The Effects of Compulsory Military Service Exemption on Education and Labor Market Outcomes: Evidence from a Natural Experiment

Huzeyfe Torun * Semih Tumen **

* Central Bank of the Republic of Turkey

** Central Bank of the Republic of Turkey and IZA

October 16, 2015

Outline

- Introduction.
- Motivation and literature.
- Data and empirical design.
- Results and discussion.
- Caveats and issues under revision.
- Conclusion.

Introduction I

- Compulsory military service (CMS) imposes certain restrictions on the education and employment decisions of young men. This is especially a concern for the countries in which the duration of service is typically long—such as Turkey.

- There is a reviving interest in understanding the impact of CMS on education and labor market outcomes. Research on CMS is useful for policy, because there is an ongoing debate about the costs and benefits of replacing the CMS with a voluntary enrollment system.

- Costs: human capital depreciation, foregone labor market experience, foregone earnings. Benefits: unique opportunities to equip individuals with valuable technical skills and discipline that may lead to increased productivity in civilian life.

Introduction II

- Besides its effect on labor market outcomes, CMS may indirectly affect educational attainment of individuals.

- In most countries, military service is delayed for the ones who are enrolled in school. Therefore, individuals may attain higher education to avoid or postpone their military service. College graduates serve in the army under better conditions. These factors may push education level up among males. Increased education may, in turn, raise earnings capacity.

- Overall, the net impact on education and earnings is ambiguous. The empirical evidence is also mixed with some studies suggesting that abolishing CMS can have positive effects on education and labor market outcomes, while others reporting zero or negative effects.

Motivation I

- We study the impact of a law—enacted on November 1999—offering the option to benefit from a one-time paid exemption from the CMS in Turkey.

- Males born on or before December 31st 1972—27 years old and above at the time of the reform—are the eligible group, while the ones born on or after January 1st 1973 are ineligible. The amount of the required payment is 15,000 Deutschmark (around 8000 USD)—20,000 Deutschmark for males above 40 years old. Around 100,000 individuals have benefited from the reform.

- The timing of the reform is purely exogenous, because the main motivation behind the reform is to partially compensate the deficit due to the devastating earthquake that took place in Izmit—a province close to Istanbul—on August 1999.

Motivation II

- Based on this reform, a male born on December 31st 1972 is offered the option to relax his military service constraints in exchange for some cash, while another one born 24 hours later is not offered the same option.
- The duration of CMS, which was 9–18 months at the time of the reform, increases the appeal of the paid exemption option.
- This natural experiment enables us to empirically assess whether the education and labor market outcomes of the ones in the treatment group differ from the outcomes of those in the control group.

Literature I

- Papers in this literature can be grouped under two categories based on their main outcome of interest: (*i*) studies focusing on wage and employment outcomes and (*ii*) those focusing on educational outcomes.
- Papers in the first category estimate the impact of both peacetime and wartime military conscription on civilian wage and employment outcomes. The results, however, are mixed and there is no consensus in the literature about the impact of CMS on wage and employment outcomes.
- Using the draft lottery for the Vietnam War as a natural experiment, Angrist (1990, *AER*) shows that veteran status has reduced civilian earnings in the US. Subsequent studies find that earnings gap between veterans and non-veterans has diminished quickly over time (Angrist and Chen 2011, *AEJApplied*, Angrist et al 2011, *AER*).

Literature II

- Imbens and van der Klaauw (1995, *JBES*) find that conscription in the Netherlands is associated with around a 5 percent loss in annual earnings relative to those who did not serve in the military and this result persists even after correcting for potential channels of selectivity.
- Bauer et al (2012, *EER*) show using a regression discontinuity design that CMS has virtually zero effects on labor market outcomes in Germany. A similar result is documented by Grenet et al (2011, *LE*) using British data.
- Angrist and Krueger (1994, *JOLE*) report that the World War II veterans earn no more than non-veterans. Card and Cardoso (2009, *AEJApplied*) find using data from Portugal that peacetime conscription has a positive effect on the labor market outcomes of low-educated males, while its effect on better-educated males is nil.

Literature III

- Papers in the second category investigate the role of compulsory military service in changing the schooling decisions of individuals. Card and Lemieux (2001, *AER*) find that draft avoidance behavior raised college attendance rates by 4-6 percentage points in the United states in late 1960s.

- Maurin and Xenogiani (2007, *JHR*) document that the reform abolishing compulsory conscription in France has reduced time spent in school among males. They argue that compulsory conscription provides incentives for males to spend extra time in school, which, in turn, leads to increased earnings potential.

- DiPietro (2013, *JPopE*) shows, on the other hand, that abolishing compulsory military service in Italy did not have any effect on college enrollment rates.

Data

- We use the 2004–2013 waves of the HLFS conducted by TurkStat. Nationally-representative, and publicly-available data set. It is the main data source for the national labor force and employment statistics for Turkey.
- To distinguish between those who were affected by the law and those who were not, we obtained additional files (not publicly available), on the y-o-b and m-o-b of respondents and merged them with the original data. The age variable would be an inaccurate measure to define eligibility.
- The reform affected those who were born on or before Dec 31, 1972. We restrict our sample to males born around the cutoff date. Males born in 1972 were at the age of 32 in 2004 and 41 in 2013. So the sample consists of prime age males and females who have mostly completed their schooling decisions. Sample size: 549,972.

Empirical Design I

- We do not observe who have actually benefited from the reform and who have not. We observe the birth dates of the survey respondents as month-year pairs and we are only able to distinguish between the eligible versus ineligible males.
- Think of a narrowly defined birth-date interval centered around the cutoff date. There exist males who have deferred their military obligations both on the left- and right-hand sides of the cutoff date. Part of the males born before the cutoff date have chosen to benefit from the exemption.
- Although the treatment is randomly assigned, not everyone in the treatment group has benefited from the reform. Our design is based on the initial assignment and not on the treatment eventually received. Our estimates should be interpreted as the "intention-to-treat" (ITT) effects, since there is imperfect compliance within the treatment group.

Empirical Design II

- The ITT estimation is often regarded in the program evaluation literature as a solution to the imperfect compliance problem. ITT analysis strictly depends on the randomized treatment assignment and ignores all sorts of non-compliance in the post-protocol period. Because of this feature, it is sometimes described with the phrase "once randomized, always analyzed."
- The ITT effect also tends to be smaller than the true average treatment effect (i.e., it likely underestimates the true causal effect), because of imperfect compliance (Angrist and Pischke 2008).
- Thus, although the ITT can be regarded as a lower-bound estimate of the impact, it is more policy relevant than the average treatment effect parameter in the empirical analysis of voluntary programs (Bloom 2008).

Empirical Design: Visual Representation

	Age variation within and across survey years																	
Survey Year	27/28	28/29	29/30	30/31	31/32	32/33	33/34	34/35	35/36	36/37	37/38	38/39	39/40	40/41	41/42	42/43	43/44	
2004	1976	1975	1974	1973	1972	1971	1970	1969										**Sliding Year-of-birth Windows**
2005		1976	1975	1974	1973	1972	1971	1970	1969									
2006			1976	1975	1974	1973	1972	1971	1970	1969								
2007				1976	1975	1974	1973	1972	1971	1970	1969							
2008					1976	1975	1974	1973	1972	1971	1970	1969						
2009						1976	1975	1974	1973	1972	1971	1970	1969					
2010							1976	1975	1974	1973	1972	1971	1970	1969				
2011								1976	1975	1974	1973	1972	1971	1970	1969			
2012									1976	1975	1974	1973	1972	1971	1970	1969		
2013										1976	1975	1974	1973	1972	1971	1970	1969	

Size 1
Size 2
Size 3
Size 4

Empirical Design III

- We try OLS, DID, and triple difference. Given that we have a sharp cutoff date, it sounds natural to try a RDD to identify the impact of the reform on the outcomes of interest. However, we avoid RDD based on an important observation.
- The cutoff date separates Dec and Jan. It is well-known that education and labor income is correlated with season of birth not only through the potential interactions between season of birth and compulsory schooling laws [see, e.g., Angrist and Krueger 1991, *QJE*], but also through the fact that children born toward the end of the year are much more likely to have wealthier and better-educated parents than children born early in the year [Buckles and Hungerman 2013, *ReStat*].
- The cutoff date accidentally captures the family background effects; therefore, RDD will likely suffer from large biases due to the season-of-birth effects.

Simple Regression Controlling the Y-o-b Trends

$$y_{i,r,t,m,s} = \alpha + \delta \cdot B_i + \boldsymbol{\theta}' \cdot \mathbf{X}_i + g(t) + f_r + f_s + f_m + \epsilon_{i,r,t,s,m},$$

where i, r, t, m, and s index individuals, regions, years of birth, months of birth, and survey years, respectively, y is the labor market outcome of interest, B is a dummy variable taking 1 if the individual is born on or before December 31st and 0 after December 31st, $\mathbf{X}$ is a vector of individual-level characteristics, $g(t)$ is a polynomial defining the time trend variable with respect to the year of birth, f_r denotes region fixed effects, f_s denotes survey-year fixed effects, f_m denotes month-of-birth fixed effects, and ϵ is an error term. The vector of individual-level characteristics, $\mathbf{X}$, includes a full set of age dummies and an urban/rural dummy.

Difference-in-Differences

$$y_{i,r,m,s} = \alpha + \beta \cdot T_i + \delta \cdot B_i \times T_i + \boldsymbol{\theta}' \cdot \mathbf{X}_i + f_r + f_s + f_m + \epsilon_{i,r,s,m},$$

where the dummy variable T takes the value 1 if the year-of-birth period is 1972–1973 and 0 if it is 1973–1974. Note that the variable B is omitted from the regression since we also include the month-of-birth dummies. Window size varies.

Triple Difference

$$y_{i,r,m,s} = \alpha + \psi \cdot M_i + \beta \cdot T_i + \xi \cdot T_i \times M_i + \phi \cdot B_i \times M_i + \gamma \cdot B_i \times T_i + \delta \cdot B_i \times T_i \times M_i + \boldsymbol{\theta}' \cdot \mathbf{X}_i + f_r + f_s + f_m + \epsilon_{i,r,s,m},$$

where M is a dummy variable taking 1 if the individual is male and 0 if female. Window size varies.

Concluding Remarks

- We find that the paid exemption reform reduces the educational attainment for the eligible men. In particular, it reduces the probability of receiving a college degree or above. This suggests that compulsory military service provides incentives to stay enrolled in college.
- There is a suggestive decline in the labor market earnings of eligible men. We conjecture that the decline in earnings is associated with the decline in educational attainment.
- Taken at face value, our findings suggest that removing the compulsory service in Turkey will likely reduce educational attainment for those who stay enrolled to defer their military obligation. This is in line with Maurin and Xenogiani (2007, *JHR*), who show that the abolition of compulsory military service in France led to a reduction in educational attainment among males and, consequently, in earnings.

Caveats and Issues Under Revision

- Males enrolled in college in the age interval of 24–29 may exhibit a non-standard educational behavior—in comparison to the majority of the males attending college. The results should be interpreted with this point in mind. Pictures/figures will be included.

- We are thinking on the possibility of having a specific RDD exercise (larger window, trend defined over months, m-o-b controls are included).

- Conditional on observing common trends, we plan to include an additional DID exercise for men and women only (droping the time dimension in the triple difference).

- We plan to include a second step (use the reform as an IV to estimate the returns to schooling).

SCHOOL ATTAINMENT

Year-of-birth window	**1972-73**	**1971-74**	**1970-75**	**1969-76**	**1972-73**	**1971-74**	**1970-75**	**1969-76**
Outcome	**Years of Schooling**				**College and Above**			
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
Treatment	-0.2605***	-0.2295***	-0.2873***	-0.0977**	-0.0169***	-0.0130***	-0.0086*	-0.0096***
	(0.0455)	(0.0437)	(0.0512)	(0.0406)	(0.0045)	(0.0043)	(0.0050)	(0.0036)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Y-o-b trends	No	Yes	Yes	Yes	No	Yes	Yes	Yes
M-o-b fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Survey-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region-of-residence fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.076	0.069	0.072	0.073	0.040	0.035	0.036	0.035
# of Obs.	67,098	134,922	199,955	264,303	67,098	134,922	199,955	264,303
Means (control group)	8.2938	8.3132	8.3966	8.4657	0.1524	0.1542	0.1588	0.1613

Table 2: **School Attainment.** ***, **, and * refer to 1%, 5%, and 10% significance levels, respectively. Y-o-b and M-o-b correspond to year of birth and month of birth, respectively. Robust standard errors are reported in parentheses. The regressions are performed only for males. Controls include a full set of age dummies and an urban/rural dummy. The dependent variable in columns [1]–[4] is the total years of completed schooling. The dependent variable in columns [5]–[8] is a dummy variable taking 1 if the individual has a college degree (and above) and 0 otherwise. We use a cubic polynomial to capture the Y-o-b trends.

SCHOOL ATTAINMENT – FEMALES

Year-of-birth window	**1972-73**	**1971-74**	**1970-75**	**1969-76**	**1972-73**	**1971-74**	**1970-75**	**1969-76**
Level of Schooling	**All**				**College and Above**			
	[1]	**[2]**	**[3]**	**[4]**	**[5]**	**[6]**	**[7]**	**[8]**
Treatment	0.0244	0.0517	0.0291	0.0504	-0.0004	0.0018	0.0014	0.0018
	(0.0383)	(0.0366)	(0.0434)	(0.0341)	(0.0034)	(0.0033)	(0.0039)	(0.0030)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Y-o-b trends	No	Yes	Yes	Yes	No	Yes	Yes	Yes
M-o-b fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Survey-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.082	0.083	0.085	0.089	0.040	0.037	0.038	0.040
# of Obs.	74,090	147,504	217,945	285,669	74,090	147,504	217,945	285,669
Means (control group)	6.8931	6.9378	7.0299	7.1357	0.0909	0.0937	0.0988	0.1043

Table 7: **Robustness Check – School Attainment Outcomes for Females.** ***, **, and * refer to 1%, 5%, and 10% significance levels, respectively. Robust standard errors are reported in parentheses. The regressions are performed only for the females. Controls include a full set of age dummies and an urban/rural dummy. The dependent variable in columns [1]–[4] is the total years of completed schooling. The dependent variable in columns [5]–[8] is a dummy variable taking 1 if the individual has a college degree (and above) and 0 otherwise. We use a cubic polynomial to capture the Y-o-b trends.

LOG REAL EARNINGS

Year-of-birth window	**1972-73**	**1971-74**	**1970-75**	**1969-76**	**1972-73**	**1971-74**	**1970-75**	**1969-76**
Level of Schooling	**All**				**High School and Above**			
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
Treatment	-0.0472***	-0.0185**	-0.0246**	-0.0031	-0.0413**	0.0097	0.0086	0.0072
	(0.0091)	(0.0088)	(0.0103)	(0.0081)	(0.0144)	(0.0141)	(0.0165)	(0.0130)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Y-o-b trends	No	Yes	Yes	Yes	No	Yes	Yes	Yes
M-o-b fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Survey-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region-of-residence fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.104	0.093	0.094	0.097	0.098	0.092	0.097	0.106
# of Obs.	38,632	77,960	115,764	152,902	17,463	35,626	52,979	70,772
Means (control group)	6.3909	6.3802	6.3755	6.3649	6.7008	6.6787	6.6597	6.6324

Table 3: **Log Real Earnings.** ***, **, and * refer to 1%, 5%, and 10% significance levels, respectively. Y-o-b and M-o-b correspond to year of birth and month of birth, respectively. Robust standard errors are reported in parentheses. The regressions are performed only for the males. Controls include a full set of age dummies and an urban/rural dummy. The earnings refer to monthly earnings. Nominal monthly earnings are deflated—taking 2004 as the base year—with CPI to obtain real monthly earnings. The sample in columns [5]–[8] is restricted to those with a high school degree and above. We use a cubic polynomial to capture the Y-o-b trends.

LOG REAL EARNINGS – FEMALES

Year-of-birth window	**1972-73**	**1971-74**	**1970-75**	**1969-76**	**1972-73**	**1971-74**	**1970-75**	**1969-76**
Level of Schooling	**All**				**High School and Above**			
	[1]	**[2]**	**[3]**	**[4]**	**[5]**	**[6]**	**[7]**	**[8]**
Treatment	0.0046	0.0075	-0.0082	0.0309	-0.0301	0.0082	0.0056	0.0086
	(0.0215)	(0.0210)	(0.0247)	(0.0194)	(0.0208)	(0.0205)	(0.0242)	(0.0189)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Y-o-b trends	No	Yes	Yes	Yes	No	Yes	Yes	Yes
M-o-b fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Survey-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.088	0.084	0.083	0.081	0.086	0.073	0.074	0.079
# of Obs.	11,448	23,057	34,364		6,740	13,699	20,824	28,007
Means (control group)	6.2748	6.2810	6.3000	6.3033	6.6684	6.6627	6.6533	6.6350

Table 8: **Robustness Check – Log Real Earnings for Females.** ***, **, and * refer to 1%, 5%, and 10% significance levels, respectively. Robust standard errors are reported in parentheses. The regressions are performed only for the females. Controls include a full set of age dummies and an urban/rural dummy. The earnings refer to monthly earnings. Nominal monthly earnings are deflated—taking 2004 as the base year—with CPI to obtain real monthly earnings. We use a cubic polynomial to capture the Y-o-b trends. The sample in columns [5]–[8] is restricted to those with a high school degree and above.

PLACEBO TREATMENT DATES (Upper panel: Dec 31, 1977 – Lower Panel: Dec 31, 1978)

Year-of-birth window	**1977-78**	**1976-79**	**1975-80**	**1974-81**	**1977-78**	**1976-79**	**1975-80**	**1974-81**
Outcome	**Years of Schooling**				**Log Real Earnings**			
	[1]	**[2]**	**[3]**	**[4]**	**[5]**	**[6]**	**[7]**	**[8]**
Treatment	-0.0130	0.0128	0.0262	-0.0042	-0.0283***	-0.0076	-0.0094	-0.0089
	(0.0455)	(0.0442)	(0.0522)	(0.0407)	(0.0080)	(0.0078)	(0.0092)	(0.0072)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Y-o-b trends	No	Yes	Yes	Yes	No	Yes	Yes	Yes
M-o-b fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Survey-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.063	0.062	0.061	0.064	0.120	0.125	0.129	0.013
# of Obs.	67,952	135,920	204,438	275,716	40,595	81,232	121,823	163,328
Year-of-birth window	**1978-79**	**1977-80**	**1976-81**	**1975-82**	**1978-79**	**1977-80**	**1976-81**	**1975-82**
Treatment	-0.0547	0.0539	0.0536	0.0552	-0.0138*	0.0185	0.0189	0.0153
	(0.0444)	(0.0431)	(0.0513)	(0.0398)	(0.0077)	(0.0175)	(0.0121)	(0.0119)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Y-o-b trends	No	Yes	Yes	Yes	No	Yes	Yes	Yes
M-o-b fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Survey-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.062	0.060	0.060	0.061	0.137	0.137	0.142	0.148
# of Obs.	69,541	139,770	209,003	275,294	41,319	82,759	123,546	161,601

Table 9: **Placebo Treatment Dates.** ***, **, and * refer to 1%, 5%, and 10% significance levels, respectively. Robust standard errors are reported in parentheses. Controls include a full set of age dummies and an urban/rural dummy. The earnings refer to monthly earnings. Nominal monthly earnings are deflated—taking 2004 as the base year—with CPI to obtain real monthly earnings. We use a cubic polynomial to capture the Y-o-b trends.

SCHOOL ATTAINMENT (DIFFERENCE IN DIFFERENCES)

Window size	**8 months**	**10 months**	**12 months**	**8 months**	**10 months**	**12 months**
Outcome	**Year of Schooling**			**College and Above**		
	[1]	**[2]**	**[3]**	**[4]**	**[5]**	**[6]**
Treatment	-0.1869***	-0.1293**	-0.1705***	-0.0180***	-0.0103*	-0.0122**
	(0.0727)	(0.0651)	(0.0578)	(0.0073)	(0.0065)	(0.0058)
Reform year	-0.0032	-0.0550	-0.0300	-0.0091	-0.0082	-0.0079
	(0.0658)	(0.0567)	(0.0502)	(0.0065)	(0.0056)	(0.0049)
Reform period	0.9283***	0.9027***	0.9362***	0.0508***	0.0499***	0.0518***
	(0.0776)	(0.0759)	(0.0745)	(0.0078)	(0.0076)	(0.0074)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
M-o-b fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Survey-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Region fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.066	0.068	0.069	0.035	0.037	0.036
# of Obs.	47,722	57,894	69,343	47,722	57,894	69,343

Table 10: **School Attainment (Difference in Differences).** ***, **, and * refer to 1%, 5%, and 10% significance levels, respectively. Robust standard errors are reported in parentheses. The regressions are performed only for the males. Controls include a full set of age dummies and an urban/rural dummy. The dependent variable in columns [1]–[3] is the total years of completed schooling. The dependent variable in columns [4]–[6] is a dummy variable taking 1 if the individual has a college degree (and above) and 0 otherwise.

SCHOOL ATTAINMENT – FEMALES (DIFFERENCE IN DIFFERENCES)

Window size	**8 months**	**10 months**	**12 months**	**8 months**	**10 months**	**12 months**
Outcome	**Year of Schooling**			**College and Above**		
	[1]	**[2]**	**[3]**	**[4]**	**[5]**	**[6]**
Treatment	0.0119	0.0340	0.0665	-0.0041	-0.0055	0.0008
	(0.0618)	(0.0557)	(0.0486)	(0.0056)	(0.0051)	(0.0044)
Reform year	-0.0040	0.0081	0.0234	0.0024	0.0023	0.0037
	(0.0550)	(0.0473)	(0.0415)	(0.0049)	(0.0042)	(0.0037)
Reform period	0.8010***	0.7930***	0.7789***	0.0463***	0.0475***	0.0446***
	(0.0687)	(0.0673)	(0.0657)	(0.0063)	(0.0062)	(0.0060)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
M-o-b fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Survey-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Region fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.082	0.080	0.081	0.038	0.037	0.037
# of Obs.	52,388	63,519	76,775	52,388	63,519	76,775

Table 12: **Robustness Check (Difference in Differences) – School Attainment Outcomes for Females.** ***, **, and * refer to 1%, 5%, and 10% significance levels, respectively. Robust standard errors are reported in parentheses. The regressions are performed only for the females. Controls include a full set of age dummies and an urban/rural dummy. The dependent variable in columns [1]–[3] is the total years of completed schooling. The dependent variable in columns [4]–[6] is a dummy variable taking 1 if the individual has a college degree (and above) and 0 otherwise.

LOG REAL EARNINGS (DIFFERENCE IN DIFFERENCES)

Window size	**8 months**	**10 months**	**12 months**	**8 months**	**10 months**	**12 months**
Level of Schooling	**All**			**College and Above**		
	[1]	**[2]**	**[3]**	**[4]**	**[5]**	**[6]**
Treatment	-0.0085	-0.0081	-0.0164	-0.0047	-0.0136	-0.0331*
	(0.0142)	(0.0127)	(0.0114)	(0.0247)	(0.0222)	(0.0201)
Reform year	-0.0325**	-0.0416***	-0.0337***	0.0186	-0.0093	-0.0050
	(0.0129)	(0.0111)	(0.0098)	(0.0228)	(0.0198)	(0.0174)
Reform period	0.0745***	0.0743***	0.0796***	0.0413	0.0451	0.0558**
	(0.0155)	(0.0152)	(0.0150)	(0.0295)	(0.0290)	(0.0284)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
M-o-b fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Survey-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Region fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.097	0.102	0.099	0.133	0.123	0.127
# of Obs.	27,915	33,908	40,412	5,842	7,330	8,731

Table 11: **Log Real Earnings (Difference in Differences).** ***, **, and * refer to 1%, 5%, and 10% significance levels, respectively. Robust standard errors are reported in parentheses. The regressions are performed only for the males. Controls include a full set of age dummies and an urban/rural dummy. The earnings refer to monthly earnings. Nominal monthly earnings are deflated—taking 2004 as the base year—with CPI to obtain real monthly earnings. The sample in columns [4]–[6] is restricted to those with a college degree and above.

LOG REAL EARNINGS – FEMALES (DIFFERENCE IN DIFFERENCES)

Window size	**8 months**	**10 months**	**12 months**	**8 months**	**10 months**	**12 months**
Level of Schooling	**All**			**College and Above**		
	[1]	[2]	[3]	[4]	[5]	[6]
Treatment	0.0307	0.0135	0.0340	-0.0055	-0.0123	0.0256
	(0.0337)	(0.0302)	(0.0267)	(0.0315)	(0.0280)	(0.0254)
Reform year	-0.0157	-0.0228	-0.0216	-0.0380	-0.0213	-0.0477**
	(0.0320)	(0.0274)	(0.0240)	(0.0293)	(0.0255)	(0.0237)
Reform period	0.1574***	0.1626***	0.1571***	0.0058	-0.0004	-0.0164
	(0.0369)	(0.0360)	(0.0353)	(0.0354)	(0.0342)	(0.0338)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
M-o-b fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Survey-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Region fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.084	0.082	0.080	0.131	0.125	0.118
# of Obs.	7,867	9,718	11,802	3,179	3,909	4,765

Table 13: **Robustness Check (Difference in Differences) – Log Real Earnings for Females.** ***, **, and * refer to 1%, 5%, and 10% significance levels, respectively. Robust standard errors are reported in parentheses. The regressions are performed only for the females. Controls include a full set of age dummies and an urban/rural dummy. The earnings refer to monthly earnings. Nominal monthly earnings are deflated—taking 2004 as the base year—with CPI to obtain real monthly earnings. The sample in columns [4]–[6] is restricted to those with a college degree and above.

SCHOOL ATTAINMENT – TRIPLE DIFFERENCE

Window size	**8 months**	**10 months**	**12 months**	**8 months**	**10 months**	**12 months**
Outcome	**Year of Schooling**			**College and Above**		
	[1]	[2]	[3]	[4]	[5]	[6]
Treatment × Male	-0.1947**	-0.1573*	-0.2531***	-0.0140*	- 0.0070	-0.0145**
	(0.0951)	(0.0853)	(0.0748)	(0.0082)	(0.0082)	(0.0072)
Reform year × Male	-0.0257	-0.0763	-0.0380	-0.0071	-0.0082*	-0.0059
	(0.0502)	(0.0465)	(0.0440)	(0.0046)	(0.0043)	(0.0041)
Reform period × Male	0.0990	0.0853	0.0799	0.0077	0.0016	0.0030
	(0.0663)	(0.0595)	(0.0523)	(0.0064)	(0.0058)	(0.0050)
Male	1.3517***	1.4068***	1.3989***	0.0587***	0.0643***	0.0646***
	(0.0346)	(0.0321)	(0.0303)	(0.0032)	(0.0030)	(0.0028)
Reform year	0.0141	0.0200	0.0229	0.0003	0.0011	0.0011
	(0.0472)	(0.0411)	(0.0368)	(0.0044)	(0.0038)	(0.0034)
Reform period	0.8266***	0.8164***	0.8282***	0.0454***	0.0484***	0.0473***
	(0.0593)	(0.0570)	(0.0545)	(0.0056)	(0.0054)	(0.0051)
Ref. year × Ref. period	0.0072	0.0256	0.0665	-0.0042	-0.0055	0.0013
	(0.0618)	(0.0557)	(0.0485)	(0.0056)	(0.0051)	(0.0044)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
M-o-b fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Survey-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Region fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.102	0.104	0.104	0.041	0.043	0.042
# of Obs.	100,110	121,413	146,118	100,110	121,413	146,118

Table 14: **Robustness Check (Triple Difference) – School Attainment.** ***, **, and * refer to 1%, 5%, and 10% significance levels, respectively. Robust standard errors are reported in parentheses. Controls include a full set of age dummies and an urban/rural dummy. The dependent variable in columns [1]–[3] is the total years of completed schooling. The dependent variable in columns [4]–[6] is a dummy variable taking 1 if the individual has a college degree (and above) and 0 otherwise.

LOG REAL EARNINGS – TRIPLE DIFFERENCE

Window size	**8 months**	**10 months**	**12 months**	**8 months**	**10 months**	**12 months**
Level of Schooling		**All**			**College and Above**	
	[1]	**[2]**	**[3]**	**[4]**	**[5]**	**[6]**
Treatment × Male	-0.0387	-0.0213	-0.0554*	-0.0087	-0.0101	-0.0659*
	(0.0365)	(0.0327)	(0.0290)	(0.0399)	(0.0356)	(0.0322)
Reform year × Male	0.0226	0.0107	0.0236	0.0431*	0.0183	0.0499**
	(0.0217)	(0.0199)	(0.0185)	(0.0246)	(0.0226)	(0.0210)
Reform period × Male	-0.0276	-0.0370	-0.0141	-0.0081	0.0009	0.0271
	(0.0255)	(0.0227)	(0.0201)	(0.0289)	(0.0257)	(0.0233)
Male	0.1488***	0.1655***	0.1472***	0.1189***	0.1281***	0.1072***
	(0.0147)	(0.0134)	(0.0125)	(0.0174)	(0.0161)	(0.0149)
Reform year	-0.0436*	-0.0427**	-0.0466***	-0.0293	0.0253	-0.0525***
	(0.0226)	(0.0203)	(0.0186)	(0.0237)	(0.0212)	(0.0195)
Reform period	0.1184***	0.1279***	0.1112	0.0334	0.0332	0.0162
	(0.0261)	(0.0240)	(0.0221)	(0.0289)	(0.0274)	(0.0262)
Ref. year × Ref. period	0.0280	0.0101	0.0368	0.0051	-0.0058	0.0316
	(0.0337)	(0.0302)	(0.0268)	(0.0315)	(0.0290)	(0.0254)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
M-o-b fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Survey-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Region fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.087	0.090	0.086	0.141	0.133	0.132
# of Obs.	35,782	43,626	52,214	9,021	11,239	13,496

Table 15: **Robustness Check (Triple Difference) – Log Real Earnings.** ***, **, and * refer to 1%, 5%, and 10% significance levels, respectively. Robust standard errors are reported in parentheses. Controls include a full set of age dummies and an urban/rural dummy. The earnings refer to monthly earnings. Nominal monthly earnings are deflated—taking 2004 as the base year—with CPI to obtain real monthly earnings. The sample in columns [4]–[6] is restricted to those with a college degree and above.