Who Benefits from General Knowledge?*

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Abstract

While vocational education is meant to provide occupational-specific skills that are directly employable, their returns may be limited in fast-changing economies. Conversely, general education should provide learning skills, but these may have little value at low levels of education. This paper contributes to this debate by exploiting a reform introduced in Spain in 1990 that postponed students' choice between these two educational pathways from age 14 to 16. To identify exogenous changes in this policy, we instrument its staggered implementation with pre-reform province shares of students in general education interacted with cohort fixed effects. Results indicate that, by shifting educational investment from vocational to general education after age 16, the reform improves occupational outcomes and wages. However, these positive effects are concentrated among middle to high-skilled individuals. In contrast, those who acquire only basic general education have worse long-term employment prospects than vocationally-trained individuals.

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1 Introduction

As many economies start experiencing the adverse effects of globalization, automation, and population ageing, a fundamental question has come back to the political debate (OECD 2019): what skills should the educational system provide? Many education experts claim that acquiring general knowledge and learning skills in school may increase individuals' ability to gain further skills, and strengthen workers' adaptability to structural labor market changes (Hampf and Woessmann 2017, Goldin and Katz 2007, Krueger and Kumar 2004). Following precisely this logic, at the beginning of the 1990s, Spain and Finland made compulsory education purely general, while Italy, Germany, Norway and Sweden introduced a more academically-oriented curriculum in the vocational track. Critics of this sort of reform argue that vocationally-trained students may find a job faster than those holding a general qualification, as occupational-specific skills are directly employable. It is also questionable to what extent general education provides marketable skills for individuals who only acquire basic education (Bertrand et al. 2019). Remarkably, in recent years, countries such as Spain and Italy have respectively re-introduced a basic vocational route or reinforced work-based learning in compulsory education.

Providing causal evidence on the relevance of these trade-offs is empirically challenging. To circumvent the issue of self-selection into different educational tracks, researchers have traditionally relied on so-called comprehensive reforms that made compulsory education purely academically-oriented (Zilic 2018, Pekkala et al. 2013, Malamud and Pop-Eleches 2011, Malamud and Pop-Eleches 2010, Pekkarinen et al. 2009, Meghir and Palme 2005). These studies find that the returns to general versus vocational education are either positive or null.[1] However, the majority of these reforms took place in the 1970s. As the structure of the labor market has greatly changed since then, it is possible that the relative returns to general versus vocational education may have also changed. Besides, many of these reforms coincided with an increase in educational attainment,

[1]For instance, Pekkarinen et al. (2009) find that a Finnish comprehensive reform implemented in the 1970s resulted in a 23 percent reduction of the intergenerational income elasticity. On the contrary, both Zilic (2018) and Malamud and Pop-Eleches (2010) find that similar reforms introduced, respectively, in Croatia and Romania, had zero effects on earnings.

making it difficult to disentangle the returns to the type of education acquired from the returns to acquiring more years of education.

This paper analyzes the effects of a comprehensive school reform on educational investment and current labor market outcomes. In 1990, the Spanish government decided to postpone students' choice between general and vocational education from age 14 to 16, and gave school districts up to nine years to implement these changes. While school-district level information on the implementation of the reform is not available, we have personally digitized the province-year data on the share of 14-16 year-old students enrolled in each track before and during the implementation period. To identify its effects, we follow Bertrand et al. (2018), Ahern and Dittmar (2012), and Stevenson (2010), and exploit the fact that provinces starting with a larger share of students enrolled in general education at age 14-16 have to make fewer changes to comply with the reform than those having a larger fraction of students attending vocational programs. Thus, we instrument the staggered implementation of the reform with the pre-reform cross-province variation in the share of students in lower secondary general education, interacted with cohort fixed effects.

The cohorts affected by the reform are those born between 1977 and 1985. We measure their educational choices and labor market status from age 25 onward using the Spanish Labor Force Survey from 2002 to 2017. To analyze wage effects and occupational outcomes, we take advantage of the large sample size offered by the matched employer-employees data set available since 2006. As we do not observe the track attended by these individuals between age 14 and 16, we de-facto identify the intention-to-treat effect of the reform.

This analysis delivers three main findings. First, while the reform has no impact on overall educational attainment, it shifts educational investment from vocational to general education after age 16. In particular, the reform increases by 10 percentage points the share of individuals acquiring general education after age 16, and decreases by 12 percentage points the share of individuals with post-16 vocational education.[2] Second, the reform generates large labor market returns for

[2]Compared to the pre-reform mean, these effects correspond, respectively, to a 27 percent increase in the share of individuals with a post-16 general degree and a 56 percent decrease in the share acquiring a vocational degree after age 16.

the affected individuals. The shift in educational investment from vocational to general education translates into a 2.4 percentage point rise in the probability of being employed in a high-skilled profession, a 3.7 percentage point decrease in the probability of working in a semi-skilled occupation,[3] and a 13 percent increase in monthly wages.[4]

However, our third key finding is that the returns to general versus vocational education are not constant along the educational distribution. The reform reduces the employment prospects of individuals who acquire only basic general education by 8 percentage points, or 12 percent relative to the pre-reform mean - with this effect being statistically different from the effect on individuals with at least a high-school diploma. Starting from the observation that the reform does not affect average educational attainment, in section 7 we extensively discuss why we rule out that these effects are fully explained by composition effects induced by the reform.

All our results are strongly significant. Yet, more fundamentally, the validity of our identification strategy relies on the three assumptions that our chosen instruments are relevant, that is correlated with the endogenous variable, exogenous, meaning uncorrelated with the error term in the main regression, and satisfy the exclusion restriction, that is they do not have any direct effects on the outcomes of interest. A first-stage F-statistic above the rule-of-thumb threshold of 10 provides strong support for the relevance assumption. As for the exogeneity assumption, what is crucial here is that baseline differences in the educational distribution interacted with cohort fixed effects do not capture differential trends across provinces in the outcomes of interest. To support this hypothesis, we perform event-study exercises showing that our instruments are not correlated with the evolution of the outcome variables before the approval of the reform. Also note that our findings are not sensitive to the inclusion of time-varying controls. Combined with favorable Sargan tests on all our main outcomes, these robustness checks strongly support the exogeneity assumption. Finally, to provide evidence in favor of the exclusion restriction, we show that our

[3]Compared to the pre-reform mean, these effects correspond, respectively, to a 24 percent higher probability of being employed in a high-skilled profession, and a 6 percent decrease in the probability of working in a semi-skilled occupation.

[4]As such, the returns to the type of education acquired seem to be slightly larger than those from years of education, estimated at 7-9 percent. As for reforms introducing a more-academic oriented curriculum in the vocational track, Bertrand et al. 2019 find that Norway's "Reform 94" generated labor market returns of around 5 percent.

results are robust to assuming that the instruments are only "plausibly exogenous" (Conley et al. 2012), and that reduced-form estimates are insignificant in a sample in which instruments are only weakly correlated with the endogenous variable (van Kippersluis and Rietveld 2018).

Additional findings shed light on the mechanisms behind the main effects. First, by using the Youth Survey conducted by the Spanish Center for Sociological Research (CIS) and measuring young people's outcomes and aspirations, we find that the reform increases young individuals' perception on the importance of studying. This result suggests that forcing students to stay in general education for two more years changes their subsequent educational choices by strengthening their academic aspirations. Second, our analysis of OECD PIAAC data suggests that this reform significantly increases the literacy skills of affected cohorts. Third, using the Labor Force Survey, we find that the reform increases cross-province migration after age 25. Taken together, these three pieces of evidence suggest that the comprehensive school reform has generated large returns to general education by strengthening educational aspirations, increasing human capital, and inducing individuals to move in search of better job matches.

As for the negative effect of the reform on the employment prospects of low-educated individuals, we find suggestive evidence that this materializes after the financial crisis. In contrast, the reform seems to increase the probability of being employed after the financial crisis for individuals with at least a high-school diploma. Taken together, these findings support the hypothesis that general education helps strengthen workers' adaptability to structural labor market changes. At the same time, they also show that general knowledge provides little marketable value at the bottom of the educational distribution.

Overall, our findings provide several contributions to the literature on the returns to general versus vocational education. First, the Spanish reform does not affect overall educational attainment. As such, relative to other contexts, this is the ideal setting to identify the actual returns to general versus vocational education, without confounding these with the effect of acquiring more years of education (Pekkala et al. 2013, Pekkarinen et al. 2009, Meghir and Palme 2005). Second, this is the first paper to analyze the effects of acquiring general relative to vocational education along the educational distribution. Our finding that individuals with a general basic background have worse employment prospects than those with vocational training complements the recent cross-country evidence provided by Hampf and Woessmann (2017), Hanushek et al. (2017). While these studies find that vocational education only provides short-term gains in youth employment for individuals with high-school qualifications or more, we show that it provides substantial larger long-term benefits at the bottom of the educational distribution.[5] Third, compared to previous studies, our estimates concern the current labor market (Zilic 2018, Malamud and Pop-Eleches 2011, Malamud and Pop-Eleches 2010). As such, they offer policy makers up-to date evidence on the trade-offs between general and vocational education. Our insights from the post-crisis period should be especially valuable in this respect. Finally, our study is complementary to the strands of papers analyzing reforms that increase the general content of vocational tracks (Bertrand et al. 2019, Dustmann et al. 2017, Hall 2016, Hall 2012, Oosterbeek and Webbink 2007) or replace school tracking with tracking by ability (Canaan 2020).

The paper proceeds as follows. Section 2 describes the institutional setting. Section 3 illustrates the empirical strategy, while section 4 introduces the data used in the analysis. Section 5 presents the main results, and section 6 shows the robustness checks. Section 7 discusses the potential mechanisms behind the main findings. Section 8 concludes.

2 Institutional setting

Until the end of the 1980s, the Spanish education system was regulated by the *Ley General de Educacion*, or LGE. In 1990, with the explicit aim of making education more inclusive and raising the competitiveness of the workforce, the Spanish parliament approved a reform, the *Ley Organica de Ordenacion General del Sistema Educativo*, or LOGSE, whose two main elements are highlighted in figure 1. First, this reform postponed students' choice between the vocational and general track from age 14 to 16. To this end, the length of primary school was shortened from 8 to 6 years, and

[5] Interestingly, Silliman and Virtanen (2019) find that, in the context of Finland, even upper secondary vocational education provides long-term labor market benefits compared to general education.

a new comprehensive lower secondary education track was created, lasting from grade 7 (age 12) to grade 10 (age 16). This educational phase takes the name of Compulsory Secondary Education, or ESO in the Spanish acronym. As a result, only upon completion of ESO, students could choose whether to leave school or to enroll into either upper secondary general education or a vocational program, both lasting two years.

Figure 2 compares the courses taught at each level of education in the old and new educational system. As we can observe, the reform did not bring major modifications to the curriculum until grade 8.[6] At secondary level, however, the reform did represent a drastic change for students who, in the absence of the reform, would have chosen to enroll in the vocational track at age 14. In the old system, lower secondary vocational programs lasted two years, and offered several branches of professional specialization. While these vocational programs gave some general knowledge, each of them was meant to provide students with the basic skills to practice a specific profession. Therefore, at least 50 percent of their instruction time was dedicated to this goal. In contrast, the old lower secondary academic track offered a curriculum that closely resembled that of the new comprehensive track (CGFP 2001).

The second element of the reform was an official rise in the compulsory schooling age from 14 to 16. Importantly, this component of the reform has no impact on the educational attainment of affected cohorts. The compulsory schooling age was raised in the entire Spanish territory starting from the school year 1991/1992. Thus, a regression discontinuity design comparing educational attainment of cohorts who turned 14 just before and just after this year appears the most appropriate strategy to identify its effect on educational attainment. Figure 3, constructed using the Labor Force Survey, depicts the relationship between month of birth and average age at highest qualification for the cohorts born between 1975-1979.[7] The x-axis is normalized so that the 0 corresponds to January 1977, as the 1977 cohort was the first to be affected by the rise in the compulsory school-leaving age. In panel A, age at highest qualification is measured at age 35, while in panel B it is

[6]Note that, by exposing young students to older peers, the shortening of primary school may potentially downward bias any positive effect of the reform.

[7]This is the available measure of educational attainment we observe in the Labor Force Survey.

measured in year 2016. No jump in this relationship is visible in either of the two panels.[8]

Two factors may help to explain this null effect. First, from the beginning of the 1980s, the minimum working age in Spain was 16 years old (Bellés-Obrero et al. 2021). Second, already prior to the 1990 rise in the compulsory school-leaving age, students who did not obtain a primary school diploma could not leave school at age 14 and had to enroll into the lower secondary vocational track. Not surprisingly, the school enrollment rate at age 16 was already 95 percent before the implementation of the LOGSE reform.

3 Identification strategy

The government gave school districts up to nine years to introduce the new comprehensive lower secondary track, and each did it at a different time. While school-district level information on the implementation of the reform is not available, we have personally digitized the province-year data on the share of 14-16 year-old students enrolled in each track before and during the period of its implementation. Figure 4 shows the evolution of this variable. Each dot represents the cross-province average of this variable plotted against the cohort that turned 14 in the year this is computed. While this share appears fairly constant at around 70 percent for the cohorts not affected by the reform, it takes off right after the reform is approved, and increases constantly up to 100 percent when the 1985 cohort turns 14. Also note that, while the cross-province variation is fairly constant before the approval of the reform, it increases substantially in the implementation period.

Our aim is to identify the exogenous variation in these mandated changes. One could be tempted to exploit the staggered implementation of the reform to estimate its impact on the affected cohorts, as done by Felgueroso et al. (2014) for instance. Yet, though much has been written on this reform, little is known about what actually determines the within- and cross-province variation in the implementation of the reform.[9] This element of uncertainty, coupled with mounting criticisms

[8]Table A.1 in the appendix complements this graphical analysis by presenting the corresponding regression discontinuity estimates. The coefficient on the impact of the reform is never significant, small in magnitude, and sensitive to the choice of the bandwidth around the January 1977 cutoff.

[9]Space constraints and economic resources may have played a role in influencing the rhythm of its implementation.

of staggered difference-in-difference estimators, have led us to consider an alternative identification strategy (De Chaisemartin and d'Haultfoeuille 2020, Borusyak et al. 2018, Goodman-Bacon 2018).

Specifically, we follow Bertrand et al. (2018), Ahern and Dittmar (2012), and Stevenson (2010) and implement an IV strategy, exploiting the fact that provinces starting with a larger share of students enrolled in the lower secondary general track have to make fewer changes to meet the government's deadline than those having a larger share of students in vocational programs. Thus, we instrument the staggered implementation of the reform with the pre-reform cross-province variation in the share of students enrolled in lower secondary general education, interacted with cohort fixed effects. This corresponds to estimating the following 2SLS model:

$$Y_{icpy} = \alpha + X'_{icp}\pi + \delta_c + \gamma_p + \theta_y + \beta \; ShareStudGen_{cp} + u_{icpy}, \tag{1}$$

where i is an individual belonging to one of the c cohorts affected by the reform, 1977-1985, born in province p,[10] and whose outcome is observed in year y. Y_{icpy} is the outcome of interest, including educational choices, labor market and occupational outcomes, and wages. As for the regressors included in the right-hand-side, X_{icp} is a vector of province-cohort time-varying factors that may be correlated with the implementation of the reform, such as the share of left-wing municipalities, and log GDP per capita, both measured when individual i is 14, and log cohort size. Besides, we control for factors that may affect educational choices on top of this reform, such as individuals'

As such, cohort size and province-level GDP per capita may be correlated with the implementation of the reform. At the same time, the reform was passed by a left-wing government, suggesting that left-wing municipalities might be quicker in implementing the reform. Another concern with this identification strategy is the potential self-selection into/out of the treatment. Some students may have opted for the new lower-secondary general track even when the vocational one was still available in their school district if they perceived that this could improve their opportunities later in life. On the contrary, as private schools had more autonomy regarding the implementation of the reform, some students may have fled from public schools to escape the reform. Table A.2, in the appendix, investigates how these variables correlate with our proxy for the implementation of the reform. Interestingly, the share of left-wing municipalities is even negatively correlated with the implementation of the reform. And overall, a clear pattern does not stand out from this table. As such, our worry is that the implementation of the reform may have been driven by a mix of observable and unobservable time-varying factors that directly affect the outcomes of interest.

[10] In the Labor Force Survey, we observe that two thirds of individuals in the affected cohorts live in their province of birth when attending high school. Although using the province of birth to link individuals to the treatment may introduce some measurement error, this allows us to circumvent any issue of endogenous migration.

gender, province unemployment rate, share of population with high-school education or more, higher-education wage premium, and the employment share in construction and manufacturing, all measured in province p when individual i is 16.[11] Next, we include cohort δ_c, birth province γ_p, and year-of-interview fixed effects θ_y. The main regressor of interest is $ShareStudGen_{cp}$, which is measured as the share of 14-16 year-old students enrolled in the lower secondary general track when cohort c is 14, as described above. We instrument it with $\sum_{c=1978}^{1985} ShareStudGen_{1976p} * \delta_c$[12], that is cohort fixed effects interacted with $ShareStudGen_{1976p}$, the share of 14-16 year-old students enrolled in general education in the last year before the reform was approved - when the 1976 cohort turned 14.

The corresponding first stage regression looks as follows:

$$\begin{aligned} ShareStudGen_{icp} &= \alpha + X'_{icp}\pi + \delta_c + \gamma_p + \theta_y \\ &+ \sum_{c=1978}^{1985} \beta_c (ShareStudGen_{1976p} * \delta_c) + \epsilon_{icpy}, \end{aligned} \tag{2}$$

with $c = 1978, \cdots, 1985$. Finally, in all regressions, we use heteroskedasticy-robust standard errors clustered at the province-of-birth level - 52 groups.[13]

The validity of this identification strategy relies on the three assumptions that the instruments chosen are relevant, exogenous, and satisfy the exclusion restriction. The assumption of relevance implies that the instruments have to be correlated with the endogenous variable. To verify whether this is the case, we will check that the F-statistic of the first stage regression, equation 2, is above the rule-of-thumb threshold of 10. As we use clustered standard errors, in what follows, we will refer to the Kleibergen-Paap F-statistic.

Second, the exogeneity assumption implies that the instruments should not be correlated with

[11] In the appendix, we also report the estimates of regressions that do not include these controls.

[12] Note the abuse of notation in the term $\sum_{c=1978}^{1985} ShareStudGen_{1976p} * \delta_c$. To be consistent in the representation of fixed effects, we should write $ShareStudGen_{1976p} * \delta_c$. However, here we choose to use the summation as we want to emphasize that we are using 8 instruments, that is, $ShareStudGen_{1976p}$ interacted with 8 cohort dummies (1978-1985).

[13] Note that, in equation 2, the interaction term between $ShareStudGen_{1976p}$ and the fixed effect for cohort 1977 is excluded to avoid collinearity with the constant.

the error term in the main regression. Here one may be worried that the pre-reform cross-province variation in the share of students enrolled in general education is not randomly assigned. Table A.3 in the appendix shows, for instance, that this variable is positively correlated with province per-capita educational expenditures or the share of individuals with at least a high-school degree. However, what is important in this context is that the interaction terms between the pre-reform cross-province variation in the share of students enrolled in general education and cohort fixed effects do not capture differential trends across provinces in the outcomes of interest. To support this hypothesis, we will perform the following event-study exercise:

$$Y_{icpy} = \alpha + X'_{icp}\pi + \delta_c + \gamma_p + \theta_y + \sum_{c=1971}^{1985} \beta_c(ShareStudGen_{1976p} * \delta_c) + e_{icpy} \quad (3)$$

In practice, this corresponds to estimating the dynamic reduced-form version of equation 1 augmented with interaction terms between the $ShareStudGen_{1976p}$ and fixed effects for the last six cohorts not affected by the reform.[14] Finding that the leads of the reform are insignificant should support the claim that our instruments do not capture pre-reform differential trends across provinces in the outcomes of interest. To further investigate the role of cross-province differential trends coinciding with the implementation of the reform, we will compare the results with and without province-cohort time-varying controls. Moreover, as we have more than one instrument, we can also test the hypothesis of exogeneity by performing the Sargan test on overidentifying restrictions.

Finally, our instruments satisfy the exclusion restriction if they do not have a direct effect on the outcomes of interest. We will bring two pieces of evidence to support this assumption. First, following van Kippersluis and Rietveld (2018), Angrist et al. (2010), and Altonji et al. (2005), we will exploit the event studies to show that, in a sample where our instruments prove to be weak in the first stage, their reduced-form effects are also insignificant. Second, we will show that our results are also robust when we relax the exclusion restriction and treat our instrument as only

[14] Note that the interaction term between $ShareStudGen_{1976p}$ and the fixed effect for cohort 1970 is set as the reference group. Also, note that this type of test has been recently proposed in the Bartik instrument literature (Goldsmith-Pinkham et al. 2020).

"plausibly exogenous" (Conley et al. 2012).

4 Data

To measure our main outcomes of interest, we use the Spanish Labor Force Survey,[15] LFS hereafter, and the Continuous Sample of Working Histories,[16] or CSWH.

The cohorts affected by the reform are those born between 1977 and 1985. We measure their educational choices and labor market status from age 25 onward using the LFS from 2002 to 2017.[17] Specifically, from this data set, we draw information on age at highest qualification, highest level of education attained, type of qualification obtained, labor market status - whether the respondent participates in the labor force, i.e. is active, and whether he/she is unemployed or employed - birth province and province of residence.

To analyze wage effects and occupational outcomes, we take advantage of the large sample size offered by the CSWH, the Spanish matched employer-employee data. Each yearly wave consists of a 4 percent non-stratified random sample of individuals who are registered with the Social Security in the reference year.[18] For each individual, the CSWH provides information on occupation held, type and duration of job contract, sector of activity, date of entering or leaving the labor market, part-time or full-time status, firm size, and establishment characteristics. Moreover, the database provides information on monthly income from tax files that have been matched to the social security sample. We have access to the matched sample for the period 2006-2017. We aggregate occupations in three categories, namely high-skilled, semi-skilled and low-skilled occupations. The first group comprises managerial and professional occupations, the second includes technical and administrative occupations, and the third one elementary and auxiliary professions.[19]

[15] *Encuesta de Población Activa* in Spanish.

[16] *Muestra Continua de Vidas Laborales* in Spanish.

[17] We start from age 25 as we want to measure educational outcomes when the majority of individuals should have concluded their educational career. Also note that when performing event studies we will use the LFS from 1995, as we will consider pre-reform cohorts as well.

[18] Once individuals enter the data set, they remain in the sample for all the subsequent years they are registered with the Social Security. The CSWH reconstructs their labor market histories back to 1967. Finally, new members are added in each wave, so that the sample is always representative of the active population.

[19] While the LFS also provides self-reported occupational outcomes, the administrative information provided by the

To investigate the mechanisms behind the main results, we further exploit two additional sources of data. First, we use the Youth Survey, or *Sondeo de la Juventud*, to study the impact of the reform on academic aspirations. This quarterly survey, conducted by the Centre for Sociological Research (CIS), collects broad information on youth lifestyles on a nationally representative random sample of individuals aged 15–29. We specifically exploit the 1996, 1997, 2001-2008, and 2012 waves, as they include a question on the importance of studying in the respondent' life. Next, we use the OECD-PIAAC Survey of Adults Skills to study the impact of the reform on skill levels. This survey is carried out every 10 years, and Spain participated only in the 2012 round. While the sample size is limited to 5000 adults (aged 16 to 65) and the data sets provides the region but not the province of birth, to the best of our knowledge, this is the only data set providing information on cognitive skills for the Spanish population. Specifically, the 2012 survey for Spain assesses skills levels in two domains: literacy, defined as the ability to understand, evaluate, use and engage with written texts, to participate in society, to achieve one's goals, and to develop one's knowledge and potential; and numeracy, referring to the ability to access, use, interpret, and communicate mathematical information and ideas in order to engage in and manage the mathematical demands of a range of situations in adult life. The PIAAC data report 10 plausible values for literacy and 10 plausible values for numeracy on a 500-point scale, obtained as predictions of individuals' skill levels from an item response model (Wu 2005). Each of these 10 values is a good measure of individuals' skills and previous studies have used only the first plausible value (Yao 2019, Hanushek et al. 2015). For completeness, we report the results of 10 separate regressions on each plausible value per skill domain. In each of these data sets, we link respondents to the treatment variable through their year and province (region) of birth.

As for the control variables, we measure them as follows. Data on variables potentially correlated with the implementation of the reform are measured by birth province p at the time cohort c turns 14, and comprise: the share of left-wing municipalities taken from the records of munic-

CSWH is more reliable. Second, the occupational classification employed in the LFS was reformed in 2011, impinging on comparability over the estimation period. Nonetheless, taking into account these caveats, in the appendix we also report the LFS estimates of the impact of the reform on occupational outcomes. As for wage data, we rely only on the CSWH, as in the LFS they are only available in the restricted, non-free-access version.

ipal electoral results;[20] GDP per capita drawn from Spanish regional accounts, and cohort size measured from Birth registries. All these variables - with the exception of cohort size which is measured at birth - are measured over the period 1991-1999 (1984-1999 in the event studies). Next, besides gender, potential drivers of educational decisions are measured by birth province p at the time cohort c turns 16 and comprise: the unemployment rate, the share of population with high-school education or more, the employment share in construction and manufacturing, and the university wage premium, all of which are taken from the LFS. While the first three are available for all cohorts from 1993 to 2001 (1986-2001 in the event studies), data on the university wage premium are only available for 1995, so that we interact this province-level value with cohort fixed effects.

Table 1 provides summary statistics for both outcomes and control variables. Note that the question on age at highest qualification has a larger non-response rate than other variables in the LFS, though this does not appear to be correlated with respondents' observable characteristics. Also note that the CSWH does not provide occupational data for the Basque Country and Navarra.

5 Main findings

We start this section by introducing the first stage regression, estimated using the Labor Force Survey, and reported in Table 2. Two things are worth noticing in this table. First, the direction and magnitude of the effects: the coefficients on the interaction terms between cohort fixed effects and the pre-reform share of students enrolled in lower secondary general education are negative and increase in magnitude for younger cohorts. The negative sign simply indicates that provinces that had a larger share of students enrolled in general education had to do smaller changes to comply with the reform. The increasing magnitude instead reflects the fact that provinces are progressively getting further away from their pre-reform share, and closer to the full implementation of the reform (i.e., 100% of students in general education). Second, the Kleibergen-Paap F-statistic,

[20] Note that municipal elections take place every 4 years, so that we assign the value of the last election to cohorts that turn 14 when no election happens.

displayed at the bottom of the table, is three times larger than the rule-of-thumb threshold of 10, which supports the hypothesis that the chosen instruments are relevant.[21]

We next present our key findings. All tables of results display the OLS results in the first row, and the IV estimates in the second one. To interpret the magnitude of the effects, we take into account the fact that the reform represents an average cross-province increase of 30 percentage points (from 70 percent to 100 percent) in the share of students in lower secondary general education. For the sake of space, from now on, we refer to this as the effect of the reform. Also note that the point estimates reported in the regression tables correspond to a 100 percent increase in the share of students in lower secondary general education.

While the reform has no significant effect on overall educational attainment,[22] Table 3 shows that it does affect the type of education individuals acquire upon completion of the new comprehensive track. In particular, column 2 of Table 3 shows that the comprehensive reform leads to a 10 percentage point increase (0.355*0.30) in the share of individuals acquiring general education after age 16, or 27 percent compared to the pre-reform mean - reported at the bottom of the table. And column 3 complements this result by showing that the reform leads to a 12 percentage point decrease in the share of individuals acquiring vocational studies after age 16, or 56 percent compared to the pre-reform mean.[23]

In turn, this shift in educational investment from vocational to general education translates into important labor market effects. While Table 4 reports no average effect at the extensive margin,

[21]Note that the F-statistic slightly changes when considering the impact on occupation and wages, as the first stage regression is estimated on the sample of employed individuals.

[22]Opponents of this type of reform stress that an academic-oriented curriculum could discourage less motivated students, and increase the risk of school dropout (Bertrand et al. 2019, Felgueroso et al. 2014). On the other hand, proponents of this type of reform claim that a comprehensive environment may offer a better learning environment for every student, and as such boost academic aspirations. Columns 1-3 of Table A.4, in the appendix, show that the reform has no impact on educational attainment at any level of education, suggesting that on average neither of these two effects seems to prevail.

[23]Columns 4-6 of Table A.4 further delve into these effects to show that this shift in educational investment from vocational to general education happens both at secondary and tertiary level. Specifically, column 4 shows that the reform increases the share of individuals with a general high-school diploma by 6 percentage points, or 49 percent compared to the pre-reform mean, while decreasing the share with a vocational high-school diploma by roughly the same amount. At tertiary level, the reform bring a 4 percentage point increase in the share of individuals with a tertiary general degree - 17 percent compared to the pre-reform mean - and a 7 percentage point reduction in the share of individuals with a tertiary vocational degree - 63 percent compared to the pre-reform mean.

Table 5 shows that the reform shifts the occupational distribution from semi-skilled to high-skilled occupations, bringing large complementary wage returns.[24] Specifically, columns 1-3 show that the reform leads to a 2.4 percentage point increase in the probability of working in a high-skilled occupation (24 percent relative to pre-reform mean), a 3.7 percentage point decrease in the probability of being employed in a semi-skilled occupation (6 percent compared to the pre-reform mean), and no average effect on low-skilled occupations.[25] Accordingly, column 4 shows that the reform increases monthly wages by 13 percent.

However, general education does not seem to offer such large relative returns all along the educational distribution. Figure 5 presents heterogeneous effects by level of education. According to these results, the reform leads to a 8 percentage point decrease (12 percent relative to the pre-reform mean) in employment prospects of individuals who leave school before age 16 - low-educated individuals hereafter - with this effect being both significant at ten percent and significantly different from that on individuals with at least a high-school diploma.[26]

Before delving into the potential mechanisms explaining these results,[27] we first provide evidence to support the validity of our identification strategy.

6 Robustness checks

This section has two main goals. First, it discusses the validity of our identification strategy. Second, it offers a discussion on the comparison between OLS and IV estimates.

[24] Note that to analyze intensive margin effects, we use the CSWH data set. While this is only representative of the active population in each year of reference, the null effect of the reform on the extensive margin should limit any concerns about composition effects on the CSWH sample. Besides, Table A.5 in the appendix shows that we obtain qualitative similar results when using the LFS to analyze occupational outcomes.

[25] Since the CSWH provides information on individuals' occupation from the year they enter the labor market until 2017, we can also analyze the effect of the reform on occupational mobility. However, the CSWH only identifies 10 occupation groups and, as in every administrative data, does not measure promotions within the same occupation. Therefore, occupational changes are quite rare in our sample, with more than 50% of individuals holding the same occupation over the period observed. Nevertheless, in Tables A.6 and A.7, we look at occupational mobility between the ages of 25-30 and 35-40, or 25-28 and 35-38 and find no significant impact of the reform on this outcome.

[26] Table A.8 in the appendix reports the corresponding detailed regression results.

[27] In particular, we will extensively discuss whether the heterogeneous effects we find may simply reflect composition effects induced by the reform.

The validity of this identification strategy relies on the assumptions that the instruments chosen are relevant, exogenous, and satisfy the exclusion restriction. Having proven the relevance of our instruments in the previous section, here we focus on the other two assumptions.

Instrument exogeneity. The main concern in this context is that the pre-reform cross-province variation in the share of students in lower secondary general education interacted with cohort fixed effects may capture underlying differential trends across provinces in the outcomes of interest. To provide evidence that this is not the case, we first test for the presence of pre-reform trends by performing the event-study exercises described in equation 3. Figures 6-8 plot the estimates of the leads and lags of the reform for each variable studied, together with 95 percent confidence intervals.[28] Reassuringly, almost all the estimates of the leads of the reform are insignificant across the different graphs. In contrast, the reduced-form dynamic estimates of the reform tend to be significant in accordance with the IV results.[29] These dynamics exclude that the instruments are correlated with unobserved factors that systematically influence the outcome variables.[30] Specifically, the event-study exercises exclude that our instruments capture pre-reform differential trends across provinces in the outcomes analyzed.

Yet, one may still argue that provinces with different pre-reform shares of students in general education follow different trends that coincide with the implementation of the reform but are not a consequence of it. While we cannot directly test for this,[31] in the appendix, Tables A.9-A.11, we show that our estimates are practically unchanged when time-varying province controls are excluded from the regressions. To us, this should be especially useful to address concerns related to post-reform trends.[32] Further backing the assumption of exogeneity, note that our instruments pass the Sargan test for over-identifying restrictions in all our main regressions.

[28]Figure A.1 in the appendix presents the event studies for educational outcomes at both secondary and tertiary level. Note that, in all these event studies, data on educational level in the provinces of Ceuta and Melilla are missing for cohorts 1970 and 1971.

[29]The sign and magnitude of the coefficients reflect the dynamics of the first stage regression.

[30]Note also that the lack of impact of the reform on previous cohorts suggests that this policy did not generate large general equilibrium effects.

[31]Note that province-cohort trends would be highly collinear with our instruments.

[32]To provide further evidence on the source of variation used in our identification strategy, in Tables A.12-A.14, we estimate the impact of the reform on our main outcomes by collapsing the data at the cohort-province level and show that the results are basically unchanged.

Exclusion restriction. While this assumption cannot be formally tested, we follow two approaches proposed by the literature to investigate its validity. The first one is what van Kippersluis and Rietveld (2018) call the "zero-first stage test", and is based on the intuition that, in a sample where the instruments prove to be weak in the first stage, the reduce-form estimates should not be significant for the exclusion restriction to hold. We implement this "zero-first stage test" on the pre-reform cohorts included in the event studies. Appendix Table A.15 shows that the F-statistics associated to a first-stage regression run on these cohorts is below the rule-of-thumb value of 10. In other words, our instruments are only weakly associated to the share of students in general education when the 1970-1975 cohorts were 14. In turn, the insignificant leads in the event studies indicate that the instruments are not directly correlated with the outcomes of these cohorts. Thus, our instruments seem to pass the "zero-first stage test".

To provide further evidence that the exclusion restriction is satisfied in our setting, we follow a second approached proposed by Conley et al. (2012) and investigate if our findings are robust to assuming that the instruments are only "plausibly exogenous". The idea here is to assume that the true model is given by:

$$\begin{aligned} Y_{icpy} &= \alpha + X'_{icp}\pi + \delta_c + \gamma_p + \theta_y \\ &+ \beta\ ShareStudGen_{cp} + \sum_{c=1978}^{1985} \mu_c (ShareStudGen_{1976p} * \delta_c) + u_{icpy}, \end{aligned} \tag{4}$$

that is we assume that our instruments have a direct effect on the outcomes give by the μ_c. To derive the β associated to the different values of the μ_c, we implement the "union of confidence intervals" method proposed by Conley et al. (2012). The starting point is to choose a prior for the support of the μ_c. While we could not use off-the-shelves estimates to form these priors, we have again exploited the event studies to select them. Specifically, for each outcome considered, we have estimated the β associated to seven μ_c comprised between the smallest and largest values of the confidence intervals on the leads of the reform. Figure 9 shows that our IV estimates remain significant throughout a vast range of priors over the direct effects of the instruments on the outcomes. In most cases, our results are robust even to assuming that the direct effect of the instruments is equal to the largest bounds of the confidence intervals of the leads of the reform.

In sum, the event studies, coupled with these additional robustness checks, support the hypothesis that the instruments only capture the exogenous variation in mandated changes of the endogenous variable.

OLS versus IV estimates. The previous section has shown that the IV results are systematically larger than the OLS ones. Two elements could contribute to explain this pattern. First, the OLS estimates could be downward-biased. This will be the case if the provinces that were systematically leading the implementation of the reform were the ones where individuals gain lower returns from general education in the labor market. We consider this a valid potential explanation as Table A.2 does not exclude that the implementation of the reform is positively correlated with a mix of observable and unobservable factors that negatively affect the outcomes of interest. Second, under a monotonicity assumption, the IV estimates can be interpreted as LATE, or the effect of the reform on those individuals that change their behaviour because of this policy.[33] Importantly, as stated by Card (2001), IV estimates based on supply-side innovations identify returns to education for a subset of individuals with relatively high returns to education. In other words, our IV estimates are likely to capture the impact on individuals with higher than average actual returns but lower than average perceived gains to general education. This is consistent with the hypothesis that the reform changes students' educational choices post-16 by helping them making better informed decisions - which we further analyze below.

In sum, the two arguments appear plausible and they could both contribute to explain why the IV estimates are larger than OLS ones.

[33]Note that in our setting the monotonicity assumption holds insofar as provinces with a smaller pre-reform share of students in general education have to make monotonically more changes to comply with the reform than those with a larger share.

7 Proposed mechanisms

This section aims to discuss the interpretation of the main findings and shed light on the mechanisms behind them. The Spanish reform has generated substantial changes in educational choices and labor market outcomes of affected cohorts. Supporters of this type of reforms generally claim that their beneficial effects may be due to a bundle of factors, including the exposure to better peers and teachers, more time to obtain information on one's own ability, as well as the actual acquisition of general skills or the signaling value of a general degree. While the data at hand do not allow us to identify peer and teacher effects, in what follows we exploit a series of different data sets to investigate the role played by students' aspirations and skills. Finally, we discuss potential explanations for the large negative effects on individuals acquiring only basic general education.

Educational aspirations. Advocates of comprehensive school reforms believe that delaying track choice may help students make better informed decisions regarding what they want to study. While we do not have information on detailed students' preferences, we explore this hypothesis by exploiting the Youth Survey, which repeatedly monitor young people's perception on the importance of studying. Table 6 shows that the reform increases by 16 percent the probability that affected cohorts answer positively to the question "How important is studying in your life?", when respondents are 18 or younger. Remarkably, this effect materializes around the age when individuals make their educational choices, while vanishes at later ages, when individuals have presumably completed their educational career (column 2).[34] While these effects may not have been large enough to translate into increased educational attainment, they appear consistent with the hypothesis that comprehensive school reforms help students strengthening their academic aspirations, potentially by prolonging their exposure to better peers and teachers.

Human capital. Next, we want to investigate the role placed by skill acquisition and cognitive ability in generating large relative returns to general education. To this end, we start with an analysis of OECD PIAAC data to investigate whether these results reflect an increase in skill levels

[34]Note that the small sample size make this evidence at most suggestive.

or mostly a signaling effect of an academic qualification.[35] Table 7 shows that the reform seems to increase individuals' literacy skills, as the majority of the estimates for the 10 plausible values are positive and significant. Estimates in Panel 2 also point to a positive effect on numeracy skills, though the coefficients are smaller and imprecisely estimated.[36] These findings suggest that the additional provision of general education brought about by the reform has translated into an actual rise in general skills.

Next, we exploit the fact that the LFS provides both respondents' province of birth and their province of residence to study whether the reform enhances the mobility of affected cohorts. Column 1 of Table 8 shows that this is not the case at younger ages, which further limits any concern about endogenous migration decisions to take advantage/escape from the new system.[37] However, the reform significantly increases the probability of migrating to a different province from age 25 onward by 29 percent compared to the pre-reform mean. This result speaks to the literature linking cognitive ability to risk attitudes and migration decisions (Dohmen et al. 2010, Jaeger et al. 2010).

Taken together, these two findings suggest that a combination of increased human capital and a higher propensity to move in search of better job matches contribute to explain the large average relative returns to general education.[38] Figure A.2, in the appendix, further adds that the migration effects are driven by middle- to highly-educated individuals, backing the hypothesis of heterogeneous effects along the educational distribution.[39]

Labor market outcomes of low educated individuals. The final aim of this section is to understand what explains the negative labor market impact of the reform on students who acquire only lower secondary general education. We consider two potential explanations, one that relies on

[35]Note that this analysis is performed at the regional level. As Spain has only 18 regions, the table reports both heteroskedasticity-robust standard errors clustered at the regional level, and wild-bootstrap p-values.

[36]Remarkably, these results resemble those of Pekkala et al. (2013) who find that the Finnish comprehensive school reform implemented in the 1977 also led to an average increase in literacy skills, but weaker effects on numeracy ones.

[37]Importantly, we also considered the possibility that the reform affects the probability of migrating abroad. While cohort-province data for this outcome are not available, the Spanish Statistical Agency provides national figures by age and year, starting from 2008. Since then, the emigration rate for the affected cohort oscillates between 0.2 and 0.5 percent, suggesting that our results could hardly be explained by any selection effect into migration.

[38]We also studied the impact of the reform on job mobility, the probability of having a permanent contract, and hours worked, but did not find significant results on these margins.

[39]Table A.16 shows the corresponding detailed regression results. As for the PIAAC analysis, note that the small sample size does not allow us to perform any heterogeneity analysis.

composition effects, the other that focuses on the relative market value of general and vocational education at different levels of the educational distribution. The composition argument would point to the fact that, by forcing students to attend the academic track, the reform may have discouraged some from continuing studying. If these students are negatively selected, this could explain their worse performance in the labor market. Two elements are worth noting when considering this explanation. First, the reform does not affect average educational attainment. Hence, for composition effects to fully explain the negative labor market effects, we should assume that two opposite dynamics would take place at the same time. On the one hand, some students, that would have left school after lower secondary education in the old system, would continue into upper secondary education in the new system. On the other hand, another part of the student population who, in the absence of the reform, would have continued into upper secondary school, would now decide to leave school. In other words, the reform should have generated both positive aspiration effects and discouragement effects of equal magnitude. If anything, the results on the Youth Survey exclude large discouragement effects. Second, discouragement effects may particularly affect those students who, in the absence of the reform, would have attended the vocational track and then continued into upper secondary professional education. But continuing into the upper secondary vocational track is still an option after the reform, which make these effects even more implausible. Taken together, all these elements make it unlikely that composition effects fully explain the negative labor market impact of the reform on students who acquire only lower secondary general education.[40]

The alternative explanation does not rely on large discouragement effects and simply assumes that the majority of students who leave school at age 16 after the reform would have done so even in the absence of the reform. It focuses instead on the relative market value of general and vocational education at different levels of the educational distribution. To further explore this channel,

[40] A valuable information to further explore this channel would be a proxy for primary-school ability, but unfortunately this is not available in the context studied. Alternatively, we considered parental education, but two issues prevent us from analyzing the impact of the policy on this variable. First, it is available only for those individuals who live with their parents, which represent at most 50 percent of individuals in our sample. More importantly, more than 95 percent of individuals for which we have this information have low-educated parents. In other words, there is not enough variation in this variable to conduct a meaningful analysis.

we analyze the timing through which heterogeneous effects materialize and how this correlates with the occurrence of the financial crisis. First, Figure 10 compares the impact of the reform on occupational outcomes and wages by level of education.[41] While acquiring general rather than vocational education translates into a shift from semi-skilled to high-skilled occupations for middle- to highly-educated individuals, it moves the occupational distribution of low-educated individuals towards low-skilled occupations. As shown in Figure 11, these were the most strongly affected in the recession following the financial crisis. Remarkably, Figures 12 and 13 provide suggestive evidence that employment trajectories of low- and middle- to highly-educated individuals start diverging precisely after the financial crisis.[42] On the one hand, Figure 12 shows that, from 2009 onward - compared to the period 2002-2008 - the reform increases the probability of being employed for middle- to highly-educated individuals by 8 percent compared to the pre-reform mean.[43] On the other hand, Figure 13 shows that after the financial crisis the reform decreases employment prospects of low-educated individuals.

In sum, it is unlikely that composition effects fully explain the large negative impact of the reform on low-educated individuals. In contrast, the dynamics of the effects on employment prospects suggest that low levels of general education offer little resilience in the labor market, especially in periods of economic turmoil.[44]

[41] Table A.17 shows the corresponding detailed regression results. Note that in the CSWH we proxy individuals' level of education by the age at which they have their first full-time job. This is because the CSWH only reports the level of education individuals have when they first register with the Social Security. We assume that low-educated individuals are those who have their first full-time job at age 18 or earlier, while individuals with high-school education or more are those who entered the labor market after age of 18. In Appendix Figures A.3 and A.4, we further check that our results are robust to the choice of the age cutoff, whether this is 16 or 17.

[42] Admittedly, these results are marginally insignificant when employing the more conservative Bonferroni correction procedure to account for multiple hypothesis testing.

[43] Tables A.18 and A.19, in the appendix, report the corresponding detailed regression results. Note that the Kleibergen-Paap F-statistic is below 10 when estimating the impact of the reform before the crisis, suggesting that we should interpret the results for this period with some caution.

[44] One may also be worried that the reform could have temporarily worsen the quality of education, causing especially large detrimental effects for individuals who only acquired lower secondary education. Yet, one of the prerequisites for the implementation of the reform was the maintenance of a fix student-teacher ratio. Still, some schools and teachers could need time to adjust to the reform. However, the fact that employment prospects of low-educated individuals deteriorate above all after the financial crisis seems to exclude that a temporary worsening of school quality may have played a major role in explained these dynamics.

8 Conclusion

During the 1990s, Spain implements a major reform that postponed students' choice between general and vocational education from age 14 to 16. We exploit this setting to bring new key insights to the longstanding debate on the trade-offs between general and vocational education. To identify the educational and labor market effects of the Spanish comprehensive reform, we instrument its staggered implementation with the pre-reform cross-province variation in the share of students in general education, interacted with cohort fixed effects.

This analysis delivers three main findings. First, the reform substantially changes educational choices after age 16, by increasing the share of individuals with an upper secondary or tertiary general degree, and decreasing the proportion of those with advanced vocational degrees. Second, general education brings large relative returns in the labor market, in the form of a higher probability of being employed in a high-skilled occupation and higher monthly wages. Moreover, in the aftermath of the financial crisis, the reform seems to increase the probability of being employed for individuals with at least a high-school diploma. However, our third key finding is that only middle to highly-educated individuals enjoy these relative returns from general education. The reform significantly worsens the employment prospects of individuals who only acquire basic general education, both at the extensive and intensive margin. As the cohorts affected by the reform are at most 40 years old, the setting analyzed does not allow us to trace the life-cycle effects of acquiring general versus vocational education, and quantify the duration of such negative effects (Hampf and Woessmann 2017). Moreover, giving the period of time analyzed (2002-2017), any breakdown of the effects by age could be confounded with the impact of the financial crisis. Yet, it is unlikely that these will only be temporary effects, as they seem to materialize after the financial crisis, when the affected cohorts are already in their 30s.

Overall, these results offer two insights. On the one hand, they show that general education provides the learning skills that strengthen workers' adaptability to structural labor market changes. On the other hand, they suggest that general knowledge provides little marketable value at the bottom of the educational distribution. These results are consistent with the consensus expressed by the literature on returns to training programs (Card et al. 2018) that low-educated individuals would benefit from interventions focused on human capital accumulation, and that these benefits may be larger during recessions.

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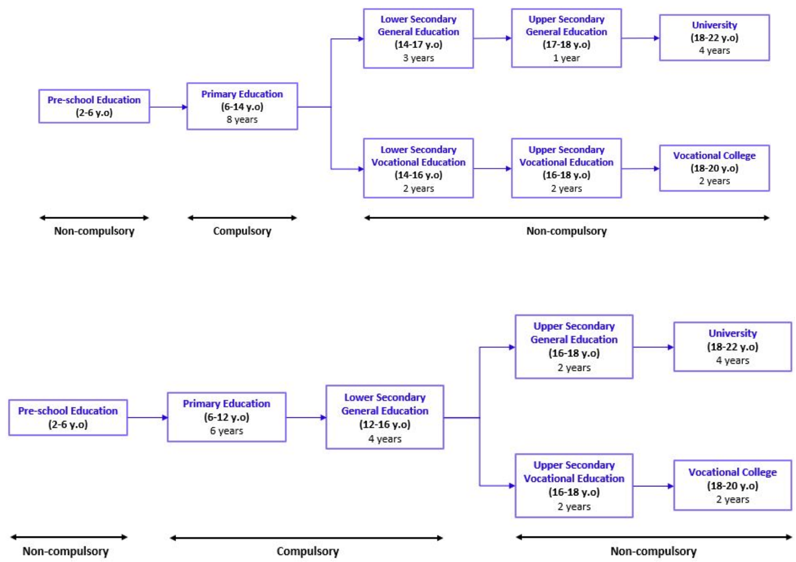
9 Graphs and tables

Figure 1: **The educational system before and after the reform**



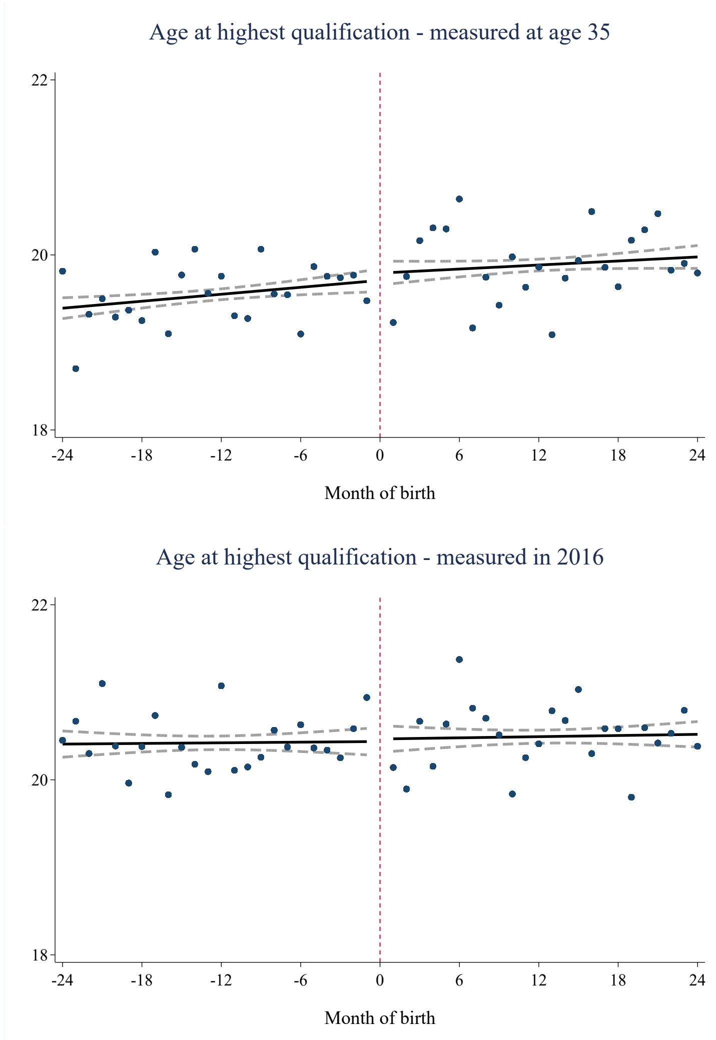
Source: Spanish Ministry of Education.
Note: This figure presents a schematic representation of the Spanish educational system. The top panel represents the old system, while the bottom one shows the new one.

Figure 2: **Courses taught before and after the reform**

<table>
<tr><th colspan="2">Old System (LGE)</th><th colspan="2">New System (LOGSE)</th></tr>
<tr><td colspan="2">Primary Education
(8 years)</td><td colspan="2">Primary Education
(6 years)</td></tr>
<tr><td colspan="2">All years
- Spanish language
- Mathematics
- Natural and social sciences
- Physical education
- Arts
- Religious education
Third Cycle
- Foreign language
- Technological education</td><td colspan="2">All years
- Spanish language
- Mathematics
- Natural and social sciences
- Physical education
- Arts
- Religious education
Second and Third Cycle
- Foreign language</td></tr>
<tr><td>Lower Secondary Vocational Education
(2 years)</td><td>Lower Secondary General Education
(3 years)</td><td colspan="2">Lower Secondary General Education
(4 years)</td></tr>
<tr><td>- Occupation-specific subjects (50%)
- Foreign language
- Physical education
- Religious education
- Spanish language and literature
- History</td><td>All years:
- Foreign language
- Physical education
- Religious education
- Spanish language and literature
- Mathematics
- Natural and social sciences
1st year:
- Music
- Arts
2nd year
- Latin
3rd year
- Philosophy
- Elective: Latin/Greek or Sciences</td><td colspan="2">- Foreign language
- Physical education
- Religious education
- Spanish language and literature
- Mathematics
- Natural and social sciences
- Visual and manual arts
- Music
- Technology</td></tr>
<tr><td>Upper Secondary Vocational Education
(2 years)</td><td>Upper Secondary General Education
(1 year)</td><td>Upper Secondary Vocational Education
(2 years)</td><td>Upper Secondary General Education
(2 years)</td></tr>
<tr><td>- Occupation-specific subjects</td><td>Compulsory:
- Spanish language and literature
- Philosophy
- Foreign language
4 Branches:
- Science and Engineering
- Health Science
- Arts
- Social Sciences</td><td>- Occupation-specific subjects</td><td>Compulsory:
-Spanish language and literature
- Philosophy/ History
- Physical education
- Foreign language
- Religious education
4 Branches:
- Science and Engineering
- Health Science
- Arts
- Social Sciences</td></tr>
<tr><td>Vocational College
(2 years)</td><td>University
(4 years)</td><td>Vocational College
(2 years)</td><td>University
(4 years)</td></tr>
</table>

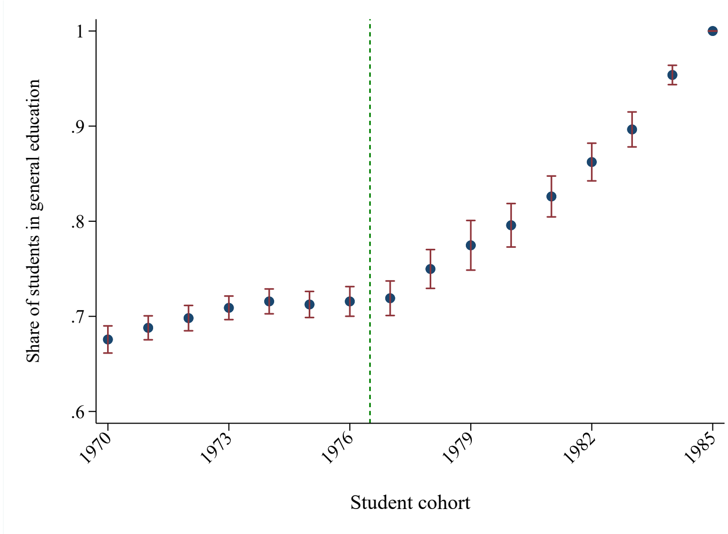
Source: Several laws (Real Decreto 1179/1992, Real Decreto 1007/1991, Real Decreto 1006/1991, Real Decreto 3087/1982, Real Decreto 710/1982, Real Decreto 69/1981, Decreto 707/1976, and Decreto 160/1975).
Note: This figure presents a comparison of the courses taught at each educational level during the old (LGE) and new (LOGSE) system.

Figure 3: **Increase in compulsory schooling and age at highest qualification**



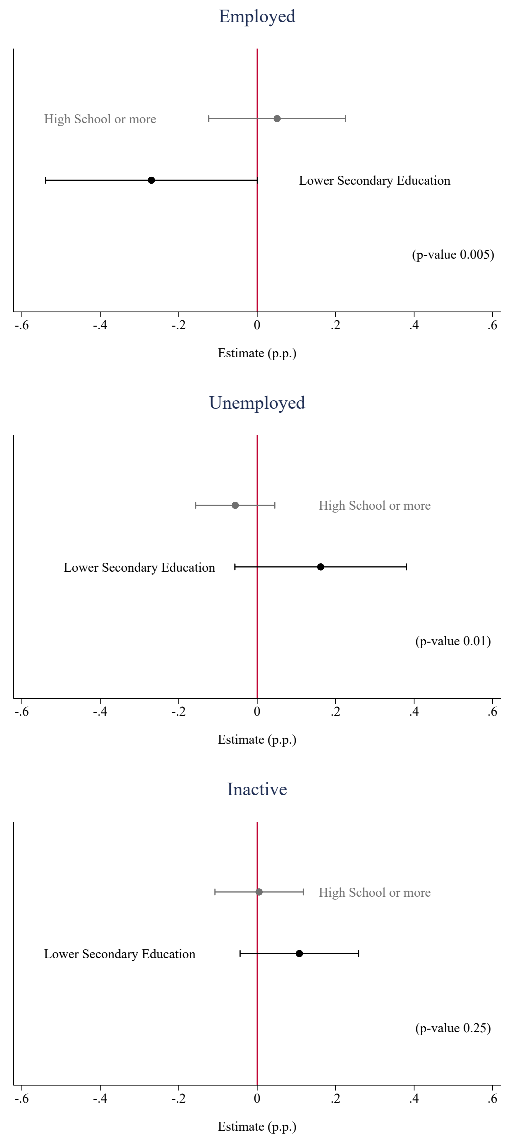
Source: Labor Force Survey, cohorts 1975-1978.
Notes: This figure shows the relationship between age at highest qualification and month of birth. The x-axis is normalized to 0 in January 1977, as 1977 is the first cohort affected by the rise in the compulsory schooling age. Each dot represents the average age at highest qualification, for each month of birth from January 1976 to December 1978. The two lines are linear fits of the dots, computed separately on each side of the 0 threshold. 95 percent confidence intervals are also displayed.

Figure 4: **Share of 14-16 year-old students in general education**



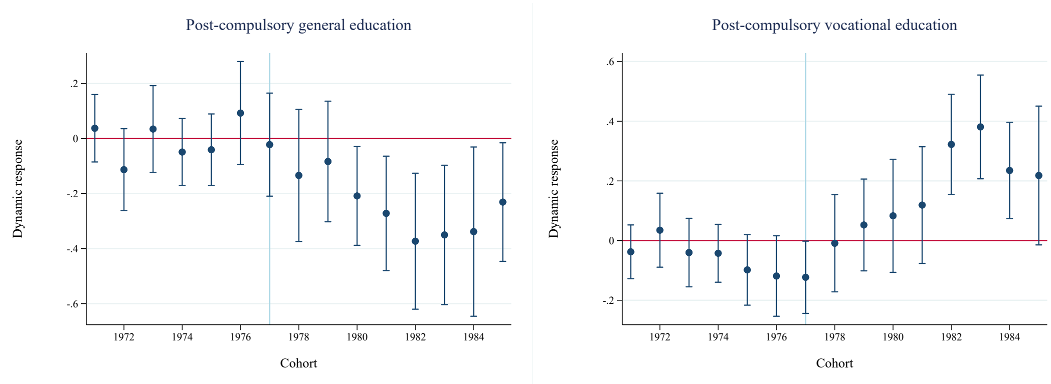
Source: Spanish Ministry of Education.
Notes: This figure reports the trends in the share of 14-16 year-old students in general education before and during the implementation of the reform. Each dot refers to the cross-province share of students enrolled in general education, when the cohort displayed is 14. The difference between the 25th and 75th percentiles (interquartile range) for each year is also reported. The green dash line lies between the last cohort not affected by the reform, and the first one that was impacted.

Figure 5: **Employment prospects by level of education**



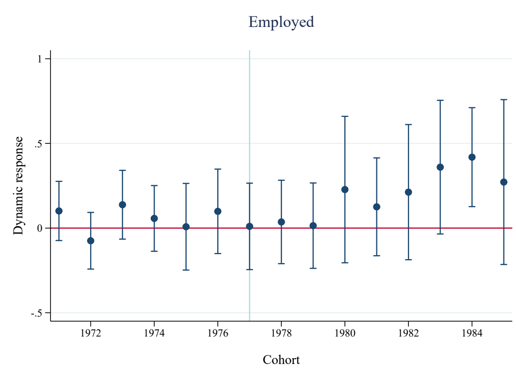
Source: Labor Force Survey 2002-2017, cohorts 1977-1985.
Notes: This figure compares the impact of the reform on employment prospects of individuals with a high-school diploma or more, and those with lower secondary education. These results are obtained from the estimation of regression 1 by subgroup. In each regression, the estimation sample includes individuals belonging to the subgroup, born between 1977 and 1985, interviewed between 2002 and 2017, and aged 25 or more when interviewed. The regressions also include the following controls: share of left-wing municipalities, and GDP per capita, both measured when the individual interviewed was 14, unemployment rate, share of population with high-school diploma or more, higher-education wage premium, share of employment in construction and manufacturing, all measured when the individual interviewed was 16, gender, log cohort size, birth-province, cohort, and year fixed effects. The regressions are estimated using heteroskedasticity-robust standard errors clustered at the province level. The figure also reports 95 percent confidence intervals and the p-value of the test on the equality of the estimated coefficients.

Figure 6: **Event studies - educational outcomes**



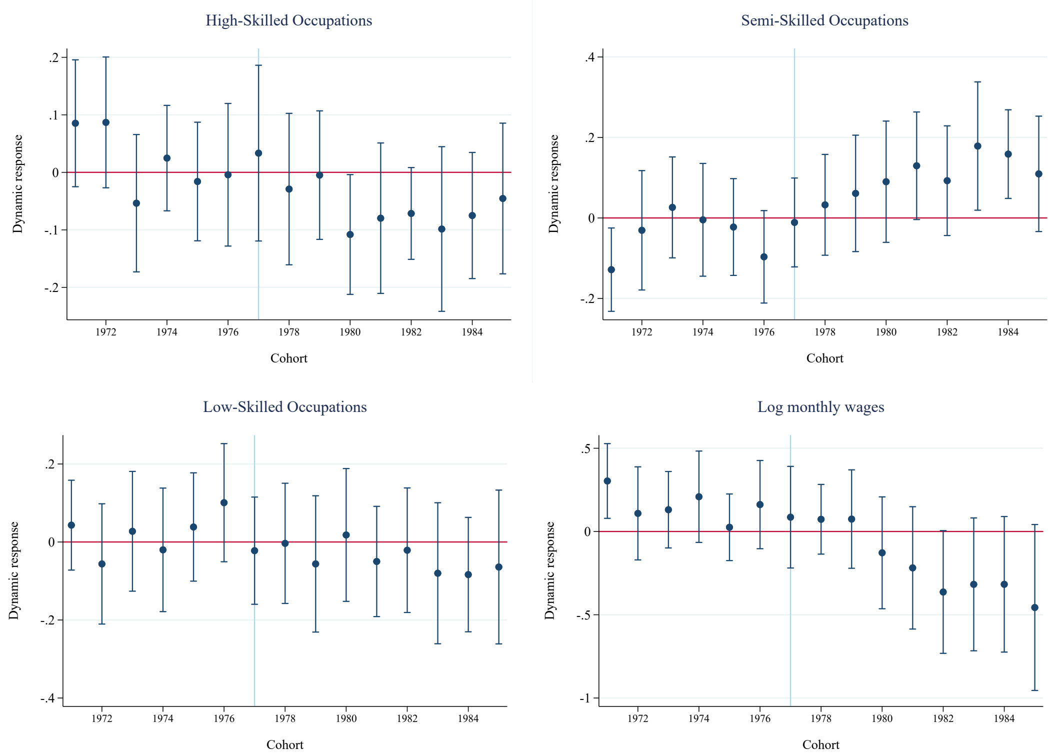
Source: Labor Force Survey 1995-2017, cohorts 1970-1985.
Notes: These graphs show the estimates of the leads and lags of the reform on educational choices, obtained from the estimation of regression 3. 95 percent confidence intervals are also reported. The outcomes considered are displayed on top of each figure.

Figure 7: **Event study - Probability of being employed - low educated individuals**



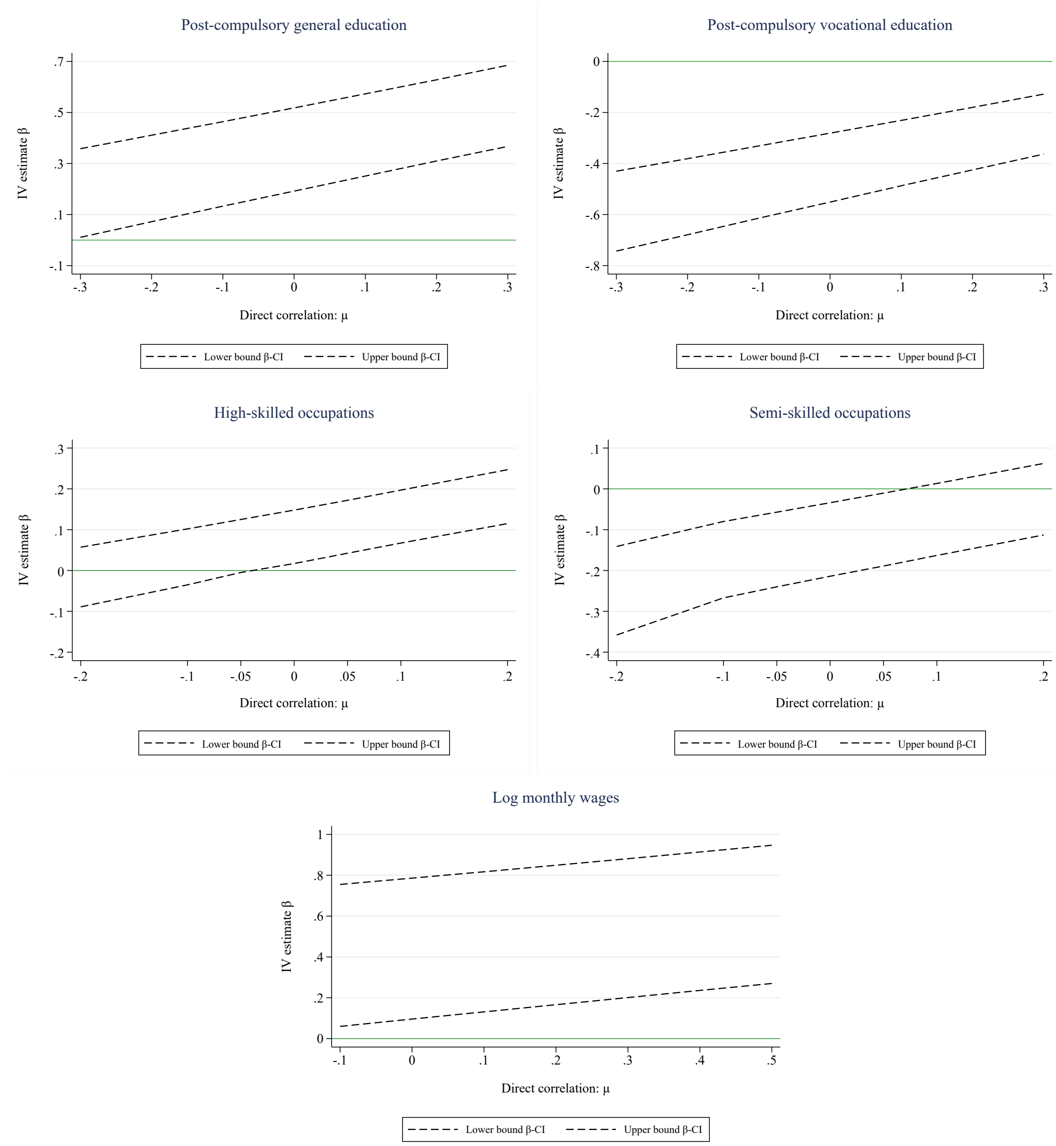
Source: Labor Force Survey 1995-2017, cohorts 1970-1985.
Notes: This graph shows the estimates of the leads and lags of the reform on employment prospects of low-educated individuals, obtained from the estimation of regression 3. 95 percent confidence intervals are also reported.

Figure 8: **Event studies - occupational outcomes and monthly wages**



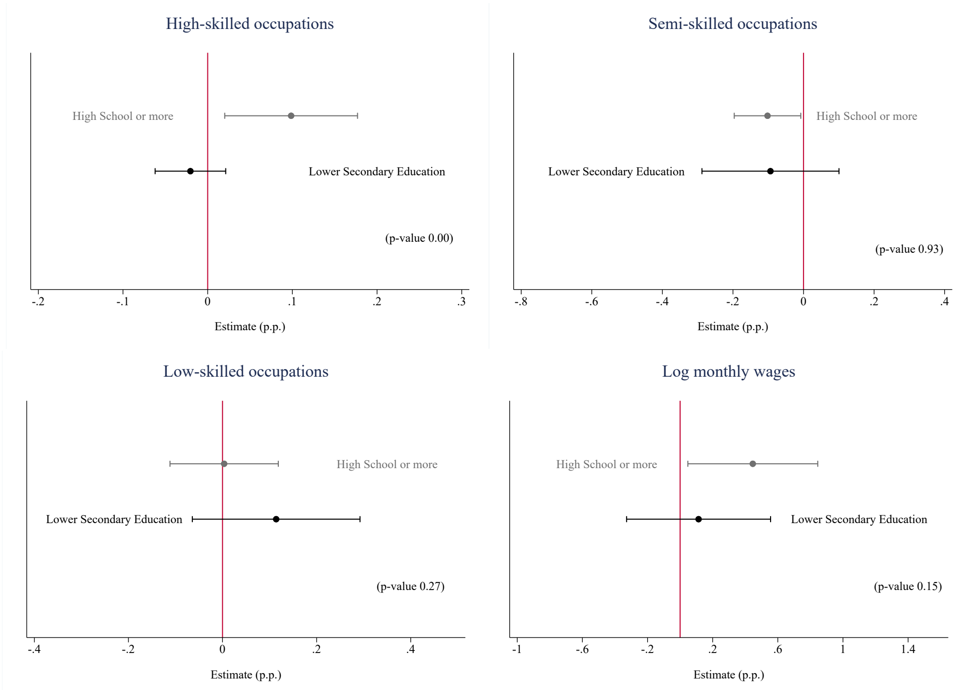
Source: Continuous Sample of Working Histories 2006-2017, cohorts 1970-1985.
Notes: These graphs show the estimates of the leads and lags of the reform on occupational outcomes and wages, obtained from the estimation of regression 3. 95 percent confidence intervals are also reported. The outcomes considered are displayed on top of each figure.

Figure 9: **IV estimates under the assumption of plausible exogeneity**



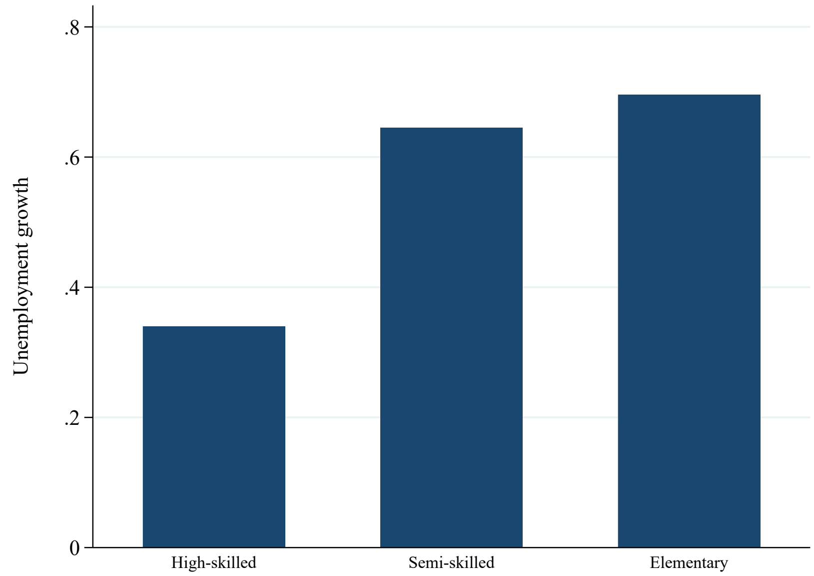
Source: Labor Force Survey 1995-2017, cohorts 1970-1985.
Notes: These graphs plot confidence intervals on the IV estimates β associated to different priors on the direct effect of the instruments on the outcomes of interest. These estimates are obtained following the "union of confidence intervals" method proposed by Conley et al. (2012). Calling μ_c the direct effect of the instruments on the outcomes, as specified in equation 4, for each outcome considered, we have estimated the β associated to seven μ_c comprised between the smallest and largest values of the confidence intervals on the leads of the reform estimated in Figures 6-8.

Figure 10: **Occupational outcomes and wages by level of education**



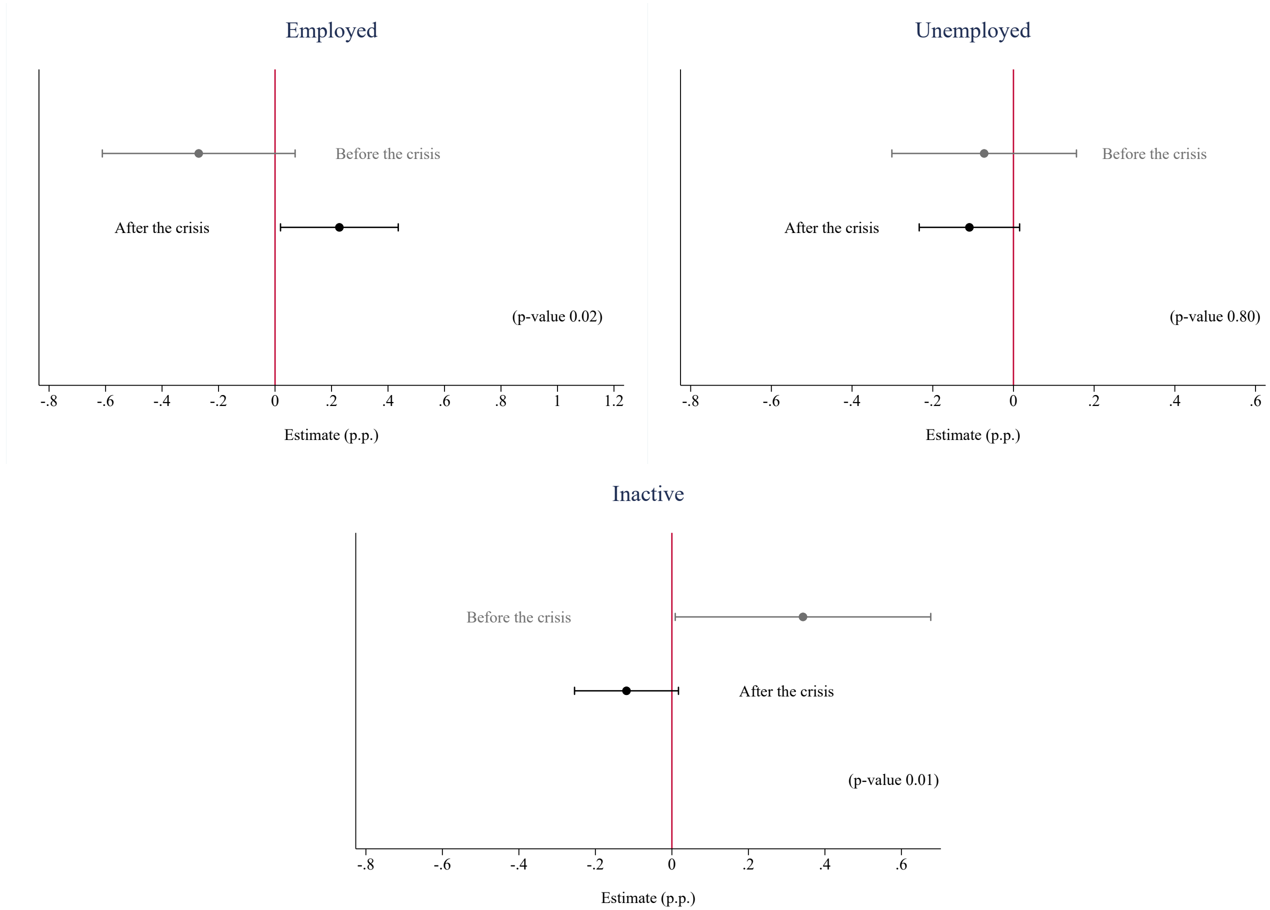
Source: Continuous Sample of Working Histories 2006-2017, cohorts 1977-1985.
Notes: This figure compares the impact of the reform on occupational and pay outcomes of individuals with a high-school diploma or more, and those with lower secondary education. These results are obtained from the estimation of regression 1 by subgroup. The estimation sample includes individuals belonging to each subgroup, born between 1977 and 1985, appearing in the CSWH between 2006 and 2017, and aged 25 or more when interviewed. The regressions also include the following controls: share of left-wing municipalities, and GDP per capita, both measured when the individual interviewed was 14, unemployment rate, share of population with high-school diploma or more, higher-education wage premium, share of employment in construction and manufacturing, all measured when the individual interviewed was 16, gender, log cohort size, birth-province, cohort, and year fixed effects. The regressions are estimated using heteroskedasticity-robust standard errors clustered at the province level. The figure also reports 95 percent confidence intervals and the p-value of the test on the equality of the estimated coefficients.

Figure 11: **Unemployment growth after the financial crisis by occupation**



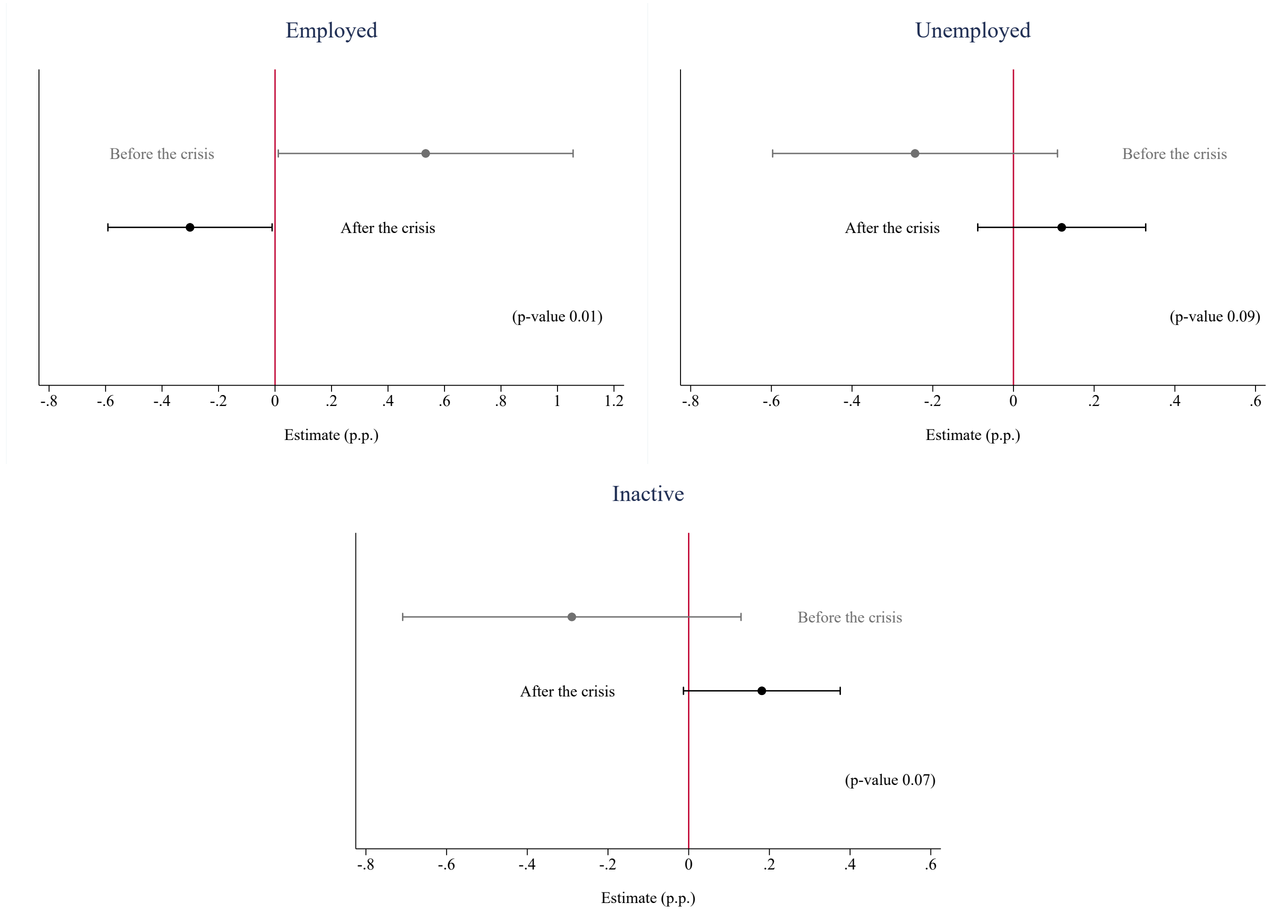
Source: Labor Force Survey 2002-2013.
Notes: This figure reports the growth in the unemployment rate between the five years pre- and post the financial crisis, by occupation previously held. The sample includes pre-reform cohorts, born before 1977.

Figure 12: **Employment prospects before/after crisis - individuals with high-school diploma or more**



Source: Labor Force Survey 2002-2017, cohorts 1977-1985.
Notes: This figure compares the impact of the reform on employment prospects of individuals with at least a high-school diploma, before and after the financial crisis. These results are obtained from the estimation of regression 1 by subgroup. In each regression, the estimation sample includes individuals belonging to the subgroup, born between 1977 and 1985, interviewed between 2002 and 2017, and aged 25 or more when interviewed. The regressions also include the following controls: share of left-wing municipalities, and GDP per capita, both measured when the individual interviewed was 14, unemployment rate, share of population with high-school diploma or more, higher-education wage premium, share of employment in construction and manufacturing, all measured when the individual interviewed was 16, gender, log cohort size, birth-province, cohort, and year fixed effects. The regressions are estimated using heteroskedasticity-robust standard errors clustered at the province level. The figure also reports 95 percent confidence intervals and the p-value of the test on the equality of the estimated coefficients.

Figure 13: **Employment prospects before/after crisis - low-educated individuals**



Source: Labor Force Survey 2002-2017, cohorts 1977-1985.
Notes: This figure compares the impact of the reform on employment prospects of individuals with lower secondary education, before and after the financial crisis. These results are obtained from the estimation of regression 1 by subgroup. In each regression, the estimation sample includes individuals belonging to the subgroup, born between 1977 and 1985, interviewed between 2002 and 2017, and aged 25 or more when interviewed. The regressions also include the following controls: gender, share of left-wing municipalities, and GDP per capita, both measured when the individual interviewed was 14, unemployment rate, share of population with high-school diploma or more, higher-education wage premium, share of employment in construction and manufacturing, all measured when the individual interviewed was 16, gender, log cohort size, birth-province, cohort, and year fixed effects. The regressions are estimated using heteroskedasticity-robust standard errors clustered at the province level. The figure also reports 95 percent confidence intervals and the p-value of the test on the equality of the estimated coefficients.

Table 1: **Summary statistics**

	Mean	Sd	Min	Max	N
LFS outcomes					
Age at highest qualification	19.91	4.59	7	40	765,354
Lower secondary education	0.33	0.47	0	1	768,701
Post-compulsory general	0.42	0.49	0	1	768,701
Post-compulsory vocational	0.26	0.44	0	1	768,701
Employed	0.71	0.46	0	1	768,701
Unemployed	0.17	0.37	0	1	768,701
Inactive	0.13	0.33	0	1	768,701
Across-province migration	0.15	0.36	0	1	768,701
CSWH outcomes					
Monthly wages	1,895	1,657	0	300,037	1,484,531
High-skilled occupations	0.09	0.29	0	1	1,438,682
Semi-skilled occupations	0.58	0.49	0	1	1,438,682
Low-skilled occupations	0.33	0.47	0	1	1,438,682
Youth Survey outcome					
Importance of studying	0.87	0.33	0	1	3,130
PIAAC outcomes					
PIAAC Literacy score					
Plausible value 1	270.65	40.66	110.19	386.21	885
Plausible value 2	268.08	41.83	78.41	395.90	885
Plausible value 3	268.60	39.37	87.93	366.41	885
Plausible value 4	269.48	41.09	98.82	382.33	885
Plausible value 5	268.70	41	97.69	374.82	885
Plausible value 6	268.27	40.42	104.38	387.67	885
Plausible value 7	268.23	40.44	88.51	376.49	885
Plausible value 8	269.43	40.86	99.36	390.64	885
Plausible value 9	269.43	41.94	95.61	380.46	885
Plausible value 10	268.61	40.73	96.03	372.71	885
PIAAC Numeracy score					
Plausible value 1	264.68	42.16	91.70	371.78	885
Plausible value 2	264.65	43.52	77.88	391.69	885
Plausible value 3	264.91	42.08	87.24	379.40	885
Plausible value 4	263.65	43.25	78.87	360.97	885
Plausible value 5	263.74	42.82	95.88	406.90	885
Plausible value 6	263.44	42.22	89.22	386.97	885
Plausible value 7	263.69	42.71	96.29	400.26	885
Plausible value 8	264.61	43.36	98.57	386.36	885
Plausible value 9	264.76	43.43	88.90	364.71	885
Plausible value 10	263.95	42.64	91.12	374.33	885
Province-Cohort Level Controls					
Cohort size	19,716.56	21,291.51	807	89,243	468
GDP per capita	9.87	2.61	5.23	19.76	468
Share left-wing municipalities	0.42	0.27	0	1	468
Unemployment rate	0.21	0.08	0.04	0.43	468
Population with high school or more	0.19	0.05	0.09	0.37	468
1995 university wage-premium	1.52	0.11	1.24	1.80	468
Employment share in construction	0.10	0.02	0.05	0.18	468
Employment share in manufacturing	0.19	0.07	0.03	0.37	468

Notes: This table reports summary statistics for outcome and control variables. Outcome variables refer to affected cohorts, born between 1977-1985. Cohort-province time-varying controls are measured as follows: share of left-wing municipalities, and GDP per capita are measured when cohort c is 14; unemployment rate, share of population with high school or more, higher-education wage premium, employment shares in construction and manufacturing are measured when cohort c is 16. Cohort size is measured at birth.

Table 2: **First stage**

	Share of students in general education (1)
ShareStudGen1976*1978 FE	-0.0605 (0.0636)
ShareStudGen1976*1979 FE	-0.278 (0.204)
ShareStudGen1976*1980 FE	-0.175* (0.102)
ShareStudGen1976*1981 FE	-0.389*** (0.122)
ShareStudGen1976*1982 FE	-0.592*** (0.132)
ShareStudGen1976*1983 FE	-0.743*** (0.117)
ShareStudGen1976*1984 FE	-0.874*** (0.0796)
ShareStudGen1976*1985 FE	-1.062*** (0.0719)
Observations	768701
Kleibergen-Paap F-stat	36.57

Source: Labor Force Survey 2002-2017, cohorts 1977-1985.

Notes: This table reports the first stage regression. The main regressors are interaction terms between the share of 14-16 years old students enrolled in general education when cohort 1976 is 14, $ShareStudGen_{1976p}$, and fixed effects for the cohorts affected by the reform. The regression also includes the following controls: share of left-wing municipalities, and GDP per capita, both measured when the individual interviewed was 14, unemployment rate, share of population with high school or more, higher-education wage premium, share of employment in construction and manufacturing, all measured when the individual interviewed was 16, gender, log cohort size, birth-province, cohort, and year fixed effects. Heteroskedasticity-robust standard errors clustered at the province level in parenthesis.

*** p<0.01, ** p<0.05, * p<0.1.

Table 3: **Educational outcomes**

	Age at highest qualification	Type of education acquired	
		General	Vocational
	(1)	(2)	(3)
OLS results			
Share Students in General Edu	0.137	0.0356	-0.0942**
	(0.358)	(0.0530)	(0.0412)
IV results			
Share Students in General Edu	1.320	0.355***	-0.416***
	(0.831)	(0.0831)	(0.0686)
Observations	765354	768701	768701
Pre-Reform Mean	19.32	0.39	0.22
Kleibergen-Paap F-stat	36.64	36.57	36.57
Sargan test p-value	0.587	0.612	0.461

Source: Labor Force Survey 2002-2017, cohorts 1977-1985.

Notes: This table reports the impact of the reform on educational choices, obtained from the estimation of regression 1. Each column refers to the outcome considered, being this the age at highest qualification (Column 1), the probability of holding an upper secondary or tertiary general qualification (Column 2), or the probability of holding an upper secondary or tertiary vocational qualification (Column 3). The first panel reports the OLS results, while the second shows the IV estimates. The estimation sample includes individuals born between 1977 and 1985, interviewed between 2002 and 2017, and aged 25 or more when interviewed. The response rate on age at highest qualification is 1% smaller than for other outcomes. Each regression also includes the following controls: share of left-wing municipalities, and GDP per capita, both measured when the individual interviewed was 14, unemployment rate, share of population with high school or more, higher-education wage premium, share of employment in construction and manufacturing, all measured when the individual interviewed was 16, gender, log cohort size, birth-province, cohort, and year fixed effects. Heteroskedasticity-robust standard errors clustered at the province level in parenthesis. The pre-reform mean refers to the mean of the outcome variables from age 25 onward, for the 1970-1976 cohorts, the last seven cohorts not affected by the reform.

*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 4: **Employment prospects**

	Employed (1)	Unemployed (2)	Inactive (3)
OLS results			
Share Students in General Edu	-0.00362	-0.00581	0.00943
	(0.0336)	(0.0234)	(0.0217)
IV results			
Share Students in General Edu	-0.0608	0.0436	0.0173
	(0.0895)	(0.0641)	(0.0483)
Observations	768701	768701	768701
Pre-Reform Mean	0.72	0.10	0.15
Kleibergen-Paap F-stat	36.57	36.57	36.57
Sargan test p-value	0.592	0.354	0.0890

Source: Labor Force Survey 2002-2017, cohorts 1977-1985.

Notes: This table reports the impact of the reform on employment prospects, obtained from the estimation of regression 1. Each column refers to the outcome considered, being this the probability of being employed (Column 1), unemployed (Column 2), or inactive (Column 3). The first panel reports the OLS results, while the second shows the IV estimates. The estimation sample includes individuals born between 1977 and 1985, interviewed between 2002 and 2017, and aged 25 or more when interviewed. Each regression also includes the following controls: share of left-wing municipalities, and GDP per capita, both measured when the individual interviewed was 14, unemployment rate, share of population with high school or more, higher-education wage premium, share of employment in construction and manufacturing, all measured when the individual interviewed was 16, gender, log cohort size, birth-province, cohort, and year fixed effects. Heteroskedasticity-robust standard errors clustered at the province level in parenthesis. The pre-reform mean refers to the mean of the outcome variables from age 25 onward, for the 1970-1976 cohorts, the last seven cohorts not affected by the reform.

*** p<0.01, ** p<0.05, * p<0.1.

Table 5: **Occupational outcomes and wages**

	Occupations			
	High-skilled	Semi-skilled	Low-skilled	Log monthly wages
	(1)	(2)	(3)	(4)
OLS results				
Share Students in General Edu	0.0389**	-0.0134	-0.0254	0.174*
	(0.0167)	(0.0248)	(0.0251)	(0.0932)
IV results				
Share Students in General Edu	0.0827**	-0.124***	0.0417	0.441**
	(0.0335)	(0.0461)	(0.0573)	(0.176)
Observations	1438682	1438682	1438682	1484531
Pre-Reform Mean	0.10	0.60	0.30	7.41
Kleibergen-Paap F-stat	35.80	35.80	35.80	47.39
Sargan test p-value	0.430	0.785	0.868	0.836

Source: Continuous Sample of Working Histories 2006-2017, cohorts 1977-1985.

Notes: This table reports the impact of the reform on occupational outcomes and monthly wages, obtained from the estimation of regression 1. Each column refers to the outcome considered, being this the probability of working in a high-skilled occupation (Column 1), semi-skilled occupation (Column 2), low-skilled occupation (Column 3), or log monthly wages. The first panel reports the OLS results, while the second shows the IV estimates. The estimation sample includes individuals born between 1977 and 1985, appearing in the CSWH between 2006 and 2015, and aged 25 or more when interviewed. Data on occupational outcomes are not provided for the Basque Country and Navarra. Each regression also includes the following controls: share of left-wing municipalities, and GDP per capita, both measured when the individual interviewed was 14, unemployment rate, share of population with high school or more, higher-education wage premium, share of employment in construction and manufacturing, all measured when the individual interviewed was 16, gender, log cohort size, birth-province, cohort, and year fixed effects. Heteroskedasticity-robust standard errors clustered at the province level in parenthesis. The pre-reform mean refers to the mean of the outcome variables from age 25 onward, for the 1970-1976 cohorts, the last seven cohorts not affected by the reform.

*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 6: **How important is to study in your life?**

	Age 15-18 (1)	Age 25+ (2)
OLS results		
Share Students in General Edu l	0.180	0.367*
	(0.137)	(0.198)
IV results		
Share Students in General Edu	0.495*	0.153
	(0.278)	(0.387)
Observations	1965	3130
Pre-Reform Mean	0.91	0.88
Kleibergen-Paap F-stat	21.17	13.85
Sargan test p-value	0.105	0.311

Source: Youth Survey 1996-2012, cohorts 1977-1985.
Notes: This table reports the impact of the reform on academic aspirations, obtained from the estimation of regression 1. The outcome considered is a binary indicator equal to one if the survey respondent declares that studying is important or very important in his life. In column 1, the outcome is measured at age 15-18, while in column 2 is measured from age 25 onward. The first panel reports the OLS results, while the second shows the IV estimates. The estimation sample includes individuals born between 1977 and 1985, interviewed between 1996 and 2012. Each regression also includes the following controls: share of left-wing municipalities, and GDP per capita, both measured when the individual interviewed was 14, unemployment rate, share of population with high school or more, higher-education wage premium, share of employment in construction and manufacturing, all measured when the individual interviewed was 16, gender, log cohort size, birth-province, cohort, and year fixed effects. Heteroskedasticity-robust standard errors clustered at the province level in parenthesis. The pre-reform mean refers to the mean of the outcome variables measured, for the 1970-1976 cohorts, when aged between 15 and 18 (Column 1) or from age 25 onward (Column 2).
*** p<0.01, ** p<0.05, * p<0.1.

Table 7: **Performance in the OECD PIAAC test**

	PV 1 (1)	PV 2 (2)	PV 3 (3)	PV 4 (4)	PV 5 (5)	PV 6 (6)	PV 7 (7)	PV 8 (8)	PV 9 (9)	PV 10 (10)
Literacy										
OLS: Share Students in General Edu	30.69	32.90	43.58	6.41	27.68	36.00	43.62	40.19	63.18	22.90
	(55.44)	(57.71)	(42.28)	(56.04)	(45.04)	(51.24)	(49.92)	(56.44)	(57.42)	(43.92)
	[0.670]	[0.667]	[0.447]	[0.930]	[0.630]	[0.560]	[0.487]	[0.617]	[0.462]	[0.672]
IV: Share Students in General Edu	123.5***	148.4**	49.28	103.3	72.05	107.4	99.16**	97.13	186.8***	88.92*
	(26.39)	(35.18)	(37.46)	(39.54)	(29.03)	(52.71)	(27.24)	(33.09)	(30.92)	(35.67)
	[0.010]	[0.012]	[0.392]	[0.105]	[0.105]	[0.140]	[0.025]	[0.112]	[0.000]	[0.090]
Observations	885	885	885	885	885	885	885	885	885	885
Pre-Reform Mean	266.2	266.1	267.5	265.3	267.8	266.7	266.7	266.9	266.8	267.9
Kleibergen-Paap F-stat	21.91	21.91	21.91	21.91	21.91	21.91	21.91	21.91	21.91	21.91
Sargan test p-value	0.419	0.549	0.475	0.642	0.524	0.547	0.539	0.575	0.653	0.608
Numeracy										
OLS: Share Students in General Edu	55.21	-4.05	54.83	43.07	55.91	66.57	40.52	34.85	80.28	35.82
	(57.16)	(49.83)	(45.16)	(60.09)	(50.39)	(46.96)	(54.31)	(53.74)	(56.79)	(48.70)
	[0.490]	[0.937]	[0.387]	[0.635]	[0.477]	[0.327]	[0.590]	[0.667]	[0.365]	[0.605]
IV: Share Students in General Edu	50.19	33.32	46.33	64.85	59.15	149.8***	49.08	-0.170	99.76*	18.65
	(40.32)	(43.28)	(49.19)	(38.52)	(40.49)	(42.91)	(41.14)	(39.31)	(35.53)	(49.21)
	[0.422]	[0.522]	[0.527]	[0.297]	[0.345]	[0.010]	[0.365]	[0.995]	[0.057]	[0.797]
Observations	885	885	885	885	885	885	885	885	885	885
Pre-Reform Mean	260.9	259.9	260.9	261.2	261.4	261.0	260.9	260.6	260.9	260.9
Kleibergen-Paap F-stat	21.91	21.91	21.91	21.91	21.91	21.91	21.91	21.91	21.91	21.91
Sargan test p-value	0.449	0.561	0.348	0.414	0.498	0.281	0.643	0.362	0.647	0.304

Source: PIAAC 2012, cohorts 1977-1985.

Notes: This table reports the impact of the reform on the performance in the OECD PIAAC test, obtained from the estimation of regression 1. Each column refers to the outcome considered, here each of the 10 plausible values (PV) for literacy level (Panel 1), or numeracy skills (Panel 2), derived from item response models. The first line of each panel reports the OLS results, while the second shows the IV estimates. The estimation sample includes individuals born between 1977 and 1985, who participated in the 2012 PIAAC test. Each regression also includes the following controls: share of left-wing municipalities, and GDP per capita, both measured when the individual interviewed was 14, unemployment rate, share of population with high school or more, higher-education wage premium, share of employment in construction and manufacturing, all measured when the individual interviewed was 16, gender, log cohort size, birth-region, and cohort fixed effects. All regression are estimated using survey weights. Heteroskedasticity-robust standard errors in parenthesis, and wild-bootstrap p-values with cluster at regional level in brackets. Sargan test calculated using a model that partials out the exogenous instruments.

*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 8: **Cross-province migration**

	16-24 (1)	25 + (2)
OLS results		
Share Students in General Edu	0.00824	0.0714*
	(0.0895)	(0.0364)
IV results		
Share Students in General Edu	-0.0560	0.166*
	(0.103)	(0.0984)
Observations	786544	768701
Pre-Reform Mean	0.10	0.17
Kleibergen-Paap F-stat	47.78	36.57
Sargan test p-value	0.685	0.218

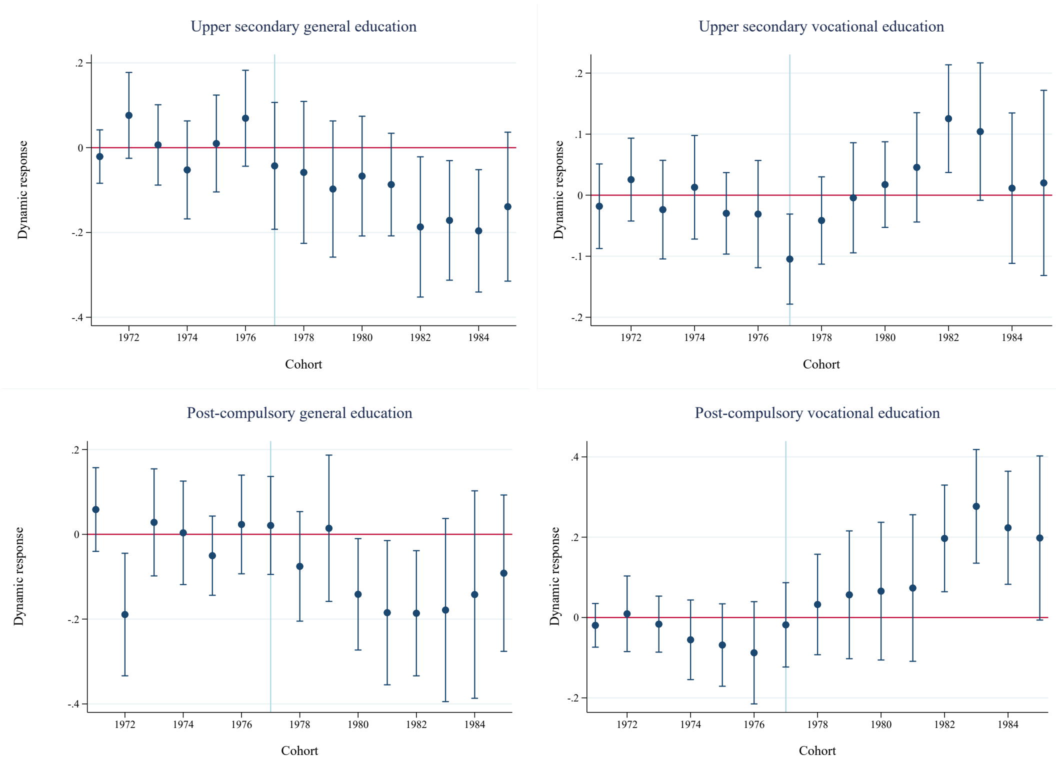
Source: Labor Force Survey 1995-2017, cohorts 1977-1985.

Notes: This table reports the impact of the reform on the probability of cross-city migration, obtained from the estimation of regression 1. Each column refers to the outcome considered, being this the probability of migrating between age 16 and 24 (Column 1), or the probability of migrating from age 25 onward (Column 2). The first panel reports the OLS results, while the second shows the IV estimates. The estimation sample includes individuals born between 1977 and 1985, interviewed between 1995 and 2017. Each regression also includes the following controls: share of left-wing municipalities, and GDP per capita, both measured when the individual interviewed was 14, unemployment rate, share of population with high school or more, higher-education wage premium, share of employment in construction and manufacturing, all measured when the individual interviewed was 16, gender, log cohort size, birth-province, cohort, and year fixed effects. Heteroskedasticity-robust standard errors clustered at the province level in parenthesis. The pre-reform mean refers to the mean of the outcome variables at age 16-24 (from age 25 onward in column 2), for the 1970-1976 cohorts, the last seven cohorts not affected by the reform.

*** p<0.01, ** p<0.05, * p<0.1.

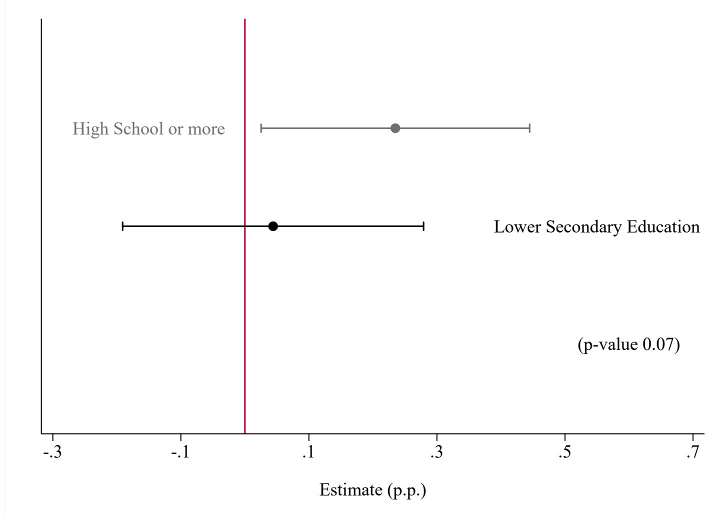
10 Appendix

Figure A.1: **Event studies - educational outcomes**



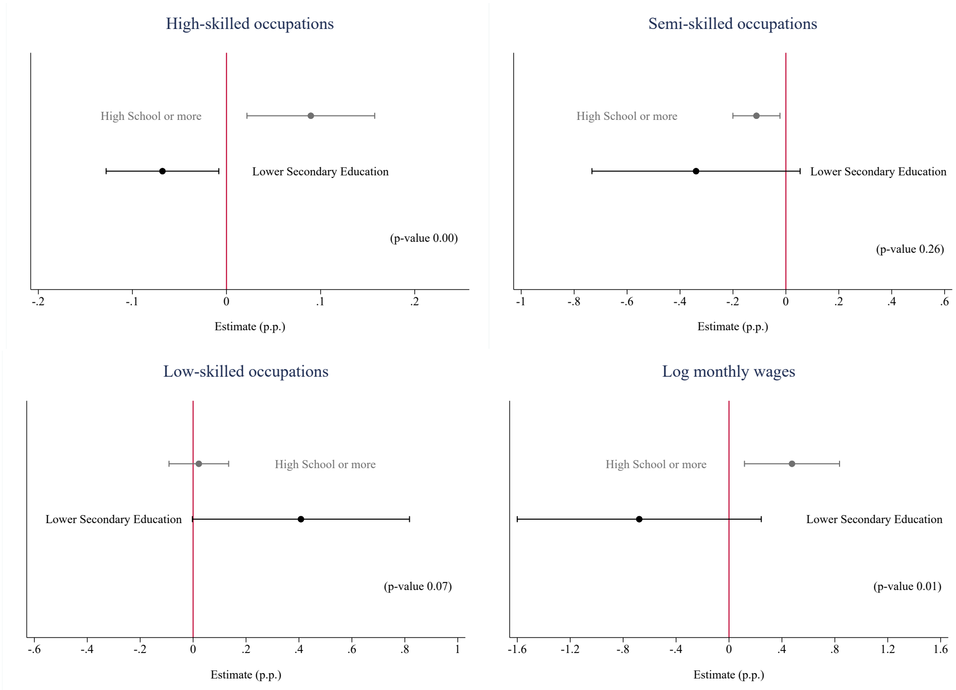
Source: Labor Force Survey 1995-2017, cohorts 1970-1985.
Notes: These graphs show the estimates of the leads and lags of the reform on educational choices, obtained from the estimation of regression 3. 95 percent confidence intervals are also reported. The outcomes considered are displayed on top of each figure.

Figure A.2: **Cross-province migration by level of education**



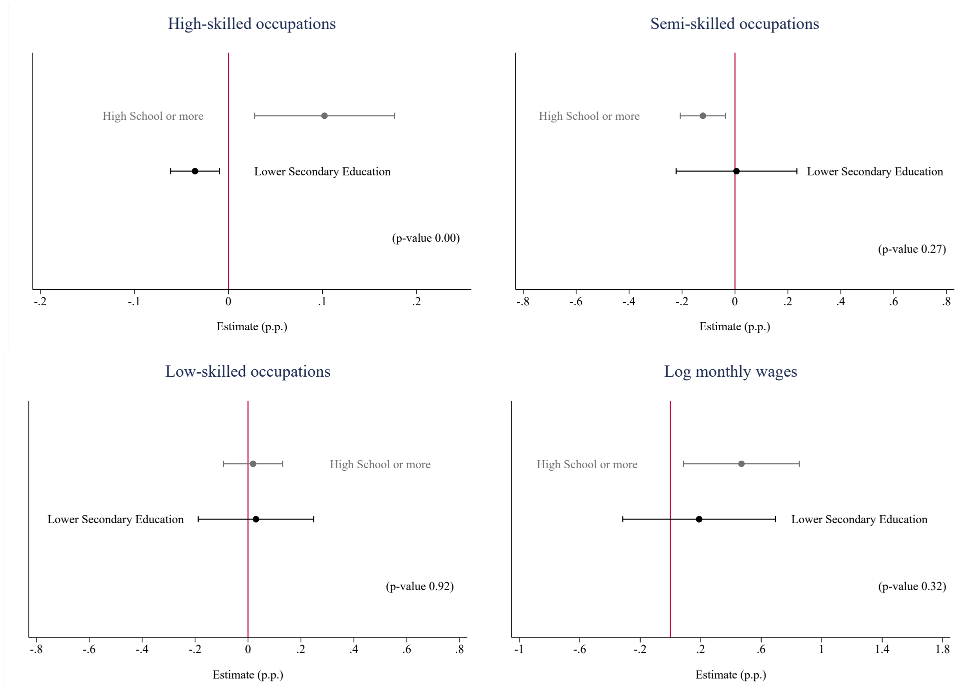
Source: Labor Force Survey 2002-2017, cohorts 1977-1985.
Notes: This figure compares the impact of the reform on the probability of cross-province migration for individuals with a high-school diploma or more, and those with lower secondary education. These results are obtained from the estimation of regression 1 by subgroup. In each regression, the estimation sample includes individuals belonging to the subgroup, born between 1977 and 1985, interviewed between 2002 and 2017, and aged 25 or more when interviewed. The regressions also include the following controls: share of left-wing municipalities, and GDP per capita, both measured when the individual interviewed was 14, unemployment rate, share of population with high-school diploma or more, higher-education wage premium, share of employment in construction and manufacturing, all measured when the individual interviewed was 16, gender, log cohort size, birth-province, cohort, and year fixed effects. The regressions are estimated using heteroskedasticity-robust standard errors clustered at the province level. The figure also reports 95 percent confidence intervals and the p-value of the test on the equality of the estimated coefficients.

Figure A.3: **Occupational outcomes and wages by level of education - age at first job 16**



Source: Continuous Sample of Working Histories 2006-2017, cohorts 1977-1985.
Notes: This figure compares the impact of the reform on occupational and pay outcomes of individuals with a high-school diploma or more, and those with lower secondary education. These results are obtained from the estimation of regression 1 by subgroup. The estimation sample includes individuals belonging to each subgroup, born between 1977 and 1985, appearing in the CSWH between 2006 and 2017, and aged 25 or more when interviewed. The regressions also include the following controls: share of left-wing municipalities, and GDP per capita, both measured when the individual interviewed was 14, unemployment rate, share of population with high-school diploma or more, higher-education wage premium, share of employment in construction and manufacturing, all measured when the individual interviewed was 16, gender, log cohort size, birth-province, cohort, and year fixed effects. The regressions are estimated using heteroskedasticity-robust standard errors clustered at the province level. The figure also reports 95 percent confidence intervals and the p-value of the test on the equality of the estimated coefficients.

Figure A.4: **Occupational outcomes and wages by level of education - age at first job 17**



Source: Continuous Sample of Working Histories 2006-2017, cohorts 1977-1985.
Notes: This figure compares the impact of the reform on occupational and pay outcomes of individuals with a high-school diploma or more, and those with lower secondary education. These results are obtained from the estimation of regression 1 by subgroup. The estimation sample includes individuals belonging to each subgroup, born between 1977 and 1985, appearing in the CSWH between 2006 and 2017, and aged 25 or more when interviewed. The regressions also include the following controls: share of left-wing municipalities, and GDP per capita, both measured when the individual interviewed was 14, unemployment rate, share of population with high-school diploma or more, higher-education wage premium, share of employment in construction and manufacturing, all measured when the individual interviewed was 16, gender, log cohort size, birth-province, cohort, and year fixed effects. The regressions are estimated using heteroskedasticity-robust standard errors clustered at the province level. The figure also reports 95 percent confidence intervals and the p-value of the test on the equality of the estimated coefficients.

Table A.1: **Rise in compulsory schooling and age at highest qualification**

	Age 35			2016		
	1975-1978	1976-1977	1974-1979	1975-1978	1976-1977	1974-1979
	(1)	(2)	(3)	(4)	(5)	(6)
Month of birth	0.0133	0.0106	0.0146***	0.00122	0.0119	0.00744
	(0.00908)	(0.0188)	(0.00335)	(0.00869)	(0.0338)	(0.00494)
Jump in the slope	-0.00559	-0.0222	-0.00515	0.000982	-0.00757	-0.00802
	(0.0147)	(0.0380)	(0.00677)	(0.0133)	(0.0442)	(0.00819)
Impact of the reform	0.0831	0.256	0.0552	0.0291	-0.111	-0.00710
	(0.224)	(0.319)	(0.175)	(0.199)	(0.300)	(0.167)
Observations	31773	15892	46846	30831	15440	45614

Source: Labor Force Survey, cohorts 1974-1979.

Notes: The table reports the RDD analysis on the impact of the 1991 rise in the compulsory school leaving age on the age at highest qualification. In the first three columns, the outcome is measured at age 35, while in the last three columns, it is measured in 2016. For each of this measure, the first column the bandwidth around the policy cutoff is +/- two years, in the second it is +/- 1 year, while in the last one it is +/- three years. Heteroskedasticity-robust standard errors clustered at the cohort level. *** p<0.01, ** p<0.05, * p<0.1.

Table A.2: **Province characteristics and implementation of the reform**

	Share of students in general education						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Log cohort size	-0.0345						-0.0501
	(0.0588)						(0.0610)
Log GDP per capita		-0.0904					-0.0617
		(0.124)					(0.116)
Share left-wing municipalities			-0.0293**				-0.0267*
			(0.0137)				(0.0135)
Share students in public schools				-0.0600			-0.0719
				(0.0862)			(0.0792)
Employment share in manufacturing					-0.167		-0.0228
					(0.191)		(0.172)
Employment share in construction						0.456*	0.343
						(0.246)	(0.235)
Observations	468	468	468	468	468	468	468

Source: Spanish Ministry of Education, Spanish Statistical Agency, and Valencian Institute of Economic Research (IVIE).

Notes: This table reports the correlation between the evolution of the share of students in lower secondary general education and province-cohort observable characteristics. In detail, each row refers to a specific province observable characteristic. Each column is a separate regression of the share of students enrolled in the lower secondary general education for the 1977-1985 cohorts over each province observable characteristic. Each regression also includes cohort, and birth-province fixed effects. Heteroskedasticity-robust standard errors clustered at the province level in parenthesis.

*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A.3: **Pre-reform share of students in general education and province characteristics**

	(1) Log per-capita expenditures on education	(2) Share of population with high-school or more	(3) Share of vocational schools	(4) Employment share in Manufacturing	(5) Employment share in Construction
ShareStudGen1976	0.135**	0.0709***	-0.129	-0.130***	-0.0145
	(0.0636)	(0.0222)	(0.0988)	(0.0419)	(0.0338)
Observations	364	360	364	358	358

Source: Spanish Ministry of Education, Spanish Statistical Agency, Labor Force Survey, and Valencian Institute of Economic Research (IVIE).

Notes: This table reports the correlation between the share of 14-16 years old students enrolled in general education when cohort 1976 is 14 and province-cohort observable characteristics. In detail, each column presents the results of separate regressions of province-observable characteristics on the share of students enrolled in lower secondary general education when cohort 1976 is 14. Each regression also includes cohort, and birth-province fixed effects. Heteroskedasticity-robust standard errors clustered at the province level in parenthesis. Data on educational level in the provinces of Ceuta and Melilla are missing for cohorts 1970 and 1971, while data on industry shares in these two provinces are not available for any cohort.

*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A.4: **Detailed educational outcomes**

	Highest educational level			Type of education acquired			
	Lower secondary	Upper secondary	Tertiary	General	Vocational	General	Vocational
OLS results							
Share Students in General Edu	0.0941**	0.0364	-0.101***	0.0254	0.0140	0.0102	-0.108***
	(0.0373)	(0.0235)	(0.0362)	(0.0276)	(0.0279)	(0.0343)	(0.0273)
IV results							
Share Students in General Edu	0.0689	0.0366	-0.105	0.199***	-0.162**	0.157**	-0.253***
	(0.0909)	(0.0769)	(0.119)	(0.0581)	(0.0633)	(0.0765)	(0.0643)
Observations	768701	768701	768701	768701	768701	768701	768701
Pre-Reform Mean	0.32	0.22	0.39	0.12	0.10	0.27	0.12
Kleibergen-Paap F-stat	36.57	36.57	36.57	36.57	36.57	36.57	36.57
Sargan test p-value	0.637	0.254	0.461	0.788	0.205	0.632	0.611

Source: Labor Force Survey 2002-2017, cohorts 1977-1985.

Notes: This table reports the impact of the reform on educational choices, obtained from the estimation of regression 1. Each column refers to the outcome considered, being this the highest educational level attained, columns 1-3, or the type of qualification obtained in post-compulsory studies, columns 4-6. The first panel reports the OLS results, while the second shows the IV estimates. The estimation sample includes individuals born between 1977 and 1985, interviewed between 2002 and 2017, and aged 25 or more when interviewed. Each regression also includes the following controls: share of left-wing municipalities, and GDP per capita, both measured when the individual interviewed was 14, unemployment rate, share of population with high school or more, higher-education wage premium, share of employment in construction and manufacturing, all measured when the individual interviewed was 16, gender, log cohort size, birth-province, cohort, and year fixed effects. Heteroskedasticity-robust standard errors clustered at the province level in parenthesis. The pre-reform mean refers to the mean of the outcome variables from age 25 onward, for the 1970-1976 cohorts, the last seven cohorts not affected by the reform.

*** p<0.01, ** p<0.05, * p<0.1.

Table A.5: **Occupational outcomes**

	Managerial	Professional	Technical	Administrative	Service and sales	Skilled agricultural	Skilled trades	Machine operative	Elementary
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
OLS results									
Share Students in General Edu	0.0147	0.0134	-0.0661**	0.00858	-0.0170	-0.0168*	0.0607**	0.0165	-0.00820
	(0.0115)	(0.0307)	(0.0263)	(0.0217)	(0.0327)	(0.00960)	(0.0251)	(0.0225)	(0.0191)
IV results									
Share Students in General Edu	0.0724**	0.00989	-0.0924*	-0.0499	0.0874	0.0192	-0.0262	-0.0163	-0.0292
	(0.0335)	(0.0744)	(0.0556)	(0.0449)	(0.0645)	(0.0222)	(0.0514)	(0.0564)	(0.0422)
Observations	511256	511256	511256	511256	511256	511256	511256	511256	511256
Pre-Reform Mean	0.05	0.16	0.13	0.11	0.18	0.02	0.14	0.10	0.10
Kleibergen-Paap F-stat	36.91	36.91	36.91	36.91	36.91	36.91	36.91	36.91	36.91
Sargan test p-value	0.106	0.744	0.875	0.756	0.469	0.171	0.170	0.275	0.659

Source: Labor Force Survey 2002-2017, cohorts 1977-1985.

Notes: This table reports the impact of the reform on occupational outcomes, obtained from the estimation of regression 1. Each column refers to the outcome considered, being this the probability of working in a managerial occupation, (Column 1), professional occupation (Column 2), technical (Column 3), administrative (Column 4), sales and services (Column 5), skilled agricultural (Column 6), skilled-trades (Column 7), machine-operative (Column 8), or elementary occupation (Column 9). The first panel reports the OLS results, while the second shows the IV estimates. The estimation sample includes individuals born between 1977 and 1985, interviewed between 2002 and 2017, and aged 25 or more when interviewed. Each regression also includes the following controls: share of left-wing municipalities, and GDP per capita, both measured when the individual interviewed was 14, unemployment rate, share of population with high school or more, higher-education wage premium, higher-education wage premium, share of employment in construction and manufacturing, all measured when the individual interviewed was 16, gender, log cohort size, birth-province, cohort, and year fixed effects. Heteroskedasticity-robust standard errors clustered at the province level in parenthesis. The pre-reform mean refers to the mean of the outcome variables from age 25 onward, for the 1970-1976 cohorts, the last seven cohorts not affected by the reform.

*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A.6: **Occupational mobility**

	Between the ages of 25-30 and 35-40		Between the ages of 25-28 and 35-38	
	Occupation mobility upwards	Δ occupation ranking	Occupation mobility upwards	Δ occupation ranking
OLS results				
Share Students in General Edu	-0.0431	-0.342	-0.0631	-0.374
	(0.0444)	(0.305)	(0.0428)	(0.302)
IV results				
Share Students in General Edu	0.0256	0.0806	-0.0213	0.101
	(0.0855)	(0.369)	(0.0880)	(0.429)
Observations	1336145	1336145	1308206	1308206
PreTreatmentMean	0.44	1.35	0.45	1.40
Kleibergen-Paap F-stat	32.07	32.07	32.09	32.09
Sargan test p-value	0.380	0.303	0.473	0.413

Source: Continuous Sample of Working Histories 2006-2017, cohorts 1977-1985.
Notes: This table presents the impact of the reform on occupational mobility. The first panel reports the OLS results, while the second shows the IV estimates. The estimation sample includes individuals born between 1977 and 1985, interviewed between 2006 and 2017. The regressions also include the following controls: share of left-wing municipalities, and GDP per capita, both measured when the individual interviewed was 14, unemployment rate, share of population with high-school diploma or more, higher-education wage premium, share of employment in construction and manufacturing, all measured when the individual interviewed was 16, gender, log cohort size, birth-province, cohort, and year fixed effects. Heteroskedasticity-robust standard errors clustered at the province level in parenthesis. The pre-reform means refer to the mean of the outcome variable for each subgroup, estimated for the 1970-1976 cohorts, the last seven cohorts not affected by the reform.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A.7: **Occupational mobility by level of education**

	Between the ages of 25-30 and 35-40				Between the ages of 25-28 and 35-38			
	Occupation mobility upwards		Δ occupation ranking		Occupation mobility upwards		Δ occupation ranking	
	High-school	Lower secondary	High-school	Lower secondary	High-school	Lower secondary	High-school	Lower secondary
OLS results								
Share Students in General Edu	-0.0445	-0.0265	-0.375	-0.226	-0.0667	-0.0333	-0.391	-0.344
	(0.0534)	(0.0880)	(0.353)	(0.376)	(0.0530)	(0.0923)	(0.350)	(0.424)
IV results								
Share Students in General Edu	0.0394	-0.0230	-0.00659	0.517	-0.00934	-0.0745	0.0653	0.121
	(0.0844)	(0.167)	(0.410)	(0.572)	(0.0865)	(0.162)	(0.470)	(0.621)
Observations	1102178	233585	1102178	233585	1077843	230000	1077843	230000
PreTreatmentMean	0.45	0.40	1.46	0.99	0.47	0.42	1.51	1.06
Kleibergen-Paap F-stat	31.92	34.09	31.92	34.09	32.06	33.58	32.06	33.58
Sargan test p-value	0.348	0.180	0.364	0.189	0.400	0.0970	0.500	0.287

Source: Continuous Sample of Working Histories 2006-2017, cohorts 1977-1985.

Notes: This table compares the impact of the reform on occupational mobility between individuals with a high-school diploma or more, and those with lower-secondary education. The first panel reports the OLS results, while the second shows the IV estimates. The estimation sample includes individuals born between 1977 and 1985, interviewed between 2006 and 2017. The regressions also include the following controls: share of left-wing municipalities, and GDP per capita, both measured when the individual interviewed was 14, unemployment rate, share of population with high-school diploma or more, higher-education wage premium, share of employment in construction and manufacturing, all measured when the individual interviewed was 16, gender, log cohort size, birth-province, cohort, and year fixed effects. Heteroskedasticity-robust standard errors clustered at the province level in parenthesis. The pre-reform means refer to the mean of the outcome variable for each subgroup, estimated for the 1970-1976 cohorts, the last seven cohorts not affected by the reform.

*** p<0.01, ** p<0.05, * p<0.1.

Table A.8: **Employment prospects by level of education**

	Employed		Unemployed		Inactive	
	High-school diploma or more (1)	Lower secondary education (2)	High-school diploma or more (3)	Lower secondary education (4)	High-school diploma or more (5)	Lower secondary education (6)
OLS results						
Share Students in General Edu	0.0233	-0.0275	-0.0301	0.0269	0.00684	0.000683
	(0.0352)	(0.0587)	(0.0213)	(0.0458)	(0.0229)	(0.0336)
IV results						
Share Students in General Edu	0.0509	-0.270*	-0.0558	0.162	0.00496	0.108
	(0.0890)	(0.138)	(0.0514)	(0.112)	(0.0575)	(0.0771)
Observations	517898	250803	517898	250803	517898	250803
Pre-Reform Mean	0.76	0.64	0.11	0.17	0.12	0.19
Kleibergen-Paap F-stat	37.81	38.18	37.81	38.18	37.81	38.18
Sargan test p-value	0.423	0.816	0.784	0.305	0.111	0.777

Source: Labor Force Survey 2002-2017, cohorts 1977-1985.

Notes: This table compares the impact of the reform on employment prospects of individuals with a high-school diploma or more, and those with lower-secondary education. Each two columns refer to the outcome considered, being this the probability of being employed (Columns 1-2), unemployed (Columns 3-4), or inactive (Columns 5-6). The first panel reports OLS effects, while the second shows IV estimates. The estimation sample includes individuals born between 1977 and 1985, interviewed between 2002 and 2017, and aged 25 or more when interviewed. Regression also include the following controls: share of left-wing municipalities, and GDP per capita, both measured when the individual interviewed was 14, unemployment rate, share of population with high-school diploma or more, higher-education wage premium, share of employment in construction and manufacturing, all measured when the individual interviewed was 16, gender, log cohort size, birth-province, cohort, and year fixed effects. Heteroskedasticity-robust standard errors clustered at the province level in parenthesis. The pre-reform means refer to the mean of the outcome variables for each subgroup, estimated from age 25 onward, for the 1970-1976 cohorts, the last seven cohorts not affected by the reform.

*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A.9: **First stage - fewer controls**

	Share of students in general education (1)
ShareStudGen1976*1978 FE	-0.0768 (0.0684)
ShareStudGen1976*1979 FE	-0.281 (0.213)
ShareStudGen1976*1980 FE	-0.158 (0.110)
ShareStudGen1976*1981 FE	-0.356*** (0.126)
ShareStudGen1976*1982 FE	-0.593*** (0.142)
ShareStudGen1976*1983 FE	-0.756*** (0.126)
ShareStudGen1976*1984 FE	-0.864*** (0.0741)
ShareStudGen1976*1985 FE	-1.088*** (0.0361)
Observations	768701
Kleibergen-Paap F-stat	208.9

Source: Labor Force Survey 2002-2017, cohorts 1977-1985.

Notes: This table reports the first stage regression. The main regressors are interaction terms between the share of 14-16 years old students enrolled in general education when cohort 1976 is 14, $ShareStudGen_{1976p}$, and fixed effects for the cohorts affected by the reform. The regression also includes birth-province, cohort, and year fixed effects. Heteroskedasticity-robust standard errors clustered at the province level in parenthesis.

*** p<0.01, ** p<0.05, * p<0.1.

Table A.10: **Educational outcomes - fewer controls**

	Age at highest qualification	Type of education acquired	
		General	Vocational
	(1)	(2)	(3)
OLS results			
Share Students in General Edu	0.0838	0.0509	-0.119***
	(0.372)	(0.0512)	(0.0380)
IV results			
Share Students in General Edu	0.891	0.331***	-0.436***
	(1.126)	(0.0937)	(0.0699)
Observations	765354	768701	768701
Pre-Reform Mean	19.32	0.39	0.22
Kleibergen-Paap F-stat	208.7	208.9	208.9
Sargan p-value	0.536	0.491	0.245

Source: Labor Force Survey 2002-2017, cohorts 1977-1985.

Notes: This table reports the impact of the reform on educational choices, obtained from the estimation of regression 1. Each column refers to the outcome considered, being this the age at highest qualification (Column 1), the probability of holding a post-compulsory general qualification (Column 2), or the probability of holding a post-compulsory vocational qualification (Column 3). The first panel reports the OLS results, while the second shows the IV estimates. The estimation sample includes individuals born between 1977 and 1985, interviewed between 2002 and 2017, and aged 25 or more when interviewed. Each regression also includes birth-province, cohort, and year fixed effects. Heteroskedasticity-robust standard errors clustered at the province level in parenthesis. The pre-reform mean refers to the mean of the outcome variables from age 25 onward, for the 1970-1976 cohorts, the last seven cohorts not affected by the reform.

*** p<0.01, ** p<0.05, * p<0.1.

Table A.11: **Occupational outcomes and wages - fewer controls**

	Occupations			
	High-skilled	Semi-skilled	Low-skilled	Log monthly wages
	(1)	(2)	(3)	(4)
OLS results				
Share Students in General Edu	0.0346	-0.0257	-0.00889	0.141
	(0.0272)	(0.0291)	(0.0227)	(0.0930)
IV results				
Share Students in General Edu	0.112	-0.215***	0.103	0.303*
	(0.0711)	(0.0542)	(0.0660)	(0.164)
Observations	1438682	1438682	1438682	1484531
Pre-Reform Mean	0.10	0.60	0.30	7.41
Kleibergen-Paap F-stat	492.1	492.1	492.1	455.9
Sargan p-value	0.521	0.759	0.768	0.852

Source: Continuous Sample of Working Histories 2006-2017, cohorts 1977-1985.

Notes: This table reports the impact of the reform on occupational outcomes and monthly wages, obtained from the estimation of regression 1. Each column refers to the outcome considered, being this the probability of working in a high-skilled occupation (Column 1), semi-skilled occupation (Column 2), low-skilled occupation (Column 3), or log monthly wages. The first panel reports the OLS results, while the second shows the IV estimates. The estimation sample includes individuals born between 1977 and 1985, appearing in the CSWH between 2006 and 2015, and aged 25 or more when interviewed. Data on occupational outcomes are not provided for the Basque Country and Navarra. Each regression also includes birth-province, cohort, and year fixed effects. Heteroskedasticity-robust standard errors clustered at the province level in parenthesis. The pre-reform mean refers to the mean of the outcome variables from age 25 onward, for the 1970-1976 cohorts, the last seven cohorts not affected by the reform.

*** p<0.01, ** p<0.05, * p<0.1.

Table A.12: **First stage - province-cohort level**

	Share of students in general education (1)
ShareStudGen1976*1978 FE	-0.0609 (0.0712)
ShareStudGen1976*1979 FE	-0.293 (0.253)
ShareStudGen1976*1980 FE	-0.149* (0.0870)
ShareStudGen1976*1981 FE	-0.438*** (0.126)
ShareStudGen1976*1982 FE	-0.643*** (0.137)
ShareStudGen1976*1983 FE	-0.782*** (0.135)
ShareStudGen1976*1984 FE	-0.880*** (0.109)
ShareStudGen1976*1985 FE	-1.022*** (0.118)
Observations	468
Kleibergen-Paap F-stat	26.28

Source: Labor Force Survey 2002-2017, cohorts 1977-1985.

Notes: This table reports the first stage regression, estimated with data collapsed at the birth-province and cohort level. The main regressors are interaction terms between the share of 14-16 years old students enrolled in general education when cohort 1976 is 14, $ShareStudGen_{1976p}$, and fixed effects for the cohorts affected by the reform. The regression also includes the following controls: share of left-wing municipalities, and GDP per capita, both measured when the individual interviewed was 14, unemployment rate, share of population with high school or more, higher-education wage premium, share of employment in construction and manufacturing, all measured when the individual interviewed was 16, share of women, log cohort size, birth-province, and cohort fixed effects. Heteroskedasticity-robust standard errors clustered at the province level in parenthesis.

*** p<0.01, ** p<0.05, * p<0.1.

Table A.13: **Educational outcomes - province-cohort level**

	Age at highest qualification	Type of education acquired	
		General	Vocational
	(1)	(2)	(3)
OLS results			
Share Students in General Edu	-0.236	0.0541	-0.119**
	(0.631)	(0.0754)	(0.0561)
IV results			
Share Students in General Edu	1.416	0.437***	-0.496***
	(1.008)	(0.101)	(0.0785)
Observations	468	468	468
Pre-Reform Mean	19.28	0.39	0.22
Kleibergen-Paap F-stat	26.28	26.28	26.28
Sargan test p-value	0.480	0.389	0.371

Source: Labor Force Survey 2002-2017, cohorts 1977-1985.
Notes: This table reports the impact of the reform on educational choices, obtained from the estimation of regression 1 with data collapsed at the birth-province and cohort level. Each column refers to the outcome considered, being this the age at highest qualification (Column 1), the probability of holding a post-compulsory general qualification (Column 2), or the probability of holding a post-compulsory vocational qualification (Column 3). The first panel reports the OLS results, while the second shows the IV estimates. The estimation sample includes individuals born between 1977 and 1985, interviewed between 2002 and 2017, and aged 25 or more when interviewed. The response rate on age at highest qualification is 1% smaller than for other outcomes. Each regression also includes the following controls: share of left-wing municipalities, and GDP per capita, both measured when the individual interviewed was 14, unemployment rate, share of population with high school or more, higher-education wage premium, share of employment in construction and manufacturing, all measured when the individual interviewed was 16, share of women, log cohort size, birth-province, and cohort fixed effects. Heteroskedasticity-robust standard errors clustered at the province level in parenthesis. The pre-reform mean refers to the mean of the outcome variables from age 25 onward, for the 1970-1976 cohorts, the last seven cohorts not affected by the reform.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A.14: **Occupational outcomes and wages - province-cohort level**

	Occupations			
	High-skilled	Semi-skilled	Low-skilled	Log monthly wages
	(1)	(2)	(3)	(4)
OLS results				
Share Students in General Edu	0.0445***	-0.00566	-0.0389	0.229*
	(0.0145)	(0.0251)	(0.0253)	(0.116)
IV results				
Share Students in General Edu	0.0684**	-0.0582	-0.0102	0.479***
	(0.0280)	(0.0372)	(0.0366)	(0.0892)
Observations	468	468	468	468
Pre-Reform Mean	0.10	0.60	0.31	7.36
Kleibergen-Paap F-stat	43.75	43.75	43.75	50.75
Sargan test p-value	0.404	0.359	0.526	0.195

Source: Continuous Sample of Working Histories 2006-2017, cohorts 1977-1985.
Notes: This table reports the impact of the reform on occupational outcomes and monthly wages, obtained from the estimation of regression 1, with data collapsed at the birth-province and cohort level. Each column refers to the outcome considered, being this the probability of working in a high-skilled occupation (Column 1), semi-skilled occupation (Column 2), low-skilled occupation (Column 3), or log monthly wages. The first panel reports the OLS results, while the second shows the IV estimates. The estimation sample includes individuals born between 1977 and 1985, appearing in the CSWH between 2006 and 2015, and aged 25 or more when interviewed. Data on occupational outcomes are not provided for the Basque Country and Navarra. Each regression also includes the following controls: share of left-wing municipalities, and GDP per capita, both measured when the individual interviewed was 14, unemployment rate, share of population with high school or more, higher-education wage premium, share of employment in construction and manufacturing, all measured when the individual interviewed was 16, share of women, log cohort size, birth-province, and cohort fixed effects. Heteroskedasticity-robust standard errors clustered at the province level in parenthesis. The pre-reform mean refers to the mean of the outcome variables from age 25 onward, for the 1970-1976 cohorts, the last seven cohorts not affected by the reform.
*** p<0.01, ** p<0.05, * p<0.1.

Table A.15: **Zero first stage**

	(1) Share of students in general education
ShareGenStud1990*1971 dummy	-0.0794*
	(0.0401)
ShareGenStud1990*1972 dummy	0.113**
	(0.0482)
ShareGenStud1990*1973 dummy	0.100**
	(0.0391)
ShareGenStud1990*1974 dummy	0.116**
	(0.0435)
ShareGenStud1990*1975 dummy	0.283***
	(0.0564)
Observations	1108437
Kleibergen-Paap F-stat	7.939

Source: Labor Force Survey 2002-2017, cohorts 1970-1975.
Notes: This table reports the first stage regression applied to the pre-reform cohorts born between 1970 and 1975. The main regressors are interaction terms between the share of 14-16 years old students enrolled in general education when cohort 1976 is 14, $ShareStudGen_{1976p}$, and fixed effects for the cohorts considered. The regression also includes the following controls: share of left-wing municipalities, and GDP per capita, both measured when the individual interviewed was 14, unemployment rate, share of population with high school or more, higher-education wage premium, share of employment in construction and manufacturing, all measured when the individual interviewed was 16, gender, log cohort size, birth-province, cohort, and year fixed effects. Heteroskedasticity-robust standard errors clustered at the province level in parenthesis.
*** p<0.01, ** p<0.05, * p<0.1.

Table A.16: **Cross-province migration by level of education**

	25 +	
	High-school diploma or more (1)	Lower secondary education (2)
OLS results		
Share Students in General Edu	0.0692*	0.0858
	(0.0371)	(0.0539)
IV results		
Share Students in General Edu	0.235**	0.0441
	(0.107)	(0.120)
Observations	517898	250803
Pre-Reform Mean	0.19	0.14
Kleibergen-Paap F-stat	37.81	38.18
Sargan test p-value	0.157	0.524

Source: Labor Force Survey 2002-2017, cohorts 1977-1985.

Notes: This table compares the impact of the reform on the probability of migrating to a different province for individuals with a high-school diploma or more, and those with lower-secondary education. The first panel reports OLS effects, while the second shows IV estimates. The estimation sample includes individuals born between 1977 and 1985, interviewed between 2002 and 2017, and aged 25 or more when interviewed. The regressions also include the following controls: share of left-wing municipalities, and GDP per capita, both measured when the individual interviewed was 14, unemployment rate, share of population with high-school diploma or more, higher-education wage premium, share of employment in construction and manufacturing, all measured when the individual interviewed was 16, gender, log cohort size, birth-province, cohort, and year fixed effects. Heteroskedasticity-robust standard errors clustered at the province level in parenthesis. The pre-reform means refer to the mean of the outcome variable for each subgroup, estimated from age 25 onward, for the 1970-1976 cohorts, the last seven cohorts not affected by the reform.

*** p<0.01, ** p<0.05, * p<0.1.

Table A.17: **Occupational outcomes and wages by level of education - age at first job 18**

	Occupations						Log monthly wages	
	High-skilled		Semi-skilled		Low-skilled			
	High-school diploma or more (1)	Lower secondary education (2)	High-school diploma or more (3)	Lower secondary education (4)	High-school diploma or more (5)	Lower secondary education (6)	High-school diploma or more (7)	Lower secondary education (8)
OLS results								
Share Students in General Edu	0.0418**	0.00473	0.00826	-0.101*	-0.0501*	0.0968	0.163	0.148
	(0.0193)	(0.00948)	(0.0268)	(0.0595)	(0.0275)	(0.0582)	(0.0976)	(0.137)
IV results								
Share Students in General Edu	0.0985**	-0.0204	-0.102**	-0.0937	0.00348	0.114	0.446**	0.114
	(0.0400)	(0.0213)	(0.0481)	(0.0991)	(0.0587)	(0.0909)	(0.204)	(0.226)
Observations	1187431	250701	1187431	250701	1187431	250701	1216921	267033
Pre-Reform Mean	0.13	0.01	0.58	0.65	0.29	0.33	7.47	7.24
Kleibergen-Paap F-stat	35.43	39.35	35.43	39.35	35.43	39.35	46.77	46.44
Sargan test p-value	0.403	0.755	0.472	0.379	0.481	0.363	0.683	0.665

Source: Continuous Sample of Working Histories 2006-2017, cohorts 1977-1985.

Notes: This table compares the impact of the reform on occupational outcomes and monthly wages of individuals with a high-school diploma or more, and those with lower-secondary education. Each column refers to the outcome considered,, being this the probability of working in a high-skilled occupation (Columns 1-2), semi-skilled occupation (Columns 3-4), low-skilled occupation (Columns 5-6) , or log monthly wages (Columns 7-8) for each subgroup. The first panel reports OLS effects, while the second shows IV estimates. The estimation sample includes individuals born between 1977 and 1985, interviewed between 2003 and 2017, and aged 25 or more when interviewed. Data on occupational outcomes are not provided for the Basque Country and Navarra. The regression also includes the following controls: share of left-wing municipalities, and GDP per capita, both measured when the individual interviewed was 14, unemployment rate, share of population with high-school diploma or more, higher-education wage premium, share of employment in construction and manufacturing, all measured when the individual interviewed was 16, gender, log cohort size, birth-province, cohort, and year fixed effects. Heteroskedasticity-robust standard errors clustered at the province level in parenthesis. The pre-reform means refer to the mean of the outcome variables for each subgroup, estimated from age 25 onward, for the 1970-1976 cohorts, the last seven cohorts not affected by the reform.

*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A.18: **Employment prospects before/after crisis - low-educated individuals**

	Employed		Unemployed		Inactive	
	Before (1)	After (2)	Before (3)	After (4)	Before (5)	After (6)
OLS results						
Share Students in General Edu	0.142	-0.0686	-0.126	0.0561	-0.0158	0.0125
	(0.0912)	(0.0677)	(0.0804)	(0.0443)	(0.0874)	(0.0460)
IV results						
Share Students in General Edu	0.533**	-0.301**	-0.244	0.120	-0.290	0.181*
	(0.266)	(0.148)	(0.180)	(0.106)	(0.214)	(0.0991)
Observations	71952	178851	71952	178851	71952	178851
Pre-Reform Mean	0.67	0.60	0.14	0.22	0.19	0.18
Kleibergen-Paap F-stat	4.990	37.05	4.990	37.05	4.990	37.05
Sargan test p-value	0.423	0.711	0.340	0.354	0.818	0.409

Source: Labor Force Survey 2002-2017, cohorts 1977-1985.

Notes: This table compares the impact of the reform on employment prospects of individuals with lower-secondary education before and after the financial crisis, i.e. between 2002-2009 and 2009-2017. Each two column refer to the outcome considered, being this the probability of being employed (Columns 1-2), unemployed (Columns 2-3), or inactive (Columns 4-5). The first panel reports OLS effects, while the second shows IV estimates. The estimation sample includes low-educated individuals born between 1977 and 1985, interviewed between 2002 and 2017, and aged 25 or more when interviewed. The regression also includes the following controls: share of left-wing municipalities, and GDP per capita, both measured when the individual interviewed was 14, unemployment rate, share of population with high-school diploma or more, higher-education wage premium, share of employment in construction and manufacturing, all measured when the individual interviewed was 16, gender, log cohort size, birth-province, cohort, and year fixed effects. Heteroskedasticity-robust standard errors clustered at the province level in parenthesis. The pre-reform means refer to the mean of the outcome variables for each subgroup, estimated from age 25 onward, for the 1970-1976 cohorts, the last seven cohorts not affected by the reform.

*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A.19: **Employment prospects before/after crisis - high-school diploma +**

	Employed		Unemployed		Inactive	
	Before (1)	After (2)	Before (3)	After (4)	Before (5)	After (6)
OLS results						
Share Students in General Edu	0.0844	0.0545	-0.0740***	-0.0180	-0.0105	-0.0366
	(0.0603)	(0.0441)	(0.0264)	(0.0255)	(0.0495)	(0.0316)
IV results						
Share Students in General Edu	-0.270	0.228**	-0.0726	-0.109*	0.343**	-0.119*
	(0.174)	(0.106)	(0.117)	(0.0635)	(0.170)	(0.0693)
Observations	154430	363468	154430	363468	154430	363468
Pre-Reform Mean	0.73	0.80	0.12	0.11	0.15	0.08
Kleibergen-Paap F-stat	4.400	40.33	4.400	40.33	4.400	40.33
Sargan test p-value	0.343	0.618	0.183	0.709	0.339	0.296

Source: Labor Force Survey 2002-2017, cohorts 1977-1985.

Notes: This table compares the impact of the reform on employment prospects of middle- and highly-educated individuals before and after the financial crisis, i.e. between 2002-2009 and 2009-2017. Each two columns refer to the outcome considered, being this the probability of being employed (Columns 1-2), unemployed (Columns 3-4), or inactive (Columns 5-6). The first panel reports OLS effects, while the second shows IV estimates. The estimation sample includes individuals with a high-school diploma or more born between 1977 and 1985, interviewed between 2002 and 2017, and aged 25 or more when interviewed. The regression also includes the following controls: share of left-wing municipalities, and GDP per capita, both measured when the individual interviewed was 14, unemployment rate, share of population with high-school diploma or more, higher-education wage premium, share of employment in construction and manufacturing, all measured when the individual interviewed was 16, gender, log cohort size, birth-province, cohort, and year fixed effects. Heteroskedasticity-robust standard errors clustered at the province level in parenthesis. The pre-reform means refer to the mean of the outcome variables for each subgroup, estimated from age 25 onward, for the 1970-1976 cohorts, the last seven cohorts not affected by the reform.

*** p<0.01, ** p<0.05, * p<0.1.