The Women Empowering Effect of Higher Education

Ahmed Elsayed*[a,b] and Alina Shirshikova†[b]

[a]IZA, Schaumburg-Lippe-Strasse 5-9, 53113 Bonn, Germany

[b]School of Business and Economics, Maastricht University, Tongersestraat 49, 6211 LM Maastricht, the Netherlands

Abstract

Exploiting a staggered rollout of establishing public universities across Egypt in 1960s-70s, we show that the opening of a new university in an individual's province significantly increased the likelihood of obtaining a higher education degree and improved labor market and marriage outcomes. The impact is more pronounced for women who are typically more socially constrained and less geographically mobile.

JEL Classification: I21, I23, J22, J24, O15, O55

Keywords: Higher education, Universities, Empowerment of women, Egypt

*Corresponding author, E-mail: elsayed@iza.org

†E-mail: a.shirshikova@maastrichtuniversity.nl

1 Introduction

It is well-established in the economic literature that more schooling is essential for escaping poverty and economic growth at both national and individual levels (see e.g., Becker, 1994; Card, 1999; Lochner, 2011). This is the case for the two genders alike, but it is particularly so for girls, given that investing in their education delivers high returns not only in terms of labor market outcomes, but also on a wide spectrum of aspects related to women's empowerment, marriage, fertility, maternal and children's health, democracy, and productivity (Duflo, 2012; Evans, Akmal, & Jakiela, 2021). However, significant gender disparities in education persisted for a long time across most developing countries due to pro-male preferences of parents and social norms which hindered women from pursuing more schooling, especially if the supply of education is comparatively less adapted to the needs of girls (Jayachandran, 2015; Meller & Litschig, 2016).

Over the last few decades, several developing countries adopted a policy agenda aiming at encouraging women to pursue more education. Moreover, most of these countries invested in increasing the supply of educational institutions across all levels of educations. This (gender-neutral) expansion of education has been shown to benefit girls more than boys (Evans & Yuan, 2021; Glick, 2008). These continuing efforts have been successful in reducing gender inequalities in education across the globe but the Middle East and North Africa (MENA hereafter) was particularly the region that saw the largest decline in gender disparity in education (World Bank, 2013). According to Evans, Akmal, and Jakiela (2021), women's education in the region has increased by more than six years over the time period 1960-2010. However, this positive pattern was not directly translated into better economic and social opportunities for women. Female labor force participation remained extremely low with only less than one-fifth of women in the region taking part[1] and the majority suffered from high levels of discrimination on several social domains (Elsayed, Namoro, & Roushdy, 2021). This trapped most of the women in the region into a vicious cycle of restricted access to labor markets, poor-quality marriage, and

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

increased fertility, leading to more dependence on men.

In this paper, we evaluate the causal impact of a public policy that aimed at making higher (university) education more available in Egypt, the epicenter and the most populous country in the MENA region, where during the 1960s and 1970s, the government aimed to construct a public university in each province. We exploit the staggered rollout of the opening of these universities to investigate the impact of access to university on higher education attainment, with a particular focus on the gender dimension. Social norms, which limited women's freedom of mobility and prevented them from obtaining education elsewhere are expected to make women benefit more from making higher education geographically closer.

Merging data on the time of university construction within individual's province together with individual-level data from the Egypt Labor Force Survey (LFS), we show that opening a local university significantly increases the probability to get higher education especially among women. The policy also has positive implications for the labor market outcomes. Our findings suggest that labor force participation among the treated women improved as an outcome of constructing a local university. While both genders benefited in terms of better quality of jobs, the impact was stronger for women, for whom the probability to work in paid employment and the probability of being engaged in high-skilled as well as top-management occupations increased significantly.

The policy also paid off in terms of better marriage outcomes for women, but not much so for men. The probability to get married to a highly-educated husband increased significantly, suggesting strong assortative mating based on education between couples. We further show that treated women have higher levels of intra-household decision-making. The overall findings suggest women's limited mobility is a binding constraint to obtaining higher education. This constraint can be relaxed by making higher education more accessible and geographically closer. This is in line with the literature highlighting that gender-neutral policies improving the supply of education often affect women more positively than men (e.g., Evans & Yuan, 2021; Glick, 2008). This can be particularly expected in a country with a strong preference for sons, such as Egypt (Elsayed & Marie, 2020). The findings also suggest that higher education is a major channel for economically and socially empowering women.

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

2 Related literature

With this paper, we contribute to three strands of the economic literature. The first deals with the returns to higher education. Higher education provides access to better-quality jobs that are not otherwise accessible, creates employment opportunities in top management and white-collar occupations, spurs social mobility, decrease inequalities, and yields as well high returns on several non-pecuniary domains. However, despite the importance of the topic for the developing world, the literature addressing the returns to higher education focuses mainly on advanced countries (e.g., Blundell et al., 2000; Card, 1995; Carneiro, Heckman, & Vytlacil, 2011; Walker & Zhu, 2008; Zimmerman, 2014), with only scarce evidence from developing countries (Hu & Bollinger, 2021; Kyui, 2016; Li, Whalley, & Xing, 2014; Peet, Fink, & Fawzi, 2015). Another important feature of this literature is the extensive emphasis on labor market returns, and the rather limited focus in considering marriage and other social outcomes (see Oreopoulos & Petronijevic, 2013; Psacharopoulos & Patrinos, 2018, for a review of this literature).

The second consists of the economic literature that measures several outcomes related to changes in the supply of educational institutions (mainly schools). For example, Duflo (2001, 2004) used the exposure to large-scale school construction in Indonesia to identify its effect on educational attainment and wages of male students, showing that the policy increased length of schooling by 0.27 years and led to a 3-7% increase in wages for the exposed cohorts. In the MENA region, Assaad and Saleh (2018) investigated school construction in Jordan and found that expansion in basic public schools increased inter-generational mobility. Lavy and Zablotsky (2015) found that the removal of travel restrictions on Israeli Arabs in 1963, which led to a reduction in the costs of primary and secondary schooling, raised female education and lowered women's fertility, but had almost no effect on men.

The third strand deals with gender differences in developing countries where women fare worse than men across several domains especially in countries with strong pro-male preferences where sons in the household are more likely than daughters to be vaccinated and get vitamin supplements (Barcellos, Carvalho, & Lleras-Muney, 2014), are breastfed longer (Chakravarty, 2015; Jayachandran & Kuziemko, 2011), and receive more child-care attention and better education (Barcellos, Carvalho, & Lleras-Muney, 2014; Choi & Hwang, 2015; Elsayed & Marie, 2020).[2] In general, our paper is one of the first that deals with higher education in the context of a developing country, evaluating the impact of making it more accessible, and estimating a broader scope of returns to higher education, not only in labor markets but also in the social aspects of marriage and social empowerment.

3 Institutional setting and data

3.1 Higher education in Egypt

The compulsory (basic) education in Egypt consists of nine grades: six years of primary school (ages 6-11), and three years of preparatory school (ages 12-14).[3] Upon successful completion of these two education levels, students could opt into the secondary stage, which comprises two alternative tracks: the vocational (technical) track and the general secondary 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 last for 4 years (i.e., ages 18-21)[4] with the only difference being that the vocational track focuses on

[2] see Jayachandran (2015) for a review of this literature.

[3] Over the time period 1988-2003, the years of primary school were cut by one year (Elsayed & Marie, 2020). The time frame we choose for our analyses does not cover this time period.

[4] Some of the programs within these institutions run for shorter (2 years) or longer (5 or 7 years) periods.

technical aspects while, the general track focuses more on preparing students for tertiary education.[5]

Before the Egyptian Revolution of 1952 higher education was the ‘education of the elite’ due to the scarce places and the high fees. As the monarchy was abolished in 1952 and a socialist government came into power in 1956, a program establishing ’education for all’ was initiated. A unified secondary school exit exam was introduced and tuition fees were abolished 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, the government constructed 15 new public universities between 1963-1976 across 15 different provinces that previously had no higher institutions on their territory (i.e., one university for each province).

The geographical expansion of higher education slowed down after 1976. In early 1980s, the government instituted several reforms to increase quality of education and combat overcrowding at universities by controlling admissions, changing examination policies and updating curriculum. In the mid-1980s, Egypt faced an economic crisis caused by a decline in oil prices that led to a significant reduction in government spending on education and initiated the policy shift towards the privatization of the economy. From 1995 onward private universities were licensed in the country. See Shann (1992) for a more detailed overview of educational policies of that time. We focus our analysis on individuals from provinces where universities were constructed between 1963-1976 and who were born between 1943-1964 to ensure that all persons in the sample were under similar circumstances in terms of higher education regulations and policies.

3.2 Data

Our empirical strategy draws upon two main sources of data. To get comprehensive information on higher education expansion, we constructed a novel dataset that contains

[5]For more information on the structure of education in Egypt, see Hanushek, Lavy, and Hitomi (2008).

information about each university in Egypt. The collected data includes information on the exact year of university construction, its location, and fields of study. To build this dataset we used national presidential decrees that, in accordance with Egyptian political tradition, legally accompany opening of each university or university branch. To eliminate possible inconsistency between the officially declared year of establishment and the de-facto year of first student intake, we double-checked the accuracy of the data by comparing it with the information provided on the official websites of universities. Note that several universities first started their operation as branches of tertiary institutions located in other provinces. Since we are interested in the date when individuals first got access to higher education, we use the date of the branch establishment instead of the date when it was separated from a parent institution and got university status. Table A1 in the Appendix lists Egyptian provinces by the year of first university (or university branch) construction.

To estimate the effect of access to university on educational attainment and longer-term outcomes in labor market and marriage, we exploit cross-sectional data from the 2006-2017 waves of the Egyptian Labor Force Survey (LFS). The LFS is a nationally-representative survey collected by the Central Agency for Public Mobilization and Statistics of Egypt (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 in a cross Egyptian provinces.

The dataset enables us to look at the impact of university expansion on the probability of having a university education and various labor market outcomes including (1) the individual's labor force participation, (2) probability of being engaged in paid employment, (3) probability of being employed in a white-collar job, (4) probability of holding a managerial position, and (6) wages. Using LFS, we can also estimate an impact of the policy on a selected number of marriage outcomes including educational and labor market characteristics of the spouse. The definitions of all variables used in the study are provided in Table A2 of the Online Appendix.

Since LFS does not contain information on fertility and social empowerment of women, we complement our analysis with data from the 1998, 2006, and 2012 waves of the Egyptian Labor Market Panel Survey (ELMPS). ELMPS is a nationally-representative panel survey that collects detailed information on family background, household structure, and marriage outcomes (OAMDI, 2016). We 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 the variables used.

3.3 Treatment, sample, and descriptive statistics

We focus on Egyptian provinces that got access to higher education during intensive 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 1943 and 1964, who had ever attended school. This results in a final sample size of 124,685 observations (35,183 women, and 89,502 men).

We use two sources of variation to define treatment: (1) variation in dates of access to higher education across provinces arising from the staggered nature of university construction, and (2) 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. Thus, we assign individuals who were 18 years old or younger when the first university in province opened to the treated group, while older cohorts from this province and individuals from provinces that have not yet got access to university education serve as a control group.

Table A3 in the Online Appendix shows the descriptive statistics for the two genders for the untreated cohorts. The table shows that the probability to have university education is 11% among women and 17% among men. Women on average receive about half a year of education less than men. While almost all men in the sample are engaged in work, women are less likely to join the labor force, with the share of labor market active women being only 28%. Women also have lower quality jobs: for those who are in employment the probabilities of having a paid job and a white collar job are 25% and 22% respectively, compared to 99% and 53% among men. Moreover, men are four times more likely to be in managerial position. However, when being in paid employment, women tend on average to receive higher wages.[6] The table further shows that the majority of the sample (99%) got married at least once and that husbands tend to be more educated and engaged in higher-quality jobs compared to wives.

4 Empirical strategy

4.1 Difference-in-difference

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

$$Y_{itr} = \beta T_{tr} + \gamma_r + \mu_t + \varepsilon_{itr} \tag{1}$$

where Y_{itr} is an outcome of interest for individual i of cohort t from province r. T_{tr} is a treatment dummy equal 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.[7] To account for any province-specific policies that could have affected cohorts non-randomly, we additionally control for province-specific time trends.

The main identifying assumption in our empirical analysis is that, in absence of university construction, treated and control individuals would have witnessed similar trends

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

[7] Due to the small number of clusters in our analysis standard errors may suffer from downward bias (Cameron, Gelbach, & Miller, 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 estimates.

in higher education attainment. We perform a formal test of the common trends assumption by estimating a specification with leads and lags in Section 4.2.

4.2 Event study

To graphically show the extent to which a university construction in one's 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:

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

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 the two genders separately. The first thing apparent is that there is 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 to students and, thus, 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. Moreover, the probability of getting higher education keeps growing across the treated cohorts, indicating a strong increase in the norm of getting higher education for girls. This is in line with the model proposed by Altonji, Blom, and Meghir (2012) predicting uncertainty about educational outcomes. As soon as more girls enroll in a university within province, younger cohorts get better informed about higher education possibilities and outcomes, which could further increase enrollment among girls.

To assure that the jump in the probability of obtaining university education was not artificially pushed by trends in higher education, we perform a falsification test by shifting the cut-off point 7 years before the actual university construction. Figure A1 in the Online Appendix shows evidence of no difference in outcomes for cohorts around this hypothetical date. We also test for different definition of treatment by using dates when university was constructed in the closest neighboring province as an alternative definition for treatment. Figure A2 in the Online Appendix shows that although the coefficients are positive for exposed cohorts of women, the effect is rather small and statistically insignificant.

5 Results

5.1 University degree attainment

Table 1 shows the estimates of Equation 1 of the impact of university construction on the probability of getting university degree for the overall sample combined and for the two genders 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 linear time trend to capture province-specific time trends. The two models provide similar results, thus, we will refer to 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 control group means.

Results in Column 2 show that in response to university construction, the probability to receive a university degree grew by 1.7 percentage points, representing an increase by about 11.5% from an average level of 15.2% for the untreated cohorts. Column 4 further shows that the impact is mainly driven by women, for whom the policy resulted in 3.7 percentage points increase in the probability to get a university degree, which corresponds to a 35% increase from the average level of 10.7% among the untreated. Men also witnessed a rise in the probability of getting a university education, however, the impact is statistically insignificant and rather small with a one percentage point increase, equivalent to about 5.8% increase from an average level of 16.7% for the untreated.[8]

We also estimate the effect on total years of schooling (Table A4 in the Online Appendix). University construction increased years of schooling for girls by 0.41 years, i.e. by about 5% from an average of 7.86 years among the untreated. The impact is smaller for men, with an increase of 0.15 years (1.8% relative to an average of 8.4 years among the untreated). To investigate the extent to which university construction could have downstream effects on the educational path prior to higher education, we evaluate the difference between the treatment and control groups in the probability to finish different levels of education. Following Equation 1, Figure A4 in the Online Appendix graphically shows the coefficient estimates based on separate estimations for each education level. The Figure shows no significant difference between the two groups on the probability to obtain educational degrees prior to university. The positive effect of the policy is clear only for the obtaining a university degree. This confirms the lack of pre-trend in earlier levels of educations between the two groups and suggests that university construction helps those who already finished at least secondary stage and are at the edge of deciding whether or not to enroll in higher education.

5.2 Internal mobility as a channel

Internal mobility (or rather lack of it) could be a channel that explains the results. While individuals can move to another province to get higher education, women are expected to be less likely to do so compared to men and this could negatively affect their ability to get access to higher education if a university does not exist in their province.[9]

[8]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 & 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 (Figure A3 in the Online Appendix).

[9]While financial constraints in terms of travel and accommodation costs could be a barrier encountered by both genders and limit their freedom of mobility to get education outside their home provinces, women have in addition higher levels of social constraints related to limited mobility and social norms against

The waves 2007-2011 of the LFS contain a detailed module on migration which enables us to check the pattern of internal mobility across provinces for the two genders prior to the policy and study the impact of university construction on mobility. Table A3 shows that for the untreated cohorts, the share of women who ever moved across provinces is 19%, which is significantly higher compared to men (12%). The purpose of migration is different between the two genders. The vast majority of women who ever migrated report family background as a reason for migration (91%) while the majority of men report work as the main purpose (62%). Across the two genders, only few individuals report study as a reason for migration (only 3% of ever-moved women and 5% of ever-moved men).

To evaluate the extent to which internal mobility was affected by the higher education policy, Table A5 in the Online Appendix estimates the effect of university construction on migration using the same specification from Equation (1). The effect is statistically insignificant for the two genders alike, suggesting that migration was not induced by construction of university. However, the table shows that, despite the economic insignificance, men are less likely to internally migrate as a response to university construction. When evaluating the effect on the probability of migration for the purpose of study, we find that treated men are less likely to report study as a purpose of migration while women do not seem to be affected. This suggests that the smaller impact of the university construction on men's higher education could be partially explained by men's reduction in migration for the purpose of education (i.e., as an outcome of the policy, the location of higher education obtained changed for men from universities outside their provinces to the newly-established local ones).

5.3 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. Female labor force participation increased by 4 percentage points, representing an increase of about 14.2% from the

women's social freedom.

average level of 28.1% among the untreated. The quality of labor market outcomes for women also improved. The probability of being in paid employment increased by 3.2 percentage points, which comprises an increase of about 13% from the average level of 24.9% among the untreated. Women exposed to higher education are by 4 percentage points more likely to work in a white-collar occupation (18.4% from an average level of 20%). The probability of being in top management also increased by 1.8 percentage points, representing an increase of 25.4% compared to the untreated cohorts, with the share of women in managerial positions equals 7.2%. However, women's wages conditional on work are not statistically different between treatment and control groups.[10]

Men are also positively affected by treatment, but the effect 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 control group. However, there is a positive impact on the probability of being in white-collar employment which increased by a one percentage point, representing an increase of 1.97% from the average of 53% among the control group. Managerial employment also increased by a similar amount of percentage points, but the effect is not statistically significant. Finally, wages among men increased by 2.9 log points in response to university construction.

5.4 Marriage and social empowerment of women

The positive impact of the policy is expected not to be limited to the labor market aspects but would also extend to the quality of marriage as families tend to use higher education (particularly of daughters) as a signal in the marriage market (see e.g., 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).

[10] 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 treatment dummy with a dummy variable for being married. Table A6 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 being employed in a managerial position which is lower for the exposed married women relative to the exposed unmarried (significant at 10% level). However, the results should be taken with caution, and are rather suggestive, given the potential endogeneity of marriage and labor market decisions.

For women, the education of partners assessed by years of education (Column 1) and the probability of having a university degree (Column 2) increased as a response to the policy. Husbands are also more likely to be formally employed (Column 3) although there is no evidence of being employed in top-management jobs (Column 4) or earning higher wages (Column 5). For men, however, marriage outcomes and the characteristics of spouses seem to be unaffected by university construction.

We support this evidence with more aspects related to the age at marriage, intra-household decision making (as a proxy for intra-household bargaining power), as well as fertility, using data from ELMPS.[11] Table B3 in the Online Appendix B shows that age at first marriage increased by about 1.2 years for the treated women from an average of about 20 years, and 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 treatment but remained unchanged at the age of 40, suggesting that access to higher education pushed women to postpone childbearing till a later age. Note, however, that due to the small number of observations these results should be taken with caution.

6 Concluding remarks

We exploit the staggered rollout of university constructions in Egypt to evaluate the impact of a policy that aimed at expanding the coverage of higher education in the 1960s-1970s by constructing universities in provinces with no prior access to higher education institutions. Using event study and difference-in-difference techniques, we document an increase in the share of individuals with higher education as an outcome of the policy. This suggests that establishing local universities made getting higher education easier and more accessible. We show that the impact is more pronounced among women who were generally less mobile and more socially constrained.

We also find a positive impact on labor market outcomes, particularly for women who experienced an increase in labor force participation and paid employment. Treated

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

women 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 treated women are more likely to get married to better-educated husbands and are more likely to be socially empowered in their households.

The findings of the paper underscore the role of mobility barriers in curbing investments in higher education, particularly among women in developing countries. Making higher education more accessible can be a successful policy for providing opportunities for economic and social empowerment of women and could contribute towards reducing gender disparities.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper

Acknowledgements

We acknowledge the comments of participants in the 26th Annual Conference of the Economic Research Forum in Cairo (Online), the 2020 IZA-ECONtribute Human Capital Workshop, the 4th IZA/WB/NJD Jobs and Development annual conference, the 2021 Oxford Centre for the Study of African Economies (CSAE) annual conference, and the Economics Research Seminar of the American University in Cairo, October 2021.

References

Altonji, Joseph G., Erica Blom, and Costas Meghir. "Heterogeneity in Human Capital Investments: High School Curriculum, College Major, and Careers". *Annual Review of Economics* 4 (2012), 185–223. ISSN: 1941-1383. DOI: 10.1146/annurev-economics-080511-110908.

Ashraf, Nava, Natalie Bau, Nathan Nunn, and Alessandra Voena. "Bride Price and Female Education". *Journal of Political Economy* 128 (2020), 591–641. DOI: 10.1086/704572.

Assaad, Ragui and Mohamed Saleh. "Does Improved Local Supply of Schooling Enhance Intergenerational Mobility in Education? Evidence from Jordan". *World Bank Economic Review* 32 (2018), 633–655. DOI: 10.1093/wber/lhw041.

Barcellos, S. H., L. S. Carvalho, and A. Lleras-Muney. "Child Gender and Parental Investments In India: Are Boys And Girls Treated Differently?" *American Economic Journal: Applied Economics* 6 (2014), 157–189. DOI: 10.1257/app.6.1.157.

Becker, Gary S. *Human Capital: A Theoretical and Empirical Analysis, with Special Reference to Education*. 3rd ed. Chicago and London: University of Chicago Press, 1994. ISBN: 0-226-04119-0.

Blundell, Richard, Lorraine Dearden, Alissa Goodman, and Howard Reed. "The Returns to Higher Education in Britain: Evidence from a British Cohort." *Economic Journal* 110 (2000), 82–99. URL: http://www.jstor.org/stable/2565942.

Cameron, A. Colin, Jonah B. Gelbach, and Douglas L. Miller. "Bootstrap-Based Improvements for Inference with Clustered Errors". *The Review of Economics and Statistics* 90 (2008), 414–427. ISSN: 0034-6535. DOI: 10.1162/rest.90.3.414.

Cameron, A. Colin and Douglas L. Miller. "A Practitioner's Guide to Cluster-robust Inference". *Journal of Human Resources* 50 (2015), 317–372. DOI: 10.3368/jhr.50.2.317.

Card, David. *Using Geographic Variation in College Proximity to Estimate the Return to Schooling*. Ed. by L.N. Christofides, E.K. Grant, and R. Swidinsky. Aspects of Labor Market Behaviour: Essays in Honour of John Vanderkamp. Toronto: University of Toronto Press, 1995. DOI: 10.3386/w4483.

Card, David. *The Causal Effect of Education on Earnings*. Ed. by Orley C. Ashenfelter and David Card. 3. Handbook of Labor Economics. Elsevier, 1999, 1801–1863. DOI: 10.1016/S1573-4463(99)03011-4.

Carneiro, Pedro, James J. Heckman, and Edward J. Vytlacil. "Estimating Marginal Returns to Education". *The American Economic Review* 101 (2011), 2754–2781. ISSN: 00028282. DOI: 10.1257/aer.101.6.2754.

Chaisemartin, Clément de and Xavier D'Haultfœuille. "Two-Way Fixed Effects Estimators with Heterogeneous Treatment Effects". *American Economic Review* 110 (2020), 2964–2996. DOI: 10.1257/aer.20181169.

Chakravarty, Abhishek. "Gender-Biased Breastfeeding in Egypt: Examining the Fertility Preference Hypotheses of Jayachandran and Kuziemko (2011)". *Journal of Applied Econometrics* 30 (2015), 848–855. DOI: 10.1002/jae.2445.

Choi, Eleanor and Jisoo Hwang. "Child Gender and Parental Inputs: No More Son Preference in Korea?" *American Economic Review* 105 (2015), 638–643. DOI: 10.1257/aer.p20151118.

Duflo, Esther. "Schooling and Labor Market Consequences of School Construction in Indonesia: Evidence from an Unusual Policy Experiment". *American Economic Review* 91 (2001), 795–813. DOI: 10.1257/aer.91.4.795.

Duflo, Esther. "The Medium Run Effects of Educational Expansion: Evidence from a Large School Construction Program in Indonesia". *Journal of Development Economics* 74 (2004), 163–197. ISSN: 0304-3878. DOI: 10.1016/j.jdeveco.2003.12.008.

Duflo, Esther. "Women Empowerment and Economic Development". *Journal of Economic Literature* 50 (2012), 1051–79. DOI: 10.1257/jel.50.4.1051.

Elsayed, Ahmed and Olivier Marie. *Less School (Costs), More (Female) Education? Lessons from Egypt Reducing Years of Compulsory Schooling*. Working Paper (No. 37). Tinbergen Institute Discussion Papers, 2020. DOI: 10.2139/ssrn.3637898.

Elsayed, Ahmed, Soiliou Daw Namoro, and Rania Roushdy. "Empowering women in conservative settings: evidence from an intervention in rural Egypt". *Review of Economics of the Household* (2021), 1–36. DOI: 10.1007/s11150-021-09576-5.

Evans, David K., Maryam Akmal, and Pamela Jakiela. "Gender Gaps in Education: The Long View." *IZA Journal of Development and Migration* 12 (2021), –. DOI: 10.2478/izajodm-2021-0001.

Evans, David K and Fei Yuan. "What We Learn about Girls' Education from Interventions That Do Not Focus on Girls". *The World Bank Economic Review* (2021), lhab007. ISSN: 0258-6770, 1564-698X. DOI: 10.1093/wber/lhab007.

Gezi, Kalil I. "The Educational System of the Arab Republic of Egypt". In: *Education around the World Series*. Washington, DC: U.S. Government Printing Office: U.S. Dept. of Health, Education, and Welfare, Office of Education, 1979.

Glick, Peter. "What Policies will Reduce Gender Schooling Gaps in Developing Countries: Evidence and Interpretation". *World Development* 36 (2008), 1623–1646. ISSN: 0305750X. DOI: 10.1016/j.worlddev.2007.09.014.

Hanushek, Eric A, Victor Lavy, and Kohtaro Hitomi. "Do students care about school quality? Determinants of dropout behavior in developing countries". *Journal of Human Capital* 2 (2008), 69–105. DOI: 10.1086/529446.

Hu, Chenxu and Christopher Bollinger. "Effects of cohort size on college premium: Evidence from China's higher education expansion". *China Economic Review* 70 (2021), 101700. ISSN: 1043-951X. DOI: 10.1016/j.chieco.2021.101700.

Jayachandran, Seema. "The Roots of Gender Inequality in Developing Countries". *Annual Review of Economics* 7 (2015), 63–88. DOI: 10.1146/annurev-economics-080614-115404.

Jayachandran, Seema and Ilyana Kuziemko. "Why Do Mothers Breastfeed Girls Less than Boys? Evidence and Implications for Child Health in India". *The Quarterly Journal of Economics* 126 (2011), 1485–1538. DOI: 10.2307/23015706.

Kyui, Natalia. "Expansion of higher education, employment and wages: Evidence from the Russian Transition". *Labour Economics* 39 (2016), 68–87. ISSN: 0927-5371. DOI: 10.1016/j.labeco.2016.01.001.

Lavy, Victor and Alexander Zablotsky. "Women's schooling and fertility under low female labor force participation: Evidence from mobility restrictions in Israel". *Journal of*

Public Economics 124 (2015), 105–121. ISSN: 0047-2727. DOI: 10.1016/j.jpubeco.2015.02.009.

Li, Shi, John Whalley, and Chunbing Xing. "China's Higher Education Expansion and Unemployment of College Graduates". *China Economic Review* 30 (2014), 567–582. ISSN: 1043-951X. DOI: 10.1016/j.chieco.2013.08.002.

Lochner, Lance. *Non-Production Benefits of Education: Crime, Health, and Good Citizenship*. Working Paper (No. 16722). National Bureau of Economic Research, 2011. DOI: 10.3386/w16722.

Meller, Marian and Stephan Litschig. "Adapting the supply of education to the needs of girls: Evidence from a policy experiment in Rural India". *Journal of Human Resources* 51 (2016), 760–802. DOI: 10.3368/jhr.51.3.0612-5000R.

OAMDI, [dataset]. Labor Market Panel Surveys (LMPS). Version 4.2 of Licensed Data Files; ELMPS 1998, 2006 and 2012. Egypt: Economic Research Forum (ERF), (2016). URL: http://erf.org.eg/data-portal/.

OAMDI, [dataset]. Harmonized Labor Force Surveys (HLFS). Version 1.0 of Licensed Data Files; LFS 2006-2017- Central Agency for Public Mobilization and Statistics (CAPMAS). Egypt: Economic Research Forum (ERF), (2019). URL: http://erf.org.eg/data-portal/.

Oreopoulos, Philip and Uros Petronijevic. "Making College Worth It: A Review of the Returns to Higher Education". *The Future of Children* 23 (2013), 41–65. ISSN: 10548289, 15501558. URL: http://www.jstor.org/stable/23409488.

Peet, Evan D., Günther Fink, and Wafaie Fawzi. "Returns to education in developing countries: Evidence from the living standards and measurement study surveys". *Economics of Education Review* 49 (2015), 69–90. ISSN: 0272-7757. DOI: https://doi.org/10.1016/j.econedurev.2015.08.002.

Psacharopoulos, George and Harry Anthony Patrinos. "Returns to Investment in Education: a Decennial Review of the Global Literature". *Education Economics* 26 (2018), 445–458. DOI: 10.1080/09645292.2018.1484426.

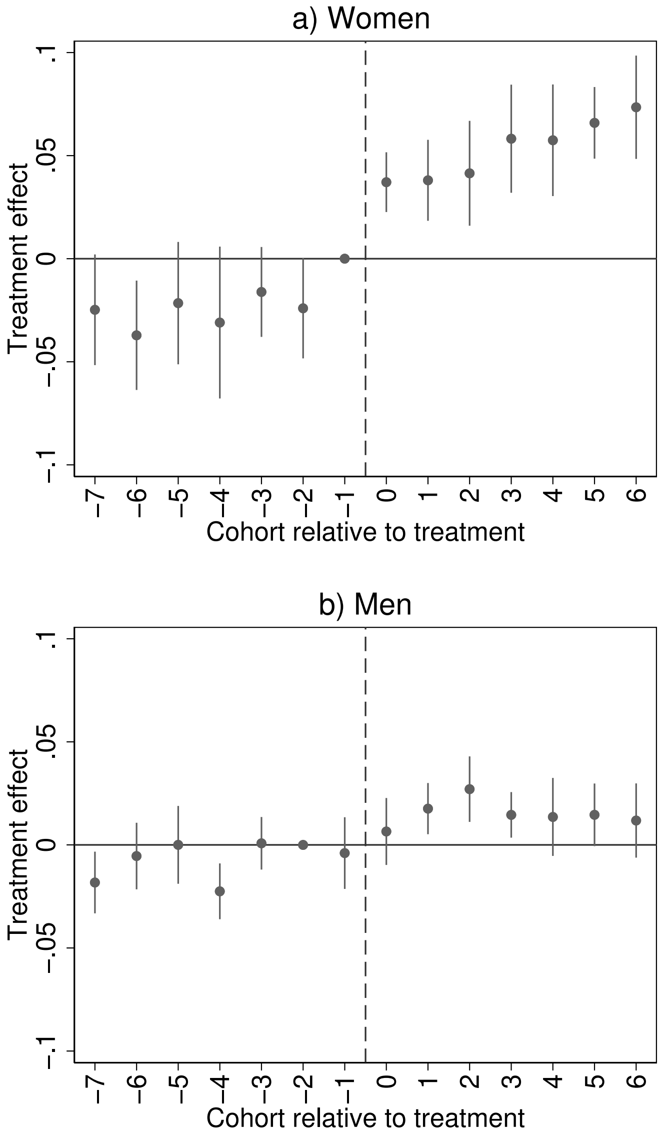
Shann, Mary H. “The Reform of Higher Education in Egypt”. *Higher Education* 24 (1992), 225–246. URL: https://www.jstor.org/stable/3447748.

Walker, Ian and Yu Zhu. “The College Wage Premium and the Expansion of Higher Education in the UK”. *The Scandinavian Journal of Economics* 110 (2008), 695–709. DOI: 10.1111/j.1467-9442.2008.00557.x.

World Bank. “Opening Doors: Gender Equality and Development in the Middle East and North Africa”. MENA Development Report (2013). URL: http://hdl.handle.net/10986/12552.

Zimmerman, Seth D. “The Returns to College Admission for Academically Marginal Students”. *Journal of Labor Economics* 32 (2014), 711–754. ISSN: 0734306X, 15375307. DOI: 10.1086/676661.

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 coefficients of relative to event cohort dummies from a regression estimating Equation 2. Dependent variable – dummy = 1 if person finished university education, zero otherwise. Each point represents the coefficient for a cohort who was of particular age at time when university opened: x=0 corresponds to the oldest treated cohort, x=-1 - youngest untreated cohort, etc. Cohort x=-1 serves as baseline. 95% CI are shown on the graphs. The vertical line indicates the moment of treatment.

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

	University degree					
	All		Women		Men	
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	0.015***	0.017***	0.035***	0.037***	0.007	0.010
	(0.005)	(0.005)	(0.008)	(0.008)	(0.005)	(0.006)
	[0.040]	[0.038]	[0.002]	[0.005]	[0.275]	[0.192]
Observations	124,685	124,685	35,183	35,183	89,502	89,502
Mean of Outcome	0.152	0.152	0.107	0.107	0.167	0.167
Effect size, %	10.12	11.49	32.91	35.00	3.98	5.83
Cohort FE	✓	✓	✓	✓	✓	✓
Province FE	✓	✓	✓	✓	✓	✓
Cohort × Province FE		✓		✓		✓

Notes: Dependent variable is a dummy variable that equals 1 if person finished university education, zero otherwise. *Treated* is a dummy variable that equals 1 if person was below age 18 when university opened, and 0 otherwise. All regressions include survey wave dummies as controls. The whole sample regressions in columns (1)-(2) also include a dummy variable for gender. Effect size calculated as coefficient of *Treated* divided on mean of the outcome for unexposed cohorts. P-value of the t-test for a difference in the coefficients between men and women equals 0.001. Standard errors clustered at province level (14 clusters) are reported in parentheses. Wild-bootstrapped p-values 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					
Treated	0.040***	0.032**	0.040***	0.018**	-0.005
	(0.013)	(0.011)	(0.007)	(0.007)	(0.026)
	[0.014]	[0.022]	[0.022]	[0.024]	[0.877]
Observations	30,933	30,933	30,933	30,933	11,723
Mean of Outcome	0.281	0.249	0.219	0.072	1.873
Effect size, %	14.20	12.93	18.43	25.36	-0.26
Panel B: Male sample					
Treated	0.000	-0.002	0.010*	0.011	0.028*
	(0.001)	(0.002)	(0.006)	(0.006)	(0.014)
	[0.911]	[0.349]	[0.083]	[0.091]	[0.069]
Observations	71,638	71,638	71,638	71,638	44,483
Mean of Outcome	0.992	0.986	0.525	0.291	1.763
Effect size, %	0.01	-0.16	1.97	3.65	1.62
t-test for difference in coefficients					
t-test p-value	0.000	0.000	0.000	0.000	0.006

Notes: *Treated* is a dummy variable that equals 1 if person was below age 18 when university opened, and equals 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 *Treated* divided on mean of the outcome for unexposed cohorts. 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 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 formal employment (3)	Spouse top management (4)	Spouse hourly wage, log (5)
Panel A: Female sample					
Treated	0.435**	0.033**	0.062**	0.014	0.007
	(0.155)	(0.014)	(0.025)	(0.012)	(0.053)
	[0.043]	[0.094]	[0.067]	[0.316]	[0.889]
Observations	22,591	25,309	9,207	25,309	9,918
Mean of Outcome	9.836	0.259	0.819	0.128	1.744
Effect size, %	4.42	12.71	7.60	11.05	0.41
Panel B: Male sample					
Treated	0.065	0.006	0.005	0.003	-0.037
	(0.089)	(0.007)	(0.015)	(0.003)	(0.023)
	[0.505]	[0.555]	[0.757]	[0.345]	[0.132]
Observations	76,474	85,264	20,279	85,264	16,442
Mean of Outcome	5.143	0.082	0.703	0.035	1.518
Effect size, %	1.26	6.72	0.74	7.65	-2.41
t-test for difference in coefficients					
t-test p-value	0.649	0.022	0.110	0.000	0.006

Notes: *Treated* is a dummy variable that equals 1 if person was below age 18 when university opened, and equals 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 *Treated* divided on mean of the outcome for unexposed cohorts. 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 are presented in brackets. $*p < 0.1, **p < 0.05, ***p < 0.01$.

APPENDIX A

Table A1. 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
Luxor	South Valley university	1970
Qena	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: Table shows a list of Egyptian provinces with the corresponding dates of an access to higher education. Greater Cairo includes Cairo, Giza, and Qalyubia. Red Sea and South Sinai provinces does not have higher education institutions on its territory and, thus, are not listed. Provinces shown in grey are not included into our sample. Port Said, Suez, Ismailia and North Sinai were excluded from sample since these provinces were affected by the Arab-Israeli conflicts during 1967-1973. Since Luxor is surrounded by Qena province, with travelling distance to Qena university being short, we assign Qena's date of university construction to Luxor.

Table A2. Definition of variables.

Variable name	Definition
Treatment variables:	
Treated	- Dummy variable that equals 1 if person was 18 years old or younger when university in her/his province opened, and 0 otherwise.
Treated by the neighbouring province	- Dummy variable that equals 1 if person was 18 years old or younger when university in the closest of the earlier affected neighbouring provinces opened, and 0 otherwise. Distance between provinces is proxied by map distance between their capital cities.
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 on the labor market (employed or looking for a job), and 0 otherwise. Sample restricted to people belonging to labor force, i.e. 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 belonging to labor force, i.e. retired, pensioners, disabled and students are excluded.
White collar	- Dummy variable that equals 1 if person performs professional, managerial, desk or administrative work, and 0 otherwise. Sample restricted to people belonging to labor force, i.e. 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 belonging to labor force, i.e. 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.
Ever married	- Dummy variable that equals 1 if individual has ever been married, 0 otherwise
Spouse's years of education	- Number of completed by spouse effective years of schooling (without grade repetition).

Outcome variables (continued):	
Spouse university degree	- Dummy variable that equals 1 if spouse has completed university education, 0 otherwise.
Spouse formal employment	- Dummy variable that equals 1 if spouse is officially hired (has written contract or social security contributions at work), and 0 otherwise.
Spouse top management	- Dummy variable that equals 1 if spouse is a senior manager, legislator or senior official, and 0 otherwise.
Spouse hourly wage, log	- Logarithm of total hourly wage of spouse from the main job. Wages are adjusted for inflation. Sample restricted to employed individuals.
Ever moved	- Dummy variable that equals 0 for individuals who have never moved from their province of birth, and equals 1 otherwise. Available only for waves 2007-2009 of the LFS.
Moved for study	- Dummy variable that equals 1 for individuals who have moved from their province of birth for study purpose, and equals 0 if moved for any other reason. Available only for waves 2007- 2009 of the LFS. Sample restricted to individuals who have ever migrated across provinces.

Table A3. Descriptive statistics.

	Women (1)	Men (2)	p-value (3)
Educational outcomes			
University degree	0.11	0.17	0.000
	(0.31)	(0.37)	
Years of schooling	7.86	8.37	0.000
	(5.14)	(5.46)	
Labour market outcomes			
Labor market participation	0.28	0.99	0.000
	(0.45)	(0.09)	
Paid job	0.25	0.99	0.000
	(0.43)	(0.12)	
White collar	0.22	0.53	0.000
	(0.41)	(0.50)	
Top management	0.07	0.29	0.000
	(0.26)	(0.45)	
Hourly wage, log	1.87	1.76	0.000
	(0.55)	(0.62)	
Marriage outcomes			
Ever married	0.99	0.99	0.000
	(0.10)	(0.06)	
Spouse's years of education	9.84	5.14	0.000
	(5.77)	(6.00)	
Spouse university degree	0.26	0.08	0.000
	(0.44)	(0.27)	
Spouse formal employment	0.82	0.70	0.000
	(0.39)	(0.46)	
Spouse top management	0.13	0.03	0.000
	(0.33)	(0.18)	
Spouse hourly wage, log	1.74	1.52	0.000
	(0.78)	(0.61)	
Migration (2007-2011)			
Ever moved	0.19	0.12	0.000
	(0.39)	(0.32)	
Moved for study	0.03	0.05	0.017
	(0.16)	(0.21)	
Moved for work	0.03	0.62	0.000
	(0.16)	(0.49)	
Moved for marriage/family reasons	0.91	0.28	0.000
	(0.29)	(0.45)	
Moved for other reasons	0.04	0.06	0.024
	(0.18)	(0.23)	

Notes: Table shows means of variables and the corresponding standard deviations (in parentheses) for unexposed individuals separately for two genders. Column (3) presents p-values for a t-test of mean difference between Female and Male samples.

Table A4. Impact of access to university on years of schooling.

	Years of schooling					
	All		Women		Men	
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	0.211***	0.241***	0.457***	0.407***	0.100	0.152**
	(0.068)	(0.046)	(0.102)	(0.084)	(0.084)	(0.070)
	[0.020]	[0.001]	[0.015]	[0.003]	[0.295]	[0.069]
Observations	124,685	124,685	24,983	24,983	89,502	89,502
Mean of Outcome	8.24	8.24	7.86	7.86	8.37	8.37
Effect size, %	2.56	2.92	5.81	5.18	1.20	1.82
Cohort FE	✓	✓	✓	✓	✓	✓
Province FE	✓	✓	✓	✓	✓	✓
Cohort × Province FE		✓		✓		✓

Notes: Dependent variable – number of completed by individual years of education (without grade repetition). *Treated* is a dummy variable that equals 1 if person was below age 18 when university opened, and 0 otherwise. All regressions include survey wave dummies as control. The whole sample regressions in columns (1)-(2) also include a dummy variable for gender. Effect size calculated as coefficient of *Treated* divided on mean of the outcome for unexposed cohorts. P-value of the t-test for a difference in the coefficients between men and women equals 0.030. Standard errors clustered at province level (14 clusters) are reported in parentheses. Wild-bootstrapped p-values are presented in brackets. $*p < 0.1, **p < 0.05, ***p < 0.01$.

Table A5. The effect of the policy on cross-province migration.

	All	Women	Men
	(1)	(2)	(3)
Panel A: Probability of internal mobility			
Treated	-0.003	0.001	-0.004
	(0.010)	(0.008)	(0.012)
	[0.815]	[0.896]	[0.767]
Observations	57,806	16,190	41,616
Mean of Outcome	0.134	0.188	0.116
Effect size, %	-2.00	0.54	-3,83
Panel B: Probability of mobility for the purpose of study			
Treated	-0.009	-0.002	-0.015*
	(0.007)	(0.015)	(0.008)
	[0.427]	[0.985]	[0.220]
Observations	5,418	1,973	3,445
Mean of Outcome	0.040	0.027	0.047
Effect size, %	-23.27	-7.18	-31.82

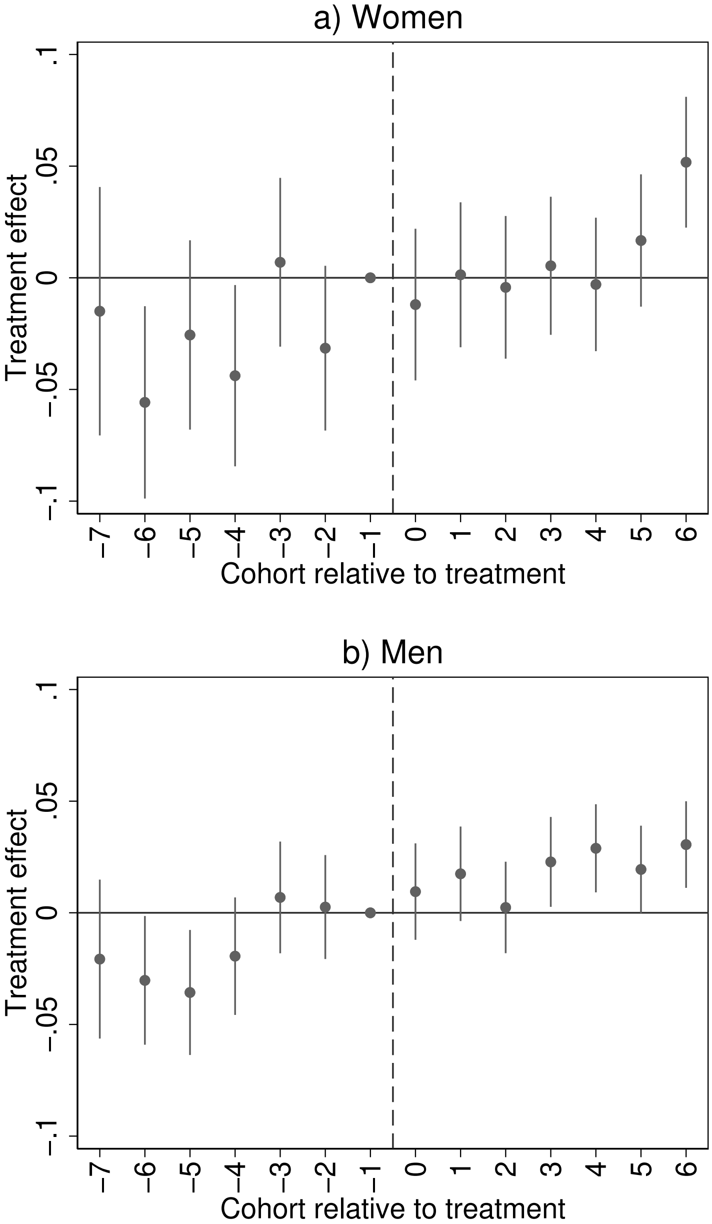
Notes: Data on migration are only available for waves 2007-2009 of the Labor Force Survey. Dependent variable in Panel A is a dummy that equals 0 if individual never moved from his/her province of birth, and equals 1 otherwise. Dependent variable in Panel B is a dummy that equals 1 if individual moved from his/her province of birth for study purpose, and equals 0 if moved for other reason. Treatment is assigned by the respondent's place of birth. All regressions include survey wave dummies, birth cohort and province FEs, and province-specific time trend as controls. Effect size calculated as coefficient of *Treated* divided on mean of the outcome for unexposed cohorts. P-value of the t-test for a difference in the coefficients between men and women equals 0.132 for Panel A and 0.233 for Panel B. Standard errors clustered at province level (14 clusters) are reported in parentheses. Wild-bootstrapped p-values are presented in brackets. $*p < 0.1, **p < 0.05, ***p < 0.01$.

Table A6. 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)
Treated **x** Married	-0.019	-0.010	-0.017	-0.014*	0.014
	(0.014)	(0.018)	(0.014)	(0.008)	(0.037)
Treated	0.053***	0.039**	0.052***	0.028***	-0.019
	(0.015)	(0.014)	(0.013)	(0.009)	(0.027)
Married	0.028**	0.000	0.038***	0.007	-0.005
	(0.012)	(0.015)	(0.010)	(0.008)	(0.038)
Observations	30,933	30,933	30,933	30,933	11,723

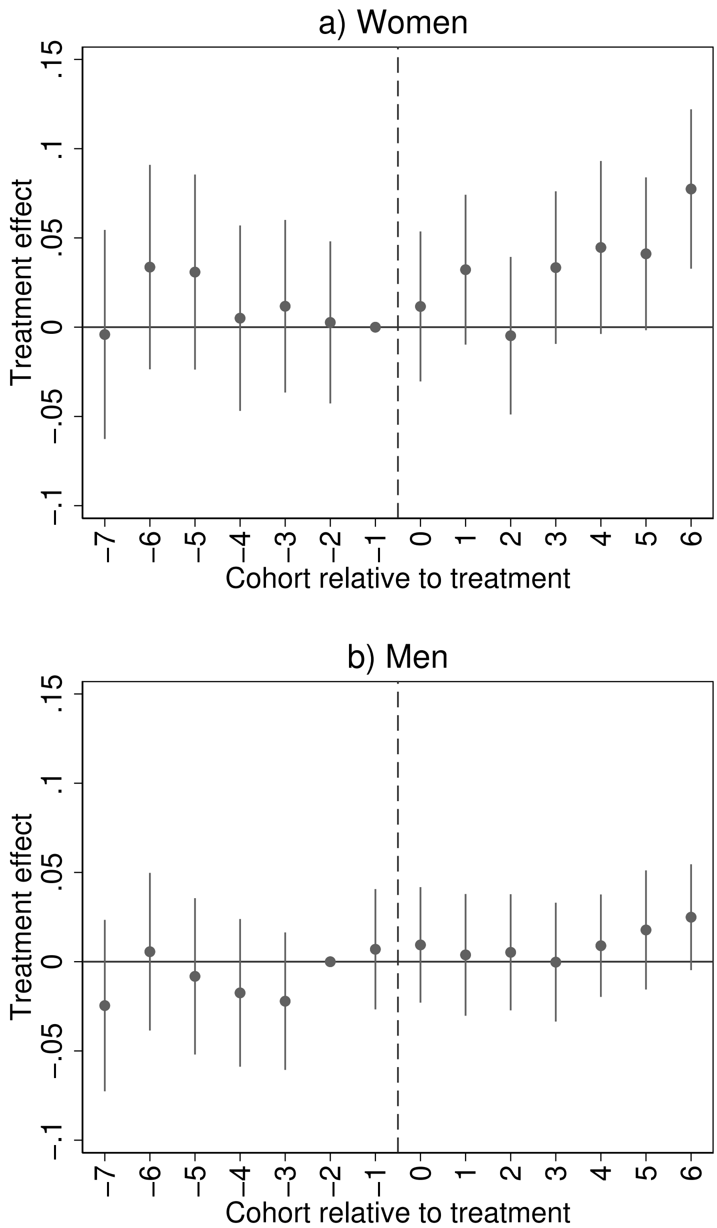
Notes: *Treated* is a dummy variable that equals 1 if person was below age 18 when university opened, and equals 0 otherwise. All regressions include survey wave dummies, birth cohort and province FEs, and province-specific time trend as controls. Standard errors clustered at province level (14 clusters)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

Figure A1: Placebo date test. Plotted coefficients of the relative to event cohort dummies by gender.



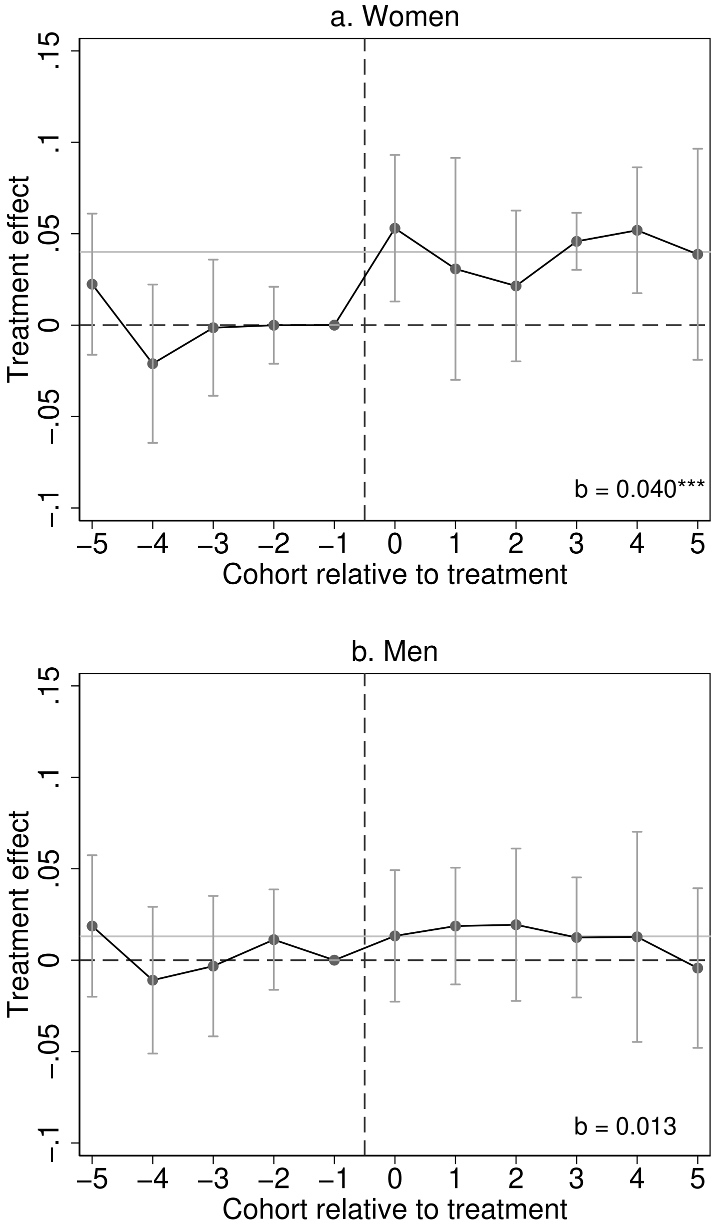
Notes: Picture shows estimates for a placebo regression (six years before the actual university construction) for Equation 2. Dependent variable – dummy = 1 if person finished university education, zero otherwise. Each point represents the coefficient for a cohort who was of particular age at the time of treatment: x=0 corresponds to the oldest treated cohort, x=-1 - youngest untreated cohort, etc. Cohort x=-1 serves as baseline. 95% CI are shown on the graphs. The vertical line indicates the implied moment of treatment.

Figure A2: Treatment defined by the neighbouring province. Plotted coefficients of the relative to event cohort dummies by gender.



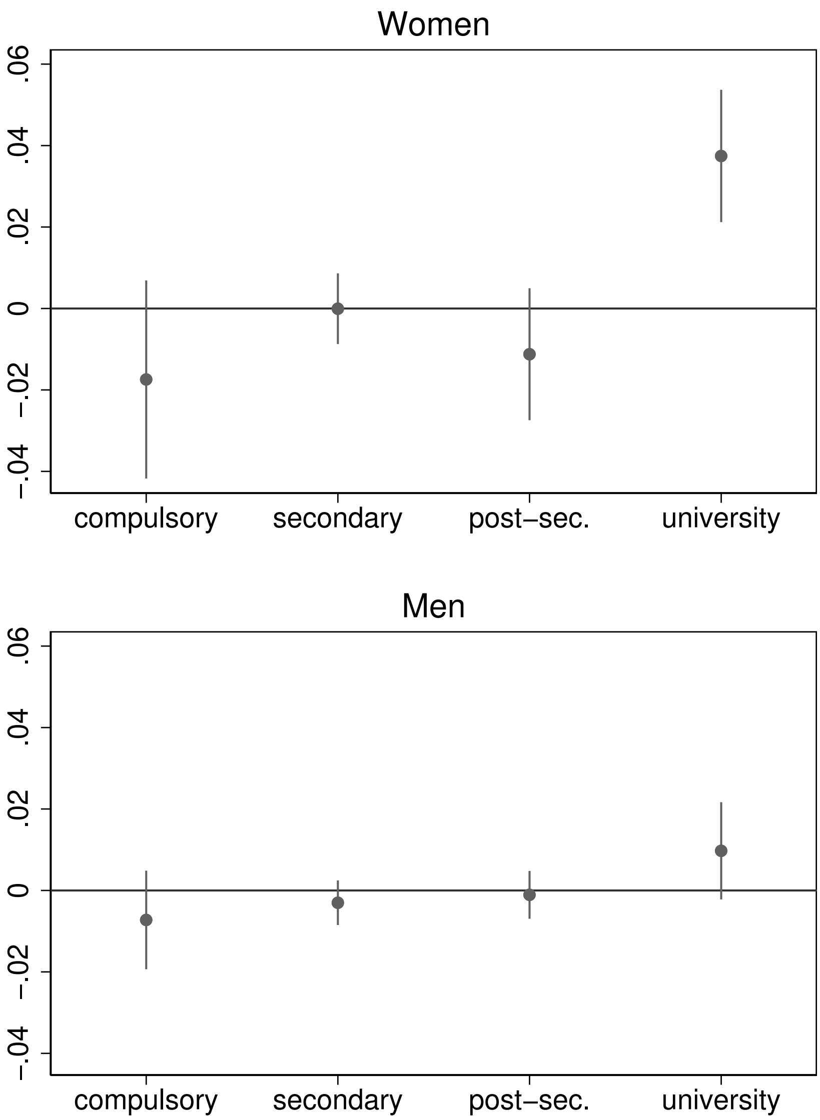
Notes: Picture shows estimates of Equation 2 where treatment is defined using a date of university construction in the closest neighboring province (measured by map distance between capital cities) that got access to higher education earlier. Dependent variable – dummy = 1 if person finished university education, zero otherwise. Each point represents the coefficient for a cohort who was of particular age at the time of treatment: x=0 corresponds to the oldest treated cohort, x=-1 - youngest untreated cohort, etc. Cohort x=-1 serves as baseline. 95% CI are shown on the graphs. The vertical line indicates the implied moment of treatment.

Figure A3: Robust to heterogeneous treatment effects DID estimates.



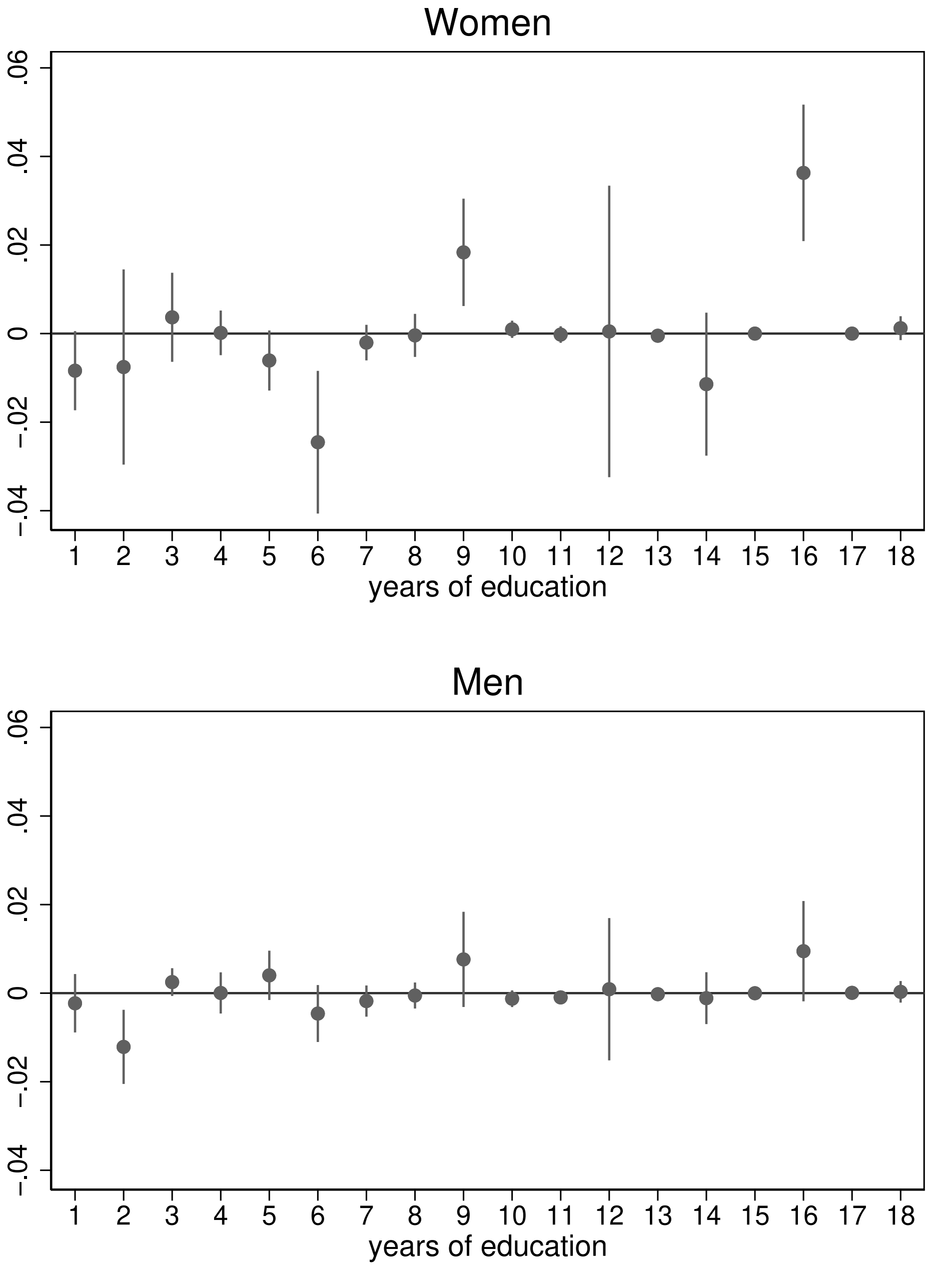
Notes: The graph presents robust to heterogeneous treatment effects estimates (Chaisemartin & D'Haultfœuille, 2020) computed by *did_multiplegt* Stata package. Dependent variable – dummy = 1 if person finished university education, zero otherwise. Estimates for x from -5 to -1 compare switchers' and non-switchers' outcome evolution before switchers switch. Them being significantly different from 0 would mean violation of parallel trends assumption. Estimates for x from 0 to 5 show switchers' treatment effect at period when they switch. Moment x=0 corresponds to the treatment occurrence, x=1 - one period after the treatment occurrence, etc. b denotes the estimated average effect of treatment and is depicted by horizontal grey line on the graphs. Standard errors are calculated using 99 bootstrap replications, 95% CI are shown on the graphs. $*p < 0.1, **p < 0.05, ***p < 0.01$.

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



Notes: The graph plots coefficients before the treatment dummy from the regressions that estimate Equation 1 for two genders with the different levels of educational attainment as dependent variables (shown on x-axis). Variable *compulsory* is a dummy indicator = 1 if person have 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 person have finished university education, zero otherwise. All regressions include birth cohort and province FEs, survey wave dummies and province-specific time trend as controls. 95% CI are shown on the graphs.

Figure A5: (auxiliary) Impact of treatment on completed years of schooling



Notes: The graphs plot coefficients before the treatment dummy from the regressions that estimate Equation 1 for two genders with the maximal completed years of education as dependent variables (shown on x-axis). Dependent variables are dummy indicators that equal one if individual have completed exactly $n \in [1; 18]$ years of schooling, and zero otherwise. All regressions include birth cohort and province FEs, survey wave dummies and province-specific time trend as controls. 95% CI are shown on the graphs.

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 restriction as in our main analysis. We are looking at individuals born between 1943 and 1964 in provinces that witnessed university construction in 1960s-1970s, who had attended school. Table B1 below provides definitions of variables we used in our analysis of 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:	
Treated	- Dummy variable that equals 1 if person was 18 years old or younger when university in her province opened, and 0 otherwise.
Social empowerment measures:	
Age at marriage	- Age of woman at her first marriage.
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 (1)	Men (2)	p-value (3)
Educational outcomes			
University degree	0.09	0.22	0.000
	(0.28)	(0.41)	
Years of schooling	7.29	9.38	0.000
	(4.44)	(4,97)	
Social empowerment measures			
Age at marriage	19.97	27.12	0.000
	(4.85)	(5.63)	
Intra-HH DM	0.08	-	-
	(0.54)		
Number of children at 30	1.03	-	-
	(1.77)		
Number of children at 40	1.41	-	-
	(2.32)		

Notes: Source - Egyptian Labor Market Panel Survey (ELMPS). Table shows means of variables and the corresponding standard deviations (in parentheses) for unexposed individuals separately for two genders. Column (3) presents p-values for a t-test of mean difference between Female and Male samples.

Table B3. Impact of access to university on social empowerment of women, ELMPS data.

	University degree (1)	Age at marriage (2)	Intra-HH DM (3)	Number of children at 30 (4)	Number of children at 40 (5)
Treated	0.069**	1.235**	0.279**	-0.342***	0.095
	(0.028)	(0.442)	(0.128)	(0.109)	(0.175)
	[0.030]	[0.080]	[0.089]	[0.153]	[0.751]
Observations	1,133	897	611	575	524
Mean of Outcome	0.089	19.969	0.078	1.027	1.411
Effect size, %	77.03	6.18	357.82	-33.33	6.7

Notes: Source - Egyptian Labor Market Panel Survey (ELMPS). The analysis is limited to women. *Treated* is a dummy variable that equals 1 if person was below age 18 when university opened, and equals 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 *Treated* 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 are presented in brackets. $*p < 0.1, **p < 0.05, ***p < 0.01$.