | | | 0.366 | | | 0.237 | | | 0.352 |
| | | | (0.276) | | | (0.293) | | | (0.274) | | | (0.379) |
| ex·Cpl | | | 0.197 | | | -0.375 | | | 0.972+ | | | 0.0536 |
| | | | (0.581) | | | (0.584) | | | (0.556) | | | (0.633) |
| Year FE | Y | Y | Y | Y | Y | Y | Y | Y | Y | Y | Y | Y |
| Division FE | Y | Y | Y | Y | Y | Y | Y | Y | Y | N | N | N |
| Army FE | 2N | N | N | N | N | N | N | N | N | Y | Y | Y |
| $N$ | 1722 | 1722 | 1722 | 1540 | 1540 | 1540 | 1725 | 1725 | 1725 | 1432 | 1432 | 1432 |

Standard errors in parentheses
$+\ p < 0.10$, $*\ p < 0.05$, $**\ p < 0.01$

Notes: All specifications use exponential models to parameterize baseline hazard rates. First three columns are for commutation dates defined as 14 days after trial; second three columns are for commutation dates defined as X days after trial where X is the time between execution and trial for nearest trial that resulted in execution; last three columns are for commutation and execution dates defined as their trial dates. Pte = Private, Sgt = Sergeant, Cpl = Corporal, Rfm = Rifleman. The effect of executing a soldier of a particular identity is read off of the coefficient on ex·(identity).

Table 6: Effects of Execution vs. Commutation on Elapsed Time Until Next Absence — Full Sample, Weak SUTVA

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| ex | 0.0195 | -0.0670 | -0.0555 | -0.00834 | 0.0213 | 0.0275 | 0.586* | 0.435+ | 0.505* | 0.566* | 0.564* | 0.560* |
| | (0.224) | (0.225) | (0.224) | (0.224) | (0.224) | (0.224) | (0.233) | (0.237) | (0.234) | (0.233) | (0.233) | (0.233) |
| desert | 0.182* | 0.171* | 0.158* | 0.173* | 0.194** | 0.199** | 0.230** | 0.248** | 0.228** | 0.233** | 0.241** | 0.242** |
| | (0.0719) | (0.0724) | (0.0724) | (0.0723) | (0.0722) | (0.0721) | (0.0694) | (0.0696) | (0.0696) | (0.0698) | (0.0701) | (0.0702) |
| ex · desert | -0.159 | -0.107 | -0.131 | -0.183 | -0.211 | -0.214 | -0.622* | -0.594* | -0.697** | -0.746** | -0.706** | -0.676** |
| | (0.239) | (0.241) | (0.241) | (0.240) | (0.239) | (0.239) | (0.246) | (0.249) | (0.247) | (0.246) | (0.247) | (0.247) |
| irish | -0.0511 | -0.0997 | -0.102 | -0.0857 | -0.0648 | -0.0542 | -0.122 | -0.113 | -0.113 | -0.106 | -0.0981 | -0.0958 |
| | (0.0776) | (0.0778) | (0.0777) | (0.0778) | (0.0778) | (0.0779) | (0.0811) | (0.0811) | (0.0812) | (0.0813) | (0.0814) | (0.0814) |
| ex · irish | 0.395* | 0.424* | 0.418* | 0.393* | 0.381+ | 0.385* | 0.364+ | 0.386+ | 0.382+ | 0.366+ | 0.352+ | 0.347+ |
| | (0.195) | (0.196) | (0.196) | (0.195) | (0.195) | (0.195) | (0.204) | (0.205) | (0.204) | (0.204) | (0.204) | (0.204) |
| ex's - 7d | | -0.0117 | | | | | | 0.115+ | | | | |
| | | (0.0698) | | | | | | (0.0676) | | | | |
| cm's - 7d | | 0.160** | | | | | | 0.145** | | | | |
| | | (0.0123) | | | | | | (0.0118) | | | | |
| ex's-14d | | | -0.0418 | | | | | | 0.100+ | | | |
| | | | (0.0537) | | | | | | (0.0513) | | | |
| cm's-14d | | | 0.118** | | | | | | 0.106** | | | |
| | | | (0.00952) | | | | | | (0.00977) | | | |
| ex's-30d | | | | -0.0817* | | | | | | 0.0569 | | |
| | | | | (0.0411) | | | | | | (0.0379) | | |
| cm's-30d | | | | 0.0767** | | | | | | 0.0642** | | |
| | | | | (0.00729) | | | | | | (0.00756) | | |
| ex's-60d | | | | | -0.0991** | | | | | | 0.0367 | |
| | | | | | (0.0340) | | | | | | (0.0289) | |
| cm's-60d | | | | | 0.0484** | | | | | | 0.0369** | |
| | | | | | (0.00597) | | | | | | (0.00584) | |
| ex's-90d | | | | | | -0.105** | | | | | | 0.0306 |
| | | | | | | (0.0312) | | | | | | (0.0248) |
| cm's-90d | | | | | | 0.0358** | | | | | | 0.0257** |
| | | | | | | (0.00556) | | | | | | (0.00513) |
| Division FE | Y | Y | Y | Y | Y | Y | N | N | N | N | N | N |
| Army FE | N | N | N | N | N | N | Y | Y | Y | Y | Y | Y |
| $N$ | 1722 | 1722 | 1722 | 1722 | 1722 | 1722 | 1432 | 1432 | 1432 | 1432 | 1432 | 1432 |

Standard errors in parentheses
+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

Notes: All specifications use the "+14" commutation date imputation method and all specifications use the exponential model to parameterize the hazard. The regressors labeled ex's-Yd or cm's-Yd are measure the cumulative effects of previous deterrence events in the unit. For executions, this is $\sum_{t_i \in E_{ex}(t^*)} e^{k(t^*-t_i)}$ where $t^*$ is the date of the commutation or execution observation, and $E_{ex}(t^*)$ is the set of the dates all previous executions in that unit occurring before $t^*$. For commutations, the corresponding effect is $\sum_{t_i \in E_{cm}(t^*)} e^{k(t^*-t_i)}$. The value $k$ is a parameter for how quickly the effects of past events are presumed to fade out: in our notion, the Y is the half-life of the effect, i.e. $\log \frac{1}{2} = kY$.

Table 7: Day-by-Day Framework, All Absences

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
|---|---|---|---|---|---|---|---|---|---|
| | est1 | est2 | est3 | est4 | est5 | est6 | est7 | est8 | est9 |
| execution | -0.00164 | -0.00164 | -0.00164 | -0.0731 | -0.0731 | -0.0731 | -0.0770 | -0.0770 | -0.0770 |
| | (0.257) | (0.167) | (0.0414) | (0.149) | (0.156) | (0.0869) | (0.109) | (0.123) | (0.0651) |
| ex·irish | 0.250 | 0.250 | 0.250 | 0.164 | 0.164 | 0.164 | -0.0225 | -0.0225 | -0.0225 |
| | (0.353) | (0.407) | (0.199) | (0.190) | (0.265) | (0.240) | (0.141) | (0.216) | (0.270) |
| irish | 0.0259 | 0.0259 | 0.0259 | 0.0221 | 0.0221 | 0.0221 | 0.0341 | 0.0341 | 0.0341 |
| | (0.127) | (0.174) | (0.140) | (0.0723) | (0.0892) | (0.0776) | (0.0520) | (0.0689) | (0.0827) |
| ex·desert | -0.377 | -0.377 | -0.377** | -0.110 | -0.110 | -0.110 | 0.00887 | 0.00887 | 0.00887 |
| | (0.275) | (0.240) | (0.0596) | (0.148) | (0.152) | (0.118) | (0.109) | (0.116) | (0.0713) |
| desert | 0.0380 | 0.0380 | 0.0380 | 0.0122 | 0.0122 | 0.0122 | 0.0121 | 0.0121 | 0.0121 |
| | (0.0583) | (0.0507) | (0.0721) | (0.0332) | (0.0236) | (0.0384) | (0.0232) | (0.0185) | (0.0299) |
| half-life | 1wk. | 1wk. | 1wk. | 1 mo. | 1 mo. | 1 mo. | 3 mo. | 3 mo. | 3 mo. |
| clustering | None | Div. | Army | None | Div. | Army | None | Div. | Army |
| $N$ | 18630 | 18630 | 18630 | 18630 | 18630 | 18630 | 18630 | 18630 | 18630 |

Standard errors in parentheses
+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

Notes: All specifications use the "+14" commutation date imputation method and include casualty, division, time and time-squared controls. The half-life row indicates the presumed exponential half-life of the effect of past events.

Table 8: Day-by-Day Framework, Irish Absences

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
|---|---|---|---|---|---|---|---|---|---|
| | est1 | est2 | est3 | est4 | est5 | est6 | est7 | est8 | est9 |
| execution | -0.772 | -0.772* | -0.772** | -0.367 | -0.367+ | -0.367 | -0.267 | -0.267 | -0.267 |
| | (0.742) | (0.353) | (0.161) | (0.263) | (0.194) | (0.281) | (0.186) | (0.216) | (0.331) |
| ex·irish | 0.781 | 0.781 | 0.781** | 0.663* | 0.663+ | 0.663** | 0.418+ | 0.418 | 0.418 |
| | (0.555) | (0.596) | (0.0781) | (0.306) | (0.352) | (0.182) | (0.253) | (0.311) | (0.276) |
| irish | 0.0694 | 0.0694 | 0.0694 | 0.106 | 0.106 | 0.106 | 0.134 | 0.134 | 0.134 |
| | (0.245) | (0.323) | (0.393) | (0.128) | (0.166) | (0.238) | (0.0902) | (0.139) | (0.205) |
| ex·desert | 0.746 | 0.746* | 0.746** | 0.360 | 0.360 | 0.360 | 0.223 | 0.223 | 0.223 |
| | (0.737) | (0.357) | (0.136) | (0.258) | (0.309) | (0.383) | (0.186) | (0.278) | (0.394) |
| desert | -0.209 | -0.209 | -0.209+ | -0.114 | -0.114 | -0.114 | -0.0573 | -0.0573 | -0.0573 |
| | (0.131) | (0.181) | (0.127) | (0.0701) | (0.0805) | (0.0886) | (0.0477) | (0.0495) | (0.0652) |
| half-life | 1wk. | 1wk. | 1wk. | 1 mo. | 1 mo. | 1 mo. | 3 mo. | 3 mo. | 3 mo. |
| clustering | None | Div. | Army | None | Div. | Army | None | Div. | Army |
| $N$ | 12960 | 12960 | 12960 | 12960 | 12960 | 12960 | 12960 | 12960 | 12960 |

Standard errors in parentheses
+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

Notes: All specifications use the "+14" commutation date imputation method and include casualty, division, time and time-squared controls. The half-life row indicates the presumed exponential half-life of the effect of past events.

Table 9: Day-by-Day Framework, Non-Irish Absences

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
|---|---|---|---|---|---|---|---|---|---|
| | est1 | est2 | est3 | est4 | est5 | est6 | est7 | est8 | est9 |
| execution | 0.242 | 0.242 | 0.242** | 0.0698 | 0.0698 | 0.0698+ | -0.00821 | -0.00821 | -0.00821 |
| | (0.267) | (0.239) | (0.0839) | (0.162) | (0.180) | (0.0392) | (0.121) | (0.135) | (0.0553) |
| ex·irish | -0.116 | -0.116 | -0.116 | -0.0647 | -0.0647 | -0.0647 | -0.157 | -0.157 | -0.157 |
| | (0.406) | (0.609) | (0.494) | (0.215) | (0.361) | (0.313) | (0.155) | (0.267) | (0.276) |
| irish | 0.0381 | 0.0381 | 0.0381 | 0.00313 | 0.00313 | 0.00313 | 0.00124 | 0.00124 | 0.00124 |
| | (0.135) | (0.166) | (0.0945) | (0.0785) | (0.0953) | (0.0352) | (0.0570) | (0.0717) | (0.0412) |
| ex·desert | -0.602* | -0.602* | -0.602** | -0.262 | -0.262+ | -0.262** | -0.0534 | -0.0534 | -0.0534 |
| | (0.291) | (0.287) | (0.0874) | (0.163) | (0.146) | (0.0226) | (0.121) | (0.108) | (0.0537) |
| desert | 0.0862 | 0.0862+ | 0.0862 | 0.0399 | 0.0399+ | 0.0399 | 0.0272 | 0.0272 | 0.0272 |
| | (0.0603) | (0.0492) | (0.0675) | (0.0350) | (0.0234) | (0.0301) | (0.0247) | (0.0179) | (0.0202) |
| half-life | 1wk. | 1wk. | 1wk. | 1 mo. | 1 mo. | 1 mo. | 3 mo. | 3 mo. | 3 mo. |
| clustering | None | Div. | Army | None | Div. | Army | None | Div. | Army |
| $N$ | 18225 | 18225 | 18225 | 18225 | 18225 | 18225 | 18225 | 18225 | 18225 |

Standard errors in parentheses

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

Notes: All specifications use the "+14" commutation date imputation method and include casualty, division, time and time-squared controls. The half-life row indicates the presumed exponential half-life of the effect of past events.