Banning women from STEM: Evidence from Iran*

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Abstract:

We study the effects of an Iranian educational policy implemented in 2012 that restricted access to higher education for women in about 30% of Iran's public universities, mostly in the popular field of engineering. Exploiting differences in exposure to the policy across gender, cohorts and regions, we find that this unexpected reduction in programs decreased university attendance of women relative to men. We investigate heterogeneous effects and mechanisms and find that the policy effect is particularly strong for women from urban households, poorer families and from more conservative provinces. Furthermore, the policy had a negative effect on the probability for women to marry at a young age and, for those women who marry, on the education and income level of their husbands. We further find a mixed impact on labor market outcomes.

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Keywords: Higher education, policy evaluation, admission ban, quotas, female labor market, marriage, urban-rural divide, Iran.

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1 Introduction

Over the course of the last century, most countries eliminated formal restrictions to higher education based on gender and ethnicity.[1] In some countries, the previously discriminating admission criteria of schools and universities have even been replaced by affirmative action policies (e.g. U.S. and India). As a consequence, over the last decades there has been a worldwide increase in higher education for ethnic minorities and for women. However, in Sub–Saharan Africa and parts of Asia women still represent a minority of undergraduate students. In Afghanistan, after the regime change in 2021, severe restrictions to schooling for women, especially for secondary and higher education, have even been reimposed.

In this paper, we analyze the consequences of an Iranian policy that restricted university admission for women at around 50 public universities (about 30% of all public universities in Iran) in the academic year 2012–13. In particular, the share of bachelor programs and seats available to women was reduced, mainly in popular and prestigious STEM fields like engineering. As reported by news outlets around the world, these restrictions led to outspoken dissatisfaction among Iranian students.[2] To justify this policy, university officials pointed out that women who study a STEM field are less likely to work after graduation. They suggested that it would thus be more efficient to reserve more seats for men. The policy should re–direct women towards more traditional subjects, and reduce competition for men in STEM fields (Asr-e Iran Analytical News, 2012). The restrictions were gradually rolled back starting from the 2013–2014 academic year.

Despite the wide media coverage, to date no detailed documentation or systematic evaluation of the effect of this policy has been conducted. This paper aims at filling this gap. We document and empirically analyze the consequences of the 2012 education

[1] Historically, universities in most countries were open only to men. In the U.S., until the mid–20th century, universities such as Stanford and Yale imposed restrictive quotas for women, some of which were only eliminated in the 1970s (Parker, 2015). Other recent examples on restrictions are Saudi Arabia, where university campuses have been segregated and the subjects women can take are limited (Hamdan, 2005), and Afghanistan during the Taliban regime (1996–2001), which banned girls from schools (Noury and Speciale, 2016). There have also been restrictions to university education for minorities. For instance, in the wake of the Second World War, Germany and Italy banned Jewish students and faculty.

[2] These concerns are well represented by the following quote by a young Iranian woman, reported by the BBC: "I wanted to study architecture and civil engineering, but access for girls has been cut by fifty per cent, and there's a chance I won't get into university at all this year." See https://www.bbc.com/news/world-middle-east-19665615.

policy, which provides a unique setting to study the effects of educational restrictions on the gender gap in higher education and on gender-specific marriage and labor market outcomes.

Whereas in Iran restrictions for women in universities were imposed in the past, the implementation of this policy came as a surprise. The country promoted education for women since the Islamic Revolution in 1979; in 2001 for the first time women outnumbered men at university and in 2011 women's admissions accounted for over 60% of total new enrollments (Rezai-Rashti and Moghadam, 2011; Moinifar, 2012). Today, differences in educational attainment among young Iranians are mainly due to regional heterogeneity and not due to gender. However, women still represent only a very small fraction of the formal labor force and experience higher unemployment rates compared to men. Female labor force participation in Iran is among the lowest in the world: in 2019, it was at 18%.[3] But among the women that are working, there are over-proportionally more with higher education.[4]

Iran has a central admission system that allocates students across universities and programs depending on their scores at the university entrance exam (*konkur*). The university entrance exam is specific to five broad fields of study (mathematics, natural sciences, humanities, arts and foreign languages) and gives access only to corresponding programs. The changes in the availability of programs were announced in August 2012, after students had taken the university entrance exam. Hence, prospective students who wished to start university in fall 2012 could apply to an unexpectedly lower number of programs. While women were excluded from only 4.6% of programs in 2011, this percentage rose to 18.6% in 2012, with particularly strong exclusions in the mathematics track, where less than 70% of the courses where open to women in 2012. Due to the central admission system where students hand in a ranked list with the programs they would like to attend, even individuals who were not originally interested in a spot in a restricted program were likely affected by the policy, as it increased competition for the remaining programs.

We expect women who applied to university in 2012 to be particularly impacted by

[3]ILOSTAT, 2021. In Egypt and Saudi Arabia women exhibit similarly low labor force participation despite education levels similar to men (UNESCO Institute for Statistics, uis.unesco.org, 2020).

[4]Appendix A provides an overview on higher education and labor market trends in Iran.

the policy. First, there is a mechanical effect due to the increase in competition among women for a smaller proportion of seats and programs in popular fields. This reduces the probability of women to enroll in these programs. The opposite should happen for men. Second, the timing of the policy – announced after the *konkur* and shortly before prospective students had to hand in their preferences for programs – meant that the restrictions were an unexpected shock, restricting the set of choices for women applying to university. The unanticipated nature of the policy meant that women had no opportunity to adjust their choice of *konkur* or to consider other available programs. This may have resulted in many of them not obtaining a seat at a public university. Furthermore, the policy is expected to affect the allocation of students to programs, potentially leading to a higher rate of mismatch. Via its effects on university education, the policy may result in long–term consequences for life outcomes, such as the decision to get married, labor force participation and employment. Nevertheless, there are two reasons why restrictions may not have had an important adverse effect on female university attendance. Women who cannot enroll in their preferred program in 2012 might postpone joining university for one year. Moreover, students could still enroll in a private university, although degrees from private universities are much less prestigious and more costly.

To identify the impact of this policy on the gender gap in education, we rely on a triple–difference strategy that uses the variation in exposure to the policy across gender, cohorts and regions. We expect the policy to have the strongest impact on the cohort of high school graduates of 2012 and should affect men and women differently. As individuals are most likely to start university at the age of 19, we identify those who are 19 in 2012 as the cohort that is most affected by the policy. High school graduates of 2013 had more time to adjust their choices in light of the 2012 changes. Individuals of older cohorts likely applied to university before 2012 and thus should also be less affected by the policy.

As an estimate relying only on the difference in gender and in birth year is likely to suffer from omitted variable bias due to other changes that might have happened at that time and that affect all women in the same cohort, our third difference is based on regional variation in the restrictions. For this, we exploit that only a subset of the 177 public universities implemented restrictions, and at various degrees.[5] Since Iranian students have

[5] See Section 3.2 for more details on the policy and for which universities participated.

a strong preference for programs at universities close to their family's residence (Ekbatani, 2021), the policy is expected to affect the spatial distribution of education outcomes: areas where stronger restrictions had been implemented should see larger disadvantages for local female high school graduates.

We use individual–level data from the Household Income and Expenditure Survey (HIES) for 2008–2021, which provides gender, education level and age of all household members. Furthermore, it is possible to identify the household's city (*shahrestan*) of residence. To determine the degree of restrictions in each city, we rely on the original course books that have information on all programs and seats offered to male and female students. To obtain the spatial distribution of restrictions, we manually coded the place of study for all programs. Our main measure of local restrictions is based as the percentage change in the share of all programs between 2011 and 2012 that are available for women in a given city.[6]

A potential concern about our identification strategy is that our measure of restrictions at the city level may be correlated with other factors or trends at the local level. Specifically, we worry that restrictions may have been stronger in more conservative regions. While the exact reasons that induced some universities and not others to implement the restrictions are unknown, we show that, on average, more conservative cities are not more likely to impose restrictions. Moreover, we show that there are no differences in trends for household characteristics in restricted versus unrestricted cities. Nevertheless, to address this potential concern in the regressions, our main specification includes province–gender–cohort fixed effects, which absorb potential confounding factors at the province level that may differentially affect women in different cohorts. We also show that controlling for the share of votes for conservative candidates in the 2009 presidential election does not affect our results. In addition, we control for potential changes in admission policies in the years around 2012, as well as for the number of seats for women at university.

We find a strong negative effect of the policy on university attendance of women relative to men for individuals who are 19 years old in 2012 and live in cities with stronger

[6] Our data contains the universe of available slots at public universities for the three main tracks (mathematics, applied sciences and humanities) in a given academic year and city, as published in advance in the course books. When a city does not have a university, we use the measure of local restrictions of the closest city with a university.

restrictions. The differential effect for women relative to men in this cohort is of about 3.2 percentage points for cities that imposed moderate restrictions (a decrease in the share of programs open to women of up to 10%). We show that the effect of the policy goes beyond the mechanical effect of universities decreasing the total number of seats available to women relative to men. Our results rather suggest that women are discouraged from studying due to the restrictions in popular programs at local universities. We also explore heterogeneous effects across regions and find a stronger negative effect on university attendance in urban areas and in conservative provinces. Moreover, the negative impact of the policy is driven by women from households with relatively low income or low parental education who may lack the funds to send the young women to study in universities further away or are less informed about the university application process. We also do not find evidence of substitution between public and private universities. While the latter did not impose restrictions for women, they are characterized by higher costs and lower quality of education, and thus are likely to be unattractive choices for displaced students.

We then show that the policy had important consequences beyond education. Our results indicate that the policy significantly affected the probability of marriage at a young age and the quality of marriage matches. Women who are more exposed to the policy are less likely to be married by the age of 28, and those who do marry are more likely to have a higher education level than their husbands. Furthermore, we find evidence of a positive impact of the policy on female employment. This is accompanied by an increase in the number of women hired for jobs at the bottom of the skill distribution. We also observe a positive impact on employment for men.

Overall, our paper highlights that education policies can have unintended consequences. Whereas this policy was meant to redirect women towards more traditional subjects at university, it instead induced women not to study at university at all. Beyond its effects on university education, the policy had unplanned effects in other important domains. Specifically, it led women to take on less traditional roles, as signaled by their lower probability to marry at a young age and their higher probability of being employed.

The remainder of the paper is structured as follows. Section 2 discusses our contributions to the related literature. Section 3 provides details on the higher education system in Iran and on the policy that we examine. Section 4 presents the data and provides details on the main variables. Section 5 details the empirical strategy. Section 6 presents and discusses the results. Section 7 concludes.

2 Contribution to the literature

Our paper contributes to various strands of the literature on gender differences in education and related life outcomes. First, our work is related to the studies on affirmative action at university (e.g. Arcidiacono and Lovenheim, 2016; Bertocchi and Bozzano, 2020; Bertrand et al., 2010; Bowen and Bok, 2016; Epple et al., 2008; Howell, 2010; Khanna, 2020). This literature mainly focuses on the effect of policies aimed at promoting higher education of historically disadvantaged groups, such as ethnic minorities or women. Closer to our paper is the work by Bagde et al. (2016), who study the effect of affirmative action for scheduled castes and women in engineering colleges in India.

As opposed to the existing literature on affirmative action, our paper studies a policy of explicitly tightening restrictions in higher education for women, which is important to study for two reasons.[7] On the one hand, this allows for evaluating the impact of removing affirmative action, as removing affirmative action is equivalent to tightening restrictions for the previously supported group. The only research that evaluates the impact of removing affirmative action is Howell (2010), which is based on simulating a model of individuals' study choices, rather than on an observed removal of affirmative action. On the other hand, existing studies on the impact of affirmative action are being criticized as affirmative action might lead to a mismatch between individuals and their study choice, and thus to undesirable outcomes (e.g., Bagde et al., 2016). In the scenario that we study, restrictions for women are first tightened and then relaxed, i.e. we study a period of "negative affirmative action". Thus, those women who still secure a seat in a restricted field are presumably qualified for their studies, and the measured impact of the policy is not confounded by a mismatch between individuals and their study choice.

[7] Related to our paper, although not focused on higher education, is the work of Noury and Speciale (2016), who analyze the effect of a ban on women's schooling during the Taliban regime in Afghanistan. Their findings indicate a negative effect of the ban on schooling and on the probability to work outside the house, a positive effect on fertility and a reduction in the age at first marriage.

Moreover, limiting female access to higher education in our setting implies affirmative action in favor of men, who represented already the majority of Iranian students in STEM fields prior to the restrictions. Fryer and Loury (2005) highlight that most affirmative action policies impact not more than a one digit percentage share of society, which makes it difficult to consider general equilibrium effects, and which is a drawback of existing studies to evaluate such policies (see, e.g., the discussion in Long, 2004, or in Boisjoly et al., 2006). Due to the central admission system in Iran, changes in admission criteria potentially affect all university applicants. During the period of our study, each year about 400.000 individuals pass the entrance exam and compete for university seats, constituting more than 20% of each birth cohort. Given the size of the affected population, our setting thus allows us to consider general equilibrium effects of affirmative action.

While numerous studies exist on the effect of affirmative action on the labor market (e.g. Arcidiacono, 2005; Arcidiacono et al., 2010; Bertrand et al., 2010), the empirical evidence on the effect of such policies on the marriage market is scarce. However, there is a vast literature drawing a link between higher education and marriage. For instance, Goldin (1997) describes that in the mid–last century women considered college rather as a marriage market, as the return from marriage was higher than the return from human capital accumulation. In recent years, however, women attend college in order to pursue a professional career. Goldin et al. (2006) observe that an increase in female college attendance in the U.S. in the last decades went hand in hand with an increase in the age of marriage, without establishing causality. This pattern has also been documented for Iran (Torabi and Abbasi-Shavazi, 2016). Closer to our paper, Kaufmann et al. (2015) find for Chile that admission to a more elite university has no impact on the likelihood of marriage or of having a child, while it has a positive impact on the spouse quality, from the perspective of females. Contrary to our work, that paper does not study the impact of university admissions restrictions on marriage market outcomes.

More generally, our paper contributes to the literature on gender inequality in access to education in developing countries, by documenting the effects of a policy that reduces access to higher education for women.[8] The literature focuses instead on policies that

[8] Bertocchi and Bozzano (2020) provide a review of the literature on the gender gaps in education and their determinants for both developed and developing countries.

aim to increase access to schooling for women, for instance building schools (Burde and Linden, 2013) or providing lavatories in schools (Adukia, 2017).[9] Furthermore, this paper relates to the literature on asymmetric effects of policies in developing countries across sub-populations (Ashraf et al., 2020; Cassan, 2019; Elsayed and Marie, 2021).

Finally, Iran is still an understudied country in the field of economics, with few exceptions. Moeeni and Tanaka (2023) investigates the effect of a hiring quota that reduced labor market opportunities for high skilled women on female education and marriage outcomes. Closer to our paper, Moeeni and Wei (2022) investigates the effect of unobserved skills on labor market outcomes of Iranian high school graduates, also relying on the 2012 university admissions policy as a source of exogenous variation in unobserved skills. Differently from that paper, we provide a comprehensive analysis of the impact of restrictions on higher education for women, considering the impact on education, marriage and the labor market.[10] Furthermore, Karimi and Taghvatalab (2018) find a positive relationship between university attendance and marriage for both men and women, exploiting for identification a large university expansion in the years 2000s. This work highlights the importance of universities for the marriage market in Iran.

3 Higher Education in Iran

The schooling system in Iran is divided into primary, lower secondary (middle) school and high school. Primary school starts at the age of 6. For the birth cohorts in this study

[9]One exception is the paper by Noury and Speciale (2016), that investigates the effect of the Taliban rule in Afghanistan on women's basic education, labor market outcomes and fertility, finding a negative impact on the first two variables and a positive impact on the latter.

[10]A further difference lies in the identification strategy. Moeeni and Wei (2022) use a sharp regression discontinuity design, where individuals born after September 1992 are considered as treated. Our identification strategy better captures the impact of the 2012 restrictions for several reasons. First, a regression discontinuity design seems less suitable in this context, since several cohorts of women may be affected by the policy; with our strategy, we are able to show the effect of the restrictions for all potentially affected birth cohorts. Second, including individuals born in 1992 in the treatment group can be problematic as they are likely affected by the admission policy implemented in 2011, as discussed in Section 3.1. Third, taking into account the spatial variation in the policy implementation is important, since due to students preferences for local universities, prospective students are not equally exposed to the policy. Fourth, by using the spatial variation in restrictions, we can disentangle the impact of the policy from other macroeconomic factors happening at the same time that can affect outcomes for women of the same cohort.

(1988–1995), it lasts for 5 years.[11] After primary school, pupils attend lower secondary school (3 years) and high school (3 years). After high school, Iranians have three options: either regular work, study for a two–year *Associate Degree*,[12] or attending university for a bachelor's degree. Students who wish to attend university need to complete an additional year of pre–university studies after high school, which prepares them for the national university entrance exam of the field of their choice.

In subsection 3.1 we present the university admission system and gender segregation policies that preceded the 2012 policy. In subsections 3.2 and 3.3 we detail the restrictions implemented to university admission in 2012 and their potential implications in terms of education. We further discuss the evolution of higher education in Iran and the link between higher education and the labor market in Appendix A.

3.1 University admission and gender segregation prior to 2012

Iran has a centralized university admission system. High school graduates who wish to attend university first need to participate in the yearly university entrance exam, called *konkur*, in early summer.

The fields of study are grouped in 5 different disciplines, each of which requires its own *konkur*: (*i*) mathematics and technical fields, (*ii*) sciences, (*iii*) humanities, (*iv*) arts, (*v*) languages. Students can take only one *konkur* in one of the first three disciplines (which are the most popular), and may take a second one in either arts or languages. The discipline of the *konkur* determines the subjects a student is allowed to study. However, there are a few exceptions: all three of the main *konkur* (mathematics, sciences, and humanities) give access to economics, accounting, chemistry, theology and psychology programs. We refer to these subjects as the 'common' or 'overlapping' subjects.

Once the results of the *konkur* have been published, prospective students hand in a list with their preferred program.[13] The list can contain up to 100 programs, and is filled

[11]The schooling system in Iran has been changed recently, and primary school is now for 6 years.

[12]An *Associate Degree* grants a more practical formation compared to a bachelor's degree. After finishing this degree students can follow a bachelor's program after having taken the national university entrance exam. These students can obtain a bachelor's degree in 2 instead of 3 academic years.

[13]Programs are defined as a specific subject–university combination, e.g. Civil engineering at Tehran university or Economics at Tabriz University. Each program is identified by a unique code.

in according to the prospective student's order of preferences. The very best performers in the entrance exam likely get their first choice and attend the most prestigious subjects (e.g., engineering, medicine and biology) at the most prestigious universities of Iran (e.g. Sharif University of Technology or University of Tehran). Those who perform worse have to be satisfied with less popular subjects at lower ranked universities.

The information on the available programs, like the number of seats by gender, is published each year after the announcement of the *konkur* results in course books. For each *konkur* a separate course book is published. Table D.6 shows an extract from one of the course books of 2012.

Table 1: Gender restrictions in programs, all fields

	Total number of programs									
	2010		2011		2012		2013		2014	
	No.	%	No.	%	No.	%	No.	%	No.	%
Women										
Programs not open	67	2.6%	125	4.6%	611	18.7%	487	14.1%	331	9.2%
Programs open	2473	97.4%	2565	95.4%	2659	81.3%	2959	85.9%	3269	90.8%
Men										
Programs not open	102	4.0%	187	7.0%	295	9.0%	158	4.6%	165	4.6%
Programs open	2438	96.0%	2503	93.0%	2975	91.0%	3288	95.4%	3435	95.4%

Notes: Data from the university course books published by the Iranian Ministry of Education (2010-2014).

With the start of Mahmoud Ahmadinejad's presidency in 2005, more conservative forces gained power in the Iranian administration. Some caps on female students had been implemented already in 2006 for a few programs (notably medicine). However, up to 2010, out of the 156 universities with programs in the three main disciplines (mathematics, sciences, and humanities), the majority did not have any gender restrictions in the admission.[14] Only 10 universities had gender segregation in 2010; these are historically male or female–only universities. In 2010, 48 universities offered programs to both men and women, but had a few programs that were already subject to gender quotas.

[14] In the course books, the columns on female and male seats just stated "female" and "male" without specifying how many seats should go to each gender.

In 2011, some first significant changes appeared in the course books: two thirds of the universities listed explicitly the number of seats allocated to each gender, mostly 50–50. Only 54 out of the 166 universities did not apply any gender quotas.

Table 1 shows the number and percentage of programs that are open for women and men in the years 2010–2014. The table shows that, in 2010 and 2011, men could apply to a lower share of programs than women. This was due to a high number of programs in midwifery, which is a female–only discipline in Iran. Between 2010 and 2011, the share of programs that men and women could apply to experienced a similar decrease (2 percentage points for women, and 3 percentage points for men).

The 2011 gender segregation policy did not get much media attention, likely due to the fact that only a small number of programs became unavailable for each gender. Still, these changes resulted in some students not being able to join their preferred program and being displaced by students of the opposite gender with a worse entry exam score. Moreover, since women represented a higher share of *konkur* candidates and had been the majority of students in the past, this change could have already led to a decrease in admissions for female students. We will thus control for this policy in our empirical analysis, and show in robustness checks that this policy is not driving our results.

3.2 The 2012 policy of banning females from STEM fields

In August 2012, after prospective students had received the results of the university entrance exam and the course books for the upcoming year had been published, students discovered unexpected and substantial restrictions in their study choices. The media soon reported that females were banned from engineering–related fields in 36 public universities.[15] Our more detailed analysis of the course books shows that the restrictions were even more extensive. From the course books we can compute that 53, and not 36, out of the 177 universities that offered Bachelor programs in that year decreased either the share of programs, the share of seats open to women, or both in 2012.

Table 1 illustrates the extent of the restrictions. While in 2011 there was a small

[15] See https://www.mehrnews.com/news/1666033 for the first Iranian national newspaper that brought these gender–specific restrictions to the admissions into the spotlight.

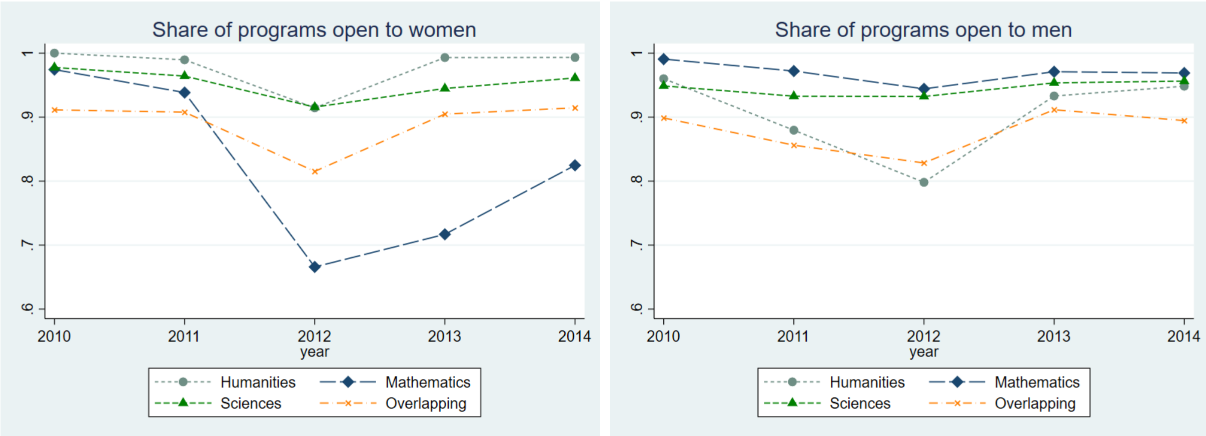
increase of 2 to 3% in the shares of non-available programs for both gender, in 2012 the share of programs not accepting women increased by 14 percentage points (from 4.6% in 2011 to 18.7%) . In contrast, the share of programs not accepting men increased by only 2 percentage points (from 7% to 9%). Due to a change in political climate, the restrictions were gradually lifted starting from 2013. By 2014, the share of programs that women could not apply to decreased to 9.2 %, and for men it decreased to 4.6 %, very close to the values for 2010. Figure D.1 displays the evolution of the total number of seats for women and men.[16]

Decisions on the admission process are taken by presidents and boards of individual universities. However, the Ministry of Sciences coordinates the central admission process, and thus is aware of the available seats proposed by each university.As the restrictions in 2012 were implemented at the university level, there was important variation across universities in their scope. Only very few universities have made official statements on the motivations of their admission policy changes in 2012, but personal preferences of the board or political ties are likely to have played a role.

In the few public statements, the main reason for the admission restrictions for women was that they would reduce female competition for males in the labor market. Furthermore, the restrictions should encourage women to engage in more traditional and domestic activities (Asr-e Iran Analytical News, 2012).This intention is obvious from the distribution of restrictions across the different programs. The largest share of restricted programs is in the traditionally male–dominated field of engineering. However, several universities also imposed restrictions for females in other fields, such as accounting or chemistry, which are preparing for occupations that had generally been perceived as suitable for women by the Iranian society. This may have contributed to the perception of the restrictions as arbitrary and unfair (Asr-e Iran Analytical News, 2012). One notable exception are the medical universities which mostly refrained from imposing gender restrictions. Only 6 out of the 55 medical universities had implemented restrictions in 2012, which is in stark

[16]The overall number of courses and seats available for both genders increases every year. This is expected, due to an increasing demand for higher education, fueled by population growth and a rising share of high school graduates, which naturally leads to an increase in the number of universities, programs and seats.

Figure 1: Admission restrictions for women and men by field



Notes: This figure shows the share of programs that are open to each gender by field in public universities. Data from University course books published by the Iranian Ministry of Education (2010-2014).

contrast to 30% of the technical universities.[17]

Figure 1 shows the share of programs available to women (left panel) and men (right panel) by field. In addition to the three main fields of the *konkur*, we create a separate category ("Overlapping") for the common subjects that can be studied after taking any of the three *konkur*. While in 2012 we observe a decrease in the share of programs available for women in all fields, the largest decrease is in mathematics, which includes technical fields such as engineering. These programs are among the most popular for male and female students, as can be seen from Table D.7, which shows the distribution of males and females across different fields in 2010. The popularity of the mathematics track among women is evident also when looking at data on *konkur* participation: in 2012 women comprised 41% of the students taking the mathematics *konkur* (Ekbatani, 2021).[18] We also observe a decrease in the share of programs available to men in all but one field (sciences), but much less than for women. Furthermore, in 2013 the share of programs for men returns roughly to the 2010 levels, while for women the share of available programs in mathematics remains low.

[17]One potential reason to this was that restrictions were considered impractical to implement in medical schools due to the requirements of the use of laboratory equipment and many practical courses (Dokouhaki and Shahrokni, 2012). Also the high demand for female doctors and health care workers has been cited as a reason why gender restrictions in this field were much less common.

[18]This figure is based on a sample of over 1/4 of all students who took the mathematics *konkur* in 2012.

A similar picture emerges when plotting the evolution of the absolute number of programs available for each gender by field (Figure D.2). In 2012, we observe a sharp increase in the number of programs available for men in the field of mathematics, and a slight decrease in the number of programs available for women. The trend in the number of programs in other fields is not influenced by the policy.

Finally, Figure D.3 illustrates the spatial distribution of restrictions to university admissions for females by year. The four maps show whether in each city there has been a decrease (red) or an increase (blue) in the share of programs women can apply for relative to the previous year. Most restrictions were implemented between 2011 and 2012. In 2012 the share of programs for females decreased in 44 cities. Furthermore, the map highlights that restrictions for females were common throughout the entire country.

3.3 Expected effects of the 2012 policy on university education

University attendance. The 2012 policy is expected to have had a differential impact on women's university attendance, as compared to men's.

First, the policy has a mechanical effect of reducing university attendance for women, as compared to men, via the decreased share of seats and programs available to women.

Second, and more importantly, the timing of the policy, announced after the *konkur* and before prospective students had to hand in their preferences for programs, meant that the restrictions were an *un*expected shock, restricting the choice set for women. The unanticipated nature of the policy meant that women had limited opportunities to adjust their choices. In fact, they could not factor in this policy when deciding which *konkur* track to take, and they had only a few weeks to reconsider which programs to apply to.[19]

In principle, the possibility to fill in up to 100 programs in their wish list can mitigate the negative effect of the reduced options and still result in a high probability to obtain a seat at a public university. However, only a small share of applicants (25% in 2012) fills in the full 100 programs in their list, and as students have a preference for universities close by, they often do not consider studying the same subject in cities further away (Ekbatani,

[19]Students normally take the entrance exam after a one-year *konkur* specific pre-university courses, making switching tracks within one year difficult.

2021). In addition, applicants might not be able to study subjects at local universities for which admissions are not restricted.[20] Finally, applicants are likely not aware of the full extent of the restrictions and the resulting degree of competition in popular programs and thus fail to fill in safe choices in their wish list.[21]

While this policy is expected to affect high school graduates born in 1993, as they applied to university for the first time in 2012,younger and older cohorts might also be affected. High school graduates applying to university the following year might be also affected, as not all restrictions were dropped the following academic year and the students' choice set was similar. However, the effect for this cohort is expected to be smaller, as for them the policy was not unexpected, i.e. they had time to adjust their study track and preference list. Older individuals might also be affected, as they may have re–taken the *konkur* in 2012. The effect for these individuals is also expected to be lower.

These mechanisms suggest a negative effect of the policy on women's university attendance compared to men's. Still, there are two reasons why we may not observe this effect. First, restrictions only affect public universities, and not private universities (see Figure D.4). Thus, women can still decide to attend private universities.[22] In our empirical analysis, our measure for university attendance refers to any university, not just to public universities. Thus, our data also captures women who study at private institutions. Second, women could delay university education by re–taking the *konkur* in the following years where restrictions are less severe. In our empirical analysis, we test whether women ever attended university, and not whether they entered university in 2012.

Allocation of students across programs. An important effect of the 2012 policy concerns the allocation of students to programs. While the restrictions are concentrated in a few fields, such as engineering, there is likely a substantial "trickle down" effect of this policy on other fields. Many women who planned to study engineering were forced to change their major and enrol in other programs within the field of their *konkur*. As

[20]Recall that changing subject is possible only between the disciplines that are in the same *konkur* track.

[21]Prospective students generally have some knowledge about the placement of students in previous years (Ekbatani, 2021). However, given the restrictions, placement results from previous years are much less informative in 2012.

[22]While in principle this is possible, private universities are no substitute to public institutions in Iran, as the former are more costly, less prestigious and some require a separate entrance examination.

there is an overlap of programs across the three *konkur* fields, this would lead to higher competition in these common programs. Moreover, students who took the mathematics *konkur* and an additional one in arts or languages might have chosen a program from one of these fields. This is likely to increase competition in those fields and affect program choices of students of other fields.

Overall, as a consequence of the 2012 policy, more university applicants in 2012 were likely to have accepted one of their less preferred programs. Thus, the policy may have lead to a higher mismatch between programs and students, in particular for females.

4 Data

4.1 Individual data: Household and Expenditure Survey

Our main data on individual characteristics such as age, gender, education and income come from the Household Income and Expenditure Survey (HIES). The HIES is conducted every year by the Statistical Center of Iran. About 38,000 households are interviewed each year. Importantly, it reports the household location at the city (*shahrestan*) level, which is the second-level administrative division of the country. The HIES covers all cities in Iran. We use data for the years 2008-2021.[23]

The survey is designed according to a rotating panel, in which households appear for three consecutive years. Each year about one-third of the households is from the same sample as in the previous year. However, in 2012 and 2018 new household samples were drawn, i.e. no individuals can be followed from 2011 to 2012 and from 2017 to 2018. The survey covers 334 cities in 2008 and 427 in 2018, covering all 31 Iranian provinces.[24]

Our main sample consists of individuals born between 1988 and 1995, aged 19 to 28 at the time of the interview, and who have attended at least high school. Since we are interested in the outcomes and effects of higher education, individuals who did

[23]The Persian calendar differs from the Gregorian calendar. The new year starts on 21 March. The exact time period included in our data is thus April 2008 to March 2022, corresponding to the years 1387-1400 of the Persian calendar.

[24]This increase in the number of cities is due to the redrawing of administrative borders.

not attend high school are not a good comparison group for studying university policy restrictions. We exclude individuals born before 1988 as there have been several changes to the education system and labor market policies in previous years, which make these cohorts less comparable to younger students.[25]

We limit our main analysis to cities with at least 50 interviewed individuals in the relevant age group during our sample period. This ensures that our results are not driven by a few outliers in these small cities. Furthermore, this has the advantage of reducing the number of city-gender and city-cohort fixed effects.[26]

One challenge with the HIES is that it only records the age at the time of the interview, not the date of birth of the individuals. Thus, in our main analysis, we assume that each individual who was 19 at the time of the interview in 2012 was born in 1993, even though some of them were born in 1992.

To make sure that we classify individuals correctly despite the absence of the exact birthday, we look at the age distribution of individuals who are studying at university. Figure D.5 splits every survey into a spring and a fall round, depending on whether the individual was interviewed in the first or the last six months of the year. While we observe an increase in the share of university students already between ages 17 and 18, the largest increase happens between ages 18 and 19.

While in the survey some individuals are interviewed up to three consecutive years, our preferred sample uses only the first appearance of an individual. This is for two reasons. First, the number of individuals that can be followed from one wave to the other is small. Second, this allows us to keep only one observation per individual, and to avoid giving too much weight to specific individuals in smaller cities if they appear several times.[27]

Table D.8 reports the summary statistics for the main variables by gender. Women and men are similar in terms of their domestic environment (number of household members, rural versus urban) and also have on average similar ages and education.[28] Women are

[25]We do not include younger cohorts as these are still too young in 2021 (last available year of the HIES).

[26]In subsection C.0.1 we provide several robustness checks on the sample.

[27]Less then 15% of the individuals in our sample can be identified as having been interviewed more than once.

[28]The variable *Highest level of education* indicates the highest education level obtained or the education

more likely to enroll at university but much less likely to participate in the labor force: only about 21% of women in our sample are in the labor force, while for men it is 68%. A similar picture emerges when we look at the share of young people that are employed and receive a salary: 7% for women versus 42% for men.

4.2 Student statistics data

We complement our analysis with data from the student statistics released by the Institute for Research and Planning in Higher Education of Iran for the years 2008-2014. This data reports the total number of newly enrolled students in Bachelor programs in each year by gender for all public and most of the private universities. It also provides aggregate numbers per gender and field of study.

To gain a first idea of the impact of the 2012 policy, in Figure D.6 we plot the evolution of the total number of newly enrolled female and male students in Bachelor programs by year. We observe a sharp decrease in university attendance for women in 2012. For men, we instead observe a much smaller decrease.[29]

4.3 Data on university programs and seats

Data on the Bachelor programs at public universities come from the course books for the academic years 2010-2014, published by National Organization of Educational Testing, affiliated to the Iranian Ministry of Education. They list for each program the course code, the subject (e.g., "Chemistry" or "Civil Engineering"), the total number of seats, the number of seats for women and the number of seats for men. We manually coded the locations of universities and their different campuses. Our data covers the course books for the in-person (full-time) programs in the three main *konkur* fields (humanities, mathematics and technical fields, and applied sciences[30]) for the public and private

level an individual is enrolled in. It is defined as follows: 1 = elementary/informal education; 2 = lower secondary school; 3 = upper secondary school; 4 = pre-university (preparation for *Konkur*); 5 = associate degree; 6 = Bachelor degree; 7 = Master degree; 8 = PhD.

[29] Notice that we exclude the private Islamic Azad University, as data for this university is only reported from 2010 onward. The patterns for 2010-2014 remain unchanged when including this university.

[30] We focus on these fields because the vast majority of students enroll in one of these. Only a very small share of students chooses arts or languages.

universities.[31]

4.4 City-level measures of restrictions

We construct an index of exposure to the policy at the city–level, based on the change in the share of programs open to women between 2011 and 2012. For cities without a university we consider the corresponding change in the closest city with a university (defined as the shortest distance between the centroids of two cities).[32]

Our main index takes integer values between -2 and +2. The values are assigned as follows: +2 if in a city (or in the closest city) the share of programs available to women decreases by more than 10% between 2011 and 2012, +1 if this share decreases by less than 10%, 0 if no change, -1 if the share of programs available to women increases by less than 10%, -2 if this share increases by more than 10%. The map in Figure 2 shows the values for each city. While most changes involve a decrease in the number of programs, a few cities experienced an increase in the share of programs available for women in 2012.

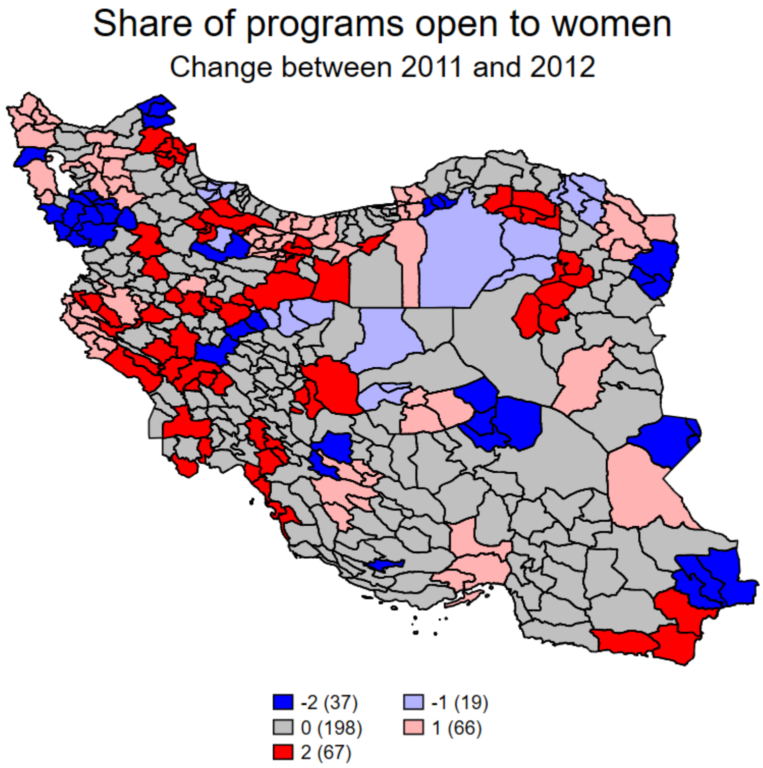
We rely on this aggregate index and not on a continuous variable as our main measure for three reasons. First, we do not necessarily expect a linear relationship between our outcome variables and a continuous restrictions variable. Stronger restrictions might have led students to consider studying in more distant cities with less restrictions. Second, as we sum up restrictions across different fields, the exact percentage change is not very informative. Third, cities with small and young universities have high changes in the continuous variable due to their low and still quite variable number of programs, thus leading to outliers.[33] By categorizing cities broadly into "somewhat" and "very" restrictive cities, we cap the influence of these few cities.

[31]The course books do not include programs offered by the private Islamic Azad University as this university has a separate entrance exam and application procedure.

[32]We rely on the change in the share of programs open to women and not in the absolute number of programs as there is substantial variation in university size across the country, and during our sample period many (but not all) universities increase their programs over the years. Using the share allows to account for differential changes in the number of programs by gender. We further consider the percentage change and not the change in percentage points to better measure the severity of a reduction in the share of programs for a given city.

[33]Similarly, high changes in this variable appear, but are not necessarily meaningful, when cities open a new university and start offering courses to women for the first time.

Figure 2: Map of restrictions in closest city with a public university



Notes: Own calculations. The data used is from the university course books published by the Iranian Ministry of Education (2010-2014).

Alternative measures of restrictions include the change in the share of programs in all university towns within 60 or 80km radius. Even though the policy also affected the share of *seats* available to women, our main measure for restrictions uses the change in *programs*. Prospective students fill in their wish list based on programs they would like to join. Thus, the number of available programs is crucial in the application process and it is expected to affect the probability of women to attend university. Robustness checks however also use measures based on seats. Summary statistics on the various restriction measures are reported in Table D.9.

Notice that our measures of exposure to the policy at the city level are based on the assumption that students are more likely to attend universities that are closer to their place of residence. Ekbatani (2021) reports a median distance of 63 km for travelling to university in 2012. He also shows that students have a strong preferences for studying in their home town. If this were not the case, our measures at the city level would not have any bite.[34]

A possible concern with the city-level measures is that locations with gender restrictions between 2011 and 2012 may be generally more open to gender segregation and are more likely to implement restrictions for women also in other years. To account for this, we construct the same index for the adjacent years (2011 to 2014) and control for these in our main empirical specification. Furthermore, in the next section we test for parallel trends, to ensure that cities with and without restrictions evolved similarly in the years prior to the policy.

5 Identification Strategy

5.1 Empirical specification

We identify the impact of the policy by exploiting differences in exposure to the policy across gender, cohorts and locations, and estimate the following equation:

$$\begin{aligned} Y_{ickt} = & \sum_{k=1}^{K} \beta_k R_c^{2012} \times cohort_k \times f_i + \alpha_1 X_{it} + \alpha_2 X_{it} \times f_i \\ & + \sum_{k=1}^{K} \lambda_k Z_c \times cohort_k \times f_i + \gamma_{cf} + \lambda_{ck} + \chi_{pkf} + \tau_{pkt} + \zeta_{pft} + \epsilon_{ickt}, \end{aligned} \tag{1}$$

where Y_{ickt} represents our different outcome variables. Our main education outcome is an indicator variable measuring whether an individual i, residing in city c, from cohort k and interviewed in year t is attending or has ever attended university.

Regional variation in exposure to the 2012 policy is measured by R_c^{2012}. This index is defined at the city level and captures the intensity of the restrictions based on the change between 2011 and 2012 in the share of programs open for women (see section 4.4). The dummy variable f_i indicates whether or not individual i is female.

The differential impact of the policy across cohorts is captured by the set of cohort

[34]Students can move to a different city for their studies. Appendix B.3 addresses in detail the issue of movers and self-selection of individuals, which could potentially bias our results.

dummies, $cohort_k$. Our sample includes the birth cohorts 1988 to 1995. The 1993 birth cohort is the most affected by the 2012 policy as in that year these individuals were 19 and the most likely age group to apply to university. Our main coefficient of interest is the β of the interaction term for the 1993 cohort. There are however also possible repercussions of the 2012 policy on later cohorts, which we can identify with interaction terms for the younger cohorts. We can test for the absence of pre-trends by examining whether the estimated coefficients for cohorts born in earlier years are significantly different from zero.

We include individual controls (X_{it}): a dummy indicating urban or rural residence and a dummy that takes the value of one if there is at least one other household member with an education level above middle school. To allow them to have differential effects across gender we also add the interaction of these two variables with $female_i$.[35]

One potential concern is that the implementation of the restrictions at the city level may not be exogenous: for instance, the restrictions may be more likely to be implemented in more conservative areas, and more conservative individuals may make different choices regarding education, marriage and labor market. If we do not control for these regional differences, our estimates may be capturing the effect of this potential omitted variable.

To address structural or cultural differences across locations, we add a series of fixed effects: city-gender (γ_{cf}), city-cohort (λ_{ck}) and province-cohort-gender (χ_{pkf}).[36] The first two capture local factors that can have differential effects by gender (γ_{cf}) or by cohort (λ_{ck}).[37] χ_{pkf} accounts for structural differences in the outcome variable between women and men for a given cohort and province, such as province-specific political trends. Importantly, by including these province-cohort-gender fixed effects, our coefficients of interest are identified by the variation in the city restrictions within provinces.

Furthermore, we account for time-varying factors at the provincial level, such as current local economic conditions that are likely to affect household income, current activities

[35] We rely on this reduced set of individual controls in order to limit the inclusion of potentially endogenous variables. In robustness checks we also add (including their interactions with gender) household size, household income quantile and the level of education of household head.

[36] These fixed effects capture also the underlying double interaction terms of our triple interactions.

[37] One feature of the centralized applications system is the presence of regional quotas aimed to improve admissions to universities for candidates from disadvantaged areas of the country. Importantly, these quotas were not affected by the 2012 policy and did not vary by gender. Therefore, the effect of these quotas is absorbed by the city-cohort fixed effects included in the regressions.

of household members or household composition. We thus add province-cohort-year (τ_{pkt}) and province-gender-year (ζ_{ptf}) fixed effects which allow these local factors and economic conditions to have a differential effect on each cohort and gender. We further add a vector of controls at the city level, Z_c, interacted again with cohort and gender dummies, to better understand the origin of the effect of the policy.

First, we add the number of seats available at the local universities in that year. While the cohort-city fixed effects account for the overall number of seats in the year the cohort most likely applies to university, they do not capture the number of seats reserved for women. However, the effect of the policy could be mechanical by restricting the number of seats for women. By adding also the number of seats available for women in 2012, interacted with cohort and gender dummies, we can see whether there is any effect of the restrictions that goes beyond the mechanical one from restricting the number or seats.

Second, we add the share of programs that is reserved for only one gender, again interacted with cohort and gender dummies. Female students might react differently to more gender segregation at university and this might be a reason why we observe a negative impact of our main restrictions variable, which measures the change in the share of programs that are open to women.

Third, as there could have been also particular gender specific policies in other years that could differentially affect cohorts, we add restriction measures for women in university admissions in 2011, 2013 and 2014 and interact these with cohort and gender dummies. For completeness, we also add the interactions of cohort and gender dummies with the number of seats available for females in the city, and the share of programs that are segregated in these adjacent years.

Finally, ϵ_{ickt} are the error terms. Standard errors are clustered by cohort and city.

We estimate Eq. 1 using a linear probability model.[38] We are particularly interested

[38] We choose to estimate a linear probability model (LPM) due to the large number of fixed effects that we include, which are problematic in non-linear models such as probit or logit. Furthermore, Horrace and Oaxaca (2006) note that the bias and inconsistency of the LPM increases with the proportion of predicted probabilities that fall outside of the interval between zero and one, and if the predicted probabilities lie within zero and one, LPM estimates are not expected be biased or inconsistent. In our analysis, all predicted probabilities lie within zero and one (except for one); thus, we conclude that the potential bias and inconsistency of the LPM are not an issue in our setting.

in the effect of the policy on the 1993 cohort. Our benchmark is the 1991 cohort. While it would be customary to choose the 1992 cohort as a benchmark, this cohort is not a good benchmark in our setting. First, men and women born in 1992 could be affected by the 2012 policy, as retaking the *konkur* is relatively common. Second, as explained in section 3.1, there was already a policy change in 2011, i.e. universities began to reserve seats for the two genders separately, which could have had an impact on the admission of students of the 1992 cohort.

In section 6.4.2 we run the same specification but look at the impact of the policy on labor market outcomes. In some robustness checks and in the analysis on labor outcomes and marriage, we will also run regressions separately by gender. In this case, our main variable of interest is reduced to the simple interaction between city level restrictions and cohort.[39]

5.2 Parallel trends

The main underlying assumption for our triple-difference strategy is that trends in the gender gap in university education, labor market outcomes and marriage do not systematically differ between cities with admission restrictions and cities without.[40]

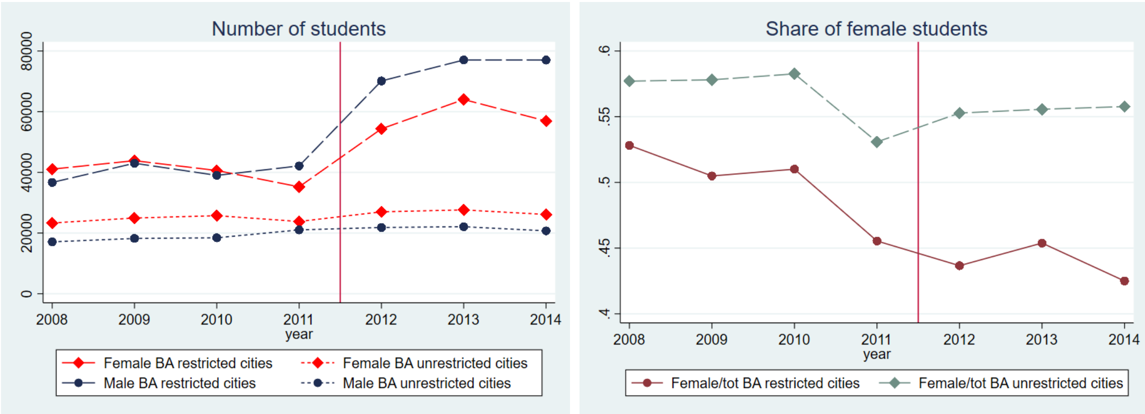
To test the validity of our empirical approach, we implement a number of checks on pre-trends using the HIES and the Student Statistics data. We first explore whether there are differences in average individual and city characteristics across treated and untreated cities, splitting the sample between cities with and without restrictions according to our main index of exposure (Table D.10).[41] We do not observe any significant differences in most variables, including university education, employment or labor force participation,

[39] When restricting the sample to one gender, our set of fixed effects is also reduced. Notably, we cannot include city-cohort fixed effects, as these would be collinear with our main coefficients of interest. We rely on province-cohort and city-year fixed effects. Province-cohort fixed effects capture province-specific shocks that might affect cohorts differentially. City-year fixed effects absorb yearly shocks at the city level, which are expected to be particularly relevant for labor market outcomes.

[40] In some specifications, where we split the sample between men and women, the identifying assumption is instead that trends in education, labor market and marriage outcomes for men and women *separately* do not systematically differ between restricted and unrestricted cities.

[41] We do this analysis for the year 2011 and for individuals who are 25-27 years old, to include only individuals likely to have finished their studies. Varying the range of survey years prior to the policy and the age of individuals does not change the results.

Figure 3: Enrollment in Bachelor degrees in public universities in cities with and without restrictions



Notes: The left panel shows the number of students enrolled in Bachelor degrees in public universities by gender in cities with and without restrictions to admissions for women. The right panel shows the share of female students. Source: own elaboration on the Student Statistics released by the Iranian Ministry of Education.

both in the whole sample and when considering men and women separately. However, cities that implemented restrictions are more likely to be urban and have on average higher incomes. We also note that the share of conservative votes in the presidential elections prior to the policy is almost 5 percentage points lower in cities with restrictions, compared to cities without. This suggests that the admission restrictions for women were not implemented in cities that are more conservative. We then assess whether there are differences in *trends* in the years prior to the policy, by comparing yearly changes for 2008 to 2011 in these variables in cities with and without restrictions. Results are reported in Table D.11. We do not find significant differences, suggesting no differences in pre-trends in individual and city characteristics between cities with and without restrictions.

Furthermore, in Figure 3 we explore whether there is a differential impact of the policy on the actual number of students in cities with and without restrictions, using the official Student Statistics data. We restrict the analysis to public universities that offer full-time courses with presence on campus, i.e. we exclude part-time, evening and distance learning.[42] In 2011, prior to the policy, we observe a decrease in the number of female

[42]Notice that, while informative, the figures in this analysis are not directly comparable with the results of the regressions that we present in the next section. This is for two reasons. First, the Student Statistics report the number of newly enrolled students each year, but do not indicate how many students are enrolled for each birth cohort. Second, in the Student Statistics data, for universities that have campuses in different locations, students are allocated to the city where the main campus is located. This is different

students in restricted cities, and a similar decrease in unrestricted cities. In particular, when plotting the share of females to the total number of students by year, trends are very similar for restricted and unrestricted cities. While the number of students of both genders increases in 2012 in cities with restrictions, the number of male students increases by substantially more. In cities without restrictions, the difference between female and male students remains constant. We can also observe parallel trends before the start of the policy for men: up to 2012 the evolution of the gender gap is the same for restricted and unrestricted cities.[43]

Figure D.7 replicates the analysis of the left panel of Figure 3, disagregated by *konkur* field: as expected, most of the effect is driven by programs requiring a *konkur* in mathematics. Also the humanities see a larger increase in enrollment for men than for women. Interestingly, in 2012 we find an increase in enrollment of women in restricted cities in art (recall that students can take an additional *konkur* in the subjects art and languages). There are no considerable changes in the sciences.

6 Results

6.1 University education

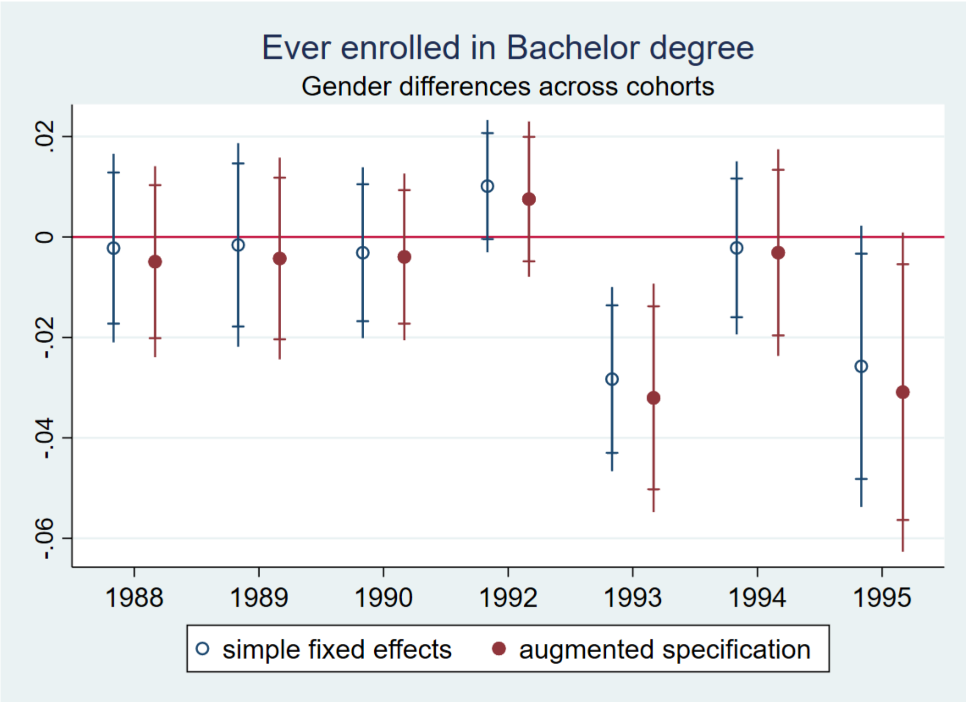
Before turning to the triple difference specification described in Section 5.1, we estimate a double difference specification, which abstracts from the regional heterogeneity of the restrictions. We compare the education outcome of men and women across the birth cohorts 1988 to 1995, i.e. individuals who were 17 to 24 years old at the time of the policy. This allows us to test if the gender gap in education from the student statistics is also visible in the individual data.

Figure 4 plots the coefficients of the cohort dummies interacted with the female dummy. The dependent variable measures whether an individual is, or has ever been, enrolled at university. We report two sets of coefficients: the first one comes from a regression

from our regression analysis: for the measure of city-level restrictions obtained from the university course books we are able to precisely determine the location of each campus.

[43]Compared to previous years, there is already a relative increase for male students in 2011, however the increase is comparable in both restricted and unrestricted cities.

Figure 4: Gender differences in university enrolment across cohorts



Notes: The horizontal axis indicates the year of birth. Reported coefficients are the cohort-female interaction terms. *Simple fixed effects*: city-gender, cohort-city, province-cohort-year and province-gender-year fixed effects. *Augmented specification*: all controls of the simple fixed effects specification plus city-year dummies and individual controls (as described in Section 5.1). 90% and 95% confidence intervals are reported. Standard errors are cluster at the city and cohort level.

that includes only a simple set of fixed effects (city-gender, cohort-city, province-cohort-year and province-gender-year); the second one is obtained from a stricter specification adding also city-year fixed effects. The coefficients of the 1993 cohort dummy×female are negative and significant. Our results indicate that the probability of studying for women compared to men is about 3-3.5 percentage points lower for this cohort as compared to the 1991 cohort. Importantly, we do not observe any pre-trends and the gender gap returns to its previous magnitude for the 1994 cohort. These results suggest that the female high school graduates of the 1993 cohort are indeed less likely to have ever enrolled in university compared to their male counterpart.

Table 2 presents our benchmark results for university education using the triple difference approach described in Eq. 1. Our basic specification in col. 1 includes only the interaction term of the 2012 admissions restrictions in city c with a dummy for female and a dummy for the 1993 cohort ($R_c^{2012} \times f_i \times Cohort_i = 1993$), the corresponding double

Table 2: The impact of local restrictions on enrollment in university

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent variable:	Ever enrolled in a Bachelor program						
							benchmark
$\mathbf{R_c^{2012} \times f_i \times Cohort_i = 1993}$	-0.029^a	-0.024^a	-0.023^a	-0.025^a	-0.018^a	-0.038^a	-0.032^b
	(0.001)	(0.005)	(0.006)	(0.007)	(0.005)	(0.011)	(0.010)
$f_i \times Cohort_i = 1993$	-0.002						
	(0.003)						
$R_c^{2012} \times f_i$	0.001						
	(0.003)						
$R_c^{2012} \times Cohort_i = 1993$	0.015^a						
	(0.002)						
$R_c^{2012} \times f_i \times Cohort_i = 1988$			0.002	0.002	-0.001	0.016	0.020
			(0.004)	(0.005)	(0.008)	(0.013)	(0.013)
$R_c^{2012} \times f_i \times Cohort_i = 1989$			-0.004	0.010	0.019^b	0.019	0.019
			(0.007)	(0.007)	(0.006)	(0.015)	(0.014)
$R_c^{2012} \times f_i \times Cohort_i = 1990$			-0.005	0.003	0.011	-0.004	-0.001
			(0.004)	(0.006)	(0.006)	(0.011)	(0.011)
$R_c^{2012} \times f_i \times Cohort_i = 1992$			0.000	0.004	0.010	0.016	0.023
			(0.005)	(0.007)	(0.007)	(0.020)	(0.019)
$R_c^{2012} \times f_i \times Cohort_i = 1994$			0.003	0.006	0.014^b	0.022	0.036^b
			(0.005)	(0.006)	(0.005)	(0.012)	(0.012)
$R_c^{2012} \times f_i \times Cohort_i = 1995$			0.014	0.028^b	0.031^a	0.023	0.018
			(0.009)	(0.012)	(0.008)	(0.025)	(0.025)
$Seatsf._c^{2012} \times f_i \times Cohort_i = 1993$				-0.000	-0.000^c	-0.000	-0.000
				(0.000)	(0.000)	(0.000)	(0.000)
$Seg._c^{2012} \times f_i \times Cohort_i = 1993$				0.022	0.075^a	0.082^c	0.061
				(0.016)	(0.013)	(0.035)	(0.035)
Individual controls ($\mathrm{X_{ift}}$)	Yes	Yes	Yes	Yes	Yes	Yes	Yes
$\sum_k Seatsf._c^a \times f_i \times cohort_k$				2012	2012	2010-14	2010-14
$\sum_k Seg._c^a \times f_i \times cohort_k$				2012	2012	2010-14	2010-14
$\sum_k R_c^a \times f_i \times cohort_k$						2011-14	2011-14
Fixed Effects	F_c, F_k, F_f & F_t	F_{cf}, F_{ck}, F_{kf} & F_t			F_{cf}, F_{ck}, F_{pkf}, & F_t		F_{cf}, F_{ck}, F_{pkf}, F_{pkt} & F_{pft}
Observations	93,718	93,700	93,700	93,700	93,700	93,700	93,700
R^2	0.061	0.109	0.110	0.110	0.112	0.113	0.148
No. of Cities	401	401	401	401	401	401	401

Notes: R denotes the restriction measure by city. c stands for city, k for birth cohort, i for individual, f for gender, p for province, and t refers to the year of the survey. *Sample:* HIES 2008-2021. Birth cohorts 1989-1995, individuals age 19 to 28 at time of interview. *Individual controls:* Urban residence and presence of another household member with education above lower middle school and their interactions with gender. Col 1 to 6 include also age_i and $age_i \times f_i$. *City controls*: Interactions of the cohort dummies and gender with i. the number of seats for women in city c in the academic year a, ii. the share of local courses that have a gender quota in city c for the academic years 2010 to 2014, and iii. the restriction variable for city c constructed (in the equivalent way as the restrictions for 2012) for the academic years 2011, 2013 and 2014. Standard errors clustered at cohort and city level in parentheses. ^{a}, ^{b} and ^{c} denote significance at the 1, 5, and 10 percent levels, respectively.

interactions and fixed effects for gender, city, cohort and year.

Our coefficient of interest, the triple interaction term, is negative and significant, indicating that the relative decrease in female university attendance compared to men is not homogeneous across Iran, but stronger in cities with restrictions. The insignificant coefficient of $f_i \times Cohort_i = 1993$ suggests that there is no significant decrease in university attendance for women relative to men for the 1993 cohort in cities without restrictions. However, the interaction term $R_c^{2012} \times Cohort_i = 1993$ shows that men from the 1993 cohort are more likely to study, compared to other cohorts, when they live in cities with restrictions. The positive effect for men is however half the size of the negative one for women. For the cohorts that are not directly affected by the policy, we find no systematic gender gap in studying between cities with and without restrictions in 2012, as suggested by the non-significant coefficient of $R_c^{2012} \times f_i$.

In col. 2 we repeat the exercise but include fixed effects at the cohort-gender, cohort-city, city-gender and year level, which capture all the double interactions. In col. 3 we further add the interaction terms for the other cohorts to test whether the policy had also an impact on individuals born before or after 1993. In both cases, the coefficient of the interaction with local restrictions is of similar magnitude and significance for women aged 19 in 2012. We find no significant effect for older or younger individuals.

However, it is possible that the decrease in the share of women studying is simply a mechanical effect of some universities decreasing the number of seats for women. Another possibility is that the effect we find results from female students avoiding universities that have implemented gender segregation. Thus, in col. 4 we add the interaction of female and cohort dummies with the number of seats for females by city in 2012, and with the share of local programs in 2012 that have gender segregation.[44] In col. 5 we repeat the same specification, but replace the cohort-gender fixed effects with province-cohort-gender fixed effects. Now, the estimate of the triple interaction term is solely based on the variation of restrictions across cities within the same province. In all these specifications our main coefficient of interest stays negative and significant with no significant decrease in magnitude. Interestingly, a higher gender segregation in 2012 is positively correlated with a decrease in the gender education gap. This indicates that in cities that had a higher share of programs with gender segregation, women were actually more likely to study.[45] The coefficient for the number of seats in 2012 for women is very close to zero

[44]We define a program as having gender segregation if either it is reserved to only one gender or it has a gender quota for seats.

[45]There are several interpretations for this finding. One of which is that many families might find it more comforting to send their daughters to universities with women-only classes. It could also be driven by cities with female oriented universities, which would lead to high numbers of segregated programs.

and not statistically significant.

An important concern in our results so far is that they may be driven by possible changes in university admission policies in other years. Thus, in col. 6 we further add the change in the share of programs open for women in the years around the policy (2011, 2013, 2014), plus the share of programs with gender segregation and the number of seats for women in all years (2010-2014). Including these additional controls actually reinforces the negative effect of the 2012 policy. This suggests that our identified policy effect is not driven by university admission policies in adjacent years.

Finally, col. 7 estimates our benchmark specification with all controls and the full set of fixed effects described in Section 5.1. The coefficient of interest remains similar and significant at the 5% level in the presence of all controls and fixed effects. It indicates that a 1 unit increase in our index of restrictions leads to a 3.2 percentage points decrease in the probability of studying at university for women, compared to men. This implies an average decrease of 3.2 percentage points in the cities that saw a moderate decrease in the share of programs open to women (up to 10%), and an average decrease of 6.4 percentage points in cities that saw stronger restrictions, for which our index of restrictions takes the value of 2.[46]

As shown in Figure 1, some restrictions for women were still present in the 2013-14 academic year. This means that the cohort of women who were 19 in 2013, i.e. those born in 1994, were also facing some restrictions to admissions. Interestingly, for this cohort we do not observe a negative effect on the probability of attending university, but a positive one, although not always significant. As opposed to the 1993 cohort, the 1994 cohort knew about the restrictions in the 2012-13 academic year well before making their choices, and thus had more time to adjust their university application. For instance, they could increase their chances to be admitted at university by considering different subjects or universities. As students already specialize in the last years of high school in one of the three broad fields (Mathematics, Sciences or Humanities), switching ad hoc to a *konkur* with less restrictions, such as Humanities or Sciences, is difficult. In Section 6.3,

[46] Notice that this interpretation requires the assumption of strong parallel trends (see Callaway et al., 2021). However, our findings do not hinge on using this index: we show in Section C that we find similar effects when using a simplified version of the restriction measure, taking values -1, 0 and 1.

we nevertheless examine this possibility by studying whether the policy had an impact on the field of study at university.[47]

Our results highlight that the 2012 policy did not have a homogeneous impact across the country, and thus contributed to regional inequalities in education.[48] Furthermore, our results are not driven by the mechanical effect of universities decreasing access for women to popular programs. Our interpretation is that these results are driven by the policy discouraging women from studying a different subject or the same subject at a different university. This effect only holds for the 1993 cohort, for which these restrictions were unexpected and left little time to adjust.

In Section C of the Appendix, we show that our results are robust to adding further controls, using alternative measures of restrictions, and making different sample choices.

6.2 Heterogeneous responses and mechanism

In this section we explore which locations and individuals are relatively more affected by the policy. This helps to understand the mechanism behind how the 2012 policy led to the observed gender education gap.

6.2.1 Regional heterogeneity and the role of private universities

Table 3 explores regional heterogeneity in the impact of the policy. First, we split the sample between between more versus less conservative provinces. A province is defined as "conservative" when its vote share for the conservative candidates in the 2009 presidential election was above 70%.[49] While we have shown in Section 5.2 that more conservative cities were not more likely to implement stricter restrictions, it is still possible that the same restriction in a more conservative area has a different impact; for instance, in these areas women may be less likely to travel long distances to university, or restrictions may be

[47]A positive effect is also observed for the 1995 cohort, although this is not statistically significant in our benchmark specification. It is also worth noting that the estimates for the 1995 cohort are based on a relatively small number individuals.

[48]We explore further the regional heterogeneity of the impact of the policy in Section 6.2.

[49]As our coefficient of interest is based on variation across cities within the same province, a split of the sample by voting of the city is not meaningful as this would reduce substantially the variation in treatment intensity within provinces.

perceived as a stronger signal that women should not study. We find that the effect of the restrictions for the 1993 cohort is indeed stronger and only significant in the conservative areas. The size of the coefficient for the less conservative provinces remains however similar in magnitude.

Second, we exploit the urban-rural divide in the following two columns. Each city (*shahrestan*) has an urban and a rural part and the differences between urban and rural areas in terms of income, education and employment are high. It is however not obvious ex-ante whether individuals in rural or urban areas are more affected by the policy under study. Universities are concentrated in urban areas; thus, restrictions may be more salient for urban students, who may have a stronger preference for programs close by. In contrast, rural students are more likely to travel longer distances to attend universities in any case, and thus may be more willing to attend universities even in further away cities. Nevertheless, rural households are more likely to be financially constrained compared to urban, and thus may be less able to afford the cost of travelling longer distances or the costs of dormitories. The latter would make restrictions in the closest university more salient for rural students.

Col. 3 and 4 of Table 3 show the results when splitting the sample between urban and rural households. Among urban individuals, we observe a negative and significant effect of the policy on university attendance for women compared to men in the cohort of interest. In the rural sample, we cannot detect any significant effect for the cohort of interest. This differential impact is in line with urban women being less inclined to attend universities that are further away, where it would have been difficult to continue living at home, or less flexible in terms of fields.

So far, we have disregarded the presence of private universities as potential substitutes for public institutions.[50] Our local restrictions variable R_{ct}^{2012} is constructed based on programs offered by public universities. However, about half of the cities with a public university also have at least one private university.[51] While private universities have on average more male than female students, they did not impose any segregation measures,

[50]Note that our dependent variable measures whether an individual has ever enrolled at a university, without distinguishing between public and private institutions. Moreover, our city-gender fixed effects control for gender differences in use of private universities by city.

[51]See Section 4.3 for the definition of private university in our sample.

Table 3: Policy effects by region

	(1)	(2)	(3)	(4)
Dependent variable:	Ever enrolled in a Bachelor program			
	Conservatives provinces	Less conserv. provinces	Urban areas	Rural areas
$R_c^{2012} \times f_i \times Cohort_i = 1992$	0.022	0.029	-0.021	0.053^b
	(0.030)	(0.028)	(0.024)	(0.019)
$\mathbf{R_c^{2012} \times f_i \times Cohort_i = 1993}$	-0.038^b	-0.032	-0.077^a	0.013
	(0.015)	(0.023)	(0.015)	(0.021)
$R_c^{2012} \times f_i \times Cohort_i = 1994$	0.047^b	0.036	0.013	0.051^c
	(0.017)	(0.022)	(0.020)	(0.027)
$R_c^{2012} \times f_i \times Cohort_i = 1995$	-0.011	0.054	-0.032	0.026
	(0.036)	(0.032)	(0.033)	(0.025)
Controls	Individual and city controls (X_{ift} & Z_{cfk})			
Fixed effects	F_{cf}, F_{ck}, F_{pkf}, F_{pkt} & F_{pft}			
Observations	48,138	45,676	57,315	36,123
R^2	0.146	0.153	0.179	0.233
No. of Cities	204	198	395	400

Notes: R denotes the restriction measure by city. c stands for city, k for birth cohort, i for individual, f for gender, p for province, and t refers to the year of the survey. *Sample:* HIES 2008-2021. Birth cohorts 1989-1995, individuals age 19 to 28 at time of interview. *Individual controls:* Urban residence and presence of another household member with education above lower middle school and their interactions with gender. *City controls*: Interactions of the cohort dummies and gender with i. the number of seats for women in city c in the academic year a, ii. the share of local courses that have a gender quota in city c for the academic years 2010 to 2014, and iii. the restriction variable for city c constructed (in the equivalent way as the restrictions for 2012) for the academic years 2011, 2013 and 2014. Regressions all include but don't show the interaction terms of local restrictions for the older cohorts. Standard errors clustered at cohort and city level in parentheses. ^{a}, ^{b} and ^{c} denote significance at the 1, 5, and 10 percent levels, respectively.

as shown in Figure D.4. Thus, women may rely on them more in areas where access to public universities is restricted.

To address the role of private universities in mitigating the effect of the 2012 policy, we first use the student statistics data to plot the number of males and females in private universities, splitting the sample by whether the university is in a city with restrictions or not (Figure D.9). We do not observe an increase in women's enrollment in private universities in restricted cities.[52]

[52]The figure also reveals that the evolution of the gender gap in enrollment is very similar in restricted

Table 4: The role of private universities

	(1)	(2)
Dependent variable:	Ever enrolled in a Bachelor program	
	Women only	Men only
$R_c^{2012} \times Cohort_i = 1993$	-0.032^b	0.025^b
	(0.013)	(0.010)
$R_c^{2012} \times Cohort_i = 1992$	0.023	0.008
	(0.016)	(0.014)
$R_c^{2012} \times Cohort_i = 1994$	-0.020	0.010
	(0.014)	(0.014)
$R_c^{2012} \times Cohort_i = 1995$	-0.021	0.013
	(0.017)	(0.020)
$R_c^{2012} \times Cohort_i = 1993 \times Private_c$	0.026	-0.008
	(0.021)	(0.013)
$R_c^{2012} \times Cohort_i = 1994 \times Private_c$	0.054^b	-0.029
	(0.019)	(0.016)
$R_c^{2012} \times Cohort_i = 1995 \times Private_c$	0.041	-0.033
	(0.023)	(0.020)
$Private_c \times Cohort_i = 1992$	0.006	0.008
	(0.020)	(0.020)
$Private_c \times Cohort_i = 1993$	-0.005	-0.001
	(0.024)	(0.016)
$Private_c \times Cohort_i = 1994$	0.027	0.082^a
	(0.024)	(0.021)
$Private_c \times Cohort_i = 1995$	-0.003	0.007
	(0.023)	(0.023)
Controls	Individual and city controls (X_{it} & Z_{ck})	
Fixed effects	F_{pk} & F_{ct}	
Observations	44,990	47,803
R^2	0.187	0.163
No. of Cities	401	401

Notes: R denotes the restriction measure by city. c stands for city, k for birth cohort, i for individual, p for province, and t refers to the year of the survey. *Sample:* HIES 2008-2021. Birth cohorts 1989-1995, individuals age 19 to 28 at time of interview. *Individual controls:* Urban residence and presence of another household member with education above lower middle school. *City controls:* Interactions of the cohort dummies with i. the number of seats for women in city c in the academic year a, ii. the share of local courses that have a gender quota in city c for the academic years 2010 to 2014, and iii. the restriction variable for city c constructed (in the equivalent way as the restrictions for 2012) for the academic years 2011, 2013 and 2014. Regressions all include but don't show the interaction terms of local restrictions for the older cohorts. Standard errors clustered at cohort and city level in parentheses. ^{a}, ^{b} and ^{c} denote significance at the 1, 5, and 10 percent levels, respectively.

Next, in Table 4 we investigate whether prospective students with better access to private universities react differently to restrictions than those who have more limited access. We do this by including triple interactions of the local restrictions variable R_c^{2012} with the cohort dummies and the dummy *Private*, which takes the value one if there is

and unrestricted cities.

Table 5: Policy effects by income and education level of household head

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable:	Ever enrolled in a Bachelor program							
	Income of household head				Education level of household			
	Below median		Above median		Low		High	
	City	Province	City	Province	HH head	Max.	HH head	Max.
$R_c^{2012} \times f_i \times Cohort_i = 1992$	0.008	-0.016	0.035	0.029	0.034^c	0.013	0.014	-0.004
	(0.023)	(0.019)	(0.026)	(0.024)	(0.016)	(0.032)	(0.029)	(0.018)
$\mathbf{R_c^{2012} \times f_i \times Cohort_i = 1993}$	-0.108^a	-0.086^a	0.007	-0.013	-0.071^a	-0.079^b	0.007	-0.030^c
	(0.015)	(0.013)	(0.018)	(0.016)	(0.015)	(0.028)	(0.020)	(0.014)
$R_c^{2012} \times f_i \times Cohort_i = 1994$	-0.012	0.009	0.078^a	0.054^b	0.012	0.038	0.083^a	0.016
	(0.026)	(0.022)	(0.018)	(0.017)	(0.020)	(0.030)	(0.023)	(0.017)
$R_c^{2012} \times f_i \times Cohort_i = 1995$	-0.053	-0.049	0.042	0.038	0.025	0.009	0.042	0.016
	(0.037)	(0.029)	(0.030)	(0.034)	(0.027)	(0.037)	(0.034)	(0.027)
Controls	Individual and city controls (X_{ift} & Z_{cfk})							
Fixed effects	F_{cf}, F_{ck}, F_{pkf}, F_{pkt} & F_{pft}							
Observations	42,018	43,494	47,247	45,766	48,775	20,245	40,420	68,795
R^2	0.237	0.236	0.210	0.214	0.205	0.341	0.243	0.171
No. of Cities	401	401	401	401	401	401	401	401

Notes: R denotes the restriction measure by city. c stands for city, k for birth cohort, i for individual, f for gender, p for province, and t refers to the year of the survey. *Sample:* HIES 2008-2021. Birth cohorts 1989-1995, individuals age 19 to 28 at time of interview. Household heads are excluded. *Individual controls:* Urban residence and presence of another household member with education above lower middle school and their interactions with gender. *City controls*: Interactions of the cohort dummies and gender with i. the number of seats for women in city c in the academic year a, ii. the share of local courses that have a gender quota in city c for the academic years 2010 to 2014, and iii. the restriction variable for city c constructed (in the equivalent way as the restrictions for 2012) for the academic years 2011, 2013 and 2014. Regressions all include but don't show the interaction terms of local restrictions for the older cohorts. Standard errors clustered at cohort and city level in parentheses. ^{a}, ^{b} and ^{c} denote significance at the 1, 5, and 10 percent levels, respectively.

a private university at equal or shorter distance than a public university. To facilitate the interpretation of estimates, we split the sample between males and females and thus compare cohorts of the same gender in the same cities.[53] We observe no significant effects of this triple interaction term with the presence of a private university for the 1993 cohort. The negative effect of the restrictions in public universities on female university attendance for this cohort remains. Thus, private universities did not mitigate the impact of the policy for women. This result suggests that private and public universities are imperfect substitutes, due to the lower quality and higher fees of private universities, and to the fact that prospective students did not have the time to adjust to the restrictions by applying more to private universities. We however see a move towards private universities for women of the following cohort, who had more time to adapt to the restrictions.

6.2.2 Heterogeneity by income and education attainment of households

The most obvious adjustment to local restrictions is moving to another city or attending a private university. But these alternatives are costly. Thus, families with higher incomes are expected to be better able to send their children to university even if they did not get a seat in a public university close by. To see whether indeed part of the negative impact of the restrictions on women's higher education is due to the incapacity of households to switch to more costly alternatives, Table 5 splits our sample according to whether the income of the household head is above or below the median income of household heads in the same city (col. 1 and 3) or the same province (col. 2 and 4).[54] We find a robust negative impact of the policy on the 1993 cohort for households whose head has an income below median, both compared to the income in the city and in the province. The coefficients are close to zero and not significant for the richer households. Moreover, there is a positive effect on the gender gap of the 1994 cohort for the richer households, indicating that these households adjusted more to the policy in the following year.

Another potential driver of our results is the lack of information about specific programs and the university application process. If this mechanism is at work, prospective students with less information are likely to be more affected by the restrictions. To investigate further this channel, we split the sample according the level of education of the household head (col. 5 and 7) or the highest level of education in the household, excluding the individuals themselves (col. 6 and 8). Prospective students living in families with higher levels of education may have better access to information; moreover, these families are also likely to attribute a higher value to a university degree and thus spend more time and effort on the university application. Our results show that the negative effect of the policy is very strong for households with low levels of education. The effect is less robust for women in more educated households. Given that the correlation between income and education level is low (around 15%), our results suggests that both the financial and the information channel are likely to play a role.

[53] Splitting the sample by gender avoids the use of quadruple interaction terms.

[54] As the level of income depends on whether a person has attended university or not, for this analysis we exclude the few individuals who are household heads.

Table 6: Restrictions impact on field of study

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable:	Maths		Humanities		Sciences		Overlapping	
$R_c^{2012} \times f_i \times Cohort_i = 1992$	-0.002	0.000	0.020	0.023	-0.020^c	-0.022^c	-0.015	-0.018
	(0.016)	(0.018)	(0.011)	(0.011)	(0.008)	(0.009)	(0.018)	(0.017)
$\mathbf{R_c^{2012} \times f_i \times Cohort_i = 1993}$	-0.040^c	-0.044^b	0.018	0.019	0.015	0.013	-0.021	-0.030^c
	(0.015)	(0.015)	(0.010)	(0.011)	(0.010)	(0.009)	(0.011)	(0.011)
$R_c^{2012} \times f_i \times Cohort_i = 1994$	-0.003	-0.008	0.027^c	0.027	-0.018^c	-0.022^b	0.008	0.001
	(0.022)	(0.024)	(0.013)	(0.013)	(0.007)	(0.007)	(0.015)	(0.014)
$R_c^{2012} \times f_i \times Cohort_i = 1995$	-0.019	-0.027	0.016	0.014	0.040	0.034	0.009	-0.001
	(0.020)	(0.019)	(0.011)	(0.010)	(0.020)	(0.019)	(0.014)	(0.015)
Individual controls	basic	full	basic	full	basic	full	basic	full
City controls (Z_{cfk})	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fixed effects	F_{cf}, F_{ck}, F_{pkf}, F_{pft} & F_{date}							
Observations	30,706	30,516	30,706	30,516	30,706	30,516	30,706	30,516
R^2	0.148	0.180	0.114	0.125	0.113	0.133	0.117	0.138
No. of Cities	385	385	385	385	385	385	385	385

Notes: R denotes the restriction measure by city. c stands for city, k for birth cohort, i for individual, f for gender, p for province, and t refers to the year of the survey. *Sample:* LFS 2013-2017. Birth cohorts 1991-1995, indidviduals age 21 to 22 at time of interview. *Individual controls:* Urban residence and presence of another household member with education above lower middle school and their interactions with gender. *City controls*: Interactions of the cohort dummies and gender with i. the number of seats for women in city c in the academic year a, ii. the share of local courses that have a gender quota in city c for the academic years 2010 to 2014, and iii. the restriction variable for city c constructed (in the equivalent way as the restrictions for 2012) for the academic years 2011, 2013 and 2014. Regressions all include but don't show the interaction terms of local restrictions for the older cohorts. Standard errors clustered at cohort and city level in parentheses. ^{a}, ^{b} and ^{c} denote significance at the 1, 5, and 10 percent levels, respectively.

6.3 Impact on the field of study and on associate degrees

We now explore the impact of the policy on other education outcomes: the field that women study at university, and the probability of enrolling in an associate degree.

We first focus on the field of study. We expect that the policy negatively affected the probability of women of the 1993 to study subjects that can be chosen after taking the mathematics *konkur*, as the majority of the restrictions affected these fields. For this we use data from the Labor Force Survey (LFS) for the years 2013-2017.[55] Due to the reduced years available, we only include the cohorts 1991-1995, i.e. individuals aged 17-21 in 2012, and we thus focus only on individuals who are 21 or 22 at the time of interview. This ensures that the cohorts included are of similar age at the moment of the survey. Our main outcome variables are dummies indicating whether individuals have ever studied at university a subject that can be chosen after taking the *konkur* in humanities, sciences or mathematics, or subjects that are common to the different *konkur*.

[55] For more details on the LFS data see Section B.2 in the Appendix.

Table 6 shows that indeed the policy increased the gender gap in studying a subject in the mathematics *konkur* field for the 1993 cohort. We also observe a negative probability for women to study subjects that are common across the three different *konkur* (col. 7 and 8).[56] This is likely due to the increased competition in these subjects: as the policy mainly affected admissions to engineering programs, women from the mathematics track were likely to divert to subjects such as accounting, economics and chemistry that are also open to students from the other *konkur* and which are very popular. Good students from the mathematics track were thus likely to crowd out women from other tracks who wanted to study these subjects. This may result in a decrease in women studying overlapping subjects if, for instance, some female students only applied for a limited number of highly competitive programs in these subjects, and eventually did not manage to obtain a seat due to the higher competition. Moreover, we note for the following cohort (age 18 at the time of the policy) a positive impact on studying a subject in the humanities fields, and a negative impact on enrolling in a subject within the sciences track. This reveals that, to some extent, the policy may have contributed to redirect women towards more traditionally female fields.

The results so far imply that the women who were most affected by the policy were less likely to study at university, particularly in the field of mathematics. One remaining question is whether these women gave up studying altogether, or whether they obtained further education by enrolling in an associate degree. We explore this option in Table 7 using again the HIES data. We observe a positive differential effect of being enrolled in an associate degree for women born in 1993 compared to men, of 1.7 percentage points (col. 1). Given that on average 11% of a cohort of high school graduates have studied for an associate degree, this represents a significant increase. We then analyze whether the observed differential effect is driven by changes in enrollment in associate degrees by women or by men. We do this by splitting the sample between women and men (col. 2 and 3): we observe a negative and significant impact for men, while for women the coefficient is positive but smaller and not significant. These results suggest that the restrictions did not represent a big push for women to continue their education by enrolling in associate

[56] Table D.12 reports results only considering individuals with university education. Here too we note a negative impact on the probability of studying a subject in the mathematics track, and a negative but insignificant impact on the probability of studying one of the common subjects.

Table 7: Associate degree

	(1)	(2)	(3)
Dependent variable:	Associate Degree		
	All individuals	Women only	Men only
$R_c^{2012} \times f_i \times Cohort_i = 1992$	0.004		
	(0.007)		
$\mathbf{R_c^{2012} \times f_i \times Cohort_i = 1993}$	0.017^c		
	(0.008)		
$R_c^{2012} \times f_i \times Cohort_i = 1994$	-0.001		
	(0.011)		
$R_c^{2012} \times f_i \times Cohort_i = 1995$	-0.020^b		
	(0.008)		
$R_c^{2012} \times Cohort_i = 1992$		-0.008	-0.014
		(0.006)	(0.008)
$\mathbf{R_c^{2012} \times Cohort_i = 1993}$		0.010	-0.022^b
		(0.007)	(0.008)
$R_c^{2012} \times Cohort_i = 1994$		-0.016^c	-0.025^b
		(0.008)	(0.009)
$R_c^{2012} \times Cohort_i = 1995$		-0.001	0.005
		(0.007)	(0.009)
Controls	X_{ift} & Z_{cfk}	X_{it} & Z_{ck}	
Fixed effects	F_{cf}, F_{ck}, F_{pkf}, F_{pkt} & F_{pft}	F_{pk} & F_{ct}	
Observations	93,700	44,990	47,803
R^2	0.091	0.112	0.121
No. of Cities	401	401	401

Notes: R denotes the restriction measure by city. c stands for city, k for birth cohort, i for individual, f for gender, p for province, and t refers to the year of the survey. *Sample:* HIES 2008-2021. Birth cohorts 1989-1995, individuals age 19 to 28 at time of interview. *Individual controls:* Urban residence and presence of another household member with education above lower middle school. *City controls:* Interactions of the cohort dummies with i. the number of seats for women in city c in the academic year a, ii. the share of local courses that have a gender quota in city c for the academic years 2010 to 2014, and iii. the restriction variable for city c constructed (in the equivalent way as the restrictions for 2012) for the academic years 2011, 2013 and 2014. Col. 1 includes also the interactions of all control variables with gender. Regressions all include but don't show the interaction terms of local restrictions for the older cohorts. Standard errors clustered at cohort and city level in parentheses. ^{a}, ^{b} and ^{c} denote significance at the 1, 5, and 10 percent levels, respectively.

degrees. Instead, men were more likely to be admitted to university, thus they have a lower probability to study for an associate degree.

Thus, our estimates indicate a negative overall impact of the 2012 policy on women's higher education. As a result, this could have had consequences on the marriage and labor markets. We investigate these potential effects in the next section.

6.4 Marriage and labor market outcomes

Changes in higher education are expected to affect individuals also further in life, in particular when it comes to labor market and marriage market outcomes. It is important to note that marriage and labor market outcomes, especially for women, are closely linked and simultaneously determined. For instance, it is well-known that married women are less likely to participate in the labor market. This is also true in the Iranian context: only 11% of married women in our sample are in the labor force, compared to 30% of unmarried women. The university policy under study here may thus affect marriage outcomes directly through university education, but also indirectly through its potential labor market effects as the decision to marry may be dependent on their labor market outcomes. At the same time, outcomes of women in the labor market can be a result of their marriage decisions. In the following two subsections we first analyze the impact of the 2012 policy on marriage and then its impact on labor market outcomes.

6.4.1 Marriage outcomes

In this section, we investigate whether the policy had an effect on marriage outcomes, in particular on the probability of getting married for young women, and on the quality of the marriage match for those who marry. Through its negative effect on university education, the 2012 policy could affect the probability of marriage at a young age for women. The overall effect is a priori ambiguous. Marriage could be considered as an alternative to university education. Thus, we may see an increase in the probability of marrying at a young age for women of the affected cohort. However, as education is highly valued in Iranian society, not having a university degree could harm women's ability to find a suitable match in the marriage market (Rezai-Rashti and Moghadam, 2011). There may also be marriage effects for the women of the 1993 cohort who do manage to attend university despite the restrictions. As shown in Section 6.3, women of this cohort are more likely to study subjects that attract more females instead of subjects that attract more males (e.g., engineering). This can decrease their chances to meet a potential partner at university.

Regarding match quality, both not being able to attend university, and studying sub-

Table 8: Women married at young age

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	All women			With university edu.			No university edu		
Dep. variable:	Being married								
Age group:	19-21	19-23	19-28	19-21	19-23	19-28	19-21	19-23	19-28
$R_c^{2012} \times Cohort_i = 1992$	-0.037	-0.010	-0.034[b]	-0.090[c]	-0.001	-0.048[b]	-0.034	0.004	0.001
	(0.027)	(0.015)	(0.013)	(0.043)	(0.021)	(0.015)	(0.039)	(0.024)	(0.017)
$\mathbf{R_c^{2012} \times Cohort_i = 1993}$	-0.094[b]	-0.045[b]	-0.050[a]	-0.069	-0.096[a]	-0.083[a]	-0.134[b]	-0.051[c]	-0.050[b]
	(0.028)	(0.014)	(0.011)	(0.058)	(0.027)	(0.018)	(0.042)	(0.023)	(0.016)
$R_c^{2012} \times Cohort_i = 1994$	-0.022	-0.008	0.002	0.060	-0.012	-0.001	-0.034	0.018	0.026
	(0.025)	(0.023)	(0.012)	(0.068)	(0.036)	(0.019)	(0.044)	(0.024)	(0.019)
$R_c^{2012} \times Cohort_i = 1995$	-0.034	-0.008	-0.014	-0.032	0.001	-0.038	-0.035	-0.001	0.012
	(0.044)	(0.017)	(0.014)	(0.080)	(0.038)	(0.026)	(0.052)	(0.028)	(0.018)
Controls	Individual and city controls (X_{it} & Z_{ck})								
Fixed effects	F_{pk} & F_{ct}								
Observations	16,571	26,069	44,990	4,786	9,078	17,472	10,894	15,863	26,074
R^2	0.217	0.212	0.248	0.336	0.292	0.305	0.276	0.286	0.333
No. of Cities	379	391	401	289	360	395	369	383	400

Notes: R denotes the restriction measure by city. c stands for city, k for birth cohort, i for individual, p for province, and t refers to the year of the survey. *Sample:* HIES 2008-2021. Women of birth cohorts 1989-1995, individuals age 19 to 21 (col. 1, 4 and 7), individuals age 19 to 23 (col. 2, 5 and 8), individuals age 19 to 28 (col 3, 6 and 9) at time of interview. *Individual controls:* Urban residence and presence of another household member with education above lower middle school. *City controls:* Interactions of the cohort dummies with i. the number of seats for women in city c in the academic year a, ii. the share of local courses that have a gender quota in city c for the academic years 2010 to 2014, and iii. the restriction variable for city c constructed (in the equivalent way as the restrictions for 2012) for the academic years 2011, 2013 and 2014. Regressions all include but don't show the interaction terms of local restrictions for the older cohorts. Standard errors clustered at cohort and city level in parentheses. [a], [b] and [c] denote significance at the 1, 5, and 10 percent levels, respectively.

jects with a lower average share of males could decrease the probability of women of the affected cohort to find a good match for marriage. Thus, for the matches that do occur, we would expect to find a decrease in quality for women of the 1993 cohort.

Probability of being married

To analyze the impact on marriage at a young age, we run regressions where the outcome is a dummy variable indicating whether the individual is married at the time of the interview. We restrict the sample to women, as the decision of whether and when to marry may be driven by different factors depending on gender, and hence the outcomes for the two groups are difficult to compare.[57] We do this analysis for women of three age groups: right after high school (age 19-21), women at the age they normally attend university (19-23) and for the full sample (19-28).

[57] The average age of marriage is also considerably lower for women than for men. In our sample more than 50% of women are married at age 24, while for men it is only 23% at this age. As our data ends when individuals of the 1993 cohort are 28 years old, for the marriage outcomes analysis we only focus on young women. In unreported robustness checks, we also investigate the impact of the policy on marriage outcomes for men. We do not find any significant impact for the 1993 cohort.

Table 8 presents the results. We observe a decrease in the probability of being married for those women who were 19 at the time of the policy in cities with stronger restrictions, for all three age ranges considered (col. 1-3). To obtain more clarity on the mechanisms, we split the sample into women with (col. 4-6) and without university education (col. 7-9). The effect is present for both subgroups.[58] The fact that we still observe a significant negative effect on the probability of being married for the age group 19-28 suggests that women are not just delaying marriage after the end of their studies. For women of younger cohorts, we see no significant impact on the probability of marrying.

Match quality

The negative effect of the 1993 cohort is consistent with women having more difficulties finding a suitable spouse. To explore whether this is indeed the case, we investigate whether there is any change in the quality of the match for the women who do marry. We measure match quality by comparing education, age and income of the spouses. These characteristics have been found in the literature to be important determinants of match formation and quality (e.g. Becker et al., 1977; Belot and Francesconi, 2013; Greenwood et al., 2014). There is ample evidence of assortative matching in terms of age and education and existing research suggests that couples that are more similar along these dimensions tend to be more stable (Cherlin, 1977; Lee and McKinnish, 2018; Weiss and Willis, 1997). Moreover, researchers have shown the importance of male income in the marriage market: for instance, young males with higher potential earnings are more likely to marry and less likely to divorce (Burgess et al., 2003). In a context such as Iran, where women's earning possibilities are limited, women may have a preference for marrying men with higher education or higher income, which signal higher long-term earning potentials.

Table 9 displays our results concerning the education of the spouse. Appendix B.1 describes how we identify couples within household. Considering all married women for whom we can observe the husband (Panel A), we see a decrease in assortative matching in terms of education: in the cohort of interest, married women are less likely to have a spouse with the same education level (col. 1). Furthermore, they are more likely to have

[58] Notice that we cannot infer causality from the regressions with the split between university and non-university educated, as university education is endogenous and there may be selection in the composition of the two groups. Hence, these results are only illustrative for the potential mechanisms.

higher education than their spouse (col. 5). When splitting the sample between women with and without higher education (Panels B and C respectively), we note that the results for the quality of matches in terms of education are driven by women without university education, while we see limited effects for women who attended university. Interestingly, for women of the 1994 cohort we find a positive impact on the education level of the spouse, and a decrease in the difference in education level of the wife and the husband. This suggests a positive impact on the quality of matches for women of this cohort, who are overall more likely to attend university. One potential explanation here could be that for them it was easier to meet a husband at university. The negative effects for the 1993 cohort of women without university education and the younger women with university education would also be in line with male university graduates postponing marriage and waiting for younger women with university education instead of marrying earlier but a woman who is less educated.

Table D.13 analyzes the effect of the policy on a couple's age difference (col. 1 and 2), on the spouse's log income (col. 3 and 4) and on log per capita income of the household into which a woman has married (col. 5 and 6).[59] We find no clear pattern concerning the age composition of couples. However, results suggest that women of the 1993 cohort in restricted cities have husbands with lower income while women of the 1994 cohort generally have husbands that are better off. This holds in particular for women with university education.

Overall, our findings indicate that women directly affected by the 2012 restrictions are less likely to marry at a young age, and those who do marry have on average spouses with lower education. It is the following generation of female university students that seems to obtain better matches instead. These results support the idea that university is an important aspect of the marriage market for young Iranians, and that a university degree acts as a signal on the marriage market.

[59]For log per capita income of the household into which a woman has married we only consider households where the household head is the husband or the husband's father. We exclude the very few households where the household head is the wife's father.

Table 9: Educational match on the marriage market

	(1)	(2)	(3)	(4)	(5)
Panel A:			*All married women*		
Dep. var:	Same level of edu	Spouse has univ. edu	Edu level of spouse	Edu diff (wife - spouse)	Edu spouse < edu wife
$R_c^{2012} \times Cohort_i = 1992$	-0.013	0.010	0.076	-0.055	0.005
	(0.020)	(0.016)	(0.075)	(0.070)	(0.022)
$\mathbf{R_c^{2012} \times Cohort_i = 1993}$	-0.068^b	-0.035	-0.136	0.088	0.051^c
	(0.022)	(0.021)	(0.084)	(0.076)	(0.026)
$R_c^{2012} \times Cohort_i = 1994$	-0.010	0.000	0.005	-0.022	0.004
	(0.029)	(0.029)	(0.088)	(0.092)	(0.027)
$R_c^{2012} \times Cohort_i = 1995$	-0.036	-0.004	0.002	-0.006	0.014
	(0.031)	(0.024)	(0.080)	(0.090)	(0.031)
Observations	15,691	15,691	15,691	15,691	15,691
R^2	0.212	0.249	0.273	0.241	0.227
Panel B:			*Married women with university education*		
$R_c^{2012} \times Cohort_i = 1992$	0.020	-0.030	-0.181	0.224	0.047
	(0.051)	(0.064)	(0.186)	(0.183)	(0.061)
$\mathbf{R_c^{2012} \times Cohort_i = 1993}$	-0.020	0.007	0.190	-0.224	-0.023
	(0.064)	(0.067)	(0.199)	(0.192)	(0.058)
$R_c^{2012} \times Cohort_i = 1994$	0.034	0.156	0.803^b	-0.824^b	-0.155
	(0.092)	(0.106)	(0.244)	(0.240)	(0.103)
$R_c^{2012} \times Cohort_i = 1995$	-0.007	-0.018	-0.103	0.083	-0.004
	(0.138)	(0.135)	(0.371)	(0.386)	(0.140)
Observations	3,050	3,050	3,050	3,050	3,050
R^2	0.412	0.443	0.479	0.476	0.435
Panel C:			*Married women without university education*		
$R_c^{2012} \times Cohort_i = 1992$	-0.024	0.030	0.113	-0.108	0.004
	(0.032)	(0.020)	(0.110)	(0.101)	(0.032)
$\mathbf{R_c^{2012} \times Cohort_i = 1993}$	-0.064	-0.026	-0.219^c	0.253^b	0.082^c
	(0.034)	(0.021)	(0.109)	(0.105)	(0.039)
$R_c^{2012} \times Cohort_i = 1994$	-0.004	-0.024	-0.120	0.105	0.043
	(0.040)	(0.029)	(0.100)	(0.094)	(0.029)
$R_c^{2012} \times Cohort_i = 1995$	-0.042	-0.007	-0.099	0.123	0.049
	(0.044)	(0.029)	(0.112)	(0.109)	(0.040)
Observations	11,438	11,438	11,438	11,438	11,438
R^2	0.260	0.276	0.290	0.284	0.274

Notes: R denotes the restrictions measure by city. c stands for city, k for birth cohort, i for individual, f for gender, p for province, and t refers to the year of the survey. *Sample:* HIES 2008-2021. Married women of birth cohorts 1989-1995 for which the husband could be identified within same household. Individuals age 19 to 30 at time of interview. *Dependent variables:* Husband and wife have the same level of education (Col. 1), dummy for husband has ever been enrolled in university (Col. 2), education level of husband (Col. 3), level of education of wife minus level of education of husband (Col. 4), dummy for husband having a lower level of education than wife (Col. 5). *Individual controls:* Urban residence. *City controls:* Interactions of the cohort dummies with (i) the number of seats for women in city c in the academic year a, (ii) the share of local courses that have a gender quota in city c for the academic years 2010 to 2014, and (iii) the restriction variable for city c constructed (in the equivalent way as the restrictions for 2012) for the academic years 2011, 2013 and 2014. All regressions include fixed effects at the province-cohort (F_{pk}) and city-year (F_{ct}) level. Regressions all include but don't show the interaction terms of local restrictions for the older cohorts. Standard errors clustered at cohort and city level in parentheses. ^{a}, ^{b} and ^{c} denote significance at the 1, 5, and 10 percent levels, respectively.

6.4.2 Labor market

The decreased university enrolment for women may also affect female employment rate and labor force participation. The mechanisms through which such impact may arise are likely different for women who attend university and women who do not. Women who do not get a seat at university may directly join the labor force, leading to a higher probability of young women being employed. However, not having a bachelor's degree might harm their chances in the labor market, particularly in the Iranian context, where university degrees are valued by employers (see Appendix A). For women who managed to get a seat at university, as seen in Section 6.3, the education policy reduced the share of women who studied in the mathematics track and in fields that can be studied after all three main tracks. In the labor market, these fields lead to occupations that are male dominated. Evidence from the U.S. shows that women who specialize in male-dominated fields have an advantage in the labor market (Belot et al., 2023); thus, if this is also valid for Iran, women who study these subjects are expected to have better chances of a good career after university.[60] Thus, women in this group may find it more difficult to find a job, or may decide not to join the labor force after studying. At the same time, more competition at university implies that women who ultimately attended university may be more skilled or motivated, and thus excel in the labor market. Furthermore, as noted in the previous section, young women's decision to participate in the labor market also depends on their marriage decisions.

We estimate the impact of the policy on several labor market outcomes: being employed for a wage, being out of work and out of study, the sectors in which individuals work, and individuals' income. Since the last year in our sample is 2021, our results capture the short-term effects of the policy on labor market outcomes.

When analyzing the impact of the 2012 policy on the labor market, interpretation of the triple difference results is not straightforward. As Iran has compulsory military service for men, which has to start within one year after graduation, male graduates are likely to join the labor market two years after their female counterparts.[61] Therefore, concerning

[60] For our cohorts of interest, most of the employed women with university education studied a subject that belongs to the 'overlapping' category.

[61] Notice that men and women generally enroll in higher education or for an associate degree directly

labor market outcomes, the male comparison cohort for the female 1993 cohort is not clear. For this reason, we also present results by gender.

Employment

We first investigate the impact of the policy on being employed or not working at all. The first outcome variable is an indicator equal to one if working for a wage.[62] The second equals one when the individual is unemployed, a housewife, working without a wage or "other". It equals zero when an individual is employed for a wage or studying.[63] We split the sample between individuals aged 19-23, the age at which young Iranians generally attend university, and older individuals aged 24-30.

Table D.14 in the Appendix investigates whether women who are missing university education because of the policy are entering the labor market instead. It thus reports results for individuals at university age (19-23). We observe no significant effect on either employment or being out of work and out of study for the women of the 1993 cohort in cities with more restrictions.[64] However, during our sample period, youth unemployment is very common in Iran. In our sample, only 16% of individuals in this age group are working for a wage and also young Iranians particularly motivated to work when not being able to join university face difficulties finding a job quickly.

Table 10 presents the results for individuals aged 24 to 30. Panel A shows triple difference results. Including all individuals with at least high school education (col. 1 and 2), we do not observe any significant effect on the probability of being employed for a wage across gender and cities for the 1993 cohort. However, we find a positive impact on being out of work and out of study. As the labor market effects of the policy may be different for university versus non-university educated, we analyze the impact of the policy separately for these two sub-populations. There is no significant impact of the

after high school. Thus, for education purposes, men and female of the same cohort are comparable.

[62]There are also individuals declaring to do unpaid work, typically women on family farms. For these individuals this variable is zero.

[63]We merge "unemployed" with the three other categories to make this measure comparable across gender. While 86% of men who are not working or studying report being unemployed, for women this group represents only about 20%. In contrast, 70% declare to be a housewife - a category that exists for only 0.2% of all males in our sample.

[64]Our results show that young women of university age from the 1994 cohort are less likely to be employed for a wage. This result is not surprising, since women from this cohort are more likely to attend university.

policy for non-university educated, but we see that the positive impact on being out of work and out of study is driven by individuals with university education.[65]

When comparing only women across different cohorts and cities (Panel B), we note for women born in 1993 a positive and significant impact on the probability of being employed of 2.7 percentage points for women in restricted cities (col. 1). In our sample, the share of individuals studying at the time of the survey is about 27% for both genders; however, 42% of males are employed versus only 7% of females. Thus, an increase in the relative probability of being employed for women of 2.7 percentage points represents a large effect. A positive impact on employment is also found when splitting the sample between women with and without university education. The positive impact on employment for women of the 1993 cohort with university education could be due to the fact that these women might be more motivated or skilled due to higher competition to enter university. While Table D.14 shows that women who do not manage to enroll at university do not tend to enter directly the labor force at a higher rate, they appear more successful in the labor market at a later stage (col. 5 and 6 of 10).

In addition, we observe significant effects on labor market outcomes for the 1994 cohort. Col. 1 and 2 in Panel A suggest a negative impact on the gender gap in employment and a positive effect on the gender gap in being out of work and out of study. When splitting the sample by gender (col. 1 and 2 of Panel B (women) and C (men)), we note that this is mainly driven by men improving their labor market outcomes. Panel B also shows for women without university education (col. 5 and 6) positive effects on employment for a wage and negative effects on being out of work and out of study for all cohorts except for 1995; this might be suggestive of pre-existing labor market trends for these women, hence these results should be interpreted with caution.

Results Panel C are further suggestive of a positive impact on labor market outcomes for men of the 1993 and 1994 cohort in cities with restrictions for female students, with a positive effect on employment and a negative one on being out of work and out of study, driven by men with university education.

[65]As noted in footnote 58, we do not give any causal interpretation to the results splitting the sample between with and without university education. We present these results with the purpose of illustrating and disentangling the potential mechanisms.

Table 10: Labor market outcomes: Employment and participation, 24-30 years old

	(1)	(2)	(3)	(4)	(5)	(6)
	All		Univ.		No univ.	
Dependent variable:	Empl. for wage	No work or study	Empl. for wage	No work or study	Empl. for wage	No work or study
Panel A:	*All individuals - triple difference*					
$R_c^{2012} \times f_i \times Cohort_i = 1992$	-0.001	0.001	-0.008	0.010	0.032	-0.021
	(0.018)	(0.021)	(0.027)	(0.023)	(0.023)	(0.026)
$\mathbf{R_c^{2012} \times f_i \times Cohort_i = 1993}$	-0.018	0.037^c	0.002	0.071^b	-0.011	0.004
	(0.018)	(0.019)	(0.027)	(0.027)	(0.020)	(0.018)
$R_c^{2012} \times f_i \times Cohort_i = 1994$	-0.059^b	0.054^b	-0.061	0.043	-0.023	0.025
	(0.019)	(0.016)	(0.038)	(0.038)	(0.022)	(0.022)
$R_c^{2012} \times f_i \times Cohort_i = 1995$	-0.015	0.002	0.088	-0.140	-0.038	0.032
	(0.026)	(0.031)	(0.055)	(0.082)	(0.032)	(0.035)
	Fixed effects: F_{cf}, F_{ck}, F_{pkf}, F_{pkt} & F_{pft}					
Observations	47,789	47,789	19,961	19,961	27,269	27,269
R^2	0.402	0.357	0.393	0.346	0.554	0.540
Panel B:	*Women*					
$R_c^{2012} \times Cohort_i = 1992$	0.027^b	-0.041^b	0.023	-0.038	0.029^b	-0.021
	(0.011)	(0.016)	(0.021)	(0.026)	(0.012)	(0.015)
$\mathbf{R_c^{2012} \times Cohort_i = 1993}$	0.027^c	-0.019	0.045^c	-0.003	0.039^c	-0.055^b
	(0.012)	(0.018)	(0.019)	(0.028)	(0.021)	(0.023)
$R_c^{2012} \times Cohort_i = 1994$	-0.007	-0.021	-0.022	-0.028	0.036^b	-0.036^c
	(0.015)	(0.020)	(0.029)	(0.031)	(0.015)	(0.017)
$R_c^{2012} \times Cohort_i = 1995$	-0.019	-0.005	-0.004	-0.059	-0.062^b	0.062^b
	(0.021)	(0.026)	(0.035)	(0.053)	(0.021)	(0.024)
	Fixed effects: F_{pk} & F_{ct}					
Observations	22,783	22,783	9,613	9,613	12,057	12,057
R^2	0.158	0.167	0.246	0.255	0.240	0.236
Panel C:	*Men*					
$R_c^{2012} \times Cohort_i = 1992$	0.015	-0.025	0.024	-0.046	-0.006	-0.004
	(0.015)	(0.015)	(0.030)	(0.029)	(0.020)	(0.019)
$\mathbf{R_c^{2012} \times Cohort_i = 1993}$	0.029^c	-0.040^b	0.060^b	-0.094^a	0.009	-0.014
	(0.014)	(0.013)	(0.020)	(0.025)	(0.021)	(0.021)
$R_c^{2012} \times Cohort_i = 1994$	0.040^c	-0.059^b	0.058	-0.138^a	0.018	-0.023
	(0.017)	(0.017)	(0.042)	(0.039)	(0.016)	(0.017)
$R_c^{2012} \times Cohort_i = 1995$	0.036	-0.041	-0.046	0.030	0.059^b	-0.055^c
	(0.031)	(0.028)	(0.059)	(0.047)	(0.023)	(0.024)
	Fixed effects: F_{pk} & F_{ct}					
Observations	24,149	24,149	8,994	8,994	13,968	13,968
R^2	0.237	0.195	0.351	0.289	0.284	0.271

Notes: R denotes the restriction measure by city. c stands for city, k for birth cohort, i for individual, f for gender, p for province, and t refers to the year of the survey. *Sample:* HIES 2008-2021. Birth cohorts 1989-1995, individuals age 24 to 30 at time of interview. *Individual controls:* Urban residence and presence of another household member with education above lower middle school. *City controls:* Interactions of the cohort dummies with (i) the number of seats for women in city c in the academic year a, (ii) the share of local courses that have a gender quota in city c for the academic years 2010 to 2014, and (iii) the restriction variable for city c constructed (in the equivalent way as the restrictions for 2012) for the academic years 2011, 2013 and 2014. Panel A includes also the interactions of all control variables with gender. Regressions all include but don't show the interaction terms of local restrictions for the older cohorts. Standard errors clustered at cohort and city level in parentheses. ^{a}, ^{b} and ^{c} denote significance at the 1, 5, and 10 percent levels, respectively.

Choice of sector

We then turn to exploring whether the 2012 policy had an impact on the sectors in which individuals work. As our results suggest that the policy impacted labor force participation, we want to test whether the type of jobs women or men got was also affected. One concern is that women who did not enter university, but started to work, could have ended up in less well paid or less prestigious jobs. To understand in which sectors women were relatively more likely to work as a consequence of the policy, we rely on three different outcome variables. First, we use a dummy for working in agriculture. Agriculture is an important sector in Iran. In our final sample, about 12% of working women and 20% of working men are in agriculture. Agriculture is mainly physically demanding. Second, we create a "low-skill sectors" dummy that indicates whether an individual is working in a sector with predominantly low skilled workers. In the Iranian context this is agriculture (for both men and women), textiles for women and construction for men. The higher the level of education, the lower the probability of being active in these sectors. Our last variable, "Other sectors" equals unity if the individual works in any sector other than the low-skilled. It is zero if the individual does not work or works in a low-skill sector.

Table 11 presents the results. As before, we focus on the 24-30-year-old to ensure that we are mainly looking at individuals who have finished their education. When considering the triple difference estimates (Panel A), we see that in cities with stronger restrictions women born in 1993 have a relatively higher probability to work in sectors requiring lower skills (agriculture and textiles), compared to men of the same cohort (col. 4). This is driven by individuals without university education (col. 6). We do not see any robust differential effect on the probability of working in agricultural or in other sectors for the 1993 cohort.

When restricting the sample to women only (Panel B), besides the previous finding on low-skill sectors (col. 4–6), we also observe a positive impact of stronger restrictions on the probability of working in agricultural (col. 1). This is again driven by women without university education (col. 4), and it suggests that, even if the admissions restrictions had a positive impact on employment for women without university education, the types of jobs that the affected women were hired for are those at the bottom of the skills distribution.

Table 11: Labor market outcomes: sectors (24-30 years old)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dependent variable:	Agriculture			Low-skilled sectors			Other sectors		
	All	Univ.	No uni	All	Univ.	No uni	All	Univ.	No uni
Panel A:	*All individuals - triple difference*								
$R_c^{2012} \times f_i \times Cohort_i = 1992$	0.009	0.026^c	0.021	0.010	0.023	0.023	-0.002	-0.006	0.005
	(0.009)	(0.014)	(0.013)	(0.012)	(0.019)	(0.022)	(0.013)	(0.025)	(0.026)
$\mathbf{R_c^{2012} \times f_i \times Cohort_i = 1993}$	0.008	0.017	0.002	0.020^c	-0.001	0.045^b	-0.023	0.026	-0.049^c
	(0.008)	(0.013)	(0.012)	(0.010)	(0.015)	(0.017)	(0.014)	(0.026)	(0.022)
$R_c^{2012} \times f_i \times Cohort_i = 1994$	-0.025^b	0.037^c	-0.047^b	-0.020	0.041^b	-0.027	-0.016	-0.079^b	0.030
	(0.008)	(0.016)	(0.018)	(0.013)	(0.017)	(0.023)	(0.017)	(0.031)	(0.022)
$R_c^{2012} \times f_i \times Cohort_i = 1995$	-0.016	0.042^c	-0.036	0.015	0.109^a	-0.006	-0.004	-0.005	-0.010
	(0.012)	(0.020)	(0.021)	(0.018)	(0.030)	(0.024)	(0.023)	(0.046)	(0.031)
	Fixed effects: F_{cf}, F_{ck}, F_{pkf}, F_{pkt} & F_{pft}								
Observations	47,789	19,961	27,269	47,789	19,961	27,269	47,789	19,961	27,269
R^2	0.296	0.400	0.381	0.311	0.383	0.398	0.296	0.350	0.414
Panel B:	*Women*								
$R_c^{2012} \times Cohort_i = 1992$	0.005	0.003	0.003	0.011	0.001	0.022^c	0.014	0.027	0.001
	(0.005)	(0.006)	(0.011)	(0.006)	(0.007)	(0.011)	(0.010)	(0.021)	(0.007)
$\mathbf{R_c^{2012} \times Cohort_i = 1993}$	0.015^b	0.004	0.030^c	0.018^b	0.003	0.037^b	0.012	0.039^c	0.001
	(0.005)	(0.003)	(0.013)	(0.006)	(0.005)	(0.013)	(0.011)	(0.020)	(0.015)
$R_c^{2012} \times Cohort_i = 1994$	0.002	0.006	-0.002	0.003	0.004	0.008	-0.013	-0.034	0.027^c
	(0.004)	(0.004)	(0.008)	(0.005)	(0.004)	(0.009)	(0.013)	(0.026)	(0.014)
$R_c^{2012} \times Cohort_i = 1995$	-0.014	0.000	-0.025	-0.014	-0.001	-0.021	-0.001	0.006	-0.039^c
	(0.009)	(0.004)	(0.015)	(0.009)	(0.007)	(0.017)	(0.020)	(0.035)	(0.017)
	Fixed effects: F_{pk} & F_{ct}								
Observations	22,783	9,613	12,057	22,783	9,613	12,057	22,783	9,613	12,057
R^2	0.223	0.292	0.305	0.198	0.269	0.266	0.155	0.248	0.220
Panel C:	*Men*								
$R_c^{2012} \times Cohort_i = 1992$	0.004	0.002	0.008	-0.009	-0.011	-0.011	0.016	0.024	0.000
	(0.011)	(0.023)	(0.013)	(0.017)	(0.027)	(0.024)	(0.017)	(0.035)	(0.024)
$\mathbf{R_c^{2012} \times Cohort_i = 1993}$	0.003	0.001	0.015	-0.027	0.026	-0.048^c	0.045^b	0.028	0.044^c
	(0.014)	(0.018)	(0.018)	(0.016)	(0.019)	(0.021)	(0.017)	(0.029)	(0.021)
$R_c^{2012} \times Cohort_i = 1994$	0.007	-0.013	0.003	-0.009	0.006	-0.020	0.018	0.016	0.011
	(0.014)	(0.026)	(0.019)	(0.017)	(0.024)	(0.021)	(0.018)	(0.052)	(0.023)
$R_c^{2012} \times Cohort_i = 1995$	0.015	-0.015	0.027	-0.011	-0.060	-0.000	0.005	-0.033	0.017
	(0.015)	(0.033)	(0.022)	(0.019)	(0.036)	(0.027)	(0.028)	(0.043)	(0.035)
	Fixed effects: F_{pk} & F_{ct}								
Observations	24,149	8,994	13,968	24,149	8,994	13,968	24,149	8,994	13,968
R^2	0.302	0.392	0.363	0.272	0.363	0.332	0.240	0.341	0.296

Notes: R denotes the restriction measure by city. c stands for city, k for birth cohort, i for individual, f for gender, p for province, and t refers to the year of the survey. *Sample:* HIES 2008-2021. Birth cohorts 1989-1995, individuals age 24 to 30 at time of interview. *Individual controls:* Urban residence and presence of another household member with education above lower middle school. *City controls:* Interactions of the cohort dummies with (i) the number of seats for women in city c in the academic year a, (ii) the share of local courses that have a gender quota in city c for the academic years 2010 to 2014, and (iii) the restriction variable for city c constructed (in the equivalent way as the restrictions for 2012) for the academic years 2011, 2013 and 2014. Panel A includes also the interaction of all control variables with gender. Regressions all include but don't show the interaction terms of local restrictions for the older cohorts. Standard errors clustered at cohort and city level in parentheses. ^{a}, ^{b} and ^{c} denote significance at the 1, 5, and 10 percent levels, respectively.

Women of the 1993 cohort with university education in restricted cities are instead more likely to work in other sectors, which require more skills (col. 8). However, we do not find any support that women with university education of the younger 1994 cohort benefited from their higher education in terms of labor market outcomes: panel A shows that these women are relatively more likely to work in agriculture (col. 2) or low skill sectors (col. 5) and less likely to work in jobs that require skills (col. 8), compared to their male counterparts.

As noted above, the policy appears to have increased employment for men of the 1993 and 1994 cohort. Contrary to what we observe for women, for men of the 1993 we see a decrease in the probability of working in low-skill sectors (col. 4–6 of Panel C) and increased the probability of working in other sectors (col. 7–9 of Panel C), particularly for men without university education. These results are consistent with some degree of substitution between men and women in the labor market: the 2012 policy increased the chances that women are employed in low-skill jobs, while men moved to high-skill jobs. However, full substitution between men and women is unlikely, as the labor market in Iran shows high levels of gender segregation.

Income

Finally, to complete the labor market results, Table D.15 shows the impact of the policy on work income, conditional on working, for individuals aged 19 to 30. Given the low number of individuals that report a personal income, we include here also individuals younger then 24.[66] We consider two outcome variables: total income and income from the main job. Both variables are transformed to logarithms. The triple difference results in Panel A do not show any significant impact of the 2012 policy on the gender gap in income for the 1993 cohort.

When splitting the sample by gender, we find opposing coefficients on income for women with and without university education for the 1993 cohort, which are in line with our findings by sector: women without university education who are found to engage more often in low-skilled activities seem to also have comparably lower wages, while women with university education are more likely to work in a more high-skilled job and experience a positive income effect.

For men, the estimates are much closer to zero. However, there seems to be a negative effect for men without university education of older generations who are likely to enter

[66] Due to the low number of observations, we have to rely on a strongly reduced set of fixed effects.

the labor market in the same time as the women who were displaced from university due to the 2012 policy.

It is important to stress here that the low number of women reporting income makes it difficult to obtain very reliable estimates; thus, we interpret these results on income as merely suggestive.[67]

From our results on the labor market outcomes, we can conclude that women who were displaced from university have a higher chance of ending up in low skilled sectors, which are also expected to pay a lower wage. The income results also indicate that these women did not experience any gains overall in terms of income. The higher labor force participation of women of this cohort is thus unlikely to translate into long-term gains for the women affected by the 2012 education policy.

7 Conclusion

This paper first documents and then investigates the effects of an education policy in Iran that was implemented in 2012. The policy restricted women's admissions to Bachelor programs at several public universities in prestigious fields such as engineering. For identification, we exploit the differential impact of the policy across cohorts, gender and cities.

Our findings suggest that this policy had a negative effect on university attendance for women of the most affected cohort (born in 1993) in restricted cities. This effect is particularly strong for women from urban households, poorer families and more conservative provinces. We instead find some positive effects on university education for the subsequent cohort, born in 1994. Next, we show that the effect of the restrictions to higher education for women extends to the marriage and labor markets: women born in 1993 are less likely to be married at a young age and more likely to be employed for a wage. Furthermore, the policy had opposing effects for these later-life outcomes for women who

[67] We also observe a positive impact on income for women from the 1995 cohort, driven by women without university education. However, this result is based on a very small number of observations: individuals from this cohort are only 26 years old in 2021, the last year of our data. Thus, only a very small number of women from this cohort report any income.

did join university and those who did not.

Our results highlight that women tend to make joint decisions in the labor market and marriage market. The 2012 policy encouraged labor market participation and improved the type of job and income for women of the 1993 cohort who still managed to enter university, while it had a clear negative impact on their probability to marry. In sharp contrast, in the following cohort women with a university degree appear to do particularly well in the marriage market and have spouses with higher levels of education, while there is no clear benefit for them in the labor market. This underscores that decisions in the labor and marriage markets have reciprocal influences. Furthermore, the positive impact on match quality for women of the 1994 cohort suggests the presence of a general equilibrium effect of the policy: some of the men who would have married women from the 1993 cohort in absence of the policy are instead marrying younger women from the 1994 cohort, who are more likely to attend university. We also observe a general equilibrium effect of this policy, which mainly targeted women studying STEM, on females in low skilled segments of the local labor market, as well as on men.

Based on our findings, we obtain three insights. First, women who were most affected by this policy tend to behave less traditionally as compared to women of other cohorts. Second, restricting female access to STEM fields implies that women with university education experience positive labor market effects. Thus, as positive affirmative action policies may lead to undesirable labor market outcomes (e.g., Bagde et al., 2016), our results indicate that negative affirmative action may lead to mirror image outcomes. Third, labor market outcomes for men improve only in terms of finding a job, but not in terms of wages.

An interesting avenue for future research is to explore the long term effects of university education on labor and marriage markets. Once additional survey years are available, it would be interesting to study if the impact of the short-term policy, effectively in place only in 2012, is mitigated due to individual adjustments in later life. Furthermore, it would be possible to study the impact on younger generations who could have more easily adjusted their *konkur* choice and thus their field of study in response to the 2012 policy. This choice in turn is expected to affect also their labor and marriage market outcomes.

This is especially interesting in a labor market, where women face stark discrimination and are often limited to specific occupations.

Finally, our paper highlight the importance to study restrictions to university. In the case of Iran, as recently as summer 2023, there have again been university programs cut on very short notice. This time the affected programs were mostly in the field of arts, which have a majority of female students (BBC, 2023).

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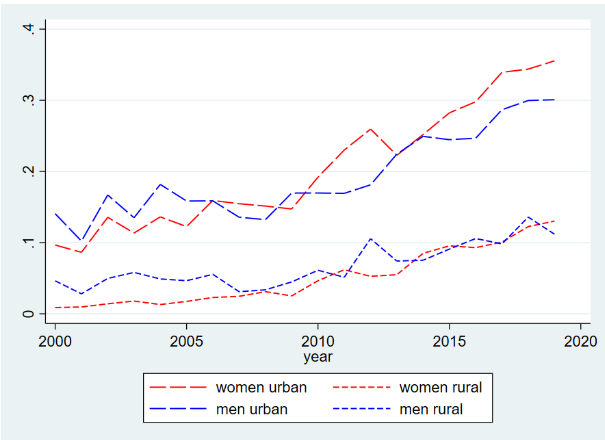
A Higher education and labor market trends in Iran

Over the last two decades, Iran has seen a large increase in the share of students in higher education, particularly for women, and men and women are now very similar in terms of educational attainment. The largest differences across individuals in terms of education are due to regional heterogeneity.

Figure A.1 shows the development of individuals with university education, by gender and location of the household (rural versus urban). Individuals from rural areas are much less likely to receive higher education compared to individuals from urban areas. In the past decade, urban women have surpassed urban men in terms of share of individuals with university education. We also observe large differences in university education by region.

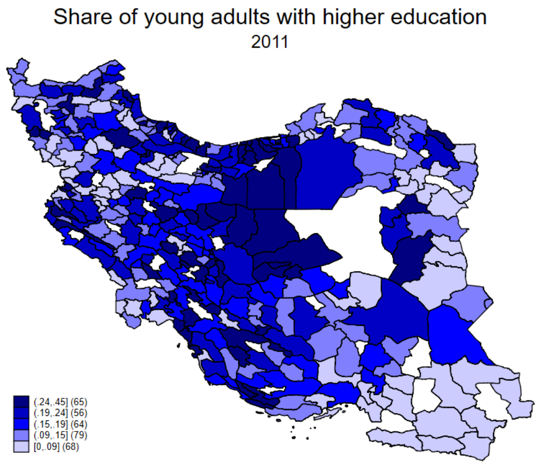
Figure A.1: Share of individuals with university education by gender and location



Notes: This figure shows the evolution of the share of individuals with some university education at age 30, by gender and by urban-rural location of the household. Data from the Household Income and Expenditure Survey.

Figure A.2 shows the share of individuals between 19 and 30 who have ever attended university by city (*shahrestan*) for the year 2011. Predominantly rural regions, such as

Figure A.2: Regional variation in higher education



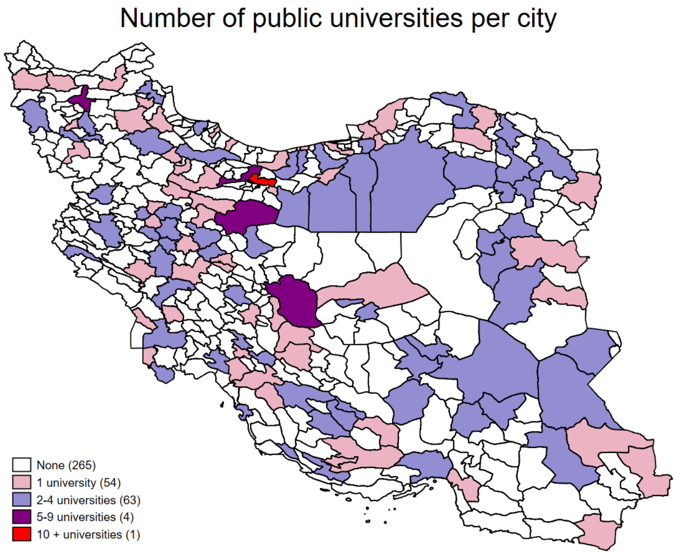
Notes: Data from the Household Income and Expendutures Survey.

the South-East of the country, have a low share of individuals with university education. The regional variation in university education may reflect differences in access to high-quality higher education. Figure A.3 depicts the number of public universities in each city in 2012. There is a low prevalence of universities in the East and South-East, with many cities without any public university. A large number of universities are concentrated in the Center-East of the country. There is only one city (Tehran) with more than 10 public universities, while most cities have between one and two.

Remarkably, the recent gains for women in terms of higher education are not reflected in labor market outcomes. Figure A.4 shows the differences in labor force participation by gender, again separately for urban and rural. There is a sharp differences between men and women, but little heterogeneity when considering the differences between rural and urban areas. Whereas nearly all men at age 30 are part of the labor force, only about 20% of urban and rural women of the same age are working or searching for a job. The share of women entering the labor force has been roughly constant in the past two decades, despite the increase in higher education attendance. Figure A.5 highlights a clear link between university education and probability of being employed: even though the importance of a university degree seemed to have decreased recently for both genders, women with higher education are significantly more likely to be employed than women without.[68] Furthermore, Figure A.6 shows the average wages by education level and

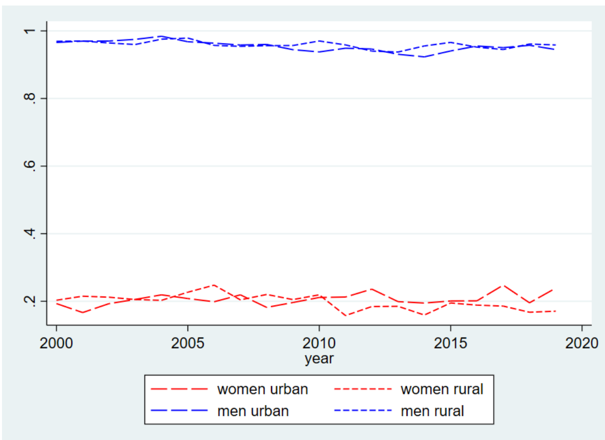
[68]The economic situation in Iran had worsened substantially since 2005, due to international economic sanctions. This has led to an overall increase in youth unemployment.

Figure A.3: Spatial distributions of public universities



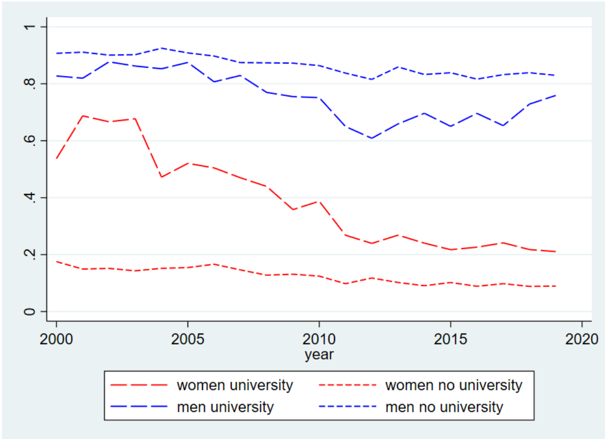
Notes: Data from the University course books published by the Iranian Ministry of Education (2012).

Figure A.4: Differences in labor force participation by gender and region (at age 30)



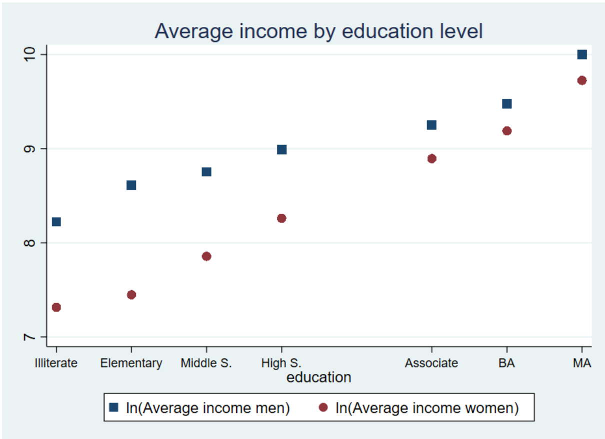
Notes: Data from the Household Income and Expenditure Survey.

Figure A.5: Differences in employment by gender and education (at age 30)



Notes: Data from the Household Income and Expenditure Survey.

Figure A.6: Returns to education by gender



Notes: This figure shows the average income at different levels of education by gender (transformed in ln). Data from the Household Income and Expenditure Survey (2000 - 2019). The sample contains around 623,000 individuals of at least 16 years old that declare a positive work income. 11% are women.

gender for individuals that report income from work. While the figure illustrates very clearly that women earn less compared to men for all levels of education, it also shows that for women the returns to education are higher. On average, a women with a high school degree earns as much as an illiterate man. Only for post-secondary education the gender wage gap decreases significantly. Overall, this indicates that restrictions to university education are likely to be harmful for women in terms of labor market outcomes.

B Individual data

B.1 Household Income and Expenditure survey: identifying individuals

The Household Income and Expenditure Survey includes households in urban and rural areas. In each sampling period, it applies a three-stage clustering based on the latest available population census. Since 2010 the sample is designed with a rotating panel feature, in the sense that households are resampled up to three consecutive years. The second rotation has been started in 2012 and then a new one in 2018.

Tracking individuals over time is not always possible. First, there is attrition of households (with replacement) and individuals (no replacement), which implies that many individuals appear less than three times in the survey. Second, with the new sampling periods starting in 2012 and 2018, no households from the respective previous year were included in the new sample. Third, the survey does not include personal identifiers, but for each survey wave household members are numbered consecutively. While member 1 is the household head, there is no clear rule for the other household members.[69] As a consequence, children of the same household regularly exchange the member number across different survey waves. Also, if a household member leaves or a new member enters, this leads to different members having he same identifier across waves. Based on household identifier, birth year, position within the household (child, spouse etc.) and gender we are however able to construct a personal identifier for all observations.

Identifying married couples In Section 6.4.1 we look at the characteristics of husbands (notably age and education). For this, we have to construct a sample of married couples. The HIES only asks the position of the individual in the household (household head, spouse, child, child in law etc) and whether an individual is married or not. It doesn't explicitly record who is married to whom. We can clearly identify couples when the husband is the household head and another women in the same household reports

[69]All individuals are classified according to their relation to the household head. The other individuals are classified as either spouse of the household head, child, bride or groom, grand-child, sibling, parent, parent-in-law or other. The household head is nearly always a male, except for when there is no other male person above the age of 18 in the household.

being the spouse of the household head. We can also identify couples if in the household there is only one married child and one child in law of the opposite sex. As soon as there is more then one married child or child in law, we cannot know who is married to whom and we thus cannot include these individuals in the analysis on the quality of the match. There are also instances when there is no spouse recorded to live at the same address at the time of the interview. Given these restrictions in the set-up of the HIES, for close to 20% of the married women in our sample we cannot identify the husband. These women are therefore not included in our analysis on match quality.

B.2 Labor force survey

Part of our results are obtained from data from the 2013–2017 waves of the Labor Force Survey (LFS), also conducted by the Statistical Center of Iran. This survey aims at documenting the composition and the current condition of the labor force. We rely on this data in Section B.3 to investigate whether students move in response to the 2012 policy. Most importantly for our purpose, the LFS contains information about the field of study of individuals. We exploit this information in Section 6.3.

While the LFS has more observations each year (about 600,000) as compared to the HIES, the sample has a few drawbacks. First, it uses a rotating panel design that interviews households up to four times, with generally two interviews in the same year, which reduces the number of individuals available in each year substantially. Second, it does not have information on the city of the households. Nevertheless, for the survey rounds 2013-2017, we can rely on the area codes of the phone numbers to attribute a city to all individuals within the same geographical cluster. However, this method is prone to error, especially in smaller or more rural areas and forces us to drop a small number of survey clusters for which identification was not clear.

B.3 Movers and self-selection

One concern with evaluating local restrictions is that people might migrate in response to the policy and that self-selection of individuals might then lead to an upward bias of our estimates. In our setting, this would be the case if in response to the policy women moved in large numbers from restricted to non-restricted cities to study there. We would then observe fewer women without a Bachelor degree in restricted areas and more in non-restricted areas. We argue that, while this could in principle be the case, in this specific setting, we do not expect self-selection to be a major source of bias for our estimates.

First, in Iran, students mostly live with their parents. In the Labor Force Survey, at least 93% of the students of our cohorts of interest (born between 1991-1995) live with their parents. Second, the Labor Force Survey asks whether a household has moved and the potential reason for it. Only 0.03% declare that they have moved for studying.[70]

It is more common that household members are currently absent (1,8% of individuals in our sample). About 85% of these are children of the household head and about 70% of these absent individuals are currently studying. This could indicate that they moved to a different city for their studies. They are in this case still associated with the location of the parental household. They should thus not bias our results.

More of a concern are households were children leave for their studies and are not related anymore to their household. For instance, students may move and stay with extended family in other cities. We can test whether we observe in our data many cases where extended family or non-family members are living in the household. We find that these households are very few with little variation over the years. When excluding these individuals, results remain similar. Another option is that children live in dormitories and are not related anymore to their parental household. This is especially likely for rural households. Given that our results are mainly driven by urban areas, where students are more likely to live at home, this is unlikely to lead to an upward bias in our case.[71]

Finally, we analyze the student statistics data to check if there is an upward kink in total number of students in unrestricted cities. This is not the case. It is actually the cities that also imposed restrictions that see an increase in the total number of students.[72] Nevertheless, students could move to non-restricted areas if there were more seats in

[70] The most common reason for moving of individuals below age 30 is following the household.

[71] Studying abroad is also an option. But this option is accessible only for a very small minority of individuals from very affluent households. Moreover, sending children abroad needs longer preparation and is unlikely to be a direct reaction to the restrictions in public universities in 2012.

[72] This increase in student numbers for restricted cities is mainly driven by Tehran.

private universities. There is however no indication that private universities had put in place similar restrictions as public ones. If anything, private universities had an interest to be more lenient in cities that had stronger restrictions in public universities to attract more of (good) female students.

We thus conclude that, while there remains a possibility that some students move in response to the policy and are not recorded, in our data there is no indication that this took place at a substantial scale and that it could invalidate our findings.

C Robustness Checks

This section provides numerous robustness checks on the choice of controls, the sample and alternative measures of city-level restrictions.

C.0.1 Additional controls and choice of sample

Table C.1 reports estimates using different sets of controls. A potential concern with the results in the previous section is that our province-cohort-gender fixed effects may be insufficient to control for local political trends. Thus, in col. 1 we add the share of votes for conservative candidates in the 2009 presidential elections by city, also interacted with gender and cohort dummies. Our coefficient of interest is not affected by the addition of these controls. Col. 2 estimates our benchmark specification including additional individual controls (household size, household income quantile and the level of education of household head, including their interactions with gender). Col. 3 uses the much more demanding city-year fixed effects instead of province-year-cohort and province-year-gender fixed effects, while col. 4 adds the city-year fixed effects in addition to those. Col. 5 interacts the two sets of province-year fixed effects with an urban dummy to allow for different urban vs. rural trends in higher education within a province. Col. 6 adds to the specification of col. 5 city-year fixed effects. In all cases our results remain similar, even with the most demanding set of controls and fixed effects. As we have 401 cities and data from 13 years, city-year fixed effects add a substantial amount of parameters to be estimated. As results are not sensitive to the inclusion of these fixed effects, we proceed

Table C.1: Robustness checks: Additional controls and fixed effects

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:	Ever enrolled in a Bachelor program					
	Vote share	Ind. controls	City-year FE		Urban-year FE	
$R_c^{2012} \times f_i \times Cohort_i = 1992$	0.024	0.019	0.020	0.023	0.016	0.017
	(0.019)	(0.019)	(0.022)	(0.021)	(0.018)	(0.020)
$\mathbf{R_c^{2012} \times f_i \times Cohort_i = 1993}$	-0.033^b	-0.037^a	-0.034^b	-0.029^c	-0.038^b	-0.035^b
	(0.010)	(0.010)	(0.013)	(0.013)	(0.011)	(0.014)
$R_c^{2012} \times f_i \times Cohort_i = 1994$	0.035^b	0.031^b	0.022	0.031^c	0.034^b	0.027
	(0.013)	(0.012)	(0.014)	(0.015)	(0.014)	(0.016)
$R_c^{2012} \times f_i \times Cohort_i = 1995$	0.016	0.015	0.020	0.021	-0.001	0.002
	(0.025)	(0.025)	(0.029)	(0.029)	(0.022)	(0.027)
$Conserv_c^{2009} \times f_i \times Cohort_i = 1993$	-0.115^b					
	(0.046)					
Controls	Individual and city controls (X_{ift} & Z_{cfk})					
Addintional ind. controls (X_{ift})		Yes				
F_{cf}, F_{pkf} & F_{kc}	Yes	Yes	Yes	Yes	Yes	Yes
F_{pkt} & F_{pft}	Yes	Yes		Yes		
F_{upkt} & F_{upft}					Yes	Yes
City-year fixed effects (F_{ct})			Yes	Yes		Yes
Observations	93,700	93,696	93,545	93,545	93,691	93,535
R^2	0.149	0.170	0.168	0.194	0.177	0.222
No. of Cities	401	401	401	401	401	401

Notes: R denotes the restriction measure by city. c stands for city, k for birth cohort, i for individual, f for gender, p for province, and t refers to the year of the survey. *Sample:* HIES 2008-2021. Birth cohorts 1989-1995, individuals age 19 to 28 at time of interview. *Individual controls:* Urban residence and presence of another household member with education above lower middle school and their interactions with gender. *City controls*: Interactions of the cohort dummies and gender with i. the number of seats for women in city c in the academic year a, ii. the share of local courses that have a gender quota in city c for the academic years 2010 to 2014, and iii. the restriction variable for city c constructed (in the equivalent way as the restrictions for 2012) for the academic years 2011, 2013 and 2014. *Additional controls:* Col 1 includes the interaction of the city's share of conservative candidates in the 2009 presidential election with cohort dummies and gender. Col. 2 includes household income quantile, household size and education level of the household head and their interactions with gender. Regressions all include but don't show the interaction terms of local restrictions for the older cohorts. Standard errors clustered at cohort and city level in parentheses. ^{a}, ^{b} and ^{c} denote significance at the 1, 5, and 10 percent levels, respectively.

with the set of fixed effects of our benchmark specification of Equation 1.

Table C.2 reports robustness checks on the sample. Tehran is Iran's economic and political center and is an outlier in the number of universities and students. In col. 1, we exclude Tehran province and show that our results are not driven by the capital.

Our restrictions measure is based on the restrictions in the closest university city. However, the closest university city may not offer any programs to women in a given year. If in the following year there is a new university that opens courses for women, or the previously male-only university opens to females, the treatment intensity of this city is set to -2 (strong increase in the share of programs for women). However, given that

Table C.2: Robustness of sample: The impact of local restrictions on enrollment in university

	(1)	(2)	(3)	(4)	(5)
Dependent variable:	Ever enrolled in a Bachelor program				
	Excluding Tehran	Excl. cities new openings	Include small cities	Age 22-28 at interview	Birth cohorts 1991-1995
$R_c^{2012} \times f_i \times Cohort_i = 1992$	0.026	0.030	0.022	0.026	0.027
	(0.019)	(0.023)	(0.019)	(0.026)	(0.019)
$\mathbf{R_c^{2012} \times f_i \times Cohort_i = 1993}$	-0.028[b]	-0.047[a]	-0.031[b]	-0.029[c]	-0.030[b]
	(0.009)	(0.011)	(0.010)	(0.014)	(0.010)
$R_c^{2012} \times f_i \times Cohort_i = 1994$	0.039[b]	0.025	0.036[b]	0.031	0.037[b]
	(0.011)	(0.016)	(0.012)	(0.018)	(0.013)
$R_c^{2012} \times f_i \times Cohort_i = 1995$	0.021	0.016	0.017	0.055[c]	0.017
	(0.024)	(0.030)	(0.024)	(0.028)	(0.024)
Controls	Individual and city controls (X_{ift} & Z_{cfk})				
Fixed effects	F_{cf}, F_{ck}, F_{pkf}, F_{pkt} & F_{pft}				
Observations	88,032	85,620	94,118	59,361	52,150
R^2	0.148	0.149	0.150	0.155	0.161
No. of Cities	386	358	423	401	401

Notes: R denotes the restriction measure by city. c stands for city, k for birth cohort, i for individual, f for gender, p for province, and t refers to the year of the survey. *Sample*: HIES 2008-2021. Col 1 to 4: Birth cohorts 1989-1995. Col 5: Birth cohorts 1991-1995. Col 1-3 and 5: Individuals age 19 to 28 at time of interview. Col 4: Individuals age 22 to 28 at time of interview. *Individual controls:* Urban residence and presence of another household member with education above lower middle school and their interactions with gender. *City controls*: Interactions of the cohort dummies and gender with i. the number of seats for women in city c in the academic year a, ii. the share of local courses that have a gender quota in city c for the academic years 2010 to 2014, and iii. the restriction variable for city c constructed (in the equivalent way as the restrictions for 2012) for the academic years 2011, 2013 and 2014. Regressions all include but don't show the interaction terms of local restrictions for the older cohorts. Standard errors clustered at cohort and city level in parentheses. [a], [b] and [c] denote significance at the 1, 5, and 10 percent levels, respectively.

studying in that city was previously not an option for women, their reference city was likely a different one than for men. In col. 2 we thus drop all cities for which the closest university city was previously male-only and starts offering programs for women during our sample period. The result of a negative impact on the gender gap in education for the 1993 cohort holds.

Col. 3 shows that the coefficients remain virtually unchanged when we include all cities (our main sample excludes cities with fewer than 50 interviewed persons in the relevant age categories). Col. 4 reduces the age bracket of the included individuals to 22 to 28-year-olds, excluding the younger ones which have a higher chance to still enroll in university after the interview for the survey. Col. 5 relies on a reduced set of birth cohorts, excluding all individuals born before 1991. The results hold with these sample restrictions, and the main coefficient of interest is similar to our benchmark result.

Table C.3: Alternative indicators for local restrictions

	(1)	(2)	(3)	(4)	(5)
Dependent variable:	Ever enrolled in a Bachelor program				
Indicator	Dummy indicator	Continuous variable	Exclude small programs	Programs within 80 km	60 km
$R_c^{2012} \times f_i \times Cohort_i = 1992$	0.025	-0.058	0.025	0.021	0.043^c
	(0.031)	(0.122)	(0.019)	(0.016)	(0.019)
$\mathbf{R_c^{2012} \times f_i \times Cohort_i = 1993}$	-0.056^b	-0.371^a	-0.031^b	-0.027^c	-0.053^b
	(0.018)	(0.072)	(0.011)	(0.013)	(0.017)
$R_c^{2012} \times f_i \times Cohort_i = 1994$	0.043^c	0.096	0.036^b	-0.016	-0.039^b
	(0.022)	(0.085)	(0.013)	(0.016)	(0.016)
$R_c^{2012} \times f_i \times Cohort_i = 1995$	0.040	-0.298^c	0.021	0.053^b	0.065^b
	(0.038)	(0.141)	(0.025)	(0.019)	(0.023)
Controls	Individual and city controls (X_{ift} & Z_{cfk})				
Fixed effects	F_{cf}, F_{ck}, F_{pkf}, F_{pkt} & F_{pft}				
Observations	93,700	81,258	93,700	93,700	93,700
R^2	0.148	0.150	0.148	0.148	0.148
No. of Cities	401	337	401	401	401

Notes: R denotes the restriction measure by city. c stands for city, k for birth cohort, i for individual, f for gender, p for province, and t refers to the year of the survey. *Sample:* HIES 2008-2021. Birth cohorts 1989-1995, individuals age 19 to 28 at time of interview. *Individual controls:* Urban residence and presence of another household member with education above lower middle school and their interactions with gender. *City controls*: Interactions of the cohort dummies and gender with i. the number of seats for women in city c in the academic year a, ii. the share of local courses that have a gender quota in city c for the academic years 2010 to 2014, and iii. the restriction variable for city c constructed (in the equivalent way as the restrictions for 2012) for the academic years 2011, 2013 and 2014. Regressions all include but don't show the interaction terms of local restrictions for the older cohorts. Standard errors clustered at cohort and city level in parentheses. ^{a}, ^{b} and ^{c} denote significance at the 1, 5, and 10 percent levels, respectively.

C.0.2 Alternative local restrictions measures

Table C.3 presents robustness checks using different measures for restrictions at the city level. In col. 1 we replace our main measure by a simpler one. The simplified measure equals unity if the share of programs open to women in the closest university city had decreased compared to the previous year, -1 if the share had increased and zero otherwise. In col. 2 we use instead the continuous variable of the percentage changes that underlies our main local restrictions measure.[73] In col. 3 we use again our variable with five different levels of the treatment intensity, but exclude all programs with less than three students. This affects only few cities and the treatment variable is highly correlated with our main measure. In col. 4 and 5 we use restrictions measures which are not based on the closest city, but on all programs within a 80 km or 60 km radius, respectively. Similar to our

[73]To avoid outliers, we restrict our sample to cities for which the change in the share of programs open to women in 2012 is within the +/- 100% range and thus drop cities which started or stopped offering programs for women.

Table C.4: Alternative indicators for local restrictions - Changes in shares of seats open to women

	(1)	(2)	(3)	(4)
Dependent variable:	Ever enrolled in a Bachelor program			
Indicator	Seats in closest uni. city			
$R_c^{2012} \times f_i \times Cohort_i = 1992$	0.001	0.001	0.003	-0.003
	(0.005)	(0.007)	(0.008)	(0.009)
$\mathbf{R_c^{2012} \times f_i \times Cohort_i = 1993}$	-0.011^a	-0.022^a	-0.022^a	-0.017^c
	(0.003)	(0.006)	(0.006)	(0.007)
$R_c^{2012} \times f_i \times Cohort_i = 1994$	0.006	0.005	0.005	0.001
	(0.004)	(0.005)	(0.006)	(0.008)
$R_c^{2012} \times f_i \times Cohort_i = 1995$	0.001	-0.002	-0.001	-0.003
	(0.004)	(0.008)	(0.010)	(0.012)
Individual controls (X_{ift})	Yes	Yes	Yes	Yes
$\sum_k Seg._c^a \times f_i \times cohort_k$		Yes	Yes	Yes
$\sum_k R_c^a \times f_i \times cohort_k$		Yes	Yes	Yes
$\sum_k Coursesf._c^a \times f_i \times cohort_k$			Yes	Yes
$\sum_k Seatsf._c^a \times f_i \times cohort_k$				Yes
Fixed effects	F_{cf}, F_{pkf}, F_{kc}, F_{pkt} & F_{pft}			
Observations	93,700	93,700	93,700	93,700
R^2	0.147	0.148	0.148	0.148
No. of Cities	401	401	401	401

Notes: R denotes the restriction measure by city. c stands for city, k for birth cohort, i for individual, f for gender, p for province, and t refers to the year of the survey. *Sample:* HIES 2008-2021. Birth cohorts 1989-1995, individuals age 19 to 28 at time of interview. *Individual controls:* Urban residence and presence of another household member with education above lower middle school and their interactions with gender. *City controls*: Interactions of the cohort dummies and gender with i. the number of seats for women in city c in the academic year a, ii. the share of local courses that have a gender quota in city c for the academic years 2010 to 2014, and iii. the restriction variable for city c constructed (in the equivalent way as the restrictions for 2012) for the academic years 2011, 2013 and 2014. Regressions all include but don't show the interaction terms of local restrictions for the older cohorts. Standard errors clustered at cohort and city level in parentheses. ^{a}, ^{b} and ^{c} denote significance at the 1, 5, and 10 percent levels, respectively.

main specification, the coefficient for the 2012 restrictions for the 1993 cohort is negative and significant. In contrast, the effects found for younger cohorts are not robust.

In Table C.4 we show estimates relying a restrictions measure constructed similarly to our main measure, but based on the share of *seats* available for women at public universities, instead of the share of programs. We show the results first without city-cohort-gender specific controls and then add these controls progressively. The gender gap in education increases for the 1993 cohort also when relying on a restrictions measure based on the number of seats.

C.0.3 Restrictions by field

We next explore whether the restrictions in the field of mathematics are driving our results. Thus, in table C.5 the aggregate city-level restrictions measure R is replaced by four separate city-level measures: *R Maths*, *R Hum*, *R Sci*, *R Common*. The first three measure the restrictions in those fields that can be studied with the *konkur* tracks mathematics, humanities, and sciences, respectively. The last one measures the restrictions in those fields that can be studied with all three *konkur* tracks. As not all university cities offer programs for all *konkur* tracks, we construct our restrictions measures based on the programs for each *konkur* track within the 80 km (col. 1) or the 60 km radius (col. 2).[74] While the restrictions have been strongest in the mathematics track, many universities have also implemented restrictions per gender in the other fields (see Table D.9).

The observed gender gap in higher education for the 1993 birth cohort appears to be driven by restrictions in the mathematics track, whereas we do not observe any consistently significant impact for the restrictions in the other tracks. This indicates that it is indeed the decrease in available programs for the mathematics track that explains the relative decrease in the number of female students.

[74] If we relied here on the shortest distance city that offers programs for the given *konkur* track, we would have different reference cities across the *konkur* tracks.

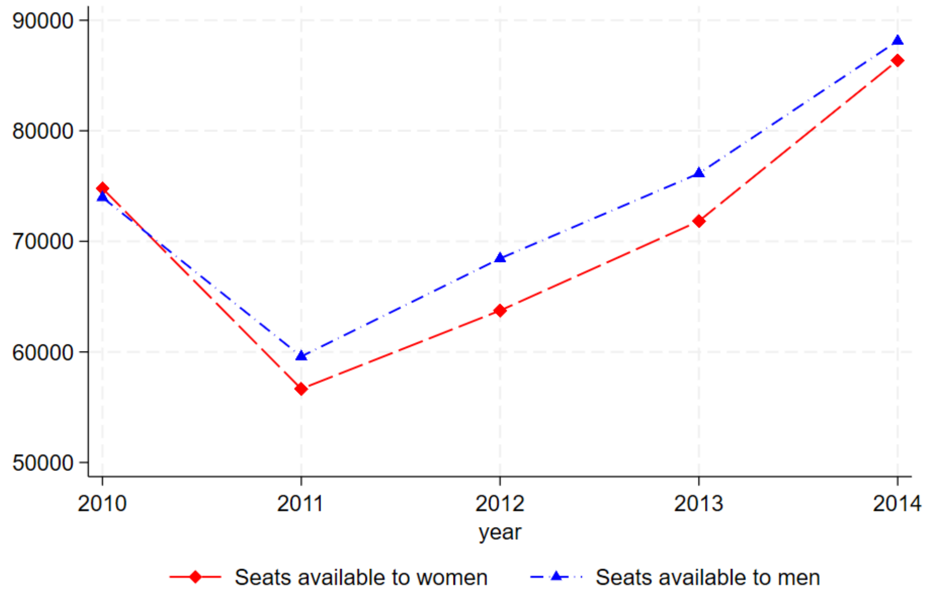
Table C.5: Restrictions by field of study

	(1)	(2)
Dependent variable:	Ever enrolled in a Bachelor program	
	Programs within 80 km radius	Programs within 60 km radius
$R\ Maths_c^{2012} \times f_i \times Cohort_i = 1992$	0.015	0.024
	(0.014)	(0.015)
$\mathbf{R\ Maths_c^{2012} \times f_i \times Cohort_i = 1993}$	-0.026^c	-0.033^c
	(0.014)	(0.016)
$R\ Maths_c^{2012} \times f_i \times Cohort_i = 1994$	-0.028	-0.009
	(0.017)	(0.013)
$R\ Maths_c^{2012} \times f_i \times Cohort_i = 1995$	-0.017	0.022
	(0.013)	(0.016)
$R\ Common_c^{2012} \times f_i \times Cohort_i = 1992$	-0.004	0.024
	(0.013)	(0.013)
$\mathbf{R\ Common_c^{2012} \times f_i \times Cohort_i = 1993}$	-0.014	-0.003
	(0.015)	(0.020)
$R\ Common_c^{2012} \times f_i \times Cohort_i = 1994$	0.004	-0.017
	(0.013)	(0.016)
$R\ Common_c^{2012} \times f_i \times Cohort_i = 1995$	0.032	0.041^c
	(0.018)	(0.020)
$R\ Sci_c^{2012} \times f_i \times Cohort_i = 1992$	0.042^c	0.035
	(0.021)	(0.020)
$\mathbf{R\ Sci_c^{2012} \times f_i \times Cohort_i = 1993}$	0.046^b	0.015
	(0.018)	(0.018)
$R\ Sci_c^{2012} \times f_i \times Cohort_i = 1994$	0.008	0.027
	(0.024)	(0.020)
$R\ Sci_c^{2012} \times f_i \times Cohort_i = 1995$	0.028	0.005
	(0.027)	(0.028)
$R\ Hum_c^{2012} \times f_i \times Cohort_i = 1992$	-0.033	-0.052^c
	(0.028)	(0.027)
$\mathbf{R\ Hum_c^{2012} \times f_i \times Cohort_i = 1993}$	0.004	-0.020
	(0.027)	(0.031)
$R\ Hum_c^{2012} \times f_i \times Cohort_i = 1994$	0.024	-0.088^b
	(0.033)	(0.026)
$R\ Hum_c^{2012} \times f_i \times Cohort_i = 1995$	0.007	-0.048
	(0.032)	(0.030)
Controls	Individual and city controls (X_{ift} & Z_{cfk})	
Fixed effects	F_{cf}, F_{ck}, F_{pkf}, F_{pkt} & F_{pft}	
Observations	93,700	93,700
R^2	0.148	0.148
No. of Cities	401	401

Notes: R denotes the restriction measure by city. c stands for city, k for birth cohort, i for individual, f for gender, p for province, and t refers to the year of the survey. *Sample:* HIES 2008-2021. Birth cohorts 1989-1995, individuals age 19 to 28 at time of interview. *Individual controls:* Urban residence and presence of another household member with education above lower middle school and their interactions with gender. *City controls*: Interactions of the cohort dummies and gender with i. the number of seats for women in city c in the academic year a, ii. the share of local courses that have a gender quota in city c for the academic years 2010 to 2014, and iii. the restriction variable for city c constructed (in the equivalent way as the restrictions for 2012) for the academic years 2011, 2013 and 2014. Regressions all include but don't show the interaction terms of local restrictions for the older cohorts. Standard errors clustered at cohort and city level in parentheses. ^{a}, ^{b} and ^{c} denote significance at the 1, 5, and 10 percent levels, respectively.

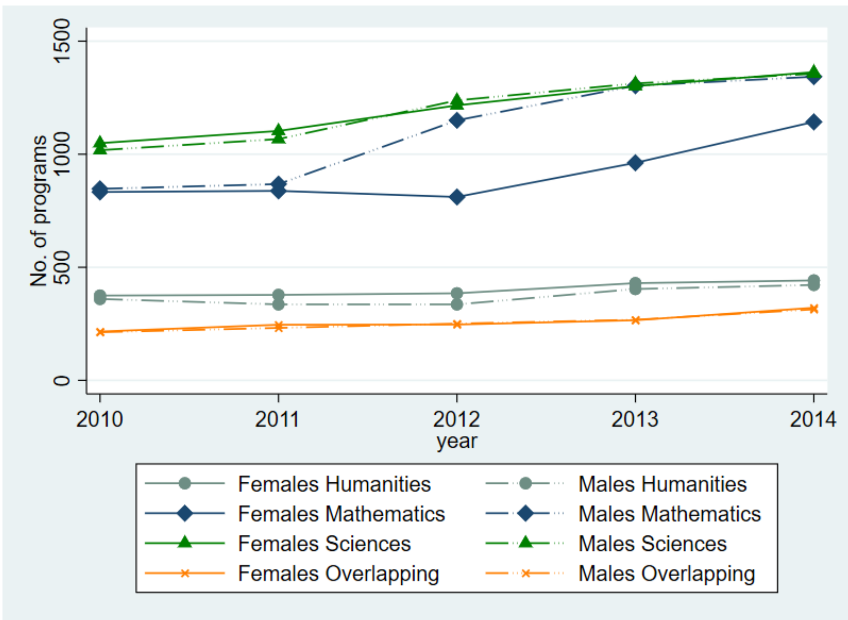
D Additional Figures and Tables

Figure D.1: Number of seats reserved for females and males in public universities



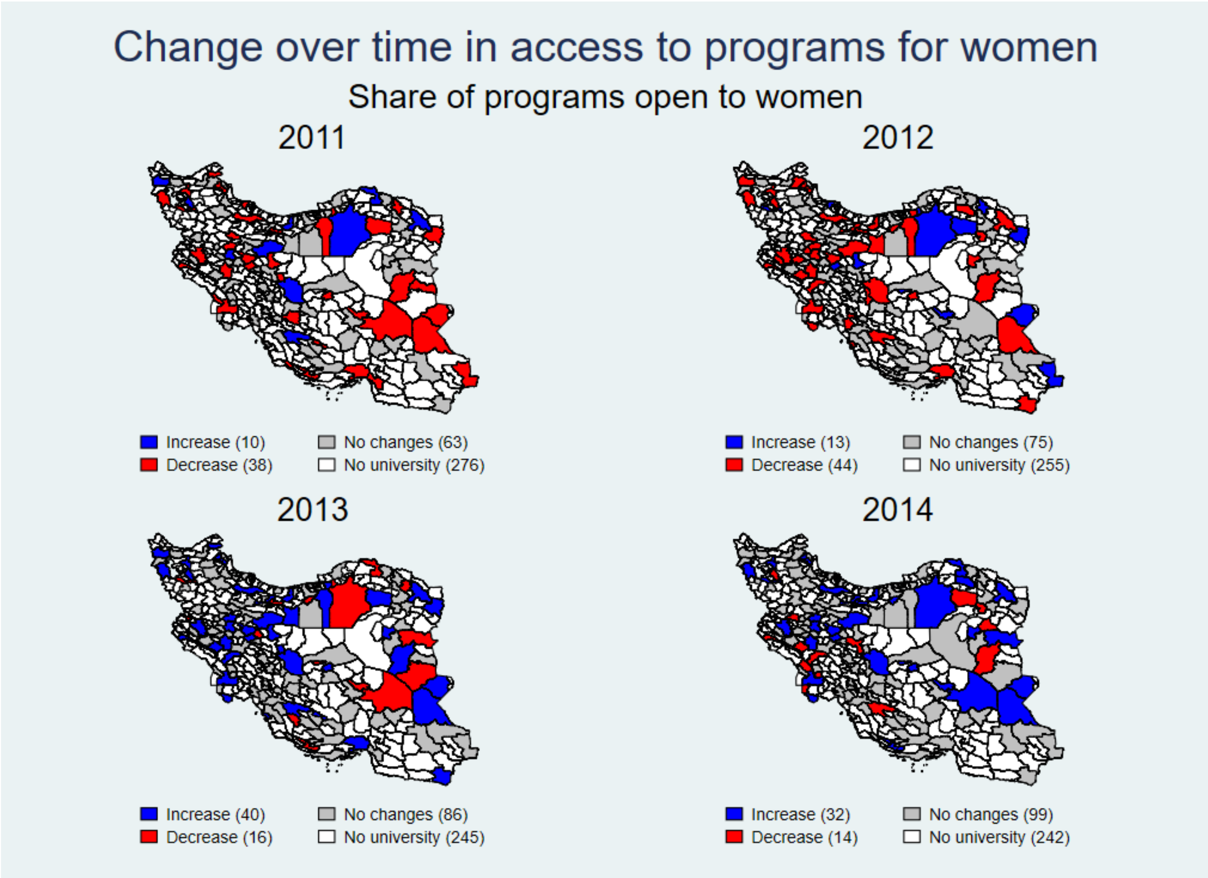
Notes: Data from university course books published by the Iranian Ministry of Education (2010-2014).

Figure D.2: Number of programs by field and gender in public universities



Notes: Data from university course books published by the Iranian Ministry of Education (2010-2014).

Figure D.3: Spatial distribution of restrictions

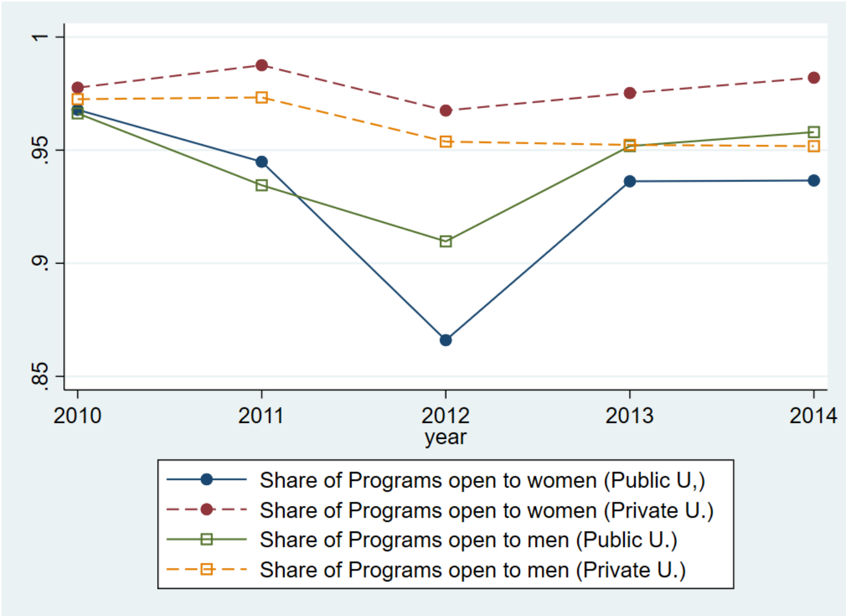


Notes: Data from the university course books published by the Iranian Ministry of Education (2010-2014). Programs are aggregated at the *shahrestan* level. The numbers in parentheses indicate the number of cities (*shahrestan*) that are in the corresponding category. The share of programs open to women is calculated as the number of programs in the city which either do no give any restrictions by gender or where at least one seat is reserved for a woman, divided by the total number of programs offered to new BA students in that year by the local universities.

Table D.6: Extract course book Maths & Physics, 2012

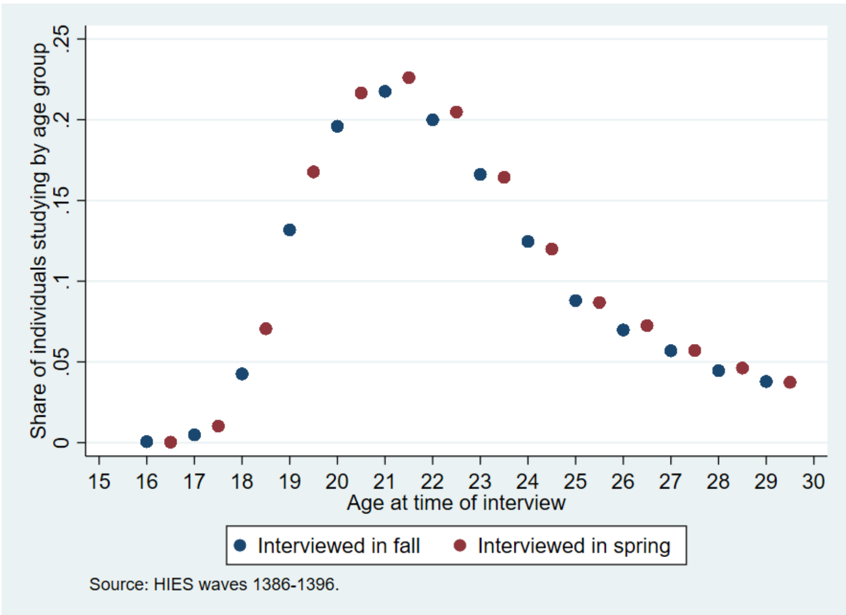
	Yasuj University			
Code	Title of BA program	Total nb of seats	Seats female	Seats male
2108	Mathematics and applications	36	18	18
2109	Physics	36	18	18
2110	Polymer Engineering	15	-	Male
2111	Chemical Engineering	20	7	13
2112	Civil Engineering	36	12	24
2113	Mechanical Engineering	36	12	24
2114	Materials Engineering	20	7	13
2115	Civil Engineering Technician	15	-	Male

Figure D.4: Admission restrictions for men and women by type of university



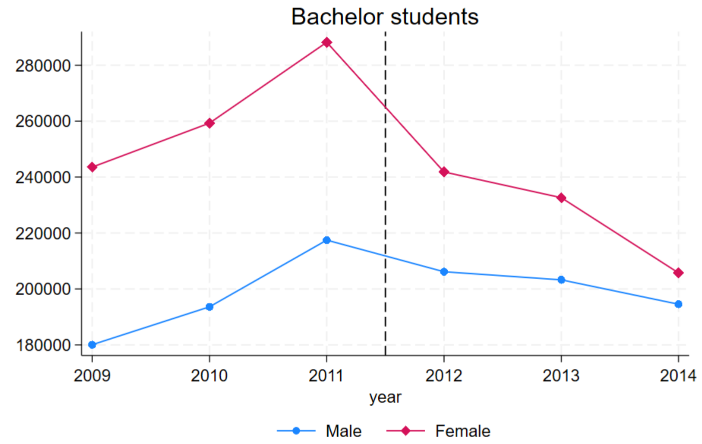
Notes: Data from the university course books published by the Iranian Ministry of Education (2010-2014).

Figure D.5: Distribution age for students at university (Bachelor or Master)



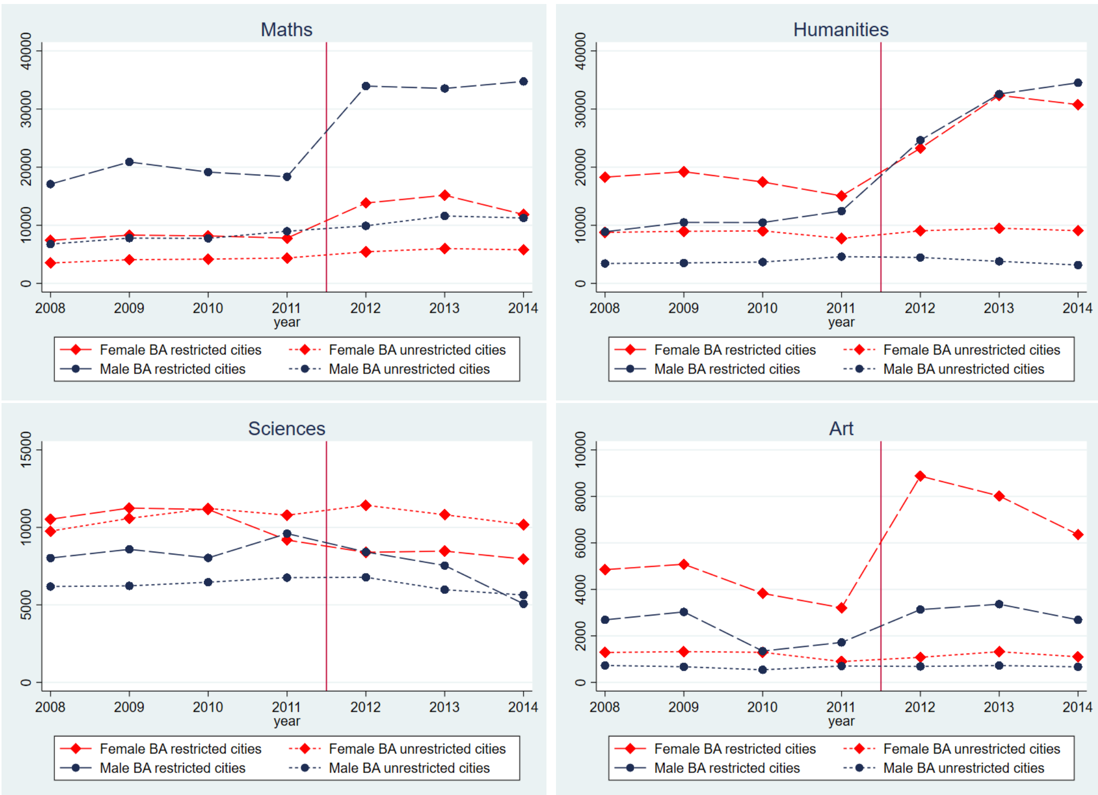
Notes: data from the Household Income and Expenditure Survey (2008-2019).

Figure D.6: Total number of students enrolled in Bachelor degrees by gender



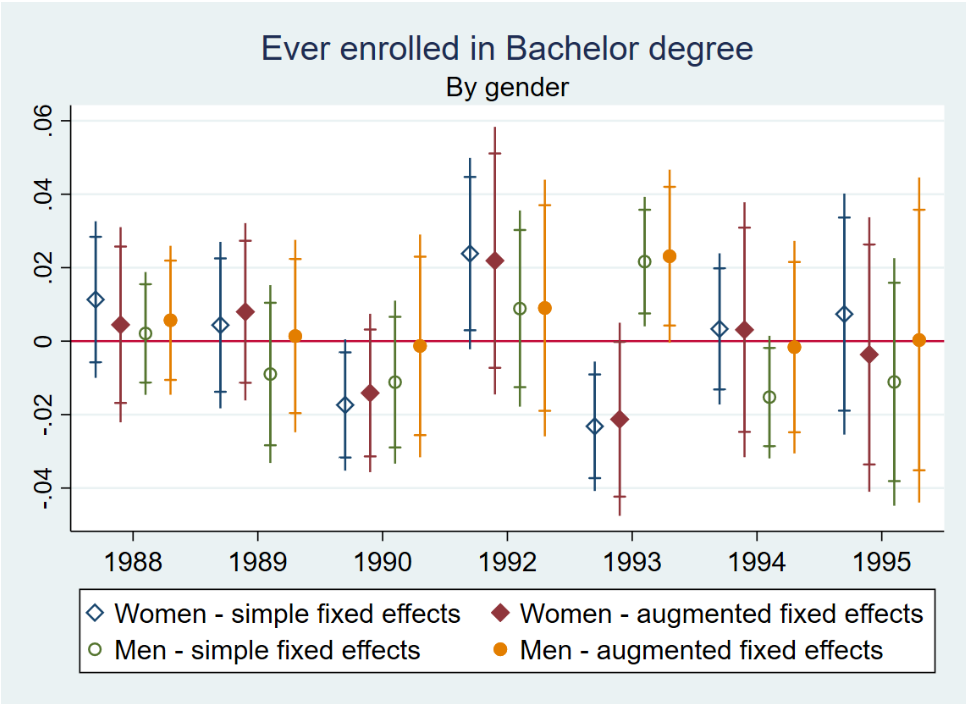
Note: Number of students newly enrolled in Bachelor degrees by gender. The data is from the Student Statistics released by the Iranian Ministry of Education. The graph excludes Islamic Azad University.

Figure D.7: Number of students enrolled in Bachelor degrees by gender and field in cities with and without restrictions



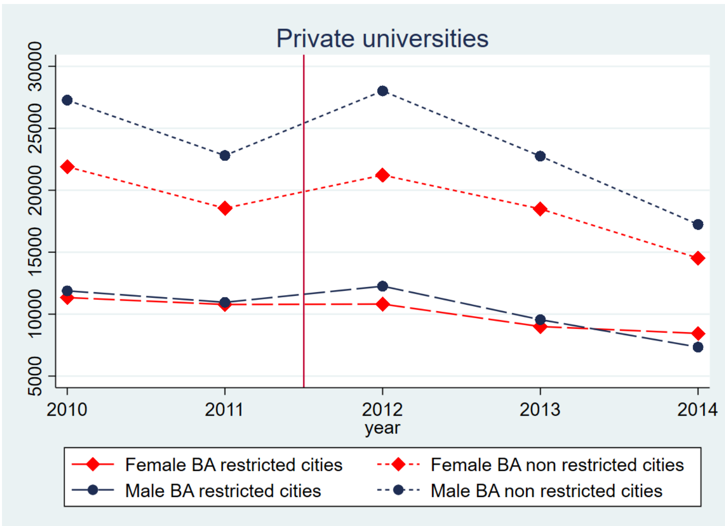
Notes: Number of students enrolled in Bachelor degrees by gender and field in cities with and without restrictions to admissions for women. The data is from the Student Statistics released by the Iranian Ministry of Education.

Figure D.8: University enrolment by gender



Horizontal axis indicates the year of birth. Reported coefficients are of the local restrictions-cohort interaction terms. Simple fixed effects include city, province-cohort and province-year dummies. Augmented fixed effects include city-year and province-cohort dummies. All regressions include the standard individual controls and city controls interacted by cohort. 90% and 95% confidence intervals are reported.

Figure D.9: Number of students enrolled in Bachelor degrees in private universities by gender in cities with and without restrictions



Notes: This graph shows the number of students enrolled in Bachelor degrees in private universities by gender in cities with and without restrictions to admissions for women. The data is from the Student Statistics released by the Iranian Ministry of Education.

Table D.7: Main study fields of university graduates, 1991 cohort

		Males	Females	Total
1 Educational sciences	N	137	412	549
	%	1.64	4.43	3.11
2 Arts and humanities	N	443	895	1,338
	%	5.30	9.62	7.57
3 Social sciences	N	374	919	1,293
	%	4.47	9.87	7.32
4 Business administration	N	2,033	2,709	4,742
	%	24.30	29.11	26.83
5 Experimental sciences	N	496	934	1,43
	%	5.93	10.04	8.09
6 Information and communication	N	51	42	93
	%	0.61	0.45	0.53
7 Engineering	N	4,035	1,886	5,921
	%	48.24	20.26	33.50
8 Agriculture	N	306	442	748
	%	3.66	4.75	4.23
9 Medicine and health	N	265	833	1,098
	%	3.17	8.95	6.21
10 Nursing and social services	N	205	204	409
	%	2.45	2.19	2.31
99 Unknown	N	20	29	49
	%	0.24	0.31	0.28
Total	N	8,365	9,307	17,672

Source: Iran Labor Force Survey, 2013-2017.

Table D.8: Descriptive statistics by gender

	Women			***Men***		
	Mean	Std.Dev.	Obs	Mean	Std.Dev.	Obs
Age	22.901	2.780	45479	22.969	2.755	48221
Married	0.393	0.488	45479	0.151	0.358	48221
HH size	4.493	1.749	45479	4.662	1.681	48221
Urban	0.643	0.479	45479	0.585	0.493	48221
Highest level of education	4.672	1.316	45479	4.611	1.285	48221
Ever enrolled in university	0.409	0.492	45479	0.362	0.481	48221
Ever enrolled in an associate degree	0.092	0.289	45479	0.127	0.333	48221
Labor force participation	0.207	0.405	45479	0.656	0.475	48221
Employed for a wage	0.066	0.249	45479	0.398	0.489	48221
Out of work and study	0.630	0.483	45479	0.310	0.462	48221
Total income	1.539	8.117	45479	11.119	21.630	48221
Income from main job	1.297	7.407	45479	9.980	20.515	48221
Total income (above 0)	25.804	21.874	2713	29.666	26.421	18074
Income from main job (above 0)	24.187	21.679	2438	28.665	25.946	16789
Low-skilled sectors	0.017	0.130	45479	0.180	0.384	48221

Notes: Data from HIES for years 2008-2021. The sample includes individuals with at least a high school degree, and of age 19-28. Income is expressed in 10000 Iranian Rials. The variables *Total income (above 0)* and *Income from main job (above 0)* are only defined for individuals with positive income levels.

Table D.9: Descriptive statistics on main city level variables

	Mean	Std.Dev.	Min.	Max.	Obs
R_c^{2012} - closest distance	0.593	1.101	-2	2	93700
R_c^{2011} - closest distance	0.256	0.977	-2	2	93700
R_c^{2013} - closest distance	-0.490	1.015	-2	2	93700
R_c^{2014} - closest distance	-0.387	0.975	-2	2	93700
R_c^{2012} - continous indicator	0.0988	0.362	-2	1	92462
R_c^{2012} - Simplified indicator	0.391	0.660	-1	1	93700
R_c^{2012} - within 80km	0.869	1.053	-2	2	93700
R_c^{2012} - within 60km	0.706	1.020	-2	2	93700
R_c^{2012} - using seats	0.159	1.463	-2	2	93700
$R_c^{2012} Maths$ - within 60 km	0.711	1.091	-2	2	93700
$R_c^{2012} Common$ - within 60 km	0.405	0.880	-2	2	93700
$R_c^{2012} Sciences$ - within 60 km	0.422	0.846	-2	2	93700
$R_c^{2012} Humanities$ - within 60 km	0.283	0.630	0	2	93700
$R_c^{2012} Maths$ - within 80 km	0.885	1.154	-2	2	93700
$R_c^{2012} Common$ - within 80 km	0.573	0.972	-2	2	93700
$R_c^{2012} Sciences$ - within 80 km	0.500	0.894	-2	2	93700
$R_c^{2012} Humanities$ - within 80 km	0.387	0.722	0	2	93700

Table D.10: Descriptive statistics for restricted and unrestricted cities

	Unrestricted cities	Restricted cities	Diff	SE	p-value	Obs
		Panel A: Full sample				
Female	0.503	0.494	0.009	0.015	0.553	324
Urban	0.388	0.494	-0.106	0.026	0.000	324
Highest level of education	2.782	2.831	-0.049	0.086	0.568	324
Ever enrolled in university	0.167	0.172	-0.005	0.014	0.740	324
Married	0.488	0.480	0.009	0.022	0.684	324
Employed for a wage	0.398	0.400	-0.002	0.020	0.923	324
Labor force participation	0.563	0.569	-0.006	0.017	0.725	324
Income quartile of household	2.553	2.708	-0.156	0.059	0.009	324
Share of conservative votes in 2009	0.695	0.649	0.047	0.017	0.007	324
		Panel B: Women				
Ever enrolled in university	0.174	0.180	-0.006	0.021	0.778	324
Employed for a wage	0.132	0.118	0.014	0.021	0.494	324
Labor force participation	0.249	0.220	0.029	0.025	0.246	324
Married	0.586	0.584	0.002	0.028	0.948	324
		Panel C: Men				
Ever enrolled in university	0.154	0.160	-0.006	0.018	0.732	324
Employed for a wage	0.666	0.664	0.002	0.028	0.943	324
Labor force participation	0.889	0.897	-0.008	0.016	0.612	324
Married	0.406	0.392	0.014	0.029	0.632	324

Notes: Data from HIES 2011. Sample includes individuals aged 25 to 27. An observation is a city.

Table D.11: Descriptive statistics for restricted and unrestricted cities: yearly changes 2008-2011

	Unrestricted cities	Restricted cities	Diff	SE	p-value	Obs
		Panel A: Full sample				
Female	0.002	-0.003	0.005	0.023	0.820	954
Urban	0.007	-0.017	0.024	0.021	0.255	954
Highest level of education	0.147	0.086	0.061	0.079	0.442	954
Ever enrolled in university	0.023	0.015	0.008	0.017	0.623	954
Married	-0.013	-0.030	0.017	0.023	0.460	954
Employed for a wage	-0.022	-0.016	-0.006	0.023	0.782	954
Labor force participation	-0.007	-0.001	-0.006	0.023	0.794	954
Income quantile of household	-0.010	-0.003	-0.007	0.061	0.906	634
		Panel B: Women				
Ever enrolled in university	0.034	0.016	0.019	0.023	0.412	845
Employed for a wage	-0.018	-0.007	-0.011	0.022	0.606	845
Labor force participation	-0.002	0.006	-0.008	0.027	0.773	845
Married	-0.018	-0.038	0.020	0.031	0.521	845
		Panel C: Men				
Ever enrolled in university	0.022	0.021	0.002	0.021	0.943	825
Employed for a wage	-0.033	-0.042	0.009	0.028	0.735	825
Labor force participation	-0.013	-0.020	0.008	0.020	0.698	825
Married	-0.026	-0.020	-0.006	0.029	0.825	825

Notes: Data from HIES, 2008-2011. Sample includes individuals age 25 to 27. Income quartile is only available for years 2009-2011. An observation is a city-year.

Table D.12: Choice of field conditional on having been enrolled

	(1)	(2)	(3)	(4)
	With university education			
Dependent variable:	Mathmatics	Humanities	Applied Sciences	Overlapping subjects
$R_c^{2012} \times f_i \times Cohort_i = 1992$	0.034	0.055^c	-0.041	-0.001
	(0.051)	(0.026)	(0.023)	(0.049)
$\mathbf{R_c^{2012} \times f_i \times Cohort_i = 1993}$	-0.059^c	0.056	0.026	-0.022
	(0.022)	(0.034)	(0.025)	(0.023)
$R_c^{2012} \times f_i \times Cohort_i = 1994$	0.005	0.046	-0.043^c	-0.008
	(0.041)	(0.032)	(0.018)	(0.033)
$R_c^{2012} \times f_i \times Cohort_i = 1995$	-0.058	-0.001	0.076	-0.007
	(0.035)	(0.030)	(0.042)	(0.030)
Controls	Individual and city controls (X_{ify} & Z_{cfk})			
Fixed effects	F_{cf}, F_{ck}, F_{pkf}, F_{pft} & F_{date}			
Observations	12,323	12,323	12,323	12,323
R^2	0.287	0.235	0.218	0.226
No. of Cities	360	360	360	360

Notes: R denotes the restriction measure by city. c stands for city, k for birth cohort, i for individual, f for gender, p for province, and t refers to the year of the survey. *Sample:* LFS 2013-2017. Birth cohorts 1991-1995, indidviduals age 21 to 22 at time of interview that have ever been enrolled in university. *Individual controls:* Urban residence and presence of another household member with education above lower middle school and their interactions with gender. *City controls*: Interactions of the cohort dummies and gender with i. the number of seats for women in city c in the academic year a, ii. the share of local courses that have a gender quota in city c for the academic years 2010 to 2014, and iii. the restriction variable for city c constructed (in the equivalent way as the restrictions for 2012) for the academic years 2011, 2013 and 2014. Regressions all include but don't show the interaction terms of local restrictions for the older cohorts. Standard errors clustered at cohort and city level in parentheses. ^{a}, ^{b} and ^{c} denote significance at the 1, 5, and 10 percent levels, respectively.

Table D.13: Characteristics of spouse and new household: Age and Income

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A:	*All married women*					
	Age diff. spouses		ln work income spouse		ln total hh pc income	
Add. controls	No	Yes	No	Yes	No	Yes
$R_c^{2012} \times Cohort_i = 1992$	-0.218	-0.222	-0.023	-0.021	-0.021	-0.025
	(0.188)	(0.188)	(0.036)	(0.037)	(0.036)	(0.037)
$\mathbf{R_c^{2012} \times Cohort_i = 1993}$	-0.069	-0.063	-0.033	-0.019	-0.068^c	-0.050
	(0.185)	(0.184)	(0.033)	(0.033)	(0.034)	(0.034)
$R_c^{2012} \times Cohort_i = 1994$	-0.084	-0.085	0.083^b	0.084^b	0.076^c	0.076^c
	(0.199)	(0.199)	(0.029)	(0.029)	(0.033)	(0.033)
$R_c^{2012} \times Cohort_i = 1995$	-0.160	-0.160	-0.002	0.000	0.013	0.016
	(0.243)	(0.243)	(0.037)	(0.042)	(0.040)	(0.043)
Observations	15,691	15,691	14,802	14,802	15,094	15,094
R^2	0.249	0.249	0.678	0.698	0.356	0.396
Panel B:	*Married women with university education*					
$R_c^{2012} \times Cohort_i = 1992$	-0.530	-0.522	-0.145	-0.129	-0.050	-0.025
	(0.358)	(0.361)	(0.099)	(0.104)	(0.082)	(0.085)
$\mathbf{R_c^{2012} \times Cohort_i = 1993}$	0.183	0.175	0.147	0.117	0.098	0.084
	(0.521)	(0.522)	(0.078)	(0.078)	(0.099)	(0.104)
$R_c^{2012} \times Cohort_i = 1994$	0.409	0.374	0.247^b	0.167	0.315^b	0.233^b
	(0.419)	(0.426)	(0.099)	(0.095)	(0.095)	(0.084)
$R_c^{2012} \times Cohort_i = 1995$	-1.278^c	-1.273^c	0.049	0.081	0.146	0.178
	(0.584)	(0.586)	(0.084)	(0.085)	(0.113)	(0.125)
Observations	3,050	3,050	2,818	2,818	2,874	2,874
R^2	0.465	0.465	0.729	0.749	0.514	0.537
Panel C:	*Married women without university education*					
$R_c^{2012} \times Cohort_i = 1992$	-0.194	-0.206	0.044	0.042	0.046	0.041
	(0.289)	(0.287)	(0.054)	(0.055)	(0.051)	(0.052)
$\mathbf{R_c^{2012} \times Cohort_i = 1993}$	0.124	0.147	-0.018	-0.006	-0.055	-0.040
	(0.210)	(0.207)	(0.037)	(0.038)	(0.035)	(0.034)
$R_c^{2012} \times Cohort_i = 1994$	0.093	0.106	0.076^c	0.079^c	0.069	0.076
	(0.263)	(0.262)	(0.037)	(0.039)	(0.046)	(0.048)
$R_c^{2012} \times Cohort_i = 1995$	0.321	0.332	0.011	0.008	0.002	0.006
	(0.296)	(0.294)	(0.042)	(0.045)	(0.046)	(0.050)
Observations	11,438	11,438	10,814	10,814	11,017	11,017
R^2	0.282	0.283	0.697	0.712	0.389	0.410

Notes: R denotes the restriction measure by city. c stands for city, k for birth cohort, i for individual, f for gender, p for province, and t refers to the year of the survey. *Sample:* HIES 2008-2021. Married women of birth cohorts 1989-1995 for which the husband could be identified within same household. Individuals age 19 to 30 at time of interview. *Individual controls:* Urban residence. *City controls:* Interactions of the cohort dummies with i. the number of seats for women in city c in the academic year a, ii. the share of local courses that have a gender quota in city c for the academic years 2010 to 2014, and iii. the restriction variable for city c constructed (in the equivalent way as the restrictions for 2012) for the academic years 2011, 2013 and 2014. All regressions include fixed effects at the province-cohort (F_{pk}) and city-year (F_{ct}) level. Regressions all include but don't show the interaction terms of local restrictions for the older cohorts. Standard errors clustered at cohort and city level in parentheses. ^{a}, ^{b} and ^{c} denote significance at the 1, 5, and 10 percent levels, respectively.

Table D.14: Labor market outcomes: Employment and participation, 19-23 years old

	(1)	(2)	(3)	(4)	(5)	(6)
	All		Women		Men	
Dependent variable:	Empl. for wage	No work or study	Empl. for wage	No work or study	Empl. for wage	No work or study
$R_c^{2012} \times f_i \times Cohort_i = 1992$	-0.018	-0.037^c				
	(0.010)	(0.019)				
$\mathbf{R_c^{2012} \times f_i \times Cohort_i = 1993}$	0.010	0.004				
	(0.012)	(0.016)				
$R_c^{2012} \times f_i \times Cohort_i = 1994$	-0.071^a	0.033^c				
	(0.016)	(0.017)				
$R_c^{2012} \times f_i \times Cohort_i = 1995$	-0.018	-0.020				
	(0.011)	(0.020)				
$R_c^{2012} \times Cohort_i = 1992$			-0.015^c	0.003	-0.002	0.023
			(0.007)	(0.019)	(0.016)	(0.018)
$\mathbf{R_c^{2012} \times Cohort_i = 1993}$			-0.007	-0.005	-0.006	-0.011
			(0.007)	(0.022)	(0.016)	(0.015)
$R_c^{2012} \times Cohort_i = 1994$			-0.018^c	-0.016	0.039	-0.014
			(0.009)	(0.024)	(0.021)	(0.021)
$R_c^{2012} \times Cohort_i = 1995$			0.005	0.034	-0.024	0.012
			(0.011)	(0.024)	(0.020)	(0.032)
Controls	X_{ift} & Z_{cfk}		X_{it} & Z_{ck}			
Fixed effects	F_{cf}, F_{ck}, F_{pkf}, F_{pkt} & F_{pft}		F_{pk} & F_{ct}			
Observations	54,498	54,498	26,069	26,069	27,412	27,412
R^2	0.270	0.182	0.176	0.176	0.241	0.172
No. of Cities	394	394	391	391	392	392

Notes: R denotes the restriction measure by city. c stands for city, k for birth cohort, i for individual, f for gender, p for province, and t refers to the year of the survey. *Sample:* HIES 2008-2021. Birth cohorts 1989-1995, individuals age 19 to 23 at time of interview. *Individual controls:* Urban residence and presence of another household member with education above lower middle school. *City controls:* Interactions of the cohort dummies with i. the number of seats for women in city c in the academic year a, ii. the share of local courses that have a gender quota in city c for the academic years 2010 to 2014, and iii. the restriction variable for city c constructed (in the equivalent way as the restrictions for 2012) for the academic years 2011, 2013 and 2014. Col. 1 and 2 include also the interactions of all control variables with gender. Regressions all include but don't show the interaction terms of local restrictions for the older cohorts. Standard errors clustered at cohort and city level in parentheses. ^{a}, ^{b} and ^{c} denote significance at the 1, 5, and 10 percent levels, respectively.

Table D.15: Labor market outcomes: Income, 19-30 years old

	(1)	(2)	(3)	(4)	(5)	(6)
	All		With univ.		No univ.	
Dependent variable:	Income I	Income II	Income I	Income II	Income I	Income II
Panel A:	*All individuals - triple difference*					
$R_c^{2012} \times f_i \times Cohort_i = 1992$	0.105	0.132	0.199	0.598	0.361	-0.007
	(0.096)	(0.121)	(0.232)	(0.361)	(0.391)	(0.425)
$\mathbf{R_c^{2012} \times f_i \times Cohort_i = 1993}$	-0.115	-0.071	0.047	0.021	0.057	-0.290
	(0.128)	(0.140)	(0.353)	(0.585)	(0.274)	(0.337)
$R_c^{2012} \times f_i \times Cohort_i = 1994$	-0.209	-0.225	0.262	0.608	-1.215^c	-1.565^b
	(0.155)	(0.144)	(0.279)	(0.360)	(0.555)	(0.523)
$R_c^{2012} \times f_i \times Cohort_i = 1995$	0.652^a	0.583^a	0.396	2.471	1.026^b	1.222^b
	(0.138)	(0.160)	(0.581)	(1.638)	(0.366)	(0.410)
	Fixed effects: F_{cf}, F_{ck}, F_{pkf} & F_{pt}					
Observations	24,190	22,350	6,484	5,701	16,528	15,410
R^2	0.356	0.383	0.478	0.515	0.418	0.434
Panel B:	*Women*					
$R_c^{2012} \times Cohort_i = 1992$	-0.030	-0.009	0.132	0.325	0.160	0.105
	(0.100)	(0.124)	(0.224)	(0.234)	(0.585)	(0.639)
$\mathbf{R_c^{2012} \times Cohort_i = 1993}$	-0.098	-0.114	0.388	0.624^b	-0.465	-1.342
	(0.123)	(0.122)	(0.272)	(0.235)	(0.540)	(0.717)
$R_c^{2012} \times Cohort_i = 1994$	-0.035	-0.070	-0.355	-0.269	-0.933	-1.418
	(0.139)	(0.144)	(0.230)	(0.279)	(1.035)	(0.995)
$R_c^{2012} \times Cohort_i = 1995$	0.676^a	0.596^a	0.311	0.294	2.112^b	2.111^c
	(0.155)	(0.168)	(0.324)	(0.278)	(0.887)	(1.111)
	Fixed effects: F_c, F_{pk} & F_{pt}					
Observations	3,207	2,857	1,774	1,515	1,127	976
R^2	0.490	0.524	0.550	0.611	0.706	0.724
Panel C:	*Men*					
$R_c^{2012} \times Cohort_i = 1992$	-0.070^c	-0.083^b	0.016	-0.047	-0.105^b	-0.105^b
	(0.030)	(0.030)	(0.064)	(0.069)	(0.033)	(0.034)
$\mathbf{R_c^{2012} \times Cohort_i = 1993}$	-0.027	-0.041	0.074	0.023	-0.071	-0.068
	(0.038)	(0.037)	(0.076)	(0.080)	(0.038)	(0.039)
$R_c^{2012} \times Cohort_i = 1994$	0.024	0.033	0.117	0.060	-0.021	-0.001
	(0.041)	(0.041)	(0.081)	(0.084)	(0.044)	(0.046)
$R_c^{2012} \times Cohort_i = 1995$	0.035	0.041	0.202^c	0.172	-0.017	-0.006
	(0.028)	(0.027)	(0.099)	(0.093)	(0.039)	(0.041)
	Fixed effects: F_c, F_{pk} & F_{pt}					
Observations	21,277	19,821	5,475	4,987	15,761	14,791
R^2	0.268	0.288	0.357	0.404	0.299	0.309

Notes: R denotes the restriction measure by city. c stands for city, k for birth cohort, i for individual, f for gender, p for province, and t refers to the year of the survey. *Sample:* HIES 2008-2021. Birth cohorts 1989-1995, individuals age 19 to 30 at time of interview and who decalare a positive individual income. *Dependent variables:* Income I refers to the ln of total individual work income. Income II corresponds to the ln of individual income from main job.*Individual controls:* Urban residence. *City controls:* Interactions of the cohort dummies with i. the number of seats for women in city c in the academic year a, ii. the share of local courses that have a gender quota in city c for the academic years 2010 to 2014, and iii. the restriction variable for city c constructed (in the equivalent way as the restrictions for 2012) for the academic years 2011, 2013 and 2014. Regressions all include but don't show the interaction terms of local restrictions for the older cohorts. Standard errors clustered at cohort and city level in parentheses. ^{a}, ^{b} and ^{c} denote significance at the 1, 5, and 10 percent levels, respectively.