The Effects of Labor Market Opportunities on Education: The Case of a Female Hiring Ceiling in Iran*

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Abstract

This paper estimates the effects of reduced labor market opportunities for high skilled women on their educational and marriage decisions. We exploit discontinuity generated by a 2010 policy in Iran limiting female employment in the public sector. This hiring quota, which worsened labor market conditions for college educated women, immediately reduced women's college attendance. We also find that those who did not enroll in college after the quota are less likely to be employed, but are more likely to get married young and have a child. Our finding highlights the importance of labor market opportunities for women's education as well as their work and family decisions.

Keywords: Gender Discrimination, Education Attainment, Returns to Education, Hiring Quota

JEL Classification: I00, J00, J71

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1 Introduction

Over the past half century, women's educational attainment and involvement in the paid labor market have substantially increased worldwide (Olivetti and Petrongolo (2017)). Even in the Middle Eastern and North African countries, whose gender educational gap has historically been the largest, the female-to-male ratio of average years of schooling increased from 0.63 in 1990 to 0.87 in 2010 (Source: Barro and Lee (2013)). Identifying the mechanism behind narrowing gender education inequality helps us understand how to promote women's empowerment, considered the most important development element to alleviate world poverty (The Millennium Development Goals, United Nations). Since education provides individuals with various incentives other than its returns to the labor market (e.g., marriage markets' returns and consumption value), it is important to quantify the force which drives the pursuit of education. While there is a rich body of empirical literature examining the determinants of women's education, relatively few papers focus on how labor market opportunities play a role in education decisions.

In this paper, we estimate the effects of a decline in labor market opportunities for high skilled women on their education by exploiting a sudden introduction of a female hiring quota in 2010 in Iran. The Iranian hiring quota, which set a maximum number of new female hires in the Iranian public sector, provides an ideal setup to estimate the effect of labor market opportunity on women's education. As the policy was introduced with the aim of creating employment opportunities for men against the background of rising unemployment, it immediately reduced the proportion of female new hires in the public sector from 25% to 14%. It had no significant impact on employment in the private sector. Since the public sector is the primary source of employment for skilled women in Iran, the quota created anticipation toward poor labor market opportunities for women with college education. Thus, this setup provides a rare opportunity to study the effect of a large-scale, sudden reduction in employment opportunities, especially in the labor market for high skilled women. By taking advantage of this setting, we study how a hiring cap—which is the opposite of affirmative action policies—affects education and marriage decisions, as well as employment trajectories, with a focus on educated women.

Our main analysis exploits a discontinuity in perceived labor market opportunities across birth cohorts by estimating the Regression Discontinuity Design (RDD) models. Isolating the effect of labor market opportunities is challenging because various confounding factors simultaneously affect labor market conditions and education outcomes. We address the identification issue by utilizing the age-based (or birth cohort-based) discontinuities as a valid RDD design. Specifically, we define the control cohorts as those who had just passed the typical graduation age at each schooling level when the quota was imposed. We focus on the effect on high school and college education attainment because we have a clear-cut control group with respect to the outcome of school attendance. As in most developed countries, most Iranian students follow the entry age cutoff rule set by the government and very few return to school as adults. We first show some descriptive evidence on how the quota suddenly reduced young women's employment in the public sector, but did not significantly affect older women's employment or any employment in the private sector. We also examine the long-run effect on their later lifetime outcomes such as labor market, marriage, and fertility outcomes by keeping track of the same cohort whose education decisions are affected by the quota. Since the RDD approach in our setup does not allow us to estimate the immediate effects on the labor market outcomes, we also apply the differences-in-differences (DID) method to estimate the overall effect of the quota. In implementing the DID approach, we exploit how tightly the quota binds across provinces and estimate the immediate effects on labor market outcomes and the spillover effect to the private sector.

Using repeated cross sectional data from the 2006-2015 Iranian Labor Force Survey (ILFS), we investigate the importance of the large-scaled changes in labor market opportunities as a determinant of women's education. We find that the employment ceiling was enforced as written in the official document and decreased women's employment and increased men's employment in the public sector (by -0.9 and 1.4 percentage points, respectively). As a result, women's unemployment rate increased (by 1.1 percentage points) and more women were discouraged from participation in the labor market (by -0.5 percentage point). We find no significant effect on employment outcomes for high school graduates, in the private sector, or high school enrollment rates for either gender.

We also find significant effects on women's college education choices and other lifetime decisions such as labor supply, marriage, and fertility. The employment quota reduced women's college attendance rate by 3.0-3.2 percentage points from 41.4% and increased that of men by 2.0 percentage points from 42.5%. Furthermore, the quota decreased the proportion of female students in college majors with strong ties to public employment (engineering, health, and science majors), and it increased in college majors with weaker associations to the public sector. We find after 2010, young women's (aged 18-24) marriage rates increased by 2.4-4.0 percentage points from 11.8% and fertility increased for these women in the treated cohort compared to the control cohort. These changes indicate that worsened labor market opportunities of treated women results in earlier entry into marriage and motherhood.

While our findings cannot be generalized to other developed countries, this study still provides interesting insights since the quota took place in Iran, in which enrollment rates exceed 90% at the primary and secondary levels for both genders, comparable to that of Western countries. As such, this paper highlights labor market returns as an important determinant of education pursuit in a country, in which education attainment of women is high while the society is highly conservative and employment possibilities are low. The implication helps us understand other middle/high income Middle East and North African (MENA) countries, such as Turkey and Egypt, whose labor market and culture are similar to those of Iran.

Empicial papers study a various determinants of education choices. An increasing number of papers show that education makes individuals more attractive to potential partners in the marriage market and the marriage market returns are one of important education determinants (Goldin (1992) and Lafortune (2013)). Other studies look at the benefits on knowledge on health (Wagstaff (1993); Kenkel (1991)) and others on better parenting (Cunha and Heckman (2009)). It is also well recognized that education is regarded not only a pure investment but also as a consumption good (Lazear (1977); Becker (2009)). These papers indicate that individuals attend college for various reasons other than its returns to the labor market. This paper belongs to this strand of the literature but focuses on labor market returns as a determinant of education.

The empirical literature on the effects of labor market returns on education can be roughly categorized into two groups: the studies that look at temporary changes in labor market opportunities and those which examine long-term changes. The temporary changes in labor market returns are mainly explored by the papers that study the impacts of the business cycle on education (e.g. Betts and McFarland (1995); Oyer (2006); Kahn (2010); Oreopoulos et al. (2012)). Our study looks at long-lasting changes in employment opportunities. Unlike in the case of recessions, in which people anticipate economic recovery sooner or later, women in our setup expected the changes in reduced employment opportunity to last semi-permanently. (i.e. they perceived that the quota would remain effective when they completed high school/college).[1]

Among the studies that estimate the effects of long-lasting changes in labor market opportunities on education are Black et al. (2005), Abramitzky and Lavy (2014), and Jensen (2012). Black et al. (2005) estimate the effects of a local labor demand shock induced by the coal boom and bust on men's education because the labor demand shocks have affected male-dominated industries. Our study focuses on women's education outcomes. Abramitzky and Lavy (2014) estimates the joint effects of labor market opportunities and parents' income on education by exploiting a pay reform that increased returns to education in Israel. They find a pay reform increased high school education and, in the long run, post-secondary schooling for both genders. The Iranian quota did not affect parents' income, allowing us to estimate the sole effect of labor market returns on education.

The paper most closely related to ours is Jensen (2012), which conducts a well-

[1]Not surprisingly, our paper finds that education attainment decreases when labor market opportunities are reduced while the literature on business cycles finds that education attainment increases during recession by lowering the opportunity cost of education.

designed experiment that provides recruitment services for young women in the IT services industry in rural India. The targeted group are mostly poor and largely uneducated in Indian villages and the experiment advertised job opportunities and increased perceived labor market opportunities in treatment villages. He finds that increasing perceived labor market opportunities increases womens' high school education. He also finds delayed marriage and childbearing for the women in the treated group immediately following the advertisement. The main differences between Jensen (2012) and our study are the targeted population and the direction of the policy impact. In our setting, the directly affected group is young college educated women or women who consider attending college. Given that returns to education are potentially heterogenous, it is important to estimate the local average treatment effects using different subsample. Our analysis complements the existing research, as it is useful in understanding the effect of labor market opportunities for high skilled women and as a driving force of women's pursuit of higher education. Also, our paper adds value to the literature by providing different policy implications based on the existing studies. While the findings of Jensen (2012) highlight the importance of information dissemination to promote women's high school education, this paper evaluates a hiring quota that actually changed the labor market economy-wide and shows how important the government policy could be in affecting women's education outcomes.

Finally, this paper complements the literature on employment quota and affirmative action policies by focusing on quota against women and its impact on post-secondary schooling decisions. There are relatively scarce studies on large-scale gender hiring quota. Most evidence on the effects of hiring quota is based on racial quota, as the comporehensive review is provided by Holzer and Neumark (2000). Racial quota has been explored in many influential papers. For example, Chay (1998) looks at the effect of the Equal Employment Opportunity Act on employment and earnings for black men, and McCrary (2007) studies how racial hiring quotas on municipal policy departments affected work-force composition of African American. More recently, Chin and Prakash (2011) investigate the effect of hiring quotas for the Indian minority on employment outcomes and occupational choice. Peck (2017) estimates the effect of discriminatory employment quota against racial minority with a focus on firm. She looks at the impact on firm exit rates, firm productivity, and its employment size. All of these papers study employment quota programs based on race and thus evaluate the effects of increasing/decreasing employment for minorities. While there are influential studies on the impact of gender hiring quota, they look at quotas for women in parliaments or executive posts, not large-scale quotas that change a wide range of occupation simultaneously.[2] Due to the scarce incidence of economy-wide hiring quotas for women like the

[2]Duflo (2005) provides a review on the literature on employment quota with focus on political reservation.

2010 Iranian quota and, correspondingly, the limited resulting data, this study helps us understand the policy impacts of hiring quota that directly affected young women with focus on their pursuit of education.

2 Background

The labor market and its culture in Iran are similar to those in middle/high income Middle East and North African (MENA) countries, such as Turkey and Egypt. In 2008-2009, before the quota was instituted, the Iranian female labor force participation rate was 15.4-16.2% (Source: Statistical Centre of Iran, otherwise noted). Women accounted for 18.43-18.49% of the Iranian labor force. Even among college educated women, only 50% of them were employed before the implementation of this policy (college educated men: 80%). Employment opportunities have not been great for either gender, especially after economic sanctions against Iran were imposed in 2006 (which lasted till 2015). During our study period of 2006-2015, the unemployment rates are consistently high at 16-21% for women and 9-12% for men.

Most high skilled workers are employed in the public sector; 77% of college educated women and 66% of college educated men work in the public sector, respectively. The public sector jobs attract educated workers for two reasons. First, partly due to technicality required for the tasks, the public sector is the main sector of employment for high skilled workers, especially women, in Iran.[3] In fact, most jobs in the public sector require college degrees, as a consequence of which, 82% of workers in the public sector have a post-secondary degree. Workers in the public sector are experts, office workers, and technicians and often classified as high-skilled workers. While only 6% of employees in the private sector are classified as experts, 64% of employees in the public sector were experts in 2008. This trend continues in 2014: 12% of employees in the private sector were experts, compared to 66% in the public sector. Second, compared to the private sector, the public sector offers stable, high-income jobs. The public sector rarely fires workers and thus provides more job security.[4] Moreover, the jobs in the public sector (mostly white-collar jobs) are relatively well paid, with the average starting salary doubling that of the private sector. As of 2016, the ranges for starting monthly salaries of the public sector jobs with no experience start from 15,000,000 Iranian rial (500 USD).[5] The salaries of private sector jobs start from approximately 8,000,000 Iranian rial (266

[3]While women have fewer employment opportunities in the private sector, mostly because of discrimination and an inflexible work schedule, no explicit discrimination against women can be found in the public sector.

[4]According to Iranian labor law, termination of the employment contract is allowed only under the following instances: death of employee; retirement of employee; total disability of employee; expiration of the duration of the employment contract; conclusion of work in task specific contracts; and resignation of the employee.

[5]The exchange ratio used here: 1 Iranian rial = 0.00003 US Dollar as of January, 2016.

USD). The portion of job openings provided in the public sector is not small. In 2008-2009, right before the quota was instituted, of those employed, 17.2% of male workers and 22.7% of female workers engaged in public sector jobs, respectively. (Source: World Bank; Statistical Center of Iran).

The labor market structure described above suggests two unique features to the Iranian labor market. First, given that the public sector, on average, pays more than the private sector and often requires a college degree, the hiring quota is expected to have reduced labor market opportunities for educated women and indirectly increased opportunities for educated men. Second, since that employment possibilities in the private sector for women are fairly low, the state has somewhat monopsonistic power over female labor.

2.1 Education

While there is a large gender gap in terms of labor force participation, women's educational attainment is similar to that of men in Iran. In Iran, enrollment rates exceed 90% at the primary and secondary levels, comparable to that of Western countries. The youth literacy rate has increased from 56% in 1976 to 97% in 2006 (World Bank). The school enrollment ratio of girls to boys aged 6 to 15 in 2009 was 98 (Source: World Bank), comparable to that of Western countries. The college attendance rate of women is also as high as that of men. In 2009, 37.3% of women aged 15-25 with a high school degree attended four-year universities while 34.3% of the corresponding men did so. Part of this parity in educational attainment can be explained by the educational subsidy provided by the government.

The Iranian educational system provides exogenous differences across birth cohort, allowing us to apply a sharp RDD method. We use the education cohort as the running variable and, for our method to be valid, we assume individuals cannot self-select into a control or treated cohort in order to avoid/receive treatment. This assumption is plausible in our setup. In Iran, one must be six years old on the starting date of the academic year in order to enter an elementary school. The cutoff is based on the age as of September 23rd in the given year. Since grade skipping or returning to school as an adult student is rare, most students enter high schools at age 15 and enter universities at age 18 in Iran (Source: The Iranian Ministry of Education).[6] In addition, there are age restrictions for enrollment at each grade. For example, the maximum age eligible for high school education is 20. While there is no age limitation for attending college, in the data, only 5% of college students are aged over 25. By exploiting this age cutoff rule, we use the cohorts that are a few years apart as treated and control groups. The details will be discussed in the empirical section.

[6] The website of the Ministry of Education http://www.medu.ir/fa/regulations/category/1?ocode=100010876

College majors that are highly relevant to public employment are engineering, health, and science majors. Although the private sectors also offer jobs to these majors, as explained before, the job perspectives are not as good as the ones provided by the public sector. The health sector is segregated from the other sectors due to the certificates required for this field.

2.2 The 2010 Female hiring quota

The 2010 Iranian hiring quota, referred to as the quota hereafter, is based on Article 230 of the Fifth Development Plan (2010-2014), a five-year scheme that the Iranian government implemented during the regime of President Mahmoud Ahmadinejad (2006-2013). According to Article 230, the government declares that they will prepare and approve the "Comprehensive Program for Women and Family Development," claiming that men should be the breadwinners in a family (For details, see the Appendix A).[7] According to the official record, the quota was only effective in the public sector, partly because the government does not have direct control over the private sector.

The hiring quota was announced in early March 2010 and immediately went into effect on March 21st of that year, New Year's Day in the Iranian calendar. This quota set a maximum number of new female hires, thus tightening the labor market for women in the public sector. That is, the imposition of the quota resulted in fewer job openings in the public sector for women and more openings for men. The timing of the policy announcement and implementation was not driven by a new policy regime. In fact, it was instituted during 2006-2015, in which Iran experienced little change in the long run economic/political trend. The policy was implemented one year after Ahmadinejad was elected for a second term. His regime continues to adopt the ideology that guided his policies in the first term. As of 2020, the hiring quota is still in effect. While there are changes in policies and economic conditions in Iran, no policy related to education or the labor market is implemented other than the hiring quota in 2010. The potential effects of other policies are discussed in the empirical section when we report robustness check analysis.

The main reason why the Iranian government imposed a female hiring ceiling was to create employment opportunities for men. The government at the time was especially concerned about men's unemployment rates, which were increasing at a worrying rate and reached a historical high of 10.8% in 2009.[8] The female hiring ceiling came to considered as an effective tool to suppress the surging unemployment rates among men by allocating new job openings to male workers.

[7] Even for female-headed households and women with an unqualified male breadwinner, this program suggests that women should engage in home-based jobs.

[8] In Iran, dominant political parties mainly compete for the men's vote since the women's vote is often diversified into relatively small parties that seek freedom and democracy.

Another reason for the hiring ceiling is that the Iranian government thought limiting employment opportunities for women would increase fertility rates. The birth rate in Iran decreased from 6.5 in 1975–1980 to 1.9 in 2005–2010 (Source: The UN's Population Division of the Department of Economic and Social Affairs). A further drop in fertility rates, resulting in an aging population, would be costly from the perspective of public finances. Thus, the government called for a reversal of Iran's population control policy. The hiring quota for female workers is considered a part of the government's attempts to increase the birthrate by reducing alternative options for women (i.e. employment).

The quota is binding for 29 out of 30 provinces in that the post-treatment share of women is smaller than pre-treatment share of women (See Table B.1 in the Appendix, which reports how the quota affected each province). While most provinces were affected by the quota, some were more affected than others. The variations in the quota intensity arise from differences in the concentration of gender-segregated workplaces across geographic location. The quota is set at around 10% in most occupations in the public sector, except where workplaces are segregated by gender, such as in hospitals and schools. According to the official documents, regardless of province, the government applied the same quota policy to each occupation group in the public sector. Exempted from the quota policy are gender-segregated workplaces. Since these gender-segregated workplaces need female public officers to serve for women, they continued to allocate a certain proportion of job openings to women even after the quota was imposed. The workplaces that commonly practice gender segregation are hospitals and schools. Since the scale of treatment is large, we will study spillover effects to the private sector as well as effects on men's employment and education outcomes. As such, this is a unique setting, in which the policy substantially restricted labor market opportunities for highly educated women.

Given relatively low employment possibilities for high skilled women in the private sector, the quota affected a non-trivial portion of the gender composition in job openings and reduced potential employment possibilities for young skilled women.[9] As such, we can expect the ceiling had a large impact on highly educated women by altering their labor market returns to education.

2.3 Iranian Culture

Iran is a country that strictly imposes Sharia, which is based on religious beliefs of Islam and considers men as the breadwinners in the family. However, Iranian culture is relatively favorable to women's working outside the home as compared with other Islamic countries (e.g. Saudi Arabia). In particular, in 1967, the Family Protection

[9]The public sector, on average, pays more than the private sector, provides more job security, and often requires a college degree (82% of workers in the public sector have a post-secondary degree).

Act was established to guarantee women's family rights.[10] Even after 1979 Revolution when the act was annulled, several demonstrations promoted women's rights,[11] and some parts of the act were reintroduced. Today, most women can freely choose their lifestyle by themselves in Iran, not forced by their parents or by husbands.

The Iranian Civil Code forbids the marriage of women younger than 15 years old and men younger than 18 years old. A social safety net involving unemployment insurance is not well provided in Iran. As such, marriage provides the best safety net to women who lose their jobs or cannot find a new job and are more likely to remain unemployed for a long time than men.

Given a relatively high college education attainment rate for both genders, people are relatively well aware of the market return on education; that is, most Iranian do not suffer from the information problem, which could be serious in other parts of the world where no/few adults have education and people have little information about the potential returns to education.

3 Data and Descriptive Evidence

3.1 Data

The main data used in the analysis is from the 2006-2015 Iranian Labor Force Survey (ILFS), a rich and large data set provided by the Statistical Centre of Iran. The data are repeated cross sections that have been collected on the same reference population under rotating panel design. The ILFS collects the data on 140,000-170,000 individuals quarterly using random sampling; it is designed to be representative of the population of Iran.[12] The total number of observations used for the main RDD analysis is 523,220 year-individual observations and 941,811 for the DID analysis. The ILFS data offer detailed information about the respondents' demographic characteristics, birth year and month, employment status, residential area, recent migration, and other important characteristics. We observe whether an individual is enrolled in school and the highest

[10]Based on this law, the minimum age of marriage increased to 15 for women and 18 for men. While in Islam, the right to divorce belongs exclusively to the husband, this law gives the divorce right to both men and women. Also, in contrast to Islamic law regarding child custody, this law determined that a court should decide whether it would be more beneficial for the child to live with the father or the mother. According to Islam, a man may marry four wives. As a result of this law, a man may marry a second wife only with permission of the courts and consent of his first wife.

[11]The symbolic event is Shirin Ebadi winning the Nobel Peace Prize for promoting women's human rights.

[12]First, the government survey agency divided the whole country into a block containing at least 250 households based on the Census. Second, they selected 85 households in each block, which is called a cluster. Then they divided each cluster into rotation groups for interviewing. That is, a part of the sample is partially replaced while the remainder is retained (i.e., partial replacement). Each rotation group consists of three households in the same neighborhood. We checked the data validity by comparing the data features of the ILFS to those from the census and other aggregate data provided by the Iranian government.

degree of education she/he has ever attained. For those who attend college during the survey period, we observe their college major. The response rates in all rounds are at 81-89%. We also use Iranian Households Income and Expenditure data (2006-2015; IHIE) for analyzing the effects on wages because ILFS data do not contain the information on wages. We have 245,927 year-individual observations in the IHIE data used for the analysis on wage. We do not use the IHIE data for the rest of the analysis because of its small sample size; the IHIS contains fewer than half of the observations in ILFS data.

In addition to the RDD method, we also apply the DID method by exploring the differences in the intensity of the quota across provinces. Indeed, the RDD provides more clear identification than the DID method, which is subject to measurement error in the treatment intensity. However, the DID method allows us to evaluate the overall effects and possible spill over effects of the quota, and thus complements the analysis well. To measure the intensity of the quota, we use the data provided by the Iranian government's Department for Women and Family Affairs. We extract the data from a list of PDF files that specifies the number of job openings in each occupation and province in the public sector in 2015. The data provide the information on how many vacant seats are open to men only (80.1% of the total seats), both genders (6.0%), and women only (13.9%). We combine the openings to both genders and women and use it as the proportion of female new hires. Since some of the job vacancies open to both genders will be taken by men, the actual proportion of the female new hires is smaller than the estimates. The variance in the proportion of the female new hires is considerate.

3.2 Sample

The main sample consists of men and women collected from the 2006-2015 ILFS data. For the RDD analysis, we use individuals' month of birth as the running variable and our sample comprises those who were born just before and after the cutoff points. For the main RDD analysis, we use individuals who are born between 1991 and 1995 for high school attendance and those born between 1986-1990 for college attendance. For the DID, we use those who were born between 1986 and 1995. Note that when we conduct the RDD analysis, we cannot compare the outcomes for the older age cohorts because the treated defined in the RDD analysis are still young in the currently available data set; the oldest is 29 years old in the most recent year in the data.

Table 1 presents summary statistics. For the main RDD analysis, we restrict our sample to 24 months just before and after the cutoff months. The cutoff month is September 1993 for high school enrollment, and September 1988 for other outcome variables. While the cohorts are a few years apart, we see differences in their later life outcomes. As this table shows, college attendance is lower for female high school

Table 1: Summary Statistics

Individual-level variables	Aug. 1988 and older	Sep. 1988 and younger
Number of observations (female)	156,648	135,090
Number of observations (male)	124,714	106,768
Age (female)	23.57	22.80
Age (male)	23.55	23.06
% Married women (Age 25)	51.33	58.52
% Women have given birth (Age 25)	46.17	54.08
% Married men (Age 25)	30.41	28.16
% Men whose wife has had a child (Age 25)	13.65	17.56
% Women who work for pay (Age 25)	14.85	10.89
% In university: girls (HSG age:18-22)	42.80	40.63
% In university: boys (HSG age:18-22)	46.87	52.32
	Aug. 1993 and older	Sep. 1993 and younger
% In school: girls (age:15-18)	84.34	84.03
% In school: boys (age:15-18)	81.19	84.83

Notes: The sample for this analysis is all individuals in the working-age who were born just before and after the cutoff points (1991-1995 for high school enrollment, and 1986-1990 for other outcome variables including college attendance, marriage, and fertility) ($15 \leq age \leq 29$).

graduates who were born after September 1988 than before. College attendance is higher for men who were born after September 1988. Moreover, marriage and fertility rates notably increased among the young female cohort. In contrast, the difference in high school attendance rates before and after the 1993 cutoff are small. Since the around-1993 cohort is young, we do not keep track of their later outcomes and thus report the characteristics only for the around-1988 cohorts. These pieces of data features suggest that the quota may have affected young men and women's education as well as work and family decisions. The causal effect of limiting employment opportunities will be empirically tested in the empirical section.

3.3 Graphical Evidence

Before conducting the formal analysis, we provide informal evidence on how the quota affected labor market opportunities. Figure 1 plots the proportion of female workers who are employed in the public sector in each calendar year. The left graph is for young women aged 19-24. We see a sudden drop in the employment rate in the year of the quota implementation. The right graphs show the fraction of women aged 25 to 28 employed in the public sector during the same time window with no dips. These graphs highlight that the quota in fact affected the employment rates in the public sector for the new hires, but not those who were already employed. Given that the public sector rarely fires workers, the quota did not directly affect those who were already employed in the public sector. As such, the ceiling is expected to have had a large effect on employment for those looking for public sector employment opportunities.

Figure 1: The 2010 Quota and The Rates of Female New Hires by Sector

Notes: The left graph depicts the proportion (%) of women aged 19-24 who are employed in the public sector (=No. of women who observed that they are employed in the public sector/ No. of women in the corresponding age group in a given year); the right presents the same statistics for women aged 25-28. The abrupt reduction occurs only for the young workers, reflecting that the 2010 quota mainly affected new hires, not the existing employees in the public sector.
Source: Calculation by the authors using Iranian Labor Force Survey (ILFS).

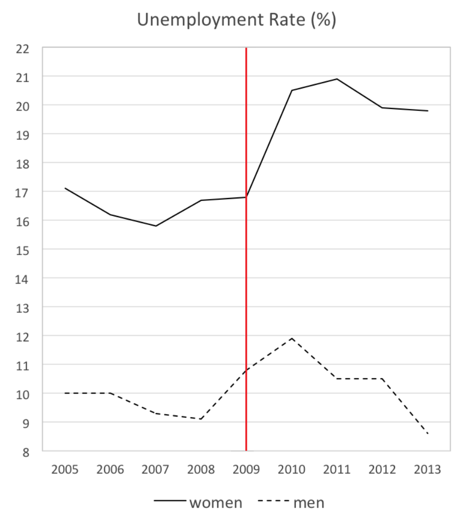
In fact, the change in total female public employment was small; after the quota was instituted, the proportion of female employment in the public sector slightly decreased from 18.1% to 16.9% (Source: Statistical Center of Iran). In contrast, the largest effects of the quota are found on the number of new female hires and on unemployment rates.

Figure 2 shows the female unemployment rates in our data sample sharply increased from 17% to 21% immediately after the quota was implemented. In contrast, there was no contemporaneous upward spike for men. While the unemployment rate did slightly increase in 2010, that increase is considered part of a trend because the male unemployment rate had already started increasing in 2008, and the increment in 2010 was smaller than in 2009. What is more, the unemployment rate for men has declined since 2010, but for women it has plateaued and remained high. These pieces of evidence suggest that the quota was effectively instituted and immediately reduced young women's labor market opportunities for new hires.

4 Empirical Strategy

The purpose of our empirical analysis is to identify the causal effects of limiting labor market opportunities on women's education decisions. Isolating the effect of labor market opportunity is challenging because various confounding factors simultaneously affect labor market conditions and education outcomes. We identify the causal effect by

Figure 2: The 2010 Quota and Unemployment Rates by Gender



Notes: Figure shows that the female overall unemployment rates sharply increased from 17% to 21% immediately after the quota was instituted in 2010. There was no obvious contemporaneous upward spike for men around that time.
Source: Calculation by the authors using Iranian Labor Force Survey (ILFS).

utilizing age-at-school-entry policies in addition to province-level variation in treatment. We first apply the regression discontinuity design (RDD) analysis by exploiting the fact that the year a typical person starts/finishes school is a discontinuous function of the birth month. Using the RDD, we examine the effects on education enrollment as well as the long-run effects on their later lifetime outcomes (e.g. their employment, wage, marriage, and fertility) of the same sample cohorts. We also apply the differences-in-differences (DID) estimation by exploiting how tightly the quota binds across province. The DID analysis helps us to evaluate the direct, immediate effects of the quota on outcomes other than education.

4.1 Regression Discontinuity Design

Our main strategy is a regression discontinuity design (RDD) that exploits a sharp cut-off across birth cohorts that divides the treated group and control group with respect to school attendance. An age-based discontinuity in our setup is considered exogenous for school enrollment, but not for other outcomes such as labor market decisions. Utilizing age-based (or birth cohort-based) discontinuities is a widely adopted application of a valid RDD design in other contexts, as discussed in Lee and Lemieux (2010).[13] Since individuals have flexibility with regard to when they will enter the labor market (e.g. some individuals may avoid the treatment by delaying graduation or attending graduate school), we cannot have a clear age cutoff that defines the control and treated group for outcomes other than education decisions. As such, we keep track of the same cohort whose education decisions are affected by the quota and examine the long-run consequences of the treatment on their later lifetime outcomes. The purpose of this analysis is to examine what happened to those who purposely pursued less education as a result of facing lower labor opportunities due to the quota.

To be specific, in looking at the effect of the quota on attending high school, we use the birth cohort of September 1993 as the cutoff and consider those who were born between September 1991 and August 1993 as the control group and those born between September 1993 and August 1995 as the treated group. Concerning its effect on college attendance, we use September 1988 as the cutoff and consider those born between September 1986 and August 1988 as the control group and those born between September 1988 and August 1990 as the treated group. The sample size for high school enrollment is 176,431 and for college attendance 119,332.

We construct regression-adjusted differences by including smooth functions of age as well as controls for province and other factors. The general form of our estimated regression is as follows:

$$Y_{it} = \gamma_1(\text{Treated}) + g(\text{Birth Cohort}) + \mathbf{Z}'_{it}\mathbf{B} + \varepsilon_{it} \tag{1}$$

where Y_{it} is the education outcome for an individual i at a given time t (calendar year). The first term is the treatment cohort dummy (Treated birth cohort) on its own. The second term, g(Birth Cohort), is a smooth function of birth cohort. The vector $\mathbf{Z}_{it}$ contains a constant term and a set of individual specific characteristics that affect education outcomes (including parents' education, province dummies, rural-urban dummies, and time dummies). The coefficient γ_1 of the birth cohort dummies captures the impact of the quota. This method enables us to identify the effect of the quota on education by exploring the discontinuities between the treated and control cohorts.

Specifying the smooth function of birth cohort demands caution since the estimated treatment effect may be sensitive to how the smooth function is estimated. In the main

[13] Some exploit age at school entry variation to study the effects of education on various outcomes. For example, Cascio and Lewis (2006) look at test scores as outcomes of interest; Oreopoulos (2006) and Dobkin and Ferreira (2010) look at earnings. Others use age-triggered treatments of social insurance eligibility to estimate the effects of social security program reforms on healthcare utilization and health-related outcomes. Card and Shore-Sheppard (2004) use age-triggered treatments of social insurance eligibility to analyze the effects of a Medicaid expansion. More recent studies estimate the effect of the insurance coverage (Card et al. (2008; 2009); Anderson et al. (2012)) and the effect of patient cost-sharing (Shigeoka (2014); Fukushima et al. (2016)) on healthcare utilization.

analysis, we present estimates of the linear spline model (separate regressions on both sides of the discontinuity) with a small window around the birth cutoff year. To be specific, we use the following regression as the baseline model:

$$Y_{it} = \gamma_1(\text{Treated}) + \delta_1(\text{Cohort - Cutoff}) + \delta_2(\text{Treated}) \times (\text{Cohort - Cutoff}) + \mathbf{Z}'_{it}\mathbf{B} + \varepsilon_{it} \tag{2}$$

In the Appendix, we re-conduct the analysis using different specifications for the smooth function (including standard linear, quadratic, and cubic functions, as well as quadratic splines) and check robustness. Further, we also show estimates of the linear spline model for various sizes of bandwidth to check the sensitivity of our results.

Since the policy variation occurs at the education cohort level, we cluster the standard errors by birth quarter and province based on the entry age cutoff rule. That is, with September cutoff adopted in Iran, we group the individuals who were born between September and August in the following year into the same cohort because they enter a school program together. This approach is equivalent to the method that collapses the individual-level data into education cohort group (Lee and Card (2008)).

The assumption that $g(\cdot)$ is a continuous function means that differences in labor market opportunities are the only source of discontinuity in outcomes around the cutoff cohort. Potential confounding factors could include other policies that affect the control and treated cohorts differently. However, this concern is mitigated by the fact that no other education or labor policies were implemented in 2010 and that no earlier policies or events affected the cohorts around the cutoff differently. Furthermore, as we show in a later section, we examine if other confounding factors drive the estimates by using pre-reform data. Specifically, we investigate whether there was any discontinuity in employment and education outcomes using the corresponding age group of older cohorts in the sample years before the quota was introduced. Using the pre-reformed data, we find no significant changes across the birth cohorts. The details will be discussed in subsection 5.3 and presented in detail in the Appendix.

4.2 Differences-in-Differences

In addition to the education outcomes and long-run effects, we are interested in how the quota affected the economy overall. To determine this, we conduct the DID analysis. We study not only spillover effects to the private sector and men's employment outcomes but also women's employment, marriage, and fertility.

Denote outcome variables as Y_{ijt} for an individual i in province j at a given time t. For each outcome, we conduct regression analysis using the following baseline specification:

$$Y_{ijt} = \gamma Post_t \times \bar{D}_j + \alpha\bar{D}_j + \lambda_t + \varphi_{rt} + \mathbf{X}'_{ijt}\mathbf{B} + \varepsilon_{ijt} \tag{3}$$

where $Post_t$ is the dummy which is equal to 1 after 2010 and 0 otherwise. The first term is the treatment intensity measure in province j ($\bar{D}_j$) with the interaction term of the post-treatment period dummy ($Post_t$). The second term is the intensity variable on its own. The third term, λ_t, is a vector of time effects to control for changes in macroeconomic conditions of the overall Iranian economy. We allow the time effects to differ between urban-rural regions by adding region-year fixed effects, φ_{rt}. Note that region-year effects can capture changes over time that affect countries within a region similarly and thus control for changes in demand and/or supply of the regional labor market. The vector $\mathbf{X}_{ijt}$ contains a constant term and a set of individual specific characteristics that affect outcome variables, including age, regional dummies, and family background (parents' education). The last term, ε_{ijt}, is the error term. Under some assumptions, the estimated coefficient γ captures how the quota affected the outcome of our interest. In running the above regression analysis, we look at the difference in treatment intensity across all provinces before and after the policy change. Instead of having a clear-cut control group, we examine how people behave differently among the differently affected provinces.

We measured the treatment intensity of the 2010 quota by the pretreatment share minus the target hiring rate of women in the public sector. Specifically, the intensity for province j is measured in the percentage difference between the pretreatment employment share in 2009 and the target hiring rate of women in 2015 in the public sector:

$$\bar{D}_j = \frac{\text{(Share of women in the public sector in 2009)} - \text{(Share of women in new hire in 2015)}}{\text{Share of women in the public sector in 2009}}$$

Ideally, we wish to measure the intensity using the data in 2009 and 2010. However, detailed information of the quota was not available until 2015. We use the data from 2015 to infer the quota in 2010. Thus, in applying this proxy, we need to assume that the quota system did not change (the proportion of female job openings is the same in each province) between 2010 and 2015. In the Appendix, Table B.2 reports the summary statistics of the intensity measurements. To check robustness, we use two other measurements of treatment intensity and report the results in the Appendix, Table D.7.[14]

Unlike the RDD analysis, this approach requires stronger assumptions that the varia-

[14]We provide support for this assumption in two ways. In the official document, it is written that the quota policies did not change once they were implemented.The new regime, which disclosed these data sources, publicly admits that the current hiring situation is discriminatory, and they are considering reverting to the policies prior to 2010 (Source: Interview with vice president in 2015). We contacted the government agency in an attempt to collect more data. The officers told us they cannot disclose the information on the quota prior to 2015, but confirmed that there was no change in the quota policies or the resulting female proportion of new hires.

tion in the treatment intensity is exogenous. We check the possible endogeneity problems and find no evidence of systematic relationship between the treatment and confounding factors. The analysis is summarized in subsection 5.3 and in the Appendix.

5 Estimation Results

We analyze the impact of this hiring quota using the RDD method and then report the effects using the DID method. After that, we summarize robustness check analysis and address endogeneity concerns. We discuss the findings at the end of this section.

5.1 Regression Discontinuity Design Estimates

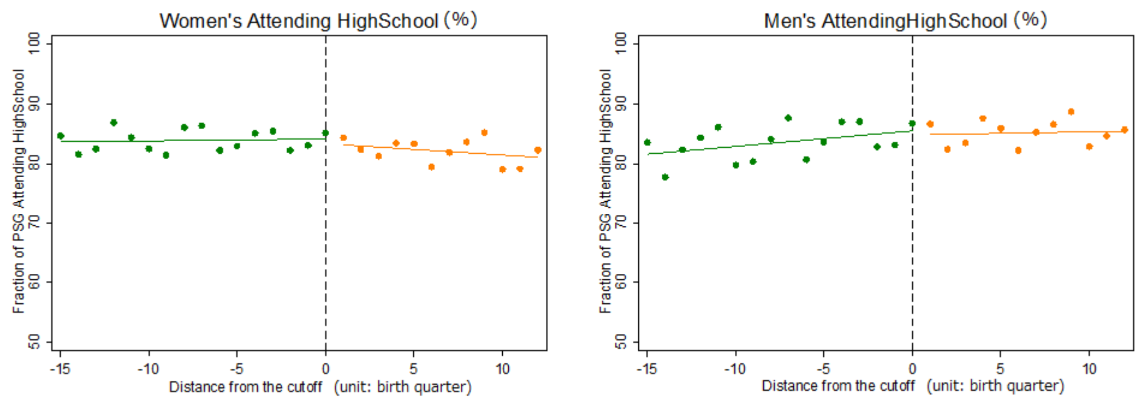
We first look at the effects on education using RDD. Using the same cutoff, we also apply the RDD to estimate the effects on outcomes other than education and interpret the results as the long-term effects of the quota (through the channel of education effects). In presenting the analysis on education outcomes, we focus on high school attendance and college attendance. Since education up to the 9th grade is compulsory in Iran, we expect that the quota to affect enrollment in high school and/or higher education, if there is any effect. We first examine the effects on high school attendance rates.

Figure 3 and the first two columns of Table 2 present the effects on the high school attendance rate for ages 15-18 by gender. We find that high school attendance rates remain similar before and after the quota for both genders. We conclude that reduced labor market opportunities in the public sector did not significantly affect individuals' decisions regarding high school attendance. If the quota induces more dropouts before entering college and reduces the proportion of high school graduates, it is hard to separate the effects on college attendance from those on high school or earlier education for some of the treated cohorts. However, we find no such effects.

Figure 4 shows the attendance rate of a university program for age 19-20 by gender.[15] The denominator is the number of high school graduates in each birth quarter and the numerator is the number of individuals who ever attended college. Since we find no changes on high school attendance, we hereafter suppose that there is little composition change in the population of individuals who are eligible to attend college. Unlike the effects on high school attendance, we see a sharp decline in women's college attendance rate at the cutoff. Note that the college attendance rate trends downward for the treated cohort while trending upward for the control cohort. This trend change shows that younger cohorts reacted more strongly to the employment ceiling, which indicates that younger cohorts have more time to adjust to a reduction of labor market opportunities.

[15]The university program defined here includes 2-yr and 4-yr programs

Figure 3: Effects of the Employment Quota on High School Attendance Rate by Gender



Source: Calculation by the authors using Iranian Labor Force Survey (ILFS).

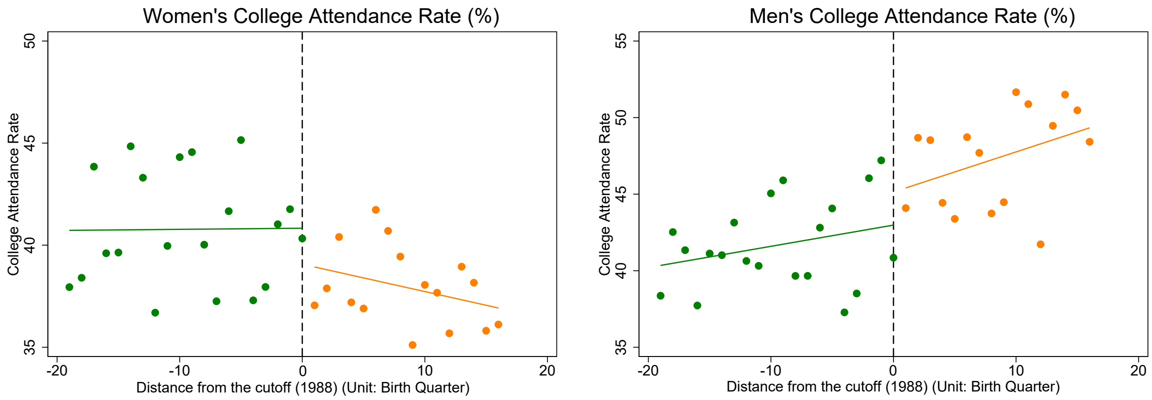
Table 2: RDD Estimates (Education)

	Sample: 1986-1995				Placebo Tests—Sample: 1977-1985			
	Attend highschool (cutoff=Sep 1993)		Attend college (cutoff=Sep 1988)		Attend highschool (cutoff=Sep 1986)		Attended college (cutoff=Sep 1981)	
Panel A: Women								
	(1) Linear	(2) Spline	(3) Linear	(4) Spline	(5) Linear	(6) Spline	(7) Linear	(8) Spline
Treated	-0.009	-0.009	-0.030***	-0.032***	0.001	0.008	0.024	0.026
	(0.017)	(0.018)	(0.004)	(0.004)	(0.005)	(0.004)	(0.027)	(0.042)
Obs.	82,815	82,815	65,202	65,202	155,511	155,511	98,001	98,001
R-squared	0.025	0.025	0.181	0.181	0.028	0.028	0.068	0.068
Mean control	0.843	0.843	0.414	0.414	0.783	0.783	0.380	0.380
Panel B: Men								
Treated	-0.005	-0.005	0.020***	0.022***	0.002	0.006	0.039	0.003
	(0.021)	(0.022)	(0.005)	(0.005)	(0.009)	(0.011)	(0.026)	(0.046)
Obs.	93,616	93,616	54,130	54,130	178,699	178,699	87,763	87,763
R-squared	0.026	0.026	0.042	0.042	0.063	0.063	0.048	0.048
Mean control	0.812	0.812	0.425	0.425	0.666	0.666	0.336	0.336

Notes: Heteroskedasticity-consistent standard errors accounting for clustering at the birth quarter-province level in parentheses. The dependent variable is an indicator for whether an individual enrolls in a specified education level. The sample is men and women aged 15 to 18 who are eligible to enroll in high school and aged 19 and 20 who complete high school and are eligible to enroll at any university programs. Control variables are individual and family characteristics including age and parents' schooling. *Significant at 10% level; **significant at 5% level; *** significant at 1% level. This table presents estimated coefficients from a linear probability model. The time period is 2006-2015 for the base specification.

Table 2 reports the main estimation results of an RDD analysis. We report the results when allowing trend changes in Columns (2) and (4). As expected from Figure 4, the college attendance rate of women decreases by 3-3.2 percentage points, meaning a 7.25-7.73% decline. We find men's college attendance rates significantly increase, but

Figure 4: Effects of the Employment Quota on College Attendance Rate (at Age 19-20) by Gender



Source: Calculation by the authors using Iranian Labor Force Survey (ILFS).

to a lesser extent than women's rates decrease. The men's attendance rate increases by 2.0-2.2 percentage points, indicating a 4.71-5.18% increase. The results indicate that the quota had an economically significant impact on college attendance for both genders.

To examine whether the estimates in fact capture the effect of the quota on college attendance, we conduct placebo tests by using the pre-reform data (1977-1985) and present the results in Columns (4)-(8) in Table 2. The estimated effects are found insignificant, indicating that the estimated effects are driven by the quota, not by confounding factors.

Now, we look at the outcomes other than education using the RDD and investigate the indirect effects of the quota through the channel of education effects. We apply the same cutoff to define the treated and control group that are directly affected by the quota with respect to their education decisions. We investigate the consequences of the treatment on later lifetime outcomes such as employment, wage, marriage, and fertility.

Table 3 presents the impact on labor market outcomes. Note that we still use September 1988 as the cutoff that determines the treated and the control groups (regarding whether the quota affected college attendance decisions) and look at employment outcomes at age 23-29. The first six columns present the effects on employment status. The results show that the treated female cohort has a significantly higher unemployment rate and lower employment rate. The last two columns present the effects on wages for each gender-education group. We find no significant effects. This finding is not surprising given that women accounted for a relatively small fraction of the Iranian labor force. In the bottom part of the table, we look at employment outcomes in the public and private sector separately in order to examine spillover effects across sectors. The results indicate that the quota affected employment rates of women and men in the

Table 3: RDD Estimates (Employment and Wage)

	Labor Force Status						Log Wage	
Sample:	1986-90 (cutoff:Sep 1988)			1979-83 (cutoff:Sep 1981)			1986-90 (cutoff:Sep 1988)	
Panel A: Women							Edu≤12	Edu ≥13
Dep. Var.	LFP	Empl.	Unempl.	LFP	Empl.	Unempl.	Log(wage)	Log(wage)
Treated	-0.005***	-0.003**	0.011**	0.002	-0.004	0.023	-0.060	0.163
	(0.002)	(0.001)	(0.005)	(0.009)	(0.005)	(0.020)	(0.052)	(0.102)
Obs.	160,492	160,492	31,070	306,926	306,926	69,766	2,637	2,021
R-squared	0.039	0.013	0.146	0.041	0.010	0.117	0.129	0.037
Mean	0.215	0.155	0.279	0.221	0.182	0.176	9.113	9.878
Panel B: Men								
Dep. Var.	LFP	Empl.	Unempl.	LFP	Empl.	Unempl.	Log(wage)	Log(wage)
Treated	0.015***	0.013***	-0.004*	-0.001	-0.005	0.004	0.031	0.132*
	(0.002)	(0.002)	(0.002)	(0.003)	(0.006)	(0.006)	(0.019)	(0.061)
Obs.	162,875	162,875	126,907	286,543	286,543	257,191	37,553	4,842
R-squared	0.209	0.171	0.030	0.108	0.099	0.026	0.097	0.083
Mean	0.875	0.755	0.136	0.927	0.839	0.094	9.726	9.937

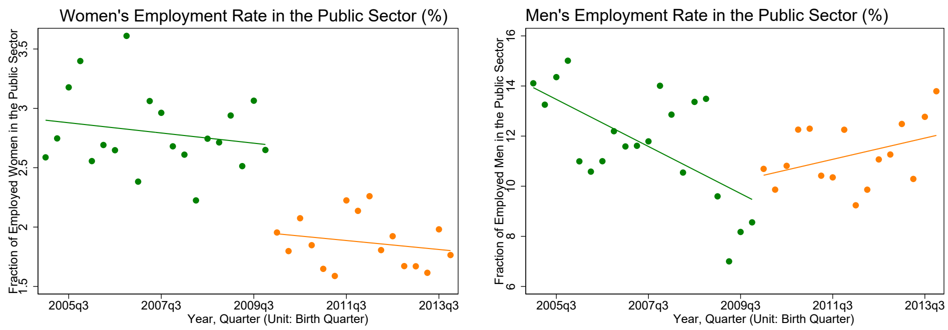
Dep. Var.	Employment			
	Public Sector		Private Sector	
Sample:	1986-1990	1979-1983	1986-1990	1979-1983
A: Women				
Treated	-0.004***	-0.001	0.002	-0.001
	(0.000)	(0.002)	(0.001)	(0.002)
Obs.	160,492	306,926	160,492	306,926
R-squared	0.018	0.056	0.006	0.008
Mean	0.034	0.05	0.041	0.039
B: Men				
Treated	0.005***	-0.008	0.001	-0.000
	(0.001)	(0.009)	(0.002)	(0.009)
Obs.	162,875	286,543	162,875	286,543
R-squared	0.038	0.058	0.071	0.042
Mean	0.093	0.123	0.352	0.363

Notes: Heteroskedasticity-consistent standard errors accounting for clustering at the birth quarter-province level in parentheses. The dependent variable is an indicator for job market statues. The sample for this analysis is women aged 23-29 because in fact, most individuals at these ages have completed. Control variables are individual characteristics including age and schooling. *Significant at 10% level; **significant at 5% level; *** significant at 1% level. This table presents estimated coefficients from a linear probability model. The time period is 2006-2015 for the base specification.

public sector, but there were no spillover effects observed in the private sector. Figures 5 and 6 show the corresponding graphs.

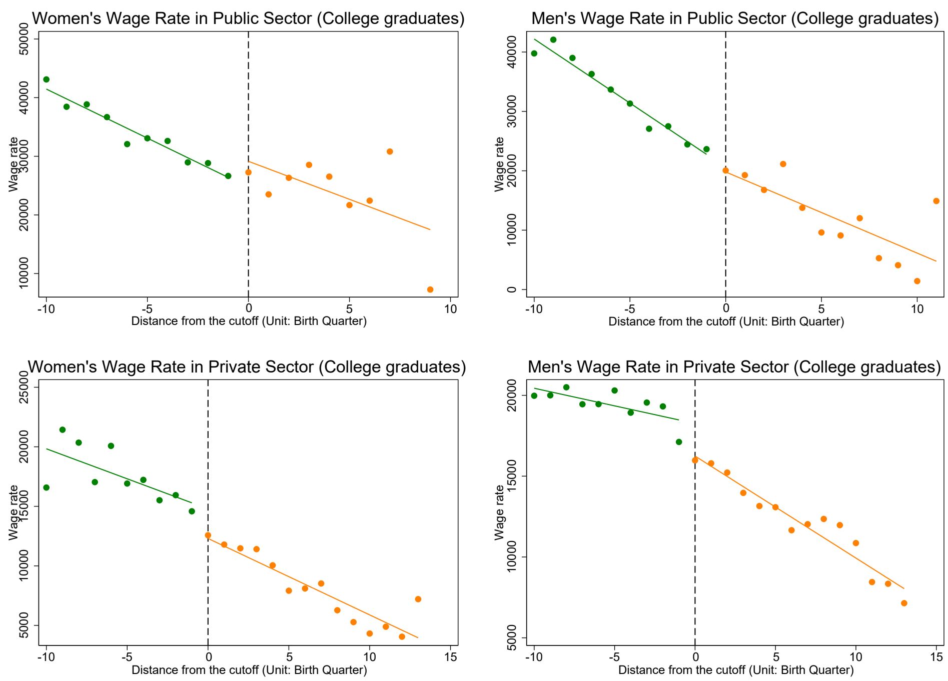
Figure 7 shows the impact on marriage for women and Table 4 presents the corresponding regression results. The first two columns look at the marriage rates for 18-24

Figure 5: Effects of the Quota on Employment in the Public Sector by Gender



Source: Calculation by the authors using Iranian Labor Force Survey (ILFS).

Figure 6: Effects of the Quota on Wage by Gender (College Graduates)

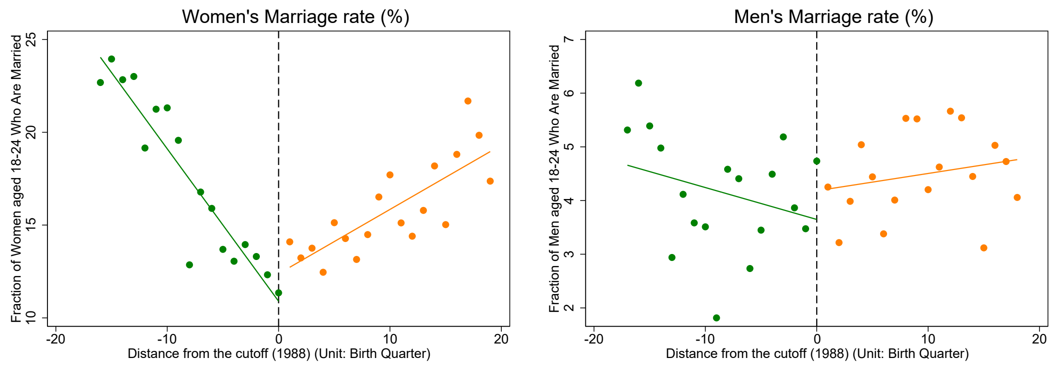


Source: Calculation by the authors using Iranian Households Income and Expenditure Survey (IHIE).

year-old women and the last two columns present the ones for 25-27 year-old women. Although not large, we find that the quota significantly increased the marriage rates of women aged 18 to 24 while it had no significant effect for women aged 25-27 or for men. The impact on young women's marriage rate is substantial and increased by 8.14-13.56% (=2.4/29.5*100; 4.0/29.5*100).

Table 5 presents the impact on fertility. We use the number of children as the dependent variable. As in the results for marriage, the first two columns look at younger women aged 18 to 24 and the last two columns look at women aged 25-27. Again, we find that the quota significantly increased fertility for the younger women and no significant

Figure 7: Effects of the 2010 Quota on Marriage Rates (Age 18-24) by Gender



Source: Calculation by the authors using Iranian Labor Force Survey (ILFS).

Table 4: RDD Estimates (Marriage)

	Marriage			
	age: 18-24		age: 25-27	
Panel A: Women				
	Linear	Spline	Linear	Spline
Treated	0.024***	0.040***	-0.010	0.003
	(0.004)	(0.004)	(0.007)	(0.007)
Obs.	189,902	189,902	58,854	58,854
R-squared	0.027	0.033	0.101	0.103
Mean control	0.295	0.295	0.572	0.571
Panel A: Men				
Treated	0.004	0.003	-0.024	-0.019
	(0.009)	(0.009)	(0.023)	(0.020)
Obs.	170,990	170,990	56,858	56,858
R-squared	0.055	0.055	0.044	0.044
Mean control	0.118	0.118	0.359	0.359

Notes: Heteroskedasticity-consistent standard errors accounting for clustering at the birth quarter-province level in parentheses. The dependent variable is an indicator for whether an individual is married or not. Control variables are individual characteristics including schooling. *Significant at 10% level; **significant at 5% level; *** significant at 1% level. This table presents estimated coefficients from a linear probability model. The time period is 2006-2015 for the base specification.

effects for older women. The magnitude of an increase is only 0.001 (0.33%) and is not economically significant. Yet, we need to be reminded that, regarding fertility outcomes, we were not able to assess the long-term impact because a relatively short data sample period (in which the oldest treated cohort is 27 years old) prevents us from looking at longer-term effects of the quota.

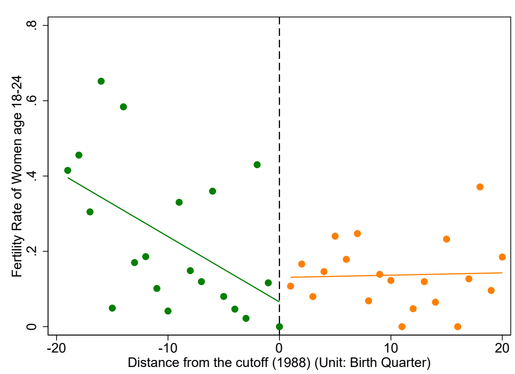
The estimates capture the indirect effect on marriage or fertility rates since the quota reduces the gain from education and labor market participation, which in turn reduces the opportunity cost of the alternative (often exclusive) options such as household work and childbearing/childrearing. Figure 8 shows the corresponding graphs for age 18-24. Given that there is no significant change for older women, the results indicate that the quota affected the timing of marriage and fertility; i.e. women get married and have children earlier.

Table 5: RDD Estimates (Women's Fertility)

	Number of Children			
	age: 18-24		age: 25-27	
	Linear	Spline	Linear	Spline
Treated	0.001***	0.001***	0.000	0.001
	(0.000)	(0.000)	(0.002)	(0.002)
Obs.	189,902	189,902	58,854	58,854
Mean control	0.299	0.299	0.823	0.823

Notes: Heteroskedasticity-consistent standard errors accounting for clustering at the birth quarter-province level in parentheses. The dependent variable is number of children. Control variables are individual characteristics including marital status and schooling. This table presents estimated coefficients from a linear probability model. The time period is 2006-2015 for the base specification.

Figure 8: Effects of the Employment Quota on Fertility (Age 18-24)



Source: Calculation by the authors using Iranian Labor Force Survey (ILFS).

In sum, we find that reduced employment opportunities greatly decreased women's college attendance rate, slightly increased men's college attendance rate, and slightly increased women's marriage rate. These analyses imply that some of women who did not enroll in school after the quota are less likely to be employed, but are more likely to get married young and have a child.

5.2 Differences-in-Differences Results

We apply the DID analysis to compare the immediate effects of the quota on employment as well as education. While the RDD analysis looks at the changes in employment outcomes for those whose education choices are distorted by the quota, the DID analysis estimates the effects of the quota by comparing more affected regions to less affected regions. We also look at spillover effects to the private sector.

Table 6: DID Results (Employment by Gender, Education, and Sector)

Panel A: Women			Total			
Education		≤12 yrs			13 yrs +	
Dep. Var.	LFP	Empl.	Unemp.	LFP	Empl.	Unemp.
Post × Intensity	0.029	0.023	0.006	-0.015***	-0.016***	0.001**
γ	(0.035)	(0.021)	(0.020)	(0.002)	(0.002)	(0.001)
Obs.	1,837,832	1,837,832	1,837,832	268,850	268,850	268,850
R-squared	0.012	0.012	0.011	0.050	0.056	0.004
Mean control	0.159	0.141	0.114	0.424	0.300	0.292
Panel B: Men			Total			
Education		≤12 yrs			13 yrs +	
Dep. Var.	LFP	Empl.	Unemp.	LFP	Empl.	Unemp.
Post × Intensity	-0.018*	-0.011	-0.007	0.005	0.016**	-0.011***
γ	(0.010)	(0.016)	(0.013)	(0.007)	(0.007)	(0.004)
Obs.	1,695,182	1,695,182	1,695,182	319,647	319,647	319,647
R-squared	0.015	0.017	0.005	0.012	0.014	0.008
Mean control	0.734	0.659	0.101	0.646	0.574	0.111

Panel A: Women	Public Sector		Private Sector	
Education	≤12 yrs	13 yrs +	≤12 yrs	13 yrs +
Dep. Var.	Empl.	Empl.	Empl.	Empl.
Post × Intensity	-0.004	-0.009**	0.007	0.027
γ	(0.014)	(0.004)	(0.028)	(0.028)
Obs.	1,837,832	268,850	1,837,832	268,850
R-squared	0.055	0.070	0.051	0.112
Mean control	0.065	0.755	0.158	0.177
Panel B: Men	Public	Private		
Education	≤12 yrs	13 yrs +	≤12 yrs	13 yrs +
Dep. Var.	Empl.	Empl.	Empl.	Empl.
Post × Intensity	-0.009	0.014***	0.017	0.007
γ	(0.014)	(0.004)	(0.023)	(0.028)
Obs.	1,695,182	319,647	1,695,182	319,647

Notes: Heteroskedasticity-consistent standard errors accounting for clustering at the province-year level in parentheses. The dependent variable is an indicator for whether an individual participated in the labor force (LFP), whether an individual had a job (in the public/private sector) (Empl.), and whether an individual was unemployed (Unemp.). *Significant at 10% level; **significant at 5% level; *** significant at 1% level. This table presents estimated coefficients from a linear probability model. The time period is 2006-2015 for the base specification. LFP stands for labor force participation.

Table 6 reports the effects of the quota on labor force participation, employment, and unemployment for women (Panel A) and men (Panel B). We divide the sample of each gender group by education attainment and look at employment outcome at age 23-29. In the first six columns, we look at the immediate impact on the rates of labor force participation, employment, and unemployment by education. The dependent variable is a dummy for employment status. We find no significant employment effects for men and women with high school education. For college-educated women, the coefficient of our interest is significantly different from zero for the outcomes of labor force participation and employment; they are estimated as -0.015 and -0.016, respectively. The numbers indicate that a 1 percentage-point decrease in the proportion of female hiring resulted in a 1.5 percentage point decrease in the propensity toward labor force participation and a 1.6 percentage point decrease in employment propensity (as reported in the table). Since the mean of the treatment intensity is 11.3, these results show that the quota reduced the probability of labor force participation and employment for female college graduates by 16.95 percentage points (=1.5 percentage points×11.3) and 18.08 percentage points (=1.6 percentage points×11.3).

We find no significant spillover effects of the quota on the labor market for non-college graduates. Such findings are consistent with the labor market structure in Iran, as Iran's labor market segregation by education is very strict. Companies often establish positions based on workers' education level. For the jobs that require a certain level of education, only applicants with that level of education are permitted to apply, e.g. applicants with a master's degree are not allowed to apply for a job that requires a bachelor's. What is more, the Iranian law sets the minimum wage according to education attainment, and educated people cannot work for a wage lower than the education-specific minimum wage (Source: Iranian Labor law, Ministry of Cooperative, Labor and Social Welfare). Even if college educated workers are willing to work for low wages, the labor market arrangement does not allow them to do so.

For men, we find that the quota increased employment and decreased unemployment. Such results are somewhat surprising given that the proportion of female workers in the entire economy is not large; women's share of employment in the public sector in the pre-treatment period is about 20%.

In the bottom part of the table, we look at employment outcomes in the public and private sectors separately in order to examine spillover effects across sectors. For college-educated women in the public sector, the coefficient of our interest is estimated as -0.9 (percentage points) for the propensity toward employment. The estimate means that a 1 percentage-point decrease in the proportion of female hiring in the job posting in the public sector in fact resulted in a 0.9 percentage point decrease in the actual employment propensity for young women in the public sector. It instead leads to a 1.4 percentage point increase in young men's employment propensity in the public sector. For others, the estimates show insignificant effects. In theory, it is possible that the private sector would react to the situation and hire more college graduates because more female educated workers were unemployed and seeking a job opportunity after the quota. However, the results indicate that there were no spillover effects observed in the private sector, which is consistent with the RDD results.

Note that the effects by college education, presented here, are underestimated given that the college attendance rate is directly affected by the policy. The true effects will be larger since some women (men) are discouraged from (encouraged into) pursuing higher education and enter the labor market without (with) college education. Nonetheless, the results are informative about the lower bound of the effect on college educated workers' employment outcomes.

Table 7 presents the DID results that estimate the effects on education by gender. We find that women's enrollment in university significantly decreased by 1.9 percentage points if a proportion of female hires in the public sector decreases by one percentage point. This result indicates that the college enrollment rate decreased by 21.47 percentage points (=1.9 percentage points×11.3) for the province that experienced a 11.3 percentage point reduction in the share of female hires. These findings imply that the quota had significant effects on women's college attendance.

Table 7: DID Results (Education)

	Attended highschool	Attended college
Panel A: Women		
Post × Intensity	-0.025	-0.019***
γ	(0.021)	(0.004)
Obs.	225,490	296,063
R-squared	0.572	0.260
Mean control	0.659	0.640
Panel B: Men		
Post × Intensity	-0.005	0.038***
γ	(0.011)	(0.004)
Obs.	316,839	304,105
R-squared	0.477	0.263
Mean control	0.590	0.625

Notes: Heteroskedasticity-consistent standard errors accounting for clustering at the province-year level in parentheses. The dependent variable is an indicator for whether an individual enrolls in a specified educational institution. Control variables are year and province fixed effects, rural-urban dummies, birth year, and family background including parent's education. *Significant at 10% level; **significant at 5% level; *** significant at 1% level. This table presents estimated coefficients from a linear probability model. The time period is 2006-2015 for the base specification. We also conducted the analysis by excluding observations in 2009, but the results are similar.
The sample for this analysis is men and women aged 14 to 25 who are eligible for enrolling in high school and aged 18 to 35 who complete high school and are eligible for enrolling at any university programs.

Overall, the results are consistent with the RDD estimates. To further highlight the importance of labor market opportunities for women as a determinant of their educational attainment, we also examine whether the quota decreased the proportion of female students in college majors with strong ties to public employment and increased college majors with weaker associations to the public sector.

Table 8 presents the effects on choice of college major. The dependent variable is an enrollment dummy for each college major at four-year universities. The results show that women do not enroll in college majors that are highly related to public sector employment, but undertake other fields that are less related to public employment. College majors that are highly relevant to public employment are engineering, health, and science majors. All of these majors experienced a larger drop in women's enrollment in more affected provinces. For example, as the intensity measure increased by 1 percentage point, women's propensity for choosing an engineering major decreased by 0.097 percentage point. That means that, in the province where the quota decreased the female share by 11.3 percentage points, the number of women majoring in engineering decreased by 1.10 (=0.97 percentage point×11.3) percentage points. While the analysis on college major provides compelling results, we do not apply an RDD analysis due to the small sample size. The findings on college major add to the literature that evaluate how expectations about outcomes affect college major choice, in which mixed results are found. Zafar (2011) uses a sample of North western University undergraduates and finds no evidence of expected earning affecting the choice of major. Arcidiacono et al. (2012) use a survey of male undergraduate students at Duke University and find both expected earnings and individual abilities significantly affect the choice of major. Our findings are consistent with the finding of the latter, highlighting the importance of labor market returns in college major choice.

Table 8: Effect on Education by Gender and College Major

College Major	Engineer.	Health	Science	Arts	Education	Business	Agri.	Service
Panel A: Women								
Treatment	-0.097**	-0.062**	-0.090**	0.145***	0.038	0.014	0.049**	0.029
γ	(0.004)	(0.003)	(0.004)	(0.043)	(0.029)	(0.059)	(0.025)	(0.016)
Observations	66,354	66,354	66,354	66,354	66,354	66,354	66,354	66,354
R-squared	0.022	0.024	0.044	0.010	0.014	0.015	0.006	0.003
Panel B: Men								
Treatment	0.004	-0.025	-0.101*	0.031	0.022	0.042	0.028	-0.014
γ	(0.006)	(0.021)	(0.052)	(0.029)	(0.017)	(0.051)	(0.023)	(0.013)
Observations	71,750	71,750	71,750	71,750	71,750	71,750	71,750	71,750
R-squared	0.044	0.009	0.032	0.093	0.014	0.017	0.009	0.005

Notes: Heteroskedasticity-consistent standard exrrors accounting for clustering at the province-year level in parentheses. The dependent variable is an indicator for whether an individual enrolls in a specified educational institution. *Significant at 10% level; **significant at 5% level; *** significant at 1% level. This table presents estimated coefficients from a linear probability model. The time period is 2006-2015 for the base specification.
The sample for this analysis is men and women who are enrolled in college during the study period.

5.3 Robustness Check

Our findings are robust to a number of choices made in the analysis. We briefly summarize the results from robustness checks here. The detailed robustness check results for RDD are reported in the Appendix, Tables C.3 to C.6 and the robustness check for DID are presented in the Appendix, Tables D.7 to D.11.

As for the RDD analysis, we examine the sensitivity of our estimates to different bandwidths, model specifications and sample periods. First, concerning sensitivity to bandwidth, we used the cohorts that are only a few years apart and obtained similar results with slightly larger standard errors. The results show that the main results are robust to the bandwidth choices. presented in the Appendix, C.3. We check sensitivity to model specifications by comparing the results with and without control variables and find that the estimated coefficients of the interaction terms are qualitatively the same regardless of the inclusion. These results are also found in the Appendix, Table C.3. Second, we check that our results are robust to different model specifications. The detailed results are found in the Appendix, Tables C.4 to C.6. Again, we find results qualitatively similar to the ones in the main analysis; the signs of the coefficients and significance are all the same. Third, as to sample periods, we use the data until 2015 for the analysis reported in the main section. We re-conduct our analysis by limiting our sample to a shorter period. In doing so, we show evidence that our findings are not driven by other policies instituted around the time of the quota implementation.[16] Shortening the sample periods limits our ability to analyze the long-run effects of the quota, but we confirm that the main estimates on school attendance are qualitatively the same.

Other important changes in policies and economic conditions are the population control implemented in 1979-93 and economic sanctions. However, these changes affect the Iranian economy much earlier than 2010 and if there is any impact, both control and treated cohorts are affected in a similar fashion. The best evidence for this is the RDD estimates using the pre-quota periods, which are found insignificant as presented earlier in the main section in Tables 2 and 3 . In sum, the above tests build confidence in the accuracy of our findings.

As for the DID analysis, we first check the sensitivity of the DID results to the measurement of treatment intensity. We apply two other measurements for treatment intensity. Regardless of definition of the variable, we find qualitatively similar results to the ones reported in the main section except regarding fertility outcomes. As for fertility, the estimated effects became insignificant/significant, depending on model specifications

[16] There are two important policies to which we must pay attention; a policy that changes the composition of college majors announced and implemented in 2012, and policies aiming at increasing fertility announced and implemented in 2015. We use 2006-2011 and 2006-2014 in the robustness check analysis.

when using the alternative measurements, but the directions of the estimated effects are consistent. The results are presented in the Appendix, Table D.7.

We examine that the treatment intensity is not correlated with confounding factors and present the data features to support exogeneity of the treatment. We first check whether the more affected areas are comparable to less affected areas. To do so, we regress the treatment intensity on province characteristics to show that our treatment is not associated with other economic conditions that affect the outcomes of our interest (See Table D.8 in the Appendix). We also show no significant differences in observable province-specific characteristics in the periods prior to the quota (See Table D.9 in the Appendix). We also re-conduct the analysis on employment and education by excluding observations in the election year 2009.[17] Without observations in 2009, the effects on employment for college-educated women are still significantly negative with a magnitude of -0.41 (percentage points), which was previously estimated and reported in the table as -0.33 (percentage points). Other estimates are found to be very similar in all the results.

We also address possible estimation bias due to other policy changes. To examine if any other policy drives differences in trends, we conduct placebo tests. We find no evidence of difference in preexisting trends across provinces. We also re-conduct the analysis on employment and education by excluding observations in the election year 2009 and find similar results.[18] More formally, we conduct a placebo test for education outcomes by using a timing different from the actual timing of the policy implementation during the treatment year. We find that none of the coefficients are significantly different from zero. We thus show that there is no obvious anticipatory effect or violation of the common trend assumption (See Table D.10 in the Appendix). These results provide additional support for the key assumption that there was no particular systematic trend change that affected education outcomes across provinces other than the 2010 quota.

5.4 Summary and Discussion

One mechanism by which the 2010 hiring policy may affect women's lifetime decisions is through changes in economic opportunities. We find students are responsive to changes

[17] The Iranian economy was affected by the presidential election from late 2008 to mid 2009. As is often the case with elections, employment surged during the election period and the published statistics might have been misreported in favor of the incumbent president. The election period ended on June 12, 2009, when the incumbent president Mahmoud Ahmadinejad won a second term. The election results were upsetting to many people in Iran, who had supported opposition candidates, and resulted in large-scale protests. However, the major turbulence caused by the protests had ceased by December 2009. The election results are not likely to change the long run economic trend during 2006-2015 largely because Ahmadinejad's policies in the second term were similar to his policies in the first term.

[18] Note that 2009 was an election year and employment typically increases in election years because the Iranian government usually attempts to increase its vote share and hence its re-election probability.

in the labor market conditions: in both the RDD and DID analysis, we find significantly negative effects on (college educated) women's labor force participation and employment and significantly positive effects on women's unemployment rates. For men, the effect on each labor market outcome is found to be opposite to that of women. Thus, we confirm that the quota indeed reduced labor market opportunities for women and there was a small, yet non-ignorable spillover effect into men's labor market.

In contrast, spillover effects of the quota on the labor market for non-college graduates are insignificant. The reduced employment opportunities for college graduates resulted in significant reduction of women's enrollment in university but did not affect high school enrollment. We also find no spillover effects to the private sector.

The findings for education, marriage and fertility are in accordance with those in Jensen (2012), which estimates the effects of higher perceived job opportunities using Indian data. He finds a 2.8 percentage-point increase in enrollment rates of any college when labor force opportunities for women increased. He also finds that women aged 15 to 21 in treated villages were 5.1 percentage points less likely to get married during the three years after being exposed to more job opportunities. He documents that women who are exposed to more job opportunities reported wanting to have 0.35 fewer children in their lifetime. In either outcome, the magnitude of the impact is larger in Jensen's paper than ours. Such a difference in results makes sense considering that Jensen's randomized experiment is conducted in rural areas in India while the policy of our interest mostly affected highly educated women in urban areas where public sector jobs are more concentrated.

What is more, we study the effects of adverse changes in labor market opportunities. The theoretical effect of a worsening of labor market conditions on education is ambiguous. On the one hand, the limited labor market opportunities for educated women could decrease their educational attainment by reducing expected earnings from additional schooling. On the other hand, poor labor market opportunities could increase educational attainment by inducing stronger competition among women to fill the few vacancies or by reducing opportunity costs of schooling (Thurow (1975); Moen (1999); Ordine and Rose (2009)). Since the overall effect of a reduction in labor market opportunities on education could go either way, it is important to conduct formal empirical analysis to estimate the importance of labor market opportunities for women's education. Overall, our findings add to Jensen (2012)'s work showing symmetric effects of positive and negative shocks to labor market opportunities: while he finds that increasing labor market opportunities increase education, our paper finds that decreasing labor market opportunities decrease education.

The findings indicate the impact on women's life-time outcomes is substantial. Given that the labor market return is a driving force for obtaining education, a reduction in future skilled labour market opportunities discourages young girls from investing in their education, leading to early marriage and childbearing. Because early marriage and pregnancy have negative effects on women's ability to obtain education and increase their potential involvement in skilled jobs in the future (Field and Ambrus (2008)), it could lower skill investment by women themselves and further reduce their employablity. Our findings imply a negative feedback loop between labor market opportunity and education, which works in a manner opposite to affirmative action policies. These negative effects are not limited to individual welfare. At the aggregate level, human capital is an important factor of productivity growth. Thus, such reduction in women's human capital can decrease future economic growth.

6 Conclusion

We investigate how limiting the number of job openings for female workers affects young women's education attainment, work, and family decisions. To do so, we exploit a quasi-natural experiment setup in Iran, whose government has restricted the number of new female hires in the public sector since 2010. By exploiting this policy change regarding Iranian female workers, we find that limiting labor market opportunities for women adversely affects women's education. We also find that the quota significantly affected women's college major choices. While the proportion of female students in college majors with strong ties to public employment decreased, it increased in college majors with weaker associations to the public sector. Instead of attending college, those women who observe fewer opportunities in the labor market are more likely to get married and have a child. Our analysis indicates that the anticipation of labor market returns is particularly important for the investment of human capital and subsequent life decisions.

In sum, this paper studies a unique setting in which the ceiling had a large impact on highly educated women by altering their labor market returns to education. The findings contribute to the literature that assesses how adverse changes in the labor market affect education decisions for highly skilled women. Needless to say, understanding the role of employment opportunity in education decisions is important for many settings and policies since education is associated not only with human capital accumulation, but also with economic growth, reduced crime rates, better health, and higher social welfare. Our analysis suggests that policy makers who would like to influence education outcomes must simultaneously address the labor market opportunities that students face after graduation, along with other means to encourage women's pursuit of education.

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Appendix

A Article 230 of the National Five-Year Development Plan

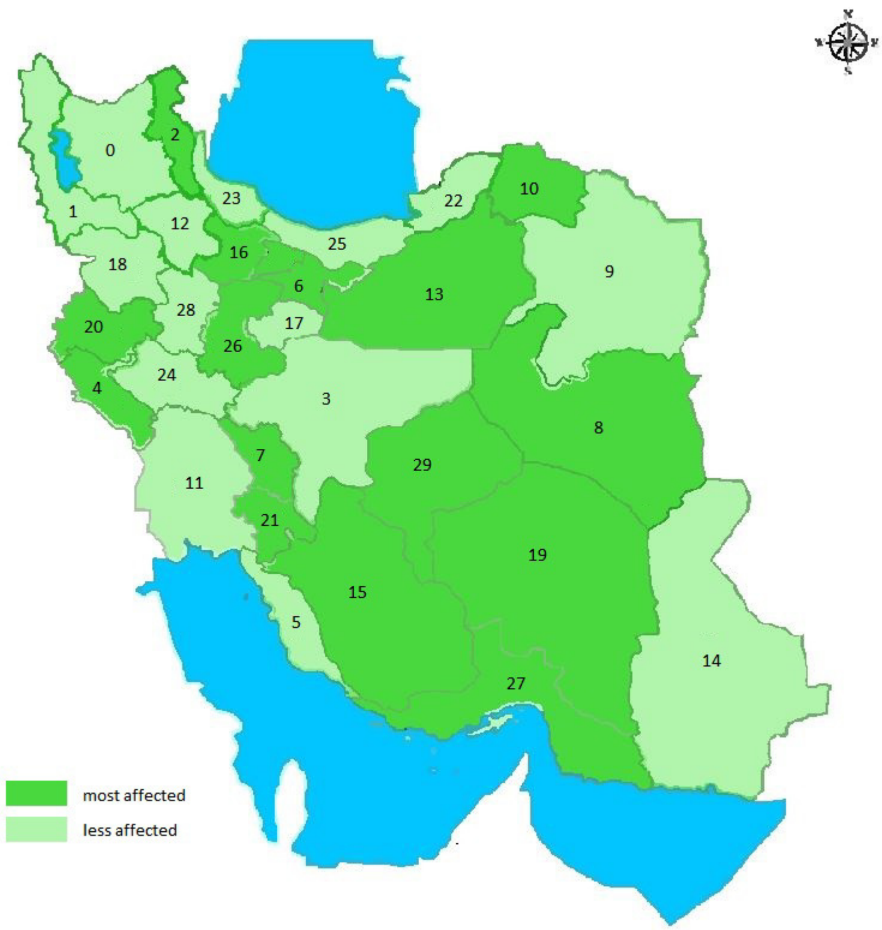
Article 230 of the fifth development plan states that "The government and all ministries and organizations, including the Center for Women and Family, are formulating a comprehensive national development program on women and family. The plan aims at strengthening the family foundation; review of the laws and regulations; prevention of social damages; economic development; creating home-based jobs for female head of household; social security; empowerment of civil society, and the reform of women's machineries." (Source: Sixty-sixth Session of the United Nations of General Assembly on October 11, 2011; Shargh newspaper. Number 2568. Tuesday, April 26, 2016 Sharghdaily.ir/News)

Note that while the document provided by the United Nations only mentioned "creating jobs for female head of household," as one of the development goals, other documents circulated in Farsi clearly state "creating home-based jobs for female head of household and low-income women".

B Treatment Intensity

The proportion of female employment in the public sector by province before and after the ceiling is presented in Figure B.1 and Table B.1. Although there is a province, Qom, whose hiring proportion in 2015 exceeds the proportion of female employment in 2009, this province is not a representative Iranian province in the sense that this area is the center of the Shiite sect of Islam and most religious schools are concentrated in this city. The gender segregation in this city has been very high traditionally; e.g. The University of Qom has two campuses: one for male students and the other for female students and two administrative offices for employees of each gender (Source: The website of The University of Qom (http://www.qom.ac.ir/Portal/Home/)). The University of Qom is one of the few universities in Iran that supports gender segregation. Administrative and educational environments are separated for women and men. The University of Qom has two separate campuses for women and men. (translated from Farsi to English by the authors. This description can also be found in the Farsi version; the English version does not contain the corresponding expression) The hospitals in Qom Province practice gender-segregation, and there are hospitals which employ only women. For example, Shahid Beheshti hospital in Qom is the first hospital to support gender segregation. In this hospital, women are treated only by female physicians and men are treated only by male physicians. Partly due to a large degree of gender segregation, this province had the lowest proportion of women in the public sector before the quota (17%), and the proportion did not decrease because the female-only sector needs to keep women employees in order to operate its gender-specific organizations (schools or hospitals). While this province is the exception, the raw data for the rest of Iran exhibits a drop both in employment size and the proportion of female workers in the public sector.

Figure B.1: Map of Iranian Provinces showing the Intensity of the 2010 Hiring Quota



Notes: Less affected provinces are the provinces with treatment intensity below median.
More affected provinces are the provinces with treatment intensity above median.
Source: Calculation by the authors using the data of job openings in each province in the public sector in 2015

Given that treatment intensity across provinces are driven by the share of gender-segregated workplaces, which are exempted from the quota, a major concern is that the distribution of hospitals and schools may be correlated with province-specific characteristics. However, we find no such correlations, as discussed in the main section. In addition, we here discuss that the treatment intensity is not correlated with the population or geographical areas. Tehran Province, which includes the capital Tehran, was among the most affected provinces: the proportion of women in the public sector

Table B.1: The proportion of female employment in the public sector by province before and after the ceiling

Province code	Province	% women in the public sector in 2009 (before the policy)	% job openings for women in 2015 (after the policy)	bind
0	East Azerbaijan	20.80	17.53	1
1	West Azarbaijan	28.41	19.87	1
2	Ardabil	29.70	15.88	1
3	Isfahan	26.57	23.88	1
4	Ilam	26.36	7.50	1
5	Bushehr	20.92	10.13	1
6	Tehran	20.69	1.86	1
7	Chaharmahal and Bakhtiari	26.93	14.06	1
8	South Khorasan	24.32	12.79	1
9	Razavi Khorasan	27.17	16.47	1
10	North Khorasan	22.66	2.95	1
11	Khuzestan	22.72	11.62	1
12	Zanjan	25.39	20.19	1
13	Semnan	35.88	10.09	1
14	Sistan and Baluchestan	28.66	23.50	1
15	Fars	26.27	9.31	1
16	Qazvin	25.55	10.23	1
17	Qom	17.72	21.48	0
18	Kurdistan	22.34	19.80	1
19	Kerman	25.76	12.70	1
20	Kermanshah	24.63	11.51	1
21	Kohgiluyeh and Boyer Ahmad	22.10	10.16	1
22	Golestan	24.33	20.37	1
23	Gilan	18.92	9.48	1
24	Lorestan	21.96	18.34	1
25	Mazandaran	19.30	13.73	1
26	Markazi	27.88	11.22	1
27	Hormozgan	27.76	10.66	1
28	Hamedan	27.31	25.47	1
29	Yazd	29.17	10.88	1

Notes: the province code corresponds to the code in Figure B.1. The indicator "bind" takes 1 if the share of female new hires in the public sector is smaller in 2015 than that in 2009.

in 2009 was 20.7% but the proportion of job openings for women (including the job openings for both genders) was only 1.9% in 2015. Isfahan, the second most populated city after Tehran, was among the less affected provinces: the proportion of women was 26.6% in 2009 and the proportion of job openings in 2015 was 23.9%. On average, the proportion of women is 25% in the pre-treatment periods, but only 10% of job openings were open to women after the quota was imposed. The proportion of female new hires in the public sector decreases by 11.3 percentage points on average. Table B.2 reports the summary statistics of the intensity measurements. To report the summary statistics of treatment intensity, we divide the sample into two subgroups according to treatment intensity (below median or above median). The less affected province group includes all working-age individuals in the provinces with treatment intensity below median. The more affected province group includes all working-age individuals in the provinces with treatment intensity above median.

Table B.2: Descriptive Statistics: Treatment Intensity

Variable	Obs	Mean	Std. Dev.	Min	Max
Intensity Measurement (%)	29	44.6	23.2	6.7	91.0
Intensity Measurement 2	29	11.3	6.3	1.8	25.8
Intensity Measurement 3 (%)	29	0.6	0.5	0.06	3.2
% of Female Workers in the Public Sector in 2009	29	25.2	3.7	18.9	35.9
% of Female New Hires in the Public Sector in 2015	29	13.9	5.9	1.9	25.5

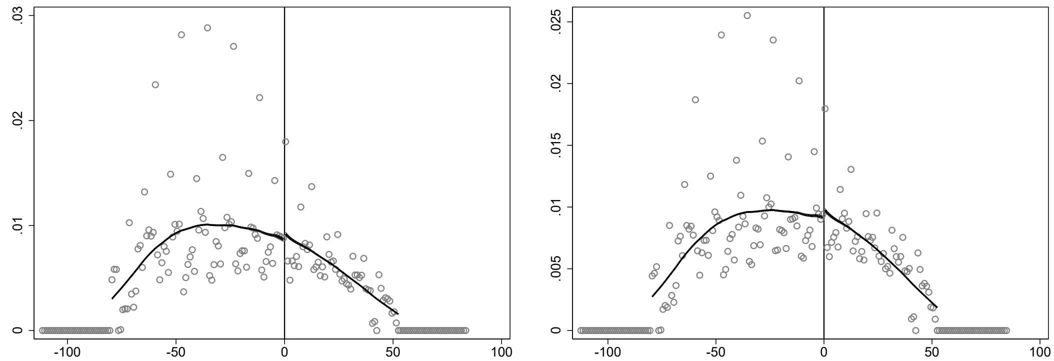
Intensity Measurement is measured as the percentage difference between the pretreatment share (% women in the public sector in 2009) and the targeted hiring rate of women in the public sector (% job openings for women in the public sector in 2015). This measurement is used for the main analysis. Intensity Measurement 2 is measured as (% women in the public sector in 2009-% job openings for women in the public sector in 2015). This measure is used for robustness check. Intensity Measurement 3 is measured as % job openings for women in the public sector in 2015 divided by the population of women at working age in 2015. This measure is also used as a robustness check.

C RDD: Robustness Checks

C.1 Sample Universe

Cell size and sample composition for men and women in the data are shown in Figure C.2. The key identification assumption is that the treated birth cohorts are not systematically different from the untreated birth cohorts in other characteristics. The assumption is violated if there is a systematic change around the 1988 birth cohort cutoff. While the true age of an individual is predetermined, it is conceivable that some people could find ways to incorrectly report their age by falsifying their birth certificates. To see if such manipulations were possible, we check the distribution of birth month. It is true that we see several spikes in August of each year, but no trend changes around the cutoff. The spikes (the higher density) are attributed to the parents' misreport of their children's birth month in an attempt to send them to school earlier (since those who were born in September or later will go to school in the following year). As seen from the graphs all of these patterns are stronger for the older cohorts because misreporting the birth date recently became more difficult. These trend changes happen gradually and there is no obvious gap before and after the 1988 cutoff (it is no surprising because at that time their parents did not know there would be a quota policy in place in 2010). Thus, we do not observe systematic changes in birth rates before and after the cutoff.

Figure C.2: Distribution of Birth Months by Gender



C.2 Different Bandwidth and Different Model Specifications

We examine the sensitivity of our RD estimates to different bandwidths. As presented in the first four columns of Table C.3 , we use the cohorts that are only a few years apart and obtained similar results with slightly larger standard errors. Thus, the main results are robust to the bandwidth choices. Next, we check the robustness of our results by comparing the results with and without control variables. The results are presented in the last three columns of the same table The estimated coefficients of the interaction terms are qualitatively the same regardless of the inclusion.

For the discrete outcomes, we also estimate limited dependent variable models. Again, we find very similar results as the ones using a linear probability model; the signs of the coefficients and significance are all the same. We re-conduct the analysis using different specifications for the smooth function. Table C.4 presents three different specifications, including the linear spline model, which is presented in the main section. The estimates are qualitatively similar in all specifications.

C.3 Adding Treatment Intensity to RD models

An RDD estimate will be contaminated in our setting if any other factor affects those who were born before and after the cutoff differently. We re-conduct the RDD analysis using the following specification:

$$Y_{it} = \gamma_1(\text{Treated}) + \gamma_2(\text{Treated}) \times \bar{D}_j + \alpha\bar{D}_j + g(Age) + \mathbf{X}'_{it}\mathbf{B} + \varepsilon_{it} \tag{4}$$

Table C.3: RDD Estimates with different bandwidth and controls (Education)

	Attended college (cutoff=September 1988)						
Bandwidth	(1)	(2)-Main	(3)	(4)	(5)	(6)	(7)
Birth Cohort	1987-89	1986-90	1985-91	1984-92	1986-90	1986-90	1986-90
Panel A: Women							
Treated	-0.029***	-0.032***	-0.036***	-0.039***	-0.032***	-0.009**	-0.018***
	(0.005)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Trend shift	-0.038***	-0.040***	-0.043***	-0.045***	-0.033***	-0.020***	-0.006***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Pre-trend	0.017***	0.025***	0.032***	0.039***	0.024***	0.014***	0.001
	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Province FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age	Yes	Yes	Yes	Yes	Yes	No	No
Parents' education	Yes	Yes	Yes	Yes	No	Yes	No
Obs.	56,941	65,202	78,485	81,252	65,202	65,202	65,202
R-squared	0.183	0.181	0.180	0.179	0.036	0.008	0.001
Mean control	0.412	0.414	0.413	0.412	0.414	0.414	0.414
Panel B: Men							
Treated	0.020**	0.022***	0.016***	0.012***	0.021***	0.010***	0.017***
	(0.006)	(0.005)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)
Trend shift	0.003	0.007***	0.009***	0.009***	0.007***	0.004**	0.004**
	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)
Pre-trend	0.025***	0.026***	0.031***	0.031***	0.026***	0.013***	0.017***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Province FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age	Yes	Yes	Yes	Yes	Yes	No	No
Parents' education	Yes	Yes	Yes	Yes	No	Yes	No
Obs.	46,350	54,130	66,001	68,644	54,130	54,130	54,130
R-squared	0.041	0.042	0.043	0.044	0.010	0.004	0.006
Mean control	0.428	0.425	0.420	0.415	0.425	0.425	0.425

Notes: Heteroskedasticity-consistent standard errors accounting for clustering at the birth cohort level in parentheses. The dependent variable is an indicator for whether an individual enrolls in a specified education level. Control variables are individual and family characteristics including age and parents' schooling. *Significant at 10% level; **significant at 5% level; *** significant at 1% level. This table presents estimated coefficients from a linear probability model. The time period is 2006-2015 for the base specification.
In the row "Trend shift," we present the estimated coefficient of the interaction term, $Treated \times (BirthYear - Cutoff)$. In the row "Pre-trend," we present the estimated coefficient of the cohort trend term, $BirthYear - Cutoff$.

where we add the interaction term of the treatment intensity measure in province j ($\bar{D}_j$) and the treatment intensity to the main specification. In particular, the coefficient γ_1

Table C.4: RDD estimates with different functional forms (Education)

	Attended college		
	Linear Spline	Quadratic	Cubic
Panel A:			
Women			
Treated	-0.032***	-0.014***	-0.026***
	(0.004)	(0.007)	(0.006)
Treated×(Birth Cohort−cutoff)	-0.040***	-0.021***	-0.035**
	(0.001)	(0.005)	(0.015)
(Birth Cohort−cutoff)	0.025***	0.003	0.015
	(0.001)	(0.004)	(0.013)
Treated×(Birth Cohort−cutoff)^2		-0.006***	-0.023***
		(0.001)	(0.007)
(Birth Cohort−cutoff)^2		-0.007***	-0.001
		(0.001)	(0.004)
Treated×(Birth Cohort−cutoff)^3			0.003***
			(0.001)
(Birth Cohort−cutoff)^3			0.001
			(0.000)
Obs.	65,202	65,202	65,202
R-squared	0.181	0.181	0.181
Mean control	0.414	0.414	0.414
Panel B:			
Men			
Treated	0.022***	0.030***	0.033***
	(0.005)	(0.009)	(0.018)
Treated×(Birth Cohort−cutoff)	0.007***	0.024***	0.032**
	(0.002)	(0.006)	(0.016)
(Birth Cohort−Cutoff)	0.026***	0.004	0.012
	(0.001)	(0.004)	(0.014)
Treated×(Birth Cohort−Cutoff)^2		0.005***	0.009
		(0.001)	(0.007)
(Birth Cohort−Cutoff)^2		-0.005***	-0.008*
		(0.001)	(0.005)
Treated×(Birth Cohort−Cutoff)^3			0.000
			(0.001)
(Birth Cohort−cutoff)^3			-0.000
			(0.000)
Obs.	54,130	54,130	54,130
R-squared	0.042	0.042	0.042
Mean control	0.425	0.425	0.425

Notes: Heteroskedasticity-consistent standard errors accounting for clustering at the birth cohort level in parentheses. The dependent variable is an indicator for whether an individual enrolls in a specified education level. Control variables are individual and family characteristics including age and parents' schooling. *Significant at 10% level; **significant at 5% level; *** significant at 1% level. This table presents estimated coefficients from a linear probability model. The time period is 2006-2015 for the base specification. The estimates presented in Column "Linear Spline" is the same as the main results.

of the birth cohort dummies captures the overall impact of the quota and the coefficient γ_2 of the interaction term captures the effects multiplied by the intensity of the quota. This specification exploits the variation in not only across birth cohorts and time, but also across provinces.

Tables C.5 and C.6 present the results for education and marriage, which are similar to the main analysis using the regression discontinuity estimator. The finding implies that the main findings are robust to adding more controls (variation in treatment intensity).

Table C.5: Adding Treatment Intensity to RD Models (Education)

Outcome	Education	
	Attended highschool	Attended college
Panel A: Women		
Treated	-0.002	-0.033***
	(0.007)	(0.005)
Treated×Intensity	-0.023***	-0.034***
	(0.011)	(0.005)
Intensity	-0.007	-0.029***
	(0.005)	(0.004)
(Birth Cohort-Cutoff)	-0.001	0.017***
	(0.001)	(0.002)
Treated × (Birth Cohort-Cutoff)	-0.002	-0.031***
	(0.006)	(0.001)
Obs.	82,815	65,202
R-squared	0.025	0.040
Mean control	0.843	0.414
Panel B: Men		
Treated	-0.011	0.014***
	(0.060)	(0.005)
Treated×Intensity	0.004	0.008
	(0.011)	(0.006)
Intensity	0.031***	0.005
	(0.005)	(0.005)
(Birth Cohort-Cutoff)	0.006***	0.015***
	(0.001)	(0.001)
Treated × (Birth Cohort-Cutoff)	-0.009	0.004**
	(0.005)	(0.002)
Obs.	93,616	54,130
R-squared	0.027	0.037
Mean control	0.812	0.425

Notes: Heteroskedasticity-consistent standard errors accounting for clustering at the birth cohort level in parentheses. The dependent variable is an indicator for whether an individual enrolls in a specified educational level. Control variables are individual and family characteristics including age and parents' schooling. *Significant at 10% level; **significant at 5% level; *** significant at 1% level. This table presents estimated coefficients from a linear probability model. The time period is 2006-2015 for the base specification.

Table C.6: Adding Treatment Intensity to RD Models (Marriage & Fertility)

Outcome	Marriage	Number of children
Treated	0.024***	0.004***
	(0.004)	(0.001)
Treated×Intensity	-0.008	-0.001
	(0.007)	(0.001)
Intensity	0.030***	-0.001
	(0.004)	(0.001)
(Birth Cohort-Cutoff)	0.050***	-0.004***
	(0.001)	(0.000)
Treated × (Birth Cohort-Cutoff)	0.035***	0.004***
	(0.002)	(0.000)
Obs.	310,147	308,919
R-squared	0.029	0.030
Mean control	0.658	0.197

Notes: Heteroskedasticity-consistent standard errors accounting for clustering at the birth cohort level in parentheses. The dependent variables are marriage (an indicator for whether an individual is married or not) and fertility rate (number of children). Control variables are individual characteristics including marital status and schooling. *Significant at 10% level; **significant at 5% level; *** significant at 1% level. This table presents estimated coefficients from a linear probability model. The time period is 2006-2015 for the base specification. We also conducted the analysis by excluding observations in 2009, but the results are similar.

D DID: Robustness Checks and Falsification Tests

In this subsection, we present the data features to support exogeneity of the treatment. In particular, we show that the treatment intensity is not correlated with confounding factors. To show that the treatment is exogenous, we regress treatment intensity on province-specific characteristics.

D.1 Measurement of Treatment Intensity

We check whether the results are different when applying different measurements of the treatment intensities. We try two other measurements for treatment intensity. One of the measurements is the proportion of women in the public sector in 2009 minus the proportion of job openings for women in the public sector in 2015 (Measure 2). Another measurement is the change in the number of public job positions divided by the working-age population for each gender in each province in order to reflect how competitive it was for women/men to acquire a job in the public sector (Measure 3). As for outcomes other than fertility, regardless of the variable definition, we find qualitatively similar results to the ones reported in the main section. Regarding fertility, the estimated effects became insignificant when using the alternative measurements while it was significant in the main analysis. The results are presented in Table D.7.

Table D.7: DID Results with Different Treatment Measures (Education)

Intensity Measurement	Measure1	Measure2	Measure3
Panel A: Women			
Treated × Intensity	-0.019***	-0.001***	-0.327*
γ	(0.004)	(0.000)	(0.182)
Obs.	296,063	296,064	296,065
R-squared	0.260	0.260	0.260
Panel B: Men			
Treated × Intensity	0.038***	0.001***	0.659***
γ	(0.004)	(0.000)	(0.176)
Obs.	304,105	304,106	304,107
R-squared	0.263	0.264	0.265

Notes: Heteroskedasticity-consistent standard errors accounting for clustering at the birth cohort level in parentheses. The dependent variable is an indicator for whether an individual enrolls in a specified educational institution. *Significant at 10% level; **significant at 5% level; *** significant at 1% level. This table presents estimated coefficients from a linear probability model. The time period is 2006-2015 for the base specification. The sample for this analysis is men and women aged 15 to 25 (18 to 35 for graduate school).

D.2 Exogeneity of Treatment Intensity

Table D.8 shows that the distribution of hospitals and schools is little correlated with province-specific characteristics, such as employment/unemployment rates or education attainment rates. In fact, we found no correlation with the exception of public sector size, which is a province characteristic related to gender-segregated workplaces. We conclude that the treatment intensity is not significantly associated with province characteristics, no matter which intensity measurements we use.

D.3 Less Affected and More Affected Provinces

To examine correlation between treatment intensity and confounding factors, we now compare the results by presenting the observable characteristics of differently affected provinces.

Table D.9 presents the results. For presentation purposes, we divide the sample into two subgroups according to treatment intensity (below median or above median). The less affected province group includes all working-age individuals with treatment intensity below median. The more affected province group includes all working-age individuals with treatment intensity above median. The comparison between the two subgroups shows that the differences are at most 3 percent of the mean value.

We also look at how treatment intensity is correlated with the level and growth of college education. To be specific, we look at how treatment intensity is associated with the proportion of women with college degrees in the pre-treatment period (2008) as

Table D.8: Are Treatment Intensity Correlated with the Labor Market Conditions or Education Trends?

	Treatment intensity proxy		
	Measure1	Measure2	Measure3
Dependent Variable			
Unemployment rate	0.0002	0.0086	-0.6419
	(0.0003)	(0.0105)	(0.7841)
Women's unemployment rate	0.0004	0.0126	-0.9422
	(0.0005)	(0.0174)	(1.3002)
Men's unemployment rate	0.0002	0.0066	-0.4938
	(0.0004)	(0.0125)	(0.9328)
Young people unemployment rate (age 15-30)	-0.0009	-0.0289	2.1505
	(0.0011)	(0.0367)	(2.7370)
Young women's unemployment rate (age 15-30)	-0.0008	-0.0260	1.9387
	(0.0007)	(0.0234)	(1.7461)
Young men's unemployment rate (age 15-30)	0.0007	0.0208	-1.5505
	(0.0006)	(0.0206)	(1.5347)
Proportion of educated young people (age 15-30)	-0.0001	-0.0028	0.0062
	(0.0002)	(0.0062)	(0.4648)
Proportion of educated young women (age 15-30)	-0.0004	-0.0121	0.9045
	(0.0003)	(0.0084)	(0.6262)
Proportion of educated young men (age 15-30)	0.0004	0.0112	-0.8370
	(0.0003)	(0.0088)	(0.6591)

Notes: Heteroskedasticity-consistent standard errors accounting for clustering at the birth cohort level in parentheses. The dependent variable is an indicator as to whether an individual enrolls in a specified educational institution. *Significant at 10% level; **significant at 5% level; *** significant at 1% level. This table presents estimated coefficients from a linear probability model. The time period is 2006-2015 for the base specification. We also conducted the analysis by excluding observations in 2009, but the results are similar.

well as "changes" in the proportion of women with college degrees in 2008. The unit of observation is each province. The correlation coefficient is 0.24 for the proportion of college-educated women and 0.007 for its change. We also use the women's average years of schooling as an alternate measurement of education in each province, but again, find no correlation.

D.4 Testing the Common Trend Assumption

A differences-in-differences approach relies on the assumption that the pretrends among differently affected areas must be comparable, and there should not be any anticipatory effects. Hence, we investigate the pretrends among differently affected areas by conducting placebo tests. To be specific, we allow a placebo treatment in a year different from the actual timing of the quota policy implementation. For example, by using the year

Table D.9: Mean, Standard Deviations, and Tests of Covariate Balance at Baseline

	Less affected provinces	More affected provinces
Household-level variables		
Number of observations	624,285	617,968
% Family with a male head	99.88	99.91
Head's years of schooling	7.36	7.56
	(4.82)	(4.73)
Spouse's years of schooling	6.22	6.67
	(4.80)	(4.80)
Family size	2.97	2.99
	(1.30)	(1.31)
Individual-level variables ($15 \leq age \leq 64$)		
Number of observations (female)	1,176,349	1,161,140
Number of observations (male)	1,119,711	1,116,793
Age (female)	34.06	34.35
	(13.31)	(13.34)
Age (male)	33.71	33.92
	(13.38)	(13.36)
% Married women	64.78	64.92
% Women have given birth	56.40	56.25
% Married men	60.56	60.42
% Men whose wife has had a child	50.81	50.52
% Women who work for pay	13.79	14.56
% In school: girls (age:15-18)	64.33	66.11
% In school: boys (age:15-18)	70.00	70.10

Less affected provinces (column (1)): all working-age individuals in the provinces with treatment intensity below median. More affected provinces (column (2)): all working-age individuals in the provinces with treatment intensity above median. Standard deviations are presented below each set of statistics.

2008 as the fake treatment year, we can check whether people anticipated the effects and whether the trends are comparable. Since this fake treatment precedes the actual date of the policy implementation, the estimator of our interest should be statistically insignificant. Table D.10 shows no effects in the other years, meaning that the policy immediately went into effect in 2010; this analysis indicate that there is no evidence of anticipatory effects or post-treatment effects.

Table D.10: Placebo DID Results (Education)

Dependent Var.	Attending college			
Fake treatment year	2007	2008	2009	2011
Panel A: Women				
Treatment	-0.006	-0.007	0.008	0.004
γ	(0.012)	(0.013)	(0.015)	(0.016)
Observations	296,063	296,063	296,063	296,063
R-squared	0.054	0.054	0.054	0.054
Panel B: Men				
Treatment	-0.016	-0.017	-0.013	-0.007
γ	(0.020)	(0.019)	(0.020)	(0.018)
Observations	304,105	304,105	304,105	304,105
R-squared	0.052	0.052	0.052	0.052

Notes: Heteroskedasticity-consistent standard errors accounting for clustering at the birth cohort level in parentheses. The dependent variable is an indicator for whether an individual enrolls in a specified educational institution. *Significant at 10% level; **significant at 5% level; *** significant at 1% level. This table presents estimated coefficients from a linear probability model. The time period is 2006-2015 for the base specification. The sample for this analysis is men and women aged 15 to 25 (18 to 35 for graduate school). Each table presents the estimated effects on education enrollment when we use a year other than 2010 as the fake treatment year.

D.5 How do Education Effects Differ by Treatment Intensity?

The previous results present effects of the quota and find a significant decline in women's four-year college attendance. Here, we look at heterogenous effects on education. In particular, we report the effects by treatment intensity to show whether reduced job openings monotonically affected the education outcomes.[19] This analysis serves as a robustness check to examine whether the results are consistent with the prediction in that the more treated areas reflect a stronger impact than the less treated ones.

Table D.11 presents the results for women divided into subgroups according to treatment intensity. We divide women into those in less affected provinces and those in more affected provinces. While we find no significant effects for less affected provinces, we find negative effects for more affected provinces. For more affected provinces, the college attendance rate went down by 9.6 percentage from 65% for any university program (two-year and four-year programs).

[19]In addition to the results reported above, we look at the effects of the hiring ceiling on wages to see how the quota affected the composition of workers in the labor market. However, we do not find any significant effects. We also look at the effects by household asset to see heterogeneous effects, but do not detect any significant difference by wealth.

Table D.11: DID Results (Education for Women in More/Less Affected Areas)

Dep. Var.	Education	
	Attended highschool	Attended college
Regions	Less Affected	
Post $\times$ Intensity	-0.012	-0.067**
γ	(0.031)	(0.030)
Obs.	115,746	144,206
R-squared	0.054	0.047
Mean control	0.658	0.648
Regions	More Affected	
Post $\times$ Intensity	-0.062***	-0.096***
γ	(0.018)	(0.012)
Obs.	109,744	151,857
R-squared	0.061	0.030
Mean control	0.659	0.632

Notes: Heteroskedasticity-consistent standard errors accounting for clustering at the province level in parentheses. The dependent variable is an indicator for whether an individual enrolls in a specified education level. Control variables are year and province fixed effects, rural-urban dummies, birth year, and family background including parent's education. *Significant at 10% level; **significant at 5% level; *** significant at 1% level. This table presents estimated coefficients from a linear probability model. The time period is 2006-2015 for the base specification. We also conducted the analysis by excluding observations in 2009, but the results are similar. The sample for this analysis is men and women aged 15 to 25 who are eligible to enroll in high school and aged 18 to 35 who complete high school and are eligible for enrolling at any university programs.