The Women-Empowering Effect of Higher Education

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Abstract

We examine the effects of the large-scale construction of public universities in Egypt during the 1960s and 1970s. We found that opening a local university increased the likelihood of obtaining higher education degrees and had long-lasting positive effects on labor market and marriage outcomes, particularly for women. We give insights on internal migration as a channel and show that migration prior to university enrollment age decreased while migration after that age increased as an outcome of university construction. Local universities reduced men's migration for study and women's migration for early marriage. The paper highlights the importance of increasing access to higher education for positive social and labor outcomes, particularly for women.

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1 Introduction

It is well-established in the economic literature that more schooling is crucial for escaping poverty and achieving economic growth at both national and individual levels (Becker, 1994; Card, 1999; Lochner, 2011). Against this backdrop, many developing countries have implemented policies to increase educational opportunities and expand the supply of educational institutions at all levels. The literature shows that these efforts have had greater impacts on girls than on boys, and have led to a reduction in gender disparities in education globally (Evans and Yuan, 2021; Glick, 2008), with the most significant decline observed in the Middle East and North Africa (MENA) region (World Bank, 2013). According to Evans et al. (2021), women's education in the MENA region has increased by more than six years from 1960 to 2010. Despite these advances, women in the region continue to face significant barriers to economic and social advancement. Female labor force participation remains low, with less than one-fifth of women participating in the workforce[1], and discrimination against women is prevalent across many social domains (Elsayed et al., 2021). This has led to a vicious cycle of restricted access to labor markets, poor-quality marriages, and high fertility, which in turn leads to greater dependence on men.

This paper examines the effects of a public policy implemented in Egypt during the 1960s and 1970s to increase access to higher education through the construction of a public university in each province (governorate). To investigate the policy's impact, we leverage the staggered roll-out of the university construction across the country. We merge data on the time of university construction in the province where individuals attended compulsory school with individual-level data on education, labor, and marriage outcomes. Our findings demonstrate that the establishment of a local university significantly increases the likelihood of higher education attainment among the exposed cohorts by 2 percentage points, a 12% increase relative to the unexposed. Additionally, we observe that the policy's impact is more prominent among women, with exposed women displaying a 4 percentage point higher probability of receiving higher education, a 29%

[1]Source: International Labour Organization, ILOSTAT database, https://ilostat.ilo.org/. Data retrieved on January 15, 2022.

increase compared to unexposed women.

The construction of a local university has positive longer-term implications for labor market outcomes, benefiting both genders in terms of improved job quality, with a significant increase in the probability of engaging in high-skilled and top-management occupations. However, the policy's impact was stronger for women. The cohorts exposed to the local university displayed a 3.9 percentage point increase in female labor force participation, a 12.8% increase relative to the unexposed. Additionally, the probability of paid employment among women increased by 3.1 percentage points, representing an 11.7% increase. The policy also impacted marriage outcomes for women, as the exposed cohorts showed a reduction of 0.73 years in the age gap between spouses and a significant increase in the probability of marrying a highly-educated husband, indicating strong assortative mating based on education. Furthermore, affected women were 20% less likely to marry at a young age (before age 18, incl.), exhibited higher levels of intra-household decision-making autonomy, and delayed fertility.

We provide suggestive evidence on the role of migration as a channel for explaining our results. We show that constructing a local university in an individual's province is associated with a delay in migration age. The share of people migrating before (or at) the age of university enrollment decreased sharply in response to the policy, and the share of those migrating after that age increased significantly. This pattern of migration delay holds true for the two genders, however, it hides variations regarding the reason for migration.

The decline in the before-18 migration for education purposes is more evident among men, suggesting that internal migration for study prevailed as a channel for obtaining higher education among men prior to local university construction. Exposed men were also less likely to migrate for work before age of 18. For women, exposure to local universities reduced the share of migrating for early marriage. This suggests that higher education came as an alternative to early marriage which adds to the improvements in women's decision-making autonomy and marriage outcomes that we observe in our results. After 18, both genders exposed to local universities are more likely, relative to the unexposed, to migrate for work and marriage. Overall, our results suggest that access to higher education increases mobility and widens the scope for economic opportunities for both genders and the scope for social empowerment among women.

The remainder of the paper is structured as follows: Section 2 briefly describes related literature and discusses our contribution, Section 3 explains the institutional settings of higher education expansion in Egypt and describes the data, Section 4 outlines the empirical strategy, Section 5 presents the main results, and Section 6 is the conclusion.

2 Related literature

Our paper contributes to the research on returns to higher education (e.g., Blundell et al., 2000; Card, 1995; Walker and Zhu, 2008; Zimmerman, 2014). The literature shows that distance to university matters for the decision to enroll in higher education as the costs of university attendance increase with distance (Carneiro et al., 2011; Nybom, 2017). Despite the importance of the topic for low- and middle-income countries, the literature on the returns to higher education in these countries is rather limited (Castelló-Climent et al., 2018; Hu and Bollinger, 2021; Kyui, 2016). Some studies have examined the impact of higher education expansion at a regional level such as Castelló-Climent et al., 2018 who used the location of Catholic missionaries in 1911 in India as an instrument for higher education and documented positive effects on development, estimated by regional light density. Other studies focused on individual-level data, for example, Kyui, 2016 who exploited regional variation in student intake capacities in Russian universities and showed positive returns to higher education in terms of employment and wages. In China, Hu and Bollinger (2021) showed evidence for an increase in unemployment and wages as an outcome of higher education expansion.

A common theme in this literature is the emphasis on labor market returns, with limited focus on the broader implications beyond employment and wages (see Oreopoulos and Petronijevic, 2013; Psacharopoulos and Patrinos, 2018, for a review of this literature). Another important observation is that only a few studies of those consider the gendered nature of higher-education enrollment and returns, despite the widespread gender disparities in education and other areas (Barcellos et al., 2014; Chakravarty, 2015;

Jayachandran and Kuziemko, 2011).

This paper adds to the current literature in three main ways. Firstly, the paper is among the first to investigate the impact of university construction using individual-level data from a country in the MENA region. The context of MENA, known for rising educational attainment among women, yet stagnant female labor force participation, together with strong social norms against women (Bursztyn et al., 2020; Elsayed et al., 2021) and a strong preference for sons (Chakravarty, 2015; Elsayed and Marie, 2020) makes it a useful step towards explaining the complex interplay between education, gender norms, and labor market outcomes in the region. Secondly, the paper extends beyond education and labor market outcomes and examines the impact of higher education on marriage, spouse characteristics, intra-household decision-making, and fertility. These aspects are crucial, given that marriage is an important component of the returns to education, particularly for women, (Chiappori et al., 2017; Goldin, 2006). Thirdly, the paper extensively investigates internal mobility as a channel for, and also as an outcome of, higher education and documents how these patterns of migration differ across the two genders.

3 Institutional setting and data

3.1 Higher education in Egypt

The compulsory education in Egypt consists of nine grades: six years of primary school (ages 6-11), and three years of preparatory school (ages 12-14).[2] Upon successful completion of these two education levels, students could opt into the secondary stage, which comprises two alternative tracks: the vocational track and the general track, both of which take three years to complete (i.e., ages 15-17). Both tracks enable students to enroll in higher education institutions which typically offer instruction for four years (i.e., ages 18-21)[3] with the only difference being that the vocational track covers technical

[2]Over the time period 1988-2003, length of primary education decreased by one year (Elsayed and Marie, 2020). Our time frame does not include this time period.

[3]Some of the programs within these institutions run for shorter (two years) or longer (five or seven years) periods.

subjects while the general track prepares students for tertiary education.[4]

Prior to 1952, higher education was the 'education of the elite', due to scarce places and high fees. After the abolition of monarchy in 1952, a socialist government came into power in 1956 and a program establishing 'education for all' commenced. The educational reforms included the introduction of a unified secondary school exit exam and the abolition of tuition fees at all public universities in 1962 (Gezi, 1979). Prior to 1963, only five (typically urban and central) provinces had at least one university within their borders (namely, Cairo, Giza, Alexandria, Asyut, and Monufia). The existing infrastructure could not accommodate the growing demand for higher education and respond to the needs of the labor market. To deal with this, between 1963-1976 the government constructed new public universities in 15 different provinces, where previously there had been no higher education institutions.[5]

The geographical expansion of higher education slowed down after 1976. In the early 1980s, the government instituted several reforms to increase the quality of education and combat overcrowding at universities, by controlling admissions, changing examination policies and updating the curriculum. In the mid-1980s, Egypt faced an economic crisis that led to significant reductions in government spending on education, and initiated the policy shift towards privatization. From 1995 onward, private universities received licenses to operate in the country. See Shann (1992) for a detailed overview of educational policies of that time.

3.2 Data

To get comprehensive information on higher education expansion, we constructed a novel dataset that contains information about each university in Egypt. The collected data includes information on the year of construction of the university, its location, and its fields of study. In Egypt, a national presidential decree announces the opening of each

[4]For more information on the structure of education in Egypt, see Hanushek et al. (2008).

[5]Information on the policy-making process that determined the order of university construction is scarce. In Table A1 of the Online Appendix, we present some characteristics of Egyptian provinces prior to university expansion and report their correlation with the policy timing. We find no evidence of a clear selective pattern of the policy timing on the basis of the education of the local population and province characteristics included.

university or university branch, and we used these decrees to build our dataset. To eliminate possible inconsistency between the official year of establishment and the year of first student intake, we cross-checked our data against university official websites. Several universities first operated as branches of tertiary institutions from other provinces. We are interested in the date when individuals first got access to higher education, so in these cases, we use the date when the branch opened rather than the date when the university became an independent institution. Table A2 in the Appendix lists Egyptian provinces by the year of the first university operation (see Figure A5 in Appendix for geographical overview).

To estimate the effect of access to university on educational attainment and longer-term labor and marriage outcomes, we exploit cross-sectional data from the 2007-2011 waves of the Egyptian Labor Force Survey (LFS).[6] These waves include detailed information about migration history and therefore enable us to identify the province in which the individual resided prior to the policy. The LFS is a nationally-representative survey collected by the Central Agency for Statistics (CAPMAS) and published by the Economic Research Forum on an annual basis (OAMDI, 2019). The survey includes an ample set of background and demographic characteristics as well as detailed information on labor force participation and job characteristics, and covers a sample of urban and rural areas across Egyptian provinces.

With this dataset we can look at the impact of university expansion on the probability of having a university education and various labor market outcomes including individual's labor force participation, probability of being engaged in paid employment, probability of being employed in a white-collar job, probability of holding a managerial position, and wages. Using LFS, we can also estimate the impact of the policy on a selected number of marriage outcomes including educational and labor market characteristics of the spouse. Table A3 of the Online Appendix provides definitions of the variables used in our study.

Since LFS does not contain information on the fertility and social empowerment of women, we complement our analysis with data from the 1998, 2006, and 2012 waves of the

[6]To capture possible changes associated with the flow of time and attrition issues, we include the cohort and province fixed effects and control for province-specific time trends and survey wave in our analysis.

Egyptian Labor Market Panel Survey (ELMPS). ELMPS is a nationally-representative survey that collects detailed information on family background, household structure, and marriage outcomes (OAMDI, 2016). We use the dataset to look particularly at the age at first marriage, fertility outcomes, and bargaining power of women proxied by the household decision-making index. See Online Appendix B for a description of the data and variables used.

3.3 Treatment, sample, and descriptive statistics

We focus on provinces that got access to higher education during the intensive phase of university construction between 1963-1976. To ensure that all cohorts in the analysis were under similar circumstances in terms of educational regulations and policies, we restrict our sample to individuals born between 1942 and 1967, who had ever attended school. This results in a final sample of 73,688 observations (21,625 women, and 52,063 men).

We define individuals exposed to higher education to be those who were 18 years old or younger when a university began operating in the province where they finished compulsory school. To determine the province of compulsory school, we used the province of residence at the age of 15, or the province of birth if there was no earlier change in location.[7] Older cohorts from the same province and individuals from provinces that have not yet had access to university education form the unexposed group. In this regard, we exploit two sources of variation: (1) variation in exposure to university across birth cohorts, since individuals who were older than 18 when higher education became available are considerably less likely to enroll, and (2) variation in dates of access to higher education across provinces arising from the staggered nature of university construction.

Table A4 in the Online Appendix shows descriptive statistics for the unexposed and exposed cohorts by gender. The table shows that the prevalence of university education is 14% (18%) among unexposed women (men). The percentage increases to 18% (21%) among the exposed women (men). While almost all men in the sample are in the labor

[7] We chose the province in which the individual finished compulsory school to mitigate bias coming from potential selective migration during the secondary stage of education. However, using the province of residence just before the university enrollment age of 18 gives similar results.

force, the share of labor market active women is only 31% for the unexposed group. This percentage increases to 50% for the exposed women. Women have lower-quality jobs compared to men, however, women exposed to higher education fare significantly better than those unexposed. Both exposed and unexposed men are more likely to be in top management compared to women in the corresponding group. However, when in paid employment, women tend on average to receive higher wages.[8] The table further shows that 99% of the sample got married at least once and that husbands tend to be more educated and engaged in higher-quality jobs than their wives.

The table also shows migration patterns across the two genders. Among the exposed cohorts, the share of women who ever moved across provinces is 21%, which is significantly higher than men (13%). The shares decreased significantly among the exposed to be 7% for women and 5% among men. The purposes of migration are different between the two genders. However, it remains the same in terms of ranking for the unexposed and exposed. The vast majority of women who ever migrated report marriage as a reason for migration (48% for the unexposed, 60% for the exposed). The majority of men report work as the main purpose (54%) and this increases to 76% for the exposed. Across women and men, only a few individuals report study as a reason for migration (only 2% of ever-moved women and 6% of ever-moved men).

4 Empirical strategy

4.1 Difference-in-differences

We use a difference-in-differences approach that takes advantage of the staggered rollout of university construction across provinces to estimate the effect of access to higher education on a set of education, labor market, and marriage outcomes. Our baseline specification model takes the following form:

$$Y_{itr} = \beta T_{tr} + \gamma_r + \mu_t + \varepsilon_{itr} \quad (1)$$

[8] This could be because women's employment is more selective and mainly driven by the more educated.

where Y_{itr} is an outcome of interest for individual i of cohort t from province r. T_{tr} is a treatment dummy equal to one for people who were 18 years old or younger when a university first opened in their province, and zero otherwise. γ_r and μ_t stand for province and birth cohort fixed effects, respectively, and ε_{itr} is an independent error term, clustered at the province level. Due to the small number of clusters in our analysis, standard errors may suffer from downward bias (Cameron et al., 2008). To address this issue, we follow Cameron and Miller (2015) and report wild bootstrapped p-values for the coefficients of interest across all model specifications. To account for any province-specific policies that could have affected cohorts non-randomly, we estimate additional models in which we control for province-specific time trends.

Recent literature on staggered difference-in-differences designs indicates that linear regressions with two-way fixed effects estimate a weighted average of treatment effects, where some of the weights could be negative and this could bias the estimates (e.g. Chaisemartin and D'Haultfœuille, 2020). We address this issue in two ways. First, we show in Section 4.2 that our results are robust to an event-study specification with leads and lags of cohorts relative to the date of university construction, which does not use comparisons between treated provinces for identification (see Figure 1). Second, we follow the methodology suggested in Chaisemartin and D'Haultfœuille (2020) and confirm our findings using an estimator robust to treatment effects heterogeneity. The impact of the policy remains statistically unchanged when we implement this approach (see Columns 3-4 of Table A5 and Figure A3 in the Online Appendix).

4.2 Event study

The main identifying assumption in our empirical analysis is that, in absence of university construction, affected and unaffected individuals would have witnessed similar trends in higher education attainment. To graphically show the extent to which a university construction in a province could affect educational outcomes of cohorts around the time of construction and check the common trend assumption, we estimate a regression of university education attainment on a vector of dummy variables reflecting individuals' cohort distance t to the year of their province treatment. Specifically, we estimate:

$$Y_{itr} = \sum_{t=-7}^{6} \alpha_t Cohort_{tr} + \gamma_r + \mu_t + \nu_{itr} \tag{2}$$

where Y_{itr} is a dummy indicator for university degree attainment for individual i of cohort t from province r. $Cohort_{tr}$ is a set of dummies indicating seven pre- and post-university opening cohorts (with the reference cohort being t = -1). γ_r and μ_t are province- and cohort-fixed effects respectively, and ν_{itr} is an independent error term, clustered at the province level.

Figure 1 plots coefficients α_t for men and women separately.[9] We find no difference in trends in higher education attainment for pre-treatment cohorts. This confirms that it is reasonable to treat the policy as an exogenous shock and further validates the staggered difference-in-differences approach. Once the university is constructed, there is a significant increase in the probability of finishing higher education for all subsequent cohorts. The effect is much more pronounced for women.

To ensure that the jump in the probability of obtaining university education was not artificially pushed by secular trends in higher education or other educational policies, we perform a falsification test by shifting the cut-off point 7 years before the actual university construction. Figure A2 in the Online Appendix shows no evidence of a difference in outcomes for cohorts around this hypothetical date. As a robustness check, we also perform a permutation test where we randomly reshuffle treatment dates among the provinces. Using 1000 random permutations, we re-ran regressions with Equation 1 (See Figure A6 in Appendix). P-values representing the probability of obtaining the estimates of treatment effects as in our analysis by chance are 0.000 for the sample of women, 0.062 for men, and 0.000 for the whole sample, suggesting that our results are not driven by patterns in data other than actual university construction.

[9] Figure A1 in the Online Appendix shows event study graphs for other outcome variables from our analysis.

5 Results

5.1 University degree attainment

Table 1 shows the estimates for Equation 1, which examines the impact of university construction on the probability of attaining a university degree for the overall sample combined and for women and men separately. We estimate the coefficients of a model specification that controls for the cohort- and province-fixed effects and a more restrictive model that additionally accounts for the interaction between province dummies and a linear time trend to capture province-specific time trends. The two models provide similar results, so we use the model with province-specific trend controls as our preferred specification. To benchmark the magnitude of the effects, we report the impact as a percentage change relative to the unexposed group's means.

Table 1. Impact of access to university on higher degree attainment.

	University degree					
	All		Women		Men	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment exposure	0.026***	0.020***	0.044***	0.040***	0.018**	0.011
	(0.005)	(0.005)	(0.012)	(0.011)	(0.006)	(0.007)
	[0.007]	[0.031]	[0.017]	[0.021]	[0.040]	[0.131]
Observations	73,688	73,688	21,625	21,625	52,063	52,063
Mean of Outcome	0.168	0.168	0.137	0.137	0.179	0.179
Effect size, %	15.25	11.74	32.26	29.32	9.92	6.32
Cohort FE	✓	✓	✓	✓	✓	✓
Province FE	✓	✓	✓	✓	✓	✓
Cohort × Province FE		✓		✓		✓

Notes: Dependent variable is a dummy variable that equals 1 if person finished university education, zero otherwise. *Treatment exposure* is a dummy variable that equals 1 if person was 18 or younger when university opened, and 0 otherwise. All regressions include survey wave dummies as controls. The whole sample regressions in Columns 1 and 2 also include a dummy variable for gender. Effect size is calculated by dividing *Treatment exposure* coefficient by mean of the outcome for untreated cohorts. We test the difference in effect between men and women by estimating a model with an interaction between treatment and gender variables. P-value of the t-test for a difference in the coefficients between women (Column 4) and men (Column 6) equals 0.009. Standard errors clustered at province level (14 clusters) are reported in parentheses. Wild-bootstrapped p-values adjusted for a small number of clusters are presented in brackets. $*p < 0.1, **p < 0.05, ***p < 0.01$.

The results in Column 2 show that following university construction, the probability of receiving a university degree grew by 2 p.p., representing an increase of about 11.7% from an average level of 16.8% for the unexposed cohorts. The impact is mainly driven by women, for whom the policy resulted in a 4 p.p. increase in the probability of receiving a university degree, which corresponds to a 29.3% increase above the average level of 13.7% among the unexposed (Column 4). Men were also more likely to receive a university education, however, the impact is not statistically significant and rather small, with a one percentage point increase, equivalent to about a 6.3% increase from an average level of 17.9% for the unexposed (Column 6).

We also estimate the effect on total years of schooling (Table A6 in the Online Appendix). University construction increased years of schooling for girls by 0.48 years, i.e. by about 5.8% from an average of 8.4 years among the unexposed. The impact is smaller for men, with an increase of 0.21 years (2.4% relative to an average of 8.6 years among the unexposed). To investigate the extent to which university construction could have downstream effects on the educational path leading to higher education, we evaluate the difference between the affected and unaffected groups in the probability of finishing different levels of education. Following Equation 1, Figure A4 in the Online Appendix shows the coefficient estimates based on separate estimations for each education level. The figure shows no significant difference between the two groups in the probability of obtaining a compulsory or secondary school qualification. The positive effect of the policy is clear only for obtaining a university degree. This confirms the lack of different pre-trends in the probability of finishing earlier levels of education between the two groups and suggests that university construction helps those who already finished at least their secondary education and are deciding whether or not to enroll in higher education.

5.2 Labor market outcomes

Table 2 presents the coefficients from Equation 1 for labor market outcomes for the two genders separately. The table clearly shows that women exposed to university construction had significantly better outcomes in the labor market. Women's labor force participation increased by 4 p.p., representing an increase of about 12.8% from the average level of 30.5% among the unexposed. The quality of labor market outcomes for women also improved. The probability of being in paid employment increased by 3.1 p.p., which comprises an increase of about 11.7% from the average level of 26.6% among the unexposed. Women exposed to higher education are by 3.4 p.p. more likely to work in a white-collar occupation (14.4% increase from an average level of 23.7%). The probability of being in top management also increased by 1.7 p.p., representing an increase of 20.7% compared to the unaffected cohorts, for whom the share of women in managerial positions is equal to 8%. However, women's wages conditional on work are not statistically different between exposed and unexposed groups.

Table 2. Impact of access to university on labor market outcomes.

	Labor force participation (1)	Paid job (2)	White collar (3)	Top management (4)	Hourly wage, log (5)
Panel A: Female sample					
Treatment exposure	0.039***	0.031**	0.034***	0.017*	0.006
	(0.014)	(0.011)	(0.010)	(0.008)	(0.022)
	[0.011]	[0.019]	[0.014]	[0.073]	[0.801]
Observations	20,490	20,490	20,490	20,490	7,872
Mean of Outcome	0.305	0.266	0.237	0.08	1.677
Effect size, %	12.77	11.66	14.35	20.65	0.36
Panel B: Male sample					
Treatment exposure	0.001	0.001	0.019**	0.013**	0.013
	(0.002)	(0.003)	(0.007)	(0.006)	(0.010)
	[0.986]	[0.666]	[0.027]	[0.034]	[0.235]
Observations	45,084	45,084	45,084	45,084	29,951
Mean of Outcome	0.994	0.990	0.481	0.204	1.572
Effect size, %	0.0	0.13	4.02	6.45	0.85
t-test for difference in coefficients between genders					
t-test p-value	0.000	0.000	0.001	0.001	0.075

Notes: *Treatment exposure* is a dummy variable that equals 1 if person was 18 or younger when university opened, and 0 otherwise. All regressions include survey wave dummies, birth cohort and province FEs, and province-specific time trend as controls. Effect size is calculated by dividing *Treatment exposure* coefficient by mean of the outcome for untreated cohorts. We test the difference in effect between men and women by estimating a model with an interaction between treatment and gender variables. P-values of the t-test for a difference in the coefficients between men and women are shown at the bottom of the table. Standard errors clustered at province level (14 clusters) are reported in parentheses. Wild-bootstrapped p-values adjusted for a small number of clusters are presented in brackets. $*p < 0.1, **p < 0.05, ***p < 0.01$.

To investigate potential substitution between women's positive labor market and marriage outcomes (which will be discussed in the next section), we estimate a model in which we interact the exposure to treatment dummy with a dummy variable for being married. Table A7 in the Online Appendix shows no significant difference in the labor market outcomes between married and unmarried women. The only exception is the probability of employment, which is lower for the exposed married women relative to the exposed unmarried (significant at 10% level). However, the results should be read with caution, given the potential endogeneity of marriage and labor market decisions.

Men are also positively affected by the policy, but the impact is less pronounced. As expected, there is no positive effect in terms of labor force participation or paid employment, given that male participation in the labor market and paid employment is already high among the unaffected group. However, there is a positive impact on the probability of being in white-collar employment, which increased by 1.9 p.p., representing an increase of 4% from the average of 48% among the unexposed cohorts. Managerial employment also increased by 1.3 p.p., or by 6.4% compared to unexposed men.

5.3 Marriage and social empowerment of women

The positive impact of the policy goes beyond labor market aspects and extends to the quality of marriage, as higher education is typically used as a signal in the marriage market, particularly for women (Ashraf et al., 2020). In Table 3 we present results for the effect of the university construction on the marriage outcomes for both genders using the same specification of Equation 1. The table shows that while marriage outcomes and the characteristics of spouses seem to be unaffected by university construction for men, women have significant and sizable effects on most of these outcomes. Exposed women are more likely to get married to more educated husbands. The husband of an exposed woman is on average third of a year more educated than a husband of an unexposed wife, representing a 3.4% increase from the average of 10 years of education, and is 3 p.p. more likely to have a university degree, representing an increase of 10.3% from the level of 29%

among the husbands of the unexposed.[10] The husbands of the exposed women are also more likely to be in better jobs with the probability of being employed in a white-collar occupation increased by 5.5 p.p., representing an increase of 24.2% from the average of the unexposed. The age gap between the two spouses decreases by three-quarters of a year, representing a decline of 12% compared to unexposed.

Table 3. Impact of access to university on marriage outcomes.

	Spouse's years of educ. (1)	Spouse university degree (2)	Spouse white collar job (3)	Spouse top management (4)	Spouse hourly wage, log (5)	Age gap between spouses (6)
Panel A: Female sample						
Treatment exposure	0.346**	0.030**	0.055**	0.018	0.028	-0.731***
	(0.165)	(0.010)	(0.023)	(0.015)	(0.023)	(0.199)
	[0.049]	[0.025]	[0.020]	[0.297]	[0.282]	[0.022]
Observations	16,903	16,903	16,903	16,903	9,289	16,903
Mean of Outcome	10.29	0.292	0.227	0.106	1.436	6.05
Effect size, %	3.36	10.31	24.25	17.29	1.98	-12.10
Panel B: Male sample						
Treatment exposure	0.035	0.011	0.008	0.005	0.001	0.031
	(0.102)	(0.007)	(0.010)	(0.005)	(0.018)	(0.120)
	[0.728]	[0.167]	[0.478]	[0.406]	[0.957]	[0.789]
Observations	49,645	49,645	49,086	49,645	9,559	49,645
Mean of Outcome	5.19	0.093	0.176	0.037	1.323	8.50
Effect size, %	0.67	11.38	4.41	12.47	0.08	0.36
t-test for difference in coefficients between genders						
t-test p-value	0.155	0.285	0.000	0.000	0.001	0.000

Notes: *Treatment exposure* is a dummy variable that equals 1 if person was 18 or younger when university opened, and 0 otherwise. All regressions include survey wave dummies, birth cohort and province FEs, and province-specific time trend as controls. Effect size is calculated by dividing *Treatment exposure* coefficient by mean of the outcome for untreated cohorts. We test the difference in effect between men and women by estimating a model with an interaction between treatment and gender variables. P-values of the t-test for a difference in the coefficients between men and women are shown at the bottom of the table. Standard errors clustered at province level (14 clusters) are reported in parentheses. Wild-bootstrapped p-values adjusted for a small number of clusters are presented in brackets. $*p < 0.1, **p < 0.05, ***p < 0.01$.

We support this evidence with more aspects related to age at marriage, intra-household

[10] Table A5 presents the results of robustness checks, including the differences-in-differences with heterogeneous treatment effects estimator proposed by Chaisemartin and D'Haultfœuille (2020) in Columns 3 and 4, and a model that restricts the analysis to individuals who never migrated from their province of birth in Columns 5 and 6. We find that the results remain robust to these alternative model specifications.

decision-making, and fertility, using data from ELMPS.[11] Table 4 shows that age at the first marriage increased by about 1.2 years for the exposed women from an average of about 20 years, and the incidence of early marriages decreased by 8.8 p.p., or by 20% compared to unexposed cohorts. Women's intra-household decision-making increased by about 0.28 standard deviations. The number of children conceived at the age of 30 decreased in response to exposure to local university but remained unchanged at the age of 40, suggesting that access to higher education pushed women to postpone childbearing. Due to the small number of observations, however, these results should be read with caution.

Table 4. Impact of access to university on social empowerment of women, ELMPS data.

	University degree (1)	Age at marriage (2)	Married before age 18 (incl.) (3)	Intra-HH DM (4)	Number of children at 30 (5)	Number of children at 40 (6)
Treatment exposure	0.069**	1.235**	-0.088**	0.279**	-0.342***	0.095
	(0.028)	(0.442)	(0.039)	(0.128)	(0.109)	(0.175)
	[0.030]	[0.080]	[0.097]	[0.089]	[0.153]	[0.751]
Observations	1,133	897	897	611	575	524
Mean of Outcome	0.089	19.97	0.436	0.078	1.027	1.411
Effect size, %	77.03	6.18	-20.24	357.82	-33.33	6.7

Notes: Source - Egyptian Labor Market Panel Survey (ELMPS). The analysis is limited to women. *Treatment exposure* is a dummy variable that equals 1 if person was 18 or younger when university opened, and 0 otherwise. All regressions include survey wave dummies, birth cohort and province FEs, and province-specific time trend as controls. Effect size calculated as coefficient of *Treatment exposure* divided on mean of the outcome for unexposed cohorts. Standard errors clustered at province level (14 clusters) are reported in parentheses. Wild-bootstrapped p-values adjusted for a small number of clusters are presented in brackets. $*p < 0.1, **p < 0.05, ***p < 0.01$.

[11] The ELMPS data shows a similar effect of university construction in a person's birth province on higher education attainment. Exposed to treatment women are 6.9 p.p. more likely to get higher education (se=0.028), significant at the 95% confidence level, which is quantitatively similar to the estimates obtained in Table 1.

5.4 Internal mobility

Table 5. The effect of exposure to the local university on cross-province migration at different age.

	Moving before age 18 (incl.)		Moving after age 18	
	Women	Men	Women	Men
	(1)	(2)	(3)	(4)
Panel A: Probability to move from birth province				
Treatment exposure	-0.195***	-0.123***	0.188***	0.123***
	(0.023)	(0.026)	(0.023)	(0.025)
	[0.001]	[0.002]	[0.001]	[0.001]
Mean of Outcome	0.109	0.059	0.095	0.069
Panel B: Probability to move for the purpose of study				
Treatment exposure	-0.000	-0.011***	0.002	0.003
	(0.001)	(0.002)	(0.001)	(0.002)
	[0.814]	[0.001]	[0.197]	[0.141]
Mean of Outcome	0.003	0.006	0.001	0.002
Panel C: Probability to move for marriage				
Treatment exposure	-0.083***	-0.003	0.120***	0.012*
	(0.010)	(0.002)	(0.014)	(0.006)
	[0.001]	[0.073]	[0.002]	[0.047]
Mean of Outcome	0.043	0.001	0.054	0.008
Panel D: Probability to move for work				
Treatment exposure	-0.002	-0.027***	0.008***	0.096***
	(0.001)	(0.006)	(0.002)	(0.019)
	[0.160]	[0.004]	[0.004]	[0.002]
Mean of Outcome	0.001	0.016	0.004	0.052
Panel E: Probability to move to Greater Cairo and Alexandria				
Treatment exposure	-0.125***	-0.075***	0.116***	0.071***
	(0.020)	(0.016)	(0.018)	(0.015)
	[0.001]	[0.001]	[0.001]	[0.001]
Mean of Outcome	0.067	0.034	0.049	0.037
Observations	21,625	52,063	21,625	52,063

Notes: Data on migration come from waves 2007-2011 of the Labor Force Survey. Dependent variable in Panel A is a dummy that equals 1 if an individual has moved from his/her province of birth, and equals 0 otherwise. The dependent variables in Panels B-D are dummy variables that equal 1 if an individual moved from his/her province of birth for study purpose (Panel B), marriage (Panel C), work (Panel D) or to Greater Cairo and Alexandria (Panel E), and equal 0 otherwise. All regressions include survey wave dummies, birth cohort and province FEs, and province-specific time trends as controls. Effect size is calculated by dividing *Treatment exposure* coefficient by the mean of the outcome for untreated cohorts. t-test p-values for the difference in coefficients between genders are 0.005, 0.075 and 0.059 in Panel A; 0.048, 0.217 and 0.647 in Panel B; 0.000, 0.084 and 0.542 in Panel C; 0.000, 0.104 and 0.073 in Panel D; and 0.325, 0.001 and 0.002 in Panel E. Standard errors clustered at province level are reported in parentheses. Wild-bootstrapped p-values adjusted for a small number of clusters are presented in brackets. As a robustness check we have also done analysis in Columns 4-6 using age of 22 (i.e. university graduation age) as a cut-off point: the results from this exercise are similar to those reported above. $*p < 0.1, **p < 0.05, ***p < 0.01$.

This section aims to analyze how the construction of universities impacts the internal mobility (migration) patterns of individuals. To better understand this phenomenon, we categorize migration by age using the age of university enrollment as a dividing point. We investigate migration before or at the age of 18 to capture the possibility of individuals strategically migrating to obtain higher education. Additionally, we study migration after that age to examine the impact of exposure to higher education on migration as an outcome variable. We define migration as inter-province migration and analyze different types of migration based on their purpose (study, work, or marriage). Our analysis employs the Labor Force Survey's detailed section on migration. Table A3 in the Appendix defines the different outcome variables used in this section.

Migration as a channel for higher education. Prior to the establishment of local universities, individuals had limited options for higher education within their province. They had to either commute to neighboring provinces or move to another province entirely. Therefore, constructing a university in the same province is expected to decrease migration for the purpose of study. We start by investigating the impact of university construction in a neighboring province on higher education attainment. To identify the closest neighboring province, we use driving distance between the individual's province of residence at the age of finishing compulsory education and the location of universities in other provinces that had earlier access to higher education. Our findings, presented in Table A8, suggest that university construction in a nearby province had no significant effect on higher education attainment. This implies that commuting may not have been a viable option due to the prohibitive cost of commuting to another province or the lack of information about the newly established university.[12]

We further explore the impact of university construction on migration patterns and the purpose of migration using the difference-in-difference specification of Equation 1. We run the analyses separately for women and men and present the results in Table 5. For migration before the university enrollment age (Columns 1 and 2), the table shows that migration decreased significantly as an outcome of university construction by 19

[12]We also found no impact of university construction in a neighboring province on cross-province migration, migration before university enrollment age, or migration for the purpose of study.

p.p. for women and by 12.3 p.p. for men. The table further shows that migration for study purposes decreased for men but not for women, suggesting that migration prevailed as a channel for obtaining higher education mostly among men before local university construction. This suggests that despite the small impact of university construction on men's higher education attainment, men have benefited from the policy by switching the location in which they pursued higher education from universities outside their provinces to the newly-established local ones, thus, saving on moving and commuting costs. The policy also postponed joining the labor market for men, possibly as unskilled workers, by giving them an opportunity to pursue a higher level of education. This is evident by the reduction in early migration for work among exposed men by 2.7 percentage points.

For women, while the policy had zero effect on migration for education, migration for marriage before 18 decreased significantly. Given that the probability to get married does not differ between exposed and unexposed women, this indicated a decline in early marriage rates among the exposed women, which goes in line with our findings in Section 5.3 documenting significant decreases in the incidence of early marriage among exposed women. We look also at the characteristics of destinations by investigating the impact of university construction on migrating to central provinces. The table shows that migration to central provinces at 18 years or younger decreased significantly for the two genders. Table A10 in the Online Appendix descriptively shows spouse characteristics for women who did not internally move (Column 1), those who moved before or at university enrollment age (Column 2), and those who migrated later (Column 3). The table shows that women who move for marriage earlier (at 18 or younger) are matched with relatively older and significantly less educated spouses, who on average earn lower wages. This suggests that the decline in early marriage and in migration for marriage at younger ages could be one of the channels for the improvement in marriage outcomes we documented earlier.

Migration as an outcome of higher education. We study the impact of university construction on migration after the university enrollment age. We find a positive impact for the two genders (Columns 3 and 4). Exposed women (men) are 18.8 p.p. (12.3 p.p.) more likely to migrate relative to the unexposed. The coefficients are very similar to the early migration but with the opposite sign, suggesting that the policy's impact on the migration was more in the direction of delaying it. The table further shows that most of the exposed women's migration is for marriage, while most of the men's migration is for work. However, we find positive impacts on women's migration for work and men's migration for marriage too. This mobility is expected to expand the pool of potential marriage partners and may allow women to find more educated husbands, even if the number of educated men within a province remains relatively constant. We also find a positive impact on migration to central provinces for the two genders, which are typically characterized by better job opportunities. These findings suggest that increased migration in response to higher education could be a channel for the positive impacts documented earlier on labor and marriage outcomes.

6 Concluding remarks

We evaluate the impact of a policy designed to expand coverage of higher education in Egypt in the 1960s-1970s by constructing universities in provinces with no prior access to higher education. Using event study and difference-in-differences techniques, we document an increase in the share of individuals with higher education as an outcome of the policy. We show that the impact is more pronounced among women, who were generally less mobile for education. We also document a positive impact on labor market outcomes, particularly for women who experienced a nearly 13% increase in labor force participation and 12% increase in paid employment. Affected women and men are also more likely to be engaged in white-collar and top-management positions. The positive effects of the policy extend to the marriage market, where affected women are more likely to marry younger and better-educated husbands, more likely to be socially empowered and have a lower probability to marry before turning 18. We show that access to higher education significantly decreased migration for study among exposed men and migration for early marriage among exposed women. We further show that access to local universities increased mobility for work and marriage at older ages for both genders, which contributed towards better labor market and marriage opportunities.

Due to lack of data on background characteristics of individuals such as the education of parents, family wealth, and household size, the paper did not address the characteristics of those who benefited most from the policy and whether the policy was successful in enhancing social mobility in the affected regions. These data limitations also prevented us from evaluating the extent to which different barriers (e.g. monetary costs of traveling, information frictions, social norms, and uncertainty about education outcomes, etc.) played a role in hindering female enrollment in higher education prior to the policy. We leave these points as an opportunity for future research. While we provide strong suggestive evidence on the role of geographic mobility, using province as a proxy for location could be an oversimplification, particularly given that some provinces are geographically extensive. Moreover, lack of information on the name of the university attended restricts our ability to track the direction of movement for the purpose of higher education.

Despite the limitations, the paper sheds light on the nexus between higher education, migration, and marriage in the MENA region. It underscores the crucial role of higher education in shaping various aspects of women's lives, including their labor market opportunities, marital prospects, and mobility. Making higher education more accessible can contribute to reducing socioeconomic gender disparities and enhancing women's empowerment.

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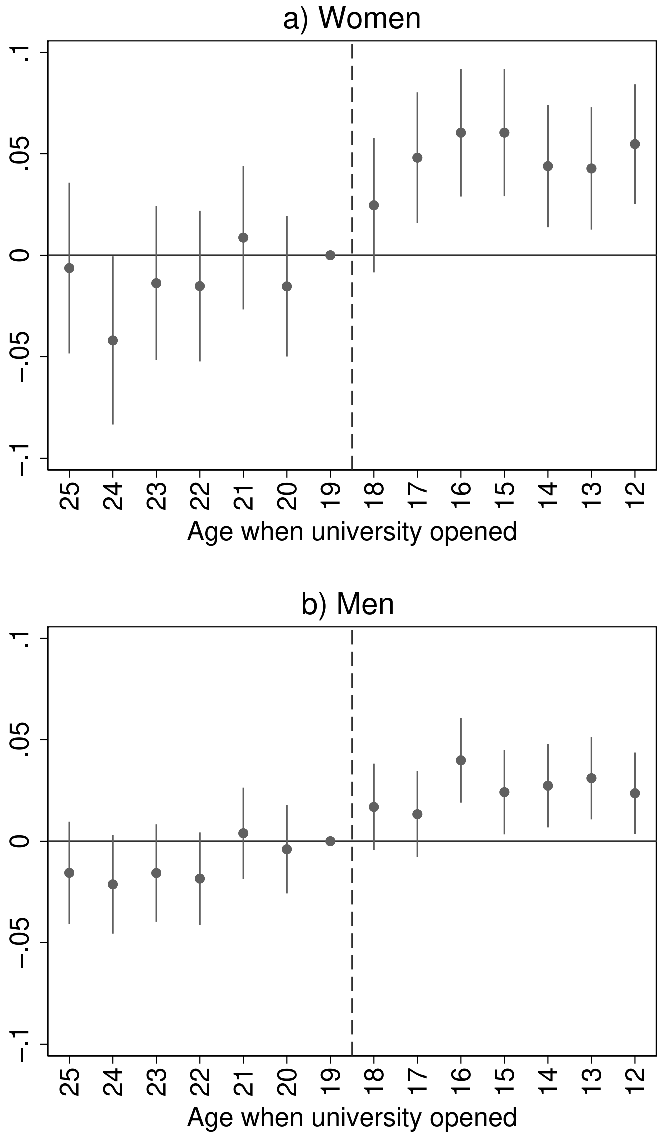
Figure 1: Impact of access to university on degree attainment. Plotted coefficients of the relative-to-event cohort dummies by gender.



Notes: The figure plots coefficient estimates and 95 percent confidence intervals for province-specific cohort dummies from Equation (2). Each point represents the coefficient of a province-specific cohort relative to the province-specific cohort aged 19 at the time when university opened. The dashed vertical line indicates the treatment.

Table 1. Impact of access to university on higher degree attainment.

	University degree					
	All		Women		Men	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment exposure	0.026***	0.020***	0.044***	0.040***	0.018**	0.011
	(0.005)	(0.005)	(0.012)	(0.011)	(0.006)	(0.007)
	[0.007]	[0.031]	[0.017]	[0.021]	[0.040]	[0.131]
Observations	73,688	73,688	21,625	21,625	52,063	52,063
Mean of Outcome	0.168	0.168	0.137	0.137	0.179	0.179
Effect size, %	15.25	11.74	32.26	29.32	9.92	6.32
Cohort FE	✓	✓	✓	✓	✓	✓
Province FE	✓	✓	✓	✓	✓	✓
Cohort × Province FE		✓		✓		✓

Notes: Dependent variable is a dummy variable that equals 1 if person finished university education, zero otherwise. *Treatment exposure* is a dummy variable that equals 1 if person was 18 or younger when university opened, and 0 otherwise. All regressions include survey wave dummies as controls. The whole sample regressions in Columns 1 and 2 also include a dummy variable for gender. Effect size is calculated by dividing *Treatment exposure* coefficient by mean of the outcome for untreated cohorts. We test the difference in effect between men and women by estimating a model with an interaction between treatment and gender variables. P-value of the t-test for a difference in the coefficients between women (Column 4) and men (Column 6) equals 0.009. Standard errors clustered at province level (14 clusters) are reported in parentheses. Wild-bootstrapped p-values adjusted for a small number of clusters are presented in brackets. $*p < 0.1, **p < 0.05, ***p < 0.01$.

Table 2. Impact of access to university on labor market outcomes.

	Labor force participation (1)	Paid job (2)	White collar (3)	Top management (4)	Hourly wage, log (5)
Panel A: Female sample					
Treatment exposure	0.039***	0.031**	0.034***	0.017*	0.006
	(0.014)	(0.011)	(0.010)	(0.008)	(0.022)
	[0.011]	[0.019]	[0.014]	[0.073]	[0.801]
Observations	20,490	20,490	20,490	20,490	7,872
Mean of Outcome	0.305	0.266	0.237	0.08	1.677
Effect size, %	12.77	11.66	14.35	20.65	0.36
Panel B: Male sample					
Treatment exposure	0.001	0.001	0.019**	0.013**	0.013
	(0.002)	(0.003)	(0.007)	(0.006)	(0.010)
	[0.986]	[0.666]	[0.027]	[0.034]	[0.235]
Observations	45,084	45,084	45,084	45,084	29,951
Mean of Outcome	0.994	0.990	0.481	0.204	1.572
Effect size, %	0.0	0.13	4.02	6.45	0.85
t-test for difference in coefficients between genders					
t-test p-value	0.000	0.000	0.001	0.001	0.075

Notes: *Treatment exposure* is a dummy variable that equals 1 if person was 18 or younger when university opened, and 0 otherwise. All regressions include survey wave dummies, birth cohort and province FEs, and province-specific time trend as controls. Effect size is calculated by dividing *Treatment exposure* coefficient by mean of the outcome for untreated cohorts. We test the difference in effect between men and women by estimating a model with an interaction between treatment and gender variables. P-values of the t-test for a difference in the coefficients between men and women are shown at the bottom of the table. Standard errors clustered at province level (14 clusters) are reported in parentheses. Wild-bootstrapped p-values adjusted for a small number of clusters are presented in brackets. $*p < 0.1, **p < 0.05, ***p < 0.01$.

Table 3. Impact of access to university on marriage outcomes.

	Spouse's years of educ. (1)	Spouse university degree (2)	Spouse white collar job (3)	Spouse top management (4)	Spouse hourly wage, log (5)	Age gap between spouses (6)
Panel A: Female sample						
Treatment exposure	0.346**	0.030**	0.055**	0.018	0.028	-0.731***
	(0.165)	(0.010)	(0.023)	(0.015)	(0.023)	(0.199)
	[0.049]	[0.025]	[0.020]	[0.297]	[0.282]	[0.022]
Observations	16,903	16,903	16,903	16,903	9,289	16,903
Mean of Outcome	10.29	0.292	0.227	0.106	1.436	6.05
Effect size, %	3.36	10.31	24.25	17.29	1.98	-12.10
Panel B: Male sample						
Treatment exposure	0.035	0.011	0.008	0.005	0.001	0.031
	(0.102)	(0.007)	(0.010)	(0.005)	(0.018)	(0.120)
	[0.728]	[0.167]	[0.478]	[0.406]	[0.957]	[0.789]
Observations	49,645	49,645	49,086	49,645	9,559	49,645
Mean of Outcome	5.19	0.093	0.176	0.037	1.323	8.50
Effect size, %	0.67	11.38	4.41	12.47	0.08	0.36
t-test for difference in coefficients between genders						
t-test p-value	0.155	0.285	0.000	0.000	0.001	0.000

Notes: *Treatment exposure* is a dummy variable that equals 1 if person was 18 or younger when university opened, and 0 otherwise. All regressions include survey wave dummies, birth cohort and province FEs, and province-specific time trend as controls. Effect size is calculated by dividing *Treatment exposure* coefficient by mean of the outcome for untreated cohorts. We test the difference in effect between men and women by estimating a model with an interaction between treatment and gender variables. P-values of the t-test for a difference in the coefficients between men and women are shown at the bottom of the table. Standard errors clustered at province level (14 clusters) are reported in parentheses. Wild-bootstrapped p-values adjusted for a small number of clusters are presented in brackets. $*p < 0.1, **p < 0.05, ***p < 0.01$.

Table 4. Impact of access to university on social empowerment of women, ELMPS data.

	University degree (1)	Age at marriage (2)	Married before age 18 (incl.) (3)	Intra-HH DM (4)	Number of children at 30 (5)	Number of children at 40 (6)
Treatment exposure	0.069**	1.235**	-0.088**	0.279**	-0.342***	0.095
	(0.028)	(0.442)	(0.039)	(0.128)	(0.109)	(0.175)
	[0.030]	[0.080]	[0.097]	[0.089]	[0.153]	[0.751]
Observations	1,133	897	897	611	575	524
Mean of Outcome	0.089	19.97	0.436	0.078	1.027	1.411
Effect size, %	77.03	6.18	-20.24	357.82	-33.33	6.7

Notes: Source - Egyptian Labor Market Panel Survey (ELMPS). The analysis is limited to women. *Treatment exposure* is a dummy variable that equals 1 if person was 18 or younger when university opened, and 0 otherwise. All regressions include survey wave dummies, birth cohort and province FEs, and province-specific time trend as controls. Effect size calculated as coefficient of *Treatment exposure* divided on mean of the outcome for unexposed cohorts. Standard errors clustered at province level (14 clusters) are reported in parentheses. Wild-bootstrapped p-values adjusted for a small number of clusters are presented in brackets. $*p < 0.1, **p < 0.05, ***p < 0.01$.

APPENDIX (for online publication)

APPENDIX A

Table A1. Mean characteristics of Egyptian provinces in 1960.

	Provinces that got access to university		
	earlier (in 1963-1971) (1)	later (in 1973-1976) (2)	corr.coef. (3)
Population:			
Population	1,426,659	1,528,811	0.09
Population density	377	314	-0.34
Intercensal growth rate 1960-1966, %	1.96	2.45	-0.07
Economic development:			
Share non-agricultural labor force, %	31.94	37,27	0.33
GDP in 2006, % of country total	2,60	2,03	-0.37
Education:			
Literacy, %	27.7	28.9	0.25
Secondary education, %	10.5	9.8	0.22
University degree, %	3.1	2.9	0.10
Number of provinces	7	6	

Notes: the table shows mean pre-expansion characteristics of Egyptian provinces grouped by the timing of access to higher education. Column (3) presents a correlation coefficient between the corresponding characteristic and year of university construction for provinces where university opened in 1963-1976. $*p < 0.1, **p < 0.05, ***p < 0.01$ show the significance level of the correlation coefficients. Measures for population and labor force come from the World Bank country economic report 1980 based on data from CAPMAS Population and Housing Census 1960. Educational characteristics are calculated using Egyptian 1986 Census data for cohorts born in the corresponding provinces before year 1945.

Table A2. List of provinces with university starting dates, Egypt.

Province	University name	First university starting date
Greater Cairo	Cairo university	1839
Alexandria	Alexandria university	1938
Asyut	Asyut university	1957
Monufia	Manoufia university	1958
Gharbia	Tanta university	1963
Sharqia	Zagazig university	1968
Kafr-Elsheikh	Alexandria University (branch)	1969
Minya	Minya university	1969
Qena/Luxor	South Valley university	1970
Sohag	Sohag university	1971
Dakahlia	Mansoura university	1973
Aswan	University of Aswan	1975
Beheira	Alexandria university (branch)	1975
Faiyum	Faiyum university	1975
Port Said	Port Said university	1975
Suez	University of Suez	1975
Ismailia	Suez Canal university	1976
Beni Suef	Beni Suef University	1976
Damietta	University of Damietta	1976
New Valley	New Valley university	1993
North Sinai	University of Arish	2016

Notes: The table shows a list of Egyptian provinces with the corresponding dates of access to public university education. Greater Cairo includes Cairo, Giza, and Qalyubia provinces. The list excludes provinces with no public universities. Provinces shown in grey are not included in the sample. Port Said, Suez, and Ismailia were excluded as they were affected by the Arab-Israeli conflicts during 1967-1973.

Table A3. Definition of variables.

Variable name	Definition
Treatment variable	
Treatment exposure	Dummy variable that equals 1 if a person was 18 years old or younger when a university in the province where she/he attended compulsory school opened, and 0 otherwise. We define individuals' province of school attendance as a birth province for those who have not moved before age of 15 (age of finishing compulsory education). If an individual reported moving at any point before age of 15, we use the province they had moved to to define exposure.
Outcome variables	
University degree	Dummy variable that equals 1 if individual finished university, and 0 otherwise.
Years of schooling	Number of effective years of education (without grade repetition) completed by person.
Labor force participation (LFP)	Dummy variable that equals 1 if individual is active in the labor market (employed or looking for a job), and 0 otherwise. Sample restricted to people who are of working age. Retired, pensioners, disabled and students are excluded.
Paid job	Dummy variable that equals 1 if individual is in a paid job (waged employee, employer or self-employed), and 0 otherwise (unpaid family worker). Sample restricted to people who are of working age. Retired, pensioners, disabled and students are excluded.
White collar	Dummy variable that equals 1 if person performs professional, managerial, or administrative work, and 0 otherwise. Sample restricted to people who are of working age. Retired, pensioners, disabled and students are excluded.
Top management	Dummy variable that equals 1 if person is a senior manager, legislator or senior official, and 0 otherwise. Sample restricted to people who are of working age. Retired, pensioners, disabled and students are excluded.
Hourly wage, log	Logarithm of total hourly wage from the main job. Wages are adjusted for inflation. Sample restricted to employed individuals.
Spouse's years of education	Number of effective years of schooling (without grade repetition) completed by spouse.
Spouse university degree	Dummy variable that equals 1 if spouse has completed university education, 0 otherwise.

Outcome variables (continued)	
Spouse white collar job	Dummy variable that equals 1 if spouse performs professional, managerial, or administrative work, and 0 otherwise.
Spouse top management	Dummy variable that equals 1 if a spouse is a senior manager, legislator or senior official, and 0 otherwise.
Spouse hourly wage, log	Logarithm of the total hourly wage of a spouse from the main job. Wages are adjusted for inflation. Sample restricted to employed individuals.
Age gap between spouses	Age difference between husband and wife calculated as husband's age minus wife's age.
Ever moved	Dummy variable that equals 0 for individuals whose place of residence coincides with their place of birth, and equals 1 otherwise.
Moved for study (marriage; work)	Dummy variable that equals 1 for individuals who have moved from their province of birth for study (marriage; work) purpose, and equals 0 otherwise. Available only for waves 2007-2011 of the LFS.
Moved to Greater Cairo and Alexandria	Dummy variable that takes the value 1 if the individual migrated to Cairo, Giza, or Alexandria (most economically developed Egyptian provinces), and equals 0 if the person migrated to another province or never migrated. Available only for waves 2007-2011 of the LFS.

Table A4. Descriptive statistics.

	Women			Men		
	Unexposed (1)	Exposed (2)	p (3)	Unexposed (4)	Exposed (5)	p (6)
Educational outcomes						
University degree	0.14	0.18	0.000	0.18	0.21	0.000
	(0.34)	(0.39)		(0.38)	(0.40)	
Years of schooling	8.40	10.34	0.000	8.56	10.15	0.000
	(5.20)	(4.73)		(5.45)	(5.01)	
Labour market outcomes						
Labor market participation	0.31	0.50	0.000	0.99	0.99	0.000
	(0.46)	(0.50)		(0.08)	(0.05)	
Paid job	0.27	0.45	0.000	0.99	0.98	0.677
	(0.44)	(0.50)		(0.10)	(0.10)	
White collar	0.24	0.42	0.000	0.48	0.57	0.000
	(0.43)	(0.49)		(0.50)	(0.49)	
Top management	0.08	0.06	0.000	0.20	0.14	0.000
	(0.27)	(0.23)		(0.40)	(0.35)	
Hourly wage, log	1.68	1.52	0.000	1.57	1.49	0.000
	(0.44)	(0.40)		(0.46)	(0.43)	
Marriage outcomes						
Ever married	0.99	0.98	0.003	0.99	0.99	0.000
	(0.10)	(0.12)		(0.07)	(0.09)	
Spouse's years of education	10.28	11.28	0.000	5.19	7.14	0.000
	(5.71)	(5.05)		(6.09)	(6.25)	
Spouse university degree	0.29	0.31	0.021	0.09	0.11	0.000
	(0.45)	(0.46)		(0.29)	(0.32)	
Spouse white collar job	0.23	0.62	0.000	0.18	0.20	0.000
	(0.42)	(0.48)		(0.38)	(0.40)	
Spouse top management	0.11	0.20	0.000	0.04	0.02	0.000
	(0.31)	(0.40)		(0.19)	(0.13)	
Spouse hourly wage, log	1.43	1.38	0.012	1.32	1.14	0.000
	(0.61)	(0.54)		(0.51)	(0.53)	
Migration						
Ever moved	0.21	0.07	0.000	0.13	0.05	0.000
	(0.41)	(0.26)		(0.34)	(0.22)	
among them:						
Moved for study	0.02	0.01	0.010	0.06	0.02	0.000
	(0.14)	(0.09)		(0.24)	(0.14)	
Moved for work	0.02	0.03	0.389	0.54	0.76	0.000
	(0.15)	(0.17)		(0.50)	(0.43)	
Moved for marriage	0.48	0.60	0.000	0.07	0.11	0.000
	(0.50)	(0.49)		(0.25)	(0.31)	
Moved for other reasons	0.47	0.37	0.000	0.34	0.11	0.004
	(0.50)	(0.48)		(0.47)	(0.31)	

Notes: table shows means of variables and the corresponding standard deviations (in parentheses) for cohorts relative to treatment separately for two genders. Exposed (Unexposed) refers to individuals who were below (above) age 18 when university in province opened. Columns 3 and 6 present p-values for a t-test of mean difference between Exposed and Unexposed groups.

Table A5. Robust DiD estimates and analysis for the sample of non-movers.

	DiD estimates		Robust estimator of Chaisemartin & D'Haultfœuille		Sample of non-movers	
	Women (1)	Men (2)	Women (3)	Men (4)	Women (5)	Men (6)
University degree	0.040***	0.011	0.043***	0.009	0.041***	0.010
	(0.011)	(0.007)	(0.015)	(0.007)	(0.013)	(0.006)
Labor force participation	0.039***	0.001	0.069***	-0.005	0.035*	-0.001
	(0.014)	(0.002)	(0.026)	(0.003)	(0.019)	(0.002)
Paid job	0.031**	0.002	0.063***	-0.005	0.023	-0.002
	(0.011)	(0.003)	(0.024)	(0.004)	(0.017)	(0.003)
White collar job	0.034***	0.019**	0.046**	0.012	0.033**	0.020***
	(0.010)	(0.007)	(0.019)	(0.018)	(0.015)	(0.006)
Top management	0.017*	0.013**	0.029**	0.002	0.014	0.016
	(0.008)	(0.006)	(0.011)	(0.005)	(0.008)	(0.010)
Hourly wage, log	0.006	0.013	-0.071	-0.021	0.030	0.028*
	(0.022)	(0.010)	(0.131)	(0.024)	(0.029)	(0.014)
Spouse's years of educ.	0.346**	0.037	0.444	0.021	0.475*	-0.032
	(0.165)	(0.102)	(0.344)	(0.178)	(0.233)	(0.119)
Spouse university degree	0.030**	0.011	0.032*	-0.008	0.038**	0.010
	(0.010)	(0.007)	(0.018)	(0.011)	(0.015)	(0.009)
Spouse white collar job	0.055**	0.008	0.038**	0.005	0.046	0.007
	(0.023)	(0.010)	(0.017)	(0.016)	(0.028)	(0.012)
Spouse top management	0.018	0.005	0.032*	0.001	0.017	0.007
	(0.015)	(0.005)	(0.018)	(0.005)	(0.019)	(0.005)
Spouse hourly wage, log	0.028	0.001	-0.160	-0.027	-0.011	-0.001
	(0.023)	(0.018)	(0.137)	(0.050)	(0.040)	(0.018)
Age gap, husband-wife	-0.731***	0.031	-0.794***	0.426	-0.296	0.065
	(0.199)	(0.120)	(0.266)	(0.305)	(0.296)	(0.157)

Notes: Columns 1-2 show DiD estimates from our main analysis for women and men, correspondingly. Columns 3-4 present the treatment effect estimates using robust to heterogeneous treatment effects estimator from Chaisemartin and D'Haultfœuille (2020). Columns 5-6 present the results of our analysis with the sample restricted to individuals who have not moved from their province of birth. $*p < 0.1, **p < 0.05, ***p < 0.01$.

Table A6. Impact of access to university on years of schooling.

	Years of schooling					
	All		Women		Men	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment exposure	0.357***	0.299***	0.602***	0.484***	0.238**	0.206**
	(0.078)	(0.058)	(0.147)	(0.120)	(0.092)	(0.083)
	[0.010]	[0.001]	[0.011]	[0.006]	[0.054]	[0.048]
Observations	73,688	73,688	21,625	21,625	52,063	52,063
Mean of Outcome	8.52	8.52	8.40	8.40	8.56	8.56
Effect size, %	4.19	3.51	7.16	5.77	2.78	2.40
Cohort FE	✓	✓	✓	✓	✓	✓
Province FE	✓	✓	✓	✓	✓	✓
Cohort × Province FE		✓		✓		✓

Notes: Dependent variable – number of years of education completed (without grade repetition). *Treatment exposure* is a dummy variable that equals 1 if person was 18 or younger when university opened, and 0 otherwise. All regressions include survey wave dummies as controls. The whole sample regressions in columns (1) and (2) also include a dummy variable for gender. Effect size is calculated by dividing *Treatment exposure* coefficient by mean of the outcome for untreated cohorts. We test the difference in effect between men and women by estimating a model with an interaction between treatment and gender variables. P-value of the t-test for a difference in the coefficients between women (Column 4) and men (Column 6) equals 0.056. Standard errors clustered at province level (14 clusters) are reported in parentheses. Wild-bootstrapped p-values adjusted for a small number of clusters are presented in brackets. $*p < 0.1, **p < 0.05, ***p < 0.01$.

Table A7. Role of marital status in benefits from the policy for women.

	Labor force participation (1)	Paid work (2)	White collar (3)	Managerial position (4)	Hourly wage, log (5)
Treatment exp. **x** Married	-0.037*	-0.022	-0.021	-0.011	0.036
	(0.018)	(0.021)	(0.020)	(0.011)	(0.044)
Treatment exposure	0.068***	0.049**	0.050***	0.025*	-0.022
	(0.018)	(0.019)	(0.016)	(0.013)	(0.036)
Married	0.013	-0.017	0.017	0.002	-0.036
	(0.011)	(0.014)	(0.013)	(0.010)	(0.047)
Observations	20,490	20,490	20,490	20,490	7,872

Notes: *Treatment exposure* is a dummy variable that equals 1 if person was 18 or younger when university opened, and 0 otherwise. All regressions include survey wave dummies, birth cohort and province FEs, and province-specific time trend as controls. Standard errors clustered at province level (14 clusters) are reported in parentheses. $*p < 0.1, **p < 0.05, ***p < 0.01$.

Table A8. Robustness check: impact of university construction in the neighboring province on university degree attainment.

	University degree		
	All	Women	Men
	(1)	(2)	(3)
Treatment exposure	0.010	0.001	0.015
	(0.014)	(0.023)	(0.018)
	[0.555]	[0.961]	[0.480]
Mean of Outcome	0.141	0.097	0.155
Effect size, %	7.35	1.43	9.65
Observations	51,681	15,997	35,684

Notes: Data on migration come from waves 2007-2011 of the Labor Force Survey. Exposure to treatment is defined using a date of university construction in the closest neighboring province that got earlier access to higher education (distance measured by driving distance between the capital city of the individual's birth province and university location). Dependent variable is a dummy variable that equals 1 if person finished university education, and zero otherwise. All regressions include survey wave dummies, birth cohort and province FEs, and province-specific time trends as controls. Effect size is calculated by dividing *Treatment exposure* coefficient by mean of the outcome for untreated cohorts. P-value of the t-test for a difference in the coefficients between men and women equals 0.018. Standard errors clustered at province level are reported in parentheses. Wild-bootstrapped p-values adjusted for a small number of clusters are presented in brackets. $*p < 0.1, **p < 0.05, ***p < 0.01$.

Table A9. The effect of exposure to the local university on cross-province migration at different age.

	Moving before age 18 (incl.)		Moving after age 18	
	Women	Men	Women	Men
	(1)	(2)	(3)	(4)
Panel A: Probability to move from birth province				
Treatment exposure	-0.195***	-0.123***	0.188***	0.123***
	(0.023)	(0.026)	(0.023)	(0.025)
	[0.001]	[0.002]	[0.001]	[0.001]
Mean of Outcome	0.109	0.059	0.095	0.069
Panel B: Probability to move for the purpose of study				
Treatment exposure	-0.000	-0.011***	0.002	0.003
	(0.001)	(0.002)	(0.001)	(0.002)
	[0.814]	[0.001]	[0.197]	[0.141]
Mean of Outcome	0.003	0.006	0.001	0.002
Panel C: Probability to move for marriage				
Treatment exposure	-0.083***	-0.003	0.120***	0.012*
	(0.010)	(0.002)	(0.014)	(0.006)
	[0.001]	[0.073]	[0.002]	[0.047]
Mean of Outcome	0.043	0.001	0.054	0.008
Panel D: Probability to move for work				
Treatment exposure	-0.002	-0.027***	0.008***	0.096***
	(0.001)	(0.006)	(0.002)	(0.019)
	[0.160]	[0.004]	[0.004]	[0.002]
Mean of Outcome	0.001	0.016	0.004	0.052
Panel E: Probability to move to Greater Cairo and Alexandria				
Treatment exposure	-0.125***	-0.075***	0.116***	0.071***
	(0.020)	(0.016)	(0.018)	(0.015)
	[0.001]	[0.001]	[0.001]	[0.001]
Mean of Outcome	0.067	0.034	0.049	0.037
Observations	21,625	52,063	21,625	52,063

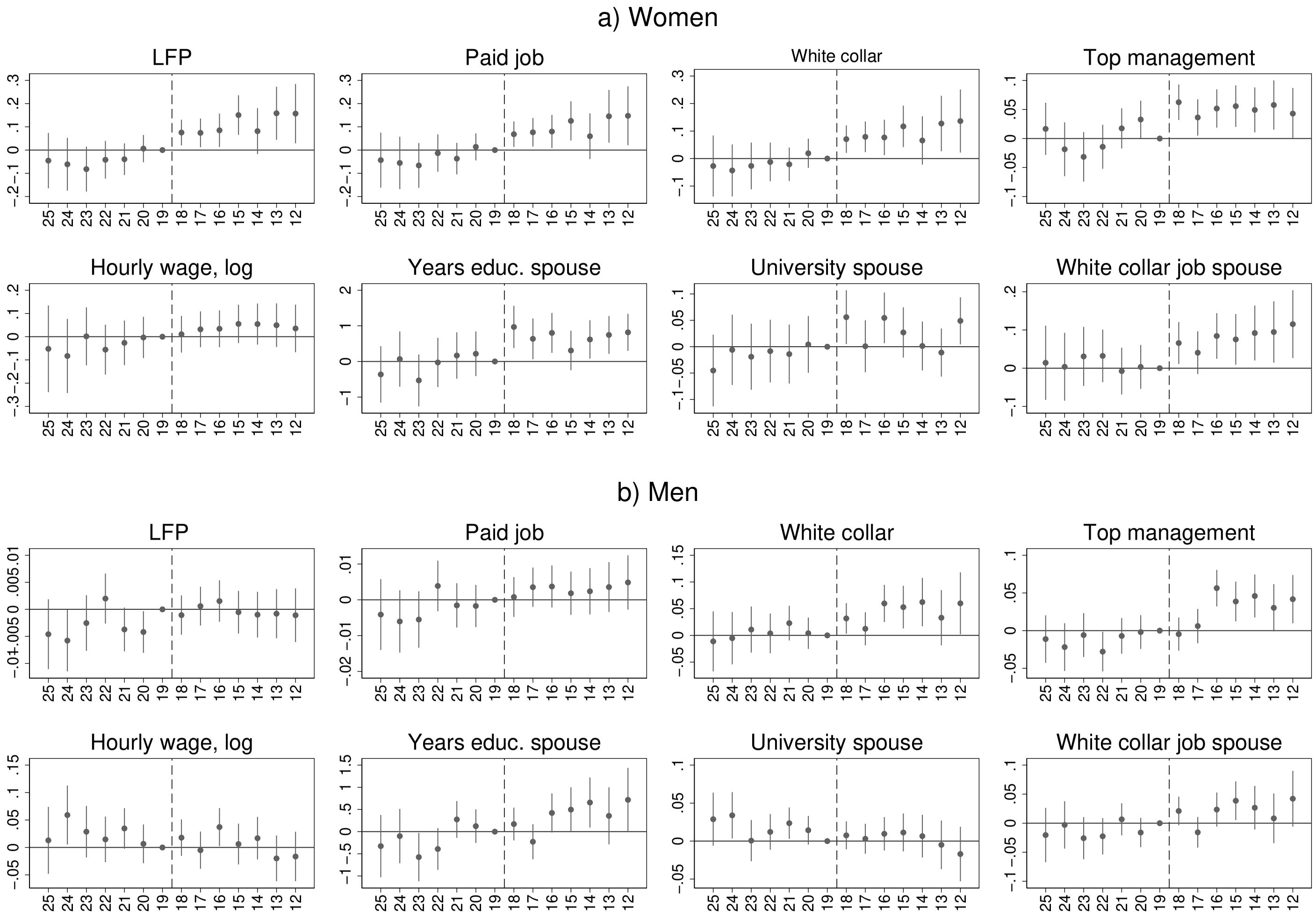
Notes: Data on migration come from waves 2007-2011 of the Labor Force Survey. Dependent variable in Panel A is a dummy that equals 1 if an individual has moved from his/her province of birth, and equals 0 otherwise. The dependent variables in Panels B-D are dummy variables that equal 1 if an individual moved from his/her province of birth for study purpose (Panel B), marriage (Panel C), work (Panel D) or to Greater Cairo and Alexandria (Panel E), and equal 0 otherwise. All regressions include survey wave dummies, birth cohort and province FEs, and province-specific time trends as controls. Effect size is calculated by dividing *Treatment exposure* coefficient by the mean of the outcome for untreated cohorts. t-test p-values for the difference in coefficients between genders are 0.005, 0.075 and 0.059 in Panel A; 0.048, 0.217 and 0.647 in Panel B; 0.000, 0.084 and 0.542 in Panel C; 0.000, 0.104 and 0.073 in Panel D; and 0.325, 0.001 and 0.002 in Panel E. Standard errors clustered at province level are reported in parentheses. Wild-bootstrapped p-values adjusted for a small number of clusters are presented in brackets. As a robustness check we have also done analysis in Columns 4-6 using age of 22 (i.e. university graduation age) as a cut-off point: the results from this exercise are similar to those reported above. $*p < 0.1, **p < 0.05, ***p < 0.01$.

Table A10. Husbands' characteristics for women by their mobility-for-marriage status

	Did not move to another province for marriage	Moved for marriage *before* school graduation age (age 18, incl.)	Moved for marriage *after* school graduation age (age 18)	p-value for difference between Col. 2 and 3
	(1)	(2)	(3)	(4)
Spouse's years of education	11.09	9.60	11.25	0.001
	(5.17)	(5.75)	(5.50)	
Spouse university degree	0.30	0.26	0.41	0.001
	(0.46)	(0.44)	(0.49)	
Spouse white collar job	0.56	0.35	0.43	0.054
	(0.50)	(0.48)	(0.50)	
Spouse top management	0.18	0.16	0.17	0.856
	(0.38)	(0.37)	(0.37)	
Spouse hourly wage	4.92	4.16	4.79	0.195
	(9.81)	(2.70)	(3.47)	
Age gap, husband - wife	6.49	9.44	6.47	0.000
	(4.65)	(6.02)	(5.37)	

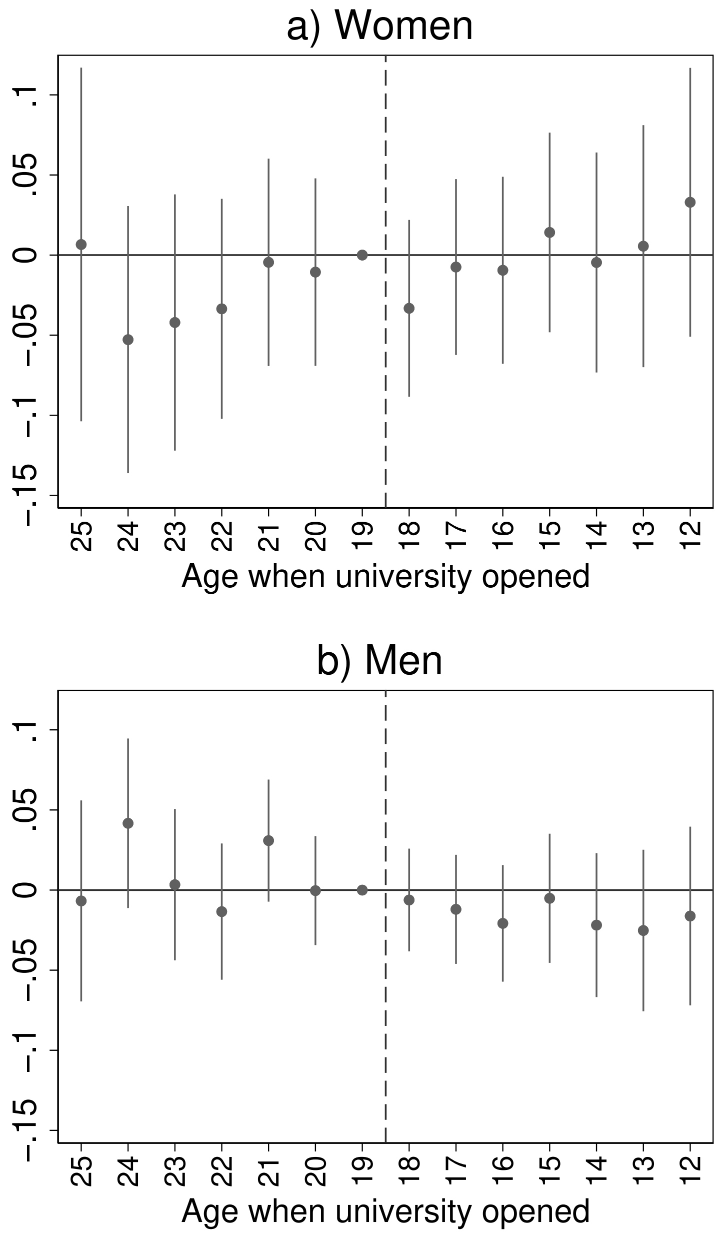
Notes: Source - waves 2007-2011 of the Egyptian LFS. The table shows means of husbands' characteristics and the corresponding standard deviations (in parentheses) for sub-samples of women who did not move for marriage (Column 1), moved for marriage before age 18 incl. (Column 2) and moved for marriage after age 19 incl. (Column 3). Column 4 presents p-values for a t-test of mean difference between Columns 2 and 3.

Figure A1: Impact of the policy on labour and social outcome variables.



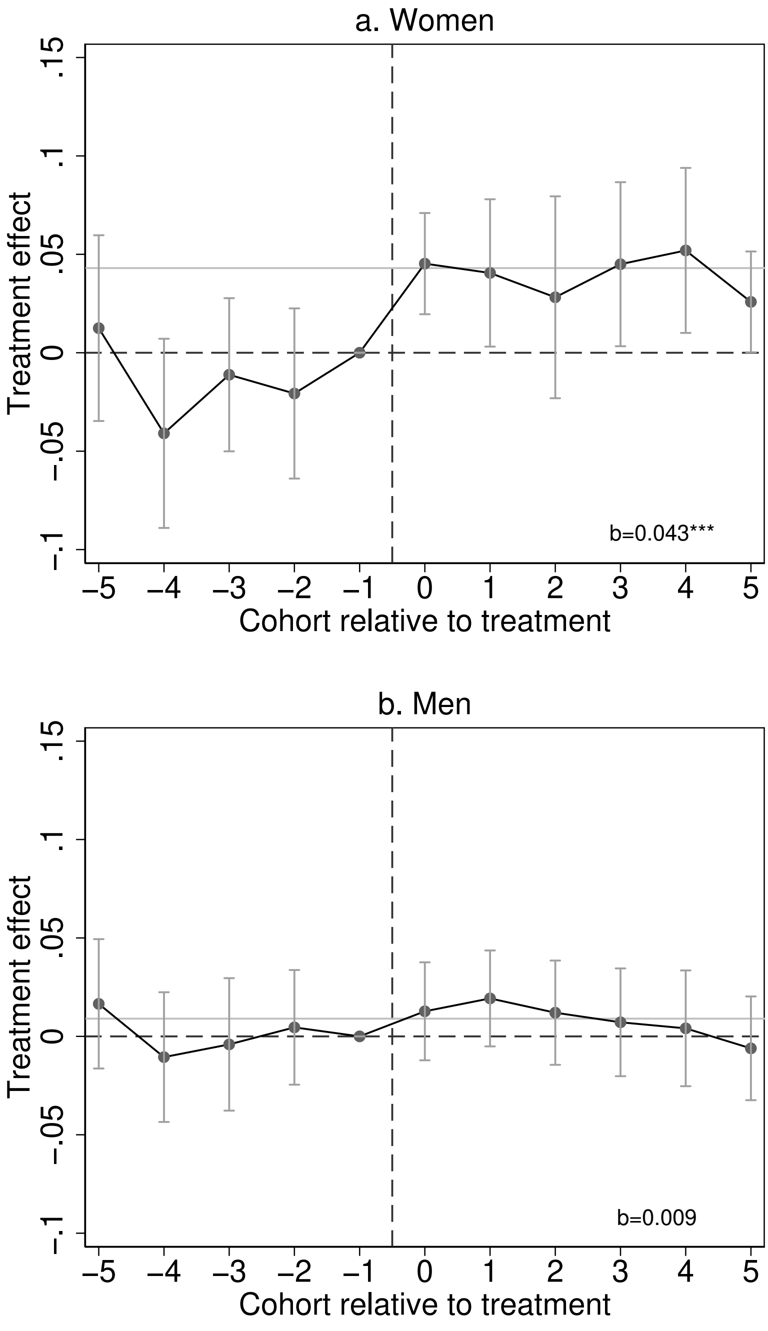
Notes: The figures plot coefficient estimates and 95 percent confidence intervals for province-specific cohort dummies from Equation (2). Each point represents the coefficient of a province-specific cohort relative to the province-specific cohort aged 19 at the time when university opened. The dashed vertical line indicates the treatment.

Figure A2: Placebo date test. Plotted coefficients of the relative-to-event cohort dummies by gender.



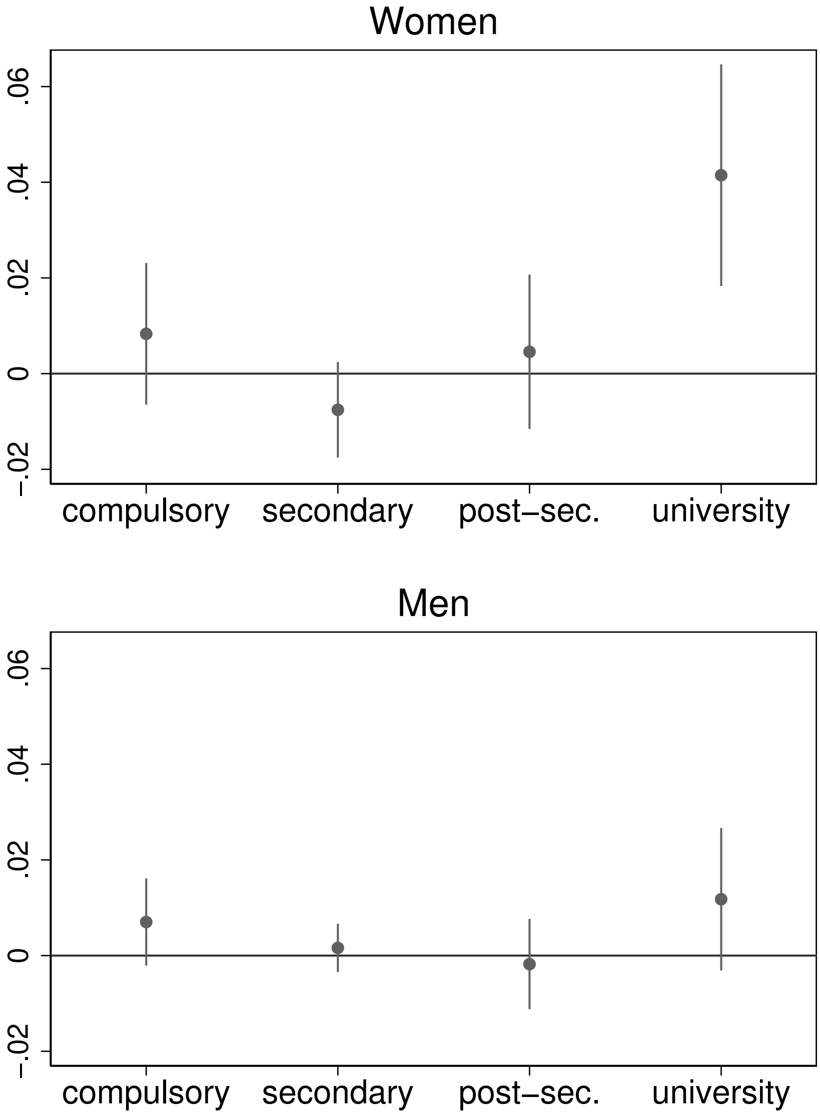
Notes: The figure plots coefficient estimates and 95 percent confidence intervals for a placebo regression of Equation 2, six years before the actual university construction. Dependent variable – dummy = 1 if the person finished university education, zero otherwise. Each point represents the coefficient of a province-specific cohort relative to the province-specific cohort aged 19 at the time of 6 years prior to university opening. The dashed vertical line indicates the hypothetical treatment.

Figure A3: Robust to heterogeneous treatment effects DiD estimates.



Notes: The graph presents robust-to-heterogeneous treatment effects coefficient estimates and 95 percent confidence intervals (Chaisemartin and D'Haultfœuille, 2020) computed by *did_ multiplegt* Stata package. Dependent variable – dummy = 1 if the person finished university education, zero otherwise. b denotes the estimated average effect of treatment and is depicted by a horizontal grey line on the graphs. A p-value of the joint-placebo test equals 0.827, supporting the parallel trend assumption. Standard errors are calculated using 99 bootstrap replications. $*p < 0.1, **p < 0.05, ***p < 0.01$.

Figure A4: Impact of the policy on educational attainment at different levels.



Notes: The graph plots coefficients and 95 percent confidence intervals of the exposure to treatment dummy from regressions that estimate Equation 1 using the different levels of educational attainment as dependent variables (shown on the x-axis) for the two genders separately. Variable *compulsory* is a dummy indicator = 1 if the person has completed compulsory education at most, and zero otherwise. *Secondary (post-sec.)* is a dummy variable = 1 for individuals with secondary (post-secondary) level as the highest educational attainment, and zero otherwise. *University* is a dummy variable = 1 if the person has finished university education, zero otherwise. All regressions include birth cohort and province FEs, survey wave dummies, and province-specific time trends as controls.

Figure A5: Roll-out of university construction in Egyptian provinces.

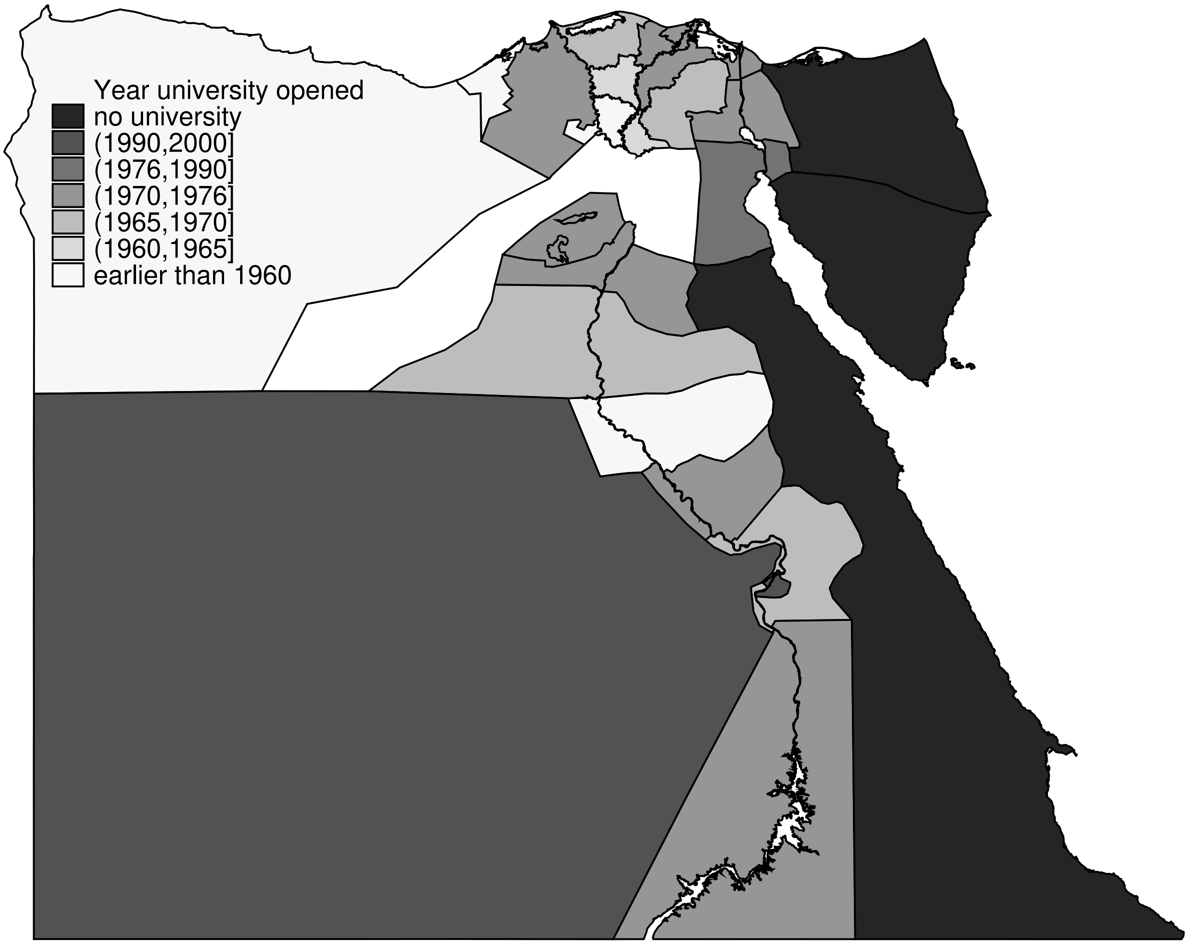
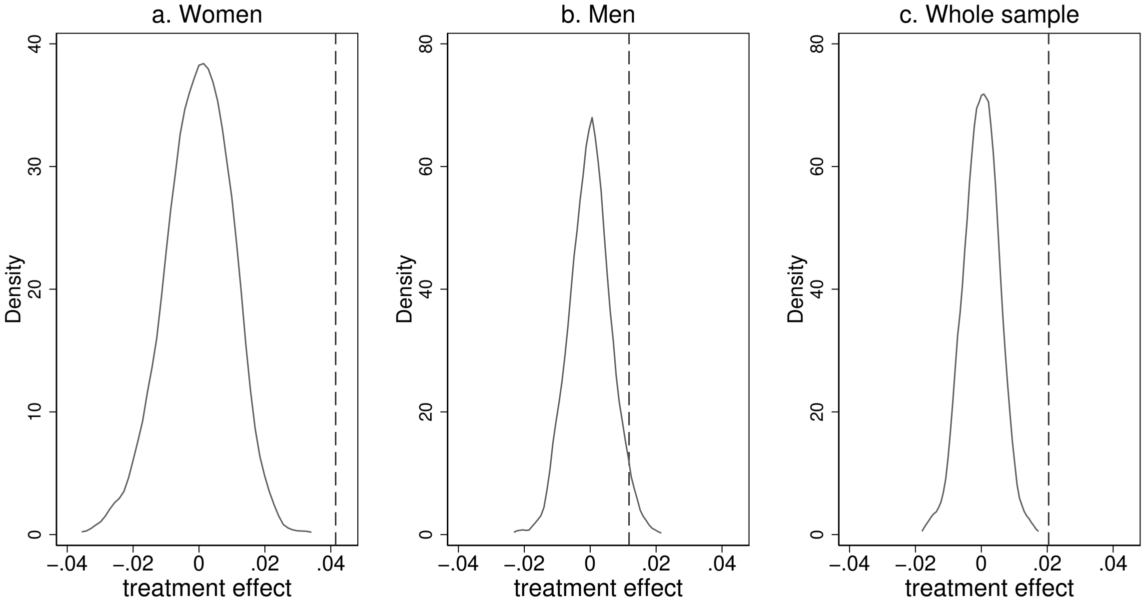


Figure A6: Permutation test, distribution of treatment effects.



Notes: The figure plots distributions of the treatment effects' estimates from the permutation test with random treatment dates assignment (1000 replications used). The dashed vertical line indicates the actual treatment effect that we obtained in our analysis.

APPENDIX B

Egyptian labor Market Panel Survey Data description: the Egypt Labor Market Panel Survey (ELMPS) is a publicly-available nationally representative longitudinal survey carried out by the Economic Research Forum (ERF) in cooperation with the Egyptian Central Agency for Public Mobilization and Statistics (CAPMAS). The survey tracks both households and individuals over two decades and contains questions on education, marriage, geographic mobility, labor market histories, and fertility experience. In this study, we employed waves 1998, 2006, and 2012 of the ELMPS and implemented all sample restrictions as in our main analysis. We are looking at individuals born between 1943 and 1964 in provinces that witnessed university construction in the 1960s-1970s, and who had attended school. Table B1 below provides definitions of variables we used in our analysis of the ELMPS data. Descriptive statistics for the final sample are shown in Table B2.

Table B1. Definition of variables, ELMPS data.

Variable name	Definition
Treatment variables:	
Treatment exposure	- Dummy variable that equals 1 if person was 18 years old or younger when university in her province of birth opened, and 0 otherwise.
Social empowerment measures:	
Age at marriage	- Age of person at first marriage.
Married before age 18	- Dummy variable that equals 1 if person was 18 years old or younger at first marriage, and 0 otherwise.
Intra-HH DM	- Index constructed based on woman's answers to a set of questions discovering if she has a say in making different decisions on within the household. The set of decisions includes: making large purchases for household; making purchases for daily needs; visiting family, friends or relatives; what food to be cooked; getting medical treatment or advice for herself; buying clothes for herself; buying clothes for children; taking children to the doctor; sending children to school; dealing with school-related issues. Each item is assigned the value of 1 if woman makes decision on her own or with husband and 0 otherwise. An index is computed by averaging z-scores and then standardizing.
Number of children at 30	- Total number of children woman had by the age of 30.
Number of children at 40	- Total number of children woman had by the age of 40.

Table B2. Descriptive statistics for ELMPS data.

	Women			Men		
	Unexposed (1)	Exposed (2)	p (3)	Unexposed (4)	Exposed (5)	p (6)
Educational outcomes						
University degree	0.09	0.12	0.121	0.22	0.20	0.357
	(0.28)	(0.32)		(0.41)	(0.40)	
Years of schooling	7.29	8.09	0.005	9.38	10.13	0.002
	(4.44)	(4.79)		(4.97)	(4.69)	
Marriage and social empowerment measures						
Age at marriage	19.97	21.22	0.003	27.12	27.32	0.488
	(4.85)	(4.96)		(4.85)	(4.96)	
Married before age 18	0.44	0.30	0.000	0.05	0.02	0.008
	(0.50)	(0.46)		(0.22)	(0.15)	
Intra-HH DM	0.08	0.48	0.000	-	-	-
	(0.54)	(0.66)		-	-	
Number of children at 30	1.03	2.58	0.000	-	-	-
	(1.77)	(1.96)		-	-	
Number of children at 40	1.41	3.89	0.000	-	-	-
	(2.32)	(2.13)		-	-	

Notes: Source - Egyptian Labor Market Panel Survey (ELMPS). Table shows means of variables and corresponding standard deviations (in parentheses) for sub-samples. Exposed (Unexposed) refers to individuals who were below (above) age 18 when university in province opened. Columns 3 and 6 present p-values for a t-test of mean difference between Exposed and Unexposed groups.