THE DETERRENT EFFECT OF THE DEATH PENALTY? EVIDENCE FROM BRITISH COMMUTATIONS DURING WORLD WAR I

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Abstract During World War I, the British military condemned over 3,000 soldiers to death, but only executed 12%; 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 handwritten trial records and "wanted" lists prepared by British military police units searching for deserters and preserved in war diaries and police gazettes. I find some limited evidence that executing deserters deterred absences, while executing Irish soldiers, regardless of the crime, spurred absences, particularly Irish absences. I present a model where perceived legitimacy of authority affects why people obey the law.

Keywords: Compliance, Legitimacy, Deterrence

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1 Introduction

Whether the death penalty deters is a classic legal and economic question that remains unanswered. Despite decades of empirical research, there is little convincing evidence that the death penalty deters any form of misbehavior (Nagin and Pepper 2012; Ehrlich 1975; Donohue and Wolfers 2005). What makes the absence of evidence intriguing is that economic theory makes an unambiguous prediction: raise the cost of some activity, then see a decrease in its incidence, be it illegal parking, homicide, or military desertion. The great econometric challenge of death penalty research is that the manner in which the death penalty is applied makes it difficult to draw definitive conclusions. Jurisdictions where 60% of the world's population resides and where death penalty is allowed arguably differ from those that do not in important ways that have independent effects on the level of crime. Disentangling the effect of the death penalty from other confounding socio-economic factors is challenging. Despite these empirical difficulties, whether the death penalty deters crime is in principle an answerable question. In an interview with the New York Times, a skeptic of existing empirical death penalty research, stated, "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."[1] Such a scenario is (thankfully) unlikely to occur, but the British Army experience during World War I may have been a close approximation: a large number of soldiers had their death sentences carried out or commuted for seemingly arbitrary reasons despite having committed essentially identical crimes. This paper uses the quasi-random application of the death penalty during World War I to 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 several advantages for the identification of any effects. First, a relatively clear source of high-frequency variation allows causal inference. Second, the executions were designed for maximum deterrence: they were immediate, brutal, and

[1] *Does Death Penalty Deter? A New Debate*, N. Y. Times, Nov. 18, 2007.

often carried out by fellow soldiers from the deserter's battalion. The promulgation of the death sentence and the execution would occur on the same day, with many soldiers being paraded in front of and shot by their comrades (Putkowski and Sykes 2007, p. 18). In contrast, contemporary executions are exacted after lengthy appeals and conducted basically in private (Katz et al. 2003). Third, many of the military crimes occurred away from the front line (deserting, quitting, sleeping, etc.; Peaty 1999, p. 200, 201, 206) and may be arguably more rational than crimes of passion, such as murder, that are the typical subject of study. Fourth, the present analysis involves comparable military units spread out across the trenches in France and Flanders instead of heterogeneous jurisdictions, such as Texas and Massachusetts. Fifth, the existence of a large, subordinated minority—the Irish—allows identification of the role of legitimacy in non-compliance to the law. Like minorities elsewhere (Donohue 2013), Irish soldiers were disproportionately sentenced to death. They were explicitly denegrated by their British counterparts (Oram 1998, 2003, pp. 9-10), who called them inferior and degenerate. By estimating the separate effects of English and Irish executions on subsequent desertion, the data can query whether deterrence alone drives behavioral responses to the law and the potential relevancy of considerations such as perceived legitimacy of the lawgiver in why people obey the law (Tyler 2006; Tyler and Huo 2002).

Section 2 provides background on the courts martial, executions, and desertions. British military officers were convinced of the deterrent power of the death penalty (Oram 2003, pp. 39, 69; Putkowski and Sykes 2007, p. 11; Babington 1983, p. 19). Over 3,300 soldiers received a death sentence, but the Commander-in-Chief executed only a fraction of condemned soldiers and returned the remainder to the trenches. Soldiers whose lives were spared normally 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; Oram 2003, p. 69). The lower panel of Figure 1 shows a plot of the distribution of death sentences and their resolutions over the course of the war (commutations—indicated by upward-

pointing tick marks—and executions—indicated by downward-pointing tick marks—were spread throughout the period). Historians believe that there were two reasons for this restraint: (a) commanders were reluctant to execute soldiers who might still make some contribution to the war effort (Oram 2003, pp. 2, 4; Moore 1975, p. 70) and (b) commanders were sensitive to political pressure and were concerned about popular anger back home.[2] These two concerns, balanced against the desire to deter desertions, led to a fairly constant execution rate of around 12%—an almost literal decimation—across divisions and across time. This decimation can be seen in Figure 2, which shows each division, represented by a circle, where the number of death sentences is on the X-axis and the number of executions is on the Y-axis. The divisions roughly line up along the 12% line. The decimation can also be seen in Figure 3, which shows the constancy in the execution rate over time.[3] These two figures are, as a first pass, on par with historians' assessment that the decision to execute or commute was a pitiless lottery.

Section 3 models the soldiers' reactions to executions. Military officers made certain that soldiers knew that desertion was punishable by execution (Appendix Figure 1). The emotional impact of watching fellow soldiers be paraded for the military crime and executed by a firing squad of their fellow soldiers, usually from that soldier's same unit, is recorded in the diaries, letters, and memoirs of that time. In the model, observing the execution can update a soldiers' beliefs about the probability of execution and also affect the perceived legitimacy of the lawgiver. Legitimacy plays a role in why people obey the law and is defined as the relative morality for following the law vs. not following the law. Would-be deserters weigh the benefits of military desertion against *economic costs*—loss of freedom, money, and physical well-being or death—*social costs*—such as shame (Beckett

[2]During the war, under questioning by Parliament, the Under-Secretary of State for War, Harold Tennant, stated: "It is not in the public interest to give statistics" for those shot overseas. Hansard HC Deb. vol. 72, col. 1935, 01 July 1915. Retrieved from http://hansard.millbanksystems.com/commons/1915/jul/01/courts-martial-death-sentences

[3]At the very beginning of the war, the rate of execution was higher because of the small number of death sentences, and at end of the war, the execution rate was lower. The execution rates are smoothed using a generalized additive model (penalized splines). Alternative methods of smoothing yield qualitatively smiliar findings.

and Simpson 1985) and loss of reputation—*and psychological costs*—the pain from violating one's duty or moral principles (Chen and Schonger 2013). Absent social/psychological costs, a soldier only motivated by economic costs will be less likely to desert following an execution. However, social and psychological costs can potentially outweigh the economic costs and sanctions. The model builds on Bénabou and Tirole (2012), which models sanctions as conveying information about the number of rule-violators in general equilibrium. A fully rational sanctioner accounts for shifts in perceived legitimacy when optimizing legal compliance, but backlash can arise if the sanctioner lacks full information and miscalculates soldiers' social/psychological costs (Chen and Yeh 2014c). This paper extends the Bénabou and Tirole (2012) model to two groups with different conceptions of duty (Chen and Yeh 2014a). With two groups, this model yields backlash by one group and compliance by the other, even with a fully-informed sanctioner. Shifts in beliefs about the number of rule-violaters erodes the legitimacy of the authority. If the social/psychological costs fall far enough for one group, then those who desert might be admired for refusing to submit to an unjust regime. This may be one of the reasons why we regard "lawbreakers" such as Nelson Mandela, Rosa Parks, and Sophie Scholl as heroes. Their apprehension and treatment by authorities contributed to the collective view of the unjustness of the legal authorities they resisted. This may also be a reason why minorities may perceive the law or lawmaker to be illegitimate (Tyler and Blader 2000; Fagan and Meares 2008), and why insurgencies may flourish when the crime is "resisting the state" and the punishment is more raids and collective punishment. 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. The period before Ireland declared independence[4] is likely one where many Irish people would have felt less loyalty to fight for the British. Of course, not all Irish would have felt less loyalty, and religion may have been an important distinction. However, my data does not distinguish between Irish Catholics and Irish Protestants, but

[4]The Irish War of Independence occurred from 1919 to 1921. Ireland declared independence in 1922.

I provide in Appendix B an extended investigation of the data that includes birthplace. I find that soldiers born in Northern Ireland were more loyal to the British than those born in the South, but were less loyal than soldiers born in Britain. All groups of Irish were subordinated minorities and the British commanders made no distinction (Oram 1998), so for expositional simplicity, "Irish" refers to both groups in this paper.

Section 4 describes the ten data sources whose analysis is made feasible by the confluence of several geographic, historical, and political factors. For British forces fighting on foreign lands geographically separated by water meant that virtually all deserters were caught. The historical progression towards more humane forms of punishment (branding and flogging were outlawed in 1879) 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; yet, changing political mores and military necessity forced officials to confirm only a small fraction. An empire near its peak kept relatively well-preserved administrative records (and, unlike the records of France and Germany, were not largely destroyed in World War II[5]), which this paper digitizes and links together. Court martial records were not released until 75 years after the war, but in the decade since their release, historians have pieced together the universe of death sentences, executions, and commutations (Oram 2003). This data is linked to lists of absentees collated after roll call every morning or more frequently and circulated among the military police searching for deserters. I measure absences using lists preserved in (1) surviving war diaries from France and Flanders, (2) Police Gazettes published in the U.K., and (3) handwritten Field General Court Martial registries of apprehended deserters. These lists come from the British National Archives. These lists are digitized and linked by unit and by date using the (4) Order of Battle,[6] which is digitized and hand-entered to obtain troop movements

[5]Conversations with Putkowski during November 24-26, 2011. As far as I am aware, there is one analysis of German desertions contained in a study comparing the British and Germany experience (Jahr 1998), but there are no lists of executions and commutations along with dates, no lists of absentees and their dates and locations, and no lists of the location or military units of the executions and commutations for other countries.

[6]Order of Battle. The Long, Long Trail. Website created and maintained by Chris Baker. Retrieved from

along with the organization of lower level units (e.g., battalion) within higher level units (e.g., division) by date. The linking process is described in Appendix A. I also employ a (5) database of all casualties (I refer to "casualties" and "deaths" interchangeably: No database is available to analyze injuries.) to measure point-in-time combat danger of each unit. The five-volume Order of Battle of Divisions provides (6) data on commanding officers at each unit level and date and identifies (7) battles that each division participated in. I use these battle locations to approximate the location of each military unit by date. Additional covariates, such as soldiers' birthdates, enlistment dates, enlistment location, and birthplace, are preserved in the (8) surviving portions of the Service and Pension databases, which is linked by soldier's name to the other datasets. I use a (9) dictionary of Irish surnames to identify soldiers of probable Irish ethnicity. Though the use of surnames introduces the risk of incorrectly identifying who is Irish, this measurement error would tend to bias any estimated effects towards zero; differences in the true causal effects of British and Irish executions would be larger than what my analyses yield. Finally, I assess the validity of the surname dictionary by using (10) the Medal Rolls Index, which contains virtually all soldiers who served, to compare against the official statistics regarding Irish enlistment. As shorthand, I will also sometimes interchange the term, "Irish soldiers," for "soldiers with Irish surnames" or "soldiers with male Irish ancestry". I will also interchange "British" with "non-Irish", but–to be clear–this means soldiers who lacked Irish surnames in my data. Some historical references, like national statistics, quote statistics on Irish soldiers that refer to Irish birthplace while others refer to Irish regiments. The historical references are not always clear when "Irish" refers to Irish regiments, which can only be ascertained from following the historical citations to the original source. Appendix B explores in detail the distinctions and delineates the definition of "Irish" between surname, birthplace, and regiment.

To examine whether executions deterred desertions, I adopt the language of potential

http://www.1914-1918.net/oob.htm.

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. Historians believe this strong randomization occurred, describing the process as a pitiless and arbitrary lottery. With over two death sentences per day, it was unlikely for the commander-in-chief or his staff to pore over the details of each and every case and make a judicious decision on whether to execute or commute based on the individual merits of the case. Others, however, are doubtful (Putkowski and Sykes 2007, p. 12, 18). If the commutation decisions were non-random, the non-randomness is possibly 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 (Putkowski and Sykes 2007, p. 17; Putkowski and Dunning 2012, p. 212). Military historians such as Julian Putkowski 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 of both the Allies and Central Powers, 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 (Oram 2003, pp. 9, 56, 61, 74).

Section 5 presents an assessment of non-randomness. First, it examines whether the information about individual condemned soldiers (e.g., Irish ethnicity, rank, age) and en-

vironmental factors (casualties, military indiscipline, recent death sentences or executions, season, army type, commanding officers' identities or ethnicities, timing with respect to major battles, and distance to the coast or to Germany) can predict the commutation decision. Second, it tries to detect non-randomness in the sequence of commutation decisions within a division by examining how similar the string of decisions is to a random string of 1s and 0s. These tests assess whether commanders targeted units for long runs of executions due to perceived indiscipline, whether commanders felt certain units were "due" for an execution or became more lenient after an execution, or whether executions were autocorrelated due to lower level units (e.g., "groups of bad apples") generating a disproportionate share of desertions and executions. These assessments do not reject the historians' conjecture that the death sentences were carried out like a pitiless lottery.

Section 6 addresses a second empirical challenge beyond non-randomness—the within-unit design means that each division is essentially serving as its own control. This method is problematic if past events in a unit's history 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. The first set of analyses assume a strong form of SUTVA where only the most recent event matters. In psychology, saliency of recent events has been attributed to the availability heuristic (Kahneman 2011) and, in economics, individuals have been modeled as having rational inattention (Sims 2003). The second set of analyses parametrically models the effects of previous events and explores whether or not the results are robust to the inclusion of prior events in the model specification. Further, the distribution of past events also represents exogenous shifts in the execution rate, which can be used to separately estimate the impact of the execution rate from the impact of an execution salience. Random variation in the sequence of commutations creates exogenous temporal variation in how strict the death penalty is applied, which allows characterizing separately the deterrence effects of execution salience (of the most recent event) from the

deterrence effects of the execution rate (across many events)–the latter is the subject of most studies in the existing literature. The third approach uses a day-by-day maximum likelihood model of absence, where each unit has some probability of experiencing absence on any particular day and I model how this probability depends on all previous death sentences, their outcomes, and distance in time. This approach combines the effects of salience and execution rate.

Section 7 presents robust evidence that executing Irish spurred rather than deterred absences. Some specifications show that executing deserters deters subsequent desertion: a deterrence effect of the execution rate is observed and appears strongest when execution rates are calculated assuming a half-life of the impact of an execution to be one month after the event. An increase in casualties in the past month also spurs desertions. In terms of magnitudes, the effect of executing Irish is roughly five times the size of the increase in log casualties in one month compared with the prior month. These patterns hold across datasets and are somewhat larger in the War Diaries dataset, which perhaps comprise the most accurate point-in-time measure of military indiscipline, mostly from July 1916-June 1917. An assessment of comparative magnitudes, however, needs to consider that casualties were not exogenous and were somewhat serially correlated. In this dataset, after Irish executions, 19% of the immediately following absences were Irish whereas after a British execution, 11% of the next absences were Irish. After commutations of either ethnicity, 13% of the next absences were Irish. These patterns are more muted in the Police Gazettes and FGCM trial registries, as trials could have been conducted in batches. The day-by-day framework also yields robust evidence that executing Irish deserters spurred Irish absences and mixed evidence that executing British deserters deterred British absences. These effects are strongest in specifications that assume the half-life of an event's impact is about 1 month. Placebo tests involving running the specifications backwards in time show that executions did not have an impact on overall absences or Irish absences before the execution. These inferences are also robust to the level of clustering of standard

errors.

The differences between this study and contemporary criminal justice scenarios are vast, so a more nuanced understanding of the differences is required to draw any policy lessons. One way to interpret the British experience in France and Flanders during WWI is that it provides an extremely low-bar test for the death penalty. Finding a deterrence effect in this context would certainly not be a strong argument, leaving aside moral issues, that the death penalty is good policy. However, a negative result showing no deterrent effect might have more policy salience since if we ever expected to find an effect, it would be in the WWI context: Executions took place almost immediately in a manner purposefully designed to maximize their deterrent effect and death sentences were given out very frequently and quite arbitrarily. Desertion is also certainly not analogous to murder, and criminals weighing the pros and cons of some potential homicidal undertaking are certainly different from soldiers weighing the pros and cons of military desertion during war. However, we would still expect that on the margin more executions should deter absences and if we find this not to be the case, it would suggest that the threat of future death for crimes is not as strong a disincentive as we might imagine.

Despite these prima facie differences, this study offers some insights potentially capable of greater generalization. The granularity and richness of the data begets questions that are sometimes ignored in the standard time-series crime rate studies. A large empirical death penalty literature summarized critically by a U.S. government task force (Nagin and Pepper 2012) and by researchers probing the sensitivity of estimates of the deterrent effect of the death penalty (Donohue and Wolfers 2005; Cohen-Cole et al. 2009; Manski and Pepper 2011) has emphasized that death penalty research should focus on *perceptions of risk* of criminal sanctions (Apel and Nagin 2011; Lochner 2007; Sah 1991): The British experience presents a setting with exogenous shifts in risk perceptions, so one focus of the historical research is soldiers' risk perceptions of capital punishment and how the military heightened these risk perceptions for potential law-breakers. Commutations were

not promulgated and trials were not public. Even if news of commutations eventually became public knowledge–an assertion about which there is conflicting historical evidence–since commutation dates were unknown to soldiers, commutations can still serve to control for factors associated with death sentences and desertion. In addition, the heightened salience of an execution relative to a commutation meant that soldiers would be more likely to update their beliefs in response to the execution (Hertwig et al. 2004).

This paper also contributes to a large literature on iatrogenic/counterdeterrent effects. Higher rates of incarceration among minorities have been attributed to the perceived illegitimacy of authority (Tyler and Huo 2002; Bowers and Pierce 1980; Bailey 2006). However, the correlation between minority incarceration and mistrust of institutions is difficult to interpret causally because punishment is not equal across groups: Incarceration may be higher because of unequal punishment rather than perceived illegitimacy of the law-giver. In addition, crime rates may be higher for unobserved reasons, so attributing causality from perceptions of authority to crime rates faces the econometric challenge of controlling for factors that correlate with both crime rates and perceptions of law's legitimacy. The quasi-random application of the death penalty across British and Irish soldiers addresses both issues. It holds punishment constant conditional on the death sentence and it controls for unobserved selection as similar soldiers were selected for execution or commutation. Furthermore, an execution provides an exogenous shift in perceived legitimacy. As to evidence that the punishment was constant to a first approximation, Appendix B reports that while soldiers with Irish surnames were deserting at a higher rate relative to those with British surnames, the fraction of Irish surnames among soldiers who deserted was the same as the fraction of Irish surnames among soldiers who received the death sentence (which was also the same as the fraction of Irish surnames among the executed soldiers), which suggests that the probability of execution conditional on desertion was similar for British and Irish soldiers.

This paper is also methodologically related to a literature to identify causal effects

using commuted prison sentences (Drago et al. 2009; Kuziemko 2012) and commuted birth sentences (abortions) (Donohue and Levitt 2001). However, Drago et al. (2009) and Kuziemko (2012) use *one-time* mass commutations to estimate deterrence effects for prisoners who have *already committed* a crime and whose sentences were commuted. Donohue and Levitt (2001) uses the staggered introduction of abortion laws (*repeated* mass commutations) to estimate the impact of unwanted children on their subsequent crime. In contrast, this paper uses *repeated*, *quasi-random* commutations, to estimate *general deterrence* for individuals who typically *have not yet committed* a crime–rather than specific deterrence for individuals who are the subject of the commutation.

Finally, social scientists and political philosophers have long speculated on the role of legitimacy in judicial institutions (Gibson et al. 1998), organizations (Suchman 1995), and nations (Lipset 1959). Many countries also struggle with non-compliance or backlash to state laws (Acemoglu and Jackson 2014; Chen et al. 2014a; Chen 2004). Previous papers in economics use theoretical models (Bénabou and Tirole 2011; Kaplow and Shavell 2006; Hurd 1999) and experimental games to suggest that perceived legitimacy of authority (Tyler 2006; Bohnet et al. 2001; Bohnet and Cooter 2003; Vertova and Galbiati 2010; Feld and Tyran 2002; McAdams and Nadler 2005, 2008) and duty (Chen and Schonger 2013, 2015) may be an important determinant of norm-abiding behavior, while this paper offers causal identification in the field on the role of legitimacy of authority in compliance to the law.

2 Historical Background

This section provides historical discussion that motivates the theoretical model and empirical framework. I focus my discussion on France and Flanders, where the majority of total soldiers employed abroad and the majority of courts martial discipline—322 of 346 soldiers executed during World War I—were located (The War Office 1922, p. 648). I begin with the general features and historical context of the British death penalty during WWI. I then describe the processes in which absentees are recorded and apprehended; ap-

prehension leads to trials for absence or desertion; trials for desertion lead to convictions; convictions lead to death sentences; and death sentences lead to executions or commutations (see Figure 4 for a flowchart illustrating the criminal justice procedure). I evaluate the historical evidence for or against the randomness of the execution or commutation decision. I examine the salience of execution to soldiers and their perception of risk. I discuss the Irish. Finally, I present calculations of the costs and benefits to desertion using available historical and administrative data.

2.1 Commanders' Beliefs about Executions Most British military officers from the World War I-era viewed the death penalty as essential to military discipline. Senior officers were, seemingly without exception, death penalty advocates, viewing it as their only recourse for maintaining discipline after corporal punishment, such as branding[7] and public flogging, was outlawed as inhumane in the previous half-century (Oram 2003, p. 38). Sir Neville Macready, a former B.E.F. Adjutant-General, stated "if you abolish the death penalty you might as well abolish the army."[8] General Horace Smith-Dorrien wrote, "There is a serious prevalence of desertion to avoid duty in the trenches, ... and I am sure that the only way to stop it is to carry out some death sentences" (Oram 2003, p. 69; Babington 1983, p. 19). Consistent with this view, Australian forces, who by law could not be executed for desertion (Peaty 1999, p. 210),[9] displayed the highest rate of absences, though of course, other unobservables may play a role. Courts martial records also indicate many instances where military officers wrote, "the state of discipline of this unit requires an example" (Department of Foreign Affairs 2004, p. 38).[10] The military made sure everyone knew about the penalties. "It is well known ... to all soldiers that

[7]Soldiers were branded on their torso with a capital "D" (Oram 2003, pp. 21-26).

[8]Jahr 1998, p. 314.

[9]The death penalty was only allowed for mutiny, desertion to or treacherous dealings with the enemy, but there were no executions.

[10]See also: Brigadier General Douglas-Smith writing, "There are still a few cases of this desertion and the full penalty is the only means by which it can be stopped" (4 Jul. 1915, trial proceedings of Private H. Burden, 1st Northumberland Fusiliers, WO 71/424, Transcription retrieved from https://blindfoldandalone.wordpress.com/the-prosecuted/names-a-g/burden-3832-private-herbert-3832-1st-northumberland-fusiliers/) in a letter recommending the execution of a soldier under his command who had been convicted of desertion.

desertion in the face of the enemy is liable to be punished by death" (Under-Secretary of State for War Harold Tennant, quoted in The Western Gazette, 28 January 1916). In some cases, soldiers who went absent, stayed away precisely because of the fear of being shot (Babington 1983, p. 30; Putkowski and Sykes 2007, pp. 108-111). When recruits joined the army, they were informed that the death penalty could be inflicted upon anyone who deserted while on active service (Moore 1975, p. 50).[11]

Military commanders not only believed the death penalty deterred desertion, but may have acted on those beliefs, using the death penalty in a manner they hoped would forestall desertions (Oram 2003, 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 offensives. If German offensives were not foreseeable to individual soldiers and their officers, then this finding is consistent with an active approach to deterrence by commanders. Oram (2003)'s visual pattern is reproduced in Figure 5, where there appear to be peaks in death sentences before British offensives but not German offensives. However, smoothing the data differently and examining additional battles yields no obvious pattern between death sentences and battles in Figure 1.[12]

Regardless of whether the death sentence was used in a pro-active manner, the *execution rate* does not appear to increase around battles (Figure 3). Nor does there appear to be a consistent pattern between execution rates and the severity of battle. Figure 6 Panel A displays casualties of German-initiated battles in red and of British-initiated battles in blue. Panel B displays all death sentences (red) and all executions (blue) and Panel C smooths the same data. If executions were being used pro-actively, we should expect a sharp increase in executions before the British-initiated offenses. However, the execution rate is fairly smooth throughout the time period.

[11] Peacetime norms of a maximum sentence of two years for desertion had been rendered irrelevant (Bowman 2006, p. 47).

[12] Neuve-Chapelle, Somme, and Third Ypres (i.e., Passchendaele) were British offensives; First Marne, Second Ypres, and Second Marne were German offensives; and Verdun was a major engagement between French and German troops.

Across divisions, death sentences varied, but not execution rates. The Guards Division and Regular Army divisions were considered the elite and professional units, so were used repeatedly in battle.[13] Divisions with good reputations received regular front-line or assault actions, while those with poor reputations were used less often or stationed in quieter sectors (Oram 2003; Department of Foreign Affairs 2004, p. 18). The Territorial Army (which began as local part-time militia and were nicknamed "Saturday Night Soldiers") and New Army divisions were less professional. These units included conscripts after universal conscription came into force in March 1916. In Figure 2, the 12 Regular army divisions are indicated in red circles, the 14 Territorial divisions are indicated in tan circles, and the 30 New Army divisions are indicated in navy circles. As Figure 2 shows, the Territorial Army and New Army divisions received fewer death sentences, but the execution rate was similar.

2.2 Desertions and Apprehensions Absentees and deserters in France were typically arrested within two weeks.[14] 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 an extended period of time, 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.[15]

[13] Conversations with Putkowski and email on November 20, 2011.

[14] Email with Putkowski on January 3, 2008. Jahr (1998, 2014) reaches the same conclusion after analyzing the infantry records of seven divisions. He finds that many soldiers' absences ended after 1 or 2 weeks, but a few soldiers were not caught for at least four weeks.

[15] Email with Putkowski on January 3, 2008.

The high rate of apprehension is consistent with available statistics, which indicate that the number of absentees and deserters is close to the number of trials for absentees and deserters. The desertion rate at home and abroad was 10.26 per 1,000 men,[16] so that in an army of 5.4 million serving in France and Flanders, there were roughly 55,400 deserters. This number may be higher if the desertion rate was higher in France and Flanders than in the U.K. or other theatres of war (e.g., Mesopotamia, Egypt and Palestine, Salonika, Italy, Gaillipoli, and other theatres). The estimated number of deserters may also be too low since not every desertion resulted 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). However, this latter type of undercount would have reduced both the number of official deserters and the number of courts martial equally. On the other hand, the estimated number of deserters may be too high, since the fog of war — stragglers, missing in action, poison gas, prisoner of war — would make it very hard to pin down the true number of deserters. Indeed, courts martial records of deserters include defenses such as: wandering into German trenches, being fired upon overnight and getting separated, an exploding latrine, or oversleeping in a dugout (Department of Foreign Affairs 2004). In addition, officers were required to report absences to the Police Gazette "as soon as it is known that a soldier has absented himself," [17] which may have further increased the likelihood of overcount. The number of absentees and deserters (55,400) would be closer

[16] "Post-war statistics show that the overall desertion rate between 1914 and 1918 was 10.26 per 1,000 men - so that in an army of 1 million men, there were over 10,000 absentees - the equivalent of a whole division of troops. The size of the problem was already becoming apparent by mid 1915, when instructions were issued by the War Office, drawing attention to the fact that there had been 1,251 desertions from the Expeditionary Force and over 20,000 desertions from the new army, reserve and other regular units." Corns and Hughes-Wilson 2007, p. 216. Since there were 146,730 members of the army struck off as deserters between Aug 1914 and March 1920 at home and abroad (https://archive.org/stream/statisticsofmili00grea#page/82/mode/2up), one may infer that Corns and Hughes-Wilson (2007) was referring to deserters at home and abroad.

[17] "When there is good ground for supposing an absentee to have deserted, the report should be rendered within 24 hours after the absence has been discovered, but in no case should it be delayed beyond 5 days." The King's Regulations and Orders for the Army (1914). London: His Majesty's Stationary Office, Para. 514, p. 117. After 21 days, an absentee was presumed to be a deserter and a court of inquiry called regarding "the illegal absence of a soldier." King's Regulations (1914), Para. 673, p. 149.

to the number of trials (44,395) to the extent that the estimated number of deserters is too high, which is consistent with deserters being invariably caught.

Official records indicate 44,395 courts martial for absentees or deserters, of which 7,361 were for desertion and 37,074 were for absence (The War Office 1922, p. 667). Focusing on the question of apprehension (and ignoring the distinction between absentee and deserter trials until the next sub-section), the figure of 44,395 suggests that at least 80% of the deserters were caught.[18] This percentage may be even higher if the number of courts martial is too low relative to the true number of deserters caught due to prosecutorial discretion. There is no direct evidence of officers actively concealing absences, though there is evidence that whenever possible, commanding officers did not report minor infractions outside their regiments (Corns and Hughes-Wilson 2007, pp. 88-90; van Emden 2010, p. 4259 of Kindle e-book).[19] If a deserter was captured far from his unit (e.g., by military police or an officer from another unit), a report would be sent to the commander of the division, and the resulting publicity would require the lower level commander to "acknowledge publicly his soldiers' crimes or to make an example" (Corns and Hughes-Wilson 2007, p. 89). Other historians are doubtful that officers would conceal absences: officers were certainly not going to record or allow anyone to accuse him of "bending" the rules because they would lay themselves open to being charged with an offense of not obeying routine orders and the Army Act. Absence was formally noted in the company disciplinary book and would have been apparent in parades. There were circumstances when a soldier's excuse was accepted (e.g., illness) but it was entirely up to the unit's commanding officer

[18] 44,395 / 55,400 = 80%.

[19] The commanding officer of a unit had wide discretion in any matter of discipline. Under Section 46 of the Army Act, the commanding officer could often deal with a case summarily by selecting a charge that was within his powers to punish (such as "Conduct to the Prejudice of Good Order and Military Discipline", Graham-Harrison 1907, pp. 298-299 (Army Act Section 40), Retrieved from http://archive.org/stream/manualofmilitary00greauoft#page/298/mode/1up), and imposing up to 28 days detention, Field Punishment, or forfeiture of pay, rather than escalating the matter to a court martial. Selective prosecution, whether due to officer whim (Sheffield 1996; Burrage 1930; Turner and Haigh 1969) or limited bandwidth before or after a major battle, have led some to describe military discipline as "negotiable" (Rubin 2013). However, email with Putkowski on February 20, 2015 suggests that commander discretion was limited to trivial violations such as a late return back from a pass, drunken behavior, dirty rifle, or being unshaven on parade.

and the latter had to bear in mind that appearing "soft" would adversely affect his own military reputation and promotion prospects.[20]

In sum, the historical record and the official statistics are consistent with deserters being invariably caught. To put this in perspective, only 40% of deserters during the U.S. Civil War were caught and deserters faced a negligible risk of death if arrested (Costa and Kahn 2003). So, perhaps it is not surprising that 14% of Union army soldiers deserted during the American Civil War compared to 1% for the British Army during World War I. During World War 2, British Army deserters were not subject to the death penalty, and the desertion rate was so high that the Army wanted to reintroduce the death penalty in 1942 but could not because of political considerations (Bond et al. 2010, p. 213).

2.3 Trials Most desertions in France and Flanders were dealt with by 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 non-officers. The FGCM was comprised of at least three officers, the president holding the rank of major or above. The court 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 (Graham-Harrison 1907). Courts martial in the field took place in private (Babington 1983, p. 13), even though they were theoretically open to the public. Private trials thus left the typical soldier with little news about death sentences or about deserters until an execution was promulgated. Soldiers who deserted in the U.K. while their unit was based in France and Flanders would be returned to their unit and tried by FGCM.[21] Typically,

[20] Email with Putkowski on February 20, 2015.

[21] In addition to the GCM and FGCM, there was also the District Court Martial (DCM), which handled desertions and AWOLs (absence without leave) in the U.K. for (a) draft dodgers (after the U.K. began conscription) and (b) those who deserted while their unit was based in the U.K. Including these individuals, the total number of soldiers and officers tried for desertion or absence at home or abroad was 126,818 from August 4, 1914 to March 31, 1920 (The War Office 1922, pp. 83-89). The 31,390 desertion trials and 51,249 absence trials by DCM, are *not* the subject of the present analysis. The DCM could only impose a maximum sentence of two years of imprisonment, unlike the FGCM and

these soldiers had failed to return to the front after furlough[22] or after convalescence in the U.K.

Convicting a soldier for desertion required showing of *intent* (Corns and Hughes-Wilson 2007, pp. 44-5). Intent to desert would be presumed if the soldier had been absent for 21 days or if there was other evidence to indicate intent of not returning (e.g., wearing civilian clothes or failing to report for a key deployment).[23] However, "in any case of doubt as to whether [desertion or absence without leave] has been committed, the court should find the accused guilty of the less[er] offence."[24] The offense of absence did not typically receive the death penalty.

Prior to the data entry commenced in this project, historians were unsure as to whether soldiers convicted for desertion were invariably sentenced to death in FGCM trials.[25] Only Jahr (1998)'s statistical analysis of 7 divisions (analyzing the same data source that is digitized for this paper) found that all FGCM trials for desertion resulted in conviction, but not every trial resulted in a death sentence.[26] My analysis of all 144,609 FGCM trials, of which, 13,309 are for desertion, is consistent with Jahr. All but 3% of soldiers tried for desertion were convicted, but a small fraction (13%) received the death sentence. Bear in mind that the FGCM trial registrars were handwritten and hand-entered, so the analysis of FGCM may not match the official statistics. Using the official statistics of 7,361 desertion trials, but only 2,004 desertion death sentences, suggests that the true

GCM, both of which could impose the death penalty. Historical discussions of sentences for desertion often do not distinguish between DCM and FGCM trials, making ambiguous statements such as, e.g., 46% of desertion trials in July 1915 resulted in sentences of less than three months (Corns and Hughes-Wilson 2007, p. 216). For example, these discussions cite the War Office statistics, which sometimes did not distinguish between DCM and FGCM trials.

[22]Officers 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.

[23]Someone missing for 21 days was presumed to be a deserter (Putkowski and Sykes 2007, pp. 13-14; King's Regulations and Orders for the Army, 1914, Para. 673, p. 149, Para. 514, p. 117).

[24]Graham-Harrison 1907 III.20, p. 19. Retrieved from http://archive.org/stream/manualofmilitary00greauoft#page/19/mod

[25]Email with Putkowski on February 20, 2015 stating the difficulty in knowing what happened to convicted deserters who were not sentenced to death as no one knows and it is otherwise speculation.

[26]By the end of 1917, sentences of over 5 years of imprisonment constituted an increasing share, while death sentences constitute a decreasing share of sentences for desertion.

conviction rate is somewhere near 100% and the true death sentencing rate near 27%. In comparison, in my data, 449 or roughly 3% of desertion trials resulted in a "not guilty" verdict and 1720 or 13% resulted in a death sentence. In sum, nearly all trials for desertion resulted in conviction but not all convictions yielded a death sentence. Note that what is important for identification of causal effects is whether executions conditional on death sentences are quasi-random, not whether executions conditional on desertions are quasi-random. In my identification strategy, commutations of death sentences serve as a control for factors correlated with the likelihood of a death sentence and desertion, regardless of the percentage of desertions or percentage of convictions that resulted in a death sentence.

Neither soldiers at the time nor British military historians of World War 1 analyzing qualitative records knew about the low rate of death sentences for convicted deserters. There appears to be no public knowledge of lesser sentences for convicted deserters. I found only two pieces of the historical record that are relevant and they have ambiguous interpretation. On a deserter's charge sheet, an officer recommends the death sentence because the soldier had absented himself shortly after another soldier's conviction and lesser sentence was promulgated: "I am firmly of the opinion that the crime was deliberately committed with the intention of avoiding duty on the Redan, more particularly as he absented himself shortly after the case of another soldier had been promulgated for a similar crime. The Officer commanding the man's Company is of the same opinion. Sentence was remitted in the case mentioned to 2 years Hard Labour" (WO 71/450.). However, a conventional reading of the evidence would suggest that what was promulgated was a conviction for *absence* (as opposed to desertion) that resulted in a 2 years hard labor sentence. In a second record, a First Division brigadier wrote: "Every infantry officer of experience will confirm my opinion that there comes a point when men will risk imprisonment or penal servitude rather than carry on their ordinary duty. They know that long sentences inflicted in war are whittled down as they pass up the military hierarchy and that if a sentence is not ended before the end of the war they may look forward to an

amnesty at the end of hostilities" (Babington 1983, pp. 18-19). This quotation does not speak to the question of whether soldiers knew that *death sentences* would be whittled down as they passed up the military hierarchy (and therefore the existence of lesser sentences for desertions). Moreover, this writing occurred before the Suspension of Sentences Act (March 1915) that ensured any *imprisonment* or *penal servitude*, the topic of the quotation, would be served after the war. As for acquittals, they were not published in the general routine orders, nor were they promulgated on parade (Committee to Enquire into the Law and Rules of Procedure Regulating Military Courts-Martial/Chairman Lord Darling 1919, para. 87). Returns would not be formally announced.

A final source is relevant to whether the low rate of death sentences for convicted deserters was public knowledge: "In a trial of a member of His Majesty's Forces, in which a conviction results, the result is always made public by means of the promulgation of the finding and sentence."[27] However, military regulations stated that informing the offender and no one else of the charge, finding, sentence, and confirmation will be sufficient promulgation to satisfy this rule (Army Act s. 53, note). Moreover, it is unlikely that the 130,936 FGCM convictions were circulated to the entire army. First, over 90 convictions a day would be unlikely to be remembered if circulated across the entire B.E.F. Second, my and Jahr (1998)'s findings of the low rate of death sentences for convicted deserters appear not to have been publicly known, suggesting little attention was paid to these convictions even if they were circulated. Third, widely circulating a large number of convictions that did not lead to death sentences would be inconsistent with repeated admonishments that "it should be remembered that on active service the usual penalty is death" for leaving post, cowardice, sleeping on post, and violence to inhabitants among other offenses (GRO signed by Smith-Dorrien, Commander of 2nd Army BEF France and Flanders, February 11, 1915 WO 95/646). Finally, unit orders that dessiminated disciplinary information from higher formations and communicated to lower formations were

[27]Undersecretary of State for War Military Harold Tennant, Hansard HC Deb. vol. 82 col. 2911, 1 June 1916. Retrieved from http://hansard.millbanksystems.com/commons/1916/jun/01/courts-martial-1.

divided into Part 1 and Part 2. Part 1 Orders generally addressed training, parades and the assignment of tasks or duties, including warnings about forthcoming movements or involvement in a raid or attack on the enemy and warnings about behaviour. These were announced on parade and posted on one or more notice boards for all soldiers to inspect. Part 2 Orders recorded everything affecting individual soldier's status, fines; promotion; posting to other formations; promotion/pay - and punishments. Copies of Part 2 Orders of a unit were circulated to the paymaster and the officer in charge of records. A sample of the Part 2 Orders has been transcribed, but most Part 2 Orders were destroyed in WW2 bombings.[28] Part 2 Orders were unlikely to be read in its entirety on a battalion parade.[29] Unit orders that were meant to be confidential were not allowed to be distributed (The War Office 1914a, p. 58, [30]). I have not come across any primary or secondary source mentioning public knowledge of low rate of death sentences for convicted deserters. In contrast to the little surviving evidence of public knowledge of deserters being treated in some manner other than execution, ample surviving evidence from eyewitnesses suggests they had no doubt that deserters really were executed.

2.4 Affirm or Commute the Death Sentence? A soldier's death sentence did not seal his ultimate fate, as each of that soldier's commanding officers (in the battalion, brigade, division, corps, and army) was responsible for submitting his 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 affirm or commute 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, this third item being a

[28] A few samples are available here for the Canadian Expedetionary Force, 21st Infantry Battalion: https://archive.org/details/21stInfantryBattalionPartIiOrders1915-1919. Search for the word "deserter" yields several men "struck off" the rolls after a court of inquiry and a search for "desertion" yields some courts martial sentences and some men put back on the rolls as returned "from desertion".

[29] Email from Putkowski on May 28, 2015.

[30] See also https://archive.org/stream/pt1fieldservicer00greauoft#page/32/mode/2up (The War Office 1914b, p. 33).

necessary ingredient to a desertion conviction (Oram 2003; Department of Foreign Affairs 2004, p. 7). Once the paperwork was complete, including all the recommendations of the soldier's superiors, 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, and my subsequent analysis supports the "pitiless lottery" hypothesis (Babington 1983).

The leading hypothesis, that the Irish were disproportionately targeted 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. I used surname dictionaries to determine who was Irish, which likely results in counting too many soldiers as Irish. However, the inference remains the same because the overcount applies to both death sentences and executions. Figure 2 corroborates the hypothesis that the Irish were not disproportionately executed. The Irish proportion of absences and death sentences is visualized in the green proportion of the vertical tick marks for each division. Divisions that had more Irish are neither systematically above nor below the 12% line.

The second leading hypothesis articulated by historians is that soldiers who previously had a (suspended) death sentence were more likely to be executed. However, in my data, 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 observers (Oram 2003; Department of Foreign Affairs 2004) does not appear in the executions. Officers, who typically came from the elite British public schools (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. Other observers claim the opposite class

bias: Commander in Chief Douglas Haig ordered his inferiors to execute more officers.[31] No significant differences in execution rates for officers are observed among those sentenced to death for desertion. These and additional statistical tests are presented later. Even strict randomness with respect to soldier characteristics is not necessary for the empirical analysis,[32] so long as these reasons to execute were not salient to the decision-making of the individual soldier weighing the decision to desert, then this non-random treatment assignment is irrelevant for the behavioral response I am trying to measure.

Records indicate that decisions to affirm or commute 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 roughly 2 death sentences per day, there would not have been time to read in detail and ponder over each and every case (Oram 2003, p. 55). 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 lower-level officers' opinions on whether to commute or execute. Officers at the corps and army level could seal a man's fate, while lower-level officer recommendations (division and below), whose career concerns created a disincentive to report indiscipline among their troops, were basically ignored (Oram 2003, p.129; Babington 1983, pp. 78-79, 103). 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 also made a recommendation of mercy (Oram 2003, p. 127). The Commander in Chief's disregard of clemency recommendations made by the courts as well as soldiers' immediate commanders (and sometimes even

[31] Burke 2001, Retrieved from http://theguardian.com/uk/2001/feb/11/jasonburke.theobserver

[32] One newspaper article alleges "frontline eugenics" to explain a spike in death sentences of men from the 35th Division, because it was composed of unusually short men (Burke 2001). However, Figure 11 shows that while there was a spike in death sentences in the 35th Division, there was no spike in the execution rate. In any event, hypotheses like these can only be tested indirectly by looking for autocorrelation or runs of executions in particular divisions.

brigade, divisional, and corps commanders, see Babington 1983, pp. 78-79) contributes to the retrospective view that the executions were a pitiless lottery.

Collective pardons or collective confirmations may have been decided jointly and be non-random. Commanders did appear to execute soldiers in pairs, for example, in the cases of two friends deserting together (see Putkowski and Sykes 2007, p. 64). In the data, all executed soldiers whose trials were held on the same day and came from the same division were also from the same battalion, and with one exception, they were also executed on the same day. Among soldiers whose death sentences were commuted but had trials on the same day and who came from the same division, in 70% of these cases, the soldiers also came from the same battalion. Chi-square tests with simulated p-values also reject the hypothesis that decisions regarding death sentences on the same date and in the same division were independent. They also reject the independence of decisions in the rare instances of British and Irish soldiers being sentenced to death on the same day and in the same division.Therefore, based on the assessment of the history and the data, I treat multiple observations of executions (commutations) on the same day in the same division as one execution (commutation) in assessments of serial correlation in execution decisions.

Official policies on commutations or commutation goals, if they existed, do not appear to have been preserved in the historical record (no written or explicit statements); however, it is unlikely there were explicit commutation goals, since there does not appear to be sufficient evidence of coordination of commutation fractions across all theatres of operation or across time according to the data.

2.5 Commutations Soldiers convicted of desertion were typically detained, awaiting final sentence (Babington 1983) or, in some cases, immediately thrown back into the trenches (Oram 2003) with the information that the sentence was being reviewed.[33] The

[33]Major Christopher Lowther (Assistant Provost Marshall 1917-1919, Member of Parliament for North Cumberland 1918-1922), Hansard HC Deb. vol. 127, pp. 1603-4, 13 April 1920. Retrieved from http://hansard.millbanksystems.com/commons/1920/apr/13/new-clause-death-sentences-appeal#column_1603.

final decision to confirm or commute occurred within two weeks of the original FGCM death sentence. The court martial registers of the Judge Advocate General (JAG) did not feature the dates of the announcement of a commutation, and so far, the exact date is unknown. For my analysis, I had to impute the commutation dates: I use the length of time between the death sentence and 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 (to hard labor, penal servitude, imprisonment, being tied to a fixed object (known as "crucifixion") for several hours per day, or reduction in rank) or the sentence was sometimes "quashed" (i.e., vacated). The soldier would be escorted from prison to his unit by military police or a couple of soldiers from the battalion who were picking up reinforcements.[34] Commuted sentences were suspended and served after the war. Military authorities were always very anxious to ensure that either a spell in jail or detention was not viewed by soldiers as a way of avoiding front line service. Through good conduct, soldiers with sentences to be served after the war could reduce or completely eliminate their sentence.[35]

Men were not told immediately what was their sentence – just that they had been guilty. This is supposedly because the sentence only became legal when it was confirmed by the commander-in-chief. Commuted sentences basically slip into the night without many others knowing about them.[36] According to the Army Act, s. 54(3)[37], acquittals were to be read out in open court. However, if he was convicted, a soldier would not know the nature of the conviction (e.g., on the charge of desertion, whether he was convicted for desertion or for the lesser offense of absence without leave) or the sentence until the night before they were promulgated (Babington 1983, pp. 15, 17). The judges serving on a court

[34]Email with Putkowski on November 20, 2011.
[35]Undersecretary of State for War Military Harold Tennant, Hansard HC Deb. vol. 70, pp. 1212-3, 8 March 1915 (available at http://hansard.millbanksystems.com/commons/1915/mar/08/army-suspension-of-sentences-bill).
[36]Email on December 24, 2007 from Gerard Oram.
[37]Graham-Harrison 1907, p. 322, Retrieved from http://archive.org/stream/,anualofmilitary00greauoft#page/322/mode/1u

martial had to swear "not to divulge the sentence of the court until it is duly confirmed"[38]. Officers would likely not have wanted to publicize commutations for fear of subsequent indiscipline in their unit and career concerns due to real or perceived indiscipline. Even if soldiers knew about their commuted sentence, it is difficult to believe that soldiers would want others to know that they were a deserter who received a commuted death sentence. Known deserters would have faced social censure or worse by their comrades in arms. A Regimental Medical Officer wrote in his diary: "To gratify a mawkish humanitarianism two or three score mean fellows are encouraged to slip away every time there is risk to their skins, so more and more average men learn to shirk with impunity; attacks fail, and losses run into untold thousands, because the most dutiful of our men are not backed up" (Dunn 1987, p. 410). One source attests that a convicted soldier would know about their death sentence before the execution or commutation decision: he would receive a secret envelope with the death sentence along with the information that the sentence was liable to revision by higher authority.[39] This would likely have only occurred after April 17, 1918 when the Under Secretary for War, Macpherson, announced that those sentenced to death would be informed of the sentence prior to confirmation, rather than after confirmation, as had been the previous policy (Peaty 1999, pp. 208-209).

Official statistics on death sentences, commutations, and executions were not made public until April 1920 (Corns and Hughes-Wilson 2007, p. 407).[40] Compared to the plentiful primary sources in diaries, letters, and memoirs indicating that executions were known, there is scarce evidence that commutations were known. The two exceptions to the rule are first-hand accounts from the Western Front of announcements of commutations of death sentences (Carrington 1965, p. 128; Arthur 2002, p. 173). Historians are doubtful

[38] Graham-Harrison 1907, pp. 318-9, Retrieved from http://archive.org/stream/anualofmilitary00greauoft#page/319/mode/1up.

[39] Christopher Lowther (Assistant Provost Marshall 1917-1919, Member of Parliament for North Cumberland 1918-1922), Hansard HC Deb. vol. 127, pp. 1603-4, 13 April 1920.

[40] A few months earlier, the Darling Report in November 1919 revealed that 89% of death sentences were commuted but not the total number.

regarding these accounts.[41] The first memoir is not correct in the recollection of crimes and dates.[42] The second account is written in a first-hand perspective but without primary source or interview.[43]

There are some general routine orders that mention death sentences being commuted. General routine orders would be circulated in writing only to officers, usually a Captain, but its entirety was unlikely to be passed on to troops. Important information would be relayed generally via an announcement on a company parade, usually three per day, and written orders were then posted on a noticeboard outside company headquarters or the guardroom.[44] In these orders, threats of execution were explicitly repeated: "for the following offences, it should be remembered that on active service the usual penalty is death" even when commutations were mentioned (GRO signed by Smith-Dorrien, Commander of 2nd Army BEF France and Flanders, February 11, 1915 WO 95/646; Routine Orders signed by Major General H.F.M. Wilson, Commander of 4th Division, BEF France & Flanders, March 23, 1915 WO 95/1449). Thus far, I have not found any general routine order that mentioned a commutation or a lesser sentence for a convicted deserter in primary or secondary sources. Despite the potential for commuted death sentences to be widely known for cases other than desertion, there is nothing written of public outrage over commuted death sentences. Executions were likely the only news about death sentences transmitted to the typical soldier. The fact that there are no dates of commutations preserved in news circulars provides further evidence consistent with there being no public announcement of commutations of death sentences and that commuted sentences

[41]Email with Putkowski on July 10, 2013.

[42]Carrington reports 3 deserters sentenced to death whose penalties were commuted to 3 months field punishment. My data indicates that in Carrington's unit, three death sentences were given out on June 11, 1916, all for Quitting and all commuted to 2 years hard labor. There were three death sentences in other battalions in his regiment that were commuted to 3 months' field punishment, but the death penalties were handed down on February 7, 1915, before Carrington's unit arrived in France on March 22, 1915.

[43]Email with Putkowski on July 10, 2013 indicates that Arthur's account is more literary rather than historical.

[44]Email with Putkowski on May 27, 2015.

basically did "slip into the night".[45]

Gaining any statistical impression about the number of men with capital sentences who were not executed was almost impossible for soldiers. The government tried very hard to keep death sentences quiet, and records were not public for 75 years. There are only soldiers' speculations that soldiers who would otherwise have been executed were instead compelled to take part in the forefront of the first available raid or assault on the enemy. That said, there is no affirmative evidence that commutations were secret. However, even if commutations were somewhat known, if individuals overweigh recently sampled information, then we would expect soldiers to update their beliefs in response to executions. The use of commutations serves as a valid control for executions for statistical inference because the commutation dates were not announced and because of the historical and statistical evidence of commutation vs. execution decision being a pitiless lottery.

2.6 Executions 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.[46] 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). Often, promulgation involved the entire battalion; sometimes other battalions in the brigade would see the execution, but probably not the entire division, whose encampment could stretch for 15 miles.

Executions were usually carried out by a squad from the victim's battalion, often witnessed by the entire battalion or whatever companies were at hand.[47] If the soldier did not die in the initial volley, an officer was on hand with a pistol to provide the coup de

[45]Email on December 24, 2007 from Gerard Oram.
[46]Email with Putkowski on February 4, 2008.
[47]Email with Putkowski on December 24, 2007.

grâce (Department of Foreign Affairs 2004, p. 8). Hearsay, rumor, and newspapers (Sellers 2003) spread the word, once the shocked members of a firing squad shared their feelings with comrades (Corns and Hughes-Wilson 2007). 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.[48] While this presumably weakens any treatment effects, the condemned soldier's fellow soldiers would learn about the execution, even if they did not personally witness it. News about all executions was also formally 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). To the extent that soldiers paid attention to executions elsewhere, this would also tend to weaken the treatment effect since the treatment and control groups become more similar.

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." A witness to another execution wrote, "I witnessed a shooting. . . . It shook me a bit" (Sellers 2003). The number of references to executions in diaries, letters and memoirs is testament to the nature of their impact: "The discipline out here is very severe. Men found absent or drunk or found out of bounds are tried by Court Martial and several men have been shot for straying away from camp. One was shot this morning" (Adamson 1997, p. 23). "The Corporal was shot in Happy Valley. For discipline's sake his whole Battalion was paraded to witness the proceeding. Other Battalions of the Brigade were close by. The Battalion was called to attention, and the firing party were ordered to fire" (Dalton 1986). In some cases, eyewitnesses felt sorry for

[48]Email with Putkowski on December 24, 2007.

both the victim and the firing squad. Eyewitness testimony suggests that even if they did not always impress soldiers in the way the army intended, executions were still salient.

Infrequency of executions likely made them more salient. My calculations indicate that a typical Regular infantry division saw 2.5 executions per year, Territorial Force divisions saw 0.5 executions per year, and New Army divisions saw 1.25 executions per year. Only a small handful of soldiers were involved in the execution or eyewitness to the body. The data does not permit estimating separate deterrent effects on the firing squad, the eyewitnesses, and those who heard about the executions. Despite or because of the small number of executions actually observed by the typical soldier, an officer was quoted as saying, "it was only fear of death that kept them at their posts" (Moore 1975, p. 62).

As a point of comparison, an estimated 2,000 French soldiers were condemned to death, of which roughly 700 were executed, a 35% execution rate. The French army required their divisions to march past the body of the executed soldier.[49] 30,000 to 40,000 French soldiers in 68 of 112 divisions (two-thirds of the army) were involved in mutinies (Englander 1998, pp.192, 196-197; Beckett 2007, pp. 306-307). The next sub-section turns to the question of whether executions could deter *or* spur desertions.

2.7 Shame, Honor, and Duty Soldiers' motivations differed. Before conscription commenced in March 1916, enthusiasm and patriotism drew some to enlist while monetary considerations like unemployment drove others (Beckett and Simpson 1985). Pamphlets and posters during the time period also indicated that women used social pressure to increase enlistment. Women's groups encouraged their members to give white feathers (the sign of the coward) to men who appeared to be of military age to shame them into service (Gullace 1997). Recruitment posters also emphasized duty.[50]

However, the Irish were less likely to be motivated by shame, honor, and duty to fight for the British. Between August 1914 and December 1915, 7.8% of Irish men between the

[49]Email with Putkowski on November 4, 2012.

[50]The Imperial War Musuem in the UK has several examples of such posters published by the Parliamentary Recruiting Committee; see, for example, http://www.iwm.org.uk/collections/item/object/14592 and http://www.iwm.org.uk/collections/item/object/28450.

ages of 15 and 49 enlisted; the equivalent figure for English and Welsh was 24.2% and for the Scots was 26.9%. After January 1916 to the end of the war, an additional 3.8% of Irish men aged 15 to 49 enlisted, whereas the numbers for England and Wales were 22.1% and for Scotland 14.6%, respectively (Public Record Office 1920). The numbers for the Irish are lower partly because Britain could not conscript in Ireland. Although recruiting in Ireland had "almost ceased" by mid-1916, conscription in Ireland, deemed as "politically unacceptable", was never introduced (Perry 1994, p. 81). During World War I, British commanding officers made explicit references to the Irish race as inferior and degenerate (Oram 1998, 2003, pp. 9-10) and Irish soldiers, in turn, perceived contempt and disregard from the British officers (Jahr 1998) and harsh treatment by the British High Command for executing so many Irish soldiers (Walker 2007, pp. 63-64). Of the 206,000 Irishmen who served in the British forces (Campbell 2005; Jeffery 2000, pp. 6-7), one out of every 600 received a death sentence (Department of Foreign Affairs 2004, p. 12),[51] whereas of the 5.2 million British who served (The War Office 1922), one out of every 2000 received a death sentence. This disproportionate sentencing (which could reflect either discrimination, perceptions of discrimination, or different rates of indiscipline), in conjunction with separatist events back home, such as the Easter Rising of 1916 that left 450 dead and 2,614 wounded (Foy and Barton 2001, pp. 210-211), suggests that the Irish would feel less duty to fight than the British. This may still be true even after conscription began in March 1916 while Irish enlistment remained voluntary, since conscription and the Easter Rising occurred around the same time.

[51] "Death sentences can be grouped into countries by reference to the *regiment* (emphasis added) in which each soldier was serving, thus enabling a comparison with the numbers recruited." p. 10. The report relies on Oram (1998), p. 59: there were 134,202 men recruited from Ireland and 239 men serving in Irish regiments were sentenced to death during the War. Assuming that regiment proxies for nationality, then 1 in 561.5 Irishmen were sentenced to death. Oram bases his inference that regiment proxies for ethnicity on the fact that of the 125 executed soldiers' parent's addresses that he could find in the Imperial War Graves Commission database, 95 of the addresses are within the traditional recruitment area of the executed man's regiment. Thus he concludes that the regiment is a proxy for origin in at least 76% of the cases. He says doing the same thing with the Soldiers Died in the Great War database would take a lifetime. Perry (1994), p. 69, identified the nationalities of all fatalities in Irish regiments. On average for the whole War, 30% of fatalities in Irish regiments were born outside Ireland. See Appendix B for further discussion.

Some historians have attributed a drop in Irish recruitment to the Easter Rising, though a substantial drop began even before the Rising.[52] The drop in recruitment led to the disbandment of 48% of Irish battalions and the remaining Irish battalions becoming comprised of only 56% Irish by the end of the war (Perry 1994, p. 69) as commanders began refilling divisions with people from any geographic background after the devastation of the Somme in order to reduce the likelihood of villages losing an entire cohort of men in a single battle (Fitzpatrick 1996). Even before the Easter Rising, U.S. newspapers noted cases of Irish officers deserting to fight for the German forces.[53] Germany attempted to supply arms to nationalists in Ireland for the Easter Rising. News of the Rising took at least a few weeks to reach soldiers on the Western Front. Irish soldiers reportedly suffered increased prejudice and hostility from their British comrades in arms after the Rising (Leonard 1996) and members of Parliament were reluctant to see Irish battalions being bailed out by English, an attitude that hardened after the Rising (Perry 1994, pp. 70, 81). According to several historians, the Rising did not weaken the morale of Irish soldiers (Denman 1992); loyalty to their regiments and comrades insulated them to some extent from changing attitudes at home (Perry 1994, p. 89).

Identity considerations likely increased the cost of following the law for the Irish soldiers who traded off the duty to fight with the material consequences for desertion. If punishment is perceived as unfair, then it can reduce the legitimacy of authority, and reduce the "ought" justification for following the law. The most extreme sanction available to authority was the death penalty. Since the death penalty was such a visible and extreme form of punishment, we might expect more executions to lead to a decrease in perceived legitimacy of the authority and consequently more crime among Irish soldiers. A vicious cycle can arise. More crime leads to more punishment, which further delegitimizes the authority. While the Irish were only 3.9% of U.K. soldiers in France and Flanders, they

[52]The drop was noted in government reports and a House of Commons report (Cmd 8168 1916; House of Commons 1916). Report on Recruiting in Ireland 1914-16, Cmd. 8168, vol. 39.
[53]Chi. Daily Trib., Mar. 20, 1916; Appendix Figure 2.

received 13.2% of death sentences (according to official statistics, see Campbell 2005; Jeffery 2000, pp. 6-7; Oram 1998, p. 59). Using surname dictionaries, I identify Irish soldiers among 21% of the desertions, 20% of the FGCM desertion and absentee trials, 19% of the death sentences, and 17% of the executions. Even if surname dictionaries yield an over-estimate of the Irish proportion by 50%, adjusting for this would still indicate that the Irish deserted at 3.5 times the rate of the non-Irish (4% of soldiers but 14% of deserters). Appendix B conducts additional analyses using birthplace and regiment information in various datasets and also reaches the conclusion that Irish soldiers were less inclined to fight. It is not feasible to merge the death sentence data to information about birthplace because little information on commuted soldiers survive and it is also not feasible to merge the soldiers sentenced to death to other datasets by name. But an analysis across datasets of the share of soldiers with Irish birthplaces or from Irish regiments yields a conclusion similar to what is reported here. My analysis advances the historical research on this question as previous historical research on Irish outcomes during World War 1 has not been conducted at the individual level [54] and no one has assessed the rates of Irish indiscipline in the three sets of absentee data digitized here.

Since there is no correlation between Irish ethnicity and execution conditional on the death sentence, the quasi-random execution of Irish soldiers allows causal identification of a potentially important mechanism for legal compliance—the perceived legitimacy of the lawgiver (Tyler 2006; Tyler and Huo 2002, pp. 141-142; Bowers and Pierce 1980; Bailey 2006), which criminologists, sociologists, and psychologists have cited to explain negative responses by minorities to state-imposed violence (Fagan and Meares 2008, pp. 214-222).

2.8 Battle Conditions A soldier's decision to desert also hinged on the battlefield conditions. While casualty rates were high, they were not as high as the probability of death conditional on desertion according to my calculations. The intensity of World War I

[54] Other methods to infer Irish ethnicity have used the casualties database (because it contains county of origin) and analyzed what happened to the Irish, but only at the level of the military unit (Perry 1994).

trench warfare meant about 12% of soldiers were killed serving on the Western Front, while an additional 37.6% were wounded (Urlanis 2003; The War Office 1922, p. 246). Disease was World War I's greatest killer. Medical services were primitive and there were no antibiotics. Poor sanitary conditions in the trenches led to dysentery, typhus, and cholera. In comparison, only 5% of soldiers were killed during the Second Boer War and 4.5% during World War II. 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. If we assume that fighting soldiers, rather than support soldiers, constitute all of the casualties then we may estimate that a soldier continuously in active and fighting mode faced a 48%[55] chance of being killed over his entire length of service and a nearly 100% chance of being injured.[56]

The peak strength of the British Army 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 the probabilities of debilitating injury or death in any given month are independent across months and 50% of soldiers are out of commission because of injury or death by 18 months, then a soldier in active, fighting duty the entire 18 months, had a 4% chance[57] of debilitating injury or death in any given month, a 7.8% chance of debilitating injury or death over 2 months, an 11.5% chance of debilitating injury or death over 3 months, and so on.

Using the number 552,471 for British casualties in France and Flanders recorded in my data and assuming a constant 11,500 soldiers dying per month, then a soldier had a 0.5%[58] chance of dying in any given month. These probabilities would vary over the course of the month since battalions generally rotated 10 days in the front, 10 days in

[55] One fighting solder with three supporting soldiers yields 4 x 12% = 48% probability of death.
[56] 4 x 37.6% > 100% probability of being injured. Many soldiers received more than one injury during the course of their service.
[57] To see this, (1-0.04)^18 = 50%.
[58] 11,500 divided by 2 million = 0.5%.

reserve, and 10 days at rest every month.[59] Because soldiers were moved at night,[60] the opportunity to desert was maximum during rotation when it was more likely to be premediated. Though some soldiers fled during battle and were later convicted of desertion (Babington 1983), desertion can be considered relatively cool-headed in contrast with another capital crime–cowardice in the face of the enemy (Peaty 1999, p. 199).[61] The fact that desertions often took place far from the front line lead some historians to conclude that desertions cannot be deemed the result of shell shock (Peaty 1999, pp. 200, 201, 206). A soldier who went missing during battle would be categorized as a straggler.[62] To the extent that the desertions in my data do reflect shell shock, however, the shell-shock motivated desertions would be captured in the error term, which should be unrelated to the execution, conditional on death sentence, if the decision to execute was random. A larger error term would also tend to make it more challenging to identify any significant effect of desertions on subsequent desertions.

There was relatively little opportunity to desert on the front—a soldier would be shot immediately—moving soldiers was like squeezing toothepaste and the battle police were always close behind.[63] Behind the front line, the formations generally went as follows: battle police, straggler posts (separately arrayed by brigade, division, corps, and army, one behind the other), traffic control and military police patrols, and furthest from the front line, examination posts. These barriers both prevented men from leaving the front line and collected stragglers for redistribution to their units forward (Sheffield 1996, p. 76).

[59]Conversations with Putkowski during November 24-26, 2011. The same 1:1:1 ratio ("4 days in the front line, then 4 days in close reserve and finally 4 at rest") is found in The Long, Long Trail (In the trenches. (n.d.). LLT. Retrieved from http://www.1914-1918.net/intrenches.htm), although this varied enormously depending on conditions, such as the weather and the availability of adequate reserve troops to rotate into front-line duty. Front line action can be subdivided further. Of the 120 days of front line action a year, "perhaps only 5-10 days were in intensive action [while] 60-100 days involved front-line trench activities without being in action" (The infantry battalion. (n.d.). LLT. Retrieved from http://www.1914-1918.net/whatbatt.htm).

[60]Conversations with Putkowski during November 24-26, 2011.

[61]For a list of definitions, see Graham-Harrison 1907, p. 267, Retrieved from http://archive.org/stream/manualofmilitary00greauoft#page/267/mode/1up.

[62]Email with Putkowski on January 26, 2008.

[63]Conversations with Putkowski during November 24-26, 2011.

The deserters who were shot immediately would not be in the lists of absentees collated after morning roll calls. Rather, these lists are more likely to include the soldiers who deserted overnight and had the opportunity to weigh some of the costs and benefits of desertion.

2.9 Rational Benchmark Before discussing the behavioral model, it is helpful to consider a theoretical benchmark of a soldier with rational expectations and complete information. Such a soldier would know that during the 2-4 weeks when he was away from the front (2 weeks in hiding plus 2 weeks in detention), he avoided the 0.5% chance of dying or 3.5% chance[64] of debilitating injury. These numbers would be divided by 3 to reflect the 1:1:1 rotation and the chance of dying being maximum while rotated to the front. Of the 7,361 trials for desertion, 2,007 resulted in a death sentence, and of these, only 12% were confirmed, so on net, a soldier had a 3.3% chance of dying because he deserted. The other 96.7% of the time, a deserter would be sent back to the trenches and face the same continuation probability of death (0.5%) or debilitating injury (3.5%) in subsequent months. These calculations suggest that the casualty rates were not so high that it would be rational for everyone to desert. This would be particularly true for soldiers who deserted while on furlough and for whom the desertion decision would be even more pre-meditated.

The discussion of a hyper-rational soldier is not meant to imply that all soldiers were deliberating with complete information when making their decisions. Rather, these calculations suggest that the decision was not obvious–to stay or to desert. Of course, these estimates reflect averages and some periods are more battle-prone than others and have higher casualties. However, soldiers would not know of impending offenses.[65] 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

[64]To see this, 4% - 0.5% = 3.5%.
[65]Email from Putkowski on October 8, 2008.

commenced, at which point there would no doubt about what was going to happen.[66]

Soldiers also did not have complete information on commutations so could have overestimated the probability of death conditional on desertion. Moreover, a soldier who chose to desert may have been optimistic and believed that he would be unlikely to be caught; or conversly, he may have believed the battle environment to be more dangerous than it actually was. Many desertions were also prompted by Dear John letters from loved ones or news about children being ill. However, the arrival of Dear John letters should not be correlated with the execution vs. commutation decision of a previous deserter. In sum, for the typical soldier, the decision to desert was not clear-cut. A rationally inattentative soldier or a soldier who overestimates from recent events can plausibly respond to shifts in beliefs upon observing an execution. This shift in risk perceptions is presumably what commanders intended in their use of the death penalty.

3 An Economic Theory of Legitimacy

3.1 Model My goal in this section is to investigate how soldiers react to an exogenous change in the economic and social/psychological cost of desertion with a formal model of legitimacy, which increases rule compliance (Ostrom 1990). Would-be deserters weigh the benefits of military desertion, such as being reunited with family or avoiding the trenches, against *economic costs*, such as loss of freedom, money,[67] and physical well-being (execution), *social costs*, such as shame (Beckett and Simpson 1985) and loss of reputation, and *psychological costs,* such as the pain from violating one's moral principles. Absent social/psychological costs, the basic idea is that a soldier will be less likely to desert following an execution, all else equal. Social and psychological costs can potentially outweigh the economic costs. I present a specific model of how desertion can respond to an execution. The framework described here is intentionally kept very simple and is based

[66]Email from Putkowski on October 8, 2008.

[67]Pay was deducted for every day "of absence either on desertion or without leave, or as a prisoner of war [if a Court of Inquiry finds that the soldier purposely allowed himself to be taken prisoner], and for every day of imprisonment awarded by a civil court or court-martial" (Graham-Harrison 1907, pp. 385-6 (Army Act SS 136 & 138). Retrieved from http://archive.org/stream/manualofmilitary00greauoft#page/384/mode/1up).

on Bénabou and Tirole (2012), except I introduce into the basic model two groups that differ in intrinsic loyalties to fight, which will affect the social and psychological costs of desertion.

Assume that a soldier weighs the benefits, B, of desertion (being reunited with family, avoiding at least some time in the trenches, avoiding death in battle, etc.) and the costs of desertion, broken down into economic, social, and psychological costs. Economic costs, C, include the family going unpaid and the probability of being caught and executed, p. Social costs are the difference between the social acclaim for being a fighter and the social shame for being a deserter. This difference is captured by S. Soldiers have some duty to fight, which enters the utility function as v_f. Not meeting one's duty entails some psychological cost (Chen et al. 2015; Chen and Schonger 2013). This term can be viewed as intrinsic motivation or loyalty, the lack of which is expressed through actions like desertion and absences without leave (Costa and Kahn 2003). v_f is assumed to be always positive because of the strong group loyalty developed in army units. Economic costs $C(p)$ are increasing in p, while social costs $S(n, i)$ is a function of n, the number of deserters, and ethnicity i. I assume that B and p are the same for both groups—casualty rates and the probability of execution conditional on going absent was the same for Irish and British.[68] Thus, the soldier makes the decision to fight ($f = 1$) or desert ($f = 0$) by maximizing:

$$U(f) = (v_f + S(n, i))f + (B - C(p))(1 - f)$$

[68] p is arguably similar since soldiers with Irish surnames constituted 21% of the desertions and 17% of the executions. B is arguably similar since the number of Irish deaths in the British Army recorded by the registrar general was 27,405, so their 13.3% casualty rate out of 206,000 Irish enlisted is similar to the overall casualty rate of 12%. The Irish Divisions (the 10th, 16th and 36th) also do not appear to have been disproportionately targeted for harsh assignments. Of course, the data does not rule out the possibility that the Irish were better fighters and assigned to harsher locations. However, the model remains the same if B and p do differ, because these parameters only affect the cutoff rule, which is already allowed to be group-specific.

Following Bénabou and Tirole (2012), I assume a single, inverted-U shaped distribution of loyalties v_f over the range of $[\underline{v}, \overline{v}]$. I assume that Irish ($i = 1$) are on the left-side of this distribution while the British ($i = 0$) are on the right-side.

The social cost function is operationalized as an inference that others make about the soldier's loyalty conditional on the choosing to fight or desert:

$$U(f) = (v_f)f + (B - C(p))(1 - f) + \mu E(v_f \mid f, i)$$

where μ is the positive weight agents put on social perceptions–their perceived loyalty or morality. Because there are two groups, perceived loyalty of soldiers depends on their ethnicity[69] as well as their decision to fight or desert. Then:

$$\text{if } f = 1 : U(1) = v_f + \mu E(v_f \mid 1, i)$$

$$\text{if } f = 0 : U(0) = B - C(p) + \mu E(v_f \mid 0, i)$$

Perceived loyalty now follows a cutoff rule. Normalize $c{=}B - C(p)$; with ordinal utilities, we can rewrite net utilities as:

$$\text{if } f = 1 : U(1) = v_f - c + \mu E(v_f \mid 1, i)$$

$$\text{if } f = 0 : U(0) = \mu E(v_f \mid 0, i)$$

This expression provides a cutoff rule, since if a soldier chooses to fight $f = 1$ at some v_f, then he would also choose $f = 1$ at any $v{>}v_f$, holding others' actions fixed in equilibrium. This is because the social motivation (shame, loss of reputation) and the economic motivation (avoiding trenches, reuniting with family, probability of death) are fixed, while the duty/psychological motivation (loyalty) increases. Thus, the cutoff rule

[69] This assumption diverges from Bénabou and Tirole (2012) and Chen and Yeh (2014c), where the perceived morality of a decision-maker only depends on the decision itself.

for ethnicity i will satisfy:

$$v^{*,i} - c + \ \mu E\left(v_f \mid 1, i\right) = \mu E\left(v_f \mid 0, i\right)$$

Define

$$\Delta\left(v, i\right) = \ E\left(v_f \mid v_f > v, i\right) - E\left(v_f \mid v_f < v, i\right)$$

Since there is a cutoff value v, where people choose to fight if their v_f is bigger than v and they choose to desert if their v_f is smaller than v, then

$$\Delta\left(v, i\right) = E\left(v_f \mid 1, i\right) - E\left(v_f \mid 0, i\right)$$

The expression motivates a sufficient condition for a fixed point. The fixed point solves the equation:

$$v^{*,i} + \mu\Delta\left(v^{*,i}\right) = c$$

A sufficient condition for a fixed point for ethnicity i is if $1 + \mu\Delta^{'}\left(v, i\right) > 0$, and $[\underline{v}, v^{*,i}]$ share of soldiers desert. The number of deserters is closely tied to the cutoff rule.

3.2 Deter or Spur? I assume that observing an execution causes soldiers to update their priors on p (the probability of being caught and executed) and n_i (the number of deserters of ethnicity i). The availability heuristic (Kahneman 2011) may lead individuals to overweigh recently sampled information (Hertwig et al. 2004), or individuals may simply have rational inattention (Sims 2003). Most soldiers would have only experienced a handful of the execution parades and the institutional memory of battalions likely fades over time,[70] making the salience of recent executions more vivid. Because of the salience

[70]Refilling divisions with people from mixed geographic and ethnic backgrounds likely further eroded institutional memory.

of recent executions, the following analysis does not rely on soldiers being completely unaware of lesser sentences, though if they were unaware, p is simply the probability of being caught for a soldier who believed that all deserters were executed and the following results hold even more strongly. A potential deserter may believe people are deserting every day and an execution updates even a fully rational soldiers' beliefs about the probability of being caught.[71] Nothing in the historical record suggests that executions would lead soldiers to think the next deserter would be *less* likely to be caught and executed.

In the absence of any social and psychological considerations, an exogenous increase in p results in fewer desertions. However, social/psychological considerations may cause the perceived loyalty of deserters to increase relative to the perceived loyalty of fighters, that is, the relative morality of deserters increases. To see how, the execution of a soldier with ethnicity i increases the perceived number of deserters of that ethnicity. This will change the social perception of deserters of ethnicity i and therefore change $\Delta(v, i)$. Observe that $v^{*,i} + \mu\Delta(v^{*,i})$ captures the marginal benefit of fighting for individuals at the cutoff: the marginal benefit is the sum of duty (loyalty) and the weight individuals put on social perceptions (shame), which is the difference in perceived morality of fighters and deserters. A rise in $v^{*,i}$ raises both $E(v_f \mid 1, i) = E(v_f \mid v_f > v, i)$ and $E(v_f \mid 0, i) = E(v_f \mid v_f < v, i)$. So, the difference: $\Delta(v, i) = E(v_f \mid 1, i) - E(v_f \mid 0, i)$ may either increase or decrease.

Since the Irish are on the left side of the distribution (See Figure 7[72]), $E(v_a \mid 1) - E(v_f \mid 0)$ will be decreasing for the Irish. In this region, the honor attached to fighting increases by less than the perceived morality of deserters, when the believed number of Irish deserters n_i increases. Adding a bit from the right-hand side, which is a proportionately large mass, will have a large effect on the mean, i.e., the perceived morality of deserters. Here, actions are strategic complements. The more people desert, the more others will. Multiple equilibria can arise if complementarity is strong enough or μ is large

[71] Moreover, if soldiers were unaware of commutations and believed all deserters were executed, then executions should not cause them to update their beliefs about the justice system in general, only, the probability of being caught.

[72] This figure is adapted from Bénabou and Tirole 2012.

enough. Indeed, when $1 + \mu\Delta^{'}(v)$ is negative, there may be unstable equilibria. Rapid social changes are possible when an ethnic group moves from one steady state to another. As for the British, who are on the right side of the distribution, $E(v_f \mid 1) - E(v_f \mid 0)$ will be increasing. Raising v^* will increase $E(v_f \mid 0, i)$ less than $E(v_f \mid 1, i)$, as $E(v_f \mid 0, i)$ will include many points in the center of the v-distribution, and so by slightly increasing the right margin, we include a few bigger v's. But for the $E(v_f \mid 1, i)$, when we have most of the remaining loyalists, cutting off a bit from the left-hand side will have a large effect on the mean, as we are cutting off a sizeable mass proportionately. The perceived morality of deserters increases less than does the social prestige of fighting. Here, actions are strategic substitutes: the more people desert, the less other people will do so.

3.3 Commander-in-Chief's decisions The optimizing Commander-in-Chief taking into account the delegitimzing effects of execution on Irish soldiers might choose to only commute the death sentences of Irish soldiers and to execute only British soldiers. Yet if this were to occur, Irish soldiers might perceive that the commander was not executing Irish, which would reduce the deterrent effect of the death penalty and reduce the effective p, leading to more Irish desertions. Thus, an optimizing commander might randomize between executing Irish and executing British, and, if anything, execute Irish soldiers at a lower rate to minimize the spurring of Irish desertions while maximizing the deterrence of British desertions. The decision-making of a Commander-in-Chief becomes a game where the targets of deterrence must weigh the likelihood of being executed, conditional upon individual characteristics. The normative commitments of different groups evolve onto separate paths (Chen 2010; Chen and Lind 2014; Chen 2011; Chen and Givati 2014). 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.

4 Data

4.1 Court Martial Death Sentences and Commutation Data My death sentence data includes all 3,342 sentences, complete with name, unit, offense, sentencing date, rank, and outcome—execution (including date) or commuted sentence, reference number in national archives, age (if soldier was executed), and theatre of war, from August 1914 to September 1923 (Oram 2003).[73] The date refers to date of death sentence, which is usually the trial date. It invariably differs from date of execution, which is listed separately.[74] 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).[75] Final sentences in the dataset are those punishments (if any) ultimately confirmed by the Commander-in-Chief. Of the 3,342 sentences, 2,724 are from the B.E.F.[76] 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 a fixed object, or reduced in rank) or the sentence was sometimes "quashed" (i.e., vacated). Figure 8A plots the distribution of crimes for those sentenced to death and Figure 8B plots the distribution of crimes for commuted and confirmed death sentences.

4.2 War Diaries Data Absentee lists are partially preserved in monthly war diaries of the Assistant Provost Marshal (APM) for the four-year period from 1914 to 1918.[77] Lists and descriptions of absentees were printed and circulated with ID Number, Rank,

[73] Original data sources include War Office (WO) records of the trials of soldiers who were executed (WO 71), registers of field general court martial (FGCM) trials (WO 213/2-26), and general court martial (GCM) trials (WO 90).

[74] I remove 1 death sentence whose execution precedes the trial date.

[75] The other offenses are: Irish rebellion, quitting post (leaving guard duty), 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 while POW, and unspecified/other, for a total of over 30 types of offenses.

[76] The remainder coming from other countries: Canada, New Zealand, etc.

[77] 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.

Name, Unit (Battalion Number, Battalion, and Regiment), Date of Absence, Description (usually including age and height, and sometimes also hair color, build, lips, complexion, eyes, teeth, facial hair, and accent; see Appendix Figure 3 for a sample image), and Reported by. The war diaries span four years, but the bulk of what was preserved in absentee lists is from July 1916-June 1917.

The absentee list was generated in the following manner.[78] 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. Among his duties for the area of his particular jurisdiction, the APM noted the number of absentees from regiments broadly on a weekly basis. Military units took roll call and attendance every morning (or more frequently). Those not present had to be categorized: killed in action, wounded, missing (prisoner-of-war or wounded), sick or straggler (lost or awaiting return from a "stragglers post" or "battle stop," where they had been gathered up by either regimental or Military Police). After a month, the names of those who were still absent and not accounted for were forwarded to the Provost Marshall at headquarters where the information was collated with other APM reports. The Provost Marshall would aggregate the material and circulate a printed updated list of the names of men absent for a month by unit for the armies at the front. The APM could then match names/descriptions to any soldier arrested. On occasion, three-month lists seemed to have appeared. These lists revised known absentees making earlier lists redundant.

4.3 Police Gazettes Data A separate absentee list is preserved in the Deserters and Absentees (D&A) supplement to the (weekly) Police Gazette from 1914 to 1918. This data includes: Office Number, Name, Rank, Regiment Number, Corps (Battalion Number, Battalion, and Regiment), Age, Height, Complexion, Hair, Eye Color, Trade (civilian occupation), Enlistment Date, Enlistment Place, Birth Place (Parish and County),[79] De-

[78] Conversations with Putkowski and the British National Archives.

[79] Appendix B describes the use of birthplace and enlistment towns with Google Maps, which puts the towns in Ireland or Britain.

sertion Date, Desertion Place, Marks and Remarks (see Appendix Figure 4). Additional biographical characteristics were merged in from soldiers' attestation papers completed upon joining the Army. Compared to the war diaries, the D&A supplement records are much more complete. However, they include absentees both at Home (where it was much easier to desert) and Abroad (in the notes, e.g., "from B.E.F."; for an example, see the "Marks and Remarks" column in Appendix Figure 4).

To provide a sense of data quality, Figure 9A shows the plot of all 126,818 absences at Home or Abroad. The sharp spike in 1914 is the day after the assassination of Archduke Ferdinand, which suggests that quite a few soldiers were reluctant to go to war. In 1916 and 1917, spikes are observed around Christmas, when soldiers are likely reminded of family. Another spike is observed at the end of the war in November 1918, consistent with exhaustion or a belief that there was no death penalty for desertion during peacetime. Although this paper digitizes the entire database, absences at Home are not subject to the death penalty, so I gleaned 3,009 B.E.F. desertions from this data source by searching for the terms "en route" or "from B.E.F." in the notes field. Figure 9B shows that these B.E.F. desertions occur throughout the war. The majority of these absentees were likely en route from B.E.F. to the U.K.[80]

4.4 Trial Records A third source of absentees is preserved in handwritten FGCM registers (WO 213/2-26), dating from January 1914 to November 1919, recording roughly 144,609 trials. The data includes: Date the Record was received, Rank, Name, Battalion Number, Regiment, Place of Trial, Date of Trial, Nature of Charge,[81] Nature (and length) of Sentence,[82] Acquittal (or Not Guilty), Remittance (i.e., commutation to a different

[80]Email with Putkowski on November 4, 2012.

[81]The charges include: Offense to Inhabitants, Mutiny, Cowardice, Absence (including absence from parade or Breaking out from barracks or camp, Striking or violence to a superior officer (and during superior officer's execution of office), Insubordinate or threatening language to a superior officer, Disobeying lawful command of superior officer, Leaving post (sentry or picquet) or asleep on sentry, Drunkenness (and while on duty), Injury and receiving (stolen) property, Losing equipment (and clothing, necessaries, etc.), Stealing and theft, Indecency, Resisting or escaping escort, Escaping confinement, Other offenses.

[82]The range of sentences includes: Death, Penal servitude, Imprisonment, Detention, Field punishment, Ignominy, Reduction in Rank and Seniority, Fine, Reduction (of pay), and Stoppages (of leave or other

sentence or sentence length), and Remarks (e.g., Suspended to serve after the war).[83]

There are 28,754 trials for absence and 13,309 trials for desertion. The number of death sentences across all crimes was 2,858; the number of death sentences for accused deserters was 1,730. 449 of the accused deserters were not guilty, but the remaining received some kind of sentence. Some of the difference from the official statistics may be attributed to data entry errors as handwriting is notoriously difficult to transcribe.[84] Note that these data are registers (see Appendix Figure 5) and not the trial proceedings themselves, most of which no longer exist. (The trial proceedings that survive are primarily of those where the accused was executed and have already been previously analyzed by historians.)

All three absentee records represent different samplings of the true desertion rate. Since I am comparing post-execution outcomes to post-commutation outcomes within a particular unit, I minimize the potential bias that results from error in measuring outcomes. For example, if desertion and absentee lists are under-inclusive because of poor preservation or if they are over-inclusive because they include those who were killed, were prisoner of war by accident, or were stragglers, these measurement errors would affect both treatment and control groups equally. Or, since only the trial date is recorded, the time delay between desertion and apprehension should be similar in both treatment and control.

4.5 Casualties Data I utilize the Soldiers Died In the Great War database containing 658,555 casualties to proxy for point-in-time danger by unit and by date. Casualties is a predictor of desertion (Costa and Kahn 2003). This data includes: regiment, battalion, surname, first (and middle) name, birthplace town and county, enlistment town and 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

privileges).

[83]Sometimes another field, Appeal from Summary Award of C.O., is present. In trials unrelated to desertion, the commanding officer could immediately dispose of the case, but the disposal was subject to the right of the accused, in certain cases, to demand trial. In the data, only 0.5% of desertion trials and 3% of absence trials come from an appeal. I do not make a different calculation of dates of absence for trials that come from an appeal since both the initial judgment and the soldier's exercise of his right to demand a trial instead of summary judgment were likely to have been immediate.

[84]For all crime variables, I interpret any mark in the column in the affirmative.

dates by military unit to control for high frequency changes in perceived danger.

An interactive version of this data can be viewed at this link.[85] B.E.F. casualties over the course of the war are displayed in the top panel and France-Flanders casualties in the bottom panel. The estimated casualties (in thousands) are expressed as number pairs in the format (German/Allies) in red and blue. Red indicates casualties from battles initiated by Germany. Another interactive version allows comparing casualties with the execution rate and is available at this link.[86] France-Flanders casualties are displayed in the top panel and death sentences in the format (execution/sentences) in the bottom panels. Figure 6 presents a screenshot. Casualties and casualty rates are the same since officers tried to fill in the divisions so they had a roughly constant size.

4.6 Irish Surnames A list of 426 Irish surnames identifies soldiers of probable Irish ethnicity.[87] The use of this data is subject to the caveats of potential measurement error. Differences between Irish and British soldiers would be underestimated to the extent soldiers are sometimes mis-categorized.

4.7 Service and Pension Records The Service and Pension Records were obtained from Ancestry.com, which digitized the original records held at the British National Archives (WO 363 and WO 364). The Service and Pension Records generally include name, age of enlistment, birth parish, birth county, residence address, regimental number, and date of attestation. The Service Records comprise roughly 2 million non-unique records.[88] The Pension Records, comprising roughly 1 million non-unique records, contain information on soldiers who were discharged from the army and claimed disability pensions, so some records also include date and place of injury. The Service and Pension Records are matched where possible by name to the death sentences dataset to provide additional covariates for checks of random assignment. Together, they provide 2.7 million

[85] https://dl.dropboxusercontent.com/u/8089659/DPdeterrence/AnimatedCharts/Casualties.FranceBEF.html.
[86] https://dl.dropboxusercontent.com/u/8089659/DPdeterrence/AnimatedCharts/FranceBEF.html.
[87] Surnames of Irish Origin. (2009) Last Name Meanings Dictionary. Retrieved from http://www.last-names.net/origincat.asp?origincat=Irish.
[88] 60% of the original 6.5 million Service Records were destroyed in a German bombing raid on London during World War II.

unique records identified by name, regiment, and residence. Even though the data is incomplete, age is useful to merge in because the capital sentences data only recorded age for executed soldiers while age is recorded in Service and Pension mostly for non-executed soldiers. Therefore, the combined data can serve to check for correlation between the execution decision and soldier's age. In this kind of analysis, missing data is dummied out and an indicator for whether age is missing is included.

4.8 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 $\rightarrow$ Regiment $\rightarrow$ Brigade $\rightarrow$ Division$\rightarrow$ Corps $\rightarrow$ Army $\rightarrow$ Army Group. Each higher level of organization contains three or four subordinate units plus headquarters and higher-level assets. A battalion consisted of 1,000 men, with 3 to 4 battalions per brigade and 3 to 4 brigades per division. With the addition of support, a division consisted of between 18,000 and 19,000 men and would occupy up to 15 miles of road while moving (Corns and Hughes-Wilson 2007, p. 108). According to historical accounts, the division commander was the highest-level commander whose commutation recommendation was ignored (Oram 2003, p.129; Babington 1983, pp. 78-79, 103). I do not know anything about ignored execution recommendations, because the records of the commuted cases were destroyed. If higher-level commanders did target based on discipline or show discretion, then the division is the highest level appropriate for analysis.

The thinness of the outcome data also 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. To be included in the analysis, units must contain at least 1 execution, 1 commutation, and 1 absence. From the war diaries data, I identify 676 usable matches preserved mostly from July 1916 to June 1917 (The upper panel of

Figure 1 plots the distribution of matches from 700 to 1100 days after the assassination of Archduke Ferdinand on June 28, 1914. World War I officially began one month later on July 28.). The median time between trial and next recorded desertion at the division level is about two weeks. From the Police Gazettes, I identify 3,009 B.E.F. deserters for all four years of the War; 1,319 of these are merge-able with the Order of Battle. From the trial data, I identify 45,824 usable matches for all four years; 4,365 of these matches are desertion trials.

4.9 Order of Battle and Merging To conduct the analysis, each event (death sentence, desertion, or casualty) must be assigned a particular division. But most of the sources list the battalion of a soldier, not his division. To determine the division, I developed 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.[89] This website gives, in mostly paragraph form, a time history of each battle unit and, in particular, the movement among divisions and brigades. Appendix C documents how the Order of Battle was entered.

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. The second form focuses on the division, and describes the movement of units into and out of the division. Combining these two data sources and focusing on the mergeable units (some units like Army Service Corps are impossible to merge) yields over 7,800 associations between battalion to brigade to division. Appendix A describes the process of merging and additional challenges, such as different spellings or abbreviations. I also track higher-level unit transfers (e.g., transfers from divisions to corps to army),[90] constituting roughly

[89]The website, available at http://www.1914-1918.net, is based on James (1978).
[90]The two data sources are LLT and Edmonds et al.

770 associations between the divisions to corps to army.

With the exception of Service and Pensions, I do not merge by soldier name across datasets. All datasets were scanned digitally and then checked, with the remaining non-scannable entries entered, by hand. The trial registers were written in cursive handwriting. The lack of computerized records from this time period makes prohibitively difficult the linking of absentees by name to their trial and to their death sentence date, if any. Absentee lists are only a sample of the universe of absentees. So any conclusion about the deserters being invariably caught rests on historians' statements and inferences based on the aggregate data.

The inability to match by name precluded the use of the Medal Rolls Index (WO 372), which was obtained in digital form from the British National Archives. Virtually all soldiers who served received at least the British War Medal for "entering a theatre of war or rendering approved service overseas" and there are 5,424,254 unique soldiers and a total of 7.8 million records ((some soldiers received multiple medals). This data provides the name, rank, regiment, regimental number, medal entitlement, first theater of war and date of entry, information on soldiers who forfeited their medal entitlement because of disciplinary infringement, and additional remarks (e.g., date of death or discharge). Merging by Medal Rolls Index would regularize the spelling of the military unit for ease of merging into the Order of Battle, but merging the absentee, casualty, and trial data directly into the Order of Battle proved more effective. Instead, I use this data to make assessments about the Irish surname dictionary by comparing against the official statistics regarding Irish enlistment, which I am able to do since this data contains roughly the universe of all enlisted soldiers.

4.10 Officers I digitize a dataset of the 2,992 commanding officers and general staff officers for division, corps, army, and general headquarters and the dates of their assignments and reassignments throughout the war (Becke 1935-1944). Officer data enables additional checks of whether the decision to execute or commute was correlated to officer

identity and ethnicity and whether the soldier's decision to desert is correlated with his officer's identity and his officer's ethnicity inferred using the surname dictionary. These data are merged into the Order of Battle.

4.11 Geographic Location The Order of Battle also contains major battles and the divisions associated with each battle. In the great majority, battles are named after the town or city in which, or near which, they took place. The towns can be entered into Google Maps, which provides geographic coordinates. I make the albeit simple assumption that divisions travel linearly and incrementally from one battle to the next to interpolate the rough location of each division on any date. Finally, these coordinates yield calculations of distances between divisions, distance to the English Channel, and distance to Berlin. This data allows estimating the approximate location of each soldier on the day of his absence. I also use Google Maps to geolocate the enlistment towns and birthplaces of soldiers when this data is available and I analyze this data in Appendix B.

5 Conditions for Causal Inference

Unless certain baseline assumptions necessary for causal inference are 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 this 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 (unobserved to the econo-

metrician) that appear at random among soldiers that desert. However, as 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.

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.

It is of course impossible to say definitively what was salient to a soldier observing the executions, much less characterize fully his decision-making process, but I can take two steps that justify my approach and inference. First, I can see if the soldier selected for execution within a unit depended upon observable characteristics, such as the soldier's national origin and rank, seasonality, and within-unit time-varying casualties and absences. 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 B.E.F. 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: recent indiscipline in the soldier's unit, number of recent casualties, location, timing of military offensives, 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 that I check whether observable characteristics are in fact correlated with the confirmation decision.

Table 1 shows the results of several regressions of observable characteristics for the deserters. No specification reveals a relationship between Irish ethnicity and probability of execution. Figure 10A illustrates that Irish soldiers were not disproportionately executed, conditional on the death sentence. Moreover, the first column of Figure 10A shows that the Irish were not disproportionately sentenced to death relative to the proportion of Irish absences.[91] Figure 10B shows that there were no time periods when Irish capital sentences were disproportionately confirmed, for example, after the Easter Rising. This result is confirmed in the set of interactions between year and Irish displayed in Table 1 in Column 5.

Column 2 shows that Privates were not disproportionately executed, and in fact, they were somewhat less likely to be executed than officers. Column 3 shows that age does not predict the execution decision. Column 4 shows that year fixed effects are jointly significant. There appears to be a decline in execution rates over time, which is consistent with Figure 3. Year fixed effects will be included in the analyses in Section 6. As for other time dimensions like seasonality and day of week, Columns 6 and 7 report that neither month fixed effects nor day of week fixed effects are jointly significant. Column 8 shows that division fixed effects are jointly significant–division fixed effects will also be included in the Section 6 analyses. Column 9 shows that the type of army—Regular, New, or Territorial, with Regular being the baseline category—do not predict execution rates. Column 10 shows that log of casualties do not predict execution rates.[92] Casualties do not predict execution rates regardless of whether it is measured in levels (e.g., measured 1-29, 30-59, or 60-89 days ago or 1-29 days in the future relative to the trial date) or in

[91] The first bar shows the relative share of Irish soldiers in the war diaries' absentee lists.
[92] All log values are calculated as 1+ the underlying variable.

first-differences to address potential serial correlation in casualties. Column 11 shows that distance to coast and distance to Berlin also do not jointly predict execution rates. All regression analyses restrict to death sentences occurring in France & Flanders before the end of World War I. Death sentences recorded with divisions or from the Labour Corps were also removed. Sample size varies for army and distance specifications because not all divisions were assigned to an army and distance data is unavailable before the first battle and after the last battle.

Table 2 Panel A shows the results of several regressions of unit-level factors such as officer identity and recent military indiscipline: number of military trials, death sentences, or executions. Panel A shows that fixed effects for Brigade unit, Corp unit, and Army unit are not jointly statistically significant. Fixed effect for officers (General Officer Commanding (GOC) for Brigade, Division, Corps, Army, and General Headquarters, and First General Staff Officer (GSO) of Division, Corp, Army, and General Headquarters[93]) are also not jointly statistically significant with the exception of division commanding officer which is significant at the 5% level. We may expect one or more significant effects given the large number of tests reported. Officers and units that appear with less than 10 frequency were categorized in a separate "other" category. Joint significance test of fixed effects for whether the officer is Irish and the soldier is Irish do not reveal systematic differential execution probability of Irish soldiers when their officers are British. Lagged measures (30 and 60 days ago) of log of military trials, log of death sentences, and execution rates also do not reveal significant relationships with the current execution decision. All regressions include fixed effects for division, year, and Irish surname.

Table 2 Panel B examines autocorrelation in execution decisions. The string of events within each unit are stacked and the first event within each unit was excluded as a dependent variable. If more than one event occurred on a day within a unit, the average outcome was calculated for that day. All regression models include year fixed effects

[93]Brigades did not have first staff officers.

and the leave-one-out (i.e. excluding the current decision) mean execution rate of the unit. Including division fixed effects would bias the estimated relationship between the current and previous decisions (Chen et al. 2014b). Separate ordinary least squares stacked autocorrelation regressions with different levels of aggregation (division, brigade, corp, army, army type, and global) do not show significant autocorrelation.

Table 3 repeats the exercise for all capital sentences regardless of crime. An exception to the randomization hypothesis is that murderers were more likely to be executed relative to other capital sentences, while privates were less likely to be executed. The lower panel of Figure 8B shows that the overwhelming majority of executions were for deserters. Table 4 repeats the specifications from Table 2 and two sets of fixed effects are jointly significant at the 10% level out of 16 tests. Analyses of Tables 1 through 4 in logit or probit specifications yield similar inferences.

5.1.2 Is the Sequence of Decisions Within a Unit Non-Random? Even if confirmation decisions are uncorrelated with observable individual and environmental characteristics, they may be correlated with unobservable time-varying characteristics within a division, such as time-varying perceived indiscipline, officer fixed effects, or lower-level groups or units of bad apples that may be correlated with subsequent absences. Confirmations may also be mean reverting. Figure 11 shows that for each of the divisions separately, while there are concentrated periods of death sentences, there do not appear to be concentrated periods of executions. Two animated graphs to explore relationships over time are available at this link,[94] which displays the cumulative measure, and at this link,[95] which displays "last 120 day" measures. In the graphs, the axes can be chosen by the user. Each division is labeled with the actual divisional number. The diameter of the circle around each division is proportional to the number of absences recorded for that unit.

Figure 12A shows a static final snapshot indicating that the execution rate in a division

[94]https://dl.dropbox.com/u/64329541/divMotionChartcumSum2.html.
[95]https://dl.dropbox.com/u/64329541/divMotionChartrollMean2.html.

is not correlated with total casualties in that division. Figure 12B shows another snapshot indicating that the execution rate in a division is not correlated with total absences (i.e., military discipline) in that division. This pattern is also visible in Figures 2 and 2B; the size of the circles correspond to the number of absentees recorded in the war diaries and police gazettes respectively. The size of the circle is not related to being above or below the 12% line.

Figure 12C shows a snapshot of the "last 120 day" animation indicating that death sentences and executions are positively and tightly correlated. Figure 12D shows that absences and casualties in the last 120 days are also positively (though less tightly) correlated. This pattern is consistent with soldiers deserting more when battlefield danger is high. However, Figure 12E shows that casualties in the last 120 days are uncorrelated with the execution rate in the last 120 days. This pattern is similar to with what was found in Tables 1 and 2. Moreover, Figure 12F indicates that absences in the last 120 days are uncorrelated with execution rates. Taken together, these figures suggest that–even if the military command[96] took into account point-in-time danger in *sentencing* soldiers to death–the Commander-in-Chief did not make the *execution* decision depend on point-in-time danger or discipline within a unit.

I next turn to a random strings test. This 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 1s and 0s (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., $\hat{s}_1, \hat{s}_2, \hat{s}_3 \ldots \hat{s}_n$). Since there were peaks and troughs in the execution rate, I treat the bootstrap samples as a vector of realized bernoulli random variables, with

[96]More specifically, the panel of officers of field rank (major, lieutenant colonel, or colonel).

the probability of a success (i.e., execution) equal to the global execution rate within fifty days of that trial time, not including the unit under consideration.

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 (they do, and the historiographical record confirms this (Putkowski and Sykes 2007, p. 64), so in these assessments of randomization, I treat multiple observations of executions (commutations) of death sentences whose trials occured on the same day and for the same division[97] as 1 observation[98]). The test also tells me if commanders targeted divisions for poor discipline or if lower level brigades or battalions generated a disproportionate share of desertions and death sentences and were targeted for discipline.

Mean-Reversion I test whether there is any form of mean reversion in the sequence, meaning that the execution in the jth case is correlated with the deviation of the actual execution *rate* in previous $j-1$ cases from the expected execution rate. This test tells me whether the Commander-in-Chief was attempting to equilibrate his decisions, considering whether a unit was "due" for an execution or whether they became more lenient after an execution.[99]

[97] Executed soldiers who had trials on the same day and same division are also from the same battalion. Among soldiers whose death sentences were commuted but had trials on the same day and same division, 70% came from the same battalion.

[98] A related question is if the collective pardon/guilt decision is independent of national origin, i.e. whether the execution/commutation outcome for British and Irish soldiers is correlated in a manner not due to chance. A Chi-Square test with simulated p values reveals that the decisions within a day are not independent.

[99] Results are similar when examining whether execution in the jth case is correlated with the deviation of the actual cumulative sum of executions in previous $j-1$ cases from the expected sum.

Longest-Run I test whether there are abnormally long "runs" without any executions or long runs without commutations. 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 recommended commuting a death sentence and the Commander-in-Chief was influenced by the lower level officer's recommendation or if lower-level groups or units of bad apples were being targeted for executions.

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 the 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 a Kolmogorov-Smirnov (KS) 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 13 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.

Appendix Figures 7A and 7B display power tests for the random strings test. In Appendix Figure 7A, m_1 represents the assumed autocorrelation between successive execution decisions (if p is the correct marginal probability of an execution, the transition probability from one execution to the next is $m_1 p$). For autocorrelation as low as 1.5, the distribution of p-values in one simulation is significantly different from the uniform CDF at the 10% level. In order to estimate the type-II error rate, thousands of strings are simulated. For each individual realization, the Null $H_0 : no\, autocorrelation$ is rejected if the p-value from the KS test is less than $\alpha = 0.05$. The fraction of incorrect decisions (failures to reject H_0) serves as an estimate for the type-II error. Appendix Figure 7B displays the

distribution of the KS p-values for 4 different values of m_1, which for convenience have already been translated into the corresponding values of the autocorrelation coefficient. For values of autocorrelation between 0.13 and 0.17, the estimated power lies between 0.74 and 0.9.

All tests examine the treatment of death sentences so as to assess whether, conditional on the death sentence, the decision to execute is as good as random. In so, commutations can serve as a control for executions, as the unobserved factors correlated with the decision to desert should be the same. These tests do not speak to whether the sentencing decision itself was random. The death sentences were not public unless confirmed, and assessing the causal effect of death sentences conditional on trial is not the subject of the analysis.

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 jth execution, a soldier's decision-making is still being affected by what occurred in the previous $j-1$ 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.[100] I assume that other motivations for

[100]These calculations treat desertions and capital sentences that occurred in pairs or groups as one observation since the decisions to execute or commute these soldiers were not independent: almost without exception, they were determined simultaneously and with identical outcome. If executions and commutations occurred on the same day, neither event is censored by the other. Absences that

desertion, captured in the error term, are uncorrelated with the execution or commutation decision.

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.

$$(1) \qquad f(y) = \lambda \exp(-\lambda y)$$

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

$$(2) \qquad \lambda = \beta_0 + \beta_{ex} ex_{ij} + \beta_{exd} ex_{ij} \cdot des_{ij} + \beta_{des} \cdot des_{ij} + \gamma^C cas_{it} + \gamma^U_j + \gamma^T_{year(j)=T}$$

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 important that my specification estimates what would have happened had the confirmed death sentence been commuted. Estimates of what would have happened had the confirmed death sentence not existed would be susceptible to spurious inferences. For example, an increase in unit size could lead to more desertions, more death sentences, and more executions (assuming constant desertion and execution rates). In fact, the battle environment or recent deser-

occurred on the day of an event is considered as occurring the previous night so do not count as the first absence after an event. Multiple absences or events on the same day from different ethnicities were considered as British as they constitute the modal soldier.

tions could be correlated with the presence of a death sentence and also with subsequent desertions. The specification can be interpreted within the theoretical model described in Section 3: *cas* controls for the cost of staying and *ex* creates exogenous variation in perceptions of costs.

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.

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

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:

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

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

These two terms measure the cumulative effects of past events, one for executions and one for commutations. They also measure idiosyncratic variation in execution rates over time within divisions since the sequence is also exogenous. Differences in the effects of these two terms characterize the effect of exogenous variation in the application of the death penalty. Neither term by itself has a causal interpretation because the number of death sentences could be endogenous.

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

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

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

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

Results of the hazard model are presented in the main tables with standard errors clustered at the division level since the weak-SUTVA parameters are constructed to be serially correlated within division. The appendix tables present results without clustering and a specification check where time is run backwards and I calculate the time until the previous absence before a treatment event.

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 execu-

tions 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 had 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 (not to be confused with the military 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 military 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, July 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 or commutation

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

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

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

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

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

where

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

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 any crime

$\beta_{sd} \equiv$ Effect of a desertion death sentence

$\beta_{so} \equiv$ Effect of a death sentence for any crime

I define a vector of parameters:

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

$X(t)$ 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 $[0, 1]$.[101] 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. β_{exd} and β_{exo} have causal interpretation. I also introduce terms for Irish executions and Irish death sentences. Results are presented only with standard errors clustered at the division level since the treatment variable is serially correlated within division. The appendix tables present a specification check where time is run backwards.

All analyses with the War Diaries data as outcome restrict the sample from day 670 to day 1085 (the start of World War 1 on July 28, 1914, is the 0-day) when the vast majority of the absence data is recorded (Figure 1). The War Diaries data represents about one-third of the war. Appendix Figure 8A visually summarizes the intuition for the duration model and Appendix Figure 8B visualizes the day-by-day approach.

7 Results

7.1 Duration Framework Table 5 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

[101] I can allow event-specific values of λ.

3, 6, and 9 use the Cox model; columns 1-3 use the +14 days imputation method, 4-6 use nearest neighbor, and 7-9 use the trial date as the commutation date. Panel A uses the War Diaries, Panel B the Police Gazette, and Panel C the FGCM Desertion Trial Registries.

I cannot detect a deterrence effect, nor can I rule out such an effect. Table 5 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 decisions occurring 14 days after the trial; commutation decisions 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 could reduce the chance to either deterrence or spurring effects since an intervening absence after the trial but before the execution or commutation can occur. The data is somewhat consistent with this bias as Panel A displays a slight spurring effect in Columns 7 and 8 and Panel C displays a slight deterring effect in Columns 1-3. Four of 27 specifications displays a significant impact of executions at the 10% level and 1 at the 5% level but they do so in opposite ways. An increase in casualties, both contemporaneous and 30 days ago, in most specifications is strongly correlated with a spurring effect on time until next absence.[102]

While I find limited evidence that executing deserters deters absence, executing Irish soldiers, relative to other executions, spurs absence. Table 6 examines how execution of

[102]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, while a positive coefficient implies a spurring effect.

different types of soldiers may have led 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. Figures 14A-14C corroborate this visually in a univariate analysis with the exponential model for each of the three datasets. The first of each pair of figures presents the effect of execution of Irish soldiers and the second of each pair presents the effect of execution of non-Irish soldiers. The top figure shows that the hazard rate of absences after executions (displayed in the red line) is generally pulled inwards to the origin, relative to the hazard rate of absences after commutations (displayed in the blue line), suggesting that executions spurred absences. The bottom figures show the opposite pattern for non-Irish executions. Table 6 shows that in some specifications, executing deserters deterred absences relative to execution of non-deserters (Panel A Columns 7-9). The negative coefficients on the Irish term suggests that Irish death sentences tend to occur when desertions are less frequent, which underlines the need to control for commutations to address the variety of factors related to the sentencing decision.

Table 7 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 days after trial imputation method for commutation dates 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 soldiers spurs absences—is robust to various controls in Columns 2-6. Moreover, the finding that executing deserters deters absence is robust using the Police Gazettes (Columns 7-12) and FGCM (Columns 13-18).

The coefficients on the cumulative measures of execution and cumulative measures of commutation are of independent interest. The difference in the coefficients represent the effect of the execution rate as opposed to execution salience, which is the focus of the

"most recent event matters" approach in the strong-SUTVA model. When the half-life is short, the two coefficients tend not to be statistically significantly distinguishable from each other. However, as the half-life is extended (1 week, 2 weeks, 1 month, 2, months, 3 months), the coefficient on the cumulative measure of executions becomes significantly more negative than the coefficient on commutations. This suggests that the execution rate has a deterrence effect and it is revealed in models that assume a longer half-life.[103]

In terms of magnitudes, the impact of executing an Irish soldier is roughly 7-8 times the effect of the change in log casualties 30 days ago. This ratio is perhaps surprisingly and reassuringly constant across datasets and specifications. Focusing on the specification assuming a half-life of one month, the difference between the cumulative execution and commutation measure is 0.12 more negative for executions than for commutations, and this is on par with the effect of a unit change in log casualties 30 days ago. Note these ratios would be smaller to the extent that the casualties data have measurement error.

Table 8 reports identity of the next absence following an execution or a commutation of an Irish or non-Irish soldier. After the execution of an Irish soldier, 19% of the absences that immediately follow are Irish, but after executing a non-Irish soldier, 11% of the immediately following absences are Irish. After a commutation, 13% of the immediately following absences are Irish regardless of the ethnicity of the soldier whose sentence was commuted. A difference-in-difference is also visible for desertion death sentences. The pattern holds most strongly for War Diaries, which is the closest to a point-in-time measure of absenteeism, and is weaker for Police Gazettes and FGCM. In all cases, the percent Irish in the next absences are higher after an Irish execution than after a non-Irish execution.

Appendix Tables 1-2 report falsification checks where the analysis is run backwards in time. I start the clock 90 days before the treatment event to ensure the subject of the death sentence does not affect the outcome variable. Appendix Tables 3-5 report results

[103]This is true for the Police Gazette and FGCM data while the pattern is less statistically significant in the War Diaries data, though the coefficient on the execution term is always more negative than on the commutation term.

without clustering of standard errors. Appendix Tables 6-7 report the backwards analysis without clustering. These robustness checks are consistent with the randomization checks reported in Section 5 and support the inference that executing Irish soldiers spurred rather than deterred absences.

7.2 Day-by-Day Framework Tables 9 and 10 present the results of the day-by-day approach under different assumptions for the half-life of an event: 1 week, 2 weeks, 1 month, 2 months, and 3 months. I use the execution date and estimated commutation date 14 days after the trial date for the treatment dates. Table 9 uses as outcome variable an indicator for whether any absence occurred. Group desertions are not independent events. Further, it may be easier to desert in pairs or groups than to desert by oneself. Table 10 uses as outcome the difference between an indicator for whether any Irish absence occurred vs. an indicator for whether any non-Irish absence occurred. The outcome takes the value of 1 if only Irish absences occurred on that day in that division, -1 if only British absences occurred, and 0 if neither or both occurred. An event that may spur the desertion of Irish soldiers may not affect the non-Irish or even deter non-Irish desertions.

When aggregating Irish and non-Irish absences together, I usually cannot detect a deterrent or spurring effect. In Table 9 Panel B, the shorter half-life specifications indicate a deterrence effect of any execution in the Police Gazettes. Table 10 shows more robust patterns that the execution of Irish soldiers spurs rather than deters Irish desertion as opposed to non-Irish desertion. The effect is observed across all half-life specifications in the War Diaries and Police Gazettes. The size of the effects are largest with the smaller half-life assumptions. The FGCM data suggest some spurring of Irish desertion relative to non-Irish desertion when it comes to the execution of deserters. Again, the size of the effects are largest with the smaller half-life assumptions. To interpret the magnitudes for one dataset, the average value of the outcome is -0.002 in the Police Gazettes and the average value of the execution-Irish term is 0.003. So multiplying the coefficient 0.012 by 0.003 yields roughly 2% of the average outcome. The standard deviation of the execution-

Irish term is 0.045 and the standard deviation of the outcome is 0.057 yields roughly 1% of the standard deviation of the outcome.

Appendix Tables 8 and 9 examine the effect of future events on previous absences. I shift the absence data by 90 days earlier to ensure the subject of the death sentence are excluded from the outcome variable. For example, March 1, 1916 would have treatment variables calculated for death sentences after March 1, 1916. I do not correlate the absence on March 1 with these treatment variables because an absence on March 1 could appear in the treatment data within three months and would be on both the left- and right-hand side of the specification. Rather, I correlate the absence on December 1, 1915 with forward-treatment (future executions and commutations) on March 1, 1916 as the placebo check. Five out of 90 coefficients on an execution term are significant at the 10% level, which is consistent with the earlier randomization checks.

7.3 Why Not Something Simpler? It is generally considered good writing practice to avoid long narratives about research blind alleys and false starts, but discussing some of the more standard (and inappropriate) approaches has value. Prior to using independently measured absences as the outcome, the project considered using courts martial for desertion resulting in death sentences 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 resulting in death sentences 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 officers are seeking lesser sentences than capital punishment following an execution since they have already "made their 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. A similar argument can be made for desertion trials.

As an example of how highly malleable the sentence charged at court martial could be, the chances of being convicted of desertion was extremely high, but the chances of being sentenced to death far lower than 100%. 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 resulting in death sentences would be a biased measure of desertion that could only be addressed if the ratio of desertions / desertion trials leading to death sentence was constant across the military and the divisional death sentencing rates were constant.

8 Conclusion

Many countries struggle with non-compliance to state laws. The prevailing strategy for addressing non-compliance is the imposition of harsh sanctions, and for most of the world's population this includes the death penalty. This is despite a lack of empirical evidence regarding the effects of the death penalty on compliance with the law. Moral issues aside, analysis of whether British executions during World War I deterred military desertions provides a novel test for the death penalty. One prerequisite for a death penalty policy is whether individuals respond to increasing subjective risk of criminal sanction (Nagin and Pepper 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 death penalty is not as strong a disincentive as we might imagine.

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 archival data on desertions, I employ three modeling approaches to estimate the deterrent effect of the death penalty: 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. I find limited evidence that executing deserters deterred absences, while executing Irish soldiers, regardless of the crime, spurred absences, especially Irish absences.

Higher rates of crime among disadvantaged groups have been attributed to mistrust of legal institutions (Tyler and Huo 2002). Disadvantaged groups in the U.S., like the Irish during World War I, are disproportionately sentenced to death (Donohue 2013). In order to investigate the relationship between legitimacy and crime, one would need a scenario where essentially identical crimes led to very different punishments for arbitrary reasons (Berdejó and Chen 2014; Chen and Spamann 2014; Chen et al. 2014b) with individuals observing the punishment and tracked before and after the event. Thus, the research design presented in this paper brings causal evidence related to issues such as the role of legitimacy in courts (Gibson et al. 1998), organizations (Suchman 1995), and nation-states (Lipset 1959). Disparate treatment permeates other parts of the judicial system (Chen and Sethi 2011; Chen and Yeh 2014b; Chen 2015). This paper suggests that state-imposed sanctions can undermine state legitimacy and provides evidence of another mechanism for legal compliance beyond deterrence.

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A Appendix: Description of Merging Process

To recap, the data comprise of absentees –> trials –> convictions –> death sentences –> executions or commutations, which has the potential causal effect on subsequent absentees. Absentees are measured in the War Diaries, Police Gazette (B.E.F. subset), and Trials (absentee and deserters subset).

Each event (death sentence, absence, or casualty) needs to be assigned a particular division. Each of six datasets:

1. Absentees-War Diaries,
2. Absentees-Police Gazettes,
3. Trials,
4. Death sentences,
5. Casualties, and
6. Officer lists.

is merged into the Order of Battle. Each dataset has a different set of keys, the most important of which is date, because the lower level military units moved between different higher level units throughout the war. Each of the six datasets is also merged into the Irish surname dictionary. The Order of Battle is itself merged into a list of battle locations to yield geocodes. The Service and Pensions data is merged by name into each of the six datasets to obtain a handful of its covariates like age and birthplace. No attempt is made to merge any other datasets by soldier name because of the difficulty in the merge due to spelling and non-unique names.

The original sources are typed, with the exception of the trial registers, which are hand written. This paper digitizes absentees, trials, officers, battle locations, and the Order of Battle. Casualties, capital sentences, Irish surnames, medal roll, and Services and Pensions Records were previously digitized. Unit names were not the same in different data sources because of data quality and different ways of spelling or abbreviation. As a first step in the data cleaning, names of military units were disabbreviated with the help of historical sources and historians. For instance, the short form (ASC, RFA, DAC, KRRC, MGC, RAMC, RAOC, RE, RGA, RHA) is changed to the respective long form (e.g. Army Service Corps, Royal Field Artillery, etc.).[104] The Order of Battle also is entered with some difficulty. Battalions are recorded both on regimental pages and divisional pages. Regiment pages appear to be more complete and reliable and are given priority in data entry. Additional information on the Order of Battle digitization is provided in Appendix C.

[104]More examples of abbreviations are provided here: http://www.1914-1918.net/abbrev.htm.

Linking the datasets to the Order of Battle is based on up to three variable keys: 1) Battalion name (sometimes company or platoon name) and/or battalion number, 2) Regiment name, and 3) Date (absence, casualty, or trial date). A regiment name typically refers to a geographic location in the U.K. Each regiment has many numbered battalions. The battalions usually travelled in different divisions. All three variable keys are necessary for an exact match. In addition, matching on only battalion number and regiment string is possible because battalions within a regiment typically had unique battalion numbers.

The data linkages are generated through two steps: exact matching and algorithms. For exact matching, all datasets employed a manual look-up table to serve as an exact translation of a unit to a combination of regiment and battalion in the Order of Battle. Cross-checking with historical sources and historians yielded a manual look-up table linking the battalion/regiment string in each of the six datasets to the equivalent battalion/regiment in the Order of Battle.

The exact lookup is composed of two steps: the actual exact lookup of the unit followed by a date matching in the Order of Battle. If both conditions yield a match, then the first round is deemed successful. Most matches were derived from this exact lookup. In a few instances, battalions are formed from existing battalions that do not have an official separate entry in Order of Battle or are renamed into new battalions that also do not have a separate entry, resulting in the soldier being listed as deserting or dying on a date when the battalion does not exist in the Order of Battle. In these instances, the nearest date on which the brigade and division is affiliated with the named battalion is used. In addition, the first date of the war and the last date of the war are assumed when the first date at the beginning of the war and last date at the end of the war for a battalion's association with a brigade and division are not explicitly stated.

In some instances, the military unit could not be identified easily by hand for an exact lookup. An algorithm is then used, which involves approximate string distance matching and the prioritization of variable keys to allow for minor typos in the original record or data entry. For example, the Order of Battle occasionally lists battalions together as a single record. In some datasets, battalion and regiment are not separated into separate variables in the original raw data. The raw data would leave the information in such an abbreviated form that the entire string was used to match against the battalion and regiment keys. This algorithmic step extracts the battalion number and separates it from the regiment, because in most cases, battalions can be uniquely identified simply by its number within a regiment. Some inference can also be made if the best possible matches all locate the battalion in the same division. In the algorithmic step, the nearest date is not used to facilitate the merge; an exact match for the date is required. Matching the regiment string also involved de-abbreviations, or if no de-abbreviation, a manual look-up. A Jaro-Winkler string distance score was calculated comparing the closest OOB regiment match

with the extracted regiment. When multiple matches were available within a string distance of 0.20, it was checked whether all potential matches yielded the same division and brigade. If only a single match was available, then any match with string distance worse than 0.05 was discarded. Finally, records without absence dates or battalion numbers were discarded.

Finally, after merging the records into the Order of Battle brigade and division, the brigades and divisions are assigned unique identifiers because the Order of Battle occasionally uses different names to refer to the same division or same brigade. One form of measurement error is unavoidable: when the Order of Battle does not record the exact day of month when a battalion moves to another division, I assume the transition occurs on the first day of the month. Other measurement errors in the merging process are assumed to be orthogonal to the execution or commutation decision.

Duplicates are also removed during the data cleaning process. Any soldier with the same first name, last name, regiment number, and record date are made unique in the followlwing datasets. Any soldier's record without a date is also dropped.

A.1 Capital Sentences merge to Order of Battle The capital sentences dataset already provides a numerical code for brigade and division, so linking to the Order of Battle merely required decoding. However, brigade data was often not included in the original data, so these were looked up and entered manually by searching for the regiment and battalion, checking that the original division data is correct for the record's date, and entering the brigade that is correspondingly listed in the Order of Battle.

A.2 Absentees-War Diaries merge to Order of Battle The War Diaries dataset is merged using the Unit (which contains battalion name, battalion number, and regiment name) to the Order of Battle. The units are first matched manually as best as possible, using historical consultations, de-abbreviations, and a look-up table. Units where there is no possible way to uniquely identify the match are dropped (e.g., a unit listed as, 1st Engineers, would be impossible to disambiguate). The second round of matching uses the Jaro string distance between the combination of battalion number, battalion name, and regiment name with its equivalent in the Order of Battle. The algorithm makes pairwise comparisons between every possible match in the Order of Battle and takes the best match if the distance is below a threshold. Battalion and regiment string distances are given equal weight in priority. However, the battalion number is required to be an exact match.

A.3 Absentees-Police Gazettes merge to Order of Battle The Police Gazettes dataset is merged using Corps (which contains battalion name, battalion number, and regiment name) to the Order of Battle. The units are first matched manually as best as possible, using historical consultations, de-abbreviations, and a look-up table. The second round of matching uses the Jaro string distance between

the combination of battalion number, battalion name, and regiment name with its equivalent in the Order of Battle. The algorithm makes pairwise comparisons between every possible match in the Order of Battle and takes the best match if the distance is below a threshold. Battalion and regiment string distances are given equal weight in priority. However, the battalion number is required to be an exact match.

Dates in the Police Gazettes did not include calendar year. Calendar year is inferred from the publication date of the newspaper gazette. For example, December absence recorded in a January gazette would have the year be set as the year prior to the publication date.

A.4 Trials merge to Order of Battle The FGCM dataset contains only Regiment name and Battalion Number. The regiment name is first matched manually as best as possible, using historical consultations, de-abbreviations, and a look-up table, to the regiment in the Order of Battle. Then, additional records with spellings that are close to the matchable FGCM regiment strings are replaced with the matchable spellings via algorithm to address minor errors in the handwriting transcription. Next, the battalion number and regiment string are used to find its equivalent in the Order of Battle. The second round of matching uses string distance between the combination of battalion number and regiment name to find its equivalent in the Order of Battle. The algorithm makes pairwise comparisons between every possible match in the Order of Battle and takes the best match if the distance is below a threshold. Because these strings are noisier than in the other datasets, both the Jaro string distance and the Levenshtein string distance are employed. The battalion number is sometimes missing in FGCM. In this scenario, all possible matches are examined and checked to see if they all yield the same brigade/division, if so, then that brigade/division is assigned. Note that because the exact date of absence is not recorded, I assume that the absence occurred one month before the trial date and deduct this month accordingly before merging with the Order of Battle. This ensures that the news a potential deserter responds to is merged in from the correct division in case the battalion has moved divisions in the meantime. However, the true trial date is kept for all analyses.

A.5 Casualties merge to Order of Battle The casualties dataset contains battalion, regiment, and battalion number. This dataset was previously digitized, so linking to the Order of Battle merely required decoding. However, some records are lost in linking to Order of Battle because the Order of Battle did not provide information on the brigade and division for some battalions. These casualties are dropped.

A.6 Officer List merge to Order of Battle The officers are already organized by each of the higher level units (Brigade, Division, Corp, Army, and General Headquarters).

A.7 Merging to Service & Pensions Linking to the Service and Pensions data requires the soldier name. The first name is often abbreviated. If so, only the first letter of the first name is used in the merge. Sometimes the raw data includes first and middle initials in capital letters without punctuation separating the two initials. The two initials would be separated before merging by name.

The matching algorithm involves a mix of exact-match requirements and minimum distance calculations. Battalion number and first letter of the first name are required to match exactly. Matching based on unit names and the remainder of a soldier name is based on approximate string distance: Levenshtein distance is used for soldier names and the Jaro distance is used for unit name. The reason for using Jaro when matching military units is that the *number* of strings describing the military unit often differs across datasets. These extra strings do not impose as much of a penalty when using the Jaro distance.

The **Levenshtein** distance counts the number of deletions (d), insertions (i) and substitutions (s) necessary to turn two strings, A and B, in the other. All characters, including spaces and punctuations, count. This distance is bounded, for instance if A contain n_A characters and B contains n_B characters, the lower bound is $n_A - n_B$ and the upper bound is n_A (if $n_A > n_B$) or n_B (if $n_B > n_A$). This distance metric is more appropriate for a single string, like a surname.

The **Jaro** distance is a heuristic measure. Let n_{AB} be the number matching characters between A and B and n_t the number of transpositions of the n_{AB} matching characters. Two characters c_A and c_B are said to be matching in A and B if and only if $c_A = c_B$ and the index (position) of c_A in A is less or equal to $\lfloor 0.5 \cdot \max(n_A, n_B) - 1 \rfloor$. Then, the Jaro distance is:

$$d_{A,B} = 1 - \frac{1}{3}\left(\frac{n_{AB}}{n_A} + \frac{n_{AB}}{n_B} + \frac{n_{AB} - n_t}{n_{AB}}\right). \tag{1}$$

This distance is bounded between 0 (exact match) and 1 (complete dissimilarity). It is also defined as 1 when there are no characters in common between A and B.

Given two vector of strings $\underline{A}$ and $\underline{B}$, each element of $\underline{A}$ is compared with each element of $\underline{B}$. Let A_1, B_1, and B_2 be strings. A_1 matches B_1 better than B_2 if and only if $d_{A_1,B_1} < d_{A_1,B_2}$. In declaring a string match of one element A_j of $\underline{A}$ with one element B_k of $\underline{B}$, two conditions must be satisfied:

1. A_j matches better to B_k than how it matches to any other element of $\underline{B}$: $d_{A_j,B_k} = \min\left\{d_{A_j,B_s},\ s = 1, \ldots, m_B\right\}$, where m_B is the number of elements in $\underline{B}$.

2. $d_{A_j,B_k} < \tau$, where τ is a deterministic threshold

A higher threshold is allowed for merging Service and Pension than for merging Order of Battle because

the data is recorded with poor quality. Data that was hand-entered as "[?]" could have been treated as a wildcard for the purposes of matching, which would have greatly increased computation time. Wildcards are dropped instead since dropping them does not affect the string distance functions much.

A.8 Irish Indicator Each of the six datasets is also merged into the Irish surname dictionary. This is based on an exact name match. A second Irish indicator is imputed using the merge with Services & Pension. The place of birth is matched to Ireland. The second indicator is used in robustness checks but is not the main indicator because the Services & Pension merge is not strong, but it is used to conduct validity checks on the surname dictionary.

A.9 Merging with Higher Order Units (Corps, Army, and General Headquarters) Linking the division to the higher order units is more straightforward because it does not involve data external to the Order of Battle. However, sometimes the Order of Battle does not report for some dates the association of a division to a particular corps or army. In this case, a match is made to the chronologically closest corps or army that the division is part of. This is because the official dates of association typically refer to the headquarters' relocation, but given the size of the unit, the soldiers themselves could take quite awhile to relocate. If the previous hierarchy is unknown for matching divisions to corps, a match is made by looking forward in time and for the next corps that the division is associated with. If the previous hierarchy is unknown for matching corps to army, a match is made by looking forward in time and for the next army that the corps is associated with, with exceptions noted below. In a handful of cases, the Order of Battle reports that the division is associated with more than one corps/army. In those cases, a match is made to the corps/army that has the longest association with the division.

Corps outside of France and Flanders are linked directly to a general headquarter. The four other general headquarters whose officers are recorded are located in Salonika, Egypt (Palestine), Gallilopi (Mediterranean), and Italy. Some corps fought first in Italy and then in France. Only corps in Italy have an associated army unit. Units associated with these corps are assigned to the appropriate army while they are fighting outside France. Army units are associated to general headquarters on exact dates.

A.10 Geolocation Linking to the battle is straightforward because it does not involve data external to the Order of Battle. A battalion is not assigned a geolocation before its first battle or after its last battle and is assumed to travel incrementally from one battle to the next. The air distance is calculated to the English Channel and to Berlin.

A.11 Final Dataset The final dataset contains 14,466 unique records of soldiers who were absent or sentenced to death. Observeable individual, unit, and environmental characteristics are merged from all other datasets. The variables are summarized below:

Environmental characteristics are derived as follows:

- **Battle environment**: Number casualties in each division and brigade at each point in time. One number for British and one for Irish.
- **Morale**: Number of absentees in each division and brigade at each point in time. One number comes from each of War Diaries, Police Gazettes, and FGCM. These numbers are further broken down by British and Irish.
- **Disciplinary environment**: Number of death sentences and number of trials in each division and brigade at each point in time. The death sentences number comes from the capital sentences dataset and the number of trials comes from the trial dataset. These numbers are further broken down by British and Irish.
- **Executions environment**: Number of executions in each division and brigade at each point in time. This number comes from the capital sentences dataset. These numbers are further broken down by British and Irish.

The environmental factors are calculated in and around a time window of 30, 60, and 90 days before, after, or before and after the current day, never including the current day (so a 30 day window is really a 29 day window before and after the current day).

The final list of variables are:

- Name - Name of the soldier
- Unit - Unit of the soldier, typically battalion
- Rank - Rank of the soldier, typically private but in some cases also specific officer rank
- Date - Absence date (War Diaries and Police Gazette), sentence date (Capital Sentences), or trial date minus 30 (FGCM dataset)
- CaseType - Crime such as desertion, absence, or quitting (FGCM dataset)
- Sentence - Commutation or execution
- Location - The city name (FGCM dataset) or a general indication such as B.E.F. (War Diaries and Police Gazette) or F&F
- OtherType - Other crimes the soldier was tried for or miscellaneous info about the sentence
- Brigade, Division, Regiment, Battalion - Name of each unit the soldier was part of on that date
- Dataset - Name of the dataset where the record comes from

- LastName, FirstName, FirstLetterLastName - Soldier's name
- DeathSent - Indicator for whether the soldier is sentenced to death (FGCM dataset)
- CorpName, ArmyName, GHQName - Name of additional units the soldier was part of on that date
- BrigOfficerName, BrigOfficerRank - Name and rank of the 1st officer in command of the soldier's Brigade
- DivGOCName, DivGOCRank, DivGSO1Name, DivGSO1Rank - Name and rank of the Division General Officer Commanding (GOC) and Division 1st grade staff officer (SO)
- CorpGOCName, CorpGOCRank, CorpBGGSName, CorpBGGSRank - Name and rank of the Corps GOC and Corps staff officer
- ArmGOCName, ArmGOCRank, ArmMGGSName, ArmMGGSRank - Name and rank of the Army GOC and Army staff officer
- GHQChiefName, GHQChiefRank, GHQCGSName, GHQCGSRank - Name and rank of the C-in-C and the staff officer of the B.E.F.
- LastName_SP, BirthParish, BirthCounty, Residence, Age, DocYear_SP, regiment_SP, FirstName_SP - Information from the Service and Pension
- Irish - Indicator of whether the soldier's surname is Irish
- DistCoast, DistBerlin - The air distance of the division of the soldier record to the English Channel and to Berlin, obtained from linking with the Geo Location dataset

B Assessing Irish ethnicity

There are three ways to assess Irish ethnicity: surname dictionary, regiment, and birthplace. Each source of data on absentees, death sentences, and casualties has name and regiment, but only three databases–Service and Pension, casualties, and Police Gazettes–have birthplace and enlistment location. For reasons described below, I use surname instead of regiment to infer Irish ethnicity. Linking by name to access these location data is presently infeasible because of slight differences in spelling, typographical errors in the originals or in transcriptions, large number of people with shared names, and the fact that the Service and Pension records are incomplete samples of the universe of soldiers who served.

However, the availability of these datasets with surname and birthplace allows:

1. assessing how closely Irish surname and Irish birthplace align.

2. assessing the relative loyalty of Irish and non-Irish. (The birthplace data is available in conceptually distinct datasets.)

3. assessing differences between being born in Ireland or Britain vs. being enlisted in Ireland or Britain.

Differences in distance in time to Irish roots can be related to differences between having male Irish ancestry vs. having Irish birthplace. Those born and raised in Britain with Irish male ancestry may be more loyal than those born and raised in Ireland, but those having male Irish ancestry may be less loyal than those without male Irish ancestry even among soldiers born and raised in Britain.

Some previous researchers have relied on regiment to infer Irish ethnicity and use regiments to base the analysis of how the World War 1 experience differed for Irish and non-Irish soldiers. The casualties database has both regiment and birthplace, so, assuming that those who died are a representative sample, it can be used to assess the validity of using regiment to infer Irish ethnicity, or more precisely, birthplace. I proceed in two steps to identify the geographic location of every birthplace and enlistment location. The first step uses a list of counties. I first construct a single address string if both parish and county are available. I then look for exact string matches with the list of counties. For example, "down" and "derry", which are Irish counties, would have to appear as a separate string. Occasionally the address would have multiple strings that match the county list. To address this, if the data has "co" or "co.", the string that appears right after would be prioritized in the matching, or if the string comes after a comma, it would be prioritized. The second step uses Google Map's API to locate any remaining locations.

Previous analysis (Perry 1994, p. 67) and my own analysis of this database indicate that some regiments are disproportionately composed of soldiers whose birthplaces were in Ireland: Royal Irish Rifles, Royal Inniskilling Fusiliers, and Royal Irish Fusiliers (primarily from northern Ireland) and Irish Guards, Royal Irish Regiment, Connaught Rangers, Royal Dublin Fusiliers, Leinster Regiment, and Royal Munster Funsiliers (primarily from southern Ireland). Among soldiers whose birthplaces could be appropriately located by the API (82% of the 660,585 casualties had birthplaces that could be located in the UK using the two-step algorithm), 38-62% of the soldiers in northern Irish regiments were born in northern Ireland, while 67-74% of soldiers in southern Irish regiments were born in southern Ireland. All percentages discussed in this section are reported as a percent of the located soldiers. 19,241 were marked as being born in southern Ireland and 10,189 were marked as being born in northern Ireland.

30% of those whose birthplaces were in Ireland were assigned to non-Irish regiments. While many soldiers were allocated to their regiments according to their birthplace, the military command changed its policy during the war. It began allocating soldiers according to need instead of by geography, which

avoided the decimation of entire youth cohorts of villages. Using regiments to base the analysis of how the World War 1 experience differed for Irish and non-Irish soldiers is therefore potentially confounded with changes in military policy.

An analysis using surnames to infer ethnicity (99,433 soldiers have Irish surnames) would be unaffected by policy changes in allocation of soldiers to regiment. Moreover, disparate treatment of and response by minorities need not be limited to soldiers being born in Ireland. Disparate treatment can affect soldiers who had Irish male ancestry. British-born soldiers without Irish ancestry but assigned to Irish regiments are unlikely to have experienced disparate treatment of the kind that soldiers with male Irish ancestry would have experienced. They may also have experienced advantaged treatment.

My analysis relies on Irish surnames rather than Irish regiment since counting British-born soldiers without Irish ancestry as "Irish" for statistical analysis can lead to a qualitatively different kind of measurement error. Finally, regiments are not always cleanly available in different datasets due to idiosyncratic abbreviations, spelling, or typographical error.

B.1 Medal Roll Using the Medal Roll, which contains the near universe of all soldiers who enlisted in World War 1, I compare the identification of soldiers with male Irish ancestry as a percentage of overall enlistment with the official government statistics reported on Irish enlistment by place of birth (which is 3.9%). The medal roll does not contain county of origin, but contains last name, first name (and, if available, middle name or initials), rank, regiment, and regiment number. There is no battalion number, battalion string, or date. Merging the Irish surname dictionary with the Medal Roll yields an estimate of 14.1% having male Irish ancestry out of 5.4 million soldiers. The figure, 14.1%, is 250% higher than the 3.9% of the U.K. soldiers in France and Flanders as reported in government statistics as being born in Ireland.

B.2 Casualties Using the casualties database, I compare the identification of soldiers with male Irish ancestry with the identification of soldiers born in Ireland. 15.1% or 99,433 of the 658,616 casualties are identified as having male Irish ancestry according to the surname dictionary. A similar 15% of the 549,884 listed as dying in France and Flanders are also identified as Irish according to surname. This 15% ratio is similar to the 14.1% of the Medal Roll identified as having male Irish ancestry. Those with Irish surnames appear to have been dying at the same rate at which they were enlisted, which suggests that soldiers with Irish male ancestry were not sent to more dangerous areas or that they were better fighters if they were. One may assume, as other historical researchers like Perry (1994) and Oram (1998) have assumed, that the casualties reflect a representative sample of the overall enrollment.

Turning to an analysis of birthplace, according to the Irish National War Memorial[105], only 49,000 of the casualties were Irish and most estimates range from 30,000 to 50,000. In a separate analysis of birthplace, 29,739 men in the casualties database were born in Ireland (Jeffery 2000, p. 150). Another source, analyzing the Irish census, reports 27,405 Irish deaths, a rate of 14% out of the enlisted Irish and "the same proportion as for the British army overall" (Fitzpatrick 1996, p. 392), which was 12%. My geolocation algorithm yields 29,430 Irish deaths. The 27,405 Irish census deaths account for 4.1% of the casualties database, which is close to the 3.9% reported in government statistics of the percent of all enlistees being born in Ireland.

Finally, three analyses of birthplace and surnames–government statistics and the medal roll surnames, Irish census and casualties surnames (Fitzpatrick 1996), and geolocation and casualties surnames–suggest that the surname dictionary results in a similar 240-250% more soldiers being identified with male Irish ancestry compared to soldiers who were born in Ireland.

B.3 Police Gazettes Using the entire police gazettes database on 152,699 deserters and absentees including those who deserted in the UK, I compare the identification of Irish soldiers using the surname dictionary with the Irish identification based on county of origin in a large sample. 21.7% of the Police Gazettes' deserters have Irish surnames. 22.7% of the B.E.F. sub-sample of deserters had Irish surnames. It is also 21% in the sample of deserters recorded in the War Diaries.

The higher percentage of soldiers counted as Irish in these desertion samples is consistent with the Irish having a lower morale and deserting at a higher rate than the British. Using birthplace data, 13.4% of B.E.F. deserters were born in southern Ireland and 4.4% were born in northern Ireland. All percentages are reported as a fraction of geolocateable soldiers. 62% of soldiers could be geolocated.

The increase in the share of soldiers with Irish birthplace in this database (17.8% of geolocateable birthplaces) as opposed to the casualties database (5.4% of geolocateable birthplaces) is noteable as it indicates that the Irish-born were more inclined to desert in the field. Dividing 17.8 by 5.4 indicates that the Irish-born were deserting roughly 3.3 times the rate at which they enlisted (assuming that the casualty statistics are representative of enlistment statistics).

Those with Irish surnames were deserting 1.5 times the rate at which they enlisted. The ratio of 22.7% to 17.8% indicates that, among deserters, Irish surnames are only 28% more frequent than Irish birthplace, not 240-250% as found in the analysis of enlistment and casualties. The dramatic decrease in the ratio of the number of soldiers with Irish surname to the number of soldiers with Irish birthplace is noteable because it suggests the length of time from Irish roots is predictive of morale.

[105]http://imr.inflandersfields.be/index.html

B.4 Irish Loyalty and Length of Time from Irish Roots Analysis of birthplace data across these datasets provides further evidence that the Irish were probably less loyal than the British. The casualties database has 1.9% of its soldiers being born in northern Ireland and 3.6% of its soldiers being born in southern Ireland. The Police Gazettes has 4.5% of its soldiers being born in northern Ireland and 9.8% of its soldiers being born in southern Ireland. In the sub-sample of BEF deserters, 4.4% were born in northern Ireland and 13.4% were born in southern Ireland. Since BEF deserters are those who deserted in the field, whereas Police Gazette desertion can be interpreted as desertion that occurred during training, the higher share of southern Irish-born in the BEF sub-sample indicates that lower morale among Irish soldiers was more relevant when choosing to desert in the field of battle than when choosing to desert in the U.K for the southern Irish.

Comparing desertion statistics with enlistment statistics indicates that lower duty to fight is also observed among the enlisted Irish-born soldiers compared to enlisted British-born soldiers. As a share of enlistment, Irish-born soldiers were 170% (dividing 4.5% + 9.8% by 1.9% + 3.6%) more likely to desert during training in the U.K., but southern Irish-born were 270% times more likely to desert in France and Flanders (dividing 13.4% by 3.6%) while northern Irish-born were 130% more likely to desert in the field (dividing 4.4% by 1.9%). This finding suggests that, despite northern Ireland remaining part of Britain after World War I, the northern Irish-born were over twice as likely to desert as British-born enlistees.

Next, I analyze enlistment location data. Analysis of enlistment location indicates that the gradient in the duty to fight is similar according to enlistment location than according to birthplace: 1.8% of casualties, 4.1% of Police Gazette, and 3.8% of Police Gazette BEF deserters were enlisted in northern Ireland while 2.2% of casualties, 7.2% of Police Gazette, and 8.1% of Police Gazette BEF deserters were enlisted in southern Ireland. As a share of enlistment, soldiers who enlisted in Ireland were 180% (dividing 4.1% + 7.2% by 1.8% + 2.2%) more likely to desert during training in the U.K., but southern Irish-enlistees were 270% times more likely to desert in France and Flanders (dividing 8.1% by 2.2%) while northern Irish-enlistees were 110% more likely to desert in the field (dividing 7.2% by 1.8%). In sum, desertion in the field was also higher for soldiers who enlisted in Ireland relative to those who enlisted elsewhere. These results suggest that birthplace is a stronger predictor of loyalty, but enlistment location and surname are also strong predictors.

Together, the fact that both the northern Irish-born and the northern Irish-enlistees were over twice as likely to desert is important to allay the concern that the use of the Irish surname does not distinguish between southern and northern Irish. Not distinguishing between the two groups in the main analyses is unlikely to be problematized by the potential for the northern Irish to have equal or heightened loyalty compared to the British. At present, I do not have historical information on Catholic or Protestant

birth parishes to ascertain whether Protestant Irish had heightened loyalty, but there is little in the discriminatory statements made by British officers indicating that they made a distinction on the basis of religion. The use of surnames is still likely superior to the use of regiment to mark "Irish" because 27% of soldiers in Irish regiments were born in Britain. The mix of Protestant and Catholic Irish would also still exist in the regiments.

Finally, even though British-born soldiers were far more loyal, British-born soldiers with Irish surnames, were disproportionately, slightly disloyal. This further supports the use of Irish male ancestry as a proxy for "Irishness". Soldiers with Irish surnames and Irish birthplaces comprise 2.0% of casualties and–with the assumption of population representative death rate–2.0% of enlistment, but 6.5% of the Police Gazette (225% more likely to desert). Soldiers with Irish surnames and British birthplaces comprise 13.2% of casualties and 15.6% of the Police Gazette (20% more likely to desert). Soldiers without Irish surnames and with Irish birthplaces comprise 3.4% of casualties and 8.2% of the Police Gazette (140% more likely to desert). Soldiers without Irish surnames and with British birthplaces comprise 81.4% of casualties and 69.7% of the Police Gazette (15% less likely to desert).

B.5 FGCM Notably, the proportion of soldiers with Irish surnames is 20% in the desertion and absence trials, 23% in the B.E.F. Police Gazette sample of deserters, 21% in the war diaries sample of deserters, and 19% of the death sentences (and 17% of executions). Assuming that the Police Gazette and war diaries sample represent the true desertion rate, the consistency in the share with Irish surnames suggests that the military command did not disproportionately target or disfavor Irish soldiers in the apprehension and trial stage nor in the sentencing and execution stage.

B.6 Irish surname vs. Irish regiment The consistency in the proportion of soldiers deemed Irish is not present when I use Irish regiment. In the B.E.F. Police Gazette sample of deserters, 17.5% are from Irish regiments. This is close to the 16.5% with Irish birthplaces. However, in the war diaries sample of deserters, only 10.5% come from Irish regiments. The reason is partly due to the fact that names are recorded better than regiments, which often appear inside an idiosyncratic spelling or abbreviation of the military unit, which can be merged to brigade and division but not always cleanly to regiment. Further corroborating the difficulty of using regiments to identify Irish ethnicity: In the capital sentences data, 8.1% come from Irish regiments. The lower percentage could mean that those from Irish regiments were treated favorably by the military justice system conditional on deserting, but no historical evidence suggests that this is the case. Alternatively, the quality of the spelling of regiments could be lowest in the capital sentences data, which used very short abbreviations, relative to the war diaries and Police Gazette data sources. For the many reasons described in this section, I use surname instead of regiment

to identify Irish soldiers.

Notably, 7.1% of executed soldiers, 7.1% of deserters with capital sentences, and 7.4% of executed deserters come from Irish regiments. The consistency of proportion of soldiers deemed Irish within-dataset is consistent with the hypothesis that the decision to execute or commute any soldier was quasi-random and unrelated to, for example, the soldier's Irish identity.

B.7 Service and Pension Records For completeness, I investigate the Service and Pension Records, a large sample based on pension eligibility, which also contains data on birthplace. Unfortunately, the quality of location data here is particularly low. Only 815,000 soldiers or 29% could be geolocated, unlike the 82% geolocation rate for the casualties data. This low rate is largely due to the fact that 1.9 million soldiers did not have birth location data. All soldiers have surnames, however. In this data, 15.3% of the 2.7 million soldiers have Irish surnames, which is consistent with the percentage found for the Medal Roll and casualties databases. On the basis of this consistency, one might infer that the data destruction during WW2 was effectively random and that the Irish were not more likely to receive pension money after WW1. However, 9.1% of geolocateable soldiers are identified as born in Ireland; I have no good reason for why there is this higher proportion. One possible reason for this is that the birthplaces of Irish soldiers were more likely to be recorded, but I have not investigated this further. Besides the birth location, another possibility to geolocate the soldiers is the residence data, but this variable is also missing for 1.3 million records.

C Order of Battle

This section describes some of the assumptions and procedures used when entering the entire Order of Battle from The Long, Long Trail (http://www.1914-1918.net/). Battalions are recorded both on regimental pages and divisional pages. Regiment pages appear to be more complete and reliable and are given priority in data entry.

Battalions, brigades, and divisions also had multiple spellings, e.g., Highland Division and 51st Highland Division and 51st Division). Priority was given to the longest spelling and, in general, to make all identifiers unique even when the original data did not provide a unique string for a single military unit. The division pages sometimes have a lot more specific information than the regiment page on the exact name of the brigade or division. The division page would be relied upon for the more complete information. Geography or unit number often uniquely identify the military unit. However, some data records needed to be dropped because there was either contradictory geography and unit number identifiers (e.g., 55th 2nd West Lancashire Division would be dropped because there was a 57th 2nd West Lancashire Division) or the information was not specific enough to identify the unit (e.g., Welsh Division).

Interpreting the Order of Battle also required several assumptions. The regimental pages are organized

by battalion. For each battalion, each of its association to a particular Brigade and Division would come with the identity of the Brigade and Division and the beginning and end date of the association. If the start or end date of a battalion is not listed and the association is chronologically the first or last for the battalion, then the first and last date of World War 1 is assumed. When only month and year are provided, the 1st of the month is assumed.

Sometimes, a battalion is amalgamated with or absorbed or formed into another battalion. In this case, the Division and Brigade of the new battalion is entered for the old absorbed battalion from the date of absorption all the way to its end date to ensure that data was not lost in merging. The other datasets would sometimes record a soldier as being part of the absorbed battalion after the battalion was absorbed. For example, 11th service battalion pioneers of the King's Liverpool Regiment was absorbed by the 15th Battalion of the Loyal North Lancashire Regiment on 17 June 1918. The details for the 11th service battalion June 17 onwards would be entered from information for the 15th battalion, because now the 11th service battalion is part of the 15th battalion.

If a battalion is formed on a certain date, say 1 Aug 1914, but is assigned a Division and Brigade at a later date, say 1 September 1914, the first entry for this battalion would be 1 Aug 1914-1 September 1914, and the Division and Brigade would be null. If a battalion was disbanded on a particular date, the date of disbandment would be the end date. In practice, this means any soldier attached to the battalion before it got assigned to a Division and Brigade or after the battalion was disbanded would not merge into the other datasets. This is because if a soldier deserted or was sentenced on a particular date in this unit, we affirmatively know that the soldier should not be part of a Division or Brigade. In contrast, if the Order of Battle simply has no information for a battalion, the soldier is attached to the Division or Brigade nearest in date (see Appendix A), because we do not know affirmatively that the soldier should not be part of a Division or Brigade.

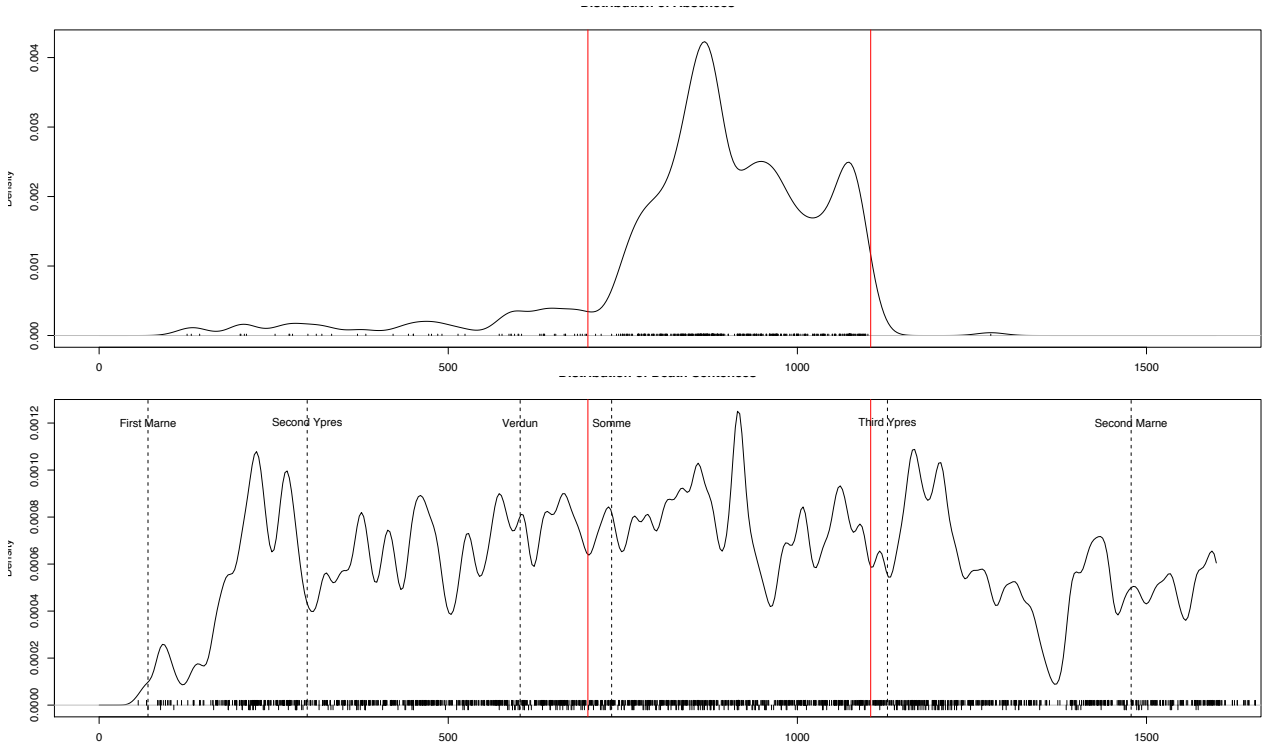
Continuous connections to Brigade or Division are assumed unless the original data said explicitly that the battalion left the Division or Brigade and there is no information on another Division or Brigade assigned. For example, if the battalion was assigned to another Division without any information on Brigade, the previous Brigade information was assumed. Similarly, previous Division information was assumed if the battalion was assigned to a new Brigade without any new information being provided on Division. In all instances, the divisional page would be cross-checked for more information. Sometimes, a battalion may move to another Division as "divisional troops", in which case no Brigade would be recorded in the data entry.

As long as there is information available on the original battalion, this information would be tracked in data entry. For instance, if a battalion is reduced to cadre strength, and personnel are transferred

to another battalion (hence, usually to another Division) and the cadre is reported to be in a different Division, information on the cadre is entered for the battalion, i.e., entries reflect the Division and Brigade information of the cadre.

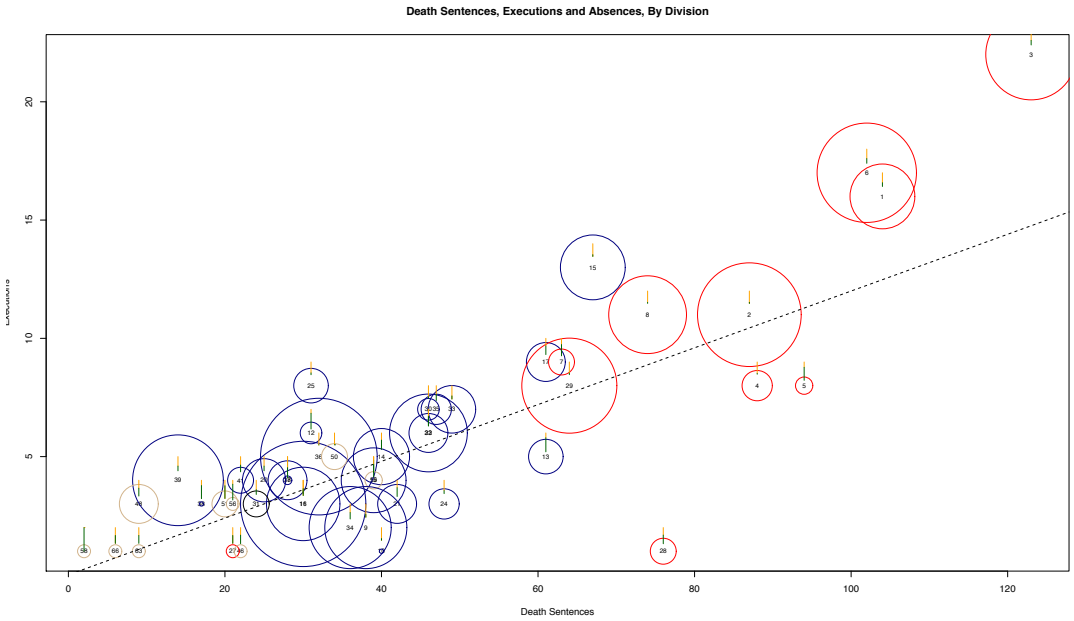
Sometimes, the Order of Battle would report the battalion assignments to be "same as" or "similar to" another battalion. Sometimes battalions would be listed together with identical battalion assignments. These information would be, as best as possible, carefully separated out to facilitate merges. Sometimes the location assignments were not detailed enough for data entry, e.g., data on cyclist units. Occasionally, absentees would be recorded as part of training reserve battalions, which were in the U.K., so these absentees were also dropped during the merge. Cavalry divisions were also ambiguous so if a battalion was listed as being associated with a numbered cavalry division and then an unnumbered cavalry division, the numbered cavalry division was assumed. Finally, sometimes the battalion has different names but are identical as far as one can tell from the Order of Battle (e.g., 483rd field company and 1 ea anglian field company); they are listed in the Order of Battle under each name in case the raw data lists soldiers as being part of one battalion or the other.

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 2B (Absences from Police Gazettes)

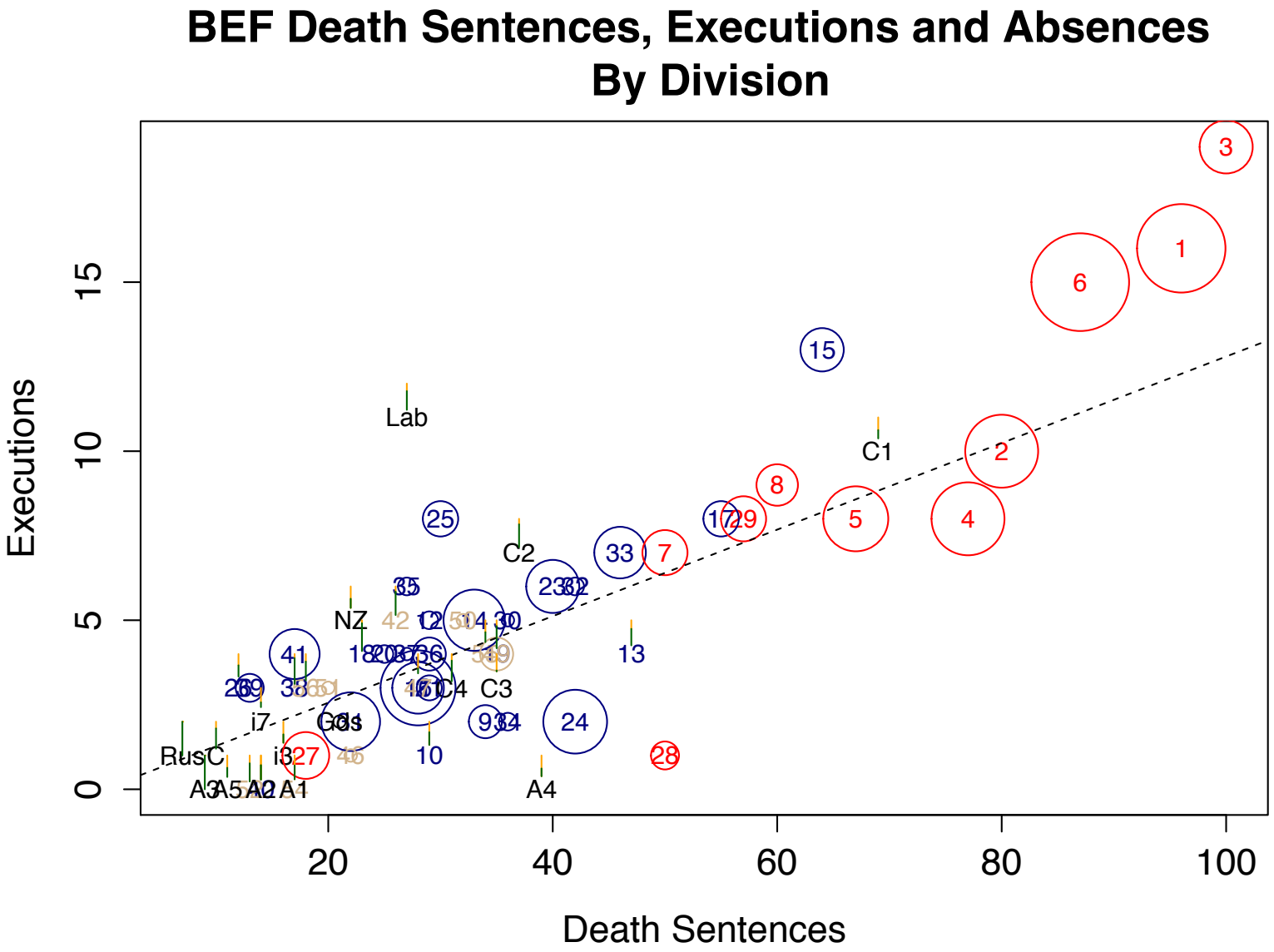


Figure 3

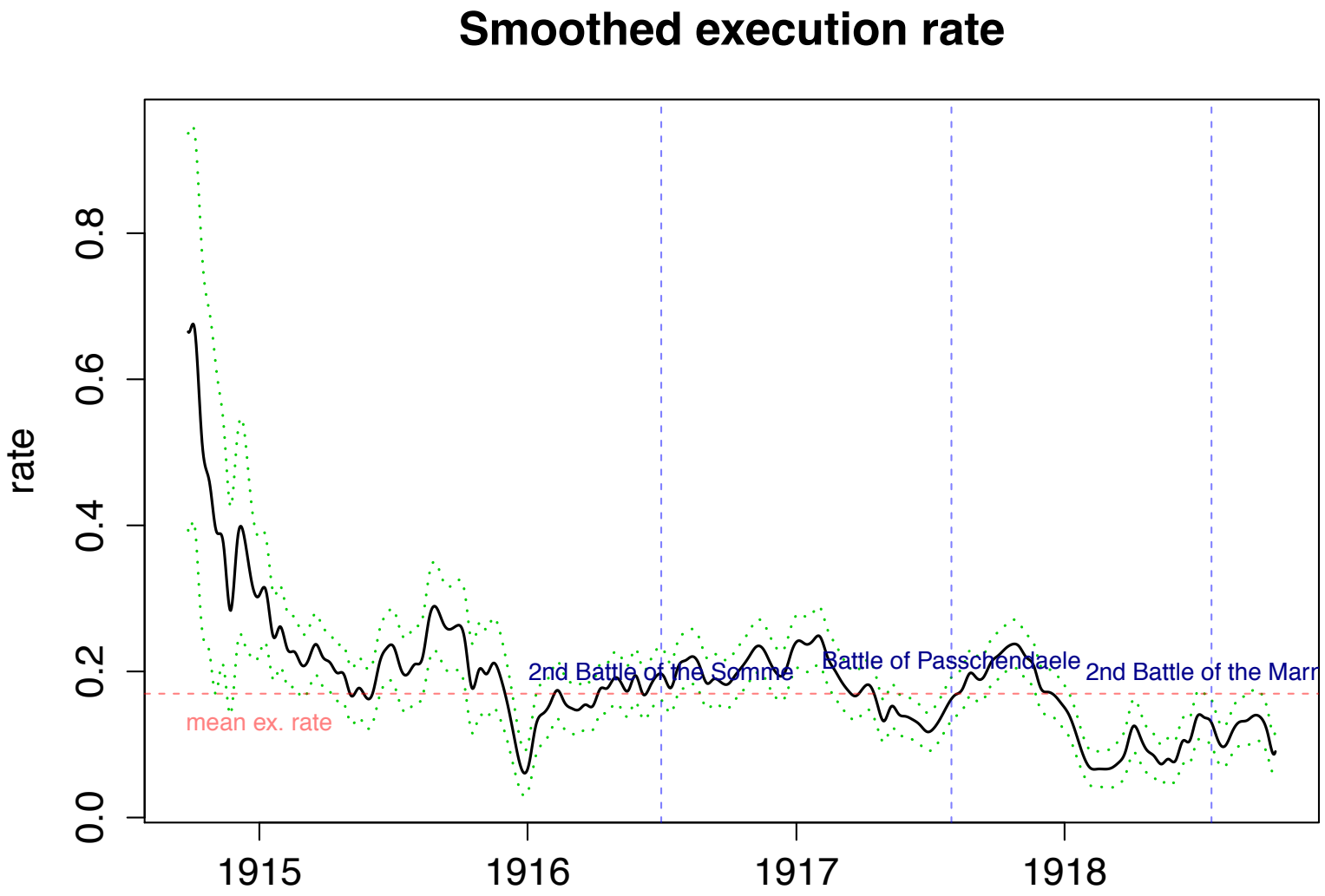

Smoothed execution rate
rate
0.0
0.2
0.4
0.6
0.8
1915
1916
1917
1918
mean ex. rate
2nd Battle of the Somme
Battle of Passchendaele
2nd Battle of the Marn

Figure 4

MILITARY LAW: Table No 7

Procedure at Field General Court-Martial.

Before Trial. Read carefully the "Charge" and particulars (if any). Compare them with the corresponding Section of the Act and satisfy yourself that the offence with which the accused is charged is an offence against the A A (See R P. 108). Charge cannot be altered by the Court. Study the whole section (including any proviso) and the notes thereon in M/M/L. Split up the offence into its component parts & make a note of the points that will have to be established by the evidence to secure a conviction

At the Trial.

Members satisfy themselves that Court is duly constituted (a)
Accused, prosecutors, witnesses, etc. admitted and Convening Order (b) read
Each accused is given his right of "challenge" (c)
The members of the Court are sworn. (d)
Everybody marched out except accused in first case (e) escort, prosecutor, and "friend of accused."
The accused is "arraigned." (f) In reply he may either:

Plead NOT GUILTY (g)

Evidence of Witnesses for PROSECUTION (j)

Accused informed he may either:—
* Give evidence on oath.
† Make a statement (not on oath),
† Hand in a written statement.
† Make no statement

Evidence of witnesses for DEFENCE.
Accused calls or no witnesses.

If accused wishes,
Evidence* as to CHARACTER before Finding.

Plead GUILTY (h)

FINDING of Guilty

SUMMARY OF EVIDENCE put in. If none, sufficient evidence taken on oath to enable Confirming Officer to judge of gravity of offence.

Accused can make statement in mitigation (k) (May be on oath).

* And call witnesses as to CHARACTER, produce testimonials, etc.

Subsequent procedure same as after the word "GUILTY" below

* Liable to cross-examination
† Not Liable

COURT CLOSED for consideration of the FINDING.

ACQUITTAL — Findings read in open Court and accused released

or

GUILTY

or

SPECIAL FINDING (l) or COURT ADJOURNS to consult Convening Officer — e.g. if doubtful point of law has arisen

Evidence as to Character (and particulars of service) after the Finding. Field Conduct Sheet put in.

Accused asked if he desires to say anything further (as to character, in mitigation of punishment, or otherwise) & record made of his statement.

The SENTENCE

After Trial

Confirmation. | Promulgation. | Review | or | Suspension of Sentence

NOTES AND REFERENCES.

(a) Vide R/P 106, 107.
(b) Page 1 of A/F A3.
(c) Vide R/P 110.
(d) Section 52 A/A. R/P 109, 111.
(e) R/P 109.
(f) R/P 112.
(g) R/P 114.
(h) Comply with R/P 35 (b) & 37.
(j) Refer to Chapter VI., M/M/L (especially paras. 30—40, 58—71).
(k) R/P 37 (f).
(l) R/P 44.

Figure 5

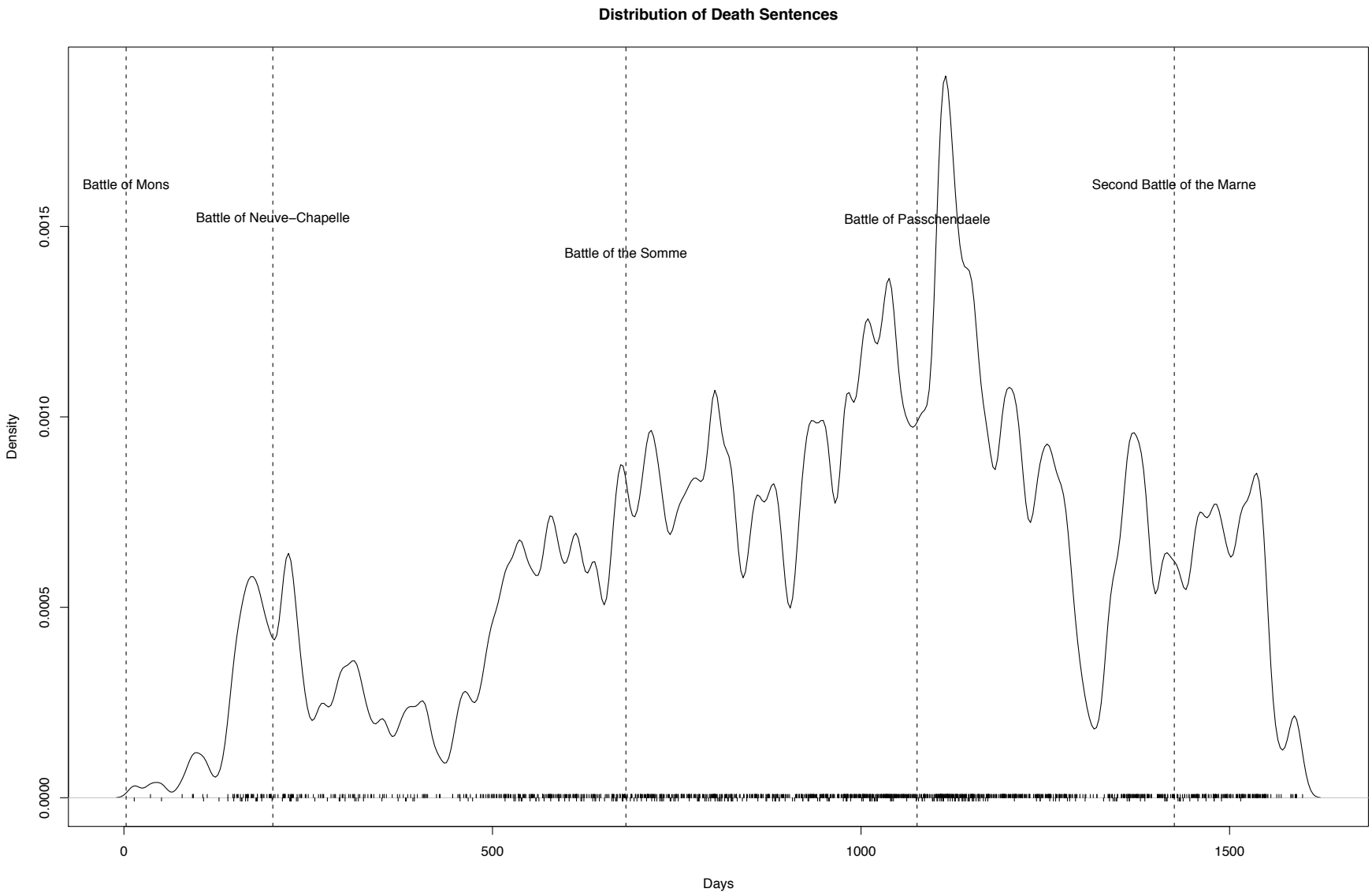
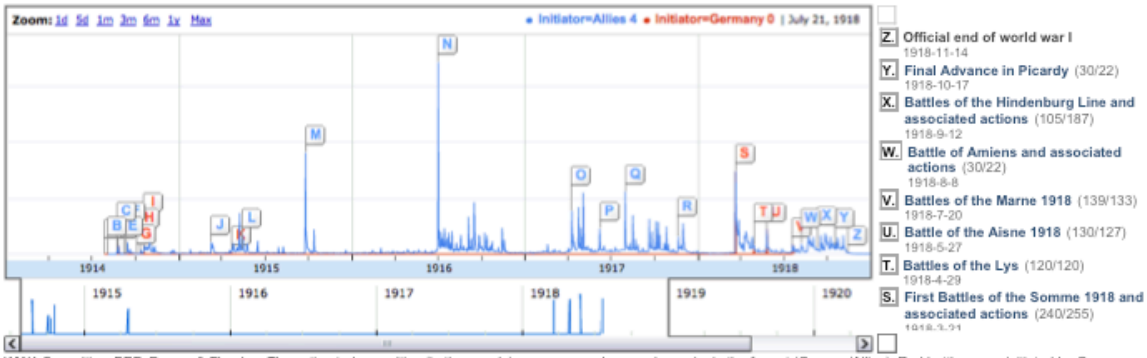
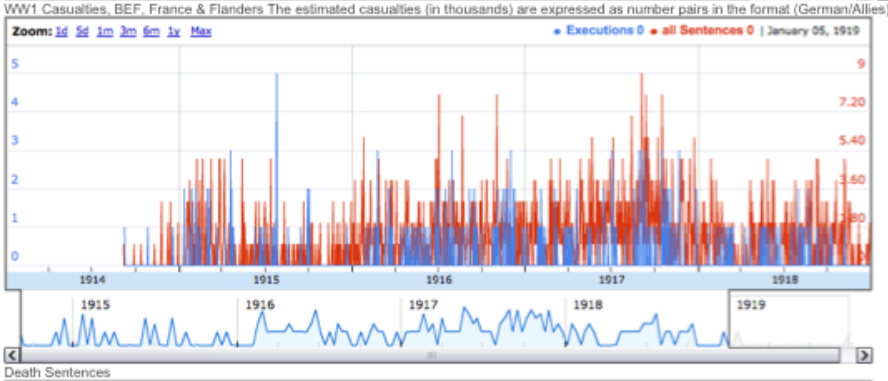

Distribution of Death Sentences
Battle of Mons
Battle of Neuve−Chapelle
Battle of the Somme
Battle of Passchendaele
Second Battle of the Marne
Density
0.0000
0.0005
0.0010
0.0015
0
500
1000
1500
Days

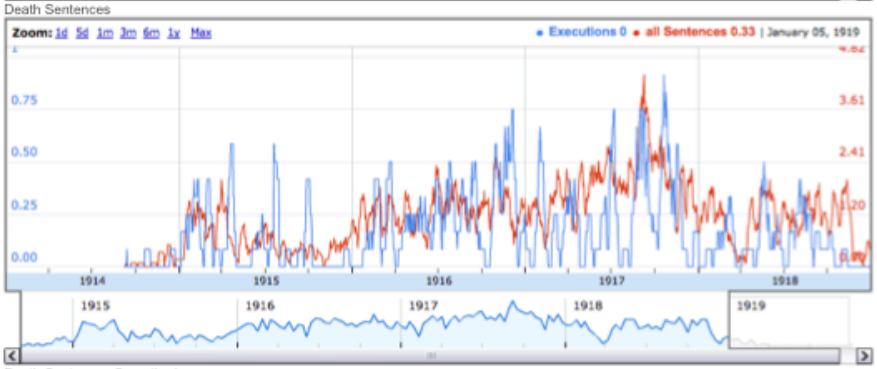
Figure 6



WW1 Casualties, BEF, France & Flanders The estimated casualties (in thousands) are expressed as number pairs in the format (German/Allies). Red battles were initiated by Germany.



Death Sentences



Death Sentences Smoothed

Figure 7

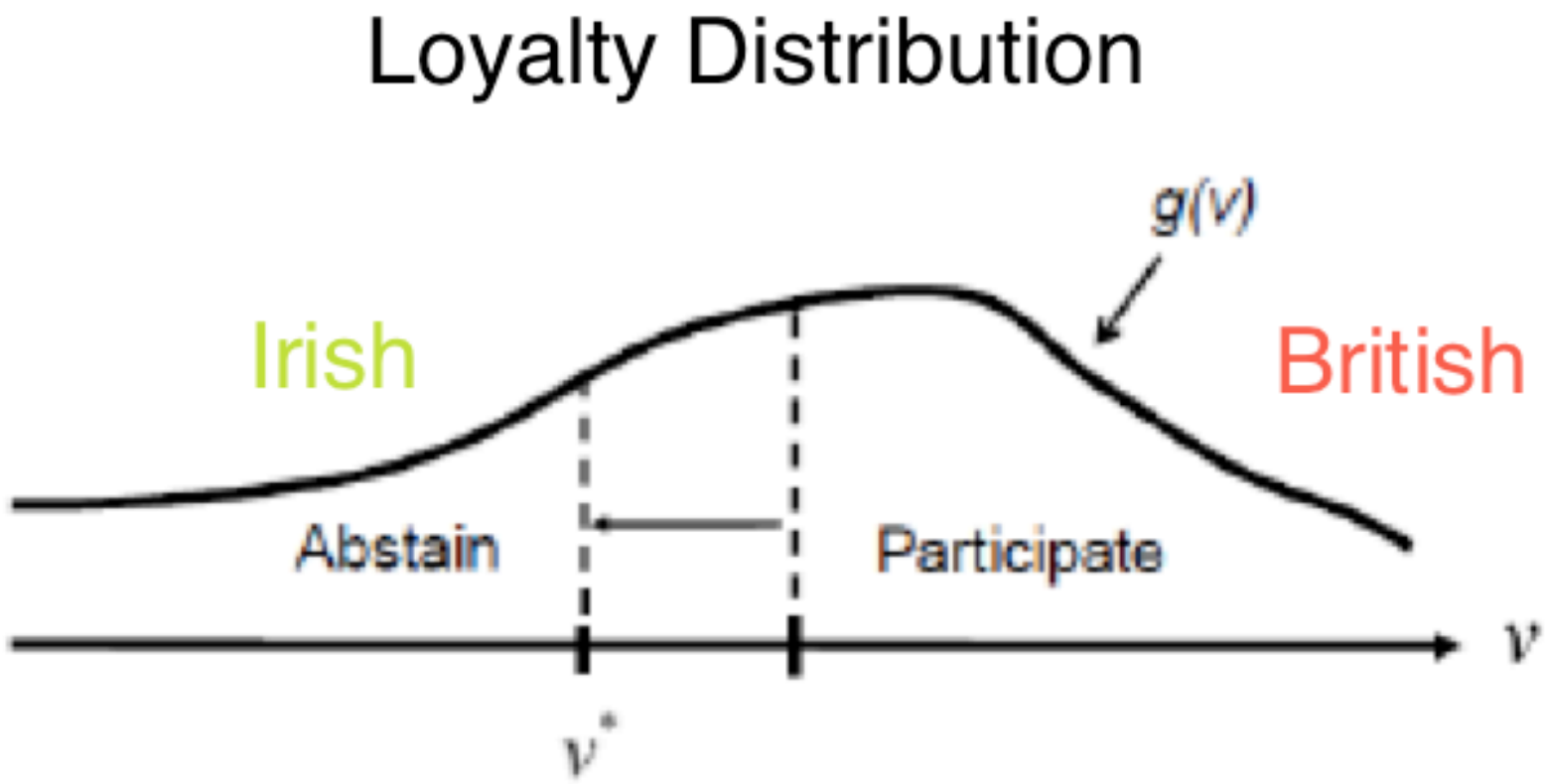

Loyalty Distribution
g(v)
Irish
British
Abstain
Participate
v
v*

Figure 8A

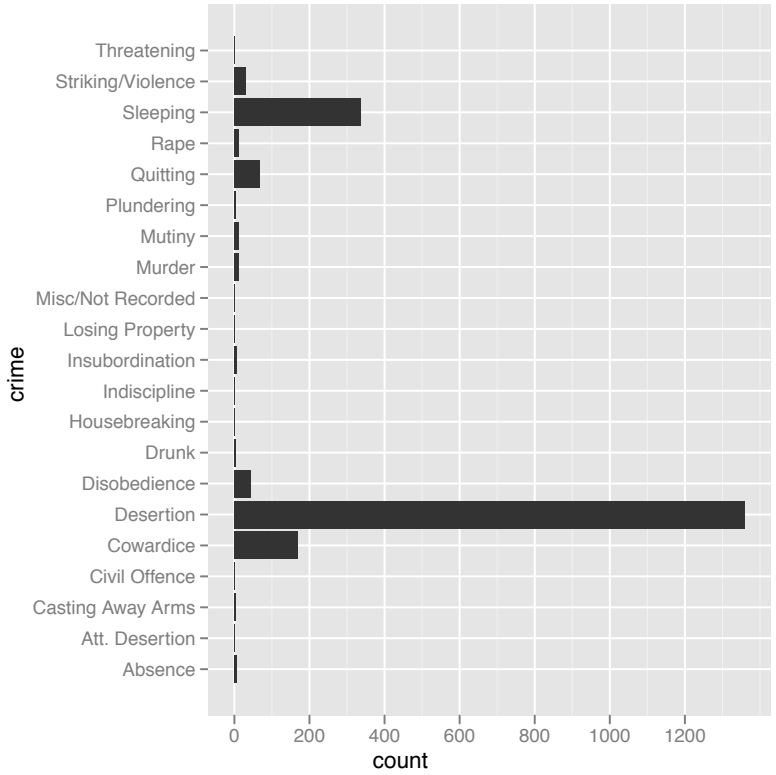


Figure 8B

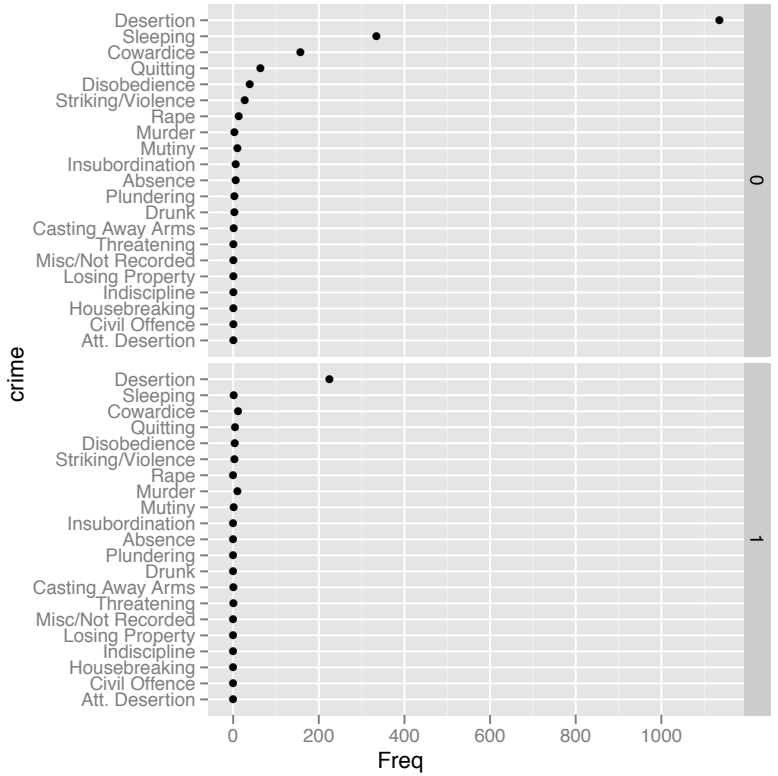
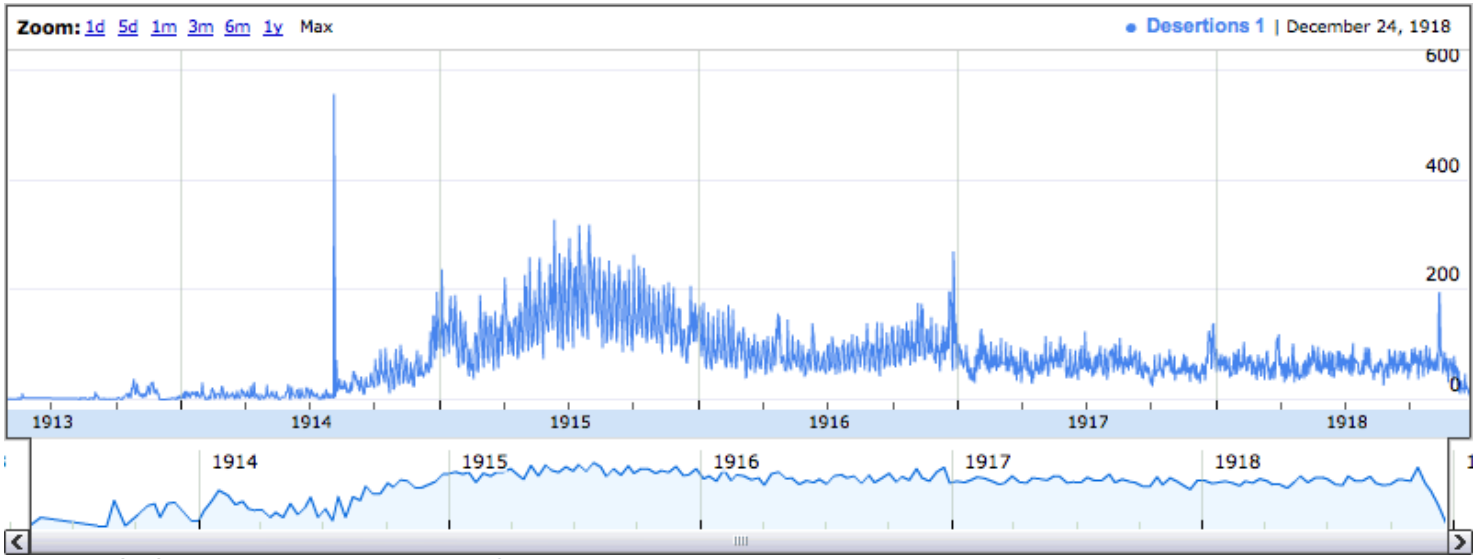


Figure 9A



Data: DesTS • Chart ID: AnnotatedTimeLineID142fa67e8691b
R version 2.15.1 (2012-06-22) • googleVis-0.2.17 • Google Terms of Use • Data Policy

Figure 9B

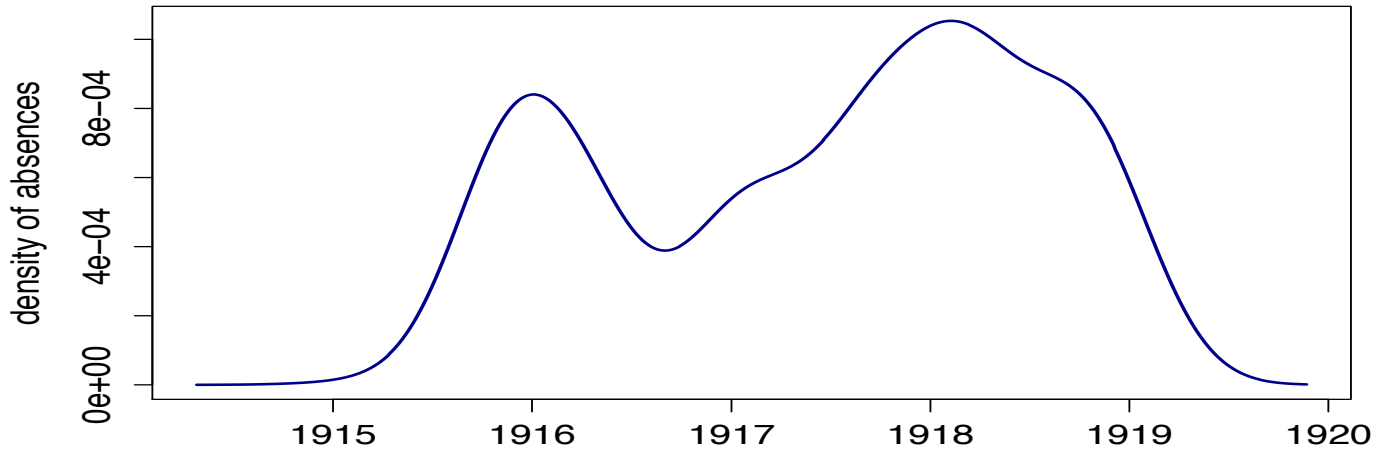


Figure 10A

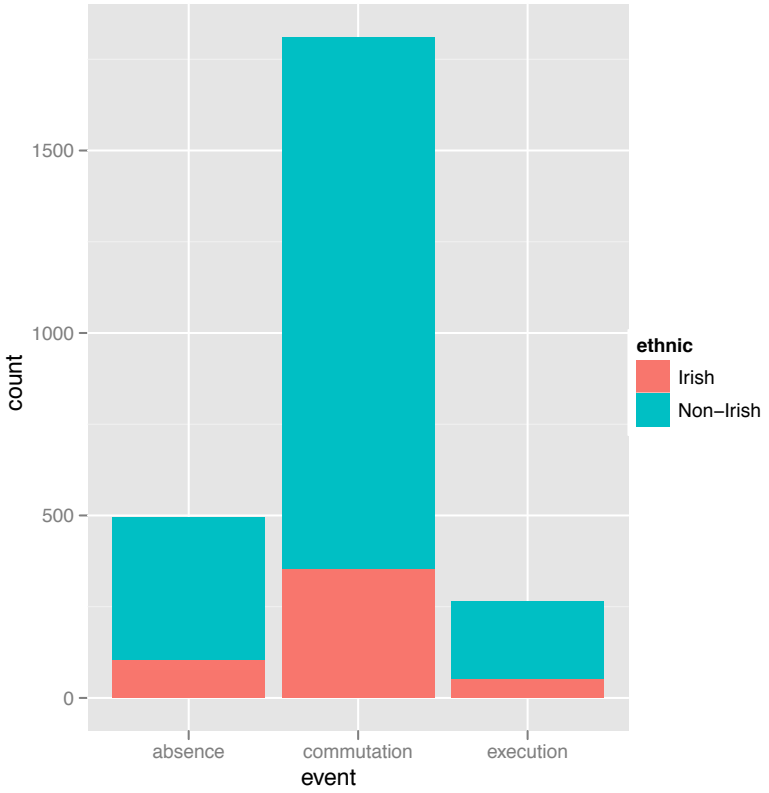


Figure 10B

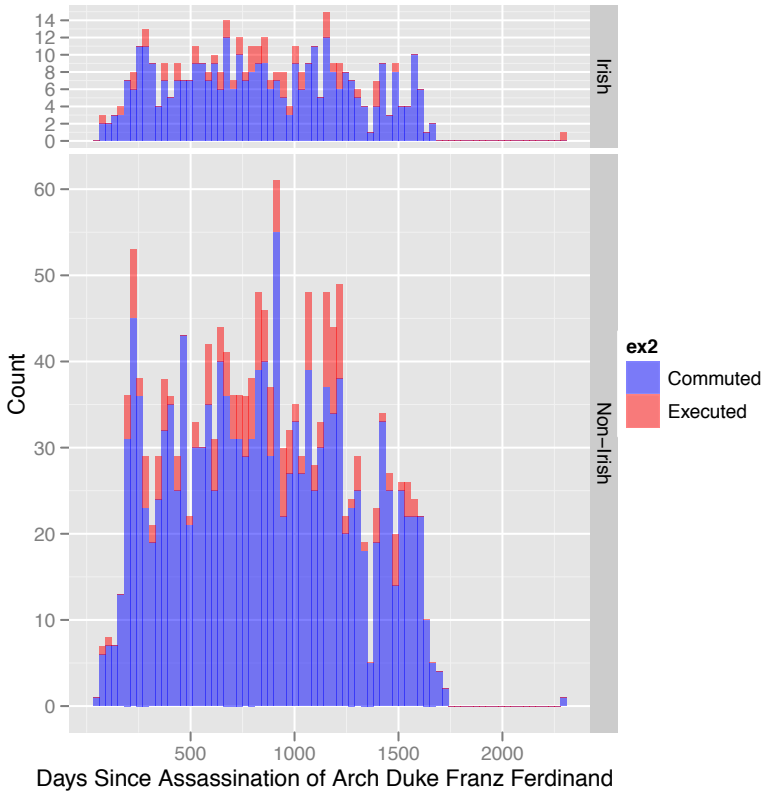
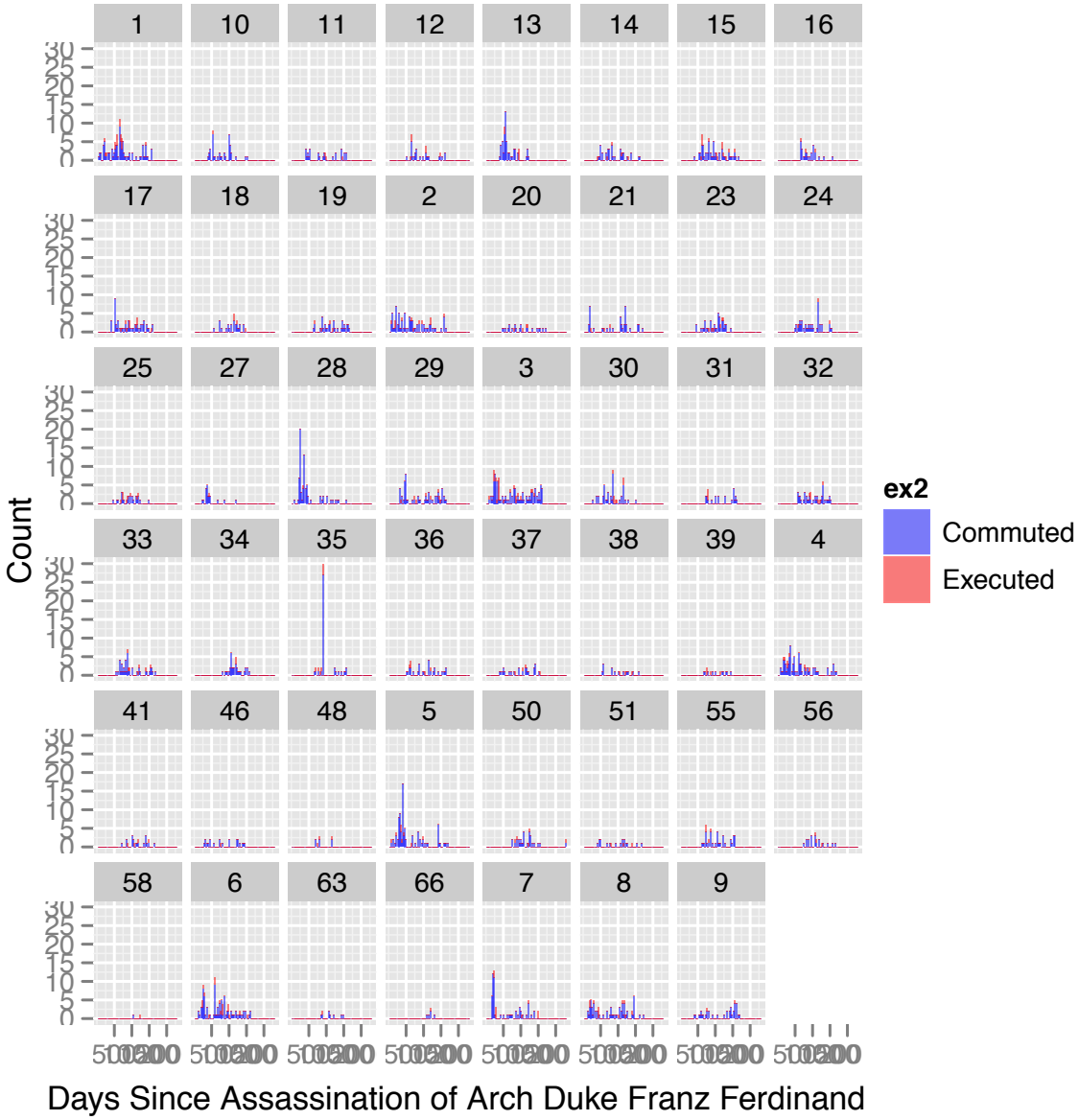


Figure 11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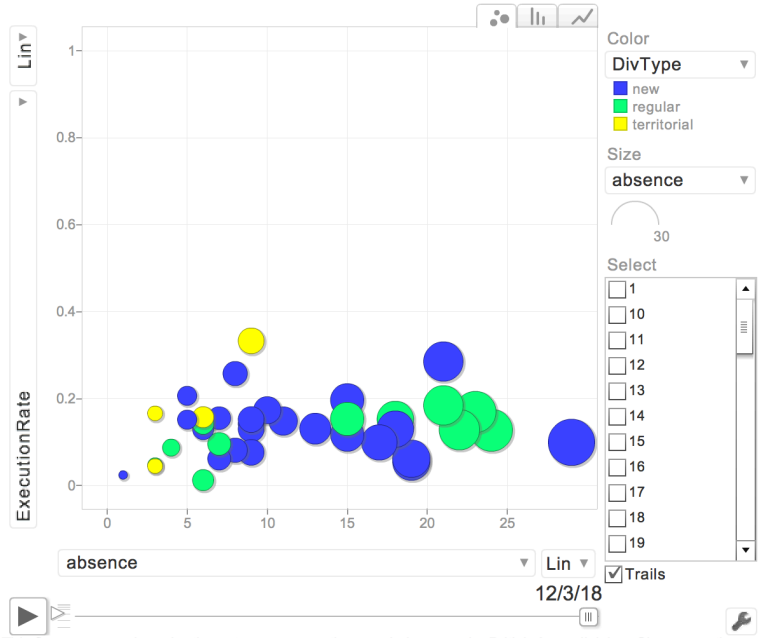
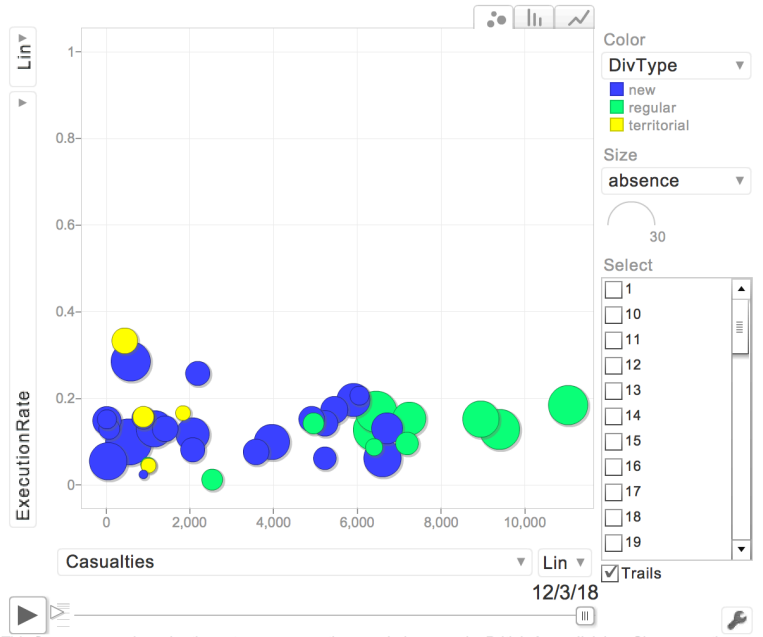
Count
Days Since Assassination of Arch Duke Franz Ferdinand
ex2
Commuted
Executed

Figure 12A



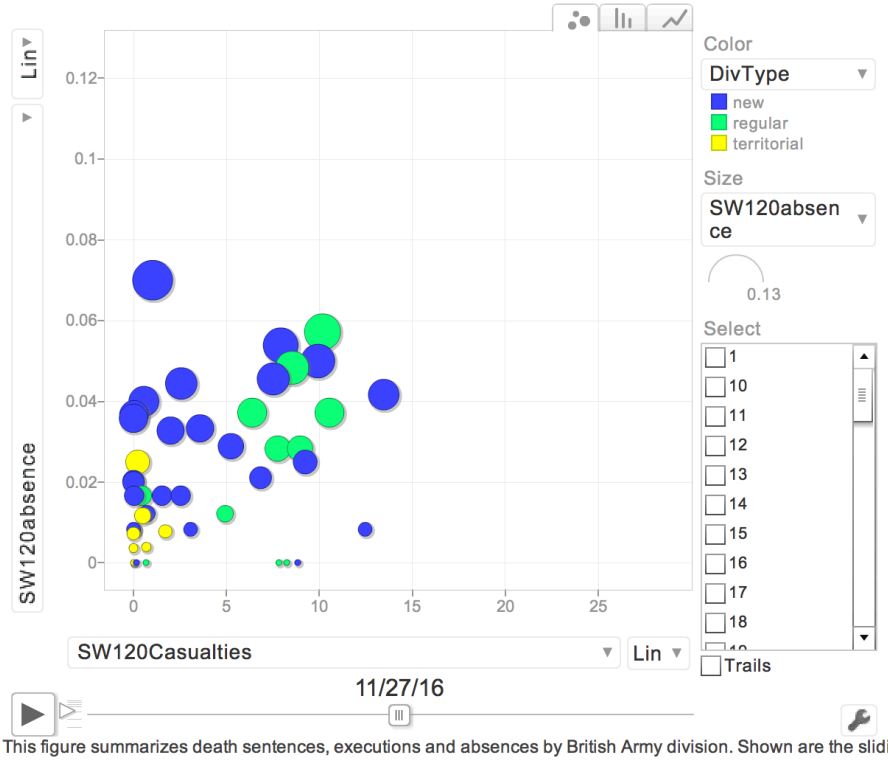
This figure summarizes death sentences, executions and absences by British Army division. Shown are the cumulative sums

Figure 12B



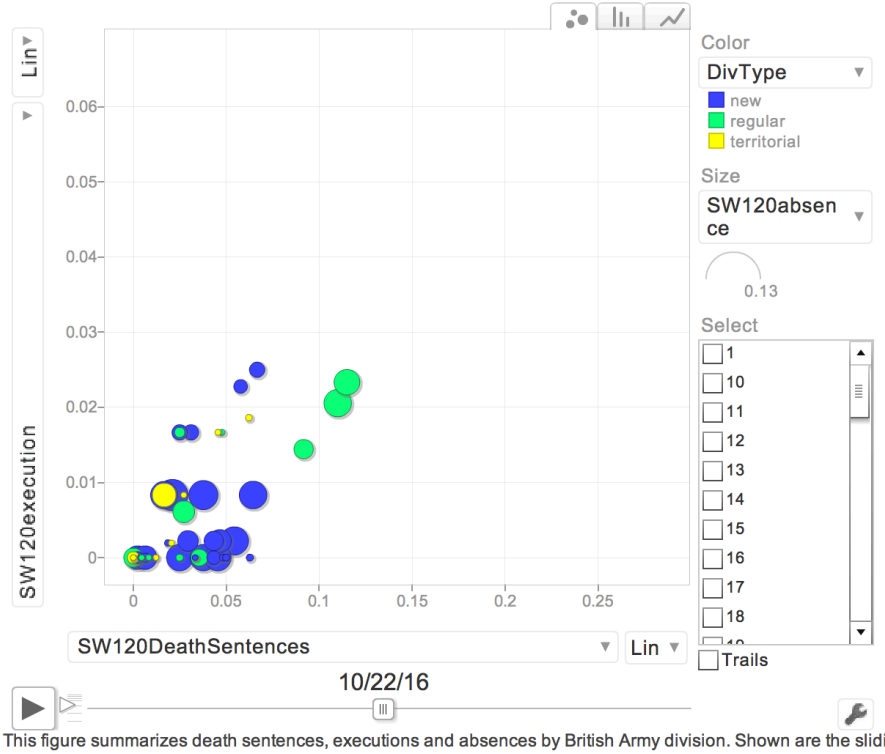
This figure summarizes death sentences, executions and absences by British Army division. Shown are the cumulative sums

Figure 12C



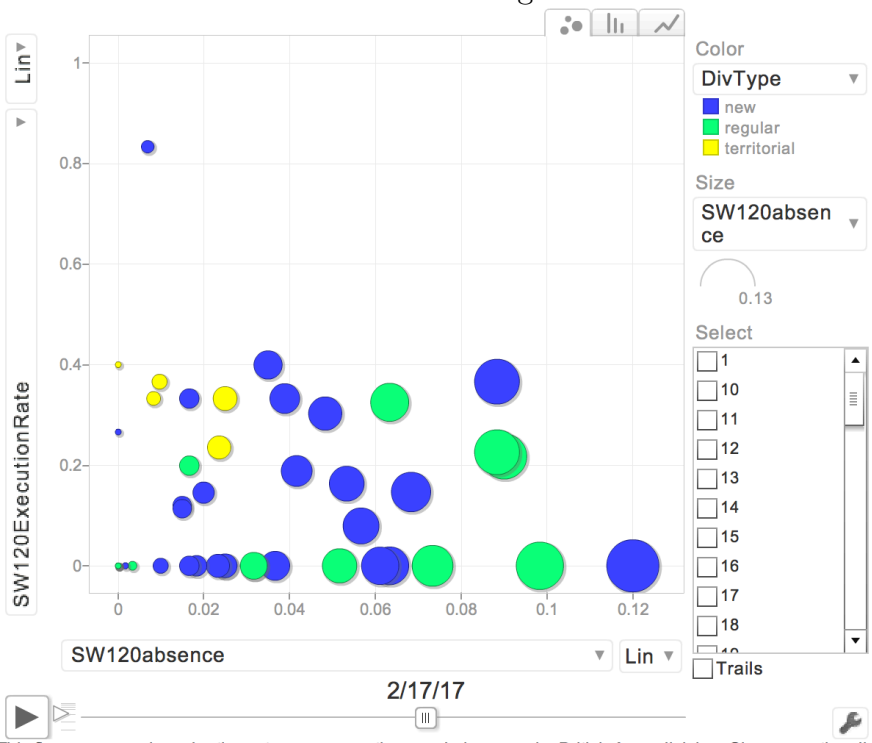
This figure summarizes death sentences, executions and absences by British Army division. Shown are the sliding window averages (120 days)

Figure 12D



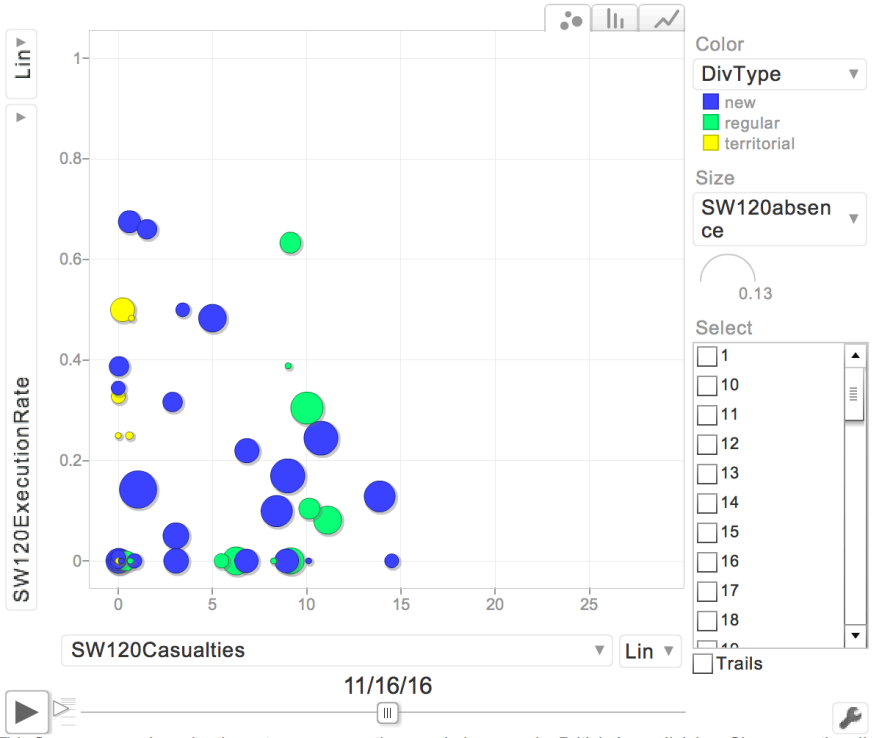
This figure summarizes death sentences, executions and absences by British Army division. Shown are the sliding window averages (120 days)

Figure 12E



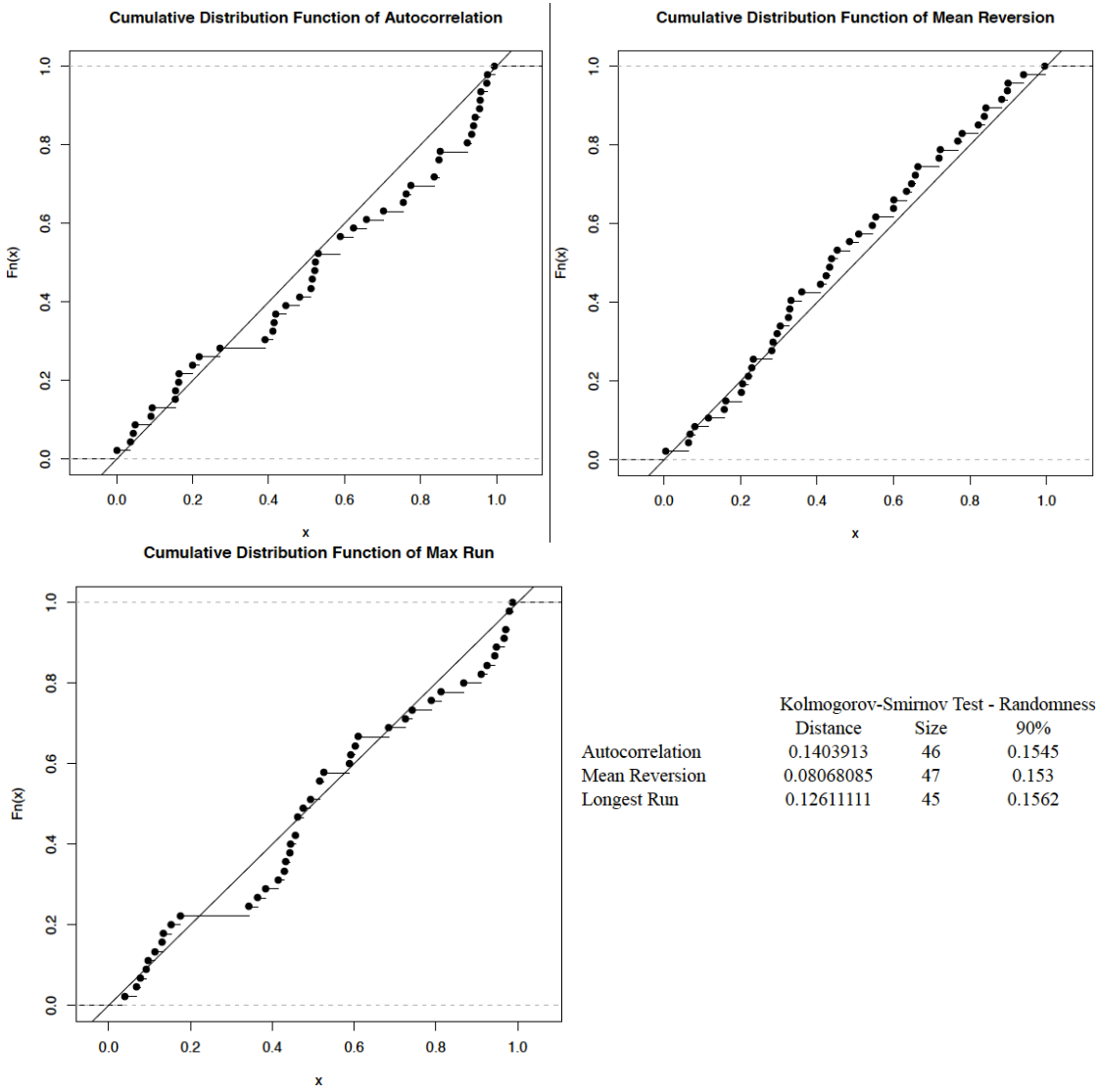
This figure summarizes death sentences, executions and absences by British Army division. Shown are the sliding window averages (120 days)

Figure 12F



This figure summarizes death sentences, executions and absences by British Army division. Shown are the sliding window averages (120 days)

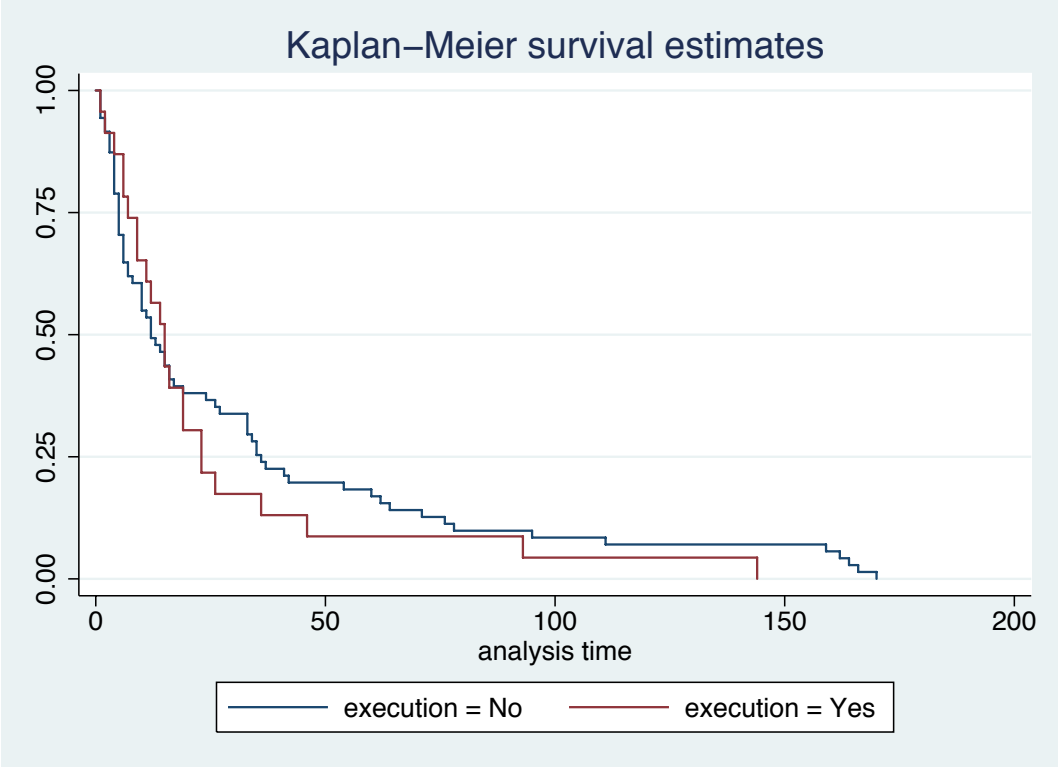
Figure 13



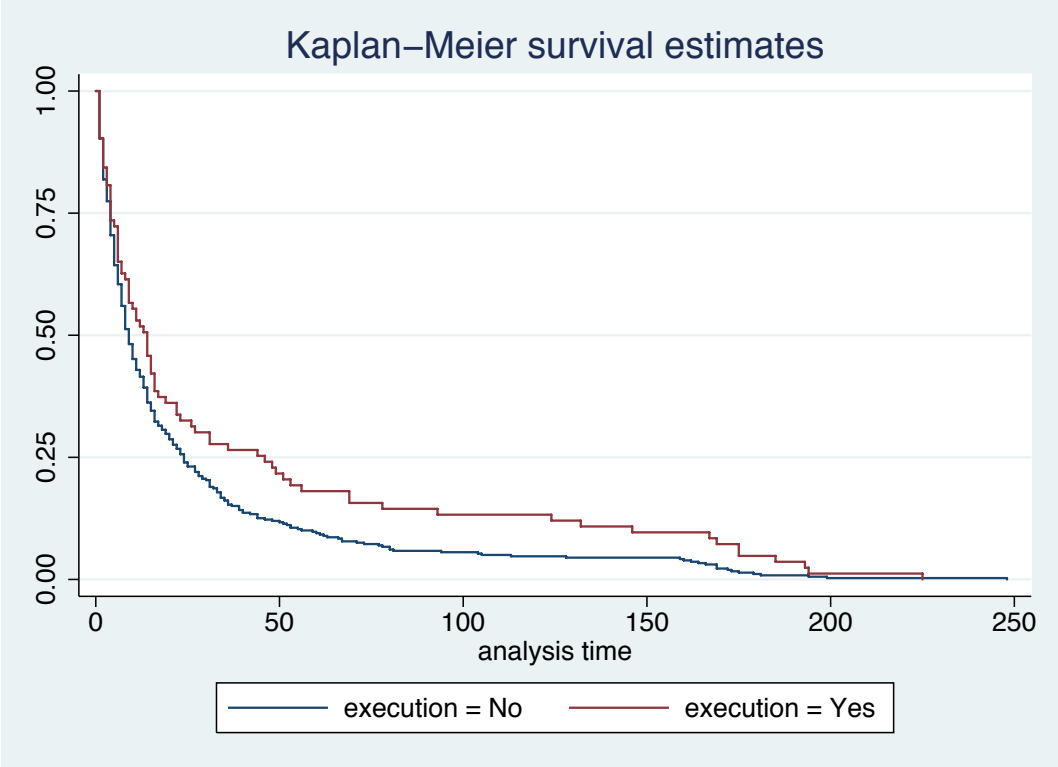
Kolmogorov-Smirnov Test - Randomness Check of P-Values

	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 14A: Non-Parametric Survival Distributions (War Diaries)

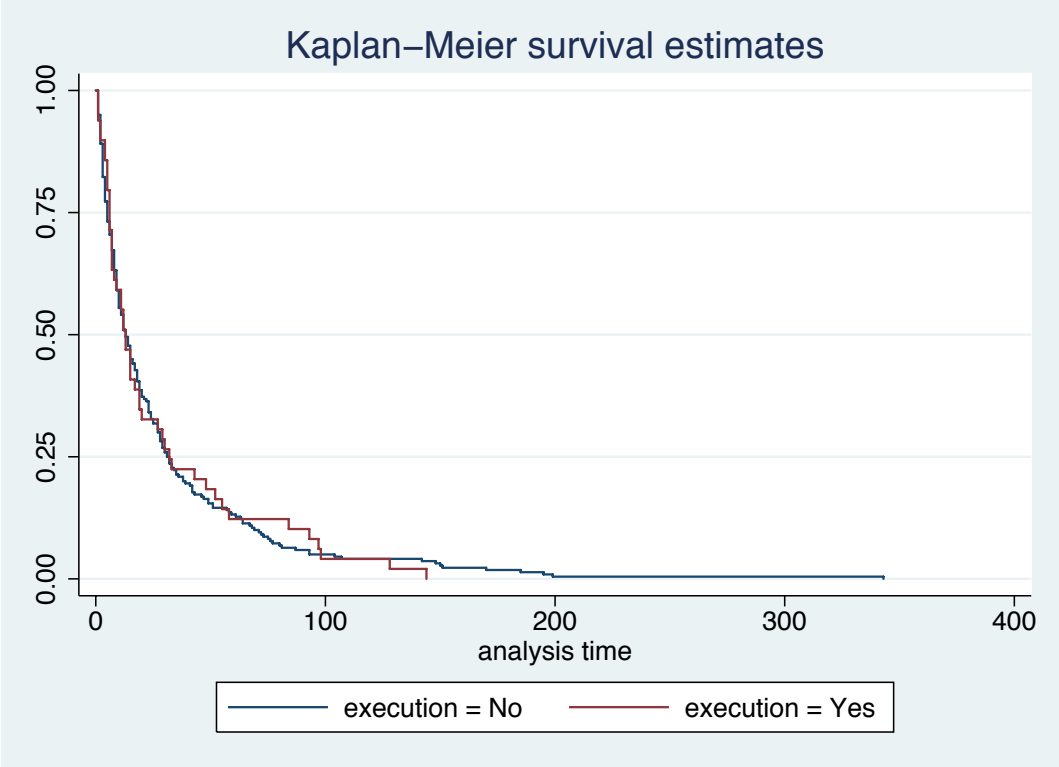


(a) Irish Executions Only

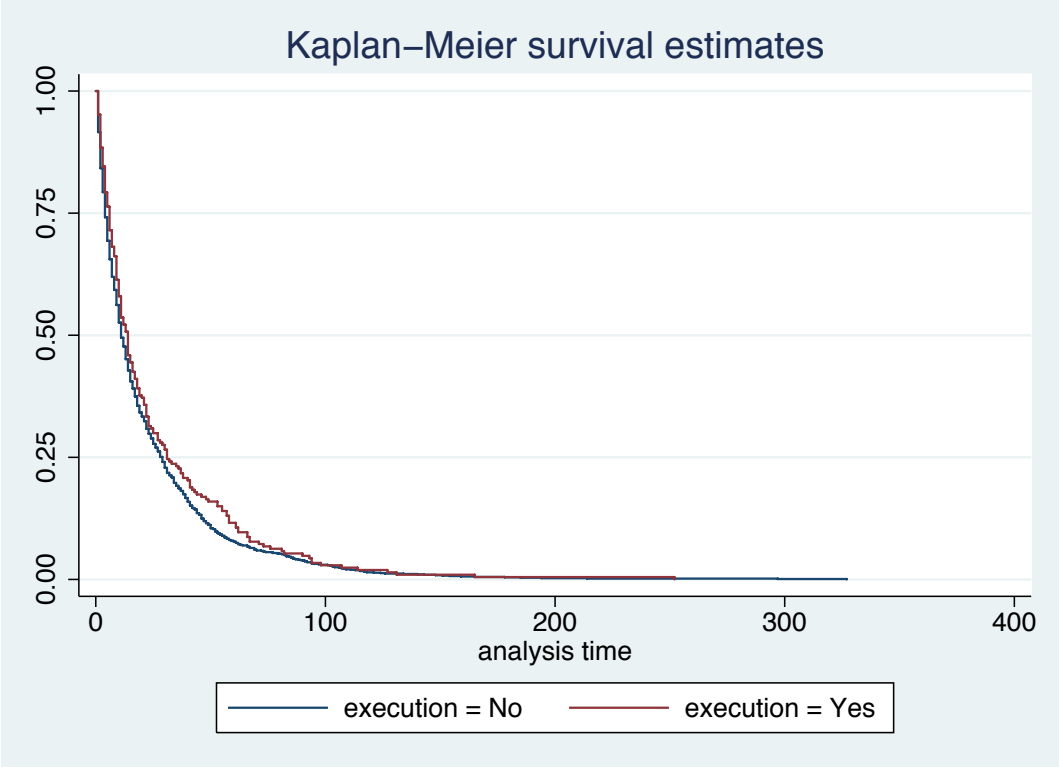


(b) Non-Irish Executions Only

Figure 14B: Non-Parametric Survival Distributions (Police Gazettes)

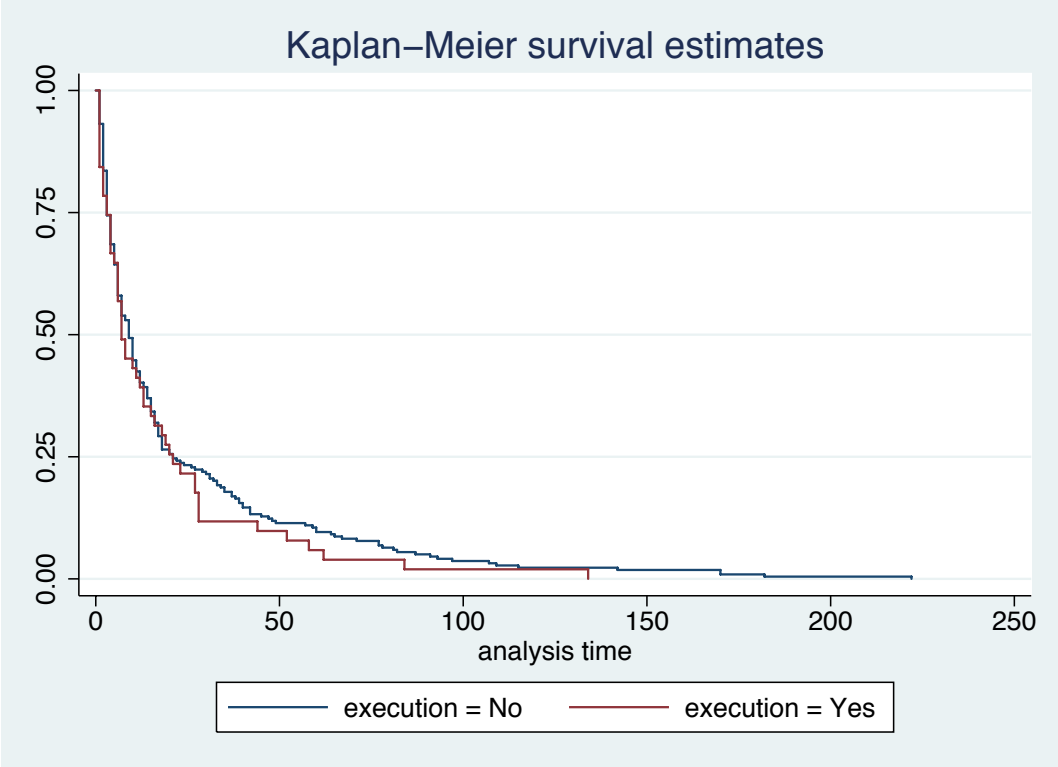


(a) Irish Executions Only

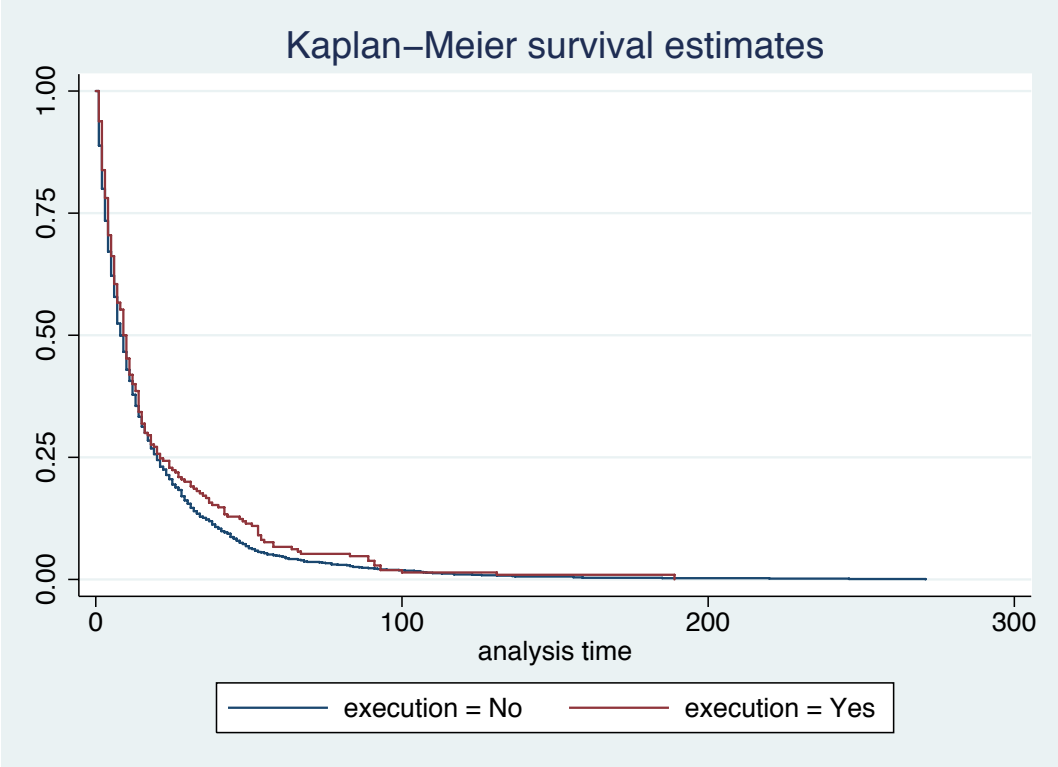


(b) Non-Irish Executions Only

Figure 14C: Non-Parametric Survival Distributions (FGCM)



(a) Irish Executions Only



(b) Non-Irish Executions Only

Table 1: Are Observable Characteristics Correlated with Execution Decisions? (Deserters)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Irish	-0.00133	-0.00162	0.00750	-0.00261	-1.93e-14	-0.000283	-0.000398	-0.00313	-0.0147	-0.00467	0.0131
	(0.0214)	(0.0214)	(0.0143)	(0.0214)	(0.230)	(0.0215)	(0.0214)	(0.0219)	(0.0244)	(0.0218)	(0.0181)
Private		-0.0577									-0.0265
		(0.0443)									(0.0371)
Age			-0.00464								0.00274
			(0.00316)								(0.00396)
1915				-0.138	-0.135					-0.210+	-0.0495
				(0.0951)	(0.107)					(0.110)	(0.115)
1916				-0.148	-0.150					-0.246*	-0.00752
				(0.0934)	(0.105)					(0.109)	(0.114)
1917				-0.205*	-0.205*					-0.288**	-0.0376
				(0.0930)	(0.104)					(0.109)	(0.114)
1918				-0.222*	-0.219*					-0.320**	-0.0678
				(0.0939)	(0.105)					(0.110)	(0.114)
Irish x 1915					-0.0152						
					(0.237)						
Irish x 1916					0.00798						
					(0.234)						
Irish x 1917					0.00133						
					(0.233)						
Irish x 1918					-0.0156						
					(0.235)						
New Army									0.0185		
									(0.0214)		
Territorial Army									-0.00713		
									(0.0303)		
ΔLog Casualties										-0.00134	-0.00493
										(0.00742)	(0.00565)
ΔLog Casualties 30 Days Ago										0.00327	-0.00292
										(0.00718)	(0.00540)
Distance to Coast											-0.000474+
											(0.000268)
Distance to Berlin											0.000240
											(0.000467)
Year Fixed Effects	N	N	N	Y	Y	N	N	N	N	Y	Y
Month Fixed Effects	N	N	N	N	N	Y	N	N	N	N	N
Day of Week Fixed Effects	N	N	N	N	N	N	Y	N	N	N	N
Division Fixed Effects	N	N	N	N	N	N	N	Y	N	Y	Y
Joint Test of Fixed Effects				0.000883	0.00575	0.750	0.258	0.0272	0.590		
Joint Test of Irish x Year FE					0.995						
Joint Test of Casualties										0.839	
Joint Test of Distance											0.209
Constant	0.151**	0.206**	0.984**	0.334**	0.333**	0.144**	0.178**	0.188*	0.162**	0.493**	0.757*
	(0.00959)	(0.0436)	(0.0801)	(0.0921)	(0.103)	(0.0306)	(0.0275)	(0.0889)	(0.0154)	(0.140)	(0.379)
N	1741	1741	1741	1741	1741	1741	1741	1741	1418	1741	1190
R-sq	0.000	0.001	0.554	0.011	0.011	0.004	0.004	0.055	0.001	0.066	0.609

Notes: All regressions use ordinary least squares on death sentences occurring in France & Flanders before the end of World War I. Death sentences recorded without Divisions or from the Labour Corps were removed. Log Casualties is calculated as log(1+Casualties). ΔLog Casualties is defined as the difference in Log Casualties 1 to 29 Days Ago vs. 30 to 59 Days Ago. ΔLog Casualties 30 Days Ago is defined as the difference in Log Casualties 30 to 59 Days Ago vs. 60 to 89 Days Ago. Distances are set to missing before the first battle and after the last battle. Territorial/New/Regular Army status is not assigned for Indian, Australian, Canadian, or New Zealand Divisions. Regressions including age also dummy out age when it is missing (i.e., assign a constant and include an indicator for age being missing). Standard errors in parentheses; + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$. Results are similar with Logit or Probit.

Table 2: Are Observable Characteristics Correlated with Execution Decisions? (Deserters)

Panel A:	Joint Test of Significance
Brigade Unit	0.106
Corp Unit	0.230
Army Unit	0.242
Brigade Commanding Officer	0.872
Division Commanding Officer	0.0211
Division 1st General Staff Officer	0.109
Corp Commanding Officer	0.527
Corp 1st General Staff Officer	0.529
Army Commanding Officer	0.214
Army 1st General Staff Officer	0.182
GHQ Commanding Officer	0.129
GHQ 1st General Staff Officer	0.277
Irish Soldier x Irish Officer FE	0.659
Military Indiscipline 30-59 & 60-89 days ago	0.482
Death Sentences 30-59 & 60-89 days ago	0.139
Execution Rate 30-59 & 60-89 days ago	0.415
Panel B:	
Aggregation level	Correlation with Lag Decision
Division	0.0495
	(0.0331)
Brigade	0.00376
	(0.0387)
Corp	0.0225
	(0.0330)
Army	0.0282
	(0.0354)
Army Type	-0.0343
	(0.0359)
All	0.0354
	(0.0508)

Notes: Data is restricted to death sentences occurring in France & Flanders before the end of World War I. Death sentences recorded without Divisions or from the Labour Corps were removed. In Panel A, each row reports a separate ordinary least squares regression and tests of joint significance of the fixed effects or measures of the recent battle environment. Military indiscipline and death sentences are calculated as log(1+number). Military indiscipline is the average of absentees and trials measured from the War Diaries, Police Gazettes, and FGCM trial registries. Lag execution rates is a set of controls comprising the numbers of executions and commutations within each time window. Units or officers that appeared with less than 10 frequency were categorized in a separate "other" category. All regression models include year, division, and Irish fixed effects. In Panel B, each row reports a separate ordinary least squares stacked autocorrelation regression. The strings of events within each unit were stacked and the first event within each unit was excluded as a dependent variable. If more than one event occurred on a day within a unit, the average outcome was calculated for that day. All regression models include year fixed effects and the leave-one-out mean execution rate of the unit. Standard errors in parentheses; + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$. Results are similar with Logit or Probit.

Table 3: Are Observable Characteristics Correlated with Execution Decisions? (All Death Sentences)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Irish	0.00998	0.00969	0.0136	0.0101	0.0619	0.0106	0.0114	0.0106	0.00105	-0.000867	0.0697	0.0117
	(0.0171)	(0.0170)	(0.0110)	(0.0171)	(0.0961)	(0.0171)	(0.0171)	(0.0173)	(0.0188)	(0.0169)	(0.0951)	(0.0141)
Private		-0.0842*									-0.0768*	-0.0160
		(0.0327)									(0.0329)	(0.0272)
Age			-0.00302									0.00153
			(0.00261)									(0.00327)
New Army									-0.00922			
									(0.0161)			
Territorial Army									-0.0143			
									(0.0236)			
Desert										0.0970	0.0994	0.0603
										(0.0673)	(0.0674)	(0.0626)
Coward										-0.00968	-0.0128	-0.00141
										(0.0713)	(0.0714)	(0.0652)
Disobedience										-0.0366	-0.0367	-0.000702
										(0.0827)	(0.0828)	(0.0781)
Murder										0.872**	0.861**	0.377**
										(0.115)	(0.115)	(0.0948)
Mutiny										0.186+	0.189+	0.0383
										(0.108)	(0.108)	(0.0953)
Quit										-0.0290	-0.0315	-0.00165
										(0.0781)	(0.0782)	(0.0705)
Sleep										-0.0820	-0.0758	-0.00236
										(0.0696)	(0.0698)	(0.0645)
Striking										0.0466	0.0491	0.113
										(0.0898)	(0.0899)	(0.0822)
Rape										-0.0467	-0.0473	0.0253
										(0.104)	(0.104)	(0.107)
ΔLog Casualties											0.000332	-0.00213
											(0.00553)	(0.00437)
ΔLog Casualties											0.00487	-0.00140
30 Days Ago											(0.00512)	(0.00410)
Distance to Coast												-0.000409+
												(0.000211)
Distance to Berlin												0.000262
												(0.000383)
Year Fixed Effects	N	N	N	Y	Y	N	N	N	N	Y	Y	Y
Month Fixed Effects	N	N	N	N	N	Y	N	N	N	N	N	N
Day Fixed Effects	N	N	N	N	N	N	Y	N	N	N	N	N
Division Fixed Effects	N	N	N	N	N	N	N	Y	N	Y	Y	Y
Joint Test of Fixed Effects				0.0790	0.252	0.486	0.111	0.0554	0.770			
Joint Test of Irish x Year FE					0.864							
Joint Test of Casualties											0.615	
Joint Test of Distance												0.152
Constant	0.123**	0.204**	0.956**	0.0824*	0.0714	0.116**	0.157**	-2.90e-13	0.138**	0.113	0.160	0.665*
	(0.00751)	(0.0321)	(0.0668)	(0.0394)	(0.0442)	(0.0250)	(0.0217)	(0.329)	(0.0119)	(0.330)	(0.332)	(0.338)
N	2408	2408	2408	2408	2408	2408	2408	2408	2044	2408	2408	1612
R-sq	0.000	0.003	0.583	0.004	0.004	0.005	0.004	0.040	0.000	0.099	0.102	0.637

Notes: All regressions use ordinary least squares on death sentences occurring in France & Flanders before the end of World War I. Death sentences recorded without Divisions or from the Labour Corps were removed. Log Casualties is calculated as log(1+Casualties). ΔLog Casualties is defined as the difference in Log Casualties 1 to 29 Days Ago vs. 30 to 59 Days Ago. ΔLog Casualties 30 Days Ago is defined as the difference in Log Casualties 30 to 59 Days Ago vs. 60 to 89 Days Ago. Distances are calculated based on the soldier's unit's participation in battles and are interpolated between battles. Distances are set to missing before the first battle and after the last battle. Territorial/New/Regular Army status is not assigned for Indian, Australian, Canadian, or New Zealand Divisions. Regressions including age also dummy out age when it is missing (i.e., assign a constant and include an indicator for age being missing). Standard errors in parentheses; + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$. Results are similar with Logit or Probit.

Table 4: Are Observable Characteristics Correlated with Execution Decisions? (All Death Sentences)

Panel A:	Joint Test of Significance
Brigade Unit	0.277
Corp Unit	0.190
Army Unit	0.328
Brigade Commanding Officer	0.670
Division Commanding Officer	0.185
Division 1st General Staff Officer	0.517
Corp Commanding Officer	0.366
Corp 1st General Staff Officer	0.0900
Army Commanding Officer	0.0688
Army 1st General Staff Officer	0.308
GHQ Commanding Officer	0.369
GHQ 1st General Staff Officer	0.455
Irish Soldier x Irish Officer FE	0.452
Military Indiscipline 30-59 & 60-89 days ago	0.325
Death Sentences 30-59 & 60-89 days ago	0.109
Execution Rate 30-59 & 60-89 days ago	0.324
Panel B:	
Aggregation level	Correlation with Lag Decision
Division	0.0195
	(0.0285)
Brigade	0.00486
	(0.0333)
Corp	0.0469
	(0.0288)
Army	-0.00508
	(0.0318)
Army Type	-0.00762
	(0.0333)
All	0.0790
	(0.0503)

Notes: Data is restricted to death sentences occurring in France & Flanders before the end of World War I. Death sentences recorded without Divisions or from the Labour Corps were removed. In Panel A, each row reports a separate ordinary least squares regression and tests of joint significance of the fixed effects or measures of the recent battle environment. Military indiscipline and death sentences are calculated as log(1+number). Military indiscipline is the average of absentees and trials measured from the War Diaries, Police Gazettes, and FGCM trial registries. Lag execution rates is a set of controls comprising the numbers of executions and commutations within each time window. Units or officers that appeared with less than 10 frequency were categorized in a separate "other" category. All regression models include year, division, and Irish fixed effects. In Panel B, each row reports a separate ordinary least squares stacked autocorrelation regression. The strings of events within each unit were stacked and the first event within each unit was excluded as a dependent variable. If more than one event occurred on a day within a unit, the average outcome was calculated for that day. All regression models include year fixed effects and the leave-one-out mean execution rate of the unit. Standard errors in parentheses; + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$. Results are similar with Logit or Probit.

Table 5: Effects of Executions vs. Commutations on Elapsed Time Until Next Absence

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

Notes: Outcome is elapsed time from death sentence resolution (execution or commutation) until next absence. "Exp", "Wb" and "Cox" use the exponential, Weibull and Cox models respectively to parameterize the baseline hazard. In columns 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. Log Casualties is calculated as log(1+Casualties). ΔLog Casualties is defined as the difference in Log Casualties 1 to 29 Days Ago vs. 30 to 59 Days Ago. ΔLog Casualties 30 Days Ago is defined as the difference in Log Casualties 30 to 59 Days Ago vs. 60 to 89 Days Ago. All specifications include division and year fixed-effects. War Diaries analysis restricts to July 1916-June 1917, which is the time window for the surviving data. Standard errors clustered at the division level in parentheses; + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

Table 6: Effects of Executions vs. Commutations on Elapsed Time Until Next Absence Differing by whether Case was a Desertion Trial and whether Soldier was Irish

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: War Diaries	Exp/+14	Wb/+14	Cox/+14	Exp/NN	Wb/NN	Cox/NN	Exp/C=T	Wb/C=T	Cox/C=T
Execution	-0.417	-0.394	-0.308	0.219	0.182	0.239	0.723*	0.627*	0.689*
	(0.736)	(0.687)	(0.670)	(0.359)	(0.324)	(0.313)	(0.338)	(0.304)	(0.273)
Desert	-0.0429	-0.0218	-0.00996	0.0470	0.0531	0.0511	0.138	0.146	0.133
	(0.305)	(0.274)	(0.240)	(0.302)	(0.265)	(0.234)	(0.311)	(0.283)	(0.246)
Ex-Desert	-0.00330	0.0467	-0.0154	-0.241	-0.161	-0.218	-0.650+	-0.555+	-0.627*
	(0.746)	(0.700)	(0.671)	(0.406)	(0.360)	(0.336)	(0.374)	(0.337)	(0.291)
Irish	-0.727**	-0.629**	-0.464**	-0.646**	-0.541**	-0.391*	-0.475+	-0.407+	-0.263
	(0.179)	(0.176)	(0.147)	(0.207)	(0.194)	(0.173)	(0.252)	(0.237)	(0.208)
Ex-Irish	1.179**	1.003**	0.805**	0.768**	0.579**	0.399*	0.619**	0.537**	0.355+
	(0.285)	(0.256)	(0.248)	(0.222)	(0.195)	(0.190)	(0.202)	(0.201)	(0.195)
N	536	536	536	536	536	536	536	536	536
Panel B: Police Gazette									
Execution	-0.372	-0.355	-0.340	0.0857	0.0890	0.0811	0.206	0.197	0.163
	(0.387)	(0.365)	(0.333)	(0.277)	(0.259)	(0.246)	(0.286)	(0.266)	(0.249)
Desert	-0.0459	-0.0409	-0.0341	-0.0245	-0.0228	-0.0212	-0.0510	-0.0488	-0.0454
	(0.0938)	(0.0888)	(0.0820)	(0.0887)	(0.0828)	(0.0772)	(0.0890)	(0.0849)	(0.0797)
Ex-Desert	0.251	0.241	0.235	-0.0773	-0.0747	-0.0611	-0.327	-0.309	-0.267
	(0.422)	(0.399)	(0.365)	(0.316)	(0.295)	(0.279)	(0.318)	(0.298)	(0.280)
Irish	-0.179	-0.172+	-0.164+	-0.187+	-0.175+	-0.169+	-0.119	-0.116	-0.114
	(0.109)	(0.103)	(0.0950)	(0.106)	(0.0990)	(0.0925)	(0.103)	(0.0983)	(0.0937)
Ex-Irish	0.431*	0.410*	0.387*	0.219	0.203	0.196	0.408*	0.392*	0.382*
	(0.196)	(0.181)	(0.161)	(0.199)	(0.185)	(0.169)	(0.207)	(0.193)	(0.177)
N	1640	1640	1640	1638	1638	1638	1640	1640	1640
Panel C: FGCM Trial Registries (Time Until Next Desertion Trial)									
Execution	-0.709	-0.648	-0.588	0.0476	0.0296	0.0233	0.0772	0.0703	0.0526
	(0.522)	(0.473)	(0.420)	(0.308)	(0.276)	(0.252)	(0.252)	(0.240)	(0.222)
Desert	0.0535	0.0411	0.0235	0.110	0.0816	0.0482	-0.0590	-0.0656	-0.0855
	(0.136)	(0.121)	(0.108)	(0.135)	(0.121)	(0.111)	(0.133)	(0.127)	(0.118)
Ex-Desert	0.442	0.397	0.351	-0.0496	-0.0232	-0.00214	-0.164	-0.148	-0.116
	(0.555)	(0.506)	(0.451)	(0.343)	(0.311)	(0.286)	(0.267)	(0.256)	(0.238)
Irish	-0.353*	-0.326*	-0.297*	-0.221	-0.196	-0.172	-0.252+	-0.243+	-0.218+
	(0.141)	(0.129)	(0.117)	(0.142)	(0.130)	(0.118)	(0.132)	(0.127)	(0.118)
Ex-Irish	0.718**	0.639**	0.560**	0.651**	0.566**	0.480**	0.556*	0.525*	0.465*
	(0.243)	(0.224)	(0.206)	(0.206)	(0.191)	(0.178)	(0.234)	(0.226)	(0.215)
N	1654	1654	1654	1654	1654	1654	1654	1654	1654

Notes: Outcome is elapsed time from death sentence resolution (execution or commutation) until next absence. "Exp", "Wb" and "Cox" use the exponential, Weibull and Cox models respectively to parameterize the baseline hazard. In columns 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 and year fixed-effects and ΔLog Casualties and ΔLog Casualties 30 Days Ago. War Diaries analysis restricts to July 1916-June 1917, which is the time window for the surviving data. Standard errors clustered at the division level in parentheses; + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

Table 7: Effects of Execution vs. Commutation on Elapsed Time Until Next Absence, Full Sample, Weak SUTVA

	War Diaries							Police Gazettes					FGCM Trial Registries (Desertion Trials)					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
Execution	-0.417	-0.390	-0.378	-0.368	-0.374	-0.389	-0.372	-0.544	-0.513	-0.475	-0.432	-0.411	-0.709	-0.919	-0.895	-0.856	-0.791	-0.752
	(0.736)	(0.798)	(0.781)	(0.742)	(0.701)	(0.679)	(0.387)	(0.527)	(0.497)	(0.447)	(0.407)	(0.393)	(0.522)	(0.699)	(0.675)	(0.622)	(0.566)	(0.541)
Desert	-0.0429	-0.0203	-0.0188	-0.0253	-0.0517	-0.0763	-0.0459	-0.0711	-0.0804	-0.0656	-0.0458	-0.0412	0.0535	0.0272	0.0179	0.0317	0.0559	0.0630
	(0.305)	(0.300)	(0.302)	(0.300)	(0.298)	(0.297)	(0.0938)	(0.0879)	(0.0859)	(0.0851)	(0.0882)	(0.0912)	(0.136)	(0.127)	(0.123)	(0.122)	(0.127)	(0.132)
Ex-Desert	-0.00330	-0.0249	-0.0306	-0.0251	0.000868	0.0202	0.251	0.312	0.284	0.258	0.234	0.228	0.442	0.518	0.504	0.474	0.430	0.411
	(0.746)	(0.782)	(0.762)	(0.728)	(0.697)	(0.683)	(0.422)	(0.562)	(0.529)	(0.480)	(0.441)	(0.426)	(0.555)	(0.741)	(0.716)	(0.663)	(0.607)	(0.580)
Irish	-0.727**	-0.769**	-0.784**	-0.822**	-0.850**	-0.836**	-0.179	-0.158	-0.172	-0.186+	-0.189+	-0.185+	-0.353*	-0.351*	-0.365*	-0.373**	-0.366*	-0.358*
	(0.179)	(0.181)	(0.190)	(0.212)	(0.226)	(0.220)	(0.109)	(0.110)	(0.109)	(0.107)	(0.108)	(0.108)	(0.141)	(0.143)	(0.144)	(0.145)	(0.144)	(0.143)
Ex-Irish	1.179**	1.258**	1.262**	1.310**	1.359**	1.347**	0.431*	0.432*	0.440*	0.437*	0.424*	0.420*	0.718**	0.726**	0.750**	0.778**	0.775**	0.761**
	(0.285)	(0.323)	(0.308)	(0.282)	(0.270)	(0.268)	(0.196)	(0.210)	(0.202)	(0.196)	(0.196)	(0.196)	(0.243)	(0.260)	(0.255)	(0.253)	(0.251)	(0.249)
ΔLog Casualties	0.0870	0.0812	0.0781	0.0721	0.0626	0.0574	0.0537+	0.0738**	0.0774**	0.0748*	0.0682*	0.0629*	0.0422	0.0597	0.0658+	0.0662+	0.0605	0.0543
	(0.0602)	(0.0602)	(0.0599)	(0.0595)	(0.0597)	(0.0603)	(0.0301)	(0.0269)	(0.0281)	(0.0292)	(0.0294)	(0.0293)	(0.0413)	(0.0371)	(0.0380)	(0.0389)	(0.0394)	(0.0398)
ΔLog Casualties	0.170**	0.173**	0.170**	0.163**	0.153*	0.148*	0.0652*	0.0619*	0.0681*	0.0719**	0.0709**	0.0689*	0.0856*	0.0826*	0.0924**	0.100**	0.0987**	0.0944**
30 Days Ago	(0.0569)	(0.0596)	(0.0599)	(0.0613)	(0.0634)	(0.0641)	(0.0286)	(0.0276)	(0.0272)	(0.0275)	(0.0275)	(0.0274)	(0.0370)	(0.0358)	(0.0350)	(0.0344)	(0.0342)	(0.0343)
Ex's - 7d		-0.194						0.214*						0.356**				
		(0.214)						(0.0939)						(0.116)				
Cm's - 7d		-0.0304						0.214**						0.183**				
		(0.158)						(0.0562)						(0.0453)				
Ex's - 14d			-0.146						0.0840						0.172+			
			(0.155)						(0.0788)						(0.0901)			
Cm's - 14d			-0.0439						0.156**						0.140**			
			(0.108)						(0.0383)						(0.0328)			
Ex's - 30d				-0.147						-0.0270						0.0216		
				(0.130)						(0.0703)						(0.0738)		
Cm's - 30d				-0.0653						0.105**						0.0990**		
				(0.0722)						(0.0267)						(0.0255)		
Ex's - 60d					-0.193						-0.0734						-0.0390	
					(0.132)						(0.0643)						(0.0667)	
Cm's - 60d					-0.0856						0.0651**						0.0623**	
					(0.0546)						(0.0211)						(0.0206)	
Ex's - 90d						-0.222+						-0.0857						-0.0569
						(0.132)						(0.0624)						(0.0650)
Cm's - 90d						-0.0887+						0.0455*						0.0425*
						(0.0480)						(0.0190)						(0.0181)
N	536	536	536	536	536	536	1640	1640	1640	1640	1640	1640	1654	1654	1654	1654	1654	1654

Notes: All specifications use the "+14" commutation date imputation method and all specifications use exponential models to parameterize baseline hazard rates. All specifications include division and year fixed-effects. Log Casualties is calculated as log(1+Casualties). ΔLog Casualties is defined as the difference in Log Casualties 1 to 29 Days Ago vs. 30 to 59 Days Ago. ΔLog Casualties 30 Days Ago is defined as the difference in Log Casualties 30 to 59 Days Ago vs. 60 to 89 Days Ago. The regressors labeled ex's-Yd or cm's-Yd measure the cumulative effects of previous deterrence events in the unit. Y is the half-life of the effect. War Diaries analysis restricts to July 1916-June 1917, which is the time window for the surviving data. Standard errors clustered at the division level in parentheses; + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

Table 8: Effects of Execution vs. Commutation on Ethnicity of Next Absence

	% Next Absence Irish		
	War Diaries	**Police Gazettes**	**FGCM (Desertion Trials)**
Panel A: All Death Sentences			
Irish Execution	19.2%	9.8%	17.2%
Non-Irish Execution	11.1%	9.0%	15.1%
Irish Commutation	13.3%	16.4%	13.0%
Non-Irish Commutation	13.1%	14.4%	12.7%
Panel B: Desertion Death Sentences			
Irish Execution	20.0%	9.3%	20.8%
Non-Irish Execution	9.5%	9.1%	16.2%
Irish Commutation	14.0%	17.8%	12.1%
Non-Irish Commutation	12.8%	15.8%	12.1%

Notes: All calculations use the "+14" commutation date imputation method. War Diaries analysis restricts to July 1916-June 1917, which is the time window for the surviving data.

Table 9: Day-by-Day Framework, All Absences

Panel A: War Diaries	(1)	(2)	(3)	(4)	(5)
Half-life	1 week	2 weeks	1 month	2 months	3 months
Execution	0.00894	0.00760	0.00804	0.00920	0.00957
	(0.0110)	(0.00925)	(0.00902)	(0.00832)	(0.00742)
Death Sentence	0.00170	0.000383	-0.000446	-0.000740	-0.000807
	(0.00202)	(0.00108)	(0.000769)	(0.000766)	(0.000768)
Ex-Irish	-0.0124	-0.00516	-0.00106	-0.00260	-0.00453
	(0.0119)	(0.0108)	(0.00944)	(0.00767)	(0.00688)
Irish	0.00608	0.00486	0.00191	0.000680	0.000635
	(0.00877)	(0.00669)	(0.00490)	(0.00353)	(0.00316)
Ex-Desert	-0.0177	-0.0135	-0.0114	-0.0111	-0.0112
	(0.0112)	(0.00890)	(0.00841)	(0.00799)	(0.00736)
Desert	0.000511	0.000805	0.00127	0.00207	0.00271+
	(0.00280)	(0.00169)	(0.00125)	(0.00131)	(0.00146)
N	20750	20750	20750	20750	20750
Panel B: Police Gazettes					
Execution	-0.0188**	-0.0174*	-0.0133	-0.00861	-0.00639
	(0.00601)	(0.00752)	(0.00794)	(0.00683)	(0.00578)
Death Sentence	0.00340	0.00348	0.00329	0.00274	0.00238+
	(0.00211)	(0.00242)	(0.00230)	(0.00166)	(0.00127)
Ex-Irish	-0.00932	-0.00846	-0.00875	-0.0115*	-0.0128*
	(0.00618)	(0.00557)	(0.00529)	(0.00539)	(0.00527)
Irish	0.00316	0.00327	0.00363	0.00399	0.00396
	(0.00486)	(0.00508)	(0.00452)	(0.00345)	(0.00270)
Ex-Desert	0.0115	0.0118	0.0102	0.00751	0.00626
	(0.00791)	(0.00860)	(0.00882)	(0.00723)	(0.00600)
Desert	-0.00385	-0.00438	-0.00419+	-0.00331+	-0.00278*
	(0.00289)	(0.00279)	(0.00247)	(0.00165)	(0.00115)
N	54605	54605	54605	54605	54605
Panel C: FGCM Desertion Trial Registries					
Execution	0.0122	0.0146	0.00819	0.000144	-0.00264
	(0.0223)	(0.0205)	(0.0158)	(0.0116)	(0.00992)
Death Sentence	0.0106*	0.00628*	0.00356	0.00210	0.00145
	(0.00403)	(0.00311)	(0.00219)	(0.00155)	(0.00134)
Ex-Irish	-0.00844	-0.0143	-0.0111	-0.00676	-0.00469
	(0.0194)	(0.0144)	(0.0102)	(0.00814)	(0.00763)
Irish	-0.000543	0.00317	0.00454	0.00489	0.00484
	(0.00855)	(0.00661)	(0.00468)	(0.00344)	(0.00304)
Ex-Desert	-0.0125	-0.0156	-0.0121	-0.00601	-0.00362
	(0.0193)	(0.0179)	(0.0147)	(0.0116)	(0.0102)
Desert	0.00236	0.00320	0.00256	0.00159	0.00108
	(0.00388)	(0.00357)	(0.00277)	(0.00204)	(0.00175)
N	59355	59355	59355	59355	59355

Notes: Outcome is whether there was any absence on that day and division. All specifcations use the "+14" commutation date imputation method and include division and year fixed-effects, ΔLog Casualties, and ΔLog Casualties 30 Days Ago. The half-life row indicates the assumed exponential half-life of the effect of past events. War Diaries analysis restricts to July 1916-June 1917, which is the time window for the surviving data. War Diaries analysis restricts to July 1916-June 1917, which is the time window for the surviving data. Standard errors clustered at the division level in parentheses; + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

Table 10: Day-by-Day Framework, Irish - non-Irish Absence

Panel A: War Diaries	(1)	(2)	(3)	(4)	(5)
Half-life	1 week	2 weeks	1 month	2 months	3 months
Execution	-0.0207	-0.0129	-0.00711	-0.00546	-0.00531
	(0.0143)	(0.0124)	(0.00923)	(0.00664)	(0.00554)
Death Sentence	-0.00106	-0.000889	-0.000577	-0.000368	-0.000286
	(0.00162)	(0.00137)	(0.00113)	(0.000938)	(0.000830)
Ex-Irish	0.0255*	0.0219*	0.0156+	0.0126+	0.0119+
	(0.0127)	(0.0105)	(0.00839)	(0.00686)	(0.00611)
Irish	0.0000700	-0.000649	-0.000233	0.000565	0.00105
	(0.00781)	(0.00643)	(0.00491)	(0.00356)	(0.00296)
Ex-Desert	0.0268+	0.0174	0.00913	0.00542	0.00468
	(0.0153)	(0.0131)	(0.00926)	(0.00665)	(0.00569)
Desert	-0.00595+	-0.00439+	-0.00241	-0.00159	-0.00158
	(0.00331)	(0.00244)	(0.00178)	(0.00149)	(0.00140)
N	20750	20750	20750	20750	20750
Panel B: Police Gazettes					
Execution	0.00661+	0.00495	0.000708	-0.00208	-0.00266
	(0.00388)	(0.00448)	(0.00454)	(0.00403)	(0.00363)
Death Sentence	-0.000576	-0.00118	-0.00120	-0.00108	-0.00100+
	(0.00143)	(0.00142)	(0.00116)	(0.000747)	(0.000544)
Ex-Irish	0.0121*	0.00937*	0.00784**	0.00760**	0.00736**
	(0.00545)	(0.00412)	(0.00289)	(0.00239)	(0.00233)
Irish	-0.00474	-0.00312	-0.00200	-0.00120	-0.000779
	(0.00314)	(0.00237)	(0.00192)	(0.00144)	(0.00119)
Ex-Desert	-0.00778	-0.00491	-0.000327	0.00160	0.00150
	(0.00595)	(0.00547)	(0.00521)	(0.00448)	(0.00388)
Desert	0.000462	0.00136	0.00123	0.000937	0.000832
	(0.00169)	(0.00167)	(0.00136)	(0.000872)	(0.000635)
N	54605	54605	54605	54605	54605
Panel C: FGCM Desertion Trial Registries					
Execution	-0.0240	-0.0183	-0.00784	0.00197	0.00559
	(0.0162)	(0.0131)	(0.00972)	(0.00716)	(0.00620)
Death Sentence	-0.00425	-0.00241	-0.00147	-0.00104	-0.000829
	(0.00514)	(0.00329)	(0.00193)	(0.00114)	(0.000885)
Ex-Irish	0.00333	0.00267	-0.000828	-0.00408	-0.00540
	(0.0160)	(0.0120)	(0.00782)	(0.00576)	(0.00510)
Irish	0.00498	0.00192	0.0000538	-0.000915	-0.00116
	(0.00575)	(0.00451)	(0.00328)	(0.00236)	(0.00202)
Ex-Desert	0.0313+	0.0258*	0.0162+	0.00644	0.00249
	(0.0157)	(0.0120)	(0.00883)	(0.00679)	(0.00618)
Desert	-0.00724	-0.00605+	-0.00400+	-0.00230	-0.00153
	(0.00517)	(0.00350)	(0.00222)	(0.00146)	(0.00120)
N	59355	59355	59355	59355	59355

Notes: Outcome is whether there was any Irish absence on that day and division minus whether there was any non-Irish absence on that day and division. All specifcations use the "+14" commutation date imputation method and include division and year fixed-effects, ΔLog Casualties, and ΔLog Casualties 30 Days Ago. The half-life row indicates the assumed exponential half-life of the effect of past events. War Diaries analysis restricts to July 1916-June 1917, which is the time window for the surviving data. War Diaries analysis restricts to July 1916-June 1917, which is the time window for the surviving data. Standard errors clustered at the division level in parentheses; + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

Appendix Figure 1

EXECUTION OF A BRITISH PRIVATE: SHOT AS A DESERTER FROM THE TRENCHES

The Manchester Guardian (1901-1959); Jan 22, 1916;
ProQuest Historical Newspapers: The Guardian (1821-2003) and The Observer (1791-2003)
pg. 9

EXECUTION OF A BRITISH PRIVATE.

SHOT AS A DESERTER FROM THE TRENCHES.

Mr. Tennant, Under Secretary for War, in Parliamentary papers, informs Mr. Farrell that Private T. Hope, of the 2nd Battalion Leinster Regiment, was tried by field general court-martial on February 14, 1915, on a charge of desertion and other minor charges. The evidence showed that he absented himself from the trenches on December 23 until February 9, when he was arrested.

"It is well known to all soldiers (adds Mr. Tennant) that desertion in the face of the enemy is liable to be punished by death. Private Hope was informed of his sentence more than twelve hours before it was carried out. The sentence was passed on February 14, and was most carefully reviewed before it was confirmed by the Commander-in-Chief on February 27. Such confirmation was strictly in accordance with law. It is obvious that counsel cannot be employed on courts-martial which take place in the field.

"The accused called no evidence such as is referred to in the latter part of the question (Mr. Farrell had asked whether it was brought to the notice of the court that on several occasions Hope had exposed himself gallantl in trench warfare), nor was any such evidenc before the court."

Appeals from Death Sentences.

In reply to Mr. King's suggestion that te days should elapse between the dismissal c the appeal of a man sentenced to be shc upon conviction on indictment and the execu tion of the sentence, Mr. Tennant says he i not sure that it would be desirable. When man has been sentenced to death and all th possible means of obtaining a reprieve hav been exhausted, to defer the execution undul does not appear to be at all desirable in th interests of the condemned.

Appendix Figure 2

IRISH SOLDIERS TRAIN TO FIGHT IN GERMAN ARMY: Deserters Eager to Humble England--Rega
Wilson, Carolyn
Chicago Daily Tribune (1872-1922); Mar 20, 1916;
ProQuest Historical Newspapers: Chicago Tribune (1849-1987)
pg. 13

IRISH SOLDIERS TRAIN TO FIGHT IN GERMAN ARMY

Deserters Eager to Humble England—Regard Teutons as Their Friends.

by CAROLYN WILSON.

What would you say if you were to see a handsome blue eyed fighter with black hair and a smile full of mischief and trouble, and dressed in a fine German uniform with a harp on either side of his collar, embroidered with tiny colored stones, and then you heard this same subject of the kaiser say in Irish you could thicken a stow with, "Bhring me a drink, ye imp o' Satan, whether I be in uniform or no, an' put it in a ginger ale glass, me bye, for good luck."

Allow me to introduce Capt. Robert Monteith, erstwhile servant of his majesty in Egypt, Africa, and India, independent follower of fortune in South American revolutions, and now captain of a company of grass-green Irishmen in the German army doing the goose-step to the tune of "They're hangin' men and women for the wearin' o' the green."

I think the tune is singularly well chosen and there would certainly be some hanging if these same wearers of the green—and what a green—ever came within the boundaries of the British empire again.

Most Are German Prisoners.

Most of them are Irish prisoners taken during the early days of the war and now exercising a pleasant freedom rehearsing for the downfall of England.

I'm sorry—they don't appeal to me. So we'll leave them out there at Zozzen, a large training camp for 7,000 soldiers, doing the goose step, while we proceed to Capt. Monteith, a gentleman of adventurous past, a colorful imagination and an amazing awe inspiring facility for emptying in quick succession ginger ale glasses filled with whisky and soda.

"I've been fighting since I was a lad," he said, "at first with the English because I knew no better. I came from the loyal Irish, ye see. But I don't believe in war—it's murder. There's no such thing as honor or fair fighting or international laws in war. I'm a Socialist, really. I believe in complete disarmament for every country. That's the only way you could ever stop it. War is the curse of the poor man."

To the hint that troops were sometimes necessary to quell riots, enforce military law in ruined cities, or catastrophes, he answered:

"Yes, but who are they out to protect—not the poor but the rich. It is always the rich man that gets the benefit of an army and pays the least proportionately toward it. That is the reason I'm fighting with the Germans—because they have the most democratic government in the world and because they protect their poor.

"God give my sword power to run down a tyrant!" and he looked fondly at it. "It should bring me luck, to be sure. One of the prettiest girls in the world kissed it and bade me Godspeed, herself an American. Ye see, I escaped from England and took my wife and babies to America for safekeeping—then I stowed away on a steamer to Christiania. There was little of the ground they didn't go over at Kirkwall, but they didn't discover me.

"In Norway, however, somebody found out about me an' a most excitin' time I had makin' my escape to Germany. An' now it's the interest of me life, knowing on which front I'm to fight. We've been training out there so long. I'm aching to be at it again. And all we boys are a bit restless with inaction.

Has Hatred for England.

"O, for a whack at England. You know what they should do, the Germans? Give Alsace and Lorraine back to France and make a separate peace, then go to it and lick England. Of course, they'd take Alsace-Lorraine back again as soon as their job was finished, but that'd keep the French quiet."

"Is that your idea of the honor of the country you are now serving?" I asked him.

"It's me idea of the honorable intentions of every country under the sun who's at war," he answered. "There is no such thing as honor or clean fighting in war—it's all murder and murderers aren't particularly honorable or fair gentlemen."

"Do you think you represent the feeling of Ireland, then, in coming here and joining the Germans?"

"There's not an Irishman living but would like to be free of England, and those of us who have come here believe this is our only chance. People call Ireland poor and decaying. Why, it's a rich country. Under proper rule it would be a wonderful country. Look at the way its literature is coming to life again. O, after the war, we'll be strong."

"Have you talked to the Irishmen interned here?"

"Have I, indeed, and what did they say to me? Nothing that I could repeat in your presence."

Company of Deserters.

At that time I didn't know that almost all the men under Capt. Monteith were deserters, although I noticed the determined evasion of my repeated questions as to their methods of reaching Germany after the war began.

Although they are a part of the German army, they seem to be existing under lax discipline, for the corporal Capt. Monteith had intended to bring with him had left camp Saturday afternoon and on Monday had not returned. I heard afterward that this handful of Irishmen cause more trouble at the camp than all the 7,000 other men. When the spirit comes upon them they go forth in a body and make life miserable for all about them.

Monteith himself was at the Adlon bar most of the time when he wasn't hunting up Sir Roger Casement or St. John Gaffney, who arrived early in February full of words against the American administration and the tyranny of the British.

Referring again to this Irish company doing the German goose step, I spoke of them rather disparagingly to an official of the war ministry.

"Probably they will never fight," he said. "In the first place, the condition on which they were accepted was that they might fight on Irish or English soil. It was merely a small courtesy paid the Irish and Irish sympathy for us through them. The Irish have done much for us, and may do more, and we haven't so many friends in the outside world today that we can afford to overlook some sincere admirers."

British Troops in Ireland.

I had with me the official figures of Irish emigration since the war began, Irish recruiting by county and month, also the number and division of English troops in Ireland, but some one who searched my cabin on the Rotterdam for notes took them all away, and I can only give you round numbers. The total recruiting of Ireland was about 97,000, of which half is from Protestant Ireland. In spite of the fact that the Irish were not included in the conscription bill, it has still been thought necessary to keep a force of between 50,000 and 60,000 British troops in Ireland, either to quell a possible revolution or for fear of a German landing, or to hunt for German submarine stations, two of which were discovered on the west coast.

In Cork a committee appointed to organize St. Patrick's day celebrations was offered the use of several companies of Irish soldiers, but refused the offer with the words: "The British army is in hostile occupation of Ireland, and it would be as absurd for Belgians to invite a contingent from the German army to participate in a Belgian national celebration."

Well informed Englishmen fear that Ireland is very near revolt.

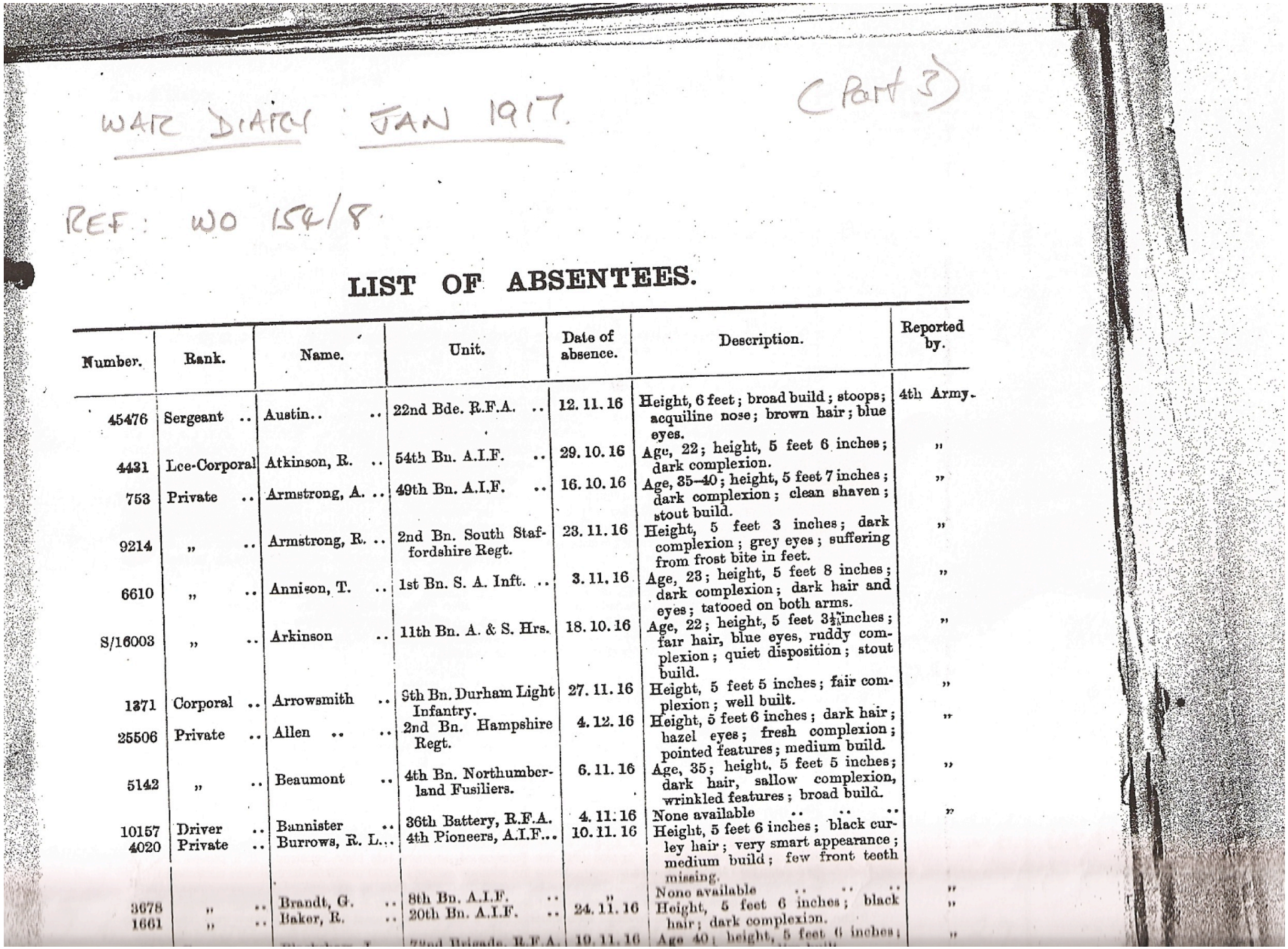
[The tenth article by Carolyn Wilson on the conditions in Germany and its aspirations will appear in tomorrow's "Tribune."]

An Irish Captain Serving Germany.

Capt. Robert Monteith.

Capt. Robert Monteith, soldier of fortune, is commander of an Irish company training to fight for Germany. He doesn't believe in war, but nevertheless longs to deliver a blow against England.

Appendix Figure 3



WAR DIARY JAN 1917. (Part 3)

REF: WO 154/8.

LIST OF ABSENTEES.

Number.	Rank.	Name.	Unit.	Date of absence.	Description.	Reported by.
45476	Sergeant ..	Austin.. ..	22nd Bde. R.F.A. ..	12. 11. 16	Height, 6 feet; broad build; stoops; acquiline nose; brown hair; blue eyes.	4th Army.
4431	Lce-Corporal	Atkinson, R. ..	54th Bn. A.I.F. ..	29. 10. 16	Age, 22; height, 5 feet 6 inches; dark complexion.	"
753	Private ..	Armstrong, A. ..	49th Bn. A.I.F. ..	16. 10. 16	Age, 35–40; height, 5 feet 7 inches; dark complexion; clean shaven; stout build.	"
9214	" ..	Armstrong, R. ..	2nd Bn. South Staffordshire Regt.	23. 11. 16	Height, 5 feet 3 inches; dark complexion; grey eyes; suffering from frost bite in feet.	"
6610	" ..	Annison, T. ..	1st Bn. S. A. Inft. ..	3. 11. 16	Age, 23; height, 5 feet 8 inches; dark complexion; dark hair and eyes; tatooed on both arms.	"
S/16003	" ..	Arkinson ..	11th Bn. A. & S. Hrs.	18. 10. 16	Age, 22; height, 5 feet 3½ inches; fair hair, blue eyes, ruddy complexion; quiet disposition; stout build.	"
1371	Corporal ..	Arrowsmith ..	8th Bn. Durham Light Infantry.	27. 11. 16	Height, 5 feet 5 inches; fair complexion; well built.	"
25506	Private ..	Allen	2nd Bn. Hampshire Regt.	4. 12. 16	Height, 5 feet 6 inches; dark hair; hazel eyes; fresh complexion; pointed features; medium build.	"
5142	" ..	Beaumont ..	4th Bn. Northumberland Fusiliers.	6. 11. 16	Age, 35; height, 5 feet 5 inches; dark hair, sallow complexion, wrinkled features; broad build.	"
10157	Driver ..	Bannister ..	36th Battery, R.F.A.	4. 11. 16	None available	"
4020	Private ..	Burrows, R. L..	4th Pioneers, A.I.F..	10. 11. 16	Height, 5 feet 6 inches; black curley hair; very smart appearance; medium build; few front teeth missing.	
3678	" ..	Brandt, G. ..	8th Bn. A.I.F. ..	"	None available	"
1661	" ..	Baker, R. ..	20th Bn. A.I.F. ..	24. 11. 16	Height, 5 feet 6 inches; black hair; dark complexion.	"
			22nd Brigade, R.F.A.	19. 11. 16	Age 40; height, 5 feet 6 inches;	"

Appendix Figure 4

PUBLISHED BY AUTHORITY.

NEW SERIES. TUESDAY, JUNE 6, 1916. VOL. XXXII., No. 3258.

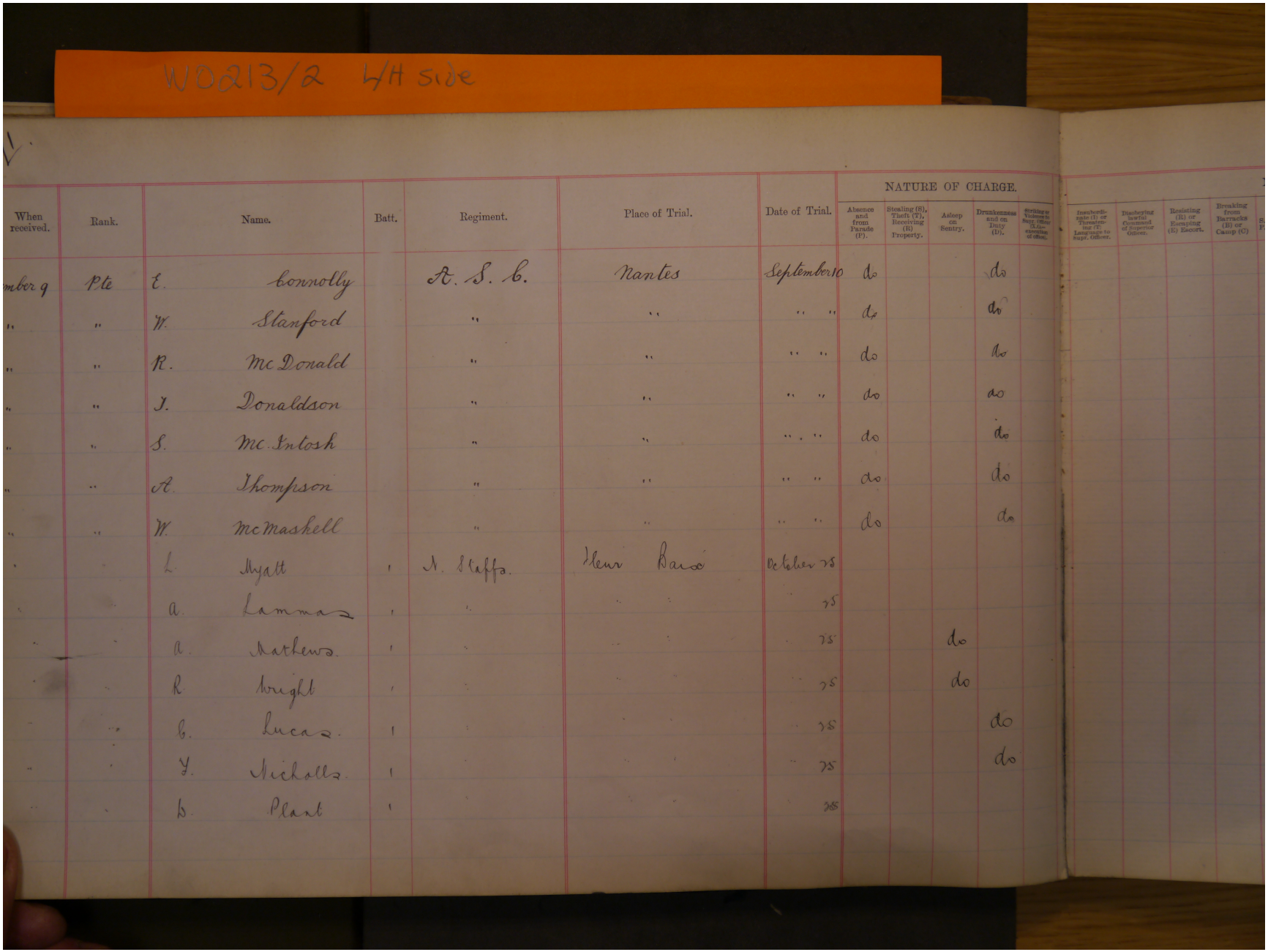
It is requested that the Admiralty and War Office Forms giving descriptions of Deserters, &c., from His Majesty's Naval and Military Services for insertion in the POLICE GAZETTE, and all communications in connection therewith, shall be addressed to THE EDITOR of the POLICE GAZETTE, New Scotland Yard, London, S.W.

WAR OFFICE, JUNE 6, 1916.

DESERTERS AND ABSENTEES FROM HIS MAJESTY'S SERVICE.

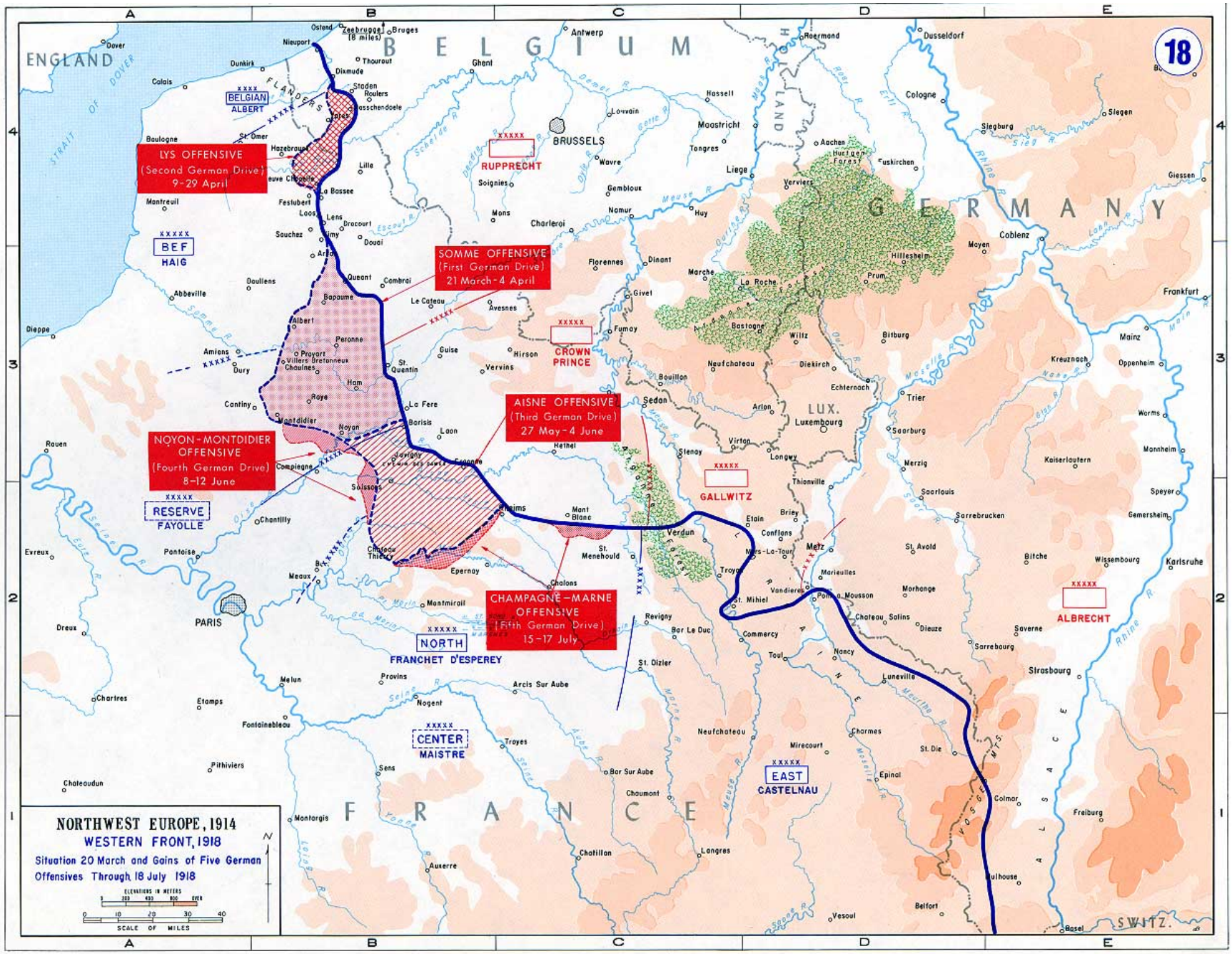
Office No	NAME.	Reg. No.	Corps.	Age.	Height.	Com-plxn.	Hair.	Eyes.	Trade.	ENLISTMENT. DATE OF	ENLISTMENT. PLACE OF	PARISH AND COUNTY IN WHICH BORN.	DESERTION. DATE OF	DESERTION. PLACE OF	MARKS AND REMARKS
1	Abbott, C.	1312	6th K.R. Rifles	—	—	—	—	—	—	—	—	—	21 May	Sheerness	
2	Alexander, J. R.	10310	W. Cav. Depôt	—	—	—	—	—	—	—	—	—	16 May	St. Helens	
3	Allwood, Herbert	8516	3rd E. Yorks R	29¼	5 4	fresh	dk brn	brn	groom	17 May '07	Beverley	Eaton, Notts	27 May	Withrnsea	2 scs chst
4	Anderson, B.	11829	3rd Highlnd Lt	—	—	—	—	—	—	—	—	—	27 May	Malleny	
5	Andrews, J.	1 07915	ArmySer.Corps	38	5 7	—	—	—	labourer	15 May '15	Holloway	Chapel Tn, Sheffield	28 May	Catterick	mole back
6	Anscombe, C.	69533	R. Engineers	38	5 8½	fresh	grey	grey	bricklayer	6 Mar. '15	Southwark	Southwark	23 May	Newark	star l frm portwine stn cross
7	Armour, Daniel	8962	Dpt Seaf Hghrs	32	5 7½	—	—	—	pro footblr	8 June '15	—	—	15 May	Ft. George	scs chks knees ttd frms
8	Armstrong, R.	1 50954	InldWtrTrnRE	26	5 6	ruddy	dk brn	brn	lighterman	—	Leeds	Leeds, Yorks	27 May	Poplar	
9	Asple, M.	19460	5th R. Dub. Fs	19½	5 7	—	—	—	gen labr	18 Mar. '15	Kilkenny	Borris, Carlow	26 May	CurraghCp	
10	Atkinson, Stanley	16565	5th CldstrmGds	27	5 8½	—	—	—	miner	31 July '15	Hackney	Durham	22 May	Windsor	cntrctd ltl fngr rt
11	Avery, C.	938	3rd R.Sussex R	42	5 7½	—	—	—	painter	3 Nov. '14	Chichester	—	26 May	Newhaven	
12	Bagge, N.	13982	Dpt S. Staffs R	21	5 11	fresh	brn	blue	choclte mkr	8 Sept. '14	Birmnghm	Birmingham	24 May	Lichfield	
13	Baillie, Henry G.	1 47600	ArmySer.Corps	31½	5 6½	—	dark	—	chauffeur	8 Nov. '15	Whiteball	Clerkenwell	26 May	Avnmouth	
14	Bain, Michael	10210	11th GordnHgh	25	5 5	—	—	—	carter	22 May '15	Dundee	Dundee, Forfar	15 May	Cambsbrrn	CF MB hrt l frm JM JD MA
15	Bain, Peter	10661	3rd Seaf. Hghrs	27½	5 5½	fresh	fair	blue	labourer	3 Apr. '08	Glasgow	Dingwall, Ross.	28 May	Cromarty	6 scs rt frm
16	Bain, Wm.	1 66075	Can. Pionr Dpt	32½	5 8	dark	black	hazel	plumber	1 Oct. '15	Ontario	Leith	10 May	Shornclife	ttd arms
17	Bankes, P.	6802	3rd R.Mun.Fus	30	5 7½	—	—	—	stoker	11 Jan. '16	Limerick	Vizers Fields, Limrk	22 May	Aghada	sc rt thigh l elbow
18	Banks, Thos	9779	3rd R.Mun.Fus	27	5 5½	fresh	dk brn	hazel	labourer	1 Mar. '12	Cork	St. Anns, Cork	24 May	Aghada	mole back
19	Barber, J.	9939	5th Res Cav. R.	38¾	5 4¾	—	—	—	shell gangr	17 Jan. '16	Barrw-i-F.	Stockton-o-Tees	21 May	York	
20	Barker, Herbert	34485	178th Bde RFA	22	5 3½	dark	brn	dark	hawker	22 July '15	Ashtn-u-L.	Hulme, Lancs	24 May	Deepcut	
21	Bateman, A.	26141	15th NrthlndFs	36	5 4½	—	—	—	collier	24 May '15	Leicester	—	24 May	RugeleyCp	man l arm
22	Bates, John	1493	28th Northd Fs	21	5 8	—	—	—	waiter	7 June '15	Nwcstl-o-T	Newcastle-on-Tyne	15 May	Nwcstl-o-T	
23	Batty, Owen	6858	3rd K.O.Sc.Bds	39¾	6 1	fair	brn	grey	gen labr	11 Aug. '14	Warrngtn	Barnsley	22 May	Inchkeith	
24	Beagleigh, E.	247	3rd R.W.Surr.R	35½	5 3½	—	brn	brn	casual labr	31 Aug. '14	London	Lambeth	21 May	Shoreham	TB l frm hrseshoe gd lck rt
25	Beddowes, —	2667	5th Yorks L.I.	—	5 6	fresh	lt brn	—	miner	31 Aug. '14	—	—	19 May	ClipstneCp	scs nose
26	Bellamy, George	19572	3rd E. Yorks R	31¼	5 5½	—	—	—	labourer	29 June '15	Hull	—	24 May	Withrnsea	l lve Maggie clspd hnds tr lve
27	Bellhouse, E.	1 70264	M.T. A.S.C.	23	5 5½	—	—	—	traveller	—	Leeds	—	22 May	Osterley Pk	
28	Billington, T.	746	ArmySer.Corps	29	5 5	fresh	dark	blue	labourer	4 Sept. '14	Accrington	Glasgow	23 May	*leave* *	* from B.E.F.
29	Blackburn, S.	36415	3rd R.WelshFus	37	5 5½	—	—	—	painter	27 Oct. '15	Birmnghm	Leeds, Yorks	9 May	Litherland	
30	Blackburrow, F. G.	34517	ArmySer.Corps	24	5 6½	fair	fair	hazel	driver	13 Aug. '14	Islington	New Southgte, Mdx	20 May	Catterick	ttd frms sc back
31	Blake, Jeremiah	70702	R. Engineers	48	5 10	—	—	—	fitter	3 Feb. '15	Westmnstr	Headford, Galway	27 May	Hghtn Rgs	
32	Bolan, William	7409	4th Scottish Rif	43½	5 5	fresh	brn	hazel	labourer	14 Aug. '14	Glasgow	Kilmarnock, Ayr	25 May	Ft. Matilda	sc rt sde chn
33	Boot, James	55764	HvArtDptRGA	28½	5 11	—	—	—	caster	3 Dec. '14	Birmnghm	—	21 May	Woolwich	ttd rt frm blu mk l eye sc r
34	Booth, Alfred	18887	3rdS. WalesBds	32	5 7½	—	—	—	miner	21 Dec. '14	Newport	Abergavenny, Mon.	28 May	Hightown	coal sc l eyebrw
35	Booth, Geo. Wm.	15576	22nd Lancs Fus	23½	5 1	—	—	—	collier	5 Jan. '15	Farnworth	—	27 May	Prees Hth	
36	Boothroyd, Walter	502	21stW.Yorks R	37½	5 4	light	lt brn	blue	woolcombr	11 Dec. '15	Bradford	Bradford, Yorks	20 May	Skipton	
37	Bottomley, R.	63901	Remounts ASC	38	5 5	—	—	—	horsekeepr	9 Apr. '15	London	Deptford	24 May	Ormskirk	sc rt knee
38	Bowles, L. S.	15226	28th Middl-x R.	30	5 4	fresh	brn	blue	silversmth	7 June '15	Holloway	—	26 May	Aldershot	
39	Bradley, Thomas	15974	4th A.&S. Hghs	21½	5 4½	—	—	—	baker	3 Jan. '16	Belfast	St. Annes, Antrim	6 May	Edinburgh	ttd arms chst nckle rnd m
40	Brady, Chas.	6369	3rd R. Ir. Fus	23	5 4½	—	—	—	labourer	14 Sept. '15	Cork	Cork	26 May	Aghada	
41	Brailsford, Fredk.	11975	11thYk.&Lcs R	30	5 6½	dark	black	brn	miner	28 Aug. '14	Rotherhm	Wrsbro' Dle, Brnsly	28 May	RugeleyCp	
42	Brandon, H.	15828	6th Royal Fus	20½	—	fresh	brn	brn	vanguard	17 Oct. '13	Hounslow	Hammersmith	13 May	Shoreham	buffalo hd love Flo wound
43	Brannon, Patrick	28841	3rd Highlnd Lt	25	5 5	—	—	—	labourer	12 Jan. '15	Stirling	Stirling	27 May	Malleny	sc rt elbw
44	Brayshaw, W. G.	24606	3rd W. Yorks R	30½	5 5	—	—	—	labourer	11 Jan. '16	Leeds	Leeds	21 May	WhitleyBy	
45	Brennan, Patrick	—	R. Irish Regt	36	5 6½	—	—	—	gen labr	22 May '16	Dublin	St. Michaels, Wxfrd	23 May	*en route**	sc ovr rt eyebrw * to Clon
46	Brett, Samuel	6159	R. Angiesey RE	33¾	5 5½	—	brn	dk brn	platelayer	17 Nov. '11	Hyde	St. Annes, Lancs	19 May	Beaumaris	
47	Brodie, A.	64338	1a Rs Bde RFA	—	—	—	—	—	—	—	—	—	27 May	Nwcstl-o-T	
48	Brophy, P.	23741	6th Res Cav. R.	21	5 4	—	—	—	—	1 May '16	Curragh	—	19 May	Dublin	
49	Bruce, N. L. McG.	2498	Dpt Gord. Hghs	24	5 8	—	—	—	clerk	15 May '16	London	Dundee	18 May	*en route**	* to Aberdeen
50	Bryan, A. E.	27652	1st GnYorks L.I	20½	5 6½	—	—	—	labourer	9 Dec. '15	Derby	Derby	22 May	Stattngbro	
51	Bryan, Patrick	8309	3rd R. Irish Rgt	30	—	fresh	brn	blue	groom	7 Sept. '14	Dublin	St. James, Dublin	24 May	Dublin	
52	Buchan, W.	24010	26thSq R.Fly.C	24	—	fresh	—	—	fitter	28 Feb. '16	—	—	26 May	Turnhouse	
53	Bunyan, Sydney	13571	Dpt S.Staffs R.	44	5 7	brn	grey	blue	labourer	7 Sept. '14	Lichfield	Crewe, Cheshire	13 May	France	
54	Burgess, Geo. Wm	25173	10th S.Staffs R.	39	5 4½	—	—	—	surveyor	2 May '16	Leicester	Long Eaton, Derby	20 May	RugeleyCp	
55	Burke, P.	25569	5th R. Dub. Fs	22	5 5	—	—	—	gen labr	28 Dec. '15	Dublin	S. Augustne, L'pool	17 May	Curragh Cp	scs chst rt shldr lmbr regn
56	Burton, E.	27843	ArmySer.Corps	29	5 6	fresh	brn	blue	—	27 Jan. '16	Manor Pk.	Forest Gate	13 May	Ashford	
57	Burton, W.	40653	51st RsBtyRFA	26½	5 6¾	fresh	dk brn	grey	gen dealer	12 Oct. '14	Tottenhm	Tottenham	6 May	BshpsStrfd	snke womn clspd hnds rt ar
58	Bushby, Thomas	11999	ArmySer.Corps	39	5 3½	—	—	—	labourer	12 May '15	London	Brixton	25 May	B.E.F.	
59	Butcher, G.	1 24211	48thRsBtyRFA	21½	5 7	—	—	—	van boy	28 Feb. '16	Lambeth	Lambeth	27 May	Weedon	
60	Butler, E.	24194	28th Middlsx R.	23	5 7	fresh	brn	fair	tailor	8 Dec. '15	Finsbury	Southampton	21 May	Aldershot	
61	Butti, Charles J.	1082	3rd Essex R.	23½	5 7½	—	brn	grey	labourer	31 Oct. '10	Stratford	—	24 May	Felixstowe	
62	Byrne, J.	18259	Dpt R.Dub.Fus	19½	5 5½	fresh	fair	blue	shoemaker	8 Jan. '15	Dublin	—	1 May	Naas	
63	Cagney, M.	7756	Dpt R. Mon.RE	40	5 5	—	—	—	engne drvr	29 July '15	Monmouth	Mertbyr, Glam.	20 May	Monmouth	
64	Cahill, Thomas	—	Dpt R. Irish Rif	22	5 7½	fresh	lt brn	blue	labourer	24 May '16	Glasgow	Cork	24 May	*en route**	sc cntre frhd * to Be
65	Cain, Samuel	27053	4th Lancs Fus	30½	5 7½	—	—	—	labourer	10 Dec. '15	Bury	Bolton, Lancs	27 May	Barrow	
66	Caines, Thomas	10503	11th S.Staffs R.	42	5 6	fresh	lt brn	grey	labourer	1 Sept. '14	Birmnghm	St. James, Brmghm	22 May	RugeleyCp	ttd frms sc rt groin
67	Calderwood, J.	22931	3rd R.Scots Fus	23½	5 4½	—	—	—	pithd man	9 Mar. '16	Ayr	Drogborn, Ayr	2 May	Ft. Matilda	sc l elbw
68	Callaghan, H.	1 27083	ArmySer.Corps	43	4 11½	—	—	—	—	10 Aug. '15	London	Shoreditch	26 May	Aldershot	strwbrry mk nck
69	Cameron, George	15080	19th Highlnd Lt	22	5 5	ruddy	dark	dk gry	shps stwrd	4 Nov. '14	Glasgow	Woodside, Glasgow	22 May	Montrose	ttd frms l ttl toe overlaps
70	Camis, F.	4749	19th K.R. Rifles	32	5 4½	sallow	black	brn	—	— Nov. '15	Woolwich	—	19 May	Wimbledn	wears glasses
71	Campbell, Allen	17072	Dpt N. Lancs R	35	5 5½	fresh	dk brn	blue	sailor	11 Oct. '14	Cardiff	Glasgow	12 Apr.	Irvine	
72	Campbell, John	3905	10th Seaf. Hghs	28½	5 6½	fresh	fair	blue	shoemakr	4 Sept. '14	Perth	Dundee	25 May	Dunfrmlne	sbrpnl wnd bnd
73	Carey, Michael	18641	12th Royal Fus	20	5 1	fresh	black	brn	musician	28 June '15	Grays	St. James, London	21 Apr.	Dover	2 scs nr l scapula
74	Carlin, John	—	Dpt R.Innis Fs	19½	5 6½	—	—	—	machinist	9 Dec. '15	Paisley	Johnstone	23 May	*en route**	mole rt cheek * to Ou
75	Carrahar, James L.	549	21st DurhamLI	31	5 6	fair	red	blue	labourer	24 Oct. '15	Sunderlnd	Sunderland	16 May	Hornsea	squint l eye
76	Carter, Alfred	8591	11th Yorks R.	21½	5 8½	—	brn	grey	labourer	31 Aug. '14	Thornaby	Middlesbro', Yorks	25 May	Rugeley Cp	
77	Carter, Evans	23131	14th E. Yorks R	25½	5 5½	—	—	—	coal heavr	12 Dec. '15	Hull	Hull, Yorks	28 May	SeatnDelvl	
78	Carter, J.	6276	Dpt R. Mon.RE	23	5 4	—	brn	blue	coal miner	14 Apr. '13	Newport	Bristol	27 May	Monmouth	ttd stdies—beauty & lve
79	Chamberlain, W. R.	2923	Yorks L.I.	32	5 6	—	—	—	fitter	13 Sept. '15	Killinghall	Hammersmith	5 May	Pontefract	
80	Chaney, Oscar J.	35780	R.A.Med.Corps	21½	5 5	fresh	brn	brn	newsvendr	17 Sept. '14	Holborn	St. Pancras	19 May	Aldershot	
81	Chappell, Charles	62.8	ArmySer.Corps	44	5 3¾	—	—	—	labourer	29 Jan. '15	London	Marylebone	23 May	B.E.F.	ttd rt frm
82	Chrichton, Wm.	—	23rd Durhm LI	26½	5 0½	—	—	—	labourer	14 Feb. '16	Nwcstl-o-T	Glasgow	14 Feb.	Nwcstl-o-T	
83	Clark, A.	80763	Signal Dpt RE	32	5 10	—	—	—	gen labr	6 Mar. '15	Enfield	Enfield, Midlsx	22 May	Haynes Pk	
84	Clark, H.	11469	5th Res Cav. R.	18½	5 4½	—	—	—	driver	13 Mar. '16	Prtsmouth	Kensington	22 May	York	
85	Clark, John Pearson	15987	3rd E. Yorks R.	38¾	5 5	—	—	—	compositor	1 Dec. '14	Hull	—	27 May	Withrnsea	
86	Clarke, A.	11268	11th GordnHgh	—	—	—	—	—	—	—	—	—	19 May	Cambsbrrn	
87	Clarke, William	42385	Dpt Nts&Dby R	19	5 9½	—	—	—	farm labr	15 Apr. '16	Derby	Newark, Nottnghm	26 Apr.	Derby	fit ft
88	Clifton, —	132	6th Rifle Bde	38	5 7½	—	brn	brn	groom	3 Sept. '14	London	St. Giles, Midlsx	16 May	Shorcham	thd arms
		28507	3rd R.Innis.Fus	29½	5 2½	—	—	—	pantryman	31 Mar. '16	Belfast	—	6 May	Londonderry	

Appendix Figure 5

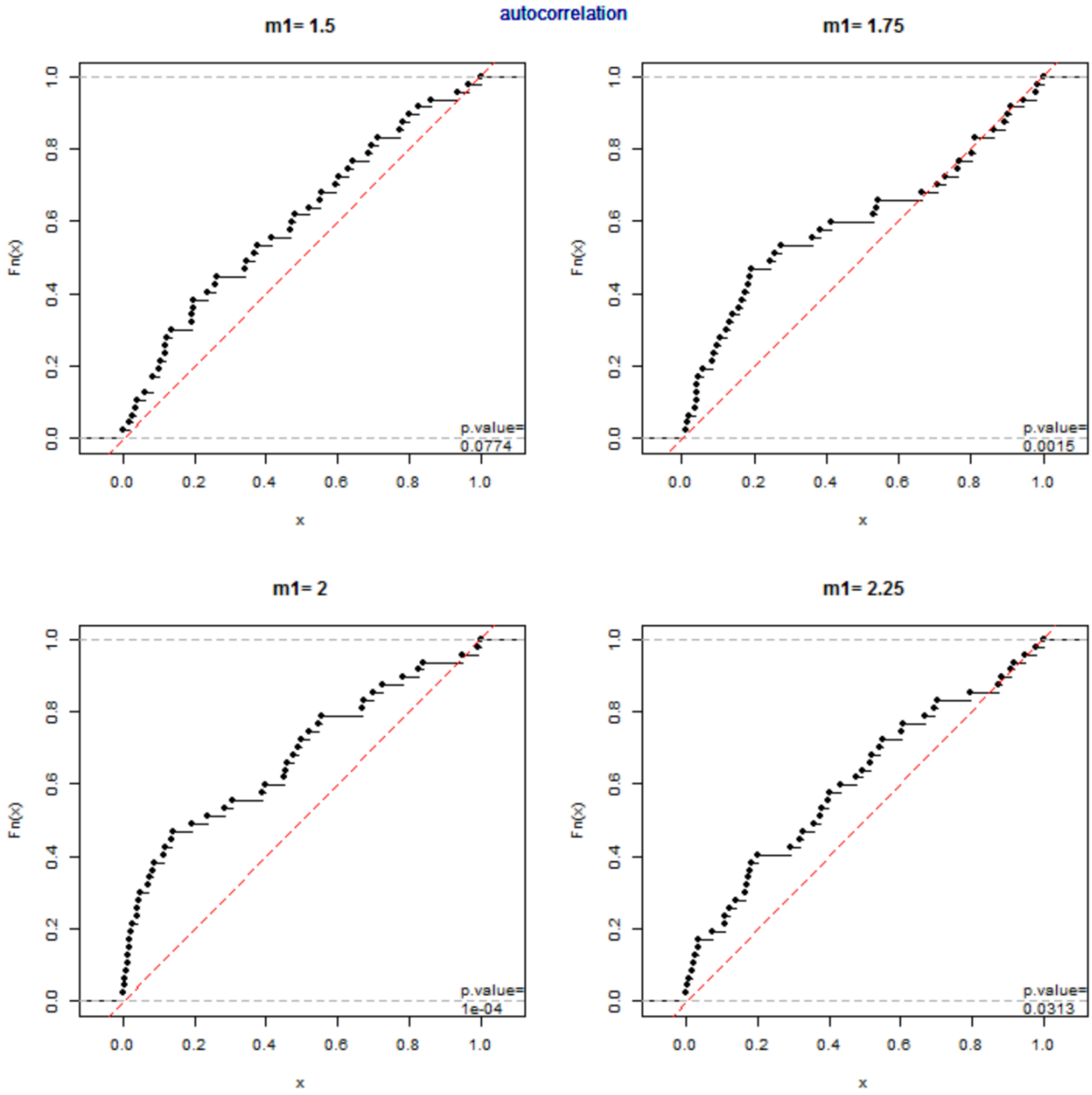


When received.	Rank.	Name.	Batt.	Regiment.	Place of Trial.	Date of Trial.	NATURE OF CHARGE. Absence and from Parade (F).	Stealing (S), Theft (T), Receiving (R) Property.	Asleep on Sentry.	Drunkenness and on Duty (D).	Striking or Violence to Supr. Officer (L) in execution of office.	Insubordinate (I) or Threatening (T) Language to Supr. Officer.	Disobeying lawful Command of Superior Officer.	Resisting (R) or Escaping (E) Escort.	Breaking from Barracks (B) or Camp (C)
mber 9	Pte	E. Connolly		R. S. B.	Nantes	September 10	do			do					
"	"	W. Stanford		"	"	" "	do			do					
"	"	R. Mc Donald		"	"	" "	do			do					
"	"	J. Donaldson		"	"	" "	do			do					
"	"	S. Mc Intosh		"	"	" "	do			do					
"	"	A. Thompson		"	"	" "	do			do					
"	"	W. McMaskell		"	"	" "	do			do					
		L. Myatt	1	N. Staffs.	Hem Barracks	October 75									
		A. Lammas	1			75									
		A. Mathews	1			75			do						
		R. Wright	1			75			do						
		C. Lucas	1			75				do					
		J. Nicholls	1			75				do					
		D. Plant	1			385									

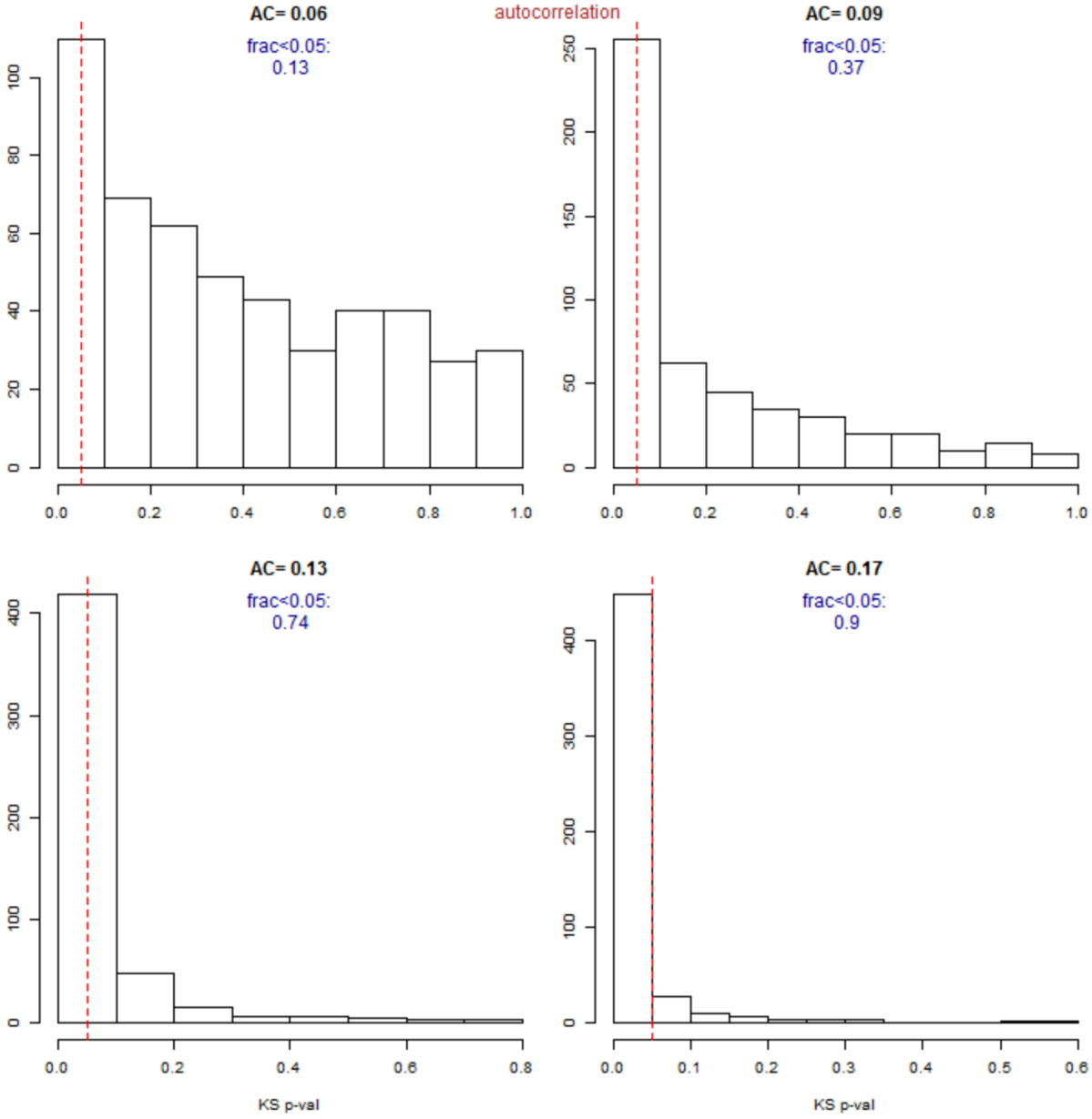
Appendix Figure 6



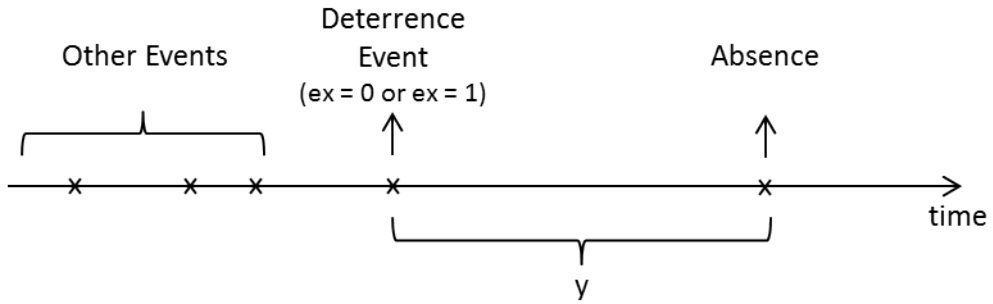
Appendix Figure 7A



Appendix Figure 7B

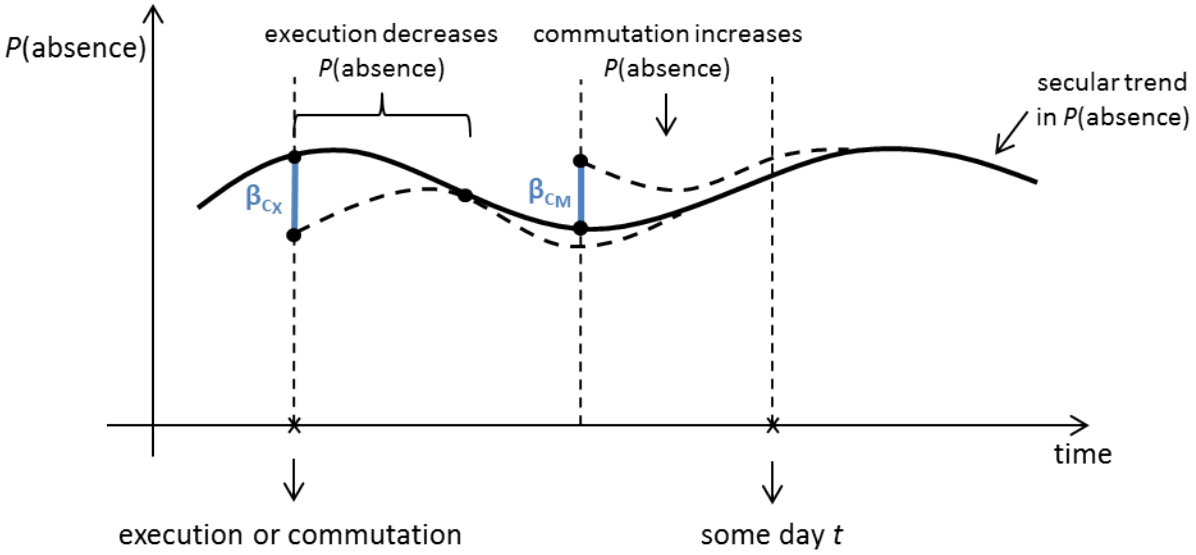


Appendix Figure 8A



y ≡ time from event (either execution of commutation) to absence

Appendix Figure 8B



Past events influence current probability, but this influence wanes over time.

Appendix Table 1: Effects of Executions vs. Commutations on Elapsed Time Until *Previous* Absence Differing by whether Case was a Desertion Trial and whether Soldier was Irish

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: War Diaries	Exp/+14	Wb/+14	Cox/+14	Exp/NN	Wb/NN	Cox/NN	Exp/C=T	Wb/C=T	Cox/C=T
Execution	0.0983	-0.0543	-0.168	0.0404	0.334	0.567	0.329+	0.425	0.556
	(0.173)	(0.251)	(0.290)	(0.257)	(0.349)	(0.439)	(0.185)	(0.281)	(0.348)
Desert	-0.0630	-0.103	-0.133	-0.0341	-0.0359	-0.0738	0.0629	0.0153	-0.0542
	(0.0957)	(0.125)	(0.151)	(0.0885)	(0.138)	(0.176)	(0.105)	(0.135)	(0.168)
Ex-Desert	-0.115	-0.0948	0.0129	-0.0608	-0.313	-0.543	-0.317+	-0.391	-0.555
	(0.193)	(0.307)	(0.368)	(0.270)	(0.351)	(0.448)	(0.186)	(0.274)	(0.340)
Irish	0.00526	-0.0540	-0.0829	0.0267	-0.0417	-0.0852	0.165+	0.137	0.135
	(0.112)	(0.132)	(0.169)	(0.110)	(0.142)	(0.164)	(0.1000)	(0.132)	(0.159)
Ex-Irish	0.126	0.316	0.405	0.0677	0.0383	0.0520	0.0120	-0.0612	-0.0820
	(0.210)	(0.243)	(0.302)	(0.240)	(0.353)	(0.412)	(0.196)	(0.303)	(0.372)
N	435	435	435	438	438	438	413	413	413
Panel B: Police Gazette									
Execution	0.0293	0.135	0.104	0.244*	0.221	0.509*	0.166+	0.0692	0.220
	(0.109)	(0.330)	(0.232)	(0.118)	(0.330)	(0.257)	(0.0878)	(0.456)	(0.291)
Desert	-0.0133	0.0375	0.0177	0.0505	0.0420	0.106	0.0266	0.110	0.0745
	(0.0371)	(0.0908)	(0.0698)	(0.0532)	(0.162)	(0.0951)	(0.0288)	(0.105)	(0.0759)
Ex-Desert	-0.0245	-0.291	-0.185	-0.247*	-0.304	-0.521*	-0.130	-0.221	-0.300
	(0.116)	(0.294)	(0.221)	(0.124)	(0.324)	(0.248)	(0.0920)	(0.424)	(0.291)
Irish	-0.0384	-0.230	-0.172	-0.0332	-0.0868	-0.0822	-0.00815	-0.188	-0.158
	(0.0466)	(0.162)	(0.108)	(0.0572)	(0.141)	(0.107)	(0.0424)	(0.161)	(0.109)
Ex-Irish	0.0594	0.215	0.200	0.0306	0.278	0.135	0.0715	0.620**	0.460*
	(0.0915)	(0.315)	(0.219)	(0.119)	(0.223)	(0.188)	(0.0870)	(0.240)	(0.190)
N	1481	1481	1481	1500	1500	1500	1479	1479	1479
Panel C: FGCM Trial Registries (Time Until Next Desertion Trial)									
Execution	-0.177	0.0193	-0.0555	0.335*	0.303	0.517*	-0.133	0.118	0.00467
	(0.262)	(0.385)	(0.351)	(0.154)	(0.331)	(0.252)	(0.242)	(0.369)	(0.349)
Desert	-0.0196	0.0557	-0.0206	0.0171	0.0717	0.0787	0.0936	0.259	0.132
	(0.0934)	(0.173)	(0.101)	(0.0677)	(0.106)	(0.0844)	(0.0810)	(0.175)	(0.108)
Ex-Desert	0.174	0.110	0.137	-0.463**	-0.427	-0.580*	0.0957	-0.0744	0.00106
	(0.261)	(0.385)	(0.352)	(0.164)	(0.331)	(0.252)	(0.256)	(0.388)	(0.377)
Irish	-0.0586	-0.0378	-0.0570	0.0165	0.0232	-0.0468	-0.0146	-0.0319	-0.0504
	(0.0910)	(0.173)	(0.121)	(0.0811)	(0.145)	(0.123)	(0.0907)	(0.162)	(0.125)
Ex-Irish	-0.0397	-0.125	-0.119	-0.00562	0.0560	0.0490	0.0572	0.0739	0.142
	(0.208)	(0.308)	(0.250)	(0.222)	(0.314)	(0.209)	(0.241)	(0.355)	(0.284)
N	1648	1648	1648	1526	1526	1526	1642	1642	1642

Notes: Outcome is elapsed time from death sentence resolution (execution or commutation) until previous absence at least 90 days before the death sentence. "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 and year fixed-effects and ΔLog Casualties and ΔLog Casualties 30 Days Ago. War Diaries analysis restricts to July 1916-June 1917, which is the time window for the surviving data. Standard errors clustered at the division level in parentheses; + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

Appendix Table 2: Effects of Execution vs. Commutation on Elapsed Time Until *Previous* Absence, Full Sample, Weak SUTVA

	War Diaries								Police Gazettes				FGCM Trial Registries (Desertion Trials)					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
Execution	0.0983	0.0960	0.0432	-0.0189	-0.0629	-0.0830	0.0293	0.0271	0.0327	0.0383	0.0396	0.0385	-0.177	-0.193	-0.205	-0.206	-0.200	-0.195
	(0.173)	(0.190)	(0.198)	(0.205)	(0.211)	(0.213)	(0.109)	(0.109)	(0.110)	(0.110)	(0.110)	(0.110)	(0.262)	(0.260)	(0.263)	(0.265)	(0.264)	(0.263)
Desert	-0.0630	-0.0575	-0.0495	-0.0481	-0.0553	-0.0606	-0.0133	-0.0146	-0.0144	-0.00981	-0.00266	0.00162	-0.0196	-0.0158	-0.0171	-0.0247	-0.0310	-0.0323
	(0.0957)	(0.0947)	(0.0949)	(0.0959)	(0.0961)	(0.0957)	(0.0371)	(0.0367)	(0.0365)	(0.0362)	(0.0356)	(0.0354)	(0.0934)	(0.0889)	(0.0872)	(0.0871)	(0.0887)	(0.0898)
Ex-Desert	-0.115	-0.116	-0.0840	-0.0505	-0.0280	-0.0166	-0.0245	-0.0225	-0.0239	-0.0240	-0.0257	-0.0267	0.174	0.143	0.152	0.163	0.173	0.175
	(0.193)	(0.202)	(0.212)	(0.221)	(0.225)	(0.226)	(0.116)	(0.116)	(0.116)	(0.116)	(0.117)	(0.117)	(0.261)	(0.259)	(0.261)	(0.263)	(0.262)	(0.261)
Irish	0.00526	0.00545	-0.00151	-0.00423	-0.00138	0.000996	-0.0384	-0.0379	-0.0383	-0.0377	-0.0359	-0.0354	-0.0586	-0.0632	-0.0587	-0.0537	-0.0532	-0.0535
	(0.112)	(0.111)	(0.110)	(0.110)	(0.109)	(0.109)	(0.0466)	(0.0465)	(0.0471)	(0.0472)	(0.0457)	(0.0446)	(0.0910)	(0.0867)	(0.0865)	(0.0869)	(0.0881)	(0.0889)
Ex-Irish	0.126	0.129	0.146	0.140	0.115	0.101	0.0594	0.0620	0.0607	0.0532	0.0499	0.0519	-0.0397	-0.0107	-0.0143	-0.0212	-0.0293	-0.0339
	(0.210)	(0.208)	(0.207)	(0.212)	(0.218)	(0.220)	(0.0915)	(0.0916)	(0.0920)	(0.0915)	(0.0893)	(0.0881)	(0.208)	(0.213)	(0.211)	(0.208)	(0.207)	(0.208)
ΔLog Casualties	0.0552	0.0552	0.0527	0.0506	0.0497	0.0493	-0.0218	-0.0220	-0.0212	-0.0212	-0.0219	-0.0218	-0.0448+	-0.0485*	-0.0498*	-0.0499*	-0.0479*	-0.0466*
	(0.0347)	(0.0353)	(0.0361)	(0.0366)	(0.0364)	(0.0364)	(0.0149)	(0.0151)	(0.0150)	(0.0146)	(0.0142)	(0.0142)	(0.0235)	(0.0220)	(0.0217)	(0.0219)	(0.0225)	(0.0228)
ΔLog Casualties	0.0783*	0.0795*	0.0749*	0.0697+	0.0662+	0.0647+	-0.00676	-0.00650	-0.00572	-0.00595	-0.00658	-0.00660	-0.0302	-0.0371+	-0.0382+	-0.0371+	-0.0337	-0.0318
30 Days Ago	(0.0321)	(0.0339)	(0.0349)	(0.0358)	(0.0360)	(0.0361)	(0.0151)	(0.0149)	(0.0150)	(0.0149)	(0.0149)	(0.0149)	(0.0220)	(0.0205)	(0.0206)	(0.0210)	(0.0216)	(0.0219)
Ex's - 7d		-0.108						0.0398						-0.209*				
		(0.148)						(0.0433)						(0.0982)				
Cm's - 7d		0.00111						-0.00178						0.126**				
		(0.0663)						(0.0266)						(0.0359)				
Ex's - 14d			-0.155						0.0197						-0.169*			
			(0.132)						(0.0381)						(0.0821)			
Cm's - 14d			0.0453						-0.00956						0.0974**			
			(0.0541)						(0.0197)						(0.0266)			
Ex's - 30d				-0.146						-0.0124						-0.0909		
				(0.117)						(0.0357)						(0.0651)		
Cm's - 30d				0.0847+						-0.0167						0.0616**		
				(0.0495)						(0.0155)						(0.0202)		
Ex's - 60d					-0.115						-0.0323						-0.0338	
					(0.107)						(0.0339)						(0.0514)	
Cm's - 60d					0.107*						-0.0193						0.0342*	
					(0.0461)						(0.0127)						(0.0167)	
Ex's - 90d						-0.0991						-0.0387						-0.0125
						(0.104)						(0.0316)						(0.0456)
Cm's - 90d						0.113*						-0.0195+						0.0232
						(0.0441)						(0.0112)						(0.0154)
N	435	435	435	435	435	435	1481	1481	1481	1481	1481	1481	1648	1648	1648	1648	1648	1648

Notes: Outcome is elapsed time from death sentence resolution (execution or commutation) until previous absence at least 90 days before the death sentence. All specifcations use the "+14" commutation date imputation method and all specifications use exponential models to parameterize baseline hazard rates. All specifications include division and year fixed-effects and ΔLog Casualties and ΔLog Casualties 30 Days Ago. The regressors labeled ex's-Yd or cm's-Yd measure the cumulative effects of previous deterrence events in the unit. Y is the half-life of the effect. War Diaries analysis restricts to July 1916-June 1917, which is the time window for the surviving data. Standard errors clustered at the division level in parentheses; + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

Appendix Table 3: Effects of Executions vs. Commutations on Elapsed Time Until Next Absence

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: War Diaries	Exp/+14	Wb/+14	Cox/+14	Exp/NN	Wb/NN	Cox/NN	Exp/C=T	Wb/C=T	Cox/C=T
Execution	-0.177	-0.144	-0.158	0.183	0.167	0.129	0.280*	0.250+	0.209
	(0.141)	(0.139)	(0.140)	(0.144)	(0.141)	(0.141)	(0.139)	(0.137)	(0.137)
ΔLog Casualties	0.0928*	0.0802+	0.0648	0.0494	0.0372	0.0159	0.124**	0.110*	0.0992*
	(0.0470)	(0.0461)	(0.0458)	(0.0473)	(0.0462)	(0.0456)	(0.0468)	(0.0461)	(0.0454)
ΔLog Casualties	0.151**	0.139**	0.108*	0.140**	0.132**	0.107*	0.208**	0.190**	0.159**
30 Days Ago	(0.0457)	(0.0449)	(0.0447)	(0.0457)	(0.0444)	(0.0441)	(0.0456)	(0.0451)	(0.0447)
N	536	536	536	536	536	536	536	536	536
Panel B: Police Gazette									
Execution	-0.0770	-0.0715	-0.0662	0.0503	0.0535	0.0567	-0.0179	-0.0133	-0.0114
	(0.0783)	(0.0781)	(0.0779)	(0.0761)	(0.0759)	(0.0758)	(0.0782)	(0.0780)	(0.0780)
ΔLog Casualties	0.0569*	0.0546*	0.0517*	0.0518*	0.0502*	0.0495*	0.0584*	0.0571*	0.0558*
	(0.0228)	(0.0227)	(0.0226)	(0.0226)	(0.0225)	(0.0225)	(0.0229)	(0.0228)	(0.0228)
ΔLog Casualties	0.0620**	0.0601**	0.0584**	0.0685**	0.0664**	0.0646**	0.0719**	0.0706**	0.0695**
30 Days Ago	(0.0199)	(0.0199)	(0.0200)	(0.0201)	(0.0201)	(0.0201)	(0.0203)	(0.0203)	(0.0203)
N	1640	1640	1640	1638	1638	1638	1640	1640	1640
Panel C: FGCM Trial Registries (Time Until Next Desertion Trial)									
Execution	-0.206+	-0.198+	-0.191+	0.135	0.121	0.114	0.0282	0.0283	0.0235
	(0.105)	(0.104)	(0.104)	(0.0991)	(0.0986)	(0.0983)	(0.107)	(0.106)	(0.106)
ΔLog Casualties	0.0476	0.0387	0.0298	0.0563+	0.0472+	0.0386	0.0369	0.0339	0.0296
	(0.0291)	(0.0289)	(0.0287)	(0.0288)	(0.0286)	(0.0283)	(0.0310)	(0.0309)	(0.0307)
ΔLog Casualties	0.0796**	0.0740**	0.0684**	0.0840**	0.0796**	0.0757**	0.0272	0.0248	0.0227
30 Days Ago	(0.0261)	(0.0260)	(0.0259)	(0.0259)	(0.0258)	(0.0258)	(0.0278)	(0.0277)	(0.0277)
N	1654	1654	1654	1654	1654	1654	1654	1654	1654

Notes: Outcome is elapsed time from death sentence resolution (execution or commutation) until next absence. "Exp", "Wb" and "Cox" use the exponential, Weibull and Cox models respectively to parameterize the baseline hazard. In columns 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. Log Casualties is calculated as log(1+Casualties). ΔLog Casualties is defined as the difference in Log Casualties 1 to 29 Days Ago vs. 30 to 59 Days Ago. ΔLog Casualties 30 Days Ago is defined as the difference in Log Casualties 30 to 59 Days Ago vs. 60 to 89 Days Ago. All specifications include division and year fixed-effects. War Diaries analysis restricts to July 1916-June 1917, which is the time window for the surviving data. Standard errors in parentheses; + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

Appendix Table 4: Effects of Executions vs. Commutations on Elapsed Time Until Next Absence Differing by whether Case was a Desertion Trial and whether Soldier was Irish

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: War Diaries	Exp/+14	Wb/+14	Cox/+14	Exp/NN	Wb/NN	Cox/NN	Exp/C=T	Wb/C=T	Cox/C=T
Execution	-0.417	-0.394	-0.308	0.219	0.182	0.239	0.723+	0.627	0.689+
	(0.446)	(0.441)	(0.439)	(0.418)	(0.414)	(0.411)	(0.397)	(0.396)	(0.393)
Desert	-0.0429	-0.0218	-0.00996	0.0470	0.0531	0.0511	0.138	0.146	0.133
	(0.176)	(0.174)	(0.172)	(0.172)	(0.169)	(0.167)	(0.178)	(0.175)	(0.172)
Ex-Desert	-0.00330	0.0467	-0.0154	-0.241	-0.161	-0.218	-0.650	-0.555	-0.627
	(0.468)	(0.464)	(0.461)	(0.439)	(0.435)	(0.431)	(0.417)	(0.415)	(0.412)
Irish	-0.727**	-0.629**	-0.464**	-0.646**	-0.541**	-0.391*	-0.475*	-0.407*	-0.263
	(0.181)	(0.182)	(0.180)	(0.178)	(0.178)	(0.177)	(0.186)	(0.185)	(0.182)
Ex-Irish	1.179**	1.003**	0.805*	0.768*	0.579	0.399	0.619+	0.537	0.355
	(0.356)	(0.357)	(0.355)	(0.359)	(0.358)	(0.355)	(0.340)	(0.340)	(0.341)
N	536	536	536	536	536	536	536	536	536
Panel B: Police Gazette									
Execution	-0.372	-0.355	-0.340	0.0857	0.0890	0.0811	0.206	0.197	0.163
	(0.232)	(0.231)	(0.230)	(0.213)	(0.213)	(0.213)	(0.219)	(0.219)	(0.219)
Desert	-0.0459	-0.0409	-0.0341	-0.0245	-0.0228	-0.0212	-0.0510	-0.0488	-0.0454
	(0.0795)	(0.0794)	(0.0792)	(0.0783)	(0.0781)	(0.0781)	(0.0780)	(0.0779)	(0.0778)
Ex-Desert	0.251	0.241	0.235	-0.0773	-0.0747	-0.0611	-0.327	-0.309	-0.267
	(0.248)	(0.247)	(0.245)	(0.231)	(0.230)	(0.229)	(0.236)	(0.236)	(0.235)
Irish	-0.179*	-0.172+	-0.164+	-0.187*	-0.175*	-0.169+	-0.119	-0.116	-0.114
	(0.0895)	(0.0892)	(0.0891)	(0.0892)	(0.0890)	(0.0890)	(0.0902)	(0.0900)	(0.0899)
Ex-Irish	0.431*	0.410*	0.387*	0.219	0.203	0.196	0.408*	0.392*	0.382+
	(0.198)	(0.197)	(0.197)	(0.198)	(0.198)	(0.197)	(0.198)	(0.198)	(0.197)
N	1640	1640	1640	1638	1638	1638	1640	1640	1640
Panel C: FGCM Trial Registries (Time Until Next Desertion Trial)									
Execution	-0.709*	-0.648*	-0.588*	0.0476	0.0296	0.0233	0.0772	0.0703	0.0526
	(0.286)	(0.283)	(0.281)	(0.260)	(0.259)	(0.258)	(0.254)	(0.253)	(0.253)
Desert	0.0535	0.0411	0.0235	0.110	0.0816	0.0482	-0.0590	-0.0656	-0.0855
	(0.0990)	(0.0982)	(0.0977)	(0.0981)	(0.0976)	(0.0972)	(0.103)	(0.103)	(0.103)
Ex-Desert	0.442	0.397	0.351	-0.0496	-0.0232	-0.00214	-0.164	-0.148	-0.116
	(0.306)	(0.303)	(0.301)	(0.281)	(0.280)	(0.279)	(0.281)	(0.280)	(0.279)
Irish	-0.353**	-0.326**	-0.297*	-0.221*	-0.196+	-0.172	-0.252*	-0.243+	-0.218+
	(0.116)	(0.116)	(0.115)	(0.113)	(0.112)	(0.112)	(0.124)	(0.124)	(0.123)
Ex-Irish	0.718**	0.639*	0.560*	0.651**	0.566*	0.480+	0.556*	0.525+	0.465+
	(0.271)	(0.271)	(0.270)	(0.252)	(0.251)	(0.250)	(0.269)	(0.268)	(0.268)
N	1654	1654	1654	1654	1654	1654	1654	1654	1654

Notes: Outcome is elapsed time from death sentence resolution (execution or commutation) until next absence. "Exp", "Wb" and "Cox" use the exponential, Weibull and Cox models respectively to parameterize the baseline hazard. In columns 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 and year fixed-effects and ΔLog Casualties and ΔLog Casualties 30 Days Ago. War Diaries analysis restricts to July 1916-June 1917, which is the time window for the surviving data. Standard errors in parentheses; + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

Appendix Table 5: Effects of Execution vs. Commutation on Elapsed Time Until Next Absence, Full Sample, Weak SUTVA

	War Diaries								Police Gazettes				FGCM Trial Registries (Desertion Trials)					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
Execution	-0.417	-0.386	-0.374	-0.363	-0.370	-0.384	-0.372	-0.542*	-0.512*	-0.475*	-0.432+	-0.411+	-0.709*	-0.919**	-0.895**	-0.856**	-0.791**	-0.752**
	(0.446)	(0.453)	(0.452)	(0.451)	(0.450)	(0.450)	(0.232)	(0.245)	(0.242)	(0.237)	(0.234)	(0.233)	(0.286)	(0.305)	(0.301)	(0.295)	(0.290)	(0.288)
Desert	-0.0429	-0.0205	-0.0188	-0.0251	-0.0512	-0.0756	-0.0459	-0.0711	-0.0804	-0.0656	-0.0458	-0.0412	0.0535	0.0272	0.0179	0.0317	0.0559	0.0630
	(0.176)	(0.177)	(0.177)	(0.176)	(0.175)	(0.175)	(0.0795)	(0.0800)	(0.0799)	(0.0796)	(0.0795)	(0.0795)	(0.0990)	(0.0998)	(0.0997)	(0.0994)	(0.0993)	(0.0992)
Ex-Desert	-0.00330	-0.0293	-0.0343	-0.0282	-0.00235	0.0168	0.251	0.310	0.283	0.258	0.235	0.229	0.442	0.518	0.504	0.474	0.430	0.411
	(0.468)	(0.472)	(0.470)	(0.467)	(0.465)	(0.464)	(0.248)	(0.260)	(0.257)	(0.253)	(0.250)	(0.249)	(0.306)	(0.323)	(0.320)	(0.315)	(0.311)	(0.309)
Irish	-0.727**	-0.766**	-0.782**	-0.821**	-0.849**	-0.836**	-0.179*	-0.158+	-0.172+	-0.186*	-0.189*	-0.185*	-0.353**	-0.351**	-0.365**	-0.373**	-0.366**	-0.358**
	(0.181)	(0.184)	(0.185)	(0.186)	(0.185)	(0.183)	(0.0895)	(0.0898)	(0.0897)	(0.0896)	(0.0896)	(0.0896)	(0.116)	(0.117)	(0.117)	(0.117)	(0.117)	(0.117)
Ex-Irish	1.179**	1.250**	1.256**	1.305**	1.355**	1.343**	0.431*	0.432*	0.440*	0.437*	0.424*	0.421*	0.718**	0.726**	0.750**	0.778**	0.775**	0.761**
	(0.356)	(0.361)	(0.361)	(0.361)	(0.359)	(0.358)	(0.198)	(0.199)	(0.198)	(0.198)	(0.198)	(0.198)	(0.271)	(0.273)	(0.273)	(0.272)	(0.272)	(0.272)
ΔLog Casualties	0.0870+	0.0811+	0.0781+	0.0721	0.0624	0.0571	0.0537*	0.0744**	0.0776**	0.0747**	0.0681**	0.0628**	0.0422	0.0597*	0.0658*	0.0662*	0.0605*	0.0543+
	(0.0465)	(0.0466)	(0.0468)	(0.0470)	(0.0474)	(0.0477)	(0.0229)	(0.0233)	(0.0234)	(0.0233)	(0.0231)	(0.0230)	(0.0291)	(0.0296)	(0.0298)	(0.0298)	(0.0296)	(0.0295)
ΔLog Casualties	0.170**	0.171**	0.168**	0.162**	0.151**	0.146**	0.0652**	0.0620**	0.0681**	0.0719**	0.0709**	0.0689**	0.0856**	0.0824**	0.0923**	0.100**	0.0987**	0.0944**
30 Days Ago	(0.0457)	(0.0456)	(0.0456)	(0.0458)	(0.0461)	(0.0462)	(0.0199)	(0.0202)	(0.0203)	(0.0203)	(0.0202)	(0.0201)	(0.0261)	(0.0263)	(0.0264)	(0.0267)	(0.0266)	(0.0265)
Ex's - 7d		-0.174						0.217**						0.355**				
		(0.170)						(0.0841)						(0.113)				
Cm's - 7d		-0.0348						0.214**						0.183**				
		(0.0909)						(0.0328)						(0.0408)				
Ex's - 14d			-0.133						0.0852						0.172*			
			(0.137)						(0.0632)						(0.0832)			
Cm's - 14d			-0.0468						0.156**						0.140**			
			(0.0670)						(0.0236)						(0.0299)			
Ex's - 30d				-0.138						-0.0264						0.0213		
				(0.114)						(0.0475)						(0.0615)		
Cm's - 30d				-0.0673						0.105**						0.0991**		
				(0.0502)						(0.0168)						(0.0219)		
Ex's - 60d					-0.186+						-0.0731+						-0.0392	
					(0.104)						(0.0386)						(0.0497)	
Cm's - 60d					-0.0872*						0.0651**						0.0623**	
					(0.0412)						(0.0130)						(0.0169)	
Ex's - 90d						-0.216*						-0.0855*						-0.0570
						(0.100)						(0.0354)						(0.0455)
Cm's - 90d						-0.0902*						0.0454**						0.0425**
						(0.0375)						(0.0116)						(0.0149)
N	536	536	536	536	536	536	1640	1640	1640	1640	1640	1640	1654	1654	1654	1654	1654	1654

Notes: All specifcations use the "+14" commutation date imputation method and all specifications use exponential models to parameterize baseline hazard rates. All specifications include division and year fixed-effects. Log Casualties is calculated as log(1+Casualties). ΔLog Casualties is defined as the difference in Log Casualties 1 to 29 Days Ago vs. 30 to 59 Days Ago. ΔLog Casualties 30 Days Ago is defined as the difference in Log Casualties 30 to 59 Days Ago vs. 60 to 89 Days Ago. The regressors labeled ex's-Yd or cm's-Yd measure the cumulative effects of previous deterrence events in the unit. Y is the half-life of the effect. War Diaries analysis restricts to July 1916-June 1917, which is the time window for the surviving data. Standard errors in parentheses; + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

Appendix Table 6: Effects of Executions vs. Commutations on Elapsed Time Until *Previous* Absence Differing by whether Case was a Desertion Trial and whether Soldier was Irish

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: War Diaries	Exp/+14	Wb/+14	Cox/+14	Exp/NN	Wb/NN	Cox/NN	Exp/C=T	Wb/C=T	Cox/C=T
Execution	0.0983	-0.0543	-0.168	0.0404	0.334	0.567	0.329	0.425	0.556
	(0.388)	(0.394)	(0.400)	(0.436)	(0.434)	(0.438)	(0.451)	(0.453)	(0.458)
Desert	-0.0630	-0.103	-0.133	-0.0341	-0.0359	-0.0738	0.0629	0.0153	-0.0542
	(0.170)	(0.171)	(0.173)	(0.169)	(0.175)	(0.179)	(0.193)	(0.196)	(0.200)
Ex-Desert	-0.115	-0.0948	0.0129	-0.0608	-0.313	-0.543	-0.317	-0.391	-0.555
	(0.413)	(0.422)	(0.427)	(0.453)	(0.457)	(0.464)	(0.468)	(0.473)	(0.478)
Irish	0.00526	-0.0540	-0.0829	0.0267	-0.0417	-0.0852	0.165	0.137	0.135
	(0.185)	(0.189)	(0.195)	(0.185)	(0.191)	(0.195)	(0.199)	(0.205)	(0.208)
Ex-Irish	0.126	0.316	0.405	0.0677	0.0383	0.0520	0.0120	-0.0612	-0.0820
	(0.386)	(0.392)	(0.403)	(0.382)	(0.390)	(0.398)	(0.392)	(0.399)	(0.406)
N	435	435	435	438	438	438	413	413	413
Panel B: Police Gazette									
Execution	0.0293	0.135	0.104	0.244	0.221	0.509*	0.166	0.0692	0.220
	(0.234)	(0.239)	(0.237)	(0.215)	(0.221)	(0.221)	(0.222)	(0.231)	(0.226)
Desert	-0.0133	0.0375	0.0177	0.0505	0.0420	0.106	0.0266	0.110	0.0745
	(0.0823)	(0.0839)	(0.0831)	(0.0849)	(0.0916)	(0.0885)	(0.0832)	(0.0846)	(0.0834)
Ex-Desert	-0.0245	-0.291	-0.185	-0.247	-0.304	-0.521*	-0.130	-0.221	-0.300
	(0.248)	(0.255)	(0.253)	(0.231)	(0.239)	(0.238)	(0.236)	(0.247)	(0.242)
Irish	-0.0384	-0.230*	-0.172+	-0.0332	-0.0868	-0.0822	-0.00815	-0.188*	-0.158+
	(0.0901)	(0.0939)	(0.0912)	(0.0905)	(0.0936)	(0.0917)	(0.0899)	(0.0941)	(0.0911)
Ex-Irish	0.0594	0.215	0.200	0.0306	0.278	0.135	0.0715	0.620**	0.460*
	(0.209)	(0.216)	(0.213)	(0.215)	(0.220)	(0.218)	(0.204)	(0.212)	(0.209)
N	1481	1481	1481	1500	1500	1500	1479	1479	1479
Panel C: FGCM Trial Registries (Time Until Next Desertion Trial)									
Execution	-0.177	0.0193	-0.0555	0.335	0.303	0.517*	-0.133	0.118	0.00467
	(0.285)	(0.293)	(0.290)	(0.235)	(0.242)	(0.240)	(0.307)	(0.316)	(0.313)
Desert	-0.0196	0.0557	-0.0206	0.0171	0.0717	0.0787	0.0936	0.259*	0.132
	(0.0930)	(0.0997)	(0.0945)	(0.0980)	(0.100)	(0.0986)	(0.0989)	(0.108)	(0.102)
Ex-Desert	0.174	0.110	0.137	-0.463+	-0.427	-0.580*	0.0957	-0.0744	0.00106
	(0.303)	(0.309)	(0.308)	(0.260)	(0.270)	(0.267)	(0.326)	(0.334)	(0.333)
Irish	-0.0586	-0.0378	-0.0570	0.0165	0.0232	-0.0468	-0.0146	-0.0319	-0.0504
	(0.108)	(0.113)	(0.110)	(0.107)	(0.112)	(0.110)	(0.112)	(0.117)	(0.114)
Ex-Irish	-0.0397	-0.125	-0.119	-0.00562	0.0560	0.0490	0.0572	0.0739	0.142
	(0.260)	(0.266)	(0.263)	(0.261)	(0.271)	(0.267)	(0.266)	(0.274)	(0.270)
N	1648	1648	1648	1526	1526	1526	1642	1642	1642

Notes: Outcome is elapsed time from death sentence resolution (execution or commutation) until previous absence at least 90 days before the death sentence. "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 and year fixed-effects and ΔLog Casualties and ΔLog Casualties 30 Days Ago. War Diaries analysis restricts to July 1916-June 1917, which is the time window for the surviving data. Standard errors in parentheses; + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

Appendix Table 7: Effects of Execution vs. Commutation on Elapsed Time Until *Previous* Absence, Full Sample, Weak SUTVA

	War Diaries							Police Gazettes					FGCM Trial Registries (Desertion Trials)					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
Execution	0.0983	0.0960	0.0432	-0.0189	-0.0629	-0.0830	0.0293	0.0271	0.0327	0.0383	0.0396	0.0385	-0.177	-0.193	-0.205	-0.206	-0.200	-0.195
	(0.388)	(0.393)	(0.395)	(0.397)	(0.397)	(0.396)	(0.234)	(0.235)	(0.235)	(0.235)	(0.235)	(0.235)	(0.285)	(0.285)	(0.285)	(0.285)	(0.285)	(0.285)
Desert	-0.0630	-0.0575	-0.0495	-0.0481	-0.0553	-0.0606	-0.0133	-0.0146	-0.0144	-0.00981	-0.00266	0.00162	-0.0196	-0.0158	-0.0171	-0.0247	-0.0310	-0.0323
	(0.170)	(0.171)	(0.171)	(0.171)	(0.171)	(0.170)	(0.0823)	(0.0824)	(0.0824)	(0.0826)	(0.0827)	(0.0828)	(0.0930)	(0.0931)	(0.0931)	(0.0930)	(0.0931)	(0.0931)
Ex-Desert	-0.115	-0.116	-0.0840	-0.0505	-0.0280	-0.0166	-0.0245	-0.0225	-0.0239	-0.0240	-0.0257	-0.0267	0.174	0.143	0.152	0.163	0.173	0.175
	(0.413)	(0.415)	(0.416)	(0.417)	(0.416)	(0.415)	(0.248)	(0.248)	(0.248)	(0.248)	(0.248)	(0.248)	(0.303)	(0.303)	(0.303)	(0.303)	(0.303)	(0.303)
Irish	0.00526	0.00545	-0.00151	-0.00423	-0.00138	0.000996	-0.0384	-0.0379	-0.0383	-0.0377	-0.0359	-0.0354	-0.0586	-0.0632	-0.0587	-0.0537	-0.0532	-0.0535
	(0.185)	(0.186)	(0.186)	(0.185)	(0.185)	(0.185)	(0.0901)	(0.0901)	(0.0901)	(0.0901)	(0.0901)	(0.0901)	(0.108)	(0.109)	(0.109)	(0.108)	(0.108)	(0.108)
Ex-Irish	0.126	0.129	0.146	0.140	0.115	0.101	0.0594	0.0620	0.0607	0.0532	0.0499	0.0519	-0.0397	-0.0107	-0.0143	-0.0212	-0.0293	-0.0339
	(0.386)	(0.387)	(0.386)	(0.386)	(0.386)	(0.386)	(0.209)	(0.209)	(0.210)	(0.210)	(0.210)	(0.210)	(0.260)	(0.261)	(0.261)	(0.261)	(0.260)	(0.260)
ΔLog Casualties	0.0552	0.0552	0.0527	0.0506	0.0497	0.0493	-0.0218	-0.0220	-0.0212	-0.0212	-0.0219	-0.0218	-0.0448	-0.0485+	-0.0498+	-0.0499+	-0.0479+	-0.0466+
	(0.0498)	(0.0500)	(0.0502)	(0.0505)	(0.0509)	(0.0511)	(0.0250)	(0.0251)	(0.0251)	(0.0250)	(0.0250)	(0.0250)	(0.0275)	(0.0277)	(0.0277)	(0.0278)	(0.0278)	(0.0278)
ΔLog Casualties	0.0783+	0.0795+	0.0749	0.0697	0.0662	0.0647	-0.00676	-0.00650	-0.00572	-0.00595	-0.00658	-0.00660	-0.0302	-0.0371	-0.0382	-0.0371	-0.0337	-0.0318
30 Days Ago	(0.0465)	(0.0479)	(0.0477)	(0.0473)	(0.0469)	(0.0467)	(0.0228)	(0.0230)	(0.0230)	(0.0230)	(0.0230)	(0.0230)	(0.0249)	(0.0252)	(0.0253)	(0.0253)	(0.0253)	(0.0252)
Ex's - 7d		-0.108						0.0398						-0.209+				
		(0.221)						(0.0976)						(0.124)				
Cm's - 7d		0.00111						-0.00178						0.126**				
		(0.0993)						(0.0387)						(0.0416)				
Ex's - 14d			-0.155						0.0197						-0.169+			
			(0.167)						(0.0706)						(0.0883)			
Cm's - 14d			0.0453						-0.00956						0.0974**			
			(0.0783)						(0.0276)						(0.0305)			
Ex's - 30d				-0.146						-0.0124						-0.0909		
				(0.133)						(0.0524)						(0.0631)		
Cm's - 30d				0.0847						-0.0167						0.0616**		
				(0.0618)						(0.0196)						(0.0226)		
Ex's - 60d					-0.115						-0.0323						-0.0338	
					(0.117)						(0.0425)						(0.0499)	
Cm's - 60d					0.107*						-0.0193						0.0342+	
					(0.0514)						(0.0151)						(0.0177)	
Ex's - 90d						-0.0991						-0.0387						-0.0125
						(0.112)						(0.0382)						(0.0447)
Cm's - 90d						0.113*						-0.0195						0.0232
						(0.0471)						(0.0133)						(0.0155)
N	435	435	435	435	435	435	1481	1481	1481	1481	1481	1481	1648	1648	1648	1648	1648	1648

Notes: Outcome is elapsed time from death sentence resolution (execution or commutation) until previous absence at least 90 days before the death sentence. All specifcations use the "+14" commutation date imputation method and all specifications use exponential models to parameterize baseline hazard rates. All specifications include division and year fixed-effects and ΔLog Casualties and ΔLog Casualties 30 Days Ago. The regressors labeled ex's-Yd or cm's-Yd measure the cumulative effects of previous deterrence events in the unit. Y is the half-life of the effect. War Diaries analysis restricts to July 1916-June 1917, which is the time window for the surviving data. Standard errors in parentheses; + p < 0.10, * p < 0.05, ** p < 0.01

Appendix Table 8: Day-by-Day Framework, Future Events and Previous Absences

Panel A: War Diaries	(1)	(2)	(3)	(4)	(5)
Half-life	1 week	2 weeks	1 month	2 months	3 months
Execution	0.0171	0.0127	0.00897	0.00550	0.00318
	(0.0218)	(0.0156)	(0.0118)	(0.0102)	(0.00921)
Death Sentence	-0.0000876	-0.000565	-0.000739	-0.000570	-0.000497
	(0.00143)	(0.00127)	(0.00111)	(0.000977)	(0.000942)
Ex-Irish	0.0127	0.0121	0.0124	0.0144	0.0158
	(0.0137)	(0.0102)	(0.00928)	(0.00953)	(0.00958)
Irish	-0.0137*	-0.0123*	-0.00919+	-0.00765	-0.00724
	(0.00511)	-0.0049	(0.00465)	(0.00457)	(0.00440)
Ex-Desert	-0.0190	-0.0147	-0.0122	-0.0101	-0.00839
	(0.0197)	(0.0147)	(0.0117)	(0.0101)	(0.00893)
Desert	0.00204	0.00298	0.00279	0.00250+	0.00239+
	(0.00196)	(0.00196)	(0.00181)	(0.00144)	(0.00130)
N	20750	20750	20750	20750	20750
Panel B: Police Gazettes					
Execution	0.00273	-0.00634	-0.00989	-0.00994	-0.00857
	(0.0214)	(0.0182)	(0.0139)	(0.0106)	(0.00915)
Death Sentence	0.00741	0.00596	0.00414	0.00250	0.00167
	(0.00452)	(0.00414)	(0.00312)	(0.00207)	(0.00160)
Ex-Irish	0.0124	0.00621	-0.000201	-0.00325	-0.00353
	(0.0161)	(0.0125)	(0.00906)	(0.00699)	(0.00599)
Irish	-0.00154	0.00198	0.00371	0.00390	0.00352
	(0.00586)	(0.00506)	(0.00443)	(0.00382)	(0.00333)
Ex-Desert	-0.00454	0.00626	0.00954	0.00830	0.00629
	(0.0206)	(0.0169)	(0.0128)	(0.00949)	(0.00818)
Desert	-0.00729	-0.00703	-0.00569	-0.00364	-0.00237
	(0.00454)	(0.00419)	(0.00341)	(0.00256)	(0.00215)
N	50465	50465	50465	50465	50465
Panel C: FGCM Desertion Trial Registries					
Execution	-0.0308*	-0.0245+	-0.0133	-0.00607	-0.00400
	(0.0141)	(0.0122)	(0.0110)	(0.00890)	(0.00705)
Death Sentence	0.00367	0.00350	0.00288+	0.00196+	0.00144+
	(0.00313)	(0.00233)	(0.00167)	(0.00111)	(0.000793)
Ex-Irish	-0.00701	-0.00497	-0.00283	-0.000694	0.000834
	(0.0167)	(0.0134)	(0.00998)	(0.00725)	(0.00612)
Irish	0.0152*	0.0122**	0.00857*	0.00532*	0.00366+
	(0.00611)	(0.00456)	(0.00345)	(0.00249)	(0.00199)
Ex-Desert	0.0231	0.0178	0.00644	0.000998	0.000752
	(0.0143)	(0.0124)	(0.0118)	(0.00979)	(0.00785)
Desert	0.00102	0.000660	0.00261	0.00364*	0.00345*
	(0.00468)	(0.00322)	(0.00227)	(0.00168)	(0.00136)
N	54855	54855	54855	54855	54855

Notes: Outcome is whether there was any absence on that day and division. All specifcations use the "+14" commutation date imputation method and include division and year fixed-effects, ΔLog Casualties, and ΔLog Casualties 30 Days Ago. The half-life row indicates the assumed exponential half-life of the effect of future events beginning 90 days in the future. War Diaries analysis restricts to July 1916-June 1917, which is the time window for the surviving data. War Diaries analysis restricts to July 1916-June 1917, which is the time window for the surviving data. Standard errors clustered at the division level in parentheses; + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

Appendix Table 9: Day-by-Day Framework, Future Events and Previous Irish - non-Irish Absence

Panel A: War Diaries	(1)	(2)	(3)	(4)	(5)
Half-life	1 week	2 weeks	1 month	2 months	3 months
Execution	-0.00885	-0.00633	-0.00499	-0.00447	-0.00374
	(0.0110)	(0.00899)	(0.00544)	(0.00531)	(0.00562)
Death Sentence	0.000503	0.000822	0.000949	0.000954	0.000943
	(0.00153)	(0.00119)	(0.000903)	(0.000834)	(0.000819)
Ex-Irish	0.00699	0.00319	0.00103	-0.00117	-0.00298
	(0.0145)	(0.00875)	(0.00680)	(0.00696)	(0.00706)
Irish	0.00239	0.00102	-0.000630	-0.000713	-0.000241
	(0.00447)	(0.00279)	(0.00254)	(0.00278)	(0.00273)
Ex-Desert	0.00150	0.00128	0.00256	0.00430	0.00474
	(0.0130)	(0.0105)	(0.00544)	(0.00435)	(0.00468)
Desert	0.00167	0.000528	-0.000378	-0.00109	-0.00135
	(0.00281)	(0.00218)	(0.00151)	(0.00116)	(0.00106)
N	20750	20750	20750	20750	20750
Panel B: Police Gazettes					
Execution	-0.0282	-0.0199	-0.0114	-0.00543	-0.00315
	(0.0178)	(0.0129)	(0.00849)	(0.00568)	(0.00453)
Death Sentence	0.000329	0.000108	-0.000266	-0.000187	-0.00000606
	(0.00221)	(0.00164)	(0.00122)	(0.000866)	(0.000701)
Ex-Irish	0.00120	0.00495	0.00567+	0.00449+	0.00339
	(0.0103)	(0.00605)	(0.00330)	(0.00249)	(0.00232)
Irish	-0.000198	-0.00167	-0.00158	-0.00139	-0.00133
	(0.00532)	(0.00397)	(0.00297)	(0.00214)	(0.00172)
Ex-Desert	0.0269	0.0187	0.0113	0.00639	0.00450
	(0.0179)	(0.0130)	(0.00874)	(0.00599)	(0.00487)
Desert	-0.00338	-0.00169	-0.000409	-0.000265	-0.000454
	(0.00262)	(0.00175)	(0.00133)	(0.00103)	(0.000886)
N	50465	50465	50465	50465	50465
Panel C: FGCM Desertion Trial Registries					
Execution	0.0332**	0.0259*	0.0148	0.00739	0.00475
	(0.0121)	(0.0109)	(0.00949)	(0.00725)	(0.00563)
Death Sentence	-0.00265	-0.00240	-0.00207	-0.00157	-0.00121
	(0.00266)	(0.00191)	(0.00133)	(0.000948)	(0.000748)
Ex-Irish	-0.00942	-0.00784	-0.00677	-0.00461	-0.00361
	(0.0122)	(0.00976)	(0.00774)	(0.00620)	(0.00553)
Irish	-0.00851+	-0.00807*	-0.00398	-0.000594	0.000457
	(0.00441)	(0.00338)	(0.00263)	(0.00218)	(0.00192)
Ex-Desert	-0.0248*	-0.0174	-0.00559	-0.000398	0.0000879
	(0.0123)	(0.0108)	(0.00925)	(0.00727)	(0.00597)
Desert	-0.00206	-0.000792	-0.00226	-0.00319*	-0.00308**
	(0.00333)	(0.00218)	(0.00155)	(0.00122)	(0.00104)
N	54855	54855	54855	54855	54855

Notes: Outcome is whether there was any Irish absence on that day and division minus whether there was any non-Irish absence on that day and division. All specifcations use the "+14" commutation date imputation method and include division and year fixed-effects, ΔLog Casualties, and ΔLog Casualties 30 Days Ago. The half-life row indicates the assumed exponential half-life of the effect of past events. War Diaries analysis restricts to July 1916-June 1917, which is the time window for the surviving data. War Diaries analysis restricts to July 1916-June 1917, which is the time window for the surviving data. Standard errors clustered at the division level in parentheses; + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$