Conscription and Crime*

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Abstract

The initiation in criminal activities is, typically, a young phenomenon. The study of the determinants of entry into criminal activities should pay attention to major events affecting the youth. In many countries, one of these important events is the mandatory participation in the military service. The objective of this study is to estimate the causal relationship between mandatory participation in the military service and crime. We exploit the random assignment through a draft lottery of young men to conscription in Argentina to identify this causal effect. Our results suggest that participation in the military service increased the likelihood of developing a criminal record in adulthood, particularly for property and arm-related crimes.

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I. Introduction

The initiation in criminal activities is, typically, a young phenomenon. Most criminals begin their participation in illegal activities as juvenile or young adult offenders. The study of the determinants of entry into criminal activities should pay particular attention to major events affecting the youth. In many countries one of these important events is the mandatory participation in the military service. In particular, a hypothesis that is worth studying is whether participation in the military service affects negatively or positively young men's propensities toward violent and criminal behavior.

The objective of this study is to estimate the causal relationship between mandatory participation in the military service (also called conscription) and crime. Military service and military training could affect young men's involvement into violent and criminal behavior through a variety of mechanisms: (i) military training teaches young men obedience and discipline, which could directly affect their rates of criminality; (ii) military service can affect the labor market prospects of young men positively or negatively. By delaying the insertion of young men into the labor market, it might affect future labor market prospects negatively, increasing their likelihood of committing property crimes. On the other hand, by improving their health and nutrition and also by extending their social network, it might affect labor market prospects positively (preventing them from committing property crimes); (iii) military training might permanently break down the mind's natural barriers to committing violent acts (Grossman, 2000; Grossman and Siddle, 2000). Furthermore, individuals who are trained in the use of weapons may respond aggressively or violently to conflict or provocation outside of the military environment; (iv) military service provides firearm training that can reduce the entry costs into crime, increasing the participation in arm-related crimes; and (v) military service incapacitates the commission of crime for long periods of time by keeping young men in military facilities and out of the streets.

In order to identify a causal relationship between conscription and crime, we need to identify a variable that affects participation in the military service but does not affect crime through other mechanisms. To solve this problem we take advantage of the Argentine conscription lottery, which randomly assigned eligibility of young males to provision of military service based on the last three numbers of their national ID. Hence, for reasons totally unrelated to their underlying levels of aggression or criminality some men were selected for conscription service whereas others were not. We then analyze the

causal effect of this randomly assigned eligibility variable on the likelihood of having a criminal record.

Previous studies have exploited the random assignment by draft lotteries to provision of military service in war times. The evidence shows that being drafted into the military can actually hurt future earnings. Angrist (1990) uses the Vietnam-era draft lottery to show that military service in the Vietnam era reduced the civilian earnings of white veterans in the US, and probably had no effect on nonwhite veterans. Angrist and Krueger (1994) used a similar strategy to show that even though simple comparisons suggest that World War II veterans earn more than non-veterans of same age, the causal effect of military service in World War II is probable negative. [1,2]

The impact of being a Vietnam War veteran on criminal and violent behavior has been analyzed by Rholfs (2005), who finds that men who were draft age in the Vietnam era are more likely than average to report having committed sexual assaults. The effect of the draft lottery on incarcerations, however, is fairly small. In order to reconcile these insignificant results for the incarceration rates with the results for self-reported sexual assault, he speculates that the military service could have made veterans more honest self-reporters, or that the reported sexual assaults could have been committed in Vietnam. [3]

Most of the subjects covered on these studies on the Vietnam draft were exposed to real combat. Instead, we focus on subjects that were drafted for military service in peace times (with the exception of two cohorts during the Malvinas war).

Section II describes the data and Section III presents our econometric methods. The results are reported in Section IV, while Section V concludes.

II. Data

[1] The natural experiment generated by the Vietnam draft lottery has been also exploited to analyze the impact of military service on alcohol consumption (Goldberg et al., 1991), and mortality (Hearst, Newman, and Hulley, 1986).

[2] At the macro level, Keller, Poutvaara, and Wagener (2006) find a negative effect of mandatory military service on countries' economic performance.

[3] Among Vietnam veterans, Yager, Laufer, and Gallops (1984) find positive correlations between combat exposure and arrests and convictions, especially for non-violent crimes. Mumola (2000) reports that veterans are no more likely to be in prison than are non-veterans of the same age and that veterans are more likely than non-veterans to be in prison for a violent offense. Bouffard (2003), in turn, finds that serving in the military does not appear to influence later criminal behavior.

From 1901 through 1995 military service in Argentina was mandatory. Initially, young males 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.

Each year a lottery system assigned a number between 1 and 1000 to each of the last three numbers of "candidate's" national IDs. 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. Final selection of individuals for military conscription from the draft-eligible was based on the pre-induction physical examination and the examination for mental aptitude.[4] 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 the duration was one year in the Army and the Air Force.

Exploiting this random assignment, we will try to answer whether conscription incentives or disincentives involvement in criminal activities. To answer this question we use individual-level datasets on men that went through the criminal justice process. We have two datasets provided by the Justice Ministry. One dataset has information of all men that went through criminal justice process since 1934 (about one million observations). This database includes information on the last three ID number and the year of birth, but it does not specify the type of crime.[5] The other dataset covers a shorter period of time, but it details the type of crime that originated the criminal process. This second database has detailed information on all men that went through the criminal justice process since 2000 (about a quarter of one million observations), and includes the last three ID numbers, year of birth, and type of crime.

We have information on cutoff numbers for cohorts of 1927 to 1975.[6] For cohorts of 1927 to 1929, 1931 to 1933, 1935 to 1936, 1938, and 1941 the cutoff number was equal to zero. For cohorts of 1966 to 1975 the cutoff number was different by military districts

[4] 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. In our case, although some upper class youth could have used influences to avoid, shorten, or ameliorate their conscription time, this was basically not an option for low class youth, the ones more prone to involvement in criminal activities.

[5] The complete ID number was not provided for confidentiality reasons.

[6] The cohort of 1976 faced the conscription draft lottery but it was not incorporated.

(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 (a difference of more than 600 numbers between the maximum cutoff and the minimum) we exclude the cohorts of 1966 to 1975 from our sample. 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 maximum and minimum cutoff numbers was small. We include these cohorts in our sample, but in order to avoid measurement error, our regressions exclude all ID numbers with lottery numbers in between the maximum and the minimum cutoff, reducing in this way the number of observations available for cohorts of 1955 and 1965.

III. Econometric Methods

In our case, estimation of average intention-to-treatment effects is straightforward. First, we define the dummy variable Low Number, which varies by ID number and cohort of birth. Low Number takes the value of one if the lottery number randomly assigned to ID i in cohort c was not draft eligible, and zero otherwise. Thus, this variable identifies the intention-to-treat on the population and is randomly assigned. We then estimate the intention-to-treat causal effect of military service on crime by estimating the following regression model:

$$\text{Crime Rate}_{ci} = a + g\ \text{Low Number}_{ci} + d_c + e_{ci}$$

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 total size of cohort c and ID i), d_c is a cohort effect, g estimates the intention-to-treat (ITT) effect, and e_{ci} is the error term.

The coefficient of interest from an IV regression (the Local Average Treatment Effect, LATE) can then be recuperated from the reduced-form equation presented above as:

$$LATE = \frac{ITT}{(p_1 - p_2)}$$

where p_1 is the probability of doing the military service given an ID number that was made eligible for military service by the lottery, and p_2 is the probability of doing the

military service given an ID number that was not made eligible for military service by the lottery (volunteers into the military service).

According to information provided by the Argentine Army,[7] the number of volunteers into the military service was basically nil during the sample period $(p_2 \cong 0)$, so the denominator in the above expression 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.

IV. Results

Our results suggest that conscription is likely to increase crime rates. In Table 1, using the dataset that includes all men that went through criminal justice process since 1934, we consistently find a reduction in crime rates on those ID numbers that were not 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%.[8] 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, and it is not significant for the earlier period. However, this 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 and these changes would be correlated with the cohorts.[9]

In Table 2 we run two false experiments to guarantee that the lottery was truly random and that we are not picking anything else in our estimates. In model (1) the sample is restricted to those observations with low number. We sort the low numbers for each cohort and divide them by their median, assigning a false treatment to the upper half of numbers. We find no difference in crime rates between these groups as one would expect, since none of them were draft eligible. In model (2) the sample is restricted to

[7] Oficina de Reclutamiento y Movilización, Estado Mayor del Ejército Argentino.
[8] We also add ID fixed effects to the model in column (1) to check the robustness of this result to a possible non-random assignment of ID numbers in the population. Results are exactly as the model in column (1), indicating that there is no evidence of this possible source of confounding in our data. All regressions mentioned but not shown are available from the authors upon request.
[9] 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.

cohorts 1956 and 1957 (which fully skipped military service because of the change in the age of incorporation from 21 years to 18 years), using Low Numbers 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.

In Table 3, we explore some differential effects. In column (1) we show that the effect of conscription on crime seems to have been homogenous for draftees providing military service during democratic and dictatorial governments. In columns (2) and (4) we show that the effect of military service on crime is larger for those draftees in the cohort that participated in the Malvinas War. Finally, we find some evidence that the effect of conscription on crime was also 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.

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 or the natural barriers to committing violent acts. It may also be the case that military service delayed the insertion of the young into the labor market affecting future opportunities. This last interpretation is consistent wit the additional deleterious effect observed for those that provided service in the Navy for two years.

To try to shed additional light on these hypotheses 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 men that went through the criminal justice process since 1934, a newer database has information on all men that went through the criminal justice process since 2000, but which details the type of crime under process.

A potential drawback of the alternative database is that the type of crime is actually specified for 37.4% of the cases. Therefore, in order to check its validity in Table 4 we explore a number of exercises. First we reproduce the results from Table 1 including all observations. For all the time periods, the coefficients on Low Number 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 4, for all the

time periods the estimated coefficients on Low Number when we include those observations where the type of crime is specified are not significantly different from coefficients obtained when the sample includes those observations where the type of crime is not specified. This result suggests that Low Number is not correlated with missing values in the database.

In Table 5, we estimate the effect of military service by type of crime for cohorts of 1958 to 1965. Remarkably, the coefficient on Low Number in column (7) is negative and significant at the 1% level suggesting an impact of military service on arm-related crime rates. This result is in line 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 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 to Low Number in the regression on property crime presented in columns (1) is negative and significant, a result that is in line with this hypothesis.

In column (1) of Table 6 we present results of the impact of conscription on participation in the formal job market. The coefficient of Low Number in this specification is positive and significant at the 10% level, also suggesting that military service has a negative impact on the probability of participating in the formal job market. The result in column (2) suggests that those men serving in the military service have a higher unemployment rate than those men not serving in the military service. We also explore the impact of the military service on income. As shown in column (3) of Table 6, serving in the military service has a negative impact on future earnings. Finally, we explore whether the estimated effects could be the result of different migration or mortality rates affecting those that provided military service. Using the national ballot registry (voting is mandatory in Argentina), column (4) of Table 6 shows that conscription does not affect the probability of being alive and living in Argentina.

V. Conclusions

The objective of this study is to estimate the causal relationship between mandatory participation in the military service and crime. A priori, different hypotheses could predict a positive or negative effect of military service on the involvement into criminal behavior. We exploit the random assignment through a draft lottery of young men to conscription in Argentine to identify this causal effect. Our results suggest that participation in the military service increased the likelihood of developing a criminal record in adulthood.

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 have reduced the entry costs into crime or the natural barriers to committing violent acts. Moreover, the significant effect of military service on crimes against the property suggests that military service may have delayed the insertion of the young into the labor market affecting their future opportunities. This hypothesis is consistent with our findings that military service has detrimental effects on future job market performance and earnings.

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Table 1. Estimates of the impact of conscription on crime

	Dependent variable: Crime Rate		
	(1)	(2)	(3)
Low Number (=1)	-.00063***	-.00025	-.00114***
	[.00020]	[.00023]	[.00031]
LATE	-0.0010	-0.0004	-0.0017
% ? LATE	-1.616	-0.712	-2.580
Cohorts	1929-65	1929-55	1958-65
Observations	34904	26976	7928

Notes: Standard errors clustered by cohort-Low Number are shown in brackets. All models include cohort dummies and are estimated by OLS. LATE is the Local Average Treatment Effect and it is estimated as the ratio of the coefficient on the Low Number variable to the proportion of men with High Number that were incorporated into the military service. % ? LATE is calculated as 100*LATE/mean crime rate of Low Number IDs. *Significant at the 10% level; **Significant at the 5% level; ***Significant at the 1% level.

Table 2. Estimates of the impact of conscription on crime– False experiments

	Dependent variable: Crime Rate	
	(1)	(2)
Very Low Number	.00007	
	(.00046)	
Low Number		-.00055
		(.00097)
Observations	5485	2000

Notes: Huber-White robust standard errors are shown in parentheses. All models include cohort dummies and are estimated by OLS. In model (1) the sample is restricted to those observations with low number. In model (2) the sample is restricted to cohorts 1956 and 1957, using Low Numbers corresponding to cohorts of 1958 and 1959. *Significant at the 10% level; **Significant at the 5% level; ***Significant at the 1% level.

Table 3. Additional estimates of the impact of conscription on crime

	Dependent variable: Crime Rate				
	(1)	(2)	(3)	(4)	(5)
Low Number (=1)	-.00073**	-.00047**	-.00054**	-.00085**	-.00096**
	[.00037]	[.00018]	[.00020]	[.00036]	[.00037]
Malvinas*Low Number		-.0015***		-.0011**	
		[.00036]		[.00049]	
Militar Government*Low Number	-.00046				
	[.00034]				
21 years old*Low Number	-.00072*				
	[.00038]				
Navy (=1)			.00063*		.00107
			[.00037]		[.00087]
Cohorts	1929-65	1929-65	1929-65	1958-65	1958-65
Observations	34904	34904	34904	7928	7928

Notes: Clustered standard errors are shown in brackets. Standard errors in models (1), (2), and (4) are clustered by cohort-Low Number and standard errors in models (3) and (5) are clustered by cohort-Low Number-Navy. 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 – Alternative database

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

Notes: Standard errors clustered by cohort-Low Number 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 5. Estimates of the impact of conscription on crime rates, by type of crime

	Dependent Variable: Crime Rate						
	(1) Against Property	(2) Sexual Attack	(3) Murder	(4) Threat	(5) Drugs	(6) White Glove	(7) Arms
Low Number (=1)	-.00025*	-.00002	.00003	-.00011**	5.63e-06	-.00021	-.00006***
	[.00014]	[.00004]	[.00004]	[.00004]	[.00005]	[.00012]	[.00002]
LATE	-.00038	-.00003	.00005	-.00017	.00001	-.00032	-.00009
% ? LATE	-1.615	-0.129	0.194	-0.710	0.036	-1.356	-0.387
Cohorts	1958-65	1958-65	1958-65	1958-65	1958-65	1958-65	1958-65
Observations	7928	7928	7928	7928	7928	7928	7928

Notes: Standard errors clustered by cohort-Low Number are shown in brackets. All models include cohort dummies and are estimated by OLS. LATE is the Local Average Treatment Effect and it is estimated as the ratio of the coefficient on the Low Number variable to the proportion of men with High Number that were incorporated into the military service. % ? LATE is calculated as 100*LATE/mean crime rate of Low Number IDs. *Significant at the 10% level; **Significant at the 5% level; ***Significant at the 1% level.

Table 6. Estimates of the impact of conscription on job market participation, unemployment, income, and mortality and migration rates

	Dependent Variable:			
	(1) Participation in the Formal Job Market	(2) Unemployment	(3) Income	(4) Registration for National Election
Low Number (=1)	.00182* [.00096]	-.00044*** [.00013]	.01744*** [.00320]	.39361 [.33672]
LATE	.00278	-.00067	.02660	.60034
% ? LATE	0.81	-0.31	0.84	0.23
Cohorts	1958-65	1958-65	1958-65	1958-65
Observations	7928	7928	7928	7928

Notes: Standard errors clustered by cohort-Low Number 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.