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ORIGINAL PAPER

Conscription and educational outcomes

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Abstract
Peacetime military service has both positive and negative effects on human capital. While it depreciates academic skills, it also enhances non-cognitive ones. 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 preceding university enrolment to deal with the first two problems. After controlling for prior academic performance and other relevant controls in a selection on observables model, we find that duration of service has a positive and significant effect on men's subsequent academic performance as measured by grade point average. Two exogenous reforms—one at the extensive margin and one at the intensive margin of military service—allow us to deal with omitted variables bias. We estimate difference-in-differences models, where female students act as a control group, and show that an increase (reduction) in the average length of army service has a positive (negative) and significant effect on men's academic performance.

Keywords Military service · Human capital · Education · Non-cognitive skills · Transferable skills · Academic outcomes

JEL Classification C21 · H56 · I21 · J24

1 Introduction

This paper examines the net effect of army length on academic performance as measured by students' grade point average (GPA). To do this, we exploit a rich dataset of

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university students in the Republic of Cyprus and two exogenous reforms. To date the empirical evidence on the effect of the military service on various educational outcomes provides ambiguous results. The reasons for this ambiguity are that different studies use different measures of educational attainment, rely on different identification strategies and analyse countries with different institutional army settings and reforms.

Compulsory military service (CMS) in the Republic of Cyprus has at least two advantages compared to other CMS regimes analysed in the literature. First, there is no self-selection bias as conscription is mandatory for every single male who turns eighteen in the year of conscription and passes the draft test. Second, there is no endogenous timing since 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 the 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 a range of factors (e.g. health reasons, family size) may both be correlated with military service and affect university academic performance. We try to overcome this identification problem by making use of two recent exogenous shocks that affected the average duration of service in the Republic of Cyprus army, centred on 2010 and 2016. An alternative form of military service was introduced in the first reform, aiming to reduce the number of individuals who avoided conscription by claiming to be unfit for service. This reform operated at the extensive margin: it compelled individuals who would otherwise be exempt from duty to join the army in an unarmed capacity, and thus proved rather successful in increasing the average duration of service by reducing the share of men who never serve. In the second reform the government decided that the compulsory military service should be reduced from 24 to 14 months for all conscripts. This policy induced changes at the intensive margin and led to a decrease in the average service duration.

For each reform we conduct a difference-in-differences analysis. By comparing the GPA of male students who were drafted in a narrow time window before and after the reform with their female colleagues who are the control group, we can avoid omitted variables bias since the reforms provide exogenous shocks to the average duration of the service. Our difference-in-differences analysis points to a significant positive (negative) influence of longer (shorter) army service on male students' GPA compared to their female peers.

Our unique dataset allows us to directly relate students' academic performance as measured by GPA and the duration of military service, captured by school terms spent in army training. We start with a selection on observables model in order to establish the correlation between GPA and time spent in the army as well as to establish the sign of the relationship. We find that time spent in the army is positively and significantly associated with higher GPA. Then, we augment our specification by successively increasing the number of control variables and observe that our main result still holds despite the coefficient of interest decreasing in magnitude. We then examine grade heterogeneity using quantile regressions and find that the effect is higher at the lower end of the GPA distribution, suggesting that army service is more beneficial for students with lower academic ability. To overcome identification issues, we perform

a difference-in-differences analysis by exploiting the two exogenous reforms and perform various placebo and sanity checks. All the results point in the same direction: compulsory military service unambiguously improved the GPA of male undergraduate students. We are contributing to the relevant literature by being the first to directly relate academic performance to conscription duration, in a framework that arguably allows for causal interpretation.

The rest of the paper is organized as follows. Section 2 reviews the existing literature with respect to educational outcomes, while Section 3 provides an overview of the theoretical background on how military training may shape educational outcomes. Section 4 describes the institutional context of the Cyprus military service and the two exogenous reforms. Sections 5 and 6 present the data and outline the empirical methodology, respectively. Section 7 presents the results, while Section 8 concludes.

2 Literature review

There has been an established and ongoing empirical research on the effect of military service on different educational outcomes. To date, the evidence on the relationship between military service and educational outcomes is mixed as researchers use different dependent variables, different identification strategies and examine different countries with different institutional army settings and reforms. In this section we review the literature on the effect of CMS on educational outcomes only. There has been other literature that examines the effect of CMS on labor market outcomes. Within that literature some papers (among others) examine the effect of CMS on employment (Siminski 2013; Autor et al. 2011), on wages (Angrist 1990; Bauer et al. 2012; Card and Cardoso 2012), and on other socioeconomic outcomes such as crime (Hjalmarsson and Lindquist 2019; Siminski et al. 2016) and health (Dobkin and Shabani 2009; Bedard and Deschenes 2006).

For the USA, Angrist (1993) using the 1987 Survey of Veterans finds that veteran benefits increase years of schooling by about 1.4 years. Card and Lemieux (2001) using various micro data sources report that draft avoidance behavior to the Vietnam War increased college attendance rates by 4 to 6 percentage points in the late 1960's. They also show that for men born in the mid-1940s, draft avoidance behavior raised their share in attaining a college degree by 2 percentage points. Similarly, Bound and Turner (2002) examine the impact of military service and the G.I. Bill on the educational attainment of World War II veterans using several micro data sources. They find the net effect of the military service and of the G.I. Bill on college completion rates to be positive and between 5 to 6 percentage points. Angrist and Chen (2011) use Census data and find that CMS increased years of schooling for white veterans by 0.33 years. They report that the increase in the years of schooling comes mainly from more years of college as opposed to other educational attainments. Further, Angrist and Chen (2011) is the only study which suggests that the educational gains related to conscription are associated with subsidies to veterans as opposed to draft avoidance behavior. A more recent study by Duncan et al. (2019) examines the relationship between voluntary military service and three educational outcomes: completing high school, obtaining a general equivalency diploma (GED) and obtaining a college degree. They exploit

variation in the desirability of military service as captured by combat fatalities at the state level and find that exposure to such fatalities at a young age increases not only the probability of dropping out of high school, but also it increases the probability of obtaining a GED later in life. They did not find statistically significant effects for the probability of obtaining a college degree, however.

For Canada, Lemieux and Card (2001) take advantage of cohort-province differences in military enlistment rates and using two micro data sources report that a law signed in 1944 to facilitate the return of veterans of WWII in civilian life had a significant effect only on the educational attainment of veterans from Ontario as opposed to veterans from Quebec.

For France, Maurin and Xenogiani (2007) using micro data note that complete abolition of compulsory conscription in 1997 significantly dropped the share of 18 to 21 years old men in education as well as the probability of high school graduation. Mouganie (2020) utilizes the same data as in the Maurin and Xenogiani (2007), but employees a different identification strategy. He finds that men who were born before the abolition of compulsory conscription acquired four to six additional months of education compared to those born just after the reform.

For Germany, Bauer et al. (2014) use two micro level datasets and report that the introduction of compulsory conscription in 1937 increased the probability of obtaining a university degree by 1.7 percentage points.

For Sweden, Grönqvist and Lindqvist (2016) combine military enlistment and administrative data and show that officer training in different military ranks has a positive effect on educational attainment. For instance, being a squad officer increases the probability of obtaining a first degree by about 3 percentage points and also increased the probability of obtaining a post-graduate degree by about 1 percentage point. The latter probability increases to a maximum of 3.9 percentage points for platoon officers.

For Denmark, Bingley et al. (2022) examine the causal effect of peacetime military service on skills. Using Danish draft army lottery data merged with skills data they find significant indirect effects of military service on numeracy and literacy skills after 10 years. Specifically, they report that military service increases enrolment and completion of vocational training especially for men with low Armed Forces Qualification Test (AFQT) scores. They suggest that the mechanism through which military service improves numeracy and literacy skills is through changes in educational careers, suggesting that men with low AFQT scores are more likely to take vocational training, not higher education, because of military service.

For Turkey, Torun and Tumen (2016) use micro data and a law change in 1999 that allowed males born before 1972 to pay and serve only twenty-one days in the army. They find that this almost complete exemption from CMS reduces the educational attainment for eligible men through reducing the probability of receiving a college degree and above.

In contrast to the above studies that report positive effects of CMS on various educational outcomes, other studies find negative effects of CMS on educational attainment. For the Netherlands, Hubers and Webbing (2015) use individual level data and examine the long-term effects of peacetime military conscription on educational attainment by exploiting a policy change that exempted a complete birth cohort from military service. They report that compulsory military service decreases the proportion of Dutch

university graduates by 1.5 percentage points and that being a conscript reduces the probability of obtaining a university degree by about 4 percentage points.

For Italy, Di Pietro (2013) uses two waves of a cross sectional survey and finds that although the abolition of compulsory military service has no effect on university enrolment, it inversely affects students from low and high social backgrounds. His results suggest that the abolition of CMS increases the probability of students from better social background to enrol into university, while it decreases the same probability for students from lower social backgrounds. Cipollone and Rosolia (2007) find that the exemption from CMS given to some specific cohorts of males living in southern Italy, as a result of an earthquake in 1980, increased their high school graduation by more than 2 percentage points.

For the UK, Buonanno (2006) uses two cross sectional surveys and reports that men born after 1943, the first cohort exempt from serving in the armed forces, were more likely to stay in full-time education longer. Keller et al. (2010) is the only study that used aggregate data for twenty-two OECD countries from 1960 to 2000. They find that conscription has a negative but statistically insignificant effect on the share of the working age population who participated in tertiary education. However, when they examine its enforcement as measured by the share of the people who are conscripted as well as by the duration of the service, they find significantly negative and sizeable effects. For instance, they report that an eight months decrease of the duration of the military service increased the share of the working age population enrolled in post secondary education by at least 8 percent.

It is clear from the above literature review that the evidence of military conscription on educational outcomes is ambiguous. But, none of the above studies has examined the effect of military constriction on academic performance while at university. Thus, this is the first paper to inform the literature about the net effect of army duration on male student academic performance compared to their female peers using a student level dataset from the higher education and two exogenous reforms.

3 Theoretical background

Although there is no straightforward way to unveil the underlying mechanism, in the sense that we do not observe the processes through which military service affects GPA, we can attempt to understand it better by means of analogy with other forms of training that may have a similar impact. For instance, composition of military training resembles apprenticeship/vocational 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, perseverance, leadership, self-assurance, and other social skills as shown in Arum (2005); Grönqvist and Lindqvist (2016). Moreover, Arum presents evidence that more discipline improves test scores for high school graduates, which given the physiology of adolescent years (i.e. between 10 and 19 years of age), can pertain to university students. For the ones 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 et al. (2014).

Heckman and Rubinstein (2001) highlight the importance of non-cognitive skills in educational attainment. Hansen et al. (2004); Heckman et al. (2006) show that, in adolescence, academic and environmental influences affect test scores without changing the intelligence level. Cunha et al. (2006), find that with adolescents, interventions had significantly smaller effect on cognitive skills, while substantially improving non-cognitive ones. Indeed, as stressed by Cunha et al. (2006) "while factors affecting IQ deficits need to be addressed at very early ages for interventions to be effective, there is evidence that later interventions in the adolescent years can affect noncognitive skills". In other words, it has been shown in the literature on education and academic achievement that different policy interventions produce different results depending on the age of subjects involved. Adolescence is, in that regard, related to notable improvements in the so-called non-cognitive skills.

This is not surprising, given the body of work (see, for instance, Huttenlocher and Dabholkar 1997, and references therein) which explains how different areas of the brain undergo intense synaptic changes at different stages of development. Neuroscience tells us that the period during which the frontal part of human brain matures starts in the adolescence and lasts well into the mid-twenties (see, for instance, Fuhrmann et al. 2015; Dow-Edwards et al. 2019). It is during this period that what is known in the literature as soft skills (like problem-solving, time management or organization) can be significantly improved, because synapses responsible for these skills develop in the frontal cortex of the brain and, as argued above, in adolescence this brain area is particularly malleable.[1]

Moreover, Heckman and Kautz (2012) provide an interdisciplinary literature review on how cognitive skills, measured by achievement test scores, have limited power in predicting future academic or labor market success, and highlight the importance of non-cognitive skills in causally predicting success for various outcomes. Humphries and Kosse (2017) using German panel data find that non-cognitive skills predict high school grade point average equally well as cognitive skills. Lindqvist and Vestman (2011) show that non-cognitive skills influence wages and employment status.

Thus, it can be argued that it is the non-cognitive abilities of the Cypriot male students that are improved through military service, especially since the cognitive combat skills acquired in the army are hardly useful in academic settings (physical training, loading a gun, distinguishing between different firearms, stepping to the march, mastering the dress code, etc.). On the other hand, disrupting education for military service should be detrimental with respect to the conscripts' academic skills (Bingley et al. 2020). Indeed, the academic abilities of conscripts acquired from school

[1] 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 et al. 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, motivation and focus (task initiation) and for some leadership skills. For instance, military environment could be strengthening adaptability in young men, thus improving persistence, planning, and task management (Collie et al. 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).

(such as knowledge in mathematics, physics, biology, language, etc.) are not employed, or further developed, during army service, hence they are likely to depreciate.[2]

Whether the potentially positive effect of army service on non-academic abilities can dominate over its negative influence on academic skills and lead to better educational outcomes is an empirical question; one that we set out to explore in this paper.

4 Army service and educational outcomes

4.1 General setting

In the year they turn eighteen, male citizens of the Republic of Cyprus are conscripted to compulsory military service, while women are exempted from the draft. This year typically coincides with the end of high school, and the majority of male students start their service a couple of months later after their high school graduation. Each prospect soldier is assessed for army aptitude. Depending on their health and family conditions, they are assigned full-term service, reduced service, or exemption from duty.[3] About two thirds of the male students in our sample (among all males) have gone through at least one school term of compulsory army service training.

During the period covered by our data (2008–2019) there were two main reforms that affected the average duration of service. The first reform increased the average time spent in the military while the latter reduced it.

Reform 1 (Extensive margin): Around 2010 a reform took place to mitigate phenomena of army avoidance that were observed at the time. Starting from 2008 to 2011 the parliament passed a series of laws which made it harder for people to avoid conscription. To this end, the institution of unarmed military service was introduced, which compelled individuals who claimed 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 number of males who served in the army, and thus increased the average duration of service of males that subsequently enrolled into university.[4] Undeniably, there are many kinds of army-avoidance strategies that were not affected by these policies (e.g. males fleeing abroad directly after high school graduation). But, a major channel of escape from duty was effectively shut down.

[2] To test whether such changes in academic and non-academic skills actually take place in our setting, we conducted a relevant survey among university students. The survey is presented in the Online Appendix and aligns, by and large, with the arguments presented above: most students report that non-academic skills improved during army service, and that those skills are useful for their university studies, while most academic skills deteriorated during the same period.

[3] Reduced service is assigned on the basis of family circumstances (e.g. to males who are parents or who are members of large families), while exemption from duty is decided on the basis of medical conditions.

[4] Information regarding the military service of every soldier is not requested by the University of Cyprus. The university only records the duration of time spent in army service, after high school graduation and before enrolment to the university. That is, we can infer if a male's army service was full-term, reduced, or whether he was completely exempt of duty; but we do not know whether the conscript served in an armed or in an unarmed capacity.

Reform 2 (Intensive margin): The standard army duration was two years in length prior to 2016. 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. This reform did not affect the number of males that served in the army. However, it had a direct negative effect on the average time spent in the army for males before enrolling to the university.[5]

We present evidence backing up the above claims regarding the effect of each reform on the average service duration, and on the fraction of males that served in the military in Section 7.2 (Tables 5 and 6). Overall, the institutional setting in place provides powerful instruments to deal with self-selection, endogenous timing and omitted variables bias. Despite that, one should issue a number of caveats which are related mostly to the classified nature of army data. Ideally, one would like to have precise information regarding the details of the military service of each male in order to be able to assess how each reform affected, not only the average duration of service, but also the army-avoidance strategies, the share of males who applied for exemption from duty after a reform, and the acceptance rate of such applications. Naturally, the army is not facilitating the provision of such figures, and research has to rely only on accessible information. In the next section we describe the available information and discuss its ability to shed light on our main research question.

5 Data

After finishing high school, Cypriot students who wish to continue their education at the university level either in the Republic of Cyprus or in Greece, take a national entrance exam before enrolling into a state university. 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 high school graduation. Successful female students can enrol at the University of Cyprus three months later, 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 before enrolling into the university.

In 2019, for instance, around 3,300 high school graduates went to study in Greece while 2,800 students enrolled into the two public universities in Cyprus. An

[5] Although there had been long discussions about the reduction in the length of the military service prior to the second reform, the actual implementation of the reform was not long anticipated as the government officially announced the reduction in the military service only four months before its implementation. In such a short period, the first post-reform enlistment cohorts could not adjust their strategy regarding the place of study as this is largely determined by the kind of high school (e.g. public or private) they decide to attend, and such choices are made at least three years prior to enlistment. Further, the students in high schools have to choose a domain of specialization at least two years before they graduate and this choice narrows down the university studies that they can then follow. While we cannot completely rule out that the announcement of the reform affected some students' decisions, it seems implausible at least for the first couple of post-reform cohorts that it could be a key determinant of their choice to study in Cyprus and of their intended domain of studies.

overwhelming majority of those students (65%) entered the University of Cyprus.[6] Students who are admitted to state universities do not pay fees. Student selection for schools within the University of Cyprus is made based on the scores from national exams and on the conditions that the schools set. We use these national exam scores to control for heterogeneity in student ability.

Our dataset includes all students enrolled at the University of Cyprus from September 2008 to September 2019.[7] We have information on their entrance exam scores, all the exams they 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.[8] For the courses with non-numeric grades (such as "success" or "failure"), we coded "pass" and "fail" as sample means, where passing exams were assigned the value of 7.37 (the sample mean of successfully passed exams), while failed exams were assigned the value of 2.4 (the sample mean of failed exams). We measure the academic performance of students by constructing their GPA across all the exams they have taken.

Extra information includes the ID number of each student,[9] the level of studies (undergraduate or postgraduate; first, second or third degree; etc.), the term of admission,[10] gender, year of birth, age on admission, district of residence, citizenship and number of academic terms (i.e. semesters) served in the army.

In the analysis we keep only students of Greek Cypriot nationality who attended a public high 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. Since the School of Medicine was established in 2013, we do not include observations of 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 September 2008 to September 2019.

Figure 1 shows the distribution of the GPA (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).[11] The majority of the observations (63.4%) lies to the right of the mean.

[6] http://www.statscy.com

[7] Our starting year is 2008 since this was the first year that admission grades were collected from the University. We have chosen to end our observation window prior to the Covid-19 pandemic as the pandemic might have affected students' academic performance as well as instructors' grading behavior.

[8] A passing grade is considered any grade, on a scale from 0 to 10, of 5 and above. Exams with non-numeric grades comprise 2.92% of our sample (at the course level).

[9] To protect privacy, student ID numbers are coded at the University level

[10] Term of admission refers to the school term when the student enrolled in the studies, not the academic term they were accepted to the university.

[11] This mass of observations can be attributed to students that do not complete their studies at the University of Cyprus (i.e. drop-outs). In the Online Appendix we replicate our main analysis using a sample that does not contain such students, to reassure the reader that our results do not hinge on their inclusion. However, for our main analysis, we utilize those observations since the effect of army training should not be restricted to students that finish their studies.

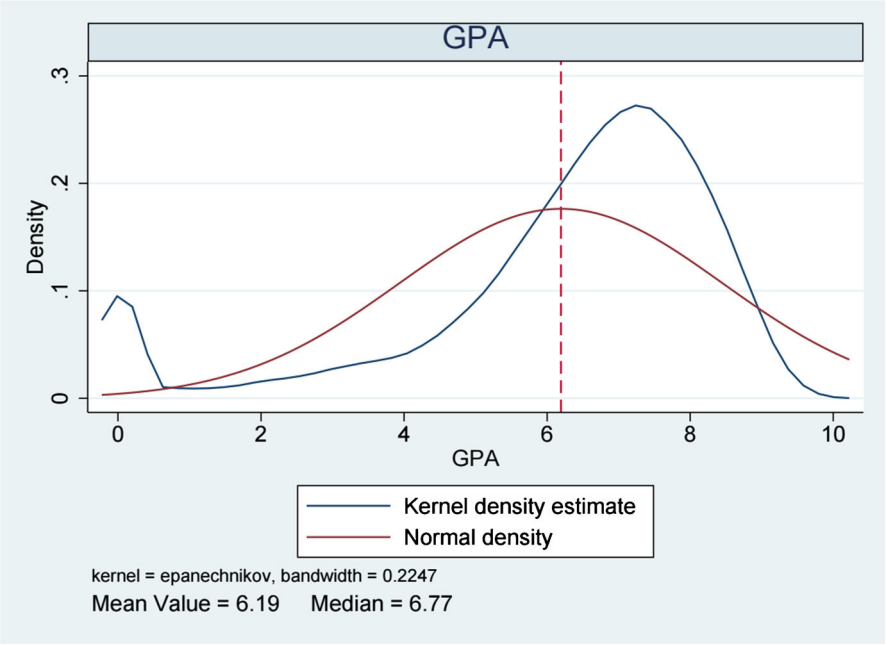

Fig. 1 Grade point average (GPA) distribution

Table 1 presents the distribution of the main variables of interest by gender.[12] Males make about one third of the sample and on average they enter university 1.41 years older than their female peers due to compulsory conscription. They have slightly lower admission grade (0.11 units), and they attain slightly lower grades while at university (0.06 units).[13]

6 Empirical strategies

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

6.1 Selection on observables analysis

In the selection on the observables analysis, we first look at the magnitude and the sign of the correlation between GPA and time spent in the army. We then examine grade

[12] Descriptive statistics for the rest of the variables (admission term, year of birth and district of student's origin) are available in the Online Appendix Table A5. Also, Online Appendix Tables A6a, A6b, A6c, A6d and A6e provide descriptive statistics analysis for each before/after sub-period pertinent to the DID analysis.

[13] The difference in admission grades between males and females is statistically significant, while the corresponding difference in GPA is not.

Table 1 Summary statistics of main variables

	All	Males	Females
Age at admission			
Mean	18.11	19.11	17.70
St.deviation	0.91	0.81	0.58
Minimum	17	17	17
Maximum	21	21	21
Admission grade			
Mean	17.82	17.74	17.85
St.deviation	1.55	1.51	1.56
Minimum	8.75	10.67	8.75
Maximum	20	19.99	20
Army term count			
Mean	0.57	1.97	0
St.deviation	1.29	1.71	0
Minimum	0	0	
Maximum	6	6	0
Grade point average (GPA)			
Mean	6.19	6.15	6.21
St.deviation	2.26	2.18	2.30
Minimum	0	0	0
Maximum	10	9.88	10

Descriptive statistics of all the other variables (year of birth, admission term, and district of student's origin) are available in the Online Appendix

heterogeneity using quantile regressions. GPA per student is our dependent variable and we include student-specific controls to our specifications:

$$GPA_i = a + \beta_1 male_i + \beta_2\ army\ term\ count_i + \sum\nolimits_{k \in K} \beta_k\ entry_i^k + u_i,$$

where $male_i$ takes the value of 1 is the student is male, 0 otherwise, *army term* $count_i$ represents the number of school terms spent in the army, and $entry_i^k$ equals one if student i enrolled at the university during school term $k \in K$, for $K = \{2008Sept, 2009Jan, 2009Sept, ..., 2019Sept\}$, zero otherwise.[14] Since there may be other factors that determine GPA, we perform a five-step sensitivity analysis by successively increasing the number of controls that may account for differences in the GPA across students. First, we add the *admission* $grade_i$ that proxies for student ability. Then, we add the variable *admission* age_i which indicates student's age at the time they enrolled at the university as age has an important effect on grades through student maturity. To control for additional cohort effects, we subsequently add *year of*

[14] No admission takes place in any month other than September or January, and in a typical year almost all admissions take place in September.

$birth_i^h$, which is a class of dummy variables, where $year\ of\ birth_i^h = 1$ if student i was born in year h, zero otherwise; with $h \in H = \{1988,..., 2001\}$.[15] Finally, to capture location differences across students we include $district_i^j$ which is a set of dummies indicating the district of student's origin, where $district_i^j = 1$ if student i originates from district j, zero otherwise; with $j \in J = \{$Nicosia, Limassol, Larnaka, Paphos, Famagusta, Kerynia, Morfou$\}$, the set of LAU 1 districts of the Republic of Cyprus.[16]

We also look at how army training impacts GPA 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 GPA benefit from the compulsory military training in the same way as their high achieving peers.

6.2 A difference-in-differences analysis

To examine the effect of the reforms on students' GPA we apply a difference-in-differences (DID) estimator. We exploit the gender-specific nature of the reforms and compare the GPA of the affected population (male students) to the GPA of the unaffected population (female students) over a short event window around each reform ($+/-$ 2 years) (see Carletti et al. 2021 for a recent application of a similar approach). If army service has a sizable effect on GPA, then the GPA of male students (treatment) should change after each reform compared to the GPA of female students (control). In specific, if the effect is positive, then after the first (second) reform the GPA of male students should increase (decrease) in relation to the GPA of female students, and vice versa.

The benchmark DID specification explains the student's GPA as a function of only three variables - the treatment dummy, the reform dummy, and their interaction, which is our variable of interest - and district dummies. 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 GPA 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 specification 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–

[15] There is a high degree of collinearity between year of birth, age at admission and school term admission. This collinearity is not perfect though as some students enrol in the university at different terms of the same year.

[16] Eurostat's LAU (Local Administrative Units) level 1 was formerly known as NUTS level 4.

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

2017 several students from both regimes enrolled. A shorter time 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 criticism.[17]

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.

Our DID specifications look as follows:

$$GPA_i = a + \beta_1 male_i + \beta_2 reform + \beta_3 male_i * reform + \sum\nolimits_{j \in J} \beta_j district_i^j + u_i$$

The DID specifications differ according to the specific reform variable employed in each one of them, and the corresponding 2-year window. We estimate four main specifications, two with actual reforms (Reform 1 and Reform 2) and two with placebo reforms (Placebo 1 and Placebo 2). As mentioned above, to check for robustness we also add as an extra control age at admission to capture age maturity effects.

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

The reforms in army duration constituted exogenous shocks in the army-acquired skills of the male student population, providing the opportunity for causal inference.

[17] While changes in such army-related variables can be viewed as exogenous for the years immediately after the reforms, one should be careful how to interpret them in the long run. Students decide whether to enroll in an English speaking (private) high school or a Greek speaking (state) high school, also depending on where they aspire to conduct their university studies. For instance, access to Cypriot and Greek state universities is significantly easier through public Greek speaking high schools, while graduates of private English speaking high schools mainly go to the UK and other foreign/private universities. It is plausible that a reform in the regulations regarding army exemptions/duration can affect the high school choice of a male student and subsequently the characteristics of male students that continue their studies in Cyprus. For this reason it is necessary to study the effect of the reforms only considering windows of short duration, definitely shorter than the three-year duration of senior high school ("lykeio"), which is the last major milestone for deciding country of university studies.

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.

7 Results

In this section, we present the results from the selection on observables and from the difference-in-differences analyses.

7.1 Selection on observables

As outlined above, we aggregate all the different course grades the student received across all their exams at the university and compute the GPA for each student, which we then regress against a set of individual-specific variables. We use a selection on observables model that we estimate through ordinary least squares and we provide robust standard errors. For the sake of comparison and brevity we present estimates for the variables of interest only.

We interpret the magnitude of the estimated coefficients with caution given that there could be other characteristics (i.e. health status, family characteristics) that affect student academic performance that we cannot control for in this analysis. Table 3 shows that males achieve lower GPA than females, ceteris paribus. For instance, column 5 suggests that a male's student GPA is about 0.3 units lower than the score of his female peer. 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 GPA at the university. For instance, column 5 suggests that an increase

Table 3 Selection on observables

	(1)	(2)	(3)	(4)	(5)
Male	-0.492***	-0.104*	-0.188***	-0.260***	-0.277***
	(0.061)	(0.059)	(0.069)	(0.081)	(0.081)
Army term count	0.258***	0.079***	0.067***	0.068***	0.076***
	(0.022)	(0.021)	(0.022)	(0.023)	(0.023)
Admission grade		0.494***	0.497***	0.496***	0.499***
		(0.013)	(0.013)	(0.013)	(0.013)
N	12,008	12,008	12,008	12,008	12,008
adj. R^2	0.019	0.125	0.126	0.126	0.133

Column 1 includes the following controls: male dummy, army length (army term count) and school term enrolment dummies; Column 2 adds admission grade; Column 3 adds admission age; Column 4 adds year of birth dummies; Column 5 adds district dummies. Robust standard errors in parentheses. Levels of significance: $*p < 0.1$; $**p < 0.05$; $***p < 0.01$

in army length by four academic semesters (two years) increases male GPA by 0.3 units, all else equal, while an increase in admission grade by one unit increases GPA by 0.5 units for both males and females.

We then examine how army duration affects male GPA at different levels of academic achievement by running quantile regressions for each of the five specifications we outlined above. The results from this exercise are presented in Table 4. As in Table 3, they suggest that the longer the length of military training, as captured by the army term count variable, the higher the GPA of male students. We find the biggest effect at the lower end (10th quantile) of the GPA distribution suggesting that army service is more beneficial for low than for high academic ability male students. Also, the effect seems to be monotonically decreasing. As we move up the GPA distribution the coefficient of the army term count variable decreases proportionally. For instance, an extra academic semester spent in the army increases GPA by about 0.4 units at the 10th quantile, while it increases it by about 0.2 units in the 90th quantile. The greater influence of CMS in the lower quantiles is similar to the results by Card and Cardoso (2012) who show that for low-educated draftees compulsory army service produces positive effects on earnings. It is also in line with results from Bingley et al. (2022) who find that the effect of military service on numeracy and literacy skills is higher for men draftees with low AFQT scores.

7.2 The difference-in-differences (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, age at admission, school term enrollment and other relevant controls, 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. In a robustness check we also control for the student's admission age, as a change in the duration of the CMS affects age on admission and thus our results could be conflated with age maturity 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

Table 4 Selection on observables, quantile regressions

	(1)	(2)	(3)	(4)	(5)
10^{th} percentile					
Male	0.036	0.219	0.005	-0.160	-0.222
	(0.296)	(0.247)	(0.262)	(0.295)	(0.266)
Army term count	0.382***	0.174**	0.158*	0.163***	0.214***
	(0.092)	(0.072)	(0.082)	(0.063)	(0.077)
Admission grade		0.619***	0.607***	0.593***	0.555***
		(0.053)	(0.046)	(0.062)	(0.055)
25^{th} percentile					
Male	-0.777***	-0.353***	-0.403***	-0.486***	-0.508***
	(0.101)	(0.088)	(0.075)	(0.111)	(0.117)
Army term count	0.313***	0.100***	0.075***	0.085**	0.079*
	(0.034)	(0.026)	(0.025)	(0.034)	(0.041)
Admission grade		0.667***	0.672***	0.669***	0.681***
		(0.019)	(0.026)	(0.025)	(0.023)
50^{th} percentile					
Male	-0.688***	-0.267***	-0.300***	-0.304***	-0.299***
	(0.057)	(0.054)	(0.056)	(0.062)	(0.084)
Army term count	0.268***	0.072***	0.063***	0.072***	0.071***
	(0.020)	(0.022)	(0.022)	(0.022)	(0.017)
Admission grade		0.565***	0.565***	0.560***	0.561***
		(0.013)	(0.014)	(0.014)	(0.015)
75^{th} percentile					
Male	-0.542***	-0.149***	-0.198***	-0.221***	-0.239***
	(0.064)	(0.049)	(0.049)	(0.055)	(0.060)
Army term count	0.241***	0.063***	0.058***	0.060***	0.065***
	(0.020)	(0.021)	(0.017)	(0.014)	(0.021)
Admission grade		0.478***	0.479***	0.481***	0.480***
		(0.009)	(0.008)	(0.007)	(0.006)
90^{th} percentile					
Male	-0.432***	0.008	-0.040	-0.018	-0.028
	(0.070)	(0.053)	(0.062)	(0.073)	(0.056)
Army term count	0.217***	0.049***	0.045*	0.049***	0.044**
	(0.021)	(0.019)	(0.023)	(0.019)	(0.021)
Admission grade		0.427***	0.427***	0.425***	0.429***
		(0.009)	(0.009)	(0.009)	(0.008)
N	12,008	12,008	12,008	12,008	12,008

Column 1 includes the following controls: male dummy, army length (army term count) and school term enrolment dummies; Column 2 adds admission grade; Column 3 adds admission age; Column 4 adds year of birth dummies; Column 5 adds district dummies. Robust standard errors 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)	(5)
Panel A					
Reform 1	0.312***				
	(0.112)				
Placebo 1		-0.130			
		(0.104)			
Placebo 2			0.039		
			(0.102)		
Reform 2				-0.208**	-0.312***
				(0.089)	(0.096)
N	1,127	1,279	1,351	1,538	1,340
adj. R^2	0.023	0.021	0.012	0.006	0.009

The dependent variable across all columns is army term count. In column 1, Reform 1 takes the value of 1 if t $\geq$ 2010, 0 otherwise. In column 2, Placebo 1 takes the value of 1 if t $\geq$ 2012, 0 otherwise. In column 3, Placebo 2 takes the value of 1 if t $\geq$ 2014, 0 otherwise. In column 4, Reform 2 takes the value of 1 if t $\geq$ 2016, 0 otherwise. Column 5 re-estimates column 4 but excludes irregular enrollments (enrollments taking place in the middle of the academic year). All columns include also district dummies. The time dimension of the reforms and of the placebos is a two-year time window around the corresponding reform/placebo (see Table 2). Robust standard errors in parentheses. Levels of significance: * p $<$ 0.1; ** p $<$ 0.05; *** p $<$ 0.01

during the years 2008 and 2009 served significantly less on average compared to the ones that enrolled in 2010 and 2011. Column 4 suggests that 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). This assures that the selection of real and placebo reform dates is appropriate.

In Table 6 we again focus on male students and conduct a similar exercise but we focus this time on a coarser variable. Instead of regressing the duration of army service on real and placebo reform dates, here the dependent variable is a dummy variable that takes the value one if the male student went to the army, zero otherwise. As we observe, students that enrolled during the years 2008 and 2009 served in the army in a significantly smaller proportion compared to the ones that enrolled in 2010 and 2011 (Column 1). Moreover, the proportion of students serving in the army did not change in a similar magnitude either after the second reform (Column 5), or about the placebo reform dates (Columns 2 and 3). Indeed, while in column 4 we report a sizable positive effect of the reform dummy on the probability that a male goes to the army, this is only an artifact of the larger number of males who finished their army service during the 2016 academic year due to the second reform (two cohorts of conscripts entered the university, instead of one). In column 5 we focus only on regular enrolments that take place at the beginning of the academic year—and hence on a sample that is comparable

Table 6 Selection of reforms and placebos

	(1)	(2)	(3)	(4)	(5)
Panel B					
Reform 1	0.096***				
	(0.029)				
Placebo 1		-0.046*			
		(0.027)			
Placebo 2			0.017		
			(0.026)		
Reform 2				0.096***	0.037
				(0.024)	(0.026)
N	1,127	1,279	1,351	1,538	1,340
adj. R^2	0.023	0.021	0.012	0.006	0.009

The dependent variable across all columns is a dummy variable that takes the value of 1 if the student went through compulsory military training, 0 otherwise. In column 1, Reform 1 takes the value of 1 if t $\geq$ 2010, 0 otherwise. In column 2, Placebo 1 takes the value of 1 if t $\geq$ 2012, 0 otherwise. In column 3, Placebo 2 takes the value of 1 if t $\geq$ 2014, 0 otherwise. In column 4, Reform 2 takes the value of 1 if t $\geq$ 2016, 0 otherwise. Column 5 re-estimates column 4 but excludes irregular enrollments (enrollments taking place in the middle of the academic year). All columns include also district dummies. The time dimension of the reforms and of the placebos is a two-year time window around the corresponding reform/placebo (see Table 2). Robust standard errors in parentheses. Levels of significance: * p < 0.1; ** p < 0.05; *** p < 0.01

in number of observations with those of the first three columns—and we find no effect of the second reform on the proportion of students that served in the army.[18]

Finally, we note that there are indications of a moderate rebound of army avoidance rates 2 years after the implementation of the first reform (column 2). This is consistent with the idea that if individuals are provided with enough time to adjust their behavior following a reform, they can be more efficient than if they are required to immediately respond to a sudden policy change. Overall, not only do we present evidence that the two reforms affected the average army duration of male students in the expected direction, but also we show that the first reform operated exclusively at the extensive margin, while the second reform induced significant changes only at the intensive margin.

We now proceed with our main DID analysis. We start by inspecting the yearly change in the gap between the GPA of male students vis-à-vis female ones. As we see in Fig. 2, the absolute value of this change takes its highest value in 2010 and its second highest in 2016; the two reform years. This provides the first indication that

[18] Irregular enrolments—i.e. enrollments taking place in the middle of the academic year—in all years, except the transition year 2016, are negligible; they represent less than 1% of the total enrolments. In 2016 the irregular enrolments reached 14% of total enrolments, and more than 92% of them were males that served in the army. Hence, including them in the tests regarding the proportion of males that served in the army naturally biases the results. Importantly, though, the results regarding army service duration are not sensitive to this shift in proportions. This is confirmed by column 5 of Table 5, where we also focus on regular enrolments.

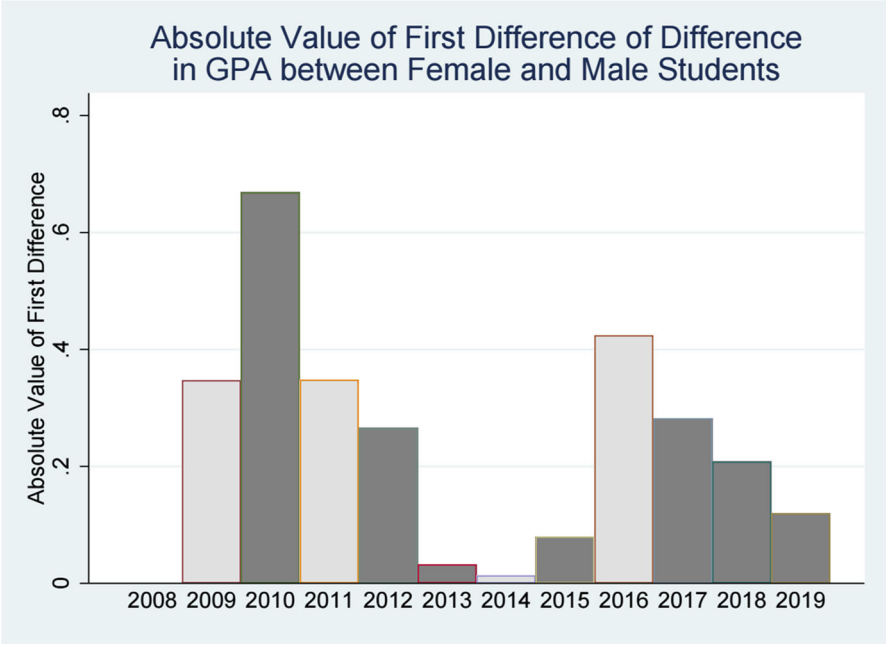

Fig. 2 Absolute value of first difference of difference in GPA between female and male students. Light grey represents positive values and dark grey negative values

the comparative GPA of male and female students changes a lot more when reforms change the policies that affect the average army duration.

The main findings are presented in Table 7. Our variables of interest 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 GPA, whereas in column 4 the interaction term between the male dummy and reform 2 predicts negatively GPA. These effects are both statistically significant, albeit at different significance levels.

In Fig. 3, we illustrate these discontinuities by means of a binned scatter-plot 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 for the male students (dashed line) changes dramatically before and after each reform, in the direction predicted by the corresponding change in average army duration.[19] For instance, the line for males discontinuously jumps upwards after the first reform suggesting an increase in GPA. In sharp contrast, it discontinuously jumps downwards after the second reform suggesting a decrease in GPA for males.

[19] Although we cannot determine the factors that can explain the downward trend in the female GPA over time, it follows the same trend as the admission grades. In unreported regressions we find that admission grades deteriorate over time both for males and females, but this deterioration is more pronounced for females over males.

Table 7 Difference-in-differences analysis

	(1)	(2)	(3)	(4)	(5)
Male × Reform 1	0.337**				
	(0.148)				
Male × Placebo 1		0.106			
		(0.148)			
Male × Placebo 2			0.044		
			(0.150)		
Male × Reform 2				-0.265*	-0.501***
				(0.148)	(0.162)
N	4,486	4,681	4,398	4,141	3,655
adj. R^2	0.007	0.008	0.006	0.010	0.017

The dependent variable across all columns is GPA. Column 1 includes a dummy variable for male, a dummy variable for reform 1 and their interaction effect. Column 2 includes a dummy variable for male, a dummy variable for placebo 1 and their interaction effect. Column 3 includes a dummy variable for male, a dummy variable for placebo 2 and their interaction effect. Column 4 includes a dummy variable for male, a dummy variable for reform 2 and their interaction effect. Column 5 includes the same variables as column 4, but males that did not serve in the army were removed from the sample. Reforms 1, 2 and Placebos 1, 2 are defined in the notes of Tables 5 and 6. All columns include also district dummies. The time dimension of the reforms and of the placebos is a two-year time window around the corresponding reform/placebo (see Table 2). Robust standard errors in parentheses. Levels of significance: *p $<$ 0.1; **p $<$ 0.05; ***p $<$ 0.01

The magnitude of changes seems to be larger than what a simple reading of the estimates in the selection on the observables analysis would suggest. For example, the differences-in-differences coefficient of the interaction variable (Male X Reform 1) in Table 7, Column 1 (0.337) along with the coefficient of the Reform 1 variable in Table 5, Column 1 (0.312) suggests that the effect is almost proportional, i.e. for x extra terms in the army the GPA increases by almost x points. The results regarding the second reform yield similar conclusions. On the contrary, the highest army term count coefficient in Table 3, Column 1 (0.258) is indicative of a significantly smaller effect. However, if one takes into account the estimated robust standard errors associated with the above coefficients, one observes that the estimates of the two approaches are not so distant.[20] That is, while there is some discrepancy between the two approaches, in statistical terms it is not as large as it appears at first sight. Any remaining differences in the estimated coefficients between the two models could be due to missing confounding variables that might cause underestimation of the true magnitude of the effect in the selection on the observables model.

[20] For instance, if we assume that the real effect of the first reform on army term duration is equal to 0.312 additional terms (as indicated by Table 5, Column 1), then, by Table 7, column 1, the lower bound of the 95% confidence interval of the effect of an increase of one term of army service on GPA is $(0.337 - 1.96 \times 0.148)/0.312 = 0.150$. This estimate is well below the relevant coefficient of the army term count variable reported in column 1 of Table 3, and very close to the upper bounds of the 95% confidence intervals that can be constructed by the relevant coefficients and robust standard errors reported in the remaining columns of Table 3.

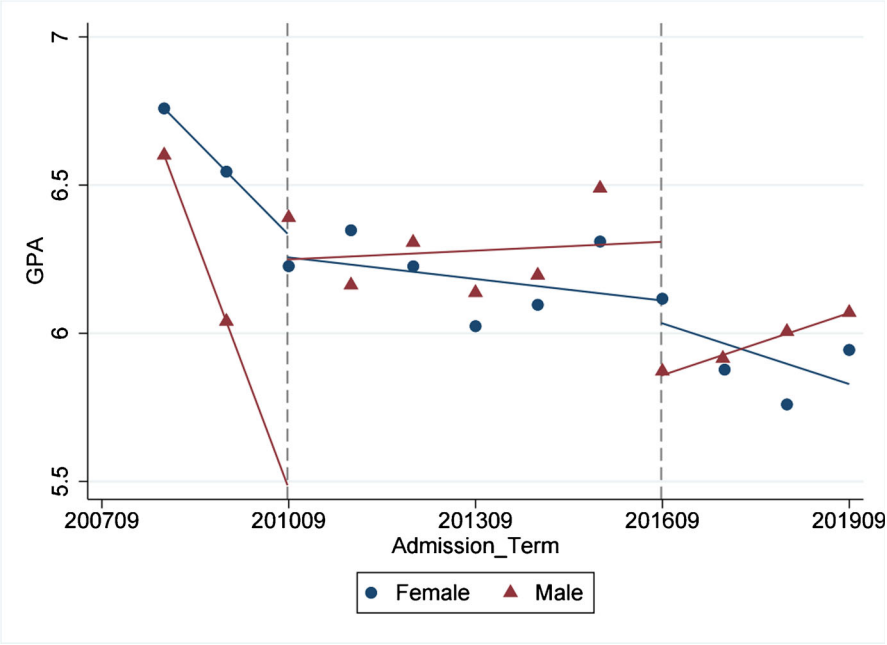

Fig. 3 Estimated GPA based on admission term with cutoffs at July 2010 and July 2016

While the signs of these effects fully align with the signs of the changes in average army service, one can further exploit the remaining data to strengthen the results. To provide further confidence that our estimates do not pick up effects that are reform-independent, we conduct a placebo analysis focusing on two placebo reform dates.[21] In columns 2 and 3 of Table 7, we repeat the same exercise with the only difference being that the placebo reform takes place two years after (before) the first (second) reform actually took place respectively. 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. These findings provide evidence that it was only at the actual reform periods that the difference between the performance of male and female students changed substantially. Indeed, these placebo regressions establish that, before the second reform, a parallel trends hypothesis cannot be rejected, something that is also visualized in Fig. 4.

Despite the fact that the second reform involved explicit changes only in the duration of army service (intensive margin), it might also have indirect effects on the number of males that served (extensive margin). Indeed, a reduced duration of army service might have led to an increased number of conscripts (conscripts have weaker incentives to try to avoid army, and the army has stronger incentives to draft as many conscripts

[21] The placebo regressions establish parallel trends before the second reform only, as our sample starts from 2008 onward. Thus, we cannot directly test the parallel trend assumption with respect to the first reform. Despite this limitation, the results regarding the first reform are valuable. Although they are weaker, they are in line with the evidence provided throughout the paper.

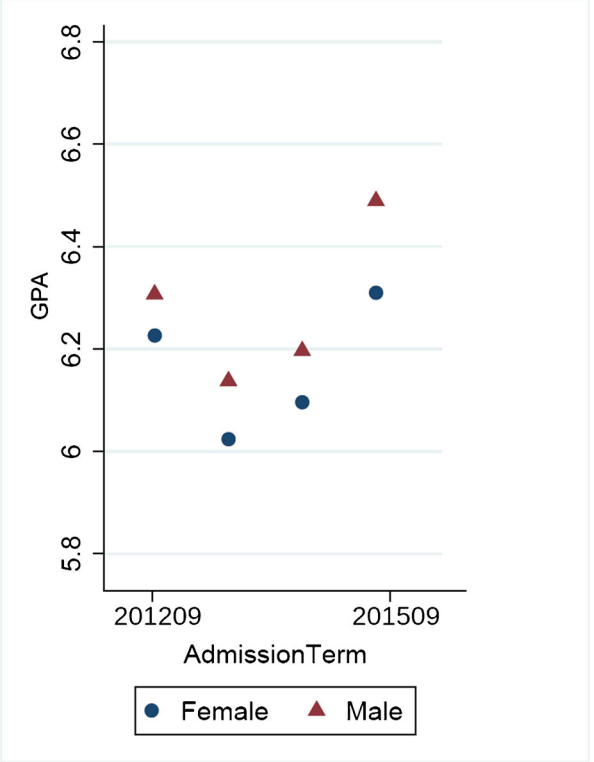

Fig. 4 Visualization of parallel trends between the GPA of female and male students before the second reform

as possible to maintain maximum capacity in existing divisions/barracks).[22] Notice, that such indirect effects, if present, should operate in the opposite direction of the former direct effects on the average duration of army service (i.e. a reduction in the time served by each conscript, could lead to an increased conscription rate, mitigating the negative effect of the reform on the average duration of army service). If this line of reasoning is sound, then by removing males that did not serve at all from the DID analysis centred around the second reform should strengthen the results. This is exactly what we find in column 5 of Table 7.

Finally, in Table 8, we replicate Table 7 but also control for admission age, as a change in the duration of the CMS affects age on admission and thus our results could be conflated with age maturity effects. The estimated coefficients across all columns remain almost intact suggesting the validity of our difference-in-differences exercise in dealing with omitted variables bias.

Despite the fact that the results from this difference-in-differences exercise reinforce the selection on observables results, it is important to revisit a number of issues outlined

[22] In our sample, the first birth cohort affected by the second reform (the birth cohort of 1997) is characterized by a higher conscription rate than the other cohorts that are pertinent to the analysis of the second reform (it is above 76%, while the other ones were below 70%).

Table 8 Difference-in-differences analysis, robustness check to adding age at admission

	(1)	(2)	(3)	(4)	(5)
Male × Reform 1	0.340**				
	(0.148)				
Male × Placebo 1		0.107			
		(0.148)			
Male × Placebo 2			0.045		
			(0.150)		
Male × Reform 2				-0.256*	-0.509***
				(0.150)	(0.166)
N	4,486	4,681	4,398	4,141	3,655
adj. R^2	0.007	0.008	0.006	0.010	0.017

This table replicates Table 7 (difference-in-differences analysis), but adds age at admission as an extra control. Robust standard errors in parentheses. Levels of significance: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

above. While the reforms indeed constituted 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 only to 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.

Overall, no single part of the empirical analysis is immune to different kinds of criticism. The selection on observables analysis is free of concerns regarding self-selection and endogenous timing but it is susceptible to possible omitted variables bias problems, while the DID exercise relies on significant yet non-instantaneous reforms. However, when viewed together they draw a consistent picture, thus providing a strong set of evidence supporting the idea that CMS positively impacts academic performance.

8 Conclusion

Given that the literature is still divided on the effects of CMS on educational attainment, we set out to examine whether and how army training affects academic performance while at university. The data available from the University of Cyprus present a unique opportunity to analyse the net effect of the duration of conscription on GPA. This is the first study to assess the possible influence of army training on GPA in such a direct

fashion. We utilize exogenous changes in CMS policy that were materialized through two reforms: a reform that increased draft coverage and thus average army service duration (first reform), and a reform that decreased average army duration (second reform). In this way, we can see how changes in both directions influenced the grades former male draftees achieved on their university exams. We find that the first reform increased the GPA of male students, whereas the second reform decreased it relative to their female peers.

The results of all of our tests suggest that compulsory army service improves student academic performance as captured by GPA. 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 apprenticeship/vocational programs. It is our belief that army training works similarly to apprenticeship/vocational programs to increase responsibility and perseverance while also improving both dexterity and communication skills. Young men who enter university are thus better prepared, in terms of enhanced non-cognitive skills, to meet the academic requirements of a tertiary education setup. This is what shows up in the data: the longer the time period male students spend in army training, prior to the university enrolment, the better their academic performance.

Our results also inform policy makers for the unintended benefits of military service on student academic performance, especially on the academic performance of low ability students. However, CMS may not be the only route that can 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, both for males and females. Of course, it is yet to be established if the proposed mechanism behind the skill development during CMS is what drives better academic performance. Future research into the effectiveness of army training in development of transferable skills could lead to a better understanding of this mechanism. Further, it could also provide evidence on the type of training programs that help advance the educational outcomes for students who may suffer from a lack of skills necessary for academic success.

Finally, it is important to stress that the identified increase in GPA that is linked with army duration might come 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 educational outcomes. Thus, it strengthens the foundation for future research on examining the mechanisms though which non-cognitive skills affect educational outcomes.

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obtained through the University of Cyprus via an application to the Data Protection Commissioner Office of the Republic of Cyprus. All necessary actions have been taken so that students cannot be identified. The authors can provide replication files upon request.

Declarations

Conflict of interest The authors declare no competing interests.

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