

Conscription and Crime: Evidence from the Argentine Draft Lottery

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September 13, 2010

Abstract

We estimate the causal effect of mandatory participation in military service on individuals' subsequent involvement in criminal activities. To identify this causal effect we exploit the random assignment of young men to conscription in Argentina through a draft lottery. Using a dataset that includes draft eligibility, participation in military service, and criminal records, we find that conscription increases the likelihood of developing a criminal record. The effects are significant not only for cohorts that provided military service during wartime, but also for those that served during peacetime. Our results do not support the introduction of conscription for anti-crime purposes.

JEL Classification: K42

Keywords: Military service, violent behavior, crime.

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I. Introduction

Criminal activity is typically concentrated among young men (Freeman, 1996; DiIulio, 1996; Levitt, 2004).¹ Most criminals begin their participation in illegal activities as juvenile or young adult offenders.² A common proposal in public debates around the world on how to reduce crime is to introduce conscription. For example, in 2007 Labor Minister Membathisi Mdladlana of South Africa proposed army conscription as a response to the high levels of criminality. In the same vein, President Jacques Chirac responded in 2005 to the violent crisis in the Paris suburbs by announcing the creation of a voluntary civil service aimed at youngsters “who failed school and are in the process of social marginalization.” In Argentina, since conscription was halted in 1995, its reimplementation has been repeatedly proposed in Congress to address the current crime wave. Similarly, reimplementing conscription in Peru has recently been proposed to reduce violence among youth.³

These policy proposals are based on the assumption that military conscription will reduce young men’s subsequent criminal behavior. For example, military training teaches young men obedience and discipline, which could reduce their rates of criminality. Moreover, by improving health and nutrition and by extending the social networks of the most deprived to include other socioeconomic groups, military service might enhance labor market prospects, preventing young men from committing property crimes. In

¹ In the US, for example, persons aged between 18 and 24 accounted for 28 percent of total property crime arrests, and 77 percent of all arrestees were male (Pastore and Maguire, 2003). For a Latin American example, see De Melo and Schneider (2010).

² On the determinants of juvenile crime, see Case and Katz (1991); Levitt (1998); Grogger (1998); Levitt and Lochner (2001); Jacob and Lefgren (2003); Kling, Ludwig, and Katz (2005); and Bayer, Hjalmarsson, and Pozen (2009).

³ For South Africa, see News24.com, “Minister moots conscription,” January 30, 2007. For France, see LeMonde.fr, “Jacques Chirac lance le service civil volontaire”, November 17, 2005. For Argentina, see LaNacion.com.ar, “Analizan la restitución de la conscripción”, October 16, 2002; LaNacion.com.ar, “Susana Giménez pidió que vuelva el servicio militar obligatorio”, March 17, 2009; LaNacion.com.ar, “El delito no es prioridad del congreso” December 7, 2009. For Peru, see Peru21.pe, “Polémica por retorno del servicio militar obligatorio”, November 17, 2009.

addition, military service incapacitates the commission of crime by young men during the time they are serving in military facilities and are thus off the streets.

However, military conscription could instead have a detrimental influence on young men's criminal behavior. First, by delaying young men's entry into the labor market, conscription might reduce their future labor market opportunities and thus increase the likelihood that they commit property crimes.⁴ Second, military service provides training in use of weapons, reducing entry costs and potentially increasing participation in armed criminal activity. Finally, conscription may create a social environment prone to violent responses, negative peer effects, and gang formation.⁵ Thus it is not clear a priori whether the overall impact of military service on crime rates is positive or negative. Empirical evidence is needed.

Previous studies have exploited the natural experiment generated by the Vietnam draft lottery in the US to analyze the impact of serving in the military during wartime on outcomes such as future earnings (Angrist, 1990; Angrist and Chen, 2007), alcohol consumption (Goldberg et al., 1991), cigarette consumption (Eisemberg and Rowe, 2009), health (Angrist et al., 2009; Dobkin and Shabini, 2009), and mortality (Hearst, Newman, and Hulley, 1986; Conley and Heerwig, 2009). In particular, some previous studies have analyzed the relationship between being a war veteran and subsequent criminal behavior (see Yager et al., 1984; Beckerman and Fontana, 1989; Bouffard, 2003; Rohlfs, 2006; Mumola, 2007; Noonan and Mumola, 2007). Although the effects are small, the existing evidence shows that exposure to combat is associated with an increase in subsequent number of arrests and likelihood of incarceration.

⁴ Milton Friedman and other economists stressed the job market costs imposed on draftees in their interventions opposing the Vietnam draft in favour of a professional army (see Oi, 1967; Tax, 1967; and the "Economists' Statement in Opposition to the Draft").

⁵ On violent responses by individuals trained in the use of weapons, see Bryant (1979). On crime and social interactions, see Glaeser, Sacerdote, and Scheinkman (1996).

However, the effects on crime of wartime military service might be quite different from the effects of peacetime conscription. Subjects exposed to combat are likely to suffer from post-traumatic disorders.⁶ Medical studies document that these patients report different attitudes toward violent crime, higher levels of self-reported aggression, and a higher incidence of potentially dangerous firearm-related behavior than comparison subjects (see McFall et al., 1999, and Freeman and Roca, 2001). Individuals serving in the military during peacetime are, in principle, not exposed to the kinds of traumatic events that cause these stress disorders.

Thus the impact on crime of serving in the military during peacetime is still an open question and, with it, the validity of those policy recommendations. The main contribution of this paper is to estimate the causal effect of peacetime military conscription on subsequent criminal behavior.

Our findings have broader policy implications than the existing literature on war veterans' subsequent criminal behavior. As a public policy, conscription is much more common than is armed conflict, which for most countries is an unwanted and infrequent occurrence.⁷ We have been able to find data on conscription for 179 countries, covering 99.8% of the world population. Among these 179 countries, 94 have mandatory military service. Among these 94 countries, only 19 are currently involved in some type of armed conflict.⁸ Thus, over 40% of the countries of the world have mandatory military service without being currently involved in armed conflict.

⁶ In particular, Post-Traumatic Stress Disorder (PTSD), the long-term emotional response to a highly traumatic event, is a diagnosis which was officially identified after the Vietnam War. PTSD is an emotional illness that develops as a result of a terribly frightening, life-threatening, or otherwise highly unsafe experience (American Psychiatric Association, 1994). For the male population, the highest prevalence rates are found among survivors of military combat. As reported by the National Center for Post-Traumatic Stress Disorder, about 30% of Vietnam veterans suffer from PTSD.

⁷ On the practice of military conscription around the world, see WRI (1998) and Mulligan and Shleifer (2005).

⁸ See <http://www.wri-irg.org/wri.htm> (accessed 2007). Out of the 85 countries without conscription, eleven are involved in an armed conflict.

In order to identify the causal effect of conscription on crime, we need to identify a variable that affects participation in conscription but does not affect crime through any other mechanisms. The draft lottery in Argentina offers an opportunity to address this question. The lottery randomly assigned eligibility of all young males to military service based on the last three numbers of their national ID. We exploit this random assignment to identify the causal effect of mandatory military service on the likelihood of later developing a criminal record.

We use a unique set of administrative data that includes draft eligibility, participation in military service, and criminal records for male cohorts born between 1958 and 1962. We complement this with data on draft eligibility and criminal records for male cohorts born between 1929 and 1976. We find that participation in conscription increases the likelihood of developing a subsequent criminal record, particularly for crimes against property and white collar crimes. We also find that conscription has detrimental effects on future job market performance.

Moreover, since our database includes two cohorts that participated in the 1982 Malvinas War between Argentina and the United Kingdom, we are able to identify the difference between being drafted into the military in peacetime versus in wartime. Our results suggest that the effect of conscription on criminal behavior is larger for draftees in the cohorts that participated in the Malvinas War than for other cohorts. However, the effects on crime are also significant for the cohorts that performed military service during peacetime. In sum, our results do not encourage the introduction of military conscription to reduce criminal behavior.

The organization of the paper is as follows. Section II describes the natural experiment and the main characteristics of military service in Argentina. Section III

discusses the data, and Section IV reports the econometric methods and results. Section V presents our conclusions.

II. Military conscription in Argentina: a natural experiment

From 1901 through 1995, military service in Argentina was mandatory. The length of service was a minimum of one year and a maximum of two years. Service began with a three-month basic instruction period (typically starting in March) during which recruits learned military norms and were exposed to combat training. After that, conscripts were allocated to a military unit to perform a specific duty, not necessarily related to military training.⁹

In earlier times, males were called to serve at the age of 21; later, this was changed to age 18. The cohort born in 1955 was the last cohort starting military service at age 21; the cohort born in 1958 was the first cohort starting serving at age 18. Because of this change, the cohorts born in 1956 and 1957 were not called at all. Recruits from the cohorts born in 1962 and 1963 participated in the Malvinas War. The cohort born in 1976 faced the draft lottery but eventually was not drafted as conscription was halted.

The eligibility of young males for military service was randomly determined, using the last three digits of their national IDs.¹⁰ Each year, for the cohort due to be conscripted the following year, a lottery assigned a number between 1 and 1,000 to each combination of the last three ID digits. The lottery system was run in a public session using a lottery drum filled with a thousand balls number 1 through 1,000. The first ball released from the lottery drum corresponded to ID number 000, the second released ball to ID number 001, and so on. The lottery was administered by the National Lottery and supervised by the

⁹ On the military service in Argentina, see Rodríguez Molas (1983).

¹⁰ Mandatory national ID numbers were issued at age 18 until 1968 (military service was then served at age 21), and at birth since that year. During the period of analysis, they are seven or eight digits long.

National General Notary in a public session. Results were broadcasted over the radio and published in the main newspapers.

After the lottery, individuals were called for physical and mental examinations. Later on, a cutoff number was announced. Individuals whose ID number had been assigned a lottery number higher than the cutoff number -and who had passed the medical examination- were mandatorily called to military service. Among the ID numbers eligible for conscription, those with the lowest lottery numbers were assigned to the Army, the intermediate numbers to the Air Force, and the highest numbers to the Navy. Conscription in the Army and the Air Force was for one year, in the Navy for two years.¹¹

Clerics, seminarians, novitiates, and any individual with family members dependent upon him for support were exempted from military service. Deferment to finish high school or attend college was granted up to a maximum of ten years until the completion of studies (Article 17 of the Law of Military Service). Deferment could also be granted without a particular reason for a maximum of two years (Article 16 of the Law of Military Service). In all cases, the lottery numbers and cutoffs used to decide the outcomes for young men requesting deferment were those of their specific cohort.

Figure 1 shows the proportion of males serving in the Argentine military by birth cohort 1880 to 1975 during the period of mandatory military service. The gradual decline from the late 1950s to the abolition of conscription in 1995 reflects a combination of the modernization of the armed forces, the gradual resolution of boundary conflicts with neighboring countries, and the reduction in the power of the military after the return to democracy in 1983.

¹¹ For example, for cohort 1959, the Navy got the individuals drew 890 to 000, the Air Force got the individuals drew 812 to 889, and the Army got the individuals drew 320 to 811. All the people who drew 1 to 319 were not drafted.

III. Data

Exploiting the random assignment of eligibility into the Argentine military service, we aim to identify whether being conscripted affects individuals' later involvement in criminal activities. To answer this question we use two datasets on criminal participation provided by the Argentine Ministry of Justice.¹² One dataset contains information about all men who have any criminal record in the adult justice system during the period 1934–2005, about one million observations. An individual has a criminal record if he was ever prosecuted for or convicted of a crime. The dataset does not specify the type, number, or year of crimes involved, so it does not indicate whether an individual committed one or more offences. The dataset also contains the year of birth and the last three digits of the national ID number for every individual included.

Our unit of observation is the set of individuals in the same birth cohort who share the same last three digits of their national ID. For confidentiality reasons, complete ID numbers were not provided. Nevertheless, since the instrument exploited for identification varies only at the birth cohort - ID digits level, this is not a problem for our econometric analysis. For each birth cohort – ID digits combination, we calculate the crime rate as the number of individuals with criminal records divided by the total number of individuals (the population size of the birth cohorts was obtained from Census data).

The second database contains detailed information on all adult men who have gone through a criminal process in the adult civil justice system during the period 2000 to 2005 (about a quarter-million observations). For each individual the data include birth year, last three digits of ID, and the types of crime committed: use of weapons, crime against property, sexual attack, threat, murder, drug trafficking, and white collar crime.

¹² *Dirección Nacional de Reincidencia, Ministerio de Justicia de la Nación.*

This dataset does not indicate the number of specific offences that an individual committed, but it does indicate all the categories of crimes he committed.¹³

These data come from the civil justice system and do not include crimes committed before adulthood or during conscription, or failure to report for induction into the military. These are accounted for by the juvenile judicial system and the military justice system.¹⁴

In addition to crime rates, we will also analyze whether participation in military service affects labor market prospects. In particular, we consider the impact of conscription on participation in the formal labor market, unemployment rates, and earnings. Participation in the formal economy was obtained from the social security database which registers social security contributions for each individual, and includes the national ID and year of birth.¹⁵ For unemployment and income data we first identified the occupation declared by each individual in the 2003 national ballot registry. We then imputed for each occupation the associated employment status and average hourly earnings, using the official household survey of May 2003.¹⁶ Unemployment rates (calculated as the share of unemployed over the active population) and average hourly earnings (in Argentine pesos) were then calculated for each set of individuals with the same birth cohort and the same last three digits of national ID.

¹³ A limitation of this alternative database is that the type of crime is only specified for 37% of the cases. Eligibility into the military service, however, is not correlated with missing crime types in the database. All results mentioned but not shown are available from the authors upon request.

¹⁴ As our datasets register adult criminal processes of individuals since 18 years of age, for the period when conscripts were called to serve at the age of 21 the database could potentially include crimes committed by individuals before being drafted. This, nevertheless, poses no threat to our identification strategy since, by virtue of randomization, crimes committed between 18 and 21 years of age will be balanced between intention-to-treat groups. This cannot happen for cohorts serving at the age of 18 as the lottery was performed the year before incorporation. Regarding contamination of the crime data with failure to report for induction into the military, that is not possible for the second dataset which includes the type of crime.

¹⁵ *Sistema Integrado de Jubilaciones y Pensiones* (SIJP) as of October of 2004. Again, for confidentiality reasons the complete national ID number was not provided. We obtained the rate of participation in the formal economy for each cohort and last three ID digits.

¹⁶ *Encuesta Permanente de Hogares*, INDEC.

We obtained lottery draft results, military service status, and cutoff numbers from the Argentine Army.¹⁷ Using the lottery draft results and the cutoff numbers by cohort, we then defined the dummy variable *Draft Eligible* which equals one if the lottery number randomly assigned to cohort c and ID i was above the cutoff and therefore draft-eligible, and zero otherwise. This *Draft Eligible* variable identifies the intention-to-treat for the population; because of the lottery design it is randomly assigned. We also constructed the treatment variable *Conscription* as the number of men of cohort c and ID i who actually served in the military divided by the total number of men of cohort c and ID i .

Our analysis focuses initially on the cohorts of 1958 to 1962, for whom we have measures of both intention-to-treat and treatment status at the cohort-ID unit level. The average size of these cohorts is 236,330 men, so there are approximately 236 males in each cohort-ID combination. For these cohorts we also obtained data on a set of pre-treatment characteristics, including origin (Argentine-born indigenous, Argentine-born non-indigenous, and naturalized citizens) and province of residence. Data availability by cohort is summarized in Table 1.

We also have the data to construct the intention-to-treat variable *Draft Eligible* for the cohorts of 1929 to 1955 and 1963 to 1975.¹⁸ For the cohorts of 1955 and 1965, however, the cutoff numbers differed for the five Army corps *-cuerpos de ejército-* in the country, and the assignment to these Army corps was by place of residence. Our data do not allow us to associate each individual with a particular Army corp. Therefore, in order to avoid measurement errors when we include the 1955 and 1965 cohorts in our sample, we exclude all ID numbers whose lottery numbers lay between the maximum and the

¹⁷ *Oficina de Reclutamiento y Movilización, Estado Mayor del Ejército Argentino.*

¹⁸ For cohorts of 1931 to 1933, 1935 to 1936, 1938, and 1941 the cutoff number was equal to zero (i.e., the whole cohort was assigned to provide military service).

minimum cutoffs across the Army corps.¹⁹ This problem becomes more severe for the cohorts of 1966-1975, as the cutoff numbers then differed very substantially across the 29 military districts in the country, and the assignment to military district was also geographically determined. Since our data do not allow us to associate each individual with a specific military district, we exclude the cohorts of 1966-1975 from the main analysis. When we do report results for these cohorts, we exclude all ID numbers whose lottery numbers lay between the maximum and the minimum cutoffs.

Although eligibility to serve was randomly determined, it is useful to examine whether individuals' pre-treatment characteristics are balanced across the draft-eligible and the draft-exempted groups. For our available pre-treatment variables, there are no statistically significant differences between the two groups (see Table 2). This suggests that the randomization of draft eligibility allows us to ignore treatment assignment for post-treatment outcomes of interest.²⁰ In addition, the results of the paper do not change when we include all these pre-treatment characteristics as control variables in the regression function.

We can also compare the medical status of the draft-eligible group and the draft ineligible group by using information from the pre-induction physical and mental examinations for the cohorts of 1958 to 1962. Although in principle it is likely that lower class youths were over-represented in the group excluded for medical reasons, it was also the case that some middle and upper class youths used influence and misreported their medical conditions to avoid conscription. Thus, even though the medical examinations

¹⁹ For example, if for a given cohort the cutoff number was 460 in Army corp 1 and 480 in Army corp 2, we exclude from the analysis the ID numbers in those cohorts that were assigned lottery numbers between 460 and 480 as we are uncertain about their eligibility status.

²⁰ We also test the balancing of pre-treatment province of residence (the country is divided into 24 provinces), and in more than 90% of the tests there are no statistically significant differences in these variables across the draft-eligible and the draft-exempt groups (see on-line Appendix, which also presents summary statistics). Similar conclusions are obtained when we regress eligibility status on all the pre-treatment characteristics (the associated F-statistic is equal to 1.32).

occurred before treatment, the results may be contaminated by the strategic behavior of those seeking to avoid military service; hence they are not necessarily orthogonal to eligibility status. Moreover, although in principle all men had to undergo medical examinations, in some years and in some districts individuals with low lottery numbers were not medically examined.²¹

In a world without strategic behavior and in which all men received medical examinations, we would expect the proportion of individuals failing the medical examination to be the same for the draft-eligible and the draft-ineligible groups. As shown in the on-line Appendix, this is not the case. Failure rates are higher for the draft-eligible group in all five birth cohorts. If these differences in failure rates were due to differences in incentives to misreport faced by those with high or low draft numbers, we would expect the individuals with draft numbers close to the final cutoff number to have similar incentives, as the exact final cutoff number was unknown at the time of the medical examination. To explore this conjecture, we calculate failure rates by eligibility status for those individuals whose draft numbers were within twenty, fifteen, and ten numbers around the final cutoff number. The difference in failure rates between the draft-ineligible and the draft-eligible decreases around the cutoff. In fact, the difference fluctuates in sign and becomes not significant (it is never both in favor of the draft eligible and significant). That is, when we control for differences in incentives to misreport the medical examination between the draft-ineligible and the draft-eligible groups, failure rates are balanced between the two groups. A graphical inspection of the relationship between the draft lottery numbers and the failure rates for the five cohorts

²¹ The medical examination took place in the period between the draft lottery and the entry into military service.

shows that the failure rate increases up to the proximity to the actual cutoff, while it remains flat for the draft numbers above it (see on-line Appendix).²²

These results provide further evidence for the exogeneity of draft eligibility. On the other hand, they also suggest that the medical examinations were manipulated. Though this renders military service status endogenous, it does not affect the consistency of the IV estimator we use in the next section to identify the effect of military service on crime.

IV. Results

We are interested in estimating the causal effect of conscription on crime rates. Formally, we want to estimate the following equation:

$$CrimeRate_{ci} = \beta + \alpha Conscription_{ci} + \delta_c + \varepsilon_{ci} \quad (1)$$

$CrimeRate_{ci}$ is the average crime rate of individuals of birth cohort c and ID i (calculated as the number of men of cohort c and last three digits of ID i who have a criminal record, divided by the total number of men of cohort c and last three digits of ID i). $Conscription_{ci}$ is the number of men of cohort c and ID i who actually served in the military divided by the total number of men of cohort c and ID i . Finally, δ_c is a cohort effect, α is the average treatment effect, and ε_{ci} is an error term. We also introduce controls for the proportion of men from each origin and province for each cohort c and ID i .

To address the endogeneity of serving in the military in the crime equation, we estimate equation (1) by 2SLS, where the endogenous dummy variable $Conscription$ is instrumented by the exogenous dummy variable $Draft Eligible$.²³

²² Of course, individuals did not know the exact cutoff number that would apply to their cohort. However, they were likely to believe that it would be close to the previous year's cutoff. In three of the five cohorts considered, the cutoff differed from the previous year's cutoff by 20 units or less. The results are similar when we replicate this exercise using the previous year's cutoff.

First-stage estimates are reported in Table 3. The point estimate of the coefficient on *Draft Eligible* from the pooled sample indicates that the probability of serving in the military for men in the cohorts 1958 to 1962 was 66 percentage points higher for those in the draft-eligible group than for those in the draft-exempted group. All first-stage effects are very precisely estimated and significantly different from zero.²⁴

Unless we are willing to assume a constant treatment effect, the IV estimator does not recover average treatment effects. Under reasonable assumptions, however, it recovers an alternative parameter denoted Local Average Treatment Effect (LATE) by Angrist, Imbens, and Rubin (1996). The LATE parameter is the average effect of treatment on those individuals whose treatment status is induced to change by the instrument (i.e., by the dummy variable *Draft Eligible*). These individuals are draft-lottery compliers, in the sense that they served in the military because they were assigned a high lottery number, but would not have served otherwise. Thus, the results reported below need not generalize to the population of volunteers or to the population of young men who under no circumstances would have passed, legitimately or not, the pre-induction medical examination.

Main Results

Our estimates of the impact of serving in the military are reported in Table 4. We report estimates with and without controls. In all models our estimates indicate that

²³ Note that our parameter of interest is the effect of serving in the military on the likelihood of developing a criminal record in adulthood. Averaging our estimating equation across cohorts-three last digits of the ID number does not affect the identification of this parameter.

²⁴ Among the treated individuals, the proportion of compliers reaches 91.6%. This is because the proportion of volunteers in the population is very low. Using the draft-ineligible individuals, we estimate that the always-taker group is 4.2% of the total population, and constitutes 8.4% of the treated group in this sample. A large part of this group is the individuals pursuing a military career. Thus, the larger group of non-compliers are the never takers. As we discussed above, the most important cause for non-complying with conscription was failure of the pre-induction physical and mental examination for either real or faked medical conditions. Unfortunately, we do not have individual level information as to characterize the population of compliers in terms of observable characteristics (relative to the sample).

serving in the military service significantly increases crime rates. As a benchmark, we first report reduced-form estimates in columns (1) and (2), and service-status OLS results in columns (3) and (4). The preferred 2SLS estimates in column (6) indicate that military service significantly increases crime rates by 3.96%. Thus, our instrumental variable results suggest that conscription raises a complier adult man's lifetime probability of being prosecuted or incarcerated by 0.27 percentage points from a baseline lifetime rate of prosecution or conviction of around 6.8%, to 7.07%.²⁵

Thus we estimate that the probability that an individual develops a criminal record increases on average by about 4% as a result of serving in the military service. The magnitude of these results can be compared to the effect of other interventions estimated in the crime literature. For example, Lochner and Moretti (2004) find that an additional year of schooling reduces the probability of being incarcerated by roughly 0.1 percentage points for whites (from a baseline rate of 0.83 percent for high school dropouts) and 0.4 percentage points for blacks (from a baseline rate of 3.6 percent for high school dropouts). Hence, in percentage terms, a year of schooling in the U.S. decreases incarceration by about 11% to 12%. Thus, leaving aside the differences in time horizons, crime definitions, and target populations, the socially negative effects of one year of conscription seem to be about one third the magnitude of the socially positive effects of one year of additional schooling.

For the cohorts of 1929 to 1955 and 1963 to 1975 we do not have information on treatment status, but we still have information on draft eligibility and crime rate. We use these data to produce intention-to-treat estimates of the impact of conscription on crime.

²⁵ Our results remain unchanged when we control for the lottery number and this variable is not significant (see on-line Appendix). The results also remain unaffected when we control for the distance between the lottery number and the cutoff, and when we introduce these controls in the first stage. In these exercises we include a *Eligible for Navy* dummy (see next subsection) as an additional control to avoid the draft number capturing the effect of the two-year Navy conscription (served by those with the highest lottery numbers).

Given random assignment, we can estimate straightforwardly the intention-to-treat causal effect of military service on crime by estimating the following reduced-form regression:

$$CrimeRate_{ci} = \beta + \gamma DraftEligible_{ci} + \delta_c + \varepsilon_{ci} \quad (2)$$

where γ is the intention-to-treat effect and everything else is as in equation (1).²⁶

As shown in columns (7) to (9) of Table 4, we consistently find higher crime rates associated with ID numbers that were eligible for military service because of the lottery.²⁷ In column (7) we present the regression for the cohorts of 1929 to 1965, where we estimate that military service significantly increases the crime rates of draft-eligible individuals by 1.58%.²⁸ In columns (8) and (9) we separate the sample by the time when the age of starting military service changed from 21 to 18. The effect is larger in the later period, reaching a rate of 2.60%. It is smaller and not significant for the earlier period.

That the effect of conscription on crime is larger for the cohorts starting military service at age 18 could be the result of the military service being particularly harmful on individuals just entering the labor market. It is well documented in the literature that early experiences in the labor market, particularly unemployment, have long-lasting effects on individuals' labor market performance (Smith, 1985). For the cohorts enrolled at age 21, the effect of military service on criminal behavior channeled through their labor market experience could be less severe, especially since firms were required to keep their jobs open and to give them a license period to serve in the military. It is also possible that younger people are just more sensitive to this treatment. However, the

²⁶ In all the regressions, population size was obtained from Census data, assigning an equal number of individuals to each cohort-id combination (that is, the size of each cohort-id combination was calculated as the size of the cohort divided by 1,000). For the cohorts 1958 to 1962 we can estimate precisely the size of each cohort-id combination. Conclusions remain unchanged when we use this alternative source for these cohorts.

²⁷ As explained above, for the cohorts 1966 to 1975 the cutoff numbers differed across the 29 military districts. The results show no change if we still include in the regressions the cohort-ID combinations from these cohorts for which we are positive there is no measurement error on their eligibility status.

²⁸ Although of course the effect of eligibility on actual provision of military service could have varied over time, for ease of comparison of results across cohorts we transform ITT estimates into percentages using the compliance rates of cohorts 1958-62 for which that information is available.

differential impact cannot be attributed solely to the change in the age of enrollment, as several conditions, including secular increases in crime and data recording, could have changed for the cohorts of 1958 to 1965 relative to the cohorts of 1929 to 1955.

Even though our study relies on well-documented randomization, we try three additional experiments to test further the exogeneity of our instrument. First, we restrict the sample to observations receiving a low number in the lottery (i.e., not eligible). We sort the low numbers for each cohort and divide them by their median, assigning a false treatment status to the IDs above the median. As it should be if the lottery was truly random, we do not find differences between the crime rates of these groups. This is particularly relevant since, as explained above, the failure rate for medical examinations was increasing up to the proximity to the actual cutoff, while it remained flat for the draft lottery numbers above that. Thus, although the two non-eligible groups created by this procedure show different conscription-avoidance activity, there are no differences in their crime rates.

Second, we exploit the change in the draft age from 21 years to 18 years. This change meant that the cohorts 1956 and 1957 fully skipped military service (cohort 1955 was drafted in 1975 and served in 1976, and cohort 1958 was drafted in 1976 and served in 1977). The individuals from cohorts 1956 and 1957 were aware well in advance of their exemption, as the age change had been approved by Congress in 1973. We impute to these cohorts the draft lottery results corresponding to cohorts 1958 and 1959 which they would have obtained given their national ID numbers if the age requirement had not changed. Since these two cohorts were actually not drafted, we should not observe any significant crime differences between the two groups, and this is indeed what we find.

Third, we take advantage of the fact that the cohort of 1976 faced the draft lottery but eventually was not drafted.²⁹ We create a fake cutoff number for this cohort using the cutoff number for the cohort of 1975. When we compare crime rates for those with “high” and “low” numbers, we find no differences in crime rates between the two groups: the coefficient for the fake dummy for being draft-eligible has the opposite sign and it is not significant (the point estimate is -0.0012 with a standard error of 0.0009). This experiment also addresses the potential concern that the outcome of the lottery could have a direct effect on crime through mechanisms other than military service. Draft avoidance activity could have a direct effect, or the lottery result could directly depress the morale of young men. For the 1976 cohort, the instrument could directly affect crime rates through the “avoidance” or “depression” effects but not through the effect on actual military service. However, we find no differences in crime rates between those who were and were not at risk of being drafted.

In Table 5 we explore the differential effects of military service in peacetime versus in wartime. The two birth cohorts that participated in the Malvinas War were those of 1962 and 1963. Although only a small fraction of the draftees in these cohorts were exposed to actual combat (out of the 202,500 men incorporated from these cohorts, approximately 12,500 conscripts actually participated in the war and had some level of combat exposure), most of those drafted were mobilized to the conflict region of Patagonia and the South Atlantic. Results in columns (1) and (2) indicate that the effect of military service on crime is larger for draftees in the two cohorts serving during the Malvinas War. It is noteworthy that the effect is also significant for the cohorts that provided military service during peacetime, which comprise most of our sample.

²⁹ The lottery took place on May 27, 1994, but conscription was later abolished by Presidential Decree on August 31, 1994; validated by Congress on December 14, 1994.

We show in columns (3) and (4) of Table 5 that the effect of conscription on crime was larger for those who served in the Navy and thus did two years of service, rather than one year for the Army and the Air Force.^{30,31} This result is consistent with the notion that early differences in the labor market experience is a channel through which military service affects criminal behavior.³²

Complementary Results

Our main results suggest that conscription increases the likelihood of developing a criminal record during adulthood. One possible explanation is that military service delays young men's entry into the labor market and thereby affects their future opportunities. This argument is consistent with our finding that serving for two years in the Navy, rather than one year in the Army and Air Force, has additional deleterious effects.

To explore further the channels through which military service may affect criminal behavior, we use an alternative dataset that includes specific types of crime. The database used so far includes information on all criminal records since the mid 1930s. This second database has information on all men who have gone through the adult criminal justice system 2000 to 2005, by specific type of crime.

³⁰ Of course, serving in the Navy can be thought as a different treatment compared to serving in the Army or in the Air Force; for instance, young men serving in the Navy may have been exposed to a more violent environment since ports are usually places with high levels of criminal activity.

³¹ We estimate the regressions in columns (1) and (2) of Table 5 in reduced form as we do not have treatment status information to identify the individuals who actually participated in the Malvinas War out of the men inducted from the 1962 cohort. Similarly, we estimate the regressions in columns (3) and (4) in reduced form as we know the Navy/Air Force/Army cutoff numbers, but we do not have actual treatment information by the armed force of incorporation. If we combine the intention-to-treat *Eligible during Malvinas War* and *Eligible for Navy* variables with the treatment variable *Conscription* in 2SLS regressions for the 1958-62 cohorts, the *Eligible during Malvinas War* and *Eligible for Navy* coefficients are positive, but not statistically significant, whereas *Conscription* remains highly significant.

³² We also explore the interaction of conscription and dictatorial (military) government. The effect of conscription on crime seems to have been homogeneous for draftees providing military service during democratic and dictatorial governments. The participation of conscripts in violations of human rights during the military dictatorship of 1976-83 was minimal, and there are no cases of conscripts being prosecuted for those types of crime.

In Table 6 we estimate the effect of military service on subsequent criminal behavior by type of crime. If participation in the military service negatively affects the labor market prospects of young men by delaying their entry into the labor market, it should particularly induce them to commit crimes which have a pecuniary purpose: white-collar crimes (fraud, scams, extortion, etc.) and crimes against property. Thus, rates for these categories of crimes should be higher for men serving in the military. Indeed we find in the regressions on crimes against property and white-collar crimes that the 2SLS coefficients associated with military service are positive and significant.

To further explore the labor market channel, in Table 7 we present results of the impact of conscription on participation in the formal job market,³³ unemployment, and earnings. Overall, our results suggest that men serving in the military have a lower probability of participating in the formal job market, a higher unemployment rate (though not significant), and lower future earnings.³⁴ The negative effect of military service on job market performance supports the hypothesis of the detrimental effect of military service on criminal behavior through the labor market.

Other investigators have also found a negative impact of military service on labor market outcomes. Angrist (1990) suggests that the private cost of conscription in terms of lost wages could be extremely high – as high as 15% of wages for white males in their mid-30s who served in the army for two years. Imbens and van der Klaauw (1995) find a somewhat lower effect in the Netherlands. They estimate the effect of compulsory military service – serving in the army for just one year - is about 5% lower earnings per

³³ Formal jobs are on average better in both pecuniary and non-pecuniary dimensions. During the period studied, between 50 and 70% of the labor force operated formally (Galiani and Weinschelbaum, 2010).

³⁴ As explained above, unemployment and income data are imputed from the occupations declared by individuals in the national ballot registry. As voting is mandatory in Argentina, every citizen who is living in the country is automatically registered. However, inmates with a firm sentence are excluded from the registry while imprisoned. Although the proportion of sentenced people at any point in time is very small, this could potentially introduce a downward bias in the absolute value of our estimates, as former conscripts are more likely to be sentenced and to have high unemployment/low earnings occupations.

year, about 10 years after completing service. Both studies present evidence suggesting that lost labor market experience is the causal mechanism for the relationship (see also Angrist, 1998).

The job market outcomes in our study are observed 19 to 26 years after military service, so we are estimating the long-term impact of military service on job market performance.³⁵ Our results showing a relatively low impact of conscription on labor market outcomes are in line with those of Angrist and Chen (2007), who measure the impact 28 to 30 years after serving in Vietnam. They also report a very small long-term impact of veteran status on job market outcomes.

V. Conclusions

A common proposal in public debates around the world on how to reduce youth's involvement in criminal activity is the use of conscription. In this study, we estimate the causal effect of serving in the military on subsequent criminal behavior. We exploit the random assignment of young men to military conscription in Argentina through a draft lottery to identify this causal effect. Our results suggest that, even though military conscription may prevent some crime by keeping young men off the streets and potentially improving their subsequent inclusion into society, mechanisms operating in the opposite direction are such that the overall impact of serving in the military is to increase the likelihood of having a subsequent criminal record. The effect is stronger for the birth cohorts who participated in the Malvinas War, but we also show a deleterious effect of peacetime conscription on subsequent criminal participation. The effect is small, but precisely estimated.

³⁵ The labor market results are not statistically significant for the 1929-55 cohorts, for which even more time had elapsed and for which we had not separately found significant crime effects.

One channel through which military service could operate is by delaying conscripts' entry into the labor market. We find that military service has detrimental effects on future job market performance, and that the effects are stronger for crimes against property and white collar crimes and for the individuals who served longer. These ancillary results are consistent with this hypothesis.

We conclude that appeals for the use of military conscription to reduce criminal behavior are not warranted. Further research would be necessary to gauge the generalization of our findings to other countries.

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Table 1. Availability of data by birth cohort

| <i>Variable</i> | <i>Cohorts with data available</i> |
|--|------------------------------------|
| Crime rates | 1929 to 1976 |
| Draft number | 1929 to 1955; 1958 to 1976 |
| Cutoff number | 1929 to 1955; 1958 to 1975 |
| Actual conscription status | 1958 to 1962 |
| Pre-treatment characteristics | 1958 to 1962 |
| Participation in the formal job market | 1939 to 1976 |
| Unemployment rate | 1929 to 1976 |
| Earnings | 1929 to 1976 |

Note: Cohorts 1956 and 1957 were not called for military service because of change in the age of incorporation. The cohort of 1976 faced the draft lottery but was never drafted. For cohorts of 1955 and 1965, the cutoff numbers differed across the five army corps. For cohorts of 1966-1975, the cutoff numbers differed across the 29 military districts.

Table 2. Differences in pre-treatment characteristics by birth cohort and eligibility group

| <i>Differences by Cohort (Draft Exempt - Draft Eligible)</i> | <i>Cohort 1958</i> | <i>Cohort 1959</i> | <i>Cohort 1960</i> | <i>Cohort 1961</i> | <i>Cohort 1962</i> |
|--|------------------------|------------------------|------------------------|------------------------|------------------------|
| Born in Argentina, not indigenous | 0.0000 (0.0002) | 0.0000 (0.0002) | -0.0002 (0.0001) | 0.0000 (0.0002) | 0.0001 (0.0002) |
| Born in Argentina, indigenous | 0.0000 (0.0002) | 0.0000 (0.0001) | 0.0001 (0.0001) | 0.0000 (0.0001) | 0.0001 (0.0001) |
| Born outside Argentina, naturalized citizen | 0.0001 (0.0001) | 0.0000 (0.0001) | 0.0001 (0.0001) | 0.0001 (0.0001) | -0.0001 (0.0002) |

Note: Standard errors are in parentheses. The level of observation is the cohort-ID number combination.

Table 3. First stage by birth cohort

| Cohort | <i>Dependent Variable: Conscription</i> | | | | | |
|----------------|---|-----------------------|-----------------------|-----------------------|-----------------------|-----------------------|
| | <i>1958-62 (1)</i> | <i>1958 (2)</i> | <i>1959 (3)</i> | <i>1960 (4)</i> | <i>1961 (5)</i> | <i>1962 (6)</i> |
| Draft Eligible | 0.6587*** (0.0012) | 0.6279*** (0.0033) | 0.6210*** (0.0027) | 0.6505*** (0.0018) | 0.6972*** (0.0017) | 0.6853*** (0.0019) |
| Constant | 0.0421*** (0.0008) | 0.0578*** (0.0030) | 0.0389*** (0.0008) | 0.0377*** (0.0008) | 0.0556*** (0.0011) | 0.0343*** (0.0007) |
| Observations | 5,000 | 1,000 | 1,000 | 1,000 | 1,000 | 1,000 |
| Method | OLS | OLS | OLS | OLS | OLS | OLS |

Notes: Robust standard errors are shown in parentheses. The level of observation is the cohort-ID number combination. Column (1) includes cohort dummies. ***Significant at the 1% level.

Table 4. Estimated impact of conscription on crime rates

| Cohort | Dependent Variable: Crime Rate | | | | | | | | |
|----------------|--------------------------------|------------------------|------------------------|------------------------|------------------------|------------------------|-----------------------|---------------------|------------------------|
| | 1958-62 (1) | 1958-62 (2) | 1958-62 (3) | 1958-62 (4) | 1958-62 (5) | 1958-62 (6) | 1929-65 (7) | 1929-55 (8) | 1958-65 (9) |
| Draft Eligible | 0.0018*** (0.00006) | 0.0018*** (0.00006) | | | | | 0.0006** (0.00003) | 0.0003 (0.00004) | 0.0012*** (0.00004) |
| Conscription | | | 0.0023*** (0.00008) | 0.0023*** (0.00008) | 0.0027*** (0.00008) | 0.0027*** (0.00008) | | | |
| % Change | 3.92 | 3.96 | 3.32 | 3.32 | 3.92 | 3.96 | 1.58 | 0.69 | 2.60 |
| Controls | No | Yes | No | Yes | No | Yes | No | No | No |
| Observations | 5,000 | 5,000 | 5,000 | 5,000 | 5,000 | 5,000 | 34,904 | 26,976 | 7,928 |
| Method | OLS | OLS | OLS | OLS | 2SLS | 2SLS | OLS | OLS | OLS |

Notes: Robust standard errors are shown in parentheses. The level of observation is the cohort-ID number combination. All models include cohort dummies. The models in columns (2), (4), and (6) include controls for origin (naturalized or indigenous) and district (the country is divided in 24 districts). In 2SLS models the instrument for *Conscription* is *Draft Eligible*. % Change for 2SLS models is calculated as 100*Estimate/mean crime rate of draft-ineligible men. For intention-to-treat models, % Change is reported as 100*Wald estimate/mean crime rate of draft-ineligible men, where the Wald estimate is calculated as ITT estimate/($p_1 - p_2$), where p_1 is the probability of serving in the military among those that are draft-eligible, and p_2 is the probability of serving in the military among those that are not draft-eligible (since we do not have information on compliance rates outside the cohorts of 1958 to 1962, in all cases we use the compliance rates for this period). **Significant at the 5% level. ***Significant at the 1% level.

**Table 5. Estimated impact of conscription on crime rates
for peacetime vs. wartime service and 1-year vs. 2-year service**

| Cohort | <i>Dependent Variable: Crime Rate</i> | | | |
|---------------------------------|---------------------------------------|------------------------|------------------------|------------------------|
| | <i>1929-65 (1)</i> | <i>1958-65 (2)</i> | <i>1929-65 (3)</i> | <i>1958-65 (4)</i> |
| Draft Eligible | 0.00047* (0.0003) | 0.0009* (0.0005) | 0.0005** (0.0003) | 0.0010** (0.0004) |
| Eligible during Malvinas War | 0.0015* (0.0009) | 0.0011 (0.0010) | | |
| Eligible for Navy (2 years) | | | 0.0007** (0.0003) | 0.0011* (0.0006) |
| Observations | 34,904 | 7,928 | 34,904 | 7,928 |
| Method | OLS | OLS | OLS | OLS |

Notes: Robust standard errors are shown in parentheses. The level of observation is the cohort-ID number combination. *Eligible during Malvinas War* is a dummy that takes the value of one for the draft eligible from cohorts of 1962 and 1963. *Eligible for Navy* is a dummy variable that takes the value of one for those ID numbers eligible to serve in the Navy. All models include cohort dummies. *Significant at the 10% level; **Significant at the 5% level.

Table 6. Estimated impact of conscription on crime rates, by type of crime

| | <i>Use of Weapons</i> | <i>Crime Against Property</i> | <i>Sexual Attack</i> | <i>Murder</i> | <i>Threat</i> | <i>Drug Trafficking</i> | <i>White Collar Crime</i> |
|---------------|-----------------------|-------------------------------|----------------------|-----------------------|----------------------|-------------------------|---------------------------|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| <i>Cohort</i> | 1958-62 | 1958-62 | 1958-62 | 1958-62 | 1958-62 | 1958-62 | 1958-62 |
| Conscription | 0.00013 (0.00011) | 0.00082** (0.00034) | 0.00013 (0.00009) | -0.00007 (0.00010) | 0.00022 (0.00014) | -0.00009 (0.00014) | 0.00064*** (0.00021) |
| % Change | 0.20 | 1.21 | 0.20 | -0.11 | 0.32 | -0.13 | 0.94 |
| Observations | 5,000 | 5,000 | 5,000 | 5,000 | 5,000 | 5,000 | 5,000 |
| Method | 2SLS | 2SLS | 2SLS | 2SLS | 2SLS | 2SLS | 2SLS |

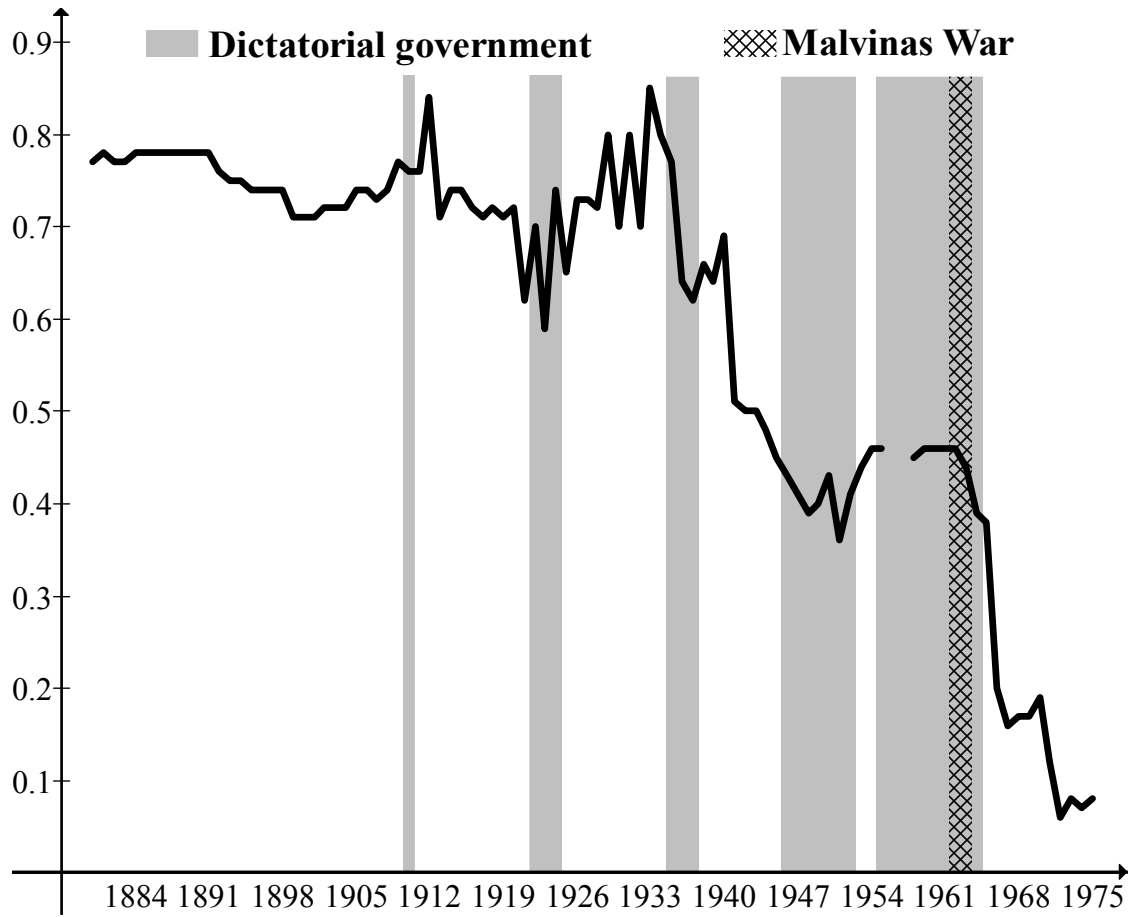
Notes: Robust standard errors are shown in parentheses. The level of observation is the cohort-ID number combination. All models include cohort dummies. The instrument for *Conscription* is *Draft Eligible*. % Change is calculated as 100*Estimate/mean dependent variable of draft-ineligible men. **Significant at the 5% level, ***Significant at the 1% level.

Table 7. Estimated impact of conscription on labor market outcomes

| <i>Cohort</i> | <i>Participation in the Formal Job Market</i> | | |
|----------------|---|-----------------------|------------------------|
| | <i>1958-62</i> (1) | <i>1958-62</i> (2) | <i>1958-65</i> (3) |
| Draft Eligible | -0.0015 (0.0014) | | -0.0018* (0.0010) |
| Conscription | | -0.0022 (0.0022) | |
| % Change | -0.65 | -0.65 | -0.80 |
| | <i>Unemployment Rate</i> | | |
| | (4) | (5) | (6) |
| Draft Eligible | 0.0005 (0.0006) | | 0.0004 (0.0005) |
| Conscription | | 0.0008 (0.0009) | |
| % Change | 0.41 | 0.41 | 0.29 |
| | <i>Earnings</i> | | |
| | (7) | (8) | (9) |
| Draft Eligible | -0.0111 (0.0070) | | -0.0176*** (0.0055) |
| Conscription | | -0.0169 (0.0106) | |
| % Change | -0.53 | -0.53 | -0.84 |
| Observations | 5,000 | 5,000 | 7,928 |
| Method | OLS | 2SLS | OLS |

Notes: Robust standard errors are shown in parentheses. The level of observation is the cohort-ID number combination. Participation in the formal job market is as of 2004. Unemployment rates and earnings are as of 2003. Earnings are hourly earnings in Argentine pesos. All models include cohort dummies. In 2SLS models the instrument for *Conscription* is *Draft Eligible*. % Change for 2SLS models is calculated as 100*Estimate/mean dependent variable of draft-ineligible men. For intention-to-treat models, % Change is reported as 100*Wald estimate/mean dependent variable of draft-ineligible men, where the Wald estimate is calculated as ITT estimate/($p_1 - p_2$), where p_1 is the probability of serving in the military among those that are draft-eligible, and p_2 is the probability of serving in the military among those that are not draft-eligible (since we do not have information on compliance rates outside the cohorts of 1958 to 1962, in all cases we use the compliance rates for this period). *Significant at the 10% level; ***Significant at the 1% level.

Figure 1. Proportion of men serving in the Argentine military, by birth cohort



Note: Cohorts 1956 and 1957 were not called to military service.