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

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Abstract

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, J24, J31

Keywords: Returns to Education, Compulsory Schooling, Occupational Choice

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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. Most 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 20th 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 5th 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.[1]

I find that the reform increases average years of education by 0.56 years among women and 0.43 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.

[1] Tsai et al. (2009) examine the effect of 1968 compulsory schooling reform in Taiwan on gender disparities in employment. Although they do not examine the effect on earnings they investigate the effect on sector of employment and type of employment.

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.[2] 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 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.[3] The margin that is evaluated is 10th versus 11th grade in Angrist and Krueger (1991) and 9th versus 10th 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 8th versus 10th grade in France

[2] 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.

[3] Devereux and Hart (2009) reanalyse the dataset and find much smaller returns to schooling in Britain with no positive return for women and 3-4% return for men.

and 10th versus 11th 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 9th 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 9th 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. Duflo (2001) investigates returns to schooling in Indonesia using primary school constructions between 1973 and 1978 as instrumental variables for educational attainment. She finds economic returns to education ranging from 6.8 percent to 10.6 percent.

There are a number of recent studies that estimate the causal effect of education on individual outcomes in Turkey. Gulesci and Meyersson, (2013) and Cesur and Mocan (2014) estimate the causal effect of education on religious expression and political behavior of individuals using the 1997 reform as an instrumental variable. In a separate study Cesur et al. (2014) estimate health returns to education. They find that among women aged 18-30 additional education due to the compulsory schooling reform does not have a significant effect on self-reported health, BMI, obesity, or smoking behavior. My paper is most closely related to the study by Aydemir and

Kirdar (2013). Aydemir and Kirdar (2013) estimate the effect of the 1997 reform using data from Turkish Income and Expediture Survey. They restrict their analysis to men only, and estimate the effect of the reform on educational attainment and the effect of education on wages.

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,

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

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 costs and benefits of schooling be characterized by

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

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 ΔS_gin the mean schooling of group g, and β_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

$$plimb_{iv} = \frac{E[\beta_g \Delta S_g]}{E[\Delta S_g]} \tag{6}$$

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.[4] The law required individuals who were born after 1986 to continue until the 8th grade.[5] The construction of additional classrooms and hiring of additional teachers were financed by extra taxes.[6] 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 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

[4] 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.

[5] The wording in the law was such that it required the fifth graders in 1997-1998 academic year to continue until the 8th 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.

[6] See Dulger (2004) for a detailed description of the compulsory schooling reform in Turkey.

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, $\bar{r}_w > \bar{r}_m$.[7] 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 schooling, i.e. $\sigma_{br} < 0$.[8] Card (1995) 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 θ is a cost shifter, $0 < \theta < 1$. The difference between new and old

[7] σ_η^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, $r_i = \bar{r} + \eta_i$.
[8] 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 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 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.[9]

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.

[9] 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.

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.

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 , η_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.[10]

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

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.[11] 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.[12] Yet, this should not introduce any bias since this is the case for all survey years regardless of the reform.[13] 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.[14]

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 of birth and analyze a sample of 574,008 individuals aged 20-29 from survey years 2004-2011.[15] 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

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

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

[13] 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.

14 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.

15 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.

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 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:

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

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. Considering the potential group structure of the error terms, I allow standard errors to be correlated within the same survey year-year of birth intersection level.[16] 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.

[16] Clustering at survey year or year of birth separately does not change the estimation results.

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.[17] 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.[18] Figure 1 and Figure 2 show that the secular trend is linear, alleviating concerns about nonlinear cohort differences.[19]

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

[17] 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.

[18] 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.

[19] 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.

an instrument to estimate the Local Average Treatment Effect of schooling. The 2SLS equation becomes

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

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 estimated value of δ can be interpreted as the causal effect of education.

6.3. Identification and Validity Concerns

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.[20] Such factors may have altered school quality in either direction, but without additional data I will treat years of schooling as a homogenous

20 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.

treatment. Gulesci and Myerson (2012) show that the compulsory schooling reform had negative effects on the expression of religiosity among ever-married women.[21] 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 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.[22]

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 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.

[21] 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.

[22] 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. This may also be due to signaling effect that brings the average quality of the pool of college graduates down.

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

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 third 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.[23] Note that my results are very similar in the narrower age ranges that exclude individuals around the military conscription age. In the second and the fourth rows I repeat the estimations for the sample from urban areas only and show that the effect on schooling was slightly lower in urban areas. Also, Table A5 in the Appendix presents the estimated results from the empirical specification that allows for differential trends at the two sides of the cutoff date.

[23] 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.

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 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 in Table A6.[24] 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.[25]

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

Table 3 shows IV estimates of the returns to education.[26] 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.[27] 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

[24] 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.

[25] Table A7 repeats the estimations in Table 3 and 4 including the college graduates in the sample. The results are qualitatively similar.

[26] Table A8 in the Appendix presents the first stage estimates and the F statistics.

[27] Table A9 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. You can also find the OLS estimates of returns to schooling in Table A9.

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 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 an 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 unlikely that this self-employment effect drives the gap in earning effects between women and men. In Panel 2 I find a small positive effect on the probability of working for wage among men.[28]

Being covered by Social Security is another mechanism through which the compulsory schooling reform can benefit individuals, especially in an economy with substantial unregistered

[28] The survey categorizes employed individuals into four groups: regular wage employees, employers, self-employed, and unpaid family worker. I combine the second and the third group under the self-employed category. Panel 1 and Panel 2 imply a decrease in the probability of being an unpaid family worker in the sample of men.

employment.[29] All Turkish employees working under an employment contract and self-employed people must be registered in Social Security System and pay the premiums. In return they receive health care and contribute to their retirement plan. Panel 3 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.[30] 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 A10 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

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

[30] Although the estimates are consistently positive across samples, the estimates are not always statistically significant. 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. In my own estimations, when I restrict the sample to those in the urban areas the positive effects get more pronounced.

pp. or -28%) and plant and machine operators and assemblers (-2.2 pp. or 18%). I observe significant positive effects 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%).[31]

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 A11 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 security and

[31] 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.

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.[32] Using two crosswalks, I obtain three numbers representing the task requirements of occupation categories regarding data, people and things. The reader should keep in mind that the DOT was developed by the US Bureau of Labor Statistics, and the crosswalks may not exactly match the complexity of tasks in Turkey.

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

[32] 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 (Tansel, 2002). 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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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***	0.040***	0.387***	0.107***	0.0316**
	(0.044)	(0.011)	(0.005)	(0.066)	(0.014)	(0.010)
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***
	(0.047)	(0.010)	(0.006)	(0.068)	(0.013)	(0.010)
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***
	(0.077)	(0.017)	(0.008)	(0.080)	(0.020)	(0.009)
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**
	(0.074)	(0.016)	(0.007)	(0.080)	(0.018)	(0.010)
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 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*
	(0.011)	(0.010)
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
	(0.015)	(0.010)
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*
	(0.004)	(0.013)
Number of observation	260,140	207,495
All individuals aged 22-27 who are not college enrollees or college graduates.	0.006	0.012*
	(0.005)	(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)	(0.015)
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)	(0.013)
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 survey year-year of birth level.

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

	Women	Men
Panel 1 - The Wage Employment Effects of Compulsory Schooling (Conditional on Being Employed)		
All individuals aged 20-29 who are not college enrollees or college graduates and employed.	0.008	0.019***
	(0.009)	(0.007)
Number of observation	59,237	153,727
All individuals aged 22-27 who are not college enrollees or college graduates and employed.	0.001	0.010
	(0.013)	(0.009)
Number of observation	35,425	101,503
Panel 2 - 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***
	(0.004)	(0.007)
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***
	(0.005)	(0.010)
Number of observation	35,425	101,503
Panel 3 - 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***
	(0.010)	(0.011)
Number of observation	59,327	153,727
All individuals aged 22-27 who are not college enrollees or college graduates and employed.	0.020	0.014
	(0.012)	(0.014)
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 survey year-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).

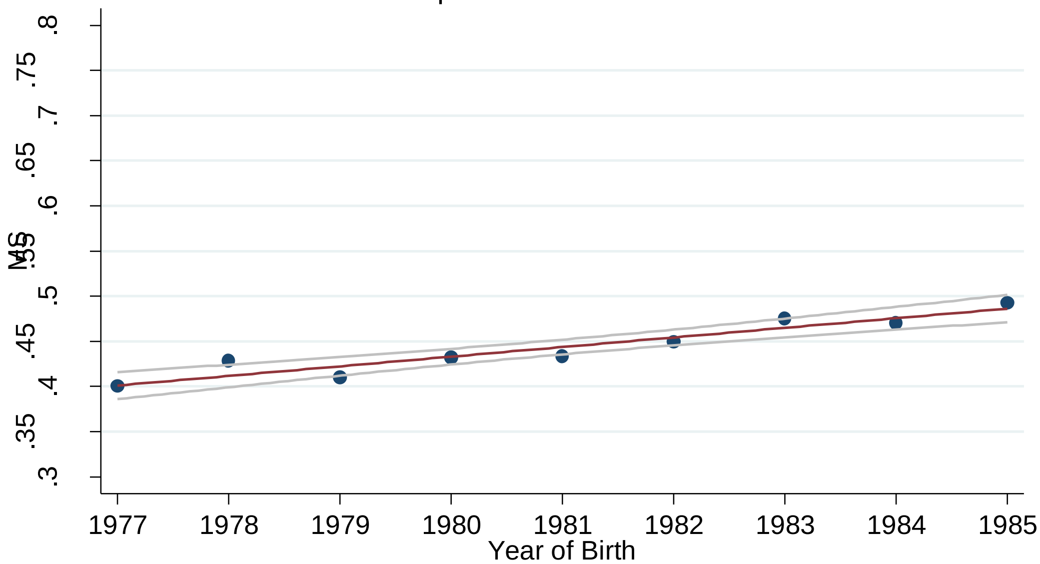
Table 9 - The Effect of the Schooling on Tasks (Women - IV estimates)

	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.

Figure 1

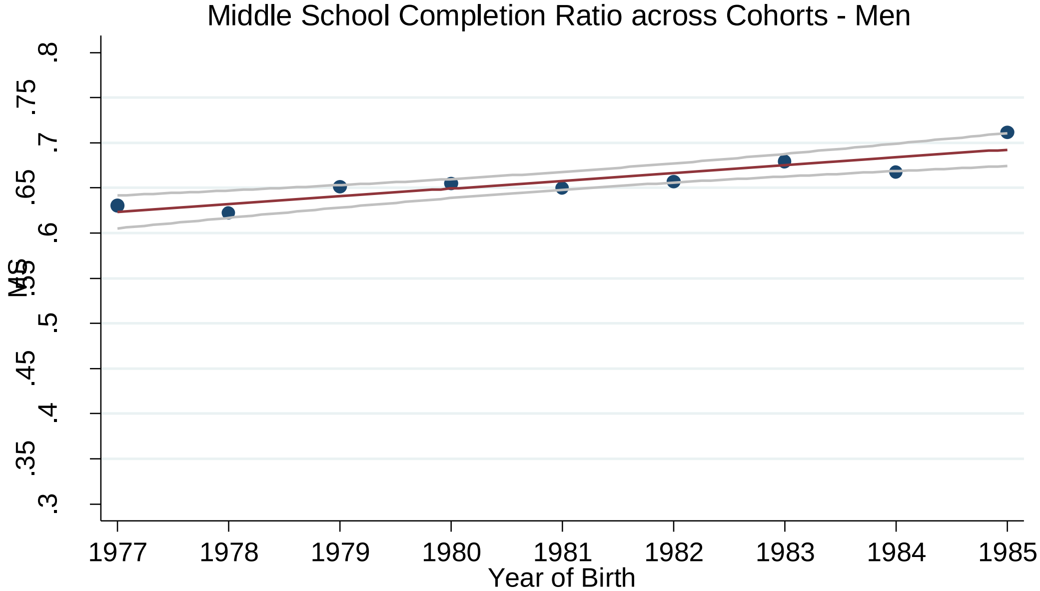
Middle School Completion Ratio across Cohorts - Women



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.

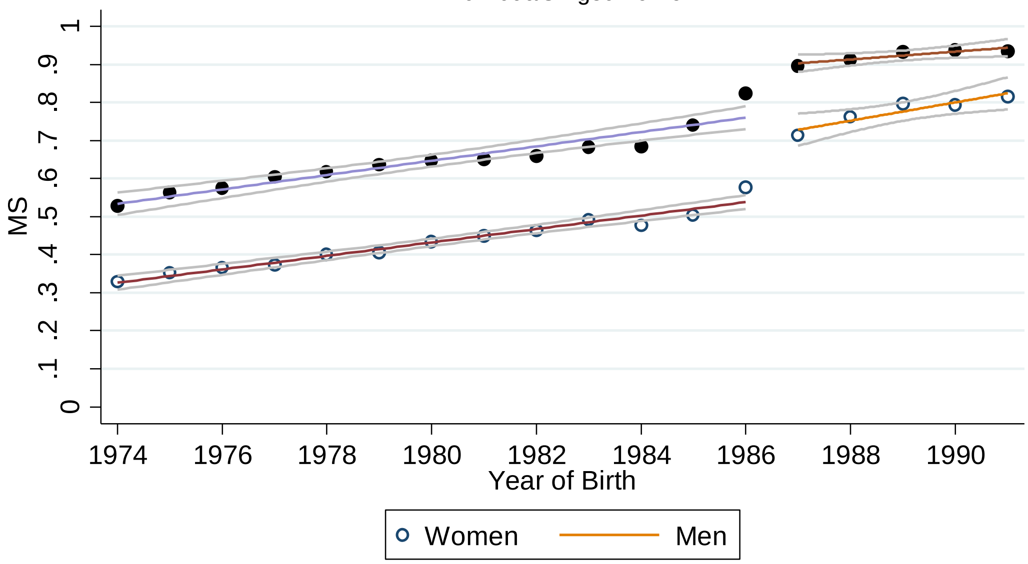
Figure 2

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



The data come from survey years 2004-2011.
It compares MS completion ratio across cohorts for individuals aged 20-29.

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
	(0.015)	(0.020)
Number of observation	18,970	20,719
All individuals aged 22-27 who have college degree.	-0.035*	-0.013
	(0.020)	(0.020)
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 survey year-year of birth level.

Table A5 - The Estimated Effect of Compulsory Schooling Reform on Schooling
(Allowing for Differential Trends on the Two Sides)

	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.	0.674***	0.112***	0.045***	0.919***	0.173***	0.094*
	(0.132)	(0.016)	(0.022)	(0.251)	(0.022)	(0.050)
Number of observation	310,226	310,226	310,226	263,782	263,782	263,782
All individuals aged 22-27.	-0.06	0.059**	-0.078***	0.472***	0.110***	0.013
	(0.167)	(0.030)	(0.014)	(0.161)	(0.034)	(0.032)
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. The empirical specifications in the table allow for differential trends on the two sides of the cutoff year. Standard errors are shown in the parentheses, clustered at the survey year-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**
	(0.004)	(0.004)	(0.003)	(0.008)
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**
	(0.004)	(0.004)	(0.004)	(0.008)
Number of observation	224,734	224,734	194,220	194,220
All individuals aged 22-27.	-0.001	0.019***	-0.003	0.016***
	(0.004)	(0.006)	(0.004)	(0.003)
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***
	(0.005)	(0.007)	(0.005)	(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 (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 and work for salary.	0.0536***	0.0001
	(0.0125)	(0.0120)
Number of observation	49,592	124,348
Panel 2- Labor Force Participation Effects of Compulsory Schooling		
All ndividuals aged 20-29 who are not college enrollees.	-0.00381	0.0199
	(0.00616)	(0.0139)
Number of observation	295,497	242,116
Panel 3- Employment Effects of Compulsory Schooling		
All individuals aged 20-29 who are not college enrollees.	-0.00751	0.0110
	(0.00618)	(0.0167)
Number of observation	295,497	242,116
Panel 4- The Wage Employment Effects of Compulsory Schooling (Conditional on Being Employed)		
All individuals aged 20-29 who are not college enrollees.	0.00793	0.0139*
	(0.00808)	(0.00803)
Number of observation	79,987	178,702
Panel 5- The Self Employment Effects of Compulsory Schooling (Conditional on Being Employed)		
All individuals aged 20-29 who are not college enrollees.	-0.000621	0.0453***
	(0.00398)	(0.00924)
Number of observation	79,987	178,702
Panel 6- The Social Security Effects of Compulsory Schooling (Conditional on Being Employed)		
All individuals aged 20-29 who are not college enrollees.	0.00242	0.0394***
	(0.0106)	(0.0129)
Number of observation	79,987	178,702

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 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 survey year-year of birth level.

Table A8 - Returns to Compulsory Schooling (Dep.Var.: Log Monthly Earnings)

	Women	Men
All individuals aged 20-29 who are not college enrollees or college graduates and work for salary.		
Year of education on logwages (IV estimate)	0.098***	0.017*
	(0.011)	(0.010)
Compulsory schooling reform on years of education (1st Stage)	0.787***	0.646***
	(0.052)	(0.031)
1st stage F-statistic	54.38	137.72
Number of observation	30,622	103,629
All individuals aged 22-27 who are not college enrollees or college graduates and work for salary.		
Year of education on logwages (IV estimate)	0.102***	-0.001
	(0.015)	(0.010)
Compulsory Schooling Reform on years of education (1st Stage)	0.860***	0.648***
	(0.075)	0.039
1st Stage F-Statistic	33.50	90.48
Number of observation	18,445	69,239

Note: The first row shows the IV estimate of the effect of one year of additional schooling on log wages. The second row shows the first stage estimate, the effect of the schooling reform on years of education. The sample specifications are given at the beginning at the panel titles. All samples consist of wage employees and exclude college graduates or college enrollees. All estimations control for survey year fixed effects, age fixed effects, region fixed effects, an indicator for urban areas, and year of birth trends. Standard errors are shown in the parentheses, clustered at the survey year-year of birth level.

Table A9 - 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*
	(0.002)	(0.011)	(0.010)	(0.010)
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
	(0.002)	(0.014)	(0.001)	(0.011)
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)	(0.015)	(0.001)	(0.010)
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)	(0.020)	(0.001)	(0.010)
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 survey year-year of birth level.

Table A10 - 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 A11 - 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).

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

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 $k_1 > 0$:

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.

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