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# Conscription and Educational Outcomes: Quasi-Experimental Evidence from the Republic of Cyprus

Ružica Savčić      Nikolaos Theodoropoulos      Dimitrios Xefteris\*

## Abstract

Peacetime military service has both positive and negative effects on human capital. While it depreciates academic abilities it also enhances non-cognitive skills. The net effect of conscription is hard to identify due to issues of self-selection, endogenous timing and omitted variables bias. We exploit the compulsory service of men in the Republic of Cyprus prior to university enrolment, to deal with the first two problems. After controlling for prior academic performance, admission age, and other relevant controls, we find that the duration of service has a positive effect on university test scores. Two exogenous reforms on the duration of the service allow us to deal with omitted variables bias. We estimate difference-in-difference models, where female students act as a control group, and show that a reduction (increase) in the length of the army service has a negative (positive) effect on male academic performance.

**Keywords:** Compulsory military service, human capital, education, non-cognitive skills, academic outcomes

**JEL codes:** C21; H56; I21; J24

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## 1. Introduction

Military service in peaceful times has ambiguous effects on human capital formation. While it can depreciate academic skills through educational disruption (Bingley, Lundborg and Vincent Lyk-Jensen, 2020), it may improve non-cognitive skills (Card and Cardoso, 2012). This paper tries to pin down the net effect of army length on human capital by exploiting a rich dataset of university students in the Republic of Cyprus and two exogenous changes in army duration.

The broader question of benefits from army service has been a topic of extensive research. To start with, it is important to distinguish between combat duty and peacetime military service, because they can affect cognitive skills in very different ways. For instance, warfare engagement can produce post-traumatic stress disorder (PTSD) which negatively impacts learning and productivity, while peacetime conscription is not associated with such effects (Hayes, Hayes and Mikedis, 2012 and Hoge, Castro, Messer, McGurk, Cotting and Koffman, 2004). There is also an essential difference between conscripts who attend army service professionally and those that are drafted for mandatory conscription. The latter group faces a much larger opportunity cost of time spent in service (Bingley et al., 2020), while the former group tends to get solid education, training and experience (Curry Hall, Harrell, Bicksler, Stewart and Fisher, 2014). Moreover, Mangum and Ball (1989) find that, in case of voluntary enlistment, military training provided nearly the same level of skills for civil employment as any civil institution, i.e. 45% to 50% of acquired skills were transferable to the work environment.

Literature on peacetime compulsory military service (CMS) has been equivocal. Card and Cardoso (2012) find positive effects of army service on earnings, though only for low-educated draftees. Switching from compulsory to optional conscription produces negative effects on education and earnings (Torun and Tumen, 2016), as does complete abolition of compulsory conscription (Maurin and Xenogiani, 2007), albeit only for draftees from lower-income families. Di Pietro (2013) finds that although the abolition of compulsory military service has no effect on university enrolment it differentially affects students from low and high social backgrounds. His results

suggest that students from better social backgrounds are more likely to enrol into university, while students from lower social backgrounds are less likely to do so. Grenet, Hart, and Roberts (2011) and Bauer, Bender, Paloyo and Schmidt (2012) conclude that military service has no long-term effects on the labour market outcomes after controlling for selection bias, while Imbens and van der Klaauw (1995) and Bingley et al. (2020) find negative effects on future earnings.<sup>1</sup> Vincent Lynk-Jensen (2018) finds that conscription reduces years of schooling as well as the probability of finishing high school at age 25 for draftees, but it does not affect crime or the probability of being unemployed.

The reason for these conflicting results may lie, in part, in the fact that different studies rely on different identification strategies to overcome the existing inherent selection bias and accompanying omitted variables bias as suggested by Hjalmarrsson and Lindquist (2016). The Republic of Cyprus mandatory conscription regime has at least two advantages compared to other CMS regimes analysed in the relevant literature. First, there is no self-selection bias as conscription is compulsory for every single male that turns eighteen in the year of conscription and passes the draft test. Second, there is no endogenous timing as military service cannot be postponed in order to pursue higher education. The compulsory nature of the conscription does not affect the propensity to enrol into college in a way it does when men are presented with an option to delay the draft.

However, even in this CMS regime, there is still the issue of omitted variables bias as there is a range of factors that could affect university grades and still those factors are correlated with military service. We try to overcome this particular identification problem by making use of two exogenous shocks in the duration of service of the Republic of Cyprus army centred on 2010 and 2016 respectively. In the first reform the alternative military service institution was introduced, aiming to reduce the number of individuals that avoided conscription claiming to be unfit for service. This reform allowed individuals, that would otherwise be exempt from duty, to join the

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<sup>1</sup> Regarding wartime service, Angrist (1990) presents evidence that war veterans earn on average lower wages than their non-draftee counterparts, while Angrist and Chen (2011) and Angrist, Stacey and Song (2011) show that this wage differential between war veterans and non-draftees diminishes throughout their working life cycle.

military in an unarmed capacity, and proved rather successful in increasing substantially the average duration of service. In the second reform the government decided that the compulsory military service should be reduced from 24 to 14 months for all conscripts. For each reform we conduct the same difference-in-differences analysis. By comparing course grades of students who were drafted in a narrow time window before and after the reform, versus their female colleagues who act as a control group, we can avoid omitted variable bias, since the reform provides an exogenous shock to the duration of the service.

The commonly held belief is that a long compulsory service implies disconnectedness from academic skills as well as an outstanding career break for young males (see, for instance, Bozick and DeLuca, 2005; Niu and Tienda, 2013). However, our difference-in-difference analysis points to a significant positive (negative) influence of a longer (shorter) army service on male students' grades in comparison with their female peers. This is an especially interesting finding, given that higher (lower) academic grades usually imply higher (lower) earnings during an individual's life cycle (Jones and Jackson, 1990; Card 1999).

By virtue of data available, we can directly relate academic outcomes, as measured by course grades, and the duration of military service, as captured by school terms spent in army training. We initially start with an ordinary regression analysis and then examine grade heterogeneity using quantile regressions. To overcome identification issues, we perform a difference-in-difference analysis by exploiting the reforms described above. All of our results point in the same direction: compulsory army service unequivocally improved grades of male undergraduate students at the University of Cyprus. Thus, we contribute to the existing literature by directly relating academic performance to conscription duration, in a framework that arguably allows a causal interpretation of our findings.

The rest of the paper is organised as follows. Sections 2 and 3 provide an overview of the theoretical and the institutional background respectively. Section 4 presents the

data. Section 5 outlines the empirical methodology and Section 6 presents the results. Section 7 concludes.

## **2. Theoretical background**

Although there is no straightforward way to unveil the underlying mechanism, in a sense that we do not observe the processes through which military service affects university test scores, we can attempt to understand it better by means of analogy with other forms of training that may have similar impact.

For instance, composition of military training resembles apprenticeship programs in that it consists of theoretical and practical work, accompanied by supervision and mentoring. Thus, it comes as no surprise that similar skills are developed in both setups, such as responsibility, self-discipline, team work and perseverance, leadership, self-assurance, and other social skills as shown in Arum (2005) and in Grönqvist and Lindqvist (2016). Moreover, Arum presents evidence that more discipline improves test scores in high school graduates, which given the physiology of adolescent years, can pertain to university students. For those coming from lower income households those training programs “provide the discipline and guidance which is often missing in their homes or high schools”, as reported by Kautz, Heckman, Diris, Ter Weel and Borghans (2014).

Heckman and Rubinstein (2001) highlight the importance of non-cognitive skills in affecting educational attainment. Hansen, Heckman and Mullen (2004) and Heckman, Stixrud and Urzua (2006) show that, in adolescence, academic and environmental influences affect test scores without changing the intelligence level. Moreover, Cunha, Lochner and Masterov (2006), find that with adolescents, interventions had significantly smaller effect on cognitive skills, while substantially improving non-cognitive ones. Finally, Heckman and Kautz (2012) provide an interdisciplinary literature review on how cognitive skills as captured by achievement tests have limited power in predicting future academic or labour market success, and highlight the importance of non-cognitive skills in causally predicting success for various outcomes. Lindqvist and Vestman (2011) highlight the importance of non-cognitive skills in affecting the probability of being unemployed and low earnings. Thus, it can

be argued that it is the non-cognitive abilities of the Cypriot students that were improved through military service, especially since the cognitive combat skills acquired in the army are hardly useful in academic settings (loading a gun, distinguishing between different firearms, stepping to the march, mastering the dress code, etc.).

Several things are known regarding the physiology behind these effects: in the adolescent years, the part of the brain that goes through biggest changes is the prefrontal cortex, which continues to grow well into the mid-twenties (Goldstein, Naglieri, Princiotta and Otero, 2014). This is the part of the brain in charge of the so-called executive functioning skills, non-cognitive skills essential in the academic setting, namely self-discipline (impulse control), cooperation (emotional control), resourcefulness (flexible thinking), applying adequate processes and procedures (working memory), adaptability (self-monitoring), planning and prioritizing, and motivation and focus (task initiation).

For instance, military environment could be stimulating adaptability in young men, which then improves persistence, planning, and task management (Collie, Holliman and Martin, 2017). It could also enhance self-sufficiency, internalizing the draftee's locus of control, helping them better understand mechanisms through which their actions affect their lives (Crawford and Cribb, 2012).

### **3. Army service and education outcomes in Cyprus**

#### **3.1 General setting**

In the year they turn eighteen male citizens of the Cyprus Republic are conscripted to compulsory military service. Women are exempted from the military service. Each prospect soldier is assessed for army aptitude. Depending on their health and family conditions, they are assigned full-term or part-term service, or full exemption from duty. About 19% of our sample of University of Cyprus students has gone through at least one school term of compulsory army service training. Given that only males are

conscripted, and that males represent about a third of our sample, the share of students that have served in the army is high.

During the period covered by our data there were two main reforms that affected the average duration of service. The first one increased the average time spent in the military while the latter reduced it.

**Reform 1:** Around 2010 a large reform took place to mitigate phenomena of army-avoidance that were widespread at the time. Starting from 2008 till 2011 the parliament passed a series of laws which made it harder for people to avoid conscription. To this end, the institution of alternative military service was introduced, which compelled individuals that claimed that they were not fit to handle weapons, or who were conscientious objectors, to serve in the army in an unarmed capacity. These institutional changes increased the average duration of service of males that subsequently enrolled into university.

**Reform 2:** The standard army duration was two years prior to 2016 and since then it has been reduced to fourteen months. As a transitional period, before the change in the policy was fully adopted, men drafted between June 2015 and June 2016 served eighteen months. Overall, this reform decreased the average time spent in the army by males before enrolling to the university.

#### **4. Data**

After finishing high school, Cypriot students who wish to continue their education at the tertiary level, take a national entrance exam before enrolling into a state university.<sup>2</sup> These exams (“pankypries”) are centralized and organized by the Ministry of Education, with prospect students being instructed which subjects to focus on given the degree they wish to obtain. The exams take place at the beginning of June after the end of the high school year at the end of May. Successful female

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<sup>2</sup> In Cyprus there are two state universities, the University of Cyprus and the Cyprus University of Technology that was established in 2004 and its first intake of students took place in the academic year 2007-2008. In this paper we use data only from the University of Cyprus.



students can enrol at the University of Cyprus in September while successful male students can enrol after they finish their military service. Male students can keep their acceptance status for up to two years and then enrol into the university.

In 2019, for instance, around 3300 high school graduates went to study abroad, namely in Greece, while 2800 students enrolled into the public universities in Cyprus, 65% of which entered the University of Cyprus and the other 35% joined the Cyprus University of Technology.<sup>3</sup> Student selection for schools within the University of Cyprus is made based on the scores from national exams and degrees the schools offer. Students do not pay fees. We use national exam scores to control for heterogeneity in student ability.

Apart from entrance exam scores, our database includes all students enrolled at the University of Cyprus since 2008 to date, all the exams the students took, the school term when the course was taken, as well as the grades they received on each exam taken, including exams with “pass” and “fail” outcomes.<sup>4</sup> For the courses with non-numeric grades (such as “success” or “failure”)<sup>5</sup>, we coded “pass” and “fail” as sample means, where passing the exam was assigned the value of 7.37 (the sample mean of successfully passed exams) and not passing it was assigned the value of 2.4 (the sample mean of failed exams).

We use individual level observations. That is, we measure the academic performance of a student by constructing her/his average grade across all the exams she/he has taken. We make use of student data: their coded ID number, the level of studies (undergraduate or postgraduate; first, second or third degree; etc), the school and department within the school, the term of admission, sex, year of birth, age on admission, district of residence, citizenship, number of academic terms (i.e.

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<sup>3</sup> [http://www.statscy.com/pagstats/2019/from\\_smaths/minmaxellada18.pdf](http://www.statscy.com/pagstats/2019/from_smaths/minmaxellada18.pdf)  
[http://www.statscy.com/pagstats/2019/sigentr\\_meanag19.htm](http://www.statscy.com/pagstats/2019/sigentr_meanag19.htm)

<sup>4</sup> A passing grade is considered any grade, on a scale of 0 to 10, of 5 and above.

<sup>5</sup> Exams with non-numeric grades comprise 2.92% of our sample (at the course level).

semesters) served in the army and number of terms spent in the Erasmus student exchange program.<sup>6,7</sup>

In the analysis we keep only students of Greek Cypriot nationality who attended a public secondary school and were between 17 and 21 years of age on admission, studying for their first degree at the undergraduate level. We also keep only the observations for which we have complete data. We restrict our analysis to the period after 2008, the first year the admission grades were available. Since the School of Medicine was established in 2013, we do not include observations from medical school students in the sample. Our final sample includes 407,336 student-course level observations relating to 12,008 students that we observe from 2008 to 2019.

[INSERT FIGURE 1 HERE]

Figure 1 shows the distribution of the average student grade (solid line). It approaches a normal distribution (dashed-dotted line) but it is slightly skewed to the left as the median (6.77) is higher than the mean (6.19). There is also a mass of observations at the left tail of the distribution (around 7.5% of exam grades are below 1). The majority of the observations (63.4 %) lies to the right of the mean.

Table 1 presents the distribution of the main variables of interest by gender.<sup>8</sup> Males make about one third of the sample and on average they enter university 1.41 years older than their female peers due to the compulsory conscription. They have slightly lower admission grade (0.11 units), and they attain slightly lower grades while at university (0.06 units).<sup>9</sup>

[INSERT TABLE 1 HERE]

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<sup>6</sup> To protect privacy, student ID numbers are coded at the University level.

<sup>7</sup> Term of admission refers to the school term when the student enrolled in the studies, not the term they were accepted to the university, i.e. when succeeding in the entrance exam.

<sup>8</sup> Descriptive statistics for the rest of the variables is available upon request.

<sup>9</sup> The difference in admission grades is statistically significant whilst the difference in mean grade is not.

## 5. Empirical Strategies

To identify the effect of the skills acquired in the Cypriot army on education outcomes, we employ two complementary empirical strategies, a direct analysis and a difference-in-differences analysis.

### 5.1 Direct analysis

In the direct approach, we apply an OLS regression analysis at the student level. We then examine grade heterogeneity using quantile regressions. We analyse student performance at the aggregate course level. Mean grade per student is our dependent variable and we include student-specific controls to our models.

$$\text{mean grade}_i = a + X_i\beta + u_i \quad (1)$$

where  $X_i$  is a vector consisting of the gender of the student (binary variable indicating whether the student is male) and the number of school terms spent in the army (Model 1).

Since there may be other factors that determine grade, we perform a five-step sensitivity analysis by successively increasing the number of controls that may account for differences in grades across students, i.e. augmenting the vector  $X_i$  above by sequentially adding extra controls. First, we add the *admission grade*<sub>*i*</sub> that proxies for student ability (Model 2). Then, the variable *age*<sub>*i*</sub> which indicates student's age at the time she/he enrolled at the university as age has an important effect on grades through student maturity (Model 3). We then add *year of birth*<sub>*i*</sub> to control for cohort effects (Model 4). To capture location differences across students we include *area*<sub>*i*</sub> which is a dummy indicating the district of student's origin (Model 5).

We also look at how the army training effect impacts upon student grade heterogeneity by means of quantile regressions. Card and Cardoso (2012) show that for low-educated draftees, compulsory army service produces positive effects on earnings. In our case, we consider whether students with low test scores benefit from the compulsory military training in the same way as their high achieving peers.

## 5.2 A difference-in-differences analysis

To examine the effect of the reforms on students' performance we apply a difference-in-differences (DID) estimator. We exploit the gender-specific nature of the reforms and compare the performance of the affected population (male students) to the performance of the unaffected population (female students) over a short event window around each reform ( $\pm 2$  years) (see Carletti, De Marco, Ioannidou and Sette, 2020 for a recent application of a similar approach). Our working hypothesis is that the performance of male students (treatment) improves after the first reform and worsens after the second reform compared to the performance of female students (control).

The benchmark DID model explains the student's performance (mean grade) as a function of only three variables – the treatment dummy, the reform dummy, and their interaction, which is our variable of interest – and district fixed effects. Given the short duration of our event window, all other characteristics are assumed to be time-invariant. To ensure, though, that our estimates are not affected by possible changes in other relevant factors, we also conduct a robustness analysis, controlling for enrolment age (which is affected by the reforms).

To establish that the control and the treatment group behave according to the parallel trend hypothesis (i.e. that the difference in the performance between male and female students does not vary significantly when there are no exogenous shocks in average army duration) we conduct suitable placebo tests: we re-estimate our basic DID model by shifting the event window two years after the first reform and two years before the second reform. The testable hypothesis is that the interaction between the treatment dummy and the placebo reform is not statistically significant.

The use of a 2-year event window is necessary since the given reforms were not instantaneous. The first reform started to take place in 2008 and was only concluded in 2011. As far as the second reform is concerned, during the academic year 2016-2017 several students from both regimes enrolled. A shorter window would not be suitable due to the described nature of the reforms, while a longer one would be

susceptible to selection concerns. Indeed, the decision of students to enrol to a Cypriot or a foreign university might be affected by the expected army service duration, so a time window of more than three years (i.e. of the duration of the senior high school) would not be immune to endogeneity issues.

Since our dataset starts with the 2008 academic year (for simplicity, we refer to the academic year that starts in the September of calendar year  $t$ , and ends in July of calendar year  $t+1$ , as year  $t$ ) and that the reforms were centred on 2010 and 2016 respectively, a natural partition in 2-year event windows is presented in Table 2.

[INSERT TABLE 2 HERE]

Our models look like this:

$$\begin{aligned} \text{mean grade}_i &= a + \beta_{1i}\text{male}_i + \beta_2\text{reform} + \beta_{3i}\text{male}_i * \text{reform} + \\ &+ \sum_{j \in J} \beta_{4ij}\text{area}_{ij} + u_i \end{aligned} \quad (2)$$

where  $\text{area}_{ij} = 1$  if student  $i$  originates from district  $j$ , and  $\text{area}_{ij} = 0$  otherwise; and  $J = \{\text{Nicosia, Limassol, Larnaka, Paphos, Famagusta, Kerynia, Morfou}\}$ , the set of LAU 1 districts of the Republic of Cyprus.<sup>10</sup>

The models differ according to the specific *reform* variable employed in each one of them, and the corresponding 2-year window. We've got four models, two with actual reforms (Reform 1 and Reform 2) and two with placebo reforms (Placebo 1 and Placebo 2). As mentioned above, for the sake of testing robustness of our results, we also use an augmented set of models, adding  $\text{age}_i$  to the equations.

To make sure that our selection of reform and placebo periods is appropriate, before the DID analysis we conduct a sanity check: we test whether male students served on average more in the period before or after the real reform, or before or after the placebo reform. This ensures that beyond the presented justification based on laws

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<sup>10</sup> Eurostat's LAU (Local Administrative Units) level 1 was formerly known as NUTS level 4.

and regulations, there is strong statistical evidence that the average duration of service changed or remained identical within a given time period.

Overall, the reforms in army duration constituted exogenous shocks in the army-acquired skills of the male student population, providing the opportunity for causal inference. At the same time, they were implemented gradually and remained immune to selection issues for only a brief period of time. Therefore, while this exercise makes a significant step towards identifying a causal relationship between non-academic skills and educational outcomes, it should be read with caution and it is, definitely, not enough to settle the issue conclusively.

## **6. Results**

In this section, we present the results from the direct approach as well as the difference-in-difference results.

### **6.1 Direct approach estimates**

We aggregate all the different course grades the student received across all her/his exams at the university and compute the average grade (*mean grade*), which we then regress against a set of individual-specific variables. We use an OLS regression model to do that and we provide robust standard errors. For the sake of comparison and brevity (given the large number of indicator variables), we present estimates for the variables of interest only. Tables with results on all the other covariates are available upon request.

We interpret the magnitude of the estimated coefficients with caution given that there could be other characteristics that affect student academic performance that we cannot control for in the direct approach. Table 3 shows that males achieve lower exam grades than females, *ceteris paribus*. For instance, column 5 suggests that a male's student exam score is 0.3 units lower than the score of his female peer, *ceteris paribus*. Although the effect is small the difference is highly statistically significant. Time spent in the army as well as admission test scores, are positively and significantly

associated with exam scores at the university. For instance, column 5 suggests that an increase in army length by four academic semesters (two years) increases male expected grade by 0.3 units, *ceteris paribus*. While, an increase in admission grade by one unit increases expected grade in the university by 0.5 units, *ceteris paribus*.

[INSERT TABLE 3 HERE]

We examine how army duration affects the academic performance of male students at different levels of academic achievement by running quantile regressions for each of the five models we outlined above. The results from this exercise are presented in Table 4 and suggest throughout that the longer is the length of military training the higher is the student's academic performance. We find the bigger effect at the lower end of the grade distribution suggesting that the service in the army is more beneficial for low than for high ability students. The greater influence of CMS in the lower quantiles is similar to the findings in Card and Cardoso (2012) who find that for low-educated draftees, compulsory army service produces positive effects on earnings.

[INSERT TABLE 4 HERE]

## **6.2. The Difference-In-Difference (DID) estimates**

The exogenous nature of army service duration (i.e. the fact that it does not follow students' choices) and the fact that we can control for prior academic achievement and age at admission provide reasons to believe that the estimated coefficients of the above analysis capture the net effect of military service on academic outcomes. However, student heterogeneity in terms of service duration based on personal (i.e. health) and family characteristics (i.e. size of the family), may conflate the interpretation of our estimates. What is necessary for the army effect to be fully disentangled from unobserved variables is to have both exogenous and random variation in the duration of army service.

As explained in the previous section, the reforms in the army service duration provide us with the necessary (as if) random variation. In the subsequent analysis we try to fully exploit it by conducting a standard DID analysis in which female students act as the control group (since they are not affected by the reform), while male students form the treatment group. In such exercises the main variable of interest is the interaction between the treatment group and the reform dummy variables, while also including the two non-interacted controls in the specification. We also control for admission age since changes in army duration also has an effect in the enrolment age. We also add district fixed effects.

Our first task is to show that our selected reform dates are indeed sensible. That is, to establish that the average army service duration of affected male students was affected as prescribed by the corresponding changes in regulations and laws. In Table 5, we focus only on male students and in column 1 we observe that the ones that enrolled during the years 2008 and 2009 served significantly less compared to the ones that enrolled in 2010 and 2011. Column 4 suggests that the male students that enrolled in 2014 and 2015 served significantly more than the ones that enrolled in 2016 and 2017. Moreover, as shown in columns 2 and 3 respectively there is no significant difference in the duration of service between students that enrolled in 2010 and 2011 (2012 and 2013) and those that enrolled in 2012 and 2013 (2014 and 2015), reassuring us that our selection of real and placebo reform dates is appropriate.

[INSERT TABLE 5 HERE]

We now proceed to our main DID analysis. The main findings are presented in Table 6. Our variables of interests in each row are the interaction terms between the male dummy and each reform or placebo reform. In column 1 we see that the interaction term between the male dummy and reform 1 predicts positively exam grades, whereas in column 4 the interaction term between the male dummy and reform 2 predicts negatively exam grades. These effects are both statistically significant, albeit at different significance levels. The magnitude of the changes seems to be larger than what a simple reading of the estimates in the benchmark analysis would suggest. For



instance, a decrease of army absence from the university due to the second reform by two semesters should explain, roughly, half of the observed interaction coefficient, but one should note that missing confounding variables might cause underestimation of the true magnitude of the effect in the benchmark model.

[INSERT TABLE 6 HERE]

In Figure 2, we illustrate these discontinuities by the means of a binned scatterplot of the data, with reference to the two reforms for male and female students respectively. As we observe, the line corresponding to female students (solid line) does not suffer from severe discontinuities around the reform points, whereas, the line of the male students (dashed line) changes dramatically before and after each reform, in the direction predicted by the corresponding change in average army duration. For instance, the line for males discontinuously jumps upwards after the first reform signifying an increase in average grade, whilst it discontinuously jumps downwards after the second reform signifying a decrease in average grade for males.

[INSERT FIGURE 2 HERE]

While the signs of these effects fully align with the signs of the changes in average army service, one can exploit the remaining data to further strengthen these main results. To ensure that our estimates do not pick up effects that are reform-independent we conduct a placebo analysis focusing on two placebo reform dates. In columns 2 and 3 of Table 6, we repeat the same exercise with the only difference being the placebo reform to have taken place two years after (before) the first (second) real reform actually took place. In each case we still consider a 2-year event window, we define the reform dummy accordingly and the corresponding interaction with the treatment group dummy. In both cases the variable of interest fails to be statistically significant. This provides evidence that it was only at the actual reform dates that the difference between the performance of male and female students changed substantially.

We then replicate the same analysis controlling also for the students' admission age, as a change in the duration of the CMS affects age on admission, which in turn, as we have showed previously, correlates with the exam test scores. The results are presented in Table 7.

[INSERT TABLE 7 HERE]

Despite the fact that the results of this exercise reinforce our benchmark findings, and the other relevant results in the literature regarding the potentially positive effect of army service on education outcomes, it is important to issue a number of caveats. While the reforms constituted indeed exogenous shocks and are sources of random variation in the duration of army service among the male student population, the possibility of endogenous reactions to the reforms should not be neglected. When students decide whether to study at a local university or a foreign one, the compulsory time gap between high school and the start of their university studies might affect their choice and induce changes in the characteristics of the population of male students at the local university. Hence, the analysis needs to be limited to only a couple of years after the reform (i.e., to students that did not have time to react and change substantially their studies planning). Moreover, the fact that the reforms were not sharp but required time to materialize also prohibits a sharp discontinuity argument. While a causal interpretation of the results is plausible, in light of the discussed limitations and particularities of the employed setup, our results take the form of qualified statements rather than unconditional assertions.

## **7. Conclusion**

Given that the literature is still divided on the effects of CMS on the labor outcomes, we set out to examine whether and how army training affects academic outcomes. The data available from the University of Cyprus presented a unique opportunity to examine the effect of the duration of conscription on academic achievement. We made use of exogenous changes in CMS policy and analyzed two reforms that took

place in Cyprus: a reform that increased the army draft coverage (the 1<sup>st</sup> reform) and a reform that decreased the CMS duration (the 2<sup>nd</sup> reform). In this way, we could see how changes in both directions influenced the grades former draftees got on their exams. We showed that the first reform lowered the average grade of male students as compared to their female counterparts whereas the second reform decreased the academic performance of former draftees.

The results of all our tests are unequivocal. Compulsory army service increased average course grade at the University of Cyprus. Further research is needed to understand fully the mechanism behind this effect. However, some interpretations can be established based on comparison to other extensively researched training/apprenticeship programs. It is our belief that the army training works similarly as apprenticeship programs to increase responsibility and perseverance while also improving both dexterity and communication skills. Young men who enter the university then are better prepared in terms of enhanced non-cognitive skills to meet the academic requirements of a tertiary education set-up. This is what shows up in our data: the longer students are exposed to the military training prior to the university enrollment the better they perform on university exams, as measured by the grades they obtain.

However, it may not only be the CMS that could provide the basis for improved academic outcomes: any kind of educational program grounded in development of non-cognitive skills would prove itself beneficial to prospect university students. Of course, it is yet to be established if the proposed mechanism behind the skill development during a course of CMS is what drove the academic performance improvement. A research into the effectiveness of army training in development of non-cognitive skills could provide a better understanding of this mechanism, but also provide evidence on the type of training programs that help advance the educational outcomes for students who may suffer from a lack of necessary skills for academic success.

Finally, it is important to stress that the identified increase in the average grade that is linked with army duration comes at considerable opportunity costs for the conscripts. Trying to understand whether the boost in academic performance compensates in the long term for the delay in entering the labor market seems as the natural next step. While our study does not aspire to address such efficiency issues, it enhances our understanding regarding the direct effects of army service on education outcomes; thus, strengthening the foundation upon future research can build upon.

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Figure 1. Distribution of Mean Grade

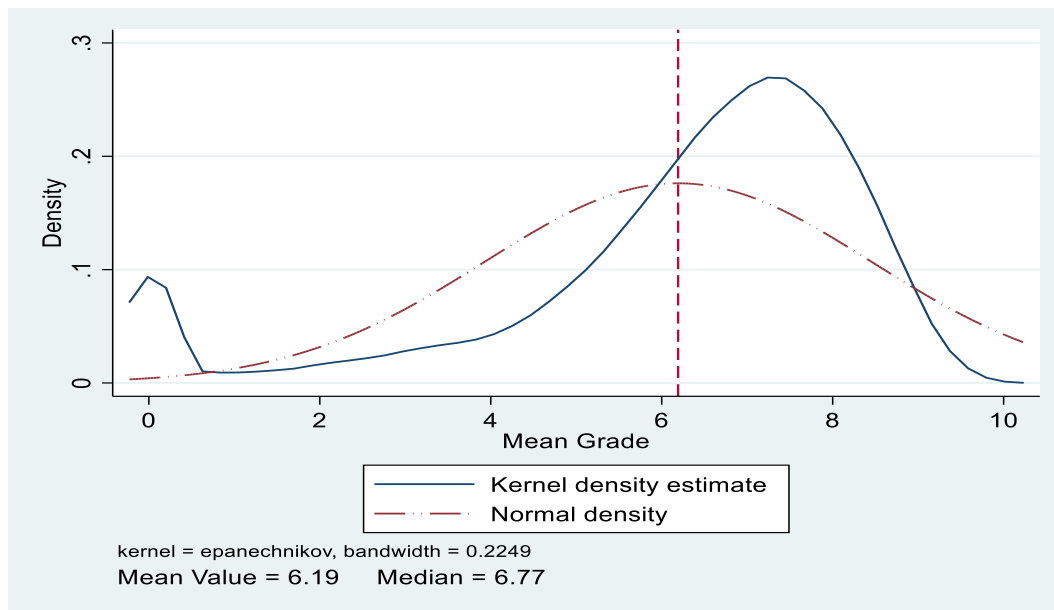


Table 1. Summary Statistics of Main Variables

		<i>Age at admission</i>	<i>Admission grade</i>	<i>Army term count</i>	<i>Mean course grade</i>	<i>Male</i>
Female students	N	8,510	8,510	8,510	8,510	8,510
	Mean	17.70	17.85	0.00	6.21	0
	St. dev.	0.58	1.56	0.00	2.30	0
	Min	17	8.75	0	0	0
	Max	21	20	0	10	0
Male students	N	3,498	3,498	3,498	3,498	3,498
	Mean	19.11	17.74	1.97	6.15	1
	St. dev.	0.81	1.51	1.71	2.18	0
	Min	17	10.67	0	0	1
	Max	21	19.99	6	9.88	1
All students	N	12,008	12,008	12,008	12,008	12,008
	Mean	18.11	17.82	0.57	6.19	0.29
	St. dev.	0.91	1.55	1.29	2.26	0.45
	Min	17	8.75	0	0	0
	Max	21	20	6	10	1

Notes. Descriptive statistics on all the other variables are available upon request.



Table 2. Academic Years Included in the 2-year Event Windows Before and After Real and Placebo Reforms.

	Before	After
Reform 1	2008, 2009	2010, 2011
Placebo 1	2010, 2011	2012, 2013
Placebo 2	2012, 2013	2014, 2015
Reform 2	2014, 2015	2016, 2017

Table 3. Direct Analysis, OLS regression

	(1)	(2)	(3)	(4)	(5)
<i>Male</i>	-0.492 <sup>***</sup> (-8.017)	-0.104 <sup>*</sup> (-1.768)	-0.188 <sup>***</sup> (-2.724)	-0.260 <sup>***</sup> (-3.197)	-0.277 <sup>***</sup> (-3.406)
<i>Army term count</i>	0.258 <sup>***</sup> (11.802)	0.079 <sup>***</sup> (3.736)	0.067 <sup>***</sup> (3.127)	0.068 <sup>***</sup> (2.977)	0.076 <sup>***</sup> (3.304)
<i>Admission grade</i>		0.494 <sup>***</sup> (38.447)	0.497 <sup>***</sup> (38.635)	0.496 <sup>***</sup> (38.437)	0.499 <sup>***</sup> (38.996)
<i>N</i>	12008	12008	12008	12008	12008
<i>adj. R<sup>2</sup></i>	0.019	0.125	0.126	0.126	0.133

Notes. Column 1 (model 1) includes the following controls: male dummy and army length (army term count); Column 2 (model 2) adds admission grade; Column 3 (model 3) adds age at enrolment, Column 4 (model 4) adds; year of birth fixed effects, Column 5 (model 5) adds district fixed effects. *t* statistics are in parentheses. Levels of significance: \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table 4. Direct Analysis, Quantile Regressions

	(1)	(2)	(3)	(4)	(5)
<b>10<sup>th</sup> percentile</b>					
<i>Male</i>	0.036 (0.142)	0.219 (1.010)	0.005 (0.015)	-0.160 (-0.538)	-0.222 (-0.832)
<i>Army term count</i>	0.382*** (5.728)	0.174** (2.345)	0.158* (1.920)	0.163** (2.377)	0.214** (2.401)
<i>Admission grade</i>		0.619*** (13.467)	0.607*** (10.271)	0.593*** (10.805)	0.555*** (8.941)
<b>25<sup>th</sup> percentile</b>					
<i>Male</i>	-0.777*** (-8.298)	-0.353*** (-5.369)	-0.403*** (-4.818)	-0.486*** (-4.962)	-0.508*** (-6.013)
<i>Army term count</i>	0.313*** (8.226)	0.100*** (3.513)	0.075*** (2.659)	0.085*** (2.781)	0.079** (2.398)
<i>Admission grade</i>		0.667*** (32.461)	0.672*** (33.393)	0.669*** (35.843)	0.681*** (27.277)
<b>50<sup>th</sup> percentile</b>					
<i>Male</i>	-0.688*** (-8.656)	-0.267*** (-3.339)	-0.300*** (-3.915)	-0.304*** (-4.376)	-0.299*** (-3.750)
<i>Army term count</i>	0.268*** (11.254)	0.072*** (2.989)	0.063*** (2.756)	0.072*** (3.982)	0.071*** (3.950)
<i>Admission grade</i>		0.565*** (45.177)	0.565*** (40.941)	0.560*** (37.408)	0.561*** (36.761)
<b>75<sup>th</sup> percentile</b>					
<i>Male</i>	-0.542*** (-10.013)	-0.149*** (-2.800)	-0.198*** (-4.388)	-0.221*** (-4.039)	-0.239*** (-4.432)
<i>Army term count</i>	0.241*** (14.812)	0.063*** (5.004)	0.058*** (4.794)	0.060*** (3.625)	0.065*** (3.951)
<i>Admission grade</i>		0.478*** (53.735)	0.479*** (63.716)	0.481*** (70.538)	0.480*** (56.984)
<b>90<sup>th</sup> percentile</b>					
<i>Male</i>	-0.432*** (-10.211)	0.008 (0.131)	-0.040 (-0.675)	-0.018 (-0.283)	-0.028 (-0.378)
<i>Army term count</i>	0.217*** (12.593)	0.049** (2.489)	0.045** (2.217)	0.049* (1.800)	0.044** (2.010)
<i>Admission grade</i>		0.427*** (42.009)	0.427*** (34.046)	0.425*** (44.377)	0.429*** (62.467)
<b>N</b>	12008	12008	12008	12008	

Notes. Column 1 (model 1) includes the following controls: male dummy and army length (army term count); Column 2 (model 2) adds admission grade; Column 3 (model 3) adds age at enrolment, Column 4 (model 4) adds; year of birth fixed effects, Column 5 (model 5) adds district fixed effects. *t* statistics are in parentheses. Levels of significance: \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Table 5. Selection of Reforms and Placebos

	(1)	(2)	(3)	(4)
Reform 1	0.312*** (2.790)			
Placebo 1		-0.130 (-1.256)		
Placebo 2			0.039 (0.387)	
Reform 2				-0.208** (-2.328)
<i>N</i>	1127	1279	1351	1538
adj. <i>R</i> <sup>2</sup>	0.023	0.021	0.012	0.006

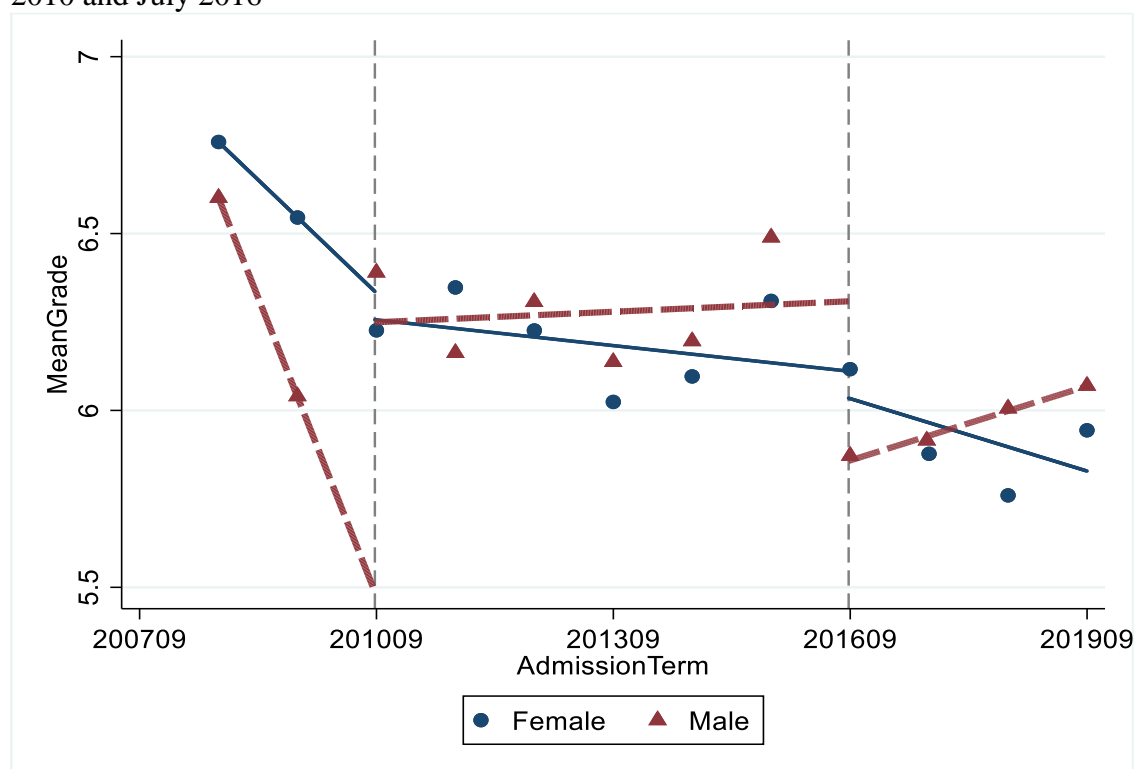
Notes. We regress variable *reform* on *army term count* in 2-year windows about the corresponding reform, using individual level observations of male students. We also include district fixed effects. Reform 1 = 1 if  $t \geq 2010$  and zero otherwise, Placebo 1 = 1 if  $t \geq 2012$  and zero otherwise, Placebo 2 = 1 if  $t \geq 2014$  and zero otherwise and Reform 2 = 1 if  $t \geq 2016$  and zero otherwise. *t* statistics are in parentheses. Levels of significance: \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table 6. Difference-In-Differences Analysis

	(1)	(2)	(3)	(4)
Male × Reform 1	0.337** (2.273)			
Male × Placebo 1		0.106 (0.715)		
Male × Placebo 2			0.044 (0.295)	
Male × Reform 2				-0.265* (-1.793)
<i>N</i>	4486	4681	4398	4141
adj. <i>R</i> <sup>2</sup>	0.007	0.008	0.006	0.010

Notes. We regress variable *mean grade* on *male*, *reform* and an interaction of the two binary variables, *male\*reform* in 2-year windows about the corresponding reform, using individual level observations. We also include district fixed effects. Reform 1 = 1 if  $t \geq 2010$  and zero otherwise, Placebo 1 = 1 if  $t \geq 2012$  and zero otherwise, Placebo 2 = 1 if  $t \geq 2014$  and zero otherwise and Reform 2 = 1 if  $t \geq 2016$  and zero otherwise. *t* statistics are in parentheses. Levels of significance: \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Figure 2. Estimated Mean Grade Based on Admission Term With Cut-Offs at July 2010 and July 2016



Notes. To create the figure, we first regress mean grade on admission term. We then group the residual admission term from this regression on 12 bins and plot the residual admission term against the corresponding residual mean grade in each bin for males and females.

Table 7. Difference-In-Differences Analysis, robustness check with Age

	(1)	(2)	(3)	(4)
Male × Reform 1	0.340 <sup>**</sup> (2.291)			
Male × Placebo 1		0.107 (0.724)		
Male × Placebo 2			0.045 (0.299)	
Male × Reform 2				-0.256 <sup>*</sup> (-1.703)
<i>N</i>	4486	4681	4398	4141
<i>R</i> <sup>2</sup>	0.007	0.008	0.006	0.010
adj. <i>R</i> <sup>2</sup>	0.010	0.006	0.008	0.007

Notes. We regress variable *mean grade* on *male*, *reform*, *age* and an interaction of the first two variables, *male\*reform* in 2-year windows about the corresponding reform, using individual level observations. We also include district fixed effects. Reform 1 = 1 if  $t \geq 2010$  and zero otherwise, Placebo 1 = 1 if  $t \geq 2012$  and zero otherwise, Placebo 2 = 1 if  $t \geq 2014$  and zero otherwise and Reform 2 = 1 if  $t \geq 2016$  and zero otherwise. *t* statistics are in parentheses. Levels of significance: \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .