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Abstract

This paper estimates the returns to schooling in Egypt using a policy reform that reduced primary school from 6 to 5 years. As a result of this reform, which was implemented in 1988, compulsory schooling declined from 9 to 8 years. The results indicate that the reform led to a substantial decline in completed years of schooling among the affected cohorts. We exploit this policy change to estimate the causal impact of schooling on wages and find that among men ages 20 to 45 the return of schooling is between 2.0 to 5.7 percent. These estimated returns are much lower than the wage returns estimated in other developing countries with the exception of Turkey where returns to schooling are found to be similarly low.

JEL Classifications: J18, J31, I21, I28

Keywords: Returns to Schooling, Policy Reform, Egypt

ملخص

تقدر هذه الورقة عوائد التعليم المدرسي في مصر باستخدام سياسات الإصلاح التي خفضت سنوات المدارس الابتدائية من 6 الى 5 سنوات. ونتيجة لهذا الإصلاح، والذي تم تنفيذه في عام 1988، انخفض التعليم الإلزامي من 9 الى 8 سنوات. وتشير النتائج إلى أن الإصلاح أدى إلى انخفاض كبير في سنوات الانتهاء من الدراسة بين الفئات المتضررة. نقوم باستغلال هذا التغيير في السياسة لتقدير تأثير مسبب للتعليم على الأجور ونجد أن من بين الرجال الذين تتراوح أعمارهم بين 20-45 يتراوح عائد التعليم لديهم بين 2.0 إلى 5.7 في المئة. هذه العائدات المقدرة هي أقل بكثير من عوائد الأجور المقدرة في البلدان النامية الأخرى باستثناء تركيا حيث تم العثور على عوائد تعليم منخفضة على نحو مماثل.

1. Introduction

The relationship between schooling and wages is one of the most studied topics in labor economics. While many studies in the context of both the developed and developing world find that earnings increase with schooling, more recent research aims to understand whether the observed relationship is causal. This is a challenging task since unobserved factors such as ability and motivation may affect both outcomes and unless one is able to properly account for them, the returns to schooling estimates may be flawed. To overcome the identification problem and estimate the returns to schooling consistently, several studies in the literature use institutional features of the schooling system or changes in it as a source of exogenous variation in years of schooling.

One of the earlier studies in this strand of literature is by Angrist and Krueger (1991) who use the quarter-of-birth as an instrument for completed years of schooling. The compulsory schooling laws in the US mandate different school start age for children born in different months of the year, which causes an exogenous change in completed years of schooling. Another set of studies from the US (e.g., Acemoglu and Angrist, 2001; Lochner and Moretti, 2004; Lleras-Muney, 2005) use the regional variation in schooling laws. The underlying identification assumptions are that the timing of the change in education policies is independent of the potential outcomes, the change in the education policies is independent of the characteristics of the regions, and the trends in the outcome variables are the same across various regions in the absence of the policy. Harmon and Walker (1995), on the other hand, use the changes in compulsory schooling duration in the U.K. in 1947 and in 1973. Their identification strategy is based on comparison of birth cohorts that are affected by the policy with those that are not. Oreopoulos (2006) uses the same change in compulsory schooling laws in the U.K. and Ireland as in Harmon and Walker but allows for a secular time trend in schooling. He finds returns that are similar in magnitude to those reported by Angrist and Krueger for the US. The time frame of analysis in both Harmon and Walker (1995) and Oreopoulos (2006) coincides with other events in particular World War II that also had a substantial effect on schooling outcomes. Therefore, the estimated returns may reflect the impact of these macro events as well. More recent work for the UK by Devereux and Hart (2010) and for Germany by Pischke and von Wachter (2008) find much lower returns. Devereux and Hart (2010) find no evidence of any positive return for women and a return for men of 4–7 percent. Pischke and von Wachter (2008) report IV estimates that suggest zero return in the German context. Thus, the available evidence in developed countries for the estimated returns to schooling vary widely, ranging from 15 percent (Oreopoulos, 2006) to no return.

There is a small but growing literature concerning the estimation of returns to schooling based on exogenous sources of variation in developing countries. Duflo (2001) uses the massive school construction program in Indonesia in the 1970s to estimate the returns to schooling. Spohr (2003) uses the change in the duration of compulsory schooling in Taiwan while Fang et al. (2012) for China. Aydemir and Kirdar (2015) also use the increase in compulsory schooling from 5 to 8 years in Turkey in 1987 to estimate the returns to schooling. Interestingly, the variation in the reported estimates of returns to schooling from these studies is just as large as the variation for develop countries, about zero returns for men in the Turkish context to a staggering 20 percent in the Chinese context.

We contribute to the returns to schooling literature by estimating the causal effect of schooling on earnings in Egypt. We use the reduction in the duration of primary school from 6 to 5 years in 1988 as an instrumental variable for years of schooling. An interesting feature of the policy change in the Egyptian context is that the compulsory schooling change resulted in a reduction in total years of schooling while most other studies use increases in school duration as an instrument. The advantage of the policy change in the Egyptian context, as discussed further in the following sections, is that there is no issue of a complier problem that confronts other studies. Moreover, those affected by the policy continue to receive the same degree, i.e. there is no sheep-skin effect, while the policy potentially changes the level of human capital. Our identification strategy is similar to other studies in the literature in that we compare birth cohorts that are affected by the policy change with those that are not.

Our findings show that the policy change led to a substantial decline in years of schooling among the affected cohorts. Using the policy change as an instrument, we find that among men age 20 to 45, the return to an extra year of schooling is between 2-5.7 percent. These estimates corroborate the evidence provided by Aydemir and Kirdar (2015) that causal effect of schooling on earnings may not be as high as those reported so far for developing countries. These results also add to the growing number of studies in the literature that question earlier findings of large returns to education in both developed and developing country contexts.

The study is organized as follows. In Section 2, we discuss the conceptual framework, which is followed by Section 3 that discusses our identification strategy. Section 4 provides an overview of education system in Egypt while Section 5 discusses the data used in the analysis. Results are presented in Section 6 followed by the conclusion in Section 7.

2. Conceptual Framework

We use the Mincer earnings equation to estimate the returns to schooling. This framework is a hedonic wage function, which shows how the labor market prices productivity enhancing traits such as schooling and experience. In particular, the Mincer earnings function specifies the logarithm of wages as a linear of function of schooling, experience, experience squared, and potentially other control variables. In this specification, the coefficient of schooling gives the “rate of return to schooling”. This specification is derived from an underlying model of compensating differences (Mincer, 1974), where given earnings and costs of schooling, the lifetime values of investing in different levels of schooling are set equal. The interest rate that achieves this equality is called the internal rate of return. Moreover, given the assumptions of the Mincer model - perfect certainty, perfect credit markets, no heterogeneity across individuals in terms of abilities and opportunities -, the internal rate of return is also equal to the coefficient of schooling (i.e. the rate of return to schooling).

There are a number of assumptions that the Mincer earnings equation makes. First, it postulates a linear relationship between schooling and the logarithm of wages. Card (1999) claims that this assumption is supported by empirical results for the US; however, Heckman et al. (2007) reports that several of the implications of the Mincer earnings equation are not supported by US earnings data. Second, the Mincer earnings framework imposes separability between the schooling and experience variables. In other words, it rules out that returns to schooling change with the experience level. Similar to other reduced-form studies, we also assume that these two assumptions hold.

Our goal is to establish the causal link between schooling and wages in the Egyptian context. The well-known problem in establishing this causal link is the endogeneity of schooling within the Mincer earnings equation, brought about by omitted variables like ability, motivation, parental connections, and so forth. To solve this problem, we need a source of exogenous variation in schooling—an instrument—which is correlated with schooling but not correlated with other determinants of wages. More precisely, the instrument needs to be independent of wages conditional on covariates and have no direct effect on wages, other than through its effect in determining years of schooling.

Using the reduction in compulsory schooling as an instrument, we carry out two-stage least squares (2SLS) estimation in addition to OLS estimation. Conceptually, it is important to understand the way OLS and 2SLS estimates compare. Many of the earlier studies in the literature find that 2SLS estimates of the returns to schooling are either higher than or not much different from the OLS estimates. Omitted variable bias resulting from the unobserved ability variable and measurement error in the schooling variable are the two main arguments used in explaining the difference between the OLS and 2SLS estimates. According to the omitted variable bias argument, one would expect the 2SLS coefficient to be lower than OLS coefficient because ability is expected to be positively correlated with both schooling and wages. However, the empirical results from earlier studies conducted in the US generally produce higher 2SLS estimates. This has led to the measurement error argument to gain popularity. Angrist and Krueger (1991), for instance, find that in some specifications IV estimates are well above the OLS estimates, whereas in other specifications they are close to OLS estimates. They conclude that ability bias is probably not so important in magnitude whereas the attenuation bias due to measurement error plays a much bigger role in the OLS estimates.

Another prominent explanation for lower OLS estimates as compared to IV estimates is heterogeneity in the 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 behavior as a result of the exogenous source of variation in schooling. The set of compliers may not be representative of the whole population and the estimates derived provide local treatment effects (LATE) for compliers. 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): the marginal individual affected by the policy change becomes more similar to the average individual in the population. Using a much larger set of compliers, Oreopoulos (2006) finds that the 2SLS estimates in the UK 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 Germany where a large fraction of the population were affected by the compulsory school law change. Imbens and Angrist (1994) also note that if there are nonlinearities in the return to schooling, estimating the return via a policy-induced increase at a given schooling level may lead to differences between the estimated LATE and the ATE. Thus, the evidence concerning how IV and OLS estimates compare is mixed. Our estimates from Egypt will contribute to this ongoing discussion.

3. Identification Method and Estimation

We use the reduction of primary school in Egypt from 6 to 5 years as an instrumental variable for years of schooling. More specifically, we exploit the variation in schooling across birth-cohorts induced by this institutional change. There is no reason to expect date-of-birth to be correlated with unobserved variables that may affect wages such as ability, motivation, or parental connections. Therefore, we expect date-of-birth to affect wages through its effect on schooling but we do not expect it to have a direct effect on wages.

We use two-stage least squares estimation to find the causal link between schooling and wages as follows:

$$s_i = \alpha_0 + \alpha_1 D_i + X^{'}\theta + u_i \tag{1}$$

$$\log w_i = \beta_0 + \beta_1 s_i + X^{'}\delta + v_i \tag{2}$$

Here, s denotes the years of schooling, w denotes the wage rate, and D is a dummy variable for the policy. The key parameter of interest is β_1, which denotes the percent change in wages when years of schooling is raised by one. Covariates are shown by X, which include controls for region of birth as well as either controls for age or birth cohort dummies.

As shown by Imbens and Angrist (1994), the 2SLS estimates have local average treatment effect (LATE) interpretation; in other words, they give the treatment effect for those whose treatment status is changed by the instrument—who are called *compliers*. This is important when there is heterogeneity in the returns to schooling across various subpopulations because as LATE is for the compliers only, it may be quite different from the average treatment effect (ATE) for the total population. As discussed in Oreopoulos (2006), the larger the set of compliers is, the closer is LATE to ATE. As will be demonstrated shortly, the policy in Egypt affects a large fraction of the population. Thus, our LATE estimate is not going to be for a small and, therefore, a potentially marginal group. Moreover, there are no "never takers"—those who are treated by the policy but do not change their behavior—due to the nature of the policy. Thus, our LATE estimate for compliers is equal to the treatment effect on the untreated—as shown by Angrist and Pischke (2009).

4. Education System in Egypt

The formal schooling system in Egypt is built on 6+3+3 system, which means six years of primary schooling, three years of lower secondary (*preparatory*) schooling and three years of upper secondary schooling. Up until 1980, compulsory schooling covered only the first tier. In 1981 (Law no. 139), compulsory schooling was increased from six to nine years, which meant that aside from primary schooling, lower secondary schooling became part of compulsory education. However, in 1988, primary schooling was reduced to five years, which effectively reduced years of compulsory schooling from nine to eight years. This decision was reversed in 1999 increasing the duration of primary schooling back to six years and compulsory schooling to nine years.

The 1988 law (no.233), which is the basis for the identification used in this study, changed the duration of compulsory basic education starting from the 1988/89 school year from nine to eight years such that 'the primary stage' became five-years long while the 'the preparatory stage' remained three-years long as before. The implementation of this law started with the fourth and fifth graders in the 1988/89 school year. The law stipulated that the fourth graders would be given the fourth grade curriculum as well as the abridged version of the fifth grade curriculum. The fourth graders would advance to the fifth grade in the 1989/90 school year and would sit for the final-year-exam of the primary-stage, after which they would advance to the first year of the preparatory school. Students enrolling in the fifth grade in the 1988/89 school year would study the same curricula as those enrolling in the 6th grade under the old system and would sit for the final exam of the primary stage with the six graders. The law also stipulated that the total number of weeks of schooling would be increased from 32 weeks to 38 weeks and the school day would be extended by a maximum of 30 percent, which would be achieved by cancelling the multiple-shift system and moving to a full-school-day system. However, the curricula and the textbooks used in the primary as well as the preparatory stage would remain the same.

The reduction in compulsory schooling by one year was motivated by the increase in student population in primary and preparatory stages and the resulting pressure on the schooling infrastructure. By cutting compulsory schooling by one year, it was hoped that class sizes and number of students per teacher, as well as multiple-shifts would reduce. Hence, the change in the law set into motion two effects that potentially have opposite effects on student achievement. Smaller classes and fewer students per teacher would potentially lead to higher student achievement. However, the elimination of the 6th grade meant less contact time and the squeezing of the curricula, which would potentially reduce student achievement - especially of those students coming from less advantageous background. Statistics on pupil per teacher do indeed show some improvement due to the policy change. While in 1987 there were 31.3 primary school students per teacher, this ratio dropped slightly to 29.9 percent in 1988 but quite drastically to 19.7 in 1989.[1] The improvement in 1989 was related to the cut in the 6th grade in that year. However, pupil per teacher ratio increased to 25.1 in 1990 and fluctuated around 23-27 from early to late 1990s. Public investment in education, on the other hand, did not show much of a change remaining around 4.5 percent of GDP in late 80s and early 90s.

Education outcomes in Egypt display a bimodal distribution (see Figure 1) with a substantial proportion of the adult population (15 years and above) either having no or very little education and another substantial proportion at least high school education. According to the 2012 round of the Egypt Labor Market Panel Study (ELMPS) 27.1 percent of the adult population are illiterate. Another 4 percent are literate but have no diploma. Those finishing primary school and middle school each constitute about 11 percent of the population. In contrast, the proportion finishing upper secondary schooling is 30.5 percent and those with at least post-secondary education are 16.9 percent. Even among the younger generation (persons aged 15-34) school attainment remains low, with 28.3 percent of this group not completing compulsory schooling. Rather low compulsory school completion rates can be related in part to weak enforcement. In a similar vein, the extension of compulsory schooling 1981 has failed to bring about a visible improvement in the proportion completing lower secondary schooling (illustrated below).

Figure 2 shows the proportion of the population belonging to various birth cohorts (X-axis) finishing at least the 6th, 8th, 9th, 11th and 12th grades.[2] It is obvious from the graph that the increase in compulsory schooling in 1981, which affected those born in 1970 and later, did not bring about a substantial improvement in the proportion finishing the 9th grade. If enforcement was strong we would have observed a jump in the proportion finishing the 9th grade following the passage of the law. However, what is quite clear from the graph is the reduction in the proportion of persons finishing at least the 12th grade. While up until the 1977 cohort the proportion finishing the 11th grade did not differ from the proportion finishing the 12th grade (implying that very few students dropped out in the 11th grade), the gap between the two rates increased substantially for later cohorts. The drop in the proportion finishing at least the 12th grade, which is around 35 percentage points, is to do with the change in the length of primary schooling. The reduction in the required years of schooling to obtain a high school diploma presents itself with a unique opportunity to study the returns to schooling.

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[1] UNESCO data base: http://data.uis.unesco.org/#

[2] As will be explained shortly, generating information on the highest grade completed is not straightforward. Here, we mechanically generate the highest grade completed by assigning 5 years of primary schooling to 1978-1993 cohorts, and six years to others.

5. Data

We use the 2012 round of the Egypt Labor Market Panel Survey (ELMPS), which provides detailed schooling and labor market information on individuals. The 2012 ELMPS follows individuals originally interviewed in the 1998 survey, with refreshment samples added both in 2006 and 2012 so that the 2012 cross-section is representative of the country at large (Assaad and Kraft, 2013).

The data include information on 7,519 wage earners between the ages of 20 and 45, who constitute our primary focus group. We also test the sensitivity of our results to the imposed age restrictions by studying age groups 25 to 45 and 30 and 40. The lower age limits are set to make sure that we do not include individuals who might still be in school. The upper age limit is due to the structure of the data: persons older than 45 years were not asked of detailed schooling information.

Generating the years of schooling variable – which is key for this study – is not straightforward because what is reported in the data is not the years of schooling but rather the highest level of schooling attended and the highest grade completed in that level. The difficulty of generating years of schooling from this information lies in the fact that we do not know who was subject to 5 years of primary schooling and who to 6 years because this information was asked of a small subset of respondents. Although we know when the law went into force and the birth date of respondents, due to late or early school start, and class repetition two individuals sharing the same birth date might be subject to different rules. The advantage of ELMPS over other data sets such as the Labor Force Survey or the Demographic Health Survey for Egypt is that it provides information on the year that the person entered primary school as well as the number of grades repeated in all stages of schooling. Hence, we have used these two sets of information - the year in which the individual started school and the information on class repetition - to determine the year in which the individual reached the 5^{th} grade in primary school. Based on this information, we, then assigned him or her to either the "treated" or the "non-treated" group.

The wage information concerns both the regular and causal wage earners. For regular wage earners, ELMPS collects information on both the basic monthly wage as well as any supplementary wages received such as bonuses over a period of a year, net of taxes. That share of the lump-sum wage supplement corresponding to a month is computed and added to the basic monthly wage to arrive at full-compensation from work. For casual wage earners, daily wages are inquired. The reference period for hours of work for both the regular and casual workers is the three months preceding the survey date. Information on number of weeks worked in the past three months, days worked per week and hours worked per day and week is collected. Hourly wages are computed by dividing monthly and daily wages into hours worked per month and per day, respectively, which are the average values for the three-month period.[3] Wages correspond to the primary job held.

In Table 1, we provide the basic descriptive statistics concerning our operational sample. Around 62% of our sample is affected by the policy change as indicated by the *treated* variable. In the overall sample of wage earners, the mean years of schooling is 10.7 years. It is, however, significantly higher among females (12.99) than males (10.2). This contrasts with mean years of schooling of 9 years among females and 10.1 years among males when the sample is not restricted to wage earners (results not presented in Table 1). This indicates that the labor force participation is very highly correlated with education among females. This is also reflected in educational attainment fractions presented in Table 1. While, for example, 25% of males have a post secondary

[3] We use the monthly and hourly wages that already exist in the data and that were computed by Caroline Krafft.

degree or higher our operational sample, this fraction is 60% among females. About 83% of our sample consists of males, which indicates that females are less likely to be wage earners. The mean age is 31.67 in the overall sample and there is little difference in the mean age between males and females.

Next, we examine the policy effect on certain schooling outcomes using graphical analysis. Figures 3 to 6 display the change by month-year of birth in average years of schooling, primary school completion, middle school completion, and high school completion. The vertical lines in all figures indicate the time of the discontinuity in January 1978. Quadratic polynomials are fit before and after the discontinuity in all figures. As can be seen in Figure 3, there is a substantial drop in average years of schooling with the policy; it falls roughly from 11.3 to 10.3 years. While a fall in the average years of schooling is expected with the policy, the magnitude of this fall (by about one year) is surprising because it would have to affect everybody for the average fall to be one year—which is not the case because an important fraction of students do not complete primary school in Egypt. Nonetheless, Figures 4 to 6 provide clues about why the fall in average years of schooling is so substantial.

Figure 4 shows an important drop in the fraction of students completing primary school. Quantitatively, this fraction drops roughly from 91 percent to 84 percent. Similarly, the fraction earning a middle school diploma also decreases, roughly from 81 percent to 73 percent, as can be seen in Figure 5. Finally, Figure 6 indicates that the percentage completing high school falls roughly from 74 percent to 68 percent. Presumably, since the new policy increased the academic burden by requiring students to complete the almost same material in a shorter period of time, it affected academic performance in a negative way, thereby decreasing school attainment levels. These significant falls in school attainment levels further contribute to the fall in average years of schooling coming from the reduction of primary school by one year. Consequently, the drop in average years of schooling reaches about one year in total, as can be seen in Figure 3.

6. Results

6.1 First stage results

We first present the first stage regression results to assess the impact of the reform on years of schooling. For this purpose, we first estimate equation (1) for the full sample and then for men and women separately. The policy dummy (*treated*) is equal to 1 for those born in 1978 or later. This definition is motivated by the law change that indicates that those in grade 4 or lower in 1988 are affected by the reduction in compulsory school from 9 to 8 years. If individuals start schooling at age 6, then this implies that those born in 1978 or later are affected by the policy. Since school start age may deviate from the expected age of 6 we also assess the sensitivity of our results to alternative definitions of the policy dummy.

The first set of results given in columns 1 through 6 in Table 2 refers to the specification where covariates include region and age. Age is introduced either as a quadratic (first three columns) or a cubic functional form (columns 4-6). The three columns under each specification employs different age groups; we start with the largest sample that includes individuals aged 20-45, and then move to smaller samples that include 25-45 and 30-40 year-olds. The results show that the coefficient referring to the policy reform (*treated*) has a negative effect, while not always significant, on years of schooling completed for all the specifications and samples. The significance of the coefficient as indicated by the t-statistics reported in the table is stronger for the specification with a cubic in age for the age group 20-45.

Table 3 estimates the first stage regressions for years of schooling replacing age controls with 4- or 5-year cohort dummies. The first three columns define separate cohort dummies for each of the birth year groups 1966-70, 1971-74, 195-79, 1980-83, 1984-87, and 1988-92. Alternative cohort definitions for the last three columns define cohorts 1 to 6 that refer to birth years 1966-70, 1971-74, 1975-78, 1979-82, 1983-87, and 1988-92, respectively. Parallel to the results in Table 2, the effect of policy indicated by the coefficient estimate of *treated* is negative. The coefficients are much more precisely estimated as indicated by larger t-values.

Tables 4a and 4b replicate Table 2 for males and females separately. While the policy is found to have a negative and significant effect on years of schooling among males in most specifications, none of the coefficient estimates for the variable *treated* is significant in Table 4b for women. Moreover, the coefficients are positive in some specifications suggesting that the difference in results between males and females is not driven by the smaller sample size for females. Tables 5a and 5b replicate Table 3 for males and females respectively using cohort dummy specifications. Similar to results in Tables 4a and 4b policy is found to have a significant and negative effect for males but not for females. The coefficient estimates are also stronger for 20-45 and 25-45 age groups. For the rest of the analysis we restrict our attention to males in age groups 20-45 and 25-45 that are found to be affected by the reduction in compulsory schooling from 9 to 8 years.

The analysis above shows that the policy change was associated with a reduction in years of schooling. Next, we turn to the question of which education groups have realized a reduction in their years of schooling. Since the policy reduced primary school from 6 to 5 years, we expect to see a reduction in years of schooling among those with primary school or more education. Table 6 estimates the first stage equation for males age 20-45. The first panel of Table 6 report results from the specification with age controls, while Panel B report results from the specification with cohort dummies. In Panel A, the first six columns refer to the specification with age and age square controls, the last six columns include age cube in the specification. In Panel B, the first six columns use the first cohort definition while the last six columns use the second cohort definition. As we move from samples 1 to 6, individuals who have the highest education level than the rest are successively dropped from the sample. Thus, while sample 1 includes all education groups, sample 2 excludes those with post secondary education or more, sample 3 excludes those with high school education or more, etc. The coefficient estimates for the *treated* variable show that the policy change led to a reduction in years of schooling among all groups with at least elementary (primary school) education while no effect is observed among those with less than elementary education. The results in both panels are largely consistent with each other.

6.2 Definition of policy dummy

As discussed above, the policy variable (*treated* dummy) is defined as 1 for those born in 1978 or later and 0 for others, based on the education law and the assumption that children start school at age six. In this section, we re-estimate the first stage regressions based on alternative cutoff dates for the birth year that designates whether the person is treated or not. This serves two purposes. First, since school start age may differ from six, those affected by the policy may include those born before or after 1978. This creates fuzziness in the cutoff date for the definition of the *treated* dummy. Estimating the first stage equation for alternative cutoff dates illustrates the importance of the potential fuzziness in the cutoff dates. Second, as we move away far enough from the cutoff date in the definition of the *treated* dummy we expect the policy variable to lose its explanatory power. This serves as a falsification exercise that the observed changes in schooling levels are indeed due to the policy change not due to some other factor.

For this purpose, we define *treated* dummy for alternative cutoff dates as follows:

$treated_i$= 1 if birth year>=1978+i , i= -5...+5

For i=0 we get the definition of policy variable used up to this point, i.e. *treated* =1 if birth year is 1978 or later, 0 otherwise. As we move away from i=0 the *treated* dummy starts including birth cohorts both affected and unaffected by the policy change.

Table 7 presents the coefficient estimates for the *treated* dummy for different cutoff dates. Panels A to D refer to alternative specifications. Panel A refers to the specification that include controls for age and age squared; Panel B includes controls for age, age squared and age cubed; Panel C includes controls for cohort dummies based on the first definition; Panel D includes controls for cohort dummies based on the second definition. Two important results emerge from Table 7. First, as we move away from the cutoff date of 1978 the first stage coefficient for the *treated* dummy becomes smaller and loses its significance. Even in some cases it changes sign and becomes positive and significant. This pattern provides strong support that the policy variable actually reflects changes in schooling due to the reduction in elementary school from 6 to 5 years, not some other factor. Second, the *treated* dummy is always larger in magnitude and much more significant when cutoff date is 1977 rather than 1978. This suggests some students may be starting school ate age seven rather six, hence are affected by policy. Alternatively, there may be significant grade repetition leading to 1977 birth cohorts being affected by the policy change. In light of these results, in the remainder of the paper we report results based on two alternative cutoff dates used for defining the *treated* dummy, i.e. 1977 and 1978.

6.3 Wage returns of schooling

The estimates of equation 2 for wage returns to schooling are presented in Tables 8 and 9. Table 8 presents the results for specifications with age controls and Table 9 presents the results for specifications with cohort dummies replacing the age variables. In each of these tables, we present the results for two age groups as specified by column headings: age 20-45 and age 25-45. Within each table, Panel A refers to the *treated* dummy variable defined as 1 if the birth year is 1978 or later, 0 otherwise; Panel B refers to the *treated* dummy variable defined as 1 if the birth year is 1977 or later, 0 otherwise. Only coefficients for key variable of interest, years of schooling, are presented. We present OLS coefficients along with 2SLS coefficients for comparison purposes.

A key distinguishing feature of Panels A and B in both Tables 8 and 9 is that the first-stage F values in Panel A are much lower than those in Panel B and are often below 10 indicating that 2SLS results in Panel A may be subject to a weak instrument bias. In Panel B, on the other hand, F values are well above 10 with a minimum value of 15 and reaching a maximum of around 100. In general, for both tables, the second definition of the *treated* dummy results in higher first-stage F-values. The first stage F values of Table 8 that employs cohort dummies are significantly higher than that of Table 8 that controls for age.

In both tables, the OLS estimates indicate that one more year of schooling is associated with a 2.2 to 2.8 percent higher earnings. Compared to the OLS estimates the 2SLS estimates are in general higher indicating that OLS estimates are downward biased. Focusing on results with a first stage F-value over 10, Table 8 2SLS results indicate a wage return between 2.0 percent (insignificant) and 4.3 percent (significant). The results in Table 9 Panel A, where F values are all above 7 but below 10, the 2SLS estimates suggest a wage return of around 9 percent. In Panel B, however, where first-stage F values are well above 10, the results suggest a wage return between 5.2 to 5.7

percent.[4] Thus, 2SLS results in Tables 8 and 9 where the first-stage F values are above 10 suggest a wage return that ranges between an insignificant 2 percent to a significant 5.7 percent. These results are much lower than estimated wage returns in other developing country contexts but higher than the results reported by Aydemir and Kırdar (2015) for Turkey, where they find a wage return that is almost zero for men and 3.8 percent for women.

6.4 Effect of schooling on employment type

The validity of our instrument requires that the instrument has no effect on wages other than through its effect on schooling. To check whether the instrument has an effect on wage earner status we define a dummy variable *wage_emp* that is equal to 1 if an individual's employment status in primary job during the past three months is a wage employee, 0 if the individual was an employer, self-employed or an unpaid family worker.

An important characteristic of the Egyptian labor market is that public employment has a large share in the economy (about 20 percent of all employed men age 20-45 worked either for a public enterprise or the government during the 3 month reference period) and most jobs are informal (about 65 percent of men age 20-45 worked in an informal job during the 3 month reference period). In developing country contexts little is known about the effect of schooling either on public employment or informality. Our context provides us an opportunity to investigate both of these issues. We define a dummy variable for public employment as well as a dummy variable for working in the formal sector. Table 10 reports the 2SLS estimates of the effect of schooling on wage employment, public employment and working in the formal sector using a linear probability model. The dummy variable *treated* is defined as 1 for those born in 1978 or later, 0 otherwise. The table reports results for the specification with age and show no evidence of an effect of schooling on wage, public, or formal employment. The first stage F-values are, however, lower than 10 therefore these results should be interpreted with caution.

7. Conclusion

This paper estimates the causal impact of schooling on wages in Egypt. The reduction in the duration of primary schooling in 1988 from six to five years and therefore, of compulsory schooling from nine to eight years offers a unique opportunity to carry out a causal estimation. An important advantage of this policy over others studied in the literature is that there is no non-compiler problem. In other words, those who are treated went to school one less year to complete primary school.

Our first stage results indicate that the reduction in schooling has led to a substantial drop in the number of schooling years completed among the affected cohorts. That the policy change has led to a reduction in years of schooling among those with at least primary school education but not for those with less than this level gives further support that the observed changes are due to the change in the schooling law. However, we also find the drop in completed years to be the case for men but not for women. Therefore, we have only been able to estimate the returns to schooling for men but not for women.

Our second stage results, where we estimate the returns to schooling within an IV framework where the exogenous change in schooling is brought about by the reduction in compulsory schooling, show that the returns to schooling in Egypt for men ages 20-45 is between 2 to 5.7

[4] Interestingly when the sample is restricted to those who work 40 hours or more during the reference week, the estimated returns to schooling is found to be close to zero (see Appendix Tables A1 and A2).

percent. These estimates are much lower than those reported in the literature for other developing countries.

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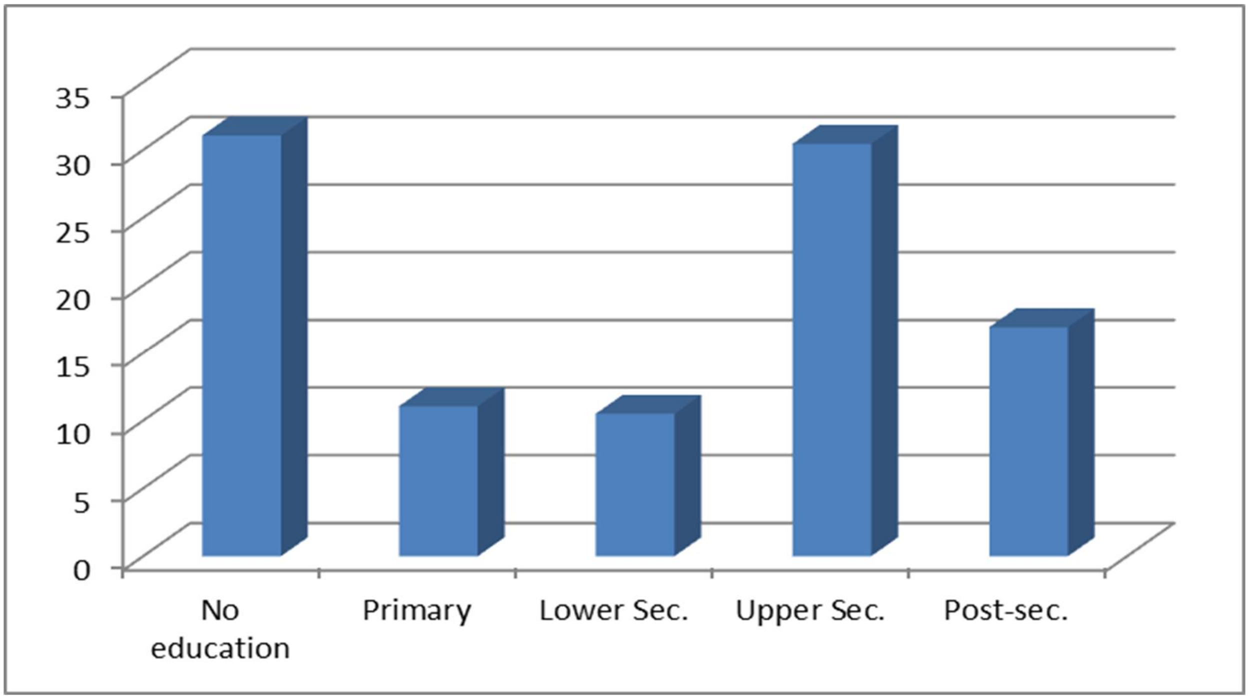
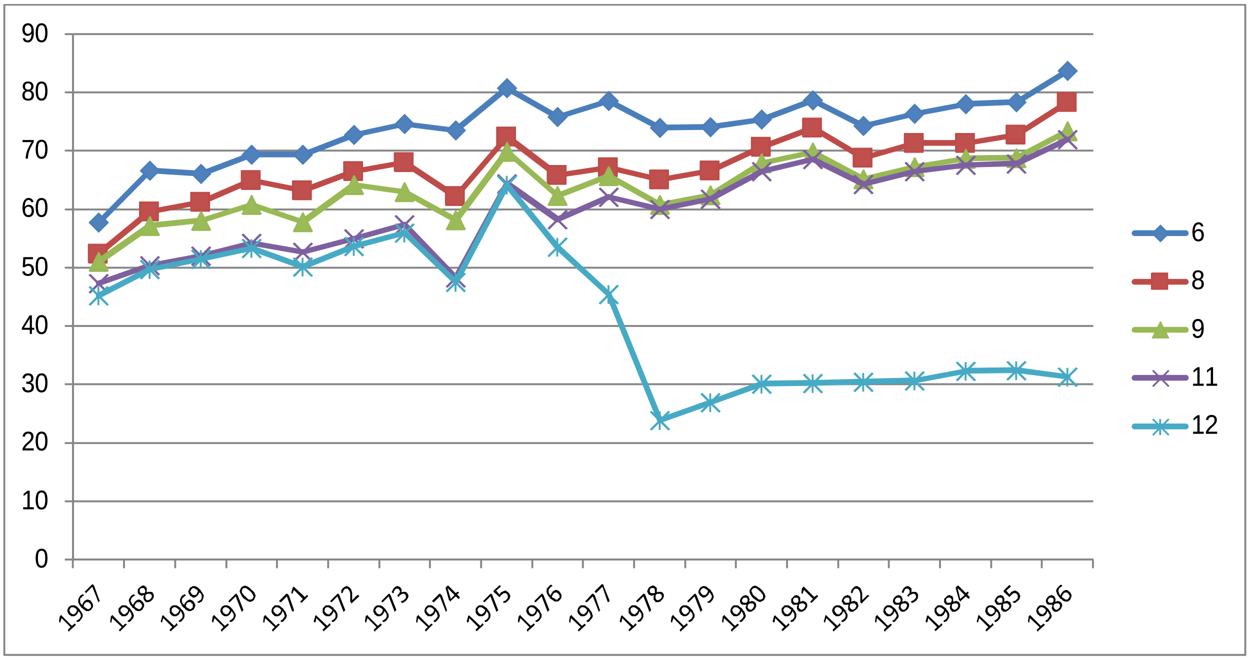
Figure 1: Proportion of Adult Population by School Attainment



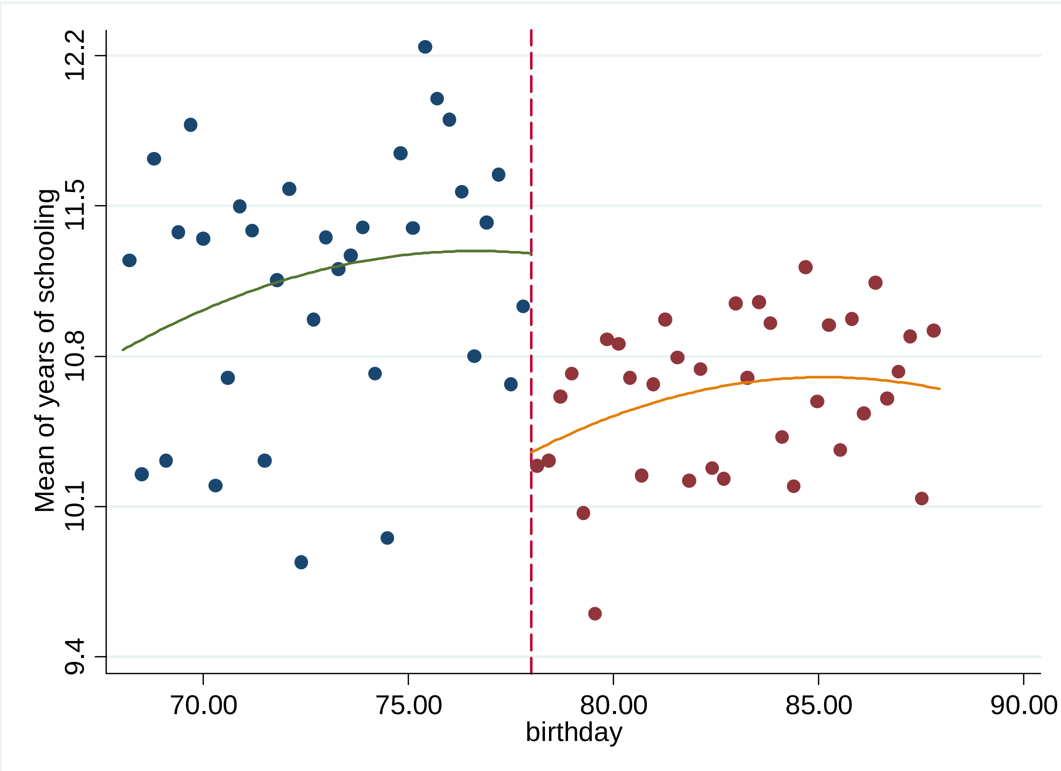
Source: 2012 ELMPS.

Figure 2: Proportion Completing at Least 6, 8, 9, 11 and 12 Grades



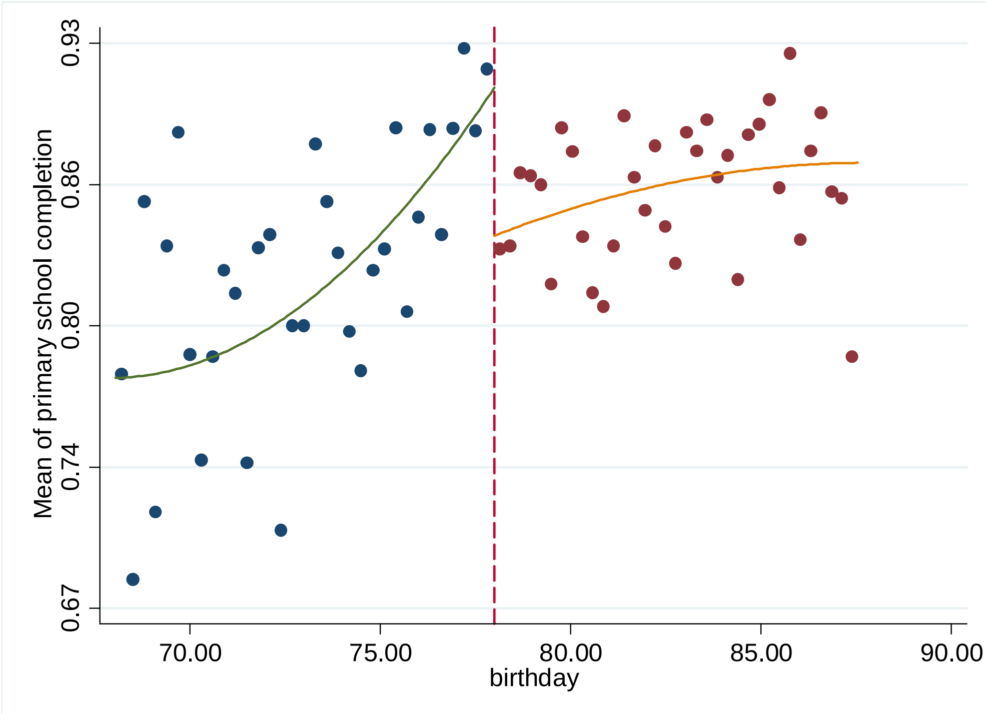
Source: 2012 ELMPS.

Figure 3: Average Years of Schooling by Month-Year of Birth



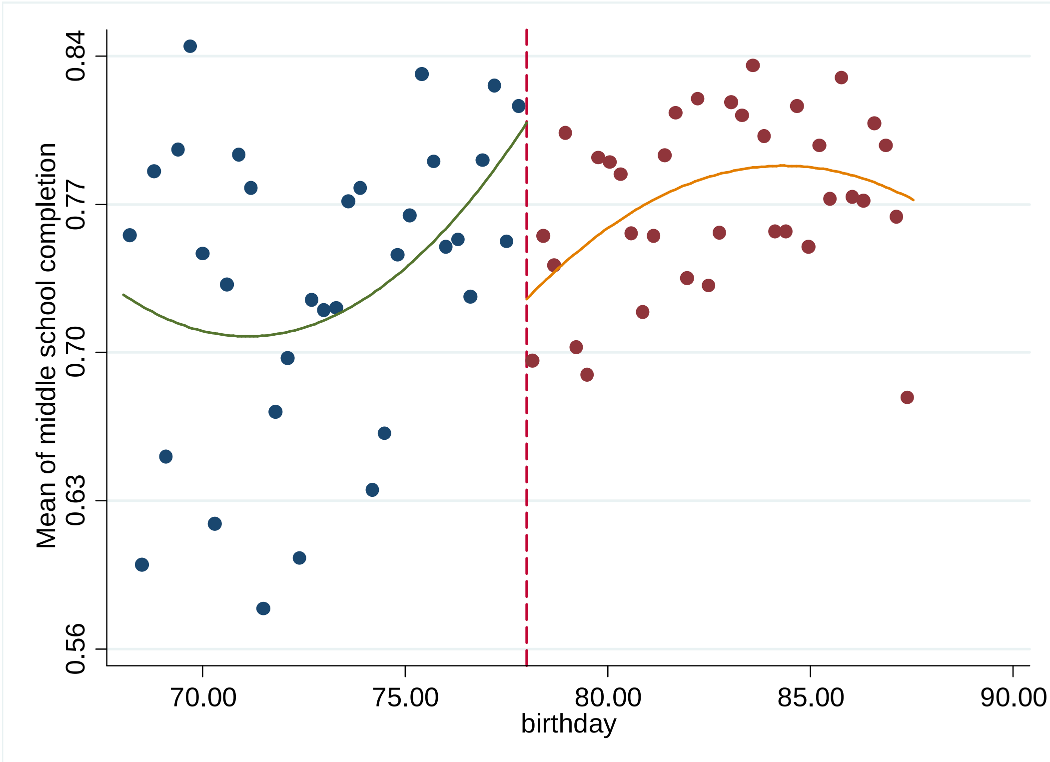
Notes: Quadratic polynomials are fit before and after the cut-off (at January 1978).
Source: 2012 ELMPS.

Figure 4: Fraction Completing Primary School by Month-Year of Birth



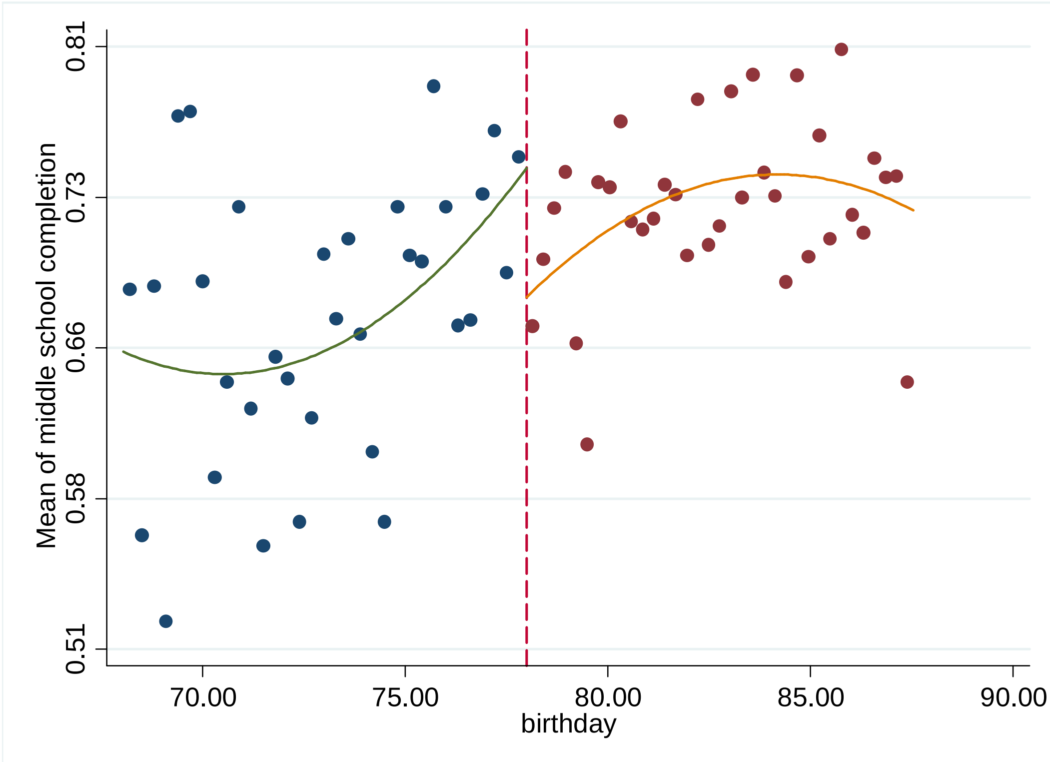
Notes: Quadratic polynomials are fit before and after the cut-off (at January 1978).
Source: 2012 ELMPS.

Figure 5: Fraction Completing Middle School by Month-Year of Birth



Notes: Quadratic polynomials are fit before and after the cut-off (at January 1978).
Source: 2012 ELMPS.

Figure 6: Fraction Completing High School by Month-Year of Birth



Notes: Quadratic polynomials are fit before and after the cut-off (at January 1978).
Source: 2012 ELMPS.

Table 1: Descriptive Statistics

	All		Male		Female	
	Mean	**Std. Dev.**	**Mean**	**Std. Dev.**	**Mean**	**Std. Dev.**
treated	0.62	0.48	0.63	0.48	0.60	0.49
years of schooling	10.69	4.58	10.22	4.55	12.99	4.01
illiterate	0.11	0.32	0.12	0.33	0.06	0.23
literate_nodip	0.03	0.18	0.04	0.19	0.01	0.12
elementary	0.10	0.29	0.11	0.31	0.02	0.15
middlesch	0.05	0.23	0.06	0.24	0.02	0.16
highsch	0.39	0.49	0.41	0.49	0.28	0.45
postsec	0.31	0.46	0.25	0.44	0.60	0.49
age	31.67	6.55	31.60	6.55	32.03	6.51
male	0.83	0.37	1.00	0.00	0.00	0.00
coh1 (1966-70)	0.11	0.31	0.11	0.31	0.12	0.33
coh2 (1971-74)	0.13	0.34	0.13	0.34	0.14	0.34
coh3 (1975-78)	0.18	0.38	0.18	0.38	0.18	0.38
coh4 (1979-82)	0.19	0.40	0.20	0.40	0.19	0.39
coh5 (1983-87)	0.26	0.44	0.26	0.44	0.27	0.44
coh6 (1988-92)	0.12	0.33	0.13	0.33	0.10	0.30
reg1 (Gr. Cairo)	0.20	0.40	0.19	0.39	0.26	0.44
reg2 (Alx, Sz C.)	0.09	0.29	0.09	0.28	0.11	0.32
reg3 (Urb. Lwr.)	0.10	0.30	0.09	0.29	0.14	0.34
reg4 (Urb. Upp.)	0.08	0.27	0.07	0.26	0.10	0.30
reg5 (Rur. Lwr.)	0.31	0.46	0.32	0.46	0.28	0.45
reg6 (Rur. Upp.)	0.22	0.42	0.25	0.43	0.10	0.30
N	7515		6286		1229	

Notes: Cohort dummies coh1 to coh6 refer to birth year groups 1966-70, 1971-74, 195-79, 1980-83, 1984-87, and 1988-92 respectively.

Table 2: First Stage Regressions, Both Genders, Parametric Age Controls

Variables	**(1)** **age [20 45]**	**(2)** **age [25 45]**	**(3)** **age [30 40]**	**(4)** **age [20 45]**	**(5)** **age [25 45]**	**(6)** **age [30 40]**
treated	-0.722***	-0.974***	-1.221***	-1.279***	-0.977***	-0.590
	[-2.6849]	[-3.430]	[-3.000]	[-3.935]	[-2.585]	[-1.227]
age	0.287***	-0.072	-0.167	2.887***	-0.043	-43.842**
	[2.870]	[-0.429]	[-0.198]	[3.396]	[-0.020]	[-2.515]
agesq	-0.005***	0.000	0.001	-0.087***	-0.001	1.268**
	[-2.500]	[0.000]	[0.083]	[-3.222]	[-0.016]	[2.506]
agecube				0.001***	0.000	-0.012**
				[2.500]	[0.000]	[-2.400]
reg2	-0.246	-0.180	-0.310	-0.244	-0.180	-0.319
	[-1.069]	[-0.715]	[-0.774]	[-1.052]	[-0.715]	[-0.826]
reg3	-0.501*	-0.421	-0.738	-0.500*	-0.421	-0.757
	[-1.730]	[-1.359]	[-1.457]	[-1.732]	[-1.349]	[-1.519]
reg4	-0.157	-0.151	-0.091	-0.152	-0.151	-0.090
	[-0.581]	[-0.489]	[-0.183]	[-0.558]	[-0.489]	[-0.180]
reg5	-1.689***	-1.823***	-1.966***	-1.685***	-1.823***	-1.958***
	[-7.191]	[-6.820]	[-5.291]	[-7.107]	[-6.810]	[-5.341]
reg6	-2.933***	-2.928***	-2.965***	-2.937***	-2.928***	-2.979***
	[-11.811]	[-9.841]	[-6.212]	[-11.670]	[-9.806]	[-6.335]
Observations	7,515	6,302	3,327	7,515	6,302	3,327
R-squared	0.065	0.062	0.065	0.066	0.062	0.068

Notes: Robust t-statistics in brackets, *** p<0.01, ** p<0.05, * p<0.1; Please refer to Table 1 for definitions of region dummies.

Table 3: First Stage Regressions, Both Genders, Cohort Dummies

Variables	(1) age [20 45]	(2) age [25 45]	(3) age [30 40]	(4) age [20 45]	(5) age [25 45]	(6) age [30 40]
treated	-1.172***	-1.168**	-1.174**	-1.406***	-1.406***	-1.413***
	[-2.798]	[-2.772]	[-2.719]	[-3.645]	[-3.629]	[-3.574]
2.coh4_alt2	-0.000	0.001				
	[-0.000]	[0.006]				
3.coh4_alt2	0.568	0.564	0.619			
	[1.383]	[1.366]	[1.452]			
4.coh4_alt2	1.178**	1.176**	1.167**			
	[2.583]	[2.557]	[2.511]			
5.coh4_alt2	1.196**	1.202**				
	[2.688]	[2.674]				
6.coh4_alt2	0.587					
	[1.176]					
2.coh4				0.000	0.002	
				[0.001]	[0.008]	
3.coh4				0.568	0.564	0.618
				[1.383]	[1.366]	[1.451]
4.coh4				1.217***	1.217***	1.300**
				[2.927]	[2.912]	[3.000]
5.coh4				1.495***	1.514***	
				[3.585]	[3.603]	
6.coh4				0.821*		
				[1.738]		
2.region	-0.252	-0.195	-0.334	-0.231	-0.173	-0.300
	[-1.093]	[-0.771]	[-0.835]	[-0.997]	[-0.677]	[-0.741]
3.region	-0.505*	-0.429	-0.739	-0.497*	-0.421	-0.729
	[-1.764]	[-1.414]	[-1.483]	[-1.730]	[-1.381]	[-1.458]
4.region	-0.148	-0.146	-0.091	-0.143	-0.139	-0.084
	[-0.543]	[-0.475]	[-0.184]	[-0.523]	[-0.452]	[-0.169]
5.region	-1.703***	-1.830***	-1.977***	-1.698***	-1.825***	-1.968***
	[-7.277]	[-6.943]	[-5.305]	[-7.228]	[-6.887]	[-5.275]
6.region	-2.949***	-2.939***	-2.978***	-2.938***	-2.925***	-2.951***
	[-11.730]	[-9.802]	[-6.308]	[-11.608]	[-9.694]	[-6.177]
Observations	7,515	6,302	3,327	7,515	6,302	3,327
R-squared	0.066	0.063	0.067	0.067	0.064	0.067

Notes: Robust t-statistics in brackets, *** p<0.01, ** p<0.05, * p<0.1; Please refer to Table 1 for definitions of region and cohort dummies.

Table 4a: First Stage Regressions, Males, Parametric Age Controls

Variables	(1) age [20 45]	(2) age [25 45]	(3) age [30 40]	(4) age [20 45]	(5) age [25 45]	(6) age [30 40]
treated	-0.953*	-1.144**	-1.405*	-1.316**	-1.032	-0.629
	[-1.729]	[-2.163]	[-1.956]	[-2.093]	[-1.397]	[-0.949]
age	0.242*	-0.038	-0.702	1.943**	-1.007	-54.082*
	[1.781]	[-0.219]	[-0.935]	[2.546]	[-0.377]	[-1.977]
agesq	-0.004*	-0.001	0.008	-0.058**	0.028	1.558*
	[-1.776]	[-0.186]	[0.722]	[-2.460]	[0.359]	[1.952]
agecube				0.001**	-0.000	-0.015*
				[2.359]	[-0.372]	[-1.934]
2.region	-0.430	-0.303	-0.483	-0.424	-0.303	-0.506
	[-1.430]	[-0.922]	[-0.909]	[-1.402]	[-0.929]	[-0.993]
3.region	-0.699*	-0.595	-0.981	-0.691*	-0.593	-1.030
	[-1.847]	[-1.454]	[-1.458]	[-1.825]	[-1.447]	[-1.563]
4.region	-0.512	-0.512	-0.402	-0.506	-0.513	-0.430
	[-1.579]	[-1.356]	[-0.678]	[-1.553]	[-1.363]	[-0.728]
5.region	-1.653***	-1.744***	-1.925***	-1.651***	-1.743***	-1.933***
	[-5.799]	[-5.267]	[-3.835]	[-5.753]	[-5.251]	[-3.943]
6.region	-2.838***	-2.847***	-2.993***	-2.840***	-2.846***	-3.033***
	[-9.411]	[-8.285]	[-5.231]	[-9.357]	[-8.270]	[-5.435]
Observations	6,286	5,242	2,795	6,286	5,242	2,795
R-squared	0.059	0.057	0.064	0.059	0.057	0.068

Notes: Robust t-statistics in brackets, *** p<0.01, ** p<0.05, * p<0.1; Please refer to Table 1 for definitions of region and cohort dummies.

Table 4b: First Stage Regressions, Females, Parametric Age Controls

Variables	(1) age [20 45]	(2) age [25 45]	(3) age [30 40]	(4) age [20 45]	(5) age [25 45]	(6) age [30 40]
treated	0.537	0.204	0.498	-0.571	-0.056	0.597
	[1.581]	[0.644]	[0.906]	[-1.294]	[-0.123]	[0.912]
age	0.421	0.010	2.857**	5.576***	2.197	-4.207
	[1.608]	[0.042]	[2.338]	[3.298]	[0.668]	[-0.203]
agesq	-0.006	-0.000	-0.041**	-0.169***	-0.066	0.164
	[-1.568]	[-0.101]	[-2.378]	[-3.293]	[-0.685]	[0.269]
agecube				0.002***	0.001	-0.002
				[3.290]	[0.698]	[-0.332]
2.region	0.562	0.296	0.388	0.515	0.291	0.391
	[1.493]	[0.755]	[0.698]	[1.350]	[0.744]	[0.705]
3.region	0.140	0.051	0.464	0.079	0.034	0.474
	[0.387]	[0.132]	[0.939]	[0.214]	[0.087]	[0.978]
4.region	1.013***	0.731*	0.743	0.990**	0.736*	0.758
	[2.805]	[2.005]	[1.433]	[2.726]	[2.004]	[1.487]
5.region	-1.137**	-1.538***	-1.647**	-1.136**	-1.536***	-1.636**
	[-2.431]	[-3.110]	[-2.307]	[-2.409]	[-3.109]	[-2.357]
6.region	-1.083*	-1.214*	-0.831	-1.145*	-1.224*	-0.817
	[-1.729]	[-1.843]	[-1.054]	[-1.807]	[-1.848]	[-1.048]
Observations	1,229	1,060	532	1,229	1,060	532
R-squared	0.041	0.045	0.057	0.049	0.045	0.057

Notes:Robust t-statistics in brackets,*** p<0.01, ** p<0.05, * p<0.1; Please refer to Table 1 for definitions of region and cohort dummies.

Table 5a: First Stage Regressions, Males, Cohort Dummies

Variables	(1) age [20 45]	(2) age [25 45]	(3) age [30 40]	(4) age [20 45]	(5) age [25 45]	(6) age [30 40]
treated	-1.355**	-1.351**	-1.355**	-1.503***	-1.504***	-1.503**
	[-2.753]	[-2.729]	[-2.667]	[-3.148]	[-3.140]	[-3.075]
2.coh4_alt2	0.145	0.142				
	[0.586]	[0.571]				
3.coh4_alt2	0.676	0.673	0.609			
	[1.310]	[1.298]	[1.179]			
4.coh4_alt2	1.351**	1.348**	1.261**			
	[2.495]	[2.473]	[2.304]			
5.coh4_alt2	1.340**	1.358**				
	[2.520]	[2.535]				
6.coh4_alt2	0.943*					
	[1.711]					
2.coh4				0.145	0.143	
				[0.588]	[0.573]	
3.coh4				0.676	0.673	0.608
				[1.311]	[1.299]	[1.179]
4.coh4				1.275**	1.277**	1.252**
				[2.428]	[2.424]	[2.351]
5.coh4				1.557***	1.587***	
				[2.995]	[3.042]	
6.coh4				1.091*		
				[2.026]		
2.region	-0.432	-0.317	-0.507	-0.407	-0.289	-0.466
	[-1.428]	[-0.961]	[-0.951]	[-1.338]	[-0.871]	[-0.867]
3.region	-0.699*	-0.604	-0.987	-0.687*	-0.592	-0.973
	[-1.848]	[-1.493]	[-1.484]	[-1.813]	[-1.458]	[-1.458]
4.region	-0.504	-0.510	-0.403	-0.500	-0.504	-0.397
	[-1.540]	[-1.352]	[-0.682]	[-1.530]	[-1.339]	[-0.674]
5.region	-1.667***	-1.755***	-1.933***	-1.661***	-1.749***	-1.924***
	[-5.861]	[-5.358]	[-3.877]	[-5.815]	[-5.310]	[-3.856]
6.region	-2.851***	-2.857***	-3.010***	-2.840***	-2.843***	-2.980***
	[-9.383]	[-8.260]	[-5.340]	[-9.301]	[-8.183]	[-5.242]
Observations	6,286	5,242	2,795	6,286	5,242	2,795
R-squared	0.060	0.058	0.066	0.060	0.058	0.066

Notes: Robust t-statistics in brackets, *** p<0.01, ** p<0.05, * p<0.1; Please refer to Table 1 for definitions of region and cohort dummies.

Table 5b: First Stage Regressions, Females, Cohort Dummies

Variables	(1) age [20 45]	(2) age [25 45]	(3) age [30 40]	(4) age [20 45]	(5) age [25 45]	(6) age [30 40]
treated	0.228	0.221	0.244	-0.009	-0.004	0.027
	[0.926]	[0.905]	[0.995]	[-0.043]	[-0.016]	[0.114]
2.coh4_alt2	-0.495	-0.463				
	[-0.894]	[-0.844]				
3.coh4_alt2	0.033	0.042	0.437			
	[0.066]	[0.085]	[1.346]			
4.coh4_alt2	-0.089	-0.071	0.214			
	[-0.167]	[-0.133]	[0.498]			
5.coh4_alt2	0.069	-0.002				
	[0.130]	[-0.004]				
6.coh4_alt2	-1.091					
	[-1.191]					
2.coh4				-0.494	-0.463	
				[-0.894]	[-0.843]	
3.coh4				0.033	0.043	0.437
				[0.066]	[0.085]	[1.345]
4.coh4				0.196	0.198	0.548
				[0.378]	[0.380]	[1.438]
5.coh4				0.290	0.226	
				[0.565]	[0.428]	
6.coh4				-0.854		
				[-0.950]		
2.region	0.541	0.301	0.369	0.540	0.298	0.353
	[1.402]	[0.768]	[0.668]	[1.403]	[0.760]	[0.639]
3.region	0.119	0.063	0.419	0.109	0.052	0.399
	[0.315]	[0.161]	[0.875]	[0.290]	[0.134]	[0.831]
4.region	1.000***	0.737*	0.720	1.009***	0.744*	0.732
	[2.823]	[2.048]	[1.383]	[2.837]	[2.059]	[1.390]
5.region	-1.148**	-1.506***	-1.680**	-1.153**	-1.511***	-1.687**
	[-2.464]	[-3.017]	[-2.343]	[-2.477]	[-3.036]	[-2.356]
6.region	-1.097*	-1.203*	-0.830	-1.102*	-1.206*	-0.833
	[-1.740]	[-1.824]	[-1.052]	[-1.752]	[-1.831]	[-1.054]
Observations	1,229	1,060	532	1,229	1,060	532
R-squared	0.044	0.046	0.052	0.044	0.046	0.052

Notes: Robust t-statistics in brackets,*** p<0.01, ** p<0.05, * p<0.1; Please refer to Table 1 for definitions of region and cohort dummies.

Table 6: First Stage Results among Males 20-45, Samples Restricted to Different Education Groups

Variables	(1) All education groups	(2) Less than post secondary	(3) Less than high school	(4) Less than middle school	(5) Less than elementary	(6) Less than literate	(1) All education groups	(2) Less than post secondary	(3) Less than high school	(4) Less than middle school	(5) Less than elementary	(6) Less than literate
Panel A - parametric age spec.												
Treated	-0.96*	-1.00***	-1.58***	-1.07***	-0.18	-0.21	-1.33**	-0.94***	-0.89*	-0.75**	0.02	0.11
	[-1.79]	[-3.30]	[-4.42]	[-3.88]	[-0.59]	[-0.56]	[-2.16]	[-2.89]	[-2.05]	[-2.24]	[0.06]	[0.27]
Age	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Agesq	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Agecube							Yes	Yes	Yes	Yes	Yes	Yes
Panel B - cohort dummies												
Treated	-1.38***	-0.93***	-1.02**	-0.67**	-0.18	-0.16	-1.52***	-1.00***	-1.51***	-1.00***	-0.43	-0.28
	[-2.89]	[-3.58]	[-2.55]	[-2.15]	[-0.53]	[-0.54]	[-3.30]	[-3.95]	[-6.58]	[-4.64]	[-1.47]	[-0.97]
Coh defn1	Yes	Yes	Yes	Yes	Yes	Yes						
Coh defn2							Yes	Yes	Yes	Yes	Yes	Yes
Observations	6,283	4,737	2,044	1,679	1,016	793	6,283	4,737	2,044	1,679	1,016	793
R-squared	0.06	0.03	0.11	0.11	0.10	0.08	0.06	0.03	0.11	0.11	0.11	0.08

Notes: All specifications include dummies for regions. Robust t-statistics in brackets, *** p<0.01, ** p<0.05, * p<0.1

Table 7: First Stage Results with Alternative Definitions of Policy (I.E. Treatment Dummy), Males 20-45

Treatment definition	(1) Birthyear >=1973	(2) Birthyear >=1974	(3) Birthyear >=1975	(4) Birthyear >=1976	(5) Birthyear >=1977	(6) Birthyear >=1978	(7) Birthyear >=1979	(8) Birthyear >=1980	(9) Birthyear >=1981	(10) Birthyear >=1982	(11) Birthyear >=1983
Panel A: age and agesq controls											
Treated	0.04	-0.24	0.18	-1.01	-1.50***	-0.95*	-0.22	0.25	0.47	0.49	0.71***
	[0.08]	[-0.38]	[0.27]	[-1.55]	[-3.98]	[-1.73]	[-0.39]	[0.56]	[1.37]	[1.57]	[3.32]
Panel B: age, agesq, agecube controls											
Treated	-0.01	-0.25	0.21	-1.10	-1.87***	-1.32**	-0.23	0.56	0.87**	0.77*	0.91***
	[-0.03]	[-0.43]	[0.30]	[-1.45]	[-5.35]	[-2.09]	[-0.31]	[1.05]	[2.30]	[1.96]	[3.21]
Panel C: First cohort defn											
Treated	-0.15	-0.57***	-0.41*	-1.68***	-1.84***	-1.35**	-0.82	-0.41*	0.33**	0.16	0.45***
	[-0.55]	[-6.78]	[-1.71]	[-5.02]	[-10.04]	[-2.75]	[-1.63]	[-1.71]	[2.56]	[0.73]	[5.50]
Panel D: Second cohort defn											
Treated	-0.15	-0.57***	-0.41*	-1.57***	-1.86***	-1.50***	-0.41*	0.42***	0.34**	0.06	-0.41*
	[-0.54]	[-6.77]	[-1.71]	[-3.48]	[-8.65]	[-3.15]	[-1.71]	[4.97]	[2.54]	[0.43]	[-1.71]

Notes: Robust t-statistics in brackets, *** p<0.01, ** p<0.05, * p<0.1

Table 8: Returns to Schooling, Males, Controls for Age

Variables	(1) age [20 45] OLS	(2) age [20 45] 2SLS	(3) age [25 45] OLS	(4) age [25 45] 2SLS	(5) age [20 45] OLS	(6) age [20 45] 2SLS	(7) age [25 45] OLS	(8) age [25 45] 2SLS
Panel A - Treated=1 if birth year>=1978								
tot_schyrs3	0.025***	0.031	0.028***	0.035	0.025***	0.046	0.028***	0.082*
	[0.004]	[0.038]	[0.004]	[0.034]	[0.004]	[0.030]	[0.004]	[0.043]
Observations	6,286	6,286	5,242	5,242	6,286	6,286	5,242	5,242
R-squared	0.081	0.079	0.080	0.078	0.081	0.062	0.081	
First-stage F value		2.99		4.68		4.38		1.95
Panel B - Treated=1 if birth year>=1977								
tot_schyrs3	0.025***	0.020	0.028***	0.022	0.025***	0.027	0.028***	0.043*
	[0.004]	[0.025]	[0.004]	[0.023]	[0.004]	[0.021]	[0.004]	[0.025]
Observations	6,286	6,286	5,242	5,242	6,286	6,286	5,242	5,242
R-squared	0.081	0.080	0.080	0.079	0.081	0.081	0.081	0.071
First-stage F value		15.86		19.84		28.61		19.75
Age	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Agesq	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Agecube					Yes	Yes	Yes	Yes

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

Table 9: Returns to Schooling, Males, Controls for Cohort Dummies

Variables	(1) age [20 45] OLS	(2) age [20 45] 2SLS	(3) age [25 45] OLS	(4) age [25 45] 2SLS	(5) age [20 45] OLS	(6) age [20 45] 2SLS	(7) age [25 45] OLS	(8) age [25 45] 2SLS
Panel A - Treated=1 if birth year>=1978								
tot_schyrs3	0.025***	0.090***	0.028***	0.090***	0.025***	0.093***	0.028***	0.093***
	[0.004]	[0.027]	[0.004]	[0.027]	[0.004]	[0.025]	[0.004]	[0.025]
Observations	6,286	6,286	5,242	5,242	6,286	6,286	5,242	5,242
R-squared	0.080		0.079		0.079		0.078	
First-stage F value		7.58		7.45		9.91		9.86
Panel B - Treated=1 if birth year>=1977								
tot_schyrs3	0.025***	0.057***	0.028***	0.057***	0.025***	0.052**	0.028***	0.052**
	[0.004]	[0.017]	[0.004]	[0.017]	[0.004]	[0.021]	[0.004]	[0.022]
Observations	6,286	6,286	5,242	5,242	6,286	6,286	5,242	5,242
R-squared	0.080	0.036	0.079	0.043	0.079	0.047	0.078	0.053
First-stage F value		100.90		100.36		74.91		74.53
Cohort controls-defn 1	Yes	Yes	Yes	Yes				
Cohort controls-defn 2					Yes	Yes	Yes	Yes

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

Table 10: 2 SLS Estimates of the Effect of Schooling on (I) Wage Employment, (Ii) Public Employment and (Iii) Formal Employment, Males

Variables	**Wage emp (1) age [20 45]**	**Wage emp (2) age [25 45]**	**Public emp (3) age [20 45]**	**Public emp (4) age [25 45]**	**Formal emp (5) age [20 45]**	**Formal emp (6) age [25 45]**
Panel A - Specification with age controls						
tot_schyrs3	0.004	-0.013	0.017	0.030	0.017	-0.017
	[0.013]	[0.034]	[0.017]	[0.031]	[0.018]	[0.043]
Observations	8,245	6,884	8,245	6,884	8,232	6,881
R-squared	0.025	-	0.125	0.126	0.169	-
First-stage F values	3.44	1.02	3.44	1.02	3.44	1.02
Age	Yes	Yes	Yes	Yes	Yes	Yes
Agesq	Yes	Yes	Yes	Yes	Yes	Yes
Agecube	Yes	Yes	Yes	Yes	Yes	Yes

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

Appendix

Table A1: Returns to Schooling, Males, Controls for Age -- Full Time Workers

Variables	(1) age [20 45] OLS	(2) age [20 45] 2SLS	(3) age [25 45] OLS	(4) age [25 45] 2SLS	(5) age [20 45] OLS	(6) age [20 45] 2SLS	(7) age [25 45] OLS	(8) age [25 45] 2SLS
Panel A - Treated=1 if birth year>=1978								
tot_schyrs3	0.025***	-0.007	0.027***	0.003	0.025***	0.017	0.027***	0.048
	[0.003]	[0.054]	[0.004]	[0.045]	[0.003]	[0.037]	[0.004]	[0.058]
Observations	5,035	5,035	4,203	4,203	5,035	5,035	4,203	4,203
R-squared	0.080	0.038	0.080	0.054	0.080	0.077	0.081	0.062
First-stage F value		2.86		3.26		1.85		0.29
Panel B - Treated=1 if birth year>=1977								
tot_schyrs3	0.025***	-0.014	0.027***	-0.010	0.025***	-0.003	0.027***	0.004
	[0.003]	[0.028]	[0.004]	[0.026]	[0.003]	[0.021]	[0.004]	[0.026]
Observations	5,035	5,035	4,203	4,203	5,035	5,035	4,203	4,203
R-squared	0.080	0.018	0.080	0.020	0.080	0.049	0.081	0.058
First-stage F value		19.76		22.84		24.70		15.79
Age	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Agesq	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Agecube					Yes	Yes	Yes	Yes

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

Table A2: Returns to Schooling, Males, Controls for Cohort Dummies -- Full Time Workers

Variables	(1) age [20 45] OLS	(2) age [20 45] 2SLS	(3) age [25 45] OLS	(4) age [25 45] 2SLS	(5) age [20 45] OLS	(6) age [20 45] 2SLS	(7) age [25 45] OLS	(8) age [25 45] 2SLS
Panel A - Treated=1 if birth year>=1978								
tot_schyrs3	0.025***	0.067*	0.027***	0.067*	0.025***	0.072**	0.027***	0.072**
	[0.003]	[0.036]	[0.004]	[0.037]	[0.003]	[0.030]	[0.004]	[0.030]
Observations	5,035	5,035	4,203	4,203	5,035	5,035	4,203	4,203
R-squared	0.079	0.006	0.080	0.015	0.078		0.079	
First-stage F value		3.02		2.99		7.30		7.19
Panel B - Treated=1 if birth year>=1977								
tot_schyrs3	0.025***	0.026	0.027***	0.026	0.025***	0.024	0.027***	0.024
	[0.003]	[0.018]	[0.004]	[0.018]	[0.003]	[0.023]	[0.004]	[0.023]
Observations	5,035	5,035	4,203	4,203	5,035	5,035	4,203	4,203
R-squared	0.079	0.079	0.080	0.080	0.078	0.078	0.079	0.078
First-stage F value		26.56		27.44		42.12		43.61
Cohort controls-defn 1	Yes	Yes	Yes	Yes				
Cohort controls-defn 2					Yes	Yes	Yes	Yes