Conscription and Crime

Sebastian Galiani
Washington University in St. Louis

Martín A. Rossi
Universidad de San Andrés

and

Ernesto Schargrodsky*
Universidad Torcuato Di Tella

Abstract

In this study 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 database that includes draft eligibility and criminal records for almost 40 cohorts, we find that participation in the military service increases the likelihood of developing a criminal record in adulthood, particularly for property and arm-related crimes. We also find that military service has detrimental effects on future performance in the labor market. 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.

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 Andres, 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 are grateful to Horacio Tarelli and Fernando Michelena for crucial help throughout this study. We also thank Rut Diamint and the Argentine Army for their cooperation. Maximiliano Appendino and Florencia Borrescio Higa provided excellent research assistance; and the World Bank provided financial support.

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 average 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 seventy-seven 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 Lance (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 then analyze the causal effect of this randomly assigned eligibility variable 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 et al. (1996).

Using a unique set of administrative data that includes draft eligibility and criminal records for all Argentine males born between 1929 and 1965, 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), alcohol consumption (Goldberg et al., 1991), and mortality (Hearst, Newman, and Hulley, 1986).[6] 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).[7]

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.[8] 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

[6] Angrist (1990) show that military service in the Vietnam era reduced the civilian earnings of white veterans. Goldberg et al. (1991), find no evidence of increased alcohol consumption among draft-eligible men. Hearst, Newman, and Hulley (1986) report a higher mortality rate for men serving the military during wartime. Bedard and Deschenes (2006) also show negative health effects for the WWII and Korean War veterans, in particular, for smoking-related diseases. Angrist and Krueger (1994) find that World War II veterans earn no more and probably less than comparable non-veterans.

[7] 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.

[8] 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.

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.[9]

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).[10] 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.

Some countries have been recently discussing the re-implementation of conscription to address youth's conflicts. For example, as a response to the high

[9] Our complementary results of detrimental effects of peace-time military service on job market performance are, instead, not original. For example, Imbens and van der Klaauw (1995) study labor market effects of mandatory military service, whereas Angrist (1998) considers voluntary military service. At the macro level, Keller, Poutvaara, and Wagener (2006) find a negative effect of mandatory military service on countries' economic performance.

[10] See the War Resisters' International webpage at http://www.wri-irg.org/wri.htm. Out of the 85 countries without conscription, 11 are involved in an armed conflict.

levels of criminality in South Africa, Labor Minister Membathisi Mdladlana proposed that army conscription could help end violent crime.[11] 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".[12] 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 presents the data and the main characteristics of the military service in Argentina. Section III presents our econometric methods. Section IV reports the results and Section V concludes.

II. Data

From 1901 through 1995, military service in Argentina was mandatory. It 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.[13]

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. 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

[11] See News24.com, "Minister moots conscription," January 30, 2007.
[12] See LeMonde.fr, "Jacques Chirac lance le service civil volontaire", ("Jacques Chirac launches the Voluntary Civil Service"), November 17, 2005.
[13] On the military service in Argentina, see Rodríguez Molas (1983).

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. A cutoff number was announced and those "candidates" whose ID number corresponded to a lottery number above the cutoff 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. We obtained lottery draft results and cut-off numbers from the Argentine Army.[14] 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.[15]

Exploiting this random assignment, we will try to answer whether conscription incentives or disincentives involvement in criminal activities. To answer this question, we use two individual-level datasets provided by the Justice Ministry.[16] 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.[17] The other dataset covers a shorter period of time, but it details the type of crime that originated the criminal record. This second database has detailed information on all men that have gone through a criminal justice process since 2000 (about a quarter-million

[14] *Oficina de Reclutamiento y Movilización, Estado Mayor del Ejército Argentino*.
[15] As pointed out by Angrist (1990), the fact that the selection process for entry into the military service was ultimately not random does not imply that the priority for induction was not randomly assigned.
[16] *Dirección Nacional de Reincidencia, Ministerio de Justicia de la Nación.*
[17] The complete ID number was not provided for confidentiality reasons.

observations), and includes the last three ID numbers, the year of birth, and the type of crime.[18]

We have information on cutoff assignment numbers for the cohorts of 1929 to 1975.[19] For the cohorts of 1966 to 1975, the cutoff number differed by military district (29 military districts), and our data do not allow the association of each individual to a particular military district. Since for some years the difference in cutoff numbers by military district was important, we exclude the cohorts of 1966 to 1975 from our main regressions. For the cohorts of 1955 and 1965, the cutoff number was different by army corp (there were 5 army corps -*cuerpos de ejército*- in the whole country), but the difference between the maximum and minimum cutoff numbers was small. We include these cohorts in our sample, but in order to avoid measurement errors, our regressions exclude all ID numbers with lottery numbers in between the maximum and the minimum cutoffs, reducing in 3.6% the number of available observations for these two cohorts.

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, and income. 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.[20] For unemployment and income data we first identified the occupation declared by each individual in the 2003 national ballot registry. As voting

[18] 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 is not possible for cohorts serving at the age of 18 as the lottery was performed the year before incorporation.

[19] For cohorts of 1931 to 1933, 1935 to 1936, 1938, and 1941 the cutoff number was equal to zero (i.e., the whole cohort was assigned to provide military service). The cohort of 1976 faced the conscription draft lottery but it was not incorporated.

[20] 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.

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 wage.[21] Unemployment rates (calculated as the share of unemployed over the active population) and average income levels were then obtained for each cohort-last three digits of national ID combination. Presence in the national ballot registry was also calculated for each cohort-last three digits of ID permutation to measure mortality plus migration rates.

III. Econometric Methods

In our case, we can estimate straightforwardly intention-to-treatment effects. First, 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. We then estimate the intention-to-treat (ITT) causal effect of military service on crime by estimating the following regression:

$$\text{Crime Rate}_{ci} = \beta + \gamma \text{ Draft Eligible}_{ci} + \delta_c + \varepsilon_{ci} \quad (1)$$

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 ID *i* who have a criminal record divided by the

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

population size of cohort *c* and ID *i*), δ_c is a cohort effect, γ is the intention-to-treat effect, and ε_{ci} is an error term.[22]

Naturally, as in any experiment, some individuals do not comply with treatment assignment. For example, in our case, individuals not passing the medical exams would not serve in the military service even though they were draft-eligible. For this reason, the intention-to-treat parameter does not inform us about the average effect of actually serving in the conscription on the likelihood of committing crimes during adulthood.

Let α be the (constant) effect of serving in the conscription on the likelihood of committing crimes during adulthood. Then, ITT = $\gamma = \alpha\ (p_1 - p_2)$, 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 (volunteers into the military service). Thus, under the assumption that $(p_1 - p_2) \neq 0$, we can indirectly estimate the average treatment effect (ATE) of serving in the conscription on crime as follows:

$$ATE = \frac{ITT}{(p_1 - p_2)} \qquad (2)$$

Note that the formula in (2) resembles the Wald estimator for the average treatment effect of serving in the conscription on crime. In this simple case, the Wald estimator is identical to the Instrumental Variables (IV) estimator, where the potentially

[22] The population size of the cohorts was obtained from Census data.

endogenous (unobserved) dummy variable Servicing in the Conscription is instrumented by the exogenous dummy variable Draft Eligible.[23]

According to information provided by the Argentine Army, the number of volunteers into the military service was basically nil during the sample period $(p_2 \cong 0)$, so the denominator in equation (2) is approximately equal to p_1. We have information on size of cohorts and total number of men incorporated into the military service by cohort, so we can estimate the value of p_1 as the ratio of incorporation to cohort size $(p_1 \cong 0.65$ in our sample period$)$.[24]

In the following section we report estimates of the intention-to-treat parameter and their standard errors clustered by cohort-eligibility. We also report point estimates of the average effect of serving in the conscription on crime both in absolute terms and as a percentage of the mean crime of the draft-ineligible group.

IV. Results

Our results suggest that conscription increases the likelihood of developing a criminal record during adulthood. In Table 1, using the dataset that includes all men that went through criminal justice process since 1934, we consistently find higher crime rates on those ID numbers that were made eligible for military service by the lottery. In column (1) 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 1.62%.

[23] However, if we do not 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).

[24] 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 misrepresenting their medical conditions. Deferment to attend college was also granted.

As explained in Section II, for cohorts of 1966 to 1975 the cutoff number differed by military district. When we repeat the model in column (1) including these cohorts and following the procedure of excluding all ID numbers with lottery numbers in between the maximum and the minimum cutoffs, the results remain unchanged.[25]

In columns (2) and (3) 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 2.58%, and it is not significant for the early period. 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 1965 relative to the cohorts of 1929 to 1955. Crime levels, for example, have been secularly growing in Argentina.

To check the robustness of these results to a possible non-random assignment of ID numbers in the population, we add ID fixed effects to the model in column (1). Results are exactly as before, indicating that there is no evidence of this possible source of confounding in our data.

[25] All regressions mentioned but not shown are available from the authors upon request.

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.

To further address the robustness of our findings, all these exercises were repeated using a Tobit specification. In all cases, the results are robust to the presence of censoring at zero in our dataset.[26]

In Table 2, we first explore differential effects of military service in peace and war times. In columns (1) and (2) we show that the effect of military service on crime is larger for those draftees in the two cohorts that participated in the Malvinas War.[27] However, the effect is also significant for the cohorts that provided military service during peace times, which comprise most of our sample.

In columns (3) and (4) we 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

[26] In our sample, the crime rate is equal to zero for 0.15 percent of cohort/ID combinations.

[27] Only a very small fraction of the draftees in these two cohorts were exposed to combat. From the 440000 men in cohorts 1962 and 1963, aproximately 12500 conscripts participated in the war and had, therefore, some level of combat exposure. The total Argentine death count (including professional and conscript troops) was 649.

of the one year served in the Army and the Air Force.[28] This result is consistent with early experience in the labor market being an important channel through which the military service affects criminal behavior.

In summary, our results suggest that participation in the military service increased the likelihood of developing a criminal record in 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 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.

A limitation of the alternative database is that the type of crime is only specified for 37% of the cases. Therefore, in order to check its validity, in Table 3 we perform a number of exercises. First, we reproduce the results from Table 1 including all observations. For all the time periods, the estimated coefficients are not significantly different from the ones obtained in Table 1. Then, we separately reproduce the results for those observations where the type of crime is specified and for those observations where the type of crime is not specified. As shown in Table 3, for all the time periods

[28] 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 estimated coefficients for the two groups are not significantly different. This result suggests that eligibility is not correlated with missing values in the database.

In Table 4, we estimate the effect of military service by type of crime for cohorts of 1958 to 1965. Remarkably, the coefficient on Draft Eligible in column (1) indicates 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 coefficient associated with Draft Eligible in the regression on property crime presented in column (2) is positive and significant, a result that is consistent with this hypothesis. The results show a significantly differential effect on violent behavior (threats), whereas there are no significant differences on sexual attacks, murder, drug trafficking, and white-collar crimes.[29]

To further explore the labor market channel, in Table 5 we present results of the impact of conscription on participation in the formal job market, unemployment, and income. Our results in the first three columns indicate 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

[29] The results are basically similar when the Malvinas War interaction is included, with the exception of the Draft Eligible coefficient on crimes against property, which loses statistical significance. In particular, the arm-related results are not different for the cohorts that participated in the Malvinas War than for the peace-time cohorts.

on job market performance supports the hypothesis of the detrimental effect of military service on criminal behavior through the labor market.

Finally, in column (4) we explore whether the estimated effects could be the result of different migration or mortality rates affecting those that served in the conscription. Using the national ballot registry (voting is mandatory in Argentina) we find that conscription does not affect the probability of being alive and living in Argentina. This result rules out potential concerns of differential attrition.[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 suggests that the firearm training received during military service may reduce the

[30] Again, the results in Table 5 are basically similar when the effects for the Malvinas War cohorts are separately estimated, although draft eligibility loses statistical significance on participation in the formal labor market.

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 earnings, 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, D.C.

Angrist, J. (1990). "Lifetime Earnings and the Vietnam Era Draft Lottery: Evidence from Social Security Administrative Records." American Economic Review 80 (3), 313-336.

Angrist, J. (1998). "Estimating the Labor Market Impact of Voluntary Military Service Using Social Security Data on Military Applicants," Econometrica Vol. 66, No. 2, pp. 249-288.

Angrist J. and A. Krueger (1994). "Why do World War II Veterans Earn More than Nonveterans?," Journal of Labor Economics, Vol. 12, No. 1, pp. 74-97.

Angrist, J., Imbens, G., and Rubin, D. (1996). "Identification of Causal Effects Using Instrumental Variables." Journal of the American Statistical Association 91 (434), 444-455.

Archer, D. and Gartner, R. (1984). "Violence and Crime in Cross-national Perspective." New Haven, CT: Yale University Press.

Bayer, P., Pintoff, R., and Pozen, D. (2007). "Building Criminal Capital Behind Bars: Social Learning in Juvenile Corrections," NBER Working Paper 12932.

Bedard, K., and O. Deschenes. "The Long-Term Impact of Military Service on Health: Evidence from World War II and the Korean War." American Economic Review, 96(1), pp. 176-94. 2006.

Bouffard, L. (2003). "Examining the Relationship between Military Service and Criminal Behavior during the Vietnam Era." Criminology 41 (2), 491-510.

Bryant, C. (1979). "Khaki-Collar Crime: Deviant Behavior in the Military Context." New York: The Free Press.

Case, A. and Katz, L. (1991). “The Company You Keep: The Effects of Family and Neighborhood on Disadvantaged Youths,” Working Paper No. 3705. Cambridge, NBER.

Clinard, M. and Abbott, D. (1973). “Crime in Developing Countries: A Comparative Approach.” New York: John Wiley.

Elder, G., Modell, J., and Parke, R. (1993). “Studying Children in a Changing World.” In Children in Time and Place: Developmental and Historical Insights, Glen H. Elder, John Modell, and Ross D. Parke (editors). Cambridge: Cambridge University Press, 3–21.

Freeman, R. (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, T. and Roca, V. (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, E., B. Sacerdote and J. Scheinkman (1996). “Crime and Social Interactions,” Quarterly. Journal of Economics 111, 507-548.

Goldberg, J., Richards, M., Anderson, R., and Rodin, M. (1991). “Alcohol Consumption in Men Exposed to the Military Draft Lottery: A Natural Experiment.” Journal of Substance Abuse 3, 307-313.

Grogger, J. (1998). “Market Wages and Youth Crime.” Journal of Labor Economics 16 (4), 756-791.

Hearst, N., Newman, T., and Hulley, S. (1986). “Delayed Effects of the Military Draft on Mortality. A Randomized Natural Experiment.” New England Journal of Medicine 314 (10), 620-624.

Hirschi, T. and Gottfredson, M. (1983). “Age and the Explanation of Crime.” American Journal of Sociology 89, 552-594.

Imbens, G. and W. van der Klauuw (1995). “Evaluating the Costs of Conscription in the Netherlands,” Journal of Business and Economic Statistics, Vol. 13(2): 207-15.

Jacob, B. and Lefgren, L. (2003). “Are Idle Hands the Devil’s Workshop? Incapacitation, Concentration, and Juvenile Crime.” American Economic Review 93 (5), 1560-1577.

Keller, K., Poutvaara, P., and Wagener, A. (2006). “Military Draft and Economic Growth in OECD Countries.” IZA Discussion Paper Number 2022.

Kling, J., Ludwig, J., and Katz, L. (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, S. (1998). “Juvenile Crime and Punishment,” Journal of Political Economy 106, 1156-1185.

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

McFall, M., Fontana, A., Raskind, M., and Rosenheck, R. (1999). “Analysis of Violent Behavior in Vietnam Combat Veteran Psychiatric Inpatients with Posttraumatic Stress Disorder.” Journal of Traumatic Stress 12, 501-517.

Mulligan, C. and Shleifer, A. (2005). “Conscription as Regulation.” American Law and Economics Review 7 (1), 85-111.

Pastore, A. and Maguire, K. (2003). “Sourcebook of Criminal Justice Statistics.” Washington, DC: Bureau of Justice Statistics.

Rodríguez Molas, R. (1983). “El Servicio Militar Obligatorio.” CEAL, Buenos Aires.

Rohlfs, C. (2007). “Does Military Service Make You a More Violent Person?: Evidence from the Vietnam Draft Lottery.” Mimeo, University of Chicago.

Smith, M. (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, T., Laufer, R., and Gallops, M. (1984). “Some Problems Associated With War Experience in Men of the Vietnam Generation.” Archives of General Psychiatry 41, 327-333.

Table 1. Estimates of the impact of conscription on crime

	Dependent Variable: Crime Rate		
Cohorts	1929-65	1929-55	1958-65
	(1)	(2)	(3)
Draft Eligible (=1)	.00063***	.00025	.00114***
	[.00020]	[.00023]	[.00031]
Wald Estimate	0.0010	0.0004	0.0017
% Change	1.616	0.712	2.580
Observations	34904	26976	7928

Notes: Standard errors clustered by cohort-eligibility are shown in brackets. All models include cohort dummies and are estimated by OLS. The Wald Estimate is calculated as the ratio of the coefficient on the Draft Eligible variable to the proportion of draft-eligible men that were incorporated into the military service. % change is calculated as 100*Wald Estimate/mean crime rate of draft-ineligible men. ***Significant at the 1% level.

Table 2. Estimates of the impact of conscription on crime: Peace vs. War Times and 1-year vs. 2-years

	Dependent Variable: Crime Rate			
Cohorts	1929-65	1958-65	1929-65	1958-65
	(1)	(2)	(3)	(4)
Draft Eligible (=1)	.00047**	.00085**	.00053**	.00096**
	[.00020]	[.00036]	[.00022]	[.00037]
% Change	1.205	1.924	1.359	2.173
Malvinas War*Draft Eligible	.00148***	.00109**		
	[.00038]	[.00049]		
% Change	3.796	2.467		
Navy (=1)			.00072*	.00107
			[.00037]	[.00087]
% Change			1.846	2.422
Observations	34904	7928	34904	7928

Notes: Clustered standard errors are shown in brackets. Standard errors in models (1) and (2) are clustered by cohort-eligibility and standard errors in models (3) and (4) are clustered by cohort-eligibility-Navy. All models include cohort dummies and are estimated by OLS. % change is calculated as 100*Wald Estimate/mean crime rate of draft-ineligible men. *Significant at the 10% level; **Significant at the 5% level; ***Significant at the 1% level.

Table 3. Estimates of the impact of conscription on crime – Alternative database

	Dependent variable: Crime Rate		
Cohorts	1929-65	1929-55	1958-65
	Database including all observations		
	(1)	(2)	(3)
Draft Eligible (=1)	.00056**	1.20e-06	.00131***
	[.00024]	[.00025]	[.00038]
	Database including those observations where the type of crime is specified		
	(4)	(5)	(6)
Draft Eligible (=1)	.00032**	.000026	.00079***
	[.00015]	[.00014]	[.00023]
	Database including those observations where the type of crime is not specified		
	(7)	(8)	(9)
Draft Eligible (=1)	.00023*	.000028	.00051**
	[.00014]	[.00015]	[.00023]
Observations	34904	26976	7928

Notes: Standard errors clustered by cohort-eligibility are shown in brackets. All models include cohort dummies and are estimated by OLS. *Significant at the 10% level; **Significant at the 5% level; ***Significant at the 1% level.

Table 4. Estimates of the impact of conscription on crime rates, by type of crime

	Dependent Variable: Crime Rate						
Cohorts	1958-65	1958-65	1958-65	1958-65	1958-65	1958-65	1958-65
	(1) Use of Arms	(2) Against Property	(3) Sexual Attack	(4) Murder	(5) Threat	(6) Drug Trafficking	(7) White Collar
Draft Eligible (=1)	.00006*** [.00002]	.00025* [.00014]	.00002 [.00004]	-.00003 [.00004]	.00011** [.00004]	-5.63e-06 [.00005]	.00021 [.00012]
% Change	0.387	1.615	0.129	-0.194	0.710	-0.036	1.356
Observations	7928	7928	7928	7928	7928	7928	7928

Notes: Standard errors clustered by cohort-eligibility are shown in brackets. All models include cohort dummies and are estimated by OLS. % change is calculated as 100*Wald Estimate/mean crime rate of draft-ineligible men. *Significant at the 10% level; **Significant at the 5% level; ***Significant at the 1% level.

Table 5. Estimates of the impact of conscription on job performance and mortality and migration rates

Cohorts	1958-65	1958-65	1958-65	1958-65
	(1) Dependent Variable: Participation in the Formal Job Market	(2) Dependent Variable: Unemployment	(3) Dependent Variable: Income	(4) Dependent Variable: Mortality + Migration
Draft Eligible (=1)	-.00182* [.00096]	.00044*** [.00013]	-.01744*** [.00320]	-.00171 [.00153]
% Change	-0.80	0.30	-0.84	-0.22
Observations	7928	7928	7928	7928

Notes: Standard errors clustered by cohort-eligibility are shown in brackets. All models include cohort dummies and are estimated by OLS. % change is calculated as 100*Wald Estimate/mean dependent variable of draft-ineligible men. *Significant at the 10% level; ***Significant at the 1% level.