EFFECT OF COMPULSORY SCHOOLING ON WOMEN'S LABOR AND MARRIAGE MARKET OUTCOMES: EVIDENCE FROM TÜRKİYE

by
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ABSTRACT

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ECONOMICS M.A. THESIS, JULY 2023

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This study examines the impact of the 1997 compulsory schooling reform in Turkey, which extended mandatory education from 5 to 8 years, on women's employment prospects and marriage market outcomes using a regression discontinuity design. The findings indicate that the reform has no significant effect on women's labor market outcomes. However, it does lead to a higher likelihood of being married until ages 17 to 21 and giving birth until ages 19 to 21. These results imply that the reform led women to get married and give birth at earlier ages. Either improvement in the human capital of women does not penetrate to labor market outcomes since the reform did not affect the common practice of leaving the labor market for marriage related reasons in Türkiye, or the reform does not improve the human capital of women.

ÖZET

ZORUNLU EĞİTİMİN KADINLARIN İŞ VE EVLİLİK PİYASALARI SONUÇLARINA ETKİLERİ: TÜRKİYE ÖRNEĞİ

ÖMER TAHA GÜLBERK

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Bu çalışmada Regresyon-Süreksizlik Deseni yöntemini kullanarak, Türkiye'de 1997 yılında hayata geçirilen ve zorunlu eğitimi 5 yıldan 8 yıla çıkaran yasanın kadınların iş edinme olasılıklarına ve evlilik piyasaları sonuçlarına etkilerini inceliyorum. Bulgularım bu eğitim reformunun kadınların işgücü piyasaları sonuçlarına istatistiksel olarak anlamlı bir etkisinin olmadığı yönünde. Öte yandan reform, kadınların 17-21 yaşlarına kadar evlenmiş olma ihtimalini ve 19-21 yaşına kadar doğurmuş olma ihtimalini artırıyor. Özetle, reform kadınların erken yaşlarda evlenmesine ve erken yaşlarda çocuk sahibi olmasına sebep oluyor. Bu durum, reformun kadınların evlilik sebebiyle işten ayrılması üzerine etkisinin olmaması veya artan eğitimin işgücünde yeterince yüksek bir getirisi olmamasından kaynaklanıyor olabilir.

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To the good people of METU Economics Community

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1. INTRODUCTION

The causal impact of education on labor market outcomes is widely discussed in the literature for high-income countries (Angrist and Krueger (1991); Pischke and Von Wachter (2008)). These results may not be valid for low and middle income countries since they differ in terms of socioeconomic and institutional backgrounds. In middle and low-income countries, the gender gap in social status is higher. An increase in women's education may lead to a reduction in this gap. However, there may be some other cultural characteristics that prevent women to participate in everyday life, including the labor market. Some part of these cultural characteristics can be boiled down to traditional gender roles, which are more binding in low and middle income countries.

Especially in low and middle income countries, there is a huge gender gap in employment rates (ILO (2023)). Low enrollment rates of women may be one of the drivers for this fact. One other driver may be the attitudes toward working women and traditional gender roles, which affects the marriage market decisions of women. To understand the potential role of education, I aim to investigate the causal impact of education on labor market outcomes through a regression discontinuity design, exploiting the exogenous variation in schooling created by the 1997 Compulsory Schooling Law (CSL) in Türkiye. The CSL in Türkiye increased compulsory schooling from 5 to 8 years. It created a discontinuous jump in the enrollment rates and middle school graduation of women. According to TDHS-2013 and TDHS-2018, 38% of the women, who quit a job at least once, leave their jobs for marriage related reasons in Türkiye. These reasons include being pregnant, childcare, and opposition of the husband or parents. I analyzed whether this common practice in Türkiye is a result of the schooling of women and their husbands.

To interpret the labor market outcomes in a more comprehensive manner, I investigate the reform's effect on the marriage market outcomes too, such as age at first marriage, age at first birth, number of child, and time to first birth after marriage. Assuming that both labor market and marriage market decisions are given by the

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partners together, and noting that leaving the labor market for marriage related reasons is common among women in Türkiye, the estimates show the reform's causal effect on these outcomes rather than the causal effect of women's education.

I use the 2013 and 2018 Turkey Demographic and Health Survey (TDHS) data, which is nationally representative of women in Türkiye aged between 15-49. The data contains information about women's education, work, and marriage history. This allows us to conduct a more detailed analysis of employment experience. The time women left school or graduated from school is also known. For example, by using this information, I calculate the total time in months women are available for work for each age. I then compute the employment experience ratio for each age, that is the total employment experience until age x divided by the time women are available for work until age x. HLFS (Household Labor Force Survey), which is a widely used dataset for analyzing labor market outcomes, has a bigger sample size, but only provides the current employment status. On the other hand, despite a smaller sample size, information on all the employment experiences is contained in the TDHS data. Hence, while TDHS provides labor market information about the same individuals over time, individuals in HLFS are observed once at the time of the survey.

Another contribution of this paper to the literature is that labor market outcomes are analyzed in the light of the marriage market decisions of women in a low income country, where the traditional gender roles are embraced more, including women not participating in the labor force if married, childcaring, etc.

I find that the reform does not have any effect on current employment, being ever employed, total employment experience in months, and total employment experience ratio of women. I then study whether this has any relation with the marriage market decisions of women. According to the analyses, the reform decreased the age at first marriage, and the age at first birth of women, while the fertility at earlier ages goes up. Additionally, the reform did not change the common practice of leaving the labor market for marriage related reasons.

The main result of this paper is that the reform does not affect the employment probabilities of women but it affects the marriage and fertility decisions.

In the next section, I provided a general outlook on the literature on marriage market outcomes and labor market outcomes of women. Then, I give information about the data I used in this study and present the descriptive statistics of the samples used. Then, I introduce the identification strategy in detail. This is followed by the results section, in which I present and discuss my estimations about marriage

and labor market outcomes. After that, robustness checks are presented. The last section discusses conclusions.

2. LITERATURE REVIEW

Firstly, I review the literature on the causal impact of education on women's labor market outcomes. Secondly, I examine the literature on women's marriage market outcomes. Lastly, I go over the existing research on the relationship between women's marriage and labor market outcomes.

Utilizing the 2002-2013 HLFS data, Aydemir and Kirdar (2017) investigate the causal impact of education on the wages of men and women by exploiting the exogenous variation in schooling using the 1997 CSL in Türkiye. They use a fuzzy regression discontinuity design due to the imperfect compliance with the reform. They found that an additional year of schooling increases women's wages by 7-8%, and men's wages by 2-3%. Additionally, they found that CSL increased the employment probability of women by around 0.4-1.1 percentage points. There is no effect of completing grades 6 to 8 on wages of women. The effect on wages for women in the overall sample is coming from the education levels that are higher than middle school. Moreover, Mammen and Paxson (2000), who study Thailand and India, find that secondary schooling[1] slightly increases the probability of being in the labor force for women in India. There is no effect of secondary schooling on labor force participation in Thailand. However, they find that post-secondary schooling increases the labor force participation of women by around 25% in both countries. Similarly, Cameron, Malcolm Dowling, and Worswick (2001) find that the causal effect of secondary schooling on the labor force participation of women differs among the five Asian countries they analyzed, while post-secondary schooling has a strong causal impact. As I focus on the effect of the reform, which makes middle school mandatory, my findings are in line with theirs.

Using the 2004-2011 HLFS data, Torun (2018) find that the 1997 CSL in Türkiye increased women's wages by 9-10%, and men's wages by 0-2%. In this study, he investigated the effect of the reform on the employment probabilities of each gender

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[1]Secondary school covers grades 9 to 12 in India, grades 7 to 12 in Thailand.

and find no significant effect. He used the exposure to the reform as an instrument.

Moussa and Omoeva (2020) present evidence from low income countries, Ethiopia, Malawi, and Uganda. By employing the rollout of universal primary education policies in these three countries, they find that increase in schooling decreases teenage sexual activity, marriage, and births, while it does not affect the labor force participation of women. Using the same policy, Chicoine (2020) find a reduction in fertility, a delay in marriage and birth, and an increase in skilled employment probability. However, there is no significant effect on overall employment probability.

Evidence from high income countries is also mixed. Angrist and Krueger (1991) estimated that the return to an additional year of schooling is 8% in U.S. Grenet (2013) find no significant effect of the compulsory schooling reform in France for both men and women, while he also finds the effect of a similar reform in England is positive and significant for women. Additionally, Pischke and Von Wachter (2008) find zero returns to education in Germany.

The literature on marriage market outcomes focuses mostly on the effect of education on teenage fertility. However, in countries similar to Türkiye, there is a rigid sequence of marriage and fertility (Kırdar, Dayıoğlu, and Koç (2018)). Investigating the impact of education on this sequence requires a closer look at outcomes such as age at first marriage, age at first birth, and number of children.

The causal impact of education on the marriage market outcomes of women is also mixed. McCrary and Royer (2011) find no effect on teenage pregnancy in the U.S., while Black, Devereux, and Salvanes (2008) find a decrease. There are also some studies finding delays in the age at marriage as a result of education in high income countries (Brien and Lillard (1994); Skirbekk, Kohler, and Prskawetz (2004)).

Kırdar, Dayıoğlu, and Koç (2018) estimated the effect of the 1997 CSL in Türkiye on teenage marriage and birth by using a regression discontinuity design. They used the TDHS-2013 dataset for the marriage market analysis. According to their findings, the reform is reducing the probability of marriage by age 16, first birth by age 17, and the number of children born to a woman by age 18, which highly contradicts with the results I find. I estimated that the reform increases the probability of marriage by age 17 to 21, first birth by age 19 to 21, and the number of children born by age 18 to 22. Moreover, they find that the reform does not have any effect on the time to first birth after marriage. The main difference of my study from Kırdar, Dayıoğlu, and Koç (2018) is that I also use TDHS-2018 data in the analyses. There are also some differences in control variables.

Güneş (2016), by using the same reform and the TDHS-2013 data, find that the CSL

decreases the probability of ever giving birth until age 22, while Gulesci, Meyersson et al. (2013) report no effect of education on age at marriage and age at first birth. The reason for these differences is the identification strategies. Güneş (2016) used an IV framework by using the variation in the exposure to the reform across cohorts and regions by the intensity of additional classrooms constructed in the birth regions of women as an instrumental variable. As Kırdar, Dayıoğlu, and Koç (2018) stated, when time trends are introduced to the model of Güneş (2016), the results disappear. Dinçer, Kaushal, and Grossman (2017) also used an IV framework, in which they use the geographical variation in the teacher-to-child ratio in birth provinces of women. They find that education decreased the number of children per woman.

Traditional attitudes have a downside causal effect on the labor supply of women (Vella (1994)). Early marriage and fertility can be linked to traditional gender roles. These marriage market decisions may influence the labor force participation of women. Most part of the literature examines the effect of fertility on the labor force participation of women. The studies generally estimate that fertility reduces the female labor supply (Angrist and Evans (1996); Becker (1991); Bloom et al. (2009)). Also, there are studies showing the impact of childcare costs on the labor supply of women. Connelly (1992) finds that higher childcare costs decrease the probability of labor force participation of women. Examining the effect of the marriage itself, apart from the fertility side of the story, may be crucial for low and middle income countries since husbands can prevent women to work in these countries. Wilson (2022) finds that preventing early marriage increases the probability of women participating in the labor force in 17 low and middle income countries. He used a difference-in-differences approach by employing the laws that increase the marriage age. Assaad, Krafft, and Selwaness (2017) present evidence from MENA countries by using an IV design. They find that early marriage reduces the likelihood of labor force participation of women.

3.   DATA

This study utilizes data from the TDHS-2013 and TDHS-2018 surveys, which cover women aged 15-49 in Turkey. TDHS data allows for a detailed analysis of the effect of women's education on labor and marriage market outcomes. The surveys provide information about women's education, marital history, childbirth history, employment history, migration history, and place of birth. Unlike earlier TDHS surveys, TDHS-2013 and TDHS-2018 include not only married but all women, which makes the data representative of the female population in Türkiye. The data consists of 17092 women, where 9746 women come from TDHS-2013.

By employing TDHS data, we have comprehensive information on women's educational attainment, such as the highest educational level, highest grade at that level, and graduation status at each education level. Furthermore, it provides information on the age at each marriage and the age at each childbirth. Moreover, TDHS includes information on women's work start-quit dates, reasons for quitting work, sector of work, and wage employment status.

TDHS-2018 data does not include Syrian women while TDHS-2013 does. Compared to the studies that used TDHS-2013 data, introducing TDHS-2018 allows me to observe women affected by the reform at their later ages. In the overall sample restricted to 8 years interval, women are between 18 and 31 years old. For 4 years interval, age ranges between 22 and 31. The fraction affected by the reform is 48% for all the intervals in Table 3.1. As bandwidth gets wider, years of schooling get higher. However, the proportion of middle school graduated women is the same for all intervals.

In the sample restricted to 8 years interval, the mean of the total employment experience is around 3 years. Furthermore, women work 20% of the time they are available for work. On the other hand, on average, the mean of number of children is 1.6, age at first marriage is 20.3, and age at first birth is 21.8. 83% of the 8 years interval sample is ever married and 71% has ever given birth. In the sample, women

get married and give birth at early ages when their labor market experiences are low. Moreover, 43% of the women who quit a job at least once in the 8 years interval sample leave their jobs for marriage related reasons.

Table 3.1 : Descriptive statistics

	4 Years Interval					6 Years Interval					8 Years Interval				
	Mean	Std	Min	Max	N	Mean	Std	Min	Max	N	Mean	Std	Min	Max	N
Birth Year	1986.42	2.29	1983	1990	4095	1986.35	3.46	1981	1992	6185	1986.32	4.59	1979	1994	8198
Age (Years)	28.64	3.43	22	36	4095	28.71	4.28	20	37	6185	28.74	5.22	18	40	8198
Years of Schooling	8.21	4.69	0	21	4095	8.25	4.71	0	22	6185	8.28	4.65	0	22	8198
Middle School Graduation	0.58	0.49	0	1	4095	0.58	0.49	0	1	6185	0.58	0.49	0	1	8198
Fraction Affected by the Reform	0.48	0.50	0	1	4095	0.48	0.50	0	1	6185	0.48	0.50	0	1	8198
Age at First Employment	18.91	5.14	8	35	2348	18.93	5.34	8	37	3499	18.88	5.51	8	38	4537
Currently Employed	0.29	0.45	0	1	4095	0.29	0.45	0	1	6185	0.29	0.45	0	1	8198
Currently Wage Employed	0.20	0.40	0	1	4095	0.20	0.40	0	1	6185	0.19	0.39	0	1	8198
Currently Nonwage Employed	0.09	0.29	0	1	4095	0.09	0.29	0	1	6185	0.10	0.29	0	1	8198
Ever Employed	0.58	0.49	0	1	4095	0.57	0.49	0	1	6185	0.56	0.50	0	1	8198
Ever Wage Employed	0.46	0.50	0	1	4095	0.45	0.50	0	1	6185	0.44	0.50	0	1	8198
Ever Nonwage Employed	0.16	0.37	0	1	4095	0.17	0.37	0	1	6185	0.17	0.37	0	1	8198
All Employment Experiences (Months)	34.62	49.20	0	240	3986	35.06	51.70	0	264	6009	35.46	55.14	0	288	7964
All Wage Employment Experiences (Months)	22.64	38.29	0	240	3986	22.52	39.64	0	264	6009	21.88	40.94	0	287	7964
All Nonwage Employment Experiences (Months)	11.98	36.58	0	240	3986	12.53	38.94	0	264	6009	13.57	42.50	0	288	7964
All Employment Experience Ratio	0.23	0.30	0	1	3984	0.23	0.30	0	1	5978	0.22	0.30	0	1	7872
All Wage Employment Experience Ratio	0.16	0.26	0	1	3984	0.16	0.26	0	1	5978	0.15	0.26	0	1	7872
All Nonwage Employment Experience Ratio	0.07	0.19	0	1	3984	0.07	0.19	0	1	5978	0.07	0.20	0	1	7872
Ever Married	0.88	0.42	0	2	4095	0.86	0.43	0	2	6185	0.83	0.45	0	2	8198
Ever Given Birth	0.75	0.43	0	1	4095	0.74	0.44	0	1	6185	0.71	0.45	0	1	8198
Number of Children	1.65	1.38	0	10	4095	1.65	1.43	0	13	6185	1.63	1.49	0	13	8198
Age at First Marriage	20.45	3.68	10	33	3451	20.40	3.74	10	36	5110	20.32	3.81	10	36	6536
Age at First Birth	21.75	3.64	12	35	3079	21.77	3.78	12	37	4560	21.75	3.88	12	38	5829
Time to First Birth After Marriage	20.87	18.49	0	227	3079	21.36	19.19	0	227	4560	21.53	19.54	0	227	5829
Marriage Related Job Leave	0.34	0.47	0	1	2377	0.33	0.47	0	1	3537	0.33	0.47	0	1	4590
Marriage Related Job Leave (Among Job Leavers)	0.44	0.50	0	1	1671	0.43	0.50	0	1	2460	0.43	0.50	0	1	3177
Rural	0.40	0.49	0	1	4095	0.40	0.49	0	1	6185	0.40	0.49	0	1	8198
Region of Birth (NUTS-2 Level):															
Istanbul	0.05	0.21	0	1	4095	0.05	0.21	0	1	6185	0.05	0.22	0	1	8198
Tekirdag	0.02	0.13	0	1	4095	0.02	0.14	0	1	6185	0.02	0.13	0	1	8198
Balikesir	0.03	0.16	0	1	4095	0.02	0.15	0	1	6185	0.02	0.15	0	1	8198
Izmir	0.03	0.16	0	1	4095	0.02	0.15	0	1	6185	0.02	0.15	0	1	8198
Aydin	0.01	0.12	0	1	4095	0.02	0.13	0	1	6185	0.02	0.13	0	1	8198
Manisa	0.02	0.15	0	1	4095	0.02	0.15	0	1	6185	0.02	0.15	0	1	8198
Bursa	0.03	0.16	0	1	4095	0.03	0.16	0	1	6185	0.03	0.16	0	1	8198
Kocaeli	0.02	0.14	0	1	4095	0.02	0.14	0	1	6185	0.02	0.15	0	1	8198
Ankara	0.03	0.18	0	1	4095	0.03	0.18	0	1	6185	0.03	0.18	0	1	8198
Konya	0.03	0.17	0	1	4095	0.03	0.17	0	1	6185	0.03	0.17	0	1	8198
Antalya	0.02	0.14	0	1	4095	0.02	0.14	0	1	6185	0.02	0.14	0	1	8198
Adana	0.05	0.22	0	1	4095	0.05	0.22	0	1	6185	0.05	0.22	0	1	8198
Hatay	0.04	0.21	0	1	4095	0.05	0.21	0	1	6185	0.05	0.21	0	1	8198
Kirikkale	0.04	0.19	0	1	4095	0.04	0.19	0	1	6185	0.04	0.19	0	1	8198
Kayseri	0.04	0.20	0	1	4095	0.05	0.21	0	1	6185	0.05	0.21	0	1	8198
Zonguldak	0.02	0.14	0	1	4095	0.02	0.14	0	1	6185	0.02	0.15	0	1	8198
Kastamonu	0.02	0.14	0	1	4095	0.02	0.14	0	1	6185	0.02	0.14	0	1	8198
Samsun	0.05	0.23	0	1	4095	0.06	0.23	0	1	6185	0.05	0.22	0	1	8198
Trabzon	0.08	0.27	0	1	4095	0.07	0.26	0	1	6185	0.08	0.27	0	1	8198
Erzurum	0.04	0.20	0	1	4095	0.04	0.21	0	1	6185	0.04	0.21	0	1	8198
Agri	0.05	0.23	0	1	4095	0.05	0.22	0	1	6185	0.05	0.23	0	1	8198
Malatya	0.04	0.20	0	1	4095	0.04	0.20	0	1	6185	0.04	0.19	0	1	8198
Van	0.06	0.23	0	1	4095	0.06	0.23	0	1	6185	0.06	0.24	0	1	8198
Gaziantep	0.05	0.21	0	1	4095	0.04	0.20	0	1	6185	0.04	0.20	0	1	8198
Sanliurfa	0.06	0.23	0	1	4095	0.06	0.23	0	1	6185	0.06	0.23	0	1	8198
Mardin	0.05	0.21	0	1	4095	0.04	0.20	0	1	6185	0.04	0.20	0	1	8198
Abroad	0.02	0.15	0	1	4095	0.02	0.14	0	1	6185	0.02	0.15	0	1	8198
Birth Month:															
January	0.12	0.32	0	1	4095	0.11	0.32	0	1	6185	0.11	0.32	0	1	8198
February	0.08	0.28	0	1	4095	0.08	0.28	0	1	6185	0.08	0.28	0	1	8198
March	0.09	0.29	0	1	4095	0.09	0.28	0	1	6185	0.09	0.29	0	1	8198
April	0.08	0.28	0	1	4095	0.08	0.28	0	1	6185	0.09	0.28	0	1	8198
May	0.08	0.27	0	1	4095	0.08	0.27	0	1	6185	0.08	0.27	0	1	8198
June	0.08	0.27	0	1	4095	0.08	0.27	0	1	6185	0.08	0.28	0	1	8198
July	0.08	0.27	0	1	4095	0.08	0.27	0	1	6185	0.08	0.26	0	1	8198
August	0.08	0.27	0	1	4095	0.08	0.28	0	1	6185	0.09	0.28	0	1	8198
September	0.08	0.28	0	1	4095	0.08	0.28	0	1	6185	0.08	0.28	0	1	8198
October	0.09	0.29	0	1	4095	0.09	0.28	0	1	6185	0.09	0.28	0	1	8198
November	0.07	0.26	0	1	4095	0.07	0.25	0	1	6185	0.07	0.25	0	1	8198
December	0.06	0.24	0	1	4095	0.06	0.24	0	1	6185	0.06	0.24	0	1	8198

Note: The sample contains women from TDHS-2013 and TDHS-2018 surveys. Intervals indicate the distance to the cutoff point, which is being born in January 1987.

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4.   IDENTIFICATION STRATEGY

Our objective is to find a causal impact of education on labor market outcomes. However, education itself is an endogenous variable, which can lead to a potential bias in our estimation. In order to deal with this endogeneity, we use the 1997 Compulsory Schooling Reform in Türkiye, which affected children who were born in 1987 or later. Those born after 1987 were subject to an additional three years of compulsory schooling, while the others were not. Since being born in 1987 or later is exogenous to the individual choices of parents and children, we use it as the treatment variable. However, as shown in the results section, the reform has statistically significant effects on the marriage market outcomes of women. Also, marriage market outcomes may have an impact on labor market outcomes. In Türkiye, it is common for women to leave their jobs due to marriage-related reasons, which is a joint decision of women and their husbands. Also husbands may be affected by the reform. Thus, our focus is on estimating the effect of the reform, rather than examining the effect of an increase in education.

We use the following regression discontinuity design (RDD) method for our study.

$Y_i = \beta_0 + \beta_1 \text{Treatment}_i + \beta_2 \text{Treatment}_i * \text{Trend} + \beta_3 \text{Survey Year}_i + \beta_4 \text{Birth Month}_i + \beta_5 \text{Rural}_i + \beta_6 \text{Region of Birth}_i$

Y_i represents the labor or marriage market outcomes of individual i. The treatment variable takes a value of 1 if the birth occurred in 1987 or later, and 0 otherwise. The trend variable is equal to the distance to the cutoff at the monthly level and the interaction between treatment and trend allows for split time trends. The rural variable is equal to 1 if the region of birth is a rural place, and 0 otherwise. RB_i is the region of birth of individual i at the NUTS-2 statistical region level. Additionally, the model includes survey year fixed effects. Given the common practice of registering children in January in Türkiye, the birth month is controlled. Standard errors are clustered at the birth month-year level. Sample weights provided by TDHS are used to ensure representativeness of the women sample with respect to the women

population aged 15-49. We set five different bandwidths, 8 years to 4 years intervals around the cutoff on each side. Results for these five samples are reported.

5.   RESULTS

5.1 Effect of the Reform on Education Outcomes

Table 5.1 shows the estimates of the impact of the reform on schooling outcomes. The reform significantly increases the probability of middle school graduation by around 10% for all the bandwidths. Its effect on high school graduation ranges between 15-18% for narrower bandwidths. As we zoom out from the cutoff point, this effect loses its significance and coefficients become closer to 0. Similar results are observed for university graduation. Thus, while the reform increased the probability of completing middle school, the effects on higher schooling levels are not clear. After showing that reform increased women's education, we can analyze its effect on women's labor market outcomes.

Table 5.1 : The reform's effect on schooling

Age (Years)	4-Years Interval	5-Years Interval	6-Years Interval	7-Years Interval	8-Years Interval
Middle School Graduation	0.071**	0.104***	0.115***	0.119***	0.137***
	(0.029)	(0.028)	(0.025)	(0.023)	(0.023)
High School Graduation	0.134***	0.183***	-0.011	0.004	0.005
	(0.042)	(0.048)	(0.036)	(0.028)	(0.026)
University Graduation	0.094**	0.132***	0.035	0.009	0.020
	(0.044)	(0.046)	(0.029)	(0.024)	(0.022)
Observations	4,095	5,123	6,185	7,211	8,198

Note: The sample contains women from TDHS-2013 and TDHS-2018 surveys. Intervals indicate the distance to the cutoff point. The running variable is birth month-year. The reform dummy is equal to 1 when the women's time of birth is greater than or equal to January 1987, 0 otherwise. Estimates are from the regression in the Identification Strategy section of the paper. Sample weights provided by TDHS surveys are taken into account. The standard errors are in parentheses and clustered at the birth month-year level.

5.2 Effect of the Reform on Labor Market Outcomes

For the analysis, we eliminated the employment experiences before age 15 and before they dropout or graduate, since full-time work and schooling are incompatible events (Dayioğlu (2005)). This gives more precise results, especially when analyzing labor market experiences by age. This also takes into account that more educated women can enter the labor market later than others.

In this section, I investigate the effect of the reform on the labor market outcomes of women. I examine the effect of the reform on women's current employment, which shows whether they are employed at the time of the survey or not. Then, I analyze their ever employment status at each age. Moreover, I estimate the effect of the reform on women's total employment experience by age. If a woman worked for 24 months until the age of 20, then her total employment experience at age 20 is equal to 24. Lastly, I looked at women's employment experience ratio by age, which is the total employment experience by age divided by the total time that women are available for work until that age. For example, if a woman worked for 24 months until the age of 20 and she graduated from school at the age of 17, then her employment experience ratio at age 20 is 2/3. Availability is calculated by the time between the selected age for analysis and the time that women dropout or graduated from the school. For women who left the school earlier than the age of 15, the time between the selected age and the time that women were at the age of 15 is calculated for availability. The idea behind calculating the employment experience ratio is to make the labor market experience of women more comparable to each other.

As can be seen from Table 5.2, the reform has no significant effect on women's current employment outcomes. Coefficients are close to 0 and not significant on all bandwidths for nonwage employment, while they are higher and only significant at the 10% level on 8 years intervals for all types of employment and wage employment. Analyzing the labor market outcomes of women by the current employment status using TDHS is not the most efficient way since the number of observations in TDHS is not as high as the Labor Force Surveys. However, by using the work history of women provided by TDHS, more precise and clear picture of women's employment experience can be achieved.

Table 5.2 : The reform's effect on current employment

Age (Years)	4-Years Interval	5-Years Interval	6-Years Interval	7-Years Interval	8-Years Interval
Currently Employed	0.006	0.036	0.038	0.040	0.043*
	(0.030)	(0.028)	(0.026)	(0.025)	(0.023)
Currently Wage Employed	0.020	0.042	0.038	0.033	0.041*
	(0.027)	(0.026)	(0.024)	(0.023)	(0.021)
Currently Nonwage Employed	-0.014	-0.007	0.000	0.006	0.002
	(0.017)	(0.017)	(0.014)	(0.013)	(0.013)
Observations	4,095	5,123	6,185	7,211	8,198

Note: The sample contains women from TDHS-2013 and TDHS-2018 surveys. Intervals indicate the distance to the cutoff point. The running variable is birth month-year. The reform dummy is equal to 1 when the women's time of birth is greater than or equal to January 1987, 0 otherwise. Estimates are from the regression in the Identification Strategy section of the paper. Sample weights provided by TDHS surveys are taken into account. The standard errors are in parentheses and clustered at the birth month-year level.

Aydemir and Kirdar (2017) analyzed women's employment probability by studying their current employment and find that the reform increased the women's employment probability by around 0.4-1.1 percentage points. However, analyzing both current and ever employment of women, I do not find such an effect. I estimate the reform's effect on the probability of being ever employed at each age. The results in Table 5.3 indicate that ever employed probability does not change for women. There is a positive and significant effect at the age of 25 for 7 and 8 years intervals. The results for wage employment in Table 5.4 show no effects. Restricting the employment outcomes to nonwage employment gives us positive and significant results after the age of 22 at 7 and 8 years intervals (Table 5.5). Hence, the results at the age of 25 are driven by nonwage employment.

Tables 5.6, 5.7 and 5.8 present results for total employment experiences of women; all employment types, wage employment and nonwage employment respectively. These results show no effect on the total employment experiences of women as well. As can be seen from Table 5.8, there are some significant results for ages 22 to 25 at 8 years interval for total nonwage employment experience. There is not a significant effect at other intervals and coefficients are low, which implies additional experience of around 1.5 months. However, these results may be misleading. Each additional year of schooling may decrease the total employment experience since full-time work and schooling are incompatible events. However, students can work part-time and go to school at the same time. Furthermore, they can work full-time during summer break. TDHS-2018 data provides information about full-time/part-time employment, but TDHS-2013 does not. Thus, full-time/part-time employment experiences are not analyzed in this study. Nonetheless, it can be argued that students going to school are not likely to work, which may lead to misinterpretation of the reform's effect on women's employment prospects. Making the analysis by employment experience

ratio eliminates this problem.

I next present results for the employment experience ratios of women. As can be seen from Table 5.9, the employment experiences ratio of women is not affected by the reform. Results of age 15 are not reported since experiences before age 15 are eliminated. Restricting employment to wage and nonwage separately implies no effect of the reform on the employment experience ratio of women as well (Tables 5.10 and 5.11). There are some significant coefficients but they are weak. Significant coefficients exist at 7 and 8 years intervals.

To summarize, the reform has no significant effect on women's employment prospects. Analyzing wage and nonwage employment separately gives the same results. The only positive effect is coming from nonwage employment of women, and it is not a robust result since the effect is only seen for wider bandwidths.

Any reform or development other than 1997 CSL that affects the education or employment prospects of women is a possible threat to the identification strategy I use. However, there is not any other education reform that affects 4 to 8 years interval samples. Moreover,the primary objective of the reform was to limit religious education (Aydemir and Kirdar (2017);Sasmaz (2015)). Thus, the reform was not influenced by the macroeconomic developments.

Table 5.3 : The reform's effect on ever employed

Age (Years)	4-Years Interval	5-Years Interval	6-Years Interval	7-Years Interval	8-Years Interval
15	0.015	0.016	0.020	0.013	0.010
	(0.035)	(0.031)	(0.028)	(0.027)	(0.025)
16	0.027	0.022	0.023	0.026	0.030
	(0.037)	(0.035)	(0.032)	(0.031)	(0.028)
17	0.014	0.002	0.001	0.002	0.009
	(0.031)	(0.030)	(0.027)	(0.026)	(0.024)
18	-0.003	0.007	0.016	0.028	0.041
	(0.034)	(0.032)	(0.029)	(0.027)	(0.025)
19	-0.012	-0.007	-0.015	0.001	0.014
	(0.031)	(0.031)	(0.027)	(0.026)	(0.024)
20	0.018	0.016	0.013	0.018	0.023
	(0.033)	(0.033)	(0.028)	(0.027)	(0.026)
21	-0.005	0.003	-0.007	0.002	0.013
	(0.031)	(0.030)	(0.026)	(0.025)	(0.023)
22	0.024	0.039	0.025	0.044*	0.045*
	(0.033)	(0.031)	(0.028)	(0.026)	(0.024)
23	0.018	0.018	0.014	0.030	0.029
	(0.032)	(0.031)	(0.029)	(0.026)	(0.025)
24	0.045	0.048	0.035	0.042	0.038
	(0.034)	(0.031)	(0.029)	(0.027)	(0.025)
25	0.040	0.049	0.049	0.060**	0.062**
	(0.034)	(0.032)	(0.031)	(0.029)	(0.028)

Note: The sample contains women from TDHS-2013 and TDHS-2018 surveys. Intervals indicate the distance to the cutoff point. The running variable is birth month-year. The reform dummy is equal to 1 when the women's time of birth is greater than or equal to January 1987, 0 otherwise. Estimates are from the regression in the Identification Strategy section of the paper. Sample weights provided by TDHS surveys are taken into account. The standard errors are in parentheses and clustered at the birth month-year level. See Table 5.21 for observation numbers.

Table 5.4 : The reform's effect on ever wage employed

Age (Years)	4-Years Interval	5-Years Interval	6-Years Interval	7-Years Interval	8-Years Interval
15	-0.004	0.004	0.001	-0.004	-0.003
	(0.029)	(0.026)	(0.024)	(0.023)	(0.022)
16	0.021	0.026	0.022	0.023	0.021
	(0.032)	(0.029)	(0.026)	(0.026)	(0.023)
17	0.016	0.015	0.012	0.010	0.010
	(0.026)	(0.024)	(0.022)	(0.021)	(0.020)
18	-0.010	0.006	0.015	0.020	0.028
	(0.030)	(0.027)	(0.025)	(0.023)	(0.022)
19	-0.019	-0.012	-0.014	-0.004	0.007
	(0.031)	(0.029)	(0.024)	(0.024)	(0.022)
20	0.002	0.002	0.002	0.002	0.004
	(0.032)	(0.030)	(0.026)	(0.025)	(0.024)
21	-0.010	-0.003	-0.012	-0.010	-0.004
	(0.030)	(0.028)	(0.025)	(0.023)	(0.022)
22	0.013	0.024	0.006	0.016	0.017
	(0.031)	(0.028)	(0.027)	(0.024)	(0.023)
23	0.001	-0.002	-0.009	-0.002	0.001
	(0.031)	(0.029)	(0.028)	(0.025)	(0.024)
24	0.020	0.024	0.011	0.011	0.013
	(0.034)	(0.030)	(0.029)	(0.026)	(0.025)
25	0.019	0.025	0.022	0.025	0.028
	(0.035)	(0.032)	(0.030)	(0.028)	(0.027)

Note: The sample contains women from TDHS-2013 and TDHS-2018 surveys. Intervals indicate the distance to the cutoff point. The running variable is birth month-year. The reform dummy is equal to 1 when the women's time of birth is greater than or equal to January 1987, 0 otherwise. Estimates are from the regression in the Identification Strategy section of the paper. Sample weights provided by TDHS surveys are taken into account. The standard errors are in parentheses and clustered at the birth month-year level. See Table 5.21 for observation numbers.

Table 5.5 : The reform's effect on ever nonwage employed

Age (Years)	4-Years Interval	5-Years Interval	6-Years Interval	7-Years Interval	8-Years Interval
15	0.029	0.017	0.026	0.023	0.020
	(0.029)	(0.026)	(0.023)	(0.020)	(0.019)
16	0.021	0.009	0.020	0.015	0.018
	(0.029)	(0.028)	(0.025)	(0.022)	(0.020)
17	0.007	-0.004	0.001	-0.001	0.004
	(0.023)	(0.022)	(0.019)	(0.018)	(0.016)
18	0.017	0.009	0.015	0.016	0.021
	(0.021)	(0.020)	(0.018)	(0.017)	(0.016)
19	0.013	0.007	0.008	0.009	0.011
	(0.023)	(0.021)	(0.019)	(0.018)	(0.016)
20	0.024	0.017	0.023	0.024	0.028
	(0.025)	(0.023)	(0.021)	(0.019)	(0.018)
21	0.012	0.006	0.009	0.016	0.023
	(0.021)	(0.019)	(0.018)	(0.016)	(0.016)
22	0.013	0.015	0.024	0.036**	0.038**
	(0.021)	(0.019)	(0.017)	(0.016)	(0.015)
23	0.013	0.010	0.020	0.034*	0.035**
	(0.022)	(0.021)	(0.019)	(0.017)	(0.016)
24	0.029	0.023	0.030	0.040**	0.040**
	(0.024)	(0.022)	(0.020)	(0.018)	(0.018)
25	0.028	0.026	0.040*	0.051***	0.053***
	(0.024)	(0.022)	(0.020)	(0.019)	(0.019)

Note: The sample contains women from TDHS-2013 and TDHS-2018 surveys. Intervals indicate the distance to the cutoff point. The running variable is birth month-year. The reform dummy is equal to 1 when the women's time of birth is greater than or equal to January 1987, 0 otherwise. Estimates are from the regression in the Identification Strategy section of the paper. Sample weights provided by TDHS surveys are taken into account. The standard errors are in parentheses and clustered at the birth month-year level. See Table 5.21 for observation numbers.

Table 5.6 : The reform's effect on total employment experience

Age (Years)	4-Years Interval	5-Years Interval	6-Years Interval	7-Years Interval	8-Years Interval
15	-0.315	-0.142	-0.053	-0.041	-0.180
	(0.873)	(0.755)	(0.698)	(0.657)	(0.612)
16	0.213	0.226	0.264	0.326	0.297
	(0.416)	(0.394)	(0.351)	(0.333)	(0.309)
17	0.312	0.220	0.210	0.346	0.361
	(0.694)	(0.659)	(0.580)	(0.545)	(0.498)
18	0.432	0.473	0.656	1.006	1.125
	(1.020)	(0.956)	(0.841)	(0.784)	(0.723)
19	-0.030	0.126	0.210	0.751	0.940
	(1.236)	(1.167)	(1.013)	(0.962)	(0.869)
20	0.216	0.503	0.547	0.882	0.880
	(1.500)	(1.440)	(1.229)	(1.188)	(1.086)
21	-0.002	0.195	-0.079	0.555	0.695
	(1.608)	(1.539)	(1.313)	(1.262)	(1.164)
22	0.134	0.805	0.417	1.342	1.454
	(1.790)	(1.697)	(1.487)	(1.417)	(1.297)
23	0.064	0.680	0.552	1.584	1.591
	(1.937)	(1.836)	(1.624)	(1.569)	(1.445)
24	0.291	0.838	1.108	2.197	1.884
	(2.270)	(2.068)	(1.815)	(1.762)	(1.621)
25	0.526	1.328	1.893	3.165	2.727
	(2.392)	(2.190)	(1.967)	(1.928)	(1.804)

Note: The sample contains women from TDHS-2013 and TDHS-2018 surveys. Intervals indicate the distance to the cutoff point. The running variable is birth month-year. The reform dummy is equal to 1 when the women's time of birth is greater than or equal to January 1987, 0 otherwise. Estimates are from the regression in the Identification Strategy section of the paper. Sample weights provided by TDHS surveys are taken into account. The standard errors are in parentheses and clustered at the birth month-year level. See Table 5.21 for observation numbers.

Table 5.7 : The reform's effect on total wage employment experience

Age (Years)	4-Years Interval	5-Years Interval	6-Years Interval	7-Years Interval	8-Years Interval
15	-0.825	-0.664	-0.626	-0.585	-0.559
	(0.784)	(0.686)	(0.625)	(0.593)	(0.547)
16	0.099	0.212	0.160	0.208	0.158
	(0.329)	(0.303)	(0.275)	(0.269)	(0.244)
17	0.238	0.335	0.236	0.308	0.222
	(0.513)	(0.475)	(0.426)	(0.409)	(0.370)
18	0.317	0.623	0.573	0.793	0.741
	(0.792)	(0.730)	(0.643)	(0.605)	(0.556)
19	-0.230	0.235	0.058	0.472	0.509
	(1.015)	(0.933)	(0.805)	(0.761)	(0.685)
20	-0.168	0.411	0.148	0.421	0.237
	(1.251)	(1.165)	(0.993)	(0.947)	(0.868)
21	-0.423	0.148	-0.408	-0.044	-0.126
	(1.331)	(1.251)	(1.088)	(1.021)	(0.953)
22	-0.314	0.435	-0.286	0.251	0.152
	(1.491)	(1.397)	(1.269)	(1.171)	(1.088)
23	-0.330	0.341	-0.334	0.248	0.082
	(1.612)	(1.542)	(1.411)	(1.302)	(1.221)
24	-0.211	0.562	0.033	0.644	0.324
	(1.903)	(1.745)	(1.580)	(1.460)	(1.372)
25	0.155	0.819	0.349	1.090	0.716
	(2.023)	(1.864)	(1.711)	(1.588)	(1.520)

Note: The sample contains women from TDHS-2013 and TDHS-2018 surveys. Intervals indicate the distance to the cutoff point. The running variable is birth month-year. The reform dummy is equal to 1 when the women's time of birth is greater than or equal to January 1987, 0 otherwise. Estimates are from the regression in the Identification Strategy section of the paper. Sample weights provided by TDHS surveys are taken into account. The standard errors are in parentheses and clustered at the birth month-year level. See Table 5.21 for observation numbers.

Table 5.8 : The reform's effect on total nonwage employment experience

Age (Years)	4-Years Interval	5-Years Interval	6-Years Interval	7-Years Interval	8-Years Interval
15	0.511	0.522	0.573	0.543	0.379
	(0.654)	(0.633)	(0.545)	(0.493)	(0.464)
16	0.115	0.013	0.104	0.118	0.139
	(0.325)	(0.310)	(0.268)	(0.241)	(0.225)
17	0.074	-0.115	-0.026	0.038	0.139
	(0.504)	(0.481)	(0.411)	(0.368)	(0.338)
18	0.115	-0.151	0.083	0.213	0.384
	(0.677)	(0.643)	(0.550)	(0.493)	(0.456)
19	0.200	-0.109	0.152	0.279	0.431
	(0.790)	(0.754)	(0.653)	(0.594)	(0.549)
20	0.384	0.092	0.399	0.461	0.644
	(0.954)	(0.911)	(0.785)	(0.723)	(0.683)
21	0.421	0.047	0.328	0.600	0.820
	(0.969)	(0.926)	(0.804)	(0.746)	(0.699)
22	0.448	0.369	0.702	1.090	1.303*
	(1.045)	(0.975)	(0.868)	(0.804)	(0.762)
23	0.393	0.338	0.886	1.335	1.509*
	(1.115)	(1.056)	(0.927)	(0.868)	(0.831)
24	0.502	0.276	1.075	1.553	1.560*
	(1.223)	(1.148)	(1.006)	(0.958)	(0.916)
25	0.371	0.509	1.544	2.075*	2.011*
	(1.320)	(1.236)	(1.107)	(1.075)	(1.054)

Note: The sample contains women from TDHS-2013 and TDHS-2018 surveys. Intervals indicate the distance to the cutoff point. The running variable is birth month-year. The reform dummy is equal to 1 when the women's time of birth is greater than or equal to January 1987, 0 otherwise. Estimates are from the regression in the Identification Strategy section of the paper. Sample weights provided by TDHS surveys are taken into account. The standard errors are in parentheses and clustered at the birth month-year level. See Table 5.21 for observation numbers.

Table 5.9 : The reform's effect on employment experience ratio

Age (Years)	4-Years Interval	5-Years Interval	6-Years Interval	7-Years Interval	8-Years Interval
16	0.008	0.008	0.014	0.016	0.018*
	(0.016)	(0.015)	(0.013)	(0.012)	(0.011)
17	0.017	0.013	0.019	0.024	0.029**
	(0.019)	(0.019)	(0.016)	(0.015)	(0.014)
18	-0.007	-0.007	-0.000	0.008	0.011
	(0.024)	(0.023)	(0.021)	(0.019)	(0.018)
19	-0.017	-0.005	-0.009	-0.001	0.003
	(0.024)	(0.023)	(0.021)	(0.019)	(0.018)
20	-0.004	0.000	-0.011	-0.002	0.005
	(0.023)	(0.022)	(0.019)	(0.018)	(0.017)
21	-0.008	0.002	-0.004	0.002	0.013
	(0.022)	(0.022)	(0.019)	(0.018)	(0.017)
22	0.017	0.024	0.017	0.025	0.031*
	(0.023)	(0.021)	(0.019)	(0.017)	(0.016)
23	0.018	0.023	0.019	0.023	0.026
	(0.023)	(0.021)	(0.019)	(0.017)	(0.016)
24	0.015	0.015	0.015	0.020	0.020
	(0.023)	(0.021)	(0.018)	(0.017)	(0.016)
25	0.018	0.020	0.021	0.029*	0.026
	(0.021)	(0.019)	(0.018)	(0.017)	(0.016)

Note: The sample contains women from TDHS-2013 and TDHS-2018 surveys. Intervals indicate the distance to the cutoff point. The running variable is birth month-year. The reform dummy is equal to 1 when the women's time of birth is greater than or equal to January 1987, 0 otherwise. Estimates are from the regression in the Identification Strategy section of the paper. Sample weights provided by TDHS surveys are taken into account. The standard errors are in parentheses and clustered at the birth month-year level. See Table 5.21 for observation numbers.

Table 5.10 : The reform's effect on wage employment experience ratio

Age (Years)	4-Years Interval	5-Years Interval	6-Years Interval	7-Years Interval	8-Years Interval
16	0.013	0.014	0.015	0.016*	0.016**
	(0.012)	(0.011)	(0.009)	(0.009)	(0.008)
17	0.019	0.018	0.019	0.022*	0.023**
	(0.015)	(0.014)	(0.012)	(0.011)	(0.010)
18	-0.004	-0.000	0.004	0.008	0.006
	(0.020)	(0.019)	(0.017)	(0.015)	(0.014)
19	-0.017	-0.001	-0.007	-0.002	0.001
	(0.020)	(0.019)	(0.017)	(0.015)	(0.014)
20	-0.005	0.002	-0.010	-0.003	-0.000
	(0.019)	(0.018)	(0.016)	(0.015)	(0.014)
21	-0.009	0.001	-0.008	-0.007	-0.001
	(0.019)	(0.018)	(0.016)	(0.015)	(0.014)
22	0.012	0.017	0.005	0.008	0.011
	(0.019)	(0.018)	(0.016)	(0.015)	(0.014)
23	0.014	0.017	0.007	0.005	0.008
	(0.019)	(0.018)	(0.016)	(0.015)	(0.014)
24	0.012	0.010	0.004	0.004	0.005
	(0.019)	(0.017)	(0.016)	(0.015)	(0.014)
25	0.018	0.015	0.008	0.012	0.010
	(0.018)	(0.017)	(0.016)	(0.015)	(0.014)

Note: The sample contains women from TDHS-2013 and TDHS-2018 surveys. Intervals indicate the distance to the cutoff point. The running variable is birth month-year. The reform dummy is equal to 1 when the women's time of birth is greater than or equal to January 1987, 0 otherwise. Estimates are from the regression in the Identification Strategy section of the paper. Sample weights provided by TDHS surveys are taken into account. The standard errors are in parentheses and clustered at the birth month-year level. See Table 5.21 for observation numbers.

Table 5.11 : The reform's effect on nonwage employment experience ratio

Age (Years)	4-Years Interval	5-Years Interval	6-Years Interval	7-Years Interval	8-Years Interval
16	-0.005	-0.006	-0.000	-0.000	0.002
	(0.011)	(0.011)	(0.009)	(0.008)	(0.008)
17	-0.002	-0.005	0.000	0.002	0.006
	(0.013)	(0.013)	(0.011)	(0.010)	(0.009)
18	-0.003	-0.006	-0.004	0.001	0.005
	(0.012)	(0.012)	(0.011)	(0.010)	(0.009)
19	-0.000	-0.003	-0.002	0.001	0.002
	(0.012)	(0.012)	(0.011)	(0.010)	(0.009)
20	0.001	-0.002	-0.001	0.001	0.006
	(0.012)	(0.012)	(0.010)	(0.009)	(0.009)
21	0.001	0.001	0.004	0.009	0.014
	(0.011)	(0.011)	(0.010)	(0.009)	(0.009)
22	0.005	0.007	0.012	0.017**	0.020**
	(0.010)	(0.010)	(0.009)	(0.008)	(0.008)
23	0.004	0.006	0.012	0.018**	0.018**
	(0.010)	(0.009)	(0.008)	(0.008)	(0.008)
24	0.003	0.004	0.011	0.016**	0.015**
	(0.010)	(0.009)	(0.008)	(0.007)	(0.007)
25	-0.000	0.005	0.013	0.017**	0.017**
	(0.010)	(0.009)	(0.008)	(0.008)	(0.008)

Note: The sample contains women from TDHS-2013 and TDHS-2018 surveys. Intervals indicate the distance to the cutoff point. The running variable is birth month-year. The reform dummy is equal to 1 when the women's time of birth is greater than or equal to January 1987, 0 otherwise. Estimates are from the regression in the Identification Strategy section of the paper. Sample weights provided by TDHS surveys are taken into account. The standard errors are in parentheses and clustered at the birth month-year level. See Table 5.21 for observation numbers.

5.3 Why The Compulsory Schooling Law Does Not Affect Employment Prospects of Women?

The reform affected the likelihood of women's middle school graduation positively and significantly. This effect on the educational attainment of women in this sample

does not spread to higher levels of education. The positive causal impact of education on labor market outcomes may come from higher levels of education (Mammen and Paxson (2000); Cameron, Malcolm Dowling, and Worswick (2001); Aydemir and Kirdar (2017)). Thus, one possible reason is that the reform only affected middle school graduation. Table 5.12 shows that the correlation of schooling with current employment probability increases with education. For the full sample, each additional year of schooling increases current employment probability by 2.3%. For the sample restricted to women with less than 9 years of schooling, each additional year of schooling increases the probability of being currently employed by 1.2%, while for the sample restricted to women with less than 16 years of schooling, the magnitude of the effect (2%) gets closer to 2.3%. Thus, the effect of education on labor market outcomes may be coming from university graduation, which is not significantly affected by the reform.

Table 5.12 : The effect of years of schooling on current employment probability

	Full Sample	Years of Sch < 16	Years of Sch < 9
Current Employment	0.023***	0.020***	0.012***
Probability	(0.001)	(0.001)	(0.002)
Observations	17,092	16,549	10,370

Note: The sample contains women from TDHS-2013 and TDHS-2018 surveys. Intervals indicate the distance to the cutoff point. The running variable is birth month-year. The reform dummy is equal to 1 when the women's time of birth is greater than or equal to January 1987, 0 otherwise. Estimates are from the regression in the Identification Strategy section of the paper. Sample weights provided by TDHS surveys are taken into account. The standard errors are in parentheses and clustered at the birth month-year level.

Leaving a job for marriage related reasons is common in Türkiye. Marriage related reasons include getting pregnant, child care, and opposition of the husband or elderly. Hence; one other reason may be that the reform increased the probability of getting married for women or the ones treated replaced the others in the marriage market and as a result they leave or stay outside of the labor market.

As can be seen from Table 5.13, the reform increased the probability of getting married at the age of 17 and decreased at the age of 20, while we do not see any effect at other ages. Moreover, the reform increased the probability of being ever married until ages 17 to 21, as shown in Table 5.14. Thus, as an effect of the reform, women seem to get married at earlier ages.

The reform also increased women's fertility for earlier ages. The probability of giving first birth at the age of 18 increased, while it decreased at the ages of 21 and 25

(Table 5.15). The probability of ever giving birth until the ages of 19 to 21 is also increased (Table 5.16). Additionally, the number of children women have until the ages of 18 to 25 also increased (Table 5.17). Thus, the reform has a strong impact on women's marriage and fertility.

The reform does not have any effect on women's probability of being ever married at the age when the surveys were conducted (Table 5.18). Thus, it is likely that the women affected by the reform get married earlier than the others.

The reform does not also have an impact on the probability of leaving the labor market for marriage related reasons (Table 5.19). To sum up, the women affected by the reform get married and give birth at earlier ages, and they leave the labor market earlier than the other ones. If treated women wait longer to have a child than the other group, it may imply that they stay longer in the labor market than the others. However, time to first birth after marriage increased by only around 3 months due to the reform (Table 5.20).

Table 5.13 : The reform's effect on age at first marriage

Age (Years)	4-Years Interval	5-Years Interval	6-Years Interval	7-Years Interval	8-Years Interval
15	-0.001	0.004	0.001	-0.004	-0.009
	(0.012)	(0.011)	(0.010)	(0.010)	(0.010)
16	0.029*	0.025	0.026*	0.015	0.006
	(0.017)	(0.015)	(0.014)	(0.013)	(0.013)
17	0.060**	0.069***	0.054***	0.054***	0.048***
	(0.024)	(0.021)	(0.019)	(0.018)	(0.017)
18	-0.015	-0.005	0.005	0.011	-0.008
	(0.020)	(0.018)	(0.017)	(0.015)	(0.016)
19	0.033*	0.018	0.013	0.009	0.015
	(0.019)	(0.019)	(0.017)	(0.015)	(0.015)
20	-0.037**	-0.044**	-0.036**	-0.034**	-0.026*
	(0.018)	(0.017)	(0.017)	(0.016)	(0.015)
21	-0.001	0.000	-0.004	-0.015	-0.008
	(0.023)	(0.020)	(0.019)	(0.018)	(0.018)
22	-0.019	-0.015	-0.016	-0.017	-0.019
	(0.022)	(0.020)	(0.018)	(0.018)	(0.017)
23	-0.022	-0.016	-0.011	-0.009	-0.009
	(0.019)	(0.017)	(0.016)	(0.015)	(0.015)
24	-0.001	-0.007	-0.011	-0.015	-0.007
	(0.024)	(0.023)	(0.021)	(0.019)	(0.018)
25	0.033	0.022	0.022	0.024	0.022
	(0.022)	(0.019)	(0.018)	(0.017)	(0.017)

Note: The sample contains women from TDHS-2013 and TDHS-2018 surveys. Intervals indicate the distance to the cutoff point. The running variable is birth month-year. The reform dummy is equal to 1 when the women's time of birth is greater than or equal to January 1987, 0 otherwise. Estimates are from the regression in the Identification Strategy section of the paper. Sample weights provided by TDHS surveys are taken into account. The standard errors are in parentheses and clustered at the birth month-year level. See Table 5.22 for observation numbers.

Table 5.14 : The reform's effect on being ever married by age

Age (Years)	4-Years Interval	5-Years Interval	6-Years Interval	7-Years Interval	8-Years Interval
15	0.001	0.002	-0.005	-0.004	-0.001
	(0.014)	(0.012)	(0.011)	(0.010)	(0.011)
16	0.022	0.017	0.015	0.008	0.004
	(0.020)	(0.017)	(0.016)	(0.015)	(0.014)
17	0.073***	0.075***	0.065***	0.058***	0.050***
	(0.027)	(0.024)	(0.022)	(0.020)	(0.019)
18	0.076**	0.083***	0.081***	0.079***	0.062***
	(0.029)	(0.026)	(0.023)	(0.021)	(0.020)
19	0.106***	0.099***	0.092***	0.086***	0.076***
	(0.026)	(0.025)	(0.022)	(0.020)	(0.020)
20	0.075**	0.064**	0.064***	0.063***	0.056**
	(0.030)	(0.027)	(0.024)	(0.023)	(0.023)
21	0.080***	0.069***	0.064***	0.050**	0.047**
	(0.025)	(0.023)	(0.021)	(0.021)	(0.021)
22	0.062**	0.054**	0.039	0.024	0.022
	(0.030)	(0.027)	(0.025)	(0.024)	(0.023)
23	0.036	0.036	0.021	0.007	0.004
	(0.030)	(0.028)	(0.026)	(0.025)	(0.024)
24	0.016	0.019	0.007	-0.008	-0.003
	(0.033)	(0.028)	(0.026)	(0.024)	(0.023)
25	0.035	0.035	0.024	0.009	0.010
	(0.033)	(0.029)	(0.027)	(0.025)	(0.024)

Note: The sample contains women from TDHS-2013 and TDHS-2018 surveys. Intervals indicate the distance to the cutoff point. The running variable is birth month-year. The reform dummy is equal to 1 when the women's time of birth is greater than or equal to January 1987, 0 otherwise. Estimates are from the regression in the Identification Strategy section of the paper. Sample weights provided by TDHS surveys are taken into account. The standard errors are in parentheses and clustered at the birth month-year level. See Table 5.22 for observation numbers.

Table 5.15 : The reform's effect on age at first birth

Age (Years)	4-Years Interval	5-Years Interval	6-Years Interval	7-Years Interval	8-Years Interval
15	-0.007	-0.009	-0.005	-0.004	-0.002
	(0.008)	(0.006)	(0.006)	(0.005)	(0.006)
16	-0.001	0.004	0.003	0.006	0.005
	(0.010)	(0.009)	(0.008)	(0.008)	(0.007)
17	-0.001	0.006	0.004	-0.002	-0.005
	(0.010)	(0.009)	(0.009)	(0.008)	(0.008)
18	0.059***	0.046***	0.042***	0.042***	0.044***
	(0.016)	(0.015)	(0.014)	(0.013)	(0.012)
19	0.022	0.020	0.025*	0.029**	0.023*
	(0.015)	(0.014)	(0.013)	(0.013)	(0.012)
20	0.017	0.004	0.010	0.008	0.009
	(0.015)	(0.014)	(0.013)	(0.012)	(0.012)
21	-0.036**	-0.040**	-0.036**	-0.029**	-0.025*
	(0.017)	(0.016)	(0.014)	(0.014)	(0.013)
22	-0.003	0.001	-0.006	-0.017	-0.012
	(0.011)	(0.011)	(0.010)	(0.010)	(0.010)
23	0.011	0.008	-0.003	-0.011	-0.022
	(0.015)	(0.015)	(0.014)	(0.014)	(0.014)
24	-0.013	-0.006	-0.008	-0.009	-0.007
	(0.016)	(0.015)	(0.013)	(0.013)	(0.012)
25	-0.034**	-0.035**	-0.037***	-0.032**	-0.030**
	(0.016)	(0.015)	(0.014)	(0.013)	(0.012)

Note: The sample contains women from TDHS-2013 and TDHS-2018 surveys. Intervals indicate the distance to the cutoff point. The running variable is birth month-year. The reform dummy is equal to 1 when the women's time of birth is greater than or equal to January 1987, 0 otherwise. Estimates are from the regression in the Identification Strategy section of the paper. Sample weights provided by TDHS surveys are taken into account. The standard errors are in parentheses and clustered at the birth month-year level. See Table 5.22 for observation numbers.

Table 5.16 : The reform's effect on ever given birth by age

Age (Years)	4-Years Interval	5-Years Interval	6-Years Interval	7-Years Interval	8-Years Interval
15	-0.000	-0.001	-0.005	-0.002	-0.003
	(0.009)	(0.008)	(0.007)	(0.006)	(0.006)
16	-0.011	-0.012	-0.011	-0.006	-0.005
	(0.012)	(0.010)	(0.009)	(0.008)	(0.008)
17	-0.005	-0.000	-0.001	0.005	0.002
	(0.017)	(0.014)	(0.013)	(0.012)	(0.012)
18	0.031	0.033*	0.023	0.022	0.026*
	(0.020)	(0.017)	(0.017)	(0.015)	(0.015)
19	0.079***	0.071***	0.069***	0.065***	0.057***
	(0.023)	(0.021)	(0.020)	(0.018)	(0.018)
20	0.082***	0.074***	0.070***	0.072***	0.060***
	(0.028)	(0.026)	(0.023)	(0.021)	(0.020)
21	0.090***	0.068**	0.066***	0.064***	0.054**
	(0.028)	(0.026)	(0.023)	(0.023)	(0.022)
22	0.058*	0.036	0.036	0.033	0.038*
	(0.029)	(0.027)	(0.025)	(0.024)	(0.023)
23	0.061**	0.036	0.025	0.016	0.016
	(0.027)	(0.026)	(0.023)	(0.022)	(0.021)
24	0.054*	0.043	0.025	0.012	0.007
	(0.031)	(0.028)	(0.026)	(0.024)	(0.023)
25	0.033	0.026	0.016	0.002	0.006
	(0.031)	(0.028)	(0.026)	(0.025)	(0.024)

Note: The sample contains women from TDHS-2013 and TDHS-2018 surveys. Intervals indicate the distance to the cutoff point. The running variable is birth month-year. The reform dummy is equal to 1 when the women's time of birth is greater than or equal to January 1987, 0 otherwise. Estimates are from the regression in the Identification Strategy section of the paper. Sample weights provided by TDHS surveys are taken into account. The standard errors are in parentheses and clustered at the birth month-year level. See Table 5.22 for observation numbers.

Table 5.17 : The reform's effect on number of children by age

Age (Years)	4-Years Interval	5-Years Interval	6-Years Interval	7-Years Interval	8-Years Interval
15	0.004	0.001	0.001	0.002	0.002
	(0.010)	(0.008)	(0.008)	(0.007)	(0.007)
16	0.003	0.007	0.003	0.007	0.006
	(0.018)	(0.015)	(0.014)	(0.012)	(0.012)
17	0.004	0.015	0.007	0.005	0.005
	(0.022)	(0.018)	(0.017)	(0.016)	(0.015)
18	0.065**	0.061**	0.045*	0.047**	0.043**
	(0.031)	(0.027)	(0.025)	(0.022)	(0.021)
19	0.095**	0.086***	0.073**	0.073***	0.066**
	(0.037)	(0.033)	(0.030)	(0.027)	(0.026)
20	0.139***	0.114***	0.102***	0.105***	0.089***
	(0.044)	(0.039)	(0.035)	(0.032)	(0.032)
21	0.119**	0.089*	0.084**	0.087**	0.079**
	(0.053)	(0.046)	(0.041)	(0.039)	(0.039)
22	0.168***	0.137***	0.106**	0.099**	0.091**
	(0.058)	(0.052)	(0.050)	(0.045)	(0.044)
23	0.172***	0.121**	0.084*	0.068	0.050
	(0.058)	(0.054)	(0.049)	(0.045)	(0.043)
24	0.194***	0.146**	0.100*	0.087*	0.086*
	(0.066)	(0.058)	(0.055)	(0.049)	(0.046)
25	0.198***	0.157**	0.111*	0.110*	0.107*
	(0.070)	(0.063)	(0.061)	(0.056)	(0.054)

Note: The sample contains women from TDHS-2013 and TDHS-2018 surveys. Intervals indicate the distance to the cutoff point. The running variable is birth month-year. The reform dummy is equal to 1 when the women's time of birth is greater than or equal to January 1987, 0 otherwise. Estimates are from the regression in the Identification Strategy section of the paper. Sample weights provided by TDHS surveys are taken into account. The standard errors are in parentheses and clustered at the birth month-year level. See Table 5.22 for observation numbers.

Table 5.18 : The reform's effect on being ever married

	4-Years Interval	5-Years Interval	6-Years Interval	7-Years Interval	8-Years Interval
Ever Married	-0.012	-0.015	-0.004	-0.015	-0.004
	(0.032)	(0.028)	(0.026)	(0.023)	(0.022)
Observations	4,095	5,123	6,185	7,211	8,198

Note: The sample contains women from TDHS-2013 and TDHS-2018 surveys. Intervals indicate the distance to the cutoff point. The running variable is birth month-year. The reform dummy is equal to 1 when the women's time of birth is greater than or equal to January 1987, 0 otherwise. Estimates are from the regression in the Identification Strategy section of the paper. Sample weights provided by TDHS surveys are taken into account. The standard errors are in parentheses and clustered at the birth month-year level.

Table 5.19 : The reform's effect on leaving job for marriage related reasons

	4-Years Interval	5-Years Interval	6-Years Interval	7-Years Interval	8-Years Interval
1.treatment	-0.011	-0.030	-0.046	-0.045	-0.041
	(0.044)	(0.038)	(0.034)	(0.033)	(0.032)
Observations	2,377	2,964	3,537	4,091	4,590

Note: The sample contains women from TDHS-2013 and TDHS-2018 surveys. Intervals indicate the distance to the cutoff point. The running variable is birth month-year. The reform dummy is equal to 1 when the women's time of birth is greater than or equal to January 1987, 0 otherwise. Estimates are from the regression in the Identification Strategy section of the paper. Sample weights provided by TDHS surveys are taken into account. The standard errors are in parentheses and clustered at the birth month-year level.

Table 5.20 : The reform's effect on time to first birth after marriage

	4-Years Interval	5-Years Interval	6-Years Interval	7-Years Interval	8-Years Interval
1.treatment	3.625***	3.845***	2.857**	1.913*	1.143
	(1.313)	(1.186)	(1.109)	(1.062)	(1.019)
Observations	3,079	3,819	4,560	5,240	5,829

Note: The sample contains women from TDHS-2013 and TDHS-2018 surveys. Intervals indicate the distance to the cutoff point. The running variable is birth month-year. The reform dummy is equal to 1 when the women's time of birth is greater than or equal to January 1987, 0 otherwise. Estimates are from the regression in the Identification Strategy section of the paper. Sample weights provided by TDHS surveys are taken into account. The standard errors are in parentheses and clustered at the birth month-year level.

Table 5.21 : Observation numbers

Age (Years)	4-Years Interval	5-Years Interval	6-Years Interval	7-Years Interval	8-Years Interval
15	2418	3037	3675	4308	4857
16	2456	3088	3739	4384	4949
17	3110	3836	4589	5330	6004
18	3241	4025	4853	5679	6489
19	3440	4270	5143	6016	6862
20	3513	4392	5331	6226	6824
21	3925	4876	5829	6544	7192
22	4039	5024	5792	6556	7244
23	4053	4808	5581	6355	7046
24	3785	4542	5323	6099	6775
25	3494	4251	5033	5793	6280

Note: The sample contains women from TDHS-2013 and TDHS-2018 surveys. Intervals indicate the distance to the cutoff point.

Table 5.22 : Observation numbers

Age (Years)	4-Years Interval	5-Years Interval	6-Years Interval	7-Years Interval	8-Years Interval
15	4095	5123	6185	7211	8198
16	4095	5123	6185	7211	8198
17	4095	5123	6185	7211	8198
18	4095	5123	6185	7211	8198
19	4095	5123	6185	7211	8172
20	4095	5123	6185	7192	7887
21	4095	5123	6139	6918	7613
22	4095	5091	5875	6654	7349
23	4070	4828	5612	6391	7086
24	3790	4548	5332	6111	6788
25	3498	4256	5040	5803	6291

Note: The sample contains women from TDHS-2013 and TDHS-2018 surveys. Intervals indicate the distance to the cutoff point.

6. ROBUSTNESS CHECKS

In this study, for regression discontinuity design to be valid, there shouldn't be any discontinuous jump at the cutoff point in control variables and background characteristics of women. Table 6.1 shows the result of the search on the discontinuity. I searched for discontinuities for 42 variables, and find that only two variables have a discontinuous jump at the cutoff.

Table 6.1 : Search for discontinuities at the cutoff for background characteristics

Variables	Treatment Effect	Standard Errors	Variables	Treatment Effect	Standard Errors
Month of Birth:			NUTS-2 Region of Birth:		
January	0.086	0.109	Istanbul	-0.040**	0.018
February	0.023	0.074	Tekirdag	-0.007	0.005
March	0.017	0.087	Balikesir	-0.001	0.006
April	0.014	0.083	Izmir	0.011	0.012
May	-0.003	0.077	Aydin	-0.003	0.008
June	0.013	0.072	Manisa	0.008	0.012
July	-0.011	0.075	Bursa	-0.004	0.010
August	-0.033	0.088	Kocaeli	-0.010	0.010
September	-0.007	0.083	Ankara	0.003	0.015
October	-0.024	0.094	Konya	-0.004	0.010
November	-0.030	0.066	Antalya	0.004	0.005
December	-0.045	0.065	Adana	-0.003	0.009
Parents' Educational Level:			Hatay	0.011	0.011
Father Sec. School Grad.	0.016	0.017	Kirikkale	0.015	0.010
Mother Sec. School Grad.	-0.004	0.025	Kayseri	-0.007	0.011
Place of Birth:			Zonguldak	0.012	0.007
Rural	0.022	0.027	Kastamonu	0.001	0.008
			Samsun	-0.005	0.013
			Trabzon	0.024**	0.010
			Erzurum	0.003	0.008
			Agri	-0.006	0.008
			Malatya	-0.002	0.008
			Van	-0.002	0.010
			Gaziantep	-0.006	0.009
			Sanliurfa	-0.004	0.011
			Mardin	0.011	0.012
			Abroad	0.002	0.009

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

RDD requires that the variation in the treatment variable is randomized. If women

manipulate their date of birth to be on the left or the right of the cutoff (to be
treated or not treated), then the treatment variable is not randomized. Regarding
this study, this is not likely. The reform was announced a couple of years before
the year 1997, and the 1987 birth cohort is the first cohort affected by the reform.
Thus, women or their parents could not manipulate the date of birth. To make sure
that there is no manipulation in the running variable, which is the birth month-year
of women, I do the manipulation test raised by Cattaneo, Jansson, and Ma (2018).
In this test, the null hypothesis is no manipulation in the running variable. I do
the test on 8 years interval sample. Since the p-value of the test is 0.24, the null
hypothesis is not rejected. Thus, the treatment variable is randomized around the
cutoff. Figure 6.1 is the plot of the test.

Figure 6.1 : Cattaneo Jansson Ma Manipulation Test
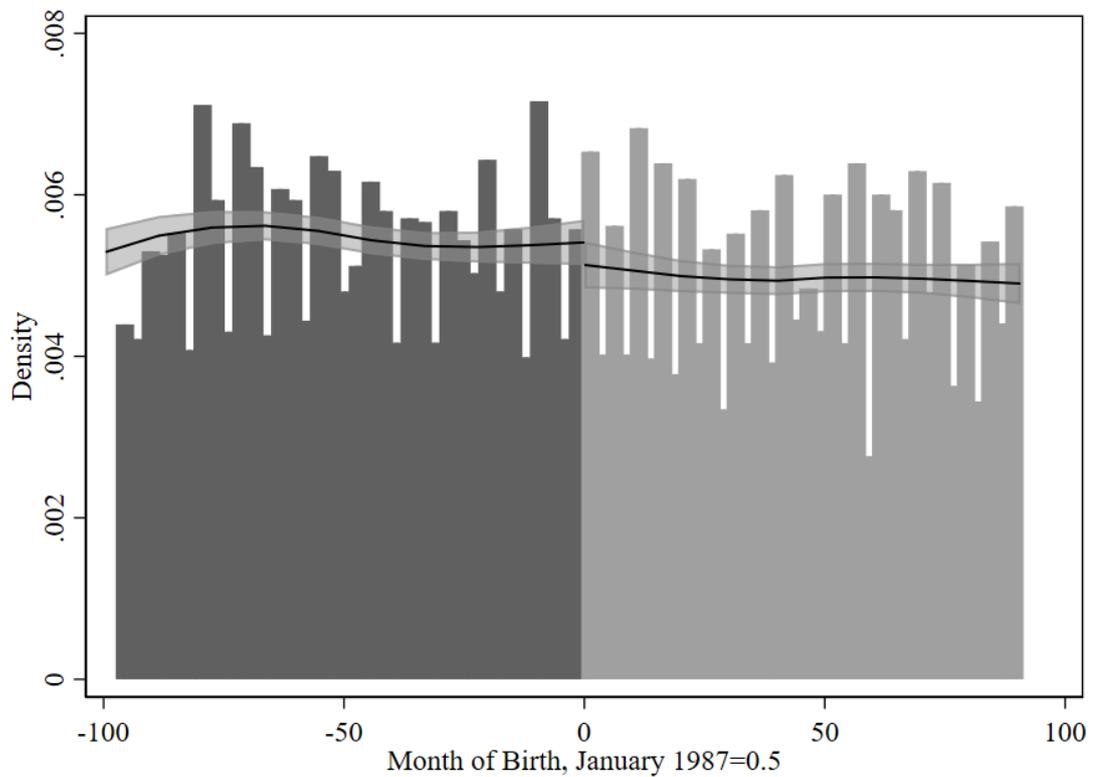
7. CONCLUSION

In this study, I estimate the causal effect of the 1997 CSL in Türkiye, which increased compulsory schooling from 5 to 8 years, on women's labor and marriage market outcomes such as employment experience, employment experience ratio, being ever employed, age at first marriage, age at first birth, number of children, etc. I find no effect on the labor market experience of women. On the other hand, the reform increased the likelihood of being ever married by ages 17 to 21, ever given birth by ages 19 to 21, and the number of children by ages 18 to 22. Moreover, the reform increased the time to first birth after marriage by around 3 months.

The reform affected only the middle school graduation of women in this sample, while higher levels of education are associated with labor market improvements (Aydemir and Kirdar (2017); Mammen and Paxson (2000); Cameron, Malcolm Dowling, and Worswick (2001)). This implies that the effect of the reform on human capital that is valued by the labor market is low. Moreover, treated women get married and give birth at earlier ages, which can be interpreted as the reform increased their value in the marriage market. The reform might have had a positive effect on marriage probability, or treated ones replaced the others in the marriage market at earlier ages. The reform does not have any impact on the common practice of leaving the labor market for marriage related reasons for women. Since women get married and have more children at earlier ages, the human capital improvements that are relevant to the labor market may be compensated by leaving their jobs for marriage related reasons. This results in a null effect of the reform on labor market outcomes.

With a new round of TDHS, it will be possible to observe the women affected by the reform at older ages. In this way, their labor market decisions after marriage can be analyzed.

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