Conscription and Crime:
Evidence from the Argentine Draft Lottery

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Abstract

We estimate the causal effect of mandatory participation in the military service on the involvement in criminal activities. We exploit the random assignment of young men to military service in Argentina through a draft lottery to identify this causal effect. Using a unique set of administrative data that includes draft eligibility, participation in the military service, and criminal records, we find that participation in the military service increases the likelihood of developing a criminal record in adulthood. The effects are not only significant for the cohorts that performed military service during war times, but also for those that provided service at peace times. We also find that military service has detrimental effects on future performance in the labor market.

JEL Classification: K42

Keywords: Military service, violent behavior, crime.

I. Introduction

The initiation in criminal activities is, typically, a young men phenomenon. Most criminals begin their participation in illegal activities as juvenile or young adult offenders (Freeman, 1996).[1] Thus, the study of the determinants of entry into criminal activities should pay particular attention to major events affecting young males.[2] In many countries one of these important events is the mandatory participation in military service.[3] Because mandatory military service, also called conscription, typically occurs before other life-shaping events (such as parenthood, marriage, and participation in the labor market), it maximizes the possibility of redirection in the behavior of young men (Elder, Modell, and Parke, 1993).

Given the extended practice of conscription around the globe, its potentiality of shaping young men's behavior, and generalized concerns about crime in several countries, it is surprising that there is no empirical evidence on the impact of conscription on young men's propensities toward violent and criminal behavior. Our main contribution to the literature is to estimate the causal effect of peace-time military conscription on crime.

A priori, different hypotheses could predict a positive or negative effect of conscription on the involvement into criminal behavior. Military conscription may have a positive influence on young men's criminal prospects through a variety of channels. First, military training teaches young men obedience and discipline, which could directly affect their rates of criminality. Second, by improving health and

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[1] Young people and males are much more likely than aged people and females to commit crime (Archer and Gartner, 1984; Clinard and Abbott, 1973; Hirschi and Gottfredson, 1983). 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).

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

nutrition and by extending the social networks of the most deprived to other socioeconomic groups, military service might improve labor market prospects, preventing young men from committing property crimes. Third, military service incapacitates the commission of crime by keeping young men in military facilities and out of the streets at a crucial age.

Alternatively, military conscription could have a detrimental influence on young men's criminal behavior. First, by delaying the insertion of young men into the labor market the conscription might worsen their future labor market opportunities, increasing the likelihood of committing property crimes.[4] Second, military service provides firearm training that reduces the entry costs into crime, potentially increasing the participation in arm-related crimes. Finally, the conscription may constitute a social environment prone to violent responses, negative peer effects, and gang formation.[5]

Thus, a priori it is not clear whether the overall impact of military service on crime rates is positive or negative, which underscores the need for empirical evidence. In order to identify the causal effect of conscription on crime we need to identify a variable that affects participation in the conscription but does not affect crime through any other mechanisms. To solve this problem, we take advantage of the conscription lottery in Argentina, which randomly assigned eligibility of 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 servicing in the conscription on the likelihood of later developing a criminal record.

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[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 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).

Using a unique set of administrative data that includes draft eligibility, participation in the military service, and criminal records for all male cohorts born between 1958 and 1962, complemented with data on draft eligibility and criminal records for the male cohorts born between 1929 and 1975, we find that participation in the conscription increases the likelihood of developing a criminal record in adulthood, particularly for pecuniary (property and white collar) crimes. We also find that the conscription has detrimental effects on future job market performance.

Previous studies exploit the natural experiment generated by the Vietnam draft lottery to analyze the impact of servicing in the military during war times on a number of 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, Chen, and Frandsen, 2009; Dobkin and Shabini, 2009), and mortality (Hearst, Newman, and Hulley, 1986; Conley and Heerwig, 2009).

In particular, previous studies have analyzed the relationship between being a war veteran and posterior criminal behavior (see Yager, Laufer, and Gallops, 1984; Beckerman and Fontana, 1989; Bouffard, 2003; Rohlfs, 2006; Mumola, 2007; Noonan and Mumola, 2007). In general, the evidence is that combat exposure is associated with an increase in the number of arrests and incarceration rates, though the effect is small.

We differentiate from this previous literature by focusing on the crime effects of subjects that were drafted for military conscription in peace times. Subjects exposed to combat are likely to suffer from post-traumatic disorders.[6] Medical studies

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[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

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). Instead, individuals serving conscription in peace time are, in principle, not exposed to the kind of traumatic events that causes these stress disorders and, therefore, the impact of serving the conscription in peace time is likely to be different from the impact of combat exposure.

Indeed, since our database includes two cohorts that were drafted during the 1982 Malvinas War between Argentina and the United Kingdom, we are able to identify the difference between being drafted into the military in peace and war times. As expected, our results suggest that the effect of conscription on criminal behavior is larger for those draftees in the cohorts that participated in the Malvinas War. The crime effects, however, are also significant for the cohorts that performed military service during peace times.

Our findings have a broader policy scope than the existing literature on criminal and violent behavior of war veterans. Conscription, as a public policy, is a much more common phenomenon than armed conflict (for most countries, an unwanted and rare event). Out of the 179 countries from which we were able to find conscription information (covering 99.8% of the world population), 94 countries have military service. Out of these 94 countries, 19 currently have an armed conflict of some type.[7] Thus, about half of the countries of the world have mandatory military service without being involved in any armed conflict. Our results suggest that higher criminality rates should be counted as an additional cost of conscription.

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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] See the War Resisters' International webpage at http://www.wri-irg.org/wri.htm (accessed 2007). Out of the 85 countries without conscription, eleven are involved in an armed conflict. See also WRI (1998).

Instead, some countries have been recently discussing the re-implementation of conscription to address youth's conflicts. For example, as a response to the high levels of criminality in South Africa, Labor Minister Membathisi Mdladlana proposed that army conscription could help end violent crime. In the same vein, President Jacques Chirac announced, as a response to the violent crisis in the Paris suburbs in 2005, the creation of a voluntary civil service aimed at youngsters "who failed school and are in the process of social marginalization." Also in Argentina, where conscription was interrupted in 1995, its reimplementation has been proposed in Congress to address the current crime wave. Similarly, there have been recent discussions in Peru on the potential benefits of reimplementing conscription to reduce violence among the youth.[8] Our results do not encourage the introduction of conscription for anti-crime or socialization purposes.

The organization of the paper is as follows. Section II describes the natural experiment and the main characteristics of the military service in Argentina. Section III presents the data, and Section IV reports the econometric methods and results. Section V concludes.

II. The natural experiment

From 1901 through 1995, military service in Argentina was mandatory. The period of service lasted for a minimum of one year and a maximum of two years. The military service consisted of a basic instruction period of three months in which recruits learned military norms and were exposed to combat training. After that,

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[8] 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; Clarin.com, "El papel de las fuerzas armadas: la responsabilidad social del estado", 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.

conscripts were allocated to a military unit to perform a specific duty, not necessarily related to military training.[9]

Young males were initially called to serve at the age of 21, and later at age 18. The last cohort serving at the age of 21 was the cohort born in 1955, whereas the first cohort serving at the age of 18 was the cohort born in 1958. Due to the age change, cohorts born in 1956 and 1957 were not called to serve in the military service. The cohort of 1976 faced the conscription draft lottery but it was not incorporated. Recruits from cohorts 1962 and 1963 participated in the Malvinas War.

Eligibility of young males to military service was randomly assigned. Each year a lottery, whose results were broadcasted by radio and published by the main newspapers, assigned a number between 1 and 1,000 to each of the last three numbers of the national IDs of the individuals of the cohort to be incorporated the following year.[10]

After the lottery, individuals were called to a physical and mental examination. Later on, a cut-off number was announced and those "candidates" whose ID number corresponded to a lottery number above the cut-off and who had approved the medical examination were called to serve in the military service. Among those lottery numbers eligible for conscription, the lowest numbers were assigned to the Army, the intermediate numbers to the Air Force, and the highest numbers to the Navy. Conscription in the Navy lasted for two years, whereas it lasted for one year in the Army and the Air Force.

Clerics, seminarists, novitiates, and any men having family members dependent upon him for support were exempted from service. Deferment to attend college or

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[9] On the military service in Argentina, see Rodríguez Molas (1983).
[10] The lottery system was run by the National Lottery in a public session using a lottery drum filled with 1,000 balls and supervised by the National General Notary. The first ball released from the lottery drum corresponded to ID number 000, the second released ball to ID number 001, and so on.

finishing high school 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 cut-offs used to decide incorporation of young men asking for deferment were those corresponding to their own birth cohorts.

Figure 1 displays the time-series of the proportion of men serving in the military by cohort for the period of mandatory military service in Argentina, corresponding to the cohorts of 1880 to 1975. The gradual decline from the late 1950s through the abolition in 1995 reflects a combination of the modernization of the armed forces, the gradual resolution of boundary conflicts with the neighboring countries, and the reduction in the power of the military since the definite return to democracy in 1983.

III. Data

Exploiting the random assignment of eligibility into the military service, we aim to identify whether serving in the conscription affects later involvement in criminal activities. To answer this question we use two datasets on criminal participation provided by the Justice Ministry.[11] One dataset has information about all men who have a criminal record in the adult justice system since 1934 (about one million observations) to 2005. An individual has a criminal record if he was ever prosecuted or convicted of a crime. The dataset also includes the last three digits of the national ID number and the year of birth, but does not specify the type, number, or year of the crimes involved. Thus, the dataset does not distinguish if an individual committed one or more offences.

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[11] *Dirección Nacional de Reincidencia, Ministerio de Justicia de la Nación.*

Our unit of observation is the combination of the cohort of birth and the last three numbers of the ID. The complete ID number was not provided for confidentiality reasons. Nevertheless, since the instrument exploited for identification only varies at the ID number-cohort level, this is not a nuisance for our econometric analysis. For each cohort-last three digits of national ID combination we calculated the crime rate as the proportion of the number of individuals with criminal records to the total number of individuals in that cohort (the population size of the cohorts was obtained from Census data).

The other dataset covers a shorter period of time, but it includes information on the type of crime that originated the criminal record (use of arms, against property, sexual attack, threats, murder, drug trafficking, and white collar). This second database has detailed information on all adult men that have gone through a criminal process in the adult civil justice since 2000 to 2005 (about a quarter-million observations without specifying the year), and includes the last three ID digits, the year of birth, and the type of crime. In terms of this dataset, it is the same if the individual committed one or more offences of the same type of crime, however, he can appear more than once if he committed different type of crimes (in this case he contributes to the crime rate in each crime category).[12]

Our data come from the civil justice and do not include crimes committed before adulthood nor during conscription (or failure to report for induction into the military) as these are accounted for by the juvenile judicial system and the military justice.[13]

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[12] A limitation of this alternative database is that the type of crime is only specified for 37% of the cases. Military service status, however, is not correlated with missing crime types in the database.

[13] 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

Aside from crime rates we will also analyze whether participation in the military service affects labor market prospects. In particular, we consider the impact of conscription on participation in the formal job market, unemployment rates, and earnings. Participation in the formal economy was precisely obtained from the social security database which registers social security contributions for each individual, and includes the national ID and year of birth.[14] For unemployment and income data we first identified the occupation declared by each individual in the 2003 national ballot registry. As voting is mandatory in Argentina, every citizen who is living in the country is automatically registered. We then utilized the official household survey of May 2003 to input for each occupation the associated employment status and average hourly earnings (in Argentine pesos).[15] Unemployment rates (calculated as the share of unemployed over the active population) and average hourly earnings were then obtained for each cohort-last three digits of national ID combination.

We obtained lottery draft results, military service status, and cut-off numbers from the Argentine Army.[16] Our analysis focuses on the cohorts of 1958 to 1962 as we have for them information on both the intention-to-treat and the treatment status at the cohort-last three digits of national ID combination. Using the lottery draft results and the cut-off numbers by cohort we define the dummy variable *Draft Eligible*, which takes the value of one if the lottery number randomly assigned to cohort *c* and ID *i* was draft-eligible (above the cut-off), and zero otherwise. Thus, the *Draft Eligible* variable identifies the intention-to-treat on the population and, by design, it is randomly assigned. In addition, we construct the treatment variable *Served in the*

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with failure to report for induction into the military, that is not possible for the second dataset which includes the type of crime.

[14] Source: SIJP, *Sistema Integrado de Jubilaciones y Pensiones* 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.

[15] *Encuesta Permanente de Hogares*, INDEC.

[16] *Oficina de Reclutamiento y Movilización, Estado Mayor del Ejército Argentino*.

Conscription as the ratio of men of cohort c and ID i who actually served in the conscription divided by the population size of cohort c and ID i.

For cohorts 1958 to 1962, we also obtained data on a set of pre-treatment characteristics, such as origin (distinguishing naturalized and indigenous citizens) and district (the country is divided in 24 provinces). Summary statistics, using the ID-cohort combination as the unit of observation, are reported in Table 1. The time-series of crime rates by eligibility status for the cohorts of 1958 to 1962 presented in Figure 2 anticipates our main result by showing higher crime rates for the draft eligible group.

We also have lottery draft results and cut-off assignment numbers to construct the intention-to-treat variable *Draft Eligible* for the cohorts of 1929 to 1955 and 1963 to 1975.[17] For the cohorts of 1955 and 1965, however, the cut-off number was different by army corp (there were five army corps -*cuerpos de ejército*- in the country and the assignment to army corps was geographic by place of residence). Since our data do not allow the association of each individual to a particular army corp, in order to avoid measurement errors when we include these cohorts into our sample we exclude all ID numbers whose lottery numbers laid in between the maximum and the minimum cut-offs.[18] This problem becomes more severe for the cohorts of 1966 to 1975, as the cut-off number differed by military district (there were 29 military districts in the country and the assignment to military district was also geographic) and the cut-off differences are large. Since, again, our data do not allow the association of each individual to a particular military district, we exclude from the main analysis the cohorts of 1966 to 1975, and, when we report results for these

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[17] For cohorts of 1931 to 1933, 1935 to 1936, 1938, and 1941 the cut-off number was equal to zero (i.e., the whole cohort was assigned to provide military service).

[18] For example, if for a given cohort the cut-off 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.

cohorts, we exclude all ID numbers whose lottery numbers laid in between the maximum and the minimum cut-offs.

Although eligibility status was randomly assigned, it is useful to examine whether the individual's pre-treatment characteristics are balanced across the two groups. As shown in Table 2, for most of the pre-determined variables available there are no statistically significant differences between the draft-eligible and the draft-exempted groups, suggesting that the randomization of draft eligibility succeeded in making treatment assignment ignorable for any post-treatment outcome of interest.[19] For those variables where the difference is statistically significant, the differences are relatively small and, as shown in the results section, the main results in the paper do not change when we include all these pre-treatment characteristics as control variables in the regression function.

We can also contrast the medical status of the draft eligible group and the draft ineligible group by taking advantage of information available for the cohorts of 1958 to 1962 on the output of the pre-induction physical and mental examination. 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 middle and upper class youths used influences and false records to avoid conscription by misreporting their medical conditions. Thus, even though the results from the medical examination occurred before treatment, they are contaminated by strategic behavior from those willing to avoid incorporation, and hence, they are not necessarily orthogonal to eligibility status. Moreover, although in principle all men had to go through the

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[19] Similar conclusions are obtained when we regress eligibility status on all the pre-treatment characteristics (the associated F-statistic is equal to 1.32).

medical examination, in some years and districts individuals with evidently low numbers were not called to take it.[20]

In a world without strategic behavior (and where all men were called for physical and mental revision), we would expect the proportion of individuals failing the medical examination to be balanced between the draft ineligible and the draft eligible groups. As shown in the first row in Table 3, this is not the case: failure rates are significantly higher for the draft eligible group in all five 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 those individuals with draft numbers close to the final cut-off number to have similar incentives; after all, the exact final cut-off number was unknown at the time of the medical examination. To explore this conjecture we calculate failure rates by eligibility status for those individuals with draft numbers within twenty, fifteen, and ten numbers around the final cut-off number. As reported in Table 3, the difference in failure rates between the draft ineligible and the draft eligible decreases around the cut-off (in fact, in many cases the sign of the difference changes), and in most cases becomes not significant (it is never both negative 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.[21] On the one hand, these results provide further evidence of the exogeneity of draft eligibility. On the other hand, they also provide evidence

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[20] The medical examination took place in the period between the draft lottery and the incorporation.

[21] Of course, individuals did not exactly know the cut-off number that would apply to his cohorts. However, they were likely to believe that the cut-off that would apply to them was going to be around the previous year cut-off. In three of the five cohorts considered, the cut-off was within the ± 20 numbers interval relative to the previous year. As a robustness check, when we replicate Table 3 using the previous year cut-off, the results do not change. What it is more, a graphical inspection for the five cohorts of the relationship between the draft lottery numbers and the failure rates shows that the latter is increasing up to the actual cut-off, while it clearly remains flat for the draft numbers above it. All results mentioned but not shown are available from the authors upon request.

suggesting 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 serving in the conscription on (cohort-ID) crime rates. Formally, we want to estimate the following equation:

$$\text{Crime Rate}_{ci} = \beta + \alpha \ \text{Served in the Conscription}_{ci} + \delta_c + \varepsilon_{ci} \quad (1)$$

where *Crime Rate*$_{ci}$ is the average crime rate of cohort c and ID i (calculated as the ratio of men of cohort c and last three digits of ID i who have a criminal record divided by the population size of cohort c and last three digits of ID i), 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 district for each cohort c and last three digits of ID i.

To address the endogeneity of serving in the conscription in the crime equation, we estimate equation (1) by Two Stage Least Squares (2SLS), where the endogenous dummy variable *Served in the Conscription* is instrumented by the exogenous dummy variable *Draft Eligible*.

Figure 3 plots the conditional probability of serving in the conscription given lottery numbers for these cohorts. The most important feature of this figure is the sharp increase in the probability of service at the cut-off points. First-stage estimates are reported in Table 4. 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 compared to those in the draft-exempted group. All first-stage effects are very precisely estimated and significantly different from zero.

Unless we are willing to assume a constant treatment effect, the IV estimator does not recover average treatment effects. Under sensible 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 conscription 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 that 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 5. We report estimates with and without controls. In all models our estimates indicate that serving in the military service significantly increases crime rates. As a benchmark, we first report reduced-form estimates in columns (1) and (2). The preferred 2SLS estimates in column (4) indicate that military service significantly increases crime rates by 3.96%. Thus, our instrumental variable results suggest that serving in the conscription raises a complier adult man's lifetime probability of being prosecuted or incarcerated by 0.27 percentage points up from a baseline lifetime rate of prosecution or conviction of around 6.8 percentage points. Hence, the estimates imply that

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conscription would raise average prosecution or conviction rate from 6.80% to 7.07%.[22]

Thus, we estimate that probability that an individual develops a criminal career 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 about one third of the socially positive effects of one year of additional schooling.

As explained above, although for the cohorts of 1929 to 1955 and 1963 to 1975 we do not have information on treatment status, we still have information on draft eligibility and crime rates. We use these data to produce intention-to-treat estimates of the impact of conscription on crime. 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:

$$\text{Crime Rate}_{ci} = \beta + \gamma \ \text{Draft Eligible}_{ci} + \delta_c + \varepsilon_{ci} \ (2)$$

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[22] In these 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 calculation for the size of the cohort.

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

As shown in columns (5) to (7) in Table 5, we consistently find higher crime rates on those ID numbers that were made eligible for military service by the lottery. In column (5) we present the regression for the cohorts of 1929 to 1965, where we estimate that military service significantly increases crime rates of draft-eligible individuals by 1.58%. In columns (6) and (7) we separate our sample by the time when military service changed the age of incorporation from 21 years to 18 years. The effect appears larger in the latter period reaching a rate of 2.60%, and it is smaller and not significant for the early period.[23]

This finding that the effect of serving in the conscription on crime is larger for those cohorts enrolled at age 18 could be the result of the military service being particularly harmful on individuals entering the labor market. As it is well documented in the literature, the early experiences in the labor market (particularly unemployment) have long lasting effects on individuals' labor market performance (Smith, 1985). Instead, for those cohorts enrolled at age 21, the effect of military service on crime channeled through the labor market could be less severe, especially since firms had to keep their jobs open and give them a license period to serve in the military service. It is also possible that younger people are just more sensitive to this treatment. However, the differential impact cannot be only attributed 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.

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

Even when our study relies on a well documented randomization, we still conduct three false experiments to further test the exogeneity of our instrument. First, we restrict the sample to those observations with 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 in the crime rates of these groups. This is particularly relevant since, as reported in footnote 21, we found that the medical failure rate was increasing up to the actual cut-off while it remains flat for the draft lottery numbers above it. Thus, although the two non-eligible groups created by this procedure show different failure rates in the medical examination, there are no differences in their crime rates.

Second, we restrict the sample to cohorts 1956 and 1957 (which fully skipped military service because of the change in the age of incorporation from 21 years to 18 years), imputing them the draft lottery results corresponding to cohorts 1958 and 1959, which they would have obtained under no age change. Since these cohorts were 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 conscription draft lottery but it was not incorporated. We create a faked cut-off number for this cohort based on the cut-offs numbers 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.[24]

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[24] The coefficient for the faked 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). The last false experiment also addresses the potential concern that the outcome of the lottery could have a direct effect on crime besides real conscription participation. For example, the lottery result could directly affect the morale of young men, depressing those who are made eligible by the lottery. In this case, the instrument would affect crime rates directly through the "depression" effect and not through its effect on serving in the

In Table 6 we first explore differential effects of military service in peace and war times. Even though only a small fraction of the draftees in the two cohorts that participated in the Malvinas War were exposed to combat (from the 440,000 men in cohorts 1962 and 1963, approximately 12,500 conscripts participated in the war and had, therefore, some level of combat exposure) most of the incorporated conscripts were mobilized to Patagonia and the South Atlantic, the conflict region. Results in columns (1) and (2) indicate that the effect of military service on crime is larger for those draftees in the two cohorts that participated in the Malvinas War. It is noteworthy that the effect is also significant for the cohorts that provided military service during peace times, which comprise most of our sample.

We also show in columns (3) and (4) of Table 6 that the effect of conscription on crime was larger for those that did the military service in the Navy, which served for two years instead of the one year served in the Army and the Air Force.[25,26] This result is consistent with early experience in the labor market being a channel through which the military service affects criminal behavior.[27]

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conscription. As explained above, when using the lottery numbers for the cohort of 1976 which faced the conscription draft lottery but it was not incorporated, there are no differences in crime rates between those that were and were not at risk of incorporation.

[25] 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.

[26] We estimate the regressions in columns (1) and (2) of Table 6 in reduced form as we do not have treatment status information to identify the individuals that actually participated in the Malvinas War out of the men incorporated 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 cut-offs numbers, but we do not have actual treatment information by armed force of incorporation. If we combine the intention-to-treat *Malvinas War* and *Navy* variables with the treatment variable *Served in the Conscription* in 2SLS regressions for the 1958-62 cohorts, the *Malvinas War* and *Navy* coefficients are positive, but not statistically significant, whereas *Served in the Conscription* remains highly significant.

[27] 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 prosecuted for those types of crime. In addition, we explore possible heterogeneity in the effects using information on the available pre-treatment characteristics for the cohorts of 1958 to 1962 subject to the constraint of only having information at the cohort-ID, not individual data level. As explained above, for these cohorts we have information on the proportion of indigenous citizens for each cohort-ID combination. We also have pre-treatment district data that we use to construct the proportion of

<u>Complementary Results</u>

Our main results suggest that conscription increases the likelihood of developing a criminal record during adulthood. One potential explanation is that military service may have delayed the insertion of the young into the labor market affecting future opportunities. The latter interpretation is consistent with the additional deleterious effect observed for those that provided service in the Navy for two years.

To try to shed additional light on the channels through which military service could have affected criminal behavior, we use an alternative dataset that covers a shorter period of time, but includes the type of crime. Whereas the database we have used so far has information on all criminal records since the mid 1930s, the newer database has information on all men that have gone through the adult criminal justice system since 2000, but details the type of crime.

In Table 7, we estimate the effect of military service by type of crime. As discussed above, one hypothesis is that participation in the military service may negatively affect the labor market prospects of young men by delaying their insertion in the labor market, thus inducing them to commit pecuniary crimes. This hypothesis implies that property and white-collar crimes, which have a pecuniary purpose, should be lower for those men not serving in the military service. In agreement with this hypothesis, the 2SLS coefficients associated with military service provision are positive and significant in the regressions on property and white-collar crimes. The reduced form results for cohorts 1958-62 and 1958-65 in the second and third panels coincide with these findings.

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individuals between 25 and 39 years old with university studies and proportion of rural population for each cohort-id combination. When we interact these three pre-treatment variables and the treatment assignment, we find that the interaction effects are not significant.

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To further explore the labor market channel, in Table 8 we present results of the impact of conscription on participation in the formal job market, unemployment, and earnings. Overall, our results suggest that men serving in the military service have a lower probability of participating in the formal job market, a higher unemployment rate (though not significant), and lower future earnings. 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.

The result of a negative impact of military service on labor market outcomes is not novel. Previous work by 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 veterans in their mid-30s for servicing in the army for two years. Imbens and van der Klaauw (1995) find a somewhat lower effect of compulsory military service in the Netherlands. They estimate the cost of military service in terms of earnings in about 5% per year –for servicing in the army for only one year about 10 years after completing service. Both articles present evidence suggesting that the causal mechanism for this relationship is lost labor market experience (see also Angrist, 1998).

However, it is worth noting that, given that job market outcomes in our study correspond to 19 to 26 years after serving in the military, we are estimating a long-term impact of military service on job market performance. In this sense, our results showing a relatively low impact of conscription on labor market outcomes are in line with the ones presented in Angrist and Chen (2007), who measure the impact 28 to 30 years after serving in Vietnam and also report very low long-term impact of veteran status on job market outcomes.

V. Conclusions

We estimate the causal effect of the participation in the military service on crime. A priori, different hypotheses could predict a positive or negative effect of conscription on the involvement into criminal behavior. We exploit the random assignment of young men to conscription in Argentina through a draft lottery to identify this causal effect. Our results suggest that, even though military conscription incapacitates the commission of crime by keeping young men out of the streets and potentially improves their inclusion into society, there are mechanisms operating in the opposite direction in such a way that the overall impact of conscription is to increase the likelihood of developing a criminal record in adulthood. Although the effect is stronger for the cohorts that participated in the Malvinas War, our original contribution is to show a deleterious effect of peace-time conscription on future criminal participation. This effect is small, but precisely estimated.

Additional evidence suggests that a particular channel through which this effect could have operated is by delaying the conscripts' insertion in the labor market. Our findings that military service has detrimental effects on future job market performance, and the stronger crime effects for pecuniary (property and white collar) crimes and for the individuals that provided longer conscription service are consistent with this hypothesis.

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Table 1. Descriptive statistics for men born 1958-1962

	Mean	Standard Deviation	Observations
Draft Eligible	0.6998	0.4584	5,000
Served in the Conscription	0.5031	0.3049	5,000
Navy	0.1196	0.3245	5,000
Malvinas War	0.1362	0.3430	5,000
Crime Variables			
Crime Rate	0.0693	0.0178	5,000
Use of Arms	0.0010	0.0024	5,000
Against Property	0.0075	0.0073	5,000
Sexual Attack	0.0007	0.0021	5,000
Murder	0.0009	0.0021	5,000
Threat	0.0017	0.0031	5,000
Drug Trafficking	0.0012	0.0028	5,000
White Collar	0.0034	0.0046	5,000
Labor Market Variables			
Participation in the Formal Job Market	0.3387	0.0470	5,000
Unemployment Rate	0.1797	0.0543	5,000
Earnings	3.1734	0.2343	5,000
Pre-Treatment Characteristics			
Argentine Born (not indigenous)	0.9986	0.0026	5,000
Indigenous Argentine	0.0009	0.0020	5,000
Naturalized Argentine	0.0005	0.0017	5,000
Pre-Treatment Characteristics – District of Residence			
Buenos Aires	0.3448	0.0326	5,000
Ciudad de Buenos Aires	0.0855	0.0186	5,000
Catamarca	0.0096	0.0064	5,000
Chaco	0.0347	0.0114	5,000
Chubut	0.0095	0.0061	5,000
Córdoba	0.0869	0.0186	5,000
Corrientes	0.0321	0.0107	5,000
Entre Ríos	0.0388	0.0121	5,000
Formosa	0.0150	0.0080	5,000
Jujuy	0.0169	0.0083	5,000
La Pampa	0.0075	0.0054	5,000
La Rioja	0.0077	0.0054	5,000
Mendoza	0.0435	0.0125	5,000
Misiones	0.0277	0.0104	5,000
Neuquén	0.0087	0.0059	5,000
Río Negro	0.0130	0.0071	5,000
Salta	0.0274	0.0102	5,000
San Juan	0.0187	0.0087	5,000
San Luis	0.0086	0.0059	5,000
Santa Cruz	0.0034	0.0038	5,000
Santa Fé	0.0863	0.0173	5,000
Santiago del Estero	0.0289	0.0108	5,000
Tierra del Fuego	0.0008	0.0019	5,000
Tucumán	0.0406	0.0121	5,000

Note: The level of observation is the cohort-ID number combination. Earnings are hourly earnings in Argentine pesos. Participation in the formal job market as of 2004. Unemployment rates and earnings as of 2003.

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

Differences by Cohort (Draft Exempt - Draft Eligible)	Cohort 1958	Cohort 1959	Cohort 1960	Cohort 1961	Cohort 1962
Argentine Born	0.0000	0.0000	-0.0002	0.0000	0.0001
(not indigenous)	(0.0002)	(0.0002)	(0.0001)	(0.0002)	(0.0002)
Indigenous Argentine	0.0000	0.0000	0.0001	0.0000	0.0001
	(0.0002)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Naturalized Argentine	0.0001	0.0000	0.0001	0.0001	-0.0001
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0002)
Districts					
Buenos Aires	0.0042	0.0007	0.0021	-0.0018	-0.0013
	(0.0026)	(0.0023)	(0.0023)	(0.0019)	(0.0019)
Ciudad de Buenos Aires	0.0022	0.0004	-0.0038***	0.0013	0.0038***
	(0.0016)	(0.0013)	(0.0010)	(0.0011)	(0.0011)
Catamarca	-0.0007	-0.0007	0.0000	0.0004	-0.0001
	(0.0005)	(0.0005)	(0.0005)	(0.0004)	(0.0004)
Chaco	-0.0005	0.0010	-0.0004	0.0006	-0.0012
	(0.0009)	(0.0007)	(0.0008)	(0.0008)	(0.0008)
Chubut	0.0004	-0.0004	-0.0002	-0.0005	-0.0005
	(0.0005)	(0.0004)	(0.0004)	(0.0004)	(0.0004)
Córdoba	-0.0015	0.0025**	0.0038**	-0.0010	0.0011
	(0.0015)	(0.0012)	(0.0015)	(0.0012)	(0.0011)
Corrientes	-0.0017*	-0.0012*	0.0011	0.0005	-0.0005
	(0.0010)	(0.0007)	(0.0007)	(0.0007)	(0.0007)
Entre Ríos	-0.0008	-0.0008	-0.0003	0.0010	-0.0008
	(0.0010)	(0.0008)	(0.0008)	(0.0008)	(0.0007)
Formosa	-0.0004	0.0008	0.0004	-0.0004	-0.0004
	(0.0007)	(0.0005)	(0.0006)	(0.0005)	(0.0005)
Jujuy	-0.0007	-0.0002	-0.0009*	0.0008	-0.0017***
	(0.0007)	(0.0005)	(0.0005)	(0.0006)	(0.0005)
La Pampa	0.0006	0.0006	0.0003	-0.0007*	0.0000
	(0.0005)	(0.0004)	(0.0004)	(0.0004)	(0.0004)
La Rioja	0.0000	-0.0008**	-0.0004	0.0000	-0.0004
	(0.0004)	(0.0004)	(0.0004)	(0.0004)	(0.0003)
Mendoza	0.0003	-0.0004	-0.0003	-0.0004	0.0018**
	(0.0011)	(0.0009)	(0.0008)	(0.0009)	(0.0008)
Misiones	-0.0009	0.0002	-0.0006	-0.0001	-0.0005
	(0.0008)	(0.0007)	(0.0007)	(0.0007)	(0.0007)
Neuquén	0.0001	0.0003	-0.0003	0.0003	-0.0003
	(0.0005)	(0.0004)	(0.0004)	(0.0004)	(0.0004)
Río Negro	-0.0004	-0.0001	-0.0001	-0.0001	0.0005
	(0.0006)	(0.0005)	(0.0005)	(0.0004)	(0.0005)
Salta	0.0000	-0.0005	0.0011*	-0.0002	0.0002
	(0.0009)	(0.0007)	(0.0007)	(0.0007)	(0.0007)
San Juan	0.0006	0.0000	-0.0007	-0.0001	0.0006
	(0.0009)	(0.0006)	(0.0006)	(0.0005)	(0.0006)
San Luis	0.0001	-0.0004	-0.0001	0.0000	-0.0003
	(0.0005)	(0.0004)	(0.0004)	(0.0004)	(0.0004)
Santa Cruz	-0.0002	-0.0004*	0.0001	0.0000	0.0003

	(0.0003)	(0.0003)	(0.0002)	(0.0002)	(0.0003)
Santa Fé	-0.0011	0.0005	0.0002	0.0016	0.0006
	(0.0014)	(0.0012)	(0.0011)	(0.0011)	(0.0011)
Santiago del Estero	-0.0001	-0.0020***	-0.0005	-0.0003	-0.0004
	(0.0008)	(0.0007)	(0.0008)	(0.0007)	(0.0006)
Tierra del Fuego	-0.0001	-0.0001	0.0000	0.0000	-0.0001
	(0.0002)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Tucumán	0.0011	0.0011	-0.0007	-0.0008	-0.0010
	(0.0011)	(0.0009)	(0.0008)	(0.0007)	(0.0008)

Note: Standard errors are in parentheses. The level of observation is the cohort-ID number combination. *Significant at the 10% level; **Significant at the 5% level; ***Significant at the 1% level.

Table 3. Differences in failure rates in the medical examination by eligibility group and cohort

Differences by Cohort (Draft Exempt - Draft Eligible)	Cohort 1958	Cohort 1959	Cohort 1960	Cohort 1961	Cohort 1962
All numbers	-0.0017	-0.0016	-0.0143***	-0.0197***	-0.0232***
	(0.0014)	(0.0013)	(0.0013)	(0.0012)	(0.0012)
20 numbers around the final cut-off number	0.0027	-0.0043	0.0009	0.0141**	-0.0060
	(0.0050)	(0.0068)	(0.0056)	(0.0053)	(0.0059)
15 numbers around the final cut-off number	0.0038	-0.0060	0.0008	0.0129*	-0.0034
	(0.0056)	(0.0085)	(0.0070)	(0.0066)	(0.0070)
10 numbers around the final cut-off number	0.0077	-0.0044	-0.0043	0.0108	0.0017
	(0.0056)	(0.0116)	(0.0090)	(0.0075)	(0.0083)

Note: Standard errors are in parentheses. The level of observation is the cohort-ID number combination. *Significant at the 10% level; **Significant at the 5% level; ***Significant at the 1% level.

Table 4. First stage by cohort

Cohorts	_Dependent Variable: Served in the Conscription_					
	_1958-62_	_1958_	_1959_	_1960_	_1961_	_1962_
	_(1)_	_(2)_	_(3)_	_(4)_	_(5)_	_(6)_
Draft Eligible	0.6587***	0.6279***	0.6210***	0.6505***	0.6972***	0.6853***
	(0.0012)	(0.0033)	(0.0027)	(0.0018)	(0.0017)	(0.0019)
Constant	0.0421***	0.0578***	0.0389***	0.0377***	0.0556***	0.0343***
	(0.0008)	(0.0030)	(0.0008)	(0.0008)	(0.0011)	(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 5. Estimates of the impact of conscription on crime rates

Cohorts	Dependent Variable: Crime Rate						
	1958-62	1958-62	1958-62	1958-62	1929-65	1929-55	1958-65
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Draft Eligible	0.0018	0.0018			0.0006	0.0003	0.0012
	(0.0006)***	(0.0006)***			(0.0003)**	(0.0004)	(0.0004)***
Served in the Conscription			0.0026	0.0027			
			(0.0008)***	(0.0008)***			
% Change	3.75	3.96	3.75	3.96	1.58	0.69	2.60
Controls	No	Yes	No	Yes	No	No	No
Observations	5,000	5,000	5,000	5,000	34,904	26,976	7,928
Method	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) and (4) include controls for origin (naturalized or indigenous) and district (the country is divided in 24 districts). In 2SLS models the instrument for *Served in the 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 conscription among those that are draft-eligible, and p_2 is the probability of serving in the conscription 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 6. Peace vs. war times and one-year vs. two-years

Cohorts	Dependent Variable: Crime Rate			
	1929-65	*1958-65*	*1929-65*	*1958-65*
	(1)	*(2)*	*(3)*	*(4)*
Draft Eligible	0.00047	0.0009	0.0005	0.0010
	(0.0003)*	(0.0005)*	(0.0003)**	(0.0004)**
Malvinas War	0.0015	0.0011		
	(0.0009)*	(0.0010)		
Navy (2 years)			0.0007	0.0011
			(0.0003)**	(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. Cohorts 1956 and 1957 were not called for military service. All models include cohort dummies. *Significant at the 10% level; **Significant at the 5% level.

Table 7. Estimates of the impact of conscription on crime rates, by type of crime

|	Use of Arms	Against Property	Sexual Attack	Murder	Threat	Drug Trafficking	White Collar
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Cohorts	1958-62	1958-62	1958-62	1958-62	1958-62	1958-62	1958-62
Served in the Conscription	0.00013	0.00082	0.00013	-0.00007	0.00022	-0.00009	0.00064
	(0.00011)	(0.00034)**	(0.00009)	(0.00010)	(0.00014)	(0.00014)	(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
	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Cohorts	1958-62	1958-62	1958-62	1958-62	1958-62	1958-62	1958-62
Draft Eligible	0.00009	0.00054	0.00009	-0.00005	0.00015	-0.00006	0.00042
	(0.00007)	(0.00022)**	(0.00006)	(0.00007)	(0.00009)	(0.00009)	(0.00014)***
% 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	OLS	OLS	OLS	OLS	OLS	OLS	OLS
	(15)	(16)	(17)	(18)	(19)	(20)	(21)
Cohorts	1958-65	1958-65	1958-65	1958-65	1958-65	1958-65	1958-65
Draft Eligible	0.00006	0.00025	0.00002	-0.00003	0.00010	-0.00001	0.00021
	(0.00006)	(0.00018)	(0.00005)	(0.00005)	(0.00007)	(0.00007)	(0.00011)*
% Change	0.15	0.56	0.04	-0.07	0.24	-0.01	0.48
Observations	7,928	7,928	7,928	7,928	7,928	7,928	7,928
Method	OLS	OLS	OLS	OLS	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. In 2SLS models the instrument for *Served in the 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 conscription among those that are draft-eligible, and p_2 is the probability of serving in the conscription 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 5% level; ***Significant at the 1% level.

Table 8. Estimates of the impact of conscription on labor market outcomes

Cohorts	Participation in the Formal Job Market		
	1958-62	1958-62	1958-65
	(1)	(2)	(3)
Draft Eligible	-0.0015		-0.0018
	(0.0014)		(0.0010)*
Served in the Conscription		-0.0022	
		(0.0022)	
% Change	-0.65	-0.65	-0.80
	Unemployment Rate		
	(4)	(5)	(6)
Draft Eligible	0.0005		0.0004
	(0.0006)		(0.0005)
Served in the Conscription		0.0008	
		(0.0009)	
% Change	0.41	0.41	0.29
	Earnings		
	(7)	(8)	(9)
Draft Eligible	-0.0111		-0.0176
	(0.0070)		(0.0055)***
Served in the 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 as of 2004. Unemployment rates and earnings as of 2003. Earnings are hourly earnings in Argentine pesos. All models include cohort dummies. In 2SLS models the instrument for *Served in the 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 conscription among those that are draft-eligible, and p_2 is the probability of serving in the conscription 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 5% level; ***Significant at the 1% level.

Figure 1. Proportion of men serving in the conscription by cohort
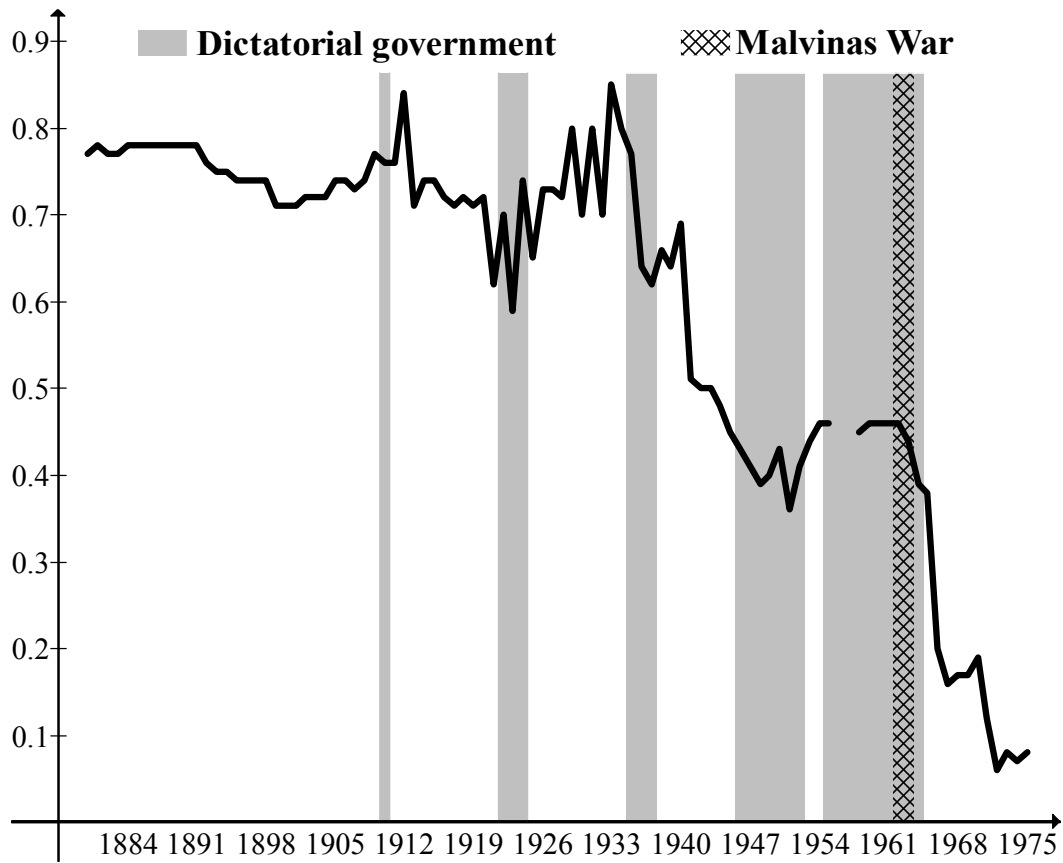
Note: Cohorts 1956 and 1957 were not called to military service.

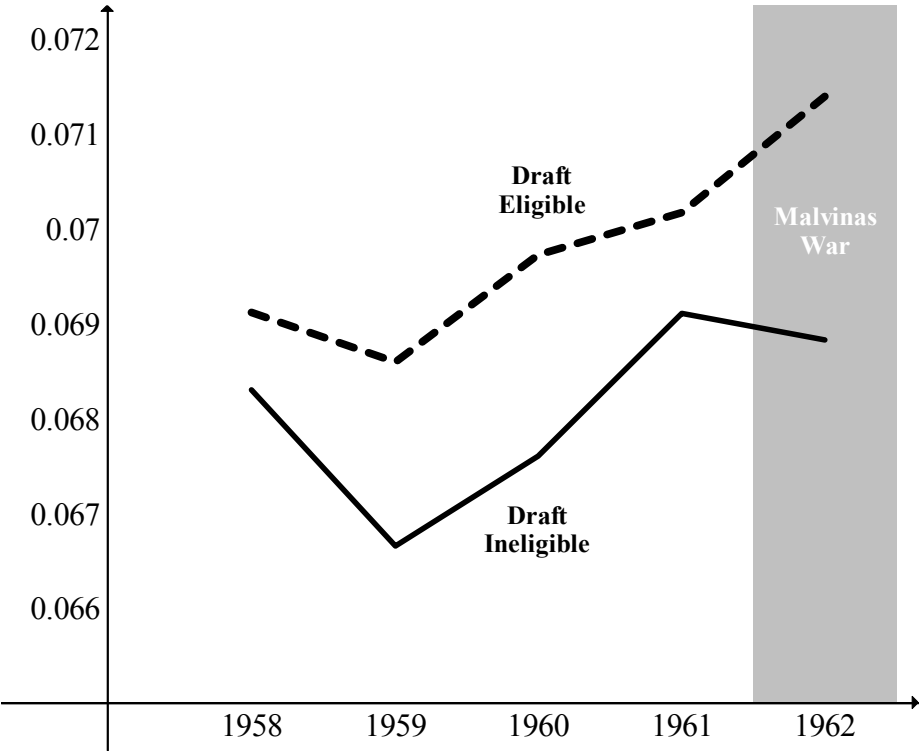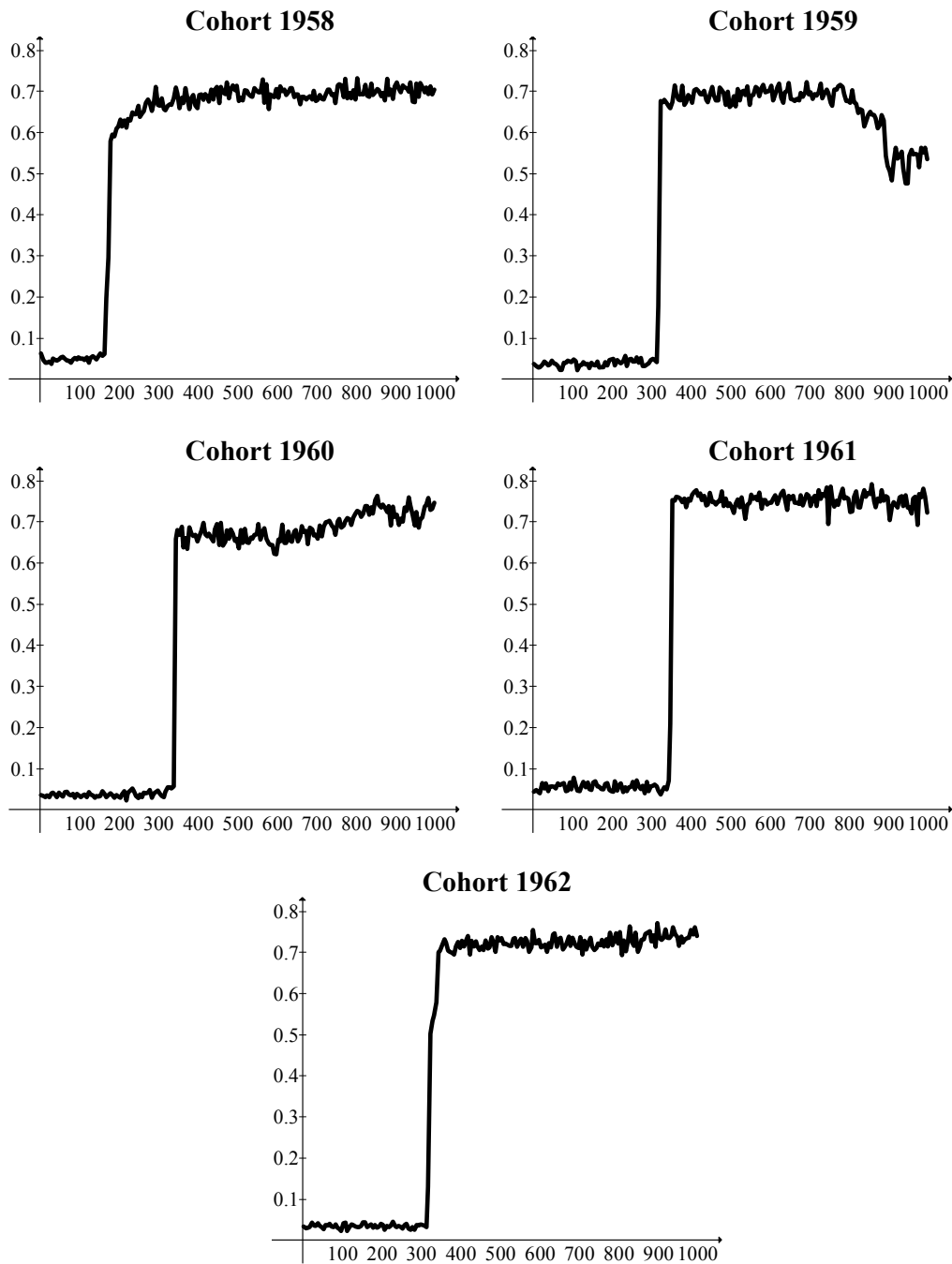**Figure 2. Crime rates by eligibility status for the cohorts 1958-62**

Figure 3. The relation between the conditional probability of serving in the conscription and draft lottery numbers for the cohorts of 1958 to 1962

Note: In order to smooth out fluctuations, we placed the 1,000 lottery numbers in 200 groups of five numbers (1 to 5 in the first one, 6 to 10 in the second one, and so on) and calculated the average within each of the groups.