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ABSTRACT

Can Compulsory Military Service Raise Civilian Wages? Evidence from the Peacetime Draft in Portugal*

Although the practice of military conscription was widespread during most of the past century, credible evidence on the effects of mandatory service is limited. Angrist (1990) showed that the Vietnam-era draft in the U.S. lowered the early-career wages of conscripts, a finding he attributed to the low value of military experience. More recent studies have found a mixed pattern of effects, with both negative (the Netherlands) and positive (in Sweden) earnings impacts. Even among Vietnam era draftees, Angrist and Chen (2011) find that the net effect on earnings by age 50 is close to zero. We provide new evidence on the long-term impacts of peacetime conscription in a "low education" labor market, using longitudinal data for Portuguese men born in 1967. These men were inducted at a relatively late age (21), allowing us to use pre- conscription wages as a control for potential ability differences between conscripts and non- conscripts. Our estimates of the average impact of military service for men who had entered the labor market by age 21 are slightly positive (1-2 percent) but not significantly different from zero throughout the period from 2 to 20 years after their service. These small average effects arise from a significantly positive later-life impact for men with only primary education, coupled with a zero-effect for men with higher education. The positive impacts for less-educated men suggest that mandatory service can be a valuable experience for poorly-educated men who might otherwise spend their careers in low-level jobs.

JEL Classification: J31, J24

Keywords: military conscription, longitudinal earnings, quasi-differences, sensitivity analysis

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Introduction

Throughout the 20th Century young men in most Western countries faced the risk of military conscription. Although compulsory service ended in the U.S. in 1973, the practice continued until very recently in many European nations, and is still widely used around the world.[1] Spurred in part by recent decisions to end conscription in Sweden, Italy, France, and Germany, there is renewed interest in understanding the impacts of mandatory service on a wide range of outcomes, including earnings (Angrist and Chen, 2011; Angrist, Chen, and Song, 2011; Grenet et al., 2011; Palayo, 2010), education (Maurin and Xenogiani, 2007; Cipollone and Rosolia, 2007; Keller et al., 2009; Bauer et al., 2009), health (Bedard and Deschenes, 2006; Dobkin, 2009; Autor et al., 2011), and crime (Galliani et al., 2011).

Revealed preference arguments suggest that conscripts will suffer economic losses from coerced service (e.g., Oi, 1967). Nevertheless, a number of analysts have argued that compulsory service could have a positive return for disadvantaged youth who face limited civilian job opportunities (e.g., Berger and Hirsch,1983; de Tray, 1982).[2] Seminal research by Angrist (1990) showed that military service reduced the earnings of Vietnam-era draftees, a finding he attributed to the low value of military experience in the civilian labor market. Subsequent research in the U.S. and other countries, however, has uncovered a surprisingly mixed pattern of impacts. Imbens and van der Klaauw (1995) found that 10 years after conscription Dutch veterans earned lower wages than those who avoided service. In contrast, Albrecht et al. (1999) estimated a persistent positive earnings premium for Swedish conscripts. Grenet et al. (2011) find no long-run impact on the wages of British conscripts; likewise, Bauer et al. (2009) find no effect for West German conscripts.[3] In a recent re-analysis of the Vietnam-era draftees, Angrist and Chen (2011) find that by age 50 they have about the same earnings as non-draftees, though slightly higher education.

In this paper we present new evidence on the long-run effects of mandatory military service, using detailed longitudinal data for Portuguese men born in the late 1960s. Several features of the

[1]In Europe, for example, Austria, Denmark, Estonia, Finland, Greece, Norway, and Switzerland all still require men to perform some form of national service. Russia and China also have mandatory military service.

[2]A similar argument was made by Bonn (1916) regarding the impact of compulsory military service on the career prospects for German men from rural areas. He asserted that "...the average German peasant who has served his term is a brighter and better man than he would have been without it." Bonn (1916, p. 61).

[3]Kunze (2002) analyzes longitudinal data for German workers and finds a complex pattern of earnings premiums for veterans.

Portuguese setting and the available data make this evidence particularly valuable. First, these men were drafted at age 21 for a period of up to 2 years of (non-wartime) service. Second, they had relatively low levels of education – mean completed schooling at age 20 for Portuguese men born in 1967 was only 6.9 years.[4] The low levels of schooling and late age of conscription meant that many men had already entered full time work prior to their service, allowing us to use pre-enlistment wages to control for unobserved ability differences between those who served and those who did not. Third, the draft was designed to staff the military, and not as a universal social program: thus only about 40 percent of the men in the cohort were drafted. A fourth unique feature is the availability of high-quality administrative data spanning the period from 1986 to 2009. This data set – known as the Quadros de Pessoal (QP) – provides wage and hours information at a specific point in time each year for all private-sector wage earners in the economy. We use these data to track the cohorts of interest from their initial entry into the labor market until middle age. We also exploit the richness of the QP data to identify individuals who were drafted into military service, using the fact that workers who left a job to serve in the military were legally treated as being on leave of absence.

Our empirical analysis shows that the average impact of military service for men who had entered the labor market by age 21 is weakly positive but close to zero throughout the period from 2 to 20 years after their service. This small average effect, however, masks a statistically significant later-life impact of about 4-5% for men with lower levels of education (under 6 years of schooling), coupled with a zero-effect for men with higher education. The positive impact for the less-educated group mirrors the findings for U.S. veterans by Berger and Hirsch (1983) and confirms that mandatory service can be a valuable experience for disadvantaged men who might otherwise spend their careers in low-level jobs. Our confidence in these findings is bolstered by two important additional results. On one hand, we find little evidence of selection on unobserved ability in the induction process that governed conscription in the late 1980s in Portugal. Consequently, estimates of the impact of military service on post-service earnings are relatively robust to different assumptions about the relative impact of unobserved ability differences in pre-conscription versus post-conscription earnings. On the other hand, we find that by 7 or 8 years after the completion of their service conscripts have virtually the same private sector employment rate as non-conscripts,

[4]Portugal ranks last in average schooling achievement among the members of the European Union (Barro and Lee, 2010).

alleviating concerns about selection bias due to differential employment rates of veterans and non-veterans.

The next section of the paper provides a brief overview of the institutional background underlying the conscription process in Portugal in the late 1980s. Section 2 discusses the QP data set and our method for identifying conscripts, based on unpaid leave-of-absence status. We then provide a comparison of the enlisted, the non-enlisted, and other individuals in the birth cohort under analysis. Section 4 presents the details of our statistical approach, which takes advantage of the availability of pre-conscription earnings data to control for potential ability differences between conscripts and non-conscripts. Section 5 presents our main findings, first using graphical techniques, then using more formal regression models, including models that explore a range of possible values for the relative effect of unobserved ability in pre-conscription versus post-conscription earnings. We also explore possible mechanisms driving the enlistment effect. Section 6 concludes.

1 Military Service in Portugal

During the 1960s and early 1970s Portugal's wars in Angola, Mozambique, and Guinea-Bissau necessitated a far-reaching conscription system. Following the overthrow of the Estado Novo regime in 1974 and the end of the colonial wars, the Portuguese military transitioned to a smaller peacetime force.[5] Throughout the 1980s and early 1990s men were at risk of conscription in the year they turned 21, and draftees were required to serve for a maximum of 2 years.[6] Individuals were called for medical and psychological evaluations in the year they turned 20. Those judged physically or mentally unfit, those convicted of serious felonies, and those deemed to fall short of the "dignity and good moral standing" required by the military were exempted from service.[7] Men could also petition to be classified as conscientious objectors: if successful, they were required to perform alternative service (e.g., working in a hospital or local public administration) for a similar period. Short-term deferments could be granted to students and individuals who were the "sole providers" for their family, but options for self-selecting out of service altogether were quite limited.

Over the 1990s a series of legal changes lowered the age of conscription to 20 (effective in 1993 for men born in 1973 or later)[8] and reduced the duration of service to a maximum of 8 months

[5] Carrington and de Lima (1996) study the impact of the large number of retornados – Portuguese nationals who returned at the end of the colonial wars – on labor market conditions in Portugal.
[6] Law 2135.
[7] Men with a criminal record but no serious felony conviction served under a special disciplinary regime.
[8] Law 30/87 and Decree-Law 463/88.

for the 1971 and 1972 birth cohorts, and to 4 months for those born after 1973.[9] Conditions for deferments and exemptions from service were also gradually eased. Finally, in 2005, peacetime conscription ended and the Portuguese military became an all-volunteer force open to both men and women.

Table 1 shows the fractions of men in each year-of-birth cohort from 1965 to 1979 who were drafted to serve in the military. This fraction ranged from 35 to 50 percent, with a dip for the 1972 and 1973 cohorts, who were both subject to the draft in 1993. Note that military service was far from universal for the men in these cohorts. Instead, the size of the draft was set to meet the needs and budget limitations of the military. Each year the Ministry of Defense established a quota for new enlistees in the Army, Air Force and Navy. The three branches then determined the number of conscripts needed in various fields of specialization and selected candidates from the lists of eligible men.[10]

Once in the military, enlistees could undertake basic skills training as well as occupational training – for example, as a chef or a truck driver. Labor laws in Portugal specify that occupational training in the military is equivalent to civilian training, allowing some conscripts to accumulate transferable human capital during their service.[11] Other legislation required employers to treat drafted men as "on leave" and re-hire them at the end of their service. This may have discouraged firms from hiring young men until their conscription status was settled. For the men who were hired and subsequently drafted, however, it presumably eased the transition back to civilian life. Finally, conscripts in good standing could re-enlist for up to 8 years of additional service, though this option was not widely exercised during the 1980s and 1990s.

2 Data on Earnings and Conscription Status

The Quadros de Pessoal

Our analysis relies on a unique administrative data set, the Quadros de Pessoal (QP), collected annually by the Ministry of Employment. The QP is a census of paid workers in the Portuguese

[9] Law 22/91, Law 30/87, and Decree-Law 463/88. These new regulations on the duration of service effectively codified practices that were already established. Indeed, by the late 1980s, conscripts were often discharged earlier than the 24 months established by law.

[10] In this sense the conscription process was more similar to the process used in the U.S. in the 1960s, prior to the draft lottery, than to a "universal service" system in countries like France.

[11] Currently, such equivalence is guaranteed for training courses in the fields of electricity, plumbing, electronics, metal works, wood and furniture works, first aid and health support, professional driving, cooking, bakery, administrative support, music, and graphic design and multimedia.

private sector: all firms with at least one paid worker are legally obligated to return information on their full roster of employees, including wages and hours of work during the appropriate reference week (in March until 1993 and in October since 1994).[12] Importantly for our purposes, during the era of mandatory service the QP asked employers to include men on leave for military duty in their roster. We assume that these workers are reported with missing values for their earnings and hours of work in the reference week, and for simplicity refer to such employees as "on leave".[13] A limitation of the QP is that government workers – who comprise just under 20 percent of the Portuguese workforce – are excluded from coverage.[14] A second limitation is that the QP provides only a snapshot of labor market outcomes in each year. Thus, individuals who are unemployed or out of the labor force at the time of the census have no labor market data for that year. Electronic records from the QP are available for the period from 1986 to 2009, and include worker and firm identifiers that allow individuals to be tracked over time and across jobs. Worker-level data are unavailable for 1990 and 2001, creating gaps in the worker histories in those two years.

Information for employees in the QP includes gender, date of birth, current educational attainment, occupation, date of hire, base earnings, supplemental payments, and hours of work. Information for employers includes industry and location of the firm, gross annual revenues, and ownership status (foreign or domestic; private versus public). We use an edited version of the QP that has been checked to verify the consistency of the longitudinal matches (see the Data Appendix). We measure a worker's gross hourly wage by dividing the sum of the individual's monthly base-wage and other regularly-paid benefits by his or her normal hours of work.[15] All wages are deflated using the Consumer Price Index (2009=100). We treat as missing any wage observation that is below 0.75 of the first percentile of wages in a given year, or above 3 times the 99th percentile.

[12]Firms are required to post their employee rosters and the corresponding salary information in a public place visible by its workers, helping to ensure the accuracy of the reported information. During the 1980s there was some under-coverage in the QP, particularly of small firms (Braguinsky et al., 2011).

[13]Firms may also fail to report earnings and hours for other reasons, including long-term illnesses, strikes, and maternity leave – see Table A.1 in the Data Appendix. Unfortunately, the reason for leave status is not available in the electronic version of the QP.

[14]Also missing from the employee rosters are contract workers. In recent years such workers have accounted for a growing share of employment (Rebelo, 2003).

[15]Reported earnings are net of the employer portion of social security taxes, but include the employee portion of the tax, currently 11 percent.

Identifying Conscripts

Ideally we could merge conscription information from military records to the QP and conduct an analysis of the impacts of compulsory service for several cohorts. Unfortunately, individual service records are not available. Thus, we have to infer conscription status from the observed data in the QP. We focus on men inducted before 1993, when the term of service was still two years, and make use of the fact that employers were instructed to report workers who had been drafted as on leave. A complication is that some of the conscripts in a given year could be inducted early in the year, before the March date of the QP, and others could be inducted after the QP was completed. We therefore identify two separate groups of conscripts: (1) men who are recorded in the QP as working full time in March of the year they turned 20 years of age, and are "on leave" (i.e., reported with missing earnings and hours) in the next *two years*; (2) men who were working full time in March of the year they turned 21, and were on leave in March in the next year.[16] We compare these two groups to men who were working full time in March of the year they turned 21 and in March of the following year (and therefore could not have been inducted into the military at age 21 and served more than a year). Note that we can only identify conscripts and non-conscripts who were working just before or just after reaching the age of 21. Narrowing the focus to these men has the important advantage that we have a full-time wage observation for each person at (approximately) age 21. For conscripts, the wage is measured just prior to entering the military at age 20 or 21; for non-conscripts, it is measured in the year they turn 21.

To implement these definitions in the QP we need to limit attention to cohorts who reached the age of 20 in 1986 or later (the first year QP data are available). Moreover, since the QP has a gap in 1990, we cannot use data for cohorts born in 1968 or 1969, as their status in the years they turned 21 or 22 is unknown. Given these constraints, we focus on men born in 1967 as our primary cohort of interest. This is the only cohort for whom the required data are available *and* who were required to serve up to two years in the military if conscripted.

Before proceeding it is important to try to verify that men who were working full time at age 20 or 21 and then recorded as on leave were actually conscripted. While we cannot offer definitive proof, we conducted a series of comparisons summarized in Figure 1 that we believe are highly supportive of our assumptions. The upper left hand panel of the figure shows the distribution of

[16] As a robustness check we consider relaxing the criteria for the first subgroup slightly by requiring that they were employed full time in March of the year they turned 20 and on leave in the next March. We show below this has very little effect on our results.

activities in each year of age from 18 to 42 for men born in 1967 who were observed working full time at either age 20 or 21. Notice that the fraction of the cohort reported on leave is relatively high for only two years – ages 21 and 22 – and that at these ages the sum of the fractions observed working or on leave is very similar to the fraction observed working at ages 23 and older. This pattern strongly suggests that leave of absence status is associated with military service. By comparison, the upper right hand panel shows similar data for women born in 1967 who were observed working full time at either age 20 or 21. For women there is no "unusual" spike in the fraction on leave at ages 21 and 22, confirming that the draft is the likely explanation for the high fraction of men on leave at these ages.

Comparisons with later cohorts of men and women, presented in the middle and lower panels of the Figure, provide further evidence that men from the 1967 cohort who were on leave at ages 21-22 were in the military. In the middle panels we show data for people born in 1977 who were observed working full time at age 19 or 20. (We adjust the requirement on age to reflect the fact that conscription occurred at age 20 for this cohort). Consistent with the very short term of military service for this cohort (4 months maximum) we see only a relatively small rise in the fraction of men classified as on leave at age 20. For women born in 1977 the patterns look very similar to those of women born 10 years earlier. Finally, in the lower panels of Figure 1 we show data for men and women born in 1987 who were observed working full time at age 19 or 20. For this cohort there was no mandatory military service, and reassuringly only a trivial fraction of men are recorded as on leave between the ages of 20 and 22. Again, the data for women look relatively similar to the data for women born 10 or 20 years earlier. Overall, we believe that these comparisons provide strong support for our use of leave-of-absence status as an indicator of conscription status for men in the 1967 cohort.

3 The 1967 Birth Cohort: Conscripts, Non-Conscripts, and Others

Although we are reasonably certain that we can identify conscripts in the 1967 cohort who had established a strong labor force attachment by age 21, and a comparison group of non-conscripts with similar early attachment, there are many other men in the cohort whose draft status is unknown, including men who were working at age 20 or 21 but whose status at age 22 is unknown, and men who had not yet entered the labor market by age 21. Table 2 presents some comparative information on the various subgroups to help contextualize our main groups of interest.

Out of the full cohort of approximately 100,000 men born in Portugal in 1967 (Pordata, 2010), 92% are observed as private-sector employees in the QP at some point between 1986 and 2009. Of these, about 5% have some inconsistency in their data (i.e., a missing or outlier wage observation if employed, or a problem linking records over time). Deletion of these observations leads to a sample of 86,909 men born in 1967 and ever observed in the QP with valid data, summarized in column 1 of Table 2. Given the low schooling attainment of the men in this cohort, most were presumably out of school by age 19. Nevertheless only about one-fifth were working full time at age 20 or 21 and meet the criteria to be potentially used in our analysis.[17]

Column 2 shows the characteristics of the entire sample of "early entrants" (i.e., those who worked full time at age 20 or 21). Interestingly, the fraction of early entrants observed in the QP at least once between 2002 and 2009 is very similar to the fraction of the entire cohort observed in that interval (62% versus 61%), while the average wage of the early entrants is about 12% lower than the average wage for the entire cohort (potentially reflecting the lower schooling of the early entrants than other men in the cohort). The fact that the mid-career outcomes of the early entrants are relatively similar to those of the overall cohort is reassuring, and suggests that inferences about the impacts of military service based on the early entrant group may be generalizable to the broader population.

Among the early entrants we identify four subgroups: *conscripts* (column 4 of Table 2), who were either working full time in March 1987 and on leave the next two years, or working full time in March 1988 and on leave the next year; *non-conscripts* (column 5), who were working full time in both March 1988 and March 1989; *missing from the QP* in 1989 (column 6) – men who were unemployed, out of the labor force, or working in the government sector in March 1989; and finally a fourth *residual* group (column 7) made up of men with a variety of employment histories in the period from 1987 to 1989 that do not fit into the other 3 groups. Both the missing and residual groups presumably include both conscripts and non-conscripts. For example, men who were working full time in March 1988 but who lost their job later in the year and were either

[17] A concern about the legal requirement that draftees be allowed to return to their job at the end of their service is that this would discourage employers from hiring men before the age of 21. We conducted a difference in differences analysis of employment rates of men and women at ages 18, 19, and 20 between cohorts born in 1967 and those born in 1987, testing whether the employment rates of the 1967 cohort of men were unusually low. This shows a small male $\times$ 1967 interaction effect (around -1.5 percentage points) potentially indicating a small discouragement effect in the hiring of men when the draft was in effect.

serving in the military or unemployed a year later are in the missing group.

Comparisons across columns 4-7 show that the 1986-88 wages of the conscripted and non-conscripted groups are quite similar, while the wages of the group who are missing from the 1989 QP are a little lower, and those of the residual group are a little higher. The conscripts have the highest education levels of the four groups of early entrants, measured either in the first year they ever appear in the QP, or in 2002. Anecdotal evidence suggests that the military generally preferred men with higher literacy and numeracy skills, leading to a systematic under-representation of the lowest educated men in the conscripted group. Nevertheless, conscripts and non-conscripts have nearly the same probability of appearing in the QP in the period from 2002 to 2009 (when they were between 35 and 42 years of age), and fairly similar hourly wages. Interestingly, the group who were missing from the QP in 1989 have a much lower likelihood of appearing in the data set during 2002-2009, suggesting strong persistence in their low rate of private-sector employment. The residual group, by comparison, has about the same likelihood of appearing in the QP in the 2002-2009 period as the conscripts and non-conscripts.

Finally, column 8 of Table 2 shows data for men born in 1967 who were not working full time in either 1987 or 1988. Some of these men were attending post-secondary schooling. Consistent with this fact, the fraction of non-early entrants with a university degree is relatively high (8%)[18], and on average they have two more years of education than the early entrants. Despite their relatively late entry to the labor market, by mid-career they are about as likely to appear in the QP as the early entrants group (61% with a wage observation between 2002 and 2009, versus 62% for early entrants as a whole). By mid-career they also have about 17% higher average wages than the early entrants (mean log wage = 1.71 versus 1.54 for early entrants), presumably reflecting their higher education.

In the remainder of the paper we mainly focus on comparisons between early entrants who can be clearly identified as conscripts (column 4 of Table 2) and those who can be clearly identified as non-conscripts (column 5). In Section 5, however, we also present a number of robustness checks in which we consider relaxing the criteria to be included in the conscript and non-conscript groups, thereby moving some of the men from the missing and residual categories into these groups. As we show, plausible changes in the definitions of the two groups have little impact on our main results.

[18] A larger share attended college, given that the drop-out rate is approximately 30%.

4 Measuring the Causal Effect of Conscription on Subsequent Earnings

In this section we lay out the econometric framework that we use to measure the causal effect of conscription on subsequent earnings. As has been emphasized in the literature (e.g., Angrist, 1990) a key problem in evaluating the impact of veteran service is the non-random selection of conscripts. We address this problem by focusing on the effect of conscription on men who had already entered full time work before they were at risk of conscription. For these men, the wage prior to entering service can be used to control for unobserved ability differences that would otherwise confound observational comparisons between conscripts and non-conscripts, as in a standard "difference-in-differences" analysis.

To proceed more formally, let S denote the level of schooling of a given man as of the date just before the conscription decision is made. We assume that schooling at that point is a function of a general measure of ability a and a random error component u_1

$$S = f(a) + u_1,$$

where $f()$ is an unknown function. Let w_0 represent the logarithm of the hourly wage that is earned by the individual just prior to the determination of conscription status. We assume that this wage depends on ability, schooling, and an additive error component v_0 that is uncorrelated with schooling and enlistment status:

$$w_0 = a + \beta_0 S + v_0.$$

Note that we are scaling ability by assuming that expected log wages in period 0 vary 1 for 1 with a, holding constant schooling. Assume that the probability of enlistment (indicated by the binary variable E) depends on an index of ability and schooling:

$$pr(E = 1) = F[g(a, S)].$$

where $F[]$ is some distribution function (e.g., a normal or logistic), and $g()$ is an unknown function. In general this specification implies that veterans and non-veterans with the same education will have different average ability. For example, suppose that $g(a, S) = \gamma_1 a + \gamma_2 S$, where γ_1 and γ_2 are both positive, i.e., the military prefers candidates with higher ability and higher schooling. Under this assumption, conscripts with low schooling will only be accepted if they have above-average ability. Finally, we assume the wage in post-enlistment period $t > 0$ depends on an additive function of ability, schooling, enlistment status, and an error component v_t:

$$w_t = \psi_t a + \beta_t S + \theta_t E + v_t, \tag{1}$$

where ψ_t is a loading factor that can vary over time and θ_t is the effect of military service on wages in period t. We assume that v_t is uncorrelated with ability, schooling, and veteran status.

In general a simple regression of wages in period t on schooling and enlistment status will yield an estimate of θ_t that has probability limit:

$$p\lim \widehat{\theta}_t^{OLS} = \theta_t + \psi_t R_{a,E|S},$$

where $R_{a,E|S}$ represents the (population) regression coefficient of enlistment status from an auxiliary regression of ability on enlistment status and schooling. If the probability of conscription is higher for more able men, conditional on schooling, then $R_{a,E|S} > 0$, causing OLS to overstate the returns to enlistment.

Assuming that w_0 and w_t both depend additively on ability we can use the quasi-differencing approach suggested by Chamberlain (1982) to obtain an equation that does not depend on ability:[19]

$$w_t - \psi_t w_0 = (\beta_t - \beta_0 \psi_t)S + \theta_t E + v_t - \psi_t v_0. \tag{2}$$

Note that $\psi_t = 1$ corresponds to the case in which ability differences have the same impact on wages at all ages. While this assumption may be appropriate for older workers, there is considerable evidence in the "employer learning" literature (e.g., Farber and Gibbons,1996; Altonji and Pierret, 2001) that ability-driven wage differentials widen in the first few years in the labor market, as labor market participants learn about the true abilities of different individuals.[20] Schoenberg (2007, Table 5), for example shows that the effect of measured AFQT scores on wages of men in the NLSY rises by a factor of 2 within the first decade of labor market entry.[21] This suggests a value of $\psi_t \approx 2$ when w_0 is measured at a relatively young age (e.g., 20 or 21 in our case) and w_t is measured in mid-career.

[19] A similar technique is used by Lemieux (1998) to estimate the effect of unions on wages using data on union status changers.

[20] The same point has arisen in studies that attempt to use income measured at a certain point as a proxy for permanent income – see Haider and Solon (2006).

[21] Similar analyses have also been conducted using the same data set by Lange (2007), and Arcidiacono et al. (2008). All these studies show a rise in the return to AFQT in the first 10 years in the labor market, particularly for men without a college education.

To obtain estimates of ψ_t that are appropriate for our context, we look first at the correlation structure of wages for *non-conscripts* in the 1967 birth cohort. We assume that their wages are generated by equation (1) with $E = 0$ and an unrestricted year effect that captures cumulative experience, real wage growth, and other factors.[22] In addition, we assume that after τ years in the labor market, ψ_t is constant (i.e., $\psi_t = \psi_\tau$ for all $t \geq \tau$). We regress wages in all other years on the wage in year τ and obtain a set of coefficients that allow us to identify the relative variances of a and v_τ, the correlation structure of the transitory earnings component v_t, and the $\psi_t's$ (up to a normalization that $\psi_0 = 1$).

Specifically, assuming that

$$w_\tau = \psi_\tau a + \beta_\tau S + v_\tau,$$

the wage in any other year t can be expressed in terms of w_τ and schooling S as:

$$\begin{aligned} w_t &= \frac{\psi_t}{\psi_\tau} w_\tau + (\beta_t - \beta_\tau \frac{\psi_t}{\psi_\tau})S + v_t - \frac{\psi_t}{\psi_\tau} v_\tau \\ &= d_t w_\tau + g_t S + e_t. \end{aligned} \tag{3}$$

Using the "omitted variables" formula, the probability limit of the OLS estimate of d_t in equation (3) is:

$$\begin{aligned} p\lim \widehat{d_t} &= \frac{\psi_t}{\psi_\tau} + \frac{cov[v_t, w_\tau|S]}{var[w_\tau|S]} - \frac{\psi_t}{\psi_\tau}\frac{cov[v_\tau, w_\tau|S]}{var[w_\tau|S]} \\ &= \frac{\psi_t}{\psi_\tau}(1 - \frac{var[v_\tau]}{var[a] + var[v_\tau]}) + \frac{cov[v_t, v_\tau]}{var[v_\tau]}\frac{var[v_\tau]}{var[a] + var[v_\tau]}. \end{aligned}$$

For simplicity we assume that $v_t = \rho v_{t-1} + \zeta_t$, with $var[\zeta_t]$ (i.e., the transitory wage component v_t is a stationary first order autoregressive process). In this case

$$\frac{cov[v_t, v_\tau]}{var[v_\tau]} = \rho^{|t-\tau|}$$

and we can rewrite the expression for plim of $\widehat{d_t}$ as:

$$\begin{aligned} p\lim \widehat{d_t} &= \frac{\psi_t}{\psi_\tau}(1-\lambda) + \lambda\rho^{|t-\tau|}, \\ \text{where} \qquad \lambda &= \frac{var[v_\tau]}{var[a] + var[v_\tau]} \end{aligned} \tag{4}$$

represents the share of the variance in wages at age τ that is attributable to the transitory component v_τ versus the permanent ability component a.

[22]We also assume that wages in any year a person is not observed in the QP are missing at random.

To estimate ρ and λ we use data from the period after age τ, when $\psi_t = \psi_\tau$. In these years equation (4) implies:

$$\widehat{d_t} = (1-\lambda) + \lambda\rho^{|t-\tau|} + \epsilon_t, \tag{5}$$

where ϵ_t represents the sampling error in $\widehat{d_t}$. We obtain estimates and standard errors for ρ and λ using standard minimum distance techniques applied to the set of estimated regression coefficients $\widehat{d_t}$ for $t > \tau$. For earlier years the implied estimate of the relative loading factor $\frac{\psi_t}{\psi_\tau}$ is:

$$\frac{\widehat{\psi_t}}{\psi_\tau} = \frac{\widehat{d_t} - \widehat{\lambda}\widehat{\rho}^{|t-\tau|}}{1-\widehat{\lambda}}. \tag{6}$$

Applying this equation to the wage observed just prior to potential enlistment at age 21 (i.e, $t = 0$, which we assume has $\psi_0 = 1$) we can obtain an estimate of the relative factor loading for year τ :

$$\psi_\tau = \frac{1-\widehat{\lambda}}{\widehat{d_0} - \widehat{\lambda}\widehat{\rho}^{\tau}}.$$

To implement this procedure we assume that the effect of ability on wages stabilizes after 12 years in the labor market (i.e., at age 33 for men who were working at age 20/21). We use the observed wage at age 33 for our non-enlistees as the values of w_τ, and regress wages in all other years on this wage and dummies for schooling categories (measured at age 21). The set of regression coefficients $\widehat{d_t}$, for t=(21, 22, ... 42) are plotted in Figure 2. We also show the predicted values for $\widehat{d_t}$ using estimated values for ρ and λ that provide the best fit to the observed data for the years after age 33, assuming $\frac{\psi_t}{\psi_\tau} = 1$. These are $\widehat{\rho} = 0.67$ (standard error = 0.05) and $\widehat{\lambda} = 0.25$ (standard error = 0.02). Note that the predicted values fit the observed data for ages 35-42 relatively well, suggesting that our simple model provides a reasonable description of the covariance structure of the non-conscripts' wages.[23] For earlier ages the fitted values assuming $\frac{\psi_t}{\psi_\tau} = 1$ are substantially larger than the observed values, suggesting that $\psi_t < \psi_\tau$ for $t < \tau$.

Normalizing $\psi_0 = 1$, the implied values of ψ_t for each age from 21 to 42 are shown in Figure 3. Consistent with the patterns observed in the employer learning literature, the values rise substantially over the first 10-12 years in the labor market. In particular, the estimate of ψ_τ for wages at ages 35-42 is 4.16 (with a boot-strapped standard error of 0.30) – substantially larger than the estimate obtained by Schoenberg (2007) in the US context.

As a check on these calculations we re-did the same analysis using the wage data for conscripts. We obtained very similar estimates of ρ and λ ($\widehat{\rho} = 0.73$ with standard error of 0.04, and $\widehat{\lambda} = 0.20$

[23]The R-squared is 0.56 for the 8 moments. The goodness of fit statistic is 35.0, which is well above any conventional critical value for a chi-square distribution with 6 degrees of freedom.

with standard error of 0.09). Interestingly, the fit of the model to the estimates of $\widehat{d_t}$ for ages 35-42 is even better for conscripts, and passes a formal goodness-of-fit test.[24] Using these parameter values and the estimate of $\widehat{d_0} = 0.22$ for conscripts, the implied value for ψ_τ for wages at ages 35-42 is 3.69 (with a bootstrapped standard error of 0.55).

Given the estimated parameters for either group we believe a reasonable point estimate for ψ_τ is 4, and a plausible upper bound is a value of 5. The similarity of the estimates of ψ_τ based on the covariance structure of wages for conscripts and non-conscripts is reassuring because it suggests that the same transformation of the pre-conscription wage can control for ability in both groups. It also implies that if the mean pre-conscription wages of the two groups are the same, wage comparisons at mid-career are unconfounded by unobserved ability differences.

5 The Effect of Conscription on Subsequent Wages

Graphical Overview

Figure 4a plots the mean log wages of conscripts and non-conscripts in the 1967 birth cohort who are observed as wage-earners in the QP at each age from 18 (i.e., in 1986) to 42 (i.e., in 2009). Note that because of missing data in the QP in 1990 and 2001 there are "holes" in the series at ages 23 and 34. Moreover, because we define conscripts based on leave status at age 22 there are no wage observations for them at that age.[25] Since the duration of military service in the late 1980s was at most two years, we interpret age 24 (i.e., data for 1991) as representing the first year of post-conscription outcomes. Examination of the wage series in Figure 4a shows that pre-conscription wages (i.e., at ages 18-21) are very similar for the two groups, as is the first post-conscription wage at age 24. Thereafter the wages of conscripts are typically a little higher (+1-2%). Figures 4b and 4c present similar data for less-educated men (under 6 years of completed schooling at age 20 or 21) and more-educated men (6 or more years of completed schooling). The wage advantage for conscripts after age 30 appears to be larger and more systematic for the less-educated group, while for the more-educated group the wage gap appears to be centered on zero.

A potential issue with later-life wage comparisons between conscripts and non-conscripts is that the two groups may have different employment rates (or different private sector employment rates, since the QP is limited to private sector workers). Figure 5a shows the fractions of each group who

[24] The test statistic is 10.63, which has a p-value of 0.10.

[25] Recall that by construction every conscript has a wage observation at age 20 or 21. In fact 76% have a wage at 20 and 82% have a wage at 21. In contrast, all non-conscripts have to have a wage at age 21 and 22.

are captured in the QP at each age from 24 to 42. In their 20's the conscripts have slightly lower employment rates than the non-conscripts (e.g., a gap of -3.6 percentage points at age 24, in 1991, and a gap of -1.9% at age 30, in 1997). After age 35, however, the gaps are uniformly small. For the less-educated group (Figure 5b) the employment gaps vary more by age, but are never larger than 3% in absolute value. For the more-educated group (Figure 5c) the gaps are a little larger and more systematic between the ages of 24 and 30, but are very small after age 35. These patterns suggest that wage comparisons between conscripts and non-conscripts under the age of 30 have to be interpreted cautiously, since the conscript group has a somewhat lower employment rate in this age range, potentially inducing a selectivity bias. After age 35, however, there is less concern about selectivity.

In the Appendix we present an extended series of graphical comparisons that supplement the findings in Figures 4 and 5. First, we show the age profiles of wages and employment for women born in 1967 who meet the same criteria as our conscript and non-conscript groups. The female "conscripts" are presumably women who took maternity leave at ages 21 and/or 22, while the "non-conscripts" are women who worked continuously at those ages. Consistent with other evidence on the costs of child-bearing (e.g., Light and Ureta, 1995), we find that women who took leave tend to have lower wages later in their careers than those who did not. The gap is particularly pronounced for higher-educated women, as might be expected if career interruptions have a higher cost for them. Employment rates of women who took leave in their early 20's are also lower than the rates for those who did not. The negative impacts of leave-taking for young women contrasts with the generally positive wage effects (and 0 employment effects) for men, and confirms that there is not a simple mechanical explanation for the male effects.

We also present graphs showing the wage and employment outcomes for all four groups of early labor market entrants defined in Table 2 (i.e., conscripts, non-conscripts, men who were missing from the QP at age 22, and the residual group of early entrants). Generally, the wages of all four groups are fairly similar, though the residual group tends to have slightly lower wages than the other three. The employment rates of the groups vary more - in particular, as noted in the discussion of Table 2, the group who were not in the QP in 1989 have substantially lower employment rates at all ages than those who were recorded as unpaid leave in that year. This reinforces our classification scheme, which treats being "on leave" (included in the QP with missing hours and wages) as different from being absent altogether from the survey.

Regression Models

Table 3a presents the estimated wage premiums for conscripts at different ages from four different sets of models. The estimates in column 1 (and associated standard errors in column 2) are from specifications that control only for education prior to the age of conscription (using a set of dummies for each possible value recorded in the QP). These very simple specifications would be appropriate if enlistment status were as good as randomly assigned, conditional on education. The models in the remaining columns of the table use the pre-conscription wage to control for potential differences in ability between conscripts and non-conscripts. In the models summarized in columns 3-4, the pre-conscription wage is simply entered as an additional control variable. In the models in columns 5-6, the wage in each year is differenced from the pre-conscription wage, imposing the assumption that the coefficient ψ_t in equation (2) is 1. Finally, in the models in columns 7-8, we use the estimated quasi-differencing factor of 4.16 implied by our analysis of the wage process of non-enlistees.

A potentially surprising feature of the estimates in Table 3a is that the estimated enlistment effect in any particular year is not very different across the four specifications. This is also true of the pooled estimates in the bottom two rows of the table. Focusing on the pooled estimates for mid-career (ages 35-42) the estimated wage impact of enlistment is 2.1% with no control for pre-enlistment wages, 2.1% when the pre-enlistment wage is entered freely in the regression model (the estimated coefficient in this case is 0.36), 2.0% when wages in all periods are differenced from the pre-enlistment wage, and 1.9% when we quasi-difference using a factor of 4.16. The remarkable stability of the estimates across different values of the differencing factor is illustrated in Figure 6a, where we plot the estimated enlistment effect for ages 35-42 against various values for the quasi-differencing factor, ranging from 0 to 5, as well as the pointwise 95% confidence intervals. Though the estimates from higher values of the quasi-differencing factor are relatively imprecise, the point estimates are essentially invariant to the value of the quasi-differencing factor. The explanation for this stability is that mean pre-enlistment wages are virtually identical for conscripts and non-conscripts. Thus, adjusting the wage in any later period by subtracting off $\psi_t w_0$ has no effect on the point estimate of θ_t from equation (2), though different choices for ψ_t do affect the sampling error of the estimates.

Importantly, the orthogonality of pre-enlistment wages and enlistment status does not arise because pre-enlistment wages are "pure noise". In fact, pre-enlistment wages are highly predictive of later wage outcomes, suggesting that they contain significant information about individual ability.

For example, the correlation of the pre-conscription wage with wages in 2000 (at age 33) is 0.36.[26] Even as late as 2009, when the men in our sample were age 42, the correlation of wages with pre-conscription wages is 0.30, and differences in pre-conscription wages can explain nearly 10% of overall wage variation.

Tables 3b and 3c report a parallel series of models estimated for the subsets of men with lower or higher levels of education just prior to enlistment. As in the overall sample, the estimated enlistment effects at each age for these two groups are quite similar across specifications. For less educated men, there is a slight negative wage effect at age 24: thereafter the effects tend to rise with age, reaching around 5% by age 40. Pooling ages 35-42, the estimated effect of conscription for men with 5 or less years of schooling at age 20 is around +4%, and is statistically significant at conventional levels for models with $\psi_t \leq 1$. For the more educated subgroup (with 6 or more years of completed schooling by age 20) the wage effects of conscription are never large or significant, and the pooled estimates for ages 35-42 are very close to 0. The patterns of the pooled mid-career estimates for different values of the quasi-differencing factor are summarized in Figures 6b and 6c. As in the overall sample, the estimated wage effects are quite robust, reflecting the fact that enlistment status is uncorrelated with pre-enlistment wages, despite the relatively high correlations of pre-enlistment wages with subsequent wage outcomes.

Assuming that pre-conscription wages are orthogonal to enlistment status, the decision of which particular value of ψ_t to use in the estimation of post-conscription treatment effects can be based on efficiency considerations. Under orthogonality between ability and enlistment status, a simple OLS regression on schooling, initial wages, and enlistment status provides the least-variance estimates. We therefore focus on this specification – i.e., the estimates presented in column 3 of Tables 3a-3c – as the basis for our "preferred" estimates.

Robustness Checks

Our analysis so far has focused on comparisons between two subgroups of men who we can easily classify as either conscripts or non-conscripts. In this section we consider the robustness of our conclusions to changes in the way that we define these two groups. The results are summarized in Table 4, where we show estimates of the pooled enlistment effect for ages 35-42 from specifications with different quasi-differencing factors, using alternative definitions of the conscripted and non-

[26] The correlations are not significantly different for non-conscripts (ρ=0.36; n=2,648) and conscripts (ρ=0.38; n=1,008).

conscripted groups. We show results for all education groups in panel a, results for men with low-education at ages 20-21 in panel b, and results for men with higher education in panel c.

Focusing first on panel a, the first row shows the results from our "baseline" sample definition (these are taken from the last row of Table 3a). In row 2 we relax our definition of "early entrants" – which is based on *full time* work at age 20 or 21 in our baseline samples – to include part time workers.[27] This expands the conscript sample by about 10% and the non-conscript sample by about 17%. Nevertheless, the estimated enlistment effects from the various specifications are all nearly identical to the baseline estimates.

Our basic conscript definition includes two groups of men: those who were working full time in March 1987 and on leave in the next two years; and those who were working full time in March 1988 and on leave in the next year. Arguably, the requirement that the first group be on leave in both 1988 and 1989 may be too strict, since some men may have been inducted in the early months of 1988 and served only a year in the military. In row 3 we expand the definition of conscripts to include men who were working full time in March 1987, on leave the next year, and observed in *any* status in March 1989. This increases the conscript group by about 40%, and has little impact on the estimates with no control for the pre-conscription wage (columns 1-2) or with the pre-conscription wage included as a regressor (columns 3-4). However, in the differenced specification (columns 5-6) the alternative sample yields a somewhat larger estimate than the baseline sample (3.8% versus 2.0%), and in the quasi-differenced specification with $\psi_\tau = 4.16$, it yields a very large positive estimate (9.1%). This is attributable to the fact that the pre-conscription wages of the added conscripts (i.e., those who were working full time in March 1987, on leave in March 1988, and not on leave in March 1989) are slightly lower than those of other groups, and when $4.16w_0$ is subtracted from wages observed at later ages they appear to have a significant wage advantage. We believe the initial wage gap between the "added" conscripts and other groups of conscripts and non-conscripts is problematic, and therefore place little weight on the large point estimate arising from the quasi-differenced model when this group is included.

In row 4 we consider narrowing the conscript group from our baseline by imposing the extra requirement that men who were working full time in March 1988 and on leave in March 1989 also were working full time in March 1987. This reduces the size of the conscript group by about 25%

[27]Thus, non-conscripts are defined as men who were observed working (with a valid wage) in 1988 and 1989, and conscripts are defined as men who either were working in March 1987 and on leave in March 1988 and 1989, or working in March 1988 and on leave in March 1989.

and leads to estimates that are slightly larger in the specifications in columns 1-4 and slightly smaller in the specifications in columns 5-8. In all cases, however, the alternative estimates are within 1 standard error of the baseline estimates.

Finally, in row 5 we address a potential non-comparability between the way we measure pre-conscription wages for conscripts and non-conscripts. Recall that non-conscripts had to be observed working at ages 21 and 22 (i.e., in 1988 and 1989), and we use their wage at age 21 as their "pre-conscription wage". Just over 80% of our conscript group are men who were observed working at age 21 and on leave at age 22: for these men we also use the wage at 21 as the pre-conscription wage. But for the other 20%, who were working at age 20 and on leave at ages 21 and 22, we use their wage at 20 as the pre-conscription wage. This may lead to some understatement of pre-conscription wage for the enlistees. As a check, we inflated the age-20 wage for the relevant subgroup by the rate of growth of wages for all men who were observed working at ages 20 and 21 (+7.15%). This probably overstates the wage growth the "early inductees" would have experienced if they had not been drafted, so we regard this adjustment as providing an upper bound on the impact of the measurement timing issue. Applying the adjustment opens up a gap in pre-enlistment wages between the conscripts and non-conscripts: the mean wage gap rises from 0.2% with the unadjusted data to 1.5% with the adjusted data. As a result, the enlistment effect from the differenced specification falls by about 1.5%, while the enlistment effect for the quasi-differenced specification falls by about 6% (4.16 times the initial gap).

Panels b and c present parallel sets of estimates for low-education and high-education men. In both cases the departures from the baseline sample lead to changes in the estimated enlistment effects that are similar to what we see in the overall sample. In particular, relaxing the full time work requirement has almost no effect on the point estimates, while the narrowed definition of conscripts in row 4 of each panel leads to estimates that are within a standard error of the corresponding baseline estimates. The only significant departure from the baseline estimates arises when using the expanded definition of conscripts in row 3 and the quasi-differenced model. For the low-educated subsample the change in sample shifts the point estimate from 4.8% to 13.6% - a large change that is entirely attributable to a gap between the pre-conscription wages of the added conscripts and those of the other conscripts and non-conscripts that is magnified in the quasi-differenced specification.

Overall we interpret the robustness checks as providing general support for the conclusions derived from our baseline sample. A caveat is that our use of pre-enlistment wages to control for unobserved ability differences between conscripts and non-conscripts is relatively sensitive to even small differences in the pre-enlistment wage gap. For our baseline sample the mean pre-enlistment wages of conscripts and non-conscripts are almost identical, and the estimated impacts of enlistment on mid-career wages are relatively robust. But changes to the samples that lead to even modest gaps in pre-enlistment wages can result in relatively large changes in the estimated mid-career effects, reflecting the fact that ability differences that are observed at a young age tend to widen substantially with experience.

Mechanisms

The estimates in Tables 3b and 3c show a significant but modestly-sized effect of conscription on the mid-career wages of low-educated men, coupled with a zero effect on higher-educated men. In this section we investigate some of the possible channels for the conscription effect, including education, occupation, industry, and location.

We begin in Table 5 by looking at the relationship between enlistment status and post-enlistment changes in education. Specifically, we construct an estimate of the change in education between age 20/21 and age 42 for each person in our baseline sample who was observed working at least once in the 2002-2009 period, and run two simple models: one including only a constant and an enlistment dummy, the second adding a set of dummies for alternative values of initial education.[28] On average the early entrants who are observed in mid-career in the QP gained 0.77 years of education over their twenties and thirties. As shown in column (1) of Table 5 the average gain is only slightly larger for the conscripts (+0.07 years, standard error = 0.05). As might be expected, however, the gains differ for men with differing levels of initial education, and since the conscripted group under-represents both very low educated men, and those with the most education, it is important to control for initial education in measuring the effect of conscription. Adding these controls (column 2) leads to small but significantly positive enlistment effect (+0.14 years, t=2.64).

The remaining columns of Table 5 show parallel models for the subgroups with less than 6 years of initial schooling and 6 or more years of initial education. In the low-education subgroup enlistees gain about $\frac{1}{4}$ of a year more schooling than non-enlistees, while in the high-education subgroup

[28] We assign education in 2009 based on the latest year that the individual is observed in the QP. Among men observed at least once in 2002-2009, 75% are last observed in 2009 and 90% are observed in 2006 or later.

there is no effect of enlistment. Assuming a return to education of about 8% in the Portuguese market in the 2000's this extra schooling would yield a 2 percentage point higher average wage for low-education enlistees: enough to "explain" about half of the wage advantage we estimate in Tables 3 and 4 for these men.

We conducted a similar analysis of cumulative labor market experience. As might be expected given the very similar probabilities of employment of conscripts and non-conscripts documented in Figures 5a-5c, however, cumulative experience of the two groups increases at the same rate over our sample period, in particular for men with only primary schooling measured at age 20/21. Thus, experience effects appear to play no role in the wage gap for conscripts with low schooling at induction.

To explore the contributions of education and other possible channels more formally, we fit a series of models for wages in the 2002-2009 period in which we sequentially add controls for variables (like post-enlistment schooling) that could have been affected by enlistment. By comparing the estimated enlistment effects with and without these controls we can determine the share of the basic enlistment effect that is "explained" by each possible channel.

Row 1 of Table 6 shows the estimated enlistment coefficients for the overall sample, while rows 2 and 3 present the coefficients for the low-education and higher-education subsets. The estimates in the first column of the table simply reproduce the estimates from the last row in columns 3-4 of Tables 3a-3c, respectively. (Recall that these models include the pre-enlistment wage, dummies for initial education, and year effects). In column 2 we present models that add dummies for current education. As expected from the findings in Table 5, this addition has little impact on the estimated enlistment coefficient for higher-education men, but leads to a noticeable reduction in the coefficient for lower-educated men. In column 3 we augment the baseline model with controls for 2-digit occupation (22 dummies). These have an even larger effect on the enlistment coefficient for low-education men, but again not much impact for higher-educated men. Industry controls (column 4) have about the same effect as current education, while controls for major cities (Lisbon and Porto) and firm size actually lead to slightly larger enlistment coefficients for the less-educated subgroup.[29] Finally, in column 7 we present models that include all the available controls. Taken together these explain about 1 percentage point of the total enlistment effect for the pooled sample

[29]The effects of log firm size and a Lisbon location are both highly significant (t-ratios over 10 in each case).

and for low-educated men.

We interpret the estimates in Tables 5 and 6 as suggesting that some, but not all, of the positive enlistment effect we find for the mid-career wages of lower-educated men is attributable to enlistees obtaining additional schooling and/or training relative to non-enlistees in the years during and after their military service. This added education allowed access to better-paying occupations and industries and increased wages by 1-2 percent. A similar-sized or even larger component appears to arise from higher wages "within" education, occupation, and industry categories.

6 Summary and Conclusions

In this paper we use detailed administrative data covering the entire private sector of the Portuguese economy to study the long-term effects of peace-time military service on the cohort of men born in 1967. These men were drafted at age 21 and spent up to two years in the armed forces. Given the very low levels of educational attainment in Portugal, many men were working full time in the years prior to the determination of their induction status. We take advantage of this feature and use pre-enlistment wages to control for potential unobserved ability differences between conscripts and non-conscripts. There is substantial evidence from existing research, and from the covariance structure of wages in our data, that ability differences observed at age 20 tend to be magnified later in the career. We fit a simple dynamic factor for wages that provides an estimate of the changing return to ability between early and mid-career that we can use to rescale differentials in the pre-conscription wage. Estimates from this model imply that the same transformation of initial wages can be used to control for ability differences in mid-career wages. Fortunately, in our main analysis sample enlistment status is orthogonal to pre-enlistment wages, conditional on pre-enlistment schooling. As a result, our estimates of the long-run wage impacts of conscription are robust to alternative procedures for eliminating the impact of unobserved ability differences.

We find a small positive, but statistically insignificant impact of military service on wages at mid-career (ages 35-42). This is similar to recent findings on the effects of peace-time conscription in Britain (Grenet et al., 2011) and West Germany (Bauer et al., 2009), and also to recent estimates of the effect of military service on Vietnam era draftees at age 40 (Angrist, Chen and Song, 2011). The small average effect, however, is comprised of a larger positive effect for men with only a primary education (about one-half of the early labor market entrants in the cohort) and a zero effect for better-educated men. The positive impact on the low-educated subgroup is partially explained by the fact that enlistees with initially low education acquire more education than non-enlistees. They also work in somewhat better-paying industries and occupations. We conjecture that the higher schooling and occupational outcomes may be attributed in part to basic skills and occupational training received in the military, though we have no direct data on the extent of this training.

Several features of the institutional setting may have contributed to the positive impact of service for less-educated men in our sample. First, these men had at most 4 years of schooling when they entered the military. A year of basic skills training could have a potentially important impact on such men – allowing some to achieve literacy, for example. Second, Portuguese law required firms to rehire draftees at the completion of their service. This may have eased the transition back to civilian life for the conscripts in our analysis, who all held full time jobs just before entering the military. Third, it is important to emphasize that the military service we study occurred during peacetime. Nevertheless, our findings confirm a longstanding belief among many analysts that coerced military service can have a positive wage impact for initially disadvantaged men, perhaps comparable in magnitude to the impact of other labor market training programs.

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Appendix

Dataset

Quadros de Pessoal (QP) data are gathered annually by the Portuguese Ministry of Employment. All firms with wage-earners are required to complete the survey. Civil servants and household workers are excluded from coverage. The coverage of agriculture is also relatively low, given its low share of wage-earners. The mandatory nature of the survey leads to extremely high response rates, and in recent years nearly all firms with wage-earners in manufacturing and services are included in the data set. Nevertheless, there was some under-coverage –particularly of very small firms– in the initial years of the QP (Portugal, MTSS, 1990).

All personnel working in the firm in a reference week (in March until 1993 and in October from 1994 onwards) are in-scope for the QP. Workers on short-term leave (e.g., sickness, maternity leave, vacation, and strikes) and those on leave for compulsory military service are also supposed to be reported. Appendix Table A.1 clarifies the coding of leaves of absence during the period under analysis.

Reported data in the QP include the firm's location, industry, employment, sales, ownership (private Portuguese-owned, private foreign-owned, or public-owned), incorporation status, and the worker's gender, age, occupation, schooling, date of hire, monthly earnings (split into several components), and hours of work. Schooling information pertains to the highest completed level of education, with the following categories: first cycle or primary education (4 years); second cycle (6 years); third cycle (9 years); high school (12 years); university.[1]

Workers are assigned an identification number, based on a transformation of the social security number, that enables tracking over time. Similarly, each firm entering the database is assigned a unique identification number and it can be followed over time. The Ministry implements several checks to ensure that a firm that has already reported to the database is not assigned a different identification number. Most of these routines are based on the detailed location of the company and its legal identification codes.

Merging data across years

We combine QP data for the period from 1986 to 2009. The following data checks and selection procedures were implemented to prepare a worker-level data set to be merged across years.

[1] Since the mid-1990s, these categories, and in particular the two highest categories, are further subdivided.

Selection of valid worker identification codes: Observations with missing or invalid worker identification codes have been dropped. This restriction led to dropping an average 5.5% percent of the observations in the original yearly data sets made available by the Ministry of Employment.

Handling of duplicate worker identification codes in a given year: Only workers whose identification number appears at a maximum of two different firms are included in the annual file we use to build a longitudinal file. This allows workers to have a maximum of two jobs during the reference week. In the case of a duplicate observation in any year, we also checked that the worker's gender, date of birth, and schooling were the same in both jobs – otherwise the observations were dropped. On average 0.6 percent of the original observations in the annual QP files are dropped because the worker appeared at 3 or more firms; 2.7 percent of the observations were dropped because the worker appeared at two firms but had different gender, age or schooling in the two jobs; and 0.1 percent of the observations were dropped because the worker was reported twice by the same firm.

After these data checks, on average 91 percent of the observations in the original yearly data sets are retained, yielding an initial panel of 50,847,109 person-year observations on 7,963,035 workers.

Checks on the consistency of the longitudinal data

We imputed age and/or schooling to missing observations whenever there was no obvious inconsistency in the reported values.[2] These imputations affected 1.2 percent and 0.9 percent of the observations in the initial panel, respectively for age and schooling.

Inconsistencies were identified if the worker's gender or date of birth was reported as changing, or if the highest schooling level was reported as decreasing over time. In such cases, the value reported in over 50% of the non-missing records was treated as the correct value (if there was such a value). Using this procedure, 0.8 percent, 2.5 percent and 7.7 percent of the observations in the initial panel were corrected for inconsistencies in reported gender, birth date and education, respectively. In cases where no value was reported more than half the time, the individual was dropped from the panel. Overall, 1.5 percent, 0.8 percent and 6.2 percent of the observations are dropped due to inconsistent information on gender, birth date or schooling, respectively.

Finally, any remaining workers with missing age or schooling were dropped (0.3 percent and 1.7 percent of the initial panel, respectively).

[2] If schooling was consistently reported (possibly increasing over time) and the values it achieved before and after a missing year were the same, the missing value was corrected; similarly, missings in the initial/final period(s) were extrapolated from the earliest/latest reported value.

The final panel data set includes 45,511,769 person-year observations for 7,159,178 workers: this represents approximately 90 percent of the initial panel.

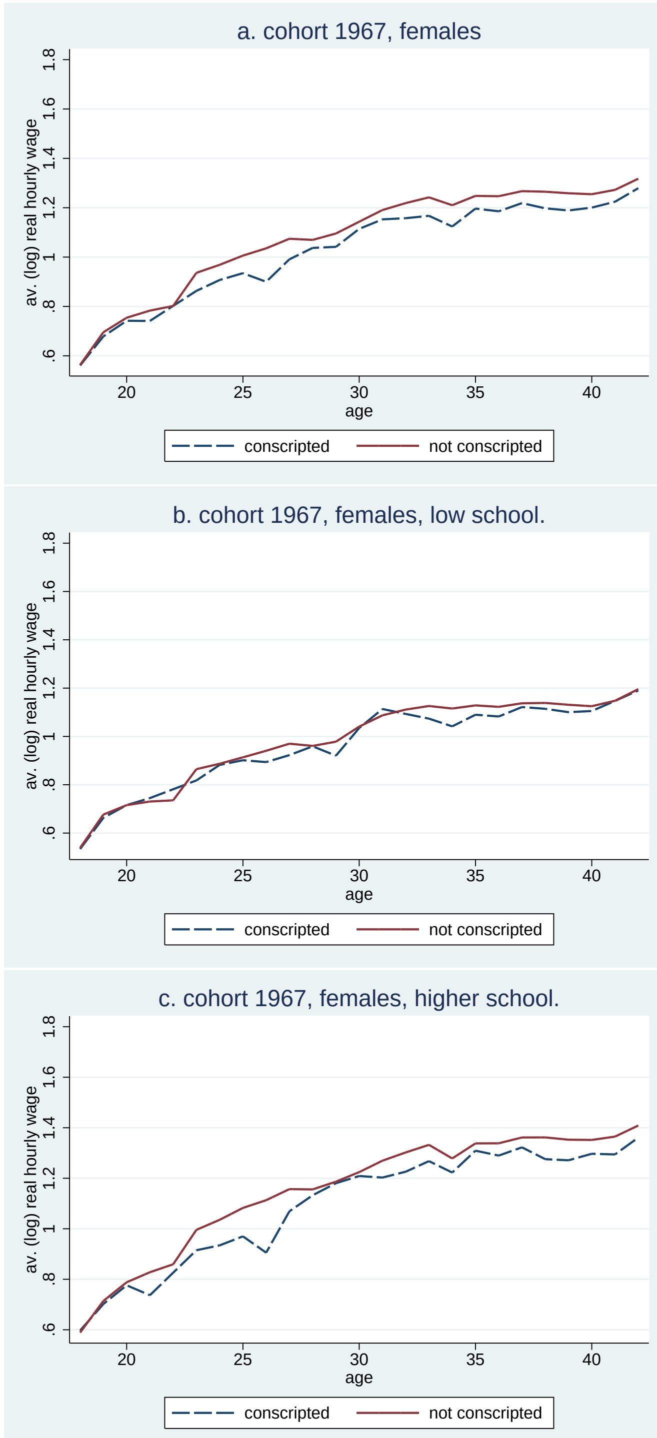
A final set of checks on the longitudinal data concentrated on the date of hire information. Dates of hire later than the date of the survey reference week were considered as mistakes and recoded to missing (affecting 3.8% of the observations). Missing information on date of hire was imputed whenever it was consistently reported for other years in the spell (affecting 4.6% of the observations). When the information was reported inconsistently across years, the date of hire reported more than one-half the time was taken as the correct one, leading to corrections for 2.1% of the observations. If after these corrections a worker had two or more dates of hire within the same employment spell the first reported date was considered the correct date (correcting 0.9% of the observations). Workers with inconsistent data after the introduction of the previous corrections were assigned missing information on date of hire (0.1% of the observations).

Table A.1: Types of leave and their coding during the period under analysis

Type of Leave	Worker Reported (1)	QP Coding of Wage	QP Coding of Hours	Coding of "on Leave"	Notes
Sickness	yes	missing, if leave longer than 3 days	missing, if leave longer than 3 days	=1	Wage paid (approx. 65% of normal wage) by the social security, after 3 days of sickness. Estimated overall rate of sickness absenteeism in Portugal: 8% (EFILWC, 1997, p. 18).
Maternity	yes	missing	missing	=1	Maternity leave started in Portugal in1976, when it lasted for 90 days. Currently, it lasts for 120 days.
Strike	yes	missing	missing	=1	Average of 0.016% work days lost per year during 1986-1996 (own computations based on Portugal, INE (2011) and Pordata (2011)).
Holiday	yes	reported	reported	=0	
Military	yes	missing	missing	=1	

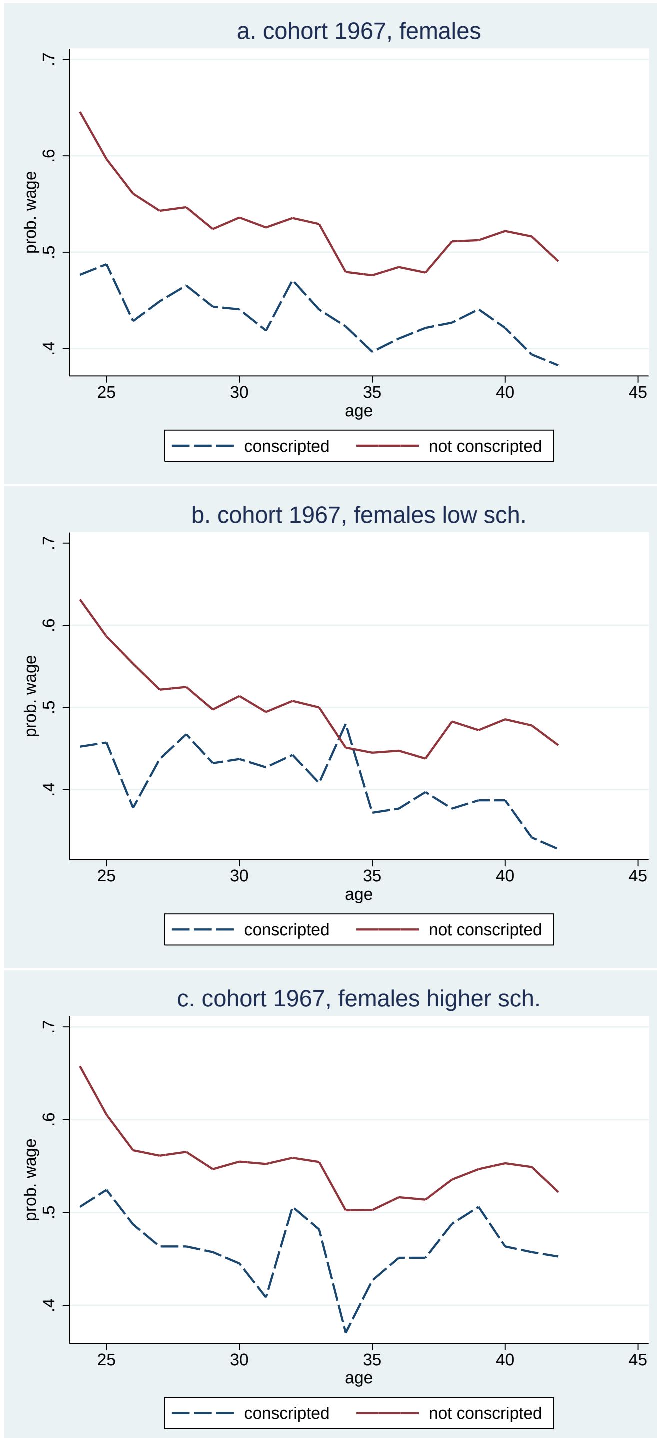
Note: (1) Instructions to fill out the questionnaire during the 1980s and 1990s stated that everyone engaged in the firm during the reference period should be listed, including: "the owner of the firm, if performing a function in the firm; unpaid and paid family members, if working in the firm more than one third of the normal duration of work; piece-rate workers; workers on short-term leave and those doing their military service" (Portugal, MT, Decree-Law 380/80, instructions on filling out column 2 of the Quadros de Pessoal form) [own translation]. Elsewhere in the instructions form, examples of short term leave are provided: sickness, maternity, holiday, strike.

Figure A.1: Hourly wages over the lifecycle, cohort 1967, females



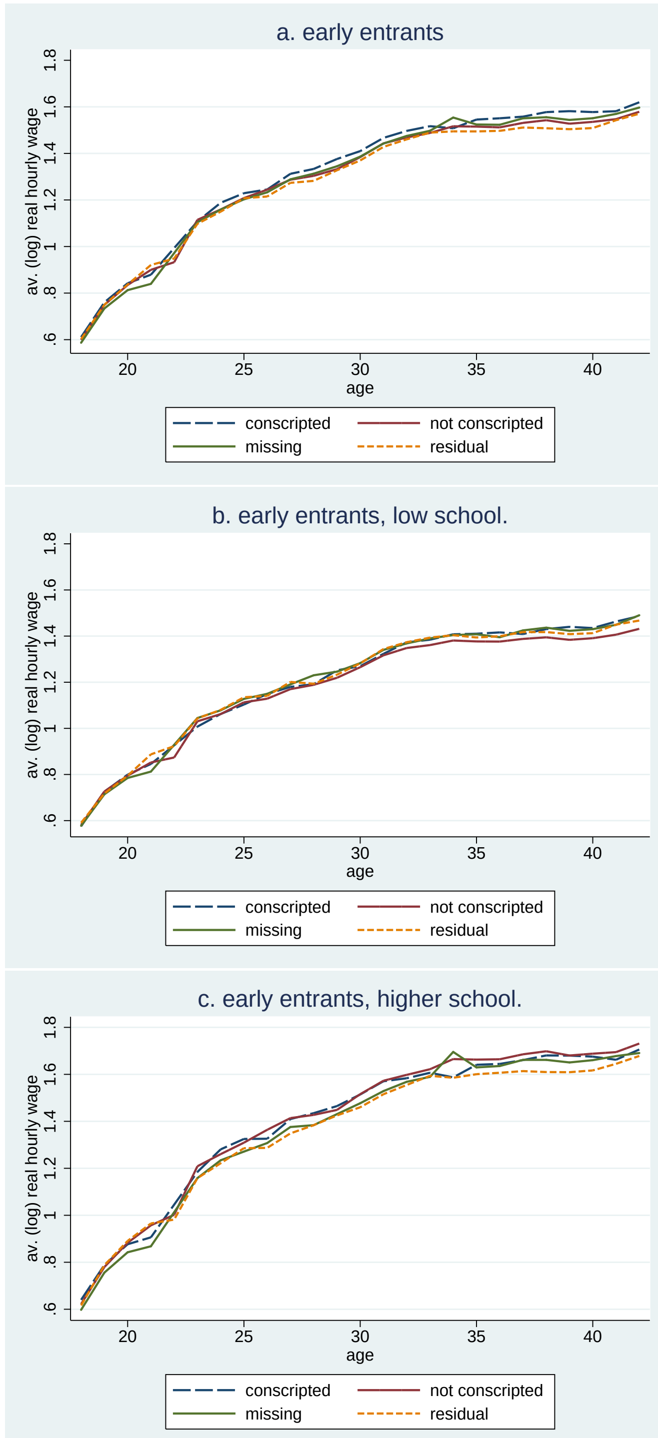
Note: 'Conscripted' is an individual working full-time in1987 or 1988 and reported on leave during the years military enlistment is due; 'non-conscripted' is an individual observed working full-time during the years military enlistment would have been due. For the cohort born 1967, military enlistment was due the year the individual turned 21 and it lasted for 24 months. Source: Computations based on Portugal, MTSS (1986-2009).

Figure A.2: Probability of earning a salary, cohort 1967, females



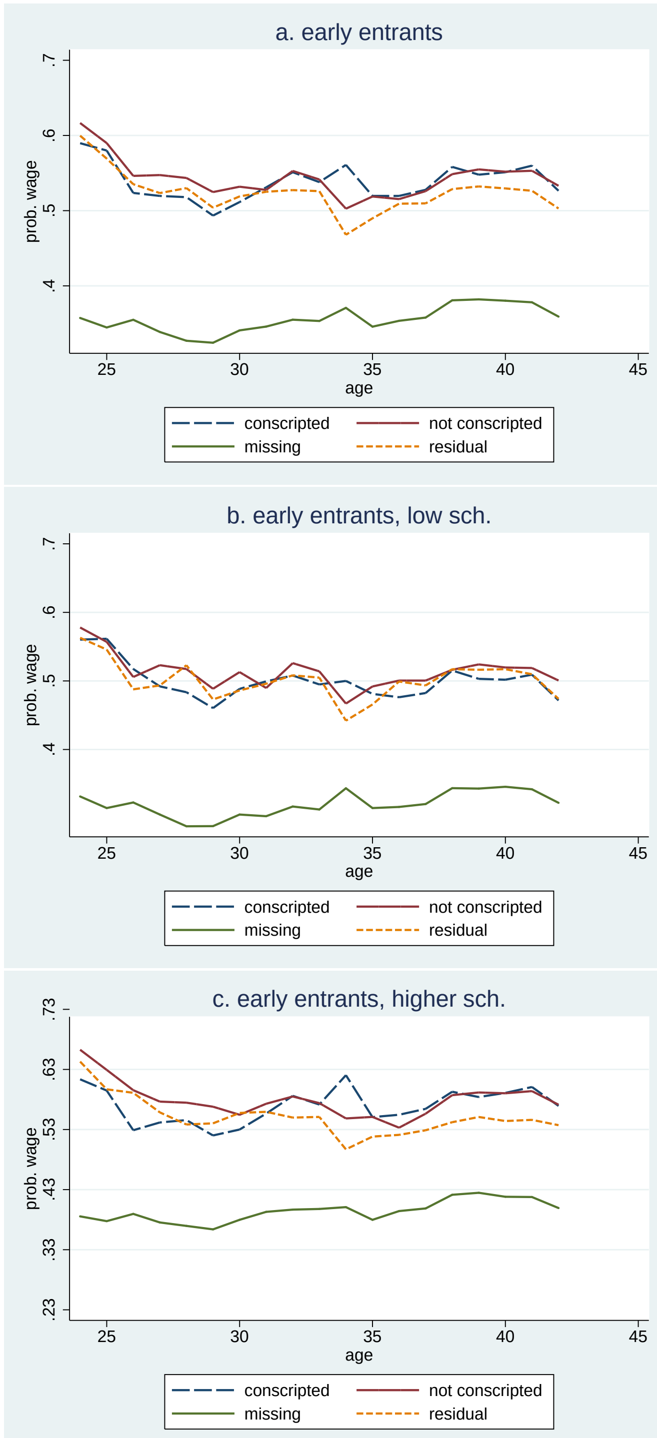
Note: 'Conscripted' is an individual working full-time the year before military enlistment is due, and reported on leave during the years military enlistment is due; 'non-conscripted' is an individual observed working full-time during the years military enlistment would have been due. For the cohort born 1967, military enlistment was due the year the individual turned 21 and it lasted for 24 months. Source: Computations based on Portugal, MTSS (1986-2009).

Figure A.3: Hourly wages over the lifecycle, cohort 1967, males early labor market entrants



Note: Conscripted is an individual working full-time in1987 or 1988 and reported on leave during the years military enlistment is due; non-conscripted is an individual observed working full-time during the years military enlistment would have been due; 'missing' is an individual not observed employed in the private sector during the years military conscription was due; the 'residual' category combines all other combinations of labor market situations from 1987 to 1989. For the cohort born 1967, military enlistment was due the year the individual turned 21 and it lasted for 24 months. Source: Computations based on Portugal, MTSS (1986-2009).

Figure A.4: Probability of earning a salary, cohort 1967, males early labor market entrants



Note: Conscripted is an individual working full-time the year before military enlistment is due, and reported on leave during the years military enlistment is due; non-conscripted is an individual observed working full-time during the years military enlistment would have been due; 'missing' is an individual not observed employed in the private sector during the years military conscription was due; the 'residual' category combines all other combinations of labor market situations from 1987 to 1989. For the cohort born 1967, military enlistment was due the year the individual turned 21 and it lasted for 24 months. Source: Computations based on Portugal, MTSS (1986-2009).

Table 1: Share of Male Cohort Enlisted

Year of Birth	Fraction Enlisted	Year of Birth	Fraction Enlisted
1965	0.35	1973	0.24
1966	0.36	1974	0.46
1967	0.40	1975	0.49
1968	0.40	1976	0.39
1969	0.36	1977	0.33
1970	0.46	1978	0.35
1971	0.41	1979	0.36
1972	0.24		

Note: Shares calculated as the ratio of conscripted males each year over the size of the cohort due to enlist (an approximation, given the possibility to defer enlistment). The values for the 1972 and 1973 cohorts represent total enlistments in 1993, divided by the total number of men in the 1972 and 1973 cohorts. Source: Computations based on Portugal, EME (2000) and Pordata (2010).

Table 2: Summary Statistics on Sub-Groups of Male Cohort Born 1967

	Complete Cohort	Early Entrants (Working Full-Time in 1987 or 1988)						Not Early Entrants
		Total	Conscripted			Missing	Residual	
			Yes/No	Yes	No			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Number of observations	86,909	18,517	6,749	1,838	4,911	9,502	2,266	68,392
Pre-enlistment:								
Share with observed wage								
1986	14.1	46.6	51.4	62.1	47.4	39.9	61.0	5.3
1987	16.5	72.3	66.4	77.5	62.3	71.5	93.2	1.4
1988	15.5	68.0	95.0	81.5	100.0	58.9	25.7	1.2
1989	13.2	36.7	72.8	0.0	100.0	0.0	82.9	6.8
Share on leave of absence								
1986	0.5	1.1	1.2	1.0	1.3	1.0	1.4	0.4
1987	0.6	0.9	1.5	1.4	1.6	0.6	0.2	0.5
1988	2.1	6.0	5.0	18.5	0.0	3.3	19.8	1.1
1989	3.4	11.8	27.2	100.0	0.0	0.0	15.0	1.2
(Log) real hourly wage								
1986	0.603	0.612	0.618	0.629	0.613	0.601	0.624	0.583
	(0.291)	(0.283)	(0.285)	(0.284)	(0.286)	(0.283)	(0.275)	(0.309)
1987	0.776	0.776	0.790	0.801	0.784	0.766	0.778	0.777
	(0.252)	(0.239)	(0.237)	(0.249)	(0.231)	(0.237)	(0.251)	(0.387)
1988	0.840	0.841	0.849	0.857	0.846	0.827	0.884	0.828
	(0.268)	(0.257)	(0.248)	(0.261)	(0.244)	(0.254)	(0.35)	(0.396)
1989	0.914	0.923	0.919		0.919		0.932	0.901
	(0.299)	(0.281)	(0.261)		(0.261)		(0.327)	(0.323)
Schooling:								
Share <=4 years at entry	40.8	53.7	53.2	46.4	55.8	53.9	54.5	37.3
Av. at entry into labor market	6.854	5.315	5.336	5.502	5.274	5.331	5.184	7.271
	(3.67)	(2.122)	(2.147)	(1.984)	(2.202)	(2.15)	(1.918)	(3.884)
Av. in 2002	7.116	5.797	5.825	6.021	5.752	5.810	5.659	7.544
	(3.659)	(2.323)	(2.331)	(2.195)	(2.376)	(2.356)	(2.15)	(3.903)
Post-enlistment:								
Share with wage observation(s), 2002-2009	60.9	62.2	72.6	72.4	72.7	52.4	72	60.6
(Log) real hourly wage, 2002-09	1.665	1.544	1.545	1.572	1.535	1.551	1.516	1.707
	(0.593)	(0.427)	(0.433)	(0.431)	(0.433)	(0.429)	(0.340)	(0.634)

Note: Early entrants are defined as men who were observed working full time in either 1987 or 1988. Conscripted men include men who were working full time in 1987, and were on leave of absence (listed on the roster of employees with missing values for wages and hours) in 1988 and 1989, plus men who were working full time in 1988 and on leave in 1989. Non-conscripted men are those who were working full time in 1988 and 1989. Missing group in column 6 are those who were working full time in 1987 or 1988 and are not present in the QP in 1989. Residual group in column 7 are all men who were working full time in 1987 or 1988 and are not included as conscripts, non-conscripts, or missing. Share with wage observation(s), 2002-2009 refers to the fraction of the group indicated in the column heading who were observed as wage earners in the QP at least once between 2002 and 2009.

Table 3a: Estimated Wage Effects of Conscription at Various Ages from Alternative Models (Pooled Education Groups)

Age	OLS Model with No Control for Wage at Age 20/21		OLS Model Including Control for Wage at Age 20/21		Differenced Model: Wage Minus Wage at Age 20/21		Quasi-Differenced Model: Wage Minus 4.16 x Wage at Age 20/21	
	Conscription Effect (1)	Std. Error (2)	Conscription Effect (3)	Std. Error (4)	Conscription Effect (5)	Std. Error (6)	Conscription Effect (7)	Std. Error (8)
19	1.0	(1.0)						
20	1.0	(0.8)						
21	0.2	(0.7)						
24	-1.4	(1.1)	-1.8	(1.0)	-2.1	(1.1)	-4.2	(3.2)
25	1.7	(1.2)	1.4	(1.1)	1.0	(1.2)	-1.0	(3.4)
26	1.5	(1.3)	1.4	(1.2)	1.3	(1.3)	0.9	(3.5)
27	1.2	(1.4)	1.1	(1.3)	0.9	(1.4)	0.0	(3.7)
28	1.8	(1.3)	1.5	(1.2)	1.1	(1.3)	-1.2	(3.6)
29	2.6	(1.4)	2.2	(1.4)	1.7	(1.5)	-1.1	(3.7)
30	0.6	(1.3)	0.6	(1.3)	0.6	(1.4)	0.6	(3.6)
31	1.4	(1.3)	1.0	(1.2)	0.5	(1.4)	-2.0	(3.6)
32	0.9	(1.3)	0.6	(1.2)	0.1	(1.3)	-2.2	(3.6)
33	1.2	(1.3)	1.2	(1.3)	1.0	(1.4)	0.4	(3.5)
35	0.8	(1.4)	0.9	(1.4)	0.9	(1.5)	1.1	(3.6)
36	2.2	(1.4)	2.3	(1.4)	2.4	(1.5)	3.2	(3.8)
37	0.4	(1.4)	0.6	(1.4)	0.9	(1.5)	2.4	(3.8)
38	2.2	(1.4)	2.1	(1.4)	2.0	(1.5)	1.3	(3.6)
39	3.0	(1.5)	2.8	(1.4)	2.6	(1.5)	1.1	(3.7)
40	3.7	(1.5)	3.5	(1.4)	3.1	(1.5)	1.3	(3.6)
41	2.5	(1.4)	2.4	(1.4)	2.2	(1.5)	1.4	(3.7)
42	1.7	(1.5)	1.8	(1.5)	2.1	(1.6)	3.3	(3.7)
Pooled: 24-33	1.1	(0.9)	0.9	(0.8)	0.6	(0.9)	-1.0	(3.2)
Pooled: 35-42	2.1	(1.2)	2.1	(1.2)	2.0	(1.3)	1.9	(3.5)

Notes: Estimated coefficients times 100 (with standard errors in parentheses) from models fit separately by age to wages of conscripts and non-conscripts. All models include dummies for education as of age 20 or 21. Models in columns 3-4 include wage measured at age 20 or 21. Models in columns 5-6 use as dependent variable wage at indicated age, minus wage at age 20/21. Models in columns 7-8 use as dependent variable wage at indicated age minus 4.16 times wage at age 20/21. Pooled estimates use sample of available person-year observations, and include year dummies. Standard errors for pooled models are clustered by person. Source: Portugal, MTSS (1986-2009).

Table 3b: Estimated Wage Effects of Conscription at Various Ages from Alternative Models for Low-Education Men

Age	OLS Model with No Control for Wage at Age 20/21		OLS Model Including Control for Wage at Age 20/21		Differenced Model: Wage Minus Wage at Age 20/21		Quasi-Differenced Model: Wage Minus 4.16 x Wage at Age 20/21	
	Conscription Effect (1)	Std. Error (2)	Conscription Effect (3)	Std. Error (4)	Conscription Effect (5)	Std. Error (6)	Conscription Effect (7)	Std. Error (8)
19	0.1	(1.5)						
20	0.6	(1.0)						
21	1.0	(1.0)						
24	-1.4	(1.5)	-1.7	(1.4)	-1.9	(1.5)	-3.6	(4.5)
25	-0.8	(1.5)	-1.0	(1.5)	-1.4	(1.6)	-3.2	(4.6)
26	0.1	(1.8)	0.7	(1.7)	1.0	(1.8)	2.9	(4.7)
27	0.9	(1.9)	0.9	(1.8)	0.9	(1.9)	0.6	(5.0)
28	1.0	(1.7)	1.1	(1.6)	1.2	(1.8)	1.9	(4.9)
29	3.1	(2.0)	2.8	(1.9)	2.7	(2.1)	1.3	(5.2)
30	-0.7	(1.8)	-0.6	(1.7)	-0.5	(1.9)	0.1	(4.9)
31	0.5	(1.7)	0.7	(1.6)	1.2	(1.8)	3.6	(5.0)
32	2.0	(1.7)	2.1	(1.6)	2.3	(1.8)	3.3	(4.8)
33	1.7	(1.7)	1.9	(1.6)	2.1	(1.8)	3.0	(4.8)
35	2.9	(1.8)	3.1	(1.8)	3.4	(1.9)	4.8	(5.0)
36	4.2	(1.9)	4.6	(1.8)	5.5	(2.0)	9.4	(5.2)
37	2.3	(1.8)	2.6	(1.8)	3.3	(2.0)	6.7	(5.2)
38	3.6	(1.9)	3.6	(1.8)	3.6	(2.0)	3.4	(5.1)
39	4.1	(1.8)	3.9	(1.8)	3.4	(2.0)	1.3	(5.1)
40	5.0	(1.9)	4.9	(1.8)	4.8	(2.0)	4.0	(5.1)
41	5.5	(1.8)	5.4	(1.8)	5.2	(2.0)	4.2	(5.1)
42	4.8	(1.8)	4.8	(1.8)	4.9	(2.0)	5.1	(5.2)
Pooled: 24-33	0.6	(1.2)	0.6	(1.1)	0.7	(1.3)	0.9	(4.4)
Pooled: 35-42	4.1	(1.7)	4.1	(1.6)	4.3	(1.8)	4.8	(5.0)

Notes: Estimated coefficients times 100 (with standard errors in parentheses) from models fit separately by age to wages of conscripts and non-conscripts. All models include dummies for education as of age 20 or 21. Models in columns 3-4 include wage measured at age 20 or 21. Models in columns 5-6 use as dependent variable wage at indicated age, minus wage at age 20/21. Models in columns 7-8 use as dependent variable wage at indicated age minus 4.16 times wage at age 20/21. Pooled estimates use sample of available person-year observations, and include year dummies. Standard errors for pooled models are clustered by person. Source: Portugal, MTSS (1986-2009).

Table 3c: Estimated Wage Effects of Conscription at Various Ages from Alternative Models for High-Education Men

Age	OLS Model with No Control for Wage at Age 20/21		OLS Model Including Control for Wage at Age 20/21		Differenced Model: Wage Minus Wage at Age 20/21		Quasi-Differenced Model: Wage Minus 4.16 x Wage at Age 20/21	
	Conscription Effect (1)	Std. Error (2)	Conscription Effect (3)	Std. Error (4)	Conscription Effect (5)	Std. Error (6)	Conscription Effect (7)	Std. Error (8)
19	1.8	(1.5)						
20	1.4	(1.1)						
21	0.1	(1.1)						
24	-1.4	(1.6)	-1.8	(1.4)	-2.2	(1.5)	-4.7	(4.6)
25	3.7	(1.8)	3.4	(1.7)	3.0	(1.8)	0.7	(5.0)
26	2.3	(1.9)	2.0	(1.8)	1.6	(2.0)	-0.8	(5.2)
27	1.5	(1.9)	1.2	(2.0)	1.0	(2.1)	-0.5	(5.3)
28	2.5	(1.9)	1.8	(1.8)	1.0	(1.9)	-3.8	(5.2)
29	2.2	(2.0)	1.6	(1.9)	1.0	(2.1)	-3.0	(5.4)
30	1.7	(2.0)	1.6	(1.9)	1.5	(2.0)	0.9	(5.3)
31	2.0	(1.9)	1.1	(1.9)	0.0	(2.0)	-6.5	(5.3)
32	-0.1	(1.9)	-0.7	(1.8)	-1.6	(2.0)	-6.5	(5.2)
33	0.9	(1.9)	0.6	(1.9)	0.3	(2.0)	-1.7	(5.2)
35	-0.8	(2.1)	-1.0	(2.0)	-1.1	(2.2)	-1.9	(5.3)
36	0.5	(2.1)	0.3	(2.0)	0.0	(2.2)	-1.8	(5.6)
37	-1.0	(2.2)	-1.0	(2.1)	-1.0	(2.3)	-0.8	(5.4)
38	1.1	(2.1)	1.0	(2.1)	0.8	(2.2)	-0.3	(5.2)
39	2.2	(2.2)	2.0	(2.1)	1.9	(2.2)	0.9	(5.2)
40	2.7	(2.2)	2.3	(2.1)	1.8	(2.2)	-0.8	(5.1)
41	0.2	(2.2)	0.1	(2.1)	-0.1	(2.3)	-0.8	(5.3)
42	-0.6	(2.3)	-0.4	(2.2)	0.0	(2.3)	2.0	(5.3)
Pooled: 24-33	1.5	(1.3)	1.0	(1.2)	0.5	(1.4)	-2.6	(4.5)
Pooled: 35-42	0.5	(1.8)	0.4	(1.7)	0.3	(1.9)	-0.4	(4.8)

Notes: Estimated coefficients times 100 (with standard errors in parentheses) from models fit separately by age to wages of conscripts and non-conscripts. All models include dummies for education as of age 20 or 21. Models in columns 3-4 include wage measured at age 20 or 21. Models in columns 5-6 use as dependent variable wage at indicated age, minus wage at age 20/21. Models in columns 7-8 use as dependent variable wage at indicated age minus 4.16 times wage at age 20/21. Pooled estimates use sample of available person-year observations, and include year dummies. Standard errors for pooled models are clustered by person. Source: Portugal, MTSS (1986-2009).

Table 4a: Robustness Checks: Estimated Wage Effects of Conscription at Pooled Ages 35-42 from Alternative Models (Pooled Education Groups)

	OLS Model with No Control for Wage at Age 20/21		OLS Model Including Control for Wage at Age 20/21		Differenced Model: Wage Minus Wage at Age 20/21		Quasi-Differenced Model: Wage Minus 4.16 x Wage at Age 20/21	
	Conscription Effect (1)	Std. Error (2)	Conscription Effect (3)	Std. Error (4)	Conscription Effect (5)	Std. Error (6)	Conscription Effect (7)	Std. Error (8)
a) Baseline sample conscripts=1,838 non-conscripts=4,911	2.1	(1.2)	2.1	(1.2)	2.0	(1.3)	1.9	(3.5)
b) Relax full-time conscripts=2,015 non-conscripts=5,775	2.6	(1.2)	2.6	(1.2)	2.6	(1.3)	2.7	(3.4)
c) Relax leave in 1989 conscripts=2,600 non-conscripts=4,911	2.1	(1.1)	2.8	(1.1)	3.8	(1.2)	9.2	(3.1)
d) Impose full-time 1987 conscripts=1,370 non-conscripts=4,911	3.0	(1.4)	2.6	(1.3)	1.9	(1.5)	-1.7	(3.9)
e) Adjust 1987 wages conscripts=1,838 non-conscripts=4,911	2.1	(1.2)	1.5	(1.2)	0.6	(1.3)	-3.9	(3.5)

Notes: In the baseline sample, conscripts include men who were working full time in 1987 and on leave in 1988 and 1989, plus men who were working full time in 1988 and on leave in 1989. Non-conscripted men are those who were working full time in 1988 and 1989. Sample in row b replaces full time work requirements for both groups with requirement that the individual be working and have a valid wage. In this sample 95 conscripts and 352 non-conscripts have missing education data: models include a dummy for missing initial education. Sample in row c modifies conscript definition to include men who were working full time in 1987 and on leave in 1988, plus men who were working full time in 1988 and on leave in 1989, thus dropping the requirement that the men who worked full time in 1987 were also on leave in 1989. Sample in row d modifies conscript definition to include men who were working full time in 1987 and on leave in 1988 and 1989, plus men who were working full time in both 1987 and 1988 and on leave in 1989, thus limiting the second group to those who were working full time in both 1987 and 1988. Sample in row e is same as baseline. However, wages of conscripts who were working full time in 1987 and on leave in 1988 and 1989 are inflated by 7.15%. Estimated coefficients times 100 (with standard errors in parentheses). All models include dummies for year and education as of age 20 or 21. Models in columns 3-4 include wage measured at age 20 or 21. Models in columns 5-6 use as dependent variable wage at indicated age minus wage at age 20/21. Models in columns 7-8 use as dependent variable wage at indicated age minus 4.16 times wage at age 20/21. Estimates use sample of available person-year observations. Standard errors are clustered by person. Source: Computations based on Portugal, MTSS (1986-2009).

Table 4b: Robustness Checks: Estimated Wage Effects of Conscription at Pooled Ages 35-42 from Alternative Models (Low-Education Men)

	OLS Model with No Control for Wage at Age 20/21		OLS Model Including Control for Wage at Age 20/21		Differenced Model: Wage Minus Wage at Age 20/21		Quasi-Differenced Model: Wage Minus 4.16 x Wage at Age 20/21	
	Conscription Effect (1)	Std. Error (2)	Conscription Effect (3)	Std. Error (4)	Conscription Effect (5)	Std. Error (6)	Conscription Effect (7)	Std. Error (8)
a) Baseline sample conscripts=821 non-conscripts=2,675	4.1	(1.7)	4.1	(1.6)	4.3	(1.8)	4.8	(5.0)
b) Relax full-time conscripts=850 non-conscripts=2,999	4.7	(1.6)	4.7	(1.5)	4.7	(1.7)	4.6	(4.9)
c) Relax leave in 1989 conscripts=1,187 non-conscripts=2,675	3.8	(1.4)	4.6	(1.4)	6.2	(1.5)	13.6	(4.3)
d) Impose full-time 1987 conscripts=608 non-conscripts=2,675	5.4	(1.9)	5.1	(1.9)	4.5	(2.1)	1.7	(5.7)
e) Adjust 1987 wages conscripts=821 non-conscripts=2,675	4.1	(1.7)	3.7	(1.6)	2.8	(1.7)	-1.3	(5.0)

Notes: In the baseline sample, conscripts include men who were working full time in 1987 and on leave in 1988 and 1989, plus men who were working full time in 1988 and on leave in 1989. Non-conscripted men are those who were working full time in 1988 and 1989. Sample in row b replaces full time work requirements for both groups with requirement that the individual be working and have a valid wage. In this sample 95 conscripts and 352 non-conscripts have missing education data: models include a dummy for missing initial education. Sample in row c modifies conscript definition to include men who were working full time in 1987 and on leave in 1988, plus men who were working full time in 1988 and on leave in 1989, thus dropping the requirement that the men who worked full time in 1987 were also on leave in 1989. Sample in row d modifies conscript definition to include men who were working full time in 1987 and on leave in 1988 and 1989, plus men who were working full time in both 1987 and 1988 and on leave in 1989, thus limiting the second group to those who were working full time in both 1987 and 1988. Sample in row e is same as baseline. However, wages of conscripts who were working full time in 1987 and on leave in 1988 and 1989 are inflated by 7.15%. Estimated coefficients times 100 (with standard errors in parentheses). All models include dummies for year and education as of age 20 or 21. Models in columns 3-4 include wage measured at age 20 or 21. Models in columns 5-6 use as dependent variable wage at indicated age minus wage at age 20/21. Models in columns 7-8 use as dependent variable wage at indicated age minus 4.16 times wage at age 20/21. Estimates use sample of available person-year observations. Standard errors are clustered by person. Source: Computations based on Portugal, MTSS (1986-2009).

Table 4c: Robustness Checks: Estimated Wage Effects of Conscription at Pooled Ages 35-42 from Alternative Models (High-Education Men)

	OLS Model with No Control for Wage at Age 20/21		OLS Model Including Control for Wage at Age 20/21		Differenced Model: Wage Minus Wage at Age 20/21		Quasi-Differenced Model: Wage Minus 4.16 x Wage at Age 20/21	
	Conscription Effect (1)	Std. Error (2)	Conscription Effect (3)	Std. Error (4)	Conscription Effect (5)	Std. Error (6)	Conscription Effect (7)	Std. Error (8)
a) Baseline sample conscripts=1,017 non-conscripts=2,236	0.5	(1.8)	0.4	(1.7)	0.3	(1.9)	-0.4	(4.8)
b) Relax full-time conscripts=1,070 non-conscripts=2,424	0.6	(1.7)	1.0	(1.7)	1.0	(1.8)	1.2	(4.6)
c) Relax leave in 1989 conscripts=1,413 non-conscripts=2,236	0.7	(1.6)	1.2	(1.6)	1.9	(1.7)	5.5	(4.3)
d) Impose full-time 1987 conscripts=762 non-conscripts=2,236	1.2	(1.9)	0.6	(1.9)	-0.1	(2.0)	-4.2	(5.2)
e) Adjust 1987 wages conscripts=1,017 non-conscripts=2,236	0.5	(1.8)	-0.1	(1.7)	-1.0	(1.9)	-5.9	(4.8)

Notes: In the baseline sample, conscripts include men who were working full time in 1987 and on leave in 1988 and 1989, plus men who were working full time in 1988 and on leave in 1989. Non-conscripted men are those who were working full time in 1988 and 1989. Sample in row b replaces full time work requirements for both groups with requirement that the individual be working and have a valid wage. In this sample 95 conscripts and 352 non-conscripts have missing education data: models include a dummy for missing initial education. Sample in row c modifies conscript definition to include men who were working full time in 1987 and on leave in 1988, plus men who were working full time in 1988 and on leave in 1989, thus dropping the requirement that the men who worked full time in 1987 were also on leave in 1989. Sample in row d modifies conscript definition to include men who were working full time in 1987 and on leave in 1988 and 1989, plus men who were working full time in both 1987 and 1988 and on leave in 1989, thus limiting the second group to those who were working full time in both 1987 and 1988. Sample in row e is same as baseline. However, wages of conscripts who were working full time in 1987 and on leave in 1988 and 1989 are inflated by 7.15%. Estimated coefficients times 100 (with standard errors in parentheses). All models include dummies for year and education as of age 20 or 21. Models in columns 3-4 include wage measured at age 20 or 21. Models in columns 5-6 use as dependent variable wage at indicated age minus wage at age 20/21. Models in columns 7-8 use as dependent variable wage at indicated age minus 4.16 times wage at age 20/21. Estimates use sample of available person-year observations. Standard errors are clustered by person. Source: Computations based on Portugal, MTSS (1986-2009).

Table 5: Change in Education

	All		Low-Education		High-Education	
	(1)	(2)	(3)	(4)	(5)	(6)
Conscription Dummy	0.07	0.14	0.22	0.26	0.05	0.04
	(0.05)	(0.05)	(.09)	(0.09)	(0.06)	(0.06)
Schooling at Age 20/21:						
Less than Primary		2.54		1.80		
		(0.21)		(0.20)		
Completed Primary (4 years)		0.76				
		(0.11)				
Completed Elementary (6 years)		0.30				0.31
		(0.12)				(0.10)
Completed Intermediate Level (9 years)		0.29				0.30
		(0.14)				(0.12)
Mean	0.77		1.04		0.50	
Standard Deviation	1.68		1.86		1.43	
Obs.	4,903		2,459		2,444	

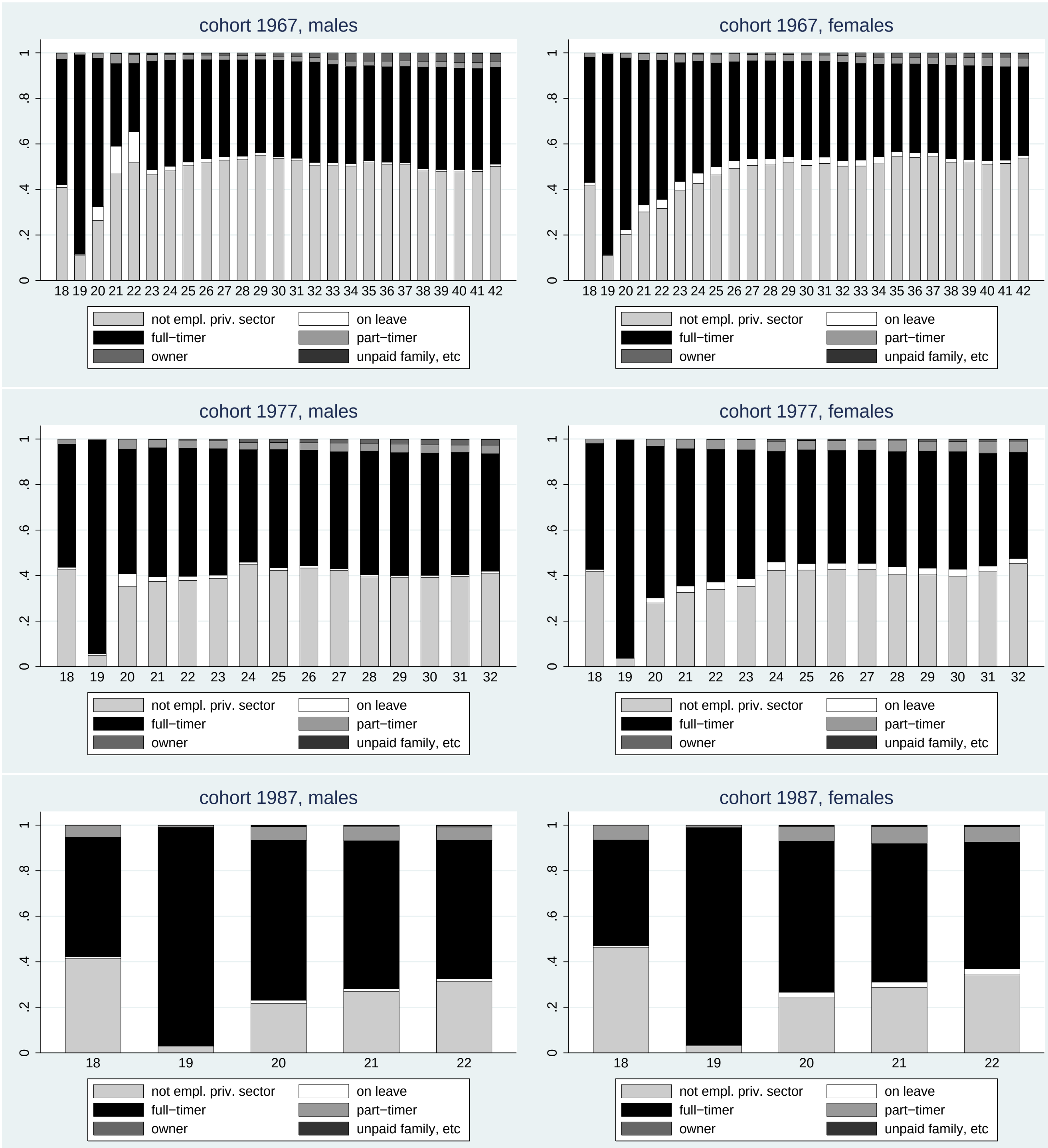
Note: Standard errors in parentheses. Sample includes conscripts and nonconscripts who were observed working as a wage earner in the QP at least once between 2002 and 2009. Dependent variable is change in schooling from last observed value (in 2002-2009 period) to initial value at age 20/21. Source: Computations based on Portugal, MTSS (1986-2009).

Table 6: Estimated Wage Effects of Conscription at Ages 35-42

	Baseline	Add Current Education (5 dummies) (2)	Add Current Occupation (22 dummies) (3)	Add Current Industry (23 dummies) (5)	Add City Effects (2 dummies) (4)	Add Firm Size (6)	Add All Controls (7)
1. Overall sample							
estimated enlistment effect	2.1	1.6	0.6	1.4	2.1	1.8	0.9
	(1.2)	(1.2)	(1.1)	(1.1)	(1.2)	(1.1)	(1.0)
Obs.	29,034	29,034	28,908	29,034	29,034	29,034	28,908
R^2	0.258	0.279	0.356	0.318	0.277	0.315	0.453
F statistic	107.471	84.909	72.11	64.888	103.997	134.663	80.045
2. Low Education at Age 20/21							
estimated enlistment effect	4.1	3.4	2.4	3.5	4.3	4.2	3.1
	(1.6)	(1.5)	(1.4)	(1.5)	(1.6)	(1.5)	(1.3)
Obs.	14,119	14,119	14,060	14,119	14,119	14,119	14,060
R^2	0.054	0.069	0.157	0.13	0.07	0.123	0.271
F statistic	25.904	20.383	22.001	21.259	25.123	39.711	30.634
3. Higher Education at Age 20/21							
estimated enlistment effect	0.4	0.2	-0.7	-0.3	0.5	-0.2	-0.9
	(1.7)	(1.7)	(1.6)	(1.7)	(1.7)	(1.6)	(1.4)
Obs.	14,915	14,915	14,848	14,915	14,915	14,915	14,848
R^2	0.227	0.254	0.342	0.292	0.251	0.29	0.452
F statistic	59.165	52.126	39.94	35.513	58.099	79.177	54.251

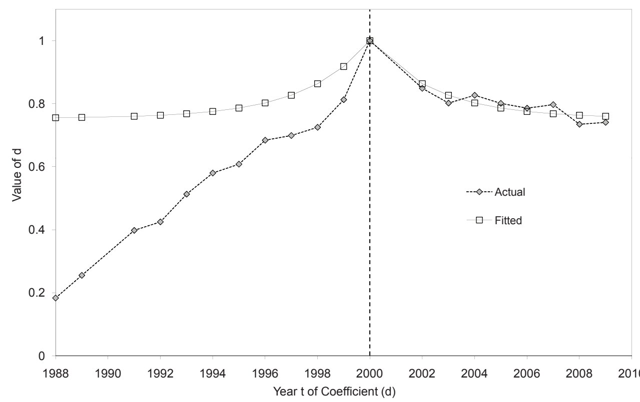
Note: Estimated coefficients times 100 (with standard errors in parentheses). The baseline specification includes controls for year (7 dummies), education as of age 20 or 21 (5 dummies) and wage measured at age 20 or 21. Estimates use sample of available person-year observations. Standard errors are clustered by person. Source: Computations based on Portugal, MTSS (1986 to 2009).

Figure 1: Labor market situation, conditional on working full-time the year before military enlistment due



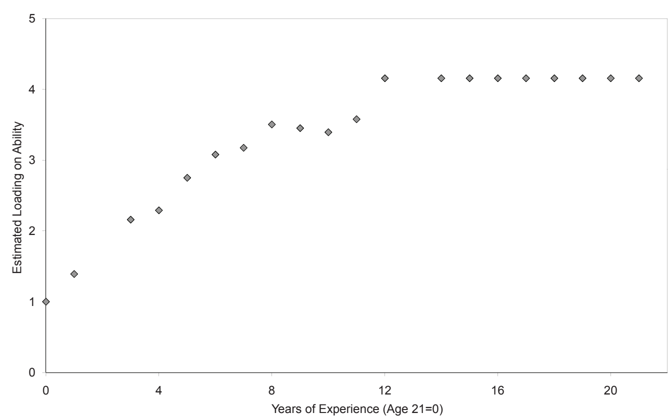
Note: For the 1967 cohort (first row), military enlistment was due the year the individual turned 21 (thus observed the year before, at age 20 or 19 depending on the date of birth) and service lasted for 24 months; for the 1977 cohort (second row), enlistment was due the year the individual turned 20 and service lasted for 4 months; for the 1987 cohort (third row), military enlistment had been abolished and the plot is conditional on working full-time the year the individual turned 20. Source: Computations based on Portugal, MTSS (1986-2009).

Figure 2: Fitted and Actual Regression Coefficients: Log Wages in Year t on Log Wage in 2000



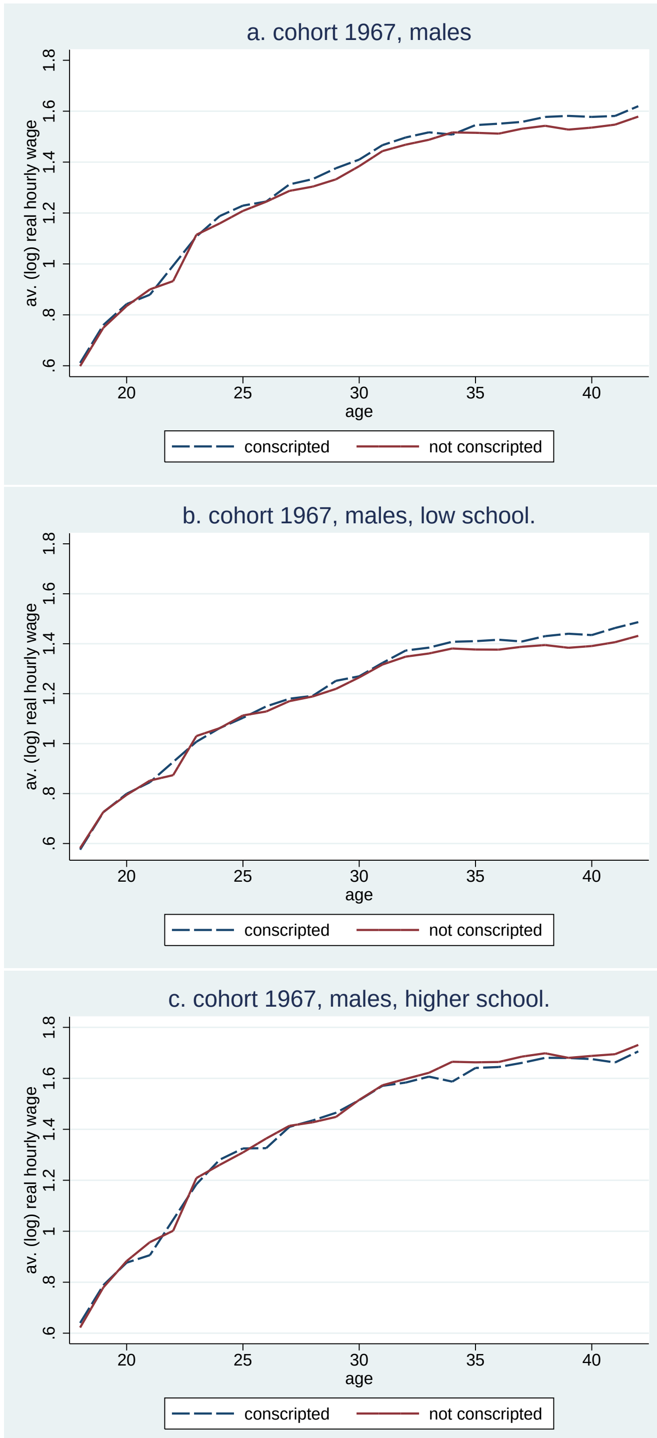
Source: Computations based on Portugal, MTSS (1986-2009).

Figure 3: Estimated Relative Factor Loadings on Ability (Value for Age 21 =1)



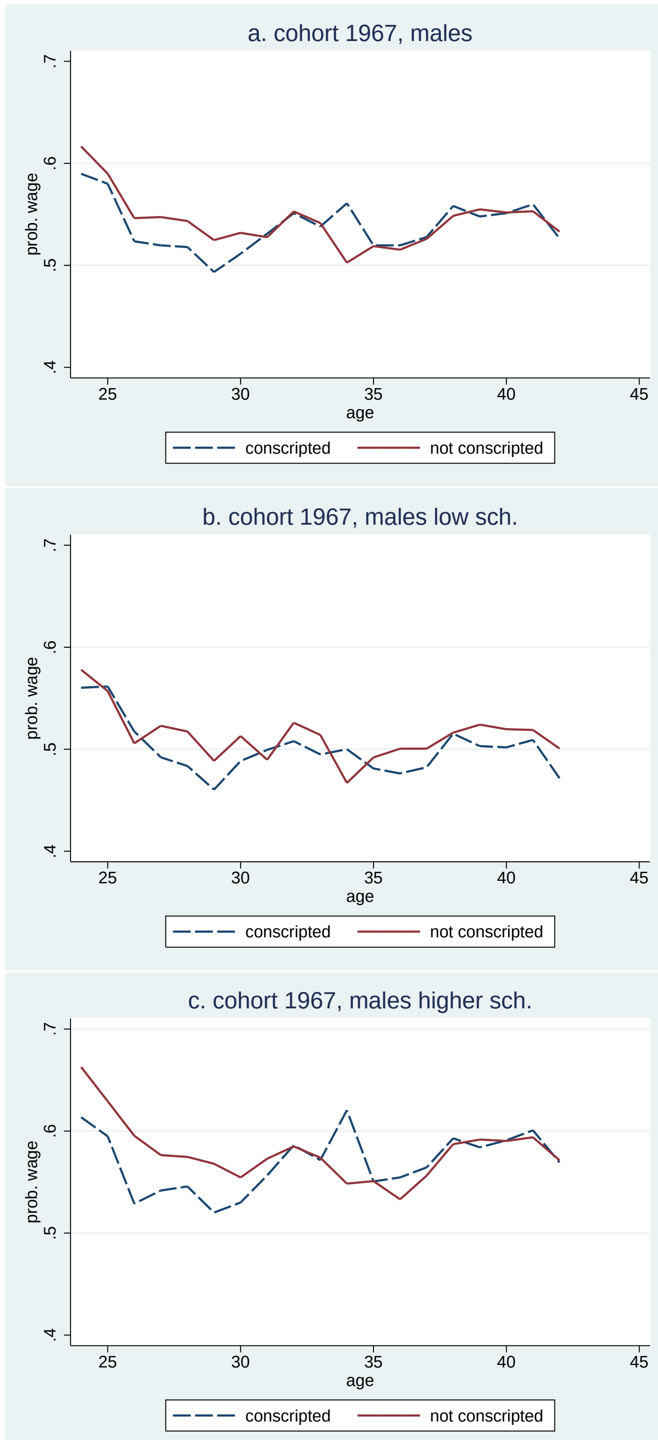
Source: Computations based on Portugal, MTSS (1986-2009).

Figure 4: Hourly wages over the lifecycle, cohort 1967: conscripted versus non-conscripted males



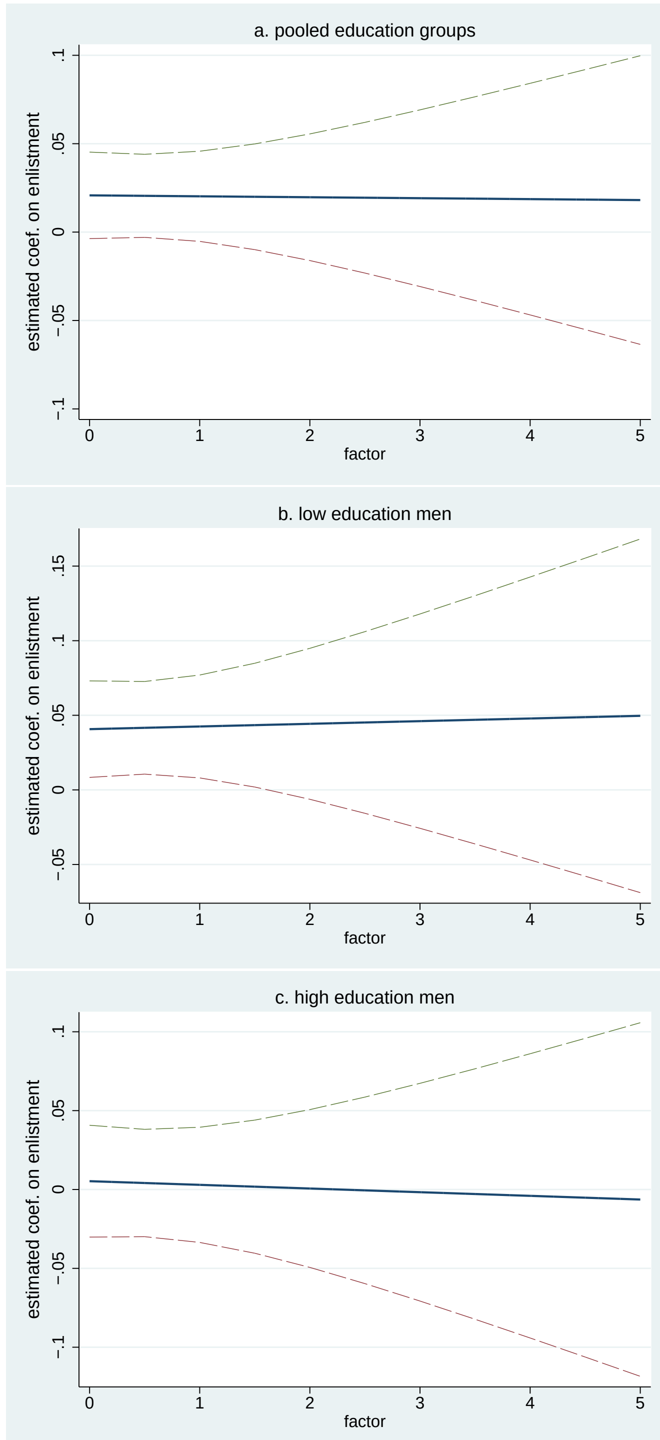
Note: Conscripted is an individual working full-time in1987 or 1988 and reported on leave during the years military enlistment is due; non-conscripted is an individual observed working full-time during the years military enlistment is due. For the cohort born 1967, military enlistment was due the year the individual turned 21 and it lasted for 24 months. Source: Computations based on Portugal, MTSS (1986-2009).

Figure 5: Probability of earning a salary, cohort 1967: conscripted versus non-conscripted males



Note: Conscripted is an individual working full-time the year before military enlistment is due, and reported on leave during the years military enlistment is due; non-conscripted is an individual observed working full-time during the years military enlistment would have been due. For the cohort born 1967, military enlistment was due the year the individual turned 21 and it lasted for 24 months. Source: Computations based on Portugal, MTSS (1986-2009).

Figure 6: Estimated impact of enlistment at ages 35-42, quasi-differencing wage regressions with alternative values of the differencing factor



Source: Computations based on Portugal, MTSS (1986-2009).