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Education Expansion, College Choice and Labour Market Success

Abstract

We study the choice of acquiring STEM and non-STEM college education using variation induced by the proximity to universities offering different types of programs. We adopt a novel methodology allowing the identification of the distribution of response types and treatment effects in a multiple unordered discrete choice setting (Heckman and Pinto, 2018). The empirical analysis is based on confidential survey data for Italy, combined with administrative information about the founding dates of all Italian universities and faculties. We find that most compliers are women at the margin of choosing STEM education versus not going to college. We simulate the effects of expanding the supply of STEM education and discover that, in addition to substantial effects on employment, the gender disparity in STEM education could potentially decrease by up to 20%.

JEL-Codes: I230, I260, I280, J310.

Keywords: monotonicity, returns to education, STEM, instrumental variables.

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1 Introduction

Recent evidence shows that the choice of a college major can be as important, or even more important, as the choice of going to college (Altonji, Blom, and Meghir, 2012; Altonji, Arcidiacono, and Maurel, 2015). For example, Kirkeboen, Leuven, and Mogstad (2016) show that choosing a science versus a humanity major may triple earnings. But investigating major choices is a challenging exercise (Arcidiacono, 2004; Beffy, Fougere, and Maurel, 2012; Mourifie, Henry, and Meango, 2020). Even in its simplest formulation, the problem usually features heterogeneous agents choosing among a potentially large number of alternatives that do not have a natural ordering (Hastings, Neilson, and Zimmerman, 2013; Kirkeboen et al., 2016). The under-representation of women in math-intensive majors, often the ones associated with the highest labour market returns, makes the choice of college majors relevant for the discussion on gender inequalities as well (Kahn and Ginther, 2017; Sloane, Hurst, and Black, 2021).

In this paper, we study the choice of college majors and its effect on labour market outcomes. We develop a simple model with three possible choices: not going to college, going to a STEM (*Science, Technology, Engineering and Math*) college and going to a non-STEM college. To address the obvious endogeneity concerns, we exploit variation in the cost of college induced by geographic distance. Our instrument is a four-valued discrete variable whose categories indicate proximity to colleges offering STEM and/or non-STEM programs. In our empirical application, a college is near to an individual if it is located in the province of residence at the age of completing compulsory education and it is far otherwise.

For the empirical investigation, we combine two sources of data. The first is the Italian Labour Force Survey (LFS) for the years 2008-2020. Besides standard demographics, the survey includes information about both the level and the field of the respondents' educational attainments. Moreover, we also observe a wide array of interesting labour market outcomes, such as employment, earnings and a set of indicators capturing various dimensions of working conditions. The second database, assembled by Cottini, Ghinetti, and Moriconi (2019), is the History of Italian Universities (HIU), which contains the dates of creation of each faculty in each university between 1861 and 2010.[1] To construct the instrument, we rely on a confidentiality agreement that allows us to match the Italian LFS with the HIU based on the province of residence of each respondent at the time of the survey.

Eventually, our empirical model consists of three potential treatments (not going to college, going to a STEM college and going to a non-STEM college) and one discrete instrument with four values. Importantly, there is no natural ordering of the treatments or of the instrument values. This is precisely the setting described in Heckman and Pinto (2018). In our specific application, there exists a total of 81 theoretically possible response types. Similarly to the standard LATE setting, where identification is achieved assuming away the presence of one response type (the defiers), we rule out the types that appear irrational in the light of a simple model that we present in Section 2. We end up retaining 8 admissible response types, which are sufficiently few to allow the identification of the full distribution of types and of the treatment effects of several interesting policy interventions.

Imbens and Angrist (1994) first noted that, beyond the usual relevance and exogeneity, standard IV estimators also require some form of monotonicity to allow the identification of interpretable treatment effects. However, the standard monotonicity assumption is inadequate with multidimensional treatments that do not have a natural ordering. Hence, the effects of multiple alternative college choices, which are intrinsically unordered, cannot be identified and estimated with standard IV methods and Heckman and Pinto (2018) provide a solution to this problem that is very well suited to our setting.

We find that about 2% of non-graduate women in our sample would acquire a STEM tertiary degree if there was a STEM college in their province of residence. The share of men along the same margin is about 1.3%, hence expanding the supply of STEM tertiary education would reduce the gender gap in STEM education by about one-fifth.[2] We also show that expanding

[1]The terms university/college and faculty/school have sometimes different meanings in different countries/education systems. We will use college and university interchangeably to indicate a single administrative unit which may offer degree programs in a variety of fields. We will also use faculty and school interchangeably to indicate individual teaching units within a college/university. Hence, a college/university is a collection of schools/faculty. Typically, each school/faculty offers one or more degree programs in a specific field. For example, the school/faculty of economics&management at the University of Bologna offers undergraduate and graduate degree programs in economics and management.

[2]The specific expansion that we consider is one that creates a new STEM faculty in any province that does not have one.

STEM education produces a sizeable increase of female employment. Back-of-envelope calculations indicate that by itself such an increase would generate economic returns equal to over 2.5 times the operating costs of the new schools. We find a small and non-significant effects on the employment of men instead, which suggest that this reform would end up reducing the gender gap in employment rates by about 3%. Finally, we show the treatment effects on most other outcomes are imprecisely estimated but the evidence is suggestive of a generalised positive effect on working conditions: higher earnings (but also more hours of work), higher likelihood of being in a job involving the supervision of co-workers and lower likelihood of working during the night or in the weekend.

Our contribution to the literature is twofold. First, we provide new evidence on the labour market effect of college majors based on college proximity, whereas the best evidence currently available is based on admission thresholds (Hastings et al., 2013; Kirkeboen et al., 2016). By doing so, we overcome the well-known local nature of evidence based on discontinuities. Of course, our results are also limited by the specific choice of instrument, which defines the types of individuals who would respond to variation in geographical proximity. However, our methodology allows us to fully identify the distribution of response types, which is helpful in characterising and interpreting the local nature of the estimates. Moreover, college proximity has a long history in the economics of education, hence our results can be directly compared to a large number of studies based on the same or similar sources of variation (Cameron and Taber, 2004; Card, 1995; Carneiro and Lee, 2011; Carneiro, Heckman, and Vytlacil, 2011; Eisenhauer, Heckman, and Vytlacil, 2015; Kling, 2001; Nybom, 2017; Rouse, 1995).

While estimates based on admission discontinuities help assess the impact of changes in the selectivity of college entry requirements, we argue that our results can be informative about a large set of policies affecting the cost of attending college, such as subsiding college fees or student loans. The main channel through which geographical proximity to institutions of tertiary education influences educational choices is via its relation to costs. The variation we use in our application is generally associated with the decision to either commute or moving residence close to the university. Most students attending college in their province of residence would do so by commuting, whereas students who attend college in a different province would change residence. We estimate that the difference in the overall cost of college attendance would be between 40% and 50%.

Obviously, our results are also specific to the Italian setting but Italy is a particularly interesting case for the study of college choices. First, it is a country where the share of college graduates is still fairly low (around 20% in 2020 compared to 32.8% in the EU), even among the most recent cohorts (29% vs 39% in the EU) and especially in STEM majors (8.8% vs 15.1% in the EU). Second, the gender gap in STEM is also particularly pronounced, approximately one-third larger than in the EU.[3] It is then plausible to imagine that education expansions can play an important role in reducing these gaps.

Our second contribution is methodological. The choice of a field of study is a very natural application of the *"unordered monotonicity"* approach of Heckman and Pinto (2018) and, to the best of our knowledge, we are the first to implement it. From the methodological point of view, the paper by Mountjoy (2022) is perhaps the closest to us but the research question - i.e. the choice of a 2-year community college versus a 4-year college - is fairly different.[4]

The rest of the paper is organized as follows. Section 2 describes our simple model of college choice. Section 3 discusses the identification of response types probabilities and of the treatment effects. Section 4 presents the data and the construction of key variables. Results are discussed in section 5. Section 6 presents some robustness checks and section 7 concludes.

2 Model

This section presents our simple model of college choice. Agents need to choose one of three possible treatments T: No college (N), non-STEM college (H) and STEM college (S). To simplify the notation, we indicate non-STEM college with H, for Humanities, although in our empirical exercise some majors that fall into this category might have non-negligible mathematical and scientific content (e.g. economics or medicine). We discuss this issue in Section 4.1 below.

[3] In Italy, only about 17% of female graduates hold a STEM degree compared to approximately 38% of men. The same figures for the EU are 28.4% (women) and 41.6% (men).

[4] Galindo Pardo (2021) also relies and builds on Heckman's method, but in a different framework, i.e. the choice among different (unordered) childcare options in Colombia.

For a generic agent i, the utilities associated with each treatment are the following:

$$U_i(N) = y_i(N) + \eta_i(N) \tag{1a}$$

$$U_i(H) = y_i(H) + \eta_i(H) - \alpha Far_i(H) \tag{1b}$$

$$U_i(S) = y_i(S) + \eta_i(S) - \alpha Far_i(S) \tag{1c}$$

where $y_i(T)$ is the (expected) potential outcome of individual i, given treatment $T = N, H, S$; $\eta_i(T)$ is the unobserved random preference of agent i for treatment T and $Far_i(T)$ is a dummy indicator for the distance of individual i's residence from a college of type T. Agents choose the alternative yielding the highest utility.

Besides rationality, the model relies on two additional assumptions. The first assumption is that college costs are exclusively affected by proximity to the college offering that specific treatment. In other words, we impose that the utility of any treatment T only depends on proximity to colleges offering treatment T, i.e. $Far_i(S)$ is excluded from $U_i(H)$ and $Far_i(H)$ is excluded from $U_i(S)$. We think that this is a reasonable assumption, as one only pays the cost of attending the chosen university. The assumption would fail if, for example, larger universities that offer both STEM and non-STEM programs were better connected to public transport and road networks. We do not expect this to be a major concern in our setting, especially given the detailed information on urban density that we include in our set of control variables.[5] The second assumption is that the marginal cost imposed by distance is constant and the same across treatments, i.e. the coefficient α is the same in equations (1b) and (1c). This assumption seems reasonable, as we do not see any particular reason to expect the distance cost to depend on the field of study.

Notice that we impose no assumptions on the joint distribution of $\{\eta_i(N), \eta_i(S), \eta_i(H)\}$. This differentiates our approach from the common multinomial models (e.g. multinomial probit or multinomial logit), which are derived by choosing a specific functional form for the joint distribution of the η_i's and writing a likelihood function for the observed choices. Compared to

[5] This assumption closely resembles the approach of Bhuller and Sigstad (2022), who consider a setting with multiple treatments and multiple instruments and impose that each instrument exclusively affects the choice of one treatment.

this parametric approach, all our estimates are completely non-parametric and exclusively rest on the above assumptions.[6]

Of course, we do maintain the exogeneity assumption of the instruments, as it is standard in any application of instrumental variables. In our setting, this can be simply stated as:

$$\{\eta_i(N), \eta_i(S), \eta_i(H)\} \perp \{Far_i(H), Far_i(S)\}$$

In Section 5.1, we provide some supportive evidence for the validity of the instrument.

3 Identification

The main objective of our empirical analysis is the identification and estimation of the distribution of response types and the treatment effects of the three education choices that we consider - No college, non-STEM college and STEM college - on various outcomes of interest. To address the obvious endogeneity concerns, we exploit variation in proximity to STEM and non-STEM colleges, which we code into a 4-value discrete instrument. In what follows, we adopt the notation in Heckman and Pinto (2018), and use T to indicate the treatment and Z to indicate the instrument:

$$T = \begin{cases} N & \text{No college} \\ H & \text{non-STEM college} \\ S & \text{STEM college} \end{cases} \quad \text{and} \quad Z = \begin{cases} FarH - FarS \\ FarH - NearS \\ NearH - FarS \\ NearH - NearS \end{cases}$$

For simplicity of exposition and notation, in this section we abstract from covariates but all our results easily generalise to the conditional case by simply replacing unconditional statistics with their conditional counterparts. We return to this point in Section 5. For notational clarity, we also suppress the individual subscript i.

As in Heckman and Pinto (2018), the combination of our multi-valued unordered treatment

[6] In Section 5.1, we do estimate a simple multinomial logit for the three educational choices that we consider but we report these results only for descriptive purposes. The estimates of the counterfactual outcomes and treatment effects that we discuss in Sections 5.2, 5.3 and 5.4 do not rely on any distributional assumption.

and the discrete instrument gives rise to a total of $3^4 = 81$ theoretically possible response types. As in the standard LATE setting of Imbens and Angrist (1994), identification of any quantity of interest is achieved by assuming away a sufficient number of response types. With a simple binary treatment and a binary instrument, it is sufficient to eliminate one response type (the so-called *"defiers"*). In more complicated setups, like ours, one needs to reduce the number of admissible types more substantially and the number and the exact nature of the quantities that can be identified crucially depend on which types are eliminated.[7]

We use the simple choice model of Section 2 to guide our selection of admissible response types. For presentational purposes, let us order the instrument values as in Table 6: $FarH - FarS$, $FarH - NearS$, $NearH - FarS$, $NearH - NearS$. Then, we can indicate response types by listing the educational choices that agents would make under each of the values of the instrument. For example, response type $N - N - S - S$ is someone who chooses not to go to college when living far from both types and also when living far from non-STEM and close to STEM; this type then chooses to go to a STEM college when living near non-STEM and far from STEM and also when living near both types of college. Clearly, if agents act rationally according to our choice model, there should be nobody in this response type. So, we assume it away. To see the irrationality of this response type more formally, consider the following contradicting implications. Response type $N - N - S - S$ chooses N when the value of the instrument is $FarH - NearS$ (which implies that $Far_i(S) = 0$). Hence, it must be that $y_i(N) + \eta_i(N) > y_i(S) + \eta_i(S)$. However, this same type also chooses S when the value of the instrument is $NearH - NearS$ (which also implies that $Far_i(S) = 0$) and it should follow that $y_i(N) + \eta_i(N) < y_i(S) + \eta_i(S)$, contradicting the previous implication.

We check all the 81 theoretical response types against our choice model and eventually only the 9 shown in Figure 1 survive.

[7] See Heckman and Pinto (2018) for a detailed discussion of identification in these contexts.

Figure 1: Admissible response types

$$\begin{array}{l} \\ FarH-FarS \\ FarH-NearS \\ NearH-FarS \\ NearH-NearS \end{array} \begin{array}{c} \begin{array}{ccccccccc} r1 & r2 & r3 & r4 & r5 & r6 & r7 & r8 & r9 \end{array} \\ \begin{pmatrix} N & N & S & N & S & N & N & H & H \\ N & S & S & S & S & N & S & S & H \\ N & N & S & H & H & H & H & H & H \\ N & S & S & S & S & H & H & H & H \end{pmatrix} \end{array}$$

Types $r1$, $r3$ and $r9$ are always-takers for treatment N, S and H, respectively. Response types $r2$ and $r6$ can be characterised as agents with mild preferences for H and S, respectively. These types would choose college S or H only when they live close to it but not otherwise. Types $r4$ and $r7$ are agents who care about the distance from college and have marginal preferences for the field of study. These types choose to go to the nearest college (if there is one near), regardless of the subject and they differ on which subject they choose when both STEM and non-STEM schools are available near them. Finally, types $r5$ and $r8$ have strong preferences for attending college regardless of distance and differ on which subject they choose when both types of schools are close.

Given the matrix of admissible response types in Figure 1, it is easy to show that the shares of agents in each type are all identified.[8] For example, the share of N-always takers (type $r1$) is equal to the share of individuals in the sample who choose N whenever the instrument value is $NearH - NearS$:[9]

$$P(r1) = P(T = N|Z = NearH - NearS) \tag{2}$$

The shares of certain types are identified by comparison of alternative educational choices by given instrument values. For example, the share of $r5$ are identified as the difference between the shares of individuals choosing S conditional on the instrument being $FarH - FarS$ and

[8]This result follows directly from Corollary C-1 in Heckman and Pinto (2018).

[9]We indicate with $P(r)$ the probability that an agent is of type r or the share of types r in the population.

$NearH - FarS$:

$$P(r5) = P(T = S|Z = FarH - FarS) - P(T = S|Z = NearH - FarS) \tag{3}$$

Average potential outcomes by response types can be identified in a similar way, although some will only be identified for combinations of response types. There obviously are no meaningful counterfactual outcomes for the always-takers, who always make the same choices regardless of the instrument. For these types, the only identifiable outcomes are those conditional on their specific choices:

$$E(Y_N|r1) = E(Y|Z = FarH - NearS) \tag{4}$$

$$E(Y_S|r3) = E(Y|Z = NearH - FarS) \tag{5}$$

$$E(Y_H|r9) = E(Y|Z = FarH - NearS) \tag{6}$$

For the other response types, identification is obtained in a similar way to the sample shares, with the additional complication that now there are three potential outcomes for each type. As an example, let us consider type $r2$. As shown in Figure 1, this type presents potential outcomes associated with treatments N and S only. We can immediately identify the potential outcome associated with treatment N. To see this, let us rewrite the observable average outcome, conditional on $T = N$ and $Z = NearH - FarS$, as the weighted average of the corresponding potential outcomes of types $r1$ and $r2$, who are the only agents choosing N when the instrument is $NearH - FarS$:

$$E(Y|T = N, Z = NearH - FarS) = \frac{E(Y_N|r1)P(r1) + E(Y_N|r2)P(r2)}{P(r1) + P(r2)} \tag{7}$$

Hence, $E(Y_N|r2)$ is identified as follows (recall that the $P(r)$ are identified for all r):

$$E(Y_N|r2)P(r2) = E(Y|T = N, Z = NearH - FarS)(P(r1) + P(r2)) \tag{8}$$

$$- E(Y|Z = FarH - NearS)P(r1) \tag{9}$$

where we have used equation 4 above.

Unfortunately, without further assumptions it is impossible to identify the alternative potential outcome for type $r2$, namely $E(Y_S|r2)$. We make progress on this issue by exploiting the results we obtain on the distribution of response types. As we discuss more in detail in Section 5.2, we find that the share of the response type $r4$ is zero. The point estimate is actually negative, very close to zero and not significantly different from zero. Given these findings, we argue that it is reasonable to also eliminate $r4$ from the matrix of admissible types. Although this type is indeed admissible according to our choice model, the data tell us that there are no agents in this group.

Having eliminated one additional response type, we can identify a larger number of average potential outcomes. The following Table 1 shows the quantities that we can identify and estimate. In Appendix 7 we show how each of them is computed.

Table 1: Identifiable mean potential outcomes

<table>
<tr><th></th><th>$r1$</th><th>$r2$</th><th>$r3$</th><th>$r5$</th><th>$r6$</th><th>$r7$</th><th>$r8$</th><th>$r9$</th></tr>
<tr><td>$E(Y_N|r)$</td><td>✓</td><td>✓</td><td>✗</td><td>✗</td><td>✓</td><td>✓</td><td>✗</td><td>✗</td></tr>
<tr><td>$E(Y_H|r)$</td><td>✗</td><td>✗</td><td>✗</td><td>✓</td><td colspan="2">✓</td><td>✓</td><td>✓</td></tr>
<tr><td>$E(Y_S|r)$</td><td>✗</td><td>✓</td><td>✓</td><td>✓</td><td>✗</td><td colspan="2">✓</td><td>✗</td></tr>
</table>

As indicated in the table, the average Y_H are identified for types $r6$ and $r7$ jointly and, similarly, the average Y_S is identified for types $r7$ and $r8$ jointly.

Treatment effects are identified as differences in average potential outcomes for the same response types. Hence, the treatment effects that we can identify are the following:

- $E(Y_S - Y_N|r2)$,
- $E(Y_S - Y_H|r5)$,
- $E(Y_H - Y_N|r6, r7)$.

In Section 5.4 we show that the quantities that we can identify are sufficient to compute the effects of several interesting hypothetical policies that expand the supply of higher education across fields.

4 Data

We combine two sources of data for our empirical exercise. The first is the official *Italian Labour Force Survey (LFS)* of the years 2008-2020.[10] The survey is conducted by the Italian National Statistical Office (ISTAT) and contains information about basic demographics and a variety of important outcomes, such as employment status, labour market participation, earnings and a set of indicators about the work environment. Important for our purposes, the LFS also includes information on educational attainment. Respondents are asked to report the level of their highest educational qualifications and, for tertiary graduates, also the field of study corresponding to such qualifications. Both the level and the field of educational qualifications are reported according to the official 1-digit ISCED classification.[11] Information on attendance of educational programs is only available for respondents who are enrolled in a program at the time of the interview and we do not know if someone attended some tertiary education but dropped out before obtaining a degree. Hence, we construct our treatment solely on the basis of educational qualifications, which are available for all respondents.The restricted-access version of the LFS, to which we obtained access, also contains the municipality of residence at the time of the survey (not the exact address). We use this information to construct our measure of proximity.[12]

The second database is the *History of Italian Universities (HIU)*, which records the dates of creation of each university and each faculty within each university between 1861 (when the Italian national state was founded) and 2010. This database was assembled for the study Cottini et al. (2019) by manually collecting all the relevant information from historical administrative sources.[13] We use the information in HIU to construct measures of college proximity, as described in Section 4.2.

We restrict the LFS sample of each survey to the cohorts of young adults aged 25-35, and we drop respondents who completed compulsory education after 1990. We also drop respon-

[10]The LFS is also available prior to 2008 but the information about wages is only disclosed since 2008.

[11]ISCED is the UNESCO International Standard Classification of Education.

[12]The data can be accessed by any researcher, both for replication purposes and for new projects, subject to the approval of a research proposal by ISTAT and conditional on accepting a confidentiality agreement. The data can only be used in the data lab of the Italian Statistical Institute. More information is available at this website: https://www.istat.it/en/information-and-services/researchers/laboratory-for-elementary-data-analysis.

[13]We are grateful to all the authors of Cottini et al. (2019) for allowing us to use their data.

dents who report being full-time students. The resulting sample includes only individuals who have completed their education and can be interpreted as a repeated cross-section of similar cohorts. We restrict the sample to those choosing compulsory education after 1990 because there is limited geographical variation in college proximity before the mid/late-1980s, when most universities were concentrated in the large cities and there was almost no supply of higher education in smaller urban centres.

Our largest final sample consists of over 230,000 individuals for whom we can observe several labour market outcomes. For all the individuals in this sample, we can define a dummy equal to one if the person is employed (in the week before the interview), and zero otherwise. For those who are in dependent employment (about 149’000 observations), we also have information on (net) monthly wages (with some missing values) and weekly hours of work.[14] In addition, we exploit the richness of the LFS to investigate indicators of non-monetary working conditions. We consider the following: a dummy equal to one if the respondent reports having some responsibility to coordinate the work of others (zero otherwise) and a dummy equal to one if the person works during the night or during the weekend shifts (in the four weeks prior to the interview). Both these indicators are available for all employed respondents but with some missing values. Table 2 reports some basic descriptive statistics on these outcomes as well as other variables of interest in our analysis, broken down by gender.

[14]Wages refer to the base salary earned the month prior to the interview. We express wages at constant 2021 prices.

Table 2: Summary statistics

	All	Women	Men
	Panel A: Outcomes		
Employment [a]	0.651	0.559	0.745
Valid obs.	230,947	116,684	114,263
Earnings [b]	1,209.32	1,087.23	1,310.246
	(452.01)	(434.22)	(441.41)
Valid obs.	119,514	54,086	65,428
Hours of work[c]	37.58	33.94	40.37
	(10.46)	(10.92)	(9.16)
Valid obs.	149,439	64,909	84,530
Night/Weekend shifts[d]	0.504	0.481	0.522
Valid obs.	149,836	65,082	84,754
Responsibilities[e]	0.135	0.114	0.152
Valid obs.	125,621	56,420	69,201
	Panel B: Controls		
Female	0.505	-	-
Age [f]	29.99	30.04	29.96
	(3.05)	(3.04)	(3.05)
Metropolitan area [g]	0.240	0.240	0.240
Urban density:[h]			
High	0.294	0.296	0.292
Medium	0.475	0.475	0.475
Low	0.231	0.229	0.233
N. observations	230,947	116,684	114,263

[a] Dummy indicator equal to 1 if the respondent is employed (either as an employee or as a self-employed).
[b] Monthly earnings (in the month prior to the interview) at 2021 prices. This variable is only available for dependent employees.
[c] Weekly hours of work (in the week prior to the interview). This variable is available for all employed respondents.
[d] Dummy indicator equal to 1 if the respondent indicates doing either night or weekend shifts (or both in the 4 weeks prior to the interview. This variable is available for all employed respondents.
[e] Dummy indicator equal to 1 if the respondent indicates coordinating the work of others. This variable is available for all employed respondents.
[f] In years.
[g] Dummy indicator for residents in municipalities that are administratively included in major urban agglomerations. There are 14 such agglomerations in the country, including Rome, Milan, Naples, etc.
[h] ISTAT definition based on population density by census tract.
Standard deviations in parenthesis (only for non-dichotomous variables).

About 65% of the individuals in our sample were employed at the time of the interview, with a large difference of about 20 percentage points across genders. A substantial gender gap of about 20% also appears in wages and hours worked. About half of the employed respondents report working in the evenings/nights (8pm-5am) or weekends (Saturday or Sunday), with a slightly higher incidence among men. Approximately 13% of the employed report having some responsibility in coordinating the work of others and men appear to be significantly more likely to do so (15% versus 11%). About half of the sample is composed of women and the average age is approximately 30 years, consistent with our focus on young cohorts.

For the validity of our instrumental variable strategy, it will be important to separate the large urban agglomerations from smaller cities and rural areas. For this reason, we collected detailed information on urban density from the National Statistical Office, which classifies the census tracts of the LFS respondents into one of three categories corresponding to high, medium and low urban density. Moreover, the municipalities around the country's largest urban centres (e.g. Rome, Milan, etc.) manage certain administrative activities jointly. These aggregations of municipalities are called *metropolitan areas* and in our data we create an indicator that takes the value of 1 if the respondent resides in a municipality that is part of one such areas (zero otherwise). The means of these indicators of urban density are also reported in Table 2.

4.1 Definition of STEM

Despite the lively debate on STEM education, there is no consensus on what exact disciplines should be considered STEM (Granovskiy, 2018). In the economics literature, the adopted definition often depends on the granularity of the available information about degree types. The LFS data we use in this paper only report the nine broad fields of the official 1-digit ISCED (1997) classification of fields of study.[15] Accordingly, we adopt a rather conservative classification of STEM and non-STEM fields of study, as follows (STEM fields in bold):

[15] The HIU database contains information at a much higher degree of granularity but it would be impossible to link this information with the degrees of the LFS respondents.

Table 3: ISCED (1997) classification of fields of study

Code	Definition	STEM
1	General programmes	No
2	Teacher training and education science	No
3	Humanities, languages and arts	No
4	Social sciences, business and law	No
5	**Science, mathematics and computing**	**Yes**
6	**Engineering, manufacturing and construction**	**Yes**
7	Agriculture and veterinary	No
8	Health and welfare	No
9	Services	No

Eventually, we consider STEM only the broad fields of "*Science, mathematics and computing*" and "*Engineering, manufacturing and construction*". This is broadly consistent with many other studies in the literature, although some categorize as STEM also selected degrees in the areas of agriculture&veterinary, medicine or economics.[16] For the interpretation of our findings, it is important to keep in mind that the group of non-STEM subjects includes many popular ones with relatively high labour market returns, such as law, medicine, management or economics.

We classify the individuals in our sample into three groups based on their highest educational qualifications. The first group is composed of respondents whose highest qualification is lower than tertiary (ISCED level 4 and lower). Respondents who report a tertiary educational qualification as their highest qualification (ISCED level 5 and higher) are also asked about the field of study and we can thus classify them into either STEM and non-STEM graduates according to the definition above. Table 4 reports the distribution of individuals in our sample into the resulting three categories.

[16]In Appendix B (Table B-9) we replicate some of our main findings using an extended definition of STEM that includes ISCED code 7 "*Agriculture and veterinary*".

Table 4: Distribution of educational outcomes

	All	Women	Men
No college (N)	0.764	0.710	0.820
non-STEM college (H)	0.175	0.238	0.110
STEM college (S)	0.061	0.053	0.070
N. observations	230,947	116,684	114,263

Over 76% of the individuals in our sample do not have a university degree, with important differences across genders. The share of women with a college education is almost 10 percentage points higher than that of men. Among the graduates, the large majority hold a non-STEM degree, particularly among women. Only 5% of women and 7% of men in our data hold a STEM degree. This is relatively low compared to other countries in Europe. Using the European Labour Force Surveys, which allow computing comparable estimates for many countries, we found that in 2020 the share of STEM graduates among the 25-35 years old varied from 6% in France to 14.9% in Sweden, with an average of about 10%. Italy is also among the countries with the lowest shares of overall graduates (about 29% against an average of 39%). So, the Italian context is particularly interesting, as there seems to be ample scope for expanding the pool of tertiary-educated individuals, both in STEM and non-STEM subjects.

4.2 Measuring college proximity

We combine the LFS and HIU databases to construct indicators of proximity to STEM and non-STEM schools for all LFS respondents at the time of completing compulsory education, which in our sample varies between 14 to 16 years old across birth cohorts.[17] We compute proximity based on the province of residence at the time of the survey. Ideally, we would like to use residence at the time when the decision to go to college was taken but unfortunately, this information is not available in the LFS. Given the low internal mobility in Italy, also among graduates, we believe that this is a minor problem but we return to this point later in this section

[17]Minimum compulsory schooling age was 10 (5 years of primary school) until 1923, then it was increased to 14 (completion of lower secondary school, i.e. 5 years of elementary school plus 3 years of lower secondary school) but it is only in the late 1960s that it was truly enforced. In 2003 compulsory schooling requirements were further extended to completing 12 years of schooling by the age of 18 (normally fulfilled at age 16).

and also in the robustness checks of Section 6.

In our preferred specification, a respondent is considered to be near a STEM school if there existed a university with a STEM faculty in her province of residence in the year she completed compulsory education. Similarly, proximity to a non-STEM school indicates that there existed a university with a non-STEM faculty in the province of residence in the year of completing compulsory education.

We use the information on proximity to college to construct a 4-valued discrete variable, which we will later use as an instrument. The four values are coded as follows:[18]

1. far from both non-STEM and STEM college, i.e. there was no university in the province of residence of the respondent in the year she completed compulsory schooling ($FarH - FarS$);

2. near non-STEM and far from STEM college, i.e. there was at least one university with a non-STEM faculty and no university with a STEM faculty in the province of residence of the respondent in the year she completed compulsory schooling ($NearH - FarS$);

3. far from non-STEM and near to STEM college, i.e. there was no university with a non-STEM faculty and at least one university with a STEM faculty in the province of residence of the respondent in the year she completed compulsory schooling ($FarH - NearS$);

4. near non-STEM and near STEM college, i.e. there was both one (or more) non-STEM and one (or more) STEM faculty (possibly but not necessarily at the same university) in the province of residence of the respondent in the year she completed compulsory schooling ($NearH - NearS$).

[18]For simplicity, we use the notation *NearH* and *FarH*, where *H* stands for Humanities, to indicate proximity to non-STEM schools.

Table 5: Distribution of college proximity

	All	Women	Men
$FarH - FarS$	0.324	0.325	0.323
$FarH - NearS$	0.018	0.017	0.018
$NearH - FarS$	0.084	0.084	0.085
$NearH - NearS$	0.574	0.574	0.574

Table 5 shows the distribution of the resulting indicator of college proximity in our sample. About one-third of the individuals do not have any institution of higher education in their province of residence and over 50% have both STEM and non-STEM schools. The rest of the sample is composed of 8.4% of respondents who only have a non-STEM school in proximity to their residence and approximately 2% who only have a STEM school. The results are similar across genders. The distribution of proximity reflects two important phenomena. The first is the concentration of the population in the urban centres, where both STEM and non-STEM schools are often present, producing a large share of individuals in the $NearH - NearS$ group. The second is the tendency of smaller (and newer) universities to only have non-STEM schools, as STEM schools are generally more expensive (as they require larger investments, mainly for scientific laboratories). As a result, the group $NearH - FarS$ is more numerous than the $FarH - Near$.

Table 6: College choices by college proximity

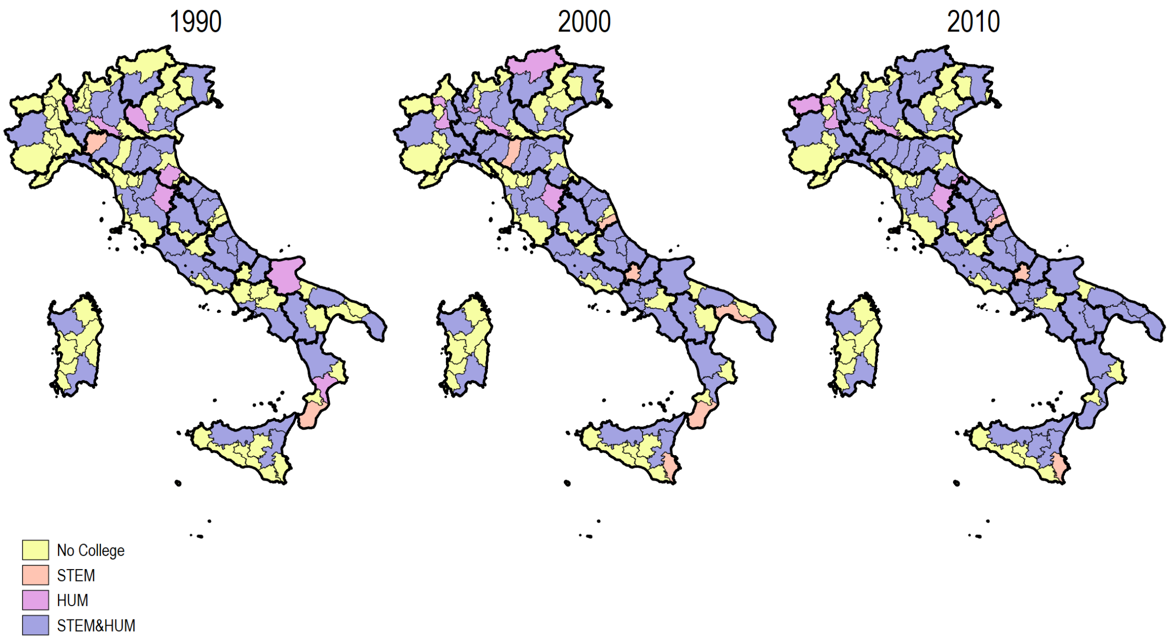
	No college (N)	non-STEM college (H)	STEM college (S)
		Panel A: All	
$FarH - FarS$	0.790	0.156	0.054
$FarH - NearS$	0.787	0.154	0.059
$NearH - FarS$	0.763	0.179	0.058
$NearH - NearS$	0.749	0.185	0.065
Total	0.764	0.175	0.061
		Panel B: Women	
$FarH - FarS$	0.738	0.215	0.046
$FarH - NearS$	0.709	0.224	0.057
$NearH - FarS$	0.704	0.253	0.043
$NearH - NearS$	0.694	0.248	0.057
Total	0.710	0.238	0.053
		Panel C: Men	
$FarH - FarS$	0.843	0.095	0.062
$FarH - NearS$	0.830	0.102	0.069
$NearH - FarS$	0.822	0.112	0.066
$NearH - NearS$	0.807	0.118	0.075
Total	0.820	0.110	0.070

Table 6 reports the joint distribution of education and college proximity and it shows that college proximity matters for college choices.

The share of individuals without college education increases by almost 4 percentage points between those who live far from both types of schools and those who live close to both. Similarly, the share of those with STEM education is 5.4% among people living far from all colleges and it increases to 6% when a STEM college exists in the province. In Section 5.1 we provide further evidence in support of the validity of the instrument.

The time variation in college proximity is mostly generated by the creation of new universities and new schools within existing universities. The maps in Figure 2 describe the expansion of the supply of tertiary education across Italy over the period 1990-2010, i.e. the years that are relevant for our empirical exercise.

Figure 2: Expansion of tertiary education institutions



The figure shows snapshots of the distribution of tertiary education institutions across Italian provinces at three points in time: 1990, 2000 and 2010. A lot of the variation happened in the earlier period, between 1990 and 2000, and was relatively uniformly distributed across the country.

Overall, 12 new universities were created between 1990 and 2010: 5 of them included both STEM and non-STEM schools since their foundation, another 5 started only with non-STEM schools and the remaining 2 started only with STEM schools.[19] In total, 32 new STEM schools (19 in existing universities) and 117 (82 in existing universities) new non-STEM schools were created between 1990 and 2010.

As a result of this process, the total number of provinces with no university decreased from 60 to 41 (over a total of about 100) and the number of provinces with both STEM and non-

[19]In many cases, these new universities were born as administrative re-groupings of one or more pre-existing schools that were previously associated with other older universities. For example, the University of Turin started teaching some classes in other smaller cities outside Turin already during the 1980s. The offer of such peripheral classes continued to expand until in 1998 it was decided to regroup them under the new Università del Piemonte Orientale. It was also often the case that the creation of a new university involved both the re-grouping of existing schools and the creation of new ones.

STEM schools increased from 41 to 59. This is a massive expansion. While in 1990 63.6% of Italians aged 18-19 (the age when the decision to go to university is usually made) lived in a province offering both STEM and non-STEM tertiary education, this figure increased by more than 10 percentage points and reached 76.9% in 2010. Conversely, the percentage of Italians in the same age group living in a province without any university decreased from 27.3% in 1990 to 19.3% in 2010.

Before concluding this section, it is worth discussing some of the choices we made in the construction of the indicator of college proximity. First, our indicator is based on the existence of STEM and non-STEM schools at the time when the individuals in our sample completed compulsory education. The obvious alternative would be to use age 18, which is when students normally make college choices. As a robustness check, we replicate our results using this alternative definition, and the results are largely unchanged (see Table B-10 in Appendix B). We prefer using compulsory schooling in our main specification because staying in school beyond the compulsory age is itself an endogenous choice.

Second, we define geographical proximity based on provinces. Italian provinces are intermediate administrative units between the larger regions (there are 20 regions in the country) and the smaller municipalities (over 8,000). For large business centres, like Rome or Milan, the province is smaller than the local labour market, whereas for smaller peripheral cities the local labour market expands across the province. The total number of provinces in Italy varied slightly over time, as some new provinces were created and others were merged. Over the period 1990-2010, the number of provinces ranged between 104 and 107. We believe that provinces are the most appropriate geographical unit for our empirical exercise because they are the administrative entities responsible for the provision of many relevant public services, notably public transport. It is normally much easier to commute within than across provinces.

In the robustness checks (Section 6), we replicate our main results using the official local labour markets instead of provinces to construct college proximity (see Table 17). We find that our findings are not particularly sensitive to the choice of the geographical unit used to measure distances. Furthermore, we experiment with a continuous measure of college proximity based on travel times between the centroids of the municipalities of residence and the municipalities of the universities/schools and we produce treatment effects based on the methodology of Mountjoy (2022) (see Table 18 in Section 6). Despite the differences in the methodologies, the results are broadly consistent with our main findings. We prefer the discrete indicator of proximity because continuous distances can be imprecise. Recall that in the LFS data we observe the municipality of residence, not the exact residential address, and in the HIU data, we observe the addresses of the university headquarters not those of the exact teaching locations (many universities operate multiple locations).

A final concern regards the use of information about residence at the time of the survey to compute proximity to colleges. Of course, it would be preferable to use residence at the time of completing compulsory education or at the time when college decisions were made. Unfortunately, we do not have this information in our data. Given the low geographical mobility of Italians, we believe that the distortion introduced by this data limitation should be minor.

Most students in our main sample graduated in the 1990s and early 2000s and official demographic statistics indicate that during this period only about 2% of Italians changed province of residence from one year to the next. Internal mobility is also low among graduates, with only about 16% of them reporting a different province of residence before university enrollment and 4 years after graduation.[20] Nevertheless, official statistics derived from the administrative records of the universities indicate that about 45% of students come from a province that is different from the location of the university. This suggests that students are responsive to the geographical proximity of universities when they make their attendance decisions but they often go back to their areas of origin once they complete their studies.

In the robustness section (Section 6) we return to this point and present empirical evidence supporting our intuition that the bias introduced by our use of residence at the time of the survey does not significantly affect our main results (see Table 16 and also Table B-8 in Appendix B).

[20]This statistics comes from the "Survey of University Graduates' Vocational Integration" conducted by the National Statistical Office.

5 Empirical results

This section presents three sets of results from our empirical analysis. First, in Section 5.2 we report the estimates of the distribution of response types. This is an important contribution because, as far as we know, this evidence is rarely presented in studies based on instrumental variables, despite being crucial for a correct interpretation of the estimated effects. Moreover, the results on the distribution of response types are particularly useful for us because they indicate that one additional type can be assumed to have zero mass. This allows us to eliminate one more column from the matrix of admissible response types, thus permitting the identification of additional counterfactual outcomes, which constitute our second set of results (Section 5.3). Finally, in Section 5.4 we use the previous estimates to evaluate the effects of a selected set of hypothetical educational reforms expanding the supply of tertiary education.

Before proceeding with these results, in Section 5.1 we present evidence in support of the validity of college proximity as an instrument for college choices.

The estimates that we produce in this section are derived by conditioning the response probabilities and the outcome variables on a set of controls. This set includes gender (when the results are produced for both genders jointly), age (and its square), dummies for the degree of urbanization of the municipality, survey dummies, regional dummies and dummies for metropolitan areas (see Section 4 for a detailed description of the data). More precisely, when we compute the probabilities of each response type, we use the probabilities of treatment conditional on the controls (and the values of the instruments). For example, equation (2) from Section 3 becomes:

$$P(r1) = P(T = N|X, Z = FarH - NearS) \tag{10}$$

where X is the set of controls. Practically, we construct the probabilities $P(T|X, Z)$ as the predictions from a simple multinomial logit model, with the three treatments as outcomes and dummies for the instrument values and the controls as explanatory variables. We show the estimates of such a multinomial model in Section 5.1 (Table 7).

Similarly, we use the expected outcomes conditional on the instrument and controls when computing potential outcomes. For example, equation (4) becomes:

$$E(Y_N|r1) = E(Y|X, Z = FarH - NearS) \tag{11}$$

We construct the conditional outcomes $E(Y|X, Z)$ as the predictions from simple linear or logit models (depending on whether the outcomes are continuous or dichotomous) with the outcomes as dependent variables and dummies for the instrument values and the controls as explanatory variables. We estimate one such model separately for each outcome that we consider.[21]

5.1 Instrument diagnostic

In this section, we present evidence supporting the validity of college proximity as an instrument for education attainment.

We start with relevance and we investigate the extent to which variation in geographical proximity is associated with college choices. Table 7 reports the results of a simple multinomial logit model for the three educational choices that we consider. The main explanatory variable of interest is our discrete indicator of college proximity and the model includes our standard set of controls. The baseline outcome is "No college" and the excluded value of the instrument is $FarH - FarS$. We estimate the model both for the entire sample and for men and women separately.

[21] We do not report the coefficient estimates from these models but they are available upon request.

Table 7: Multinomial logit for college choice

	All		Women		Men	
Type of college	non-STEM	STEM	non-STEM	STEM	non-STEM	STEM
FarH-NearS	0.003	0.310***	0.001	0.301***	0.014	0.314***
	(0.047)	(0.073)	(0.059)	(0.110)	(0.081)	(0.098)
NearH-FarS	0.060**	-0.049	0.054*	-0.119*	0.066	-0.003
	(0.024)	(0.040)	(0.030)	(0.062)	(0.042)	(0.051)
NearH-NearS	0.116***	0.171***	0.117***	0.194***	0.117***	0.152***
	(0.016)	(0.024)	(0.019)	(0.037)	(0.028)	(0.032)
Female	0.934***	-0.125***	-	-	-	-
	(0.012)	(0.018)				
Age	0.149***	0.106*	0.112**	0.039	0.212***	0.156*
	(0.040)	(0.062)	(0.049)	(0.094)	(0.067)	(0.083)
Age sq.	-0.003***	-0.003**	-0.003***	-0.004***	-0.003***	-0.003**
	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)
N. observations	230’090		115’919		114’171	

Multinomial logit coefficients. ”No college” is the baseline outcome and ”$FarH - FarS$” is the excluded value of the proximity indicator. Additional controls include urbanization of the municipality, survey dummies, regional dummies and dummies for metropolitan areas. Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1.

Results indicate that geographical proximity to college operates as expected. Compared to being far from any college, the probability of going to a STEM college increases when one lives close to one. The same holds for non-STEM colleges and the results hold for both men and women. We also find that women are less likely to graduate in STEM, which appears to be more common among the younger cohorts.

Table 8: Predicted probabilities of college choices by college proximity

	P(No college (N))	P(non-STEM (H))	P(STEM (S))
		Panel A: All	
$FarH - FarS$	0.794	0.152	0.055
	(0.002)	(0.002)	(0.001)
$FarH - NearS$	0.778	0.149	0.073
	(0.007)	(0.006)	(0.005)
$NearH - FarS$	0.788	0.160	0.052
	(0.003)	(0.003)	(0.002)
$NearH - NearS$	0.771	0.166	0.063
	(0.001)	(0.001)	(0.001)
		Panel B: Women	
$FarH - FarS$	0.731	0.222	0.047
	(0.003)	(0.002)	(0.001)
$FarH - NearS$	0.719	0.219	0.062
	(0.011)	(0.010)	(0.006)
$NearH - FarS$	0.726	0.233	0.041
	(0.005)	(0.005)	(0.002)
$NearH - NearS$	0.705	0.241	0.054
	(0.002)	(0.002)	(0.001)
		Panel C: Men	
$FarH - FarS$	0.839	0.099	0.062
	(0.002)	(0.002)	(0.001)
$FarH - NearS$	0.819	0.098	0.082
	(0.009)	(0.007)	(0.007)
$NearH - FarS$	0.834	0.105	0.061
	(0.004)	(0.003)	(0.002)
$NearH - NearS$	0.821	0.109	0.070
	(0.002)	(0.001)	(0.001)

Predicted probabilities based on the multinomial logit estimates of Table 7 computed at the mean of all covariates. Standard errors (obtained by delta-methods) in parenthesis.

To give a sense of the magnitudes of the differences induced by college proximity, Table 8 shows the predicted probabilities of each educational choice based on the estimates of our multinomial model for each value of the proximity indicator and setting all other explanatory variables at the mean of the sample. Predicted probabilities indicate that college proximity can change the probability of obtaining a STEM degree by about 2 percentage points, from 5.5% when living far from a STEM school and near a non-STEM school to 7.3% when living close to STEM and far from non-STEM. This is a sizeable effect of about 40% and it is larger for women (50%) than men (33%), and highly statistically significant[22].

Having established the relevance of the instrument, we now turn to its exogeneity and we present two pieces of evidence to support it. In our setting, there are two obvious threats to the exogeneity of college proximity. First, one may worry that STEM and non-STEM schools may be more likely to be present in areas with favourable labour markets for graduates in their fields. Second, one may also worry that individuals who are more interested and could benefit more from college education in a specific field may move to areas offering college education in such field. Of course, instrument exogeneity cannot be directly tested but we can provide evidence suggesting that neither of the above concerns is particularly worrisome in our setting.

In Table 9 we use aggregate historical data at the provincial level to investigate whether universities are more likely to be created in areas with stronger labour markets. For this exercise, we combine the HIU data, which allows us to identify for every province and every year whether a STEM and/or a non-STEM school was present, with official statistics about the time series of employment by province and sector. The coefficients in the table are obtained from regressions with a dummy indicator for the presence of a university in the province as the dependent variable and the 10-year lagged employment indicators on the right-hand-side. We lag employment by 10 years because the process of creating new universities or faculties is a long one. In Panel A of Table 9, we use only the overall employment rate and the shares of employment in manufacturing and services (agriculture is the residual group), which are available for the entire period. In Panel B, we use the employment rate of people in the age group 15-29 and the share of employment in high-skilled occupations, which are presumably particularly relevant for university graduates but are available only for more recent years.[23]

Results show that, if anything, universities are more likely to be created in provinces where employment stagnates but the effects are quantitatively small and often statistically insignificant. The largest effect that we obtain (column 1, Panel A) indicates that a 1 percentage point increase in the employment rate of the province (10 years earlier) is associated with about 1 percentage point lower probability of seeing a new STEM school (over an average of 32.7%).

[22]This is broadly consistent with the results obtained by Fabre (2023) for France which show that students' demand for higher education is elastic to geographic proximity.

[23]The share of employment in high-skilled occupations corresponds to ISCO groups 1 and 2, i.e. managers and professionals.

Table 9: Lagged employment and college expansion

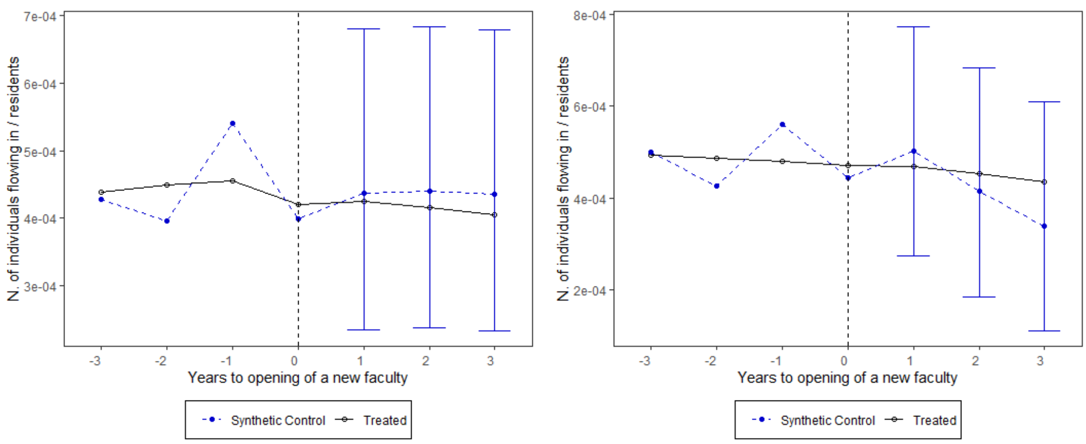
	Type of college in the province		
	STEM school	Non-STEM	STEM or non-STEM
		Panel A	
Employment rate[a]	-0.009***	-0.006*	-0.006
	(0.003)	(0.004)	(0.004)
Manufacturing employment[a,b]	0.000	0.004**	0.004**
	(0.002)	(0.002)	(0.002)
Service employment[a,b]	-0.001	-0.002	-0.002
	(0.004)	(0.005)	(0.005)
Observations	660	660	660
Mean of dep. variable	0.327	0.383	0.400
		Panel B	
Employment rate age 15-29[a]	-0.006	-0.005	-0.012
	(0.010)	(0.013)	(0.013)
High-skill employment[a,b,c]	0.007	-0.002	-0.003
	(0.019)	(0.023)	(0.023)
Observations	220	220	220
Mean of dep. variable	0.432	0.523	0.532

[a] Lagged 10 years.
[b] Share of total employment.
[c] Share of employment in ISCO occupations 1 (Managers) and 2 (Professionals).
Dependent variables are dummy indicators for the presence of a college (generic or of a specific type) in the province. The set of controls includes decade and province dummies. Data cover the period 1951-2011. *** p<0.01, ** p<0.05, * p<0.1

These results are consistent with the findings in Cottini et al. (2019), who suggest that the creation of new universities or faculties is more commonly driven by local political factors than considerations related to the labour market.

Figure 3: University expansion and migration flows

(a) Women, age 18-39 (b) Men, age 18-39



Synthetic controls constructed using provincial per-capita GDP. The outcome variables are the ratios of new residents in the province over the number of residents. There are 6 new creations of universities in the sample period, 2002-2008.

The second piece of evidence that we present to support the exogeneity of the instrument is in Figure 3. We investigate whether the creation of a new university in a province produces an increase in the inflow of young residents. We do so using a synthetic control approach and exploiting the 6 episodes of university expansions that we observe in the HIU data during the period 2002-2008, the only years when we also have information on new and existing residents by province from the National Statistical Institute. For each of the 6 treated provinces, we create synthetic controls using as donors the provinces that never experienced the creation of a new university over the sample period and matching them based on per-capita income.[24] The outcome variables that we consider are the ratios of new residents aged 18-39 over the stock of existing residents in the same age group, separately for women and men.[25] Results indicate no detectable difference in such flows between treated and controls, suggesting that, in the Italian context, people do not relocate to areas where new universities are created. Although

[24]Adding matching variables further reduces precision.

[25]Results for all age groups are qualitatively similar and are available from the authors upon request.

this analysis is somewhat under-powered due to the small number of units (there are a total of 107 provinces and only 6 are treated), there does not seem to be any trend in the differences between treated and controls, regardless of the large standard errors. This result is consistent with the low propensity to move of the Italian population.

5.2 Response Types

Table 10 reports the estimated shares of each response type, both for the entire population and separately by gender. As already discussed in Section 3, we set the population shares of type $r4$ to zero. The point estimate of the share of individuals in this type is negative and equal to -0.008 (-0.0013 for women and -0.004 for men).[26] Hence, we assume that there are no individuals in this group in our population of interest and we rescale the shares of all other types to guarantee that they sum to one.

We construct confidence intervals by replicating our entire estimation procedure using 200 bootstrapped samples stratified by survey year. We use the same bootstrapping procedure to compute confidence intervals for all the estimates reported in Sections 5.3, 5.4 and 6.[27]

Table 10: Response type probabilities

	$r1$	$r2$	$r3$	$r5$	$r6$	$r7$	$r8$	$r9$
Total	0.765	0.017	0.051	0.003	0.004	0.008	0.004	0.148
	[0.755,0.773]	[0.012,0.023]	[0.048,0.054]	[0.000,0.006]	[0.000,0.015]	[0,0.019]	[0.000,0.014]	[0.138,0.157]
Women	0.697	0.021	0.041	0.005	0.005	0.008	0.005	0.217
	[0.681,0.708]	[0.012,0.029]	[0.037,0.045]	[0.001,0.009]	[0.000,0.019]	[0.000,0.024]	[0.000,0.019]	[0.200,0.229]
Men	0.811	0.013	0.060	0.001	0.003	0.011	0.003	0.097
	[0.799,0.821]	[0.004,0.02]	[0.056,0.065]	[0.000,0.004]	[0.000,0.014]	[0.000,0.025]	[0.000,0.013]	[0.087,0.108]

The table presents the estimated response type probabilities, conditional on gender, age (and age squared), urbanization of the municipality, survey dummies, regional dummies and dummies for metropolitan areas. Bootstrapped 90%-confidence intervals in squared parenthesis (200 replications).

The first important finding emerging from Table 10 is the predominant incidence of always-takers. For readability, we group them in Table 11. When considering men and women together, the total share of always-takers of any educational choice is 96.4%. This implies that the share of individuals whose educational choices might be influenced by policies manipulating distance to college (or policies affecting college costs in a comparable way), i.e. the various types of

[26] Our non-parametric estimation procedure does not guarantee that the estimated population shares are positive.

[27] We prefer reporting confidence intervals instead of standard errors because the distribution of the estimates (either small-sample or asymptotic) is unknown.

compliers, is around 3.6%. Interestingly, the incidence of always-takers is significantly higher among men (96.8%) than among women (95.5%), suggesting that the latter are more responsive to interventions that modify college proximity.

Table 11: Shares of always-takers by gender

	Total	Women	Men
N always-takers ($r1$)	0.765	0.697	0.811
H always-takers ($r9$)	0.148	0.217	0.097
S always-takers ($r3$)	0.051	0.041	0.060
Total	0.964	0.955	0.968

The low share of compliers is common in studies using instrumental variables. For example, the seminal study by Card (1995) uses data on US men who made their college decisions around the mid-60s and finds that those whose decisions were influenced by college proximity, the compliers, were about 9% of the total. In the well-known book by Angrist and Pischke (2009), the authors review some influential papers and document that the share of compliers is often very low, in the order of 1% to 30% of the treated population and 4% to 90% of the untreated population. In our setting, they are 15% of all graduates.

Going back to Table 10, we also notice that the largest group of switchers is $r2$. These are individuals at the margin of not going to college and going to a STEM college but who never consider going to a non-STEM college. The overall share of this response type is 1.7% but it is higher (2.1%) among women than men (1.3%). Encouraging women to undertake STEM education seems to be an important policy objective for many governments and this result indicates that interventions changing proximity to STEM colleges may be effective at improving gender balance.

Table 10 also suggests that trying to convince people to switch fields of tertiary education is unlikely to produce major changes. The types that are at the margin of STEM and non-STEM education ($r5, r7$ and $r8$) jointly represent about 1.6% of the sample and not all changes in the instrument would make them switch fields.

In Section 5.4, we use these estimates to compute the treatment effects, on both educational choices and outcomes, of several hypothetical reforms expanding the supply of tertiary education.

5.3 Potential outcomes and treatment effects

Our setting also allows the identification of several average potential outcomes, as described in Table 1. In some cases, we can also compare alternative potential outcomes for the same response types and thus compute proper treatment effects. Specifically, for response type $r2$ we can compare the average potential outcome under STEM education and under no college: $E(Y_S|r2) - E(Y_N|r2)$. For response type $r5$, we can compute the treatment effect of STEM vs non-STEM college: $E(Y_S|r5) - E(Y_H|r5)$. Finally, for types $r6$ and $r7$ jointly we can compute the treatment effect of non-STEM education against the counterfactual of no college: $E(Y_S|r6, r7) - E(Y_N|r6, r7)$. These are the only three treatment effects that are identified in our setting.

In this section, we present estimates of the identifiable average potential outcomes and treatment effects for the labour market outcomes presented in Section 4, Table 2. For expositional simplicity, we show here the full set of results for employment and only the identified treatment effects for all other outcomes. The full results for all other outcomes are shown in Appendix B.

Table 12 reports the results for employment, for the entire sample and separately by gender. Given the small size of certain response types, the estimates of certain counterfactual outcomes are imprecise and the confidence intervals span the entire support $[0, 1]$. Nevertheless, for the three types of always-takers ($r1, r3$ and $r9$), the confidence intervals are quite narrow. We estimate that the employment rate of the N-always taker is 69.7% and it increases to 82.9% for the H-always takers and to 87.5% for the S-always takers. Similar patterns are observed by gender.

Table 12: Counterfactual means and treatment effects of employment status

	$r1$	$r2$	$r3$	$r5$	$r6$	$r7$	$r8$	$r9$
				Panel A: Total				
Potential outcomes:								
$E(Y_N\|r)$	0.697	0.365	-	-	0.858	0.903	-	-
	[0.69, 0.70]	[0.00, 0.76]	-	-	[0.00,1.00]	[0.00,1.00]	-	-
$E(Y_H\|r)$	-	-	-	0.769	0.817		0.903	0.829
	-	-	-	[0.00,1.00]	[0.69,0.92]		[0.00,1.00]	[0.81, 0.85]
$E(Y_S\|r)$	-	0.885	0.875	0.381	-	0.601		-
	-	[0.83, 0.94]	[0.85, 0.90]	[0.00, 0.78]	-	[0.19, 0.90]		-
Treatment effects:								
$E(Y_H - Y_N\|r)$	-	-	-	-	-0.181		-	-
	-	-	-	-	[-0.31,-0.08]		-	-
$E(Y_S - Y_N\|r)$	-	0.519	-	-	-	-	-	-
	-	[0.092, 0.908]	-	-	-	-	-	-
$E(Y_S - Y_H\|r)$	-	-	-	-0.338	-	-	-	-
	-	-	-	[-0.51, 0.17]	-	-	-	-
				Panel B: Women				
Potential outcomes:								
$E(Y_N\|r)$	0.594	0.122	-	-	0.853	0.680	-	-
	[0.59, 0.60]	[0.00, 0.43]	-	-	[0.00,1.00]	[0.00,1.00]	-	-
$E(Y_H\|r)$	-	-	-	0.670	0.844		0.680	0.789
	-	-	-	[0.00,1.00]	[0.63,1.00]		[0.00,1.00]	[0.76, 0.82]
$E(Y_S\|r)$	-	0.770	0.826	0.338	-	0.506		-
	-	[0.70, 0.84]	[0.78, 0.86]	[0.00,0.67]	-	[0.00, 0.95]		-
Treatment effects:								
$E(Y_H - Y_N\|r)$	-	-	-	-	-0.153		-	-
	-	-	-	-	[-0.37,0.00]		-	-
$E(Y_S - Y_N\|r)$	-	0.648	-	-	-	-	-	-
	-	[0.34, 0.82]	-	-	-	-	-	-
$E(Y_S - Y_H\|r)$	-	-	-	-0.332	-	-	-	-
	-	-	-	[-1.00, 0.29]	-	-	-	-
				Panel C: Men				
Potential outcomes:								
$E(Y_N\|r)$	0.795	0.854	-	-	0.773	0.971	-	-
	[0.79, 0.80]	[0.00, 1.00]	-	-	[0.00,1.00]	[0.92,1.00]	-	-
$E(Y_H\|r)$	-	-	-	0.752	0.858		0.971	0.903
	-	-	-	[0.00,1.00]	[0.75,0.93]		[0.92,1.00]	[0.87, 0.93]
$E(Y_S\|r)$	-	0.985	0.936	0.361	-	0.782		-
	-	[0.95, 1.00]	[0.91, 0.95]	[0.00,1.00]	-	[0.44, 1.00]		-
Treatment effects:								
$E(Y_H - Y_N\|r)$	-	-	-	-	-0.128		-	-
	-	-		- -	[-0.25,-0.01]		-	-
$E(Y_S - Y_N\|r)$	-	0.245	-	-	-	-	-	-
	-	[-0.041, 0.977]	-	-	-	-	-	-
$E(Y_S - Y_H\|r)$	-	-	-	-0.391	-	-	-	-
	-	-	-	[-1.00, 0.34]	-	-	-	-

All quantities are computed conditional on gender, age (and age squared), urbanization of the municipality, survey dummies, regional dummies and dummies for metropolitan areas. Bootstrapped 90%-confidence intervals in squared parenthesis (200 replications).

The largest group of compliers is $r2$. This group shows higher employment rates when choosing STEM education than when choosing not to acquire any university degree. The treatment effect is equal to 51.9 percentage points for the entire sample and it is substantially larger for women (64.8) than men (24.5).

The other treatment effects that can be estimated are for response type $r5$, and for $r6$ & $r7$ jointly. The $r5$s is the smallest group in our analysis and, not surprisingly, the treatment effects reported in Table 12 are highly imprecise, with confidence intervals that cross the zero line for all samples. The $r6$ and $r7$ also account for a small share of the sample (around 1%) and the confidence intervals around the counterfactual outcomes are large.

Table 13 shows the three treatment effects that are identified in our setting, namely $E(Y_S - Y_N|r2), E(Y_S - Y_H|r5)$ and $E(Y_H - Y_N|r6, r7)$, on all outcomes. Despite some imprecise estimates, several interesting findings emerge.

Response type $r2$ is composed of individuals with weak preferences for STEM education, which they undertake only if its costs are not too high otherwise they prefer not going to any college (see Table 10). This group appears to benefit from STEM education in most dimensions. STEM education generally guarantees to these compliers higher employment opportunities and better working conditions (in terms of shorter working hours, fewer night/weekend shifts, or more job responsibilities), even though it may come at the expense of lower earnings (conditional on working). There also are some interesting gender differences in these effects. The employment effect of STEM education is larger and more precisely estimated for women, while the effect on job responsibility seems to be concentrated mostly on men.

Response types $r5$ can be characterised as individuals with strong preferences for STEM education and against non-tertiary education (they never choose N, under any value of the instrument). Only when STEM schools are far and there is a non-STEM school nearby, then they choose non-STEM. This is the only group of compliers for whom we can compare potential outcomes under STEM and non-STEM education but unfortunately, the associated treatment effects are quite heterogeneous, both across outcomes and genders, and never precisely estimated. As a general tendency, we observe that the choice of STEM vs non-STEM education leads to lower employment and earnings. This may appear surprising but it is important to keep in mind that the category of non-STEM education includes some fields that are generally associated with positive labour market returns, such as law, business, economics or medicine.[28]

Finally, response types $r6$ and $r7$ are individuals who exhibit a low propensity to undertake college education, and, only do so when they live near a school, regardless of the field (see Table 10). For these compliers obtaining a non-STEM degree (compared to not going to university)

[28]See Kirkeboen et al. (2016) for estimates of the labour market returns of these fields based on admission discontinuities.

Table 13: Treatment effects

	$E(Y_S - Y_N \mid r2)$			$E(Y_S - Y_H \mid r5)$			$E(Y_H - Y_N \mid r6, r7)$		
	Total	Women	Men	Total	Women	Men	Total	Women	Men
Employment[a]	0.519	0.648	0.245	-0.338	-0.332	-0.391	-0.181	-0.153	-0.128
	[0.096,0.908]	[0.343,0.816]	[-0.041,0.977]	[-0.512,0.178]	[-1.000,0.296]	[-1.000,-0.344]	[-0.306,-0.076]	[-0.373,0.000]	[-0.251,-0.003]
Earnings[a]	-35.794	-38.448	-355.636	-2331.72	-1341.117	-2151.051	154.154	451.667	151.894
	[-488.480,415.480]	[-615.800,442.840]	[-1690.800,580.710]	[-11938.940,125.895]	[-4475.312,200.572]	[-11941.52,9272.520]	[-607.700,896.150]	[-429.390,1341.950]	[-765.440,922.500]
Hours of work[b]	-7.353	-11.825	-4.696	2.345	17.292	-23.859	-1.777	10.196	-8.103
	[-18.370,1.710]	[-29.820,2.170]	[-26.982,9.628]	[-40.980,46.430]	[-9.320,51.750]	[-105.000,3.230]	[-16.910,12.670]	[-8.300,35.496]	[-24.020,4.320]
Night/Weekend shifts[c]	-0.278	-0.070	-0.386	-0.493	0.427	-0.944	-0.346	-0.255	-0.256
	[-0.740,0.245]	[-0.688,0.297]	[-0.748,0.174]	[-1.000,0.389]	[-0.372,1.000]	[-1.000,-0.563]	[-0.668,0.218]	[-0.791,0.502]	[-0.689,0.368]
Responsibilities[d]	0.253	-0.068	0.391	-0.119	0.117	-0.234	-0.403	-0.012	-0.498
	[0.005,0.415]	[-0.482,0.252]	[0.154,0.609]	[-0.972,0.783]	[-0.587,0.998]	[-1.000,0.333]	[-0.937,0.123]	[-0.445,0.293]	[-0.868,0.002]

[a] Dummy indicator equal to 1 if the respondent is employed (either as an employee or as a self-employed).
[b] Monthly earnings (for both employees and self-employed) in 2021 euros.
[c] Weekly hours (for both employees and self-employed).
[d] Dummy indicator equal to 1 if the respondent indicates doing either night or weekend shifts (or both).
[e] Dummy indicator equal to 1 if the respondent indicates coordinating the work of others.
All quantities are computed conditional on gender, age (and age squared), urbanization of the municipality, survey dummies, regional dummies and dummies for metropolitan areas. Bootstrapped 90%-confidence intervals in squared parenthesis (200 replications).

seems to make it more difficult to find employment but allows accessing better jobs, both in terms of earnings, hours of work and night/weekend shifts (not for job responsibilities).

Of course, these results need to be interpreted with great caution, because of the imprecision of the estimates and also because the underlying nature of the response types is difficult to characterise.

5.4 Simulations of education expansions

The treatment effects reported in Table 13 are interesting per se, but of limited practical use as policies that target specific response types are hard to design, especially since these groups are not observable. However, we can use the treatment effects and the response type probabilities that we produced in the previous sections to estimate the effects of hypothetical reforms that expand the supply of tertiary education.[29]

We start with a focus on STEM education, because of the lively debate, both in academia and in policy circles, suggesting that there might be substantial unmet demand for STEM graduates and that women might be especially discouraged from undertaking this type of studies due to a variety of cultural and institutional constraints. Table 14 presents the estimated impacts of a first hypothetical reform, which creates a new STEM school in any province that does not have one. In our setting, the reform would imply the creation of 48 new STEM schools, with the objective of having (at least) one such school available in each province.

By creating new STEM schools, the reform would affect the educational choices of four groups of individuals. The first group includes individuals in response types $r2$ who reside far from a STEM school, either $FarH - FarS$ or $NearH - FarS$. As a result of the reform, the location status of these people changes to $FarH - NearS$ and $NearH - NearS$, respectively. These individuals change their education choices and decide to acquire a STEM degree instead of not going to college. The second group is composed of the types $r5$ who live near a non-STEM school but far from STEM schools ($NearH - FarS$). The reform changes their location status to $NearH - NearS$, which induces them to switch from non-STEM to STEM. The third

[29]In this section, we abstract from considerations related to the quality of education institutions. Of course, this is an important issue and the expansion of tertiary education may either increase or decrease average quality depending on how it is implemented. Nevertheless, our data and our analysis do not allow us to investigate this issue and we leave it for further research.

group is composed of types $r7$ who are $FarH - FarS$ and they also react to the reform by deciding to attend STEM college instead of not going to college at all. Finally, there are the types $r8$ who are $FarH - FarS$ and they change from non-STEM to STEM college.

Given the orthogonality assumption of the instrument, we can simply compute the sizes of each of these groups by multiplying the population shares of the response types by the corresponding shares of the instrument values. For example, the share of response types $r7$ who live $FarH - FarS$ is equal to the product of the estimated share of types $r7$ (0.008) and the share of those living $FarH - FarS$ (0.324), hence $0.008 \times 0.324 = 0.0026$.

The first row of Table 14 reports the changes in the shares of STEM and non-STEM graduates that the reform would produce. As a result of the switches by the four groups described above, the share of STEM graduates in the population would increase by 1.1 percentage points, which corresponds to an 18% increase over the baseline share of approximately 6.1%. Most of this increase comes from those who choose not to go to college in the absence of the policy but a small share (0.2%) is due to the types $r5$ and $r8$ who switch to STEM from non-STEM. These effects are stronger for women than for men, so the reform would result in narrowing the gender gap in STEM by about one-fifth.

Table 14: Effects of creating a STEM school in every province[a]

	[1] Total		[2] Women		[3] Men	
	ΔS	ΔH	ΔS	ΔH	ΔS	ΔH
Δ Graduates	0.011	-0.002	0.013	-0.002	0.010	-0.001
	[0.008,0.015]	[-0.005,0.000]	[0.008,0.018]	[-0.006,0.000]	[0.006,0.015]	[-0.005,0.000]
	Treatment effects					
	ATE	ATT	ATE	ATT	ATE	ATT
Employment[b]	0.002	0.231	0.004	0.351	0.000	0.040
	[-0.001,0.006]	[-0.073,0.549]	[0.000,0.01]	[0.008,0.679]	[-0.002, 0.003]	[-0.176,0.392]
Earnings[c]	5.019	433.238	0.084	7.622	6.359	568.776
	[-0.214,11.543]	[-30.141,924.727]	[-22.876, 13.076]	[-1471.775,910.148]	[-0.883,15.233]	[-134.358,1236.883]
Hours of work[d]	0.095	8.157	0.09	6.272	0.084	7.347
	[-0.032,0.227]	[-3.359,18.323]	[-0.082,0.306]	[-7.606,20.344]	[-0.245,0.308]	[-29.125,26.826]
Night/Weekend shifts[e]	-0.004	-0.380	-0.004	-0.25	-0.004	-0.349
	[-0.009,0.001]	[-0.789,0.106]	[-0.010,0.002]	[-0.668,0.153]	[-0.009,0.002]	[-0.762,0.227]
Responsibilities[f]	0.006	0.487	0.002	0.151	0.007	0.655
	[0.002,0.009]	[0.290,0.666]	[-0.002,0.007]	[-0.170,0.455]	[0.003,0.011]	[0.446,0.878]

[a] The table reports the estimated effects of a hypothetical reform that creates a STEM school in any province that does not have one.
[b] Dummy indicator equal to 1 if the respondent is employed (either as an employee or as a self-employed).
[c] Monthly earnings (for both employees and self-employed) in 2021 euros.
[d] Weekly hours (for both employees and self-employed).
[e] Dummy indicator equal to 1 if the respondent indicates doing either night or weekend shifts (or both).
[f] Dummy indicator equal to 1 if the respondent indicates coordinating the work of others.
All quantities are computed conditional on gender, age (and age squared), urbanization of the municipality, survey dummies, regional dummies and dummies for metropolitan areas. Bootstrapped 90%-confidence intervals in squared parenthesis (200 replications).

The second panel of Table 14 shows the estimated treatment effects for all the outcomes that we consider. We compute both the average treatment effect (ATE) and the average treatment effect on the treated (ATT). For the computation of the ATE, we simply calculate the treatment effects for the four groups that we described before and whose educational choices changed due to the reform. Considering that the treatment effects are zero for all the non-treated, the ATE is:

$$\begin{aligned} ATE_1 &= P(g1)E(Y_S - Y_N|r2) + P(g2)E(Y_S - Y_H|r5) \\ &+ [P(g3) + P(g4)] \left[E(Y_S|r7, r8) - \frac{P(g3)E(Y_N|r7) + P(g4)E(Y_H|r8)}{P(g3) + P(g4)} \right] \end{aligned} \tag{12}$$

where we indicate with $P(g)$ the shares of the four groups of treated that we described above:

$$
\begin{aligned}
P(g1) =& P(r2)\left[P(Z = FarH - FarS) + P(Z = NearH - FarS)\right] \\
P(g2) =& P(r5)P(Z = NearH - FarS) \\
P(g3) =& P(r7)P(Z = FarH - FarS) \\
P(g4) =& P(r8)P(Z = FarH - FarS)
\end{aligned}
$$

The average treatment effects on the treated (ATT) are computed similarly to the ATE, but averaging over the four groups of treated:

$$ATT_1 = \frac{ATE}{P(g1) + P(g2) + P(g3) + P(g4)} \tag{13}$$

Notice that, although we cannot identify the counterfactual outcome Y_S separately for $r7$ and $r8$, both these types choose S when the reform is implemented and their joint counterfactual is identified. Hence, the effect of the reform for these two types is fully identified, as shown in equation 12.

The results reported in Column [1] of Table 14 show that an expansion of the supply of STEM colleges produces non-negligible effects on employment, earnings and working conditions. In Column [2] we observe large and statistically significant returns for women, who experience an increase in employment of about half a percentage point. This is a large ATE and it is generated by a large positive effect of over 35 percentage points for those in the treated groups (ATT). For women, we also find an increase in hours of work and a generalised improvement in other working conditions. The magnitudes of these effects are large, but in most cases not statistically significant. The effects on earnings are small and insignificant.

In Column [3], we observe positive effects on both employment and income for men, although neither of them is statistically significant. Men also experience an important increase in hours and an improvement in overall working conditions, i.e. a reduction in night and weekend shifts and an increase in the degree of responsibility. This last effect is the only one that is statistically significant.

The estimated effects imply that the gender gap in employment would be reduced by approximately 1 percentage point or 3% over the baseline gap of 33.2%.

As expected, the ATEs are smaller than the ATTs and this is due to the small size of the treated groups. Yet, some approximate cost-benefit analysis suggests that the effects of the reform that we consider can be substantial. Abstracting from the negligible effects on male employment and from any effect on earnings (which are imprecisely estimated), the simple increase in female labour supply can generate approximately 200 million euros per year in additional (private) revenues.[30] On the cost side, we can approximate the cost of operating 48 new STEM schools to be around 75 million euros.[31] So, even considering only the effects on female employment, the reform would generate economic returns equal to over 2.5 times the operating costs of the new schools ($200/75 million = 2.67$) and the income taxes collected on the newly employed women, corresponding to approximately 20 million euros, would cover over one-fourth of the total cost.

[30]There are approximately 3.3 million women aged 25-35 in Italy, so the reform would produce an additional $0.004 * 3.3 million \approx 13'200$ employed women. Assuming they would earn about $15'000$ euros gross per year (corresponding to approximately $1'100$ net per month, matching the descriptive statistics in Table 2), the total (private) returns from the reform would be in the order of $15'000 * 13'200 = 198'000'000$ per year.

[31]We base this calculation on OECD (2019), which reports that the Italian government spends around 5'200 euros per year for each university student (at 2021 prices). Considering that a STEM faculty in Italy has an average 300 students, we obtain the cost reported in the text ($5'200 * 300 * 48 = 74.9$ million). Notice incidentally that the average of 300 students per STEM faculty corresponds surprisingly well to our estimate of 13'200 new employed women ($300 * 48 = 14'400$).

Table 15: Effects of alternative education expansions

	Total		Women		Men	
Panel A: (at least) one STEM school in every existing university[a]						
	ΔS	ΔH	ΔS	ΔH	ΔS	ΔH
Δ Graduates	0.002	0.000	0.002	0.000	0.001	0.000
	[0.001,0.002]	[-0.001,0.000]	[0.001,0.003]	[-0.001,0.000]	[0.000,0.002]	[0.000,0.000]
	ATE	ATT	ATE	ATT	ATE	ATT
Employment[d]	0.001	0.404	0.001	0.443	0.000	0.202
	[0.000,0.001]	[0.03,0.76]	[0.000,0.002]	[0.16,0.704]	[0.000, 0.001]	[-0.078,0.945]
Panel B: (at least) one non-STEM school in every province[b]						
	ΔS	ΔH	ΔS	ΔH	ΔS	ΔH
Δ Graduates	-0.001	0.005	-0.002	0.006	-0.001	0.005
	[-0.002,0]	[0.003, 0.008]	[-0.003,-0.001]	[0.003,0.010]	[-0.002,0]	[0.003, 0.009]
	ATE	ATT	ATE	ATT	ATE	ATT
Employment[d]	-0.001	-0.179	-0.001	-0.169	-0.001	-0.119
	[-0.002,0]	[-0.308, -0.013]	[-0.003,0.001]	[-0.429,0.122]	[-0.001,0]	[-0.231,0.015]
Panel C: (at least) a STEM and (at least) a non-STEM school in every province[c]						
	ΔS	ΔH	ΔS	ΔH	ΔS	ΔH
Δ Graduates	0.007	0.004	0.009	0.004	0.005	0.005
	[0.005, 0.010]	[0.002, 0.007]	[0.005, 0.012]	[0.002, 0.009]	[0.002, 0.008]	[0.002, 0.009]
	ATE	ATT	ATE	ATT	ATE	ATT
Employment[d]	0.003	0.257	0.004	0.336	0.000	0.054
	[0, 0.005]	[-0.004,0.507]	[0.001,0.008]	[0.116,0.563]	[-0.001,0.003]	[-0.100,0.336]

[a] The estimated effects refer to a hypothetical reform that creates a STEM school in any existing university that does not have one.
[b] The estimated effects refer to a hypothetical reform that creates a non-STEM school in any province that does not have one.
[c] The estimated effects refer to a hypothetical reform that creates a STEM and non-STEM school in every province that does not have either one or the other.
[d] Dummy indicator equal to 1 if the respondent is employed (either as an employee or as a self-employed). All quantities are computed conditional on gender, age (and age squared), urbanization of the municipality, survey dummies, regional dummies and dummies for metropolitan areas. Bootstrapped 90%-confidence intervals in squared parenthesis (200 replications).

Table 15 reports the results of some alternative hypothetical reforms that expand the supply of tertiary education. For brevity, the table only reports the effects on the shares of graduates and on employment. The full results are reported in Appendix B-2.

In Panel A we consider a reform that creates a new STEM school in any existing university that does not have one. As a result of this intervention, there would be no university that is composed exclusively of non-STEM schools. Over the period 1990-2000, the number of universities composed only of non-STEM faculties was relatively stable around approximately 10 units, so the reform would imply the creation of a new STEM faculty in each of these universities. This is a cheaper and less conspicuous expansion of tertiary education than the reform considered in Table 14, both because it requires the creation of fewer schools and also because it does not involve the creation of new universities but only new faculties within existing universities.

Now only response type $r2$ reacts to the reform and only if they are $NearH - FarS$. For this group, the reform changes the value of the instrument to $NearH - NearS$. As a result, there are only some individuals who were not going to college who now decide to acquire STEM education. Similar to the previous reform, the effects on employment are positive and statistically significant for women and smaller and insignificant for men.

The reform considered in Panel B is symmetric to the one in Table 14 and it consists in creating a new non-STEM school in any province in which there is none. As a result of this reform, every province in the country would have at least one non-STEM school. Most compliers would be induced to change educational choices by such a reform. Depending on their locations, some response types of groups $r4$, $r6$, $r7$ and $r8$ would change from choosing not to attend college to attending a non-STEM course, while some individuals in groups $r5$ and $r7$ would switch from STEM to non-STEM education. This is reflected in the estimated changes in the shares of graduates, which indicate a small decline of STEM (about 0.1 of a percentage point) and an increase in non-STEM (about half of a percentage point). The effects are slightly larger for women than for men. The effects on employment are generally negative but small and imprecisely estimated.

Finally, in Panel C we consider a reform that combines the creation of STEM and non-STEM schools and aims at guaranteeing that each province has at least one of each. Under this intervention, students would all have both types of schools in the proximity of their residence and choices of field of study would then be driven exclusively by preference considerations. The result would be an increase of the share of graduates of over 1 percentage point or 4.6%

over the average share in our sample. Given that the reform would eventually create more STEM than non-STEM faculties, the effect is substantially larger for STEM graduates and, as usual, for women. Consistent with our previous findings, the effect on employment would be positive and concentrated on women.

6 Robustness checks

In this section, we present a few robustness checks that support the validity of our results.

First, we address the issue of the time when we observe residential location in our data. In the LFS, we only have information about the respondent's place of residence at the time of the survey. Ideally, we would like to know where they were living when they made their educational decisions and, unfortunately, this information is not available. We have already argued that the implications of this problem are presumably minor given the low geographical mobility of the Italian population.

To investigate the issue more thoroughly, we try to reconstruct what the distribution of our indicator of college proximity would look like if we were able to observe it at the time when educational decisions were made. To do so, we assume that a certain share of graduates (in either STEM or non-STEM subjects) did move from a location that was far from the school they eventually attended and decided to stay in that location after obtaining their degree. This mobility pattern implies that a certain number of STEM (non-STEM) graduates that we observe living close to a STEM (non-STEM) school at the time of the LFS survey, were in fact living far from that school when they made their education decisions. Hence, we randomly select some graduates who are observed living close to the school they chose and we recode the values of their instruments according to the following rules:

- if the selected person is a STEM graduate and the observed value of the instrument is $FarH - NearS$ or $NearH - NearS$, we recode the instrument to either $FarH - FarS$ or $NearH - FarS$ (at random);
- if the selected person is a non-STEM graduate and the observed value of the instrument is $NearH - NearS$ or $NearH - FarS$, we recode the instrument to either $FarH - NearS$

or $FarH - FarS$ (at random);

Table 16 replicates some of our main results using the recoded version of the instrument computed assuming that the share of graduate movers is 16%.[32] According to the "Survey of University Graduates' Vocational Integration" conducted by the Italian National Statistical Office, 16% is the share of graduates who, 4 years after graduation, reside in a different province than before enrolling at university.

Table 16: Replication of main results with internal mobility[a]

	Total		Women		Men	
	Panel A: Treatment effects on Employment					
$E(Y_S - Y_N \| r2)$	0.357		0.445		0.163	
	[0.173,0.552]		[0.233,0.697]		[-0.052,0.457]	
$E(Y_S - Y_H \| r5)$	-0.361		-0.297		-0.302	
	[-1.000,0.275]		[-1.000,0.293]		[-0.052,0.457]	
$E(Y_H - Y_N \| r6, r7)$	-0.134		-0.107		-0.092	
	[-0.234,-0.041]		[-0.234,0.000]		[-0.186,0.097]	
	Panel B: Effects of the reform[b]					
	ΔS	ΔH	ΔS	ΔH	ΔS	ΔH
Δ Graduates	0.017	-0.002	0.021	-0.003	0.015	-0.001
	[0.014,0.021]	[-0.005,0.000]	[0.017,0.027]	[-0.008,0.000]	[0.011,0.019]	[-0.005,0.000]
	ATE	ATT	ATE	ATT	ATE	ATT
Employment	0.004	0.223	0.006	0.302	0.001	0.071
	[0.001,0.007]	[0.039,0.411]	[0.002,0.011]	[0.063,0.555]	[-0.002,0.005]	[-0.122,0.312]

[a] The replication is carried out by recoding the values of the instrument for a random 16% of graduates residing close to a school of the type they attended. The recoding replaces the value of the instrument to being far from the school. See text for details.
[b] The reform considered here consists of creating a STEM school in any province that does not have one. The main results for this reform are reported in Table 14.
All quantities are computed conditional on gender, age (and age squared), urbanization of the municipality, survey dummies, regional dummies and dummies for metropolitan areas. Bootstrapped 90%-confidence intervals in squared parenthesis (200 replications).

For brevity, we only report results of the treatment effects for employment (to be compared with those in the first row of Table 13) and of the first hypothetical reform creating a STEM school in every province that does not have one (to be compared with the first 2 rows of Table 14).[33] Inspection of Table 16 indicates that all estimates are closely comparable to those ob-

[32] We also change the values of the other geographical control variables (i.e. region, degree of urbanization and residence in a metropolitan area) to the median values of the respondents with the same education, survey and instrument value.

[33] The replications of all other results in Sections 5.2, 5.3 and 5.4 are available upon request.

tained using the instrument based on observed residence. In Table B-8 in Appendix B-3, we also report a slightly modified version of this robustness check, where we impose that all the graduate movers move from a province in the South of the country to a province in the North. We do so because this is the most typical mobility pattern and it may have implications for the functioning of the instrument. Nevertheless, the results are virtually the same as in Table 16.

In a second robustness check, we explore the implications of using provinces as the geographical aggregation to define college proximity. In our main analysis, we classify respondents as being close to a school of a certain type (STEM or non-STEM) if there is one such school located in their province of residence. We argue that provinces are the appropriate geographical units for our empirical exercise because several public services that are relevant in our setting (e.g. public transport) are organised and managed at this administrative level. Nevertheless, we acknowledge that there is a certain degree of arbitrariness in this choice and in Table 17 we replicate our main results using local labour markets instead of provinces.[34] Also in this case, we only report a subset of the replicated results, which are extremely comparable to the main estimates in Sections 5.3 and 5.4 (the full set of results is available upon request).

[34]Local labour markets are defined by the National Statistical Institute according to the international convention of having a minimum of 75% of individuals residing and living in the same local labour market. There are 611 local labour markets in Italy.

Table 17: Replication of main results using Local Labour Markets[a]

	Total		Women		Men	
	Panel A: Treatment effects on Employment					
$E(Y_S - Y_N \mid r2)$	0.520		0.657		0.272	
	[0.153,0.883]		[0.344,0.827]		[-0.038,0.972]	
$E(Y_S - Y_H \mid r5)$	-0.432		-0.311		-0.337	
	[-1.000,0.104]		[-1.000,0.315]		[-1.000,0.283]	
$E(Y_H - Y_N \mid r6, r7)$	-0.183		-0.144		-0.162	
	[-0.309,-0.068]		[-0.314,0.000]		[-0.346,0.000]	
	Panel B: Effects of the reform[b]					
	ΔS	ΔH	ΔS	ΔH	ΔS	ΔH
Δ Graduates	0.011	-0.001	0.013	-0.003	0.010	-0.001
	[0.007,0.015]	[-0.004,0.000]	[0.008,0.019]	[-0.008,0.000]	[0.005,0.016]	[-0.005,0.000]
	ATE	ATT	ATE	ATT	ATE	ATT
Employment	0.003	0.240	0.005	0.385	0.001	0.065
	[0.000,0.006]	[-0.380,0.602]	[0.001,0.009]	[0.062,0.698]	[-0.002, 0.005]	[-0.178,0.453]

[a] The replication is carried out by defining college proximity based on local labour markets instead of provinces.

[b] The reform considered here consists of creating a STEM school in any province that does not have one. The main results for this reform are reported in Table 14.

All quantities are computed conditional on gender, age (and age squared), urbanization of the municipality, survey dummies, regional dummies and dummies for metropolitan areas. Bootstrapped 90%-confidence intervals in squared parenthesis (200 replications).

In a final robustness check, we use a continuous measure of college proximity and apply the methodology of Mountjoy (2022) to produce estimates of the returns of STEM and non-STEM education on the probability of employment. In our main analysis, we prefer using a discrete measure of proximity because it reduces mismeasurement. Nevertheless, we can also compute proximity as the distance between the centroid of the respondent's municipality of residence and the centroid of the municipality where the nearest STEM and non-STEM schools are located. We are obliged to use the centroids of the municipalities because we do not observe the exact addresses of either the LFS respondents or the colleges, hence the resulting distances are likely affected by measurement error.

Table 18: Marginal treatment effects (MTE) using continuous distance from college

	[1] Net Effect	[2] Democratization[a]		[3] Diversion[b]	
		Share	Effect	Share	Effect
MTE of non-STEM (H)	2.436	1.224	1.914	-0.224	-0.533
	[1.831,3.328]	[1.058,1.406]	[1.121,2.755]	[-0.406,-0.058]	[-3.603,2.396]
MTE of STEM (S)	11.325	-1.369	-6.176	2.369	0.295
	[-82.439,154.447]	[-26.447,13.867]	[-16.845,-3.323]	[-12.867,27.447]	[0.105,0.540]

[a] Effect due to compliers changing their educational choices from No-college to STEM (row 1) or from No-college to non-STEM (row 2).
[b] Effect due to compliers changing their educational choices from non-STEM to STEM (row 1) or from STEM to non-STEM (row 2).
Marginal treatment effects (MTE) computed (and decomposed) at the mean distance from non-STEM (32.45 Km) and STEM colleges (35.17 km) following the methodology in Mountjoy (2022). All quantities are computed conditional on gender, age (and age squared), urbanization of the municipality, survey dummies, regional dummies and dummies for metropolitan areas. Bootstrapped 90%-confidence intervals in squared parenthesis (200 replications).

Results are reported in Table 18 and represent marginal treatment effects (computed at the mean distances observed in the sample), so they are difficult to compare to our main estimates. However, they are consistent with our main finding that most compliers are at the margin between not going to college and choosing a STEM degree (cfr. Column [1]). This effect can be decomposed of a democratization effect and a diversion effect. The former refers to the effect due to compliers switching from non-college to STEM (row 1) or non-STEM (row 2) when the instrument changes marginally (cfr. Column [2]). The latter refers to the effect due to compliers switching from non-STEM to STEM (row 1) or vice-versa (row 2) when the instrument changes marginally (cfr. Column [3]).[35] In line with our main results, the democratization effect of STEM is the largest of all those reported in Table 18, whereas the diversion effects are small and point in the opposite direction (i.e. reducing employment). The large confidence intervals presumably reflect both the small size of the population of compliers and the error of measurement.

In the Appendix B-3, we additionally check the robustness of our main findings to varying the definition of STEM subjects and the age of respondents when college proximity is measured. In Table B-9, we replicate results including "Agriculture and veterinary" (ISCED-7) into the group of STEM fields of study. In Table B-10, we construct college proximity based on universities and faculties that were operating when the respondents were 18 years old (in-

[35]To facilitate the comparison, we adopt the same terminology of Mountjoy (2022) despite the notions of "democratization" and "diversion" do not necessarily have the same meaning in our setting.

stead of when they completed compulsory education, as in the main analysis). In both cases, results are consistent with those reported in Section 5, at least qualitatively.

7 Conclusions

In many industrialised countries, there seems to be an under-supply of STEM graduates, especially among women. In this paper, we apply the recent methodology of Heckman and Pinto (2018) to investigate the role of college proximity in the choice of enrolling in college but also the choice of field of study. We find that, even in a setting where the geographical coverage of college supply is quite dense, proximity to the institutions of higher education can still play an important role in influencing these decisions.

We find that women are more responsive than men to expansions of the supply of STEM education. Consistently, we also show that the women who would respond to such an expansion would also benefit quite unambiguously at least in terms of their rates of employment. Simple policy simulations based on our estimates indicate that expanding the supply of STEM tertiary education could substantially reduce the gender gap in both fields of study and in employment.

We believe that our results can be interpreted quite generally. The type of variation in college proximity that we exploit broadly corresponds to the choice of commuting rather than moving residence or to a difference in living costs of about 40%. Hence, our results can be informative about a broad set of policies aimed at reducing the cost of college and they indicate that such policies have the potential to substantially reduce the gender gap in STEM education and in labour market outcomes.

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Appendix A: Additional formulas for identification

A-1 Response types

$$P(r2) = P(T = S|Z = NearH - NearS) - P(T = S|Z = FarH - FarS) - P(r4) \tag{A-1}$$

$$P(r3) = P(T = S|Z = NearH - FarS) \tag{A-2}$$

$$P(r4) = P(T = H|Z = NearH - FarS) - P(T = H|Z = NearH - NearS) - P(r5) \tag{A-3}$$

$$P(r6) = P(T = S|Z = NearH - NearS) - P(T = S|Z = FarH - FarS) - P(r7) \tag{A-4}$$

$$P(r7) = P(T = S|Z = FarH - NearS) - P(r8) \tag{A-5}$$

$$P(r8) = P(T = H|Z = FarH - FarS) - P(T = H|Z = FarH - NearS) \tag{A-6}$$

$$P(r9) = P(T = H|Z = FarH - NearS) \tag{A-7}$$

A-2 Potential outcomes

$$
\begin{aligned}
E(Y_S|r2) = \Big(&E(S|Z = NearH - NearS)(P(r1) + P(r2)) \\
&- E(S|Z = FarH - FarS)(P(r3) + P(r5))\Big)/P(r2)
\end{aligned} \tag{A-8}
$$

$$
\begin{aligned}
E(Y_H|r5) = \Big(&E(H|Z = NearH - FarS)(P(r5) + P(r6) + P(r7) + P(r8) + P(r9)) \\
&- E(H|Z = NearH - NearS)(P(r6) + P(r7) + P(r8) + P(r9))\Big)/P(r5)
\end{aligned} \tag{A-9}
$$

$$
\begin{aligned}
E(Y_S|r5) = \Big(&E(S|Z = FarH - FarS)(P(r3) + P(r5)) \\
&- E(S|Z = NearH - FarS)P(r3)\Big)/P(r2)
\end{aligned} \tag{A-10}
$$

$$
\begin{aligned}
E(Y_N|r6) = \Big(&E(N|Z = FarH - NearS)(P(r1) + P(r6)) \\
&- E(N|Z = NearH - NearS)P(r1)\Big)/P(r6)
\end{aligned} \tag{A-11}
$$

$$
\begin{aligned}
E(Y_N|r7) = \Big(&E(N|Z = FarH - FarS)(P(r1) + P(r2) + P(r6) + P(r7)) \\
&- E(N|Z = FarH - NearS)(P(r1) + P(r6)) \\
&- E(N|Z = NearH - FarS)(P(r1) + P(r2)) \\
&+ E(N|Z = NearH - NearS)P(r1)\Big)/P(r2)
\end{aligned} \tag{A-12}
$$

$$
\begin{aligned}
E(Y_H|r6r7) = \Big(&E(H|Z = NearH - NearS)(P(r6) + P(r7) + P(r8) + P(r9)) \\
&- E(H|Z = FarH - FarS)(P(r8) + P(r9))\Big)/(P(r6) + P(r7))
\end{aligned} \tag{A-13}
$$

$$
\begin{aligned}
E(Y_H|r8) = \Big(&E(H|Z = FarH - FarS)(P(r8) + P(r9)) \\
&- E(H|Z = FarH - NearS)P(r9)\Big)/P(r8)
\end{aligned} \tag{A-14}
$$

$$
\begin{aligned}
E(Y_S|r7r8) = \Big(&E(S|Z = FarH - NearS)(P(r2) + P(r3) + P(r7) + P(r8)) \\
&- E(S|Z = NearH - NearS)(P(r2) + P(r3))\Big)/(P(r7) + P(r8))
\end{aligned} \tag{A-15}
$$

B Appendix: Additional empirical results

B-1 Counterfactual outcomes and treatment effects

Table B-1: Counterfactual means and treatment effects for earnings[a]

	$r1$	$r2$	$r3$	$r5$	$r6$	$r7$	$r8$	$r9$
				Panel A: Total				
Potential outcomes:								
$E[Y_N/R=r]$	1173.105	1552.998	-	-	8593.3131	348.139	-	-
	[1169.783,1176.771]	[1115.725,2009.9]	-	-	[2538.630,11957]	[15.48,1284.456]	-	-
$E[Y_H/R=r]$	-	-	-	3324.241	1489.953	-1.000	348.139	1499.379
	-	-	-	[903.642,11957]	[1238.771,1762.971]	[-1,-1]	[15.48,1284.456]	[1443.671,1559.063]
$E[Y_S/R=r]$	-	1517.204	1568.288	992.521	-	1806.35	-1.000	-
	-	[1400.735,1624.255]	[1526.646,1615.3]	[15.48,1834.105]	-	[1003.649,2611.864]	[-1,-1]	-
Treatment effects:								
$E(Y_H-Y_N\|r)$	-	-	-	-	154.154	-1.000	-	-
	-	-	-	-	[-607.704,896.151]	[-1,-1]	-	-
$E(Y_S-Y_N\|r)$	-	-35.794	-	-	-	-	-	-
	-	[-488.470,415.475]	-		–	-	-	-
$E(Y_S-Y_H\|r)$	-	-	-	-2331.72	-	-	-	-
	-	-	-	[-11938.94,125.895]	-	-	-	-
				Panel B: Women				
Potential outcomes:								
$E[Y_N/R=r]$	1045.025	1422.261	-	-	4178.876	1822.833	-	-
	[1039.323,1049.961]	[978.179,2033.24]	-	-	[1424.694,5205]	[20.64,5205]	-	-
$E[Y_H/R=r]$	-	-	-	2305.775	1313.184	-1.000	1822.833	1315.975
	-	-	-	[898.586,5160]	[925.044,1730.261]	[-1,-1]	[20.64,5205]	[1251.18,1375.862]
$E[Y_S/R=r]$	-	1383.812	1412.396	964.659	-	1992.141	-1.000	-
	-	[1253.304,1508.961]	[1326.092,1499.531]	[29.204,1555.375]	-	[1085.946,3534.566]	[-1,-1]	-
Treatment effects:								
$E(Y_H-Y_N\|r)$	-	-	-	-	451.667	-1.000	-	-
	-	-	-	-	[-429.39,1341.95]	[-1,-1]	-	-
$E(Y_S-Y_N\|r)$	-	-38.448	-	-	-	-	-	-
	-	[-615.797,442.844]	-		–	-	-	-
$E(Y_S-Y_H\|r)$	-	-	-	-1341.117	-	-	-	-
	-	-	-	[-4475.312.200.572]	-	-	-	-
				Panel C: Men				
Potential outcomes:								
$E[Y_N/R=r]$	1285.631	2024.587	-	-	8914.47	88.776	-	-
	[1281.565,1289.349]	[1120.567,3209.977]	-	-	[2692.421,11957]	[15.48,621.305]	-	-
$E[Y_H/R=r]$	-	-	-	4239.361	1693.42	-1.000	88.776	1755.021
	-	-	-	[15.48,11957]	[1425.552,2047.159]	[-1,-1]	[15.48,621.305]	[1617.651,1901.305]
$E[Y_S/R=r]$	-	1668.951	1708.958	2088.311	-	1625.661	-1.000	-
	-	[1402.22,1847.489]	[1640.448,1781.511]	[15.48,11957]	-	[439.878,2734.87]	[-1,-1]	-
Treatment effects:								
$E(Y_H-Y_N\|r)$	-	-	-	-	151.894	-1.000	-	-
	-	-	-	-	[-765.441,922.499]	[-1,-1]	-	-
$E(Y_S-Y_N\|r)$	-	-355.636	-	-	-	-	-	-
	-	[-1690.802,580.708]	-		–	-	-	-
$E(Y_S-Y_H\|r)$	-	-	-	-2151.051	-	-	-	-
	-	-	-	[-11941.52,9272.52]	-	-	-	-

[a] Monthly earnings (for both employees and self-employed) in 2021 euros.
All quantities are computed conditional on gender, age (and age squared), urbanization of the municipality, survey dummies, regional dummies and dummies for metropolitan areas. Bootstrapped 90%-confidence intervals in squared parenthesis (200 replications).

Table B-2: Counterfactual means and treatment effects for hours of work[a]

	$r1$	$r2$	$r3$	$r5$	$r6$	$r7$	$r8$	$r9$
				Panel A: Total				
Potential outcomes:								
$E(Y_N\|r)$	36.356	46.329	-	-	79.963	9.135	-	-
	[36.2,36.4]	[37.3,57.1]	-	-	[0,105]	[0,35.2]	-	-
$E(Y_H\|r)$	-	-	-	45.359	35.454	-1.000	9.135	38.314
	-	-	-	[0,105]	[30.9,39.9]	[-1,-1]	[0,35.2]	[37.1,39.4]
$E(Y_S\|r)$	-	38.976	37.678	47.704	-	46.408	-1.000	-
	-	[37.3,40.7]	[36.9,38.4]	[25.7,105]	-	[36.1,59.8]	[-1,-1]	-
Treatment effects:								
$E(Y_H - Y_N\|r)$	-	-	-	-	-1.777	-1.000	-	-
	-	-	-	-	[-16.9,12.6]	[-1,-1]	-	-
$E(Y_S - Y_N\|r)$	-	-7.353	-	-	-	-	-	-
	-	[-18.4,1.7]	-		–	-	-	-
$E(Y_S - Y_H\|r)$	-	-	-	2.345	-	-	-	-
	-	-	-	[-40.9,46.4]	-	-	-	-
				Panel B: Women				
Potential outcomes:								
$E(Y_N\|r)$	33.28	47.304	-	-	81.085	8.176	-	-
	[33.1,33.4]	[33.4,65.4]	-	-	[0,105]	[0,37.2]	-	-
$E(Y_H\|r)$	-	-	-	22.659	33.789	-1.000	8.176	36.008
	-	-	-	[0,45.6]	[24.8,42.6]	[-1,-1]	[0,37.2]	[34.6,37.4]
$E(Y_S\|r)$	-	35.48	34.737	39.951	-	55.173	-1.000	-
	-	[33.2,38.3]	[33.3,36.1]	[25.3,59.2]	-	[37.7,91.2]	[-1,-1]	-
Treatment effects:								
$E(Y_H - Y_N\|r)$	-	-	-	-	10.196	-1.000	-	-
	-	-	-	-	[-8.3,35.5]	[-1,-1]	-	-
$E(Y_S - Y_N\|r)$	-	-11.825	-	-	-	-	-	-
	-	[-29.8,2.2]	-	-	-	-	-	-
$E(Y_S - Y_H\|r)$	-	-	-	17.292	-	-	-	-
	-	-	-	[-9.3,51.7]	-	-	-	-
				Panel C: Men				
Potential outcomes:								
$E(Y_N\|r)$	39.115	47.975	-	-	57.688	17.345	-	-
	[39.1,39.2]	[33.6,71.1]	-	-	[0,105.1]	[0,105.1]	-	-
$E(Y_H\|r)$	-	-	-	91.157	37.97	-1.000	17.345	40.243
	-	-	-	[45.7,105]	[33.5,42.1]	[-1,-1]	[0,105.1]	[38.5,41.9]
$E(Y_S\|r)$	-	43.279	40.345	67.298	-	39.65	-1.000	-
	-	[39.9,46.2]	[39.5,41.2]	[0,105]	-	[24.4,53.5]	[-1,-1]	-
Treatment effects:								
$E(Y_H - Y_N\|r)$	-	-	-	-	-8.103	-1.000	-	-
	-	-	-	-	[-24.1,4.3]	[-1,-1]	-	-
$E(Y_S - Y_N\|r)$	-	-4.696	-	-	-	-	-	-
	-	[-26.9,9.6]	-	-	-	-	-	-
$E(Y_S - Y_H\|r)$	-	-	-	-23.859	-	-	-	-
	-	-	-	[-105,3.2]	-	-	-	-

[a] Weekly hours (for both employees and self-employed).
All quantities are computed conditional on gender, age (and age squared), urbanization of the municipality, survey dummies, regional dummies and dummies for metropolitan areas. Bootstrapped 90%-confidence intervals in squared parenthesis (200 replications).

Table B-3: Counterfactual means and treatment effects for night/weekend shifts[a]

	$r1$	$r2$	$r3$	$r5$	$r6$	$r7$	$r8$	$r9$
				Panel A: Total				
Potential outcomes:								
$E(Y_N\|r)$	0.49	0.581	-	-	0.675	0.901	-	-
	[0.486, 0.494]	[0.021,1]	-	-	[0,1]	[0.221,1]	-	-
$E(Y_H\|r)$	-	-	-	0.704	0.532	-1.000	0.901	0.401
	-	-	-	[0,1]	[0.304,0.764]	[-1,-1]	[0.221,1]	[0.346,0.46]
$E(Y_S\|r)$	-	0.303	0.313	0.211	-	0.315	-1.000	-
	-	[0.191, 0.405]	[0.272,0.361]	[0,0.938]	-	[0,0.995]	[-1,-1]	-
Treatment effects:								
$E(Y_H - Y_N\|r)$	-	-	-	-	-0.346	-1.000	-	-
	-	-	-	-	[-0.668,0.218]	[-1,-1]	-	-
$E(Y_S - Y_N\|r)$	-	-0.278	-	-	-	-	-	-
	-	[-0.74,0.245]	-	-	-	-	-	-
$E(Y_S - Y_H\|r)$	-	-	-	-0.493	-	-	-	-
	-	-	-	[-1,0.389]	-	-	-	-
				Panel B: Women				
Potential outcomes:								
$E(Y_N\|r)$	0.463	0.304	-	-	0.802	0.822	-	-
	[0.456,0.47]	[0,0.957]	-	-	[0,1]	[0,1]	-	-
$E(Y_H\|r)$	-	-	-	0.32	0.519	-1.000	0.822	0.371
	-	-	- [0,-1]		[0.149,1]	[-1,-1]	[0,1]	[0.306,0.44]
$E(Y_S\|r)$	-	0.234	0.283	0.747	-	0.188	-1.000	-
	-	[0.08,0.351]	[0.217,0.348]	[0.312,1]	-	[0,0.964]	[-1,-1]	-
Treatment effects:								
$E(Y_H - Y_N\|r)$	-	-	-	-	-0.255	-1.000	-	-
	-	-	-	-	[-0.791,0.502]	[-1,-1]	-	-
$E(Y_S - Y_N\|r)$	-	-0.07	-	-	-	-	-	-
	-	[-0.688,0.297]	-	-	-	-	-	-
$E(Y_S - Y_H\|r)$	-	-	-	0.427	-	-	-	-
	-	-	-	[-0.372,1]	-	-	-	-
				Panel C: Men				
Potential outcomes:								
$E[Y_N\|r]$	0.5	0.8	-	-	0.432	0.754	-	-
	[0.494,0.505]	[0.201,1]	-	-	[0,1]	[0,1]	-	-
$E(Y_H\|r)$	-	-	- 0.964		0.56	-1.000	0.754	0.4
	-	-	-	[0.803,1]	[0.284,908]	[-1,-1]	[0,1]	[0.296,0.508]
$E(Y_S\|r)$	-	0.415	0.335	0.02	-	0.468	-1.000	-
	-	[0.2,0.704]	[0.268,0.401]	[0,0.038]	-	[0,1]	[-1,-1]	-
Treatment effects:								
$E(Y_H - Y_N\|r)$	-	-	-	-	-0.256	-1.000	-	-
	-	-	-	-	[-0.689,0.368]	[-1,-1]	-	-
$E(Y_S - Y_N\|r)$	-	-0.386	-	-	-	-	-	-
	-	[-0.748,0.174]	-	-	-	-	-	-
$E(Y_S - Y_H\|r)$	-	-	-	-0.944	-	-	-	-
	-	-	-	[-1,-0.563]	-	-	-	-

[a] Dummy indicator equal to 1 if the respondent indicates doing either night or weekend shifts (or both).
All quantities are computed conditional on gender, age (and age squared), urbanization of the municipality, survey dummies, regional dummies and dummies for metropolitan areas. Bootstrapped 90%-confidence intervals in squared parenthesis (200 replications).

Table B-4: Counterfactual means and treatment effects for job responsibilities[a]

	$r1$	$r2$	$r3$	$r5$	$r6$	$r7$	$r8$	$r9$
				Panel A: Total				
Potential outcomes:								
$E(Y_N\|r)$	0.107	0.076	-	-	0.941	0.000	-	-
	[0.104, 0.11]	[0,0.303]	-	-	[0.507,1]	[0,0]	-	-
$E(Y_H\|r)$	-	-	-	0.726	0.192	-1.000	0.000	0.281
	-	-	-	[0,1]	[0, 0.385]	[-1,-1]	[0,0]	[0.229,0.331]
$E(Y_S\|r)$	-	0.329	0.235	0.607	-	0.982	-1.000	-
	-	[0.252,0.423]	[0.198,0.274]	[0,1]	-	[0.833,1]	[-1,-1]	-
Treatment effects:								
$E(Y_H - Y_N\|r)$	-	-	-	-	-0.403	-1.000	-	-
	-	-	-	-	[-0.937,0.123]	[-1,-1]	-	-
$E(Y_S - Y_N\|r)$	-	0.253	-	-	-	-	-	-
	-	[0.005,0.415]	-	-	-	-	-	-
$E(Y_S - Y_H\|r)$	-	-	-	-0.119	-	-	-	-
	-	-	-	[-0.972,0.783]	-	-	-	-
				Panel B: Women				
Potential outcomes:								
$E(Y_N\|r)$	0.087	0.317	-	-	0.951	0.005	-	-
	[0.083,0.091]	[0,0.736]	-	-	[0.568,1]	[0,0]	-	-
$E(Y_H\|r)$	-	-	-	0.326	0.139	-1.000	0.005	0.22
	-	-	-	[0,1]	[0,0.379]	[-1,-1]	[0,0]	[0.167,0.271]
$E(Y_S\|r)$	-	0.249	0.137	0.443	-	0.613	-1.000	-
	-	[0.148,0.361]	[0.091,0.197]	[0,1]	-	[0,1]	[-1,-1]	-
Treatment effects:								
$E(Y_H - Y_N\|r)$	-	-	-	-	-0.012	-1.000	-	-
	-	-	-	-	[-0.445, 0.293]	[-1,-1]	-	-
$E(Y_S - Y_N\|r)$	-	-0.068	-	-	-	-	-	-
	-	[-0.482,0.252]	-	-	-	-	-	-
$E(Y_S - Y_H\|r)$	-	-	-	0.117	-	-	-	-
	-	-	-	[-0.587,0.998]	-	-	-	-
				Panel C: Men				
Potential outcomes:								
$E(Y_N\|r)$	0.123	0.022	-	-	0.712	0.011	-	-
	[0.12,0.127]	[0,0.199]	-	-	[0,1]	[0,0]	-	-
$E(Y_H\|r)$	-	-	-	0.889	0.324	-1.000	0.011	0.345
	-	-	-	[0,1]	[0.115,0.542]	[-1,-1]	[0,0]	[0.243,0.46]
$E(Y_S\|r)$	-	0.413	0.311	0.655	-	0.999	-1.000	-
	-	[0.215,0.620]	[0.256,0.377]	[0,1]	-	[1,1]	[-1,-1]	-
Treatment effects:								
$E(Y_H - Y_N\|r)$	-	-	-	-	-0.498	-1.000	-	-
	-	-	-	-	[-0.868,0.002]	[-1,-1]	-	-
$E(Y_S - Y_N\|r)$	-	0.391	-	-	-	-	-	-
	-	[0.154,0.609]	-	-	-	-	-	-
$E(Y_S - Y_H\|r)$	-	-	-	-0.234	-	-	-	-
	-	-	-	[-1.000,0.333]	-	-	-	-

[a] Dummy indicator equal to 1 if the respondent indicates coordinating the work of others.
All quantities are computed conditional on gender, age (and age squared), urbanization of the municipality, survey dummies, regional dummies and dummies for metropolitan areas. Bootstrapped 90%-confidence intervals in squared parenthesis (200 replications).

B-2 Simulations of alternative hypothetical reforms

Table B-5: Effects of creating a STEM school in every existing university[a]

	Total		Women		Men	
	ΔS	ΔH	ΔS	ΔH	ΔS	ΔH
Δ Graduates	0.002	0.000	0.002	0.000	0.001	0.000
	[0.001,0.002]	[-0.001,0.000]	[0.001,0.003]	[-0.001,0.000]	[0.000,0.002]	[0.000,0.000]
	Treatment effects					
	ATE	ATT	ATE	ATT	ATE	ATT
Employment[b]	0.001	0.404	0.001	0.443	0.000	0.202
	[0.000,0.001]	[0.03,0.76]	[0.000,0.002]	[0.16,0.704]	[0.000, 0.001]	[-0.078,0.945]
Earnings[c]	-0.302	-197.985	-0.47	-243.842	-0.295	-383.253
	[-1.025,0.468]	[-727.144,242.263]	[-1.429,0.632]	[-948.878,227.858]	[-1.211,0.615]	[-1212.766,442.996]
Hours of work[d]	-0.009	-5.878	-0.012	-6.07	-0.007	-6.547
	[-0.022,0.004]	[-16.36,2.201]	[-0.036,0.014]	[-21.352,6.335]	[-0.024,0.009]	[-30.644,6.573]
Night/Weekend shifts[e]	-0.001	-0.291	0.000	0.006	-0.001	-0.417
	[-0.001,0.000]	[-0.718,0.155]	[-0.001,0.001]	[-0.479,0.364]	[-0.001,0.000]	[-0.753,0.214]
Responsibilities[f]	0.000	0.189	0.000	-0.023	0.000	0.328
	[0.000,0.001]	[0.003,0.367]	[-0.001,0.001]	[-0.393,0.238]	[0.000,0.001]	[0.091,0.593]

[a] The table reports the estimated effects of a hypothetical reform that creates a STEM school in any existing university that does not have one.
[b] Dummy indicator equal to 1 if the respondent is employed (either as an employee or as a self-employed).
[c] Monthly earnings (for both employees and self-employed) in 2021 euros.
[d] Weekly hours (for both employees and self-employed).
[e] Dummy indicator equal to 1 if the respondent indicates doing either night or weekend shifts (or both).
[f] Dummy indicator equal to 1 if the respondent indicates coordinating the work of others.
All quantities are computed conditional on gender, age (and age squared), urbanization of the municipality, survey dummies, regional dummies and dummies for metropolitan areas. Bootstrapped 90%-confidence intervals in squared parenthesis (200 replications).

Table B-6: Effects of creating a non-STEM school in every province[a]

	Total		Women		Men	
	ΔS	ΔH	ΔS	ΔH	ΔS	ΔH
Δ Graduates	-0.001	0.005	-0.002	0.006	-0.001	0.005
	[-0.002,0]	[0.003, 0.008]	[-0.003,-0.001]	[0.003,0.010]	[-0.002,0]	[0.003, 0.009]
	Treatment effects					
	ATE	ATT	ATE	ATT	ATE	ATT
Employment[b]	-0.001	-0.179	-0.001	-0.169	-0.001	-0.119
	[-0.002,0]	[-0.308, -0.013]	[-0.003,0.001]	[-0.429,0.122]	[-0.001,0]	[-0.231,0.015]
Earnings[c]	-0.866	-194.872	-0.142	-32.385	-0.479	-114.172
	[-3.932,2.258]	[-923.124,417.199]	[-4.191,4.15]	[-803.16,682.478]	[-3.428,4.142]	[-833.034,744.592]
Hours of work[d]	-0.011	-2.305	0.054	8.969	-0.048	-9.653
	[-0.062,0.042]	[-14.303,7.995]	[-0.043,0.149]	[-6.539,24.753]	[-0.103,0.01]	[-22.807,1.722]
Night/Weekend shifts[e]	-0.002	-0.328	0.000	-0.049	-0.002	-0.300
	[-0.004,0.001]	[-0.646, 0.137]	[-0.004,0.003]	[-0.522,0.517]	[-0.004,0.001]	[-0.691,0.224]
Responsibilities[f]	-0.002	-0.363	0.000	-0.024	-0.003	-0.505
	[-0.004,0]	[-0.76, 0.057]	[-0.002, 0.002]	[-0.432,0.282]	[-0.005,-0.001]	[-0.85,-0.071]

[a] The table reports the estimated effects of a hypothetical reform that creates a non-STEM school in any province that does not have one.
[b] Dummy indicator equal to 1 if the respondent is employed (either as an employee or as a self-employed).
[c] Monthly earnings (for both employees and self-employed) in 2021 euros.
[d] Weekly hours (for both employees and self-employed).
[e] Dummy indicator equal to 1 if the respondent indicates doing either night or weekend shifts (or both).
[f] Dummy indicator equal to 1 if the respondent indicates coordinating the work of others.
All quantities are computed conditional on gender, age (and age squared), urbanization of the municipality, survey dummies, regional dummies and dummies for metropolitan areas. Bootstrapped 90%-confidence intervals in squared parenthesis (200 replications).

Table B-7: Effects of creating a STEM and non-STEM school in every existing university[a]

	Total		Women		Men	
	ΔS	ΔH	ΔS	ΔH	ΔS	ΔH
Δ Graduates	0.007	0.004	0.009	0.004	0.005	0.005
	[0.005, 0.010]	[0.002, 0.007]	[0.005, 0.012]	[0.002, 0.009]	[0.002, 0.008]	[0.002, 0.009]
	Treatment effects					
	ATE	ATT	ATE	ATT	ATE	ATT
Employment[b]	0.003	0.257	0.004	0.336	0.000	0.054
	[0, 0.005]	[-0.004,0.507]	[0.001,0.008]	[0.116,0.563]	[-0.001,0.003]	[-0.100,0.336]
Earnings[c]	-0.082	-15.4	1.195	69.989	-0.679	-99.417
	[-2.366,2.327]	[-247.566,173.483]	[-2.621,4.606]	[-232.089,299.669]	[-3.773,3.228]	[-531.05,239.746]
Hours of work[d]	-0.059	-5.31	-0.054	-4.2	-0.062	-6.584
	[-0.103,-0.017]	[-10.079,-1.362]	[-0.138,0.03]	[-12.188,2.323]	[-0.111,-0.01]	[-14.586,-1.168]
Night/Weekend shifts[e]	-0.003	-0.271	-0.002	-0.111	-0.003	-0.315
	[-0.006, 0]	[-0.534,0.019]	[-0.005,0.002]	[-0.395,0.162]	[-0.007,0]	[-0.562,0.022]
Responsibilities[f]	0.000	0.004	-0.001	-0.051	-0.001	-0.058
	[-0.002, 0.002]	[-0.14,0.157]	[-0.003, 0.002]	[-0.29,0.124]	[-0.003,0.002]	[-0.329,0.169]

[a] The table reports the estimated effects of a hypothetical reform that creates a STEM and non-STEM school in any existing university that does not have one.
[b] Dummy indicator equal to 1 if the respondent is employed (either as an employee or as a self-employed).
[c] Monthly earnings (for both employees and self-employed) in 2021 euros.
[d] Weekly hours (for both employees and self-employed).
[e] Dummy indicator equal to 1 if the respondent indicates doing either night or weekend shifts (or both).
[f] Dummy indicator equal to 1 if the respondent indicates coordinating the work of others.
All quantities are computed conditional on gender, age (and age squared), urbanization of the municipality, survey dummies, regional dummies and dummies for metropolitan areas. Bootstrapped 90%-confidence intervals in squared parenthesis (200 replications).

B-3 Additional robustness checks

Table B-8: Replication of main results with South-North internal mobility[a]

	Total		Women		Men	
	Panel A: Treatment effects on Employment					
$E(Y_S - Y_N \mid r2)$	0.397		0.522		0.181	
	[0.185,0.615]		[0.261,0.77]		[-0.045,0.491]	
$E(Y_S - Y_H \mid r5)$	-0.349		-0.311		-0.287	
	[-1,0.114]		[-1,0.315]		[-1,0.572]	
$E(Y_H - Y_N \mid r6, r7)$	-0.156		-0.144		-0.094	
	[-0.263,-0.049]		[-0.314,0]		[-0.181,0.029]	
	Panel B: Effects of the reform[b]					
	ΔS	ΔH	ΔS	ΔH	ΔS	ΔH
Δ Graduates	0.015	-0.001	0.019	-0.001	0.013	-0.001
	[0.012,0.019]	[-0.003,0]	[0.014,0.023]	[-0.005,0]	[0.009,0.018]	[-0.003,0]
	ATE	ATT	ATE	ATT	ATE	ATT
Employment	0.003	0.227	0.007	0.366	0.001	0.074
	[0.000,0.008]	[-0.001,0.542]	[0.001,0.013]	[0.073,0.644]	[-0.002, 0.005]	[-0.128,0.371]

[a] The replication is carried out recoding the values of the instrument for a random 16% of graduates residing close to a school of the type they attended. The recoding replaces the value of the instrument to being far from the school and imposes that all movers move from a province in the South to one in the North. See Section 6 for details.

[b] The reform considered here consists of creating a STEM school in any province that does not have one. The main results for this reform are reported in Table 14.

All quantities are computed conditional on gender, age (and age squared), urbanization of the municipality, survey dummies, regional dummies and dummies for metropolitan areas. Bootstrapped 90%-confidence intervals in squared parenthesis (200 replications).

Table B-9: Replication of main results with extended STEM definition[a]

	Total		Women		Men	
	Panel A: Treatment effects on Employment					
$E[Y_S - Y_N \mid r2]$	0.06		0.149		0.073	
	[-0.115,0.621]		[-0.964,1]		[-0.032,0.481]	
$E[Y_S - Y_H \mid r5]$	-0.367		-0.057		-0.542	
	[-1,1]		[-1,1]		[-1,0]	
$E[Y_H - Y_N \mid r6, r7]$	-0.065		0.022		-0.069	
	[-0.299,0.356]		[-0.32,0.647]		[-0.323,0.491]	
	Panel B: Effects of the reform[b]					
	Δ S	Δ H	Δ S	Δ H	Δ S	Δ H
Δ Graduates	0.005	-0.001	0.004	-0.001	0.007	-0.002
	[0.002,0.008]	[-0.004,0]	[0,0.009]	[-0.004,0]	[0.002,0.011]	[-0.004,0]
	ATE	ATT	ATE	ATT	ATE	ATT
Employment	0	-0.090	0.000	-0.121	0	-0.015
	[-0.001,0.001]	[-0.284,0.210]	[-0.002,0.002]	[-1.000,0.667]	[-0.001, 0.002]	[-0.167,0.257]

[a] The replication is carried out including ”Agriculture and Veterinary” (ISCED-7) into the group of STEM fields of study.

[b] The reform considered here consists of creating a STEM school in any province that does not have one. The main results for this reform are reported in Table 14.

All quantities are computed conditional on gender, age (and age squared), urbanization of the municipality, survey dummies, regional dummies and dummies for metropolitan areas. Bootstrapped 90%-confidence intervals in squared parenthesis (200 replications).

Table B-10: Replication of main results with educational choices at age 18[a]

	Total		Women		Men	
	Panel A: Treatment effects on Employment					
$E[Y_S - Y_N \| r2]$	0.365		0.407		0.24	
	[-0.045,0.862]		[-0.201,0.889]		[-0.031,0.747]	
$E[Y_S - Y_H \| r5]$	-0.625		-0.407		-0.564	
	[-1,0.102]		[-1,0.223]		[-1,-0.033]	
$E[Y_H - Y_N \| r6, r7]$	-0.152		-0.001		-0.104	
	[-0.309,-0.068]		[-0.289,0.464]		[-0.225,0.092]	
	Panel B: Effects of the reform[b]					
	Δ S	Δ H	Δ S	Δ H	Δ S	Δ H
Δ Graduates	0.009	0	0.009	-0.001	0.011	0
	[0.007,0.012]	[-0.002,0]	[0.005,0.014]	[-0.004,0]	[0.007,0.015]	[-0.002,0]
	ATE	ATT	ATE	ATT	ATE	ATT
Employment	0.001	0.139	0.002	0.263	0.000	-0.013
	[-0.002,0.006]	[-0.208,0.546]	[-0.002,0.007]	[-0.137,0.748]	[-0.002, 0.002]	[-0.183,0.241]

[a] The replication is carried out with college proximity based on universities and faculties that were operating when the respondents were 18 years old.

[b] The reform considered here consists of creating a STEM school in any province that does not have one. The main results for this reform are reported in Table 14.
All quantities are computed conditional on gender, age (and age squared), urbanization of the municipality, survey dummies, regional dummies and dummies for metropolitan areas. Bootstrapped 90%-confidence intervals in squared parenthesis (200 replications).