4-22-2014

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Abstract

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Keywords

wages, education, Turkey, women, education

Disciplines

Business | Education

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ABSTRACT

In 1997 Turkey passed a law making middle school completion compulsory, increasing the mandatory education from 5 to 8 years. At the time of this policy change, only 3-in-5 students were completing middle school in Turkey. In this paper, I investigate the effect of this law on educational attainment, the impact of the increase in education on wages, and explore how this varied across individuals. My results indicate that the fraction of children completing middle school increased more than 20 percentage points as a result of this reform. The effects were especially pronounced for girls (particularly those living in rural areas): I estimate that as a result of the reform, an additional half a million girls attained a middle school diploma. There are also considerable spillover effects into high school completion rates. Despite the large policy-induced increase in educational attainment, I find little evidence of a corresponding increase in labor force participation or full-time work. The results suggest large wage gains of about 14 percent per year of schooling, with these benefits concentrated among females. Taken together, my findings demonstrate that the policy change induced a dramatic change in educational attainment among the youth of this predominantly Muslim developing country, but that the economic benefits of the change were limited to women.

I thank Mark Duggan for his guidance and helpful comments and Alptekin Avcioğlu, Berk Avcioğlu, Ekrem Cunedioğlu and Bahadir Dursun for their help in obtaining the data. Michael Grossman, Nate Barrett, Colin Cannonier, Aysit Tansel and seminar participants at the 38th Annual Conference of the Association for Education Finance and Policy in New Orleans on March 15, 2013 as well as participants of the Carroll Round Conference at Georgetown University provided valuable comments. I thank Martin Asher, Bethany Schell, Walter Licht, Arlene Fernandez and David Grossman for their support. Summer funding for this research was provided by Wharton SPUR Program. I thank the Trustees' Council of Penn Women and the Wharton Public Policy Initiative for providing travel funding for the AEFP Meeting and the Carroll Round. Finally, I thank Naci Mocan for his support and encouragement.

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"Milletleri kurtaranlar, yalniz ve ancak öğretmenlerdir"
"Teachers are the one and only people who save nations"
-Mustafa Kemal Atatürk

I. Introduction

In 1997 Turkey passed the "Basic Education Law," which made middle school completion compulsory for the first time. As shown in Figure 1, this reform created a significant increase in educational attainment for the students in grade 5 and younger starting in the fall of 1997. Before the reform, only 3-in-5 workers in Turkey were attaining a middle school education. After the implementation of the reform, this number increased by about 20 percentage points and, in less than ten years after the reform, over 90 percent of students were completing middle school or a higher degree.

I exploit exposure to the sudden onset of this reform as an instrument for educational attainment to estimate the causal impact of education on workers' wages. I compare students who just missed the reform's mandate because they were too old to those who were in the first cohorts to be impacted by it. These two groups of students are for all accounts similar except that those in the "treatment" group happened to be born a few years later and therefore were forced by the legislation to attain more education.

My findings demonstrate that the reform did have a significant effect in increasing middle school completion and that it had a larger effect among women.[1] The reform also increased both the

[1] The effectiveness of such a reform remains relevant as Turkey once again changed its compulsory schooling laws in January 2012 to now mandate high school education as well (Finkle, 2012). It should be noted that under the 1997 Law students stayed at the same school for grades 1-8. The 2012 legislation mandates 12 years of schooling such that students can now move into a new school after the first four years. This enables students to start religious education in grade 5 instead of having to defer to grade 9

high school and college completion rates, while decreasing the propensity to complete a vocational high school. I find that there is no return to the middle school diploma among men. The return to education is approximately 14 percent per year for women with a high school education or less. This finding is similar to results from studies in other countries (Duflo 2001, Webbink 2006, Maurin and Xenogiani 2007, Brunelle et al 2009, Fang et al. 2012[2]).

Turkey's legislation is interesting to consider because of the larger global position of the country. In 2011, Turkey's GNI per capita was $10,410, putting the country in the World Bank's upper-middle income category. Life expectancy at birth is 74 years and 99 percent of 74 million people living in Turkey have access to an adequate amount of water from an improved source. Furthermore, Turkey has the 18th largest economy in the world with an average annual growth rate of 5 percent over the last decade (World Bank). Yet Turkey is still a developing country. The infant mortality rate is 12 out of 1000 live births, which is higher than Malaysia, Cuba, or Saudi Arabia (World Bank) and the poverty rate is 18 percent (World Bank). The country is ranked only as "partly free" in evaluation of political rights and civil liberties (Freedom House 2014). It should also be noted that only about one quarter of women participate in the labor force.

Turkey is poised in an interesting position as a predominantly Muslim country straddling relationships with both Europe and the Middle East. Thus, its own policies and the effectiveness of these policies may provide relevant information for a wide-range of countries that are similar from a cultural or economic development perspective. Accurately estimating the returns to education is important in policy implementation in both Turkey and its peer nations. Furthermore, as Turkey has recently again increased the amount of compulsory schooling to high school or more, it is important

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under the 1997 law. Showing that the policy in 1997 was actually effective is useful as the country moves forward in implementing compulsory schooling laws one more time.

[2] It should be noted that the Fang et al study finds a 20 percent return to *one* additional year of education in China. This estimate is considerably higher than my findings and other similar findings for developing countries, which tend to estimate single digit returns per one additional year of school.

to have an accurate estimate of the benefits that these kinds of reforms provide for students later in life.

II. Literature Review

Estimating the causal effect of education on wages and other labor market outcomes is complicated because it is necessary not only to control for factors that impact the outcomes but also other factors that are closely correlated with both education and earnings. Furthermore, some of these factors are not easy to measure. For example, intelligence and motivation may directly impact both educational attainment and wages. However, a data set containing wage information is unlikely to have information on respondent's IQ level or motivation.

Creative methods have been developed to deal with this endogeneity problem, including using sets of twins, presumably coming from identical genetic background and households, therefore eliminating some unobservable characteristics. Bonjour et al. (2003) study sets of identical twins in the United Kingdom. For women, they find that the return to education is 7.7 percent per year of schooling. Ashenfelter and Rouse (1998) conduct a similar study using identical twins from the Twinsburg Twins Festivals in 1991, 1992 and 1993. They find that for these twins, one extra year of schooling provides a 9 percent increase in wages. While these studies are certainly valuable, especially in trying to gauge the role of individual ability or motivation, the data sets used are limited and uncommon. Furthermore, although these twins have the same genetics and upbringing, it is unclear why one twin decides to obtain more education than the other. There are likely other unobservable characteristics that even studies of twins cannot control for.

As an alternative strategy, exogenous 'natural experiments' have been used as instruments for individuals' level of education. Angrist and Krueger (1991) use quarter year of birth as an instrument and consider students' birth dates in proximity to the age in which they start schooling (and thus the age in which they can drop out of school). Using this instrument, Angrist and Krueger find that individuals have on average a 7.5 percent higher wages as a result of one additional year of

education. It should be noted that these policies affect people on the margin – not those who would have gone onto college anyway or dropped out of elementary school, but those students who perhaps would not have completed the additional year of schooling but were forced to because of the state law.

Oreopoulos (2006) uses the change in the legal school-leaving age in the United Kingdom that had a major effect on the dropout behavior of 14 to 15 year olds. He compares his estimates of the returns to education to the local average treatment effect obtained from the U.S. and Canada and finds large returns of 10-14 percent per year. Furthermore, he demonstrates that the local average treatment effect found by previous research and the average treatment effect in the population in his study are similar. He concludes that the unbiased estimates of return to schooling are substantial regardless of the size and coverage of the treatment.

There have been other studies that creatively use changes in public policy as instruments for education. Maurin and Xenogiani (2007) use a change in conscription laws in France as an instrument. The end of military conscription in France led to a decrease in educational attainment, since men were no longer incentivized to go to university to avoid military service. They use this instrument to estimate the return to one additional year of education and find a 13 percent increase in wages for men coming from low socio-economic status backgrounds in France. Brunello, Fort, Weber (2009) survey 12 European countries and use these countries' compulsory education laws after World War II as instruments. They find that the laws reduce the conditional wage dispersion and that students from the lowest socio-economic backgrounds benefit the most from the reforms. Furthermore, Webbink (2006) finds that a reduction in the mandated years of university in the Netherlands decreased wages by 7 to 9 percent for those who attained less education. Generally, the literature finds that in Western and developed countries each year of extra education has a return somewhere between 7 and 10 percent.

Education laws have also been studied in developing countries. Duflo (2000) examines a reform in Indonesia during the 1970s, which built 61,000 primary schools across the country. She finds that there was a significant increase in educational attainment, as well as a 6.8 to 10.6 percent increase in wages per year of education. Similarly, Fang et al (2012) consider compulsory education reforms in China during 1986. Using the reforms as an instrument, they report an extremely large 20 percent return to an additional year of schooling.[3]

There also exists a small body of work on returns to education in Turkey. Krueger (1972) examines the returns to higher education in Turkey by using the 1965 Turkish census. She finds a 20-25 percent return for secondary school and university education. Tansel (1994) uses data on urban wage earners who were 15-64 years old in 1987 and finds that the return to an additional year of education ranges from 9 to 22 percent for men, and 3 to 18 percent for women, depending on their level of education. More recent work in Turkey has also studied the 1997 Basic Education Law that I examine in this paper. Tansel and Daoud (2011) report that returns to education is 4-10 percent for men and 6-15 percent for women in Turkey. None of these studies, however, accounted for the endogeneity of education.

III. Overview of the Turkish Legislation

In 1997 Turkey passed the Basic Education Law (No. 4306), which increased compulsory education from five years to eight years. Since the start of the Turkish Republic in 1923, compulsory education entailed only five years of primary school. The 1997 legislation was the first change in the compulsory education law since 1923 to create a substantial impact on the level of attained education.

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[3] In developing countries where the baseline of educational attainment is lower, the returns to even a small increase in education are large and significant.

There were many reasons for the passage of the 1997 legislation, the majority of which were political. In 1996 Turkey officially entered the European Customs Union and there was serious concern from the Turkish government that European Union membership negotiations would not begin without a universal standard of eight years of mandated education (Dulger 2004). The law also was an attempt by a secular government to restrict religious education by extending the minimum mandated secular education to eight years. In the old system, students could start religious or vocational schooling immediately after their five years of mandated elementary education were complete. With the implementation of the 1997 law, however, students who would elect for religious education would be forced to wait another three years to begin such instruction. The passage of Law No. 4306 closed down all vocational and religious electives such that all students now faced a standardized eight-year curriculum (Dulger 2004). All these political motives may have led to the fact that the law was passed extremely quickly and with little to no public discourse.

Starting in the fall of 1997, all students enrolled in grades 1-5 were mandated to complete 8 years of primary education. Before this time, only about 60 percent, or 3-in-5 students were completing middle school. The law sought to quickly increase this number, primarily by encouraging enrollment. The government was able to immediately and successfully begin increasing enrollment due to several factors. First, there was significant support from outside agencies such as the World Bank, IMF and other NGOs, which helped to quickly put the law into action. Furthermore, the private sector in Turkey made substantial contributions to fund the efforts. Along with raising taxes, this support has allowed the national government to spend $3 billion annually to fund the reform (Dulger 2004). The government immediately took action by increasing both the number of schools as well as the number of teachers. Furthermore, the financial incentives for teachers to serve in rural areas were also increased. In any rural areas where schools could not be built quickly, the government established bussing programs to bring students to nearby schools (OCED 2007). In some cases when schools were too far away for students to commute, the government offered enrollment to

free boarding schools. Finally, the government abolished the primary school diploma, instead only offering a diploma once eight years of school (compared to five) had been completed. Prior to the legislation, students could have received an elementary school diploma after 5 years of school. The new law made it only possible to receive a diploma once elementary and middle school had been completed. It was hoped that the abolishment of a five-year diploma would help incentivize staying in school for the extra three years.

Though at the time the law was heavily criticized for its quick enactment and lack of public discourse, the legislation was ultimately able to quickly begin enrolling record numbers of students into schools. The combination of all previously mentioned efforts dramatically increased the percentage of students who completed middle school. It should be noted that I observe only the highest level of attained education for students and have no metric for the quality of schooling. Especially in the first few years after the reform, it is likely that school quality was diminished, either by larger class sizes or less qualified teachers as the government scrambled to meet the new demand. Additionally, even 10 years after implementation the middle school completion rate did not reach 100%. This may be partially attributed to some families asking for request to have their children exempt (especially for rural girls), or to the fact that some families may have opted to willfully ignore the mandate. This is most likely to the be case in rural areas, especially if the local municipality is not aggressive about truancy and upholding the mandate. Still, even with this potentially reduced quality of schooling and inability to have 100% of children in school, I find evidence of some significant returns to education for those students who were on the margin and received an extra three years of schooling as a result of the reform.

IV. Dataset and Methodology

I use data from the 2011 and 2012 Household Labor Force Survey (HLFS) in Turkey, made available by the Turkish Institute of Statistics[4]. This survey is conducted annually in Turkey and is similar to the United States Current Population Survey. Population weights are assigned to all observations in the data, thus all descriptive statistics and regressions are weighted to be nationally representative. The HLFS collects demographic information, including age, sex, marital status, living situation, location of residence, highest level of completed schooling and so on. Beyond these demographic questions, the survey primarily asks questions about behaviors and outcomes related to work. This includes employment status, sector of employment, average and bonus wages, and firm size. I consider people ages 20-29 in 2011 and 21-30 in 2012, who would have been ages 6-15 when the reform took place in 1997. The treatment group contains those ages 20-24 in 2011 and 21-25 in 2012, since they would have been ages 6-10 in 1997 and therefore surely affected by the reform. I use those ages 26-29 in 2011 and 27-30 in 2012 as my control group, since they would have been ages 12-15 in 1997 and therefore would have just missed the reform. For those who were 11 in 1997, half would have been treated and half would have missed the legislation, depending on their month of birth. Because the data do not allow me to accurately estimate the month of birth, I assign a value of ½ to everyone who was age 25 in 2011 or 26 in 2012, thus giving this group a 50 percent probability of treatment. All estimates are robust when I omit these people all together. The dataset contains 150,335 observations for the most inclusive sample, which considers both sexes and all education levels from ages 20-30 (20-29 in 2011 and 21-30 in 2012).

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[4] For Figures 5-7 in the paper is use the 2002, 2004, and 2006 HLFS datasets, respectively. These datasets are by all means equal to the 2011 and 2012 HFLS, except that ages are reported in groups instead of distinct years. See Section 5 for more details.

For all analyses the dummy variable "Middle School Completion or Higher" refers to completing middle school or a higher degree of education (high school, university, graduate work etc.). This allows me to capture everyone in the sample who has at least a middle school diploma. The survey only asks for the highest level of *completed* education, therefore imputing the years of schooling creates an estimate that is rough at best. For example, a student may declare their highest level of completed education as elementary school. Thus, they will be assigned a value of 5 for the measure of completed years of school. However it is possible that the student could have been in school through 7^{th} grade, for example, but did not complete the 8^{th} grade. This would mean that they really had seven years of schooling instead of five. As a result, the measure of years of schooling will understate the true value and make students who completed 5^{th} grade and those who completed 7^{th} grade look the same. This makes a binary variable for completion of middle school or higher a more useful measure of educational attainment than the crude measure of years of schooling.

For my analysis of wages, I first constrain the regressions to those whose highest level of education is high school or less. This is because I show that the reform increased both the propensity to complete middle school as well as high school education (see Appendix). The assumption of this approach is that the reform did not directly affect the composition of children pursuing schooling beyond high school. In a companion set of analyses I also run wage regressions using the entire sample without constraining education level.

V. The Impact of the Reform on Educational Attainment

Before using the implementation of the reform as a plausibly exogenous source of variation in educational attainment, it is necessary to demonstrate that the reform did in fact create a significant change in the propensity to complete middle school. While passing legislation is a significant act, generally the more difficult part is the implementation of that legislation. By a herculean effort, the Turkish government was able to actually put the legislation into practice in only a few months after it

was passed. This was possible primarily through busing and boarding schools in rural areas which temporarily brought students into schools before there was time to build new schools and hire more teachers.

Graphical representation of the percentage of individuals who completed middle school or higher demonstrates the dramatic increase in educational attainment as caused by the legislation. In Figure 1, age in 2011 is measured along the horizontal axis, with percentage per age group having completed middle school or higher along the vertical axis. The averages depicted in Figure 1 are calculated using sampling weights. About 50 percent of 32-33 year old individuals have at least a middle school education. As expected, the rate is lower for older individuals, and higher for younger ones, due to the significant trend in increasing educational attainment. Thus, older cohorts have less education than younger cohorts as depicted by the downward trend in Figure 1. There is a sharp break, however, from this trend for those ages 25 and younger in 2011. As mentioned above, while some of the 25 year-olds would have been treated by the reform, all those ages 24 and younger in 2011 would have certainly been treated by the reform. Figure 1 shows a clear jump in the rate of middle school completion for those who are 25 or younger in 2011, demonstrating that the passage and implementation of the law did have a significant impact in increasing the level of attained education. Figure 2 presents the same information using the 2012 survey, where the horizontal axis shows age in 2012. Figure 2 is very similar to Figure 1 with one difference - as expected, the jump in average middle school completion rate is now observed among the 26 year olds, as well as the 25 year olds.

The average effect of this legislation was different for males and females. As shown in Figure 3, the fraction of girls completing middle school was also around 1-in-2 prior to the policy change and this sharply increased for subsequent cohorts. In Figure 4 I display these rates for rural females, a group differentially targeted by the reform. As the figure makes clear, educational attainment was

much lower initially for females living in rural areas and the jump in attainment was substantially larger for them as well.

Furthermore, the legislation was also differentially targeted at some groups, most notably, females living in rural areas. Through targeted busing and extra funding to these rural areas, the government significantly increased the number of rural girls enrolled in public schools. Figure 4, which presents the rate of middle school completion among rural females for various cohorts in 2011, shows the effectiveness of the reform for this group. The percent of rural females completing middle school or higher is about 30 percent for 26 and 27 year olds, but is its nearly 70 percent for 22 and 23 year olds. This is a large and clear change in a small amount of time. While other subgroups, like urban females, urban males and rural males also showed significant gains, for the sake of brevity I only present the gains to females and rural females as they were the targeted group for the legislation and showed the most dramatic changes.

In summary, Figures 1 through 4 show that-the reform created significant variation in educational attainment that can be used to estimate the effect of schooling on wages and other market outcomes. Of course, it should be mentioned that these figures only account for the number of diploma completions and do little to provide information about the relative quality of the schooling these treated cohorts face. To the extent that class sizes increased as a result of the reform or newly hired teachers were not as skilled as incumbent teachers, the policy may have affected the quality of schooling for those who already would have completed middle school. I discuss this issue further below.

Though Figures 1-4 show a clear break in the middle school completion rates between those who were young enough to be treated by the law and the older cohorts, this is not definitive proof that the legislation is what created this jump. Perhaps it is possible that there is something special that has happened to those age 24 and younger in 2011 (25 and younger in 2012) that caused this break in the educational attainment trend. To investigate whether this is the case, I consider Household Labor

Force Survey data from the years of 2002, 2004 and 2006 (Figures 5, 6, & 7). Unfortunately, the surveys in these years asked people for their age in brackets (15-19, 20-24, 25-29, 20-24 etc.) instead of their age to the specific year. Therefore, for the sake of comparison, I reclassify the 2011 data so that ages are reported in brackets identical to the earlier survey (which is plotted in Figure 8). I once again graph the percentage completing middle school or higher on the vertical-axis and plot the age brackets on the horizontal-axis.

People who were 15 and younger in 2002 would have been the first to be treated by the 1997 reform. In Figure 5, the first age group in the horizontal axis is 15-19. This means that only 1/5 of the people in this age bracket in 2002 would have been treated by the reform. Figure 5 is consistent with this as it shows perhaps a slight but not hugely noticeable break in the trend of middle school completion. About 45 percent of those who are 25-29 years old in 2002 have a middle school diploma. The rate goes up to about 55 percent for those who are 20 to 24, and it about 63 percent among those who are 15-19 in 2002. Thus, Figure 5 mostly demonstrates the general age trend that we would expect in plotting educational attainment.

Figure 6 uses the data from the survey conducted in 2004. Those who were 17 and younger in 2004 would have been treated by the reform. That is, 3/5 of the people in the 15-19 age bracket in 2004 would have been treated. This is represented by the size of the dot in Figure 6, which is larger than the dot in Figure 5. Therefore, the rate of middle school completion should be higher among the 15-19 year olds in 2004 compared to 15-19 year olds in 2002. Indeed, we see in Figure 6 that the rate of middle school completion is about 55 percent for those who are 20-24 in 2004, which is the same as the rate of the same age group in 2002. However, the middle school completion rate in 2004 is 70 percent among the 15-19 year olds.

Two years later in 2006, those people who were treated by the reform would have been ages 19 and younger. This means that the entire 15-19 age bracket in 2006 felt the effect of the legislation. This is represented by a large dot in Figure 7. Now the jump in Figure 7 is even more pronounced

than the jumps in Figures 5 and 6. Specifically, in 2006, the middle school completion rate is about 58 percent among the 20-24 year olds, which is similar to the rate among the same age group in 2004 and 2002 (see Figures 5 and 6). However, the middle school completion rate jumps up to more than 80 percent among those who are 15-19 in 2006. So, there is more than 20 percentage point difference in middle school completion rate between the treated group (15-19 year olds in 2006) and the non-treated group (20-24 year olds in 2006). Finally, in the 2011 survey data, all individuals ages 24 and younger would have been treated by the reform. Figure 8 shows that in 2011 data, there is a large jump in the rate of middle school completion for both the 15-19 age bracket and the 20-24 age bracket. In other words, the cohort that was the first to be treated had higher levels of middle school completion and when looking at graphs depicted in Figures 5-8, it is clear that this break is moving through time as the cohort aged. This jump is both specific to this cohort and significantly different from the age trend that was already in effect. These graphs, along with the above description of the legislation, strongly suggest that the legislation is the cause of the dramatic increase in middle school completion and a potentially good instrument for educational attainment.

VI. Variable Descriptions and Summary Statistics

Table 1 presents the definitions and the descriptive statistics of key variables for the whole sample, treatment and control groups for both males and females. Note again that the treatment is associated with exposure to a mandated education of eight years, which is equivalent to middle school diploma. The sample presented in the top panel of Table 1 contains 144,247 observations for those who were age 20-29 in 2011 and 21-30 in 2012 and have non-missing information on education, age, gender and location of residence. This sample is also used to estimate the impact of the reform on education. People who were 24 and younger in 2011 (25 and younger in 2012) would have certainly been forced into obtaining more schooling as a result of the reform. In the regressions these individuals are considered to be "treated" by the law and are assigned a dummy value of 1.

People ages 26 to 29 in 2011 (27-30 in 2012) would have missed the reform and are assigned a value of 0 for the treatment variable. I cap the maximum age at 30 because I do not want the range of ages to become too wide; for example those who are 35 are not necessarily comparable to those who are 20.

Individuals who were 25 years old in 2011 (26 in 2012) may or may not have been exposed to the reform depending on which month they were born in, meaning that in 1997 they would have been in the 5^{th} or 6^{th} grade based on if they were born before or after the age cutoff date for the 6^{th} grade. This cutoff is dependent on students' month of birth – thus determining the year in which they were enrolled in the first grade. Because such information is not available, a value of ½ is assigned to the treatment value for individuals age 25 (26 in 2012), which assumes that the probability of treatment by the reform is 50 percent. The results remain the same if the 25 (26 in 2012) year-old age group is uniformly considered to be part of the control group or is omitted all together. As seen in Figures 1-8, there of course would have been some additional increase in educational attainment for the younger students simply because they are younger. Therefore the regressions control for this age effect.

The top panel of Table 1 shows that the percentage of individuals who completed middle school or higher is larger in the treatment groups for both males and females. Of males who just missed the reform only 67.8 percent completed middle school or higher; this jumps to 91.1 percent for the treatment group. This difference is even more dramatic for females, given that only 48.5 percent of the control group completed middle school or higher, compared to 78.3 percent of the treatment group.

The survey asks various questions about wages for those who are currently working, including the amount that the respondent earned in the previous week (reported in New Turkish Lira). A variable for the survey year beginning in 2011 is included in the regression to control for the effect of inflation that may have lead to wage increases between the 2011 and 2012 surveys. I also control

for the effect that working in a small firm versus a large firm may have on wages, as it has been demonstrated in the literature that firm size may be predictive of wage levels.[5] Respondents working at the time of the survey indicated the number of people in their work place. In the sample, the majority of firms fall into the bracket containing 10 to 24 employees. I label a firm as "small" if it has fewer than 10 employees. As shown in Table 1, about 34 percent of the full-time workers with any level of education and about 43 percent of those who have a middle school education or less work in small firms.

The bottom panel of Table 1 presents the descriptive statistics for individuals for whom the highest education achieved is high school and who work at least 30 hours per week. Here, the male treatment group has a higher percentage of individuals working in small firms in comparison to men in the control group, while the female treatment group has a probability of working in a small firm that is not statistically different from the control group. Yet again, the raw means presented in Table 1 do not capture effects from other variables. For example, controlling for region is important because people living in rural areas are more likely to work in small workplaces, yet this effect is not captured by the means alone.

In the unadjusted weighted means reported in Table 1, the treated males and females (younger individuals, ages 20-24 in 2011 and 21-25 in 2012) have a lower mean wage. For example, consider full-time female workers with all education levels. Those in the control group with any level of education have an hourly mean wage of 6.1 New Turkish Lira (approximately 3.4 US Dollars), while females in the treatment group have a mean wage of 4.3 New Turkish Lira (approximately 2.3 US Dollars). This is likely because experience contributes greatly to wage levels,

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[5] There has been significant literature on this phenomenon. See the wage outcomes section for more discussion.

and younger individuals (who are in the treatment group), have less experience than older ones who are in the control group.

Finally, the bottom panel of Table 1 presents similar results, only constraining to those who are working and have an education of high school or less. This group will become important and they will be of interest in later regressions. Examination of this section of Table 1 shows that the same general patterns as above hold. For example, while the mean probability of completing middle school or higher is 0.853 for males in the control group, it is only 0.494 for the treatment group. The same trend follows for females, with a probability of 0.820 for those who are treated and 0.505 for those in the control group.

VII. Impact of the Reform on Educational Attainment

Next, I turn to regression analysis to more accurately estimate the effect of the reform on educational attainment while controlling for other factors that may also impact schooling. For example, I control for age, sex, region of residence, firm size as well and year fixed effects. The key parameter of interest is the coefficient on the variable for exposure to the reform, as this demonstrates the impact of the reform on schooling attained. The key assumption in all these analyses is that there were no other factors at the time that would be differentially impacting the educational attainment of the treatment group beyond that captured by the pre-existing trend that is clear from Figure 1.

Regression analyses support and are consistent with the conclusions that can be drawn from examining Figures 1 through 8, which demonstrate the impact of the reform's impact on average educational attainment. Table 2 shows these regression results for different groups of the population. The results provide estimates of the magnitude of the reform on middle school completion. In the regressions the unit of observation is the individual and the dependent variable is a dummy variable that equals one if the respondent completed middle school or higher at the time of the survey (in 2011 or 2012), and zero otherwise. The entries are the estimated coefficients from linear probability

models and all standard errors are clustered at age-by-province. Clustering by treatment-by-region provides almost the same estimate of standard errors. Here, the key independent variable is a dummy that indicates if the person was "treated by the law," i.e. if they were old enough to be mandated into more schooling by the reform. Each regression controls for age, sex and province of residence and urban residence (population of more than 20,000). When I control for these variables, I find that the reform increases the probability of middle school completion by 15 percentage points for all respondents in the sample (column 1 of Table 2).

I next explore whether and to what extent the effect of the reform varies across groups. For example, the results in column 2 of Table 2 demonstrate that the reform increased the likelihood that males graduated from middle school by 13.1 percentage points, but had an even larger impact for females, increasing their probability of completing middle school of 17.0 percentage points. In percentage terms, this implies an increase by 16 percent for males from their pre-policy average and a 27 percent increase for females. This result is likely because the legislation itself was written to emphasize an increase in educational attainment for the female population, which traditionally has lower educational attainment levels in Turkey than its male counterpart. Given that there are about 3.2 million girls ages 20-24 in 2012 in Turkey, my estimates imply that because of the reform an additional 540,000 women in this age range in 2012 achieved their middle school diploma. Applying the estimates to future cohorts (i.e. those younger than 20) suggests that about 108 thousand girls at each age will complete middle school as a result of the reform.

It is also possible to break down the sample by urban and rural subcategories. For males, the effect varies with respect to their status as living in rural or urban areas. The reform made urban males 11.7 percentage points more likely to complete middle school or higher (a 14% increase), while it made rural males 17.0 percentage points more likely to complete middle school or higher (a 23% increase). This difference between urban and rural males may largely be attributed to the fact that rural areas had more resources poured into them to implement the legislation. Additionally, rural

males may have more to gain on the margin from a middle school education than their urban counterparts. The results are different for females in that the impact was greater for females living in urban areas, with the legislation creating a 17.8 percentage point (26% increase) increase for those in urban areas compared to a 15.4 percentage point (32% increase) increase for those living in rural areas. But, measured proportionally, the effect of the reform was larger for both males and females in rural areas given the much lower rates of baseline educational attainment there.

It should be noted that the coefficients of the age variable are negative in Table 2, which shows that the older cohorts have a lower propensity to have at least a middle school diploma. This is a representation of the negative trend of schooling as a function of age, shown in Figures 1 to 8.

I also run regressions for the sample of individuals where the highest level of attained education is capped at high school. This means that I am comparing students who completed middle school or high school (but not vocational school, college or a higher degree) to students who completed elementary school or less. The regression results presented in Table 3 demonstrate that the reform increased the probability of obtaining a middle school diploma by 21.7 percentage points in this sample (37 percent increase). For males there is a 22.4 percentage point increase (32 percent increase); similarly there is a 21.2 percentage point increase for females, which translates into a 42 percent increase. The greatest increase comes for rural males, who – as a result of the reform – have a 25.6 percentage point increase in the probability of attaining a middle school diploma, which is a 41 percent increase from the sample average of 63.0 percent. All these would suggest that the reform was successful in increasing educational attainment of those students who were on the margin of completing middle school or not. This group is of particular interest as they potentially have the most to gain from this additional educational attainment.

I also examine if there is a spillover effect of the law. As such, I run regressions of the probability of completing high school, probability of completing vocational school or the probability of completing college. I find that the reform has increased the probability of completing high school

or college, while it has decreased the propensity to obtain vocational school diploma. In other words, the reforms seems to have moved the students away from vocational schools towards traditional high schools. This suggests that there is some positive externality from the middle school education that results in students continuing their education. These results are presented in Appendix Table 1.[6]

VIII. Wage Regressions

Theoretically it should be expected that an increase in schooling would result in an increase in wages. First, I seek to examine if the shock in additional schooling caused people to join the labor force, to choose full time work over part time, or to work in a small firm. As shown in the Appendix, additional schooling did not have an effect on any of these outcomes.

Respondents in the survey were classified as working, not working but seeking work, or not seeking work at the time of the survey. Those who were working were asked, among other things, their working hours for the current workweek as well as their average hours worked. The sample averages of these two questions were extremely close, which suggests that there was little over-reporting in working hours. Thus, I use the number of hours worked in the past week as my general measure of labor supply. This variable is used both as a cutoff for those working full time (defined as working more than 30 hours per week) as well as in the calculation of hourly wages.

The base equation I estimate is as follows:

$$\log W_i = \beta_0 + \beta_1 Educ_i + \Psi_i' X_i + \sum_{j=1}^{25} \alpha_j \Pr ov_{ij} + \varepsilon_i \qquad (1)$$

[6] Yuret (2010) finds that although the law was targeted at making middle school compulsory, it had a side effect of increasing high school attainment by 3.2 percent. Furthermore, he finds that the increase in educational attainment proves to be the most significant for students coming from the lowest socio-economic backgrounds.

where $logW_i$ is the natural logarithm of nominal hourly wages and *Educ* is a binary variable for having completed middle school. Potential differences in price levels between regions are captured by a set of 25 dummies representing the province of residence and by a year fixed effect variable to account for the change in the level of wages due to the survey year (2011 vs. 2012). The vector X includes age, a binary variable for living in an urban versus rural area, a binary variable that indicates if the worker is working in a small firm (defined as "small" if there are fewer than 10 employees). Age is an important control to include because people who are older generally have more experience and therefore tend to earn higher wages. Industry of employment is not controlled for because the worker's choice of industry may be determined by education. There is no information in the data about work experience. Although this variable can be approximated as (Age minus years of schooling minus 6), it cannot be included to the regressions because it depends on years of schooling and age and is thus a linear combination of other vectors in the regression. Equation (1) does not include marital status because this is an outcome variable which itself may depend on education. Some individuals may decide to delay their marriage decision as a result of their increased level of education due to the reform, and inclusion of marital status in the wage equation would bias the estimated effect of education on wages (Angrist and Pischke 2009, pp. 64-66).

Having a middle school diploma is endogenous and in the IV models the first-stage regression involves regressing *Educ* on exposure to the treatment by the reform as well as other exogenous variables. Treatment equals one for individuals ages 20-24(21-25) during 2011(2012) (thus certain to have been affected by the reform), equals zero for individuals ages 26(27)-29(30) during 2011(2012) (thus certain to have missed the reform) and equals ½ for individuals age 25(26) during 2011(2012) (who had a 50% probability of being affected by the reform). As mentioned earlier, dropping the 25(26) year olds from the sample did not change the results.

I examine if having a middle school education (by instrumenting middle school completion with the 1997 legislation) creates a significant increase in wages. I only consider those workers who are working full time (30 hours or more a week) and who were not enrolled in school at the time of the survey. It should be noted that exposure to the reform does *not change* the propensity to work full-time or the propensity to be in the labor force and thus concerns about changes in the composition of workers potentially biasing the estimates are much less of an issue. Specifically, regressions of full-time work as well as labor force participation on being treated by the reform do not produce significant coefficients. These results are presented in Appendix Table 2 and Appendix Table 3.[7]

In order to account for correlation in wages of people of the same age who are living in the same area, standard errors are clustered by age per province. This created 130 clusters (5 age groups times 26 provinces).[8] I also report standard errors that are clustered at the treatment by province level (2 categories, treated or not, for 26 provinces provides a total of 52 clusters) as well as the robust standard errors. For the instrumental variable regressions there is almost no difference between these three methods of calculating the standard errors.

In first set of regressions I cap the highest level of education at the high school level. Given the reform increased the propensity to complete both middle and high school, I first consider students who earned *at most* a high school education. I also estimate, however, the wage regressions using (i)

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[7] In the regressions where full-time work or labor force participation is the dependent variable, these variables are regressed on the treatment dummy, rather than education. This is because full-time work and labor force participation decision depend on wages and wages depend on education. So, the impact of education cannot be identified independently. As a result, I estimate reduced-form regressions of full-time work and labor force participation.

[8] The HLFS partitions the country into 26 provinces. Most of these provinces include three cities, although come provinces consist of one large city, such as Istanbul, Ankara and Izmir. Age is categorized into five groups (20&21, 22&23, 24&25, 26&27, 28&29&30) and then a cluster is created for each province and group, creating 26x5 = 130 clusters

the sample of workers who attained a middle school education or less, (ii) the sample of workers who have graduated from high school, vocational school or a lower level of education, (iii) and the sample of workers with all levels of education, including those with a college degree or higher. Finally, the top and bottom one percent of the wage distribution are omitted from the regressions, as these constitute outliers, possibly due to reporting/coding errors in salaries or work hours. For example, for women the first percentile of the wage distribution is 0.625 Turkish Lira (about 49 cents) whereas in 2011 the legal minimum wage was 629 Turkish Lira per month, or 3.9 TL per hour (about $2.1) for a 40-hour work per week.

Table 4 presents the results of the wage regressions from the IV specifications, as well as their OLS counterparts. For the entire sample (which includes both males and females) OLS produces a statistically significant coefficient for middle school completion variable, indicating about 7 percent return to a middle school diploma. The IV estimate is larger and indicates a return of about 10 percent (for three years of additional education – assuming that the average student did complete the mandated three years of middle school education).

As discussed by Oreopoulos (2006), IV estimates could be larger than OLS estimates if the local average treatment effect of the IV only approximates the return for a peculiar, non-representative group. However, he demonstrates that in a large scale education intervention in the U.K., the IV results are similar to those obtained from the local average treatment effects estimated in other studies that were based on limited exposure to treatment. In this paper, the exposure to treatment is nearly universal as the Turkish law mandated middle school completion. Therefore, the IV estimates are likely to be credible, regardless of their magnitude in comparison to OLS estimates.

The results show no statistically significant return to the middle school diploma for males. For females in the sample, however, while OLS estimates produce a significant 18 percent return to

the middle school diploma the IV estimates show a statistically significant return of 43 percent (about 14 percent per year of schooling). [9] The sample sizes for females are small at only 4,520 observations because females in Turkey have a very low labor force participation rate. One possible reason that the effects may be larger for females is that having more females in the classroom may increase the returns to schooling for other females who were already attending through middle school by giving them more peers.

The bottom panel of Table 4 presents the mean wages (not in logs) in each sample. It also displays the coefficient of the treatment in the first-stage regression and the F-value of the instrument in the first stage. For all three IV regressions the F-values are far over 10, which suggests that the instrument is strong. The main takeaway from these results is that for males with high school education or less who received additional education as a result of the reform there was little impact, whereas for females there is about a 41 percent return to the diploma, or about a 14 percent return per year.

The literature has shown consistently that large firms tend to pay higher wages[10]. Some reasons for this may be that large firms are trying to pay higher wages to avoid unionization, or are offering inferior working conditions (Brown and Medoff, 1989). To take this into account, I control for firm size in all my regressions. Consistent with the findings of previous research, I find that Turkish workers in larger firms earn higher wages than those in small firms. Specifically, workers in small firms (those with fewer than 10 workers) earn wages that are 12-20 percent lower than their counterparts in large firms. This impact is largest for women at nearly 20 percent. In the appendix,

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[9] In this case, the coefficients are 0.169 and 0.358, but because the outcome variable is log wage I calculate the effect as 100x[exp(0.169)-1]% and 100x[exp(0.358)-1]% , respectively. For all coefficients over 0.1 in my wage equations I estimate the percent impact by this formula.

[10] For example, it has been shown to be true in Zimbabwe, as well as Ghana and Kenya that large firms pay higher wages than their smaller counterparts (Velenchick, 1997, Soderbom et al, 2004). There is also evidence from the U.S. that workers in large firms get an unexplained wage premium (Troske, 1999).

I show that having a middle school diploma, induced by the reform, does not change the propensity to work in a small firm.

Column (1) Table 4 also shows that males earn about 13 percent more than females, holding constant age, education, urban residence and firm size. This result is similar to the male-female wage gap reported in other countries.

Table 5 demonstrates the wage regressions for males with different levels of education. Column (2) of Table 5 is the same as column (3) of Table 4, where for males with a high school diploma or less the impact increases of the diploma is insignificant. This is true for all levels of education for males, including middle school or less, high school and vocational school or less, and all levels of education.

Table 6 shows the same specifications for females. Column (2), which is the same as column (5) of Table 4, shows that the coefficient of middle school completion is 0.358, indicating that wages of females with a high school education or less are 43 percent higher as a result of the middle school diploma. However, adding the additional 1,200 or so students who completed vocational school (column 3) decreases the coefficient to 0.255 – a smaller return of 29 percent. This is not surprising because vocational high school graduates typically earn wages that are lower than standard high school graduates. Unlike males, analyzing the entire sample of females (column 4 of Table 6) produces a significant coefficient of 0.645 on the education variable. This estimate is very large, though it must be remembered that this group includes anyone from college graduates to those with only an elementary school education, so there is substantial variation in their wages.

IX. Conclusion and Discussion

Although economists have used different methods to estimate the causal effect of education, the overwhelming majority of the studies have used data from developed countries. In this paper I exploit an education reform in Turkey which mandated middle school education for all students, and

increased the compulsory education from 5 to 8 years in 1997. I use data from the 2011 and 2012 Household Labor Force Surveys (equivalent to the CPS in the U.S.) and consider treatment by this law based on year of birth as an instrument for educational attainment. I find that (consistent with descriptive evidence presented in Figures 1-8) the law was successful in increasing the propensity to complete middle school or higher. Furthermore, the law increased the propensity to obtain a high school or college degree – with larger effects on women, but it decreased the propensity to obtain a diploma from a vocational high school.

The paper demonstrates that for females there are significant returns to education as a result of a middle school diploma. Instrumental variables regressions show that for men there is no statistically significant wage increase as a result of the additional education. For women, however, the return is 43 percent for the middle school diploma, which is an almost 14 percent return per year of school.[11] Increased education had no impact on labor force participation, full-time work, or the propensity to work in a small firm.

The large returns to education for women are not surprising because of two points. First, female wages on average are low. For example, average female wages of those with at most a middle school education is 3.04 Turkish Lira, which is lower than the legal minimum wage. Furthermore, female wages are about 17 percent lower than male wages with comparable age and education (although male job experience is likely to be greater). Second, the labor force participation of women is low (about 25 percent) and the results show that the reform did not influence the labor force participation behavior. This means that increased education did not influence female labor supply greatly. Thus, it is unlikely a change in female labor supply that is driving in increase in

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[11] The returns to education are generally higher in developing countries (Psacharopoulos 1994). For example, Fang et al (2012) estimated a 20 per cent return per year of education in China.

female wages. Rather, perhaps the increase in female wages as a result in education is manifesting itself through increased demand for female labor.

In summary, these findings show that mandating middle school completion in Turkey did have a significant impact in terms of increasing attained education, and that increased schooling has translated into higher wages, especially for females. These results have potential implications regarding education mandates in other developing nations, especially for those in the Middle East. Crucial to this, however, is a commitment to bring girls into schools along with boys, something that may be more of a challenge in the Middle East than was the case in Turkey. Overall, these results show promising evidence that increasing educational attainment has positive wage outcomes.

Figure 1

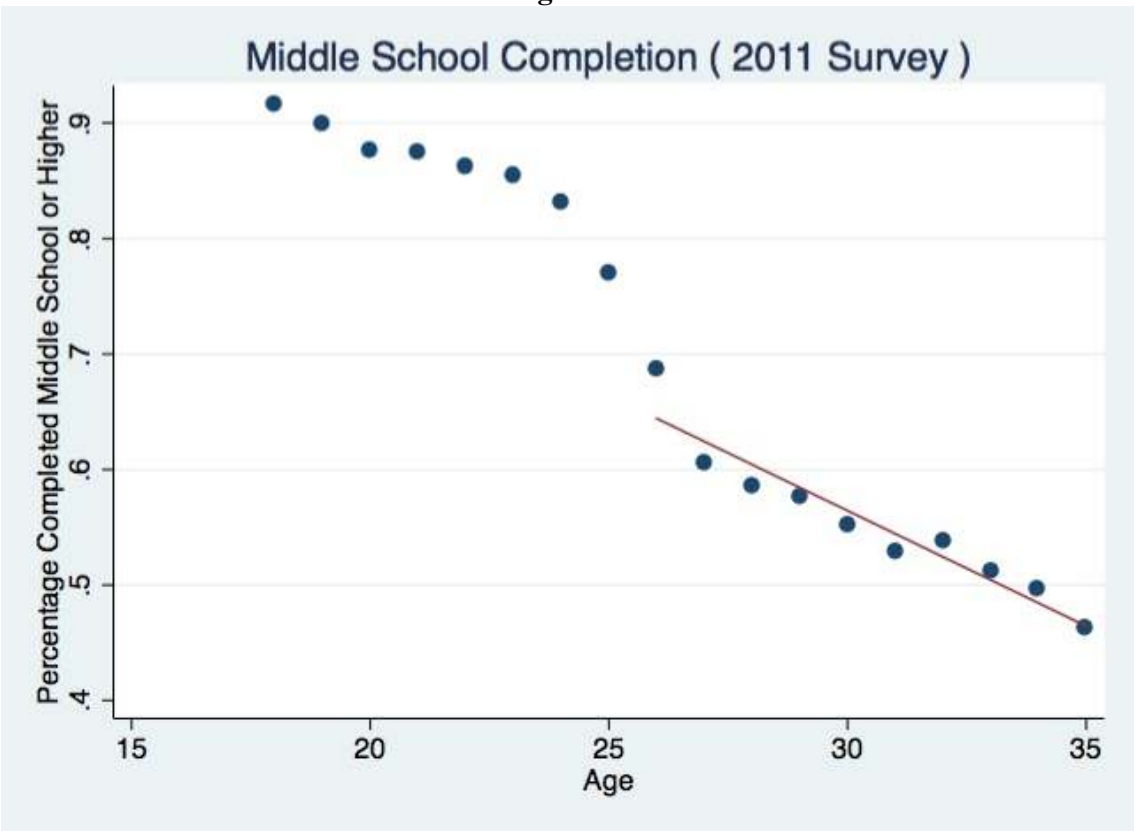

Figure 2

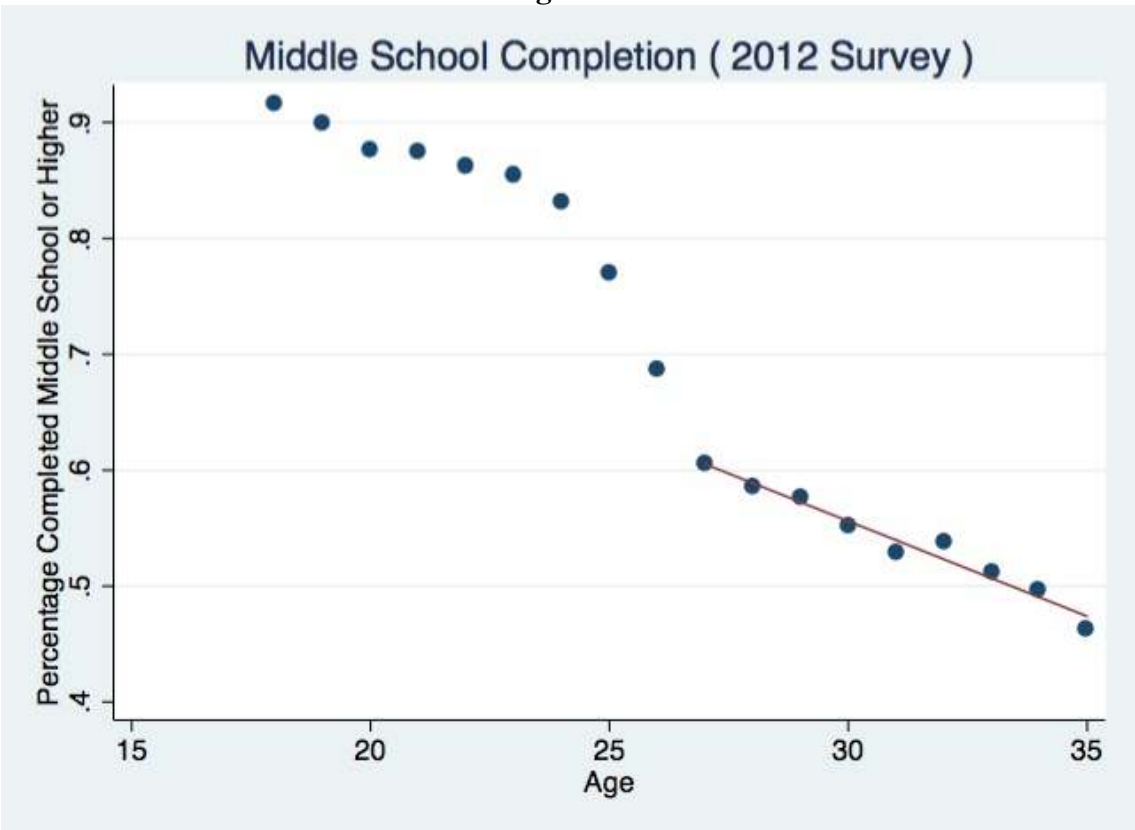

Figure 3

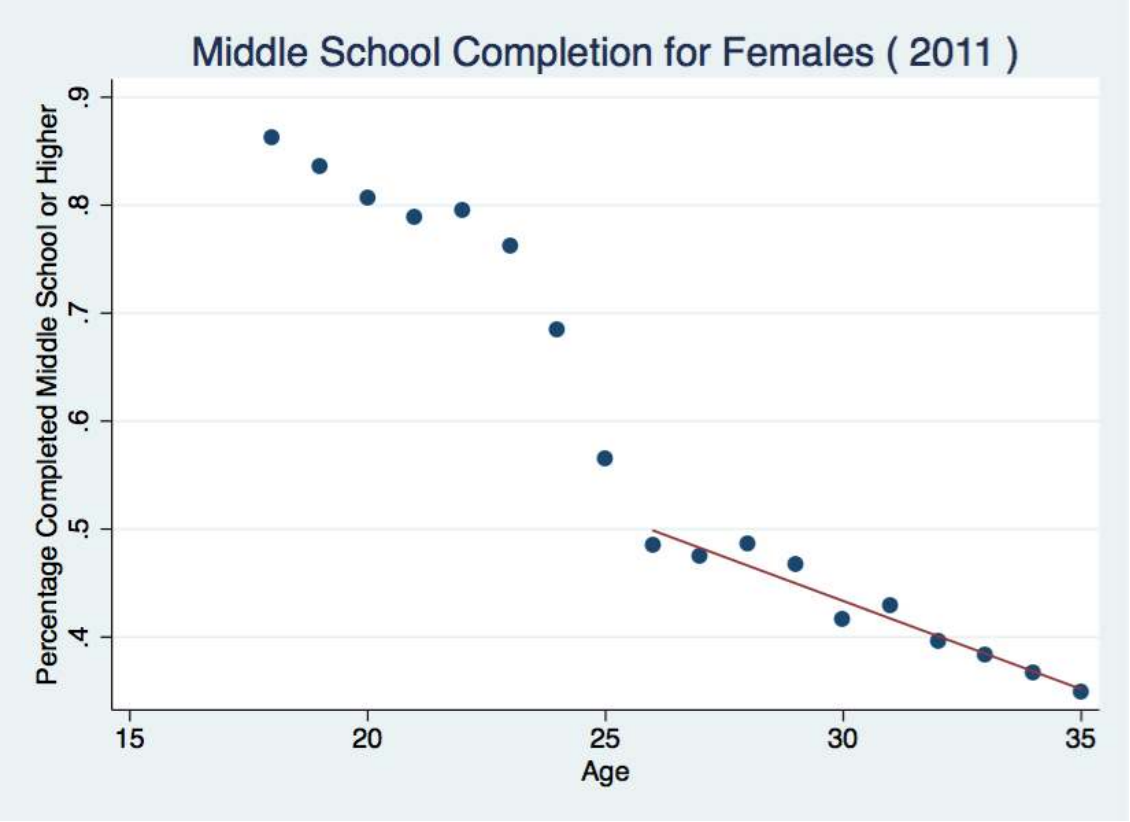

Figure 4

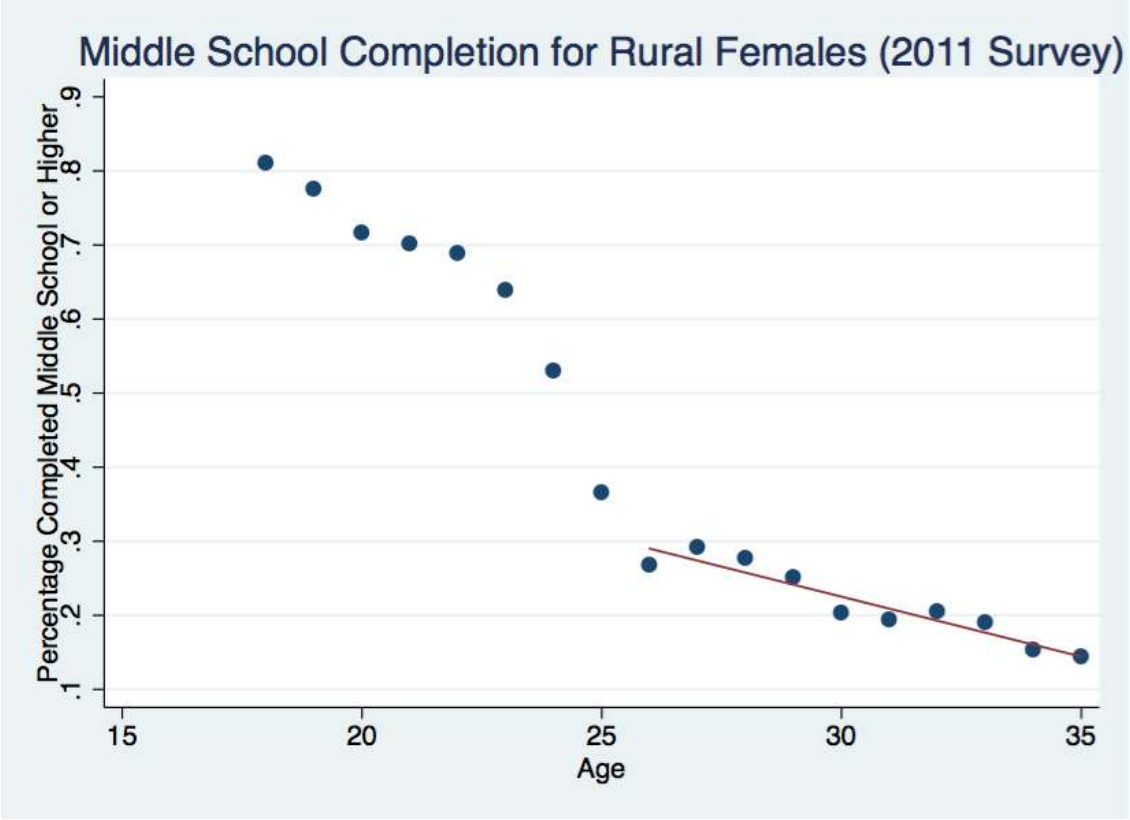

Figure 5

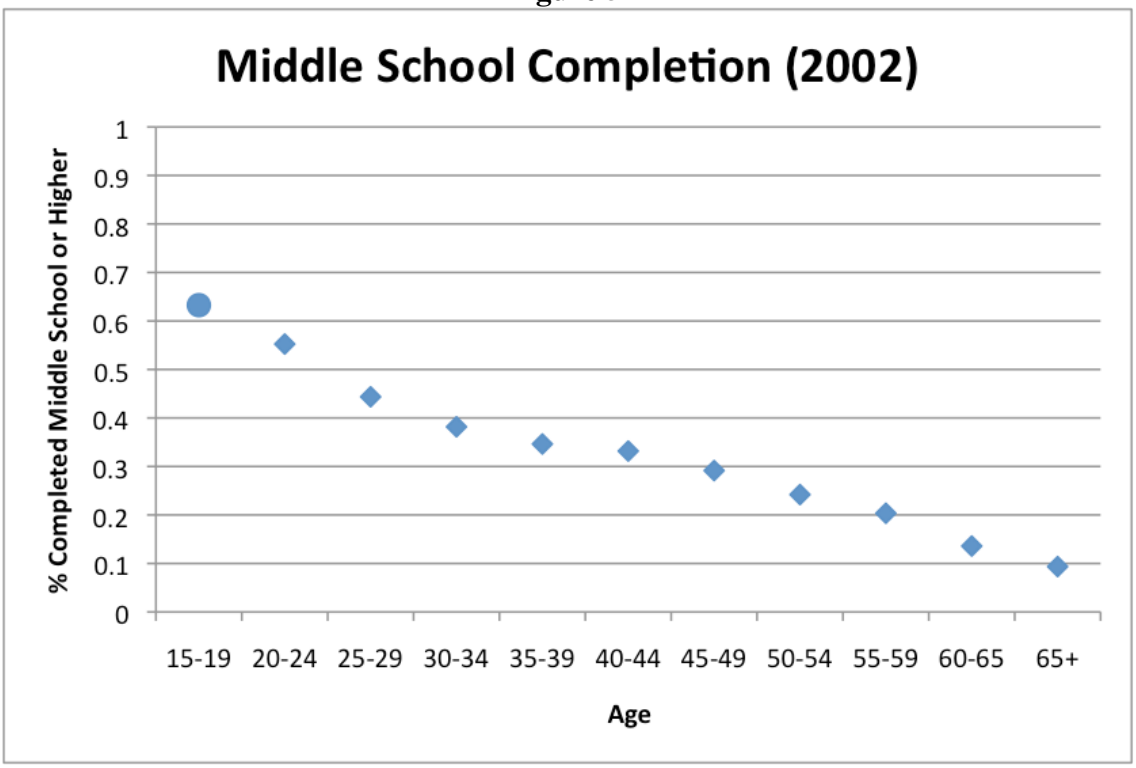

Figure 6

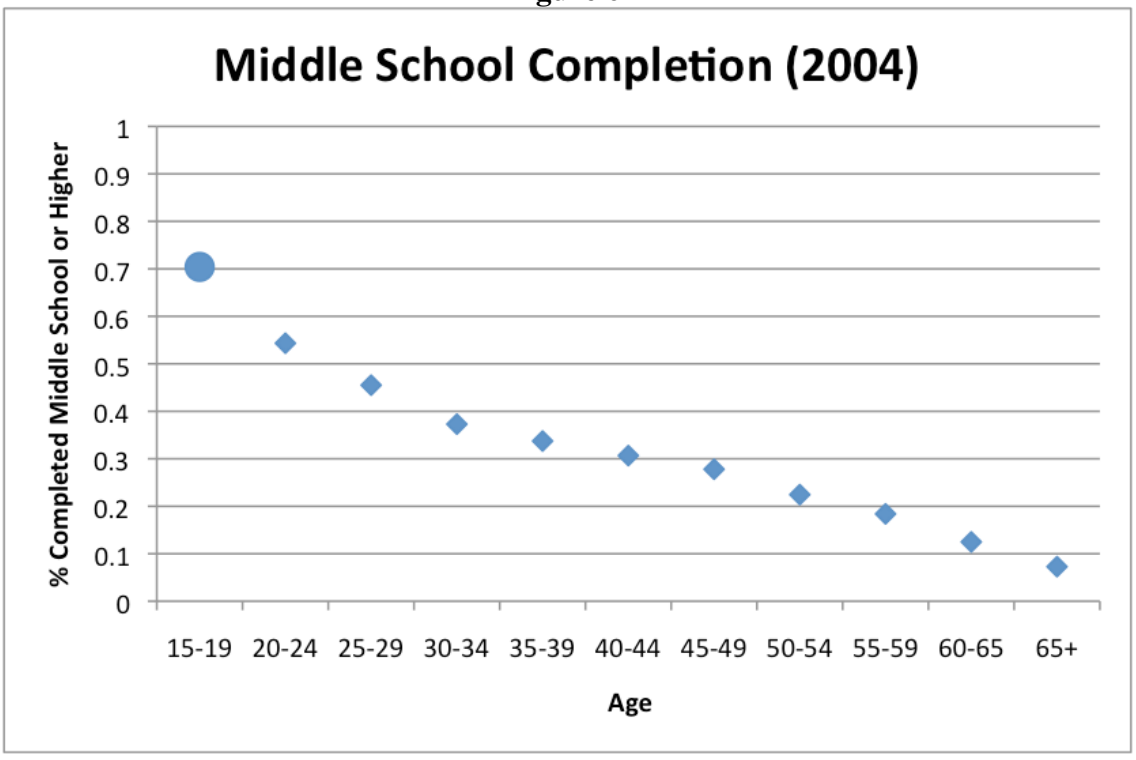

Figure 7

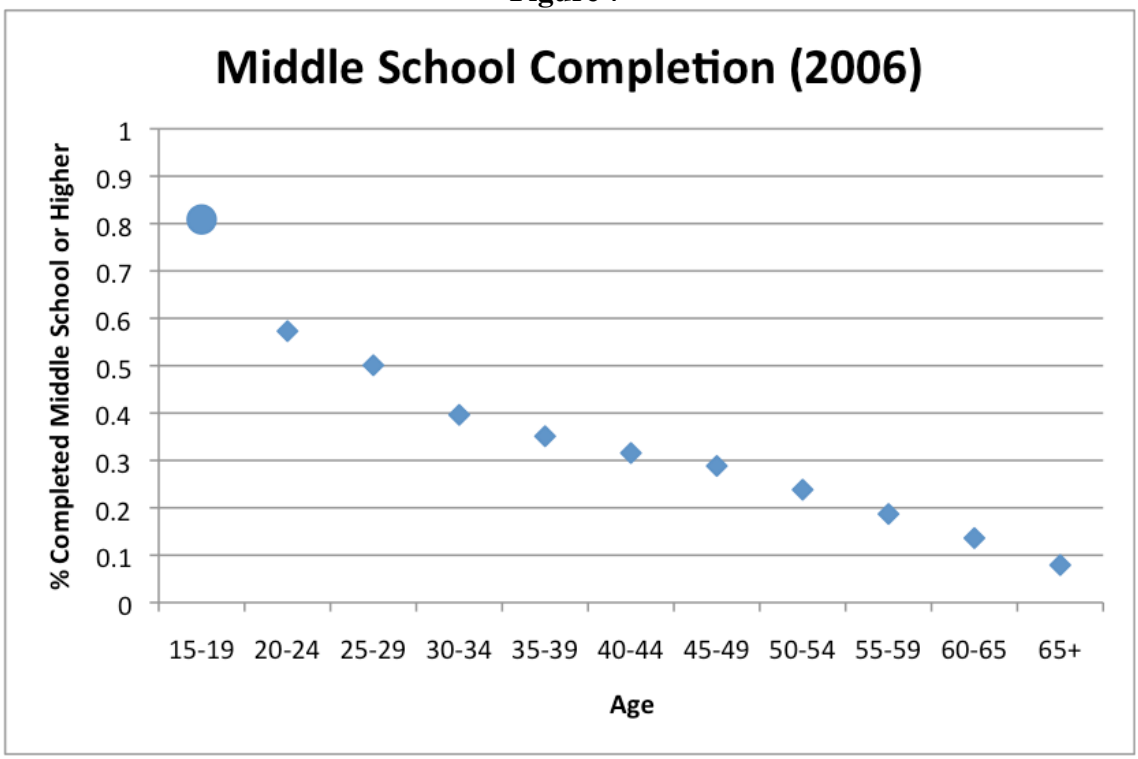

Figure 8

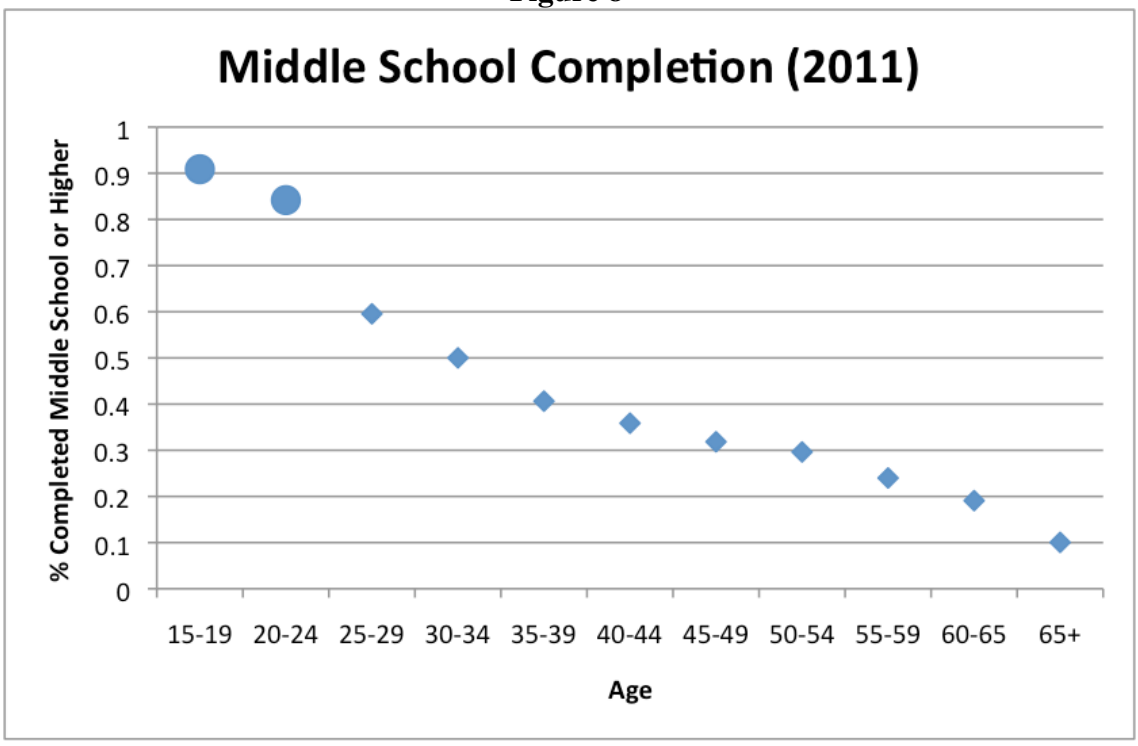

Table 1
Summary Statistics

Variable	Definition	Entire Sample (1)	Treated Females (2)	Treated Males (3)	Control Females (4)	Control Males (5)
		Age: 20-30 in 2011-2012	Age: 20-24 in 2011 21-25 in 2012		Age: 26-29 in 2011 27-30 in 2012	
Middle School Completion	Dummy =1 if completed middle school or higher	0.717 (0.450)	0.783* (0.412)	0.911* (0.284)	0.485* (0.500)	0.678* (0.467)
	N‡	144,247	36,366	33,224	30,914	28,902
		Full Time Workers with All Levels of Education				
Middle School Completion	Dummy =1 if completed middle school or higher	0.811 (0.392)	0.919* (0.273)	0.904* (0.295)	0.828* (0.377)	0.701* (0.458)
Small Firm	Dummy=1 if working in a small firm (fewer than 10 workers)	0.340 (0.474)	0.303* (0.460)	0.415* (0.492)	0.236* (0.425)	0.331* (0.471)
Wage	Hourly Nominal Wage in Full-Time Work (weekly hours>30)	4.793 (2.889)	4.300* (2.521)	3.988* (2.159)	6.101* (3.653)	5.092* (2.984)
Urban	Dummy=1 if the person lives in an urban area (population > 20,000)	0.832 (0.374)	0.861* (0.346)	0.787* (0.409)	0.900* (0.300)	0.834* (0.372)
	N‡	42,591	4,901	11,793	5,154	15,962
		Full Time Workers with Education High School or Lower				
Middle School Completion	Dummy =1 if completed middle school or higher	0.661 (0.473)	0.820* (0.384)	0.853* (0.354)	0.505* (0.500)	0.494* (0.500)
Small Firm	Dummy=1 if working in a small firm (fewer than 10 workers)	0.430 (0.495)	0.371 (0.483)	0.481* (0.500)	0.365 (0.482)	0.413* (0.492)
Wage	Hourly Nominal Wage in Full-Time Work (weekly hours>30)	3.626 (1.337)	3.190* (1.156)	3.452* (1.240)	3.456* (1.298)	3.898* (1.412)
Urban	Dummy=1 if the person lives in an urban area (population > 20,000)	0.799 (0.400)	0.820* (0.384)	0.770* (0.421)	0.846* (0.361)	0.811* (0.391)
	N	23,854	2,248	7,786	1,820	9,405

‡ : The number of observations of columns (2)-(5) do not add up to the number of observations reported in column (1) of the entire sample because the means of the treatment and control groups reported in columns (2)-(5) do not contain, the 25/26-year olds (for 2011 and 2012 respectively). The dependent variable is a dichotomous indicator that takes the value of one if the person has middle school education as his/her highest level of attained education. Control variables include age, sex, urban residence, year fixed effects and dummies for the 26 provinces of residence. Regressions that included age-squared provided the same results. The sample includes all respondents who were ages 20 to 29 in 2011 and 20-30 in 2012. Standard errors (in parentheses) are clustered at the province-by-age level (130 clusters) to eliminate correlation between workers of the same age living in the same area * indicates a significant difference between means as indicated by a t-test

Table 2

The Effectiveness of the Reform in Increasing the Propensity for Middle School Completion
(Using people 20-30 Years of Age, *with Any Level of Education*)
Dependent Variable: Middle School Completion or Higher

	Entire Sample	Males	Females	Urban Males	Rural Males	Urban Females	Rural Females
Coefficient of Treatment	0.149*** (0.013)	0.131*** (0.012)	0.170*** (0.019)	0.117*** (0.012)	0.170*** (0.023)	0.178*** (0.019)	0.154*** (0.039)
Age	-0.023*** (0.002)	-0.020*** (0.002)	-0.025*** (0.004)	-0.017*** (0.002)	-0.029*** (0.004)	-0.018*** (0.003)	-0.046*** (0.006)
Male	0.167*** (0.009)	.	.	.	.	.	.
Urban	0.144*** (0.011)	0.102*** (0.011)	0.184*** (0.013)	.	.	.	.
Year Fixed Effect	-0.026*** (0.003)	-0.023** (0.004)	-0.028*** (0.006)	-0.024*** (0.005)	-0.018 (0.009)	-0.022*** (0.005)	-0.046*** (0.010)
Province Dummies	Y	Y	Y	Y	Y	Y	Y
Mean Middle School Completion	0.717	0.801	0.639	0.824	0.726	0.686	0.482
N	144,247	69,467	74,780	53,010	16,457	57,731	17,049

The dependent variable is a dichotomous indicator that takes the value of one if the person has middle school education as his/her highest level of attained education. Control variables include age, sex, urban residence, year fixed effect (year=2011) and dummies for the 26 provinces of residence. Regressions that included age-squared provided the same results. The sample includes all respondents who were ages 20 to 29 in 2011 and age 20-30 in 2012. Standard errors (in parentheses) are clustered at the province-by-age level (130 clusters) to eliminate correlation between workers of the same age living in the same area * : $p < 0.10$, **: $p < 0.05$, *** : $p < 0.01$

Table 3

The Effectiveness of the Reform in Increasing the Propensity for Middle School Completion
(People 20 to 30 Years of Age and *Highest Attained Education is High School or Lower*)
Dependent Variable: Middle School Completion

	Entire Sample	Males	Females	Urban Males	Rural Males	Urban Females	Rural Females
Coefficient of Treatment	0.217*** (0.019)	0.224*** (0.016)	0.212*** (0.029)	0.207*** (0.018)	0.256*** (0.029)	0.231*** (0.030)	0.165** (0.046)
Age	-0.034** (0.003)	-0.029*** (0.003)	-0.038*** (0.005)	-0.028*** (0.003)	-0.034*** (0.005)	-0.033*** (0.005)	-0.050*** (0.008)
Male	0.198*** (0.009)	.	.	.	.	.	.
Urban	0.111*** (0.010)	0.090*** (0.012)	0.130*** (0.011)	.	.	.	.
Year Fixed Effect	-0.027*** (0.005)	-0.023*** (0.005)	-0.030*** (0.008)	-0.025*** (0.006)	-0.020*** (0.010)	-0.022*** (0.008)	-0.049*** (0.011)
Province Dummies	Y	Y	Y	Y	Y	Y	Y
Mean Middle School Completion	0.591	0.697	0.502	0.721	0.630	0.542	0.393
N	99,670	45,576	54,094	33,352	12,224	39,530	14,564

The dependent variable is a dichotomous indicator that takes the value of one if the person has middle school education as his/her highest level of attained education. Control variables include age, sex, urban residence, year fixed effect (year=2011) and dummies for the 26 provinces of residence. Regressions that included age-squared provided the same results. The sample includes all respondents who were ages 20 to 29 in 2011 and age 20-30 in 2012. Standard errors (in parentheses) are clustered at the province-by-age level (130 clusters) to eliminate correlation between workers of the same age living in the same area * : $p < 0.10$, **: $p < 0.05$, *** : $p < 0.01$

Table 4

Wage Regressions for Workers
with Highest Education High School or Lower

	Entire Sample		Males		Females	
	IV	OLS	IV	OLS	IV	OLS
	(1)	(2)	(3)	(4)	(5)	(6)
Middle School Completion	0.098**	0.073***	0.059	0.055***	0.358**	0.169***
	(0.047)	(0.009)	(0.050)	(0.011)	(0.121)	(0.014)
	[0.039]	[0.012]	[0.046]	[0.013]	[0.120]	[0.014]
	{0.043}	{0.005}	{0.049}	{0.006}	{0.096}	{0.012}
Age	0.026**	0.025***	0.026***	0.026***	0.031***	0.023***
	(0.003)	(0.001)	(0.004)	(0.002)	(0.006)	(0.002)
Male	0.132***	0.133***				
	(0.011)	(0.011)	.	.	.	.
Urban Residence	0.003	0.005	-0.012	-0.012	0.068**	0.090***
	(0.011)	(0.011)	(0.012)	(0.011)	(0.027)	(0.021)
Small Firm	-0.134***	-0.135***	-0.123***	-0.123***	-0.199***	-0.193***
	(0.010)	(0.011)	(0.009)	(0.010)	(0.021)	(0.020)
Year Fixed Effect (2011 dummy)	-0.104***	-0.105***	-0.104***	-0.104**	-0.107***	-0.113***
	(0.005)	(0.005)	(0.006)	(0.005)	(0.012)	(0.012)
Province Dummies	Y	Y	Y	Y	Y	Y
Mean Wage	3.63	3.63	3.70	3.70	3.33	3.33
Coefficient of Treatment in the 1st Stage Regression	0.257***	.	0.248***	.	0.276***	.
	(0.021)		(0.022)		(0.047)	
F-Statistic of the Instrument	153.15		123.14		34.31	
N	23,854	23,854	19,352	19,352	4,520	4,520

The dependent variable is the log of nominal wages. The table reports both IV and OLS estimates for the whole sample (everyone who was ages 20-29 in 2011 or 20-30 in 2012 who was working full time) as well as males and females in the sample. The instrument is being born in the cohort exposed to the legislation. All regressions control for age, sex, urban residence, firm size, year fixed effects and province of residence. Standard errors (in parentheses) are clustered at the province-by-age level (130 clusters) to eliminate correlation between workers of the same age living in the same area. Standard errors in straight brackets [] are clustered at the treatment-by-province level (52 clusters) and standard errors in curly brackets {} are robust standard errors *$p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 5

Instrumental Variables Wage Regressions for Workers *with Various Levels of Education*
Males

	Highest Level of Education			
	Middle School or Less (1)	High School or Less (2)	High School/Vocational School or less (3)	All Education Levels (4)
Middle School Completion	0.044	0.059	0.053	-0.142
	(0.041)	(0.050)	(0.060)	(0.098)
Age	0.024***	0.026***	0.026***	0.032***
	(0.004)	(0.004)	(0.003)	(0.004)
Urban Residence	-0.005	-0.012	-0.007	0.048
	(0.011)	(0.012)	(0.012)	(0.013)
Small Firm	-0.115***	-0.123***	-0.142***	-0.277***
	(0.010)	(0.009)	(0.008)	(0.014)
Year Fixed Effect (2011)	-0.096***	-0.104***	-0.101***	-0.121***
	(0.007)	(0.006)	(0.005)	(0.009)
Province Dummies	Y	Y	Y	Y
Mean Wage	3.58	3.70	3.83	4.62
Coefficient of Treatment in the 1st Stage Regression	0.302***	0.228***	0.197***	0.143***
	(0.028)	(0.022)	(0.017)	(0.013)
F-Statistic of the Instrument	125.63	123.14	129.00	113.26
N	15,027	19,352	25,067	31,336

The dependent variable is the log of nominal wages. The table reports IV estimates for males in the whole sample (everyone who was ages 20-29 in 2011 or 20-30 in 2012 who was working full time) at various levels of education. These cutoffs are middle school or less, high school or less, high school or vocational school or less, and all education levels, respectively. The instrument is being born in the cohort exposed to the legislation. All regressions control for age, urban residence, firm size, year fixed effects and province of residence. Standard errors (in parentheses) are clustered at the province-by-age level (130 clusters) to eliminate correlation between workers of the same age living in the same area. *$p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 6

Instrumental Variables Wage Regressions for Workers with *Various Levels of Education*
Females

	Highest Level of Education			
	Middle School or Less (1)	High School or Less (2)	High School/Vocational School or less (3)	All Education Levels (4)
Middle School Completion	0.152 (0.127)	0.358** (0.121)	0.255* (0.138)	0.645** (0.267)
Age	0.017* (0.010)	0.031*** (0.006)	0.028*** (0.005)	0.064*** (0.004)
Urban Residence	0.086** (0.026)	0.068** (0.027)	0.081** (0.028)	0.043 (0.038)
Small Firm	-0.188*** (0.015)	-0.199*** (0.021)	-0.195*** (0.019)	-0.306*** (0.017)
Year Fixed Effect (2011)	-0.128*** (0.015)	-0.107*** (0.012)	-0.109*** (0.008)	-0.100*** (0.008)
Province Dummies	Y	Y	Y	Y
Mean Wage	3.04	3.33	3.58	5.26
Coefficient of Treatment in the 1st Stage Regression	0.292*** (0.051)	0.276*** (0.047)	0.199*** (0.034)	0.132*** (0.026)
F-Statistic of the Instrument	32.20	34.31	33.85	25.25
N	2,762	4,520	6,206	11,218

The dependent variable is the log of nominal wages. The table reports IV estimates for females in the whole sample (everyone who was ages 20-29 in 2011 or 20-30 in 2012 who was working full time) at various levels of education. These cutoffs are middle school or less, high school or less, high school or vocational school or less, and all education levels, respectively. The instrument is being born in the cohort exposed to the legislation. All regressions control for age, urban residence, firm size, year fixed effects and province of residence. Standard errors (in parentheses) are clustered at the province-by-age level (130 clusters) to eliminate correlation between workers of the same age living in the same area. *$p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix

Spillover Effects of the Reform

My findings show that one significant side effect of the reform was to increase people's propensity to complete levels of schooling beyond just middle school. Appendix Table 1 shows that the reform led to a 5.4 percentage point increase in probability of high school or college completion, while it decreased the probability of vocational school completion by 2.0 percentage points. Furthermore, when the propensity to complete college alone is considered, we see that the reform made students 6.0 percentage points more likely to complete that level of higher education.

Potential Selection Problems

One potential concern in my analysis is the possibility that people are selecting into labor force, full time work or their industry of employment as a result of their level of attained education. In other words, what if more education as a result of the reform causes people to enter the labor force, work fulltime (where they would have worked part time or not at all before) or causes people to work in a high skill industry over a low skill industry? If this is the case, I should see significant coefficients for regression that predict these outcomes as a result of treatment under the reform. If such a selection problem exists, then there would be correlation between the instrument and the controls in the regression, thus confounding what appears to be a significant coefficient in my analysis. To show that this is not the case, I run the aforementioned regressions and show that the impact of education on these outcomes is indeed not significant. These results are displayed in Appendix Tables 2-4.

Appendix Table 1

The Effectiveness of the Reform in Increasing the Propensity to Complete High School, Vocational School, or College

	High school or College (no vocational school)	Vocational high school only	College or higher
	Entire Sample		
Coefficient of Treatment	0.054***	-0.020***	0.060***
	(0.010)	(0.005)	(0.015)
Age	-0.007***	-0.006***	0.017***
	(0.002)	(0.001)	(0.003)
Male	0.048***	0.054***	0.011**
	(0.008)	(0.004)	(0.005)
Urban Residence	0.182***	0.027***	0.098***
	(0.008)	(0.004)	(0.006)
Year fixed effect	-0.017***	-0.005**	-0.013**
	(0.004)	(0.002)	(0.004)
Province Dummies	Y	Y	Y
N	144,247	144,247	144,247
		Males	
Treatment	0.019	-0.025***	0.025*
	(0.015)	(0.007)	(0.015)
Age	-0.010***	-0.008***	0.015***
	(0.003)	(0.001)	(0.003)
Urban Residence	0.179***	0.006	0.095***
	(0.009)	(0.006)	(0.007)
Year fixed effect	-0.019**	-0.004	-0.018**
	(0.005)	(0.004)	(0.005)
Province Dummies	Y	Y	Y
N	69,467	69,467	69,467
		Females	
Treatment	0.089***	-0.014**	0.096***
	(0.014)	(0.006)	(0.019)
Age	-0.004*	-0.005***	0.020***
	(0.003)	(0.001)	(0.004)
Urban Residence	0.184**	0.047***	0.099***
	(0.008)	(0.004)	(0.006)
Year Fixed effect	-0.015**	-0.005**	-0.009**
	(0.004)	(0.003)	(0.004)
Province Dummies	Y	Y	Y
N	74,780	74,780	74,780

Each column has a dependent variable that is a dichotomous indicator that takes the value of one if the person has high school education. Vocational education, or college as his/her highest level of attained education respectively. Control variables include age, sex, urban residence, year fixed effects and dummies for the 26 provinces of residence. Regressions that included age-squared provided the same results. The sample includes all respondents who were ages 20 to 29 in 2011 and 20-30 in 2012. Standard errors (in parentheses) are clustered at the province-by-age level (130 clusters) to eliminate correlation between workers of the same age living in the same area * : $p < 0.10$, **: $p < 0.05$, *** : $p < 0.01$

Appendix Table 2

The Impact of the Reform on the Probability of working Full time (Respondents with High School or Less), OLS Reduced Form Regressions

	Entire Sample		Males		Females	
Treatment	-0.016**	-0.009	0.004	0.017	-0.025	-0.062
	(0.007)	(0.015)	(0.007)	(0.013)	(0.019)	(0.045)
Age	-0.004**	.	0.004**	.	-0.013***	.
	(0.001)		(0.001)		(0.003)	
Urban Residence	0.098***	0.098***	0.055***	0.055***	0.174***	0.175***
	(0.007)	(0.007)	(0.005)	(0.005)	(0.015)	(0.015)
Male	0.191***	0.191***				
	(0.017)	(0.017)	.	.	.	.
Year Fixed Effect	0.001	0.002	0.003	0.005	0.003	-0.002
	(0.004)	(0.004)	(0.003)	(0.003)	(0.009)	(0.010)
Province Dummies	Y	Y	Y	Y	Y	Y
Age Dummies	N	Y	N	Y	N	N
N	45,000	45,000	32,399	32,399	12,601	12,601

The dependent variable is a dichotomous indicator that takes the value of one if the person was working full time (30 hours or more) at the time of the survey. The main independent variable of interest is "Treatment," which is a dichotomous variable that equals one if the respondent was part of the cohort exposed to the reform (age 5-11 in 1997). Control variables include age, sex, urban residence, year fixed effects and dummies for the 26 provinces of residence. Regressions that included age-squared provided the same results. The sample includes all respondents who were ages 20 to 29 in 2011 and 20-30 in 2012. Standard errors (in parentheses) are clustered at the province-by-age level (130 clusters) to eliminate correlation between workers of the same age living in the same area * : $p < 0.10$, **: $p < 0.05$, *** : $p < 0.01$

In this regression, the variables are regressed on the treatment dummy, rather than education. This is because the full-time work decision depends on wages and wages depend on education. So, the impact of education cannot be identified independently. As a result, I estimate reduced-form regressions of full-time work and labor force participation.

Appendix Table 3

The Impact of the Reform on the Probability of Labor Force Participation, High School or Less Education
OLS Reduced Form Regressions

	Entire Sample		Males		Females	
Treatment	0.051**	0.018	0.117***	-0.003	0.007	0.028
	(0.016)	(0.017)	(0.033)	(0.023)	(0.015)	(0.025)
Age	0.023***	.	0.055***	.	-0.002	.
	(0.003)		(0.007)		(0.003)	
Urban Residence	-0.105***	-0.104***	-0.042***	-0.039***	-0.157***	-0.157***
	(0.008)	(0.008)	(0.009)	(0.009)	(0.010)	(0.010)
Male	0.540***	0.538***	.	.	.	.
	(0.019)	(0.019)				
Year Fixed Effect	0.013**	0.008*	0.032***	0.017**	-0.004	-0.003
	(0.005)	(0.005)	(0.006)	(0.006)	(0.007)	(0.007)
Province Dummies	Y	Y	Y	Y	Y	Y
Age Dummies	N	Y	N	Y	N	N
N	99,670	99,670	45,576	45,576	54,094	54,094

The dependent variable is a dichotomous indicator that takes the value of one if the respondent is in the labor force. The main independent variable of interest is "Treatment," which is a dichotomous indicator that equals one if the respondent was born in the cohort that was exposed to the reform (age 5-11 in 1997). Control variables include age, sex, urban residence, year fixed effects and dummies for the 26 provinces of residence. Regressions that included age-squared provided the same results. The sample includes all respondents who were ages 20 to 29 in 2011 and 20-30 in 2012. Standard errors (in parentheses) are clustered at the province-by-age level (130 clusters) to eliminate correlation between workers of the same age living in the same area * : $p < 0.10$, **: $p < 0.05$, *** : $p < 0.01$

In this regression, the variables are regressed on the treatment dummy, rather than education. This is because the labor force participation decision depends on wages and wages depend on education. So, the impact of education cannot be identified independently. As a result, I estimate reduced-form regressions of full-time work and labor force participation.

Appendix Table 4

The Impact of Middle School Completion on the Probability of Working in a Small Firm, IV Regressions (Respondents with High School or Less)

	Entire Sample	Males	Females
Middle School Completion	0.080	0.072	-0.013
	(0.065)	(0.062)	(0.107)
Age	0.006	0.003	0.007
	(0.004)	(0.004)	(0.007)
Urban	-0.175***	-0.137***	-0.259***
	(0.014)	(0.014)	(0.031)
Male	-0.013	.	.
	(0.018)		
Year Fixed Effect	0.019**	0.014	0.026
	(0.009)	(0.009)	(0.014)
Province Dummies	Y	Y	Y
First Stage	0.247***	0.245***	0.267**
	(0.019)	(0.020)	(0.037)
F Stat	168.44	150.18	51.61
N	35,451	27,896	7,555

The dependent variable is a dichotomous indicator that takes the value of one if the respondent works in a small firm, where a small firm is defined as having 10 or fewer employees. The regressions are IV regressions exposure to the reform is instrumented on completing middle school or a higher level of education. Control variables include age, sex, urban residence, year fixed effects and dummies for the 26 provinces of residence. Regressions that included age-squared provided the same results. The sample includes all respondents who were ages 20 to 29 in 2011 and 20-30 in 2012 who are working more than 30 hours a week. Standard errors (in parentheses) are clustered at the province-by-age level (130 clusters) to eliminate correlation between workers of the same age living in the same area * : $p < 0.10$, **: $p < 0.05$, *** : $p < 0.01$

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