The Deterrent Effect of the Death Penalty? Evidence from British Commutations During World War I

Daniel L. Chen*

October 2012

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 World War I, I statistically 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 executions, 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 and preserved in war diaries. I find limited evidence that executing deserters deterred absences, while executing non-deserters and Irish soldiers, regardless of the crime, spurred absences in general, and Irish absences in particular. These findings suggest that legitimacy may play an important role in why people obey the law.

Keywords: Compliance, Legitimacy, Deterrence

JEL codes: N44, K14, K42, P48

*Chair of Law and Economics, ETH Zurich, chendan@ethz.ch. I would like to thank Gerard Oram for providing the death sentence data, numerous colleagues for helpful comments and discussions, and several dedicated research assistants. Work on this project was conducted while I received financial support from the Ewing Marion Kauffman Foundation, Institute for Humane Studies, John M. Olin Foundation, Petrie-Flom Center, and Templeton Foundation.

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 2005). What makes this absence of evidence so intriguing to some observers 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 U.S., states that allow the death penalty differ from states that do not in important ways that probably have independent effects on the level 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 World War I 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 World War I, 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.

On another level, the data allow me to examine the potential role of legitimacy in why people obey the law. Do people follow the law because they fear punishment or because they think the law and the lawgiver has legitimacy and the law ought to be followed? Minorities in the U.S. are disproportionately sentenced to death (Donohue 2011) and higher rates of crime among disadvantaged groups have been attributed to mistrust of legal institutions (Tyler and Huo 2002) and brutalization (Bowers and Pierce 1980; Bailey 2006). Irish soldiers during World War I were roughly six times more likely to be sentenced to death and executed than British soldiers and Ireland declared independence shortly after the war. Examining whether Irish soldiers were spurred to desert following executions of Irish soldiers thus contributes to a broader debate — whether, as in a classical law and economics framework, deterrence alone drives behavioral responses to the law or whether considerations such as legitimacy explain why people obey the law. While social scientists and political

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

philosophers have long speculated on the role of legitimacy in organizations (Suchman 1995), courts (Gibson et al. 1998), and democracies (Lipset 1959), no causal field evidence examining actual behaviors exists to date.

1.2 Historical Context

British Commanders of the era were convinced of the deterrent power of the death penalty (Oram and Putkowski 2005). 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. The lower panel of Figure 1 shows a plot of the distribution of death sentences and their resolution over the course of the war (the upward-pointing tick marks indicate a commutation and the downward-pointing tick macks indicate an execution). Historians believe that 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% across divisions (see Figure 2) and across time (see Figure 3) — an almost literal decimation — with most soldiers being executed by a firing squad of their fellow soldiers, 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. A soldier could not get a safe jail sentence that would have allowed him to leave the trenches.

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 (Babington 1983; Putkowski and Sykes 2007) 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 World War I 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 and environmental factors 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 to see if commanders targeted units for executions during certain time periods or whether commanders felt certain units were "due" for an execution or became more lenient after an execution.

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 three 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 specification. Strong SUTVA can lead to underestimates since it assumes control groups are unaffected. My third approach uses a day-by-day, non-parametric model of absence to ensure identification does not come from functional form assumptions on the hazard rate of absence. Because it includes all prior events, the non-parametric dynamic treatment design also incorporates idiosyncratic variation over time in the execution rate.

1.4 Unit of Analysis

It is necessary to choose a unit of analysis for the study. Military organizations are obviously hierarchical and there is a great deal of discretion in choice of unit-size. The casualty data and absence data is at the battalion level, so I could in principle choose any unit from this level up to the Corps. While there are exceptions, in general, the sequence of military units listed from lowest to highest was: Battalion → Regiment → Brigade → Division→ Corps → Army → Army Group. Each higher level of organization contains three or four subordinate units plus headquarters and higher-level assets. According to at least one historical account, the division commander was the highest-level commander whose recommendation was ignored (Putkowski and Sykes 2007). If higher-level commanders did target bad units or show discretion, then the division is the highest level appropriate for analysis.

The thinness of the outcome data compels a fairly high-level of organization, even though the salience of an execution and hence its deterrence effect (if any) would be strongest at lower levels of organization. The Order of Battle, which contains dates when each battalion is associated with a particular division, provides incomplete information on intermediate levels of units. The absence data I am able to collect (thus far) is relatively thin. I am able to identify 1,061 usable matches from the Military Police wanted lists preserved in war diaries from France and Flanders; they are preserved mostly for four months in late 1916 (Figure 1 upper panel), so I restrict the analysis of war diaries data to the 899 absences that occur during this window. The median time between trial and next recorded desertion in the field at the level of a division is 15.5 days. For a complete coverage, I turn to the Police Gazettes published in the U.K. for a four-year universe of roughly 2,800 BEF deserters who were absent for at least a month.

1.5 Literature

In addition to a large empirical death penalty literature summarized critically by Donohue and Wolfers (2005) as well as a recent U.S. government task force (Nagin and Pepper, eds 2012), my paper builds on a literature examining determinants of desertion (Costa and Kahn 2003), a literature using commuted prison sentences to identify causal effects of prison on subsequent crime (Drago et al. 2009; Kuziemko 2011), and a literature using alternative settings (Kuziemko 2006) and structural models with different identification assumptions to estimate the deterrent effect of the death penalty (Cohen-Cole et al. 2009; Manski and Pepper 2011).

Finding a deterrence effect in the context of World War I would certainly not be a strong argument, leaving aside moral issues, that the death penalty is good policy. However, the British experience may provide a mechanism experiment (Ludwig et al. 2011) for death penalty policy — are individuals less likely to do some action after seeing someone executed for doing that action — and a low-bar test. 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 World War I context. The World War I 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 (Katz et al. 2003).

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. Yet my calculations suggest that desertion led to a higher probability of dying. Desertion is also arguably a more "rational" and victimless crime than murder. 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 for crimes is not as strong a disincentive as we might imagine.

Despite these differences, this study, beyond exploring an interesting historical question, offers some insights with potentially greater generalizability. 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. However, if the sentence is applied non-randomly, 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 task force (Nagin and Pepper, eds 2012) emphasized that death penalty research should focus on perceptions of the risk of criminal sanctions (Apel and Nagin 2011; Lochner 2007; Sah 1991), so risk perceptions will be one focus of my historical research.

1.6 Irish

Criminologists, sociologists, and psychologists have documented negative responses by minorities to state-imposed violence (Fagan and Meares 2008); these negative responses have been attributed to mistrust of legal institutions (Tyler and Huo 2002) and brutalization (Bowers and Pierce 1980; Bailey 2006). Executions of non-deserters and Irish thus allows identification of the potential role of legitimacy in why people obey the law (Tyler 2006). During World War I, British commanding officers made explicit references to the Irish race as inferior and degenerate (Oram 2011, 2003, pp. 9-10) and the Irish, in turn, perceived discrimination by the British High Command against them for their high rates of execution (Putkowski and Sykes 2007) — of the 206,000 Irishmen who served in the British forces (Campbell 2005; Jeffery 2000, pp. 6-7), one out of every 300 received a death sentence,[2] whereas of the 5.2 million British who served (Office 1922), one out of every 2000 received a death sentence. With the higher rate of execution, in combination with separatist events back home (the Easter Rising of 1916 left 318 dead and 2,217 wounded (Foy and Barton 2000)), we may expect the Irish to be less willing to fight for the British. In fact, U.S. newspapers noted cases of Irish officers deserting to fight for the German forces (Chicago Daily Tribune; Mar 20, 1916). By studying the deterrent or delegitimizing effects of the death penalty among British and Irish soldiers during World War I, this paper contributes field evidence to a literature that is primarily experimental (Tyler 2006; Bohnet et al. 2001; Bohnet and Cooter 2003; Galbiati and Vertova 2008; Tyran and Feld 2002; McAdams and Nadler 2005, 2008), theoretical (Kaplow et al. 2006; Hurd 1999), or survey-based (Tyler and Huo 2002) on whether legitimacy affects compliance with the law.

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 discusses a theoretical framework for 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] My numbers differ somewhat from the 1 out of every 600 ratio in government reports (Departmentof Foreign Affairs 2004, p. 12) because I use the number of Irish who served, rather than recruits, as the point of comparison, and I use a surname dictionary to measure who is Irish.

2 Historical 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 4 for a flowchart illustrating the criminal justice procedure). Except where otherwise cited, the information presented in this section comes from discussions with British military historian Julian Putkowski, co-author of Oram and Putkowski (2005) and Putkowski and Sykes (2007), and the Departmentof Foreign Affairs (2004). The information presented here motivates the conceptual framework as well as the empirical analysis and provides context for the datasets. I focus particularly on the randomness of the confirmation (i.e. execution) or commutation decision. I also focus on the salience of execution to soldiers in order to motivate the appropriate unit of analysis.

2.1 Commander's Beliefs about Executions

Most British military officers from the World War I-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 after corporal punishment, such as branding (Oram 2003, pp. 21-26) and public flogging, was outlawed as inhumane in the previous half-century (Oram 2003, p. 38). 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 and Sykes 2007). 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 (Oram 2003, p. 38). Courts martial records indicate many instances where military officers wrote, "the state of discipline of this unit requires an example" (Departmentof Foreign Affairs 2004, p. 38). Oram (2003) shows a time series of courts martial and casualties and suggests that death sentences peaked shortly before the start of British offensives but not German offenses. If German offensives were not foreseeable to individual soldiers and their officers, then this finding is consistent with an active approach to absences by commanders. The potential increase in death sentences before British offenses is, moreover, unlikely to reflect the greater volume of desertions (and hence potential for executions) preceding a military action since soldiers would not know of impending offenses. Infantry soldiers would typically have only 12-24 hours advance notice, even if they were in the front line or reserve trenches. Furthermore, when it came to a major offensive, they could not have anticipated anything about the scale of preparations until the artillery barrage commenced, at which point there would no doubt about what was going to happen.

My data does not suggest, however, that death sentences peaked before British offensives. Vertical bars in Figure 1 indicate major engagements. Somme and Third Ypres were British offenses, First Marne, Second Ypres, and Second Marne were German offenses, and Verdun was a major engagement between French and German troops. No consistent pattern appears between the number of death sentences and the battles. Still, I control for casualties because of the possible influence of casualties on death sentences and desertions. Controlling for casualties would add precision since the conformation decision is not correlated with casualties.

2.2 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[3] suggests that the costs of deserting and factors contributing to the probability of being caught (and ultimately executed) remained roughly the same after an execution or a commutation. Of course, soldiers' beliefs about apprehension may respond to an execution.

The high rate of apprehension is consistent with official statistics, which indicate the desertion rate "abroad" to be 10.26 per 1,000 men (Corns and Hughes-Wilson 2007, p. 216), so that in an army of 5.4 million serving in France and Flanders, there were roughly 55,400 absentees. Records indicate 44,395 courts martial, with 7,361 soldiers were tried for desertion and 37,074 were tried for absence (Office 1922, p. 649). The estimated number of absentees may too high, moreover, because of World War I phenomena – stragglers, missing in action, poison gas, surrendering without a fight – all would make it very hard to pin down the true number of deserters. Indeed, courts martial records of deserters indicate typical defenses include: wandering into German trenches, being fired upon overnight and getting separated, an exploding latrine, oversleeping in a dugout (Department of Foreign Affairs 2004). While statistics "abroad" include France and Flanders, Mesopotamia, Egypt and Palestine, Salonika, Italy, Gaillipoli, and other theatres, I focus on France and Flanders because it comprises over 60% of the total soldiers employed abroad and the vast majority of courts martial discipline: 322 of 346 soldiers executed during World War I were executed in France and Flanders (Office 1922, p. 648).

[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).

2.3 Trials

Most of desertions that occurred overseas in France and Flanders 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). Indeed, the GCM was generally reserved for officers, while the vast majority of deserters were regular or volunteer infantrymen. 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 (Department of Foreign Affairs 2004, p. 7). 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 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). Private trials thus left the typical soldier with little news about death sentences or about deserters until an execution was promulgated.

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 (In March 1916, the U.K. began conscription of single and, eventually, married men up to the age of 51) as well as those on furlough from the front or returning after convalescence in the UK. Offices had 3-4 furloughs a year and elite soldiers could get 10 days out of 1 year. All soldiers eventually received a furlough if they served at least 1 year, but the leave would be cancelled if there was a military engagement. Including these individuals, the total number absent at home or abroad was 126,818 (Office 1922, pp. 83-89). 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. I analyze separately deserters on furlough or enroute to or from BEF because they could respond to executions.

Not every trial in FGCM or GCM resulted in a death sentence, however. For example, 46% of desertion trials in July 1915 resulted in sentences of less than three months (Corns and Hughes-Wilson 2007, p. 216). In mid-1915, the War Office issued instructions to punish deserters much more severely, though there was likely still some discretion in categorizing the soldier as absent or deserter, the latter offense requiring a showing of intent (Babington 1983) and the former offense not typically receiving the death penalty. Nor did every desertion result in a trial. Some soldiers who tried to run away were driven back by officers threatening to kill them on the spot (Moore 1975, p. 66) and some actually were killed on the spot, with rumors of unjust executions circulating among soldiers (Oram 2003, p. 15).

2.4 Affirm or Commute the Death Sentence?

Procedure

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 (Oram 2003, Department of Foreign Affairs 2004, p. 7).

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, e.g., age, domestic responsibilities, prospects, civilian character, peacetime occupation, and whether he was a regular, territorial, volunteer, or conscript. That said, this claim does not have consensus among historians, though my analysis tends to support the "pitiless lottery" hypothesis (Babington 1983). At the very least, the leading counter-hypothesis, that the Irish were disproportionately discriminated against and executed, does not hold up, *conditional* on the death sentence: in my data, 19% of death sentences and 17% of executions were of Irish soldiers. The second leading factor articulated by historians is whether a soldier previously had a death sentence. However, 92% of commutations are of first-time death sentences while 95% of executions are of first-time death sentences. Finally, the class bias suggested by some observors (Oram 2003, Department of Foreign Affairs 2004) do not appear in the executions: officers, who typically came from British public schools[4] (Department of Foreign Affairs 2004, p. 12), constitute 4.4% of death sentences and 7% of executions, while privates constitute 91% of death sentences and 82% of executions.

Indeed, consistent with the pitiless lottery hypothesis, records indicate that decisions could be arbitrary, with identical extenuating circumstances apparently accepted in some cases and rejected in others (Department of Foreign Affairs 2004, p. 3). Commanders-in-Chief, Generals Haig and French, could not possibly have had time to exercise individual scrutiny of each dossier, if only because, with almost 2 death sentences per day on average, there would not have been time to read in detail and ponder over each and every case (Oram 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 2007), 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; Brigadier General Sir Anthony Farrar Hockley, however, believed that the decision of the divisional commander was pivotal in the confirmation process.

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,

[4]e.g. Eton College

it is unlikely there were explicit commutation goals. Some generals never executed anyone[5] while others[6] were instrumental in the deaths of many condemned men (only two generals, French and Haig, however, are relevant for my data, so alternative empirical strategies using changes in the commander-in-chief are not possible). 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.

Unit of Analysis

The one-page summary presented to the Commander-in-Chief indicated the performance of lower-level military units, so where able, I check for randomization within other levels of hierarchy as well. The confirmation process explicitly requested reviewing officers to consider the state of discipline of their units. 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, 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, ten days reserve, and ten days rest and a battalion in the line would rotate companies between two lines of reserve or support trenches and the forward (firing) line. Since, Corps and Army level concurrences could seal a man's fate, and lower-level officer recommendations (division and below), who did not have the incentive to report indiscipline because of career concerns, were basically ignored, I use the division as my main unit of analysis. A battalion consisted of 1,000 men, and a division consisted of between 18,000 and 19,000 men (Corns and Hughes-Wilson 2007, p. 108).

While the historical evidence suggest that unit-level factors affected decision-making and that some units were targeted, it does not appear to first-order in a visual examination of execution rates by division. Each circle in Figure 2 represents a division and the dotted line indicates the 12% execution rate. Some units were targeted for death sentences, but the data do not suggest units were targeted for executions conditional on the death sentences: units align closely to the 12% execution rate. Figure 2 also shows that units with a higher proportion of Irish soldiers, indicated by the proportion of each unit's tick mark that is green, do not fall disproportionately above the 12% line.

Perceptions of Decisions to Execute or Commute

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 statistically investigate the

[5] e.g. Ivor Maxse

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

randomness assumption, the historical evidence is enough to suggest that at least within a particular military unit, which exact soldier that was executed — at least from the perspective of the other soldiers in that unit — was essentially random. That is, an executed soldier may have been known for being "a bundle of nerves" (Moore 1975) or Irish ("Deserter Shot. Facts about court-martial on an Irish private." The Western Gazette, 28 January 1916), characteristics that may have played a contributing factor to the desertion, but soldiers would not have believed they were not eligible to be executed because of some observable characteristic of the executed soldier.

As far as I know, there are no written records of public outrage to cancelled death sentences nor any record of knowledge about commutations; executions were likely the only news about death sentences transmitted to the typical soldier. Eyewitnesses were left in no doubt that deserters really were executed. Gaining a statistical impression about the number of men who were not executed was almost impossible by soldiers; the government tried very hard to keep death sentences quiet and records were not public for 75 years. In fact, everyone knew about the penalties ("It is well known ... to all soldiers that desertion in the face of the enemy is liable to be punished by death." The Western Gazette, 28 January 1916) and when recruits joined the army, they were informed that the death penalty could be inflicted upon anyone who deserted while under orders to proceed on active service (Moore 1975, p. 50).

Battalion commanders frequently recommended commutation of death sentences, only to be overruled [by the High Command] (Oram 2003, p. 129). Moreover, in most cases the court [martial], in passing a sentence of death made a recommendation of mercy (Oram 2003, p. 127). The disregard of clemency recommendations by soldiers' immediate commanders (and sometimes even the brigade, divisional, and corps commanders (Babington 1983, pp. 78-79)) and the courts may have contributed to a sense that many executions were unjust — if the soldiers were aware of the clemency recommendations at the same time they became aware of the executions.

2.5 Commutations

After the trial, soldiers found guilty may have been detained (Babington 1983) or thrown back into the trenches (Oram 2003). The convicted soldier would continue to be held in custody. The decision to confirm or commute occurred within two weeks of the original Field General Court Martial death sentence. 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. 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.

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. The soldier would then be escorted from prison to their unit by military police or a couple of soldiers from the battalion who were picking up reinforcements. Some records indicate that soldiers with commuted death sentences received special treatment, e.g. positioned as front-line fodder to receive additional punishment (Coppard

1969). This behavior by military commanders increases the punishment for desertion in a soldier's calculus, but since commutations were not promulgated, the de facto execution of commuted death sentences is unlikely to raise SUTVA concerns in estimating the impact of executions on desertions. I do not use the type of sentence reduction in my analysis since soldiers would not have information about the commutation.

In any event, battalions were always being rotated in and out of the front lines — and reinforcements might reach a battalion that was "at rest" or away from the front for other reasons. But combat was "business as usual" for most troops. Nor do records indicate whether soldiers were sent back to the same military unit — they would have been assigned where needed — but even if they did return to the original battalion (which was more likely if the soldier had been apprehended, tried, and commuted quickly), neither the soldier nor the officer would want to make it known that the soldier had deserted and received a commuted death sentence due to shame or career concerns.

Military authorities were always very anxious to ensure that either a spell in jail or a suspended sentence was not viewed by soldiers generally as a way of avoiding front line service. Reduced sentences were served after the war. In some cases, soldiers even continued to serve in the trenches for several weeks while the death sentences were reviewed by officers in the military heirarchy.

2.6 Executions

Timing and Promulgation

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). 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 2007). 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 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 (Department of Foreign Affairs 2004, p. 8).

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 (Corns

[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.

and Hughes-Wilson 2007). 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).

Saliance of Executions

Executions 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. Nothing in the diaries and memoirs suggest that executions caused soldiers to think that more people were deserting.

Soldiers were likely exposed to very few promulgated executions. My calculations suggest that a typical Regular infantry division saw 2.5 executions per year (the 12 Regular army divisions are indicated in red circles in Figure 2), Territorial Force divisions saw 0.5 executions per year (the 14 Territorial divisions are indicated in tan circles), and New Army divisions saw 1.25 executions per year (the 30 New Army divisions are indicated in navy circles). The Territorial and New Army divisions also received fewer death sentences; the soldiers in these units were less professional (these units included conscripts) and the commanders possibly had lower expectations for these soldiers or were more worried about adverse effects of discipline (Oram 2003, Department of Foreign Affairs 2004, p. 18). Despite the low number of observed executions, soldiers are quoted as saying, "it was only fear of death that kept them at their posts" (Moore 1975, p. 62).

3 Theoretical Framework

3.1 An Economic Model of Legitimacy

According to the economic model of crime, would-be criminals weigh the benefits of crime—loot, eliminating one's rivals, vengeance, enjoying drugs etc.—against the costs, which can be either overt, such as loss of freedom, money and physical well-being or psychological and social, such as shame, loss of reputation and the pain from violating one's moral principles.

When policy changes alter the overt costs, say by increasing the jail sentences associated with some crime, the common-sense prediction is that the increased costs will reduce the "demand"

[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.

for that crime and ceteris paribus, we should observe less of the more heavily penalized activity. However, one particular form of confounding is that following an increase in overt costs, social costs may increase or decrease or remain unchanged. Depending on the size and direction of the social costs effect, the prediction of more punishment, less crime might not hold true.

When social and overt costs are "complements," an increase in overt costs leads to an increase in social costs. One possible reason why the two costs may be complements is that a change in overt costs might signal to a population how serious a crime is to the community and the social order (Benabou and Tirole 2011). As an example, suppose I knew that there was a $100 for fine dumping chemical A down the drain, but a $50,000 fine and three years in imprisonment for dumping chemical B. Without any knowledge of chemistry or toxicology, I might decide to have a dimmer view of Chemical B dumpers and be myself less willing to dump B, even if I was certain there was no chance I would be caught. In this sense, law has expressive effects, where the social incentives reinforce the deterrent effects of the law.

When the costs are "substitutes," an increase in the overt costs decreases the social costs. The situation seemingly likely to generate this scenario is when the law or the imposing authority is already viewed as illegitimate — a tougher sanction only heightens the injustice. The overt effects might still dominate, but those who do break the law might be admired for refusing to submit to an unjust regime. One of the reasons we admire "lawbreakers" such as Nelson Mandela, Rosa Parks, Sophie Scholl is because of the severity of the punishments they faced and our collective view of the unjustness of the legal authorities they resisted.

One way to formalize this conception is as follows:

$$U(a) = (v_a + y)\,a - C(a) + e\overline{a} + \mu E(x \mid a)_s$$

where v_a is intrinsic motivation (over the range of $[\underline{v}, \overline{v}]$), y is extrinsic payoff, $C(a)$ is the cost of the action, $e\overline{a}$ is the public good aspect of the good, and μ is the weight agents put on social perceptions, $E(x \mid a)_s$, which is other people's perception of your intrinsic motivations (Benabou and Tirole 2011). I conjecture that the execution tells Irish soldiers that e is negative, that it is no longer worth it to fight for the British. This could have social multipliers through $E(x \mid a)_s$, as desertion becomes more normalized, if not honorable.

Using this framework, we could decompose the crime response from a policy change into the overt effect that arises directly from the harsher or more lenient punishment, and the social effect, which can be positive or negative. In most countries, at most times, it seems likely that the overt effect, $C(a)$, dominates, but in cases where the social effect is likely to be strong and move in an opposite direct from a significant sub-population to the overt effect, it seems likely that we could find cases where increasing punishment increased crime.

While these "more-punishment, less-crime" crimes might seem like a theoretical possibility rather than a common occurence, they are the key concept in the study of insurgency. The classic insurgency strategy is to provoke a government or occupying power into counter-productive, heavy-handed responses. If the crime is "resisting the state" and the punishment is more raids, collective punishment, etc., you might get more resistance after more punishment. This is not so surprising because insurgency is precisely the kind of situation where state legitimacy is in question and the (inverted) social shame response to crime is likely to be strongest. Rather than just explaining insurgencies, this two-component model of crime seems particularly applicable to the situation of persecuted or disadvantaged minorities, who may not view the law as particularly legitimate (Tyler 2000). If the intensity of punishment is increased and punishment is perceived as unjust, it can reduce the "ought" justification for following the law.

To investigate the relationship between legitimacy, punishment intensity and crime, one would need a scenario where identical crimes led to very different punishments for arbitrary reasons and where these punishments were observed by different populations with different beliefs about the legitimacy of the law. While this paper is related to a controversial and contentious policy-oriented literature on the deterrent effect of the modern death penalty, it can also tell us about how people react to exogenous changes in punishment severity, how they change their behavior in response to those changes, and how attitudes about state legitimacy and punishment mediate these responses.

3.2 Soldiers' Decisions

I hypothesize that execution-commutation observations cause soldiers to update prior distributions on the probability of being executed if they desert and, for some soldiers, reduce the social cost of deserting. Observing executions can cause discontinuous changes in behavior whether because people are rationally inattentative (Sims 2003) or because people overestimate from rare events when they occur and overweight recently sampled information. (Hertwig et al. 2004).

Holding fixed the social cost of deserting, 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. Soldiers were conscious of the death penalty and executions were salient, so this context provides an upper bound on deterrent effects of the death penalty.

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 (Beckett 1985), family was not paid, and probability of death). While I examine the assumption that commutation decisions were random from an econometric perspective to isolate causal effects, the soldiers would not have perceived the decisions as random, since they had little or no information on commuted death sentences and likely only saw or heard about executions. I hypothesize that soldiers (or their battalions, since most soldiers would have only experienced a handful of the execution parades) learn and forget (i.e., the posterior slackens back to the uninformative prior over time). Later, I relax the assumption that the most recent event matters.

Calculations of Costs and Benefits

The intensity of World War I trench warfare meant about 12% of the soldiers were killed serving on the Western Front, while an additional 37.6% became wounded (Urlanis 1971; Office 1922, p. 246).[9] Total battle casualties, including wounded, missing, and prisoners was 56% (Office 1922, p. 248). 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. If we assume all of the casualties fall on fighting soldiers, not support soldiers, then we may estimate that a soldier continuously in active and fighting mode faced a 48% chance of being killed over their entire length of service (and nearly 100% of being injured).

The peak strength in France and Flanders was 2 million men and 5.4 million men saw some service in this theatre. Assuming 2 million men served each year and a constant replacement of soldiers, then the typical soldier's length of service was 1.5 years. If a soldier was in active, fighting duty the entire 18 months, he had a 4% chance of debilitating injury or death in any given month, 7.8% chance of debilitating injury or death over two months, 11.5% chance of debilitating injury or death over three months, and so on. This calculation assumes independent probabilities of debilitating injury / death in any given month and that 50% of soldiers are out of commission because of injury or death by 18 months. Using the number 552,471 of British casualties in France and Flanders recorded in my data and assuming a constant 11,500 soldiers dying per month, then a soldier saw 0.5% chance of dying in any given month. These probabilities would vary over the course of the month since battalions rotated 10 days in the front, 10 days in reserve, and 10 days at rest every month.[10] Because soldiers were moved at night, their opportunity to desert was maximum during rotation. There was little opportunity to desert on the front — a soldier would be shot immediately — moving soldiers was like squeezing toothepaste and the battle police were right behind you (conversations with Putkowski, 2011).

A hyper-rational soldier weighing the costs and benefits would know that during the 2-4 weeks when he was away from the front (two weeks in hiding, two weeks in detention), he avoided the 0.5% chance of dying (or 3.5% chance of debilitating injury). Of the 7,361 trials for desertion 2,007 resulted in a death sentence, so he had a 3.3% chance of dying because he deserted. This number would be far lower to the extent the detemination of desertion or absence (for which there were 37,074 trials) was very fluid. The other 96.7% of the time, he would be sent back to the trenches. Because commutations were not publicized, I assume the soldier only experiences the social cost, if any, for desertion regardless of the trial outcome, and did not experience additional social cost when fighting (though one may calculate that the social cost is only experienced while the soldier

[9] In comparison, only 5% were killed during the Second Boer War and 4.5% killed during World War II.

[10] Conversations with Putkowski. The same 1:1:1 ratio is found in http://www.1914-1918.net/intrenches.htm, though http://www.spartacus.schoolnet.co.uk/FWWfrontline.htm aportions 2:1:1 ratio.

is alive).

Of course, soldiers might not believe that they would be caught. A soldier would desert if the utility from being reunited with family increased for exogenous reasons. Many desertions were prompted by Dear John letters from spouses or news about children being ill. A rationally inattentative soldier or a soldier who overestimate from rare events would increase the subjective likelihood of execution after seeing or hearing about an execution. Thus, ignoring social costs, the effects of increasing the probability of applying the death penalty are clear: if some crime is now thought to hold a higher chance of leading to the death penalty, we should see less of it.

Social Costs

For Irish soldiers, the social cost of fighting for the British is unclear at first glance. Since a total of 206,000 Irishmen served in the British forces during the war, and the number of Irish deaths in the British Army recorded by the registrar general was 27,405, they did not appear to suffer disproportionately nor did they appear to be treated substantially as cannon fodder (of course, they may have been better fighters who were assigned to harsher locations). Nor were the Irish Divisions (the 10th, 16th and 36th) disproportionately targeted for harsh assignments (because of manpower shortage, the commander stopped refilling divisions with people from the same geographic background) (Fitzpatrick 1996).

Identity considerations likely increased the the cost for Irish of following the law. If punishment is perceived as unfair, then it can reduce the legitimacy and authority, and reduce the "ought" justification for following the law. The most extreme sanction available to authority is the death penalty. Since the death penalty was such a visible and extreme form of punishment, we might expect more punishment to lead to less legitimacy and more crime. A vicisious cycle can arise. More crime leads to more punishment, which further delegitimizes the government. While the Irish were only 3.7% of U.K. soldiers in France and Flanders, they comprise 21% of the deserters, 19% of the death sentences, and 17% of the executions.

4 Data

I employ four datasets: one on court martial death sentences, executions, and commutations; one on absenteeism; one on casualties; and one on a list of Irish surnames, which I use to identify soldiers of probable Irish ethnicity (differences between Irish and British would be underestimated to the extent that soldiers are mis-categorized).[11]

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,

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

and other information, from August 1914 to September 1923 (Oram 2003). Only data that overlaps with absences are used in the analysis. 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).[12] Final sentences in the dataset are those punishments (if any) ultimately confirmed 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

The data on immediate absentees (discovered the day after) 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). The war diaries span four years, but the bulk of what was preserved in absentee lists is in four months of late 1916 (Figure 1).

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

[12]The other offenses are: Irish rebellion, quitting post, striking senior officer, mutiny (which could involve absence but was more related to collective act or conspiracy), offense against inhabitant (i.e. rape), 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, conspiracy, robbery, theft, attempted assassination, attempted murder, attempted desertion, housebreaking, losing army property, pillaging, aiding enemy whilst POW, and unspecified/other, for a total of over 30 types of offenses.

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.

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.

4.4 Police Gazettes Data

I have obtained a list of absences and desertions from a second source, the Deserters and Absentees (D&A) supplement to the (weekly) Police Gazette. The details 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. Deserters at home were not subject to the death sentence, however.

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.[13] 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

[13] 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.

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.

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 jth 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, or if commanders targeted units for poor discipline.

Mean-Reversion I test whether there is any form of mean reversion in the sequence, meaning that the execution in the nth 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 or whether they became more lenient after 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.

Taken together, these tests of randomization help address possible confounds. Simply showing that residuals behave like a random string does not address the possibility that observables are correlated with executions and observables are randomly distributed over time. Simply showing that observables are uncorrelated with executions does not address the possibility of higher level of non-randomization on the part of military officers if observables are randomly distributed over time.

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 nth 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, λ, 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 θ. 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 indicator for whether the death sentence led to execution is important to address spurious inferences were I to only use the sample of executions. For example, an increase in unit size could lead to more desertions, more death sentences, and more executions (assuming constant desertion and execution rates). This would lead to a spurious inference of no deterrence.

The Weak-SUTVA Approach I assume that past events matter, but that they fade out exponentially, according to some parameter k. I test values of k such that $k = -\frac{\log \frac{1}{2}}{\Delta t}$ where Δt takes values of 7, 14, 30, 60 and 90, corresponding to deterrence-effect half-lives of 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? Of course, if executions and commutations are random, then the distribution of past events should smooth out, but the estimates would be less precise.

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: $i = 1 \dots I$

Time $t = 1 \dots T$ 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 kth 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

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

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 β_{cm} is a measure of the deterrent effect of a commutation, while β_{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 λ 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 β. F is the link-function whose range is $[0,1]$.[14] 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 β and γ 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.

[14] We can allow event-specific values of λ

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.[15]

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 models. Divisions were not segregated by ethnicity. Of over 2,100 absences, 340 were Irish.

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 World War I 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. To see why targeting divisions presents a confound, perhaps divisions closer to the front faced higher rates of executions and lower desertion rates because soldiers perceived it would be more difficult to desert or because the soldiers who did desert were shot on the spot; alternatively, Corp-level officers

[15] 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 λ 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.

may perceive bad discipline, which may drive executions, but discipline may be mean reverting. 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.

In sum, first, when I assume a strong form of SUTVA in which I posit that only the most recent event matters, I find limited evidence that executing deserters deterred absences (it disappears when controlling for division fixed effects), while executing non-deserters and Irish soldiers, regardless of the crime, spurred absences in general. Strong SUTVA can lead to effects to be biased towards 0 since it assumes control groups are unaffected. 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 specification. I find stronger evidence that executing deserters deterred absences and executing Irish soldiers spurred Irish absences in particular.

8 Why Not Something Simpler?

It is generally considered good writing practice to avoid long narratives about research blind alleys and false starts, but I believe discussing some of the more standard (and inappropriate) approaches has value. Prior to using independently measured absences as the outcome, I considered using courts martial for desertion as the outcome. This method had one obvious advantage in that it did not require the collection of additional data, but it is problematic. The first problem is that commanders had discretion over how a particular case of desertion was handled. As such, a lack of courts martial in a particular unit following an execution might not tell us anything about the number of desertions — it is entirely possible (and even probable) that the commander is prosecuting fewer cases or seeking lesser sentences than capital punishment following an execution since he has already "made his point." In contrast, reporting absences was not really under the commanders' discretion: not reporting missing soldiers would have been abetting their desertion — this lack of discretion makes absences a superior measure.

As an example of how highly malleable the sentence charged at court martial could be, the chances of being tried for desertion depended to a great extent on a soldier's rank: compared to soldiers, officers were charged with lesser offences or got convicted on a lesser, alternate charge (e.g. Drunkenness or AWOL instead of Desertion), getting cashiered instead of facing a firing squad. Moreover, after a death sentence was passed and a soldier was executed, that soldier's lower-level commander might alter his own prosecution style in a way different than he would if that sentence had been commuted. If that were to be true, courts martial would be a biased measure of desertion that could only be addressed if the ratio of desertions / desertion trials leading to conviction was constant across the military and the divisional conviction rates were constant.

The time-series-like structure of the data and the large number of comparable units suggests a panel data model is appropriate. However, one often overlooked problem with the panel approach is that both the initial starting point and the length of the periods are quasi-arbitrary. Given the episodic nature of war, the logical breakpoints between time-periods are the start and end of offensives — not months or quarters. Using a fixed time period creates situations in which a court-martial at the last day of a period is modeled as having an effect on a subsequent court martial that might have occurred the next day, and yet a small shift in period length or start point might lead to both events being in the same period. In early investigations, I found that even small changes in the period definitions or start dates could have dramatic coefficient effects, and that the standard errors and implied p-values were not plausible.

9 Conclusion

Analysis of whether British executions during World War I deterred military desertions provides a low-bar test for the death penalty. Moral issues aside, one prerequisite for a death penalty policy is whether individuals respond to increasing subjective risk of criminal sanction (Nagin and Pepper, eds 2012). A negative finding showing no deterrent effect on military desertions would suggest that, even in a context where the death penalty was designed for maximum deterrence (immediate executions, public, and wide promulgation), the threat of death may have less of an effect than some presume.

The confluence of geographic, historical, and political factors make it possible to study the British application of the death penalty. Unlike France and Germany (whose records were destroyed in World War II), a country fighting on foreign, geographically separate territory meant that virtually all deserters and absentees were caught. Since the tangible benefits of deserting were negligible, executions would only change the perceived cost of deserting for most soldiers; for the Irish, executions would change the social cost of fighting for the British. Second, the general progression towards more humane forms of punishment (outlawing branding and public flogging) meant that the death penalty was considered by the British military to be the only way to maintain discipline, leading to a high number of death sentences, over 3,000 during a 4-year period. Yet, changing political mores forced military officials to confirm only a small fraction, 12%, of death sentences. Third, the relatively well-preserved administrative records of an empire at its peak, albeit released 75 years after the war, allow statistical analysis of a large number of data sources, many of which I digitize for the first time.

With over two death sentences per day, historians believe that the decision to execute or commute was basically a random process, which I statistically corroborate. Using this result and new archival data on desertions preserved in war diaries in France and Flanders during World War I, I find limited evidence that executing deserters deterred absences. I find stronger evidence that executing non-deserters and Irish soldiers, regardless of the crime, spurred absences. I employ three modeling approaches to ensure that identification of dynamic treatment effects does not come from functional form assumptions: strong SUTVA, where only the most recent event matters and I study the time from an execution or commutation until next absence; weak STUVA, where I also control for the effects of more distant events; and a day-by-day non-parametric approach, where I estimate the probability of absence as it depends on the cumulation of past executions and commutations, but impose structural assumptions about the half-life of these events.

On another level, the finding of iatrogenic effects, where minorities react negatively to state-imposed violence (Fagan and Meares 2008), resonates with contemporary U.S. policy debate. Disadvantaged groups in the U.S., like the Irish during World War I, are disproportionately sentenced to death (Donohue 2011). Higher rates of crime among disadvantaged groups have been attributed to mistrust of legal institutions (Tyler and Huo 2002). This paper thus contributes causal evidence to a broader question — whether, as in a classical law and economics framework, deterrence alone drives behavioral responses to the law or whether legitimacy can explain why people obey the law (Tyler and Huo 2002). While social scientists and political philosophers have long speculated on the role of legitimacy in organizations (Suchman 1995), courts (Gibson et al. 1998), and democracies (Lipset 1959), no causal evidence in the field exists to date.

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A Data Appendix

This appendix describes how I developed the order of battle, death penalty, desertion, and casualty datasets. The first section describes the various sources for our data. The second section describes the method that I used to cull the data from the sources. The third section describes how the data was merged into a coherent whole.

A.1 Data Sources

There are four main datasets. The first dataset is for the order of battle, which relates to the structure of the British Army throughout World War I. The second dataset contains information about who deserted, when, and from what unit. The third dataset contains information about who was sentenced to death, when, and whether their sentenced was commuted or executed. And finally, the fourth dataset concerns casualties occurring as a result of war time service.

A.2 Order of Battle

The order of battle dataset describes the hierarchical relationship between various battle units. The British armed forces were organized into various hierarchical levels, which were—from broadest to most narrow—army, corps, division, brigade, and finally battalion. This means that there were several armies under the central command of General Headquarters (GHQ). Under each army, there were several corps. Under each corps, there were several divisions, and so on.

The focus in this paper has been to assign each event (death sentence, desertion, or casualty) to a particular division. But most of the sources list the battalion of a soldier, not his division. To determine the division, I had to develop a table of division assignments for each battalion. Complicating this effort was the fact that battalions changed divisions throughout the war—in response to particular strategic goals or needs of the divisions. The order of battle dataset provides the means to determine, for a given battalion on a given date, which division was commanding.

To develop this dataset, I relied primarily on the Long, Long Trail (LLT) website, available at http://www.1914-1918.net. This website gives, in mostly paragraph form, a time history of each battle unit and in particular the movement among divisions. The website gives this data in two main forms.

The first form focuses on the battalion (or other unit), and describes in chronological order the movements of that unit. This type of data is organized based on the kind of battalion. There is a section for infantry battalions, yeomanry battalions, mounted battalions, machine gun battalions, artillery brigades and batteries, Royal Engineer companies, Army Service Corps companies, and Royal Army Medical Corps units. Other, higher-order organizational groups are also described—including Labour Corps, Tank Corps, and Army Ordnance Corps—but individual unit history is not given for these groups.

The second form focuses on the division, and describes the movement of units into and out of the division. There are a total of 76 British, 5 Australian, 5 Canadian, 19 Indian, 2 New Zealand, and 12 Mounted divisions described on the site. The British Divisions are the most important to this paper—because they comprised the bulk of the armed forces.

The major task in obtaining the records was to determine, for each battalion, the dates it joined and left a division. A second task was to determine if the battalion was known by any other name. Some units only appear in the divisional page. For instance, the Australian and Canadian forces are only available by reference the Australian and Canadian division pages. Divisional employment companies and sanitary sections also only appear on the division pages. Thus, unit information is collected from the division pages, and that information is used for those units that appeared nowhere else on the site. The division pages tended to be less precise about unit movements (giving movements by month instead of by specific date). Therefore, when possible, I used the unit page instead of the division page.

A.3 Merging to Division Level

The information about desertion was culled from the war diaries described in the main text. Additional information come from the Police Gazette, a periodical that gave a list of deserters every two weeks. Each entry in the Police Gazette gives identifying information about the individual—name, regiment number, height, hometown, etc.—as well as his assigned unit and the date of his desertion. The unit information is typically an abbreviated form of the battalion, where the abbreviation is whatever a clerk decided to enter. The lack of consistency in abbreviations creates a difficulty—which will be discussed later—in trying to determine which unit is actually being described. In addition to this inconsistency, not every entry contains enough information to determine a particular unit; for example, an entry may only provide enough information to determine the regiment—but not the battalion.

A.3.1 Infantry, Yeomanry, and Cavalry Regiments

Infantry, yeomanry, and cavalry battalions were members of regiments. The regiments were (and still are) independent, mostly regional recruiting and training centers. The regimental system works in parallel with the division system. Battalions are part of regiments; but they are also under the command of divisions. Battalions from the same regiment could be spread among divisions. Thus, the regiments serve as a separate hierarchical structure. The main difference is that a battalion would rarely if ever move from one regiment to another—and even if it did, it is unclear whether the battalion moved; or the old battalion disbanded from the old regiment, a new battalion was formed in the new regiment, and the personnel was transferred from old to new. In our data, we treat battalion movement as if a new battalion were created.

As a result of their membership in regiments, infantry, yeomanry, and cavalry battalion data is given in the regiment pages on the LLT website. Each regiment page gives an ordered list of the regiment's battalions. And then each battalion entry shows the chronological list of unit movement. I would thus develop multiple battalion records, and then multiple division assignment records for each battalion.

A.3.2 Royal Field Artillery

The field artillery was not regimented. The artillery, however, did have its own special hierarchical structure. The artillery was separated into brigades, which were further comprised of batteries. In merging datasets, artillery brigades are considered to be the equivalent of battalions, and so the artillery unit records in the battalions table are for brigades. The LLT website gives more information at the brigade level, so it made sense to treat artillery brigades the equivalent of battalions for the purposes of matching. The LLT site did give some sparse information on batteries—in terms of which brigade a given battery was in at the start of the war. But because the batteries shifted from brigade to brigade during the war, there is insufficient information to decisively place a given battery in a given division. To the extent that a given event record matches to a battery, there is less confidence in the identity of the matched division. Finally, each of the artillery brigades is assigned to the "Royal Field Artillery" regiment. Although the RFA was not a regiment, it is treated as such for matching.

A.3.3 Royal Engineers, Machine Gun Corps, Royal Army Medical Corps, and Army Service Corps Units

For each of these types of units, the unit information is obtained from their respective pages on the LLT site. Royal Engineers, Machine Gun Corps, Royal Army Medical Corps, and Army Service Corps Units are treated as regiments. Again, these are not truly regiments, but it is treated as such for matching.

A.4 Data Linking

The records in the various event datasets were quite similar. They all had a soldier's name, the soldier's unit (on the day of the event), and the date of the event. But, the unit name tended to be abbreviated, and abbreviated in an inconsistent way. I thus had two tasks: (1) determine the battalion that is being described by the abbreviated unit designation, and (2) determine the division for that battalion.

The first task is complicated by the variety and inconsistency in abbreviation. For example, there were 37,397 unique unit abbreviations in the desertion dataset. For each battalion, there may be several dozen different ways that a clerk decided to abbreviate. For instance, the 3rd King's Own Scottish Borderers had 121 different abbreviations in the desertion dataset. The unit names in the desertion dataset tended to be very short, often including only the first letters of the words in the regiments' names. So, for instance, the King's Own Scottish Borderers would be abbreviated "K.O.S.B." But, other abbreviation forms existed as well: "K.O. Sco. Brds", "KO Sco Bds", and so on.

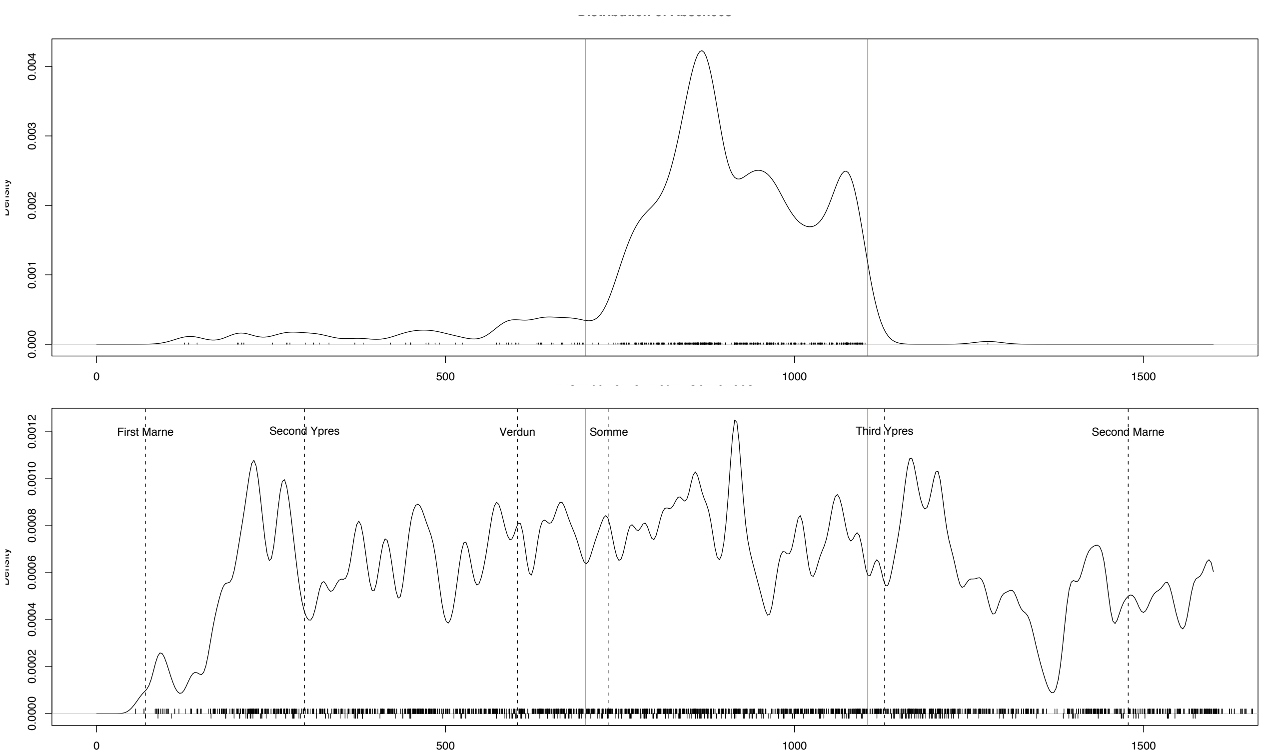
The basic logic for finding possible matches is to first split the unit name into two parts—the battalion part (usually a number or ordinal, like 1, 1st, or 1/1st); and the regiment part (KOSB or RFA, essentially whatever was not the battalion part). For the regiment part, the string is stripped of any spaces and non-alphanumeric characters. It is then converted into a regular expression for searching. In the regular expression search string, a non-greedy wildcard matching zero or more characters is added in between each of the characters in the original string. The regular expression is further modified by adding back the battalion number at the beginning, followed by a wildcard. The search regular expression is then matched against all battalions. Any battalion that matches the regular expression goes into a queue. If there is only one match in the queue at the end of the process, it is presumed to be a good match. If there are more than one match in the queue, the results are manually matched.

So, for example, if the original record said a deserter's unit was "3rd K.O.S. Bds", this string is first split into "3rd" (the battalion) and "K.O.S. Bds" (the regiment). The periods and spaces from the original string are removed, and wildcards are added. The wildcard is the sequence of characters, ".*?", which breaks down as match any character (.), zero or more times (*), in a non-greedy way (?). Non-greedy means match only enough characters to get to the next letter in the series, or match the least number of characters that still reaches a good match. And then finally, the battalion number is added back in at the beginning of the regular expression, and the final regular expression is "3rd.*?K.*?O.*?S.*?B.*?d*?s".

When doing the matching, the battalion we are looking for begins with "3rd", then any number of characters, then K, then as many characters as needed (including zero) before getting to an O, then O, and so on. The following formatted, matched string demonstrates the way the wild cards work: "*3rd K*ing's *O*wn *S*cottish *B*or*d*erer*s*". The italicized characters are matched by the explicit characters, and the non-italicized characters are matched by the wild cards.

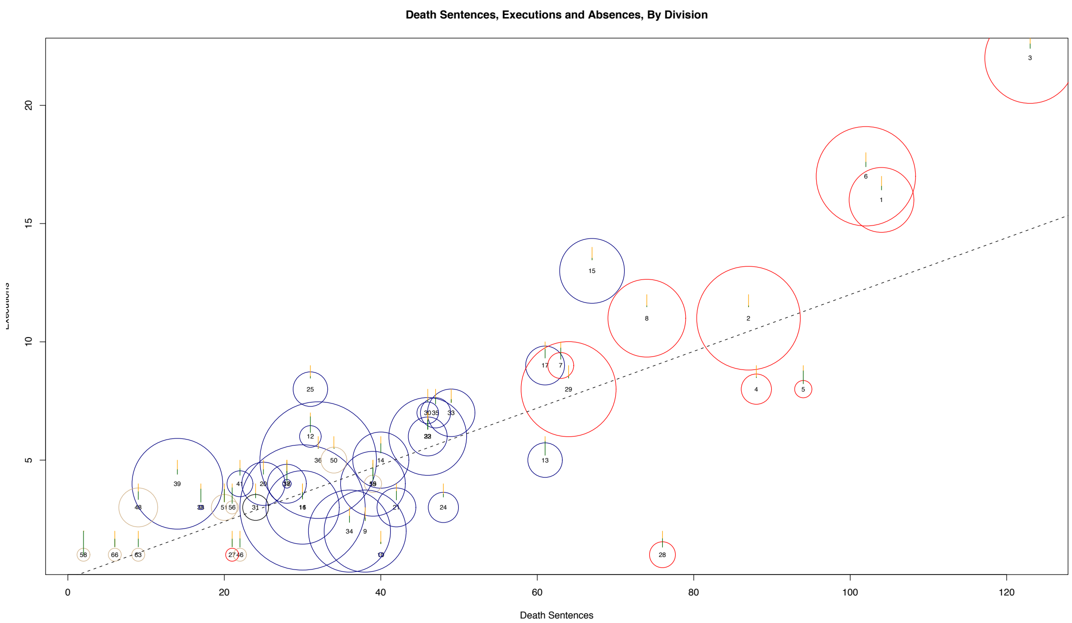
Once I determined the battalion for each record, I then used to the date of the event to determine the appropriate division. I used the order of battle dataset to accomplish this. The order of battle dataset has a table that links a battalion to a division between a starting date and ending date. Thus, for each event record, a record in the battalion-division table matches the battalion when the event date is between the starting and ending dates. A battalion may not have belonged to a division on a certain date, in which case the event is not assigned to this division.

Figure 1: Death Sentences and Outcomes for BEF Units



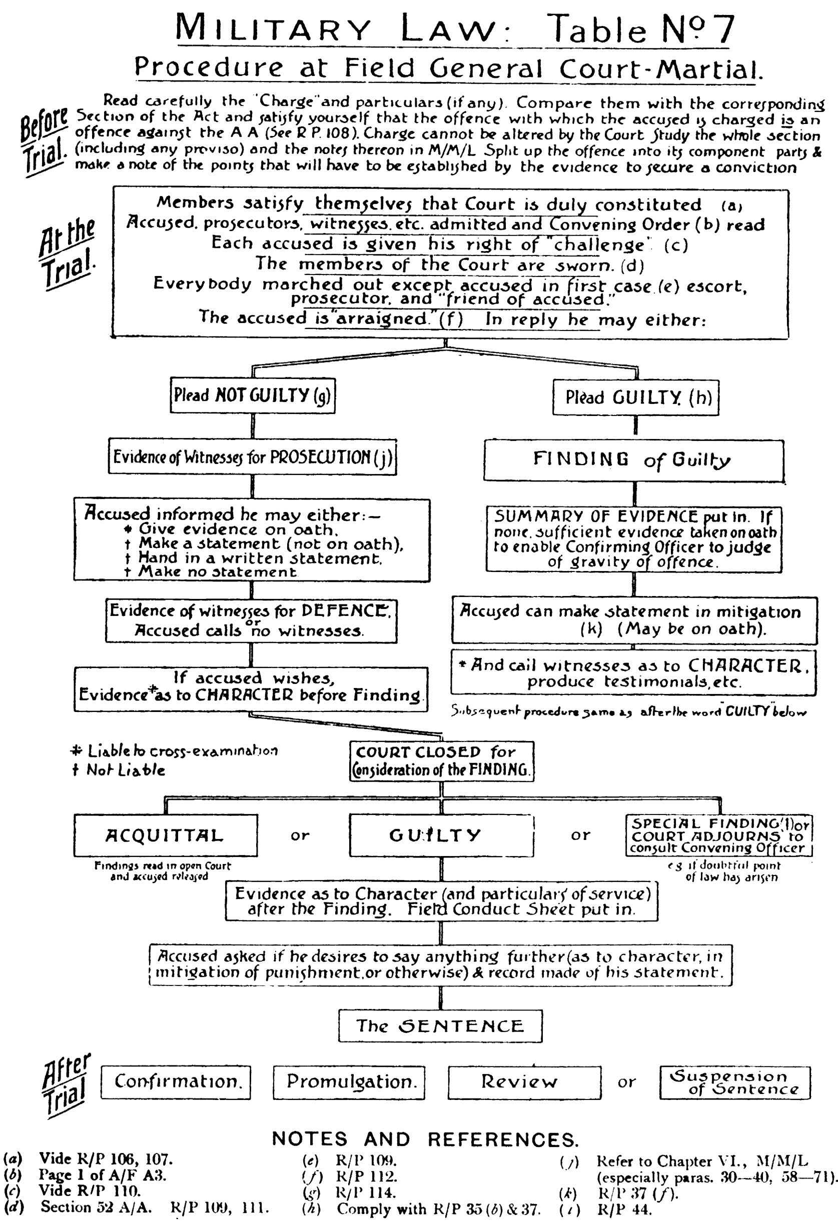
Note: This figure shows the distribution of death sentences during the course of the war. The dotted vertical lines indicate the start of major British offensives. The sequence of tick marks along the bottom axis represent each death sentence, with upward-pointing ticks indicating a commutation and downward-pointing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

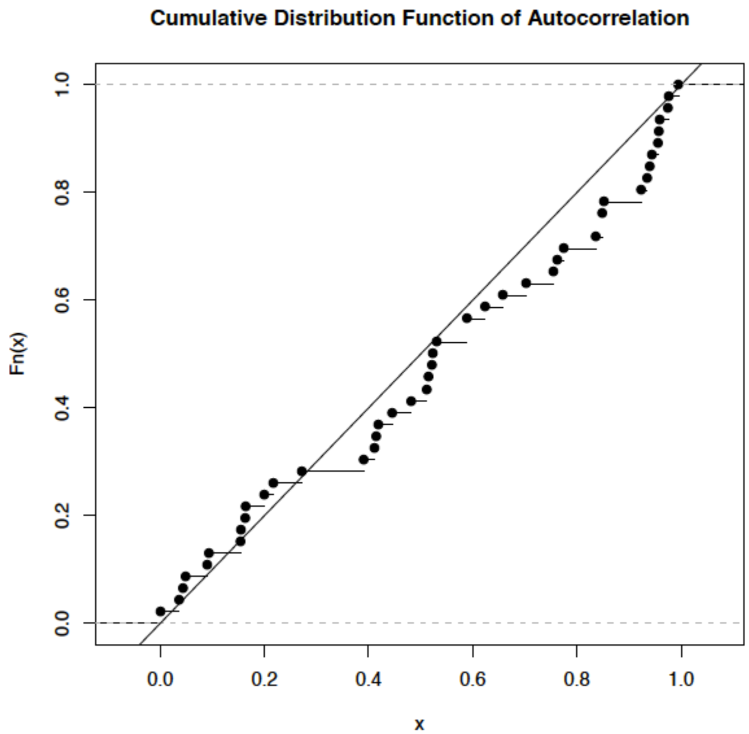
Figure 2: British Army Divisions

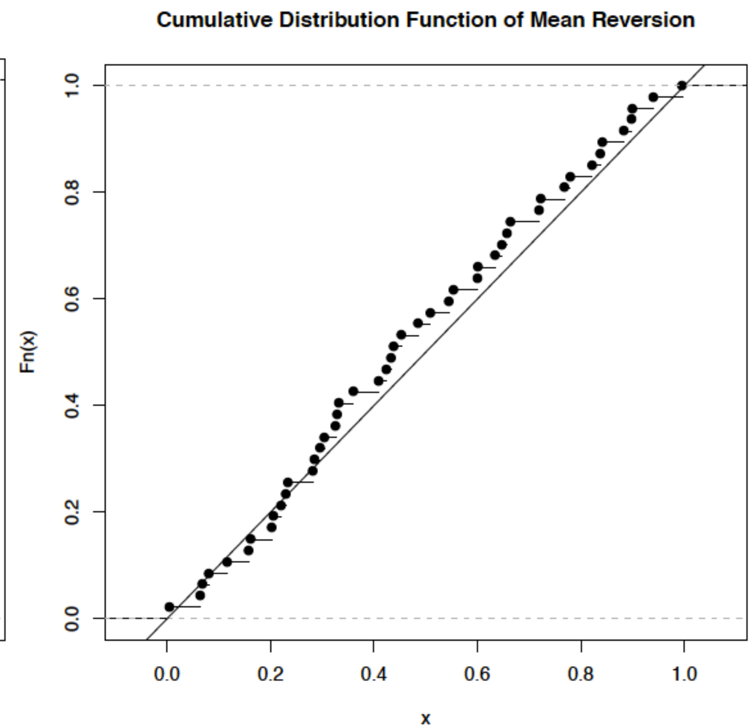


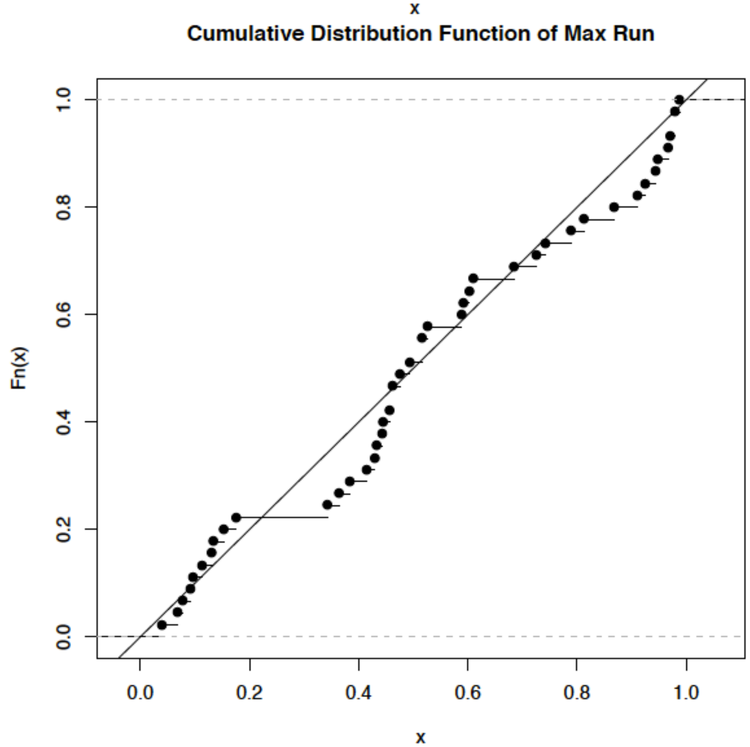
Note: This figure summarizes death sentences, executions and absences by British Army division. The x-axis the number of death sentences passed in a division, while the y-axis is the count of executions. Each division is labeled with its actual divisional number. The diameter of the circle around each 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







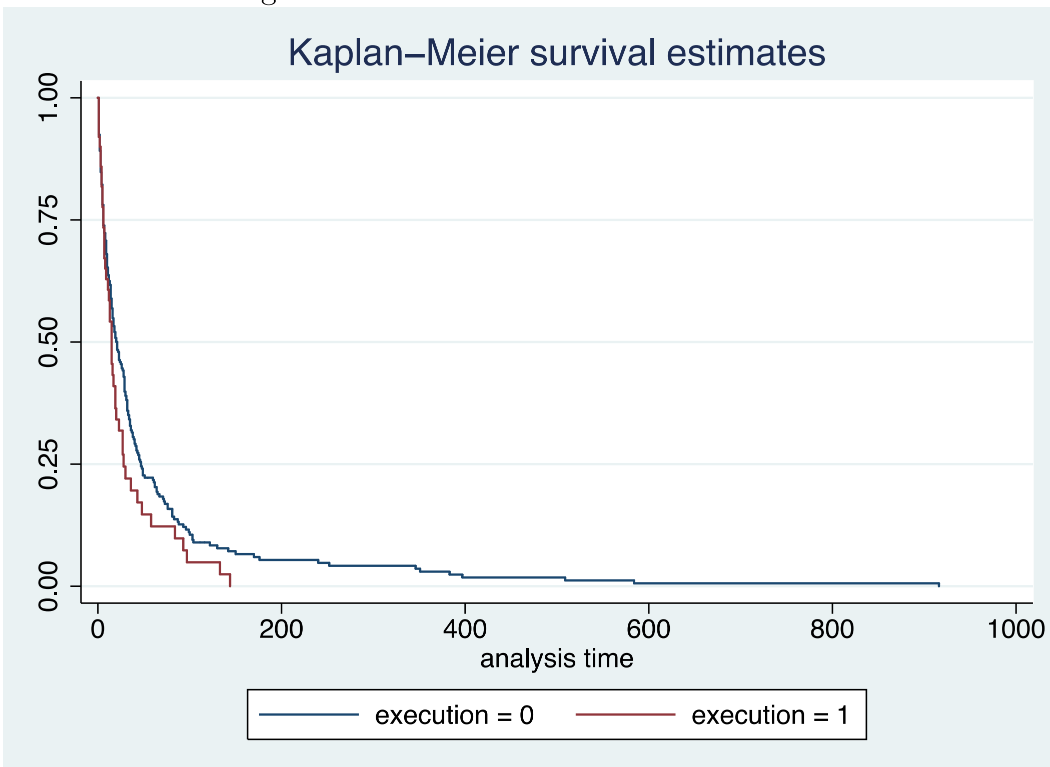


Kolmogorov-Smirnov Test - Randomness Check of P-Values

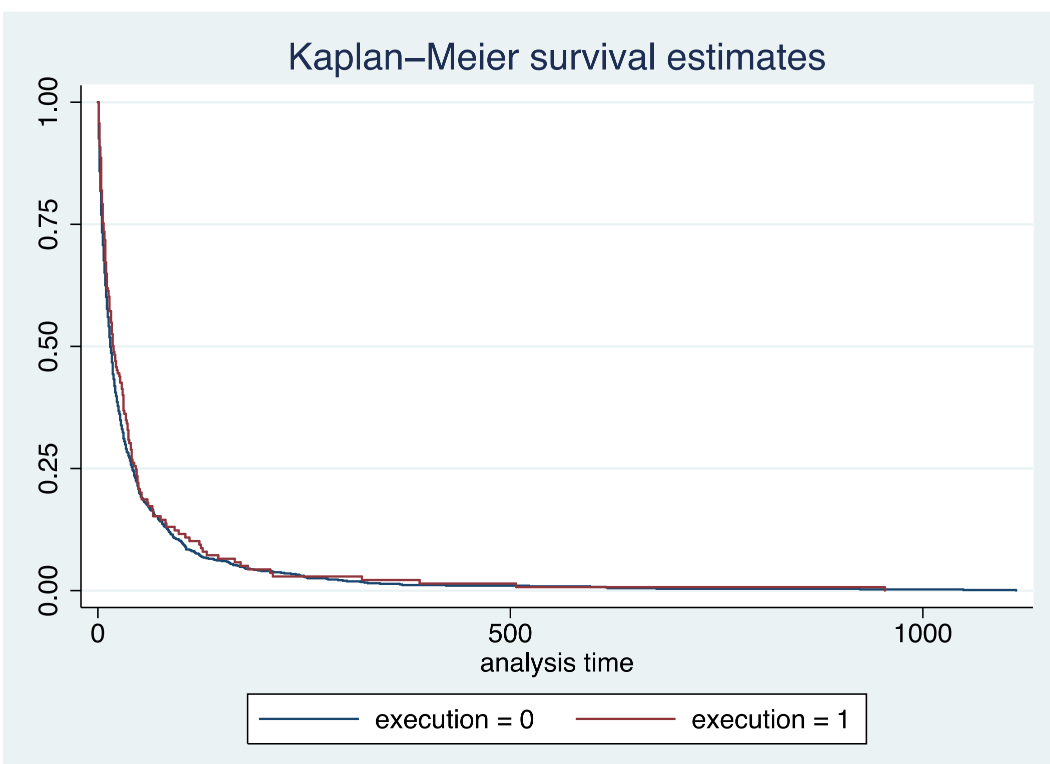
	Distance	Size	90%	95%	99%
Autocorrelation	0.1403913	46	0.1545	0.1767	0.2195
Mean Reversion	0.08068085	47	0.153	0.1748	0.2171
Longest Run	0.12611111	45	0.1562	0.1786	0.2219

Figure 4

Figure 5: Non-Parametric Survival Distributions

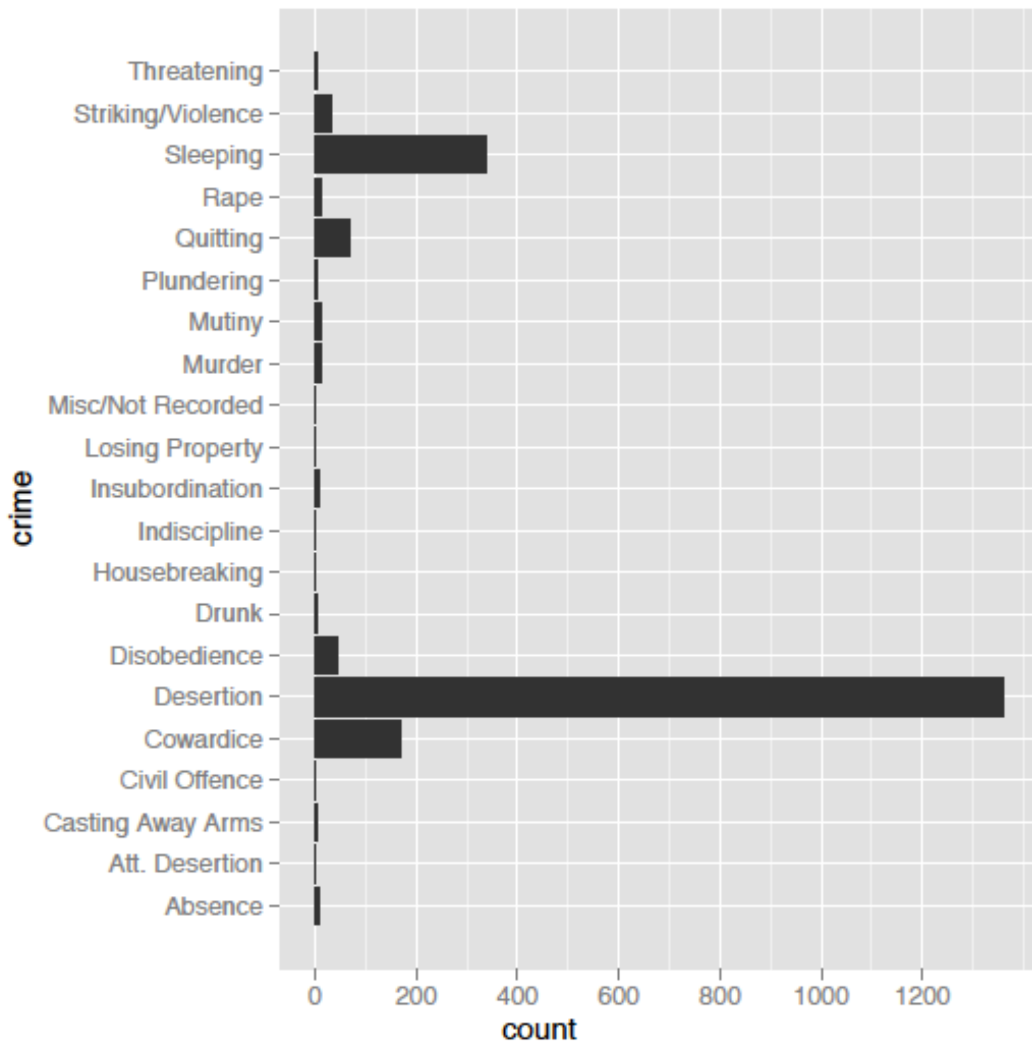


(a) Anti-Deterrence Effect, Irish Executions Only



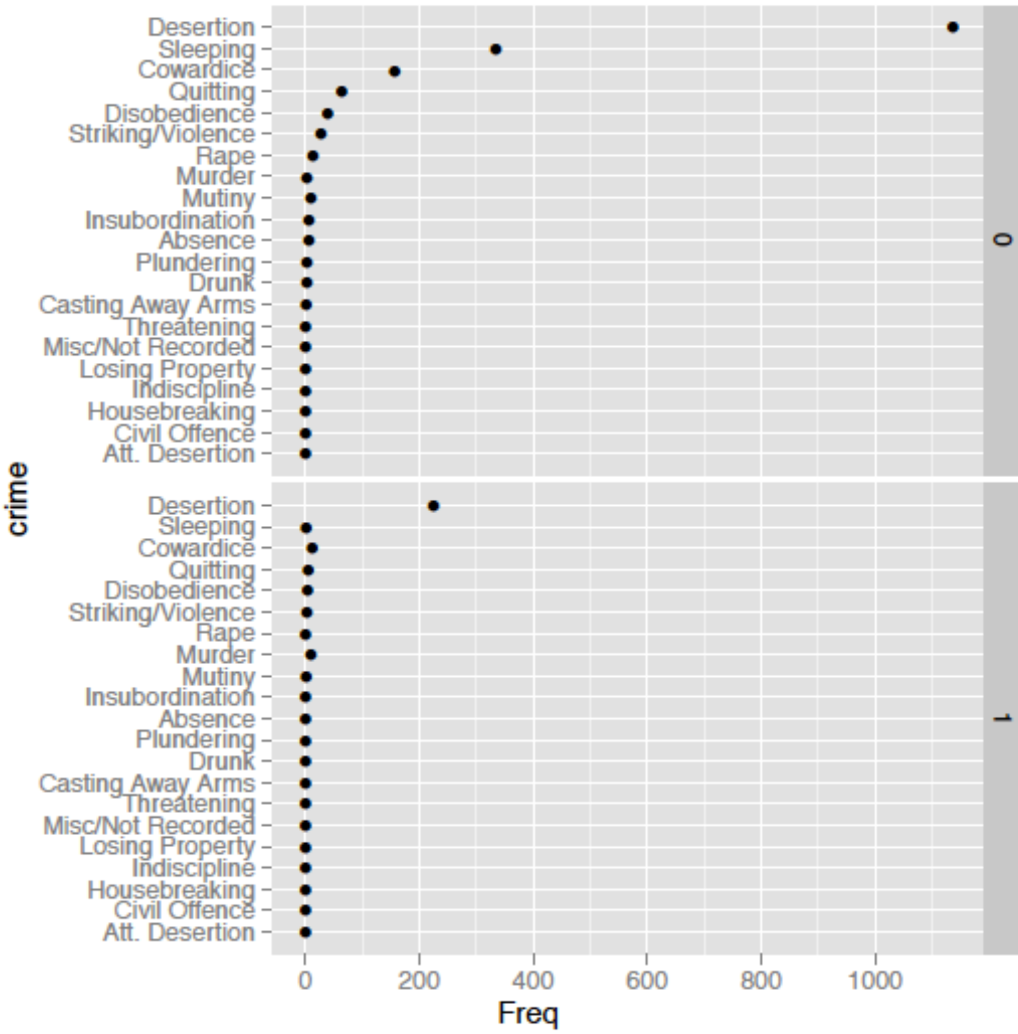
(b) No Deterrence Effect, Non-Irish Executions Only

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

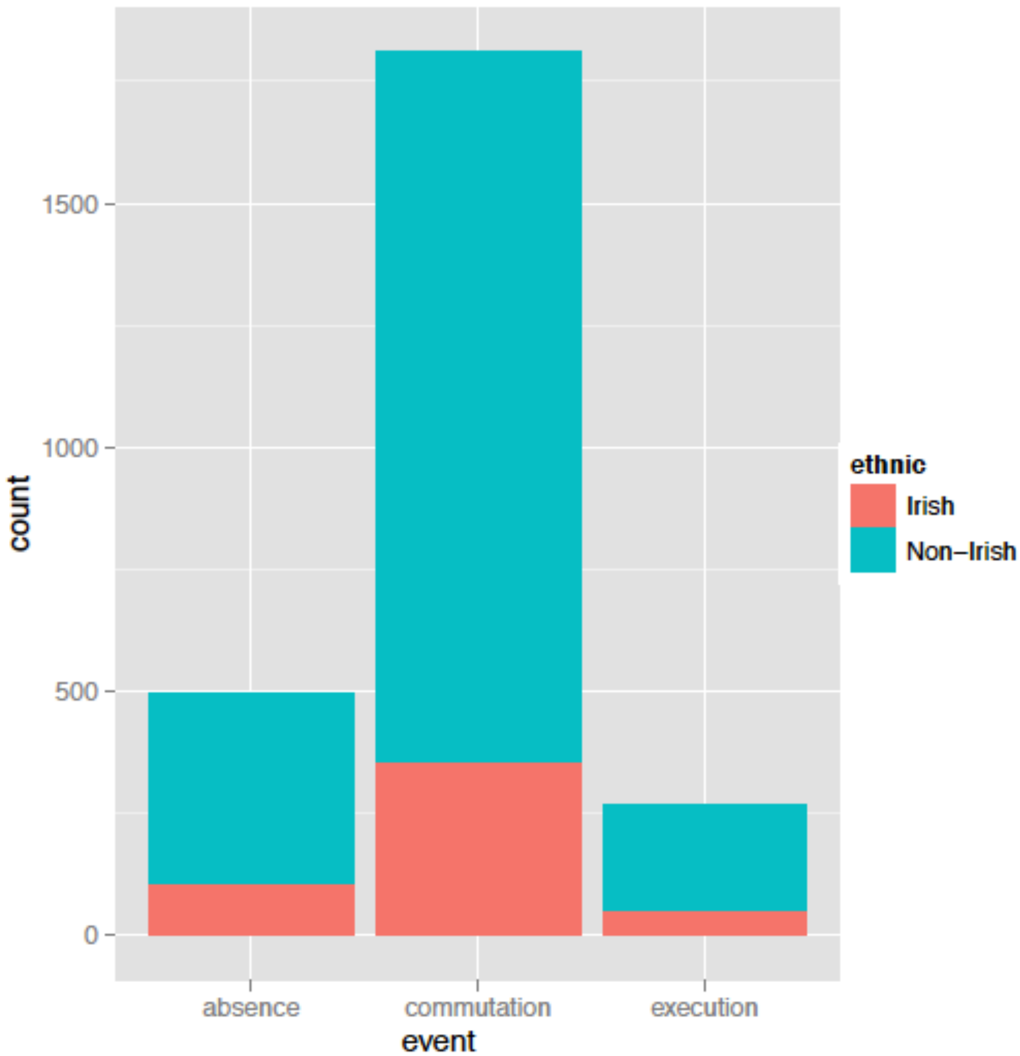

1500
1000
500
0
count
absence
commutation
execution
event
ethnic
Irish
Non–Irish

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) Exp/+14	(2) Wb/+14	(3) Cox/+14	(4) Exp/NN	(5) Wb/NN	(6) Cox/NN	(7) Exp/C=T	(8) Wb/C=T	(9) 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 $\Longrightarrow$ 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) +14	(2) +14	(3) +14	(4) NN	(5) NN	(6) NN	(7) T=C	(8) T=C	(9) T=C	(10) +14	(11) +14	(12) +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) est1	(2) est2	(3) est3	(4) est4	(5) est5	(6) est6	(7) est7	(8) est8	(9) 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.