

Dishonesty and Public Employment[†]

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We exploit a natural experiment to study the causal link between dishonest behavior and public employment. When military conscription was mandatory in Argentina, eligibility was determined by both a lottery and a medical examination. To avoid conscription, individuals at risk of being drafted had strong incentives to cheat in their medical examination. These incentives varied with the lottery number. Exploiting this exogenous variation, we first present evidence of cheating in medical examinations. We then show that individuals with a higher probability of having cheated in health checks exhibit a higher propensity to occupy nonmeritocratic public sector jobs later in life. (JEL D91, J45, K42, O15)

We provide evidence on the causal relationship between dishonest behavior and public employment. Our empirical strategy exploits a natural experiment (the draft lottery in Argentina) that provides exogenous variation in the incentives to engage in dishonest behavior, in a real-world context, and with high-stake consequences. In particular, we study the causal link between draft evasion in early adulthood and later selection into public sector jobs.

Military conscription in Argentina was mandatory for almost all of the twentieth century. Eligibility for military conscription was determined by a public lottery based on the last three digits of citizens' national IDs and by a medical examination. Following the lottery, all males were called to have a medical examination. Later on, the government set a cutoff number. Individuals whose ID number had been assigned a lottery number higher than the cutoff and who had passed the medical exam were mandatorily called to military conscription, and those below or at the cutoff or who had failed the examination were exempt. The high-stake nature of the medical examination outcome (avoiding unpaid military service) created strong incentives to cheat. At the time of this examination, individuals did not know the exact cutoff number that would apply to their cohort but could have expectations based, mainly, on previous years' cutoffs. Thus, individuals with draft numbers far

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below the expected cutoff had weaker incentives to cheat than those closer to and above the expected cutoff. In this way, the lottery induced exogenous variation in the incentives to falsify health conditions.

Using individual data for the universe of male Argentines born between 1958 and 1962 (more than one million men), we first report evidence of cheating in the pattern of exemptions for medical reasons. The rate of failure in the medical examination significantly rises as the lottery results increase from the lowest numbers to the proximity of the cutoff. We then exploit this variation to show that individuals who randomly faced stronger incentives to cheat in their conscription health checks also had a higher propensity to become public employees later in life.

We provide a series of further analyses that reinforce the causal interpretation of our results. First, we asked specialized physicians to distinguish between easy-to-cheat and hard-to-cheat conditions from the list of reasons for failing medical exams. Easy-to-cheat conditions were easier to fake or exaggerate for the exam and/or harder or costlier to verify than hard-to-cheat conditions. Strategic behavior should be mostly observed in easy-to-cheat conditions, and this is exactly what we find. Second, for women in the same 1958–1962 cohorts, we impute the draft lottery results to their ID numbers. Since women were not drafted for conscription, there should be no relationship between ID numbers and public employment, and this is indeed what we find. Third, we find no relationship between the random distance to the cutoff and public employment among eligible men (i.e., those above the cutoff). This is as expected since all men in this group faced similar incentives to cheat. Fourth, we perform a placebo experiment to challenge the validity of the exclusion restriction exploiting the fact that the cohort of 1976 faced the draft lottery but was not called for the medical examination (nor drafted) because compulsory conscription was abolished. The absence of effects in this placebo exercise suggests that the draft lottery results had no impact on employment outcomes through mechanisms other than cheating in the medical examination.

We explore possible underlying channels for our findings. First, we show that draft evasion has no effect on having a formal job in the private sector. This finding is congruent with a self-reputation channel in which early actions that weaken moral self-restraints can make future temptation opportunities more desirable, as shown in the theoretical models by Bénabou and Tirole (2004, 2011) and Dal Bó and Treviö (2013). These habit models provide a conceptual framework that helps to interpret our findings. Public sector jobs, with life stability, low effort provision, absenteeism, and corruption possibilities, could be examples of high-temptation opportunities when compared to private sector occupations. Engaging in dishonest behavior during the formative years (i.e., cheating in the draft medical examination) may have weakened these youth moral self-restraints, decreasing the reputational cost of future misbehavior and affecting future career paths. Our empirical evidence is consistent with these theoretical results.

Second, we report that draft evasion is related to future nonmeritocratic public employment, where there is more scope for arbitrary hiring, but not to meritocratic public jobs. This result is compatible with a learning channel in which the successful experience of cheating in a high-stake situation during the formative years familiarized these youths with the potential use of family contacts, influences, and

monetary resources for dishonest behavior. These individuals may have then used similar tools to get coveted nonmeritocratic public sector jobs, which in Argentina are typically accessed through personal connections.

While suggestive, these additional results are not conclusive on the underlying mechanisms behind our results. The finding that dishonest behavior among young Argentine males positively predicts future public employment could also be due to other, perhaps complementary, mechanisms. For instance, some personal characteristics (such as pacifism, guilt, or stress) could become exacerbated by cheating the draft, and this may in turn affect the probability of working in the public sector.

Our paper relates to several strands of literature. There is growing evidence that individuals differ in their propensity to engage in dishonest behavior (Fischbacher and Föllmi-Heusi 2013; Arbel et al. 2014; Abeler, Nosenzo, and Raymond 2019). In addition, recent empirical studies find that dishonesty is not an entirely congenital, time-invariant trait but rather that individuals respond to incentives, institutions, and cultural norms (see Fisman and Miguel 2007; Dahl, Kostøl, and Mogstad 2014; Lowes et al. 2017; and Ajzenman 2021). In line with this literature, our evidence suggests that cheating can be induced and learned, but, in a forward step, we also explore the causal effect of dishonesty on future behavior.

Our paper is also related to lab experiments that study the correlation between individual dishonesty and revealed preferences for working in the public sector (Hanna and Wang 2017; Banerjee, Baul, and Rosenblat 2015; Barfort et al. 2019). Our contribution is to show a causal link between individual dishonesty and the probability of actually working in the public sector by exploiting a natural experiment with high-stake consequences.

Given the importance of bureaucracy quality for the proper functioning of modern states, our analysis also ties in with the literature on personnel economics of the state (see Finan, Olken, and Pande 2017) and the use of connections to access public employment (Colonnelli, Prem, and Teso 2020; Brassiolo, Estrada, and Fajardo 2020).

Finally, our paper contributes to the literature that looks at the long-term impact of events that occur during early adulthood (Angrist 1990; Angrist and Chen 2011; Galiani, Rossi, and Schargrodsky 2011; Cantoni et al. 2017; Roth and Wohlfart 2018) by suggesting that the experience of dishonest behavior during the “impressionable” years can have long-lasting consequences.

I. Background: Military Conscription and Public Employment in Argentina

Military Conscription in Argentina.—Masculine military conscription in Argentina was mandatory from 1901 to 1994. Our analysis focuses on five cohorts born between 1958 and 1962, for which we have individual data on conscription status and on medical examination results.

The eligibility of young males for conscription was determined through a lottery and based on the last three digits of their national IDs, a unique lifelong number assigned to every citizen at age 16 for the cohorts in our study. Around April of each year, the National Lottery organized a public session supervised by the National General Notary for all the males turning 18 years old in that year. The results were widely disseminated through live radio broadcasting and printed newspapers. In this

lottery, balls numbered 1 to 1,000 were blindly extracted from a drum. The first ball released from the drum corresponded to the last three ID digits 001, the second to 002, and so forth.

After the lottery, and irrespective of the assigned number, all men in the cohort were summoned to a compulsory medical examination in military premises, which covered both mental and physical status. Individuals were called for the health revision by the order of their national ID, not by the lottery number.

After the medical examinations, a cutoff number was determined. Individuals assigned a lottery number below or at the cutoff were deemed exempt, and those with an assigned lottery number above the cutoff (and who had passed the medical examination) were mandatorily drafted to military conscription.¹ Among eligible individuals, those with the lowest lottery numbers were assigned to the army, the intermediate numbers to the air force, and the highest numbers to the navy. Under a gradual trend of reduction in the use of conscripts since the middle of the twentieth century (see Galiani, Rossi, and Schargrodsky 2011), the determination of the cutoffs for each cohort depended on the overall political, budgetary, and national defense situation. At each force, conscription began with an additional medical examination at the time of incorporation (around February of the year after the lottery), followed by a three-month military training period and a final assignment to a specific military unit.²

Providing up to two years of unpaid military service and potentially delaying studies and labor market insertion represented a significant load for young males. Galiani, Rossi, and Schargrodsky (2011) find that conscription in Argentina had detrimental effects on labor and crime outcomes. These high costs of conscription created strong incentives to obtain spurious exemptions by faking physical or mental impediments during the medical examination, by forging medical studies, and by using family and personal connections, exchanges of favors, and bribes (Gayol and Kessler 2018).³ Premeditated strategies, some of them requiring weeks of preparation, included the faking or exaggeration of psychological, sight, or hearing conditions; the gain or loss of weight before the exam for people close to critical body mass limits; the use of products to trigger respiratory or dermatological reactions; and the deliberated delay of medical treatments before the exam, *inter alia* (Cantilo 2000; Garaño 2010).

Public Employment in Argentina.—Argentina is a federal country comprised of 23 provinces and one autonomous capital city. It has a comparatively high level of public employment, stemming from three levels of government (national, provincial,

¹Exemption to service was granted to clerics, to individuals providing family support or having a younger brother in the same cohort, and to graduates from the armed forces' secondary schools (*liceos*). Deferment to finish high school or college was granted for a maximum of ten years. Deferment was also granted without a particular reason for up to two years. In these deferral cases, the lottery numbers and cutoffs used to establish eligibility were those of the individual's birth cohort.

²For more details on military conscription in Argentina, see Rodríguez Molas (1983) and Galiani, Rossi, and Schargrodsky (2011).

³Cheating was potentially risky. The military service law typified the crime of undue exemptions. In 1993, military personnel were brought to court for declaring draftees physically unfit in exchange for bribes (*Clarín*, "Detectan más casos de coimas para evitar el servicio militar," October 17, 1993, 41).

and municipal) as well as from three branches of power (executive, legislative, and judiciary).

Public employees in Argentina enjoy attractive labor conditions such as weak absenteeism punishment, low effort requirements, and life stability (see Oliveros 2021a; Cabot 2021). Although there are some pockets of relatively high quality workforce (most notably, in decentralized bodies), with recruitment processes and promotions depending on entry examinations, diplomas, training, and evaluations, the norm for public employment throughout the twentieth century has been clientelism, undue influence, and nepotism (O'Donnell 1988; Oliveros 2021b). This is particularly true for public employment in the general administration, at subnational levels, and in less specialized positions, which have been traditionally accessed through family, personal, and clientelistic connections. Overall, Argentina's bureaucracy is of relatively low quality, especially given the country's level of development and stock of human capital (Bambaci, Spiller, and Tommasi 2007; Stein et al. 2008). Moreover, the country shows relatively high corruption levels according to international rankings.⁴

II. Data and Identification Strategy

Population and Draft Lottery.—We rely on population data provided by the Argentine Army on all male citizens born between 1958 and 1962 who were alive at age 18. The total number of men in these five cohorts is 1,088,114. Online Appendix Table A1 presents the population size and lottery cutoff numbers for each cohort. Our dataset includes individual data on draft lottery results, conscription status, and a set of pretreatment characteristics that includes origin (Argentine-born nonindigenous, Argentine-born indigenous, and foreign-born naturalized citizens) and region of residence at age 16 (one year before the draft lottery). Using individual lottery results and cohort cutoff numbers, we define *DistanceToCutoff* as (the absolute value of) the difference between each individual's lottery number and the conscription cutoff for his cohort. We also define the dummy variables *DraftExempt*, which takes the value of 1 for men whose lottery number was below or at the cutoff and therefore were not eligible to serve and 0 for those whose lottery number was above the cutoff and were thus eligible, and *DraftEligible*, which is the complement of *DraftExempt*. In our dataset, 29.9 percent of individuals were draft exempt (70.1 percent eligible) and 13.61 percent overall failed the medical examination (online Appendix Table A2 presents summary statistics of our main variables).

Medical Examination.—Our dataset indicates whether an individual failed the medical examinations (without distinguishing if this occurred at the main or preincorporation exam). With this information, we define the dummy variable *FailedMedicalExamination*. In these failure cases, the database also provides the reason for medical exclusion from a coded list of 506 “conditions.”⁵

⁴In the 2022 Corruption Perceptions Index by Transparency International, Argentina gets 38 points (below the world mean and median levels, with no significant changes during the previous decade) on a 0-to-100 scale from highly corrupt to very clean.

⁵The armed forces' records from this period referred to these exemption conditions as “diseases,” “disorders,” or “pathologies.” We deliberately avoid using this terminology, and we instead refer to “conditions” that were

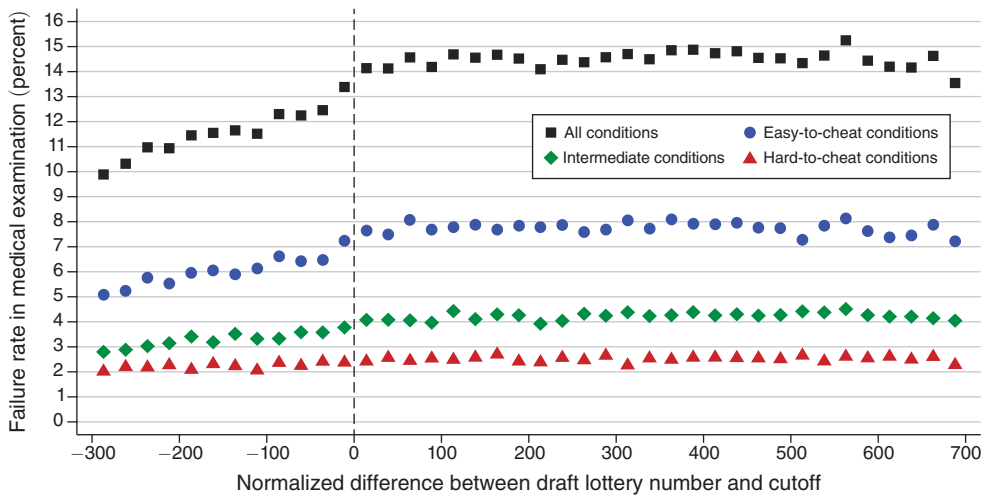


FIGURE 1. FAILURE RATE IN MEDICAL EXAMINATION AS A FUNCTION OF DRAFT LOTTERY NUMBER

Notes: The number of observations is 1,088,114 (325,298 draft exempt in the range $(-300;0]$ and 762,816 draft eligible in the range $(0;700]$) and corresponds to men in the 1958–1962 cohorts. The figure depicts a binned scatter-plot of the failure rate in medical examination by 40 quantiles of the normalized difference between the draft lottery number and the year's eligibility cutoff for all conditions that distinguishes between easy-to-cheat, intermediate, and hard-to-cheat conditions. The average cutoff for the five cohorts was 300, so for each cohort the distance below the cutoff (i.e., the exempt) was normalized as $(-300;0]$ and as $(0;700]$ for the distance above the cutoff (i.e., for those eligible). All plots include cohort fixed effects and a control for air force draftees, who were subject to a more stringent medical examination at the time of incorporation as conscripts had to be fit to participate in flights. Online Appendix Figure A2 presents the results for all conditions without this air force control. The plot was made using the binsreg package by Cattaneo et al. (2019).

In the absence of strategic behavior, we would expect the proportion of individuals failing the medical examination to be the same for the draft eligible and the draft exempt. As lottery numbers are random, they should be uncorrelated with true underlying medical conditions. However, failure rates are, on average, 3.3 percentage points (or 29.2 percent) higher for the draft eligible than for the draft exempt (see online Appendix Table A3). The differences are significant for every cohort. Moreover, and most importantly for our identification strategy, Figure 1 (which presents failure rates as a function of cohort-normalized distance to the average cutoff of 300) shows that the failure rate increases with the lottery numbers from about 9.7 percent at the origin up to 13.8 percent near the cutoff (from below). Above the cutoff, the failure rate remains relatively flat at an average of 14.6 percent.^{6,7} These discrepancies in failure rates are consistent with the different incentives to cheat in the medical examination by those at risk of becoming draft eligible. Individuals with

considered motives for exemption from military service in Argentina at that time, some of which were certainly not pathological nor disabling (e.g., sexual orientation).

⁶Part of the increase in failure rates at the cutoff reflects the fact that some drafted individuals did not pass the second preinduction medical examination (the draft exempt never reached that stage).

⁷The increase in failure rates as a function of lottery numbers in Figure 1 is reflected in a strongly significant regression coefficient of 0.01 below the threshold, whereas it becomes a nonstatistically significant coefficient of 0.0001 above the threshold. We can reject the equality of these coefficients at less than 1 percent significance.

draft numbers far below the cutoff had fewer incentives to cheat than those closer to and above the cutoff.

At the time of the first examination, individuals did not know the exact cutoff number that would apply to their cohort. However, the previous year's cutoff (as well as political, budgetary, and national defense factors that affected each cohort's intake) provided a natural reference to that year's actual cutoff.⁸ Online Appendix Figure A1 presents failure rates by draft lottery number for each cohort separately, including both the actual and previous year's cutoffs. For three out of the five cohorts considered, the cutoff differed from the previous year's cutoff only by 30 units or less. One exception is the cohort of 1958, for which the previous year's cutoff is quite low (at 24 over 1,000).⁹ This suggests that cheating incentives could start from quite low numbers, as the slope of Figure 1 suggests. Moreover, the uncertainty about the exact cutoff also implies a potential positive slope for a range of lottery numbers above it.

The findings that failure rates are higher for the draft eligible than for the draft exempt and that failure rates increase within the draft-exempt group as the lottery number gets closer to the cutoff suggest that the medical examinations were manipulated. Indeed, the failure rate was 9.7 percent for the lowest ten draft numbers (i.e., close to the intercept in Figure 1 if we draw a regression line around the points to the left of the cutoff), who were virtually certain of not being drafted. This level is probably a proxy of the real underlying rate of exempt-worthy conditions. In turn, the average failure rate for those above the cutoff was 14.6 percent, which suggests that about one-third of those who failed the medical examination did not have a true underlying condition but were cheating.¹⁰

Identification Strategy.—Whereas the draft lottery literature (i.e., Angrist 1990) typically instruments conscription with the lottery and studies the causal effect of conscription itself on a range of outcomes, our identification strategy instead exploits the increasing incentives to cheat in medical examinations induced by lottery numbers for (eventually) draft-exempt individuals (the positive slope to the left of the cutoff in Figure 1). In particular, we use the distance to the cutoff (from below) as an instrument for whether these individuals cheated in the medical examination, which allows us to estimate the causal impact of draft evasion on subsequent outcomes in a regression framework. Online Appendix Table A4 shows that our instrument is orthogonal to the limited set of available individual characteristics.

We can also provide some additional evidence on the plausibility of this instrument. We asked physicians from the Ministries of Defense and Security of

⁸The five cohorts we consider were conscripted under the 1976–1983 military dictatorship. Although an increase in the intake of conscripts could be expected, the military government aimed to reduce public spending.

⁹Although inevitably noisier, the overall pattern of the plots in online Appendix Figure A1 is similar to that of Figure 1. For the two cohorts for which the previous year's cutoff is substantially below the actual cutoff (1958 and 1959), the slope gets milder to the right of the previous year's cutoff, but it is clearly positive up to the actual cutoff. For the five cohorts separately, and for both the actual and previous year's cutoff, the below-the-cutoff slope coefficients are always positive and highly significant.

¹⁰Conscription in the army and the air force lasted one year, whereas draftees spent up to two years in the navy. We could thus expect an additional cheating incentive to avoid navy conscription. However, considering both the actual and previous year's interforce cutoffs, we do not observe a pattern of higher failure rates for those assigned to the navy, suggesting that the one-year conscription already provided enough incentives for those willing to cheat.

Argentina, as well as occupational physicians in charge of monitoring the medical conditions of employees who request paid medical leave from their employers, to classify the 506 medical exemption conditions into three groups: conditions that were more difficult/costly to verify with the technologies of the time and/or those for which it was easier to delay treatment or to exaggerate the symptoms for the exam (for example, psychological conditions, visual deficiencies, hearing loss, and breathing problems); conditions that were hard to fake and/or easy to verify (such as amputations, severe oligophrenia, spina bifida, tuberculosis, or poliomyelitis); and a residual intermediate group.¹¹ In our sample of draft-exempt individuals, easy-to-cheat and hard-to-cheat conditions represent 53.2 percent and 17.8 percent of the failed medical exams, respectively.

Figure 1 also depicts medical examination failure rates due to hard-to-cheat, intermediate, and easy-to-cheat conditions. Inspection of the figure indicates that about 4.8 percent of draft-exempt individuals showed these easy-to-cheat conditions for lottery numbers close to zero. This rate rises steeply to 7.6 percent close to the cutoff and then remains flat around 7.8 percent to the right of the cutoff. Intermediate conditions show a milder slope below the cutoff and, again, remain nearly constant above it. Instead, hard-to-cheat conditions remain almost flat throughout the whole range.¹² This evidence is compatible with our identification strategy: we observe the increase in medical examination failures when approaching the lottery cutoff for conditions that were more pliable to manipulation (and a steeper slope as faking was easier), whereas this pattern is significantly attenuated for conditions that were easily verifiable or difficult to fake. We interpret these slopes as differences in behavior since true medical conditions should be uncorrelated with random lottery results.

Employment Data.—For employment information, we rely on administrative data on social security records of wage earners provided by a credit-scoring agency. The source is the employer-employee matched database that covers the universe of registered wage earners in Argentina (for details, see Cruces, Rossi, and Schargrodsky 2023). Being in this dataset indicates that the individual was a formal wage earner at some point in 2010–2016. The dataset also distinguishes between public and private sector of employment and provides some limited information on the type of employment activity for public employees.

Our main outcome variable is whether an individual is a *PublicSectorEmployee*, which takes the value of 1 if the individual was a public employee at some point in the period 2010–2016 (16.74 percent) and 0 otherwise. The other outcome for wage employment in this database is being a *PrivateSectorEmployee* (36.68 percent).¹³

¹¹ Online Appendix 1 provides the list of the conditions included in these three subsets.

¹² We can reject the null hypotheses that the below-the-cutoff slope coefficients are equal between the three types of medical conditions and that they are equal to their corresponding (always nonsignificant) above-the-cutoff coefficients. Moreover, in a semielasticity model that takes into account the different prevalence of medical conditions, we cannot reject that the below-the-cutoff estimated rates of increase are similar for easy-to-cheat and intermediate conditions, but we strongly reject the equality of any of these two coefficients with that of hard-to-cheat conditions.

¹³ These percentages are in line with the proportion of formal wage earners in the male population in this age range in Argentina (see Gasparini and Tornarolli 2009). For example, in the EPH national household survey for the second semester of 2022, formal wage earners represent 46.5 percent of the total population of men aged 45–60.

Moreover, we classify public sector jobs into two categories. We label as *MeritocraticPublicSectorEmployee* those jobs that require some type of diploma (universities, scientific institutions, health sector, etc.) or entry examination (the judiciary) or those subject to some form of intensive training and evaluation (armed and security forces). These meritocratic positions represent 20.9 percent of overall public sector jobs in our sample, whereas the remaining 79.1 percent are labeled as *Non-MeritocraticPublicSectorEmployee*.¹⁴

Attrition.—The reasons for not appearing in this wage earner dataset include inactivity, unemployment, informal employment, self-employment, business ownership, international migration, or death. As the males in our conscription sample were born between 1958 and 1962, they were 48–58 years old when we observe their employment status in our data (from 2010 to 2016).

Since we are considering employment outcomes more than 30 years after the draft lottery, a natural concern is potential attrition. To measure attrition in our population, we rely on another database: the 2013 national electoral roll, around the midpoint of the 2010–2016 period for which we have employment data. Voting in Argentina is compulsory, and registration is automatic and based on the same individual national ID number used for the draft lottery. In our context, we define attrition as not being present in this official administrative dataset, which could be the result of either death or international migration.

Of the 1,088,114 individuals in the 1958–1962 cohorts that were alive at age 18 and were thus included in the draft, 101,225 (9.3 percent) are not present in the 2013 electoral roll. Online Appendix Table A5 shows that the probability of being alive and living in Argentina in 2013 is orthogonal to the distance to the lottery cutoff (our instrument). This result rules out potential concerns of differential attrition by lottery assignment. After removing attriters from the sample, we are left with 986,889 individuals for our analysis of employment outcomes.

III. Main Results, Falsification Tests, and Potential Mechanisms

We are interested in estimating the causal effect of cheating the medical examination in early adulthood on employment outcomes later in life. We start by estimating the following equation:

$$(1) \text{ EmploymentOutcome}_{ic} = \alpha + \beta \text{ FailedMedicalExamination}_{ic} + \delta_c + \varepsilon_{ic},$$

where i indexes individuals, c indexes cohorts, β is the coefficient of interest, δ_c is a cohort fixed effect, and ε_{ic} is an error term.

Our empirical strategy instruments *FailedMedicalExamination* with the randomly assigned *DistanceToCutoff* in order to estimate the causal effect of cheating on employment outcomes. The instrument is not valid for the draft-eligible group, as all these individuals faced similar cheating incentives regardless of their distance to the cutoff as pictured in Figure 1. Moreover, a sizeable fraction of the draft-eligible

¹⁴Online Appendix 2 provides additional details on the employment database and on the classification of meritocratic and nonmeritocratic public sector jobs.

TABLE 1—MEDICAL EXAMINATION, EMPLOYMENT, AND DISTANCE TO CUTOFF

	<i>Public Sector Employee</i> (1)	<i>Private Sector Employee</i> (2)	<i>Failed Medical Examination</i> (3)	<i>Public Sector Employee</i> (4)	<i>Public Sector Employee</i> (5)	<i>Failed Medical Examination</i> (6)	<i>Public Sector Employee</i> (7)	<i>Public Sector Employee</i> (8)
<i>FailedMedical Examination</i>	−1.62 (0.21)	−4.74 (0.27)			17.55 (7.84)			18.72 (7.90)
<i>DistanceToCutoff</i>			−0.0098 (0.0006)	−0.0017 (0.0008)				
<i>DistanceToPrevious Year'sCutoff</i>						−0.0107 (0.0007)	−0.0020 (0.0008)	
Estimation method	OLS	OLS	OLS (first stage)	OLS (reduced form)	2SLS	OLS (first stage)	OLS (reduced form)	2SLS
Dependent variable mean	16.58	37.09	10.99	16.58	16.58	10.99	16.58	16.58

Notes: Standard errors clustered at the last three digits of ID/cohort level are shown in parentheses. The 295,611 observations correspond to the draft-exempt men in the 1958–1962 cohorts present in the 2013 electoral roll. All models include cohort fixed effects. *DistanceToCutoff* is the absolute value of the difference between each individual's lottery number and the conscription cutoff for his cohort. *DistanceToPreviousYear'sCutoff* is the absolute value of the difference between each individual's lottery number and the conscription cutoff for his previous cohort (and it is set to 0 when draft-exempt individuals have lottery numbers above the previous year's cutoff but below their cohort's cutoff). *FailedMedicalExamination* is a dummy variable that equals 1 if the individual failed the conscription medical examinations and 0 otherwise. *PublicSectorEmployee* is a dummy variable that takes the value of 1 if the individual was a wage earner in the public sector at some point in the period 2010–2016 and 0 otherwise. *PrivateSectorEmployee* is a dummy variable that takes the value of 1 if the individual was a wage earner in the private sector at some point in the period 2010–2016 and 0 otherwise. Dummy variables are normalized to 0/100 so that results represent percentage points. In the two-stage least squares (2SLS) model in column 5, *FailedMedicalExamination* is instrumented with *DistanceToCutoff*—the first stage of this regression is presented in column 3. In the 2SLS model in column 8, *FailedMedicalExamination* is instrumented with *DistanceToPreviousYear'sCutoff*—the first stage of this regression is presented in column 6. The Montiel-Pflueger effective *F*-statistics for the first-stage regression are 268.25 for *DistanceToCutoff* and 267.32 for *DistanceToPreviousYear'sCutoff*, with a critical value of 37.4 for a 5 percent worst-case bias (Pflueger and Wang 2015; Montiel Olea and Pflueger 2013).

individuals ended up in the conscription, which, as mentioned, had direct effects on formal employment, earnings, and related outcomes. Thus, we focus the main analysis on the draft-exempt sample of 295,611 individuals.

Main Results.—In Table 1, we first present the simple OLS estimates of the model in equation (1) for the outcomes *PublicSectorEmployee* (column 1) and *PrivateSectorEmployee* (column 2). The results are as expected: since a majority of the individuals who failed their medical examination did it because of real health conditions, we find a robust negative relationship between employment and failure in the medical examination. For *PublicSectorEmployee*, the coefficient of −1.62 percentage points indicates a reduction of 9.77 percent with respect to the dependent variable mean. The correlation is even stronger for *PrivateSectorEmployee*. Individuals with some preexisting medical condition, as signaled by failing the conscription medical examination, exhibited lower formal employment levels later in life.¹⁵

As discussed previously, however, a fraction of failures in medical examinations can be attributed to some form of cheating. The evidence in columns 1 and 2 of

¹⁵ Online Appendix Table A6 shows similar results for the draft eligible and for the whole sample.

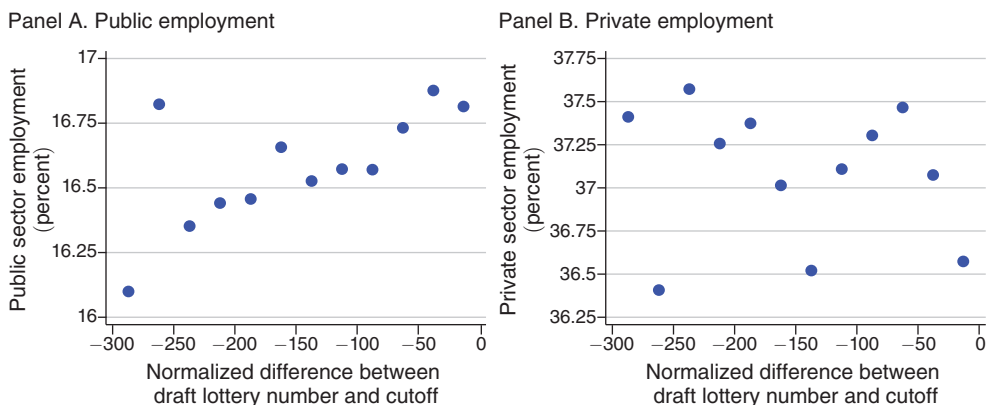


FIGURE 2. REDUCED FORM: PUBLIC AND PRIVATE EMPLOYMENT AS A FUNCTION OF DISTANCE TO THE CUTOFF

Notes: The 295,611 observations correspond to the draft-exempt men in the 1958–1962 cohorts present in the 2013 electoral roll. The figure depicts a binned scatterplot of public and private employment levels for 12 quantiles of the normalized difference between the draft lottery number and the year's eligibility cutoff. The average cutoff for the five cohorts was 300, so the distance below the cutoff (i.e., the exempt) was normalized for each cohort as $(-300; 0]$. The plot was made using the binsreg package by Cattaneo et al. (2019).

Table 1 does not allow us to separate the potential effect of cheating from that of real physical or mental conditions on employment outcomes. But even if we could separate the true and cheating components of *FailedMedicalExamination*, the cheating component is likely endogenous when we attempt to measure its effect on employment outcomes. For example, belonging to a middle- or upper-class family, or having relatives already working in the public sector, can affect the ability both to cheat in the medical examination (for instance, by using connections in the military or the connivance of physicians to be classified as physically unfit) and to find a future job in the public sector.

Thus, for the draft-exempt group we estimate equation (1) by 2SLS, using *DistanceToCutoff* as an instrument for the potentially endogenous variable *FailedMedicalExamination*. Our main results are presented in columns 3–5 of Table 1. Column 3 first shows the relationship between *FailedMedicalExamination* and *DistanceToCutoff* for the draft-exempt group—essentially, a regression version of the pattern in Figure 1 to the left of the cutoff. The high statistical significance of the coefficient of -0.0098 indicates a strong first-stage relationship.

Column 4 of Table 1 and panel A of Figure 2 present the reduced-form relationship between *PublicSectorEmployee* and *DistanceToCutoff*. The coefficient is negative and statistically significant. Thus, draft-exempt individuals who were closer to the draft cutoff also exhibited a higher probability of being public sector employees.

Finally, column 5 of Table 1 presents the 2SLS estimates. The coefficient is positive and statistically significant. It indicates that those who successfully cheated the medical examination to avoid being drafted for military service were 17.55 percentage points more likely to become a public employee later in life.¹⁶

¹⁶Our main results are robust to the inclusion of geographic and other pretreatment controls (see online Appendix Table A7).

As the true cutoff was unknown at the time of the medical examinations, a potential concern is whether the relevant threshold for cheating decisions was instead the previous year's cutoff. Columns 6–8 of Table 1 show that all results are very similar when using the distance to the previous year's cutoff instead of the actual cutoff.¹⁷

Under reasonable assumptions, the 2SLS estimate recovers the local average treatment effect (Angrist, Imbens, and Rubin 1996), which identifies the causal effect of cheating the medical examination at age 18 on future public employment for the subset of compliers: young men who were induced to successfully cheat on the medical examination by being assigned a lottery number in the proximity of the cutoff but would have not cheated if they were sufficiently further away. Thus, compliers in this setting are probably those who *ex ante* had the means, money, and/or contacts to cheat if needed.¹⁸

Falsification Tests.—The cheating interpretation of our first stage could be challenged if the stringency of the medical examination varied with the distance to the cutoff. However, individuals were called for medical examination by the order of their national ID, not by their lottery number. Even if military physicians knew the examinees' lottery numbers, we should expect them to be more lenient at failing individuals with very low numbers, who would be exempt from military service anyway, making superfluous any in-depth medical examination to prove the veracity of a claimed medical condition. If, instead, military physicians were more thorough in scrutinizing medical conditions of individuals closer to the cutoff, these true health conditions should make future public (and private) employment less likely, as the first two columns of Table 1 show, in contradiction with our findings.

Panel A of Table 2 reports results from three falsification tests. First, the coefficient in column 1 indicates that there is no relationship between the random distance to the cutoff and public employment among men with lottery numbers above the cutoff. This is as expected since all men above the cutoff faced similar incentives to cheat.¹⁹

Second, we constructed a mirror database for Argentine women born 1958–1962 and assigned them the lottery number corresponding to their last three ID digits. This should capture any correlation between draft numbers and public employment from a spurious pattern at the assignment of Argentine national ID numbers. The estimates are presented in Table 2, panel A, column 2 for the women with an ID number that would have placed them in the draft-exempt group. For women, there is no relationship between these imputed lottery numbers and the probability of being a public sector employee.

Third, we use a placebo experiment to challenge the validity of the exclusion restriction in our identification strategy. The lottery number did not have any further use nor consequences later in life and only played a role at the time of the conscription draft.

¹⁷ As online Appendix Figure A1 already suggested (see also footnote 9), the first-stage coefficient is steeper for the previous year's cutoff than for the actual cutoff.

¹⁸ Consistently, compliers are overrepresented in more developed provinces as proxied by poverty and education levels.

¹⁹ Strictly speaking, because the cutoff was uncertain, the cheating incentives could still be milder in the near proximity above the cutoff than for higher lottery numbers. The result in Table 2, panel A, column 1 is robust to setting arbitrarily higher cutoff numbers (for instance, 500), which would leave no uncertainty about being drafted to the individuals above it.

TABLE 2—FALSIFICATION TESTS AND FURTHER RESULTS

<i>Panel A. Falsification tests: draft-eligible men, “draft-exempt” women, and the 1976 cohort</i>			
	<i>PublicSectorEmployee</i> (1)	<i>FemalePublicSectorEmployee</i> (2)	<i>PublicSectorEmployee</i> (1976) (3)
<i>DistanceToCutoff</i>	0.0002 (0.0002)	−0.0003 (0.0008)	
<i>DistanceToPreviousYear’sCutoff</i>			0.00007 (0.00034)
Observations	691,278	289,339	229,305
Sample	Draft-eligible men	Women with exempt ID	Draft-exempt men
Cohorts	1958–1962	1958–1962	1976
Dependent variable mean	16.81	19.40	15.21
Estimation method	OLS	OLS	OLS
<i>Panel B. Private employment and public sector meritocratic/nonmeritocratic jobs</i>			
	<i>PrivateSectorEmployee</i> (1)	<i>MeritocraticPublicSectorEmployee</i> (2)	<i>Non-meritocraticPublicSectorEmployee</i> (3)
<i>FailedMedicalExamination</i>	−7.46 (9.82)	−0.439 (3.676)	17.99 (7.05)
Observations	295,611	295,611	295,611
Sample	Draft-exempt men	Draft-exempt men	Draft-exempt men
Cohorts	1958–1962	1958–1962	1958–1962
Dependent variable mean	37.09	3.395	13.18
Estimation method	2SLS	2SLS	2SLS

Notes: Standard errors clustered at the last three digits of ID/cohort level are shown in parentheses. All models include cohort fixed effects. Models in panel A are estimated using OLS. Models in panel B are estimated using 2SLS, where *FailedMedicalExamination* is instrumented with *DistanceToCutoff*. *FemalePublicSectorEmployee* is a dummy variable that takes the value of 1 if the woman was a wage earner in the public sector at some point in the period 2010–2016 and 0 otherwise. *PublicSectorEmployee(1976)* is a dummy variable that takes the value of 1 if the individual born in 1976 was a wage earner in the public sector at some point in the period 2015–2019 and 0 otherwise. *DistanceToPreviousYear’sCutoff* is the absolute value of the difference between each individual’s lottery number and the conscription cutoff for his previous cohort. *MeritocraticPublicSectorEmployee* is a dummy variable that takes the value of 1 if the individual was a wage earner in the public sector in an activity that requires some type of diploma, entry examination, or intensive training and evaluation at some point in the period 2010–2016 and 0 otherwise. *Non-meritocraticPublicSectorEmployee* is the complement of *MeritocraticPublicSectorEmployee*. For the definitions of *DistanceToCutoff*, *FailedMedicalExamination*, *PublicSectorEmployee*, and *PrivateSectorEmployee*, see notes to Table 1. Dummy variables are normalized to 0/100 so that results represent percentage points.

However, a potential concern is that our instrument could have a direct effect on employment outcomes through channels other than dishonest behavior in the medical examinations. For instance, being closer to the cutoff may have caused stress or may have depressed the morale of young men even before the medical exams, with a potential effect on career choices later in life.

For this placebo experiment, we take advantage of the abolishment of compulsory conscription in Argentina in 1994, which led to the cohort of 1976 facing the draft lottery but not being called for the medical examination (and not drafted). Between the lottery and the conscription abolishment, there were three months during which some young men were at risk of being drafted depending on their distance to the (eventual) cutoff. If increased stress (or other mechanisms not operating through

cheating in the medical examination) were responsible for our results, we would expect to find a significant relationship between *DistanceToPreviousYear'sCutoff* and *PublicEmployment* for men born in 1976.

Table 2, panel A, column 3 reports the results of this placebo exercise. The reduced-form coefficient of *DistanceToPreviousYear'sCutoff* on *PublicEmployment* is small and not statistically significant, suggesting that the instrument has no effect on employment outcomes through mechanisms other than cheating the medical examination. The findings from these three falsification tests reinforce the causal interpretation of our results.

Potential Mechanisms.—To explore possible underlying channels for our findings, we provide two further exercises. First, we compare public and private employment. Table 2, panel B, column 1 shows that draft evasion has no effect on having a formal job in the private sector, as inspection of the reduced form plotted in panel B of Figure 2 also indicates. This finding is congruent with a self-reputation channel in which cheating during the formative years weakened moral self-restraints and decreased the reputational cost of future misbehavior, making future high-temptation opportunities more desirable (Bénabou and Tirole 2004, 2011; Dal Bó and Treviò 2013). Public sector jobs, with life stability, corruption opportunities, and weak controls for absenteeism and effort provision, could be examples of high-temptation occupations compared to private sector positions.

Second, within the public sector, we compare meritocratic positions that require some type of diploma, entry examination, or intensive training and evaluation, relative to nonmeritocratic jobs. Table 2, panel B, columns 2 and 3 show that draft evasion is strongly related to nonmeritocratic employment, where there is more scope for arbitrary hiring, but not to meritocratic employment. This result is compatible with a learning channel in which the successful experience of cheating in a high-stake situation during their formative years acquainted these youths with the potential use of family connections and influences for dishonest behavior. These individuals may have then used similar tools to get coveted nonmeritocratic public service jobs, which in Argentina have been traditionally accessed through family, personal, and clientelistic connections. Instead, these contacts may not be enough to attain public meritocratic positions nor private sector jobs which have further individual requirements.

These two potential channels coincide in suggesting a process of adverse selection into public service in Argentina, a developing country with relatively high corruption levels. However, our findings could also correspond to other, perhaps complementary, mechanisms. Pacifists, for example, may choose to dodge the draft, and that experience may then prime them to engage in public service. Guilt could also lead cheaters into public service to make up for their early fraud. Please note that the causal link we established requires that these (or other) personal characteristics become exacerbated because of dodging the draft (the mere correlation of fixed personal features with preferences for public service cannot alone explain our results). Although we can speculate that public service motivated by pacifism or guilt could more likely lead to look for meritocratic rather than nonmeritocratic public jobs, which is contrary to what we find, we cannot discard the presence of these (or other) channels. The available data do not allow us to be conclusive on the identification of the precise mechanisms behind our results.

IV. Final Remarks

Given the size and scope of modern states, the quality of state bureaucracy is a main factor for economic progress. Honesty, in particular, can be a crucial attribute of government officers. We exploit a unique source of variation in the incentives to cheat during the impressionable years and find that individuals with a higher probability of having evaded military conscription by faking their medical exam exhibit a higher propensity to become public employees later in life. Our lottery identification strategy, the falsification tests, and the complementary evidence by types of medical condition and public employment indicate that there is a causal link. This evidence suggests that cheating can not only be induced but can also have lasting effects on future behavior.

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