Estimates of the Return to Schooling in a Developing Country: Evidence from a Major Policy Reform in Turkey

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2013

Preliminary, Please do not cite

Estimates of the Return to Schooling in a Developing Country: Evidence from a Major Policy Reform in Turkey

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This version: December 2013

Abstract

This paper uses a major change in the compulsory schooling policy in Turkey – which increased the mandatory duration from five to eight years -- to estimate the causal effect of education on earnings. The policy reform brought about a substantial rise in schooling attainment due to the high dropout rates at the end of compulsory schooling, the duration of extension, as well as the spillover effects of the policy on non-compulsory schooling years. Our results show that the 2SLS estimates of the returns to education are much larger estimates than the OLS estimates. These results also constitute the first causal estimates of the returns to education in the Turkish context and one of the few in developing country contexts.

I. Introduction

A growing body of literature estimates the return to education using different techniques to overcome the biases involved in an OLS framework. These studies are mostly conducted for developed countries in the North American and European contexts. This paper aims to estimate the causal effects of the education on earnings in a developing country context using a major reform that increased the duration of compulsory schooling from 5 to 8 years. Due to high dropout rates at the end of compulsory school period**,** this policy change in the Turkish context affected a large fraction of the student population – which brings our local average treatment effect estimate closer to the average treatment effect, as in the case of Oreopoulos (2006).

Our identification strategy for estimating the rates of return to schooling is similar to that of earlier studies that use the discontinuities in school attainment due to changes in school leaving ages (e.g. Oreopoulous 2006, Devereux and Hart 2010). The estimated returns of 2SLS strategy are at the high end of the reported results from similar 2SLS results in developed country contexts. This result is consistent with earlier studies that suggest that the earnings premium by level of education is steeper in developing countries (Patrinos et al 2010). A comparison of the OLS results to 2SLS results also indicates large downward biases in the OLS estimates similar to findings from other studies.

In the section we provide a review of earlier literature followed by a discussion of the data sources used in this study in Section 3. Section 4 outlines the methodology while Section 5 presents the preliminary research results. Section 6 concludes.

II. Previous literature

Earlier studies on the economic returns to schooling point out the relationship between education and economic growth. Becker (1964) and Griliches (1970) report that rising education levels in the U.S. after the World War II contribute substantially to its economic growth. The literature that follow this finding address the issue that how much of this observed economic return to schooling is due to the ability bias—does schooling really make people more productive, or is it that more able people are more likely to acquire higher levels of schooling? In a seminal work, Griliches (1977) show that the ability bias—which causes overestimation of the actual returns to schooling—is small; and, in fact, other source of biases like measurement error—which cause underestimation of the actual return—could be more important.

Since the 1990s, with the pioneering study of Angrist and Krueger (1991), many studies use certain institutional features of schooling systems in various countries as a source of exogenous variation in schooling so that the returns to schooling in terms of earnings can be estimated consistently. Angrist and Krueger show that in the U.S. individuals who are born earlier in the year have, on average, a higher level of schooling than individuals who are born later in the year—which they attribute to the compulsory schooling laws in the U.S.. Angrist and Krueger and other studies that exploit differences in school start age use quarter-of-birth as an instrumental variable for completed years of schooling, as well as the interactions of quarter of birth with year of birth and state of birth as additional instruments.[1] Carneiro and Heckman (2002) find an association between the quarter of birth and certain indicators for early childhood development. Such an association could mean that quarter of birth influences wages not only through schooling investment but also through other channels as well, which would invalidate the instrument that assumes that quarter of birth is independent of tastes for schooling and schooling ability. Bound et al. (1995) point out the weak instrument problem and the resulting asymptotic bias in 2SLS estimates toward the OLS estimate in this context that is further addressed by Staiger and Stock (1997). Several studies in the literature also employ alternative instruments based on distance to nearest college (Kane and Rouse, 1993; Card, 1995; Connely and Uusitalo, 1997; Maluccio, 1997), family background variables (Card, 1995), new school construction (Duflo, 2001) at various locations.

Changes in compulsory schooling laws as instrumental variables for schooling have been employed by several studies in the literature. Harmon and Walker (1995) use the changes in compulsory schooling duration in the U.K. in 1947 and in 1973 as a source of exogenous variation in completed years of schooling to find the return to schooling in terms of earnings. Their identification strategy is based on comparison of birth cohorts that are

[1] Compulsory schooling laws, as an instrument, are also used by Acemoglu and Angrist (2001) in their analysis of human capital externalities.

affected by the policy with those that are not affected. A limitation of the Harmon and Walker study is that they do not allow for a secular time trend in schooling of successive birth cohorts. Oreopoulos (2006) uses the same instrument that Harmon and Walker (1995) utilized – the change in compulsory schooling law in the U.K. and Ireland context. His identification strategy also relies on a comparison of the birth cohorts that are affected by the policy with those that are not. Unlike the Harmon and Walker study, however, Oreopoulos (2006) accounts for birth cohort effects on education, as well as the effect of the education policy. An important issue in these studies is that the time frame of their analysis also covers other events—in particular, World War II—that also had a substantial effect on schooling outcomes. This issue is important since the methodology requires the timing of the policy be independent of the conditions for the whole economy. Other studies that utilize compulsory school laws to estimate returns to schooling include Devereux and Hart (2010) in the UK context, Pischke and von Wachter (2008) in the German context.

Most of the above mentioned studies find that 2SLS estimates of returns to schooling are either higher than or not much different from the OLS estimates. This finding is not consistent with the ability bias argument – which argues that more able people acquire higher levels of schooling; therefore, the coefficient for schooling in the OLS estimates also captures the effect of unobserved ability.

There have been a number of arguments in the literature as to this apparent contradiction. Bound and Jaeger (1996) argue that the unobserved differences between the characteristics of treatment and control groups in IV estimation could cause a bias—in the upward direction—that is much bigger than the ability bias. This would especially be important in studies where the instrument is far from being ideal, in other words far from satisfying the conditional independence assumption. Another potential explanation to the lower OLS estimates compared to 2SLS estimates, as put forward by Angrist and Krueger (1991), is the attenuation bias in OLS estimates due to measurement errors. This bias would be more important if the measurement error is especially high for individuals who are most affected by the source of exogenous variation in schooling. In fact, Kane et al. (1997) report some evidence for this.

A third explanation for the lower OLS estimates compared to IV estimates is due to heterogeneity in returns to education. Imbens and Angrist (1994) illustrate that the 2SLS estimates give the effect for the set of compliers—those who are induced to change their schooling problem as a result of the exogenous source of variation in schooling. The set of compliers may not be representative of the whole population. For instance, in Angrist and Krueger (1991), individuals who are forced to have longer schooling due to compulsory schooling laws are those with lower levels of education. Card (1999, 2001) argues that if these individuals with low levels of schooling have higher marginal rate of returns to schooling, the 2SLS estimates would be higher than OLS ones, as found in the literature. In other words, the estimates derived from the changes in compulsory school leaving age provide local treatment effects (LATE) for compilers – the group affected by the policy change. Oreopoulos (2006) notes that as the fraction of the population affected by the policy change increases, the LATE estimate approaches the average treatment effect (ATE). Using a much larger set of compliers Oreopoulos (2006) finds 2SLS estimates in the UK context that are not much different than that found by Angrist and Krueger. Devereux and Hart (2010), however, find much lower returns in the same context. Pischke and von Wachter (2008) similarly report much lower IV estimates (zero returns) than the OLS estimates in the German context. Thus, there is mixed evidence from these developed country contexts about the IV and OLS estimates of the returns to education.

In this paper, similar to Harman and Walker (1995) and Oreopoulos (2006), we compare birth cohorts that are affected by the 1997 education policy in Turkey (1986 and later birth cohorts) with those that are not affected (1985 and earlier birth cohorts). Our identification strategy allows for a time trend in schooling as well as earnings outcomes, in addition to the effect of the education policy. Also, in the time frame of our analysis, there were no macro events that had a major impact on schooling. According to the national education statistics during the 1996-97 education year, the year before the law change, at the primary school - covering grades 1 to 5- the net male enrollment rate was % 91,8 while the corresponding net enrollment rate at the junior high school –covering grades 6 to 8- was % 60,6 (TUIK, Education Statistics). The 1997 reform that extended the compulsory school from 5 to 8 years, therefore, potentially affected close half of school age children at the junior high school level. By 2000-01 school year, four years after the law was enacted the net enrollment rate among males at the compulsory schooling stage (grades 1 to 8) increased to 99,8 %. Although these official statistics on enrollment rates overestimate regular school attendance it is clear that the law increased schooling substantially for the cohorts affected by the policy. This study contributes to the literature on returns to education by providing estimates from a developing country context where the set of compilers affected by the policy change was a large group similar to the Oreopoulos (2006) study. Even though there are previous studies that estimate the returns to schooling in Turkey (e.g. Tansel 1994, 1996), these studies do not address the endogeneity of the schooling variable, and, therefore, do not estimate its causal effect. This is the first study to estimate this causal effect using the compulsory schooling policy change in 1997.

III. Data

In the analysis we use Turkish Income and Expenditure Surveys (TIES) that are available annually from 2002 to 2010. TIES surveys are nationally representative, and include a rich set of information on earnings and hours of work, as well as information on education and a number of demographic characteristics. We restrict our analysis to men only because a low fraction of women are employed in Turkey. In this data, the age of respondents is grouped in intervals of five (15-19, 20-24, and so on). Therefore, we can know the year of birth also in five-year intervals only. The 1997 education reform affected children who were born at or after 1986. The members of the earliest birth-cohort affected by the policy, the 1986 birth-cohort, were 24 years old in 2010—the latest year at which data are available. In other words, among the men that are affected by the education policy in our sample, the highest age is 24. Therefore, we restrict our analysis to 15- to 24-year-old men who report positive earnings at their current job or previous job(s) within the last year. Table 1 presents the structure of our data, which includes calendar years 2002 to 2010 and year-of-birth cohort intervals from 1978-82 to 1991-95. The numbers in each cell in Table 1 indicates the sample size. The total sample includes 7,949 men aged 15 to 24.

Ideally, we would account for the new education policy using a dummy variable, which takes the value of 1 for individuals born at or after 1986 and 0 otherwise. However, since the birth-year variable is grouped in 5-year intervals, the education policy variable in our study can take values strictly between 0 and 1. The value of the education-policy variable by year-of-birth cohort-intervals is illustrated in Table 2. For the 1981-85 and earlier birth-cohort intervals, the value of the policy variable is 0 because none of the birth-cohorts in these intervals is affected by the policy. Among the 1982-86 birth-cohorts, only the 1986 birth-cohort (one out of five) is affected by the policy; therefore, the value of the policy variable is 0.2. Among the 1983-87 birth-cohorts, both 1986 and 1987 cohorts are affected, so the value of the policy variable is 0.4, and so on. For the 1986-90 and later birth-cohort intervals, the value of the policy variable is 1 because all birth cohorts in these intervals are affected by the policy.

The information on education in TIES surveys is available in the form of completed degree. TIES also contains detailed information on earnings, as well as hours of work. TIES data do not include information on completed years of schooling, but includes information on the highest completed schooling degree (illiterate, literate but no degree, primary school [5 years, old system], primary school [8 years, new system], secondary school, technical secondary school, high school, technical high school, 2-year college, 4-year college, Masters or Ph.D. degree). Therefore, we refer to the Turkish Demographic and Health Surveys—which include information on both the completed degrees and completed years of schooling—to learn about the distribution of years of schooling conditional on the highest completed schooling level. Using this information, we generate the mean years of schooling for each one of the highest completed schooling levels in the TIES[2].

In the TIES, earnings are reported at the annual level. For those who are employed at the time of the interview, there is information on cash earnings, in-kind earnings, as well as bonuses and premiums; the sum of these elements constitutes annual earnings in the primary job. The earnings information for self-employed people is in the form of cash earnings and in-kind earnings only. For those who are not employed at the time of the interview but were employed for some time during the last year, there is information on cash and in-kind earnings from that job(s) during the last year, the sum of which give us annual earnings for

[2] According to the 2008 wave of the Turkish Demographic and Health Survey, the mean years of schooling for people with a certain degree would be well approximated by the minimum years of schooling required for that degree; in other words, 5 years for primary school degree, 8 years for secondary school degree, 11 years for high school degree, 13 years for two-year college degree, 15 years for four-year college degree, 17 years for Master's degree. The average of years of schooling is very close to 0 for illiterate people, and close to 2 years for those who are literate but have no degree.

these people. Agricultural earnings are ignored because the sample is restricted to residents in urban areas only.

The variation across workers in annual earnings would reflect the differences in the hourly earnings—which is a measure of their productivity—as well as the number of hours they work within a year. The annual hours of work could differ across educational groups because more educated workers may be less likely to be employed at a given time or more likely to work for longer hours (Card [1999] provides evidence for the latter in the U.S. context.). Therefore, the primary measure of earnings that we are going to use in this study is earnings per hour.

The number of months worked is available for all people who worked for some time within the last year. For workers who are employed at the time of the interview, this is the number of months worked in the current job; whereas, for workers who are unemployed at the time of interview, this is the number of months employed in the previous job(s) within the last year. When somebody is employed at the time of interview, but also had another job within the last year, only the number of months in the current job is available. (Therefore, accordingly, only annual earnings in the current job are taken in the earnings calculation.) The number of months is multiplied by 52/12 to find the annual number of weeks worked.

TIES also includes information on the number of hours worked per week. Again, for those who are employed at the time of interview, this is for the current job; and, for those who are not employed at the time of interview, this is for the previous job within the last year. (There is also information on weekly hours of work and earnings from additional job(s); however, since there is no information on the number of months worked in this job(s), they are not included in the calculation of either annual earnings or annual hours of work.)

The number of weeks worked is multiplied by the weekly hours of work to find the annual hours of work. Then, annual earnings are divided by annual hours of work to find hourly earnings. Hourly earnings (wages) at each year are adjusted to 2002 values, using the consumer price index for the month of July at each yearIn the estimation of the human capital earnings function, we will use the logarithm of hourly earnings due to the shape of the distribution of the hourly earnings variable. We also account for a number of family as well as employment characteristics of the male individuals in our sample. These include relation to household head, marital status, type of employment, occupation, and sector of employment. The details of these variables are provided in the appendix.

Table 3 presents descriptive statistics for the final sample. The mean value of years of schooling in the sample is 8.4 years. Roughly one half of the people in the sample are affected by the new education policy. While 37 percent of the observations are aged 15 to 19, 63 percent are aged 20 to 24. The mean value of tenure is 1.94 years. Most of the men in our sample (83.5 percent) are the sons of the household heads because most unmarried young men in these ages still live with their parents (as well as some married ones) in Turkey. The most common sectors of employment are manufacturing (31 percent), sales and repairs (25.6 percent), and construction (9.3 percent).

IV. Methodology

We use the variation in the years of compulsory schooling across different birth cohorts to identify the causal impact of schooling on earnings. The expected value of years of schooling changes according to this discontunuity in the exposure to the policy across year-of-birth cohorts. Moreover, the relationship between years of schooling and the covariate that determines the timing of the discontinuity (the forcing variable, which is year of birth) is continuous. Similary, there is a continuous relationship between the outcome variable (wages) and the forcing variable (year of birth), except for at the discontinuity. Therefore, the structure of our data fits a regression discontinuity design, where we can use the discontinuity in the exposure to the policy as an instrumental variable for years of schooling. [3] In particular, we estimate the following set of equations using two-stage least squares:

$$\log w_i = f(x_i) + \rho S_i + \alpha A_i + \beta Y_i + Z_i \gamma + \eta_i, \tag{1}$$

$$S_i = f(x_i) + \pi D_i + \alpha A_i + \beta Y_i + Z_i \gamma + \xi_i, \tag{2}$$

[3] Lee and Lemieux (2010) provide a review of regression-discontinuity design. Hahn et al. (2001) construct a theoretical framework for regression-discontunity design, and Angrist and Lavy (1999) and van der Klaauw (2002) are examples to some of the earliest applications. Oreopoulos (2006) uses regression-discontinuity design in his analysis of the effects of compulsory schooling laws on earnings in the UK.

where w denotes wages, S schooling, A age, Y calendar year, and Z control variables; and f(.) stands for continuous relationship between the outcome variables and x, the forcing variable (year of birth). In equation (1), ρ is the causal effect of schooling on wages and η is the error term. In equation (2), π is the causal effect of the education policy on years of schooling, D_i refers to the treatment variable, and ξ is the error term. We also estimate a reduced form of equation (1), given by

$$\log w_i = f(x_i) + \sigma D_i + \alpha A_i + \beta Y_i + Z_i \gamma + \eta_i . \tag{3}$$

As explained in the data section, the value of the treatment variable depends on the forcing variable as follows:

$$D_i = \begin{cases} 0 & if \quad x \leq 1981-1985 \\ 0.2 & if \quad x = 1982-1986 \\ 0.4 & if \quad x = 1983-1987 \\ 0.6 & if \quad x = 1984-1988 \\ 0.8 & if \quad x = 1985-1989 \\ 1 & if \quad x \geq 1986-1990 \end{cases} . \tag{4}$$

A critical aspect of any regression discontinuity design is to distinguish the effect of this discontinous jump, D, from the smooth function, f(.), in year-of-birth. Ideally, one would like to use a very flexible functional form for f(.); however, since the discontinous jump in our data is distributed over a few years, a very flexible f(.) could partly capture the effect of the policy around the discontinuity. Moreover, since the number of year-of-birth groups that we have in the estimation is relatively few (14 groups as can be seen in Table 1), disentengaling the effect of the policy from a very flexible underlying relationship between year-of-birth and the outcome variables would be difficult. Therefore, we use only linear and quadratic functions of year of birth in the estimation.

Since the sample includes only 15- to 24-year-old men and age is grouped in five-year intervals, equations (1) and (2) include a dummy variable for the 20- to 24-year-old age group. We use data from 9 different calendar years (2002 to 2010); therefore, equations (1) and (2) also inlcude calendar year dummies. However, for identification reasons, both 2002 and 2003 dummies are omitted as the baseline category. Standard errors are clustered at the level of birth-year groups because the policy variable does not exhibit variation across individuals within a birth-year group.

In an alternative specification, we add tenure to equation (1) as follows:

$$\log w_i = f(x_i) + \rho S_i + \alpha A_i + g(T_i) + \beta Y_i + Z_i \gamma + e_i, \qquad (5)$$

where T denotes tenure, g(.) is the functional form of tenure, and e the error term.

The validity of the instrument requires that the value of the instrument is randomly distributed so that the treatment and control groups are on average the same in terms of pre-treatment characteristics. This is quite acceptable in this study as the value of the instrument depends on the year-of-birth, and neigbouring year-of-birth cohorts would on average be the same. The exclusion restriction assumption requires that the instrument has no direct on wages, apart from its indirect effect through schooling. In our context, there is no reason to think about the effect of the policy on wages because the timing of the policy was very much independent of the economic conditions. The timing of the event was related to the political conditions of the time.

V. Results

V.I Preliminary analysis

Figure 1 illustrates how the log hourly wage rate changes over the life-cycle by educational attainment using the 2005 wave of TIES. There are substantial differences in wages across educational groups; for instance, among the 30 to 34 year old age group, college graduates earn roughly twice as much as and high school graduates earn roughly 50 percent more than primary school and secondary school graduates. The other key difference is in the shape of the age-earnings profiles of education groups. The profiles of groups with lower educational attainment are flatter, in particular, that of men with no degrees. On the other hand, among younger age-groups, wages rise much faster for men with college degrees.

A key assumption made in Mincer's human capital earnings function is that the log hourly wage rate is linear in years of schooling; in other words, every additional year of schooling makes the same percentage change in hourly wage rate, regardless of completed years of schooling. Card (1999) assesses the validity of this assumption in the U.S. context, and finds that this is in fact a reasonable assumption. Figure 2 shows how mean log hourly wages change with completed years of schooling, in order to test this assumption in the Turkish context. After 8 years of schooling, the relationship between log hourly wages and years of schooling is very close to being linear. However, before 8 years of schooling, the relationship is much less linear; in fact, there is little differences between the log hourly wages for primary school and secondary school graduates according to the 2005 wave of TIES.

Table 4 displays the relationship between education (years of schooling) and earnings (measured as log annual earnings and log hourly earnings), as well as the relationship between education and hours of work (measured as log annual hours, months worked, and log weekly hours). According to these OLS estimates based on the 2005 wave of TIES, an extra year of schooling is associated with a 10.2 percent increase in annual earnings and a 9.6 percent increase in hourly earnings. The changes in annual earnings and hourly earnings are very similar in the Turkish case because there is a weak relationship between education and annual hours of work; an extra year of schooling is associated with a 0.5 percent increase in the annual hours work. (The same increase for the U.S. is estimated as 4.2 percent by Card [1999].) However, column (4) in Table 1 shows that educated men work, on average, for a higher number of months. This indicates that men with lower levels of education are much more likely to be unemployed. On the other hand, column (5) of Table 1 shows that once employed, men with lower educational attainment work for longer hours; there is a negative relationship between years of schooling and weekly hours of work. This last finding is in stark contrast to the finding for the U.S. (Card, 1999) where there is a positive association between education and weekly hours of work.

V.II Estimation results

Table 5 presents the estimation results using pooled TIES data for the specification with a linear functional form for year-of-birth. In column (1), the estimates for the first stage of the 2SLS estimation, shown by equation (2) earlier, are given. According to these estimates, the education policy increases schooling by 0.61 years. It is more complicated to interpret age, year-of-birth, and calendar year effects because for instance, holding age constant, increasing year-of-birth by one also increases the calendar year by one as well. In column (2), the estimates for the reduced form wage equation, shown by equation (3) earlier, are given. These estimates suggest that, the new education policy increases the wage rate by 18 percent. In column (3), the OLS estimation results of the main wage equation—equation (1)—are displayed. The returns to schooling according to the OLS estimates are relatively low: an additional year of education increases wages by about 3 percent. Finally, column (4) displays our main results: 2SLS estimates. The returns to schooling estimates from the 2SLS estimation are substantially higher: an additional year of schooling increases wages by 30 percent.

The specification used in the estimates displayed in Table 6 allows for a more flexible functional form for year of birth; it is quadratic. According to these estimates, the new education policy increases schooling by about one complete year. As a result of increasing the school leaving age by one year, Oreopoulos (2006) reports that schooling increases by about 0.35 to 0.45 years in the UK context while Pischke and von Wachter (2008) report an increase of 0.28 years among basic school students in Germany. In the Turkish context, where compulsory schooling increased by three years, the resulting schooling increase corresponds to about 0.33 year increase per one year increase in compulsory schooling. Thus the magnitude of the increase in schooling in this context is in line with results reported for other countries. The OLS estimate for returns to schooling is very similar to that in Table 5; an additional year of schooling increases earnings by about 3 percent. However, the 2SLS estimate of returns to schooling, with a quadratic functional form for year of birth, is lower: an additional year of schooling increases wages by about 20 percent.

The fact that the 2SLS estimates of the returns to schooling change significantly according to the functional form of the forcing variable reflect the limitation of our data set in the way that the time span of our data is relatively short. However, we find the estimates with the quadratic time trend more reliable because the estimate of the impact of the education policy on years of schooling—that the education policy increases schooling by about one year for men—is in accordance with the findings of Kirdar et al. (2013), who analyze the impact of the same education policy on years of schooling with a much longer time frame of data using the Turkish Demographic and Health Surveys.

The results of the estimation of equation (4), which accounts for tenure, are given in Table 7 (which includes a linear functional form for year-of-birth) and Table 8 (which includes a quadratic functional form for year-of-birth). In the specification with tenure, accounting for year-of-birth with a linear or a quadratic functional form makes little difference in both OLS and 2SLS estimation of the wage equation. According to the OLS estimates, an extra year of schooling increases wages by 3.6 percent, whereas according to the 2SLS estimates, an extra year of schooling increases wages by 17 percent.

It is not surprising that in the specifications that control for tenure, the estimated returns to education are smaller because, otherwise, education captures also the effect of tenure. However, it is not clear which specification is to be preferred because tenure is itself endogenous and affected by education. In other words, giving a person an extra year of schooling would also change his/her tenure level at a certain level of age.

An important concern about our identification strategy is that the education policy could also change the employment patterns of the 15- to 24-year-olds, as well as their wages. For instance, if the education reform induced an individual to stay in high school – who would otherwise work – the education reform would also change the composition of the people before and after the policy whose wages we compare. To investigate this possibility, we ran a regression of employment status on the policy dummy variable as well as a number of control variables including a quadratic year of birth time trend, age group dummy variable, calendar year dummies, relationship to the household head and relationship status. The results of this regression are presented in Table 9. In panel (a), the sample includes all 15- to 24-year-olds, whereas in panel (b) the sample is restricted to 20- to 24-year-olds only. As can be seen from the table, the education reform has a negative effect on employment probability for both samples. In panel (a), for the total sample of 15- to 24-year-olds, the education policy decreases the employment probability by almost 6 percentage points.

VI. Conclusion

This study estimated the causal effect of education on wages in Turkey. For this purpose, we exploited the exogenous change in schooling outcomes as a result of the education policy reform in 1997, which extended the duration of compulsory schooling from five to eight years. A regression-discontinuity design is conducted to identify the effect of this exogenous change in years of schooling on wages. We employed a number of alternative empirical specifications, in particular with respect to the polynomial form the year-of-birth time trend, to disentangle the effect of the education policy from the secular time trend in wages.

We find that the ordinary least squares (OLS) estimate of the returns to schooling is 0.035; in other words, an extra year of schooling increases wages by 3.5 percent in Turkey. However, the two-stage-least-squares estimate is much higher: according to this, an extra year of schooling increases wages by 17 percent in Turkey. This estimate for Turkey is higher than those estimated for the US or the UK. In a developing country context – where capital is relatively scarce – we would expect the returns to schooling to be higher. Moreover, while the "compliers" – those who are induced to change their behavior by the instrument – in the studies using compulsory schooling laws in the U.S. (e.g., Angrist and Krueger [1991]) are likely to come from the lower end of the ability distribution (Rosenzweig and Wolpin, 2000), the compliers in our study are much more representative of the total population due to the high drop-out rates after compulsory schooling in Turkey (Kırdar et al., 2013). Hence, the compliers in our sample could face higher returns to schooling.

Since the compliers in our study are much more representative of the total population, our LATE is close to the ATE – as in the case of Oreopoulos (2006). Assuming a linear relationship between log wage and schooling, this allows us to compare the OLS and 2SLS estimates without much concern about the representativeness of the compliers. That the 2SLS estimate is much higher than the OLS estimate points out to an acute endogeneity problem in the OLS estimate of the returns to schooling. This suggests that the measurement error bias – which would attenuate the OLS estimate of returns to schooling – is likely to be very important in the Turkish setting whereas the omitted variable bias – which would inflate the OLS estimate – is not very important. That the omitted variable bias is not important in the Turkish setting is very plausible because school attendance, as well as success in school, in Turkey is more of a result of opportunities than a result of ability – compared to those in Western countries.

Another interesting feature of the experiment in our study is that it changes schooling behavior in a wide range of schooling levels. Since the duration of the extension is three years, it affects behavior in the 6^{th}, 7^{th} and 8^{th} grades. In addition, strong spillover effects of the policy on high school grade levels are reported (Kırdar et al., 2013) – which means that the policy affects schooling behavior in high school grade levels (9^{th}, 10^{th} and 11^{th} grades) as well. This implies that our estimate of the returns to schooling is an average of the returns to schooling in various grade levels. Therefore, to the degree that there are non-linearities in the wage-schooling locus, it is easier to interpret our estimate of the returns to schooling as the effect of an additional year of schooling on wages than the estimates in the US or UK setting (see, e.g., Angrist and Krueger [1991] and Oreopoulos [2006]) where the returns to schooling are estimated at certain age levels – which may be quite different from the returns to schooling at other ages.

At the same time, there are important limitations of our analysis due to data limitations. The oldest birth-cohort that is affected by the new policy was 24 years old in 2010, the latest year in our data. Therefore, we need to restrict our analysis to 15- to 24-year-olds. However, for this age group, the education reform affected wages also through its effect on employment outcomes. Our analysis also reveals that the education reform decreased the employment probability for this age group, and therefore the occurrence of positive wages. This could potentially cause an important bias in our estimates.

Figure 1: Hourly Wage Profiles for Men (TIES, 2005)

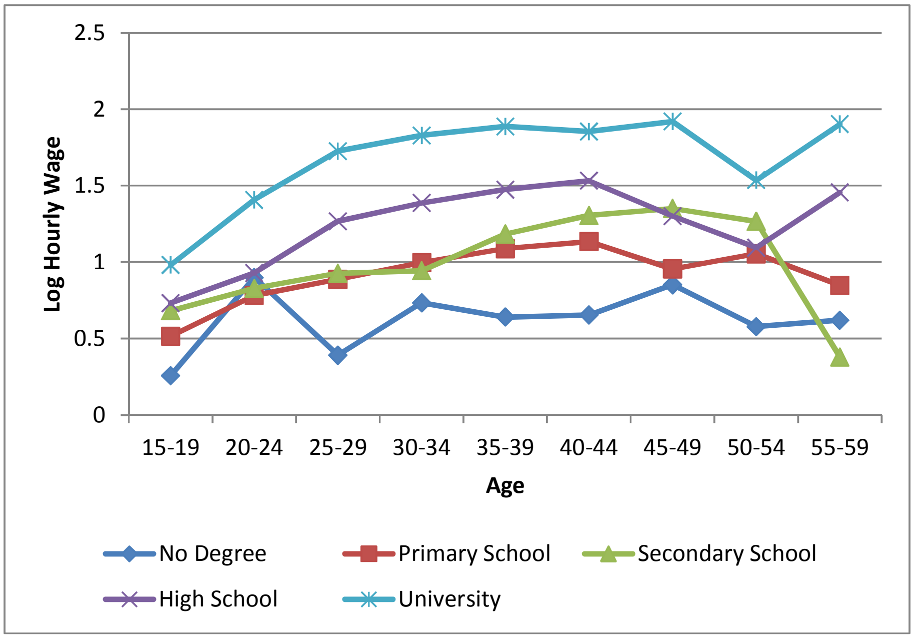

Figure 2: Relationship between Mean Log Hourly Wages and Years of Education (Men Aged 30-49 in TIES, 2005)

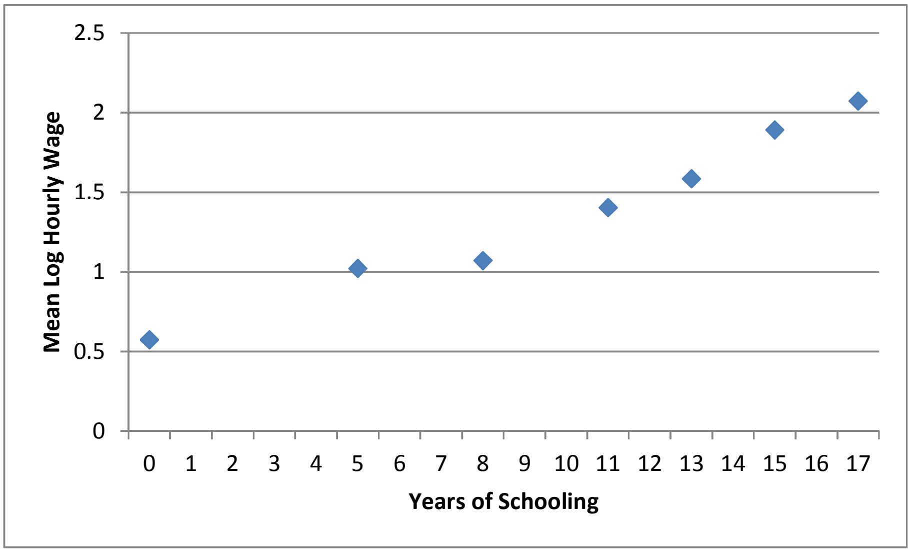

Table 1: Structure of Data: Year-of-Birth Groups vs. Calendar Year

	Year									
	2002	2003	2004	2005	2006	2007	2008	2009	2010	Total
Year of Birth										
1978-82	637	0	0	0	0	0	0	0	0	637
1979-83	0	1,141	0	0	0	0	0	0	0	1,141
1980-84	0	0	410	0	0	0	0	0	0	410
1981-85	0	0	0	424	0	0	0	0	0	424
1982-86	0	0	0	0	430	0	0	0	0	430
1983-87	407	0	0	0	0	453	0	0	0	860
1984-88	0	570	0	0	0	0	430	0	0	1,000
1985-89	0	0	194	0	0	0	0	542	0	736
1986-90	0	0	0	256	0	0	0	0	538	794
1987-91	0	0	0	0	250	0	0	0	0	250
1988-92	0	0	0	0	0	241	0	0	0	241
1989-93	0	0	0	0	0	0	283	0	0	283
1990-94	0	0	0	0	0	0	0	332	0	332
1991-95	0	0	0	0	0	0	0	0	411	411
Total	1,044	1,711	604	680	680	694	713	874	949	7,949

Notes: The sample includes 15- to 24-year-old men for whom wage data are available.

Table 2: Value of the Policy Variable over Birth-Year Cohorts

	Value of "Policy" Variable						
	0	0.2	0.4	0.6	0.8	1	Total
Year of Birth							
1978-82	637	0	0	0	0	0	637
1979-83	1,141	0	0	0	0	0	1,141
1980-84	410	0	0	0	0	0	410
1981-85	424	0	0	0	0	0	424
1982-86	0	430	0	0	0	0	430
1983-87	0	0	860	0	0	0	860
1984-88	0	0	0	1,000	0	0	1,000
1985-89	0	0	0	0	736	0	736
1986-90	0	0	0	0	0	794	794
1987-91	0	0	0	0	0	250	250
1988-92	0	0	0	0	0	241	241
1989-93	0	0	0	0	0	283	283
1990-94	0	0	0	0	0	332	332
1991-95	0	0	0	0	0	411	411
Total	2,612	430	860	1,000	736	2,311	7,949

Notes: The sample includes 15- to 24-year-old men for whom wage data are available.

Table 3: Descriptive Statistics

	Mean	St. Dev.	Min	Max	N
Log (Wage)	-0.063	0.784	-8.450	4.810	7949
Years of Education	8.392	3.074	0	17	7949
Education Policy	0.494	0.413	0	1	7949
Age Group					
15-19	0.370	0.483	0	1	7949
20-24	0.630	0.483	0	1	7949
Tenure	1.942	2.237	0	16	7146
Calendar Year					
2002	0.131	0.338	0	1	7949
2003	0.215	0.411	0	1	7949
2004	0.076	0.265	0	1	7949
2005	0.086	0.280	0	1	7949
2006	0.086	0.280	0	1	7949
2007	0.087	0.282	0	1	7949
2008	0.090	0.286	0	1	7949
2009	0.110	0.313	0	1	7949
2010	0.119	0.324	0	1	7949
Relationship to Household Head					
Household Head	0.093	0.290	0	1	7949
Son of Household Head	0.835	0.372	0	1	7949
Other	0.073	0.260	0	1	7949
Relatioship Status					
Partnered	0.154	0.361	0	1	7949
Single	0.846	0.361	0	1	7949
Employment Type					
Salary-earner	0.805	0.397	0	1	7949
Wage-earner	0.136	0.343	0	1	7949
Business-owner	0.013	0.111	0	1	7949
Self-employed	0.046	0.210	0	1	7949
Type of Occupation					
Upper-level Administrative	0.034	0.181	0	1	7949
Professional	0.027	0.163	0	1	7949
Assistant Professional	0.057	0.231	0	1	7949
Office and Customer Services	0.063	0.243	0	1	7949
Services and Sales Workers	0.250	0.433	0	1	7949
Skilled Workers in Agriculture, Forestry	0.002	0.046	0	1	7949
Artisans	0.301	0.459	0	1	7949
Technicians/Operators	0.104	0.305	0	1	7949
Unskilled Workers	0.161	0.367	0	1	7949
Sector of Employment					
Agriculture, Forestry, Fishing	0.022	0.147	0	1	7949
Mining	0.004	0.061	0	1	7949
Manufacturing	0.309	0.462	0	1	7949
Electricity, Gas, and Water Services	0.003	0.054	0	1	7949
Construction	0.093	0.291	0	1	7949
Sales, Repairs	0.256	0.437	0	1	7949
Hotels and Restaurants	0.086	0.280	0	1	7949
Shipping, Telecommunications	0.059	0.235	0	1	7949
Financial Services	0.006	0.075	0	1	7949
Real Estate Businesses	0.050	0.217	0	1	7949
Public Administration, Defense	0.013	0.112	0	1	7949
Education	0.011	0.106	0	1	7949
Health, Social Services	0.089	0.284	0	1	7949

Table 4: Relationship of Education with Annual and Hourly Earnings, Annual and Weekly Hours of Work, and Months Worked (TIES, 2005)

	Dependent Variable				
	Log Annual Earnings	Log Hourly Earnings	Log Annual Hours	Months Worked	Log Weekly Hours
Education Coefficients	0.102***	0.096***	0.005*	0.140***	-0.014***
	(0.004)	(0.003)	(0.003)	(0.012)	(0.002)
No. obs	5040	5014	5108	5136	5136
R-Squared	0.246	0.224	0.040	0.093	0.017

Notes: Education is measured as years of schooling. The education coefficients come fromregressions of the dependent variables on a linear education term, as well as a cubic polynomial of potential experience (age-years of schooling-6). Samples include 15 to 64 year old men.

Table 5: Estimation Results with a Linear Time Trend for Year of Birth

	Reduced Form		OLS	2SLS
	Years of Education	Log Wage	Log Wage	Log Wage
	(1)	(2)	(3)	(4)
Years of Education			0.0304***	0.301***
			[0.00395]	[0.0814]
Policy Dummy	0.608***	0.183***		
	[0.128]	[0.0317]		
Year of Birth Cohort Trend	0.315***	0.196***	0.198***	0.102***
	[0.0962]	[0.0252]	[0.0328]	[0.0366]
Age Effects (Baseline: 15-19 age group)				
20-24 age group	2.674***	1.491***	1.387***	0.687***
	[0.496]	[0.129]	[0.163]	[0.231]
Year Effects (Baseline: 2002, 2003)				
2004	-0.129	-0.114**	-0.111**	-0.0749
	[0.181]	[0.0427]	[0.0512]	[0.0593]
2005	-0.637**	-0.214***	-0.193**	-0.0228
	[0.221]	[0.0643]	[0.0837]	[0.0953]
2006	-0.854**	-0.468***	-0.432***	-0.211*
	[0.325]	[0.0902]	[0.117]	[0.122]
2007	-1.199**	-0.613***	-0.557***	-0.252
	[0.488]	[0.133]	[0.165]	[0.155]
2008	-1.370**	-0.978***	-0.909***	-0.567***
	[0.503]	[0.142]	[0.178]	[0.174]
2009	-1.691**	-1.107***	-1.019***	-0.599***
	[0.618]	[0.168]	[0.216]	[0.219]
2010	-2.264***	-1.301***	-1.191***	-0.620**
	[0.717]	[0.194]	[0.248]	[0.262]
Relation to Household Head (Baseline: Other)				
Household Head	0.406***	0.101**	0.0897**	-0.0207
	[0.131]	[0.0337]	[0.0321]	[0.0470]
Son of Household Head	0.305**	-0.0956***	-0.106***	-0.187***
	[0.119]	[0.0205]	[0.0206]	[0.0516]
Relationship Status (Baseline: Single)				
Partnered	-1.070***	0.00818	0.0385	0.330***
	[0.101]	[0.0238]	[0.0242]	[0.0945]
Type of Employment (Baseline: Salary Earner)				
Wage Earner	-0.927***	-0.0591	-0.0322	0.219***
	[0.0907]	[0.0348]	[0.0355]	[0.0843]
Business Owner	-0.721**	0.720***	0.742***	0.937***
	[0.287]	[0.0736]	[0.0766]	[0.125]
Self-Employed	-0.840***	0.117*	0.143**	0.370***
	[0.214]	[0.0655]	[0.0646]	[0.0868]

Continued next page

Table 5, cont'd

Occupation (Baseline: Upper-level administrative)				
Professional	2.056***	0.432***	0.371***	-0.186
	[0.420]	[0.0895]	[0.0968]	[0.273]
Assistant Professional	0.455*	0.0297	0.0166	-0.107
	[0.252]	[0.0800]	[0.0836]	[0.137]
Office and Customer Services	0.0918	-0.125	-0.127	-0.153
	[0.246]	[0.0892]	[0.0915]	[0.128]
Services and Sales Workers	-1.104***	-0.313***	-0.279***	0.0191
	[0.205]	[0.0869]	[0.0877]	[0.118]
Skilled Workers in Agriculture, Forestry	-0.597	-0.188	-0.182	-0.00831
	[0.542]	[0.282]	[0.281]	[0.307]
Artisans	-2.203***	-0.294***	-0.225**	0.369*
	[0.207]	[0.0823]	[0.0844]	[0.195]
Technicians/Operators	-1.813***	-0.144*	-0.0895	0.400**
	[0.250]	[0.0810]	[0.0851]	[0.179]
Unskilled Workers	-2.178***	-0.329***	-0.263**	0.326*
	[0.176]	[0.0896]	[0.0892]	[0.186]
Sector of Employment (Baseline: Agriculture, Forestry, Fishing)				
Mining	0.0667	0.368	0.359	0.348
	[0.800]	[0.247]	[0.245]	[0.294]
Manufacturing	0.469	0.157**	0.135**	0.0159
	[0.301]	[0.0522]	[0.0475]	[0.0858]
Electricity, Gas, and Water Services	1.583**	0.288	0.239	-0.187
	[0.581]	[0.172]	[0.168]	[0.192]
Construction	0.914***	0.126**	0.0895*	-0.149
	[0.288]	[0.0542]	[0.0487]	[0.106]
Sales, Repairs	0.620**	-0.0631	-0.0898	-0.249***
	[0.271]	[0.0621]	[0.0571]	[0.0835]
Hotels and Restaurants	0.231	0.0389	0.0256	-0.0306
	[0.309]	[0.0630]	[0.0583]	[0.0813]
Shipping, Telecommunications	0.715**	0.0500	0.0188	-0.165
	[0.287]	[0.0858]	[0.0825]	[0.107]
Financial Services	1.946***	0.251**	0.186*	-0.334*
	[0.511]	[0.112]	[0.105]	[0.183]
Real Estate Businesses	1.336***	-0.0160	-0.0649	-0.417***
	[0.371]	[0.0671]	[0.0623]	[0.157]
Public Administration, Defense	1.790***	0.621***	0.562***	0.0836
	[0.488]	[0.104]	[0.104]	[0.240]
Education	2.503***	0.332***	0.250**	-0.421
	[0.369]	[0.108]	[0.109]	[0.281]
Health, Social Services	-0.0725	-0.199**	-0.204**	-0.177**
	[0.260]	[0.0761]	[0.0734]	[0.0770]
Constant	4.467***	-2.622***	-2.804***	-3.964***
	[1.257]	[0.332]	[0.402]	[0.509]
Number of Observations	7,949	7,949	7,949	7,949
R-squared	0.230	0.242	0.252	-0.614
Notes: Robust standard errors in brackets; *** p<0.01, ** p<0.05, * p<0.1				

Table 6: Estimation Results with a Quadratic Time Trend for Year of Birth

	Reduced Form		OLS	2SLS
	Years of Education	Log Wage	Log Wage	Log Wage
	(1)	(2)	(3)	(4)
Years of Education			0.0304***	0.200***
			[0.00397]	[0.0471]
Policy Dummy	1.004***	0.201***		
	[0.0962]	[0.0505]		
Year of Birth Cohort Trend	0.154***	0.189***	0.215***	0.158***
	[0.0280]	[0.0273]	[0.0371]	[0.0300]
Year of Birth Cohort Trend Squared	0.00715***	0.000322	-0.000894	-0.00111**
	[0.000842]	[0.000615]	[0.000635]	[0.000464]
Age Effects (Baseline: 15-19 age group)				
20-24 age group	2.945***	1.503***	1.360***	0.915***
	[0.101]	[0.123]	[0.196]	[0.154]
Year Effects (Baseline: 2002, 2003)				
2004	-0.127	-0.114**	-0.111**	-0.0882**
	[0.131]	[0.0442]	[0.0463]	[0.0380]
2005	-0.651***	-0.215***	-0.192**	-0.0847
	[0.0495]	[0.0654]	[0.0827]	[0.0674]
2006	-0.919***	-0.471***	-0.427***	-0.287***
	[0.0676]	[0.0892]	[0.123]	[0.0900]
2007	-1.327***	-0.618***	-0.547**	-0.353***
	[0.0925]	[0.128]	[0.182]	[0.120]
2008	-1.585***	-0.988***	-0.891***	-0.671***
	[0.142]	[0.138]	[0.198]	[0.134]
2009	-1.997***	-1.120***	-0.992***	-0.722***
	[0.142]	[0.165]	[0.237]	[0.165]
2010	-2.685***	-1.320***	-1.151***	-0.783***
	[0.165]	[0.191]	[0.277]	[0.197]
Relation to Household Head (Baseline: Other)				
Household Head	0.397**	0.101***	0.0907**	0.0217
	[0.132]	[0.0334]	[0.0317]	[0.0337]
Son of Household Head	0.303**	-0.0957***	-0.105***	-0.156***
	[0.119]	[0.0204]	[0.0206]	[0.0332]
Relationship Status (Baseline: Single)				
Partnered	-1.076***	0.00791	0.0399	0.223***
	[0.101]	[0.0237]	[0.0243]	[0.0637]
Type of Employment (Baseline: Salary Earner)				
Wage Earner	-0.921***	-0.0588	-0.0326	0.125**
	[0.0905]	[0.0346]	[0.0354]	[0.0531]
Business Owner	-0.739**	0.719***	0.744***	0.867***
	[0.289]	[0.0727]	[0.0763]	[0.113]
Self-Employed	-0.837***	0.118*	0.143**	0.285***
	[0.215]	[0.0656]	[0.0648]	[0.0878]

Continued next page.

Table 6, cont'd

Occupation (Baseline: Upper-level administrative)				
Professional	2.056***	0.432***	0.370***	0.0213
	[0.421]	[0.0895]	[0.0969]	[0.149]
Assistant Professional	0.451*	0.0296	0.0168	-0.0606
	[0.253]	[0.0798]	[0.0837]	[0.104]
Office and Customer Services	0.0857	-0.126	-0.127	-0.143
	[0.247]	[0.0888]	[0.0917]	[0.108]
Services and Sales Workers	-1.109***	-0.313***	-0.279***	-0.0914
	[0.206]	[0.0865]	[0.0877]	[0.128]
Skilled Workers in Agriculture, Forestry	-0.616	-0.189	-0.176	-0.0656
	[0.544]	[0.281]	[0.282]	[0.297]
Artisans	-2.209***	-0.294***	-0.225**	0.147
	[0.207]	[0.0820]	[0.0845]	[0.171]
Technicians/Operators	-1.821***	-0.145*	-0.0883	0.219
	[0.253]	[0.0805]	[0.0850]	[0.167]
Unskilled Workers	-2.185***	-0.329***	-0.262**	0.107
	[0.176]	[0.0892]	[0.0890]	[0.165]
Sector of Employment (Baseline: Agriculture, Forestry, Fishing)				
Mining	0.0727	0.369	0.361	0.354
	[0.790]	[0.247]	[0.244]	[0.255]
Manufacturing	0.486	0.158***	0.135**	0.0605
	[0.303]	[0.0522]	[0.0472]	[0.0598]
Electricity, Gas, and Water Services	1.590**	0.289	0.238	-0.0289
	[0.581]	[0.172]	[0.168]	[0.181]
Construction	0.925***	0.126**	0.0906*	-0.0588
	[0.289]	[0.0544]	[0.0481]	[0.0651]
Sales, Repairs	0.633**	-0.0625	-0.0892	-0.189***
	[0.273]	[0.0619]	[0.0569]	[0.0654]
Hotels and Restaurants	0.249	0.0397	0.0253	-0.0100
	[0.309]	[0.0628]	[0.0582]	[0.0581]
Shipping, Telecommunications	0.721**	0.0503	0.0209	-0.0938
	[0.288]	[0.0858]	[0.0816]	[0.0818]
Financial Services	1.971***	0.252**	0.185	-0.141
	[0.511]	[0.112]	[0.104]	[0.134]
Real Estate Businesses	1.357***	-0.0150	-0.0651	-0.286**
	[0.374]	[0.0668]	[0.0623]	[0.112]
Public Administration, Defense	1.803***	0.622***	0.562***	0.262
	[0.491]	[0.104]	[0.104]	[0.170]
Education	2.506***	0.332***	0.251**	-0.169
	[0.370]	[0.108]	[0.109]	[0.193]
Health, Social Services	-0.0599	-0.198**	-0.204**	-0.186***
	[0.261]	[0.0764]	[0.0729]	[0.0684]
Constant	5.018***	-2.597***	-2.859***	-3.600***
	[0.417]	[0.307]	[0.449]	[0.391]
Observations	7,949	7,949	7,949	7,949
R-squared	0.231	0.242	0.252	-0.088
Notes: Robust standard errors in brackets; *** p<0.01, ** p<0.05, * p<0.1				

Table 7: Estimation Results with Tenure and a Linear Time Trend for Year of Birth

	Reduced Form		OLS	2SLS
	Years of Education	Log Wage	Log Wage	Log Wage
	(1)	(2)	(3)	(4)
Years of Education			0.0356***	0.169***
			[0.00350]	[0.0530]
Policy Dummy	0.783***	0.133***		
	[0.147]	[0.0365]		
Tenure	-0.252***	0.0355***	0.0445***	0.0780***
	[0.0398]	[0.00549]	[0.00577]	[0.0182]
Tenure Squared	0.00686	-0.00264***	-0.00290***	-0.00380***
	[0.00398]	[0.000434]	[0.000467]	[0.000986]
Year of Birth Cohort Trend	0.357***	0.176***	0.171***	0.115***
	[0.0653]	[0.0232]	[0.0274]	[0.0232]
Age Effects (Baseline: 15-19 age group)				
20-24 age group	3.074***	1.354***	1.229***	0.834***
	[0.340]	[0.123]	[0.140]	[0.154]
Year Effects (Baseline: 2002, 2003)				
2004	-0.261*	-0.108***	-0.0996**	-0.0635**
	[0.137]	[0.0287]	[0.0336]	[0.0273]
2005	-0.799***	-0.167***	-0.138**	-0.0316
	[0.148]	[0.0517]	[0.0584]	[0.0528]
2006	-1.106***	-0.352***	-0.307***	-0.165**
	[0.224]	[0.0756]	[0.0887]	[0.0665]
2007	-1.456***	-0.482***	-0.419**	-0.236**
	[0.371]	[0.129]	[0.141]	[0.0940]
2008	-1.853***	-0.805***	-0.722***	-0.491***
	[0.354]	[0.123]	[0.139]	[0.105]
2009	-2.203***	-0.884***	-0.782***	-0.511***
	[0.425]	[0.146]	[0.168]	[0.126]
2010	-2.760***	-1.040***	-0.914***	-0.573***
	[0.500]	[0.173]	[0.196]	[0.156]
Relation to Household Head (Baseline: Other)				
Household Head	0.452***	0.126***	0.111***	0.0491
	[0.147]	[0.0282]	[0.0259]	[0.0396]
Son of Household Head	0.327***	-0.0799***	-0.0918***	-0.135***
	[0.101]	[0.0218]	[0.0203]	[0.0305]
Relationship Status (Baseline: Single)				
Partnered	-0.933***	-0.00490	0.0272	0.153***
	[0.0933]	[0.0202]	[0.0210]	[0.0587]
Type of Employment (Baseline: Salary Earner)				
Wage Earner	-0.820***	-0.0888**	-0.0598	0.0500
	[0.0963]	[0.0331]	[0.0339]	[0.0652]
Business Owner	-0.607*	0.704***	0.726***	0.807***
	[0.321]	[0.0731]	[0.0784]	[0.0964]
Self-Employed	-0.617***	0.0757	0.0976	0.180**
	[0.203]	[0.0661]	[0.0656]	[0.0745]

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Table 7, cont'd

Occupation (Baseline: Upper-level administrative)				
Professional	2.381***	0.513***	0.429***	0.110
	[0.403]	[0.0828]	[0.0971]	[0.198]
Assistant Professional	0.578*	0.000866	-0.0194	-0.0969
	[0.284]	[0.0708]	[0.0763]	[0.105]
Office and Customer Services	0.260	-0.0861	-0.0948	-0.130
	[0.270]	[0.0921]	[0.0970]	[0.117]
Services and Sales Workers	-1.006***	-0.340***	-0.304***	-0.170
	[0.256]	[0.0870]	[0.0894]	[0.114]
Skilled Workers in Agriculture, Forestry	-1.116	-0.324	-0.295	-0.135
	[0.685]	[0.315]	[0.316]	[0.314]
Artisans	-2.000***	-0.326***	-0.254***	0.0124
	[0.257]	[0.0828]	[0.0841]	[0.150]
Technicians/Operators	-1.676***	-0.178**	-0.118	0.106
	[0.293]	[0.0812]	[0.0867]	[0.150]
Unskilled Workers	-2.152***	-0.343***	-0.266**	0.0211
	[0.220]	[0.0904]	[0.0907]	[0.157]
Sector of Employment (Baseline: Agriculture, Forestry, Fishing)				
Mining	-0.461	0.407	0.423	0.485*
	[0.802]	[0.264]	[0.258]	[0.263]
Manufacturing	-0.0214	0.217***	0.216***	0.220***
	[0.265]	[0.0600]	[0.0540]	[0.0423]
Electricity, Gas, and Water Services	1.053	0.391**	0.356**	0.213
	[0.739]	[0.154]	[0.152]	[0.163]
Construction	0.438*	0.157**	0.139**	0.0832*
	[0.208]	[0.0678]	[0.0624]	[0.0474]
Sales, Repairs	0.176	-0.0192	-0.0270	-0.0490
	[0.234]	[0.0674]	[0.0620]	[0.0459]
Hotels and Restaurants	-0.375	0.0728	0.0852	0.136**
	[0.271]	[0.0805]	[0.0741]	[0.0667]
Shipping, Telecommunications	0.216	0.156*	0.146*	0.120*
	[0.234]	[0.0780]	[0.0745]	[0.0630]
Financial Services	1.060**	0.149	0.112	-0.0303
	[0.436]	[0.180]	[0.169]	[0.111]
Real Estate Businesses	0.954***	0.00968	-0.0261	-0.152**
	[0.304]	[0.0792]	[0.0724]	[0.0654]
Public Administration, Defense	1.310**	0.502***	0.454***	0.281**
	[0.525]	[0.133]	[0.122]	[0.111]
Education	1.593***	0.230	0.171	-0.0392
	[0.401]	[0.131]	[0.119]	[0.0945]
Health, Social Services	-0.359	-0.119	-0.107	-0.0579
	[0.206]	[0.0716]	[0.0689]	[0.0607]
Constant	6.349***	-1.570***	-1.791***	-2.644***
	[0.555]	[0.217]	[0.231]	[0.415]
Number of Observations	7,146	7,146	7,146	7,146
R-squared	0.247	0.272	0.287	0.063
Notes: Robust standard errors in brackets; *** p<0.01, ** p<0.05, * p<0.1				

Table 8: Estimation Results with Tenure and a Quadratic Time Trend for Birth Year

	Reduced Form		OLS	2SLS
	Years of Education	Log Wage	Log Wage	Log Wage
	(1)	(2)	(3)	(4)
Years of Education			0.0356***	0.170***
			[0.00350]	[0.0446]
Policy Dummy	1.050***	0.179***		
	[0.145]	[0.0548]		
Tenure	-0.250***	0.0358***	0.0445***	0.0784***
	[0.0395]	[0.00555]	[0.00576]	[0.0162]
Tenure Squared	0.00677	-0.00266***	-0.00290***	-0.00381***
	[0.00397]	[0.000432]	[0.000464]	[0.000944]
Year of Birth Cohort Trend	0.293***	0.165***	0.171***	0.115***
	[0.0217]	[0.0168]	[0.0268]	[0.0211]
Year of Birth Cohort Trend Squared	0.00523***	0.000911	-2.37e-05	2.03e-05
	[0.00131]	[0.000577]	[0.000584]	[0.000464]
Age Effects (Baseline: 15-19 age group)				
20-24 age group	3.259***	1.387***	1.229***	0.831***
	[0.0906]	[0.0873]	[0.146]	[0.132]
Year Effects (Baseline: 2002, 2003)				
2004	-0.259**	-0.107***	-0.0996**	-0.0632**
	[0.110]	[0.0285]	[0.0336]	[0.0271]
2005	-0.808***	-0.168***	-0.138**	-0.0306
	[0.0646]	[0.0478]	[0.0588]	[0.0497]
2006	-1.150***	-0.359***	-0.307***	-0.163***
	[0.0590]	[0.0611]	[0.0898]	[0.0622]
2007	-1.545***	-0.498***	-0.418**	-0.234***
	[0.104]	[0.0994]	[0.143]	[0.0870]
2008	-2.003***	-0.831***	-0.721***	-0.490***
	[0.161]	[0.0930]	[0.143]	[0.0937]
2009	-2.417***	-0.921***	-0.781***	-0.509***
	[0.124]	[0.116]	[0.173]	[0.116]
2010	-3.056***	-1.091***	-0.913***	-0.570***
	[0.181]	[0.135]	[0.205]	[0.143]
Relation to Household Head (Baseline: Other)				
Household Head	0.447***	0.125***	0.111***	0.0485
	[0.147]	[0.0283]	[0.0258]	[0.0368]
Son of Household Head	0.327***	-0.0800***	-0.0918***	-0.136***
	[0.100]	[0.0218]	[0.0203]	[0.0286]
Relationship Status (Baseline: Single)				
Partnered	-0.937***	-0.00572	0.0272	0.154***
	[0.0933]	[0.0201]	[0.0210]	[0.0532]

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Table 8, cont'd

Type of Employment (Baseline: Salary Earner)				
Wage Earner	-0.815***	-0.0878**	-0.0598	0.0511
	[0.0955]	[0.0332]	[0.0340]	[0.0585]
Business Owner	-0.619*	0.702***	0.726***	0.808***
	[0.323]	[0.0721]	[0.0779]	[0.104]
Self-Employed	-0.616***	0.0759	0.0976	0.181**
	[0.204]	[0.0661]	[0.0656]	[0.0804]
Occupation (Baseline: Upper-level administrative)				
Professional	2.383***	0.513***	0.429***	0.107
	[0.403]	[0.0828]	[0.0973]	[0.163]
Assistant Professional	0.577*	0.000647	-0.0194	-0.0977
	[0.284]	[0.0706]	[0.0763]	[0.0967]
Office and Customer Services	0.258	-0.0865	-0.0948	-0.130
	[0.270]	[0.0918]	[0.0970]	[0.114]
Services and Sales Workers	-1.008***	-0.340***	-0.304***	-0.168
	[0.257]	[0.0866]	[0.0893]	[0.125]
Skilled Workers in Agriculture, Forestry	-1.132	-0.326	-0.295	-0.133
	[0.687]	[0.315]	[0.315]	[0.326]
Artisans	-2.004***	-0.327***	-0.254***	0.0150
	[0.258]	[0.0824]	[0.0839]	[0.155]
Technicians/Operators	-1.681***	-0.179**	-0.118	0.108
	[0.295]	[0.0806]	[0.0864]	[0.157]
Unskilled Workers	-2.155***	-0.343***	-0.266**	0.0239
	[0.220]	[0.0902]	[0.0906]	[0.160]
Sector of Employment (Baseline: Agriculture, Forestry, Fishing)				
Mining	-0.456	0.408	0.423	0.485*
	[0.793]	[0.264]	[0.259]	[0.258]
Manufacturing	-0.00904	0.219***	0.216***	0.220***
	[0.266]	[0.0603]	[0.0539]	[0.0419]
Electricity, Gas, and Water Services	1.060	0.392**	0.356**	0.211
	[0.738]	[0.154]	[0.152]	[0.172]
Construction	0.442*	0.158**	0.139**	0.0827*
	[0.209]	[0.0680]	[0.0624]	[0.0479]
Sales, Repairs	0.185	-0.0176	-0.0270	-0.0492
	[0.235]	[0.0674]	[0.0618]	[0.0480]
Hotels and Restaurants	-0.361	0.0752	0.0852	0.137**
	[0.270]	[0.0806]	[0.0740]	[0.0642]
Shipping, Telecommunications	0.219	0.157*	0.146*	0.120*
	[0.232]	[0.0781]	[0.0745]	[0.0649]
Financial Services	1.078**	0.152	0.111	-0.0316
	[0.437]	[0.180]	[0.169]	[0.117]
Real Estate Businesses	0.971***	0.0126	-0.0262	-0.153**
	[0.304]	[0.0791]	[0.0721]	[0.0702]
Public Administration, Defense	1.315**	0.503***	0.454***	0.279***
	[0.526]	[0.133]	[0.122]	[0.105]
Education	1.592***	0.230	0.171	-0.0413
	[0.401]	[0.131]	[0.118]	[0.0912]
Health, Social Services	-0.350	-0.117	-0.107	-0.0574
	[0.205]	[0.0716]	[0.0689]	[0.0631]
Constant	6.296***	-1.579***	-1.791***	-2.653***
	[0.402]	[0.185]	[0.234]	[0.387]
Observations	7,146	7,146	7,146	7,146
R-squared	0.247	0.272	0.287	0.059

Notes: Robust standard errors in brackets; *** p<0.01, ** p<0.05, * p<0.1

Table 9: Effect of the Education Policy on Employment Status

A) 15- to 24-year-olds		B) 20- to 24-year-olds	
Policy Dummy	-0.0574***	Policy Dummy	-0.159**
	[0.0111]		[0.0611]
Year of Birth Trend	-0.0488***	Year of Birth Trend	0.0802***
	[0.00222]		[0.00634]
Year of Birth Trend Squared	-0.00113***	Year of Birth Trend Squared	-0.00154
	[6.57e-05]		[0.00129]
Age Effect (Baseline: 15-19 age group)			
20-24 age group	-0.0752***		
	[0.00776]		
Year Effects (Baseline: 2002, 2003)		Year Effects (Baseline: 2002)	
2004	0.109***	2003-2004	-0.128***
	[0.0105]		[0.00266]
2005	0.217***	2005-2006	-0.177***
	[0.00677]		[0.00549]
2006	0.306***	2007-2008	-0.198***
	[0.00483]		[0.0185]
2007	0.378***	2009-2010	-0.172***
	[0.00664]		[0.0275]
2008	0.504***		
	[0.00711]		
2009	0.624***		
	[0.00879]		
2010	0.722***		
	[0.0102]		
Relation to Household Head (Baseline: Other)		Relation to Household Head (Baseline: Other)	
Household Head	0.170***	Household Head	0.192***
	[0.0186]		[0.0185]
Son of Household Head	-0.0113	Son of Household Head	0.0212
	[0.0100]		[0.0196]
Relationship Status (Baseline: Single)		Relationship Status (Baseline: Single)	
Partnered	0.288***	Partnered	0.281***
	[0.0112]		[0.0128]
Constant	0.603***	Constant	0.371***
	[0.0111]		[0.0197]
Observations	21,437	Observations	8,860
R-squared	0.170	R-squared	0.097

Notes: Robust standard errors in brackets; *** p<0.01, ** p<0.05,

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Appendix A – Control variables

i) Relation to household head: This is categorized into three in the analysis: i) household head, ii) son of household head, iii) other.

ii) Marital Status: This is categorized into two in the analysis: i) partnered (married/cohabiting), ii) single (never married/divorced/ widowed/separated)

iii) Type of Employment: There are 4 categories: i) salary earner, ii) wage earner, iii) business owner, iv) self-employed.

iv) Occupation: There are 9 categories: i) upper-level administrative; ii) professional; iii) assistant professional; iv) office and customer services; v) services and sales workers; vi) skilled workers in agriculture, forestry, etc.; vii) artisans; viii) technicians/operators; ix) unskilled workers.

v) Sector of Employment: There are 13 categories: i) agriculture, forestry, fishing; ii) mining; iii) manufacturing; iv) electricity, gas, and water services; v) construction; vi) sales, repairs; vii) hotels and restaurants, viii) shipping, telecommunications; ix) financial services; x) real estate businesses, xi) public administration, defense; xii) education, xiii) health, social services.