# Policy Impacts on Early Labor Market Outcomes

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## Abstract

Low labor force participation and employment among young people is a common concern around the world, especially in low and middle-income countries. The increasing availability of micro-data in these countries allows us to investigate the factors that affect young people's labor market outcomes. In my dissertation, I investigate the impact of two important policies on early labor market outcomes: Compulsory military service and compulsory schooling.

Previous research on military conscription exclusively focuses on the effect of military service on *subsequent* labor market outcomes. In the first chapter I examine the effect of peacetime conscription on early labor market outcomes of potential conscripts *before* they are called up for service. In a simple theoretical framework with costly job search and no job security, I show that an expected interruption in civilian life reduces the incentive of teenagers to search for a job. Moreover, when firms bear the cost of on-the-job training, an expected interruption may reduce employers' likelihood of offering a job to expected future conscripts. Using micro-data from Turkey, Argentina, Peru and Spain, I present evidence that the anticipation of compulsory conscription reduces the labor force participation of teenage men by 6.7 percent compared to men in their twenties, and employment by 11 percent, while raising unemployment in this group by 9 percent. Interestingly, I find opposite effects on teenage women who are not subject to conscription. Women experience a 7.5 percent decrease in the labor force participation and a 10-13 percent decrease in employment after the abolition of conscription, suggesting a high degree of substitutability between men and women.

Many government policies – old age pensions, immigration restrictions, military conscription-alter the labor market participation of some people and therefore may also shift labor demand for others. Yet, finding policy variation that alters the labor supply of one group at a single moment and in large numbers is difficult. In the second chapter, I study how the outflow of conscription-age men from local labor markets in Turkey affects the employment opportunities of men and women at close ages. I use unique variation in the military recruitment seasons across cities in Turkey. I find that the outflow of 10 men aged 20-21 increases the number of employed women and men at close ages by 1.6 and 4.6 respectively. Also, since most men who enlist in the military at 20 years old are not college graduates, I find that similarly educated men and women are affected more by the inflow or outflow of conscription-age men compared to men and women with a college degree. These results show that the evaluation of military conscription policies should also incorporate the effect on those who do not serve, and the government policies that affect work incentives of one group can have aggregate effects on other workers.

The third chapter investigates the effect of compulsory schooling on early labor market outcomes. The 1997 reform in Turkey which extended compulsory schooling from 5 to 8 years provides an opportunity to estimate the returns to schooling in a middle-income country. The availability of a rich set of early labor market variables also provides an opportunity to assess mechanisms through which returns to schooling occur. I find quite small effects of compulsory schooling on earnings of men but large positive effects on earnings of women who work, without raising their overall low rate of labor force participation. In terms of mechanisms, I find that women who worked moved into

higher skill and formal sector jobs, which involved more complicated tasks on average. I propose that differential marginal costs of schooling explain the low average schooling level among women before the reform and the very different outcomes of the reform for men and women.

**JEL Codes:** I21, J21, J24, J31, J61, H56, R23.

**Keywords:** Military Service, Labor Force Participation, Youth Unemployment, Difference-in-Differences, Labor Substitutability, Returns to Education, Compulsory Schooling.

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# Chapter 1

# Ex-Ante Labor Market Effects of Compulsory Military Service

## 1. Introduction

Throughout the twentieth century, the armed forces in many countries recruited personnel through conscription. Given the compulsory military service (CMS) laws, a significant portion of sixty year-old males around the world served in the military when they were young. However, since the end of the Cold War many countries have moved from conscription to all-volunteer armies, especially in North America and Europe. France, considered the first country to introduce modern conscription, abolished it in 1997, and most recently Germany abolished conscription in 2011. Yet some European countries and many Asian and African countries including Austria, Brazil, Greece, Denmark, Egypt, Israel, Russia, South Korea, and Turkey still use conscription to supply their armed forces with qualified personnel. Among the latter group, the duration of compulsory service and the possibility of eliminating it have frequently been debated. Turkey is now reducing the duration of compulsory service from 15 months to 12 months. The decision to switch from a conscription army to a professional one is partly strategic and partly economic.

In this paper, I examine the effect of peacetime conscription on the early labor market outcomes of potential conscripts *before* they are called up for service. Using micro-data from four countries, I present evidence that the existence of compulsory conscription reduces the labor force participation and employment of male teenagers who are waiting to be called into service. Similarly, I observe an increase in male teenage employment and a decrease in female teenage employment after the abolition of conscription. The economic effects in advance of conscription have not been studied previously, to my knowledge.

I use both cross-country and within-country strategies to investigate the effect of peacetime conscription on the early labor market outcomes of potential conscripts. In a simple theoretical model with costly job search and no job security, I show that an expected interruption in civilian life reduces the incentive of teenagers to search for a job. Moreover, when firms bear the cost of on-the-job training, an expected interruption may reduce the employers' likelihood of offering a job to expected future conscripts.

I use labor force survey micro-data from Turkey (which has CMS), Argentina and Peru (which do not have CMS), and Spain, which eliminated it in 2001. My results show that CMS decreases the labor force participation and employment of male teenagers at ages at which they are waiting to be called into service. I also show that the existence of CMS in Spain reduced the chance of finding a job, and that its elimination increased employment of teenage men while decreasing employment of teenage women.

I start my analysis by pointing out the low labor force participation of males in their late teens in Turkey, focusing on young men who finished their schooling at an early age and comparing them to men in their twenties with the same level of education. The differences between the two groups may reflect unobserved characteristics that keep teenage men out of the labor force. To test this possibility, I incorporate data from the “control” countries, Peru and Argentina, and show that, although labor force participation trends are very similar in all three countries among men in their twenties with the same level of education, labor force participation is significantly lower in Turkey among teenage men. Finally, using the Encuesta de Población Activa (EPA) before and after Spain abolished CMS in 2001, I present within-country evidence that the abolition of CMS increased the labor force participation and employment of teenage men and decreased it for teenage women. Using a difference-in-differences methodology, I find that the abolition of CMS increased the labor force participation of teenage men by 5.9 percentage points (6.7 percent) compared to men in their twenties and increased their employment by 6.7 percentage points (11 percent). Therefore it decreased the likelihood of unemployment by around 3.1 percentage points (9 percent). Finally, I find mirroring results for teenage women, suggesting substitutability in the labor market. Teenage women experience a 7.5 percent decrease in the labor force participation after the abolition of conscription.

The first contribution of this study is to highlight the anticipation effects of CMS, as the previous literature focuses on the effect of military service on subsequent labor market outcomes only. By showing the ex-ante effects of CMS, I point out one cause of low labor force participation among youth in the countries with CMS, which is a common concern around the world.

Second, I show one channel through which conscription may affect future labor market outcomes of individuals. It is well documented that early labor market experiences have significant effects on later labor market outcomes (Smith, 1985). This channel that occurs before conscription may partially explain why the previous literature finds either negative or no effect of conscription on later wages, even though some hypothesize that the effect should be positive. Third, Galiani et al (2011) exploit the Argentine draft lottery and show that military conscription raises the likelihood of later criminal activity, particularly property crimes. This study highlights another channel through which this may occur, involving the lack of teenage labor market experience. Similarly, Jacob and Lefgren (2003) show that idleness among high school students causes them to engage in more property crimes. Finally, I exploit a credible natural experiment to demonstrate the substitutability in the labor market across types of workers.

In the next section, I review the literature on the effects of military service. Section 3 provides a brief institutional background of CMS in the four countries. Section 4 presents a theoretical framework that motivates the effect of CMS on teenage labor market outcomes. Section 5 describes the data sets and the key variables, and explains my sample selection from each country. Section 6 covers the empirical strategy and presents the results. Section 7 concludes.

## 2. Literature Review

The economics literature informs the debate about compulsory military service in a number of ways. One branch of the literature analyzes the budgetary and opportunity cost of a conscription army versus a professional army given the goal of supplying the armed forces with qualified personnel (Warner and Asch 2001). Another branch investigates how the existence of CMS affects aggregate labor market characteristics relative to other countries with no CMS. Given the high number of countries switching from conscription to professional armies in the last two decades, it is surprising that there is little evidence on aggregate labor market effects (Feldman, 2009).

Finally, a recent branch of the literature examines the effect of military service on individual outcomes such as labor market performance, educational attainment, cigarette and alcohol consumption, and criminal activity. These papers use within-country variation in veteran status across citizens and generally attempt to find exogenous variation in veteran status. Angrist (1990) and Angrist and Krueger (1994) investigate the effect of serving in the U.S. military during wartime on future labor market outcomes in an instrumental variables framework, whereas Imbens and van der Klaauw (1995) and others examine the effect of peacetime conscription in Europe. Bound and Turner (2002) and Barr (2013) investigate whether military service increased veterans' educational attainment through G.I. benefits in the U.S., and Goldberg et al (1991), Eisemberg and Rowe (2009), and Galiani et al (2011) estimate the effect on later substance use and criminal behavior.[1] This paper belongs to the second and third branches of the literature by using both cross-country and within-country empirical strategies and by focusing on individual outcomes.

I focus attention here on papers that analyze labor market outcomes as a result of conscription. As mentioned above, the frequent switches from a conscription army to a professional army provides variation in conscription status across countries. Yet most cross-country studies in the literature examine the relationship between military expenditure and economic growth using cross-section or panel data on the expenditure

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[1] The literature on draft avoidance behavior of individuals can be considered as investigating the ex-ante effects of conscription on educational attainment (Card and Lemieux, 2002; Maurin and Xenogiani, 2007).

and economic growth of countries (Stroup and Heckelman, 2001; Deger and Sen, 1995; Shieh et al, 2002). Keller et al (2009) focus on conscription and examine the relationship between conscription and economic growth in OECD countries. Feldman (2009) uses data on 73 economies to investigate the effect of labor regulation on unemployment around the world. He finds that, no or short military conscription is correlated with lower unemployment among the total labor force. Yet none of these studies investigates the causal link between conscription and labor market outcomes in depth. In this study, I use cross-country comparisons to present evidence on the effect of conscription on aggregate labor market characteristics.

Identifying the effect of conscription on individual outcomes is a difficult task because there is non-random selection to the military service. Even countries with universal conscription allow certain exemptions based on characteristics such as health and occupation. Thus comparing the individual outcomes of interest between veterans and non-veterans is misleading unless the researcher also controls carefully for the characteristics that determine the veteran status. However, data on such characteristics are rarely available to the researchers. For example, if the army recruits its members after medical and psychological examinations, an ordinary least squares (OLS) estimate of the effect of military service on labor market outcomes would be upward biased. If men with relatively fewer civilian opportunities are more likely to enlist in the military or men from wealthy families are less likely to enlist, an empirical approach that does not control for civilian earnings potential or family background would underestimate the benefit or overestimate the cost of military service (Angrist, 1990; Angrist and Krueger, 1994). Some papers that focus on individual outcomes therefore use instrumental variables strategy to examine the effect of military service, while others, like this paper, obtain intent-to-treat estimates on the cohorts who are eligible for military conscription.

Angrist (1990) and Angrist and Krueger (1994) investigate the effect of wartime military service during Vietnam War and World War II on future labor market outcomes, and use date of birth as an instrumental variable for the probability of being drafted. These studies found that white Vietnam-era veterans earn 15 percent lower annual wages than non-veterans 10 years after the war, and World War II veterans earn no more, possibly less, than non-veterans. Imbens and van der Klaauw (1995) use variation in the aggregate military enrollment rates across cohorts in a grouped-data IV framework to identify the causal effect of peacetime military service in the Netherlands on future earnings. They find that, 10 years after service, those who served in the military earn 5 percent lower annual wages than those who did not. Grenet et al (2011) exploit the discontinuous change in the probability of conscription into the British army during the post-war period in a regression discontinuity design (RDD) and find no effect on earnings of peacetime compulsory military service. Similarly, Bauer et al (2012) use RDD based on a discontinuity in the probability of being drafted across cohorts in Germany and find no significant effect of conscription on future wage or employment of the former conscripts. Paloyo (2010) revisits conscription in Germany in a difference-in-differences framework and verifies that CMS has no effect on the labor market performance of conscripts.

The papers mentioned above focus on the effect of military service on future labor market outcomes. Also, it is worth noting that they identify the Local Average Treatment Effect, with treatment being the actual conscription. My approach differs because I investigate the ex-ante outcomes of CMS before the actual call to service. My findings should be interpreted as Intent-to-Treat estimates, or the effect of the existence of conscription in a country. In this sense my work is similar to the study from the retirement literature by Friedberg (2003) who points out that impending retirement reduces the incentive of older workers to invest in new computer skills. This is because approaching retirement reduces the time to recoup the investment in new skills. It is also important to note that wartime conscription may have different effects than peacetime conscription. Although both types of military service cause a loss of job market experience among those who serve, the war-time military service may have a higher physical and psychological cost.

## 3. The Institutional Background

In the first part of the empirical analysis I compare Turkey, which still has CMS, to Peru and Argentina, middle-income countries which abolished CMS in 1999 and 1995, respectively.[2] To do this, I use labor force micro data for the years 2004-2011 for each of these countries. Then, I use Spanish micro-data before and after the abolition of conscription in 2001 to estimate the effect on ex-ante labor market outcomes of individuals. In this section, I first describe the "treatment", focusing on the institutional details of CMS in Turkey. Then, I switch to the details of control group countries, Peru and Argentina. Finally, I present the institutional details of military service in Spain before and after the abolition of CMS.

[2] It should be noted that, in both countries, the government retains the right for conscription in case of emergency. Yet, in practice, both armies currently consist of volunteers.

Conscription in Turkey was introduced in 1916. Since then, all able-bodied men have to enlist in the military when they turn 20.[3] All men from a particular cohort are called for medical and psychological examinations around their 19th birthdays. Those who are fit for service are called up when they turn 20. A second group of men who have temporary health problems are deferred from service. A third group of men who have severe health problems or extreme BMI values are permanently exempted from military service. Unlike some other countries, there is no exemption based on one's occupation.[4] Thus, the number of permanent exemptions is negligible. Those who are required to serve are not necessarily called up immediately. Individuals who are enrolled in higher education can defer their military service until they reach 29. High school graduates or two-year college graduates can defer their service until the age of 22 or 23. These age differences in service requirements are a major reason that I focus most of my analysis on people who have completed their education at a younger age.

The duration of CMS in Turkey has changed several times throughout the century reaching its maximum during World War II. A legal change in 2003 shortened the duration of CMS from 18 months to 15 months. The most recent change was made in October 2013, and it shortened the duration of service form 15 months to 12 months effective January 2014. As will be explained in the next section, the data from Turkey, Argentina and Peru cover the years 2004-2011. The duration of CMS in Turkey was 15 months for most men during these years. Yet military service duration varies depending on one's higher education status. Those who have four-year college degree either serve

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[3] Women are exempt from compulsory military service in Turkey, but they are allowed to join the army as professional military officers.

[4] For example, police and fire men are not exempt from the military service. Yet there are other forms of exemptions. For example, a man whose brother lost his life during military service or was seriously injured is exempt from compulsory military service. The laws do not allow conscientious objection.

for 12 months as an officer candidate among military officers, or for 6 months as a sergeant among enlisted soldiers. The final allocation of college graduates between 12-month service and 6-month service depends on both individual preferences and the necessities of the army. Men who studied in certain fields, such as medicine or engineering, are more likely to be assigned 12-month officer jobs. Unlike the other conscripts, college graduates who serve for 12 months are paid.[5] All conscripts receive basic training for around two months and after that they are allocated to their divisions for active duty. Although the exact number of conscripts has varied over time, the Turkish armed forces comprise around 200,000 officers and professional soldiers and around 400,000 conscripts.

Peru modified its military service act in 1999 and made military service voluntary for any physically and mentally-able Peruvian aged 18-30. After the switch to a professional army, the government increased the benefits of joining the armed forces by providing technical job training and education benefits. Currently the Peruvian armed forces comprise around 120,000 active- duty personnel in total.

Compulsory military service in Argentina was created in 1901 and was suspended after the murder of an 18 year-old conscript by two soldiers in 1994. Before the suspension, 18-year old Argentineans were randomly selected and recruited for 14 months. In the all-volunteer army, soldiers are paid for their service and they can stay in the armed forces up to ten years. The number of active personnel in the Argentinean armed forces is around 70,000. Neither of these countries had CMS between 2004 and 2011.

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[5] In practice all conscripts receive extremely small, symbolic salaries.

The length of compulsory military service in Spain was nine months, shorter than the fifteen months in Turkey, before its abolition in December 2001. The total armed forces comprised around 250,000 personnel in 1999, and conscripts constituted more than half of it. By 2002, the size of the armed forces was below 200,000. Before the abolition of conscription in Spain, men were called up for physical and psychological examination before the age of 19. Those who were fit for service were called up in the year that they turn 19. Individuals who had temporary health problems could defer the service. Those who were not physically or mentally fit for military service were permanently exempt, as in Turkey.

Also, as in Turkey, those who were not exempt from military service did not necessarily begin to serve immediately. There were a number of reasons for deferring military service. One common reason for deferment was education. Individuals who were enrolled in school could defer the military service for as many as seven years. Conscientious objection was also popular in Spain. According to Spanish law conscientious objectors were allowed to do alternative public service in institutions such as hospitals for the same duration and pay. The ratio of conscientious objectors for military service in 2001 to those who actually enrolled in the armed forces for serving in 2001 was more than a quarter, but one's status as conscientious objector should not alter the ex-ante effects of conscription on labor markets.

The intent of the Spanish government to abolish compulsory service was known as early as 1999. Initially the laws said that CMS would be abolished by the end of 2002. In early 2001, another law changed the end of conscription to December 2001. Similar to the draft avoidance behavior of individuals in Card and Lemieux (2002) and Maurin and

Xenogiani (2007), this information might have led more individuals to stay in school and defer military service. For those who deferred service in 2000 and 2001, the deferment practically meant an exemption from military service.

The institutional details of compulsory military service in Spain affect my analysis in two important ways. First, since individuals who are enrolled in school at the conscription age can easily defer military service, I exclude college graduates and those who were enrolled in school when surveyed. I concentrate on those who finish schooling at a relatively early age, at or before sixteen. This way, the individuals in my sample are more likely to be affected by CMS laws and its abolition. Second, the fact that individuals expected the abolition of the conscription would make them more likely to ignore the possibility of conscription and join the labor force. Thus, the estimated effect of the existence of CMS in Spain should be a lower bound.

## 4. Theoretical Framework

In this section, I present a simple three-period model with costly job search and no job security to illustrate labor force participation decisions of individuals in their late teens. I show that an expected interruption in civilian life in period 2 reduces the incentive of teenagers to search for a job in period 1.[6] Comparative statics analysis provides some testable predictions that I investigate empirically. When we look at the employer side, similar results would also arise in three-period model of hiring with on the-job-training. An expected interruption reduces the incentive of employers to hire workers in the first period. Although the theoretical framework presented here aims to

[6] The same effect may arise if continuous labor market experience is more valuable, in terms of building human capital, than interrupted labor market experience.

show the effect of compulsory military service, it applies to any expected interruption in the civilian life.

### 4.1. Labor Force Participation Decision

I focus on a three period model in which individuals may be exogenously called up for military service in the second period. Three periods represent the early labor market experience of young men in a country with compulsory conscription. The first period can be thought of as the late teenage years, the years before the conscription age. The second period can be thought of as the conscription age, at which young men may be called up for the service. The third period corresponds to the years after the conscription age.

In the model, individuals begin life without a job. Each period, individuals receive non-wage benefit, $c$, regardless of their employment status. This benefit represents home production, family support or the very small compensation given in the army. Individuals discount future periods by a discount factor, $\beta$, where $0< \beta<1$. They can be called up for military service in the second period with exogenous probability $\eta$. In the first period, they decide whether to search for a job. Job search is a costly effort that happens at the beginning of a period. I assume that anyone who pays the search cost, $z$, finds a job. Individuals who find a job start working in the same period and receive a wage, $w$, for that period. Once an individual finds a job, there is no separation unless he is conscripted into the army in the second period, in which case he loses his job. If an individual who worked in the first period is called up for service in the second period, he needs to search for a job in the third period if he wants to work. Everyone knows whether or not he will be called up for military service at the beginning of the second period. Therefore, an individual who worked in the first period does not start working in the second period if he is called up. Similarly, an individual who did not work in the first period does not pay a search cost in the second period if he knows that he will join the armed forces.

Individuals optimize between searching and not searching at the onset of each period in which they do not have jobs and are not in the armed forces. Each individual balances the cost of search with the wage benefit of employment considering the probability of losing his job due to compulsory conscription. The benefits differ across periods. The search cost, $z$, differs across individuals but stays the same for each individual across periods. Although the cost of search, $z$, has a distribution $f(z)$ across individuals, I will use a generic $z$ in the following model, and refer to the distribution at the end.

I approach the optimization problem in a recursive way. There is no decision making for individuals who worked in the second period. They continue working in the third period. However, an individual who did not work in the second period, either because he lost his job due to compulsory conscription or he did not search for a job in the first two periods, compares the search cost and the wage of the last period. In the third period, the value function for an individual who did not work in the second period is

$$V_3 = max\{not\ search, search\} = max\{c, c - z + w_3\}.$$

Individuals who worked in the first period continue working in the second and third period if they are not called up for military service. In addition, there is no decision making for conscripts in the second period. However, civilian individuals who did not work in the first period choose whether or not to search in the second period. If they search in the second period, they will not need to search again in the third period. In the second period, the relevant value function for a civilian individual who did not work before is

$$V_2 = max\{not\ search, search\} = max\{c + \beta V_3, c - z + w_2 + \beta(c + w_3)\}.$$

In the first period, individuals decide whether or not to search taking into account the probability of conscription in the second period. The value function for an individual in the first period is

$$V_1 = \{not\ search, search\}$$

$$V_1 = max\{c + \beta[\eta\ (c + \beta V_3) + (1 - \eta)V_2],$$
$$c - z + w_1 + \beta[\eta\ (c + \beta V_3) + (1 - \eta)(c + w_2 + \beta(c + w_3))]\}.$$

Since the value of the choice in the third period affects the choices in the previous periods as well, one should know whether a jobless individual searches in the third period. There are two possible cases regarding the choice in the third period based on the value of search cost relative to the wage in the third period.

***Case 1: No search in the third period:***

Assuming that $z > \mathrm{w}_3$, an individual does not search in the last period if he does not have a job at that point. Thus, $V_3$ boils down to $\overline{V_3} = c$. Individuals do not find it worth paying the search cost for the third period wage only. A civilian individual who is not called up for service and who did not work in the first period knows that if he does not search in the second period he will only get $\overline{V}_3 = c$ in the third period. So his problem in the second period becomes

$$V_2 = max\{c + \beta c, c - z + w_2 + \beta(c + w_3)\}.$$

Assuming that $z < w_2 + \beta w_3$, a jobless civilian individual chooses to search in the second period.[7] Then $V_2$ boils down to $\overline{V_2} = c - z + w_2 + \beta(c + w_3)$. So when individuals start the first period, they know that if they choose not to search and they are not called up for military service, then the discounted value at the beginning of the second period will be $\overline{V_2} = c - z + w_2 + \beta(c + w_3)$. Then, individuals at the beginning of the first period face the problem

$$V_1 = max\Big\{c + \beta\big[\eta\,(c + \beta c) + (1 - \eta)\,\big(c - z + w_2 + \beta(c + w_3)\big)\big],$$
$$c - z + w_1 + \beta[\eta\,(c + \beta c) + (1 - \eta)(c + w_2 + \beta(c + w_3))]\Big\}.$$

This choice reduces to comparing $-\beta\,(1 - \eta)z$ with –z+ $w_1$. An individual in the first period chooses not to search if $-\beta\,(1 - \eta)z$ > –z+ $w_1$ i.e. $z > \frac{w_1}{(1- \beta+\beta\eta)}$, and chooses to search if $z < \frac{w_1}{(1- \beta+\beta\eta)}$.

The threshold value of the search cost is $\hat{z} = \frac{w_1}{(1- \beta+\beta\eta)}$, and I will discuss comparative statics later. If individual search cost $z_i$ is smaller than $\hat{z}$, then an individual pays the search cost in the first period and starts working. If $z_i$ is bigger than $\hat{z}$, then the individual does not find it worth paying the search cost and stays out of the labor force in

[7] We can also consider the case with $w_2 + \beta w_3 < z$. Then, civilian individuals who did not work in the first period do not search in the second period. This makes jobless civilian individuals indifferent between conscription and civilian life in the second period. In either case, they will receive the non-wage benefit, $c$, in the second and third periods. Then, there is a threshold value for $z$ above which individuals do not search in the first period either.

the first period. We should also check the previous two assumptions. The assumptions $w_3 < z$ and $z < w_2 + \beta w_3$ do not restrict the z values too much. There is a range of z values that satisfies $w_3 < z < w_2 + \beta w_3$, and lead some individuals to search and others not to search in the first period.

***Case 2: Search in the third period:***

If we assume that $z < w_3$, then individuals with such $z$ values search in the third period if they do not have jobs at that point. Then, $V_3$ boils down to $\bar{V_3} = c - z + w_3$. The optimization problem of a civilian individual in the second period who is not called up for service and who did not work in the first period becomes

$$V_2 = max\{c + \beta(c - z + w_3),\ \ c - z + w_2 + \beta(c + w_3)\}.$$

He chooses to search in the second period as long as $w_2 - z > -\beta z$ $i.e.$ $z < \frac{w_2}{1-\beta}$. Assuming that he searches, $V_2$ becomes $\bar{V_2} = c - z + w_2 + \beta(c + w_3)$.[8] Thus, individuals at the beginning of the first period face the following choice

$$\begin{aligned} V_1 = max\Big\{&c + \beta\big[\eta\left(c + \beta(c - z + w_3)\right) + (1-\eta)\left(c - z + w_2 + \beta(c + w_3)\right)\big]\ , \\ &c - z + w_1 \\ &+ \beta[\eta\ (c + \beta(c - z + w_3)) + (1-\eta)(c + w_2 + \beta(c + w_3))]\Big\}. \end{aligned}$$

An individual at the beginning of the first period chooses not to search if $z > \frac{w_1}{(1-\beta+\beta\eta)}$, and chooses to search if $z < \frac{w_1}{(1-\beta+\beta\eta)}$. Here, we should check the previous two

[8] There is potentially another case. If $w_3 > z > \frac{w_2}{1-\beta}$ then a jobless civilian individual who is not conscripted does not search in the second period although he is willing to search in the third period. Because $\beta$ is typically close to 1, this case is unlikely to realize unless the third period wage is far bigger than the second period wage.

assumptions. There is a range of $z$ values that satisfies $z < w_3$ and $< \frac{w_2}{1-\beta}$ , and lead some individuals to search and others not to search in the first period.

Tables 1 and 2 show search behavior of individuals with different values of $z_i$ , given two possible wage sequences. For the first case, the threshold values for the three periods are $\hat{z} = \frac{w_1}{(1- \beta+\beta\eta)}$, $w_2 + \beta w_3$ and $w_3$, respectively. For the second case, they are $\hat{z} = \frac{w_1}{(1- \beta+\beta\eta)}$, $\frac{w_2}{1-\beta}$, and $w_{3,}$ respectively. In almost any wage structure, we expect that $\frac{w_2}{1-\beta} > w_2 + \beta w_3 > w_3$. Yet, the threshold value of the search cost in the first period, $\hat{z} = \frac{w_1}{(1- \beta+\beta\eta)}$, can be smaller or larger than $w_3$. Table 1 illustrates the search behavior of individuals where $\hat{z} = \frac{w_1}{(1- \beta+\beta\eta)} > w_3$, and Table 2 does the same where $\hat{z} = \frac{w_1}{(1- \beta+\beta\eta)} < w_3$.[9] Note that first period search is monotonic in the search cost. Table 1 shows that, for intermediate values of the search cost, someone may search in the first period, but will not search in the third period if they are drafted in the second period. Similarly, Table 2 shows that, for intermediate values of the search cost, some people will not search in the first period and then search in the third period if they are drafted in the second. In both tables, as the threshold value of the search cost decreases, fewer individuals search in the first period.

In either of the cases above, the threshold value for searching in the first period can be defined for the ratio of search cost to first-period wage. This threshold value is $\frac{\hat{z}}{w_1} = \frac{1}{1-\beta+\beta\eta}$. If the search cost-first period wage ratio exceeds this value, then an individual does not search in the first period.

[9] One can consider more different wage structures using the same reasoning.

**4.2. Comparative Statics**

Next, I use the model to develop predictions for the effect of conscription regulations on the search effort of young men.

**i.** The model shows that anything that affects the threshold value for the search cost, $\widehat{z}$, affects the labor force participation of young men. Given $w_1, w_2, w_3,$ and discount factor $\beta$, and an underlying distribution of $z$, $f(z)$, the first partial derivative of $\hat{z} = \frac{w_1}{(1-\beta+\beta\eta)}$ with respect to $\eta$ is negative. Therefore, as the probability of conscription in a country increases, the mass of individuals with a $z$ value less than $\hat{z}$ gets smaller, and less people join the labor force in the first period. Equivalently, if a country abolishes conscription, $\hat{z}$ increases, and labor force participation among young men increases as well. A similar reasoning applies to cross-country analysis.

**ii.** Everything else constant an increase in the first period wage increases the threshold value for first period search, $\widehat{z}$. The cumulative distribution $F(\hat{z})$ increases and we observe more people with search cost values under the threshold. This implies that if the wage rate for teenage men increases, more men join the labor force before they reach the age of conscription.

**iii.** An increase in the discount factor, $\beta$, increases the threshold value for the search cost $\hat{z}$. Thus, more people pay the search cost and start working in the first period. As explained above, with some probability of conscription, the second and third periods are the same across all individuals regardless of their employment status in the first period. The three periods become independent from each other, and the discount factor does not affect the first period decision. With some probability of not being conscripted, individuals balance cost of search in the first period with the cost of search tomorrow, in the second period. An increase in the discount factor makes individuals more likely to pay the search cost early.

As mentioned in Section 2, the ex-ante labor market effect of conscription has received little attention. The theoretical framework above shows its potential effects on individuals before they are called up for service. Next, I illustrate how compulsory conscription also affects the hiring decision of employers.

### 4.3. The Hiring Decision

Similar results would also arise in a three-period model of hiring with on-the-job training. This model looks at the other side of the coin. Employers choose whether or not to hire a job applicant considering the expected interruptions in the applicant's labor market experience.

Suppose that, rather than a fixed search cost, there is a one-time training cost for all new hires at the expense of employer. If a worker leaves the firm due to conscription, then he needs to be trained again in his third period. This is in line with many studies that refer to the depreciation effects of compulsory conscription.  This also fits to the case where training is firm-specific. If a worker who worked for a firm in the first period is called up for military service, he quits the job and joins the armed forces in the second period. Once he is back to civilian life in the third period, he is no different than other job applicants to employers.[10]

[10] This is consistent with the labor force participation model in section 4.1. There is no job security.

In this theoretical framework, in the absence of conscription, an employer would prefer hiring someone in the first period of his life because he is able to gain from the productivity of the worker for three consecutive periods and only pays the training cost once. The advantage of younger applicants compared to older ones decreases as the conscription probability in a country increases. Similarly, if a country abolishes conscription completely, the employment likelihood of men younger than the age of conscription rises.

## 5. Data

I focus my entire analysis on men (and occasionally women) who finished their schooling at an early age, before conscription age, so that schooling and labor force participation decisions are not confounded, especially because schooling may lead to deferred conscription. Throughout my analysis, I compare men in their late teens and men in their twenties with the same low level of education. Because men with different ages may differ for other reasons, I compare statistics for men in their late teens and in their late twenties across countries and within Spain before and after the abolition of CMS. If there is a common age-labor force participation profile for men in their twenties across these countries, but not for men in their late teens when they face CMS, it suggests that CMS causes the different outcomes. To undertake this analysis, I incorporate data from one country that retains CMS, Turkey, two countries that no longer have CMS, Peru and Argentina, and one country that recently abolished CMS, Spain in 2001. Below, I describe the data from the four countries.

### 5.1. Common Definitions

My theoretical model suggests that in a country with CMS such as Turkey, the existence of compulsory conscription would affect the ex-ante labor market outcomes of individuals who are about to choose between joining the labor force and waiting for conscription. Therefore, I concentrate my empirical examination on young men who finished schooling at relatively early ages. College students can defer military service in all three countries and they are already above 20 when they graduate from college, so they can choose to do military service right after graduation. Inefficiency due to anticipation of service is therefore not a serious concern for college graduates. Consequently, in all three samples described below, I exclude those who are enrolled in school or who have any college education. Unlike urban areas, individuals in rural areas, especially with family farms, always have some work to do on the farm. The surveys are designed in a way that anyone who finished schooling is considered employed if he works for the family business. Therefore, labor force participation or employment figures in the rural areas are less informative about labor market decisions and opportunities for young men. Thus, I drop the rural population in the three samples.

I generate three schooling categories in each country. Those who have primary/elementary school degree or less are in the first group. They have six years of schooling or less. Those who have a middle school degree in Turkey or are secondary school drop outs in Peru and Argentina are the second group. Finally, those who have a high school degree in Turkey or a secondary school degree in Peru or Argentina are the third group. In the next section, cross-county analysis is carried out separately for these three samples: those who have elementary school degree or less, middle school degree or less, and high school degree or less.

### 5.2. Household Labor Force Survey of Turkey

I use the 2004-2011 releases of the Household Labor Force Survey, Hanehalkı İşgücü Anketi (HİA), conducted by the Turkish Statistical Institute. Each survey covers about 150,000 households and 500,000 individuals annually. This micro-data set is a cross-section. Like other labor force surveys around the world, the survey excludes the institutional population such as prisoners and conscripts in the armed forces. Therefore, I have fewer observations of men than women at the age of 20. Respondents report demographic and detailed labor market characteristics. The original public version of the dataset provided individual age in ranges. This makes it difficult to see how labor force participation changes within narrow age categories, which is needed for this study. I obtained additional files on the exact age of respondents from the Institute and merged them with the original data.[11] Among many labor market characteristics, I focus on whether or not the individual is in the labor force and is actively searching for a job or employed.

I restrict my sample to individuals aged 17-30 in any of the survey years from 2004 to 2011. During these years, military service in Turkey was compulsory, as described in Section 3, 15 months of service were required for non-college graduates. It is worth restating that college graduates can defer military service until they finish the college, confounding the opportunity to look at behavior during ages at which they are anticipating conscription.

[11] I would like to thank the staff in the Labor Force Statistics Department of Turkish Statistical Institute for providing me the access to these supplementary data.

About 4 percent of the sample between 17 and 30 is missing year of birth information, so I drop them. I also drop those who live in rural areas with population less than 20,000. Finally, I drop those who were enrolled in school or who have any college degree, leaving 48% of the original sample aged 17-30. So I analyze a sample of 419,023 individuals aged 17-30 from survey years 2004-2011. Table A1 in the Appendix provides sample statistics for the main variables, separately for women and men of the baseline sample used in this paper.

### 5.3. National Household Survey of Peru

I use the 2004-2011 releases of the National Household Survey, Encuesta Nacional de Hogares (ENAHO) micro-data to investigate labor force participation across ages in Peru. The survey is conducted by the National Statistics Institute of Peru continuously every week. The data set is a cross-section with a panel subsample that begins in 2008. Each year the institute surveys more than 20,000 households and collects information on employment, income, education and household demographics among other things. The survey is organized in 29 modules. For this study, I merge the information from modules on household characteristics, education and employment status. Since there is a panel structure as well, I drop all duplicates from the data.

For comparability across countries, I restrict my sample to individuals aged 17-30 in any of the survey years from 2004 to 2011. Peru did not have compulsory military service in any of these survey years. The country had already switched to a professional army in 1999. Residential areas are categorized based on the number of households. I drop observations from rural areas with fewer than 4,000 households. In addition I drop those who were enrolled in school or who have any college degree. This leaves 54,132 observations over eight years. Table A2 in the Appendix presents sample statistics for the main variables of interest, separately for women and men.

### 5.4. Household Continuous Survey of Argentina

The data used to investigate Argentina come from the Household Continuous Survey of Argentina, Encuesta Permanente De Hogares (EPH). This is a panel household survey conducted by the National Institute of Statistics and Census Institute in 31 urban centers populated by at least 100,000 residents. The survey contains information on employment, demographic, economic and social characteristics of the non-institutional population. The EPH represents more than 60% of the country’s population. The survey is a rotating panel that is conducted over the whole year and is published four times a year. Each selected household is interviewed in four waves of the survey. They are surveyed in two consecutive quarters, then not surveyed for two quarters, and then appear in the sample again for two consecutive quarters. I drop all duplicates from the data and concentrate my analysis on its cross-sectional aspect.

Because the survey is only representative of urban areas, I do not restrict the sample based on the area of residence. Similar to the previous data, I restrict my sample to individuals aged 17-30 in any of the survey years from 2004 to 2011. Note that Argentina did not have compulsory military service during this period. It had abolished conscription in 1995. Similar to the previous two samples, I also drop those who were enrolled in school or who have any college degree from the sample. This makes the sample size 100,362 over eight survey years. Table A3 in the Appendix presents the sample statistics, separately for women and men.

**5.5. Economically Active Population Survey of Spain**

Spain abolished CMS at the end of 2001. Young men who finished schooling at early ages expected compulsory conscription at the age of 19 before 2001, and they did not worry about such an interruption after 2001. As such, I conduct a similar analysis as with the cross-country data using the Economically Active Population Survey, Encuesta de Población Activa (EPA), from Spain, conducted by the National Statistics Institute. This is a rotating quarterly survey administered to approximately 60,000 households each quarter. The rotation implies that each household remains in the sample for six consecutive quarters. The survey is continuously carried out each week during a quarter. The reference period for most questions in the survey is the week before the interview. The survey contains a variety of questions about demographic, social and economic characteristics of non-institutional individuals.[12]

For my empirical analysis, I am not interested in the panel structure of the survey. Instead, I generate a repeated cross-section sample from survey years 1999-2004 using the first wave of each year.[13] The dataset provides age in ranges. I restrict my sample to the age groups 16-19, 20-24, and 25-29. The fact that age is given in categories does not prevent analysis as the first age group is exactly the treatment group, and the rest is my

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[12] The survey excludes the population in institutional dwellings such as hospitals, hotels, barracks and prisons. According to National Statistics Institute documentation, this group constitutes 0.6 percent of the total population in 2001 Census.

[13] Technically, I should be able to track individuals across years and drop those who appear more than once. Yet I have noticed problems when I use household and person identifiers to track individuals. Therefore, I picked the first wave of each year to minimize the repetition of the same observations. Even when I use the data from all waves of the survey, the estimated effects are very similar.

control group.[14] The age group 16-19 expected conscription before 2001, and they did not worry about it after 2001. In order to restrict my empirical analysis to those who finished schooling at early ages, I dropped those who continued schooling after the age of 16, or who were enrolled when surveyed. Since conscription primarily affects citizens, I also dropped non-citizens from the sample. This leaves me with 62,717 observations from the survey years 1999-2004. Table A4 in the Appendix provides sample statistics for main variables of interest.

Tables A1-A3 provide a comparison of the final samples from the three countries. The average ages among women and men across the three samples are quite similar, given my sample selection criteria. In my final samples of non-college graduate individuals, the high school completion rate among men is 41% in Turkey, 66% in Peru, and 38% in Argentina. Similarly, 32% of men In Turkey, 11% of men in Peru, and 29% of men in Argentina have an elementary school degree or less. The overall labor force participation is quite similar at 84% in Turkey, 86% in Peru, and 89% in Argentina among men aged 17-30. The employment rate is lower in the sample from Turkey compared to the samples from Peru and Argentina.

## 6. Empirical Analysis and Results

In this section I describe my empirical framework and present evidence that compulsory military service reduces young men's labor force participation even before they are called up for service. I start by pointing out the low labor force participation of teenage men in Turkey who finished schooling at an early age compared to similar men

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[14] Although the conscription age is 19, I cannot reject that a fraction of the second group (20-24) might be affected by the abolition of CMS. This should not be a big fraction as my sample consists of men who left schooling before the age of 16.

in their twenties with the same level of education. One explanation for the sharp increase in labor force participation around age 20 may be unobserved age-specific characteristics that are not related to compulsory conscription. To test this possibility, I incorporate micro-data from two other middle-income countries, Peru and Argentina. Graphically, I show that although the labor force participation trends are quite similar in the three countries among men in their twenties, labor force participation is significantly lower in Turkey among teenage men. Then, I present regression results that show significant differences in the age profile of labor force participation in Turkey, compared to both Peru and Argentina. I also show that a difference-in-differences strategy that uses women as control group would provide misleading results on the ex-ante labor market effects of CMS. This is because the assumption of common trends between men and women is clearly violated.

Finally, I present within-country evidence from Spain which abolished the CMS in 2001. I use difference-in-differences (DD) estimation, with men in their twenties as a control group, and show that the abolition of CMS increased the labor force participation and employment of men in their late teens in Spain. I also find that the increase in labor force participation of teenage men after the abolition indirectly affected labor force participation and employment of teenage women in the reverse. Evidence on the substitutability of workers in response to compulsory conscription has also been neglected in the literature.

**6.1. The Labor Force Participation among Teenage Men in Turkey**

Figure 1a separately illustrates the labor force participation rates of young men and women with a high school degree or less using the Household Labor Force Survey micro-data from Turkey covering 2004-2011. The sample contains individuals from urban areas who are not enrolled in school and who do not have a college degree. Although labor force participation ratio trends upward for men, it jumps after the age of twenty. It is around 60% before the age of 20 for men with 11 or fewer years of schooling, and it rises to 88% by the age of 22. For women, it is as low as 25% before and after age 20 and smooth throughout these ages. I narrow down my sample to those with a middle school degree or less education in Figure 1b, and to those with only an elementary school degree in Figure 1c. In all three figures, labor force participation of men jumps as they move from their teens to twenties in Turkey. When we investigate the employment likelihood in the sample, we observe similar patterns in Figures 2a, 2b, and 2c. The employment rate is around 55% before the age of 20 for men with 11 or fewer years of schooling, and it rises to 70% by the age of 22.

Other reasons besides CMS are unlikely to explain the observed jump. Labor laws in Turkey do not prevent teenage men from working at the age of 17-20. Certain heavy industries are not allowed to employ individuals below age 18, but this is only a small share of the total employers, and this restriction would not directly affect individuals' decision regarding labor force participation. While labor demand for very young individuals may be low, it does not explain why there would be a sudden jump for men but not for women at any particular age. Compulsory military service is a major policy that affects men aged 20-21. As the theoretical framework suggests, the compulsory conscription may be discouraging young men from searching for job before and around the conscription age.

In addition, Figures 1a-2c illustrate that the labor force participation and employment trends are quite different between men and women. Overall, men have rising labor force participation across ages, whereas it is relatively stable for women across ages. This shows that assuming common trends between men and women and using women as a control group is misleading in the analysis of military conscription. As will be explained later, instead of using women as a control group, I use men from other ages as a control group in my difference-in-differences analysis of military conscription in Spain.

### 6.2. Cross-Country Evidence on the Effect of CMS: Turkey, Peru and Argentina

#### 6.2.1. Graphical Evidence

One explanation for the sharp increase in labor force participation around age 20 may be unobserved age-specific characteristics that are not related to compulsory conscription. If we observe similar patterns of labor force participation among men in countries without compulsory military service then it would be difficult to attribute low labor force participation in Turkey to compulsory military service. In other words, the theory suggests that, everything else constant, we should observe lower labor force participation among teenage men in countries with CMS than in countries without CMS. In order to examine this, I incorporate micro-data from Peru and Argentina. Although no two countries are the same, these three countries are middle income countries. Yet, the latter two abolished CMS in 1999 and 1995 respectively. As explained in the Section 5, the sample from each country covers the survey years 2004-2011, excludes the rural population, those who have any college degree, and those who were enrolled in school at the time of the survey. I compare elementary school graduates in Turkey to primary school graduates in Peru and Argentina, middle school graduates to secondary school dropouts in Peru and Argentina, and high school graduates in Turkey to secondary school graduates in Peru and Argentina.

Figure 3a illustrates the labor force participation ratio of men with a high school degree at most across ages using ENAHO micro-data from Peru and HİA micro-data from Turkey. Square data points represent the mean labor force participation rate in Turkey, and diamond data point represent that in Peru. Men from Turkey and Peru with no college degree have extremely similar labor force participation rates in their twenties. After ages 20-21, the data points are nearly identical. Yet before ages 20-21, labor force participation is considerably lower in Turkey. When I examine the trends among men with a middle school degree at most in Figure 3b, or among men with an elementary school degree in Figure 3c, I find the same patterns. In both figures, labor force participation is noticeably lower in Turkey among teenage men. The gap is more evident at ages 19 and 20, just below the conscription ages. For example, Figure 3b shows that, among men with a middle school degree or less at the age of 19, the labor force participation rate is 80% in Turkey versus 86% in Peru, while it is almost identical at the age of 22. The same pattern applies to employment status of men in the two countries. Figures 4a, 4b, and 4c show that men younger than 20 have a lower likelihood of employment in Turkey than in Peru, by around 20 percentage points. The employment rate increases substantially in Turkey over ages 21 to 23 and comes much closer at those ages to the rate in Peru, although it remains a few points lower until age 28 or so.

These figures reject the idea that very low labor force participation among teenage men in Turkey is due to unobserved characteristics common across countries. Although the labor force participation and employment trends are extremely similar in both countries among men in their twenties, they are significantly lower in Turkey among teenage men. This supports the theoretical prediction that compulsory conscription has ex-ante labor market effects on individuals before they are actually called for service.

Next, I use micro data from Argentina in a similar fashion to Peru. Note that in the comparison between Turkey and Peru, I can restrict both samples to urban areas. I restricted the Turkish sample to urban areas with more than 20,000 inhabitants, and the Peruvian sample to areas with more than 4,000 households, which is very close to 20,000 inhabitants on average. The EPH micro-data from Argentina covers only urban areas with more than 100,000 inhabitants, so the Argentinean sample may differ from the Turkish sample more than the Peruvian one. Still, graphical evidence from the comparison of samples from Turkey and Argentina support my hypothesis.

Figures 5a, 5b, and 5c illustrate the labor force participation rate of men across ages using EPH micro-data from Argentina and HIA micro-data from Turkey. Again, the labor force participation trends are nearly identical in both countries for men older than 20-21. Yet Figure 5a shows that labor force participation among men younger than 20 is lower in Turkey by about 10 percentage points, compared to Argentina. The same pattern applies to employment status of men in two countries. Men older than 20-21 have very similar patterns of employment in both countries; men in the Turkish sample consistently have lower employment likelihood than men in the Argentinean sample by 2-3 points. Yet Figure 6a shows that men at the age of 19 have 9 percentage points lower likelihood of employment in Turkey than in Argentina.

It is worth noting that in Figures 5b and 6b, in the sample with a middle school degree or less education, 17 and 18 year-old men in Turkey have higher labor force participation rate and higher likelihood of employment than 17 and 18 year-old men in Argentina. For the ages 19-20, however, this is reversed, and men in Turkey have lower labor force participation and lower likelihood of employment. This supports the idea that the effect of expected conscription is concentrated around the conscription ages.

**6.2.2. Regression Analysis**

The previous sub-section presented highly convincing graphical evidence that the labor force participation profile of young men in Turkey, which has compulsory military service, is significantly different from those in Peru and Argentina. Teenage men in Turkey have lower labor force participation and employment likelihood than their counterparts in Peru and Argentina. Now I will carry out a simple regression analysis to show the above-mentioned difference analytically.

Similar to the graphical analysis, I use data of individuals aged 17-30 from survey years 2004-2011 for each country separately. Then I exclude those who are from rural areas, those who are enrolled in school, and those who have any college degree. Next, I generate three samples from each country: high school degree at most, middle school degree at most, elementary/primary school degree at most. In total I have nine subsamples. Then, in each sample I estimate the following regression equation

$$Y_{ijt} = \beta_0 + \beta_1 Age_j + \beta_2 AgeSquare_j + \beta_3 Younger\_20_{jt} + \sum_{t=2005}^{2011} \beta_t Year_t + \epsilon_{ijt}$$

where $Y_{ijt}$ is the labor force participation status or employment status of individual $i$, at age $j$, in survey year $t$, and $Younger\_20_{jt}$ is a binary indicator that takes the value 0 for individuals older than 20 and 1 for those who are 20 or younger. The survey year effects control for time specific characteristics such as labor market conditions that are constant across ages. The regression equation controls for quadratic age trends in labor force participation or employment likelihood. The residual variable $\epsilon_{ijt}$ is assumed to have a zero conditional mean. Considering the potential group structure of the error terms, in all specifications I allow standard errors to be correlated within the same survey year.[15] My strategy identifies whether and how much the younger individuals differ from the overall labor force participation or employment trend.

Table 3 reports the estimated effects of being below the conscription age in nine subsamples from three countries. 20 year-old individuals who left schooling at earlier ages in Turkey and show up in the survey and therefore have not been conscripted are likely to be waiting for the conscription.[16] Therefore they are part of the treated group. Yet there is a small possibility that they are exempt from military service due to other unobserved characteristics that make their labor supply different. To eliminate this

[15] Clustering at the age-group level provides smaller standard errors. I picked the approach with larger standard errors.
[16] Recall that all three surveys cover the non-institutional population. Yet, someone who turned 20 and shows up in the survey may be waiting to be conscripted as the actual conscription happens in one of four periods during the year that an individual turns 20.

concern, I try dropping men at ages 20-21 from the samples at the expense of losing some of the treatment group. In each panel, the first three columns present the estimated effects from the regression for men aged 17-30, and the second three columns exclude men aged 20-21. In all subsamples from Turkey, being younger than 20 is correlated with a significant decrease in labor force participation after controlling for quadratic age trends. However, for the other two countries without CMS, being younger than 20 is not correlated with a decrease in labor force participation. For example, the first column shows that among the sample of individuals without any college degree, being younger than 20 in Turkey is correlated with a 19 percentage point decrease in the labor force participation likelihood compared to those in their twenties. However, the estimated effect is either positive or statistically insignificant in Peru and Argentina. In columns 4-6 that exclude men aged 20-21, the estimates for Turkey are still negative and statistically significant whereas the estimates for Peru and Argentina are either positive or statistically insignificant.

The regressions verify the findings of the graphical analysis. Turkey has significantly lower labor force participation than the other two countries among teenage men compared to men in their twenties. In a cross-country analysis, one cannot expect the estimates from two countries to be the same. Yet, in the current case, the magnitude of the estimated effect of being younger than 20 in Turkey is many times bigger than the estimated effect in Peru and Argentina. The estimated effect in Turkey is large and statistically significant, whereas it is concentrated around zero in Peru and Argentina. Table 4 repeats the same exercise for the effect on employment likelihood. Again, I find a significant negative effect for the indicator for being younger than 20 in Turkey, and insignificant effects in the samples from Peru and Argentina.[17]

In addition, I combine the samples from pairs of countries together and estimate the following regression equation separately for each pair

$$Y_{ijtc} = \alpha_0 + \alpha_1 Age_j + \alpha_2 AgeSquare_j + \alpha_3 CountrywithCMS_c + \alpha_4 Teenage_j + \alpha_5 Teenage_j * CountrywithCMS_c + \sum_{t=2005}^{2011} \alpha_t Year_t + \vec{X}'_{tc}\vec{\vartheta} + \varepsilon_{ijtc}.$$

where $Y_{ijtc}$ is the labor market outcome of individual $i$, in age group $j$, in survey year $t$, in country $c$. $CountrywithCMS_c$ is an indicator that takes the value one if the individual is from the Turkish sample, zero otherwise. $\vec{X}'_{tc}$ is a vector of country-year specific controls such as GDP per capita, overall labor force participation, male labor force participation, overall unemployment and male unemployment rate. Finally, the random variable $\varepsilon_{ijtc}$ measures unobserved characteristics that affect labor force participation of individuals, and it is assumed to have zero conditional mean.

We are interested in the interaction of the indicator for being a teenager with the indicator for being in a country with CMS, Turkey. Panel 1 of Table 5 illustrates that labor force participation among men younger than 20 is lower in Turkey by about 13 percentage points, compared to Peru. The magnitude is smaller among men with a middle school degree or less, and slightly larger among men with an elementary school degree or less. Panel 2 illustrates that labor force participation among men younger than 20 is lower

[17] Note that the magnitude of the estimated effect for Peru is significantly larger than the magnitude for Turkey in the sixth column of Table 4 only, for the sample with an elementary school degree at most.

in Turkey compared to Argentina in the sample with a high school degree or less (by 12 percentage points), and the sample with elementary school or less (by 6.3 percentage points). Yet, teenage men in Turkey with middle school degree or less have slightly higher labor force participation (by 3 percentage points) compared to Argentina. When I drop men aged 20-21 from the sample in the last three columns, because some of them may not be awaiting conscription and may be different on unobserved grounds, I still find that labor force participation among teenage men in Turkey is significantly lower than among those in Peru and Argentina. Table 6 repeats the exercise for the effect on employment.[18] Panel 1 of Table 6 shows that the employment likelihood among teenage men in Turkey is significantly lower than among those in Peru. Panel 2 provides rather insignificant estimates for the effect on the employment likelihood.[19]

Although the cross-country evidence presented above is clear and intuitive, one can still have questions regarding the identification of the effect of CMS. Since no two countries are exactly the same, individuals may be concerned about two countries having different labor market structures or social characteristics for teenage men. Also, the definitions of labor force participation, employment, and schooling levels slightly differ across countries. To deal with these concerns, I generated similar employment and schooling categories across countries as explained in Section 5. Also, having very similar trends across ages after the conscription age in all three counties and controlling for country-year specific characteristics ameliorates the aforementioned concerns.

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[18] One may be concerned that the recent recession affects teenagers differently across countries. Table A7 and A8 in the Appendix restrict the sample period to years before the great recession, 2004-2007. The findings are similar.

[19] In particular, the estimates in the second and fifth columns of Panel 2 in Table 6 are counterintuitive. Those two columns correspond to the sample of Figures 6b that I mentioned at the beginning of Section 6.2.

### 6.3. The Ex-Ante Labor Market Effects of the Abolition of Conscription in Spain

In this section, I investigate the impact of abolishing CMS in Spain using the Encuesta de Poblacion Active (EPA) 1999-2004 in a difference-in-differences framework. The previous figures showed that assuming common labor force participation trends between men and women is misleading. Paloyo (2010) investigates the effect of conscription in Germany on future earnings in a DD framework using women as a control group. Yet he assumes common trends between men and women across cohorts rather than ages, which lessens the concerns that arose when I compared men and women in Turkey. Consequently, I use men in their twenties as a control group and show that the abolition of CMS in 2001 actually increased the labor force participation and employment of teenage men in Spain. Moreover, the employment rate of teenage men conditional on being in the labor force also improved. Lastly, I find that abolishing CMS adversely affected labor market opportunities of teenage women.

As mentioned in Section 3, there were a number of groups that could defer or get an exemption from service. Exemptions and deferments were particularly common in the years directly preceding the abolition of CMS. Since deferments and exemptions are closely related to education, I restrict my sample to individuals who finished schooling at the age of sixteen, a relatively young age. Thus, they would not be deferring compulsory military service due to enrollment in college. Although alternative public service is the same as military conscription in terms of the duration of interruption, individuals who left schooling at an early age are less likely to be conscientious objectors, and in any case conscientious objectors face the same labor market interruption.

Figures 7a, 7b, and 7c illustrate labor force participation among Spanish men across years separately for three age groups: 16-19, 20-24, and 25-29. In all three figures, the labor force participation rate among teenage men jumps as they move from the pre-2001 period to post-2001 period, while it is stable among men in their twenties. It is around 88% before 2001 for teenage men with 10 or fewer years of schooling, and it rises to 94% after the abolition of CMS in 2001. For men aged 20-24 it barely moves from 96% to 97% and for men aged 25-29 it stays at 96%.

When we examine the employment likelihood in the sample, we observe similar patterns in Figures 8a, 8b, and 8c. The average employment rate is 59% among teenage men with 10 or fewer years of schooling before 2001, and it rises to 68% after the abolition of CMS. The change among men in their twenties is as small as 2 percentage points. Figures 9a, 9b, and 9c illustrate that employment rate conditional on labor force participation among teenage men also increases after 2001, whereas it is mostly stable for men in their twenties before and after 2001. Table A5 in the Appendix presents the average values for these characteristics before and after the abolition of CMS separately for age groups and education levels. Next, I will estimate the impact of the abolition of CMS in a DD framework.

Because I investigate the effect of the existence of CMS on ex-ante labor market outcomes, the estimated effects should be interpreted as Intent-to-Treat estimates. Note that the Intent-to-Treat estimates will be smaller than Average Treatment Effect estimates unless everyone in the country actually expects to serve in the military when they turn 19.[20]

The estimation strategy compares the labor market outcomes of teenage men at the same age before and after the abolition of CMS in December 2001. In order to control for any unobserved change in the overall labor market characteristics over this period, I use men in their twenties as the control group. So, my identification strategy identifies the effect of the abolition of conscription by comparing relative differences in labor market outcomes across age groups before and after abolition affected the younger age group. This identification strategy relies on the common trends assumption in the absence of any policy change; I assume that the trends among teenage men and trends among men in their twenties would be similar if there were no change in CMS laws. Note that this does not necessitate that the levels of labor market outcomes be similar across age groups, but rather that the level within each age group changes in a similar fashion across years.

I estimate the following regression equations

$$Y_{ijt} = \delta_0 + \delta_1 Abolition_t + \sum_{j=1}^{2} \theta_j AgeGroup_j + \sum_{j=1}^{2} \psi_j AgeGroup_j * Abolition_t$$

$$+ \delta_2 Year_t + \sum_{k=2}^{k=18} \omega_k Area_k + \vec{X}_t' \vec{\vartheta} + \epsilon_{ijt}$$

[20] Note that unlike the previous literature, the treatment in my paper is not serving in the armed forces. The treatment is the expectation of military service, regardless of whether an individual actually serves or not. So the ITT Effect equals the ATT when everyone expects the conscription.

$$Y_{ijt} = \gamma_0 + \gamma_1 Abolition_t + \gamma_2 Teenage_j + \gamma_3 Teenage_j * Abolition_t + \gamma_4 Year_t + \sum_{k=2}^{k=18} \omega_k Area_k + \vec{X}_t' \vec{\vartheta} + \varepsilon_{ijt}.$$

The dependent variable $Y_{ijt}$ is the labor market outcome of individual $i$, in age group $j$, in survey year $t$, and $Abolition_t$ is a binary indicator that takes the value 0 for observations from survey years 1999, 2000, 2001 and 1 for observations from 2002, 2003, 2004. $\delta_2$ captures the linear year trend, and $\omega_k$ captures the fixed effects for eighteen autonomous communities in Spain. $\vec{X}_t'$ is a vector of year specific controls such as GDP per capita, labor force participation, and unemployment rate. The random variable $\epsilon_{ijt}$ contains unobserved factors that affect the labor market outcomes of individuals and is assumed to have zero mean conditional on all other variables. There are three age groups in the first regression model: 16-19, 20-24, 25-29, and two age groups in the second model: teenager (16-19) and non-teenager (20-29); recall that single ages are not revealed in the Spanish data. The theoretical model predicts that those who are under the conscription age (16-19) will be particularly affected by its abolition. The estimated value of $\psi_1$ in the first model captures the effect of the abolition of conscription on teenage men compared to men aged 25-29, and $\gamma_3$ in the second model captures the effect of the abolition of conscription on teenage men compared to men in their twenties.

The following tables present the estimated effects of the abolition of compulsory military service in Spain on ex-ante outcomes such as labor force participation, employment status and employment likelihood conditional on being in the labor force. In Tables 7-9, the first panel shows the coefficient estimates from the first regression equation and treats the age group 25-29 as the default category. The second panel shows the coefficient estimates from the second regression equation and treats non-teenagers as the default category. Similar to the previous regressions, I generate three sub-samples according to education levels. The three columns in each table investigate the inclusive samples with 10 years of schooling or less, 8 years of schooling or less, and 6 years of schooling or less, respectively.

Panel 1 in Table 7 shows that among men with ten years of schooling or less, those aged 16-19 are 5.9 percentage points (6.7%) more likely to be in the labor force after the abolition of conscription compared to men aged 25-29.[21] The estimated effect is similar among men with 8 years of schooling or less, and greater among men with 6 years of schooling or less, at 19.7 percentage points (30%). These estimated coefficients are statistically and economically significant. Panel 2 results, which combine both older groups together into a single control group, are very similar, verifying that the results are not driven by strange behavior of one of them.

Table 8 shows the estimated effect of the abolition of CMS on employment status of young men. As expected, the estimates are positive and statistically and economically significant across the six specifications. Panel 1 shows that men aged 16-19 are 6.9 percentage points (11%) more likely to be employed after the abolition of conscription compared to men aged 25-29. Again, the estimated effect in the second sample is similar, and the effect in the sample with 6 years of schooling or less is much greater, at 17 percentage points (40%). Panel 2 results are very similar to those in Panel 1.

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[21] Torun (2014) shows that the outflow of conscription-age men increases teenage men's employment opportunities. Thus, the ex-ante effect may be even larger than the estimates here.

A deeper look at the results shows that the gain in employment of teenage men after the abolition of CMS is greater in magnitude than the gain in labor force participation. Labor force participation can be considered a unilateral decision by individuals themselves, whereas employment depends on hiring decisions of employers. The results suggest that the abolition of conscription also affected the hiring behavior of employers. As I explained in the theoretical model in Section 4.3, employers may be more likely to hire teenage men when they do not worry about an expected interruption. The only exception to this observation is the sample with 6 years of schooling or less. The effect on the employment for this group is slightly smaller than the effect on labor force participation.

Table 9 confirms these observations by focusing on employment conditional on labor force participation. Although the estimates are statistically insignificant, they are consistently positive across the six sample specifications. Panel 1 in Table 9 suggests that a teenage man who is actively searching for a job is 3 percentage points (4.5%) more likely to be hired by an employer after the abolition of CMS compared to men aged 25-29.

### 6.4. The Effect of Abolition of Conscription on Teenage Women

The theoretical model implies that men who expect an interruption in their labor market experience reduce their search effort, and employers who expect their teenage employees to be conscripted reduce their hiring of teenage workers. The previous sub-section provided novel empirical evidence on the ex-ante labor market effects of CMS in Spain. Next, I will point out another interesting consequence. Since teenage women may be close substitute for teenage men in the labor market, teenage women may benefit from CMS because it reduces the labor supply of teenage men. Theoretically, we can even expect effects on women of other ages, but the substitutability between 16-19 year-old men and 16-19 year-old women should be higher than the substitutability between 16-19 men and 25-29 women. Below, I estimate the previous two regression equations for the sample of women in a similar fashion to men and present the estimates in Tables 10-12.[22]

Panel 1 in Table 10 shows that, among the sample with ten years of schooling or less, women aged 16-19 are 6.5 percentage points (7.5%) less likely to be in the labor force after the abolition of conscription compared to women aged 25-29. The estimated effect is similar and statistically significant among women with 8 years of schooling as well. It is negative but loses statistical significance among women with 6 years of schooling or less. There are also smaller negative effects for women aged 20-24, who may also be substitutes in the labor market for men aged 16-19.[23] The estimated coefficient for the effect on teenage women's labor force participation is consistently negative in all of the specifications. Panel 2 results are very similar to those in Panel 1. Notably, these results are similar in magnitude, while opposite in sign, for teenage men.

I present the estimated effect of the abolition of CMS on employment of young women in Table 11. As expected, the estimates are consistently negative across six specifications and statistically significant in four of them. Panel 2 results are very similar to those in Panel 1. Table 12 presents the estimated effect on the employment rate of

[22] Tables 7-9 and tables 10-12 have the same regression equations except that the second group investigates the sample of women.

[23] Torun (2014) shows that the outflow of conscription-age men from labor market has a substitution effect in the labor market. Thus, the estimates here show the effect of the abolition of conscription.

teenage women conditional on being in the labor force. The estimates are statistically insignificant, yet consistently negative.

## 7. Conclusion

In this paper, I investigate an effect of compulsory military service that has not received attention in the literature. I examine the effect of peace-time conscription on early labor market outcomes of potential conscripts *before* they are called up for service. Many papers in the literature estimate the post-service labor market effects of compulsory conscription, with some finding negative effects and some finding no effect on later wages. In a simple theoretical model with costly job search and no job security, I show that an expected interruption in civilian life reduces the incentive of teenagers to search for a job. Moreover, when the firms bear the cost of on-the-job training, such an expected interruption may reduce employers' likelihood of offering a job to expected future conscripts.

Using labor force micro-data from four countries including Turkey, Peru, Argentina and Spain, I make the following contributions to the compulsory military service literature. First, I highlight the anticipation effects of CMS. By showing the ex-ante effects of CMS, I point out one cause of low labor force participation among youth in countries with CMS, a common concern around the world. Second, I show one channel through which conscription may affect future labor market outcomes of individuals. Adverse effects of CMS on the teenage labor market experience may partially explain why the previous literature finds either negative or no effect of conscription, rather than positive effects that some have hypothesized. Third, idleness and poor labor market experience among teenage men caused by expected conscription may explain why the previous studies find a correlation between conscription and later criminal activity. Finally, I point out that CMS affects not only teenage men, but also indirectly affects teenage women. The effect of conscription on labor market outcomes of teenage women has not apparently been investigated in the literature. This also contributes to the literature on substitutability in the labor market showing that teenage men and women are highly substitutable.

Using micro-data from Turkey, Peru and Argentina, I show that, although labor force participation and employment trends are very similar in all three countries among men in their twenties, they are significantly lower in Turkey (which has CMS) among teenage men compared to Peru and Argentina (which do not). Also, using the Active Population Survey from Spain, before and after Spain abolished conscription in 2001, I present within country difference-in-differences evidence that the abolition of CMS increased the labor force participation and employment likelihood of teenage men while decreasing the labor force market opportunities of teenage women.

Thus, unlike the future labor market outcomes of CMS, ex-ante labor market effects are consistently negative and statistically significant across specifications and sample selections. While the decision to switch from a conscription army to a professional one is partly strategic and partly economic, I point out an effect on labor market opportunities of youth.

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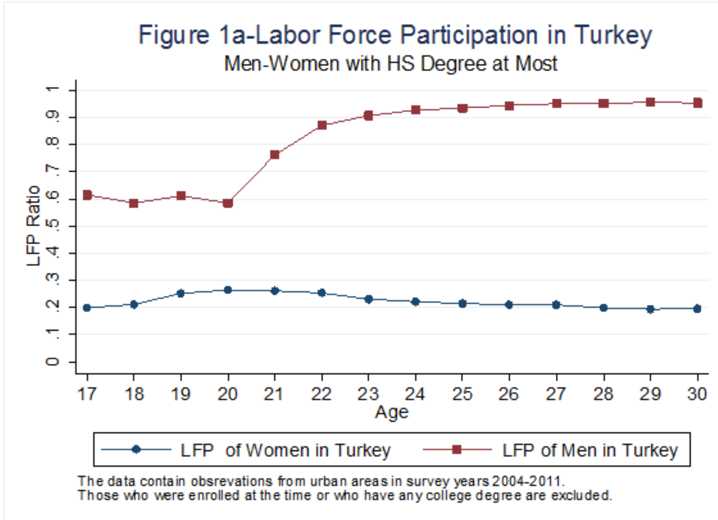


Figure 1a-Labor Force Participation in Turkey
Men-Women with HS Degree at Most
LFP Ratio
0
.1
.2
.3
.4
.5
.6
.7
.8
.9
1
17
18
19
20
21
22
23
24
25
26
27
28
29
30
Age
LFP of Women in Turkey
LFP of Men in Turkey
The data contain obsrevations from urban areas in survey years 2004-2011.
Those who were enrolled at the time or who have any college degree are excluded.

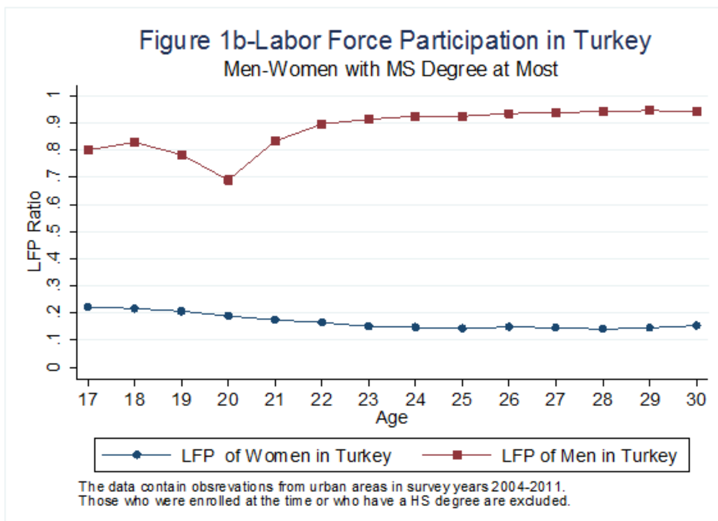



Figure 1b-Labor Force Participation in Turkey
Men-Women with MS Degree at Most
LFP Ratio
0
.1
.2
.3
.4
.5
.6
.7
.8
.9
1
17
18
19
20
21
22
23
24
25
26
27
28
29
30
Age
LFP of Women in Turkey
LFP of Men in Turkey
The data contain obsrevations from urban areas in survey years 2004-2011.
Those who were enrolled at the time or who have a HS degree are excluded.

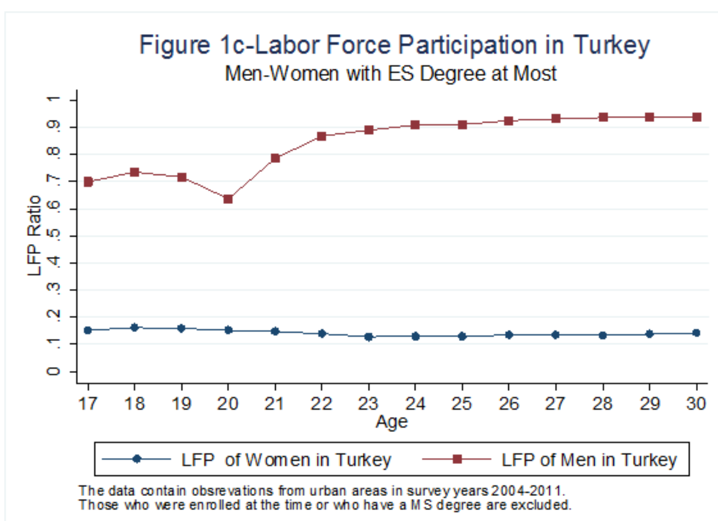



Figure 1c-Labor Force Participation in Turkey
Men-Women with ES Degree at Most
LFP Ratio
0
.1
.2
.3
.4
.5
.6
.7
.8
.9
1
17
18
19
20
21
22
23
24
25
26
27
28
29
30
Age
LFP of Women in Turkey
LFP of Men in Turkey
The data contain obsrevations from urban areas in survey years 2004-2011.
Those who were enrolled at the time or who have a MS degree are excluded.

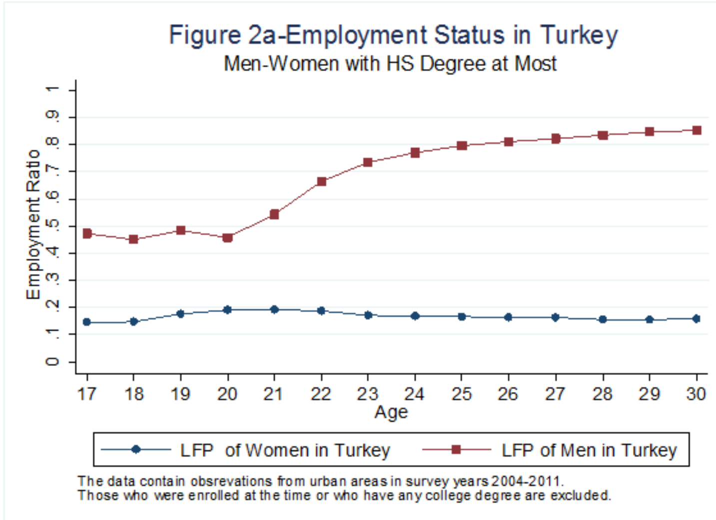


Figure 2a-Employment Status in Turkey
Men-Women with HS Degree at Most
Employment Ratio
0 .1 .2 .3 .4 .5 .6 .7 .8 .9 1
17 18 19 20 21 22 23 24 25 26 27 28 29 30
Age
LFP of Women in Turkey
LFP of Men in Turkey
The data contain obsrevations from urban areas in survey years 2004-2011.
Those who were enrolled at the time or who have any college degree are excluded.

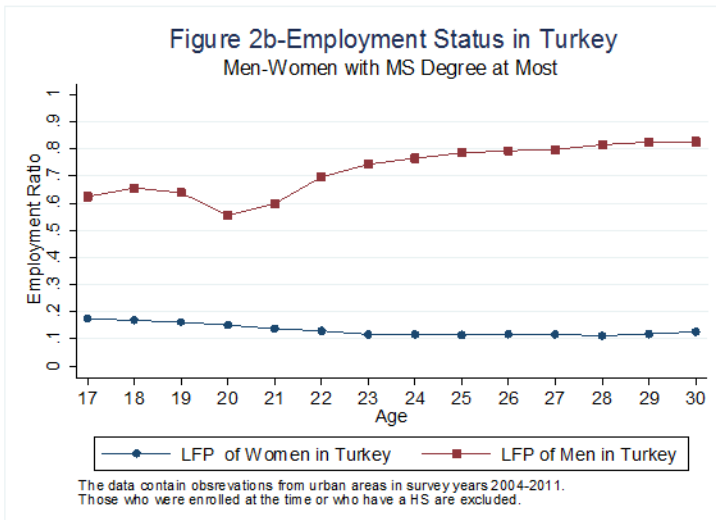


Figure 2b-Employment Status in Turkey
Men-Women with MS Degree at Most
Employment Ratio
0 .1 .2 .3 .4 .5 .6 .7 .8 .9 1
17 18 19 20 21 22 23 24 25 26 27 28 29 30
Age
LFP of Women in Turkey
LFP of Men in Turkey
The data contain obsrevations from urban areas in survey years 2004-2011.
Those who were enrolled at the time or who have a HS are excluded.

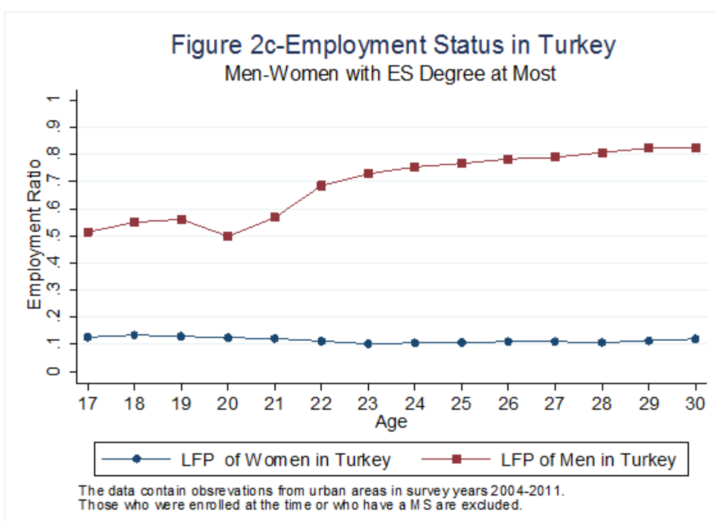


Figure 2c-Employment Status in Turkey
Men-Women with ES Degree at Most
Employment Ratio
0 .1 .2 .3 .4 .5 .6 .7 .8 .9 1
17 18 19 20 21 22 23 24 25 26 27 28 29 30
Age
LFP of Women in Turkey
LFP of Men in Turkey
The data contain obsrevations from urban areas in survey years 2004-2011.
Those who were enrolled at the time or who have a MS are excluded.

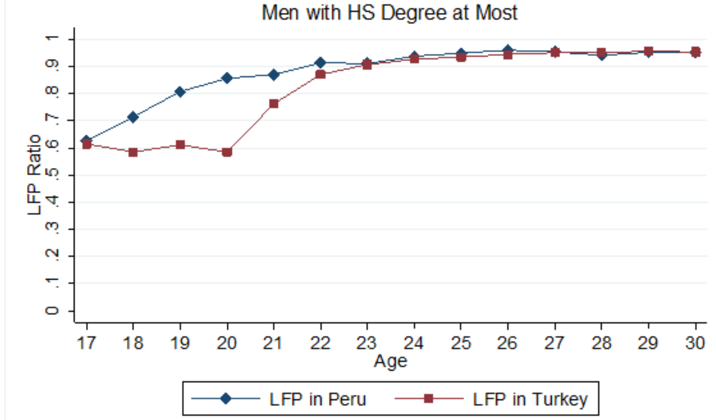



Figure 3a-Labor Force Participation in Turkey and Peru

Men with HS Degree at Most

The data cover survey years 2004-2011 in both countries.
Those who were enrolled at the time or who have any college degree are excluded.

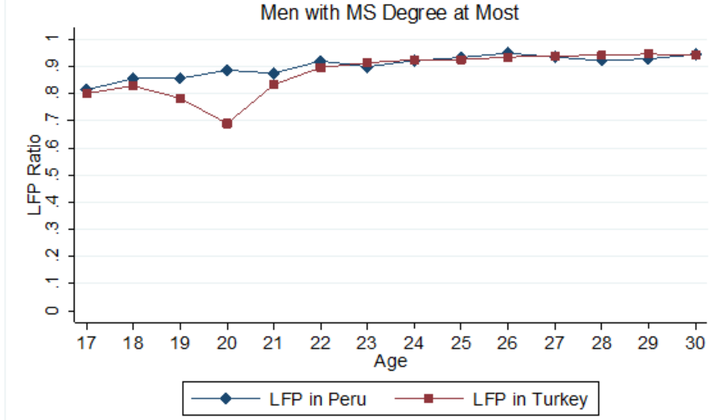



Figure 3b-Labor Force Participation in Turkey and Peru

Men with MS Degree at Most

The data cover survey years 2004-2011 in both countries.
Those who were enrolled at the time or who have HS are excluded.

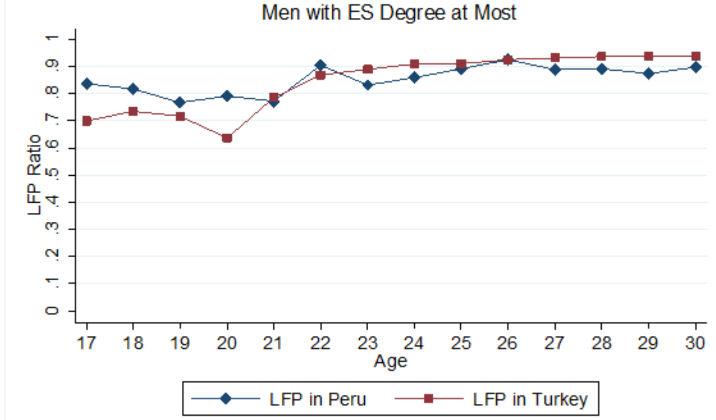



Figure 3c-Labor Force Participation in Turkey and Peru

Men with ES Degree at Most

The data cover survey years 2004-2011 in both countries.
Those who were enrolled at the time or who have MS degree are excluded.

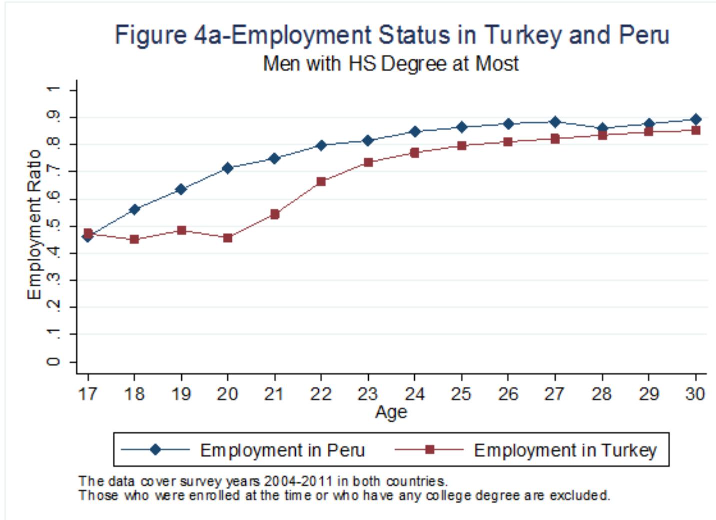


Figure 4a-Employment Status in Turkey and Peru
Men with HS Degree at Most
Employment Ratio
0 .1 .2 .3 .4 .5 .6 .7 .8 .9 1
17 18 19 20 21 22 23 24 25 26 27 28 29 30
Age
Employment in Peru
Employment in Turkey
The data cover survey years 2004-2011 in both countries.
Those who were enrolled at the time or who have any college degree are excluded.

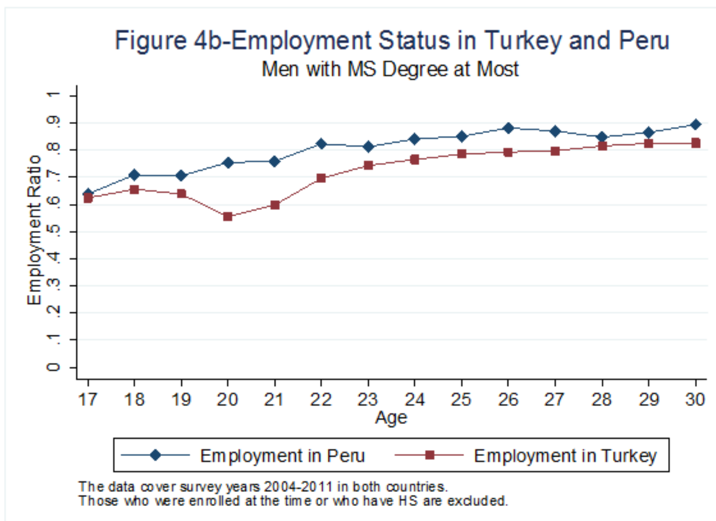



Figure 4b-Employment Status in Turkey and Peru
Men with MS Degree at Most
Employment Ratio
0 .1 .2 .3 .4 .5 .6 .7 .8 .9 1
17 18 19 20 21 22 23 24 25 26 27 28 29 30
Age
Employment in Peru
Employment in Turkey
The data cover survey years 2004-2011 in both countries.
Those who were enrolled at the time or who have HS are excluded.

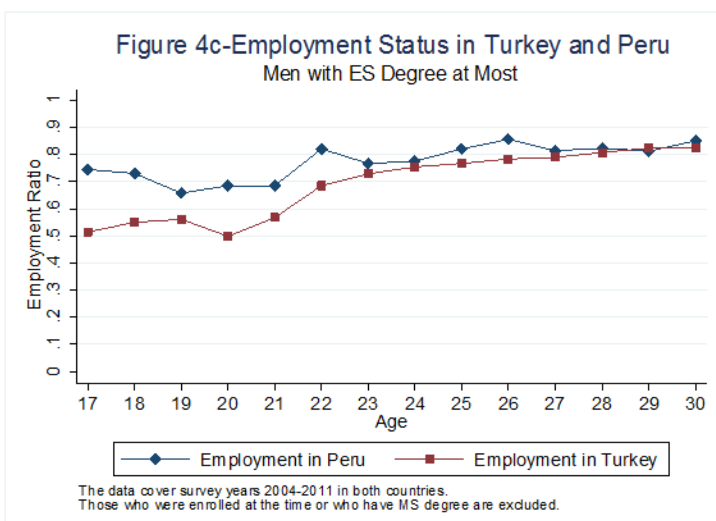
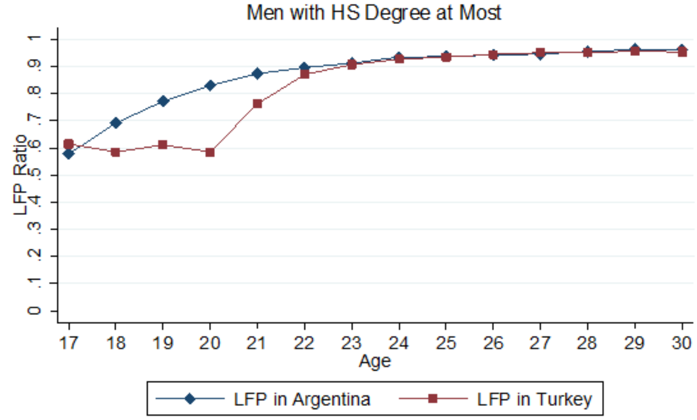



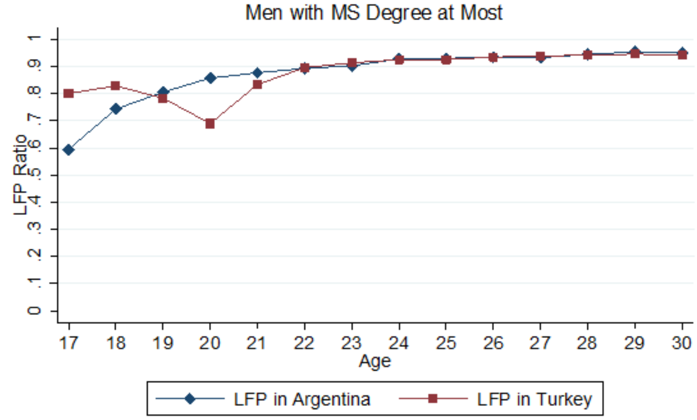
Figure 4c-Employment Status in Turkey and Peru
Men with ES Degree at Most
Employment Ratio
0 .1 .2 .3 .4 .5 .6 .7 .8 .9 1
17 18 19 20 21 22 23 24 25 26 27 28 29 30
Age
Employment in Peru
Employment in Turkey
The data cover survey years 2004-2011 in both countries.
Those who were enrolled at the time or who have MS degree are excluded.

Figure 5a-Labor Force Participation in Turkey and Argentina
Men with HS Degree at Most



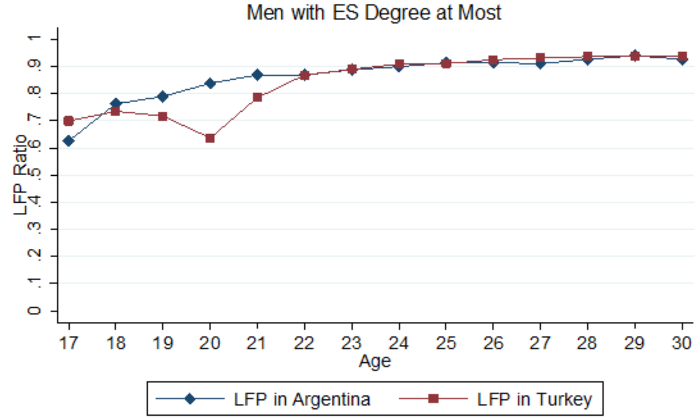

The data cover survey years 2004-2011 in both countries.
Those who were enrolled at the time or who have any college degree are excluded.

Figure 5b-Labor Force Participation in Turkey and Argentina
Men with MS Degree at Most




The data cover survey years 2004-2011 in both countries.
Those who were enrolled at the time or who have HS are excluded.

Figure 5c-Labor Force Participation in Turkey and Argentina
Men with ES Degree at Most




The data cover survey years 2004-2011 in both countries.
Those who were enrolled at the time or who have MS degree are excluded.

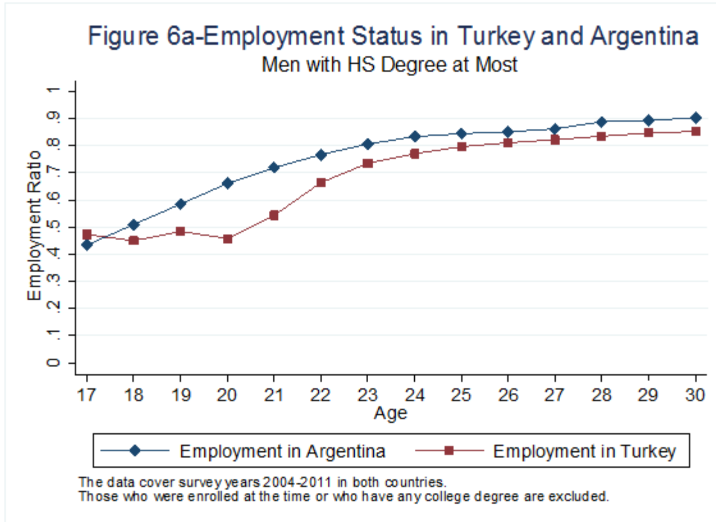


Figure 6a-Employment Status in Turkey and Argentina
Men with HS Degree at Most
Employment Ratio
0
.1
.2
.3
.4
.5
.6
.7
.8
.9
1
17 18 19 20 21 22 23 24 25 26 27 28 29 30
Age
Employment in Argentina
Employment in Turkey
The data cover survey years 2004-2011 in both countries.
Those who were enrolled at the time or who have any college degree are excluded.

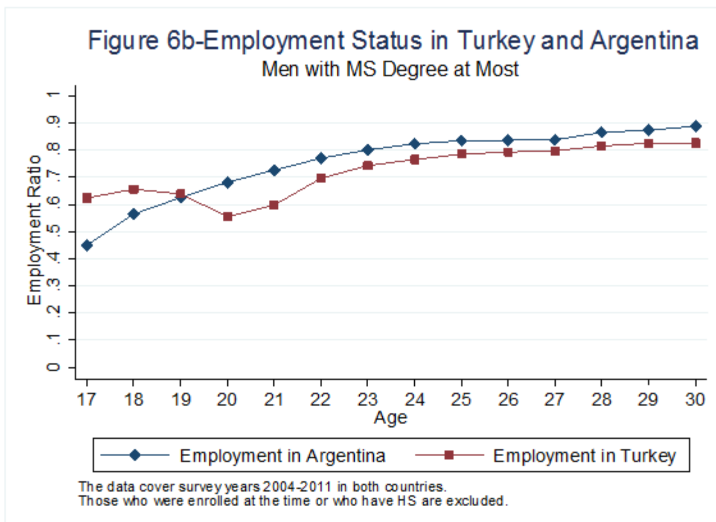


Figure 6b-Employment Status in Turkey and Argentina
Men with MS Degree at Most
Employment Ratio
0
.1
.2
.3
.4
.5
.6
.7
.8
.9
1
17 18 19 20 21 22 23 24 25 26 27 28 29 30
Age
Employment in Argentina
Employment in Turkey
The data cover survey years 2004-2011 in both countries.
Those who were enrolled at the time or who have HS are excluded.

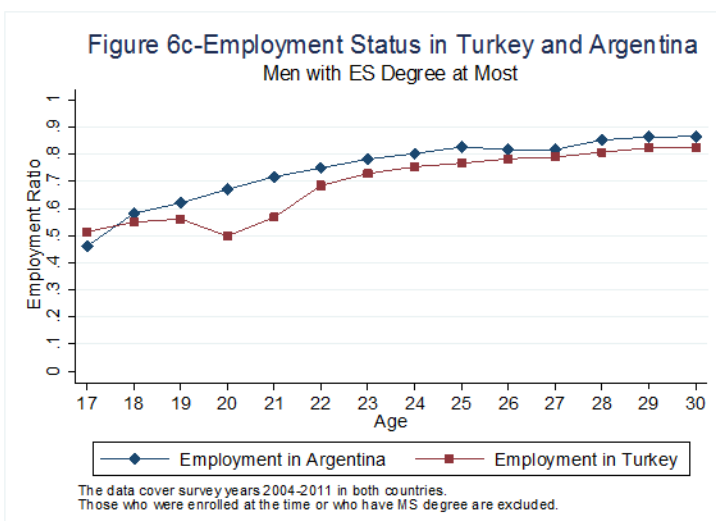


Figure 6c-Employment Status in Turkey and Argentina
Men with ES Degree at Most
Employment Ratio
0
.1
.2
.3
.4
.5
.6
.7
.8
.9
1
17 18 19 20 21 22 23 24 25 26 27 28 29 30
Age
Employment in Argentina
Employment in Turkey
The data cover survey years 2004-2011 in both countries.
Those who were enrolled at the time or who have MS degree are excluded.

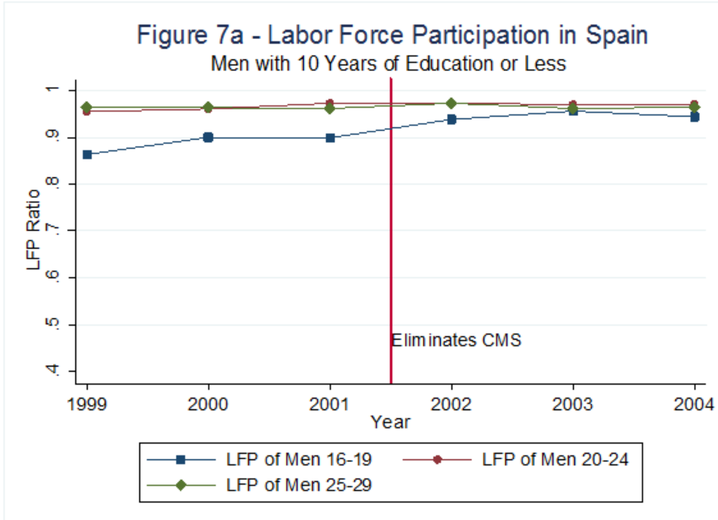


Figure 7a - Labor Force Participation in Spain
Men with 10 Years of Education or Less
LFP Ratio
.4
.5
.6
.7
.8
.9
1
Eliminates CMS
1999
2000
2001
2002
2003
2004
Year
LFP of Men 16-19
LFP of Men 20-24
LFP of Men 25-29

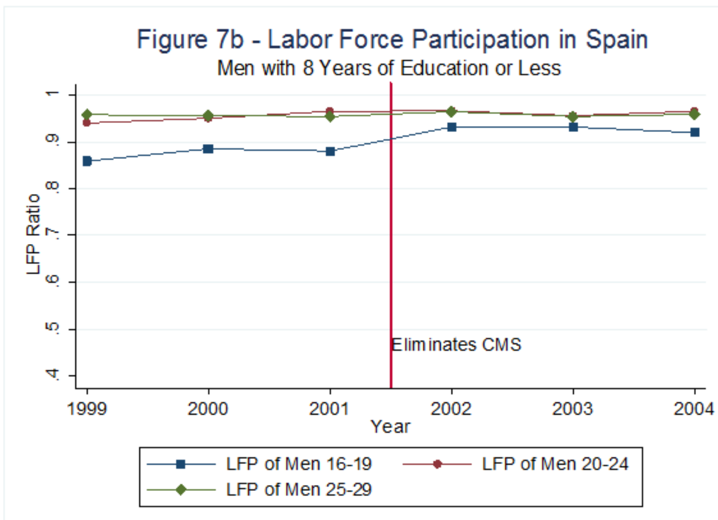



Figure 7b - Labor Force Participation in Spain
Men with 8 Years of Education or Less
LFP Ratio
.4
.5
.6
.7
.8
.9
1
Eliminates CMS
1999
2000
2001
2002
2003
2004
Year
LFP of Men 16-19
LFP of Men 20-24
LFP of Men 25-29

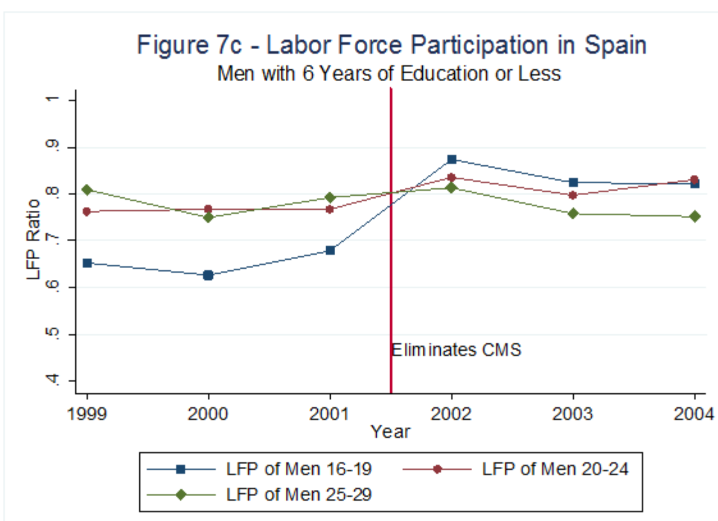



Figure 7c - Labor Force Participation in Spain
Men with 6 Years of Education or Less
LFP Ratio
.4
.5
.6
.7
.8
.9
1
Eliminates CMS
1999
2000
2001
2002
2003
2004
Year
LFP of Men 16-19
LFP of Men 20-24
LFP of Men 25-29

Figure 8a - Employment Status in Spain

Men with 10 Years of Education or Less

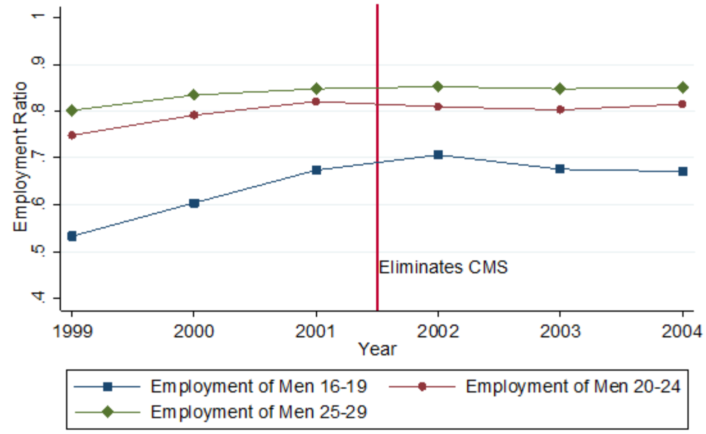



Figure 8b - Employment Status in Spain

Men with 8 Years of Education or Less

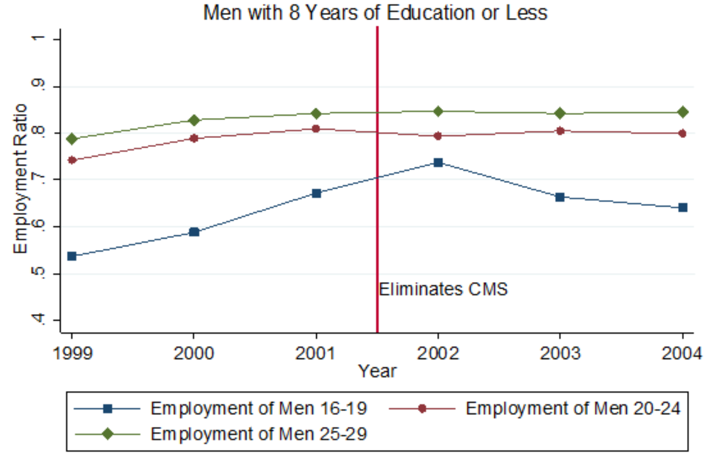
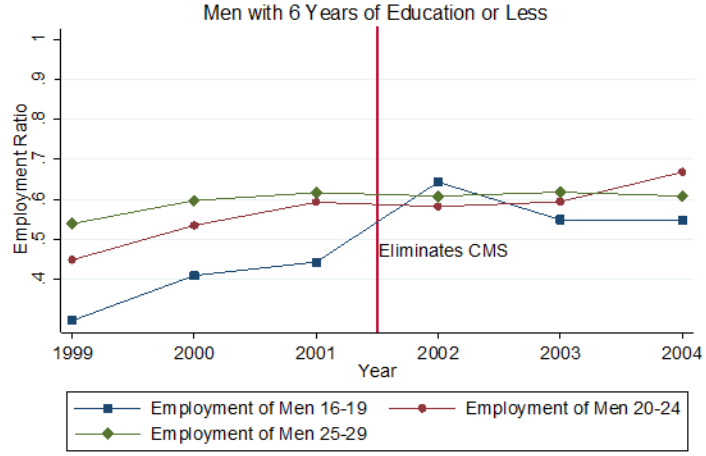



Figure 8c - Employment Status in Spain

Men with 6 Years of Education or Less



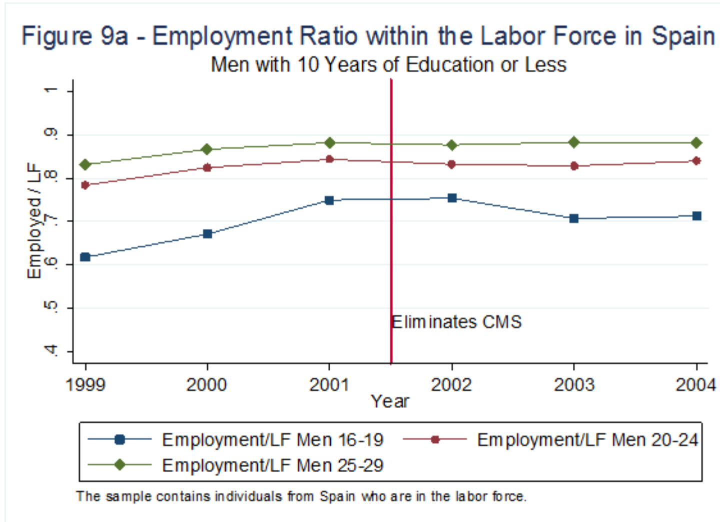


Figure 9a - Employment Ratio within the Labor Force in Spain
Men with 10 Years of Education or Less
Employed / LF
.4
.5
.6
.7
.8
.9
1
Eliminates CMS
1999
2000
2001
2002
2003
2004
Year
Employment/LF Men 16-19
Employment/LF Men 20-24
Employment/LF Men 25-29
The sample contains individuals from Spain who are in the labor force.

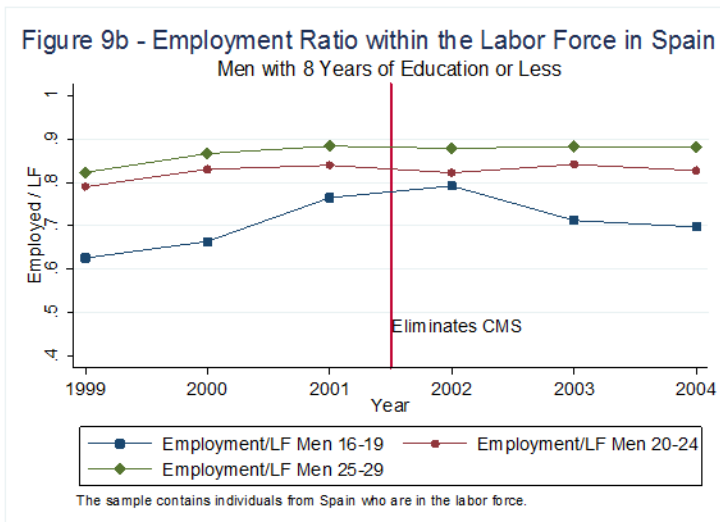


Figure 9b - Employment Ratio within the Labor Force in Spain
Men with 8 Years of Education or Less
Employed / LF
.4
.5
.6
.7
.8
.9
1
Eliminates CMS
1999
2000
2001
2002
2003
2004
Year
Employment/LF Men 16-19
Employment/LF Men 20-24
Employment/LF Men 25-29
The sample contains individuals from Spain who are in the labor force.

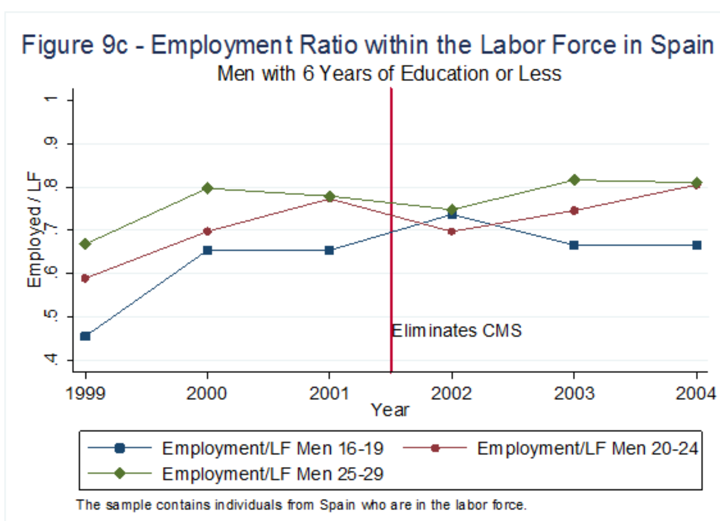


Figure 9c - Employment Ratio within the Labor Force in Spain
Men with 6 Years of Education or Less
Employed / LF
.4
.5
.6
.7
.8
.9
1
Eliminates CMS
1999
2000
2001
2002
2003
2004
Year
Employment/LF Men 16-19
Employment/LF Men 20-24
Employment/LF Men 25-29
The sample contains individuals from Spain who are in the labor force.

**Table 1 - The Distribution of Search Cost, $z$, and the Search Behavior of Civilian Individuals in Each Period (Given Wage Stucture I)**

| | | $w_1$ | $w_2$ | $w_3$ | $\frac{w_1}{1-\beta+\beta\eta}$ | $w_{2}+\beta w_3$ | $\frac{w_2}{1-\beta}$ Search Cost $z$ |
|---|---|---|---|---|---|---|---|
| A jobless civilian individual in period 1 | Search | Search | Search | Search | Not Search | Not Search | Not Search |
| A jobless civilian individual in period 2 | Search | Search | Search | Search | Search | Not Search | Not Search |
| A jobless civilian individual in period 3 | Search | Search | Search | Not search | Not Search | Not Search | Not Search |

Note: Each row shows the labor force situation of a civilian individual with a search cost, z, value shown at the distribution line. Civilian individuals in the second period of their life are those who are not called up for service. The threshold value for search decision in the first period is $w_1/(1-\beta+\beta\eta)$. Wages are exogenously given, and individuals make their search decisions according to their z values.

**Table 2 - The Distribution of Search Cost, $z$, and the Search Behavior of Civilian Individuals in Each Period (Given Wage Structure II)**

| | $w_1$ | $w_2$ | $\frac{w_1}{1-\beta+\beta\eta}$ | $w_3$ | $w_{2+}\beta w_3$ | $\frac{w_2}{1-\beta}$ | *Search Cost* $z$ |
|---|---|---|---|---|---|---|---|
| A jobless civilian individual in period 1 | Search | Search | Search | Not Search | Not Search | Not Search | Not Search |
| A jobless civilian individual in period 2 | Search | Search | Search | Search | Search | Not Search | Not Search |
| A jobless civilian individual in period 3 | Search | Search | Search | Search | Not Search | Not Search | Not Search |

Note: Each row shows the labor force situation of a civilian individual with a search cost, z, value shown at the distribution line. Civilian individuals in the second period of their life are those who are not called up for service. The threshold value for search decision in the first period is w1/(1-β+βη). Wages are exogenously given, and individuals make their search decisions according to their z values.

**Table 3 - The Effect of Being Younger than Twenty on Labor Force Participation of Men**

| | Men Aged 17-30 | | | Men Aged 17-19 & 22-30 | | |
|---|---|---|---|---|---|---|
| | HS at Most | MS at Most | ES at Most | HS at Most | MS at Most | ES at Most |
| | *The Sample from Turkey* | | | | | |
| Younger than 20 | -0.195*** | -0.116*** | -0.105*** | -0.235*** | -0.086*** | -0.080*** |
| | (0.012) | (0.010) | (0.014) | (0.023) | (0.008) | (0.018) |
| Number of Obs. | 189,150 | 110,117 | 61,470 | 173,149 | 102,400 | 58,327 |
| | *The Sample from Peru* | | | | | |
| Younger than 20 | 0.045** | 0.014 | -0.036 | 0.052** | -0.013 | -0.132 |
| | (0.015) | (0.030) | (0.044) | (0.020) | (0.042) | (0.094) |
| Number of Obs. | 18,204 | 6,140 | 2,016 | 15,395 | 5,284 | 1,765 |
| | *The Sample from Argentina* | | | | | |
| Younger than 20 | 0.002 | 0.040** | 0.026 | -0.002 | 0.065** | 0.056* |
| | (0.013) | (0.016) | (0.022) | (0.015) | (0.025) | (0.024) |
| Number of Obs. | 47,558 | 29,256 | 13,903 | 40,601 | 24,917 | 12,149 |

*Note*: Each cell shows the estimated coefficient for the indicator of being 20 years old or younger. The dependent variable is a dummy variable that takes one if individual is in the labor force. The educational categories correspond to 11 years or less, 8-9 years or less, and 5-6 years or less, respectively. Each of the nine sub-samples excludes women, rural population, those who were enrolled in school when surveyed. All estimations control for survey year fixed effects and quadratic age trends. The first three columns contain men aged 17-30, and the last three columns drop men at the age of 20 or 21 from the samples. Robust standard errors are shown in parentheses, clustered at the survey year level.

**Table 4 - The Effect of Being Younger than Twenty on Employment Likelihood of Men**

| | Men Aged 17-30 | | | Men Aged 17-19 & 22-30 | | |
|---|---|---|---|---|---|---|
| | HS at Most | MS at Most | ES at Most | HS at Most | MS at Most | ES at Most |
| | | | *The Sample from Turkey* | | | |
| Younger than 20 | -0.085*** | -0.009 | -0.031* | -0.119*** | 0.004 | -0.030 |
| | (0.011) | (0.010) | (0.015) | (0.021) | (0.011) | (0.028) |
| Number of Obs. | 189,150 | 110,117 | 61,470 | 173,149 | 102,400 | 58,327 |
| | | | *The Sample from Peru* | | | |
| Younger than 20 | 0.019 | 0.004 | -0.066 | 0.006 | -0.035 | -0.179* |
| | (0.016) | (0.031) | (0.056) | (0.023) | (0.052) | (0.086) |
| Number of Obs. | 18,204 | 6,140 | 2,016 | 15,395 | 5,284 | 1,765 |
| | | | *The Sample from Argentina* | | | |
| Younger than 20 | -0.024 | -0.005 | -0.004 | -0.067** | -0.018 | -0.002 |
| | (0.014) | (0.019) | (0.022) | (0.020) | (0.024) | (0.025) |
| Number of Obs. | 47,558 | 29,256 | 13,903 | 40,601 | 24,917 | 12,149 |

*Note*: Each cell shows the estimated coefficient for the indicator of being 20 years old or younger. The dependent variable is a dummy variable that takes one if individual is employed. The educational categories correspond to 11 years or less, 8-9 years or less and 5-6 years or less respectively. Each of the nine sub-samples excludes women, rural population, those who were enrolled in school when surveyed. All estimations control for survey year fixed effects and quadratic age trends. The first three columns contain men aged 17-30, and the last three columns drop men at the age of 20 or 21 from the samples. Robust standard errors are shown in parentheses, clustered at the survey year level.

**Table 5 - The Effect of CMS on Labor Force Participation of Teenage Men - Pooled Sample**

| | Men Aged 17-30 | | | Men Aged 17-19 & 22-30 | | |
|---|---|---|---|---|---|---|
| | HS at Most | MS at Most | ES at Most | HS at Most | MS at Most | ES at Most |
| *Panel 1 - Turkey vs. Peru (Peru is the default country)* | | | | | | |
| Younger than 20*Turkey | -0.130*** | -0.063*** | -0.144*** | -0.106*** | -0.037** | -0.131*** |
| | (0.008) | (0.009) | (0.019) | (0.011) | (0.011) | (0.024) |
| Younger than 20 | -0.050** | -0.048*** | 0.034 | -0.110*** | -0.047*** | 0.042 |
| | (0.016) | (0.009) | (0.022) | (0.029) | (0.011) | (0.028) |
| Turkey | -0.008 | 0.048** | 0.061 | 0.025 | 0.121*** | 0.157 |
| | (0.015) | (0.019) | (0.104) | (0.025) | (0.021) | (0.109) |
| Number of Obs. | 207,354 | 116,257 | 63,486 | 188,544 | 107,684 | 60,092 |
| *Panel 2 - Turkey vs. Argentina (Argentina is the default country)* | | | | | | |
| Younger than 20*Turkey | -0.119*** | 0.030*** | -0.063*** | -0.090*** | 0.079*** | -0.025 |
| | (0.017) | (0.009) | (0.015) | (0.024) | (0.014) | (0.014) |
| Younger than 20 | -0.058** | -0.094*** | -0.022 | -0.119** | -0.117*** | -0.029 |
| | (0.020) | (0.008) | (0.017) | (0.034) | (0.012) | (0.018) |
| Turkey | -0.004 | 0.018 | -0.177** | -0.124 | -0.069 | -0.252*** |
| | (0.042) | (0.047) | (0.061) | (0.069) | (0.056) | (0.071) |
| Number of Obs. | 236,708 | 139,373 | 75,373 | 213,750 | 127,317 | 70,476 |

*Note:* Each column presents results from a separate Linear Probability Model regression. The dependent variable is an indicator for being in the labor force. The regression model compares the teenage labor force participation in Turkey vs. Peru in Panel 1, and in Turkey vs. Argentina in Panel 2. The ages are grouped into two categories; men younger than 20, and men older than 21. Men older than 21 is the default category. The variable of interest is theYounger than 20*Turkey. The sample in the separate columns contain individuals with HS degree or less, MS degree or less, and ES degree or less, respectively. Each sample excludes women, rural population, and those who were enrolled in school when surveyed. All estimations control for survey year fixed effects and quadratic age trends. All estimations control for GDP per capita, overall labor force participation, male labor force participation, overall unemployment, and male unemployment rate of the countries. The first three columns contain men aged 17-30, and the last three columns drop men at the age of 20 or 21 from the samples. Robust standard errors are shown in parentheses, clustered at the survey year level.

**Table 6 - The Effect of CMS on Employment Likelihood of Teenage Men – Pooled Sample**

| | Men Aged 17-30 | | | Men Aged 17-19 & 22-30 | | |
|---|---|---|---|---|---|---|
| | HS at Most | MS at Most | ES at Most | HS at Most | MS at Most | ES at Most |
| *Panel 1 - Turkey vs. Peru (Peru is the default country)* | | | | | | |
| Younger than 20*Turkey | -0.044*** | 0.007 | -0.136*** | -0.022* | 0.027 | -0.136*** |
| | (0.009) | (0.018) | (0.031) | (0.009) | (0.017) | (0.033) |
| Younger than 20 | -0.033** | -0.014 | 0.094** | -0.086*** | -0.023* | 0.093** |
| | (0.010) | (0.011) | (0.030) | (0.020) | (0.011) | (0.038) |
| Turkey | 0.066* | 0.146** | 0.118* | 0.105** | 0.227*** | 0.193* |
| | (0.030) | (0.059) | (0.055) | (0.032) | (0.056) | (0.086) |
| Number of Obs. | 207,354 | 116,257 | 63,486 | 188,544 | 107,684 | 60,092 |
| *Panel 2 - Turkey vs. Argentina (Argentina is the default country)* | | | | | | |
| Younger than 20*Turkey | -0.022 | 0.103*** | -0.004 | 0.004 | 0.139*** | 0.026 |
| | (0.013) | (0.007) | (0.025) | (0.015) | (0.007) | (0.027) |
| Younger than 20 | -0.055*** | -0.082*** | -0.020 | -0.113*** | -0.111*** | -0.043 |
| | (0.015) | (0.009) | (0.019) | (0.025) | (0.011) | (0.025) |
| Turkey | 0.277** | 0.190** | 0.101 | 0.134 | 0.032 | -0.036 |
| | (0.086) | (0.080) | (0.102) | (0.135) | (0.101) | (0.105) |
| Number of Obs. | 236,708 | 139,373 | 75,373 | 213,750 | 127,317 | 70,476 |

*Note:* Each column presents results from a separate Linear Probability Model regression. The dependent variable is an indicator for being employed. The regression model compares the teenage labor force participation in Turkey vs. Peru in Panel 1, and in Turkey vs. Argentina in Panel 2. The ages are grouped into two categories; men younger than 20, and men older than 21. Men older than 21 is the default category. The variable of interest is the Younger than 20*Turkey. The sample in the separate columns contain individuals with HS degree or less, MS degree or less, and ES degree or less, respectively. Each sample excludes women, rural population, and those who were enrolled in school when surveyed. All estimations control for survey year fixed effects and quadratic age trends. All estimations control for GDP per capita, overall labor force participation, male labor force participation, overall unemployment, and male unemployment rate of the countries. The first three columns contain men aged 17-30, and the last three columns drop men at the age of 20 or 21 from the samples. Robust standard errors are shown in parentheses, clustered at the survey year level.

**Table 7 - The Effect of Abolition of CMS on Labor Force Participation of Teenage Men in Spain**

| | 10 Years of Schooling or Less | 8 Years of Schooling or Less | 6 Years of Schooling or Less |
|---|---|---|---|
| | *Panel 1* | | |
| Age group 16-19*Abolition | 0.059** | 0.052*** | 0.197*** |
| | (0.015) | (0.009) | (0.017) |
| Age group 20-24*Abolition | 0.007 | 0.010 | 0.067** |
| | (0.006) | (0.007) | (0.025) |
| Age group 16-19 | -0.079*** | -0.084*** | -0.126*** |
| | (0.012) | (0.008) | (0.017) |
| Age group 20-24 | -0.001 | -0.006 | -0.019 |
| | (0.006) | (0.007) | (0.016) |
| Abolition | -0.001 | 0.022*** | 0.275*** |
| | (0.005) | (0.004) | (0.008) |
| Number of Observations | 37,862 | 22,818 | 2,787 |
| | *Panel 2* | | |
| Teenager*Abolition | 0.055*** | 0.048*** | 0.167*** |
| | (0.012) | (0.008) | (0.019) |
| Teenager | -0.078*** | -0.081*** | -0.117*** |
| | (0.010) | (0.006) | (0.014) |
| Abolition | 0.002 | 0.026*** | 0.291*** |
| | (0.002) | (0.001) | (0.010) |
| Number of Observations | 37,862 | 22,818 | 2,787 |

*Note*: Each column presents results from a separate Linear Probability Model regression. The dependent variable is an indicator for being in the labor force. Panel 1 groups the ages into three categories, 16-19, 20-24, 25-29. Age group 25-29 is the default category. The variable of interest is the Age group 16-19*Abolition. Panel 2 groups the ages into two categories; teenagers and non-teenagers. Non-teenagers are the default category. The variable of interest is the Teenager*Abolition. The sample in the separate columns contain individuals with 10 years of schooling or less, 8 years of schooling or less, and 6 years of schooling or less, respectively. All estimations control for 18 survey area fixed effects and the survey year fixed effects. All estimations also control for GDP per capita, male labor force participation, and male unemployment rate of the countries. Robust standard errors are shown in the parentheses, clustered at the survey year level.

**Table 8 - The Effect of Abolition of CMS on Employment Likelihood of Teenage Men in Spain**

| | 10 Years of Schooling or Less | 8 Years of Schooling or Less | 6 Years of Schooling or Less |
|---|---|---|---|
| | *Panel 1* | | |
| Age group 16-19*Abolition | 0.067* | 0.074* | 0.169*** |
| | (0.026) | (0.032) | (0.041) |
| Age group 20-24*Abolition | 0.001 | -0.004 | 0.068 |
| | (0.008) | (0.007) | (0.034) |
| Age group 16-19 | -0.226*** | -0.221*** | -0.196*** |
| | (0.025) | (0.021) | (0.024) |
| Age group 20-24 | -0.041*** | -0.038*** | -0.061** |
| | (0.007) | (0.004) | (0.020) |
| Abolition | -0.014 | -0.036*** | 0.011 |
| | (0.007) | (0.005) | (0.012) |
| Number of Observations | 37,862 | 22,818 | 2,787 |
| | *Panel 2* | | |
| Teenager*Abolition | 0.066** | 0.075* | 0.140** |
| | (0.023) | (0.032) | (0.043) |
| Teenager | -0.207*** | -0.204*** | -0.169*** |
| | (0.021) | (0.019) | (0.015) |
| Abolition | -0.011** | -0.037*** | 0.038** |
| | (0.004) | (0.004) | (0.012) |
| Number of Observations | 37,862 | 22,818 | 2,787 |

*Note:* Each column presents results from a separate Linear Probability Model regression. The dependent variable is an indicator for being employed. Panel 1 groups the ages into three categories, 16-19, 20-24, 25-29. Age group 25-29 is the default category. The variable of interest is the Age group 16-19*Abolition. Panel 2 groups the ages into two categories; teenagers and non-teenagers. Non-teenagers are the default category. The variable of interest is the Teenager*Abolition. The sample in the separate columns contain individuals with 10 years of schooling or less, 8 years of schooling or less, and 6 years of schooling or less, respectively. All estimations control for 18 survey area fixed effects and the survey year fixed effects. All estimations also control for GDP per capita, male labor force participation, and male unemployment rate of the countries. Robust standard errors are shown in the parentheses, clustered at the survey year level.

**Table 9 - The Effect of Abolition of CMS on Employment of Teenage Men In Spain Conditional on LF Participation**

| | 10 Years of Schooling or Less | 8 Years of Schooling or Less | 6 Years of Schooling or Less |
|---|---|---|---|
| | | *Panel 1* | |
| Age group 16-19*Abolition | 0.031 | 0.044 | 0.057 |
| | (0.026) | (0.035) | (0.046) |
| Age group 20-24*Abolition | -0.005 | -0.013* | 0.025 |
| | (0.004) | (0.005) | (0.029) |
| Age group 16-19 | -0.181*** | -0.172*** | -0.158*** |
| | (0.021) | (0.021) | (0.020) |
| Age group 20-24 | -0.041*** | -0.034*** | -0.066** |
| | (0.003) | (0.002) | (0.023) |
| Abolition | -0.019** | -0.060*** | -0.268*** |
| | (0.006) | (0.004) | (0.016) |
| Number of Observations | 36,162 | 21,598 | 2,162 |
| | | *Panel 2* | |
| Teenager*Abolition | 0.033 | 0.049 | 0.047 |
| | (0.024) | (0.036) | (0.043) |
| Teenager | -0.161*** | -0.156*** | -0.130*** |
| | (0.019) | (0.022) | (0.016) |
| Abolition | -0.019*** | -0.064*** | -0.245*** |
| | (0.004) | (0.005) | (0.017) |
| Number of Observations | 36,162 | 21,598 | 2,162 |

*Note:* Each column presents results from a separate Linear Probability Model regression. The dependent variable is an indicator for being employed. Panel 1 groups the ages into three categories, 16-19, 20-24, 25-29. Age group 25-29 is the default category. The variable of interest is the Age group 16-19*Abolition. Panel 2 groups the ages into two categories; teenagers and non-teenagers. Non-teenagers are the default category. The variable of interest is the Teenager*Abolition. The sample in the separate columns contain individuals with 10 years of schooling or less, 8 years of schooling or less, and 6 years of schooling or less, respectively. All samples exclude those who are not in the labor force. All estimations control for 18 survey area fixed effects and the survey year. All estimations also control for GDP per capita, male labor force participation, and male unemployment rate of the countries. Robust standard errors are shown in the parentheses, clustered at the survey year level.

**Table 10-The Effect of Abolition of CMS on**
**Labor Force Participation of Teenage Women in Spain**

| | 10 Years of Schooling or Less | 8 Years of Schooling or Less | 6 Years of Schooling or Less |
|---|---|---|---|
| | *Panel 1* | | |
| Age group 16-19*Abolition | -0.065*** | -0.080** | -0.047 |
| | (0.016) | (0.024) | (0.036) |
| Age group 20-24*Abolition | -0.032*** | -0.035*** | -0.033 |
| | (0.005) | (0.008) | (0.048) |
| Age group 16-19 | 0.112*** | 0.127*** | 0.120** |
| | (0.014) | (0.003) | (0.034) |
| Age group 20-24 | 0.131*** | 0.140*** | 0.111*** |
| | (0.004) | (0.007) | (0.023) |
| Abolition | -0.003 | 0.016** | -0.258*** |
| | (0.004) | (0.006) | (0.027) |
| Number of Observations | 24,855 | 15,226 | 1,920 |
| | *Panel 2* | | |
| Teenager*Abolition | -0.051** | -0.065** | -0.032 |
| | (0.015) | (0.023) | (0.028) |
| Teenager | 0.056*** | 0.069*** | 0.075** |
| | (0.013) | (0.002) | (0.026) |
| Abolition | -0.012*** | 0.012*** | -0.265*** |
| | (0.003) | (0.003) | (0.013) |
| Number of Observations | 24,855 | 15,226 | 1,920 |

*Note*: Each column presents results from a separate Linear Probability Model regression for the sample of women. The dependent variable is an indicator for being in the labor force. Panel 1 groups the ages into three categories, 16-19, 20-24, 25-29. Age group 25-29 is the default category. The variable of interest is the Age group 16-19*Abolition. Panel 2 groups the ages into two categories; teenagers and non-teenagers. Non-teenagers are the default category. The variable of interest is the Teenager*Abolition. The sample in the separate columns contain individuals with 10 years of schooling or less, 8 years of schooling or less, and 6 years of schooling or less, respectively. All estimations control for 18 survey area fixed effects and the survey year fixed effects. All estimations also control for GDP per capita, female labor force participation, and female unemployment rate of the countries. Robust standard errors are shown in the parentheses, clustered at the survey year level.

**Table 11 - The Effect of Abolition of CMS on Employment Likelihood of Teenage Women in Spain**

| | 10 Years of Schooling or Less | 8 Years of Schooling or Less | 6 Years of Schooling or Less |
|---|---|---|---|
| | *Panel 1* | | |
| Age group 16-19*Abolition | -0.061** | -0.068* | -0.082 |
| | (0.018) | (0.028) | (0.063) |
| Age group 20-24*Abolition | -0.028** | -0.033* | -0.074 |
| | (0.007) | (0.017) | (0.059) |
| Age group 16-19 | -0.034* | 0.006 | 0.030 |
| | (0.014) | (0.022) | (0.062) |
| Age group 20-24 | 0.089*** | 0.108*** | 0.084* |
| | (0.005) | (0.012) | (0.042) |
| Abolition | 0.027*** | 0.028** | -0.072* |
| | (0.005) | (0.009) | (0.029) |
| Number of Observations | 24,855 | 15,226 | 1,920 |
| | *Panel 2* | | |
| Teenager*Abolition | -0.049** | -0.054* | -0.052 |
| | (0.015) | (0.023) | (0.052) |
| Teenager | -0.073*** | -0.039* | -0.004 |
| | (0.012) | (0.017) | (0.047) |
| Abolition | 0.017*** | 0.022*** | -0.100*** |
| | (0.003) | (0.003) | (0.013) |
| Number of Observations | 24,855 | 15,226 | 1,920 |

*Note*: Each column presents results from a separate Linear Probability Model regression for the sample of women. The dependent variable is an indicator for being employed in Table 10. Panel 1 groups the ages into three categories, 16-19, 20-24, 25-29. Age group 25-29 is the default category. The variable of interest is the Age group 16-19*Abolition. Panel 2 groups the ages into two categories; teenagers and non-teenagers. Non-teenagers are the default category. The variable of interest is the Teenager*Abolition. The sample in the separate columns contain individuals with 10 years of schooling or less, 8 years of schooling or less, and 6 years of schooling or less, respectively. All estimations control for 18 survey area fixed effects and the survey year fixed effects. All estimations also control for GDP per capita, female labor force participation, and female unemployment rate of the countries. Robust standard errors are shown in the parentheses, clustered at the survey year level.

**Table 12 - The Effect of Abolition of CMS on Likelihood of Teenage Women in Spain Conditional on LF Participation**

| | 10 Years of Schooling or Less | 8 Years of Schooling or Less | 6 Years of Schooling or Less |
|---|---|---|---|
| | | *Panel 1* | |
| Age group 16-19*Abolition | -0.028 | -0.026 | -0.097 |
| | (0.020) | (0.034) | (0.102) |
| Age group 20-24*Abolition | -0.013 | -0.019 | -0.092 |
| | (0.009) | (0.016) | (0.077) |
| Age group 16-19 | -0.128*** | -0.091** | -0.068 |
| | (0.014) | (0.026) | (0.099) |
| Age group 20-24 | 0.004 | 0.021 | 0.044 |
| | (0.006) | (0.012) | (0.064) |
| Abolition | 0.032*** | 0.020* | 0.138* |
| | (0.006) | (0.009) | (0.059) |
| Number of Observations | 20,264 | 12,043 | 1,088 |
| | | *Panel 2* | |
| Teenager*Abolition | -0.021 | -0.018 | -0.057 |
| | (0.016) | (0.031) | (0.078) |
| Teenager | -0.130*** | -0.101*** | -0.088 |
| | (0.011) | (0.020) | (0.071) |
| Abolition | 0.025*** | 0.012** | 0.101** |
| | (0.002) | (0.004) | (0.032) |
| Number of Observations | 20,264 | 12,043 | 1,088 |

*Note:* Each column presents results from a separate Linear Probability Model regression for the sample of women. The dependent variable is an indicator for being employed. Panel 1 groups the ages into three categories, 16-19, 20-24, 25-29. Age group 25-29 is the default category. The variable of interest is the Age group 16-19*Abolition. Panel 2 groups the ages into two categories; teenagers and non-teenagers. Non-teenagers are the default category. The variable of interest is the Teenager*Abolition. The sample in the separate columns contain individuals with 10 years of schooling or less, 8 years of schooling or less, and 6 years of schooling or less, respectively. The sample in Table 11 excludes those who are not in the labor force. All estimations control for 18 survey area fixed effects and the survey year fixed effects. All estimations also control for GDP per capita, female labor force participation, and female unemployment rate of the countries. Robust standard errors are shown in the parentheses, clustered at the survey year level.

## Appendix Figures and Tables

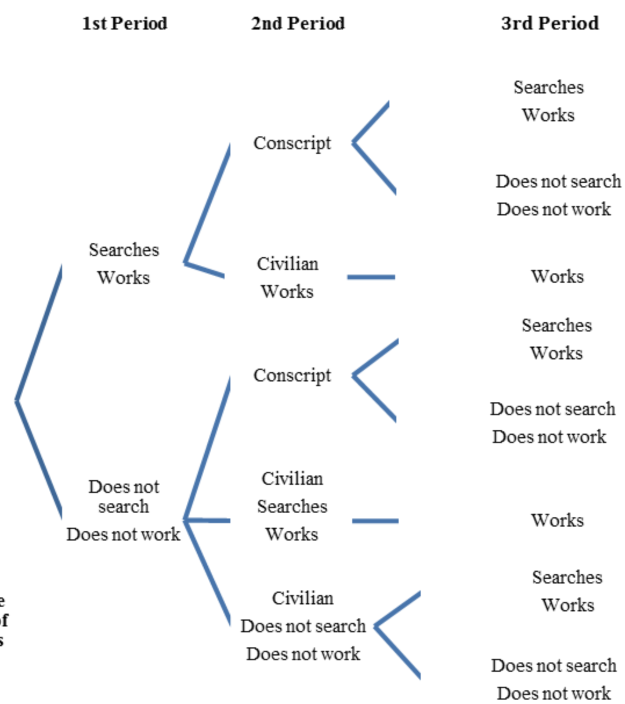



**Figure A1**
**The Choice Structure of Individuals**

**Table A1 - Descriptive Statistics for the Sample of Individuals between 17 and 30 (Turkey)**

| | Women | | Men | |
|---|---|---|---|---|
| | **Mean** | **S.D.** | **Mean** | **S.D.** |
| **Age** | 23.966 | 3.980 | 24.122 | 4.024 |
| **% Less than Middle School** | 0.517 | 0.500 | 0.325 | 0.468 |
| **% Middle School Only** | 0.174 | 0.379 | 0.257 | 0.437 |
| **% High School Only** | 0.309 | 0.462 | 0.418 | 0.493 |
| **% in Labor Force** | 0.221 | 0.415 | 0.848 | 0.359 |
| **% Employed** | 0.167 | 0.373 | 0.707 | 0.455 |
| **Number of Observations** | 229873 | | 189150 | |

*Note:* The sample contains individuals aged 17-30 from all survey years 2004-2011. The sample excludes the rural population, those who were enrolled in school when surveyed, and those who have college degree. Degree attainments are defined in an exclusive way.

**Table A2 - Descriptive Statistics for the Sample of Individuals between 17 and 30 (Peru)**

| | Women | | Men | |
|---|---|---|---|---|
| | **Mean** | **S.D.** | **Mean** | **S.D.** |
| **Age** | 23.114 | 4.121 | 22.914 | 4.073 |
| **% Primary School or Less** | 0.162 | 0.368 | 0.111 | 0.314 |
| **% Secondary School Drop Out** | 0.214 | 0.410 | 0.227 | 0.419 |
| **% Secondary School** | 0.624 | 0.484 | 0.663 | 0.473 |
| **% in Labor Force** | 0.645 | 0.478 | 0.868 | 0.339 |
| **% Employed** | 0.498 | 0.500 | 0.754 | 0.430 |
| **Number of Observations** | 18447 | | 18204 | |

*Note:* The sample contains individuals aged 17-30 from all survey years 2004-2011. The sample excludes the rural population, those who were enrolled in school when surveyed, and those who have college degree. Degree attainments are defined in an exclusive way.

**Table A3 - Descriptive Statistics for the Sample of Individuals between 17 and 30 (Argentina)**

| | Women | | Men | |
|---|---|---|---|---|
| | **Mean** | **S.D.** | **Mean** | **S.D.** |
| **Age** | 24.030 | 3.796 | 24.089 | 3.789 |
| **% Primary School or Less** | 0.260 | 0.439 | 0.292 | 0.455 |
| **% Secondary School Drop Out** | 0.305 | 0.460 | 0.323 | 0.468 |
| **% Secondary School** | 0.435 | 0.496 | 0.385 | 0.487 |
| **% in Labor Force** | 0.538 | 0.499 | 0.892 | 0.311 |
| **% Employed** | 0.431 | 0.495 | 0.780 | 0.414 |
| **Number of Observations** | 41611 | | 47588 | |

*Note:* The sample contains individuals aged 17-30 from all survey years 2004-2011. The sample excludes the rural population, those who were enrolled in school when surveyed, and those who have college degree. Degree attainments are defined in an exclusive way.

**Table A4 - Descriptive Statistics for the Sample of Individuals between 16 and 29 (Spain)**

| | Women | | Men | |
|---|---|---|---|---|
| | **Mean** | **S.D.** | **Mean** | **S.D.** |
| **6 Years of Schooling or Less** | 0.077 | 0.267 | 0.074 | 0.261 |
| **8 Years of Schooling** | 0.535 | 0.499 | 0.529 | 0.499 |
| **10 Years of Schooling** | 0.387 | 0.487 | 0.397 | 0.489 |
| **% in Labor Force** | 0.815 | 0.388 | 0.955 | 0.207 |
| **% in Labor Force** | 0.541 | 0.498 | 0.784 | 0.411 |
| **Number of Observations** | 24855 | | 37862 | |

*Note:* The sample contains individuals from age groups 16-19, 20-24, 25-29 from all survey years 1999-2004. The sample excludes those who were enrolled in school when surveyed, and those who have more than 10 years of schooling. Degree attainments are defined in an exclusive way.

**Table A5 - Descriptive Statistics for the Spanish Sample by Age and Schooling Level - Men**

| | **1999-2000-2001** | | | **2002-2003-2004** | | |
|---|---|---|---|---|---|---|
| | 10 Years or Less | 8 Years or Less | 6 Years or Less | 10 Years or Less | 8 Years or Less | 6 Years or Less |
| *Ages 16-19* | | | | | | |
| **% in Labor Force** | 0.884 | 0.872 | 0.653 | 0.945 | 0.928 | 0.840 |
| **% in Employed** | 0.594 | 0.587 | 0.372 | 0.686 | 0.686 | 0.580 |
| **% in Employed within the LF** | 0.672 | 0.673 | 0.570 | 0.726 | 0.739 | 0.691 |
| **% Married** | 0.006 | 0.008 | 0.018 | 0.008 | 0.010 | 0.008 |
| *Ages 20-24* | | | | | | |
| **% in Labor Force** | 0.962 | 0.950 | 0.766 | 0.971 | 0.962 | 0.819 |
| **% in Employed** | 0.783 | 0.776 | 0.514 | 0.809 | 0.799 | 0.617 |
| **% in Employed within the LF** | 0.814 | 0.817 | 0.672 | 0.833 | 0.830 | 0.754 |
| **% Married** | 0.055 | 0.063 | 0.110 | 0.054 | 0.060 | 0.098 |
| *Ages 25-29* | | | | | | |
| **% in Labor Force** | 0.963 | 0.956 | 0.786 | 0.966 | 0.959 | 0.774 |
| **% in Employed** | 0.826 | 0.817 | 0.580 | 0.850 | 0.844 | 0.612 |
| **% in Employed within the LF** | 0.857 | 0.855 | 0.738 | 0.880 | 0.881 | 0.790 |
| **% Married** | 0.278 | 0.298 | 0.273 | 0.259 | 0.275 | 0.291 |

*Note:* The sample contains men from age groups 16-19, 20-24, 25-29 from all survey years 1999-2004. The sample excludes those who were enrolled in school when surveyed, and those who have more than 10 years of schooling.

**Table A6 - Descriptive Statistics for the Spanish Sample by Age and Schooling Level - Women**

| | 1999-2000-2001 | | | 2002-2003-2004 | | |
|---|---|---|---|---|---|---|
| | 10 Years or Less | 8 Years or Less | 6 Years or Less | 10Years or Less | 8 Years or Less | 6 Years or Less |
| | *Ages 16-19* | | | | | |
| **% in Labor Force** | 0.857 | 0.851 | 0.635 | 0.822 | 0.787 | 0.589 |
| **% in Employed** | 0.453 | 0.465 | 0.296 | 0.455 | 0.449 | 0.272 |
| **% in Employed within the LF** | 0.529 | 0.547 | 0.465 | 0.554 | 0.571 | 0.461 |
| **% Married** | 0.045 | 0.054 | 0.094 | 0.040 | 0.057 | 0.073 |
| | *Ages 20-24* | | | | | |
| **% in Labor Force** | 0.877 | 0.865 | 0.616 | 0.875 | 0.851 | 0.631 |
| **% in Employed** | 0.586 | 0.580 | 0.358 | 0.616 | 0.600 | 0.317 |
| **% in Employed within the LF** | 0.669 | 0.670 | 0.581 | 0.704 | 0.705 | 0.548 |
| **% Married** | 0.189 | 0.206 | 0.279 | 0.178 | 0.193 | 0.269 |
| | *Ages 25-29* | | | | | |
| **% in Labor Force** | 0.747 | 0.725 | 0.503 | 0.779 | 0.749 | 0.536 |
| **% in Employed** | 0.499 | 0.475 | 0.269 | 0.559 | 0.534 | 0.317 |
| **% in Employed within the LF** | 0.668 | 0.654 | 0.534 | 0.718 | 0.713 | 0.591 |
| **% Married** | 0.565 | 0.586 | 0.503 | 0.505 | 0.529 | 0.414 |

*Note:* The sample contains women from age groups 16-19, 20-24, 25-29 from all survey years 1999-2004. The sample excludes those who were enrolled in school when surveyed, and those who have more than 10 years of schooling.

**Table A7 - The Effect of CMS on Labor Force Participation of Teenage Men (Pre-Recession Period)**

| | Men Aged 17-30 | | | Men Aged 17-19 & 22-30 | | |
|---|---|---|---|---|---|---|
| | HS at Most | MS at Most | ES at Most | HS at Most | MS at Most | ES at Most |
| *Panel 1 - Turkey vs. Peru (Peru is the default country)* | | | | | | |
| Younger than 20*Turkey | -0.141*** | -0.055** | -0.115*** | -0.125*** | -0.028 | -0.097*** |
| | (0.006) | (0.010) | (0.007) | (0.008) | (0.012) | (0.006) |
| Younger than 20 | -0.017 | -0.039** | 0.006 | -0.048* | -0.046*** | 0.013 |
| | (0.009) | (0.008) | (0.013) | (0.016) | (0.005) | (0.012) |
| Turkey | 0.075*** | 0.089*** | -0.071*** | 0.116*** | 0.263*** | 0.178*** |
| | (0.009) | (0.005) | (0.003) | (0.009) | (0.006) | (0.006) |
| Number of Observations | 104,059 | 56,492 | 34,441 | 94,450 | 52,161 | 32,086 |
| *Panel 2 - Turkey vs. Argentina (Argentina is the default country)* | | | | | | |
| Younger than 20*Turkey | -0.157*** | 0.019 | -0.038** | -0.142*** | 0.057** | -0.001 |
| | (0.016) | (0.012) | (0.011) | (0.019) | (0.016) | (0.013) |
| Younger than 20 | -0.010 | -0.081*** | -0.051* | -0.039 | -0.096*** | -0.050** |
| | (0.011) | (0.005) | (0.019) | (0.017) | (0.014) | (0.015) |
| Turkey | -0.052 | -0.025 | 0.000 | -0.051 | -0.028 | 0.003 |
| | (0.024) | (0.018) | (0.015) | (0.022) | (0.017) | (0.019) |
| Number of Observations | 118,338 | 68,431 | 40,943 | 106,686 | 62,387 | 37,817 |

*Note:* Each column presents results from a separate Linear Probability Model regression. The dependent variable is an indicator for being in the labor force. The regression model compares the teenage labor force participation in Turkey vs. Peru in Panel 1, and in Turkey vs. Argentina in Panel 2. The ages are grouped into two categories; men younger than 20, and men older than 21. Men older than 21 is the default category. The variable of interest is theYounger than 20*Turkey. The sample in the separate columns contain individuals with HS degree or less, MS degree or less, and ES degree or less, respectively. I drop survey years after the global recession, 2008,2009, 2010, 2011. Each sample excludes women, rural population, and those who were enrolled in school when surveyed. All estimations control for survey year fixed effects and quadratic age trends. All estimations also control for GDP per capita, male labor force participation, and male unemployment rate of the countries. The first three columns contain men aged 17-30, and the last three columns drop men at the age of 20 or 21 from the samples. Robust standard errors are shown in parentheses, clustered at the survey year level.

**Table A8 - The Effect of CMS on Employment Likelihood of Teenage Men( Pre-Recession Period)**

| | Men Aged 17-30 | | | Men Aged 17-19 & 22-30 | | |
|---|---|---|---|---|---|---|
| | HS at Most | MS at Most | ES at Most | HS at Most | MS at Most | ES at Most |
| *Panel 1 - Turkey vs. Peru (Peru is the default country)* | | | | | | |
| Younger than 20*Turkey | -0.039** | 0.039 | -0.081* | -0.020 | 0.059* | -0.079* |
| | (0.009) | (0.021) | (0.027) | (0.014) | (0.020) | (0.032) |
| Younger than 20 | -0.019 | -0.023 | 0.046 | -0.044* | -0.027 | 0.045 |
| | (0.010) | (0.019) | (0.027) | (0.014) | (0.017) | (0.045) |
| Turkey | 0.273*** | 0.485*** | 0.148*** | 0.363*** | 0.644*** | 0.316*** |
| | (0.011) | (0.020) | (0.011) | (0.013) | (0.013) | (0.008) |
| Number of Observations | 104,059 | 56,492 | 34,441 | 94,450 | 52,161 | 32,086 |
| *Panel 2 - Turkey vs. Argentina (Argentina is the default country)* | | | | | | |
| Younger than 20*Turkey | -0.045* | 0.106*** | 0.033 | -0.025* | 0.136*** | 0.073* |
| | (0.015) | (0.011) | (0.030) | (0.010) | (0.007) | (0.027) |
| Younger than 20 | -0.024 | -0.074*** | -0.045 | -0.058** | -0.091*** | -0.079** |
| | (0.013) | (0.012) | (0.030) | (0.015) | (0.011) | (0.019) |
| Turkey | -0.024 | 0.013 | 0.096*** | -0.035** | 0.006 | 0.090*** |
| | (0.012) | (0.019) | (0.010) | (0.011) | (0.017) | (0.013) |
| Number of Observations | 118,338 | 68,431 | 40,943 | 106,686 | 62,387 | 37,817 |

*Note:* Each column presents results from a separate Linear Probability Model regression. The dependent variable is an indicator for being employed. The regression model compares the teenage labor force participation in Turkey vs. Peru in Panel 1, and in Turkey vs. Argentina in Panel 2. The ages are grouped into two categories; men younger than 20, and men older than 21. Men older than 21 is the default category. The variable of interest is theYounger than 20*Turkey. The sample in the separate columns contain individuals with HS degree or less, MS degree or less, and ES degree or less, respectively. I drop survey years after the global recession, 2008,2009, 2010, 2011. Each sample excludes women, rural population, and those who were enrolled in school when surveyed. All estimations control for survey year fixed effects and quadratic age trends. All estimations control for GDP per capita, male labor force participation, and male unemployment rate of the countries. The first three columns contain men aged 17-30, and the last three columns drop men at the age of 20 or 21 from the samples. Robust standard errors are shown in parentheses, clustered at the survey year level.

**Table A9 - Compulsory Military Service in European Union Member Countries**

| Country | CMS exists | Abolished in |
|---|---|---|
| **Austria** | **Yes** | |
| Belgium | No | 1994 |
| Bulgaria | No | 2008 |
| Croatia | No | 2008 |
| **Cyprus** | **Yes** | |
| Czech Republic | No | 2005 |
| **Denmark** | **Yes** | |
| **Estonia** | **Yes** | |
| **Finland** | **Yes** | |
| France | No | 1997 |
| Germany | No | 2011 |
| **Greece** | **Yes** | |
| Hungary | No | 2004 |
| Ireland | No | |
| Italy | No | 2005 |
| Latvia | No | 2007 |
| Lithuania | No | 2008 |
| Luxembourg | No | |
| Malta | No | |
| Netherlands | No | 1997 |
| Poland | No | 2009 |
| Portugal | No | 2004 |
| Romania | No | 2007 |
| Slovakia | No | 2006 |
| Slovenia | No | |
| Spain | No | 2001 |
| Sweden | No | 2010 |
| United Kingdom | No | 1960 |

**Table A10 - Compulsory Military Service in NATO Member Countries**

| Country | CMS exists |
|---|---|
| Albania | No |
| Belgium | No |
| Bulgaria | No |
| Canada | No |
| Croatia | No |
| Czech Republic | No |
| **Denmark** | **Yes** |
| **Estonia** | **Yes** |
| France | No |
| Germany | No |
| **Greece** | **Yes** |
| Hungary | No |
| Iceland | No |
| Italy | No |
| Latvia | No |
| Lithuania | No |
| Luxembourg | No |
| Netherlands | No |
| **Norway** | **Yes** |
| Poland | No |
| Portugal | No |
| Romania | No |
| Slovakia | No |
| Slovenia | No |
| Spain | No |
| **Turkey** | **Yes** |
| United Kingdom | No |
| United States | No |

# Chapter 2

# With Whom are Young Workers Substitutable? The Impact of the Outflow of Conscription-Age Men on Local Labor Markets

## 1. Introduction

The end of the Cold War renewed the public debate about military conscription. A lengthy previous literature has investigated the effect of military service on individual outcomes of conscripts such as labor market performance, educational attainment, substance use, and criminal activity. A number of researchers found either negative or no effect of conscription on the future wage or employment status of former conscripts, while Torun (2014) found a negative effect on their labor force participation prior to service.[1] Also, they showed that military conscription provides potential conscripts with an incentive to stay on in the educational system in order to postpone their military service.[2] Yet, there is very little empirical research exploring the effects of military conscription on individuals other than conscripts.

In countries with compulsory military service, the army pulls a significant portion of young men out of the labor force every year. The outflow of conscription-age men from local labor markets may increase the chance of employment of men and women at close ages. The military recruitment policy in Turkey generates exogenous variation in

[1] See Angrist (1990), Angrist and Krueger (1994), Imbens and van der Klauw (1995), Grenet (2011), Bauer et al (2012), Paloyo (2010) for the effect of conscription on future labor market outcomes.
[2] See Card and Lemiux (2002) and Maurin and Xenogiani (2007) for evidence on draft avoidance behavior.

the conscription-age male population, assuming no short-term migration across cities, because recruitment occurs randomly in different seasons in different cities. In this paper, I investigate the substitutability effect of military conscription in local labor markets using this unique variation. I find that the outflow of conscription-age men from local labor markets significantly increases the employment of men and women at close ages.

In order to study the substitutability effect of military conscription, I use the 5% sample of 2000 Census data and age-education-city level tabulations of the whole population from Turkey. In Turkey all able-bodied men have to enlist in the military in the year in which they turn 20. The actual recruitment of a certain cohort happens in one of the four seasons. The army randomly assigns a recruitment season for each city, and this generates exogenous variation in the conscription-age male population across cities. At a certain point in a year, the cities that have sent the conscripts have a lower conscription-age male population than others. The lower conscription-age male population can be interpreted as a negative labor supply shock. I find that the employment of men and women at close ages is higher in the cities with lower conscription-age male population compared to those that sent their conscripts.

I start my analysis by estimating the effect of higher or lower male population at conscription age on the employment likelihood of men and women at close ages in a city using the census micro data. I find that the conscription-age male population affects men at close ages more than women. Next, I investigate the effect by education level. Since college students are not required to enlist in the military when they turn 20, most conscripts aged 20-21 are not college graduates. I find that similarly educated men and women are better substitutes with conscription-age men than college graduates. In the second part of the empirical analysis, I estimate the effect of the conscription-age male population by using city-level tabulations. I find that an outflow of 10 men aged 20-21 increases the number of employed women and men at close ages in the city by 1.6 and 4.6 respectively.

The first contribution of this study is to show that military conscription has a significant effect on labor market outcomes of individuals other than conscripts as well. Since early labor market experience is crucial for future labor market outcomes, even temporary employment of young men and women due to an outflow of conscripts may significantly enhance their human capital.

Second, I exploit a credibly exogenous variation to demonstrate the substitutability across types of workers in local labor markets. It is important to know the extent of substitutability in order to understand consequences of programs that affect the labor market incentives of a specific group of people. For example, knowledge of substitutability between immigrants and natives is crucial to understanding the effect of immigrant supply shocks on labor market outcomes of native individuals. Smith (2012) points out the differential impact of immigration across ages, showing that the effect of adult immigrants is larger on teenage natives than on adult natives. Boustan et al. (2010) find that the effect of internal migrant arrivals on local labor market outcomes is larger than that of foreign-born immigrants. The evidence in my paper sheds light on the potential consequences of similar labor supply shocks.

In the next section, I review two areas of the literature. First, I summarize the main findings of the previous research on economic impacts of military conscription.

Second, I review the immigration literature in relation to the findings in this paper. Section 3 presents the institutional details of the military recruitment policy in Turkey. Section 4 describes the sample from 2000 Census micro data and the city-level tabulations, and explains my sample selection. Section 5 covers the empirical strategy and presents the results. Section 6 concludes.

## 2. Literature Review

My paper builds on two areas of the economic literature. First, I contribute to the literature that examines the effect of military conscription on labor market outcomes. The recent literature on military conscription examines the effect of military service on conscripts' individual outcomes. Because of non-random selection into military service, identifying the causal effect of conscription is a challenging endeavor. Therefore, researchers employ a number of empirical strategies including instrumental variables strategies, regression discontinuity designs, and difference-in-differences frameworks to overcome these difficulties.

Angrist (1990) and Angrist and Krueger (1994) investigate the effect of wartime military service on future labor market outcomes in an instrumental variables framework. They find that white Vietnam-era veterans earn 15 percent lower annual wages than non-veterans 10 years after the war, and World War II veterans earn no more, and possibly less, than non-veterans. Using regression discontinuity designs, Grenet et al. (2011) and Bauer et al. (2012) find no significant effect of conscription on future wages of former conscripts in the U.K. and Germany, respectively. Paloyo (2010) revisits conscription in Germany in a differences-in-differences framework and verifies that conscription has no effect on the labor market performance of former conscripts. Although the estimates vary across countries, previous research finds either negative or no effect of conscription on future labor market outcomes of former conscripts.

In Torun (2014) I examine the effect of conscription on early labor market outcomes of potential conscripts before they are called up for service. Unlike in the paper here, I use micro data from Turkey (which has compulsory conscription), and Peru and Argentina (which do not have compulsory conscription) and show that compulsory military service in Turkey reduces the labor force participation and employment of teenage men who are waiting to be called into service compared to teenage men in Peru and Argentina. I also find that the abolition of compulsory military service in Spain increased the labor force participation and employment of teenage men compared to men in their late twenties.

There is little research examining the link between conscription policies and labor market outcomes of individuals other than conscripts. In Torun (2014) I show that the abolition of compulsory military service in Spain also has a significant effect on labor market outcomes of teenage women who were not subject to conscription. I find that teenage women experience a 7.5 percent decrease in their labor force participation and a 10 percent decrease in employment after the abolition of conscription for men in Spain. Both conscription-age men who otherwise would be in the army and teenage men who would be waiting for the service probably became more likely to participate in the labor force displacing some young women out of the market. These two factors combined together lead to a large effect on labor market outcomes of teenage women in Spain.

Although this paper and Torun (2014) support each other's findings, the current paper differs in a number of ways. First, it provides evidence on the substitutability effect of military conscription using a completely different source variation. The variation across cities that I use in this paper allows me to identify the substitutability effect of conscription-age men only. There is no heterogeneity in the anticipation of military conscription across cities, which is my focus in Torun (2014); teenage men anticipate the compulsory military service in the year they turn 20. Thus, the negative effect of anticipation of military service on labor force participation is common across cities, and all non-student teenage men are discouraged from participating in the labor force. Therefore, the findings in the current paper should be interpreted as the effect of outflow or inflow of conscription-age men only rather than the combined effect of conscription-age men and teenage men. Second, both employers and employees know that, in general, there is low labor supply by men aged 20-21 across cities. Yet, the variation across cities in military recruitment seasons generates short term differences between cities that sent the conscripts and cities that have not. So, unlike the evidence from Spain, the findings should be interpreted as the short-run effect of the outflow or inflow of conscription-age men. Third, the current paper uses 2000 Census micro data from Turkey and its city-level tabulations whereas Torun (2014) use labor force micro data from a number of countries.

My paper relates to a second area of literature that examines the impact of immigrant labor supply shocks on local labor markets. A large literature has examined the labor market effects of immigrant inflows using a variety of methodological approaches. Much of the literature uses variation in the fraction of immigrants across cities to estimate the displacement and wage effects of immigration. The estimates from this literature cluster around zero and do not support the common belief that immigrants have an adverse impact on labor market outcomes of native population[3]. This spatial correlation literature suffers from the critique that immigrants are not randomly assigned to destinations but rather they tend to move to the areas with good labor market opportunities. In addition, Aydemir and Borjas (2011) argue that the sampling error in the measures of supply shift commonly used in the literature may lead to large attenuation bias in the estimated wage effects, pushing the estimates toward zero. A second group of papers use natural experiments to identify the effect of immigration on local labor market outcomes including Card (1990) and Friedberg (2001), and find no adverse effect of immigrants on the earnings and employment opportunities of native workers. These studies are related to my paper insofar as they estimate the substitutability between types of workers and generally focus on local labor markets.

While a large body of literature examines the labor market effects of immigration, very few researchers have looked at the effect of internal migrants on labor market outcomes. Boustan et al. (2010) examine the effect of internal migration on labor market of host cities in the U.S. during the Great Depression era. Glitz (2012) estimates the impact of inflow of ethnic Germans from Eastern Europe to Germany. The migrants investigated in the latter two papers spoke the language of the host countries and were similar in appearance. Thus, both studies find significant displacement effects.

My paper relates to this area of literature in a number of ways. First, since conscription-age men are residents of the same city as the affected people, I expect

[3] See Altonji and Card (1991), Friedberg and Hunt (1995), and LaLonde and Topel (1991) for this literature.

substitutability between conscription-age men and non-conscript individuals at close ages to be larger than that found in the immigration literature. Second, unlike the immigration literature, the supply shock investigated in this paper is mostly concentrated around 20-21 year-old male population with no college degree. This detail allows me to estimate the substitutability across genders and age groups. Also, I am able to examine the impact of supply shock of non-college graduate men on individuals with and without college degree separately. Third, given other city characteristics the variation in the number of conscription-age men occurs due to the army's randomization of military recruitment seasons in a year. Also, because the relative differences in conscription-age male population across cities is a temporary one, relocation of individuals at close ages is very unlikely.[4] These two facts ameliorate the endogeneity concerns common in the spatial correlation approach.

Ultimately, there is a related literature that estimates the effect of social security programs that induce retirement of elderly on employment of the young. Gruber et al. (2010) present a number of papers that investigate the relation between the incentives for older persons to leave the labor force and youth employment in twelve countries. Overall, the authors conclude that there is no evidence that the labor force participation of elderly reduces the job opportunities of the young. Although the nature of retirement policies is quite different than that of conscription policies, both alter the labor market participation of some people and therefore may also shift labor demand for others. Unlike the

[4] Some cities send the conscripts earlier than others therefore have a lower conscription-age male population than other cities for a number of months. Yet, it is unlikely that individuals at close ages temporarily move to the cities with low male populations.

retirement policies, the evidence in my paper shows that conscription policies have a larger effect on the job opportunities of the young.

## 3. Institutional Background

Turkey relies on compulsory military service to supply its armed forces with qualified personnel. Young men are called for medical and psychological examination in the year in which they turn 19. Among others, individuals with severe health problems and extreme BMI values are permanently exempted from the service.[5] In principle, all able bodied men have to enlist in the military in the year in which they turn 20. However, those who are physically and mentally fit for the service are not necessarily called up immediately. Individuals who are enrolled in higher education can defer their military service until they reach 29, and non-college graduates or two-year college graduates can defer until the age of 22 or 23.

So, a young man who was born in 1980 goes through the medical examination in 1999, and he is expected to start his service in 2000 unless he is enrolled in school.[6] The duration of military service in Turkey was 18 months between 1995 and 2003.[7] The unit in the military that a man joins and the region where he serves are determined by the military. The majority of men with no college degree are assigned to the army, and relatively fewer men are assigned to the Air Force and the Navy.

[5] See Torun (2014) for a detailed description of the military conscription regime in Turkey.
[6] Note that there are other legitimate reasons for deferral such as temporary sickness. Yet, education is the major legitimate reason for deferring the military service.
[7] The duration of military service has changed twice since 1995. It was 18 months from 1995 to 2003, and 15 months from 2003 to 2014. Currently the military service lasts 12 months in Turkey. Since I use 2000 Census micro data in this paper, the relevant duration is 18 months.

As mentioned, someone born in 1980 is supposed to start his military service in 2000. Yet, the exact season that he starts varies across cities. The enlistments happen in one of the four seasons: February, May, August, and November. Every year, each city is randomly assigned an enlistment season. This randomization generates the unique variation in conscription-age male population across cities. The assignment of enlistment seasons is apparently exogenous; cities in the same province are assigned different enlistment seasons, and the assigned season changes across years. Since the enlistment seasons for 2000 (the previous Census year) are not publicly available, in Table A5, I present enlistment seasons in 2010, 2011, and 2012 for cities of a single province to highlight the random structure of this assignment.

The enlistment season of a young man depends on the registered city of his family. This registered city is not necessarily the same as the city in which an individual was born or lives. Yet, there is a very strong correlation between where an individual lives and his family's registered city. Because internal migration is almost exclusively from small cities to metropolitan areas, I expect the correlation to be weaker in large cities. In addition, a number of cities in a metropolitan area constitute a single local labor market. As will be mentioned in the next section, for these two reasons I drop very large cities from the sample.

Now, I describe how the random assignment of enlistment seasons affects the conscription-age male population of a city at a certain point in time. The 2000 Census was done in October. Since military service lasts 18 months, any man who had enlisted in the military in 1998 returned to his city before October 2000. Regarding the enlistment seasons in 1999 and 2000, a city falls into one of the 16 categories. For example, some cities send the 1979 cohort in February 1999 and the 1980 cohort in February 2000, while some others send them in February 1999 and November 2000. So, in October 2000, when the survey was done, most cities lack two cohorts of men, some others lack one cohort of men and only one group of cities lacks no single cohort. This generates the unique variation in conscription-age male population across cities. Table A6 shows different combinations of enlistment seasons and corresponding missing cohorts from cities.

**4. Data and Sample Selection**

In order to leverage city level variation in the conscription-age male population, I need a data set that contains information on individuals' labor market characteristics and the city in which they live. The 2000 Census sample is the only micro-data that contains this information.[8] It includes approximately 5 percent of the population, around 3.5 million observations coming from 81 provinces and 923 cities. Among many other characteristics, I can observe the gender, employment status, highest degree of schooling attained, marital status, and age of individuals. Also, I can observe the city of birth, city of residence and city of survey for all individuals. I consider an individual as a resident and a potential employee in the city in which he or she was surveyed.[9]

The size of the census sample is quite big, and it is representative of the population. Yet, deriving the conscription-age male population in a city from the micro data may cause sampling errors that could lead to attenuation bias, as shown by Aydemir

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[8] Samples from previous censuses can also be used for the same purpose. Yet, the most recent one has more updated questions, and the tabulations I received for the previous censuses are less detailed than the tabulations for 2000 Census. 2000 Census was the last census in Turkey that is done in person across the country. No census was done in 2010.

[9] Less than 5 percent of individuals have different values for city of residence and city of survey. When declaring population of cities, the Turkish Statistical Institute uses the city of survey as well.

and Borjas (2011). In order to avoid this problem, I obtained a complementary data set from the Turkish Statistical Institute that contains the city-gender-age-education specific population of all cities.[10]

I do not have data on the actual number of recruits in each city. Rather, I use the presence or absence of men of the conscripted age range in October 2000 as a measure of whether conscription has taken place. Therefore, the first part of the empirical analysis estimates the effect of conscription-age men on the employment likelihood of individuals at close ages using 2000 Census micro data and city-level tabulations. The second part of the analysis looks at the same question using city level data only. Since I am interested in individuals who are potentially in the labor force, in the census micro data, I exclude institutional populations that live in prisons, hospitals or barracks.[11] Also, I exclude those who lived in dorms and boarding houses when surveyed, and who stated that enrollment in school was their reason for not being in the labor force.[12] The questionnaires are designed such that individuals living in rural areas, especially those with family farms, are mostly considered employed if they finished their schooling. So, the employment figures in rural areas are less informative about the effect of military conscription policies. Thus, I drop the rural population from my sample. I also generate four schooling categories: those with elementary school degree or less, those with middle school degree, those with high school degree, and those with some college education.

[10] I would like to thank the staff in the Turkish Statistical Institute for their generous help.

[11] Since conscripts do not serve in their hometown, I cannot use knowledge of people that are surveyed in barracks to infer whether young men of city enlisted in the military at the time of Census.

[12] Although the census asks highest degree held by individuals, it does not ask if they are currently enrolled in schooling. Therefore, I exclude students using the information on place of residence (dorms, boarding houses) and the reason for not being in the labor force.

City-level tabulations used in this study exclude the institutional population who live in prisons, hospitals or barracks, and those who live in rural areas. The Institute provided data on the non-institutional population in each city center separately by gender, individual age, education level, and employment status. There are 923 cities in the initial sample. The urban city center population ranges from 683 to 807,934. Since I look at the effect of the conscription-age male population on the employment likelihood of a narrowly defined group of people, I have very few observations from very small cities. Thus, both in the micro data sample and city-level tabulations I drop observations from cities with less than 10,000 urban residents. In addition, a number of large cities in metropolitan areas constitute a single local labor market. A low number of conscription-age men in a large city may not lead to more employment opportunities for individuals at close ages if they are found in high numbers in the neighboring city. For example, a city jurisdiction in Istanbul does not constitute a local labor market in itself. Thus, both in micro data and city-level tabulations I drop observations from cities with more than 150,000 urban residents. This leaves me with 445 medium-sized cities with urban population between 10,000 and 150,000.[13] Among the non-institutional population across 445 cities, the share of men is around 46 percent among those aged 17-19 and 48 percent among those aged 22-24. However, the share of men in the population is as low as 37 percent among those aged 20-21. I use this variation in the conscription-age male population across cities to investigate its effect on employment opportunities of other individuals.

## 5. Empirical Analysis and Results

[13] The number of city with less than 10,000 urban residents was 404, and the number of cities with more than 150,000 urban residents was 74.

In this section I describe my empirical framework and present evidence that the outflow of conscription-age men affects employment opportunities in local labor markets. In the first part, I estimate the effect of the conscription-age male population on the employment likelihood of individuals at close ages using a Linear Probability Model. I use the sample of 2000 Census micro data and city-level tabulations. The micro data allow me to control for individual characteristics such as marital status, education level, and a set of province of birth dummies. In the second part, I estimate the effect on the number of employed individuals at close ages using the city-level tabulations only. This allows me to estimate the effect of outflow of a conscription-age man on the number of employed individuals at close ages in the city.[14] Also, the use of city-level tabulations eliminates the possibility of sampling error at the expense of lacking individual controls in the regression equations.

In both parts, I examine the effect on teenagers and adults in their early twenties separately. In addition, I look at the differential effect on people with and without a college degree. The underlying assumptions are that there are no other reasons why the share of men in the local population deviates from the average except due to conscription.[15]

### 5.1. The Employment Likelihood of Individuals at Close Ages

In line with findings from the immigration literature and my recent paper on compulsory military service, I expect the existence of more conscription-age men in a

[14] I can convert the results from the Linear Probability Model estimations into levels under certain assumptions. Yet, since I have data on the actual populations of cities I prefer to estimate at the city-level.
[15] As mentioned, the share of men in the population is as low as 37 percent among those aged 20-21, and close to fifty percent for other age groups. This supports the assumption mentioned above.

city to reduce the employment opportunities of individuals at close ages. As mentioned in Section 4, I restrict my sample to non-institutional individuals from medium-sized city centers. I investigate the effect of the conscription-age male population on the employment likelihood of men and women at close ages by estimating the following regression equation

$$Y_{ijcp} =$$

$$\beta_0 + \beta_1 \ln(20-21 yearoldmalepop)_{cp} + \beta_2 \ln(ownpop)_{jcp} + \beta_3 \ln(citypop)_{cp} +$$

$$\sum_{k=17,18,19,22,23,24} \beta_k \ln(\text{malepop } at\ age\ k)_{cp} +$$

$$\sum_{k=17,18,19,22,23,24} \alpha_k \ln(\text{femalepop } at\ age\ k)_{cp} + \sum_{p=2}^{81} \gamma_p Province_p +$$

$$\sum_{p=2}^{81} \delta_p\ Provinceofbirt_p + \vec{X}'_{ijcp}\vec{\vartheta} + \epsilon_{ijcp}. \qquad (1)$$

$Y_{ijcp}$ is a binary indicator that shows the employment status of individual *i*, at age group *j*, in city *c*, and province $p$. The key regressor $\ln(20-21 yearoldmalepop)_{cp}$ shows the log of the total number of non-institutional male population aged 20-21 in city $c$.[16] The next two variables control for the own population of the group under investigation and the total urban population of the city. Ideally, I would use data on the recruitment seasons for 1999 and 2000 as instrumental variables for the conscription-age male population in a city. However, this is not publicly available until 2009. One can be concerned that some cities that are not appealing for young men may have poor employment opportunities for other individuals as well. This would lead to an upward bias in the estimated effect of the conscription-age male population on other individuals, less negative or more positive.

[16] Note that the sample contains non-institutional population after the restrictions mentioned in the Section 4. Yet, it does not distinguish between those who are in the labor force and those who are not. This is because employment is my outcome of interest, and labor force participation decision can be endogenous with this outcome.

One can also argue that less appealing cities for young men may be more appealing for young women. Then, the cities with a low conscription-age male population would have better employment opportunities for young women, leading to a downward bias in the estimates. To address these concerns, I include logs of male and female population at other ages from 17 to 24. These control variables would capture most of the unobserved city characteristics that attract conscription-age men to a city or push them away. Next, I include two sets of dummy variables for province of survey and province of birth of each individual. Thus I am leveraging within-province variation in the populations of conscription-age men after controlling for city characteristics. Finally, $\vec{X}'_{ijcp}$ includes individual characteristics such as marital status and completed years of schooling. Finally, the random component $\epsilon_{ijcp}$ represents the unobservable individual characteristics affecting their employment likelihood, and it is assumed to have zero conditional mean. I cluster errors at the city level to allow for correlation in the same city, which is the level of variation of my key regressor. I estimate this regression equation separately for men and women from different age groups and education levels.

Table 1 reports the estimated effect of the conscription-age male population on the employment likelihood of similarly-aged women (in the first row) and men (in the second row). The estimated effects of a change in the conscription-age male population are statistically significant for both women and men and are bigger for the latter. The first column shows that a 10 percent increase in the conscription-age male population in a city reduces the employment likelihood of women aged 17-24 by 0.36 percentage points. Considering the mean employment rate of 16% among women aged 17-24 (as shown in Table A8), this corresponds to a 2 percent decrease in the employment likelihood of young women. The table shows no evidence of differential effects across narrower age groups among women.

The second row reports the estimated effect among men in their late teens or early twenties (omitting the conscription ages of 20-21). The first column shows that a 10 percent increase in conscription-age male population in a city reduces the employment likelihood of men aged 17-19 and 22-24 by 1.1 percentage points (or 2 percent of the mean). The estimated effect for men in their late teens is larger than for men in their early twenties.[17] Thus, all estimates are economically and statistically significant.

Next, I investigate the effect of the conscription-age male population on individuals of various education levels. Table 2 reports the estimated effect for women (in the first panel) and men (in the second panel) divided into four education levels.[18] Recall that college enrollees can postpone their military service until the age of 29. Thus, those who enlisted in the military at 20 are very unlikely to have a college degree. They have either an elementary, middle or high school degree. Therefore, I expect variation in the conscription-age male population to primarily affect non-college graduates in a local labor market. In addition to the control variables in regression equation (1), I control for the log of male or female population from the corresponding age and education group in that city. The first three columns show that, for both women and men, all estimated effects are negative across individuals with an elementary, middle or high school degrees. The estimates in the fourth column (completed some college) are statistically insignificant whereas the estimates in the fifth column (grouping together all who have

[17] The formal coefficient test shows that the two estimates are not significantly different each other. The p-value is 0.11.
[18] I restrict my sample to individuals aged 22-24 to be able to observe individuals with college degree as well.

not attended college) are negative and statistically significant for men and women. The conscription-age male population in a city adversely affects men and women with no college degree, again affecting men more than women. Table A1 and Table A2 repeat the estimations above using the observations from cities with urban population between 10,000 and 50,000 people only. The results are qualitatively and quantitatively very similar.

To sum up, Tables 1 and 2 present novel evidence that the conscription-age male population in a city affects the employment opportunities of men and women at close ages. Although, this is a novel finding in the military conscription literature, it is in line with findings from research on immigration that show significant displacement effect of internal migration. The outflow and inflow of conscription-age men can be interpreted as temporary internal migration. The substitutability effect is greater for men than for women, suggesting that women are less likely to be in similar jobs, and it is only evident for those who have not attended college.

### 5.2. The Number of Employed Individuals at Close Ages – City Level Estimations

In the previous subsection, I used 2000 Census micro data to investigate the effect the conscription-age male population on the employment likelihood of men and women at close ages. Now, I turn to city-level tabulations and estimate the effect on the number of employed men and women in a city. As described in Section 4, the city-level tabulations represent the non-institutional population living in the urban city centers and exclude very small and very large cities. The sample size for the city level regressions is 445.

I start by estimating the effect of the conscription-age male population in a city on the number of employed men and women at close ages. I estimate the following regression equation, which is similar to the earlier specification:

$$\ln(employedpop)_{jcp} =$$
$$\beta_0 + \beta_1 \ln(20 - 21 yearoldmalepop)_{cp} + \beta_2 \ln(ownpop)_{jcp} + \beta_3 \ln(citypop)_{cp} +$$
$$\sum_{k=17,18,19,22,23,24} \beta_k \ln(\text{malepop } at\ age\ k)_{cp} +$$
$$\sum_{k=17,18,19,22,23,24} \alpha_k \ln(\text{femalepop } at\ age\ k)_{cp} + \sum_{p=2}^{81} \gamma_p Province_p + \epsilon_{jcp}. \qquad (2)$$

The dependent variable is the log of the number of employed men or women from age group *j*, in city *c*, and province *p*. The main explanatory variable is the log of the number of conscription-age men in a city. I control for the male and female populations at other ages to capture the unobserved city characteristics that attract or push out young people. I also control for province fixed effects. Finally, the random component $\epsilon_{jcp}$ represents the unobservable factors affecting the number of employed individuals in a city, and it is assumed to have zero conditional mean.

The first row in Table 3 shows that a 10 percent increase in the conscription-age male population in a city reduces the number of employed women aged 17-24 by 1.6 percent. Breaking down the sample by age, the estimated effect is consistently negative for women in their late teens and early twenties and similar in magnitude. The second row shows that a 10 percent increase in the conscription-age male population in a city reduces the number of employed men aged 17-19 and 22-24 by 2.1 percent.[19] The effect

[19] The estimates from the city-level estimations are very similar to the results from the micro-data. The estimates were around 2 percent among women and men in the previous subsection, and 1.6 percent and 2.1 percent here.

is negative and statistically significant at either side of the conscription age and similar in magnitude. Table A7 shows that the average number of conscription-age men across cities is 370, the average number of employed women aged 17-24 is 369, and the average number of employed men aged 17-19 and 22-24 is 816. Thus, the existence of 37 more conscription-age men in a city lead 6 women and 17 men at close ages to be unemployed. This is equivalent to saying that the outflow (inflow) of 10 men aged 20-21 increases (decreases) the number of employed women and men at close ages in the city by 1.6 and 4.6 respectively. The magnitude is slightly larger than the displacement effect found in Boustan et al. (2010) and Glitz (2012). This is understandable as the first study examines the effect of cross-city migration and the second city examines the effect of ethnic Germans who migrated from Eastern Europe to Germany. Conscription-age male population and individuals at close ages are residents of the same city, and therefore should be closer substitutes.

Next, I estimate the effect of the conscription-age male population in a city on the number of employed women (in the first panel of Table 4) and men (in the second panel) who are aged 22-24 with various education levels. I expect substitutability between conscription-age men and non-college graduates to be higher than between conscription-age men and college graduates. I estimate the previous regression equation with an additional variable that controls for the log of men or women population from the corresponding age and education group in that city.

The first three columns show that there is no differential effect across women with elementary, middle and high school degrees. The fourth column shows that more conscription-age male population in a city affects some college-graduate women negatively, yet it does not affect the employment opportunities of college-graduate men significantly. However, the fifth column clearly shows that the conscription-age male population in a city adversely affects non-college graduate women and men. A 10 percent increase in the conscription-age male population in a city reduces the number of employed non-college graduate women and men aged 22-24 by 1.9 percent, and 2.2 percent respectively.[20] Table A3 and table A4 show qualitatively and quantitatively similar results using the observations from cities with urban population between 10,000 and 50,000 only.[21]

## 6. Conclusion

Despite a large body of literature on the impact of military conscription on conscripts' labor market outcomes, there is little work done to analyze its impact on other individuals. I investigate the substitutability effect of military conscription in local labor markets using unique variation that arises because military recruitment policy in Turkey generates exogenous variation in the conscription-age male population across cities at a certain point in time within each year. My results suggest that the outflow of conscription-age men from local labor markets increases the employment opportunities of men and women at close ages.

Leveraging this variation, and using both 2000 Census micro data and city-level tabulations, I find that the outflow of 10 men aged 20-21 increases the number of

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[20] The estimates in the fifth column of Table 2 that use the micro data correspond to 2.9 percent and 1.6 percent respectively.

[21] In my all estimations, I also estimated the effect of conscription-age male population in a city using the population levels instead of logs. Although the estimates quantitatively differ from Table 3 and Table 4, they were qualitatively similar. Modeling a linear relationship between logs of populations is more commonly used in the literature, and I prefer this specification as well.

employed women and men at close ages in the city by 1.6 and 4.6 respectively. The displacement effect is slightly larger than the estimated effect in the immigration literature. I argue that similarities in culture and language make conscription-age men and individuals at close ages closer substitutes in the labor market. Since most men who enlist in the military at 20 are non-college graduates, I find that non-college graduate men and women are affected more by the inflow or outflow of conscription-age men compared to men and women with a college degree.

This paper highlights the significant effect of military conscription on labor market outcomes of individuals other than conscripts. Future research will have to weigh the disruptive effects on labor markets against the value of temporary employment experience for young men and women which may enhance their human capital.

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**Table 1 - Effect of Conscription-Age Male Population on Employment Likelihood of Men and Women at close ages**

| | Employment of Women 17-24 | Employment of Women 17-19 | Employment of Women 20-21 | Employment of Women 22-24 |
|---|---|---|---|---|
| log 20-21 year-old male population | -0.036** | -0.039* | -0.039* | -0.028 |
| | (0.017) | (0.022) | (0.021) | (0.019) |
| Number of Observations | 48,343 | 16,962 | 12,888 | 18,493 |
| | Employment of Men 17-19, 22-24 | Employment of Men 17-19 | | Employment of Men 22-24 |
| log 20-21 year-old male population | -0.110*** | -0.135*** | | -0.086*** |
| | (0.021) | (0.029) | | (0.024) |
| Number of Observations | 31,549 | 14,393 | | 17,156 |

*Note:* Each cell shows the estimated coefficient for log of 20-21 year-old male population in a city. The dependent variable is a dummy variable that takes one if an individual is employed. The observations from cities with less than 10,000 or more than 150,000 urban populations are excluded. All samples exclude rural population, institutional population, and those who were enrolled in school when surveyed. All estimations control for province of survey fixed effects, province of birth fixed effects, marital status, and years of education. Also there are 12 regressors controlling for the log of non-student and non-institutional male and female population in each city at ages 17, 18, 19, 22, 23, and 24. Standard errors are in parentheses and clustered at the city level.

**Table 2 - Effect of Conscription-Age Male Population on Employment Likelihood of Men and Women Aged 22-24 across Education Levels**

| | 5 Years of Schooling or Less | 8 Years of Schooling | 11 Years of Schooling | 13 Years of Schooling or More | 11 or Less Years of Schooling |
|---|---|---|---|---|---|
| | | | *Women* | | |
| log 20-21 year-old male population | -0.027* | -0.002 | -0.071** | 0.033 | -0.034** |
| | (0.015) | (0.047) | (0.035) | (0.070) | (0.016) |
| Number of Observations | 10,444 | 1,523 | 4,750 | 1,776 | 16,717 |
| | | | *Men* | | |
| log 20-21 year-old male population | -0.056* | -0.080 | -0.090** | 0.025 | -0.096*** |
| | (0.032) | (0.055) | (0.038) | (0.070) | (0.025) |
| Number of Observations | 6,847 | 2,425 | 6,122 | 1,762 | 15,394 |

*Note:* Each cell shows the estimated coefficient for log of 20-21 year-old male population in a city. The dependent variable is a dummy variable that takes one if an individual is employed. The observations from cities with less than 10,000 or more than 150,000 urban populations are excluded. All samples exclude rural population, institutional population, and those who were enrolled in school when surveyed. All estimations control for province of survey fixed effects, province of birth fixed effects, and marital status of individuals. Also there are 12 regressors controlling for the log of non-student and non-institutional male and female population in each city at ages 17, 18, 19, 22, 23, and 24. I also control for log of non-institutional female or male population at ages 22-24 and log of non-institutional female or male population at ages 22-24 in the relevant education level in that city. Standard errors are in parentheses and clustered at the city level.

**Table 3 - Effect of Conscription-Age Male Population on the Number of Employed Men and Women at close ages**

| | Employment of Women 17-24 | Employment of Women 17-19 | Employment of Women 20-21 | Employment of Women 22-24 |
|---|---|---|---|---|
| log 20-21 year-old male population | -0.160* | -0.118 | -0.167* | -0.187** |
| | (0.088) | (0.117) | (0.101) | (0.082) |
| Number of Observations | 445 | 445 | 445 | 445 |
| | Employment of Men 17-19, 22-24 | Employment of Men 17-19 | | Employment of Men 22-24 |
| log 20-21 year-old male population | -0.211*** | -0.232*** | | -0.198*** |
| | (0.042) | (0.055) | | (0.039) |
| Number of Observations | 445 | 445 | | 445 |

*Note:* Each cell shows the estimated coefficient for log of 20-21 year-old male population in a city. The dependent variable is the log of the number of employed men or women in the age group given at the top of each column. The cities with less than 10,000 or more than 150,000 urban populations are excluded. All samples exclude rural population, institutional population, and those who were enrolled in school when surveyed. All estimations control for province of survey fixed effects. Also there are 12 regressors controlling for the log of non-student and non-institutional female or male population in each city at ages 17, 18, 19, 22, 23, and 24.

**Table 4 - Effect of Conscription-Age Male Population on the Number of Employed Men and Women Aged 22-24 across Education Levels**

| | 5 Years of Schooling or Less | 8 Years of Schooling | 11 Years of Schooling | 13 Years of Schooling or More | 11 or Less Years of Schooling |
|---|---|---|---|---|---|
| *The Sample from Cities with 10,000-150,000 Population* | | | | | |
| log 20-21 year-old male population | -0.126 | 0.090 | -0.241*** | -0.111** | -0.189** |
| | (0.125) | (0.126) | (0.090) | (0.046) | (0.087) |
| Number of Observations | 445 | 445 | 445 | 445 | 445 |
| *The Sample from Cities with 10,000-150,000 Population* | | | | | |
| log 20-21 year-old male population | -0.176*** | -0.246*** | -0.181*** | -0.055 | -0.222*** |
| | (0.045) | (0.056) | (0.044) | (0.045) | (0.039) |
| Number of Observations | 445 | 445 | 445 | 445 | 445 |

*Note:* Each cell shows the estimated coefficient for log of 20-21 year-old male population in a local labor market. The dependent variable is the log of the number of employed men or women in the education group given at the top of each column. The cities with less than 10,000 or more than 150,000 urban populations are excluded. All samples exclude rural population, institutional population, and those who were enrolled in school when surveyed. All estimations control for province of survey fixed effects. Also there are 12 regressors controlling for the log of non-student and non-institutional female or male population in each city at ages 17, 18, 19, 22, 23, and 24, and log of non-institutional female or male population at ages 22-24 in the relevant education level in that city

## Appendix A: Tables

**Table A1 - Effect of Conscription-Age Male Population on Employment Likelihood of Men and Women at close ages**

| | Employment of Women 17-24 | Employment of Women 17-19 | Employment of Women 20-21 | Employment of Women 22-24 |
|---|---|---|---|---|
| log 20-21 year-old male population | -0.0268 | -0.032 | -0.032 | -0.013 |
| | (0.019) | (0.026) | (0.025) | (0.021) |
| Number of Observations | 22,845 | 8,117 | 6,079 | 8,649 |

| | Employment of Men 17-24 | Employment of Men 17-19 | | Employment of Men 22-24 |
|---|---|---|---|---|
| log 20-21 year-old male population | -0.105*** | -0.120*** | | -0.085*** |
| | (0.026) | (0.037) | | (0.030) |
| Number of Observations | 14,997 | 6,678 | | 8,319 |

*Note:* Each cell shows the estimated coefficient for log of 20-21 year-old male population in a city. The dependent variable is a dummy variable that takes one if an individual is employed. The observations from cities with less than 10,000 or more than 50,000 urban populations are excluded. All samples exclude rural population, institutional population, and those who were enrolled in school when surveyed. All estimations control for province of survey fixed effects, province of birth fixed effects, marital status, and years of education. Also there are 12 regressors controlling for the log of non-student and non-institutional male and female population in each city at ages 17, 18, 19, 22, 23, and 24. Standard errors are in parentheses and clustered at the city level.

**Table A2 - Effect of Conscription-Age Male Population on Employment Likelihood of Men and Women Aged 22-24 across Education Levels**

| | 5 Years of Schooling or Less | 8 Years of Schooling | 11 Years of Schooling | 13 Years of Schooling or More | 11 or Less Years of Schooling |
|---|---|---|---|---|---|
| | | | *Women* | | |
| log 20-21 year-old male population | -0.003 | 0.004 | -0.032 | -0.055 | -0.012 |
| | (0.021) | (0.062) | (0.041) | (0.090) | (0.019) |
| Number of Observations | 4,831 | 760 | 2,265 | 793 | 7,856 |
| | | | *Population-Men* | | |
| log 20-21 year-old male population | -0.013 | -0.077 | -0.084 | 0.059 | -0.089*** |
| | (0.041) | (0.074) | (0.051) | (0.102) | (0.032) |
| Number of Observations | 3,175 | 1,203 | 3,122 | 819 | 7,500 |

*Note:* Each cell shows the estimated coefficient for log of 20-21 year-old male population in a city. The dependent variable is a dummy variable that takes one if an individual is employed. The observations from cities with less than 10,000 or more than 50,000 urban populations are excluded. All samples exclude rural population, institutional population, and those who were enrolled in school when surveyed. All estimations control for province of survey fixed effects, province of birth fixed effects, and marital status of individuals. Also there are 12 regressors controlling for the log of non-student and non-institutional male and female population in each city at ages 17, 18, 19, 22, 23, and 24. I also control for log of non-institutional female or male population at ages 22-24 and log of non-institutional female or male population at ages 22-24 in the relevant education level in that city. Standard errors are in parentheses and clustered at the city level.

**Table A3 - Effect of Conscription-Age Male Population on the Number of Employed Men and Women at close ages**

| | Employment of Women 17-24 | Employment of Women 17-19 | Employment of Women 20-21 | Employment of Women 22-24 |
|---|---|---|---|---|
| log 20-21 year-old male population | -0.138 | -0.089 | -0.157 | -0.160 |
| | (0.105) | (0.136) | (0.120) | (0.097) |
| Number of Observations | 345 | 345 | 345 | 345 |

| | Employment of Men 17-19, 22-24 | Employment of Men 17-19 | | Employment of Men 22-24 |
|---|---|---|---|---|
| log 20-21 year-old male population | -0.213*** | -0.241*** | | -0.196*** |
| | (0.052) | (0.067) | | (0.047) |
| Number of Observations | 345 | 345 | | 345 |

*Note:* Each cell shows the estimated coefficient for log of 20-21 year-old male population in a city. The dependent variable is the log of the number of employed men or women in the age group given at the top of each column. The cities with less than 10,000 or more than 50,000 urban populations are excluded. All samples exclude rural population, institutional population, and those who were enrolled in school when surveyed. All estimations control for province of survey fixed effects. Also there are 12 regressors controlling for the log of non-student and non-institutional female or male population in each city at ages 17, 18, 19, 22, 23, and 24.

**Table A4 - Effect of Conscription-Age Male Population on the Number of Employed Men and Women Aged 22-24 across Education Levels**

| | 5 Years of Schooling or Less | 8 Years of Schooling | 11 Years of Schooling | 13 Years of Schooling or More | 11 or Less Years of Schooling |
|---|---|---|---|---|---|
| | | | *Women* | | |
| log 20-21 year-old male population | -0.152 | -0.002 | -0.217* | -0.138** | -0.153 |
| | (0.140) | (0.121) | (0.112) | (0.058) | (0.100) |
| Number of Observations | 345 | 345 | 345 | 345 | 345 |
| | | | *Men* | | |
| log 20-21 year-old male population | -0.149*** | -0.228*** | -0.177*** | -0.0786 | -0.211*** |
| | (0.053) | (0.069) | (0.053) | (0.057) | (0.047) |
| Number of Observations | 345 | 345 | 345 | 345 | 345 |

*Note:* Each cell shows the estimated coefficient for log of 20-21 year-old male population in a local labor market. The dependent variable is the log of the number of employed men or women in the education group given at the top of each column. The cities with less than 10,000 or more than 50,000 urban populations are excluded. All samples exclude rural population, institutional population, and those who were enrolled in school when surveyed. All estimations control for province of survey fixed effects. Also there are 12 regressors controlling for the log of non-student and non-institutional female or male population in each city at ages 17, 18, 19, 22, 23, and 24, and log of non-institutional female or male population at ages 22-24 in the relevant education level in that city

**Table A5 - Recruitment Seasons across Cities from a Province**

| City Code | City Name | Recruitment in 2010 | Recruitment in 2011 | Recruitment in 2012 |
|---|---|---|---|---|
| 101 | SEYHAN | February | May | August |
| 102 | YÜREĞİR | May | February | May |
| 103 | ALADAĞ | May | February | May |
| 104 | CEYHAN | August | November | May |
| 105 | FEKE | August | November | February |
| 106 | İMAMOĞLU | February | August | May |
| 107 | KARAİSALI | August | November | February |
| 108 | KARATAŞ | May | August | November |
| 109 | KOZAN | February | August | May |
| 110 | POZANTI | May | February | May |
| 111 | SAİMBEYLİ | November | May | August |
| 112 | TUFANBEYLİ | November | May | August |
| 113 | YUMURTALIK | August | November | May |

*Note:* The table presents the recruitment seasons of the cities in Adana province in 2010, 2011, and 2012.

**Table A6 - Recruitment Seasons across Cities and the Outflow of Conscription-Age Men**

| | | | | | | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| **Recruitment Season in 1999** | F | F | F | F | M | M | M | M | A | A | A | A | N | N | N | N |
| **Recruitment Season in 2000** | F | M | A | N | F | M | A | N | F | M | A | N | F | M | A | N |
| **1979 cohort are in the city** | Yes | Yes | Yes | Yes | No | No | No | No | No | No | No | No | No | No | No | No |
| **1980 cohort are in the city** | No | No | No | Yes | No | No | No | Yes | No | No | No | Yes | No | No | No | Yes |

*Note:* There are 16 types of cities regarding the 1999 and 2000 recruitment seasons. Since the military service is 18 months, for cities that send 1979 cohort men in February 1999, those men are back in the city by October 2000, the date of 2000 Census. All other cities that send 1979 cohort men after February lack that cohort of men in the city. Similarly, cities that send 1980 cohort men before October 2000 lack those cohorts of men. However, the cities that send 1980 cohort in November 2000 still have those men.

**Table A7 - Average Population of Age-Gender-Education Groups**
**Cities with 10,000-150,000 Population**

| **Population Group** | **Mean** |
|---|---|
| Number of 20-21 year-old men | 370 |
| Number of 17-24 year old women | 2255 |
| Number of 17-24 year old employed women | 369 |
| Number of 17-19, 22-24 year old men | 1454 |
| Number of 17-19, 22-24 year old employed men | 816 |
| Number of 17-19 year old women | 776 |
| Number of 17-19 year old employed women | 119 |
| Number of 17-19 year old men | 663 |
| Number of 17-19 year old employed men | 337 |
| Number of 20-21 year old women | 608 |
| Number of 20-21 year old employed women | 96 |
| Number of 22-24 year old women | 871 |
| Number of 22-24 year old employed women | 152 |
| Number of 22-24 year old men | 791 |
| Number of 22-24 year old employed men | 479 |
| Number of 22-24 year old women with college degree | 89 |
| Number of 22-24 year old employed women with college degree | 54 |
| Number of 22-24 year old men with college degree | 82 |
| Number of 22-24 year old employed men with college degree | 53 |
| Number of 22-24 year old women with no college degree | 781 |
| Number of 22-24 year old employed women with no college degree | 99 |
| Number of 22-24 year old men with no college degree | 708 |
| Number of 22-24 year old employed men with no college degree | 424 |

*Note:* I calculated the mean values using the city-level tabulations that I received from the Turkish Statistical Institute. I exclude cities that have urban population less than 10,000 or more than 150, 000, which leaves me with 445 observations. All values correspond to the city centers, and exclude the rural population in each city jurisdiction. Also, I exclude the institutional population and those who were out of labor force due to enrollment in school.

**Table A8 - Mean Employment Ratio of Age-Gender-Education Groups**
**Cities with 10,000-150,000 Population**

| **Population Group** | **Mean** |
|---|---|
| Employment ratio among 17-24 year old women | 0.16 |
| Employment ratio among 17-19, 22-24 year old men | 0.54 |
| Employment ratio among 17-19 year old women | 0.16 |
| Employment ratio among 17-19 year old men | 0.49 |
| Employment ratio among 20-21 year old women | 0.16 |
| Employment ratio among 22-24 year old women | 0.16 |
| Employment ratio among 22-24 year old men | 0.58 |
| Employment ratio among 22-24 year old women with college degree | 0.62 |
| Employment ratio among 22-24 year old men with college degree | 0.64 |
| Employment ratio among 22-24 year old women with no college degree | 0.12 |
| Employment ratio among 22-24 year old men with no college degree | 0.57 |

*Note:* I calculated the mean values using the city-level tabulations that I received from the Turkish Statistical Institute. I exclude cities that have urban population less than 10,000 or more than 150, 000, which leaves me with 445 observations. All values correspond to the city centers, and exclude the rural population in each city jurisdiction. Also, I exclude the institutional population and those who were out of labor force due to enrollment in school.

# Chapter 3

# Compulsory Schooling and Early Labor Market Outcomes in a Middle-Income Country

## 1. Introduction

Higher educational attainment is seen as a major way for individuals to improve their income and for nations to increase economic growth. Many education policies such as compulsory schooling laws are predicated on this notion. In contrast to the strong positive correlation between educational attainment and income, persistent within and across countries, most of the evidence for a causal effect of schooling on income comes from research on high income countries. Moreover, most of the papers from the latter category focus only on wage returns to schooling. The papers that investigate compulsory attendance laws in the U.K. and North America find high returns for both genders (Angrist and Krueger, 1991; Oreopoulos, 2006a). Recent work extends the investigation of returns to compulsory schooling to other European countries such as France and Germany, and found zero effect of compulsory schooling on wages for both genders (Pischke and von Wachter 2008; Grenet 2012).

There has been little work focusing on causal effects of compulsory schooling in middle income countries, which have relatively different socioeconomic and institutional backgrounds than high income countries. Since the gender gap in labor force participation and educational attainment in middle income countries is greater, analysis of compulsory schooling in a middle income country gains added relevance. I investigate the causal effect of compulsory schooling on labor market outcomes using exogenous variation in Turkey. I also present evidence that advances our understanding of the factors that help explain differential returns to compulsory schooling across genders.

Most of the previous research focusing on high income countries uses compulsory attendance laws in the second and third quarters of the 20$^{th}$ century. Therefore, the samples used in those studies mostly consist of individuals observed at ages 30-50. Moreover, those attendance laws affect individuals at the margin attending high school. The 1997 reform in Turkey extended mandatory schooling from five to eight years and affected 35 percent of the relevant cohort, who were dropping out after 5$^{th}$ grade.

I adopt Card's (1999) theoretical framework in order to motivate the analysis of this reform. I explain how individuals with heterogeneous abilities and costs of schooling choose different levels of schooling and how this endogeneity affects the interpretation of causal estimates. Given the potential heterogeneity, I interpret the estimation results within the Local Average Treatment Effect (LATE) framework of Imbens and Angrist (1994). In interpreting the empirical results, the theoretical framework suggests that women have higher marginal costs of schooling. This can explain why women's average schooling levels were low before the reform, and why the LATE estimate of the return to schooling for women in Turkey is higher than for men.

My work builds on previous research on compulsory schooling laws in a number of ways. First, I focus on the relatively understudied group of middle income countries, and estimate returns to schooling at the middle school margin. Second, because the compulsory schooling reform is recent, I can examine the early-career effects of additional schooling. This is not possible for many other countries with earlier reforms, when data were not available. Third, exploiting the richness of the data, I investigate additional outcomes including employment status, self-employment, and formal social security coverage. I also examine the effect of additional schooling on job choice and resulting tasks done at jobs as potential mechanisms through which earning gains occur. To my knowledge, this is the first paper that examines the channels of earning gains after a compulsory schooling reform.

I find that the reform increases average years of education by 0.61 years among women and 0.38 years among men. An additional year of education due to the compulsory schooling reform increases monthly earnings by about 9-10% among non-college bound women and 0-2% among non-college bound men. Another mechanism through which educational attainment induced by the compulsory schooling reform benefits individuals is through increased likelihood of social security participation. I do not find any significant effect of an additional year of compulsory schooling on employment likelihood for either gender.

When I focus on job choice, I find that women who worked are significantly more likely to move into higher skill and formal sector jobs. They are more likely to move from the private to public sector and from manufacturing to finance and business services and to community and social services. Moreover, using the Dictionary of Occupational Titles (DOT, 1991) I show that women with additional schooling are more likely to have occupations that require more complicated tasks in relation to data, people and things.

In the next section, I review the returns to schooling literature with a focus on the papers that use compulsory schooling reforms in Instrumental Variables (IV) framework. Section 3 provides a theoretical framework that motivates the analysis of this reform. Section 4 provides institutional background and theoretical predictions. Section 5 describes the data and the key variables used in the analysis. Section 6 covers the estimation strategy and provides a discussion of potential identification problems. Section 7 presents the results, and Section 8 concludes.

## 2. Literature Review

Part of the literature on returns to education investigates the non-monetary returns, such as health, longevity and political participation (Lochner, 2011). The other part of the literature, on which I will focus, examines the causal impact of schooling on earnings. The recent work leverages exogenous variation in schooling generated by institutional changes to identify the causal effect of schooling on earnings while recognizing that various institutional changes may have very different effects on individuals at different points of the earning and schooling distributions.[1] For example, Angrist and Krueger (1991) use quarter of birth as an instrument for schooling combined with laws related to the minimum school exit age, and find around an 8 percent return to an additional year of schooling in the U.S. Using changes in the minimum school exit age in 1947 and 1973 as instruments for schooling, Harmon and Walker (1995) estimate returns to schooling on the order of 15% for British males. Their estimation strategy has been criticized since they look at a wide range of cohorts and do not discuss the potential cohort effects in the

[1] Earlier studies of returns to schooling focused on twins with different schooling levels or used family background as a control in the least squares estimation or as an instrument for schooling to deal with the potential endogeneity of schooling. Much of the former group suffers from the critique that the family background may be correlated with labor market outcomes through other ways than educational attainment.

paper. More recently, Oreopoulos (2006a) exploits similar minimum schooling laws in the U.K. in a regression discontinuity design, and reports returns of 14% in Britain and 14-21% in Northern Ireland. The margin that is evaluated is $10^{th}$ versus $11^{th}$ grade in Angrist and Krueger (1991) and $9^{th}$ versus $10^{th}$ grade in Oreopoulos (2006a).

However, the research on compulsory schooling in other countries finds somewhat different effects. Grenet (2012) compares compulsory schooling reforms in France and England in a regression discontinuity design. The margin that is evaluated is $8^{th}$ versus $10^{th}$ grade in France and $10^{th}$ versus $11^{th}$ grade in England. His 2SLS estimates show that the earnings return to compulsory schooling is insignificant and close to zero in France for women and men, and is positive and significant, at about 6 percent, in England and Wales. He also reports that the effect of raising the minimum school leaving age on the probability of being employed is essentially zero, except among women in England. Grenet (2012) argues that, because the reform in France did not induce pupils to receive more academic qualifications (unlike the reform in England and Wales), additional compulsory schooling in France did not translate into higher wages. Pischke and von Wachter (2008) exploit variation across German states in the timing of the introduction of $9^{th}$ grade in the secondary schools and find zero returns to education. The authors argue that the zero returns to an extra year of schooling after the reform arises because the German education system provides the basic skills necessary for the labor market earlier than other countries, before the $9^{th}$ grade. Stephens and Yang (2012) reevaluate compulsory attendance laws in the U.S. controlling for changes in school quality, and find no effect on the subsequent earnings of individuals.

The papers mentioned above focus on high-income countries, investigate the effect of schooling at the high school margin, and often examine the later labor market outcomes. Also, they do not show the mechanisms through which returns to compulsory schooling occur. My paper differs because I investigate the early labor market outcomes of compulsory middle school in a middle income country. I also investigate job choice and tasks done at jobs as potential mechanisms through which earning gains occur after a compulsory schooling reform.

## 3. Theoretical Framework

I interpret the case of Turkey within Card's (1999) framework of schooling investment, which in turn builds on Becker (1967). The theoretical framework provides intuition to support my empirical analysis, showing that schooling and returns to schooling are potentially endogenous and guiding the interpretation of the estimation results. It offers an explanation for the different estimates of the return to schooling for men and women.

### 3.1. Endogeneity of Schooling

Card (1999) assumes that individuals maximize the following utility function by choosing the optimal level of schooling S,

$$U(S, y) = logy(S) - h(S) \tag{1}$$

where y$(S)$ denotes the average level of earnings an individual with schooling level S receives, and $h(S)$ is a convex function representing the individual cost of schooling such as forgone earnings, tuition, fees and psychological costs. Including heterogeneity in costs and benefits of schooling yields heterogeneity in optimal schooling levels. Following Card (1999), let the marginal benefits and costs of schooling be characterized by

$$\frac{y'(S)}{y(S)} = b_i - k_1 S \qquad\qquad h'(S) = r_i + k_2 S \tag{2}$$

where $b_i$ and $r_i$ are random variables with means $\bar{b}$ and $\bar{r}$, $b_i = \bar{b} + \varphi_i$, $r_i = \bar{r} + \eta_i$ where $b_i$ corresponds to abilities, $r_i$ corresponds to psychological and monetary costs of schooling. $k_1$ and $k_2$ are non-negative constants, representing the concavity of the earning function and convexity of the cost function respectively. The optimal schooling level that solves the problem is

$$S_i^* = (b_i - r_i)/k \tag{3}$$

where $k = k_1 + k_2$. This shows how individual schooling decisions depend on individual returns and costs and why we expect different returns to schooling at different schooling levels.

In this framework with heterogeneous returns, Card shows that the Ordinary Least Squares (OLS) estimate of the effect of schooling on log wages is different from the average effect because individuals with higher returns acquire more schooling. Moreover, because compulsory schooling laws affect some subgroups more than others, IV estimates do not provide an average return. Card (1999) shows that an instrument that causes a proportional reduction in the marginal cost leads the IV estimate to be different than the average marginal returns in the population.

When a binary instrument affects a certain subgroup only in a dichotomous way, the estimation results can be conveniently interpreted within the LATE framework developed by Angrist and Imbens (1994) and Angrist et al. (1996). Suppose that the population can be categorized into a number of groups of individuals with common latent values of costs and benefits, and the policy intervention induces a certain subgroup to receive an additional year of schooling. Angrist and Imbens (1994) shows that a monotonicity assumption together with independence of the instrument implies

$$\frac{E(Y_i|Z_i=1)-E(Y_i|Z_i=0)}{E(S_i|Z_i=1)-E(S_i|Z_i=0)} = E(\, Y_i(1) - Y_i(0)|S_i(1) \geq S_i(0)\,) \tag{4}$$

where $Z_i$ indicates exposure to the reform, $Y_i(S)$ indicates the labor market outcome as a function of additional year of schooling, and $S_i(z)$ is a binary indicator for additional year of schooling as a function of the instrument. The IV estimate shows the average effect of an additional year of schooling among those who are induced to receive an additional year by the reform.

If there is variable treatment intensity and treatment effect heterogeneity, then the IV estimate can be interpreted as a weighted average of causal responses to a unit change in treatment (Angrist and Imbens, 1995). Suppose that the population can be categorized into groups of individuals with common values for ability and taste terms ($\alpha_g,\ b_g\ ,\eta_g$), and the policy intervention leads to a change $\Delta S_g$in the mean schooling of group g, and $\beta_g$ denotes the marginal return to schooling for group g in the absence of intervention. Then, following Card's notation, the IV estimator of the return to schooling has the probability limit

$$plimb_{iv} = \frac{E[\beta_g \Delta S_g]}{E[\Delta S_g]} \quad (6)$$

where expectations are taken with respect to the probability distribution of the population across cells. The IV estimator is consistent with the average marginal returns to schooling if there are identical marginal returns across subgroups ($\beta_g = \bar{\beta}$) or change in schooling is homogenous across subgroups (Card, 1999). This framework allows me to interpret my estimates as revealing the LATE for people induced to attain more schooling after the reform.

## 4. Background and Theoretical Predictions

In Turkey, children start the five-year primary school after they turn six. Primary school attendance has been mandatory for decades, and any subsequent attainment was voluntary. Following primary school, students had the option to choose either three-year secular middle school or three-year vocational middle school. The 1997 reform extended mandatory schooling to eight years for individuals born after 1986 and merged the primary and middle schools into a single entity.[2] The law required individuals who were born after 1986 to continue until the $8^{th}$ grade.[3] The construction of additional classrooms and hiring of additional teachers were financed by extra taxes. Double-session schooling became more common, especially in the beginning of the program, and some schools with small number of students in rural areas were closed in the process, requiring some

[2] Using 2000 census data, I find that the share of vocational middle school graduates among those who drop out after the middle school was extremely small (2.5%) for individuals aged 20-24. So the change in the composition of middle school types is unlikely to affect the estimated outcomes significantly. Thus, I take years of education as a homogenous treatment.

[3] The wording in the law was such that it required the fifth graders in 1997-1998 academic year to continue until the $8^{th}$ grade. Considering the school starting age, that cohort corresponds to those who were born after 1986. Note that some individuals who were born in 1987 but started school early may not be exposed to the law, and someone who was born in 1986 and started school late may be exposed to the law. Individuals who turn 14 were allowed to drop out at the end of that academic year, regardless of the grade.

students to commute longer distances than before. The short-term estimated effect of the compulsory schooling law should be viewed as the combined effect of these treatments.

Before the reform, the average schooling level among women was significantly lower than among men: 45 percent of women aged 20-29 and 65 percent of men aged 20-29 had at least a middle school degree before the reform. Either a high marginal cost of schooling or low marginal returns to schooling among women might explain this observation. Below, I use Card's framework to distinguish between these two assumptions about the underlying distribution of latent characteristics.

I adopt the cost shifter example in Card (1999) to model the extension of mandatory schooling. Although the amendment to the law requires individuals to attend school until the 8th grade, in practice construction of new classrooms, hiring of new teachers and the campaign itself acted as a cost shifter for individuals who would drop out in the absence of the reform. The legal change and the campaign for middle school education partially eliminated the social barriers for individuals. In Table 1, I will show that main predictions also hold when I model the schooling reform as a direct enforcement instead of a proportional reduction in the cost.

Suppose we have data from two samples of individuals, men and women, with the same $b_i, \eta_i, \sigma_\eta^2$, $\sigma_{b\eta}$ distributions, but with higher mean marginal cost of schooling for women than men, $\bar{r}_w > \bar{r}_m$.[4] Assume that log earnings are linear in schooling, i.e. $k_1 = 0$, and within each sample those with low marginal costs also have higher returns to

[4] $\sigma_\eta^2$ is the variance of $r_i$, and $\sigma_{br}$is the covariance of $b_i$ with $r_i$. Note, the marginal cost is defined as the following, $r_i = \bar{r} + \eta_i$.

schooling, i.e. $\sigma_{br} < 0$.[5] Card (1994) motivates the assumption of negative covariance between marginal costs and marginal returns in the following way. Children of high ability parents will partially inherit the high ability. Moreover, since the high ability parents earn high income and have strong taste for schooling, children will have less marginal disutility of schooling. Propositions 1 and 2 follow.

*Proposition 1: When women have a higher marginal cost of schooling than men, a policy that causes a proportional reduction in the marginal cost leads to bigger changes in schooling in the sample of women.*

Card (1999) shows that the treatment effect of a proportional reduction in the marginal cost is larger for individuals with lower schooling levels within the same sample. I implement the same reasoning to compare two samples. The optimal schooling level for individual $i$ after the reform is $S_i^{*\prime} = (b_i - \theta r_i)/k$ where $\theta$ is a cost shifter, $0 < \theta < 1$. The difference between new and old average schooling levels is $\Delta\bar{S} = \overline{S^{*\prime}} - \overline{S^*} = \frac{\bar{r}(1-\theta)}{k_1+k_2}$. So, the change in schooling level is expected to be bigger among women, as $\bar{r}_w > \bar{r}_m$.

*Proposition 2: When women have a higher marginal cost of schooling than men, estimation that uses this policy as an instrumental variable leads to higher estimated effects among women than among men.*

In each sample, the effect of the schooling reform is bigger for individuals with higher marginal costs and lower marginal returns. The IV estimator reflects a weighted

[5] The case with $k_1 > 0$ does not change Propositions 1 and 3. As long as the concavity is not so big, Proposition 2 and Proposition 4 are intact as well.

average of the returns to schooling, weights being the changes in schooling. Since the weights associated with low return individuals are bigger in the sample of men relative to sample of women, the IV estimate for men is smaller.[6]

The alternative explanation for pre-reform low average schooling levels among women is that they have lower marginal returns to schooling. Once we assume the same $\varphi_i, r_i, \sigma_\eta^2$, $\sigma_{br}$ distributions in both samples, but that women have lower mean marginal returns to schooling than men, $\bar{b}_w < \bar{b}_m$, propositions 3 and 4 follow instead of propositions 1 and 2.

*Proposition 3: When women have a lower return to schooling than men, a policy that causes a proportional reduction in the marginal cost leads to equal changes in schooling in both samples.*

As I showed in the explanation of the first proposition, the reform operates through marginal costs, and different marginal benefits do not affect the change in schooling levels.

*Proposition 4: When women have a lower return to schooling than men, estimation that uses this policy as an instrumental variable leads to lower estimated effects among women than among men.*

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[6] In other words, since the reform affects the sample with higher average marginal cost in more balanced way, observations will have more similar weights, and women will have a bigger IV estimate than men. Note that this point involves a slightly different situation than having different Local Average Treatment Effects within the same population. I am pointing that, even if the average marginal return to education is the same across two populations, we may have different IV estimates. See Theory Appendix for the formal proof.

If the average marginal returns for women are lower than for men, then $\bar{\beta}_w < \bar{\beta}_m$. IV estimates will reflect the weighted average of marginal returns, $plimb_{iv} = \frac{E[\beta_g \Delta S_g]}{E[\Delta S_g]}$. Since the reform affects schooling levels in two samples in the same way, weights will be the same in both samples. The IV estimate will be lower among women than among men since women have lower average returns in the first place. The Theory Appendix provides a formal proof.

To clarify the case, I propose the following example which models the reform as completely enforced. There are four subgroups of individuals with common cost and benefit terms ( $b_g$ ,$\eta_g$) within each subgroup, and log earnings are linear in schooling, i.e. $k_1 = 0$. Let $\sigma_{b\eta} < 0$, and $b_1 < b_2 < b_3 < b_4$ and $\eta_1 > \eta_2 > \eta_3 > \eta_4$ , so those with higher marginal costs of schooling also have lower marginal returns to schooling. Men and women from the same group $g$ have the same characteristics except for the mean marginal cost, $\bar{r}_w > \bar{r}_m$.

Using equation (3), the optimal schooling level would be higher in the sample of men as $\bar{r}_w > \bar{r}_m$. For simplicity, suppose the subgroups of women choose 1, 2, 3, and 4 units of schooling respectively, and the subgroups of men choose 2, 3, 4, and 5 units of schooling. Note that women who have 1 unit of schooling have the same innate characteristics except for $\bar{r}$ as men who have 2 units of schooling. Now, the compulsory schooling reform makes it mandatory to finish at least 3 units of schooling. As shown in Table 1, the IV estimate will reflect the marginal returns of the first and second group in the sample of women, but the marginal returns of the first group in the sample of men. By assumption $b_1 < b_2$, so the LATE in the sample of women will be bigger than the LATE in the sample of men. Table A1 in the Appendix shows that the alternative case with lower returns to schooling among women than among men but identical costs across genders predicts lower LATE estimates in the sample of women than in the sample of men, which does not match my findings in Section 7.

Thus, the empirical results in the following sections do not only show the effect of the reform on educational and labor market outcomes, but also help us in distinguishing between two explanations of low schooling level among women than among men. In the empirical analysis, if I find that the reform has a larger effect on schooling among women than among men, this supports the explanation that higher marginal costs of schooling cause low level of schooling among women than among men. Similarly, finding larger wage returns to schooling among women than men using Instrumental Variables methodology supports the same explanation.

**5. Data**

I use data on exact age along with education and labor force status for a large sample, some of whom were of middle school age before it became mandatory and some after. I use the 2004-2011 releases of the Household Labor Force Surveys conducted by the Turkish Statistical Institute. Each survey covers about 150,000 households and 500,000 individuals annually. The respondents report demographic and detailed labor market characteristics. The original version of the dataset provided age in ranges. This makes it almost impossible to distinguish individuals affected by the reform from those not affected and impedes the evaluation of the reform. I obtained additional files on the exact age and year of birth of respondents from the institute and merged them with the original data in order to evaluate the compulsory schooling reform.[7]

Since the reform happened recently, I restrict my sample to individuals aged 20-29 in any of the survey years from 2004 to 2011 and draw conclusions about early career outcomes.[8] The time frame allows me to examine early-career effects of making middle school mandatory. However, this could create selection problems for two reasons. First, if the reform is expected to affect college education, then some of the treated groups would be still in college and would not have any labor market outcomes. Later, I show that there is no significant effect of the reform on college attendance. Second, I cannot observe individuals who are performing military service. Therefore, I have fewer observations of men than of women aged 20.[9] Yet, this should not introduce any bias since this is the case for all survey years regardless of the reform.[10] Table A3 in the Appendix provides the sample statistics for the main variables, separately for women and men for the baseline sample used in this paper. The gap between men and women in schooling levels, labor force participation (0.83 vs. 0.33) and employment (0.69 vs. 0.26) is evident in the table.[11]

The age variable in the data shows the completed age of individuals. 4 percent of the sample between 20 and 29 is missing year of birth information. I drop those missing year

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[7] I would like to thank the staff in the Labor Force Statistics Department of Turkish Statistical Institute.

[8] Table A2 in the Appendix shows the treated and untreated age-cohort groups.

[9] Military service for male Turkish citizens is mandatory and unless postponed for educational or health purposes, men are recruited at the age of 20.

[10] There would be a selection problem if the reform significantly affected college attendance since college graduates are allowed to postpone the recruitment after the graduation. In other words, the composition of men in the sample would vary across years. As a second check, I also provide the estimation results for the age interval 22-27 at the expense of smaller sample. The results are generally the same.

[11] The low labor force participation among women makes it an interesting margin to examine. Later in the text, I find an insignificant effect of compulsory schooling on women's labor force participation.

of birth and analyze a sample of 574,008 individuals aged 20-29 from survey years 2004-2011.[12] Exposure to the compulsory schooling reform is represented by a binary indicator that takes the value one for those who were born after 1986. The variable for earnings shows the monthly wages and includes overtime work payments and bonuses and is available for only salary workers. The non-response rate for wage information among salary workers is quite small, at 5%.

Among many labor market characteristics, I observe whether or not the individual is in the labor force, employed, self-employed, and registered in the Social Security System. I can also observe the occupation and the economic activity of the respondent, which would not be possible in administrative data, especially for people working in the unregistered economy.

In the last section of empirical findings, I investigate the effect of the compulsory schooling on the tasks that are done by individuals using two cross-walks between three different classification methods. In the survey data occupations are classified using the International Standard Classification of Occupation (ISCO-88). On the other hand, the Dictionary of Occupational Titles (DOT) which is developed by the U.S. Bureau of Labor Statistics assesses the complexity of responsibility and judgment in each occupation regarding the data, people and things, and assigns a value for each of these aspects for any occupation. I match the occupation categories in ISCO-88 to the categories in DOT and evaluate the effect of the reform on the complexity of tasks done

[12] I also have a full set of results for the sample that includes those with missing year of birth variable. I impute the year of birth subtracting age from year of survey, and repeat the estimations for the whole sample. The results are nearly identical. I can provide those upon request.

by individuals. The detailed description of the match and other key variables is provided in the Data Appendix.

## 6. Empirical Strategy

Since I have repeated cross section data on education and labor market outcomes, I will essentially compare individuals in cohorts who are of the same age but in different years, with some cohorts exposed to the compulsory schooling reform and some not. I estimate all relationships separately for men and women. I start by estimating the effect of the reform on educational attainment. Then, I provide the IV estimation equation, and finally I examine potential identification and validity concerns.

### 6.1. Estimation of the Effect of the Reform on Educational Attainment

I estimate the effect of the compulsory schooling reform on educational attainment separately for men and women using the following baseline equation:

$$S_{ijt} = \beta_0 + \vec{X}'_{ijt}\vec{\beta} + \sum_{21}^{29} \alpha_{1j}\, Age_j + \sum_{2005}^{2011} \alpha_{2t}\, Year_t + \theta CSL_{jt} + \varepsilon_{ijt} \quad (5)$$

where $S_{ijt}$ is the education level of individual *i*, at age *j*, in survey year *t* and $CSL_{jt}$ is a binary indicator that takes the value 0 for individuals born in or before 1986 and 1 for those born after 1986. $\vec{X}_{ijt}$ is a vector of individual controls such as year of birth, type of residential area and the region of residence. The year of birth controls for the linear trends in cohorts and the two fixed effects capture residential characteristics that are invariant across individuals. The availability of multiple survey years allows me to distinguish age effects from survey year effects, while controlling linearly for birth year to capture a secular trend that affected birth cohorts differently. The age fixed effects capture characteristics such as life experience that are invariant across survey years. The survey year effects control for time specific characteristics such as labor market conditions that are constant across ages. The random disturbance term $\varepsilon_{ijt}$ captures unobservable individual specific characteristics that affect their school attendance, and it is mean zero independent of all the covariates. Considering the potential group structure of the error terms, I allow standard errors to be correlated within the same year of birth.[13] My strategy identifies the effect of the compulsory schooling reform by comparing the relative differences in schooling across ages before and after compulsory schooling affected some ages.

The estimation strategy in Harmon and Walker (1995) was criticized by Card (1999) and Oreopoulos (2006a) for not including birth cohort effects. This concern is less of a problem in my study. The sample in that paper contains individuals from a wide range of ages and years of birth covering more than fifty years period in total. In contrast, my sample focuses on individuals on a narrow age range. Moreover, besides including a full set of indicators for ages and survey years, I also control linearly for year of birth of individuals.[14] One may still be concerned about non-linear year of birth effects. In order to see the cohort trend in the absence of treatment, I look at the middle school completion ratio for individuals at the age 26, using only untreated years of birth.[15] Figure 1 and

[13] Clustering at age or survey year or both does not change the estimation results significantly.
[14] O'Brien (2000) shows that including full set of indicators for ages and survey years effectively controls for linear trends in year of birth. Yet, I already have the year of birth data and it varies within the same age and survey year, breaking the perfect collinearity with age and survey year indicators. Therefore I also include year of birth among control variables.
[15] The fact that observations are obtained from different survey years should not affect the calculated middle school completion ratio since the definition of middle school degree is quite clear and invariant across survey years.

Figure 2 show that the secular trend is linear, alleviating concerns about nonlinear cohort differences.[16]

While the first stage estimation is a necessary step for estimating labor market returns to schooling, it is also informative as a policy evaluation. The estimates show the overall effect of the policy across genders and residential areas. It also demonstrates whether completing middle school may induce some people to continue on to high school.

### 6.2. An Instrumental Variables Analysis of Returns to Compulsory Schooling

The theoretical model in Section 3 demonstrated that schooling choices are endogenous, depending on individual returns to schooling. I use exposure to the compulsory schooling law as an instrument to estimate the Local Average Treatment Effect of schooling. The 2SLS equation becomes

$$Y_{ijt} = \gamma_0 + \vec{X}'_{ijt}\vec{\gamma} + \sum_{21}^{29} \vartheta_{1j}\, Age_j + \sum_{2005}^{2011} \vartheta_{2t}\, Year_t + \delta \hat{S}_{ijt} + \epsilon_{ijt}, \qquad (6)$$

where the excluded instrument from the Equation 6 is a binary indicator that takes the value 0 for individuals born in or before 1986 and 1 for those born after 1986, and $Y_{ijt}$ is the outcome of interest such as log wage. The random disturbance $\epsilon_{ijt}$ includes unobservable individual characteristics that affect their earnings, and it is assumed to have zero conditional mean. The estimated value of $\delta$ can be interpreted as the causal effect of education.

### 6.3. Identification and Validity Concerns

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[16] Similarly, when I look at the middle school completion ratio for individuals aged 20-29 using an untreated survey year I observe a linear trend across untreated years of birth. In this case, the fact that observations are obtained from different ages should not matter for middle school completion ratios since all individuals are above 20.

My identification strategy relies on comparing the labor market outcomes of individuals who were born before and those who were born after the mandatory middle-school cutoff date, controlling for age and survey year effects and for a linear birth cohort trend. I show later that the compulsory schooling reform in Turkey had a strong effect on educational attainment after controlling for other factors, so the instrument has the power to help explain the endogenous variable.

I also require that the compulsory schooling reform not be correlated with labor market outcomes through other channels beyond educational attainment. To my knowledge, there is no other policy change which affects only individuals who were born after 1986. Yet, the mandate for middle school completion may have had further effects, as it led to the merger of small schools and to the use of double-shift schooling.[17] Such factors may have altered school quality in either direction, but without additional data I will treat years of schooling as a homogenous treatment. Gulesci and Myerson (2012) show that the compulsory schooling reform had negative effects on the expression of religiosity among ever-married women.[18] This should be considered as a part of the treatment as well.

In order to test whether the reform is correlated with wages in ways other than through schooling, I try a placebo test, in which I estimate the effect of the reform on earnings of individuals with college degree (and I show later that college attainment was not affected by the law). In Table A4 in the Appendix, I do not find any effect of

[17] Double-shift schooling refers to the school system with two shifts. One group of students use the classrooms in the morning and another group of students use in the afternoon. A higher number of students can be taught in the same school building.

[18] The authors claim that there is not selection into the sample of ever-married women since the likelihood of getting married was not affected by an additional year of schooling among women. I also find that schooling reform did not have a significant effect on women's marital status.

mandatory middle school on college graduate wages of men. I do find a small negative effect for women, which might lead to a downward bias of my estimated effect of schooling on women's wages if some factor other than mandatory middle school changed the wages of all women during the treatment period.[19]

Since the reform in Turkey affected mostly those who drop out after primary school and partially those who drop out after middle school, the IV will recover the local average treatment effect among these groups. Since almost half of the female population and a quarter of the male population were affected in Turkey, the LATE estimated in this paper may be close to the ATE of schooling, especially for the sample of women (Oreopoulos, 2006a).

## 7. Results

In this section, I first present the estimated impact of the reform on different margins of educational attainment for women and men separately. This sheds light on the effectiveness of the reform for different subgroups. Second, I present the IV estimates of the returns to schooling. Besides the estimated effects on monthly earnings, I show the estimated effect on other labor market outcomes such as employment likelihood and social security coverage. Similarly, I present the estimated effect of the reform on occupational choice using a multinomial logit, as well as the estimated effect on occupational tasks done by individuals.

### 7.1. The Effect of the Reform on Educational Attainment

[19] This could reflect the general equilibrium effects college graduate women's wages. But an increase in the number of middle school graduate women, who are a small part of the total labor force, is unlikely to affect college graduates' wages.

Figure 3 illustrates the jump in middle school completion rates for cohorts born after 1986 using the sample of individuals aged 20-29 from all survey years. Although the middle school completion ratio trends upward for both women and men throughout the period, it jumps after the year 1986. Over three years, the fraction of individuals with no middle school degree fell from about 45% to 20% among women and from about 20% to 5% among men. The shrinking of the educational gap between men and women after the reform is also evident.

Table 2 reports the estimated effects from the compulsory schooling reform. The samples consist of men and women aged 20-29 in the first row, and aged 22-27 in the second row. The effect of mandatory middle school is sizable, with an increase of around 0.60 years of education among women and about 0.40 years of education among men. This effect corresponds to 0.15 - 0.20 years increase per each year mandated, very similar to what Oreopoulos (2006b) finds in Canada.[20] Note that the results are very similar the broader or narrower age ranges that exclude individuals around the military conscription age. In Table A5 I repeat the estimations splitting the sample and show that the effect on schooling was slightly more prominent in rural areas.

The reform also raised the rate of middle school degree attainment by 17 percentage points (37%) for women and 11 percentage points (16%) for men. Recall Proposition 1, which states that, when women have higher marginal cost of schooling than men, the reform leads to bigger gains in schooling for women, and Proposition 3 states that when women have lower returns to schooling than men, the reform leads to equal changes in

[20] Oreopoulos (2006b) finds that a one-year increase in the number of mandatory school years is associated with a 0.18 increase in average grade attainment.

schooling for both. These estimates support Proposition 1 rather than Proposition 3 and suggest higher marginal costs of schooling for women than men. Much smaller increases in high school completion are estimated (3-4 percentage points). I neither observe nor expect any effect on postsecondary education.[21] Therefore, I drop from the sample those who were in college or who had graduated from college when surveyed in my later estimates. This makes the treatment and control groups comparable in terms of educational attainment. It also prevents potential bias that may arise from some unobserved changes in the behavior of college graduates.

### 7.2. The Effect of Compulsory Schooling on Earnings and Social Security Coverage

Table 3 shows IV estimates of the wage returns to education, building on the first-stage estimates from above. Panel 1 shows that an additional year of education due to the compulsory schooling reform increases monthly earnings by about 9-10% among women. The estimated effect among men is around zero and is statistically insignificant.[22] The higher estimated return in the sample of women is in line with expectations motivated by the theoretical framework and knowledge of the baseline institutional characteristics in Turkey. Proposition 2 stated that, when women have higher marginal cost of schooling than men, estimation that uses this policy as an instrumental variable leads to higher estimated effects among women than among men, and Proposition 4 states that when women have lower returns to schooling than men, the

[21] Since some observations in my sample are still enrolled in college, I estimate the effect of the reform on college enrollment and college graduation separately. I interpret the former as the upper bound since some individuals will drop out, and interpret the latter as the lower bound. Table A6 in the Appendix shows that all estimates are concentrated around zero, with negative lower bounds and positive upper bounds.

[22] Table A7 in the Appendix presents the estimated returns to schooling for sample of individuals from urban areas as well. In all samples women have higher returns to schooling than men, and returns to schooling among men are around 0-2 % and statistically insignificant.

estimated effects will be lower among women. The evidence supports Proposition 2 rather than Proposition 4, suggesting, as above, that women face higher marginal costs of schooling.

Next, I investigate the effect of compulsory schooling at other margins. I find that compulsory schooling does not significantly increase the likelihood of being employed and has only a small effect on the labor force participation men. Pischke and von Wachter (2008) and Grenet (2012) fail to find any effect of compulsory schooling reforms on the probability of being employed in Germany and France respectively. This result mitigates the concerns, especially for women, that earning effects are confounded by the changing composition of working individuals.

In Panel 1 of Table 4, among those who are employed, I find a slight increase in the probability of being self-employed in the sample of men and an extremely small decrease for women. Choosing to be self-employed may be a response to not finding better paying salary jobs in the market. Since I do not observe the earning information for self-employed individuals, I cannot include them in the earnings estimation. Yet, it is very unlikely that this small self-employment effect drives the gap in earning effects between women and men.

Being covered by Social Security is another mechanism through which the compulsory schooling reform can benefit individuals, especially in an economy with substantial unregistered employment.[23] All Turkish employees working under an employment contract and self-employed people must be registered in Social Security

[23] Unregistered employment constitutes 40% of the total employment in June 2012 (Turkstat).

System and pay the premiums. In return they receive health care and contribute to their retirement plan. Panel 2 in Table 4 shows that, conditional on being employed, an additional year of schooling increases the probability of Social Security coverage by 2-3 percentage points.[24] In results that are not shown, I observe that these gains are more pronounced among urban residents. This is the first set of results showing an effect of compulsory schooling on formal sector participation.

### 7.3. The Effect of Compulsory Schooling on Occupations and Economic Activities

Focusing on the sample of women who work for a salary, I examine occupational choice and job characteristics to provide an additional view on the mechanism of adjustment. This is the sample that has big earning gains from the reform, and it corresponds to 55% of employed non-college bound women aged 20-29. Previous research on the returns to compulsory schooling did not consider such effects.

First, I investigate women's occupations using a linear probability model in an Instrumental Variables framework. Respondents who are employed are assigned one of the nine one-digit ISCO-88 occupation categories. Table A8 in the Appendix shows the occupation categories and their percentage shares among working men and women. I create binary indicators for each occupation category, and examine the effect of the reform on the probabilities of choosing each occupation separately. In the first panel of Table 5, I find significant negative effects of an additional year of schooling on being in the occupation of craft and related trade workers (-2.8 pp. or -28%) and plant and machine operators and assemblers (-2.2 pp. or 18%). I observe significant positive effects

[24] Although the estimates are consistently positive across samples, the estimates are statistically insignificant in two of them. The estimated effect in the sample of men aged 22-27, and the estimated effect in the sample of women aged 20-29 are statistically insignificant.

on the occupation category of technicians and associate professionals (3.7 pp. or 28%), and insignificant positive effects on the occupation category of service workers. In order to allow for the interdependency of choices among different occupations, I also estimate the effect of the reform using a multinomial logit model. I present the average marginal effect of the reform on the probability of choosing a certain occupation. Overall, the results are similar, with slightly lower values in magnitude for the multinomial logit than for the separate linear regressions. Panel 2 in Table 5 shows that individuals who were subject to the reform are less likely to work as craft and related trade workers (-2.2 pp. or 22%) or plant and machine operators (-1.3 pp. or 10%), and more likely to work as technicians and associate professionals (3 pp. or 22%).[25]

Next, I examine the effect of the reform on the industrial sector of individuals. Each respondent is assigned one of the nine sectoral categories in the survey. Table A9 shows the economic activities and their percentage shares among working women and men. In Table 6, Panel 1 shows the OLS, and Panel 2 shows the multinomial logit model, which is similar but with lower magnitudes. Allowing for interdependencies among different economic activities, the compulsory schooling reform decreases the probability of working in the manufacturing sector by 4.2 percentage points (12%) and increases the probability of working in community, social and personal services by 4.7 percentage points (23%) among women working for salary.

Next, I investigate the effect of compulsory schooling on the likelihood of working in the public sector. Public sector jobs offer more certainty about benefits such as social

[25] Note that the results from linear probability model show the effect of an additional year of schooling in an IV framework whereas the results from multinomial logit regression show the effect of the reform.

security and retirement, more job security, and better work conditions than many private jobs. Only in the 2009-2011 survey years are respondents asked whether they work in the public sector, private sector, or neither. Using a LPM in an IV framework, Table 7 shows that an additional year of schooling increases the likelihood of working in the public sector by around 3 percentage points among the non-college bound women working for salary.

In the rest of this section, I examine the effect of an additional year of schooling on the tasks done by individuals. Dictionary of Occupational Titles (DOT) provides information on the task requirements of occupations in relation to data, people and things. Each occupation is assigned a number for each of these groups, with lower numbers for occupations that require more complicated tasks and higher numbers for those requiring less complicated tasks, as demonstrated in Table 8.[26] Using two crosswalks, I obtain three numbers representing the task requirements of occupation categories regarding data, people and things.

Table 9 presents the IV estimates of the effect of the compulsory schooling on tasks for the same sample of women working for salary. A negative estimate implies that women who receive more schooling due to the compulsory schooling reform work in occupations that require more complicated responsibilities. The estimates are all negative in both samples, and statistically significant for the tasks regarding people (0.09 or 1.5%).

**8. Conclusion**

[26] In order to obtain task composition for each two-digit ISCO occupation category given in the survey data, I exploit two crosswalks. The first one matches DOT occupation categories to SOC 2000, and the second matches SOC 2000 categories to ISCO 88. The details of the match are provided in the Data Appendix.

Using a recent change in compulsory schooling law in Turkey, I make three main contributions to the returns to schooling literature. First, unlike most of the previous research, I investigate the effect of schooling at the middle school margin in the relatively understudied group of middle income countries. Therefore, the findings from this paper are more relevant for many middle income countries considering similar policy changes than are the findings from high income countries such as the U.S. and the U.K. Second, I examine early-career effects of additional schooling. This is not possible for many other countries that had compulsory schooling reforms in the early or middle twentieth century when data were not available. Third, exploiting the richness of the data, I examine additional outcomes such as employment status, self-employment, and social security coverage. To my knowledge, this is the first paper that investigates job choice and tasks done at jobs as potential mechanisms through which earning gains occur after a compulsory schooling reform.

I find that an additional year of compulsory schooling increases monthly earnings by about 9-10% among non-college bound women and around 0-2% among non-college bound men. I do not find significant effect on employment likelihood for either gender. Yet, I find positive significant gains in social security participation from the compulsory schooling reform for both men and women, implying a decline in the unregistered economy. I also find that women who worked were significantly more likely to move into higher skill and formal jobs after the reform. They are also more likely to carry out occupations that require more complicated tasks in relation to data, people and things.

Given the relatively high returns to schooling among women, one may ask why so many girls drop out after the elementary school. One common explanation points to credit constraints. Although this might be part of the explanation, there is little reason to assume that credit constraints are more serious for girls than boys. Another explanation that fits the evidence provided above rests on the costs associated with schooling. In Turkey, it is commonly accepted that social and psychological cost associated with girls' schooling are higher than with boys' schooling. Considering the reform as a cost shifter, we can argue that it was welfare increasing for individuals who would have dropped out in the absence of the reform. On the other hand, if we consider the reform as a pure enforcement, then the reform will not be welfare increasing since it simply requires the social costs to be borne by the girls or their families. Once we consider the intra-household bargaining, girls who wanted to go to school (those who receive less education than optimal) but were not allowed by more powerful parents may benefit from the reform even under the pure enforcement scenario.

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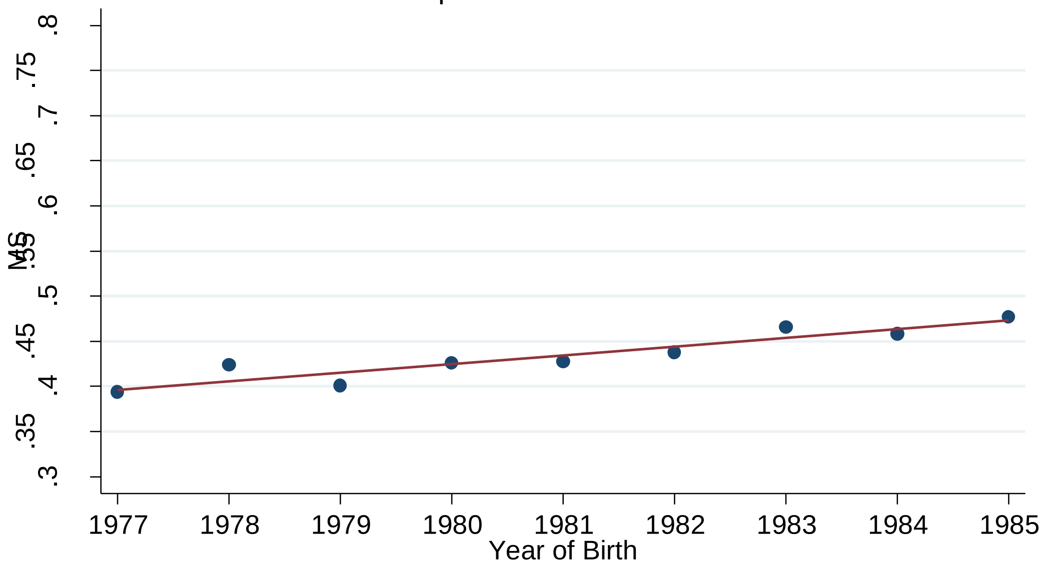
Figure 1

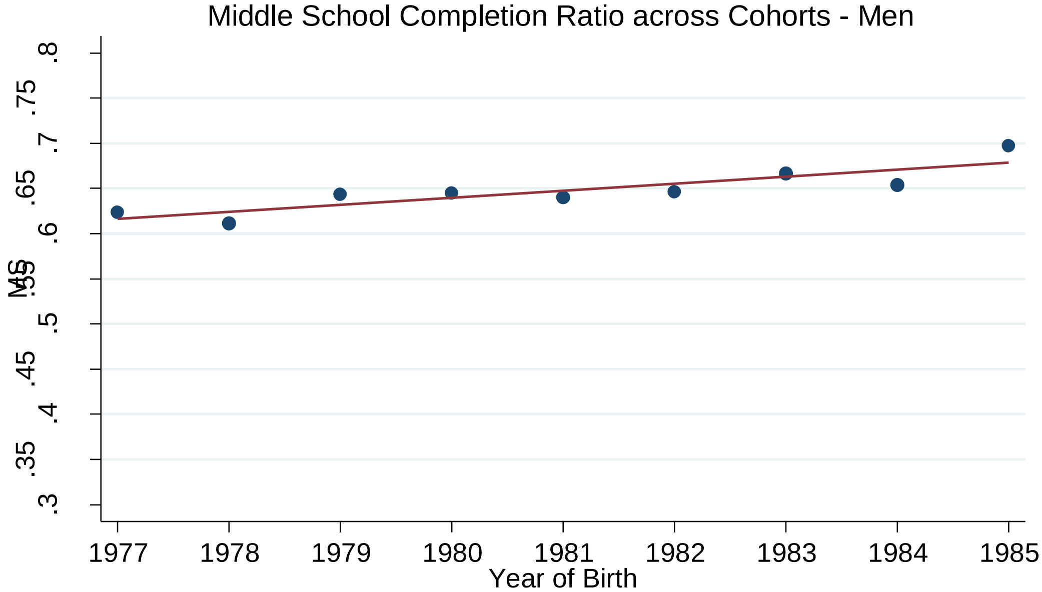
Middle School Completion Ratio across Cohorts - Women




The data come from survey years 2004-2011.
It compares MS completion ratio across cohorts for individuals aged 26.
No observation in this sample is affected by the law.

Figure 2

Middle School Completion Ratio across Cohorts - Men




The data come from survey years 2004-2011.
It compares MS completion ratio across cohorts for individuals aged 26.
No observation in this sample is affected by the law.

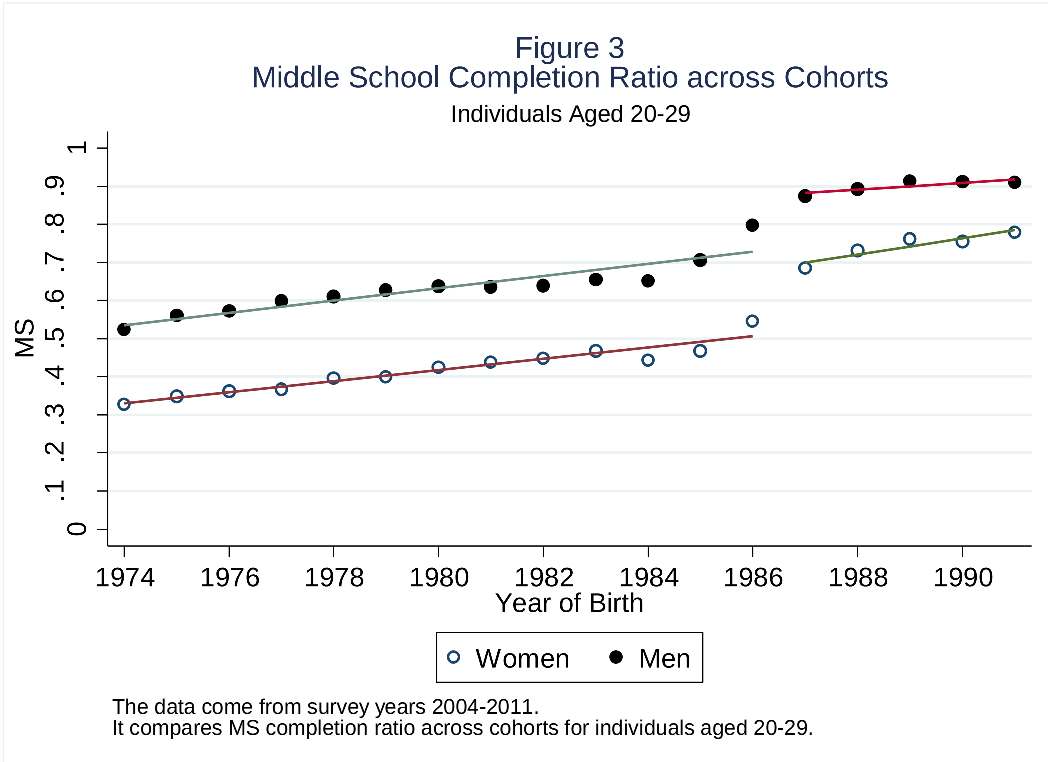


Figure 3
Middle School Completion Ratio across Cohorts
Individuals Aged 20-29
MS
0
.1
.2
.3
.4
.5
.6
.7
.8
.9
1
1974
1976
1978
1980
1982
1984
1986
1988
1990
Year of Birth
Women
Men
The data come from survey years 2004-2011.
It compares MS completion ratio across cohorts for individuals aged 20-29.

**Table 1 – Optimal Schooling Levels when Average Marginal Cost is Higher among Women**

**Women**

| | | | | |
|---|---|---|---|---|
| **Characteristics** | $b_1, \eta_1, \bar{r}_w$ | $b_2, \eta_2, \bar{r}_w$ | $b_3, \eta_3, \bar{r}_w$ | $b_4, \eta_4, \bar{r}_w$ |
| **Optimal Schooling** | **<u>1</u>** | **<u>2</u>** | 3 | 4 |

**Men**

| | | | | |
|---|---|---|---|---|
| **Characteristics** | $b_1, \eta_1, \bar{r}_m$ | $b_2, \eta_2, \bar{r}_m$ | $b_3, \eta_3, \bar{r}_m$ | $b_4, \eta_4, \bar{r}_m$ |
| **Optimal Schooling** | **<u>2</u>** | 3 | 4 | 5 |

*Note:* Marginal returns to schooling across subgroups can be ranked as $b_1 < b_2 < b_3 < b_4$ and marginal costs of schooling across subgroups can be ranked as $\eta_1 > \eta_2 > \eta_3 > \eta_4$. Also note that $\bar{r}_w > \bar{r}_m$. The estimated return is LATE for the underlined subgroups in each sample, and it gives higher IV estimate for women.

**Table 2 - The Estimated Effect of Compulsory Schooling Reform on Schooling**

| | Women | | | Men | | |
|---|---|---|---|---|---|---|
| | Years of Education | Middle School Degree | High School Degree | Years of Education | Middle School Degree | High School Degree |
| All individuals aged 20-29. | 0.611*** | 0.175*** | 0.0402*** | 0.387*** | 0.107*** | 0.0316** |
| | (0.0844) | (0.0240) | (0.00801) | (0.121) | (0.0301) | (0.0118) |
| Number of observation | 310,226 | 310,226 | 310,226 | 263,782 | 263,782 | 263,782 |
| All individuals aged 22-27. | 0.672*** | 0.170*** | 0.0542*** | 0.448*** | 0.122*** | 0.0317** |
| | (0.124) | (0.0303) | (0.0126) | (0.146) | (0.0379) | (0.0140) |
| Number of observation | 187,665 | 187,665 | 187,665 | 168,437 | 168,437 | 168,437 |

*Note:* Each cell shows the estimated coefficient for the indicator of exposure to compulsory schooling reform. The dependent variables are given at the top of each column. The middle school and high school degrees are defined as binary indicators in a cumulative way. All estimations control for survey year, age, and region fixed effects, an indicator for urban areas, and year of birth trends. Standard errors are shown in the parentheses, clustered at the year of birth level.

**Table 3 - Earnings and Social Security Returns to Compulsory Schooling (IV Estimates)**

| | Women | Men |
|---|---|---|
| *Panel 1 - Returns to Compulsory Schooling (D.V.: Log Monthly Earnings)* | | |
| All individuals aged 20-29 who are not college enrollees or college graduates and work for salary. | 0.098*** | 0.017 |
| | (0.014) | (0.015) |
| Number of observation | 30,622 | 103,629 |
| All individuals aged 22-27 who are not college enrollees or college graduates and work for salary. | 0.102*** | -0.001 |
| | (0.018) | (0.015) |
| Number of observation | 18,445 | 69,239 |
| *Panel 2- Labor Force Participation Effects of Compulsory Schooling* | | |
| All individuals aged 20-29 who are not college enrollees or college graduates. | 0.006 | 0.021* |
| | (0.005) | (0.012) |
| Number of observation | 260,140 | 207,495 |
| All individuals aged 22-27 who are not college enrollees or college graduates. | 0.006 | 0.012* |
| | (0.006) | (0.007) |
| Number of observation | 156,062 | 133,965 |
| *Panel 3- Employment Effects of Compulsory Schooling* | | |
| All individuals aged 20-29 who are not college enrollees or college graduates. | 0.006 | 0.014 |
| | (0.005) | (0.018) |
| Number of observation | 260,140 | 207,495 |
| All individuals aged 22-27 who are not college enrollees or college graduates. | 0.007 | 0.004 |
| | (0.007) | (0.015) |
| Number of observation | 156,062 | 133,965 |

*Note:* Each cell shows the effect of one year of additional schooling on the dependent variable given at the panel title. The sample specifications are given at the beginning of each row and the sample sizes are provided beneath the sample description. All samples exclude college graduates or college enrollees. All estimations control for survey year, age, and region fixed effects, an indicator for urban areas, and year of birth trends. Standard errors are shown in the parentheses, clustered at the year of birth level.

**Table 4 - Self Employment and Social Security Coverage Effects of Compulsory Schooling (IV Estimates)**

| | Women | Men |
|---|---|---|
| *Panel 1 - The Self Employment Effects of Compulsory Schooling (Conditional on Being Employed)* | | |
| All individuals aged 20-29 who are not college enrollees or college graduates and employed. | -0.005 | 0.038*** |
| | (0.003) | (0.011) |
| Number of observation | 59,327 | 153,727 |
| All individuals aged 22-27 who are not college enrollees or college graduates and employed. | -0.012** | 0.034*** |
| | (0.006) | (0.012) |
| Number of observation | 35,425 | 101,503 |
| *Panel 2 - The Social Security Effects of Compulsory Schooling (Conditional on Being Employed)* | | |
| All individuals aged 20-29 who are not college enrollees or college graduates and employed. | 0.006 | 0.033* |
| | (0.007) | (0.017) |
| Number of observation | 59,327 | 153,727 |
| All individuals aged 22-27 who are not college enrollees or college graduates and employed. | 0.020** | 0.014 |
| | (0.009) | (0.018) |
| Number of observation | 35,425 | 101,503 |

*Note*: Each cell shows the effect of one year of additional education on the dependent variable given at the panel title. The sample specifications are given at the beginning of each row and the sample sizes are provided beneath the sample description. All samples exclude college graduates or college enrollees. All estimations control for survey year, age, and region fixed effects, an indicator for urban areas, and year of birth trends. Standard errors are shown in the parentheses, clustered at the year of birth level.

**Table 5 - The Effect of the Reform on Occupational Choice**

| Technicians, Associate Professionals | Clerks | Service Workers, Sales Workers | Skilled Agricultural and Fishery Workers | Craft and related Trade Workers | Plant & Machine Operators, Assemblers | Elementary Occupations |
|---|---|---|---|---|---|---|
| *Panel 1- The Effect of the Schooling on Occupations (IV Estimates- Women Working for Salary)* | | | | | | |
| 0.037*** | 0.005 | 0.019 | -0.003 | -0.028*** | -0.023* | -0.002 |
| (0.011) | (0.017) | (0.015) | (0.002) | (0.011) | (0.012) | (0.012) |
| 33,301 | 33,301 | 33,301 | 33,301 | 33,301 | 33,301 | 33,301 |
| *Panel 2- The Effect of the Reform on Occupations (IV Estimates- Women Working for Salary) Average Marginal Effects from Multinomial Logit Model* | | | | | | |
| 0.030*** | 0.007 | 0.006 | -0.002 | -0.023** | -0.013 | 0.005 |
| 0.011 | 0.013 | 0.011 | 0.002 | 0.010 | 0.012 | 0.011 |
| 33,301 | 33,301 | 33,301 | 33,301 | 33,301 | 33,301 | 33,301 |

*Note:* In Panel 1, each cell shows the estimated effect of an additional year of compulsory schooling on the probability of working in the occupation given at the top of each column using Linear Probability Model in an IV framework. In Panel 2, each cell shows the estimated effect of the reform on the probability of working in the occupation given at the top of each column using multinomial logit estimation. All estimations are conditional on being a salary worker. The sample includes non-college bound women aged 20-29 from survey years 2004-2011, working for salary. All estimations control for survey year, age, and region fixed effects, an indicator for urban areas, and year of birth trends. Standard errors are shown in the parentheses, clustered at the year of birth level.

**Table 6 - The Effect of the Reform on Economic Activities**

| Agriculture, Forestry | Manufacturing | Construction | Trade, Restaurants and Hotels | Transportation, Communication, and Storage | Finance, Real Estate, Business Services | Community, Social and Personal Sevices |
|---|---|---|---|---|---|---|
| *Panel 1- The Effect of the Schooling on Economic Activities (IV Estimates- Women Working for Salary)* | | | | | | |
| -0.004 | -0.062*** | -0.002 | 0.007 | 0.001 | 0.001 | 0.059*** |
| (0.008) | (0.015) | (0.003) | (0.018) | (0.004) | (0.013) | (0.009) |
| 33,301 | 33,301 | 33,301 | 33,301 | 33,301 | 33,301 | 33,301 |
| *Panel 2- The Effect of the Reform on Economic Activities (IV Estimates- Women Working for Salary) Average Marginal Effects from Multinomial Logit Model* | | | | | | |
| -0.002 | -0.042*** | -0.002 | 0.003 | 0.001 | 0.004 | 0.048*** |
| 0.006 | 0.016 | 0.002 | 0.014 | 0.004 | 0.009 | 0.009 |
| 33,301 | 33,301 | 33,301 | 33,301 | 33,301 | 33,301 | 33,301 |

*Note:* In Panel 1, each cell shows the estimated effect of an additional year of compulsory schooling on the probability of working in the economic activity given at the top of each column using Linear Probability Model in an IV framework. In Panel 2, each cell shows the estimated effect of the reform on the probability of working in the economic activity given at the top of each column using multinomial logit estimation. All estimations are conditional on being a salary worker. The sample includes non-college bound women aged 20-29 from survey years 2004-2011, working for salary. All estimations control for survey year, age, and region fixed effects, an indicator for urban areas, and year of birth trends. Standard errors are shown in the parentheses, clustered at the year of birth level.

**Table 7 -The Effect of Compulsory Schooling on the Likelihood of Working in the Public Sector (Women-IV Estimates)**

| | |
|---|---|
| All women aged 20-29 who are not college enrollees or college graduates and working for salary. | 0.036*** |
| | (0.014) |
| Number of observation | 13,051 |
| All women aged 22-27 who are not college enrollees or college graduates and working for salary. | 0.024* |
| | (0.014) |
| Number of observation | 7,747 |

*Note:* The estimate shows the effect of an additional year of compulsory school on the probability of working in the public sector.

**Table 8 - Tasks Performed in Occupations - Dictionary of Occupational Titles-1991**

| Data | People | Things |
|---|---|---|
| 0 Synthesizing | 0 Mentoring | 0 Setting Up |
| 1 Coordinating | 1 Negotiating | 1 Precision Working |
| 2 Analyzing | 2 Instructing | 2 Operating-Controlling |
| 3 Compiling | 3 Supervising | 3 Driving-Operating |
| 4 Computing | 4 Diverting | 4 Manipulating |
| 5 Copying | 5 Persuading | 5 Tending |
| 6 Comparing | 6 Speaking-Signaling | 6 Feeding-Off bearing |
| | 7 Serving | 7 Handling |
| | 8 Taking Instructions-Helping | |

*Note:* As a general rule, worker functions involving more complex responsibility and judgment are assigned lower numbers in these three lists while functions which are less complicated have higher numbers (US Department of Labor).

**Table 9 - The Effect of the Schooling on Tasks (Women - IV estimates)**

| | Data | People | Things |
|---|---|---|---|
| All individuals aged 20-29 who are not college enrollees or college graduates and work for salary. | -0.093 | -0.096* | -0.041 |
| | (0.069) | (0.054) | (0.028) |
| Number of observation | 13,051 | 13,051 | 13,051 |
| All individuals aged 22-27 who are not college enrollees or college graduates and work for salary. | -0.067 | -0.112** | 0.024 |
| | (0.076) | (0.044) | (0.054) |
| Number of observation | 7,747 | 7,747 | 7,747 |

*Note:* Each cell shows the estimated effect of an additional year of compulsory schooling on task requirement of the occupations done by individuals regarding the data, people and things. All estimations are conditional on being a salary worker. The sample includes non-college bound women aged 20-29 from survey years 2004-2011, working for salary. All estimations control for survey year, age, and region fixed effects, an indicator for urban areas, and year of birth trends. Standard errors are shown in the parentheses, clustered at the year of birth level.

## Appendix A: Tables

**Table A1 – Optimal Schooling Levels when Average Marginal Return is Lower among Women**

**Women**

| | | | | |
|---|---|---|---|---|
| **Characteristics** | $b_{1w}, \eta_1, \bar{r}$ | $b_{2w}, \eta_2, \bar{r}$ | $b_{3w}, \eta_3, \bar{r}$ | $b_{4w}, \eta_4, \bar{r}$ |
| **Optimal Schooling** | $\underline{1}$ | $\underline{2}$ | 3 | 4 |

**Men**

| | | | | |
|---|---|---|---|---|
| **Characteristics** | $b_{1m}, \eta_1, \bar{r}$ | $b_{2m}, \eta_2, \bar{r}$ | $b_{3m}, \eta_3, \bar{r}$ | $b_{4m}, \eta_4, \bar{r}$ |
| **Optimal Schooling** | $\underline{2}$ | 3 | 4 | 5 |

*Note:* Marginal returns to schooling across subgroups can be ranked as $b_{1i} < b_{2i} < b_{3i} < b_{4i}$ for I = w, m. Marginal costs of schooling across subgroups can be ranked as $\eta_1 > \eta_2 > \eta_3 > \eta_4$. Also note that $\bar{r}_w = \bar{r}_m = \bar{r}$. In this figure, average marginal return to schooling among women is lower than among men. Since everything else is the same across genders, note that by construction men with 2 units of schooling should have bigger marginal return than women with 2 units of schooling. So $b_{1w} < b_{2w} < b_{1m}$ should be the case. The estimated return is LATE for the underlined subgroups in each sample, and it gives a higher IV estimate for men under the assumption of lower average marginal returns among women.

**Table A2 - The Cohorts Affected by the Law**

| | 20/21 | 21/22 | 22/23 | 23/24 | 24/25 | 25/26 | 26/27 | 27/28 | 28/29 | 29/30 |
|---|---|---|---|---|---|---|---|---|---|---|
| **2004** | 1983 | 1982 | 1981 | 1980 | 1979 | 1978 | 1977 | 1976 | 1975 | 1974 |
| **2005** | 1984 | 1983 | 1982 | 1981 | 1980 | 1979 | 1978 | 1977 | 1976 | 1975 |
| **2006** | 1985 | 1984 | 1983 | 1982 | 1981 | 1980 | 1979 | 1978 | 1977 | 1976 |
| **2007** | 1986 | 1985 | 1984 | 1983 | 1982 | 1981 | 1980 | 1979 | 1978 | 1977 |
| **2008** | 1987 | 1986 | 1985 | 1984 | 1983 | 1982 | 1981 | 1980 | 1979 | 1978 |
| **2009** | 1988 | 1987 | 1986 | 1985 | 1984 | 1983 | 1982 | 1981 | 1980 | 1979 |
| **2010** | 1989 | 1988 | 1987 | 1986 | 1985 | 1984 | 1983 | 1982 | 1981 | 1980 |
| **2011** | 1990 | 1989 | 1988 | 1987 | 1986 | 1985 | 1984 | 1983 | 1982 | 1981 |

Note: The shaded cells are affected by the Compulsory Schooling Law. Each year of birth may correspond to two ages in each survey year depending on whether the survey month is past the month of birth.

**Table A3 - Descriptive Statistics for the Sample of Individuals between 20 and 29**

| | **Women** | | **Men** | |
|---|---|---|---|---|
| | Mean | S.D. | Mean | S.D. |
| **Age** | 24.54 | 2.86 | 24.76 | 2.76 |
| **% Less than Middle School** | 0.49 | 0.50 | 0.29 | 0.46 |
| **% Middle School Only** | 0.13 | 0.33 | 0.19 | 0.39 |
| **% High School Only** | 0.26 | 0.44 | 0.38 | 0.49 |
| **% College and above** | 0.12 | 0.33 | 0.14 | 0.35 |
| **% in Labor Force** | 0.33 | 0.47 | 0.83 | 0.38 |
| **% Employed** | 0.26 | 0.44 | 0.69 | 0.46 |
| **Log of Monthly Wages** | 6.40 | 0.58 | 6.39 | 0.52 |
| **Social Security Coverage** | 0.53 | 0.50 | 0.61 | 0.49 |
| **Self-employed** | 0.06 | 0.24 | 0.15 | 0.36 |
| **Number of Observations** | 310226 or 49592 or 79987 | | 263782 or 124348 or 178702 | |

*Note:* The sample contains individuals from all survey years 2004-2011 aged 20-29 (310226 for women, 263782 for men). Degree attainments are defined in an exclusive way. Average wages are calculated among the salary workers who are not enrolled in postsecondary education (49592 for women, 124348 for men), and social security coverage and average self-employment rate is calculated among those who are employed and not enrolled in postsecondary education (79987 for women, 178702 for men). The rest of the mean values are calculated among the whole sample.

**Table A4 - Wage effects of the Reform for College Graduates**

| | Women | Men |
|---|---|---|
| All individuals aged 20-29 who have college degree. | -0.035*** | 0.004 |
| | (0.011) | (0.023) |
| Number of observation | 18,970 | 20,719 |
| All individuals aged 22-27 who have college degree. | -0.035* | -0.013 |
| | (0.019) | (0.017) |
| Number of observation | 13,068 | 13,220 |

*Note:* Each cell shows the effect of the compulsory schooling reform on log monthly earnings among college graduate individuals. All estimations control for survey year, age, and region fixed effects, an indicator for urban areas, and year of birth trends. Standard errors are shown in the parentheses, clustered at the year of birth level.

**Table A5 - The Estimated Effect of Compulsory Schooling Reform on Schooling**

| | Women | | | Men | | |
|---|---|---|---|---|---|---|
| | Years of Education | Middle School Degree | High School Degree | Years of Education | Middle School Degree | High School Degree |
| All individuals aged 20-29. | 0.611*** | 0.175*** | 0.040*** | 0.387*** | 0.107*** | 0.0316** |
| | (0.084) | (0.024) | (0.008) | (0.121) | (0.030) | (0.012) |
| Number of observation | 310,226 | 310,226 | 310,226 | 263,782 | 263,782 | 263,782 |
| All individuals from urban areas aged 20-29. | 0.518*** | 0.150*** | 0.040*** | 0.328** | 0.092*** | 0.029** |
| | (0.087) | (0.022) | (0.008) | (0.119) | (0.028) | (0.013) |
| Number of observation | 224,734 | 224,734 | 224,734 | 194,220 | 194,220 | 194,220 |
| All individuals aged 22-27. | 0.672*** | 0.170*** | 0.054*** | 0.448*** | 0.122*** | 0.032** |
| | (0.124) | (0.030) | (0.013) | (0.146) | (0.038) | (0.014) |
| Number of observation | 187,665 | 187,665 | 187,665 | 168,437 | 168,437 | 168,437 |
| All individuals from urban areas aged 22-27. | 0.588*** | 0.151*** | 0.053*** | 0.442*** | 0.113*** | 0.038** |
| | (0.115) | (0.027) | (0.010) | (0.146) | (0.036) | (0.016) |
| Number of observation | 136,590 | 136,590 | 136,590 | 123,265 | 123,265 | 123,265 |

*Note:* Each cell shows the estimated effect of compulsory schooling reform. The dependent variables are given at the top of each column. The middle school and high school degrees are defined as binary indicators in a cumulative way. All estimations control for survey year, age, and region fixed effects, an indicator for urban areas, and year of birth trends. Standard errors are shown in the parentheses, clustered at the year of birth level.

**Table A6 - The Estimated Effect of Compulsory Schooling Reform on College Degree**

| | Women | | Men | |
|---|---|---|---|---|
| | College Degree | College Enrollment | College Degree | College Enrollment |
| All individuals aged 20-29. | -0.009 | 0.010* | -0.007* | 0.018*** |
| | (0.006) | (0.006) | (0.004) | (0.005) |
| Number of observation | 310,226 | 310,226 | 263,782 | 263,782 |
| All individuals from urban areas aged 20-29. | -0.013* | 0.009 | -0.009* | 0.018*** |
| | (0.007) | (0.007) | (0.004) | (0.005) |
| Number of observation | 224,734 | 224,734 | 194,220 | 194,220 |
| All individuals aged 22-27. | -0.001 | 0.011** | -0.003 | 0.016*** |
| | (0.004) | (0.008) | (0.004) | (0.004) |
| Number of observation | 187,665 | 187,665 | 168,437 | 168,437 |
| All individuals from urban areas aged 22-27. | -0.007 | 0.016 | -0.003 | 0.022*** |
| | (0.006) | (0.009) | (0.004) | (0.004) |
| Number of observation | 136,590 | 136,590 | 123,265 | 123,265 |

*Note:* Each cell shows the estimated coefficient for the indicator of exposure to compulsory schooling reform. The dependent variable in the first and the third columns is a binary indicator that takes the value one for individuals who have a college degree. The dependent variable in the rest takes the value one for those who have college degree or who were in college when surveyed. All estimations control for survey year, age, and region fixed effects, an indicator for urban areas, and year of birth trends. Standard errors are shown in the parentheses, clustered at the year of birth level.

**Table A7 - Returns to Compulsory Schooling Estimates for Log Monthly Earnings**

| | **Women** | | **Men** | |
|---|---|---|---|---|
| | OLS | IV | OLS | IV |
| All individuals aged 20-29 who are not college enrollees or college graduates, and work for salary. | 0.051*** | 0.098*** | 0.024*** | 0.017 |
| | (0.004) | (0.014) | (0.008) | (0.015) |
| Number of observation | 30,622 | 30,622 | 103,629 | 103,629 |
| All individuals from urban areas aged 20-29 who are not college enrollees or college graduates, and work for salary. | 0.049*** | 0.108*** | 0.024*** | 0.009 |
| | (0.004) | (0.017) | (0.002) | (0.016) |
| Number of observation | 25,624 | 25,624 | 82,363 | 82,363 |
| All individuals aged 22-27 who are not college enrollees or college graduates, and work for salary. | 0.050*** | 0.102*** | 0.022*** | -0.001 |
| | (0.002) | (0.018) | (0.001) | (0.015) |
| Number of observation | 18,445 | 18,445 | 69,239 | 69,239 |
| All individuals from urban areas aged 22-27 who are not college enrollees or college graduates, and work for salary. | 0.049*** | 0.111*** | 0.022*** | -0.008 |
| | (0.002) | (0.021) | (0.001) | (0.014) |
| Number of observation | 15,560 | 15,560 | 54,782 | 54,782 |

*Note:* Each cell shows the effect of one year of additional schooling on log wages. Schooling is measured as completed years of schooling. The first and the third columns present the least squares estimates whereas the second and the fourth present the IV estimates. The sample specifications are given at the beginning of each row and the sample sizes are provided beneath the sample description. All samples exclude college graduates or college enrollees. All estimations control for survey year, age, and region fixed effects, an indicator for urban areas, and year of birth trends. Standard errors are shown in the parentheses, clustered at the year of birth level.

**Table A8 - Main Tasks and duties of the person in the workplace (ISCO 88)**

| Code | Occupation | % share among women working for salary | % share among men working for salary | % share among all employed women | % share among all employed men |
|---|---|---|---|---|---|
| 1 | Legislators, senior officials and managers | 0.9 | 1.34 | 1.25 | 6.28 |
| 2 | Professionals | 1.79 | 1.31 | 1.06 | 1.03 |
| 3 | Technicians and Associate Professionals | 13.28 | 6.04 | 7.69 | 4.83 |
| 4 | Clerks | 23.96 | 6.43 | 13.85 | 4.9 |
| 5 | Service Workers, and shop and market sales workers | 20.47 | 20.85 | 13.84 | 18.72 |
| 6 | Skilled agricultural and fishery workers | 0.44 | 0.82 | 31.48 | 10.19 |
| 7 | Craft and related trades workers | 10.06 | 27.11 | 8.32 | 23.04 |
| 8 | Plant and Machine operators and assemblers | 12.39 | 18.16 | 7.21 | 14.79 |
| 9 | Elementary Occupations | 16.71 | 17.94 | 16.31 | 16.23 |
| Number of Observations | | 33,301 | 109,632 | 59,327 | 153,727 |

*Note:* The percentage shares are calculated for the non-college bound individuals aged 20-29 using survey releases 2004-2011.

**Table A9 - Economic Activity of the Local Unit in Which the Person Works (Regrouped Categories from of NACE1.1)**

| Code | Economic Activity Name | % share among women working for salary | % share among men working for salary | % share among all employed women | % share among all employed men |
|---|---|---|---|---|---|
| 1 | Agriculture, forestry, hunting and fishing | 5.47 | 2.73 | 39.47 | 13.26 |
| 2 | Mining and quarrying | 0.07 | 1.25 | 0.04 | 0.92 |
| 3 | Manufacturing | 34.3 | 35.64 | 22.21 | 27.82 |
| 4 | Electricity, gas and water | 0.15 | 0.72 | 0.11 | 0.63 |
| 5 | Construction | 1.3 | 10.6 | 0.78 | 8.71 |
| 6 | Wholesale and retail trade, restaurants and hotels | 26.86 | 28.15 | 18.01 | 30 |
| 7 | Transportation, communication and storage | 2.45 | 5.21 | 1.45 | 5.38 |
| 8 | Finance, insurance, real estate and business services | 8.92 | 6.91 | 5.25 | 5.31 |
| 9 | Community, social and personal services | 20.49 | 8.79 | 12.68 | 7.97 |
| Number of Observations | | 33,301 | 109,632 | 59,327 | 153,727 |

*Note:* The percentage shares are calculated for the non-college bound individuals aged 20-29 using survey releases 2004-2011.

## Appendix B: Data

I use Household Labor Force Survey micro data releases from 2004 to 2011. After merging the original data sets with the single age data and year of birth data, I have the following numbers of observations in each year respectively: 472823, 490008, 497132, 481600, 481149, 503329, 522171, and 517076 (3,965,288 observations in total). After appending data from eight years, I drop those who are younger than 20 or older than 29. This leaves me with 600,000 observations. Then I drop observations with inconsistent age and year of birth information (I drop if year of birth is less than (survey year-age-one) or year of birth is greater than (survey year-age)). This leaves me with 599,485 observations. 25477 (4%) observations among the remaining ones do not have year of birth information, and I drop them as well. Thus I am left with 574,008 observations aged 20-29 from survey years 2004-2011.

The regressions that investigate the effect of the reform on educational attainment use this sample. The sample of non-college bound individuals excludes those who attended college or had graduated from college when surveyed. When I estimate the wage returns to schooling, I drop individuals with monthly wages less than 100 Turkish Liras or more than 4,000 Turkish Liras. This corresponds to less than 1 percent of total wage earners with wage information. As I mentioned in Section 5, only 5% of the salary workers have missing wage information.

### Key Variables:

**Schooling:** The survey does not provide the exact years of education. Instead it provides the information on the highest completed degree. Therefore I categorize individuals into four: Less than middle school, Middle School Graduates, High School Graduates and College Graduates. In the HLF micro data, those who are in categories 0, 1, 2 are considered in "Less than Middle School" category. Although this category may technically involve who do not have any education at all, the share of this group should be ignorable. Vocational middle school and general middle school are considered in the same category in the survey. I follow the survey design and call them Middle School Graduates. Similarly, vocational and general high school graduates are grouped as High School Graduates. Finally, those who have two-year college degree or a higher degree are grouped as College Graduates.

The completed years of education is calculated through assignment of usual years of education to each degree. I assign five years of education to those who have less than middle school degree, eight years to those with middle school degree, eleven years to those with high school degree and fifteen years to the college graduates.

**Age and Year of Birth:** As mentioned in the text, I obtained additional data on single age and year of birth of individuals from the Turkish Statistical Institute. I observe the age and year of birth information of 574,008 individuals aged 20-29 from survey years 2004-2011.

**Employment Status:** The surveys from 2004 to 2008 categorize employment status into five: regular, casual, employer, self-employed and unpaid family worker, whereas surveys 2009-2011 combine the first two into the same category of regular or casual workers. To be consistent, I combine the first two into the same category in the early surveys as well. The category of regular or casual workers is used as the sample of salary workers.

**Status of workplace:** Only in the 2009-2011 releases I observe the status of workplace. There are three categories: private, public and other. While investigating the effect of the reform on sectors, the binary indicator for public sector takes the value one for those who are in the second category, and zero for those in the first and the third category.

**Type of residence:** The HLS considers the settlements with population of 20000 and less as rural, and settlements with bigger population as urban. I follow this categorization in my analysis.

**Marital Status:** I categorize “single that is never married” and “living together without getting married” into the category of “single”, and categorize “married”, “married but not living together”, “divorced” and “widowed” into the category of “ever-married”. So I generate a binary indicator for being ever-married.

**Computing DOT Task Means for ISCO-88 Categories:**

Each DOT category has an assigned number for the task requirement of the occupation in relation to data, people and things. My ultimate target is to obtain assessments of these requirements for ISCO occupation categories in the HLF survey. I start by matching more than 12000 thousand DOT occupation categories to around 800 SOC-2000 occupation categories. Then I calculate the simple average of three assigned values for each SOC category so that I obtain the task requirement of around 800 SOC occupations regarding data, people and things. Then I match SOC occupation categories to 26 ISCO-88 categories in my survey data, and calculate the simple average of the task values once more. As a result, I obtain three values for each ISCO-88 occupation category regarding the task requirements in relation to data, people and things. Occupation categories that require more complicated tasks have lower values, and those that require less complicated tasks have higher values.

## Appendix C: Theory

Remember that following Card (1999) I let the marginal costs and benefits of schooling be characterized by

$$\frac{y'(S)}{y(S)} = b_i - k_1 S \qquad\qquad h'(S) = r_i + k_2 S$$

where $b_i$ and $r_i$ are random variables with means $\bar{b}$ and $\bar{r}$, $b_i = \bar{b} + \varphi_i$, $r_i = \bar{r} + \eta_i$ and $k_1$ and $k_2$ are non-negative constants, where $b_i$ corresponds to abilities, $r_i$ corresponds to psychological and monetary costs of schooling. $k_1$ and $k_2$ represent the concavity of earning function and convexity of cost function respectively. The optimal schooling level that solves the problem is

$$S_i^* = (b_i - r_i)/k \qquad\qquad \text{where } k = k_1 + k_2.$$

Proofs of propositions 1 and 3 are quite intuitive and shown in the text. Below, I will provide the proofs of propositions 2 and 4 respectively.

**Proof of Proposition 2:**

Suppose we have data from two samples of individuals, men and women, with the same $b_i, \eta_i, \sigma_\eta^2$, $\sigma_{b\eta}$ distributions, but with higher mean marginal cost of schooling for women than men, $\bar{r}_w > \bar{r}_m$. Assume that log earnings are linear in schooling, i.e. $k_1 = 0$, and those with low marginal costs also have higher returns to schooling, i.e. $\sigma_{b\eta} < 0$.

The instrumental variable estimator that uses a compulsory schooling reform that leads to a proportional reduction in the marginal cost of schooling has the following probability limit (Card 1999).

$plimb_{iv} = \bar{\beta} + \frac{1-\theta}{k(\pi_1-\pi_0)}\left\{\frac{\sigma_\eta^2 k_1}{k} + \sigma_{b\eta}\left(1-\frac{k_1}{k}\right)\right\}$ where $(\pi_1 - \pi_0) = \frac{\bar{r}(1-\theta)}{k}$.

Since $k_1 = 0$,the whole term simplifies to the following,

$plimb_{iv} = \bar{\beta} + \frac{1}{\bar{r}}\ \sigma_{b\eta}$ which implies

$plimb_{iv,\ women} = \bar{\beta}_w + \frac{1}{\bar{r}_w}\ \sigma_{b\eta}$ and $plimb_{iv,\ men} = \bar{\beta}_m + \frac{1}{\bar{r}_m}\ \sigma_{b\eta}$.

By assumption, $\bar{\beta}_w = \bar{\beta}_m$, $\bar{r}_w > \bar{r}_m$, and $\sigma_{b\eta} < 0$ and the covariance is the same across genders. Therefore, women who have higher mean marginal costs in the first place would have less negative deviation from the average marginal returns. Although, the average returns are the same across two samples, the researcher obtains higher IV estimate among women.

**Proof of Proposition 4:**

The alternative explanation for pre-reform low average schooling levels among women is that they have lower marginal returns to schooling. Then we assume the same$\varphi_i, r_i, \sigma_\eta^2,\ \sigma_{b\eta}$ distributions in both samples, but that women have lower mean marginal returns to schooling than men, $\bar{b}_w < \bar{b}_m$. Again assume also that log earnings are linear in schooling, i.e. $k_1 = 0$, and those with low marginal costs also have higher returns to schooling, i.e. $\sigma_{b\eta} < 0$. Similar to the previous case, we have

$plimb_{iv,\ women} = \bar{\beta}_w + \frac{1}{\bar{r}_w}\ \sigma_{b\eta}$ and $plimb_{iv,\ men} = \bar{\beta}_m + \frac{1}{\bar{r}_m}\ \sigma_{b\eta}$.

By assumption, $\bar{\beta}_w < \bar{\beta}_m$, $\bar{r}_w > \bar{r}_m$ and $\sigma_{b\eta} < 0$ and the covariance is the same across genders. Therefore women have a lower average returns in the first place and the negative deviation is the same in both samples. Thus, the IV estimation leads to lower estimated effects among women than among men, which does not fit to my empirical findings.

**Relaxing Linearity of Earnings Assumption**

Once we relax the assumption of linear log earnings in schooling, Propositions 1 and 3 are not affected at all. Propositions 2 and 4 are still intact under certain conditions.

**Proof of Proposition 2 when $\boldsymbol{k_1} > 0$ :**

Suppose we have data from two samples of individuals, men and women, with the same $b_i, \eta_i, \sigma_\eta^2$, $\sigma_{b\eta}$ distributions, but with higher mean marginal cost of schooling for women than men, $\bar{r}_w > \bar{r}_m$. Also assume that those with low marginal costs also have higher returns to schooling, i.e. $\sigma_{b\eta} < 0$. Now, assume that log earning is concave function of schooling, i.e. $k_1 > 0$. The IV estimator has the following probability limit

$plimb_{iv} = \bar{\beta} + \frac{1}{\bar{r}}\left\{\frac{\sigma_\eta^2 k_1}{k} + \sigma_{b\eta}\left(1 - \frac{k_1}{k}\right)\right\}$ where $\bar{\beta} = \bar{b} - k_1\bar{S}$.

When we plug $\bar{S}$=$(\bar{b} - \bar{r})/k$ in the equation, we obtain

$plimb_{iv,women} = \bar{b}\left(1 - \frac{k_1}{k}\right) + \frac{k_1\bar{r}_w}{k} + \frac{1}{\bar{r}_w}\left\{\frac{\sigma_\eta^2 k_1}{k} + \sigma_{b\eta}\left(1 - \frac{k_1}{k}\right)\right\}$ and

$plimb_{iv,men} = \bar{b}\left(1 - \frac{k_1}{k}\right) + \frac{k_1\bar{r}_m}{k} + \frac{1}{\bar{r}_m}\left\{\frac{\sigma_\eta^2 k_1}{k} + \sigma_{b\eta}\left(1 - \frac{k_1}{k}\right)\right\}$.

The first terms in each equation are the same. Since $\bar{r}_w > \bar{r}_m$ the second term is bigger among women than among men. Therefore, $\frac{\sigma_\eta^2 k_1}{k} + \sigma_{b\eta}\left(1 - \frac{k_1}{k}\right) < 0$ is a sufficient condition for having $plimb_{iv,women} > plimb_{iv,men}$. In other words unless the concavity of log earnings is so big the Proposition 2 holds.

**Proof of Proposition 4 when $k_1 > 0$ :**

Assume the same$\varphi_i, r_i, \sigma_\eta^2,\ \sigma_{b\eta}$ distributions in both samples, but that women have lower mean marginal returns to schooling than men, $\bar{b}_w < \bar{b}_m$. Also assume that those with low marginal costs also have higher returns to schooling, i.e. $\sigma_{b\eta} < 0$. This time assume that log earning is a concave function of schooling, i.e. $k_1 > 0$. The IV estimator has the following probability limit

$plimb_{iv} = \bar{\beta} + \frac{1}{\bar{r}}\left\{\frac{\sigma_\eta^2 k_1}{k} + \sigma_{b\eta}\left(1 - \frac{k_1}{k}\right)\right\}$ where $\bar{\beta} = \bar{b} - k_1\bar{S}$.

When we plug $\bar{S}$=$(\bar{b} - \bar{r})/k$ in the equation, we obtain

$plimb_{iv,women} = \bar{b}_w\left(1 - \frac{k_1}{k}\right) + \frac{k_1\bar{r}}{k} + \frac{1}{\bar{r}}\left\{\frac{\sigma_\eta^2 k_1}{k} + \sigma_{b\eta}\left(1 - \frac{k_1}{k}\right)\right\}$ and

$plimb_{iv,men} = \bar{b}_m\left(1 - \frac{k_1}{k}\right) + \frac{k_1\bar{r}}{k} + \frac{1}{\bar{r}}\left\{\frac{\sigma_\eta^2 k_1}{k} + \sigma_{b\eta}\left(1 - \frac{k_1}{k}\right)\right\}$.

The second and the third terms are the same in both equations. Since we assumed $\bar{b}_w < \bar{b}_m$, $plimb_{iv,women} < plimb_{iv,men}$. In other words, even under concavity of earnings Proposition 4 holds. Lower return to schooling among women leads to lower IV estimates among women than among men, which does not fit to my empirical findings.