

The Effects of Peacetime and Wartime Conscription on Criminal Activity

Sebastian Galiani
Washington University in St. Louis

Martín A. Rossi
Universidad de San Andrés

and

Ernesto Schargrodsky*
Universidad Torcuato Di Tella

May, 2009

Abstract

We estimate the causal effect of participation in the mandatory 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 for all Argentine males born between 1958 and 1962, 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.

* Sebastian Galiani, Department of Economics, Washington University in St. Louis, Campus Box 1208, St Louis, MO 63130-4899, US, galiani@economics.wustl.edu; Martín Rossi, Universidad de San Andrés, Vito Dumas 284, B1644BID, Victoria, Buenos Aires, Argentina, mrossi@udesa.edu.ar; Ernesto Schargrodsky, Universidad Torcuato Di Tella, Saenz Valiente 1010, C1428BIJ, Buenos Aires, Argentina, eschargr@utdt.edu. We thank Horacio Tarelli, Fernando Michelena, Rut Diamint, and the Argentine Army for their cooperation. We are also grateful to two anonymous referees for helpful comments and suggestions. Maximiliano Appendino, Florencia Borrescio Higa, David Lenis, Andrés Maggi, and Esteban Petruzzello provided excellent research assistance. We acknowledge financial support by the World Bank and the Weidemaum Center at Washington University in St. Louis.

I. Introduction

The initiation in criminal activities is, typically, a young men phenomenon.¹ Most criminals begin their participation in illegal activities as juvenile or young adult offenders (Freeman, 1996). 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 the 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

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, Pintoff, and Pozen (2007).

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, the military conscription may 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 their 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 impact of military service on crime rates is positive or negative, which underscores the need of empirical evidence. In order to identify a causal link between conscription and 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 developing a criminal record.

⁴ Milton Friedman and other economists stressed the job market costs imposed on draftees in their interventions opposing the draft in favour of a voluntary 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 the relationship between 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 Argentine males born between 1958 and 1962, we find that participation in the conscription increases the likelihood of developing a criminal record in adulthood, particularly for property and arm-related crimes. We also find that the conscription has detrimental effects on future job market performance and earnings.

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), and mortality (Hearst, Newman, and Hulley, 1986). In particular, previous studies have analyzed the impact of being a Vietnam War veteran on criminal and violent behavior (see Yager, Laufer, and Gallops, 1984; Bouffard, 2003; and Rohlfs, 2007).⁶

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.⁷ 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). Instead, individuals serving conscription in peace time are, in principle, not exposed to the kind of traumatic events that causes these stress

⁶ Yager, Laufer, and Gallops (1984) report positive correlations between combat exposure and arrests and convictions. Bouffard (2003) finds that military service reduces later offending in general. Rohlfs (2007) finds large effects of combat exposure on violent behavior.

⁷ 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.

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 (WRI, 1998).⁸ 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 crime rates should be counted as an additional cost of conscription.

Although mandatory participation in the military service in Argentina was interrupted in 1995, there have been recent discussions on the possibility of re-implementing conscription to address criminality problems among the youth.⁹ Some other countries have also 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

⁸ See the War Resisters' International webpage at <http://www.wri-irg.org/wri.htm>. Out of the 85 countries without conscription, eleven are involved in an armed conflict.

⁹ See the project presented by legislators Miguel Jobe y Antonio Rattín in 2002.

that army conscription could help end violent crime.¹⁰ 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”.¹¹ Our results do not encourage the introduction of conscription for anti-crime or socialization purposes.

The rest 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 our econometric methods. Section IV reports the results and Section V concludes.

II. 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, conscripts were allocated to a military unit to perform a specific duty, not necessarily related to military training.¹²

Young males initially were 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. 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.¹³

¹⁰ See News24.com, “Minister moots conscription,” January 30, 2007.

¹¹ See LeMonde.fr, “Jacques Chirac lance le service civil volontaire”, (“Jacques Chirac launches the Voluntary Civil Service”), November 17, 2005.

¹² On the military service in Argentina, see Rodríguez Molas (1983).

¹³ Recruits from the cohorts of 1962 and 1963 were incorporated, respectively, in March 1981 and March 1982. The war started in April 1982.

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 1000 to each of the last three numbers of the national IDs of the individuals of the cohort to be incorporated the following year.¹⁴ Later on, a cut-off number was announced and those “candidates” whose ID number corresponded to a lottery number above the cut-off were eligible to serve on 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.

Final selection of individuals for military conscription from the draft eligible was based on the pre-induction physical examination and on the examination for mental aptitude. Clerics, seminarists, novitiates, and any men having family members dependent upon him for support were exempted from service. Deferment to attend college or 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 be also 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 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.

III. Data and econometric methods

¹⁴ The lottery system was run by the National Lottery in a public session using a lottery drum filled with 1,000 balls. The first ball released from the lottery drum corresponded to ID number 000, the second ball released to ID number 001, and so on.

Exploiting the random assignment of eligibility into the military service, we will try to answer whether serving in the conscription incentives or disincentives later involvement in criminal activities. To answer this question we use two individual-level datasets on criminal participation provided by the Justice Ministry.¹⁵ One dataset has information about all men that have gone through the adult criminal justice system for being prosecuted or convicted of a crime since 1934 (about one million observations) and includes information on the last three ID numbers and the year of birth, but does not specify the type of crime involved.¹⁶

Our unit of observation is the combination of the cohort of birth and the last three numbers of the ID. 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). We consider an individual to have a criminal record if the individual was ever prosecuted or convicted. That is, in terms of our data, it is the same if the individual committed one or more offences and if he was convicted or not. Our data comes from the civil justice and do not include crimes committed during conscription (these are accounted for by the military justice).

The other dataset covers a shorter period of time, but it details 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 justice process since 2000 (about a quarter-million observations), and includes the last three ID numbers, the year of birth, and the type of crime.¹⁷

¹⁵ *Dirección Nacional de Reincidencia, Ministerio de Justicia de la Nación.*

¹⁶ The complete ID number was not provided for confidentiality reasons.

¹⁷ 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

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.¹⁸ 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).¹⁹ 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 and cut-off numbers from the Argentine Army.²⁰ We have information on cut-off assignment numbers for the cohorts of 1929 to 1975.²¹ For the cohorts of 1955 and 1965 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 is geographic), and for the cohorts of 1966 to 1975 the cut-off number differed by military district (there were 29 military districts in the country and the assignment to military district is also geographic). Since our data do not allow the association of each individual to a particular army corp or military district, in

committed by individuals before being drafted. This is not possible for cohorts serving at the age of 18 as the lottery was performed the year before incorporation.

¹⁸ Source: SIJP, *Sistema Integrado de Jubilaciones y Pensiones*. 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.

¹⁹ *Encuesta Permanente de Hogares*, INDEC.

²⁰ *Oficina de Reclutamiento y Movilización, Estado Mayor del Ejército Argentino*.

²¹ 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).

order to avoid measurement errors when we include these cohorts into our sample we exclude all ID numbers with lottery numbers in between the maximum and the minimum cut-offs.

Using the information on the cut-off numbers by cohort we define the dummy variable Draft Eligible, which varies by the last three numbers of the ID and the cohort of birth. Draft Eligible takes the value of one if the lottery number randomly assigned to ID i in cohort c was draft-eligible, and zero otherwise. Thus, the Draft Eligible variable identifies the intention-to-treat on the population and, by design, it is randomly assigned.

For the cohorts of 1958 to 1962 we have information on whether the individual was actually exposed to the treatment (i.e., served in the military). We construct the variable Served in the Conscription as the ratio of men of cohort c and ID i who served in the conscription divided by the population size of cohort c and ID i . For these cohorts we also obtained individual-level data on a set of pre-treatment characteristics, such as origin (native, naturalized, and indigenous) and district (the country is divided in 24 districts).

Summary statistics, using the ID-cohort 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 is shown in Figure 2.

Although eligibility status was randomly assigned, it is useful to examine whether, *ex post*, individual's pre-treatment characteristics are balanced across the two groups. As shown in Table 2, for most of the variables there are no statistically significant differences in observables across the draft-eligible and the draft-exempted groups, suggesting that the randomization was successful in ensuring orthogonality between covariates and treatment assignment. 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 substantially if we include all these pre-treatment characteristics as explanatory variables.²²

Medical examination

Although, in principle, it could appear that lower class youth are more likely to be over-represented in the group excluded for medical reasons, in practice, casual evidence suggest that this was not the case, mainly because middle and upper class youths could have used influences to avoid conscription by misreporting their medical conditions.

To explore the possibility that some men might have misreported their medical condition in order to avoid being incorporated we take advantage of information available for the cohorts of 1958 to 1962 on the output of the pre-induction physical and mental examination. This examination was taken by all men of the cohort in the period between the draft lottery and the incorporation; thus, even though the results from the medical examination are pre-treatment, they might had been contaminated by strategic behavior from those willing to avoid incorporation.

In a world without strategic behavior we would expect the proportion of individuals failing the medical examination to be balanced between the draft eligible and the draft ineligible 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

²² We also run two false experiments to guarantee that the lottery was truly random and that we are not capturing anything else in our estimates. First, the sample is restricted 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 to the upper half of numbers. As one would expect, we find no difference in crime rates between these groups, since none of them were draft-eligible. We then 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 the draft lottery results corresponding to cohorts 1958 and 1959. Again, since these cohorts were not drafted, we should not observe any significant crime differences between the two groups. This is indeed the case. All results mentioned but not shown are available from the authors upon request.

cohorts. If these differences in failure rates were due to differences in incentives 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 examination. To explore this conjecture we calculate failure rates by eligibility status for those individuals with draft numbers within twenty, fifteen, ten, and five numbers around the final cut-off number. As reported in Table 3, the difference in failure rates between the draft eligible and the draft ineligible decreases (in fact, in some cases the sign of the difference changes), and in most cases becomes not significant. That is, when we control for differences in incentives to misreport the medical examination between the draft eligible and the draft ineligible groups, failure rates are balanced between the two groups.

IV. Econometric methods and results

We are interested in estimating the causal effect of serving in the conscription on 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 National ID i who have a criminal record divided by the population size of cohort c and last three digits of National ID i), δ_c is a cohort effect, α is the treatment effect, and ε_{ci} is an error term.

To address the potential endogeneity of serving in the conscription in the crime equation, we estimate equation (1) by Two Stage Least Squares (2SLS), where the potentially 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. More formal 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 were originally draft eligible but did not pass the pre-induction medical examination.

Two-Stage Least Squares estimates of the impact of serving in the military are reported in Table 5. We report estimates with and without controls. As a benchmark, we also report reduced-form and Ordinary Least Squares (OLS) estimates. In all models our estimates indicate that serving in the military service significantly increases crime rates. The preferred (2SLS) estimates indicate that military service significantly increases crime rates by 3.91%. Thus, our instrumental variables results

suggests that serving in the conscription raises an Argentinean 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%. Hence, the estimates imply that conscription would raise a typical man's prosecution or conviction rate from roughly 6.8% to roughly 7.07%.²³

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:

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

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

As shown in columns (7) to (9) 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 (7) we present the regression for the cohorts of 1929 to 1975, where we estimate that military service significantly increases crime rates of draft-eligible individuals by 1.06%. In columns (8) and (9) 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 1.64%, and it is smaller and not significant for the early period.

²³ 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.

Our 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 could have changed for the cohorts of 1958 to 1975 relative to the cohorts of 1929 to 1955.²⁴

Even when our study relies on a well documented randomization, one can still harbor some potential concerns regarding the exogeneity of the treatment. For instance, it could be that the outcome of the lottery directly affects 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 conscription. To address this concern 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 cutoff number for this cohort based on the cutoffs numbers for the cohort of 1975 and we compare crime rates for those with “high” and “low” numbers. The coefficient for the faked dummy for being draft eligible has the opposite sign and it is not significant (the point estimate is -0.001 with a standard error of 0.001). This result suggests that the treatment is indeed exogenous,

²⁴ 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.

since there are no differences in crime rates between those that were and were not at risk of incorporation.

In Table 6 we 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 conscripts incorporated where mobilized to the conflict region. Results in columns (1) to (3) indicate that the effect of military service on crime is larger for those draftees in the two cohorts that participated in the Malvinas War.²⁵ 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.

Estimates in columns (4) to (6) show 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. This result is consistent with early experience in the labor market being an important channel through which the military service affects criminal behavior.

Our results suggest that conscription increases the likelihood of developing a criminal record during adulthood. Several explanations arise. Perhaps the firearm training received during military service reduced the entry costs into crime. Also military service may have delayed the insertion of the young into the labor market affecting future opportunities. The latter interpretation is consistent with the

²⁵ 2SLS results only include one cohort (1962) that participated in the war.

additional deleterious effect observed for those that provided service in the Navy for two years.²⁶

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 the late 1990s, but details the type of crime.

In Table 6, we estimate the effect of military service by type of crime. Remarkably, our estimates indicate a positive impact of military service on arm-related crime rates. This result is consistent with the hypothesis that firearm training received during military service reduces entry costs into crime, though the value of the coefficient indicates that the impact through this pathway is relatively small.

As discussed above, an alternative 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 property crimes. This hypothesis implies that property crime should be lower for those men not serving in the military service. The coefficients associated with Draft Eligible in the regressions on property crime are positive and significant, results that are consistent with this hypothesis.²⁷

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, unemployment, and

²⁶ 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.

²⁷ The results also show a significantly differential effect on white collar crime. We are not able to precisely estimate the effects on violent behavior (threats), drug trafficking, sexual attacks, and murders (though we can rule out very large effects).

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, 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 labor market effects, however, are small.

The result of a negative impact of military service on labor market outcomes is not novel. 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. Angrist (1990) and Angrist and Chen (2007) present evidence indicating that the causal mechanism for this relationship is lost labor market experience.²⁸ If the loss-of-experience story is true, then we would expect to see similar effects of military service for men who served during times of peace and during times of war. To evaluate in a convincing way whether such a negative effect exists, it would be necessary to measure the effects of conscription on earnings in both wartime and peacetime. The draft lottery in Argentina presents a unique opportunity to test this prediction because the natural experiment provides sources of identification in both peacetime and wartime situations.²⁹ The results are not conclusive: we find that the participation in the formal economy is lower, unemployment rates are higher, and earnings are lower for those draftees in the cohorts that participated in the Malvinas War, but the effect of military service on earning and unemployment are not significantly different from zero (these results are available from the authors upon request). This is consistent with the hypothesis that job market effects are due to lost job experience.

²⁸ Other related papers are Imbens and van der Klaauw (1995), who study labor market effects of mandatory military service, and Angrist (1998) who considers voluntary military service.

²⁹ We thank an anonymous referee for arising this point.

It is worth noting that, given that job market outcomes correspond to 20 to 27 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 statistically significant but relatively low impact of conscription on labor market outcomes are comparable to the ones presented in Angrist and Chen (2007), who measure the impact 28 to 30 years after serving in Vietnam and report very low long-term impact of veteran status on job market outcomes.³⁰

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 teaches them obedience and discipline, 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 showing a deleterious effect of peace-times conscription on future criminal participation.

Additional evidence suggests two particular channels through which this effect could have operated. The significant effect of military service on arm-related crimes

³⁰ Previous results focused on average effects. To explore possible heterogeneity in the effects we use information on the available pre-treatment characteristics for the cohorts of 1958 to 1962. As explained above, for these cohorts we have individual level information on whether the individual is an indigenous citizen. We also have pre-treatment district data. We use these district data to construct two socioeconomic indicators: proportion of individuals between 25 and 39 years old with university studies and proportion of rural population. When we make the interaction between these three pre-treatment variables and the treatment assignment, we find that the interaction effects are not significant.

suggests that the firearm training received during military service may reduce the entry costs into crime. Moreover, the significant effect of military service on property crimes suggests that conscription affects young men future opportunities by delaying their insertion into the labor market. This hypothesis is consistent with our findings that military service has detrimental effects on future job market performance and with the stronger crime effects for the individuals that provided longer conscription service.

References

- American Psychiatric Association (1994). "Diagnostic and Statistical Manual of Mental Disorders." Fourth Edition, Washington DC.
- Angrist, Joshua (1990). "Lifetime Earnings and the Vietnam Era Draft Lottery: Evidence from Social Security Administrative Records." *American Economic Review* 80 (3), 313-336.
- Angrist, Joshua (1998). "Estimating the Labor Market Impact of Voluntary Military Service Using Social Security Data on Military Applicants." *Econometrica* 66 (2), 249-288.
- Angrist, Joshua and Stacey Chen (2007). "Long-Term Consequences of Vietnam-Era Conscription: Schooling, Experience, and Earnings." Unpublished paper.
- Angrist, Joshua, Guido Imbens, and Donald Rubin (1996). "Identification of Causal Effects Using Instrumental Variables." *Journal of the American Statistical Association* 91 (434), 444-455.
- Archer, Dane and Rosemary Gartner (1984). "Violence and Crime in Cross-national Perspective." New Haven, CT: Yale University Press.
- Bayer, Patrick, Randi Pintoff, and David Pozen (2007). "Building Criminal Capital Behind Bars: Social Learning in Juvenile Corrections." NBER Working Paper 12932.
- Bouffard, Leana (2003). "Examining the Relationship between Military Service and Criminal Behavior during the Vietnam Era." *Criminology* 41 (2), 491-510.
- Bryant, Clifton (1979). "Khaki-Collar Crime: Deviant Behavior in the Military Context." New York: The Free Press.

Case, Anne and Lawrence Katz (1991). "The Company You Keep: The Effects of Family and Neighborhood on Disadvantaged Youths." Working Paper No. 3705. Cambridge, NBER.

Clinard, Marshall and Daniel Abbott (1973). "Crime in Developing Countries: A Comparative Approach." New York: John Wiley.

Elder, Glen, John Modell, and Ross Parke (1993). "Studying Children in a Changing World." In *Children in Time and Place: Developmental and Historical Insights*, Glen Elder, John Modell, and Ross Parke (editors). Cambridge: Cambridge University Press, 3-21.

Freeman, Richard (1996). "Why Do So Many Young American Men Commit Crimes and What Might We Do About It?" *Journal of Economic Perspectives* 10 (1), 25-42.

Freeman, Thomas and Vincent Roca (2001). "Gun Use, Attitudes toward Violence, and Aggression among Combat Veterans with Chronic Posttraumatic Stress Disorder." *Journal of Nervous & Mental Disease* 189 (5), 317-320.

Glaeser, Eduard, Bruce Sacerdote and José Scheinkman (1996). "Crime and Social Interactions." *Quarterly Journal of Economics* 111, 507-548.

Goldberg, Jack, Margaret Richards, Robert Anderson, and Miriam Rodin (1991). "Alcohol Consumption in Men Exposed to the Military Draft Lottery: A Natural Experiment." *Journal of Substance Abuse* 3, 307-313.

Grogger, Jeff (1998). "Market Wages and Youth Crime." *Journal of Labor Economics* 16 (4), 756-791.

Hearst, Norman, Thomas Newman, and Stephen Hulley (1986). "Delayed Effects of the Military Draft on Mortality. A Randomized Natural Experiment." *New England Journal of Medicine* 314 (10), 620-624.

Hirschi, Travis and Michael Gottfredson (1983). "Age and the Explanation of Crime." *American Journal of Sociology* 89, 552-594.

Imbens, Guido and Wilbert van der Klauuw (1995). "Evaluating the Costs of Conscription in the Netherlands." *Journal of Business and Economic Statistics* 13 (2), 207-215.

Jacob, Brian and Lars Lefgren (2003). "Are Idle Hands the Devil's Workshop? Incapacitation, Concentration, and Juvenile Crime." *American Economic Review* 93 (5), 1560-1577.

Kling, Jeffrey, Jens Ludwig, and Lawrence Katz (2005). "Neighborhood Effects on Crime for Female and Male Youth: Evidence from a Randomized Housing Voucher Experiment." *Quarterly Journal of Economics* 120 (1), 87-130.

Levitt, Steven (1998). "Juvenile Crime and Punishment," *Journal of Political Economy* 106, 1156-1185.

Levitt, Steven and Lance Lochner (2001). "The Determinants of Juvenile Crime." In *Risky Behavior among Youths: An Economic Analysis*, edited by Jonathan Gruber. Chicago: University of Chicago Press.

McFall, Miles, Alan Fontana, Murray Raskind, and Robert Rosenheck (1999). "Analysis of Violent Behavior in Vietnam Combat Veteran Psychiatric Inpatients with Posttraumatic Stress Disorder." *Journal of Traumatic Stress* 12, 501-517.

Mulligan, Casey and Andrei Shleifer (2005). "Conscription as Regulation." *American Law and Economics Review* 7 (1), 85-111.

Pastore, Ann and Kathleen Maguire (2003). "Sourcebook of Criminal Justice Statistics." Washington, DC: Bureau of Justice Statistics.

Rodríguez Molas, Ricardo (1983). "El Servicio Militar Obligatorio." CEAL, Buenos Aires.

Rohlf, Chris (2007). "Does Military Service Make You a More Violent Person?: Evidence from the Vietnam Draft Lottery." Mimeo, University of Chicago.

Smith, Marvin (1985). "Early Labor Market Experiences of Youth and Subsequent Wages." *American Journal of Economics and Sociology* 44 (4), 391-400.

Tax, Sol (1967), *The Draft: A Handbook of Facts and Alternatives*. University of Chicago Press.

War Resisters' International (1998). "Refusing to Bear Arms: A Worldwide Survey of Conscription and Conscientious Objection to Military Service." London: War Resisters' International.

Yager, Thomas, Robert Laufer, and Mark Gallops (1984). "Some Problems Associated With War Experience in Men of the Vietnam Generation." *Archives of General Psychiatry* 41, 327-333.

Table 1. Descriptive statistics for men born 1958-1962

	<i>Mean</i>	<i>Standard Deviation</i>	<i>Observations</i>
Draft Eligible	0.6998	0.4584	5000
Served in the Conscription	0.5031	0.3049	5000
Navy	0.1196	0.3245	5000
Malvinas War	0.2000	0.4000	5000
		Crime Variables	
Crime Rate	0.0693	0.0178	5000
Use of Arms	0.0010	0.0024	5000
Against Property	0.0075	0.0073	5000
Sexual Attack	0.0007	0.0021	5000
Murder	0.0009	0.0021	5000
Threat	0.0017	0.0031	5000
Drug Trafficking	0.0012	0.0028	5000
White Collar	0.0034	0.0046	5000
		Labor Market Variables	
Participation in the Formal Job Market	0.3387	0.0470	5000
Unemployment Rate	0.1797	0.0543	5000
Earnings	3.1734	0.2343	5000
		Pre-Treatment Characteristics	
Native citizens	0.9707	0.0194	5000
Indigenous Citizen	0.0009	0.0019	5000
Naturalized Citizen	0.0001	0.0007	5000
		Pre-Treatment Characteristics – District of Birth	
Buenos Aires	0.3448	0.0326	5000
Ciudad de Buenos Aires	0.0855	0.0186	5000
Catamarca	0.0096	0.0064	5000
Chaco	0.0347	0.0114	5000
Chubut	0.0095	0.0061	5000
Córdoba	0.0869	0.0186	5000
Corrientes	0.0321	0.0107	5000
Entre Ríos	0.0388	0.0121	5000
Formosa	0.0150	0.0080	5000
Jujuy	0.0169	0.0083	5000
La Pampa	0.0075	0.0054	5000
La Rioja	0.0077	0.0054	5000
Mendoza	0.0435	0.0125	5000
Misiones	0.0277	0.0104	5000
Neuquén	0.0087	0.0059	5000
Río Negro	0.0130	0.0071	5000
Salta	0.0274	0.0102	5000
San Juan	0.0187	0.0087	5000
San Luis	0.0086	0.0059	5000
Santa Cruz	0.0034	0.0038	5000
Santa Fé	0.0863	0.0173	5000
Santiago del Estero	0.0289	0.0108	5000
Tierra del Fuego	0.0008	0.0019	5000
Tucumán	0.0406	0.0121	5000

Note: The level of observation is the cohort-ID number combination. Earnings are hourly earnings in Argentine Pesos. Participation in the formal job market, unemployment rates, and earning correspond to 2003.

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

<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>
Native citizens	0.0021 (0.0013)	0.0001 (0.0008)	-0.0001 (0.0007)	-0.0001 (0.0004)	0.0002 (0.0008)
Indigenous citizens	0.0000 (0.0002)	0.0000 (0.0001)	0.0001 (0.0001)	0.0000 (0.0001)	0.0001 (0.0001)
Naturalized citizen	0.0000 (0.0001)	0.0000 (0.0000)	0.0000 (0.0000)	0.0001 (0.0001)	0.0000 (0.0001)
<i>Districts</i>					
Buenos Aires	0.0042 (0.0026)	0.0007 (0.0023)	0.0021 (0.0023)	-0.0018 (0.0019)	-0.0013 (0.0019)
Ciudad de Buenos Aires	0.0022 (0.0016)	0.0004 (0.0013)	-0.0038*** (0.0010)	0.0013 (0.0011)	0.0038*** (0.0011)
Catamarca	-0.0007 (0.0005)	-0.0007 (0.0005)	0.0000 (0.0005)	0.0004 (0.0004)	-0.0001 (0.0004)
Chaco	-0.0005 (0.0009)	0.0010 (0.0007)	-0.0004 (0.0008)	0.0006 (0.0008)	-0.0012 (0.0008)
Chubut	0.0004 (0.0005)	-0.0004 (0.0004)	-0.0002 (0.0004)	-0.0005 (0.0004)	-0.0005 (0.0004)
Córdoba	-0.0015 (0.0015)	0.0025** (0.0012)	0.0038** (0.0015)	-0.0010 (0.0012)	0.0011 (0.0011)
Corrientes	-0.0017* (0.0010)	-0.0012 (0.0007)	0.0011 (0.0007)	0.0005 (0.0007)	-0.0005 (0.0007)
Entre Ríos	-0.0008 (0.0010)	-0.0008 (0.0008)	-0.0003 (0.0008)	0.0010 (0.0008)	-0.0008 (0.0007)
Formosa	-0.0004 (0.0007)	0.0008 (0.0005)	0.0004 (0.0006)	-0.0004 (0.0005)	-0.0004 (0.0005)
Jujuy	-0.0007 (0.0007)	-0.0002 (0.0005)	-0.0009* (0.0005)	0.0008 (0.0006)	-0.0017*** (0.0005)
La Pampa	0.0006 (0.0005)	0.0006 (0.0004)	0.0003 (0.0004)	-0.0007* (0.0004)	0.0000 (0.0004)
La Rioja	0.0000 (0.0004)	-0.0008** (0.0004)	-0.0004 (0.0004)	0.0000 (0.0004)	-0.0004 (0.0003)
Mendoza	0.0003 (0.0011)	-0.0004 (0.0009)	-0.0003 (0.0008)	-0.0004 (0.0009)	0.0018** (0.0008)
Misiones	-0.0009 (0.0008)	0.0002 (0.0007)	-0.0006 (0.0007)	-0.0001 (0.0007)	-0.0005 (0.0007)
Neuquén	0.0001 (0.0005)	0.0003 (0.0004)	-0.0003 (0.0004)	0.0003 (0.0004)	-0.0003 (0.0004)
Río Negro	-0.0004 (0.0006)	-0.0001 (0.0005)	-0.0001 (0.0005)	-0.0001 (0.0004)	0.0005 (0.0005)
Salta	0.0000 (0.0009)	-0.0005 (0.0007)	0.0011* (0.0007)	-0.0002 (0.0007)	0.0002 (0.0007)
San Juan	0.0006 (0.0009)	0.0000 (0.0006)	-0.0007 (0.0006)	-0.0001 (0.0005)	0.0006 (0.0006)
San Luis	0.0001 (0.0005)	-0.0004 (0.0004)	-0.0001 (0.0004)	0.0000 (0.0004)	-0.0003 (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

<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>
All numbers	-0.0017 (0.0014)	-0.0016 (0.0013)	-0.0143*** (0.0013)	-0.0197*** (0.0012)	-0.0232*** (0.0012)
20 numbers around the final cut-off number	0.0027 (0.0050)	-0.0043 (0.0068)	0.0009 (0.0056)	0.0141** (0.0053)	-0.0060 (0.0059)
15 numbers around the final cut-off number	0.0038 (0.0056)	-0.0060 (0.0085)	0.0008 (0.0070)	0.0129* (0.0066)	-0.0034 (0.0070)
10 numbers around the final cut-off number	0.0077 (0.0056)	-0.0044 (0.0116)	-0.0043 (0.0090)	0.0108 (0.0075)	0.0017 (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

	<i>Dependent Variable: Served in the Conscription</i>					
	<i>1958-62</i>	<i>1958</i>	<i>1959</i>	<i>1960</i>	<i>1961</i>	<i>1962</i>
	<i>(1)</i>	<i>(2)</i>	<i>(3)</i>	<i>(4)</i>	<i>(5)</i>	<i>(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)	0.0579*** (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. Model (1) includes cohort dummies. ***Significant at the 1% level.

Table 5. Estimates of the impact of conscription on crime rates

<i>Cohorts</i>	<i>Dependent Variable: Crime Rate</i>								
	<i>(1)</i> <i>1958-62</i>	<i>(2)</i> <i>1958-62</i>	<i>(3)</i> <i>1958-62</i>	<i>(4)</i> <i>1958-62</i>	<i>(5)</i> <i>1958-62</i>	<i>(6)</i> <i>1958-62</i>	<i>(7)</i> <i>1929-75</i>	<i>(8)</i> <i>1929-55</i>	<i>(9)</i> <i>1958-75</i>
Draft Eligible	0.0018 (0.0006)*** {0.0002}***	0.0018 (0.0006)*** {0.0002}***					0.0006 (0.0002)*** {0.0002}***	0.0003 (0.0004) {0.0002}	0.0010 (0.0003)*** {0.0003}***
Served in the Conscription			0.0023 (0.0008)*** {0.0004}***	0.0022 (0.0008)*** {0.0004}***	0.0026 (0.0008)*** {0.0009}**	0.0027 (0.0008)*** {0.0003}***			
% Change	2.58	2.60	3.32	3.30	3.75	3.94	1.08	0.46	1.67
Controls	No	Yes	No	Yes	No	Yes	No	No	No
Observations	5,000	5,000	5,000	5,000	5,000	5,000	39,928	26,976	12,952
Method	OLS	OLS	OLS	OLS	2SLS	2SLS	OLS	OLS	OLS

Notes: Robust standard errors are shown in parentheses. Standard errors clustered by cohort-eligibility are shown in braces. 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 Served in the Conscription is Draft Eligible. % Change is calculated as 100*Estimate/mean crime rate of draft-ineligible men. ***Significant at the 1% level.

Table 6. Peace vs. war times and one-year vs. two-years

Cohorts	<i>Dependent Variable: Crime Rate</i>					
	(1) 1929-75	(2) 1958-75	(3) 1958-62	(4) 1929-75	(5) 1958-75	(6) 1958-62
Draft Eligible	0.0005 (0.0003)* {0.0002}***	0.0008 (0.0004)** {0.0003}***		0.0006 (0.0002)** {0.0002}***	0.0009 (0.0004)** {0.0003}***	
Malvinas War *Draft Eligible	0.0014 (0.0009)* {0.0004}***	0.0011 (0.0009) {0.0004}***				
Served in the Conscription			0.0024 (0.0010)** {0.0004}***			0.0026 (0.0009)** {0.0005}***
Served in the Conscription *Malvinas War Navy (2 years)			0.0014 (0.0020) {0.0004}***	0.0007 (0.0003)** {0.0004}*	0.0011 (0.0006)* {0.0010}	0.0001 (0.0008) {0.0014}
Observations	39,928	12,952	5,000	39,928	12,952	5,000
Method	OLS	OLS	2SLS	OLS	OLS	2SLS

Notes: Robust standard errors are shown in parentheses. Standard errors clustered by cohort-eligibility are shown in braces. 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; ***Significant at the 1% level.

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

	<i>Use of Arms</i>	<i>Against Property</i>	<i>Sexual Attack</i>	<i>Murder</i>	<i>Threat</i>	<i>Drug Trafficking</i>	<i>White Collar</i>
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
<i>Cohorts</i>	1958-62	1958-62	1958-62	1958-62	1958-62	1958-62	1958-62
Served in the Conscription	0.0001 (0.0001) {0.00005}**	0.0008 (0.0003)** {0.0002}***	0.0001 (0.0001) {0.0001}*	-0.0001 (0.0001) {0.0001}	0.0002 (0.0001) {0.0001}**	-0.0001 (0.0001) {0.0001}	0.0006 (0.0002)*** {0.0001}***
% Change	0.20	1.20	0.20	-0.11	0.33	-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

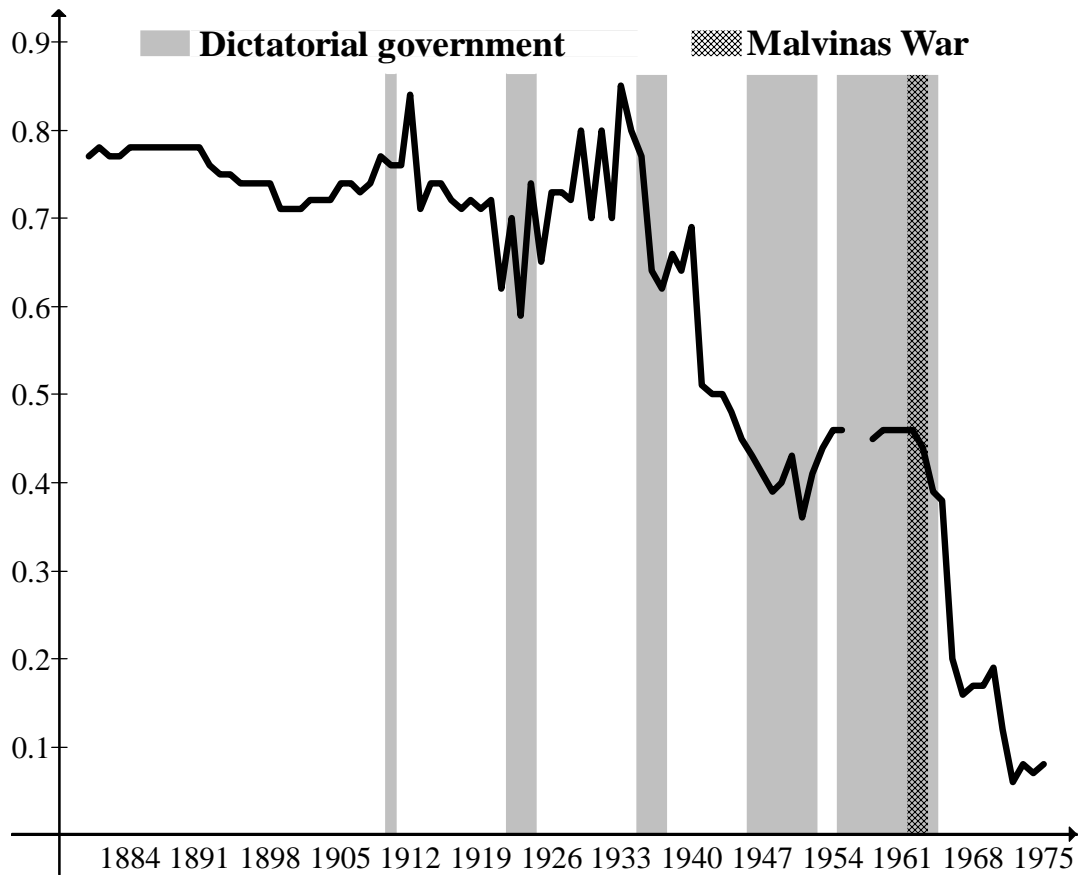
Notes: Robust standard errors are shown in parentheses. Standard errors clustered by cohort-eligibility are shown in braces. The level of observation is the cohort-ID number combination. All models include cohort dummies. The instrument for Served in the Conscription is Draft Eligible. % Change is calculated as 100*Estimate/mean crime rate of draft-ineligible men. *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

	<i>Participation in the Formal Job Market</i>		
	(1)	(2)	(3)
Draft Eligible	-0.0015 (0.0014) {0.0015}		-0.0006 (0.0009) {0.0008}
Served in the Conscription		-0.0022 (0.0021) {0.0023}	
% Change	-0.43	-0.65	-0.17
	<i>Unemployment Rate</i>		
	(4)	(5)	(6)
Draft Eligible	0.0005 (0.0006) {0.0002}***		0.0001 (0.0005) {0.0003}
Served in the Conscription		0.0008 (0.0009) {0.0003}**	
% Change	0.27	0.41	0.03
	<i>Earnings</i>		
	(7)	(8)	(9)
Draft Eligible	-0.0111 (0.0070) {0.0031}***		-0.0153 (0.0043)*** {0.0026}***
Served in the Conscription		-0.0169 (0.0106) {0.0048}***	
% Change	-0.35	-0.53	-0.56
Cohorts	1958-62	1958-62	1958-75
Observations	5,000	5,000	12,952
Method	OLS	2SLS	OLS

Notes: Robust standard errors are shown in parentheses. Standard errors clustered by cohort-eligibility are shown in braces. The level of observation is the cohort-ID number combination. Earnings are hourly earnings in Argentine Pesos. Participation in the formal job market, unemployment rates, and earnings correspond to 2003. All models include cohort dummies. In 2SLS models the instrument for Served in the Conscription is Draft Eligible. % Change is calculated as 100*Estimate/mean dependent variable of draft-ineligible men. *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 for military service.

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

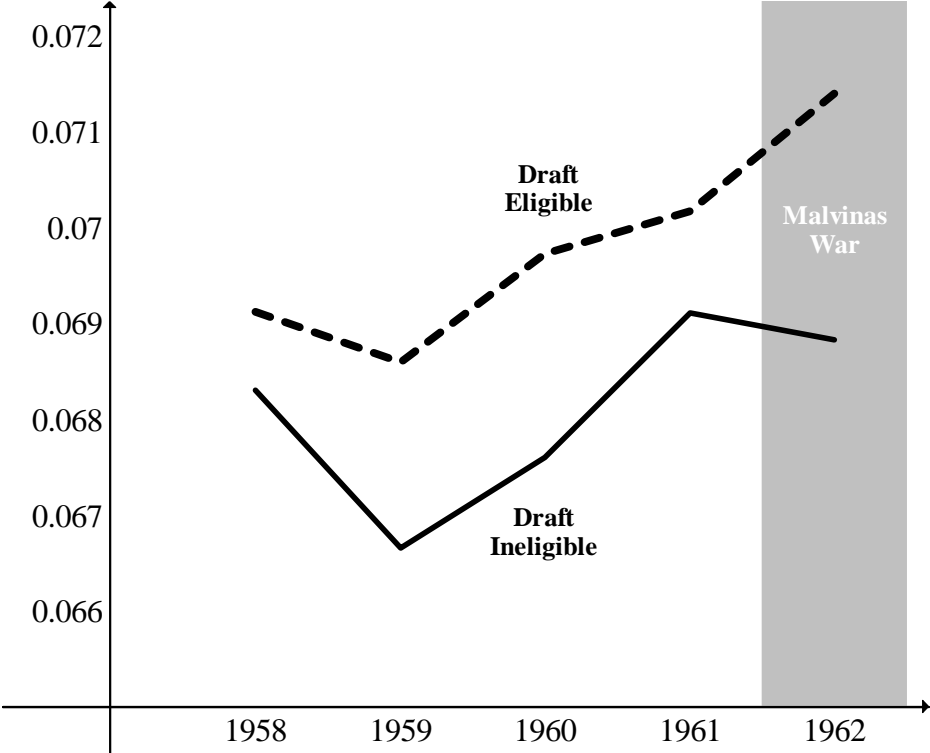
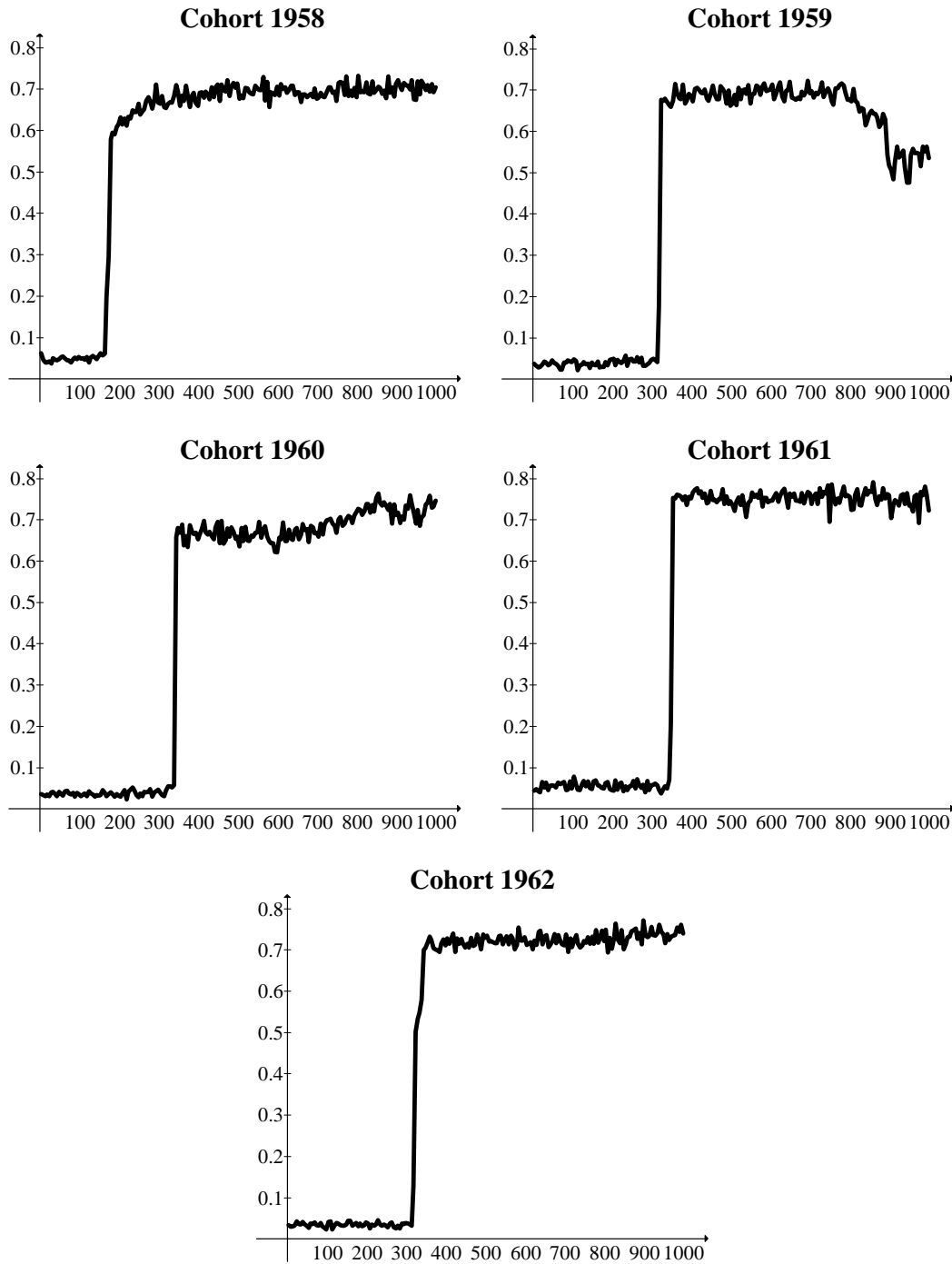


Figure 3. The relation between the conditional probability of serving in the conscription and draft lottery numbers for the cohorts of 1958 to 1962



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.