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# ABSTRACT

## Who Benefits from General Knowledge?*

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 sheds light on this debate by exploiting a recent Spanish reform that postpones students' choice between these two educational pathways from age 14 to 16. To identify exogenous changes in its staggered implementation, we instrument this with the pre-reform across-province variation in the share of students in general education. Results indicate that, by shifting educational investment from vocational to general education after age 16, the reform improves occupational outcomes, and results in a significant rise in monthly wages. The effects are larger after the financial crisis, but 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.

**JEL Classification:** I26, I28, J24

**Keywords:** general versus vocational education, heterogeneous returns, financial crisis

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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 vs 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

[1]For instance, Pekkarinen et al. (2009) find that a Finnish comprehensive reform implemented in the 1970s resulted in a 23% 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.

increase in educational attainment, 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 recent comprehensive school reform on educational investment and 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 across-province variation in the share of students in 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-Employee 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 two main findings. First, by shifting educational investment from vocational to general education after age 16, without affecting overall educational attainment, the reform generates large labor market returns for the affected individuals. Specifically, a 30 percentage point increase in the share of 14-16 year-old students enrolled in general education[2] increases by 10 percentage points the share of individuals who complete general

[2]Note that the reform brings the share of 14-16 year-old students enrolled in the lower-secondary general track from a baseline share of 70 to 100 percent. From now on, for brevity, we will refer to this 30 percentage point increase in the share of students enrolled in compulsory general education as the effect of the reform.

studies after age 16, and decreases by 13 percentage points the proportion of individuals with a post-compulsory vocational qualification, with these effects taking place both at secondary and tertiary level.[3] In turn, this shift in educational investment from vocational to general education translates into a 3 percentage point rise in the probability of being employed in a high-skilled profession, a 5 percentage point decrease in the probability of working in a semi-skilled occupation,[4] and a 12 percent increase in monthly wages.[5]

However, our second 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 compulsory basic general education by 9 percentage points, or 15 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.[6]

All these results are highly significant and robust to different model specifications. Yet, more fundamentally, the validity of our identification strategy relies on the assumption that baseline differences in the educational distribution do not capture differential trends across provinces in the outcomes of interest. To support this hypothesis, we perform event-study exercises showing that the evolution of the outcome variables is comparable across provinces before the approval of the reform.

Additional findings shed light on the mechanisms behind the main effects. First, our analysis of OECD PIAAC data suggests that this reform significantly increases the literacy skills of affected cohorts. Besides, using the Labor Force Survey, we find that the reform increases across-province migration of individuals with at least a high-school diploma after age 25. Taken together, these two pieces of evidence suggest that the larger labor market

[3]Compared to the pre-reform mean, these effects correspond, respectively, to a 26 percent increase in the share of individuals who complete general studies after age 16, and a 58 percent decrease in the proportion of individuals with a post-compulsory vocational qualification.

[4]Compared to the pre-reform mean, these effects correspond, respectively, to a 30 percent higher probability of being employed in a high-skilled profession, and a 9 percent decrease in the probability of working in a semi-skilled occupation.

[5]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.

[6]Note that, on average, the reform does not change the composition of this group, as it does not affect the decision to continue studying after compulsory education.

returns enjoyed by middle- to highly-educated individuals may be explained by a combination of increased human capital, and higher propensity 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, when it translates into a lower labor force participation rate. 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 (Pekkala et al. 2013, Pekkarinen et al. 2009, Meghir and Palme 2005). As such, this is the ideal setting to identify the actual returns to general vs vocational education, without confounding these with the effect of acquiring more years of education. 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 across-country evidence provided by Hampf and Woessmann (2017) and 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. 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).

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 *L*ey General de Educacion. 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 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*, could students choose whether to leave school or to enroll into either upper secondary general education or a vocational program, both lasting two years.

While the reform did not bring about major modifications until grade 8,[7] it represented 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 background in Spanish, Math, a foreign language and civics, each of them was meant to provide students with the basic skills to practice a specific profession. On the contrary, the new comprehensive track mainly offers an academic curriculum up until age 16.

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

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 2, 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.[8] 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 measured in year 2016. No jump in this relationship is visible in either of the two panels.[9]

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. 2017). 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 15 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 3 shows the evolution of this variable. Each

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

[9] 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.

dot represents the across-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 across-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.[10]. To do so, 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 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 across-province variation in the share of students enrolled in general education, interacted with cohort-fixed effects. This corresponds to estimating the following 2SLS model:

$$Y_{icpy} = \alpha + X'_{cp}\pi + \delta_c + \gamma_p + \theta_y + \beta_0 \widehat{ExposureReform}_{cp} + u_{icpy} \tag{1}$$

where $i$ is an individual belonging to one of the $c$ cohorts affected by the reform, 1977-1985, born in province $p$,[11] and whose outcome is observed in year $y$. $Y_{icpy}$ is the outcome of interest,

[10]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 across-province variation in the implementation of the reform. Space constraints and economic resources may have played a role. 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. 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. This element of uncertainty, coupled with mounting criticisms of staggered difference-in-difference estimators, have led us to consider an alternative identification strategy (De Chaisemartin and D'Haultfoeuille 2019, Borusyak et al. 2018, Goodman-Bacon 2018).

[11]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

including educational choices, labor market and occupational outcomes, and wages. The main regressor of interest is $ExposureReform_{cp}$, which is measured as the share of 14-16 year-old students enrolled in the lower-secondary general track when cohort $p$ is 14, as described above. We instrument it with $\sum_{c=1978}^{1985} \beta_c * ShareGenStud_{p1976} * \delta_c$, that is cohort fixed effects $\delta_c$ interacted with $ShareGenStud_{p1976}$, 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.

In all regressions, we also control for 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 province unemployment rate, share of population with high-school education or more, higher-education wage premium, all measured in province $p$ when individual $i$ is 16.[12] Finally, we include cohort, birth province, and year-of-interview fixed effects. As for standard errors, we use heteroskedasticy-robust standard errors clustered at the province-of-birth level - 52 groups.

The validity of this identification strategy relies on two assumptions. First, the instruments have to be relevant, that is they have to be correlated with the variable we want to instrument. To verify whether this is the case, we will estimate the first stage regression:

$$ExposureReform_{cp} = \alpha + X'_{cp}\pi + \delta_c + \gamma_p + \theta_y + \sum_{c=1978}^{1985} \beta_c * ShareGenStud_{p1976} * \delta_c + u_{icpy} \quad (2)$$

and check that the F-statistic is above the rule-of-thumb threshold of 10.[13]

Second, the instruments have to satisfy the exclusion restriction, i.e., they have to be uncorrelated with the error term in the main regression. In this context, this corresponds to

the treatment may introduce some measurement error, this allows us to circumvent any issue of endogenous migration.

[12] In the appendix, we also report the estimates of regressions that do not include these province-cohort varying controls.

[13] Note that, in equation 2, the interaction term between $ShareGenStud_{p1976}$ and the fixed effect for cohort 1977 is excluded to avoid collinearity with the constant.

assuming that baseline differences in the educational distribution do not capture differential trends across provinces in the outcomes of interest. To support this hypothesis, we will perform the following event-study exercise:

$$Y_{icpy} = \alpha + X'_{cp}\pi + \delta_c + \gamma_p + \theta_y + \sum_{c=1971}^{1985} \beta_c * ShareGenStud_{p1976} * \delta_c + u_{icpy} \quad (3)$$

In practice, this corresponds to estimating the dynamic reduced-form version of equation 1 augmented with interaction terms between the $ShareGenStud_{p1976}$ and fixed effects for the last six cohorts not affected by the reform.[14] Were we to find that the these interaction terms are not statistically different from zero, this would strongly support the hypothesis that the evolution of the variables studied is comparable across provinces before the approval of the reform.[15]

Finally, note that a priori it is not clear whether IV estimates should be larger or smaller than the OLS ones. As IV estimates capture the effect on compliers - individuals who are induced to change the type of education acquired because of the reform - they could be larger than the OLS ones. Next, the direction of the bias in OLS estimates is also ambiguous, as it depends on the correlation between factors driving the implementation of the reform and the outcomes of interest.

# 4 Data

To measure our main outcomes of interest, we use the Spanish Labor Force Survey,[16] LFS hereafter, and the Continuous Sample of Working Histories,[17] or CSWH.

The cohorts affected by the reform are those born between 1977 and 1985. We measure

[14] Note that the interaction term between $ShareGenStud_{p1976}$ and the fixed effect for cohort 1970 is set as the reference group.

[15] As we have more than one instrument, at the bottom of each table, we will also report the p-value of the Sargan test associated with the outcome analyzed.

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

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

their educational choices and labor market status from age 25 onward using the LFS from 2002 to 2017.[18] 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% non-stratified random sample of individuals who are registered with the Social Security in the reference year.[19] 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.[20]

To investigate the mechanisms behind the main results, we further 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

[18] 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.

[19] 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.

[20] While the LFS also provides self-reported occupational outcomes, the administrative information provided by the CSWH may be 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.

on literacy and numeracy skills for the Spanish population. Specifically, the data set reports individuals performance over 10 answers, measured on a scale from 0 to 500. We standardize the answer to each question, and consider each individual's worst answer.[21] 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 municipal electoral results;[22] 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, 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, and the university wage premium, all of which are taken from the LFS. While the first two are available for all cohorts from 1983 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 stats 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

[21] Results are qualitatively similar when considering the median or best answer.

[22] 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.

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. This simply indicates that the larger the pre-reform share of 14-16 year-old students enrolled in general education, the slower is the rhythm of implementation of the reform. Second, the Kleibergen-Paap F-stat, 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.[23]

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 across-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,[24] 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 (0.348*0.30) in the share of individuals acquiring general education after age 16, or 26 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 13 percentage point decrease in the share of individuals acquiring post-compulsory vocational studies, or 58 percent compared to the pre-reform mean.[25]

[23]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.

[24]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.3, 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.

[25]Columns 4-6 of table A.3 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 60 percent compared to the pre-reform mean, while decreasing the share with a vocational high-school

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, table 5 shows that the reform shifts the occupational distribution from semi-skilled to high-skilled occupations, bringing large complementary wage returns. Specifically, columns 1-3 show that the reform leads to a 3 percentage point increase in the probability of working in a high-skilled occupation (30 percent relative to pre-reform mean), a 5 percentage point decrease in the probability of being employed in a semi-skilled occupation (9 percent compared to the pre-reform mean), and no average effect on low-skilled occupations. Accordingly, column 4 shows that the reform increases monthly wages by 12 percent.[26]

However, such large relative returns to general education are not equally enjoyed along the educational distribution. Figure 4 presents heterogeneous effects by level of education.[27] According to these results, the reform leads to a 9 percentage point decrease (14 percent relative to the pre-reform mean) in employment prospects of individuals who acquire only compulsory education - 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.[28]

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

# 6 Robustness checks

The validity of this identification strategy relies on the assumptions that the instruments chosen are both relevant and exogenous. This section provides evidence in favor of this second

diploma by roughly the same amount. At tertiary level, postponing tracking from age 14 to 16 translates into a 5 percentage point increase in the share of individuals with a tertiary general degree - 16 percent compared to the pre-reform mean - and a 8 percentage point reduction in the share of individuals with a tertiary vocational degree - 69 percent compared to the pre-reform mean.

[26]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.4 in the appendix shows that we obtain qualitative similar results when using the LFS to analyze occupational outcomes.

[27]Note that, as this reform does not affect educational attainment, this appears the ideal setting to study its effects along the educational distribution.

[28]Table A.5 in the appendix reports the corresponding detailed regression results.

assumption. The main concern in this context is that the pre-reform across-province variation in the share of students in general education may be picking up underlying trends in the outcomes of interest. Figures 5-7 plot the estimates of the leads and lags of the reform from regression 3, together with 95 percent confidence intervals.[29] Reassuringly, almost all the estimates of the leads of the reform are insignificant across the different graphs, suggesting that the evolution of the variables studied is comparable across provinces before the approval of the reform. On the contrary, the reduced-form dynamic estimates of the reform tend to be significant in accordance with the IV results.[30]

Further backing the exogeneity assumption, note that our instruments pass the Sargan test for over-identifying restrictions in all our main regressions. In the appendix, tables A.8-A.10, we also show that our estimates do not vary much when time-varying province controls are excluded from the regressions, which should further support the validity of our identification strategy.

Finally, regarding the comparison between the OLS and IV estimates, the previous section has shown that IV results are systematically larger than OLS ones. Most likely, this suggests that the LATE is much larger than the average effect on all affected individuals.

# 7 Proposed mechanisms

This section aims to shed light on the mechanisms behind the main results. First, we want to understand what brings middle- to highly-educated individuals to experience 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 or mostly a signaling effect of an academic qualification.[31] Column 1 of table 6 shows that the reform leads to a significant

[29]Figure A.1 in the appendix presents the event studies for educational outcomes at both secondary and tertiary level.

[30]Note that, as in the case of the first stage regression, the sign of the estimated coefficients are opposite to the IV effect, reflecting the fact that the speed of implementation of the reform is slower in provinces that have a larger share of 14-16 year-old students in general education pre-reform.

[31]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 95-percent wild-bootstrap confidence intervals.

0.52 standard deviation increase in individuals' literacy skills. Point estimates in column 2 point to a similar effect on numeracy skills, but the coefficient is imprecisely estimated.[32]

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 7 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.[33] However, the reform significantly increases the probability of migrating to a different province from age 25 onward by 30 percent compared to the pre-reform mean.[34] 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 the large relative returns to general education for middle- to highly-educated individuals may be explained by a combination of increased human capital and a higher propensity to move in search of better job matches.[35]

The second aim of this section is to understand the heterogeneous returns to general education. Figures 9 and 10 provide suggestive evidence that employment trajectories of the two

[32]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. Table A.11, in the appendix, shows that the estimates remain qualitatively similar but imprecisely estimated when time-varying controls are excluded from the regressions. Finally, in tables A.12, and A.13, we complement this analysis on the worst performance, by showing that results are similar but marginally insignificant when considering, respectively, the median and best performance across the 10 PIAAC questions.

[33]We also considered the possibility that the reform affects the probability to migrate 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 1 and 5 percent, suggesting that our results could hardly be explained by any selection effect into migration.

[34]This result is just marginally insignificant when time-varying controls are not included, as shown in table A.14.

[35]Figures 8 and A.2, in the appendix, further back this hypothesis by showing that the positive wage effects, as well as the increased probability of working in high-skilled occupations and migration effects, are driven by middle- to highly-educated individuals. Tables A.6 and A.7 show the corresponding detailed regression results. Note that when performing the heterogeneity analysis with the CSWH, figure 8, 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 the 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. Finally, we also studied the impact of the reform on the probability of having a permanent contract, and hours worked, but did not find significant results on these margins.

educational groups start diverging after the financial crisis.[36] On the one hand, figure 9 shows that from 2009 onward - compared to the period 2002-2008 - the reform not only decreases employment prospects of low-educated individuals, but also increases by one third their probability of leaving the labor market, compared to the pre-reform mean. On the other hand, figure 10 shows that, after the financial crisis, the reform increases the probability of being employed for middle- to highly-educated individuals by 8 percent compared to the pre-reform mean.[37] One potential explanation for such different effects can be found by looking back at figure 8. 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. Overall, these results are consistent with the hypothesis that general education provides the learning skills that strengthen workers' adaptability to structural labor market changes. However, they also show that general knowledge provides little marketable value at the bottom of the educational distribution.

# 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 two main findings. On the one hand, general education brings large relative returns in the labor market, in the form of a higher probability of being employed in

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

[37] Tables A.15 and A.16, 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.

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, which supports the hypothesis that general education strengthens workers' adaptability to structural labor market changes.

However, our second key finding is that only middle to highly-educated individuals enjoy these relative returns from general education. The reform strongly worsens the employment prospects of individuals who only acquire compulsory education, both at the extensive and intensive margin. Importantly, this does not seem to be a temporary effect that vanishes over the life-cycle - as suggested by Hampf and Woessmann (2017) - but materialized after the financial crisis, when the affected cohorts are already in their 30s. This finding has important policy implications. In particular, it suggests that incentivizing low-educated individuals to acquire occupational-specific skills may strongly improve their employability, especially in periods of economic turmoil.

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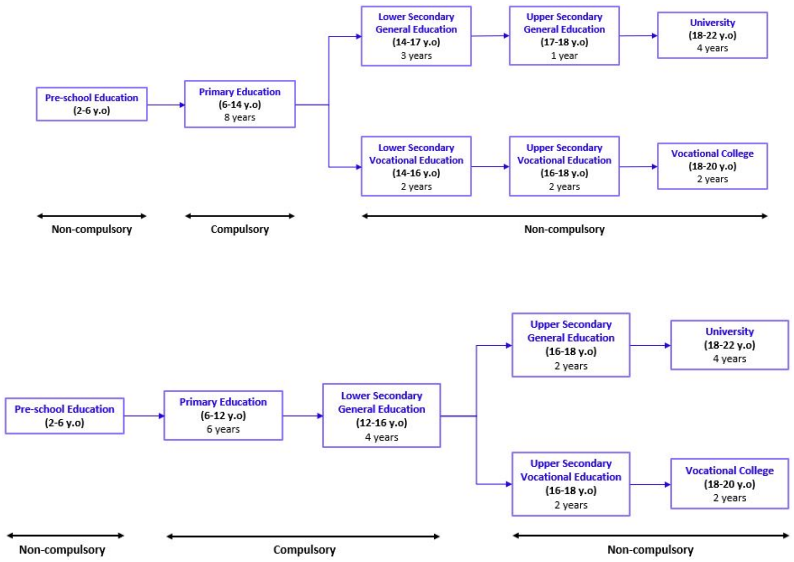
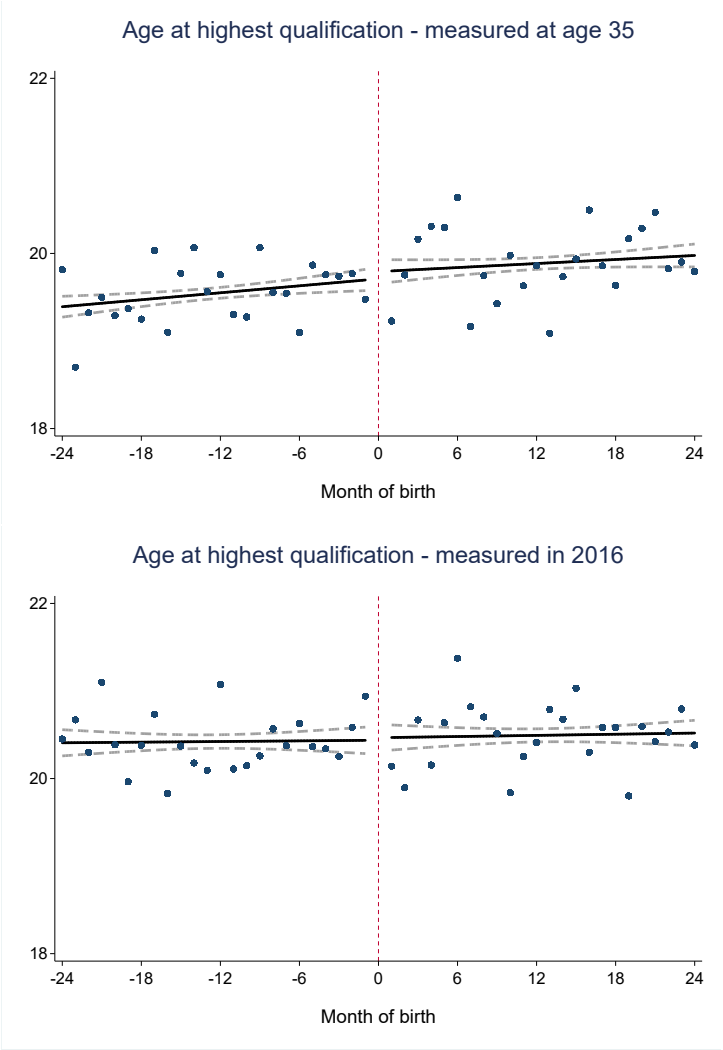
# 9 Graphs and tables

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



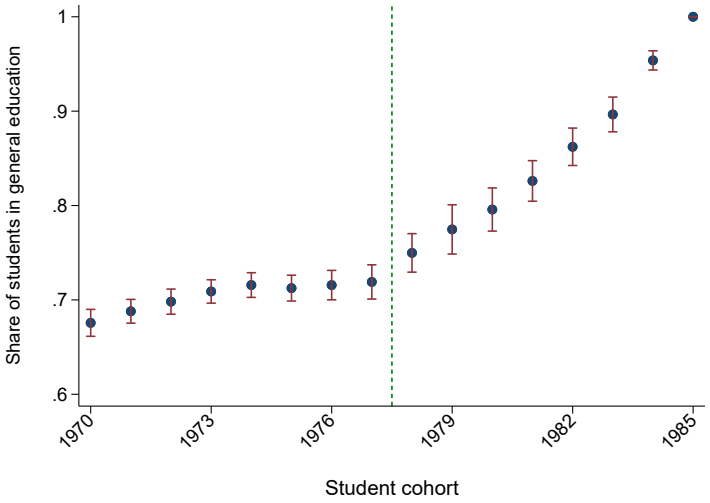

*Source:* Spanish Ministry of Education.
*Note:* This figure presents a schematic representation of the Spanish educational system. The top panel represents the old system, while the bottom one shows the new one.

Figure 2: **Increase in compulsory schooling and age at highest qualification**



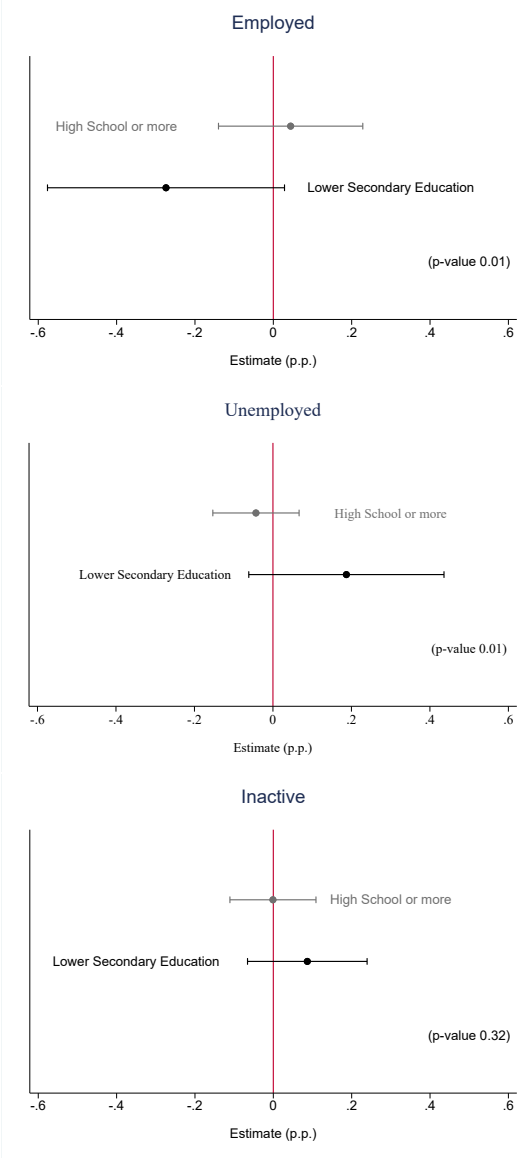

*Source:* Labor Force Survey, cohorts 1975-1978.
*Notes:* This figure shows the relationship between age at highest qualification and month of birth. The x-axis is normalized to 0 in January 1977, as 1977 is the first cohort affected by the rise in the compulsory schooling age. Each dot represents the 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 its side of the 0 threshold. 95 confidence intervals are also displayed.

Figure 3: **Share of 14-16 year-old students in general education**



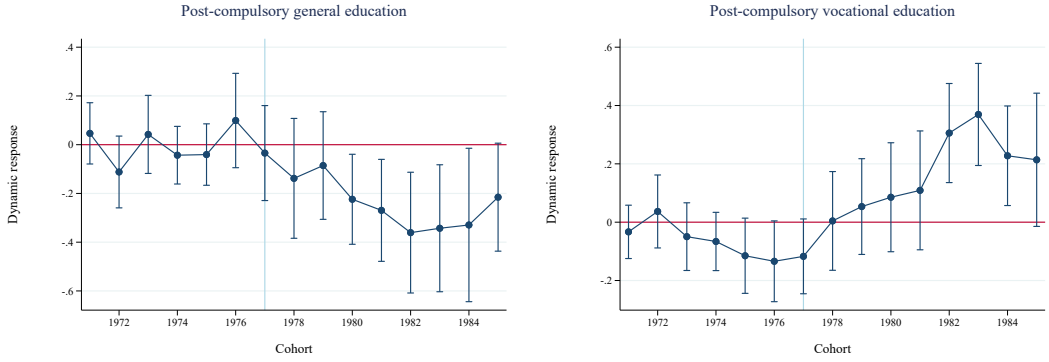

*Source:* Spanish Ministry of Education.
*Notes:* This figure reports the trends in the share of 14-16 year-old students in general education before and during the implementation of the reform. Each dot refers to the across-province share of students enrolled in general education, when the cohort displayed is 14. The difference between the 25th and 75th percentiles (interquartile range) for each year is also reported.

Figure 4: **Employment prospects by level of education**



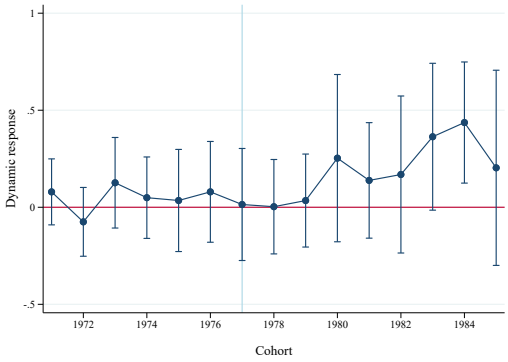

*Source:* Labor Force Survey 2002-2017, cohorts 1977-1985.
*Notes:* This figure compares the impact of the reform on employment prospects of individuals with a high-school diploma or more, and those with lower-secondary education. These results are obtained from the estimation of regression 1 by subgroup. 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, all measured when the individual interviewed was 16, 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 the p-value of the test on the equality of the estimated coefficients.

Figure 5: **Event Studies - educational outcomes**



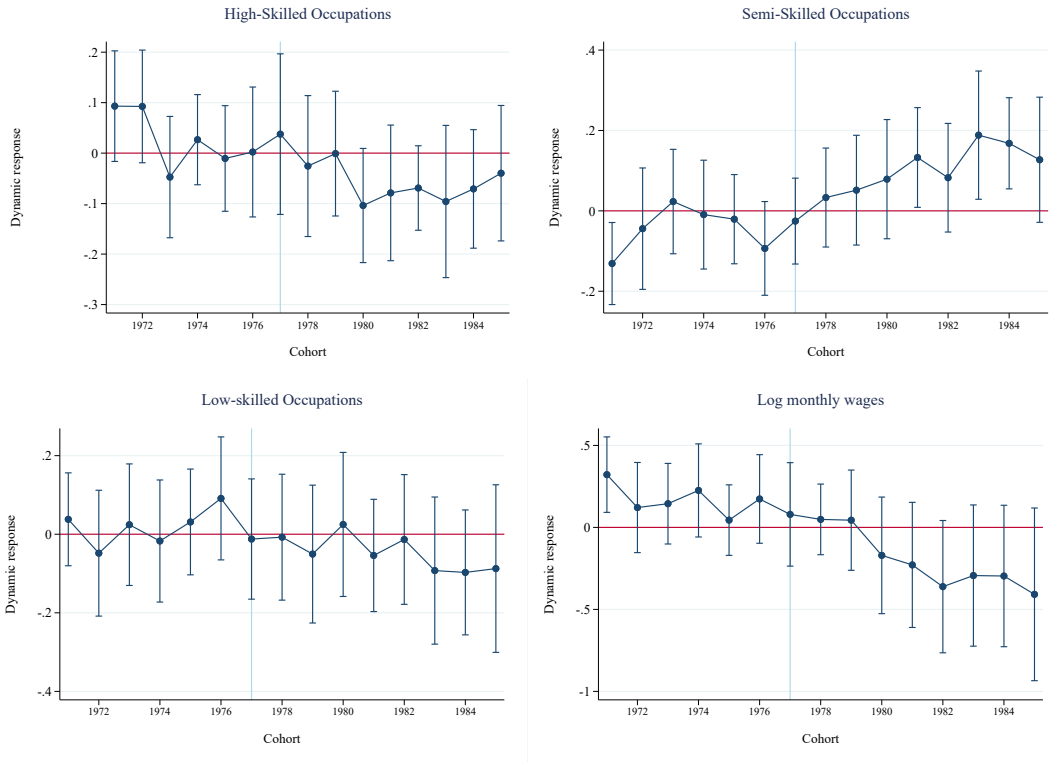

*Source:* Labor Force Survey 1995-2017, cohorts 1970-1985.
*Notes:* These graphs show the estimates of the leads and lags of the reform on educational choices, obtained from the estimation of regression 3. 95 percent confidence intervals are also reported. The outcomes considered are displayed on top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

Figure 6: **Event-study - Probability of being employed - low educated individuals**



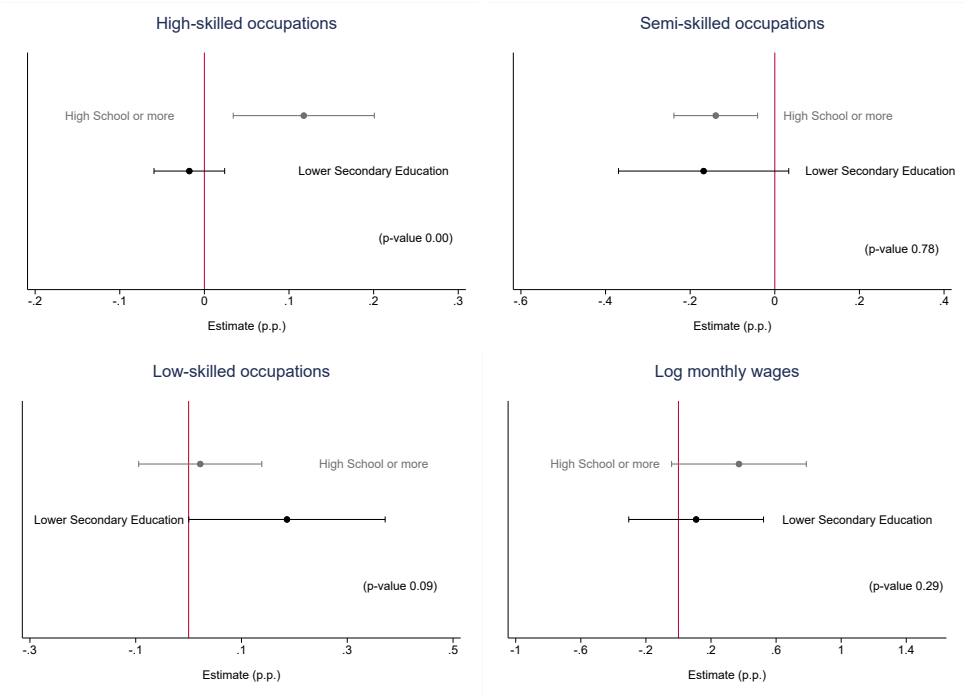

*Source:* Labor Force Survey 1995-2017, cohorts 1970-1985.
*Notes:* This graph shows the estimates of the leads and lags of the reform on employment prospects of low-educated individuals, obtained from the estimation of regression 3. 95 percent confidence intervals are also reported.

Figure 7: **Event Studies - occupational outcomes and monthly wages**



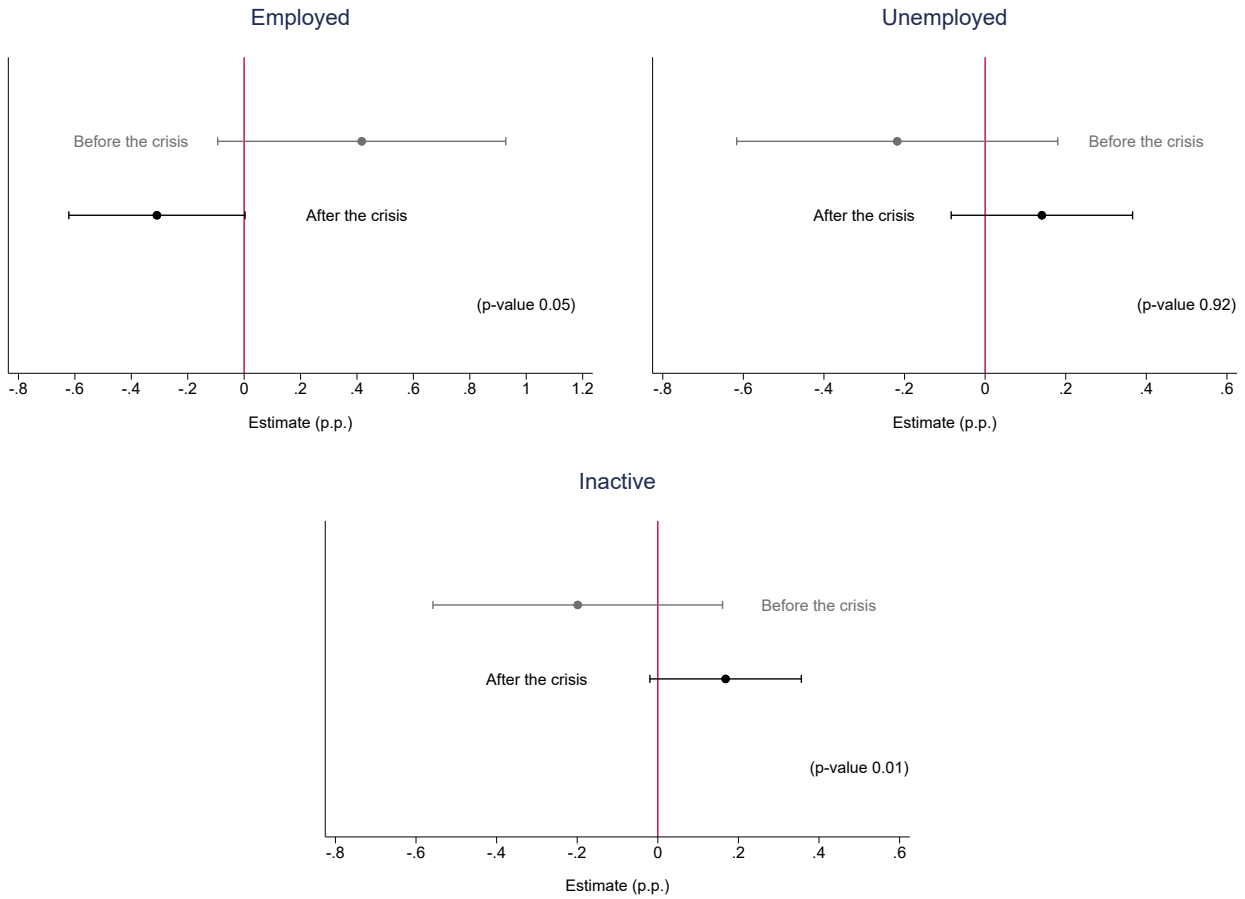

*Source:* Continuous Sample of Working Histories 2006-2017, cohorts 1970-1985.
*Notes:* These graphs show the estimates of the leads and lags of the reform on occupational outcomes and wages, obtained from the estimation of regression 3. 95 percent confidence intervals are also reported. The outcomes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

Figure 8: **Occupational outcomes and wages by level of education**



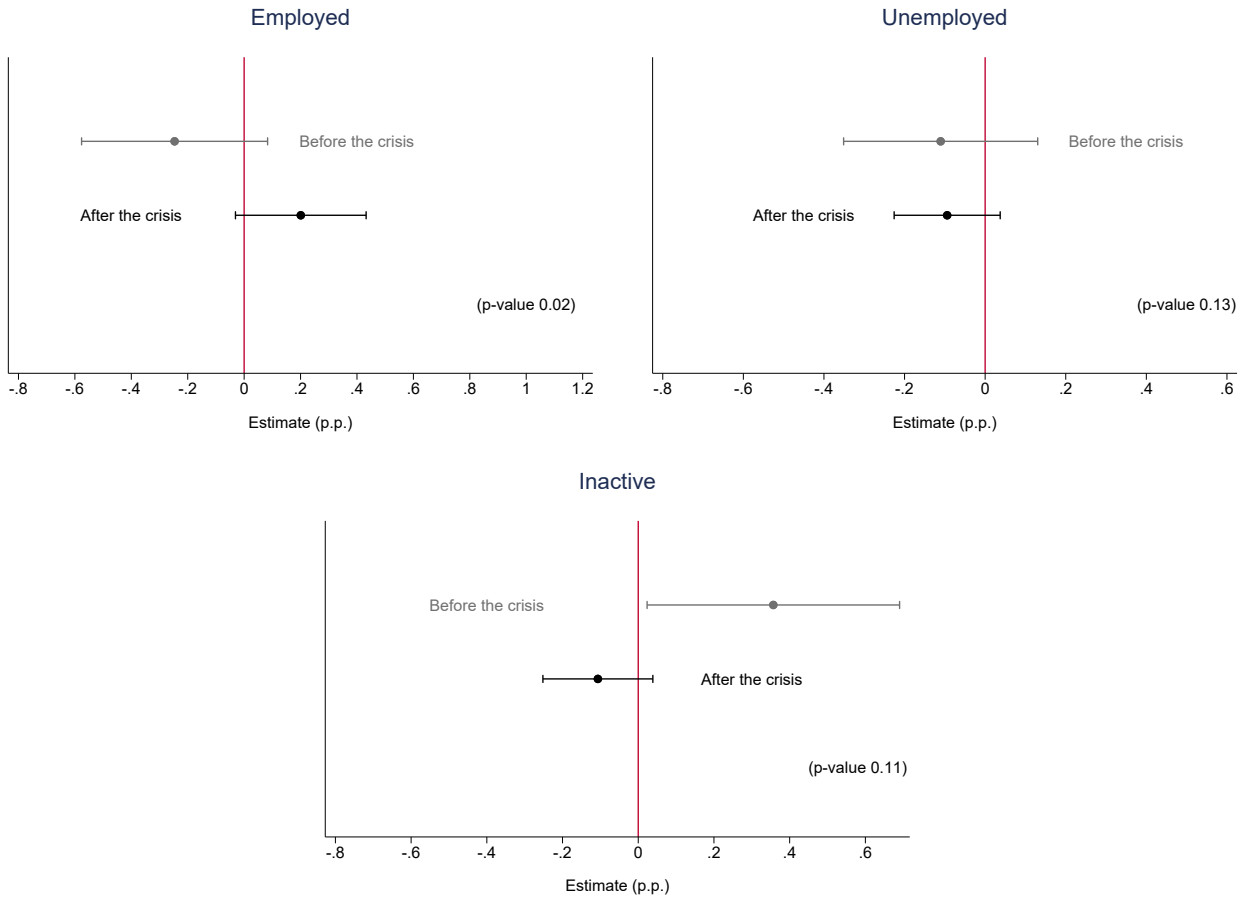

*Source:* Continuous Sample of Working Histories 2006-2017, cohorts 1977-1985.
*Notes:* This figure compares the impact of the reform on occupational and pay outcomes of individuals with a high-school diploma or more, and those with lower-secondary education. These results are obtained from the estimation 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, all measured when the individual interviewed was 16, 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 the p-value of the test on the equality of the estimated coefficients.

Figure 9: **Employment prospects before/after crisis - low-educated individuals**



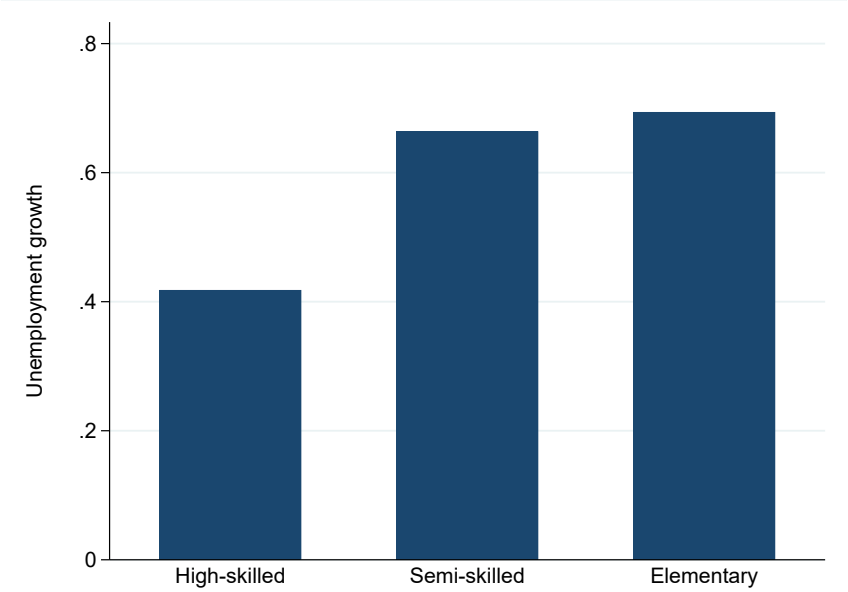

*Source:* Labor Force Survey 2002-2017, cohorts 1977-1985.
*Notes:* This figure compares the impact of the reform on employment prospects of individuals with lower-secondary education, before and after the financial crisis. These results are obtained from the estimation of regression 1 by subgroup. In 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: 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, all measured when the individual interviewed was 16, 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 the p-value of the test on the equality of the estimated coefficients.

Figure 10: **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, all measured when the individual interviewed was 16, 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 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.

Table 1: **Summary statistics**

| | Mean | Sd | Min | Max | N |
|---|---|---|---|---|---|
| **LFS outcomes** | | | | | |
| Age at highest qualification | 19.91 | 4.59 | 7.00 | 40 | 765,354 |
| Compulsory education | 0.32 | 0.47 | 0.00 | 1.00 | 768,701 |
| Post-compulsory general education | 0.42 | 0.49 | 0.00 | 1.00 | 768,701 |
| Post-compulsory vocational education | 0.25 | 0.44 | 0.00 | 1.00 | 768,701 |
| Employed | 0.71 | 0.46 | 0.00 | 1.00 | 768,701 |
| Unemployed | 0.17 | 0.37 | 0.00 | 1.00 | 768,701 |
| Inactive | 0.13 | 0.33 | 0.00 | 1.00 | 768,701 |
| Across-province migration | 0.15 | 0.36 | 0.00 | 1.00 | 768,701 |
| **CSWH outcomes** | | | | | |
| Monthly wages | 1,895 | 1,657 | 0 | 300,037 | 1,484,533 |
| High-skilled occupations | 0.09 | 0.28 | 0.00 | 1.00 | 1,438,774 |
| Semi-skilled occupations | 0.58 | 0.49 | 0.00 | 1.00 | 1,438,774 |
| Low-skilled occupations | 0.33 | 0.47 | 0.00 | 1.00 | 1,438,774 |
| **PIAAC outcomes** | | | | | |
| PIAAC Literacy score | 0.00 | 0.96 | -3.33 | 2.40 | 2,118 |
| PIAAC Numeracy score | 0.00 | 0.95 | -3.32 | 2.32 | 2,118 |
| **Controls** | | | | | |
| Cohort size | 19,716 | 21,291 | 807 | 89,243 | 468 |
| GDP per capita | 9870 | 2610 | 5230 | 19760 | 468 |
| Share left-wing municipalities | 0.42 | 0.27 | 0.00 | 1.00 | 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 | 52 |

*Notes:* This table reports summary statistics for outcome and control variables. Outcome variables refer to affected cohorts, born between 1977-1985. PIAAC outcomes are the smallest value among 10 standardized scores, calculated over the performance of the cohorts 1970-1985. 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 are measured when cohort $c$ is 16. Cohort size is measured at birth.

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

Table 2: **First stage**

| | Share of students in general education (1) |
|---|---|
| ShareGeneral1976*1978 FE | -0.0573 |
| | (0.0637) |
| ShareGeneral1976*1979 FE | -0.267 |
| | (0.204) |
| ShareGeneral1976*1980 FE | -0.167 |
| | (0.0998) |
| ShareGeneral1976*1981 FE | -0.379*** |
| | (0.122) |
| ShareGeneral1976*1982 FE | -0.572*** |
| | (0.131) |
| ShareGeneral1976*1983 FE | -0.723*** |
| | (0.118) |
| ShareGeneral1976*1984 FE | -0.856*** |
| | (0.0804) |
| ShareGeneral1976*1985 FE | -1.045*** |
| | (0.0705) |
| Observations | 768701 |
| Kleibergen-Paap F-stat | 35.16 |

*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, *ShareGeneral*1976, 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, all measured when the individual interviewed was 16, 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 | 0.0575 | 0.0294 | -0.101** |
| | (0.337) | (0.0527) | (0.0418) |
| **IV results** | | | |
| Share Students in General Edu | 1.149 | 0.348*** | -0.435*** |
| | (0.860) | (0.0865) | (0.0663) |
| Observations | 765354 | 768701 | 768701 |
| Pre-Reform Mean | 19.32 | 0.39 | 0.22 |
| Kleibergen-Paap F-stat | 35.24 | 35.16 | 35.16 |
| Sargan test p-value | 0.581 | 0.641 | 0.537 |

*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. 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, all measured when the individual interviewed was 16, 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 | -0.00485 | -0.00185 | 0.00670 |
| | (0.0357) | (0.0257) | (0.0214) |
| **IV results** | | | |
| Share Students in General Edu | -0.0678 | 0.0624 | 0.00541 |
| | (0.0972) | (0.0724) | (0.0476) |
| Observations | 768701 | 768701 | 768701 |
| Pre-Reform Mean | 0.72 | 0.10 | 0.15 |
| Kleibergen-Paap F-stat | 35.16 | 35.16 | 35.16 |
| Sargan test p-value | 0.537 | 0.358 | 0.126 |

*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, all measured when the individual interviewed was 16, 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 | 0.0408** | -0.0126 | -0.0282 | 0.160* |
| | (0.0173) | (0.0256) | (0.0242) | (0.0914) |
| **IV results** | | | | |
| Share Students in General Edu | 0.101*** | -0.164*** | 0.0632 | 0.385** |
| | (0.0362) | (0.0512) | (0.058 | (0.181) |
| Observations | 1438774 | 1438774 | 1438774 | 1484533 |
| Pre-Reform Mean | 0.10 | 0.60 | 0.30 | 7.4 |
| Kleibergen-Paap F-stat | 36.37 | 36.37 | 36.37 | 54.13 |
| Sargan test p-value | 0.418 | 0.805 | 0.842 | 0.812 |

*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, all measured when the individual interviewed was 16, 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: **Performance in the OECD PIAAC test**

| | Literacy (1) | Numeracy (2) |
|---|---|---|
| **OLS results** | | |
| Share Students in General Edu | -0.00383 | 0.344 |
| | (1.184) | (0.957) |
| | [-2.81, 1.87] | [-1.776, 2.016] |
| **IV results** | | |
| Share Students in General Edu | 1.807* | 0.778 |
| | (0.933) | (1.005) |
| | [-.4865, 4.037] | [-2.103, 3.614] |
| Observations | 1109 | 1109 |
| Kleibergen-Paap F-stat | 65.64 | 65.64 |
| Sargan test p-value | 0.614 | 0.367 |

*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, being this the standardized score of the worst answer given in the literacy test (Column 1), or the standardized score of the worst answer given in the numeracy test (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, who participated in the 2012 PIAAC test. Each regression also includes the following cohort-region time-varying 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, all measured when the individual interviewed was 16, log cohort size, birth-region, cohort, and year fixed effects. Heteroskedasticity-robust standard errors in parenthesis, and wild-bootstrap confidence intervals 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 7: **Across-province migration**

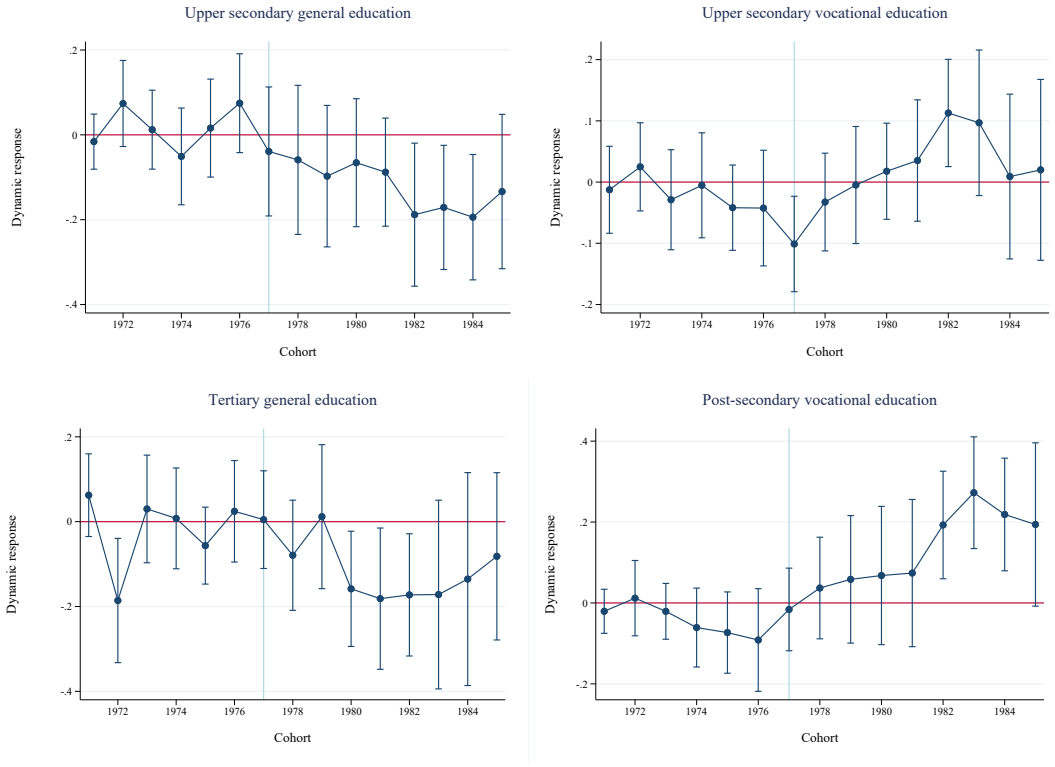
| | 16-24<br>(1) | 25 +<br>(2) |
|---|---|---|
| **OLS results** | | |
| Share Students in General Edu | 0.0105 | 0.0720* |
| | (0.0890) | (0.0373) |
| **IV results** | | |
| Share Students in General Edu | -0.0407 | 0.180* |
| | (0.0935) | (0.0989) |
| Observations | 786544 | 768701 |
| Pre-Reform Mean | 0.10 | 0.17 |
| Kleibergen-Paap F-stat | 53.91 | 35.16 |
| Sargan test p-value | 0.683 | 0.210 |

*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. 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, all measured when the individual interviewed was 16, 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.

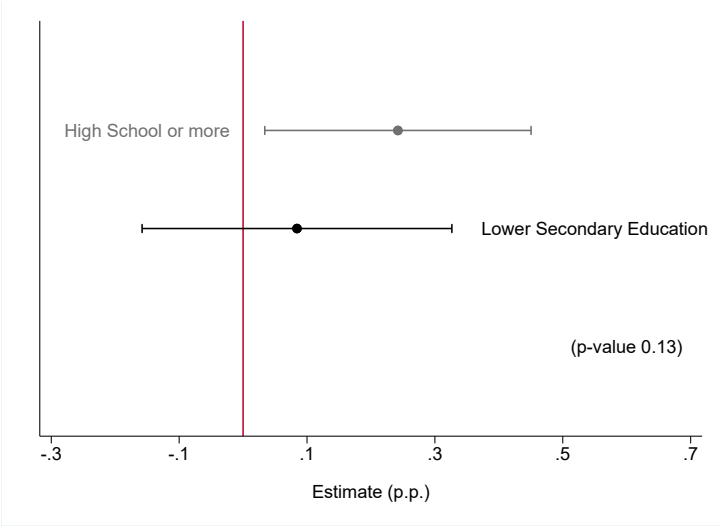
# 10 Appendix

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



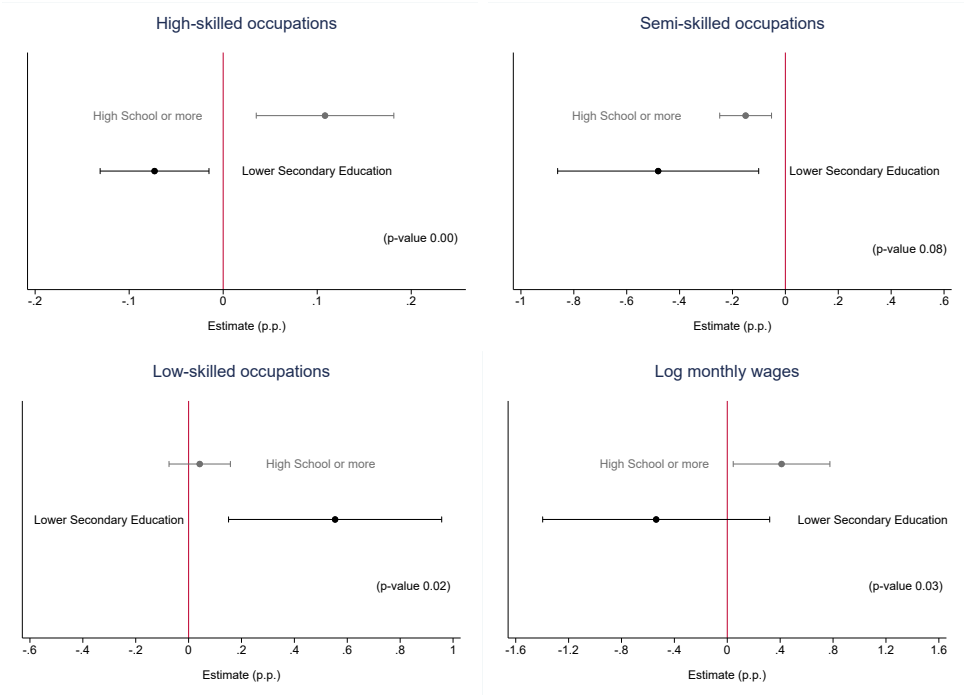

*Source:* Labor Force Survey 1995-2017, cohorts 1970-1985.
*Notes:* These graphs show the estimates of the leads and lags of the reform on educational choices, obtained from the estimation of regression 3. 95 percent confidence intervals are also reported. The outcomes considered are displayed on top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

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



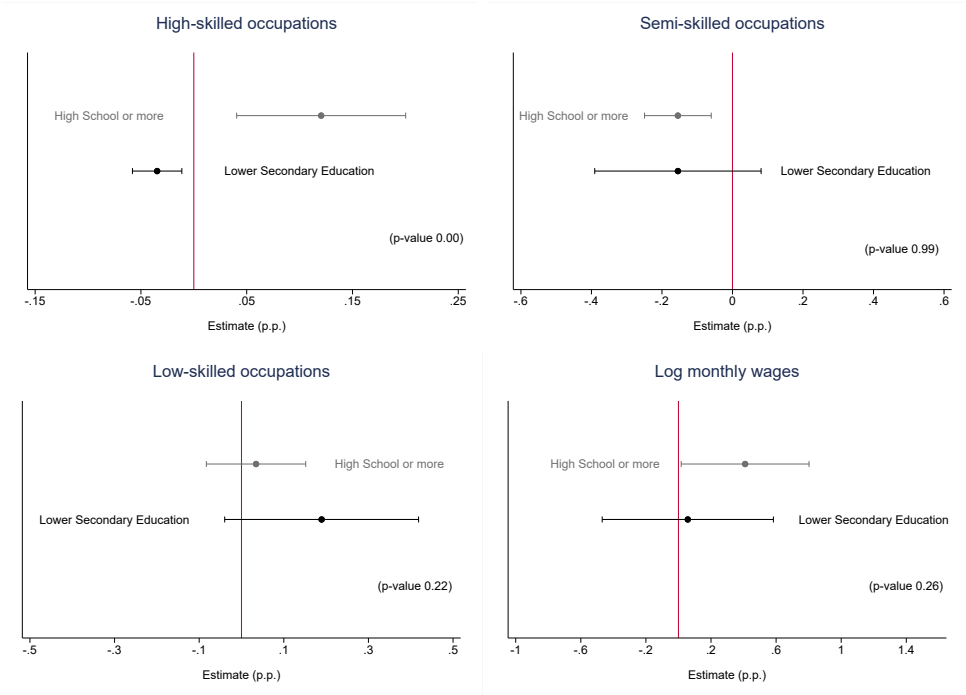

*Source:* Labor Force Survey 2002-2017, cohorts 1977-1985.
*Notes:* This figure compares the impact of the reform on the probability of across-province migration for individuals with a high-school diploma or more, and those with lower-secondary education. These results are obtained from the estimation 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, all measured when the individual interviewed was 16, 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 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, all measured when the individual interviewed was 16, 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 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, all measured when the individual interviewed was 16, 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 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 province 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) |
| Log cohort size | -0.0345 | | | | -0.0505 |
| | (0.0588) | | | | (0.0611) |
| Log GDP per capita | | -0.0904 | | | -0.0641 |
| | | (0.124) | | | (0.120) |
| Share left-wing municipalities | | | -0.0293** | | -0.0297** |
| | | | (0.0137) | | (0.0137) |
| Share students in public schools | | | | -0.0600 | -0.0779 |
| | | | | (0.0862) | (0.0810) |
| Observations | 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: **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 | 0.0639* | 0.0361 | -0.110*** | 0.0263 | 0.00985 | 0.00317 | -0.111*** |
| | (0.0377) | (0.0234) | (0.0366) | (0.0273) | (0.0284) | (0.0348) | (0.0276) |
| **IV results** | | | | | | | |
| Share Students in General Edu | 0.0714 | 0.0448 | -0.139 | 0.201*** | -0.157** | 0.147* | -0.279*** |
| | (0.0969) | (0.0700) | (0.115) | (0.0576) | (0.0617) | (0.0754) | (0.0611) |
| Observations | 768701 | 768701 | 768701 | 768701 | 768701 | 768701 | 768701 |
| Pre-Reform Mean | 0.39 | 0.22 | 0.12 | 0.10 | 0.39 | 0.27 | 0.12 |
| Kleibergen-Paap F-stat | 35.16 | 35.16 | 35.16 | 35.16 | 35.16 | 35.16 | 35.16 |
| Sargan test p-value | 0.691 | 0.277 | 0.481 | 0.779 | 0.235 | 0.683 | 0.652 |

*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, all measured when the individual interviewed was 16, 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.4: **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 | 0.0152 | 0.00511 | -0.0663** | -0.00204 | -0.0226 | -0.0150 | 0.0740*** | 0.0244 | -0.00811 |
| | (0.0116) | (0.0302) | (0.0260) | (0.0241) | (0.0338) | (0.00962) | (0.0252) | (0.0242) | (0.0193) |
| **IV results** | | | | | | | | | |
| Share Students in General Edu | 0.0751** | 0.000899 | -0.0918* | -0.0813 | 0.0889 | 0.0216 | -0.000679 | -0.00797 | -0.0352 |
| | (0.0340) | (0.0683) | (0.0538) | (0.0543) | (0.0640) | (0.0222) | (0.0562) | (0.0581) | (0.0431) |
| 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 | 33.35 | 33.35 | 33.35 | 33.35 | 33.35 | 33.35 | 33.35 | 33.35 | 33.35 |
| Sargan test p-value | 0.097 | 0.67 | 0.872 | 0.817 | 0.317 | 0.134 | 0.052 | 0.29 | 0.661 |

*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, all measured when the individual interviewed was 16, 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: **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** | 0.417 | -0.309* | -0.218 | 0.141 | -0.199 | 0.168* |
| | (0.261) | (0.159) | (0.203) | (0.115) | (0.183) | (0.0959) |
| **IV results** | 0.0442 | -0.274* | -0.0433 | 0.187 | -0.000961 | 0.0867 |
| | (0.0940) | (0.154) | (0.0561) | (0.127) | (0.0560) | (0.0779) |
| 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 | 35.71 | 40.21 | 35.71 | 40.21 | 35.71 | 40.21 |
| Sargan test p-value | 0.438 | 0.729 | 0.786 | 0.293 | 0.152 | 0.789 |

*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, all measured when the individual interviewed was 16, 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.6: **Occupational outcomes and wages by level of education**

| | 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** | 0.0476* | 0.00364 | 0.0198 | -0.0915 | -0.0674** | 0.0878 | 0.147 | 0.132 |
| | (0.0264) | (0.0104) | (0.0278) | (0.0589) | (0.0294) | (0.0567) | (0.0978) | (0.125) |
| **IV results** | 0.117*** | -0.0178 | -0.139*** | -0.168 | 0.0220 | 0.186** | 0.372* | 0.109 |
| | (0.0425) | (0.0213) | (0.0505) | (0.103) | (0.0594) | (0.0947) | (0.211) | (0.211) |
| Observations | 1111512 | 241693 | 1111512 | 241693 | 1111512 | 241693 | 1216351 | 267605 |
| Pre-Reform Mean | 0.13 | 0.01 | 0.58 | 0.65 | 0.29 | 0.34 | 7.47 | 7.24 |
| Kleibergen-Paap F-stat | 35.87 | 36.55 | 35.87 | 36.55 | 35.87 | 36.55 | 53.66 | 46.11 |
| Sargan test p-value | 0.388 | 0.713 | 0.423 | 0.426 | 0.461 | 0.407 | 0.650 | 0.633 |

*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, all measured when the individual interviewed was 16, 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.7: **Across-province migration by level of education**

| | 25 + | |
|---|---|---|
| | High-school diploma or more (1) | Lower secondary education (2) |
| **OLS results** | 0.0700* | 0.0877 |
| | (0.0381) | (0.0547) |
| **IV results** | 0.242** | 0.0843 |
| | (0.106) | (0.124) |
| Observations | 517898 | 250803 |
| Pre-Reform Mean | 0.19 | 0.14 |
| Kleibergen-Paap F-stat | 35.71 | 40.21 |
| Sargan test p-value | 0.156 | 0.533 |

*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, all measured when the individual interviewed was 16, 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.8: **First stage**

| | Share of students in general education (1) |
|---|---|
| ShareGeneral1976*1978 FE | -0.0768 |
| | (0.0684) |
| ShareGeneral1976*1979 FE | -0.281 |
| | (0.213) |
| ShareGeneral1976*1980 FE | -0.158 |
| | (0.110) |
| ShareGeneral1976*1981 FE | -0.356*** |
| | (0.126) |
| ShareGeneral1976*1982 FE | -0.593*** |
| | (0.142) |
| ShareGeneral1976*1983 FE | -0.756*** |
| | (0.126) |
| ShareGeneral1976*1984 FE | -0.864*** |
| | (0.0741) |
| ShareGeneral1976*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, $ShareGeneral1976$, and fixed effects for the cohorts affected by the reform. The regression also include birth-province and year of interview fixed effects. Heteroskedasticity-robust standard errors clustered at the province level in parenthesis.

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

Table A.9: **Educational outcomes**

| | Age at highest qualification | Type of education acquired | |
|---|---|---|---|
| | | General | Vocational |
| | (1) | (2) | (3) |
| **OLS results** | | | |
| Share Students in General Edu | 0.0838 | 0.0509 | -0.120*** |
| | (0.372) | (0.0512) | (0.0368) |
| **IV results** | | | |
| Share Students in General Edu | 0.891 | 0.331*** | -0.433*** |
| | (1.126) | (0.0937) | (0.0684) |
| 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 | 0.255 |

*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.10: **Occupational outcomes and wages**

| | Occupations | | | |
|---|---|---|---|---|
| | High-skilled | Semi-skilled | Low-skilled | Log monthly wages |
| | (1) | (2) | (3) | (4) |
| **OLS results** | | | | |
| Share Students in General Edul | 0.0344 | -0.0253 | -0.00913 | 0.141 |
| | (0.0272) | (0.0297) | (0.0227) | (0.0930) |
| **IV results** | | | | |
| Share Students in General Edu | 0.114 | -0.222*** | 0.109 | 0.303* |
| | (0.0711) | (0.0555) | (0.0666) | (0.164) |
| Observations | 1438774 | 1438774 | 1438774 | 1484533 |
| Pre-Reform Mean | 0.10 | 0.59 | 0.31 | 7.29 |
| Kleibergen-Paap F-stat | 492.3 | 492.3 | 492.3 | 457.02 |
| Sargan p-value | 0.52 | 0.77 | 0.792 | 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.11: **Performance in the OECD PIAAC test**

| | Literacy (1) | Numeracy (2) |
|---|---|---|
| **OLS results** | | |
| Share Students in General Edu | -0.345 | 0.00753 |
| | (1.333) | (1.165) |
| | [-3.011, 1.821] | [-2.376, 2.112] |
| **IV results** | | |
| Share Students in General Edu | 1.225 | 1.275 |
| | (1.540) | (1.907) |
| | [-3.555, 4.456] | [-4.458, 5.386] |
| Observations | 1109 | 1109 |
| Kleibergen-Paap F-stat | 57.57 | 57.57 |
| Sargan test p-value | 0.75 | 0.95 |

*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, being this the standardized score in the worst answer of the literacy test (Column 1), or the standardized score in the worst answer of the numeracy test (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, who participated in the 2012 PIAAC test. Each regression also includes cohort, and year fixed effects. Heteroskedasticity-robust standard errors in parenthesis, and wild-bootstrap confidence intervals 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 A.12: **Median performance in the OECD PIAAC test**

| | Literacy (1) | Numeracy (2) |
|---|---|---|
| **OLS results** | | |
| Share Students in General Edu | 0.420 | 0.236 |
| | (1.095) | (0.953) |
| | [-1.987, 1.905] | [-1.987, 1.905] |
| **IV results** | | |
| Share Students in General Edu | 1.506 | 0.306 |
| | (0.975) | (1.002) |
| | [-.8024, 3.907] | [-2.162, 2.839] |
| Observations | 1109 | 1109 |
| Kleibergen-Paap F-stat | 65.64 | 65.64 |
| Sargan test p-value | 0.68 | 0.5 |

*Source:* PIAAC 2012, cohorts 1977-1985.

*Notes:* This table reports the impact of the reform on the median performance in the OECD PIAAC test, obtained from the estimation of regression 1. Each column refers to the outcome considered, being this the standardized score in the median answer of the literacy test (Column 1), or the standardized score in the median answer of the numeracy test (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, who participated in the 2012 PIAAC test. Each regression also includes the following cohort-region time-varying 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, all measured when the individual interviewed was 16, log cohort size, birth-region, cohort, and year fixed effects. Heteroskedasticity-robust standard errors in parenthesis, and wild-bootstrap confidence intervals 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 A.13: **Best performance in the OECD PIAAC test**

| | Literacy (1) | Numeracy (2) |
|---|---|---|
| **OLS results** | | |
| Share Students in General Edu | 0.352 | 0.249 |
| | (0.924) | (0.955) |
| | [-1.927, 1.893] | [-1.945, 1.975] |
| **IV results** | | |
| Share Students in General Edu | 1.192 | 0.209 |
| | (0.922) | (1.041) |
| | [-1.391, 3.069] | [-2.489, 2.719] |
| Observations | 1109 | 1109 |
| Kleibergen-Paap F-stat | 65.64 | 65.64 |
| Sargan test p-value | 0.61 | 0.52 |

*Source:* PIAAC 2012, cohorts 1977-1985.

*Notes:* This table reports the impact of the reform on the best performance in the OECD PIAAC test, obtained from the estimation of regression 1. Each column refers to the outcome considered, being this the standardized score in the best answer of the literacy test (Column 1), or the standardized score in the best answer of the numeracy test (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, who participated in the 2012 PIAAC test. Each regression also includes the following cohort-region time-varying 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, all measured when the individual interviewed was 16, log cohort size, birth-region, cohort, and year fixed effects. Heteroskedasticity-robust standard errors in parenthesis, and wild-bootstrap confidence intervals 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 A.14: **Living in a different province**

| | 16-24<br>(1) | 25 +<br>(2) |
|---|---|---|
| **OLS results** | | |
| Share Students in General Edu | 0.0528 | 0.0886* |
| | (0.0785) | (0.0471) |
| **IV results** | | |
| Share Students in General Edu | 0.0442 | 0.205 |
| | (0.105) | (0.128) |
| Observations | 786544 | 768701 |
| Pre-Reform Mean | 0.10 | 0.17 |
| Kleibergen-Paap F-stat | 213.4 | 208.9 |
| Sargan test p-value | 0.498 | 0.337 |

*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. 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 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.

Table A.15: **Employment prospects before/after crisis - low-educated individuals**

| | Employed | | Unemployed | | Inactive | |
|---|---|---|---|---|---|---|
| | Before crisis | After crisis | Before crisis | After crisis | Before crisis | After crisis |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| **OLS results** | 0.149* | -0.0770 | -0.121 | 0.0627 | -0.0277 | 0.0143 |
| | (0.0829) | (0.0684) | (0.0798) | (0.0456) | (0.0768) | (0.0449) |
| **IV results** | 0.417 | -0.309* | -0.218 | 0.141 | -0.199 | 0.168* |
| | (0.261) | (0.159) | (0.203) | (0.115) | (0.183) | (0.0959) |
| 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 | 4.74 | 43.61 | 4.74 | 43.61 | 4.74 | 43.61 |
| Sargan test p-value | 0.333 | 0.607 | 0.27 | 0.342 | 0.69 | 0.314 |

*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, all measured when the individual interviewed was 16, 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.16: **Employment prospects before/after crisis - high-school diploma +**

| | Employed | | Unemployed | | Inactive | |
|---|---|---|---|---|---|---|
| | Before crisis | After crisis | Before crisis | After crisis | Before crisis | After crisis |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| **OLS results** | 0.0876 | 0.0510 | -0.0765*** | -0.0162 | -0.0111 | -0.0348 |
| | (0.0629) | (0.0457) | (0.0276) | (0.0260) | (0.0504) | (0.0320) |
| **IV results** | -0.247 | 0.201* | -0.110 | -0.0942 | 0.357** | -0.107 |
| | (0.168) | (0.118) | (0.123) | (0.0671) | (0.170) | (0.0741) |
| 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 | 3.9 | 43 | 3.9 | 43 | 3.9 | 43 |
| Sargan test p-value | 0.310 | 0.620 | 0.175 | 0.669 | 0.333 | 0.352 |

*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, all measured when the individual interviewed was 16, 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.