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WORKING PAPER

The scarring effect of early non-employment

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Abstract

This paper investigates whether early non-employment has a causal impact on workers' subsequent career. The analysis is based on a sample of low educated youth graduating in Belgium between 1994 and 2002. To correct for selective incidence of non-employment, we instrument early non-employment by the provincial unemployment rate at graduation. Since the instrument is clustered at the province-graduation year level and the number of clusters is small, inference is based on wild bootstrap methods. We find that one percentage point increase in the proportion of time spent in non-employment during the first two and a half years of the career decreases six years after graduation annual earnings from salaried employment by 10% and annual hours worked by 7%.

Keywords: youth unemployment, scars, instrumental variable, wild bootstrap

JEL Classification: J31, J64

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1 Introduction

High levels of youth unemployment are a great concern for policy makers, especially since the start of the Great Recession (Bell and Blanchflower, 2010). Historically, however, unemployment rates have been higher for young workers than for older workers. This is understandable, since younger workers have the least experience and hence are often the easiest to remove. Moreover, they lose or leave a job more often than older workers, because job shopping helps them to find a good match (Yedid-Levi et al., 2014). For instance, Topel and Ward (1992) find that two third of job changes and wage growth occur in the first ten years of workers' career. This initial high turnover may involve also short spells in unemployment. Therefore, youth unemployment need not to be necessarily detrimental to workers' career, if this is part of the process to find stable employment.[1] Other views predict that the experience of youth unemployment may entail long-term penalties in terms of reduced wages and persistent unemployment. These outcomes are explained by human capital loss (Pissarides, 1992), which may arise from the depreciation of existing capital - if the worker is not subject to any kind of training when not employed - as well as from forgone work experience (Ellwood, 1982). An alternative explanation comes from the signaling model, in which past unemployment records are interpreted by employers as signals of low productivity in a context of imperfect information (Lockwood, 1991).[2]

A broad evidence suggests that the consequences of experiencing youth unemployment are not just temporary. A number of studies provide evidence of persistence in youth unemployment: for UK, Gregg (2001) finds strong state dependence for new graduates experiencing early unemployment. Schmillen and Umkehrer (2013) show similar results for students graduating from the German apprenticeship program. Based on a sample of young long-term unemployed in Belgium, Cockx and Picchio (2013) find that past unemployment duration negatively affects the probability to be employed. Other studies show that youth unemployment entails long-term penalties on earnings or wages: Gartell (2009) and Gregg and Tominey (2005) find persistent negative effects on earnings and wages for Sweden and UK, respectively. Results for the US show short-lived penalties of youth unemployment on subsequent employment, but long-run penalties on wages (e.g. Ellwood, 1982; Mroz and Savage, 2006). Moreover, Mroz and Savage (2006) find strong evidence of human capital "catch-up" response to youth unemployment. By investing in training when unemployed, youth mitigate the human capital losses induced by unemployment

[1]According to the literature on job displacement, the costs of job loss for young workers are smaller and less persistent than for mature workers (e.g. Kletzer and Fairlie, 2003; Topel, 1990). This is because young workers, unlike mature workers, experience steep earnings growth in subsequent work experience, after a job loss. At the same time, earnings growth of displaced young workers is below the levels of young non-displaced workers: i.e. most of the earnings loss is due to foregone wage growth due to displacement.

[2]Kroft et al. (2013) send fictitious resumes to real job postings in US and find that the likelihood of receiving callbacks for interviews significantly decreases with the length of the unemployment spells, mostly in the first 8 months. This is evidence that employer screening plays a primary role in generating duration dependence.

and, consequently, the earnings losses, which however persist 9 years after unemployment. This evidence is important from a policy point of view, as it provides grounds to advocate policies that aim to reduce the scars of youth unemployment: for instance, integrating youth in the labor market be means of wage subsidy programs; or, providing training schemes to young unemployed.

The goal of this study is to contribute to the existing empirical evidence on the long-term consequences of early labor market performances for Flanders, the most prosperous of three Belgian regions. Our analysis is based on very rich data combining survey data with administrative data. Therefore, we are able to evaluate the impact of early labor market outcomes on a range of labor market outcomes available for the first twelve years after graduation: hours worked and earnings for salaried public and private sector employment, salaried and self-employment. This gives us a comprehensive view of the long-term consequences of youth's early labor market outcomes.

The main identification problem is the presence of unobserved factors that may affect early as well as subsequent labor market performances, thereby introducing endogeneity. We address this problem by means of an Instrumental Variable (IV) approach, where the provincial unemployment rate at graduation is used as instrument for early labor market outcomes. An advantage of this methodology is that it exploits the *exogenous* variation of the instrument to disentangle the causality between early and subsequent labor market performances from the spurious correlation induced by unobserved individual characteristics.

Throughout the analysis we distinguish between low and high educated new graduates because in Belgium labor market institutions differ for blue and white collar workers, and this creates different sources of rigidities for the low and the high educated.[3] For instance, white collars are sheltered by a very strict employment protection legislation (EPL), which represents the main rigidity for high skilled workers. In contrast, blue collar workers have quite loose EPL,[4] but are supported by a short-time work compensation program (STC) that subsidies temporary reduction of labor force during downturns. This introduces rigidity in the labor market of blue-collar workers since it allows employers to tie their employees to them and has therefore similar consequences as EPL for white collar workers. Moreover, (sectoral) minimum wages - among the highest of OECD - are likely to be binding for low educated youth. Together with a quite generous unemployment insurance (UI) system,[5] minimum wages are therefore more relevant sources of rigidity for the low educated. Based on the same data of this study, Cockx

[3]In the data there is a clear correspondence between low (high) educated and blue (white) collar workers: see Table 4 in Appendix A.

[4]Note that since the beginning of 2014 a single employment contract has been introduced in Belgium, stipulating the same EPL for white and blue collar workers.

[5]UI covers also unemployed school-graduates without employment records with no time limits and loose job search requirements. Until 2012, school graduates below age 26 were entitled to UI 9 months after registration at the Public Employment Service. Since 2012 the waiting period has been extended from 9 to 12 months.

and Ghirelli (2014) investigate the scars of graduating in downturns for Flanders and find that the low and the high educated undergo different hysteresis mechanisms, which are explained by the distinct labour market institutions designed for white and blue collars. While a downturn at graduation is found to inflict a persistent adverse effect on earnings for both low and high educated workers, the latter are penalized in terms of hourly wages and not of annual hours worked (because of strict EPL for white-collars and not binding minimum wages); the reverse holds for the low educated.

Hence, as a consequence of unfavorable labor market conditions at graduation, the high educated downgrade to lower-paying jobs whereas low educated workers remain unemployed more often. This suggests that, in Flanders, the long-term effects of graduating in downturns occur through the loss of early work experience for the latter and through the acceptance of lower-paying jobs for former. In particular, for the low educated the scar may persist since unemployment spells convey bad signals to the employers, or because of human capital depreciation which makes the unemployed less attractive to employers or, possibly, because of processes of discouragement or habituation (Clark, 2003; Clark et al., 2001). The high educated instead may find it difficult to upgrade to a position matching their initial aspirations as a consequence of the (useless) accumulation of human capital - which is specific to lower-paying position - and due to the high barriers to enter white-collar positions.

In this study we test the former hypothesis: that is, whether the low educated workers suffer from long-term penalties from experiencing early non-employment. We identify causality by means of an IV approach where the provincial unemployment rate at graduation is used as instrument for early non-employment. Unfortunately, we could not test the second aforementioned hypothesis - i.e. if, for the high educated, the average level of hourly wages earned in the first years after graduation has long-run repercussions on their subsequent career - as the instrument has not sufficient explanatory power to implement the IV approach. The interested reader can find this exercise in Appendix H. Since we consider individuals graduating in the period 1994-2002 for the low educated in the 5 Flemish provinces, inference hinges at most on 45 clusters. This raises the possibility of underestimating standard errors due to few clusters. We tackle this problem applying wild bootstrap [6] to the IV approach, following Davidson and MacKinnon (2010).

The paper is organized as follows. In the next section we briefly review the literature. Section 3 describes the data. In Section 4 we explain the estimation strategy: we discuss the IV approach, including the way in which we deal with the problem of inference with few clusters. Section 5 discusses the results for the low educated and presents some sensitivity analysis. Section 6 concludes.

[6] Wild bootstrap preserves the group structure of the data (Cameron et al., 2008).

2 Literature Review

To our knowledge, two studies have investigated the long-term impacts of unemployment for Belgium. Gangji and Plasman (2007) study the adverse effect of the incidence of unemployment on re-entry wages considering a representative sample of Belgian workers aged 18-64 in the 1994-2002 period. They address the problem of endogeneity due to unobserved individual characteristics with a fixed effect (FE) estimation, thereby exploiting only the within-individual variation of the data.[7] They find that the incidence of unemployment is associated with a 5.1% penalty in hourly wages. Of course, this is an average of heterogenous effects comprising workers with different ages and skills. In Belgium, as argued in the Introduction, low and high skilled workers may undergo different wage penalties due to the different labor market institutions. Moreover, the incidence of unemployment for youth is likely to have different consequences than for mature workers.

The second study by Cockx and Picchio (2013) is focused on the long-run effects of youth long-term unemployment. It considers all Belgian school-graduates aged 18-25 who in 1998 were registered to the National Employment Office and remained unemployed for at least 9 months,[8] and follow them for each quarter until the end of 2002. They use a mixed proportional hazard model with competing risks where omitted heterogeneity is tackled as follows. The unobserved individual characteristics that are uncorrelated with the regressors are integrated out of the model. As for the unobserved characteristics that are correlated with the regressors, they impose a functional form restriction on the relationship between the former and the latter, thereby allowing for the dependence.[9] The causality of lagged endogenous unemployment duration, the parameter of interest, is identified by the exclusion restrictions generated by the time variation of the exogenous variables and by having multiple realizations per individual of the outcome variable. They find evidence of strong negative duration dependence in the job finding probability: further prolonging the unemployment spell by one year reduces the probability to find a job in the following 2 years from 60% to 16% for men. Of course these results apply to the specific sub-sample of long-term unemployed youth.

We contribute to this literature by providing new evidence on the long-term effects of early non-employment for low educated youth by means of IV approach, where the identification of the causality between early non-employment and subsequent labor market outcomes comes from the exogenous variation of the provincial unemployment rate at graduation. Note that our IV method shares some similarities with the aforementioned approaches - in the way they deal

[7]The sample is restricted to workers whose wage is observed at least twice in the period. Selectivity is addressed by Heckman's two-step estimator, with the number of children and house property as exclusion restrictions.

[8]In Belgium, before 2012 school-graduates were eligible to UI benefits if still unemployed 9 months after the registration to the National Employment Service.

[9]As a consequence, regressors' coefficients cannot be given a structural interpretation as cannot be separated out from the unobservables.

with the presence of omitted individual factors correlated with the variable of interest. The FE approach reaches consistency exploiting only the time-variation of the data, i.e. the variation within- and not across-individuals. Hence, the identification comes from the part of the variation that is uncorrelated with the (fixed) unobservables. The IV identification strategy is similar, as it exploits the (exogenous) variation of the instrument that is correlated with the endogenous regressor. Similarly, in mixed proportional hazard models, the time variation of the exogenous time-varying variables generate exclusion restrictions that are used for the identification. In sum, the difference between the methods implemented in the existing literature and our approach boils down to the type of *exogenous* variation that is exploited to identify causality.

For other countries, some studies have already applied IV to identify the long-term effects of youth unemployment exploiting the variation in the local unemployment rate before the first entry in the labor market.[10] Based on UK data, Gregg (2001) and Gregg and Tominey (2005) study the effect of youth unemployment on subsequent unemployment spells and wages, respectively: in both cases, the local unemployment rate at age 16 (the end of compulsory education) is used as instrument for early unemployment. The latter is defined as the cumulative months of unemployment or non-employment in the period of age 16-23, while the dependent variables are measured at age 33 and 42. The sample is restricted to students born in 1958 graduating up to age 21. The results show strong adverse effects of early unemployment on later wages and employment. For Germany, Schmillen and Umkehrer (2013) focus on a sample of students graduating from the apprenticeship program between 1978 and 1980 and investigate the relationship between the cumulative days spent in unemployment in the first 8 years after graduation and the total days spent in unemployment in the subsequent 16 years. The regressor of interest is instrumented by the local unemployment rate at graduation. They find significant and long-lasting scarring effects, especially at the right tail of the unemployment distribution.

In a nutshell, the estimation strategy shared by these studies is based on the idea that the variation in the labor market conditions at school leaving is exogenous to the individual, and therefore generates a variation in the individual early unemployment that is unrelated to unobserved factors that may influence both early and adult performances. In particular, their identification relies on the exclusion restriction that the labor market conditions at graduation affect the dependent variables uniquely through early unemployment. Therefore, the underlying identifying assumption is that the scarring effect of graduating in downturns occurs entirely through the reduced early work experience. For Flemish low educated, Cockx and Ghirelli (2014) provide evidence of this pattern. However, the aforementioned exclusion restriction also rules out the possibility that this scar arises through the acceptance of lower-paying wages. This is precisely how the scar takes place for Flemish high educated (Cockx and Ghirelli, 2014). The

[10] A related study is Neumark (2002), who identifies the causality of early job stability on adult wages with IV, where indicators of job stability in the first 5 years since graduation are instrumented by labor market conditions faced in this early period.

reliability of this exclusion restriction is not discussed in this literature. In contrast, in this study we adapt the estimation strategy in such a way that the exclusion restriction is most likely valid in light of the aforementioned evidence: this means using for the low educated a measure of early non-employment as endogenous variable.[11]

3 Data

The analysis is based on the Sonar survey database, a representative sample of three birth-cohorts of Flemish youth - born in 1976, 1978 or 1980, which were interviewed at age 23, 26 and 29.[12] The surveys register retrospectively and on monthly basis the most important activity of the respondents, among which education. Based on this information, graduation is identified to occur in the first month that education has been interrupted for at least for 4 months. The surveys also contain control variables for the analysis, which are measured at age 17, such that they are predetermined at graduation: father's and mother's education (years of completed education since age 12), the type of educational program (general, technical, vocational, part-time vocational or apprenticeship) in which the individual is enrolled at age 17, and the number of repeated grades at age 17 since secondary education. From this database we calculate for each individual the number of completed years of education, i.e. the number of grades successfully passed from the start of secondary education until graduation. Based on the latter we divide the sample in low and high educated, with the former having completed at most secondary education and the latter having a higher level of education.[13] We make this distinction throughout the analysis, because, as mentioned in the Introduction, the minimum wage is more likely to be binding for low educated youth at the start of the career and because EPL-strictness varies significantly between white and blue collar workers.

The original Sonar sample contains about 9000 individuals, 3000 for each birth cohort. We restrict it as follows, to increase the homogeneity of the sample. We exclude few observations (0.17% of the sample) who dropped out from schooling before the end of compulsory education, which in Belgium is set at age 18. We focus on men since female labor supply is different from male labor supply due to mothering.[14] We drop individuals who attended special needs and arts education, who were not Belgian or did not speak Flemish at home, or who did not reside in Flanders at graduation. We retained individuals graduating from age 18 and 24, as students graduating with more than 24 years old are less than 5% of the sample. After eliminating, in addition, individuals with missing or inconsistent values in variables, we are left with a final

[11] For the high educated we use a measure of early wage as endogenous regressor. Since the instrument is weak in the IV approach, results are relegated to Appendix H. Hereafter we will focus on the low educated.

[12] For more details, see Sonar (2004a, 2003, 2004b).

[13] Low educated are those with at most 6 years of completed education (7 years if enrolled in vocational track at age 17). High educated are those with higher years of completed education.

[14] Long-term effects of early labor market outcomes for women are equally interesting and left for future research.

sample of 3586 low and high educated men. From this, we focus on 1902 low educated, who graduated in the period 1994-2002. Descriptive statistics of the final sample is in Appendix A.

The survey data are matched to administrative data of Belgian Social Insurance institutions centralized at the Cross Roads Bank of Social Security, which give us access to high quality information on individuals' labor market outcomes for a sufficiently long time span after graduation. In particular, these data report quarterly information on the registration as self-employed, as well as earnings and time worked in dependent employment (for both public and private sector), between year 1998 and 2010. For salaried workers we construct log annual earnings and log annual hours worked. The log-transformation allows us to interpret the coefficients of interest as semi-elasticities. Note that we retain in the analysis also non-salaried employed by adding value one to the continuous variables before taking the logarithmic transformation. Hence, non-salaried employed have zero log-earnings and zero log-hours worked. As a consequence our estimates on continuous outcomes are unconditional on being salaried employed. This rules out the problem of selectivity due to restricting to the salaried employed, i.e. a potentially positively selected group. Yet it introduces another complication since the distribution of the outcome variables has a mass point at zero. The fraction of corner solutions is however quite small, as only 15% of the low educated are censored at zero at the moment of the evaluation. As a consequence, we will rely on OLS.[15] In addition, we construct three employment indicators: salaried employment, defined by positive earnings from salaried employment; self-employment, based on the registration as self-employed for at least one day during the calendar year; overall employment, which is the sum of self- and salaried employment. Note that salaried employed who are also registered as self-employed in the same calendar year are considered self-employed. These outcomes are measured 6 or 8 years after graduation. This choice is due to the availability of the administrative data (1998-2010) and by the fact that we want to measure the dependent variables as late as possible for all graduation cohorts. Since the low educated graduate in the period 1994-2002, the last graduation cohort is followed until potential experience 8. Later than that the sample gets smaller as the last graduation cohorts progressively drop out from the sample. At the same time, we want to get an idea of the persistence of the scar: hence we choose to evaluate the outcomes also two years earlier, i.e. at potential experience 6. Table 7 in Appendix A shows descriptive statistics of the outcome variables.

The administrative data also provide additional control variables measured at age 17: living in single parent household, not living together with either parents and the number of other household members by age class. Descriptive statistics of the control variables are reported in Table 6 of Appendix A.[16] Finally, the administrative data give us access to yearly information

[15]OLS estimation provides approximations of the unconditional effects, as it does not take into account the corner solutions at zero. In principle, Tobit models would be more appropriate.

[16]For details on the construction of the control and outcome variables, see Section S.1 of the Supplementary Online Appendix of Cockx and Ghirelli (2014): http://users.ugent.be/~bcockx/Ascars.pdf.

on the province of residence between the year in which individuals turn age 18 and 2010.

From the year of graduation onwards, we associate each calendar year to a *potential* year of labor market experience,[17] which corresponds to zero in the year of graduation. Potential experience 0 lasts from the month subsequent to graduation until December of that calendar year. Therefore, its length (measured in months) is computed as $12 - month\_of\_graduation$, i.e. it depends on the month of graduation: for a June graduate - which amounts to 90% of the sample - it lasts 6 months. All subsequent years of potential experience have a duration of 12 months. Our regressor of interest is a measure of the time spent in non-employment at potential experience 0-2, relative to the potential total hours if one would work full-time during the whole period. We express it as a proportion in order to take into account the fact that the reference period changes depending on the month of graduation, thereby ensuring that early non-employment is comparable across individuals. On average this period corresponds to 2.5 years after graduation (30 months), as 90% of the sample graduates in June. For simplicity hereafter we will refer to this reference period as to its average, i.e. 2.5 years after graduation.

This endogenous regressor can be measured precisely, by exploiting the administrative data on hours worked in salaried employment. However, the latter are available only since 1998, while students graduate since 1994 in our sample. Therefore, we base this variable on the administrative data for the individuals graduating since 1998 (68.5% of the sample), and exploit information from Sonar (survey data) whenever potential experience 0-2 occurs before 1998 (31.5% of sample). The reason why we combine administrative with survey data is to maximize the sample of study, thereby exploiting the variation of the instrument for the entire graduation period 1994-2002, rather than for the restricted period 1998-2002. Of course, the disadvantage is that the data on time worked in the Sonar database are less precise and hence we have to make some assumptions to convert this information into hours worked:[18] this certainly introduces measurement error in the endogenous regressor.

Briefly, the endogenous regressor is constructed as follows (for details, see Appendix B): first, define the reference period as the entire calendar year for potential experience 1 and 2, and the part of calendar year following the month of graduation for potential experience 0; sum up all hours worked including self-employment in the reference period (a);[19] compute the potential total hours if one would work full-time during the whole period (b); express early non-employment as $100 * (b - a)/b$. As already mentioned, the denominator takes into account the fact that the reference period changes depending on the month of graduation and ensures comparability across individuals. This variable measures the intensity of early non-employment. It is equal to zero if in the reference period one has always worked as much as a full-time

[17] This terminology is borrowed from the literature. “Potential” underlines that the variable counts all calendar years since graduation, as opposed to *actual* experience which endogenously considers only years in employment.

[18] These are imputed from monthly employment indicators assuming full-time employment (see Appendix B).

[19] For self-employed we assume the working regime of full-time salaried employed (see Appendix B).

salaried employed, and above zero if one has worked less intensively than the full-time regime or if one has not worked for some time. Given the possibility of measurement error arising from the combination of survey and administrative data, Section 5.1 performs a sensitivity analysis for the restricted graduation period 1998-2002, where only administrative data are exploited to measure the endogenous regressor.

A final source of information is the Labor Force Survey (LFS), which provides long time series of the provincial unemployment rates for Flanders. In our analysis, we use the provincial unemployment rate (15-64) at graduation as instrument for early non-employment. Figure 2 in Appendix C plots this series from year 1993 until 2011. Note that the literature typically exploits more disaggregated unemployment rate series.[20] For Belgium, provincial unemployment rates are the most disaggregated data available for the period considered. The main drawback is that the inference relies on too few clusters, as the identification of the effects of interest comes from the variation of the unemployment rates by provinces and years. To the extent that we tackle this problem with wild bootstrap (see Section 4.4), provincial data are not much of a limitation. In contrast, more aggregated series provide the advantage of reducing the problem of endogenous migration, that would arise if new graduates offset the long-term effects of early non-employment by moving or commuting into provinces where there are more job opportunities. Our data suggest that in Flanders less than 2% of individuals change province of residence in the 1998-2010 period. However, as Flanders is a relatively small region, people could commute to work across provinces. In this case, we would underestimate long-term effects of early non-employment.[21] However, the magnitude of the inter-provincial variation in the unemployment rate reported in Figure 2 demonstrates that mobility and commuting are limited and far from eliminates all inter-provincial variation.[22]

4 Estimation Strategy

We are interested in the causal relationship of early non-employment, say Y^0, on subsequent labor market outcomes of interest Y for the low educated. Namely, we want to estimate an equation of the following type, where X is a vector of control variables that will be defined below and ϵ is an idiosyncratic error term:

$$Y = aY^0 + bX + e \text{ with } e = (\theta + \epsilon) \tag{1}$$

[20] At district level (Schmillen and Umkehrer, 2013) and by wards (Gregg, 2001; Gregg and Tominey, 2005).

[21] If anything, this should be more worrying for the high educated, since they are (1) less liquidity constrained because of high expected wages or better working conditions and (2) more mobile due to higher motivation to find jobs that meet their expectations about wages/job profiles.

[22] This is because LFS series are based on the province of residence and not of job location. Hence if workers commute to avoid the adverse local labor market conditions, this evens out the provincial variation in the unemployment rate.

The main identification problem is the presence of some factors θ, unobserved to the researcher, that may affect both early non-employment and subsequent labor market performances, thereby introducing endogeneity. Therefore, OLS estimates will be biased as a consequence of these omitted factors. We remove this bias by means of a two-stage least squares (2SLS) estimator, where the provincial unemployment rate at graduation is used as instrument (Z) for early non-employment.[23] In practice, the identification strategy relies on the variation of the provincial unemployment rate at graduation Z, which is exploited to generate an *exogenous* variation in early non-employment Y^0, which is then used to identify the causal relation of interest. In accordance with the traditional IV approach, we assume that the effect of interest is homogeneous.[24] In this framework, the 2SLS identifies the causal effect of interest under two conditions:

1. Z is uncorrelated with e. This implies that Z does not *directly* affect the outcome Y (exogeneity), and that any *indirect* effect of Z on Y occurs uniquely through the endogenous regressor Y^0 (exclusion restriction). This is an identifying assumption.

2. Z is correlated with Y^0, conditional on the controls X in (1) (strength). This condition can be tested by means of the F statistic of the excluded instrument in the first stage regression.

Note that in this framework the IV estimate refers to the entire population since the causal effect of interest is assumed to be homogeneous across individuals. Next section discusses in detail the identifying assumption 1. In particular, we will carefully examine which factors may violate the exclusion restriction and define the specification in such a way that the latter is most likely satisfied conditional on the covariates.

4.1 The Instrumental Variable Approach: identifying assumptions

Together, Condition 1 and 2 require that the instrument explains the endogenous regressor, but that at the same time it is exogenous in model (1). This has the following implications.

First, it amounts to rule out reverse causality between Z and Y^0, that is Z affects Y^0 but not the other way around. If it were the case, Z would be endogenous in (1) because of the correlation with Y^0, and as consequence it should be included as additional regressor in the specification. We exclude the possibility of reverse causality since the instrument and the endogenous regressor are measured at the provincial and individual level respectively, and an aggregate variable cannot be caused by an individual variable.

[23] A similar approach has been used by Neumark (2002), Schmillen and Umkehrer (2013), Gregg (2001) and Gregg and Tominey (2005).

[24] This is of course a restrictive assumption. In section 5.2 we discuss the interpretation of IV under heterogeneous effects, where the IV estimator identifies a weighted average of local average treatment effects (LATEs) under additional assumptions: Stable Unit Treatment Value Assumption (SUTVA) and Monotonicity.

Second, the exogeneity assumption requires that the unemployment rate Z cannot affect the unobserved composition of new graduates by year and province. If this were the case, the relation between the instrument and early non-employment would spuriously reflect changes in the composition of graduates rather than causality, which would introduce selectivity. To rule this out, one needs to assume that students choose the moment of graduation independently of the business cycle (exogeneity of timing of graduation), and that before graduation they do not move to provinces where the unemployment rate is lower relatively to others (exogeneity of place). We test the former condition in Section S.5 of the Supplementary Online Appendix of Cockx and Ghirelli (2014), and demonstrate that the duration between the end of compulsory education at the age of 18 and each year of potential graduation is unrelated to the provincial unemployment rate in those years.[25] As for mobility, almost nobody (0.44%) change residence between the first year that our data inform about the place of living, i.e. on December 31 of the year in which the individual turns 17, and the year of graduation. Therefore, the issue can be safely ignored. On this basis, we argue that in our sample the choice of graduation is independent from the labor market conditions. However, we cannot rule out endogeneity due to commuting, for instance if students enrolled into universities located in provinces where they expected to find more jobs in the future.[26]

Third, the exclusion restriction requires that the instrument is not correlated with any of the omitted factors in model (1). If this holds, one can assume that the scars of graduating in downturns for the low educated are determined *exclusively* by the proportion of time spent in early non-employment. Accordingly, the persistence of these penalties should be rationalized entirely by the loss of human capital originated by early non-employment, or by the fact that the latter is perceived as a signal of bad quality by employers (see Introduction). This assumption is consistent with the evidence of Cockx and Ghirelli (2014), who found that adverse labor market conditions at graduation inflict to the low educated big initial penalties on earnings and hours worked, which fade away slowly. The absence of a similar impact on wages is due to the presence of minimum wages, which are likely to be binding for the low educated at the start of the career. Instead, the scars on earnings and hours worked are explained by the labor market rigidities that prevents workers reallocation: the short-term work compensation program that in hard times ties the employers to the employees, and the EPL (flexible for blue collars while rigid for white collars) which pushes the high educated to downgrade and hence increases the competition for low skilled positions. As a consequence, the low educated who graduate in downturns are

[25]In a discrete duration model, an indicator of graduating since age 17 is regressed on birth cohort dummies, individual characteristics and the province of living measured at age 17, the elapsed duration in education since age 17, and the unemployment rate in each potential year of graduation (interacted with the elapsed duration), testing whether the coefficients of latter interactions are jointly significantly different from zero. The test deals with selectivity induced by unobserved heterogeneity. It uses the same sample as this study.

[26]We do not have information on the location of universities in which students graduate nor of subsequent jobs.

rather likely to experience longer periods of non-unemployment at the start of the career. This has repercussions in the long-term, according to the aforementioned evidence.[27] Based on the latter, we argue that early non-employment is the relevant channel to explain long-term effects of adverse labor market conditions at graduation for the low educated.

Of course things can be a bit more blurry if we consider a wider definition of reservation wage which incorporates also the future wage growth linked with seniority in addition to the current wage. In this case, low educated graduating in a downturn may not only experience higher early non-employment, but could also accept lower-quality jobs, i.e. with less steep wage profile than the jobs accessed during a tight labor market. The unemployment rate at graduation would entail a growing negative impact on subsequent wages as a consequence of accepting this initial job, and this would represent a violation of the exclusion restriction when wages are the outcome of interest. This possibility is discussed in Cockx and Ghirelli (2014), where the unemployment rate at graduation has a negative impact on wages starting from potential experience 6.[28] This evidence is compatible with the aforementioned hypothesis and hence represents a violation of the exclusion restriction when wages are evaluated. Thus, we restrict the analysis on hours worked and earnings, since the long-term penalties on the these outcomes are compatible with the idea that early non-employment is the main driver of the scars.

Moreover, other channels may as well contribute to explain the long-term penalties of labor market conditions at graduation: these channels would invalidate the exclusion restriction if not included in the specification. An example is the persistence of the unemployment rate series. If the current unemployment rate affects the outcomes, the correlation between the unemployment rate at graduation and the current unemployment rate violates the assumption that the instrument affects the outcomes only through early non-employment. To prevent that, it is important to additionally control for the current unemployment rate, as typically done in the literature. However, this may not be sufficient, as in principle one should control for all unemployment rate series up to the moment of evaluation (Oreopoulos et al., 2012, 2008). To keep a parsimonious specification, we add the average unemployment rate between the end of the early period and the moment of evaluation - between potential experience 3 and 6.[29]

More generally, the problem of the persistence of the unemployment rates refers to the literature on wage determination, which investigates how the sequence of labor market conditions experienced by a worker affects current wages (Beaudry and DiNardo, 1991). According to this view, labor markets operate as spot markets if current wages are affected by current unemployment rates and not by past ones. In contrast, wages result from long-term implicit contracts

[27]Note that this scar may be nuanced by the extensive use of STC for blue collars: they will experience long periods of unemployment or reduced activity, but they are more likely to be called back.

[28]In the benchmark model this growing negative impact is not statistically significant, but it is significant in the sensitivity analysis (see Table S.20 of the Supplementary Online Appendix of Cockx and Ghirelli, 2014).

[29]To rule out multicollineariy, we run a sensitivity analysis only including current unemployment rate (see Table 12 in Appendix F). The stability of the results ensures that multicollinearity is not driving the results.

if past unemployment rates explain current wages despite current ones: with costless mobility, the minimum unemployment rate since hiring should matter the most, as workers are able to renegotiate the wage once better labor market conditions arise; if instead mobility is costly, the unemployment rate at hiring should be the relevant one. Beaudry and DiNardo (1991) found that, once the minimum unemployment rate since hiring is included together with the unemployment rate at hiring, the former but not the latter significantly explains current wages. This is consistent with the idea that wages are negotiated according to long-term implicit contracts with renewals.[30] In this case, the exclusion restriction may be violated if the unemployment rate at graduation mistakenly picks up the effect of the minimum unemployment rate since hiring, because of the persistence of the unemployment rates. To prevent that, we include the minimum unemployment rate since graduation in the specification.[31]

Finally, other violations of the exclusion restriction may be due to, for instance, differences in institutions that could be correlated both with the unemployment rate at graduation and with the outcomes. We therefore include province fixed effects to ensure that permanent differences across provinces violate the exclusion restriction. Similarly, we include province-specific time trend to capture whatever time-varying provincial heterogeneity, such as changes in legislations, that may be correlated with the instrument and the labor market outcomes at the moment of evaluation. Next section presents the equation of interest in light of all these arguments.

4.2 The Equation of Interest

To avoid clutter, we state the following definitions: t is the observation period, which runs from graduation until the moment of evaluation of the outcomes of interest T, i.e. 6 or 8 years after graduation; t_0 is the time of measurement of predetermined individual controls, which corresponds to the year in which individuals are aged 17; t_1 is the time window in which we measure early non-employment, i.e. on average the first 2.5 years after graduation[32]. We estimate the following equation, where subscript i indicates the individual, g the graduation year and p the province of residence at graduation:

$$\begin{aligned} &y_{igpT} = \alpha + \beta y^0_{igpt_1} + \gamma_1 UR_{pT} + \gamma_2 \overline{UR}_p + x'_{it_0}\delta + \zeta minUR_{pt} + \eta_p + \omega_p T + f(g) + e_{igpT} \\ &\text{with } e_{igpT} = \theta_i + \epsilon_{ipgT} \end{aligned} \tag{2}$$

[30] Recently Hagedorn and Manovskii (2013) criticize this interpretation. They argue that wages are still determined by spot markets and not by long-term implicit contracts. They show that, once the current match quality is taken into account, past labor market conditions no longer play a role in the wage determination.

[31] Note that the hypothesis of long-term implicit contracts seems more likely for the high educated, for instance to capture returns in human capital accumulation. In contrast, recent evidence has shown that labor markets operate like spot markets for the low educated (Kilponen and Santavirta, 2010; Devereux and Hart, 2007).

[32] This time window corresponds to potential experience 0-2, i.e. from the month after graduation until December of the second subsequent calendar year.

- y_{igpT} represents the following outcomes of interest measured in T, i.e. 6 or 8 years after graduation: three indicators of salaried, self- and overall employment, as well as log hours worked and log earnings in salaried employment. Before taking the logarithm of continuous variables we add value one, so that non-salaried employed at the moment of evaluations are included with value of zero after the logarithmic transformation. Therefore, the effects on continuous outcomes are unconditional on being salaried employed. The reason why do this is twofold: first, the instrument is not strong enough to estimate effects *conditional* on salaried employment, but it becomes relevant when non-salaried employed are also included in the sample.[33] Second, unconditional effects refer to the entire population of workers and avoid the problem of selectivity when focusing on the sub-population of salaried employed. We take the log of continuous outcomes to interpret the estimates as semi-elasticities.

- $y^0_{igpt_1}$ is the endogenous regressor representing early non-employment: it is expressed as the percentage of time spent in non-employment in period t_1, relative to potential total hours if one would work full-time during the whole period.

- UR_{pT} is the current unemployment rate in the province of graduation, i.e. measured at the moment of evaluation T. It ensures that the exclusion restriction is not violated by the correlation between current local labor market conditions and local labor market conditions at graduation.

- $\overline{UR}_p$ is the time average of the unemployment rate in the period subsequent to the measurement of early non-employment, i.e. from potential experience 3 to 6. Together with UR_{pT} , it controls for the persistence of the unemployment rate series.

- $minUR_{pt}$ is the minimum unemployment rate in the province of residence of graduation over the entire period t. It controls for the possibility that wages are determined by long-term contracts which are renegotiated by the workers during upturns. Under this assumption, the persistence of the unemployment rate series (i.e. the correlation between $minUR_{pt}$ and the instrument) and the correlation between the outcomes and the minimum unemployment rate could violate the exclusion restriction.

- x_i is a set of individual control variables, predetermined since measured in t_0: birth cohort dummies, family composition, parental education, repeated years since secondary education as well as the educational track at age 17.

- η_p is fixed effects for the province of living at graduation: it controls for time-fixed provincial heterogeneity, that is all differences across provinces that are constant over time: e.g. differences in institutions, or in the structure of the economy.

[33] In the first stage, the F statistic is about 4 in the conditional case and reaches 9 in the unconditional one.

- $\omega_p T$ is the provincial specific linear time trends, which are included because the unemployment rates exhibit differential downward time trends (see Figure 2 in Appendix C). More generally, it controls for any time-varying provincial heterogeneity, for instance for changes in legislations or in the structure of the economy at the provincial level.

- $f(g)$ is a linear spline in the graduation year, which controls for aggregate shocks affecting all provinces over the graduation period. We impose a piece-wise linear specification because graduation year fixed effects absorb too much variation and as a consequence the instrument becomes weaker in the first stage. The spline is formulated as $f(g) = \alpha + \sum_{j=0}^{2} \beta_j.(g - 3j)\mathbf{1}[g \geq 3j]$ with $g = 1, .., 9$.

- ϵ_{igpT} is an i.i.d. error term, while θ_i represents unobserved individual factors correlated with $y^0_{it_1}$, thereby introducing endogeneity.

β is the coefficient of interest which represents the effect of one percentage point increase in the proportion of time spent in early non-employment on subsequent outcomes of interest (employment rates, hours worked and earnings) for the low educated: in presence of scarring we expect a negative β. The OLS estimate of β is biased due to the correlation between θ_i and $y^0_{igpt_1}$.

For all dependent variables, we estimate (2) by OLS or 2SLS. Thus, we estimate linear probability models for discrete labor market outcomes. For continuous variables we report unconditional OLS effects and hence do not take into account that these outcomes are left-censored at zero. However, we believe that this is a minor issue, since the fraction of corner solutions in the sample is quite small: only 15% of the low educated are not salaried employed at potential experience 6.[34] In any case, we provide heteroscedastic-robust standard errors in all estimations to account for the fact that the dependent variables are dichotomous or censored at zero.

4.3 The Bias and Its Direction

The aforementioned bias can go in both directions. To see this, consider a simplified version of (2): $y = \beta y^0 + \theta w + \epsilon$, where y^0 is individual early non-employment, y is an individual subsequent outcome of interest (i.e. employment rates, hours worked, and earnings) w is a fixed individual characteristic. By assumption $E(y^0 \epsilon) = 0$ and $E(w\epsilon) = 0$, and we expect a negative β. Let $Cov(y^0, w) \neq 0$. If w is observed, the OLS give unbiased estimators of β and θ: $\beta_{OLS} = \frac{Cov(y^0,y)}{Var(y^0)}$ and $\theta_{OLS} = \frac{Cov(w,y)}{Var(w)}$. If instead w is omitted, the OLS estimator is biased since $\beta_{OLS} = \frac{E(y^0,y)}{E(y^0)^2} = \frac{E[y^0(\beta y^0 + \theta w + \epsilon)]}{E(y^0)^2} = \beta + \theta \frac{Cov(y^0,w)}{Var(y^0)}$. The direction of the bias depends on the sign of θ - the relationship between early non-employment and the outcome of interest -

[34] In Schmillen and Umkehrer (2013) the dependent variable, the number of days spent in unemployment in prime age, is censored at zero for almost 60% of cases. They use Tobit models.

and the correlation between the former and the omitted factor. Below we discuss four possible sources of bias and their corresponding sign.

- Ability and Motivation: Everything else equal, more able and motivated individuals are more likely to perform well in the labor market at any point in time: therefore these factors are negatively correlated with early non-employment and positively correlated with the outcomes of interest. The overall bias is negative, so that OLS overestimate the (negative) scarring effect of early non-employment.

- Returns to job search: heterogeneous returns may arise because of differences in the search intensity or in the methods of search chosen. Ceteris paribus, individuals with higher returns search more and more successfully (also on-the-job), and therefore perform better in the labor market. Hence, the outcomes of interest are positively correlated with returns to search. At the same time, in the first phase of job shopping they may alternate jobs with short spells in non-employment, if they find it optimal to consume leisure when young and their opportunity cost is lower.[35] This may generate a positive correlation between returns to job search and early non-employment (Neumark, 2002). Under these assumptions, the bias is positive and OLS underestimate scarring.

- Liquidity constraints: everything else equal, individuals with high liquidity constraints have low reservation wages because they need to earn a salary. Thus, we expect liquidity constraints to be negatively correlated with early non-employment. At the same time, these individuals are likely to accept low quality jobs because of their low reservation wage, which is likely to translate into worse labor market performances over time. We therefore expect also a negative correlation with the outcomes of interest. The resulting bias is positive so that OLS underestimate scarring.

- Measurement error: as explained in Section 3, we introduce measurement error in the construction of early non-employment, because we use information from the Sonar database to impute hours worked in the first 2.5 years since graduation for students graduating before 1998, which are not observed in the administrative data. Measurement error in the endogenous regressor reduces OLS estimates towards zero (Hausman, 2001), thereby underestimating the scarring effect of early non-employment.[36]

To recapitulate, we expect OLS to overestimate the negative effect of early non-employment on the outcomes of interest, if the bias comes from ability. In contrast, the OLS estimate will be overestimated if the bias is due to returns to job search, measurement errors in the endogenous

[35] Neumark (2002) justify this assumption as follows: in a standard life cycle utility-maximization model individuals are more likely to consume leisure at the point in the life cycle when their wages are low.

[36] In linear models, OLS estimates of y^0 are underestimated due to measurement errors and the bias can be eliminated with IV. However, this does not holds for non-linear models (Amemiya, 1985; Hausman et al., 1995).

regressor or liquidity constraints. The literature is in favour of the latter hypothesis (Neumark, 2002; Schmillen and Umkehrer, 2013; Gregg and Tominey, 2005; Gregg, 2001).

4.4 Inference

It is well known that standard errors are underestimated in a micro-level regression with grouped covariates because it is assumed that each observation is independent of all others while the information of the grouped covariates is repeated within each cluster. Therefore, correct inference requires to take into account that the independent information of the grouped covariates is at the group level, which can be done with cluster-robust standard errors (Moulton, 1990; Angrist and Pischke, 2008, ch.8). In our 2SLS we use a grouped variable, the unemployment rate at graduation, as instrument for the endogenous regressor, early non-employment, which varies at the individual level. Therefore, the identification of causality comes from the variation by province and time of the provincial unemployment rates at graduation, which is exploited to construct the fitted values of the first stage.[37]

The clustered estimator is consistent provided that the number of clusters is large enough, as consistency is determined by the law of large numbers. This is because, given the grouped structure of the data, the relevant unit are clusters and not observations. Since we consider the low educated graduating in the 5 Flemish provinces in the period 1994-2002, inference hinges on 44 clusters.[38] This raises the possibility of underestimating standard errors due to few clusters. Empiricists tend to agree that 50 clusters is enough when clusters have roughly the same size, but that a higher number of clusters is required when clusters are unbalanced (Cameron et al., 2008; MacKinnon and Webb, 2014). Applying the clustered estimator when clusters are too few is likely to worsen the bias, as cluster robust standard errors may be even smaller than conventional ones. This is what we find by comparing conventional and cluster robust standard errors of 2SLS estimations (see Table 1), which suggests that we have too few clusters.

We tackle this problem with wild restricted efficient residual bootstrap (WRE bootstrap) proposed by Davidson and MacKinnon (2010), which are designed for 2SLS in context of heteroscedasticity or clustered data. This procedure combines the restricted efficient residual bootstrap (RE bootstrap) proposed by Davidson and MacKinnon (2008) for the 2SLS, with wild bootstrap, which allows for intra-cluster correlation and heteroscedasticity (Cameron et al., 2008). For completeness, we apply wild bootstrap also to the t statistic of the instrument in the first stage, as well as to the t statistic of the regressor of interest when estimating (2) by OLS.[39]

[37]In 2SLS, the bias of the conventional variance estimator with grouped data is determined by the intra-class correlation of the second stage residuals (ρ_e) and by the intra-class correlation of the first stage fitted values (ρ_x). ρ_x is highest with grouped regressors in the first stage. As for OLS, $\rho_e > 0$ does not matter for standard errors as long as ρ_x is zero, but also a small ρ_e can give important bias with $\rho_x > 0$ (Angrist and Pischke, 2008, ch.8).

[38]Clusters are 44 since $g2002p3$ is empty. Table 5 in Appendix A shows the distribution across clusters.

[39]In the first stage the instrument is grouped, hence we need to cluster. In contrast, when estimating (2) by

The bootstrap procedures are explained in detail in Appendix D.

Because of few clusters, also the F statistic of the first stage is overestimated. To adjust it, we exploit the fact that in case of one instrument the F statistic is the square of the t statistic of the instrument in the first stage: i.e. with G clusters, $F(1, G-1) = t^2(G-1)$. Therefore, the bootstrap F statistic is the critical value of the $F(1, G-1)$ distribution that corresponds to the bootstrap P-value of the t statistic of the instrument in the first stage.[40]

5 Results

Table 1 summarizes the results for the low educated from estimating (2) by OLS and 2SLS on alternative labor market outcomes, measured 6 years after graduation. As a matter of space, we report only the effects of interest, i.e. the effect of early non-employment β in the structural equation and the impact of the instrument in the first stage regression. The complete regressions are reported in Appendix E. Odds and even columns show heteroscedastic-robust and cluster robust standard errors, respectively. The former takes into account that the dependent variables are dichotomous or censored at zero, while the latter allows for intra-cluster correlation induced by the fact that the instrument varies at the *gp* level. The fact that the 2SLS cluster robust standard errors are smaller than the 2SLS heteroscedastic-robust ones (columns 3 and 4 in Panel A) suggests that clustering is ineffective because of too few clusters. We ensure to make correct inference by bootstrapping the t statistic of the effects of interest and by reporting the corresponding P-value.

Panel B summarizes the results of the first stage. We report the original F statistic (10.51) as well as the bootstrap one (9.25), which accounts for the problem of few clusters. As expected, the former is overestimated. According to the Stock-Yogo critical values, the IV estimator of β over-rejects the null, as it leads to a rejection rate close to 15% when the true rejection rate is 5% (Stock and Yogo, 2005).[41] Hence, because of this test size, the IV estimates should to be taken with caution. The reported estimate suggests that one percentage point (*pp*) increase in the unemployment rate at graduation increases early non-employment by 5 *pp*. For Flanders, the unemployment rate rises on average by 1.4 *pp* in the 1994-2010 period (and by 1.6 *pp* in the Great Recession in 2008). Thus, graduating in an average downturn increases the proportion of hours spent in non-employment early in the career by about 7% (1.4×5).

OLS the regressor of interest varies at the individual level: hence, clustering is not a major issue. For completeness we provide both heteroscedastic-robust and cluster-robust standard errors.

[40] We are aware of only one study by Baltagi et al. (2013) on the performance of wild bootstrap applied to the F statistic in context of heteroscedastic - but not clustered - data. Bootstrapping directly the F test in our wild bootstrap procedure did not always yield the expected results (sometimes, the bootstrap P-value of the F statistic was smaller than the P-value of the original F). For this reason, we relied on the bootstrap P-value of the t statistic of the instrument in the first stage.

[41] With one instrument, the critical value for maximal size test of 10% and 15% is 16.3 and 8.96.

The upper part of Panel A refers to the employment indicators. The sign of the estimates suggests that early non-employment has a positive (negative) impact on the probability to be self- (salaried) employed, but the size of the effect is very small. In contrast to OLS, 2SLS are not significant: this may be a consequence of too small power of the test, because of the limited variation of the instrument. The null hypothesis of the exogeneity test is largely not rejected for all indicators, indicating that both estimators are consistent but the OLS is more efficient than the 2SLS one.[42] We therefore focus on OLS: for one *pp* increase in early non-employment, the probability to be salaried (overall) employed decrease by 0.17% (0.12%). These effects are statistically significant. Self-employment increases by 0.05%, but the impact is statistically insignificant.

More significant effects are shown in the bottom part of Panel A, which reports the unconditional effects of interest on continuous labor market outcomes. The null of the exogeneity test is rejected in all cases, meaning that the 2SLS estimator is consistent while the OLS one is not. A comparison between the estimates suggests that OLS underestimate scarring, which is in line with the hypothesis that the bias is caused by returns to search, liquidity constraints or measurement errors in the endogenous regressor, and consistent with what found in the literature. The 2SLS results indicate that one *pp* rise in early non-employment reduces earnings and hours worked by 10% and 7%, respectively (column 4). Both estimates are highly significant (at 1% level). Note that, for the cluster robust case (column 2 and 4) the P-values are computed according to the $t(G-1)$ distribution (with G being the number of clusters), to make a conservative inference.[43] However, the P-values of column 4 may be still underestimated due to the small number of clusters. We tackle this computing the bootstrap P-value for the t statistic of β. The latter is higher than the P-value from cluster robust standard errors, but still lower than 0.05. Hence, despite the small number of clusters, the impact of early non-employment on continuous outcomes is significant.

These estimates suggest that the low educated who, as a consequence of graduating in the adverse labor market conditions, found it difficult to get a stable position at the start of the career (i.e. the compliers), are still significantly penalized in terms of hours worked and earnings, 6 years after graduation. The results on hours worked are not directly comparable but consistent with the existing literature, which reports persistent effects of early unemployment on subsequent unemployment: for UK new graduates aged 16-21, Gregg (2001) estimates that a

[42]With clustered standard errors, the exogeneity test is defined as the difference of two Sargan-Hansen statistics: one for the equation with a smaller set of instruments where the suspect regressor is treated as endogenous, and one for the equation where the suspect regressor is treated as exogenous. Under the null that both sets of instruments are valid (i.e. the suspect regressor is exogenous), the statistic is distributed as $\chi^2(1)$. Note that this statistic is not corrected for the problem of few clusters. Hence, the P-value may be too small.

[43]In Stata this is automatically done in clustered OLS, but not in clustered 2SLS, where the P-values in the second stage are computed with the Normal distribution. In this case, the estimate remains significant at 1% level. Table 8 in Appendix E reports the clustered 2SLS with the significance level based on the Normal distribution.

3-months increase in the unemployment duration before age 23 significantly increases the time out of work between age 28 and 33 by 2 months. Schmillen and Umkehrer (2013) focus on new graduates from the German apprenticeship program and find that one additional day of unemployment during the first 8 years since graduation increases unemployment in the following 16 years by 0.96 days.

Table 8 in Appendix E show the entire OLS and 2SLS regressions. The individual controls show the expected signs: in the first stage, grade repetitions in secondary education is positively associated with early non-employment, while technical, vocational and apprenticeship programs are associated with a lower proportion of time spent in early non-employment, compared to general education. This suggests that the former programs ease the transition from school to work. An interesting result refers to mother education, which has a positive effect on early non-employment in the first stage, whereas a significant and negative (positive) impact on hours worked, earnings and salaried employment (self-employment) in OLS. This may capture the effect of unobserved liquidity constraints on the time worked, so that less constrained individuals (associated to higher mother's education) spend more time in early non-employment and work less hours in salaried employment (with consequent lower earnings). In the same spirit, low educated individuals with low educated mothers are also more likely to opt for salaried employment (with expected stable income under long-term contracts), while they are more likely to engage in (riskier) self-employment if their mothers are high educated. These effects become insignificant in 2SLS, to the extent that the endogeneity problem due to omitted liquidity constraints in (2) is tackled by the IV approach.

We repeat the analysis measuring the outcomes 8 years after graduation, to investigate the persistence of such scar. These results are reported in Table 2. The effects are qualitatively similar to the ones measured 2 years earlier: the estimates on discrete outcomes remain very small, while the adverse effects on continuous outcomes are still significant, but smaller than the ones measured at experience 6. These results may suggest that, for low educated youth, the scar originating from early non-employment on continuous outcomes persists still 8 years after graduation, i.e. it fades away slowly. However, the bootstrap F statistic in the first stage decreases to 6, warning against the problem of weak instrument. In this case, we know that 2SLS estimator is biased towards the OLS one. Therefore, we cannot discriminate to what extent 2SLS are able to identify the fact that the scar decreases over time, and to what extent they are simply biased towards the OLS: according to the F statistic, we may worry about the latter possibility. Thus, unfortunately these results are not very informative.

Table 1: Effect of Interest on Outcomes Measured 6 Years After Graduation for Low Educated.

Panel A: Effect of early non-employment in the structural equation:					
		OLS		2SLS	
	standard errors†	robust	cluster $g*p$	robust	cluster $g*p$
outcomes:		(1)	(2)	(3)	(4)
salaried empl.	coeff	-0.00169***	-0.00169***	-0.00256	-0.00256
	se	(0.00034)	(0.00041)	(0.00375)	(0.00290)
	P-val§		0.00019		0.38202
	Bootstrap P-val‡		0		0.45646
	Exogeneity test P-val§§				0.767
self-empl.	coeff	0.00054*	0.00054	0.00248	0.00248
	se	(0.00030)	(0.00041)	(0.00338)	(0.00258)
	P-val		0.19177		0.34175
	Bootstrap P-val		0.18619		0.37437
	Exogeneity test P-val				0.438
overall empl.	coeff	-0.00115***	-0.00115***	-0.00008	-0.00008
	se	(0.00021)	(0.00025)	(0.00207)	(0.00151)
	P-val		0.00005		0.95655
	Bootstrap P-val		0		0.96697
	Exogeneity test P-val				0.467
log earnings	coeff	-0.0269***	-0.0269***	-0.1002**	-0.1002***
	se	(0.0033)	(0.0040)	(0.0419)	(0.0291)
	P-val		2.51E-08		0.0013
	Bootstrap P-val		0		0.0060
	Exogeneity test P-val				0.00970
log hours worked	coeff	-0.0203***	-0.0203***	-0.0722**	-0.0722***
	se	(0.0024)	(0.0029)	(0.0307)	(0.0207)
	P-val		8.96E-09		0.0011
	Bootstrap P-val				0.0060
	Exogeneity test P-val				0.0113

Panel B: Effect of the instrument in the first stage : OLS			
outcome:	*standard errors:*	robust	cluster $g*p$
early non-empl.	coeff	5.4615***	5.4615***
	se	(1.7273)	(1.6848)
	P-val		0.00230
	Bootstrap P-val		0.00400
	F stat		10.51
	Bootstrap F stat††		9.25

*** p<0.01, ** p<0.05, * p<0.1. Standard errors between parentheses. Panel A reports results from estimating β in (2) on outcomes measured at potential experience 6. β is the effect of one *pp* increase in $y^0_{igpt_1}$, i.e. the % of hours spent in non-employment in the first 2.5 years after graduation relative to potential total hours if one would work full-time during the whole period. For clustered standard errors, we report the P-value and the wild bootstrap P-value. Column 1-2 (3-4) show OLS (2SLS). In 2SLS the provincial unemployment rate at graduation is used as instrument for $y^0_{igpt_1}$. Panel B shows the effect of the instrument on $y^0_{igpt_1}$ in the first stage and the corresponding F statistic.

† Robust indicates heteroscedastic-robust standard errors; clusters are defined by year g and province of residence at graduation p (G=44 clusters).

§ The P-value from clustered standard errors is computed using the $t(G-1)$ distribution, with G=44 (stars are reported accordingly).

‡ Bootstrap P-values are computed according to the wild bootstrap procedures explained in Appendix D for 999 repetitions.

†† Bootstrap F statistic is the F statistic corresponding to the bootstrap P-value of the t statistic of the instrument: we rely on the equivalence between F and t distribution: for $G = 44$, $t^2(G-1) = F(1, G-1)$.

§§ With clustered standard errors, this test is defined as the difference of two Sargan-Hansen statistics: one for the equation where y^0_{t1} is treated as endogenous and one for the equation where $y^0_{t_1}$ is treated as exogenous. Under the null that $y^0_{t_1}$ is exogenous, the statistic is distributed as $\chi^2(1)$.

Table 2: Effect of Interest on Outcomes Measured 8 Years After Graduation for Low Educated.

Panel A: Effect of early non-employment in the structural equation:					
		OLS		2SLS	
	standard errors†	robust	cluster $g * p$	robust	cluster $g * p$
outcomes:		(1)	(2)	(3)	(4)
salaried empl.	coeff	-0.00119***	-0.00119**	-0.00009	-0.00009
	se	(0.00035)	(0.00046)	(0.00430)	(0.00323)
	P-val§		0.01252		0.97670
	Bootstrap P-val†		0.01401		0.94494
	Exogeneity test P-val§§				0.739
self-empl.	coeff	0.00030	0.00030	-0.00087	-0.00087
	se	(0.00031)	(0.00043)	(0.00385)	(0.00354)
	P-val		0.48347		0.80640
	Bootstrap P-val		0.48448		0.82282
	Exogeneity test P-val				0.744
overall empl.	coeff	-0.00089***	-0.00089***	-0.00097	-0.00097
	se	(0.00019)	(0.00021)	(0.00220)	(0.00187)
	P-val		0.00013		0.60815
	Bootstrap P-val		0		0.68068
	Exogeneity test P-val				0.966
log earnings	coeff	-0.0230***	-0.0230***	-0.0674*	-0.0674**
	se	(0.0033)	(0.0042)	(0.0394)	(0.0311)
	P-val		2.34E-06		0.0360
	Bootstrap P-val		0		0.0841
	Exogeneity test P-val				0.162
log hours worked	coeff	-0.0172***	-0.0172***	-0.0487*	-0.0487**
	se	(0.0024)	(0.0032)	(0.0290)	(0.0224)
	P-val		2.53E-06		0.0355
	Bootstrap P-val		0		0.0861
	Exogeneity test P-val				0.171

Panel B: Effect of the instrument in the first stage : OLS			
outcome:	*standard errors:*	robust	cluster $g * p$
early non-empl.	coeff	4.9717***	4.9717***
	se	(1.6605)	(1.6060)
	P-val		0.00345
	Bootstrap P-val		0.01802
	F stat		9.584
	Bootstrap F stat††		6.05

*** p<0.01, ** p<0.05, * p<0.1. Standard errors between parentheses. Panel A reports results from estimating β in (2) on outcomes measured at potential experience 8. β is the effect of one *pp* increase in $y^0_{igpt_1}$, i.e. the % of hours spent in non-employment in the first 2.5 years after graduation relative to potential total hours if one would work full-time during the whole period. For clustered standard errors, we report the P-value and the wild bootstrap P-value. Column 1-2 (3-4) show OLS (2SLS). In 2SLS the provincial unemployment rate at graduation is used as instrument for $y^0_{igpt_1}$. Panel B shows the effect of the instrument on $y^0_{igpt_1}$ in the first stage and the corresponding F statistic.

† Robust indicates heteroscedastic-robust standard errors; clusters are defined by year g and province of residence at graduation p (G=44 clusters).

§ The P-value from clustered standard errors is computed using the $t(G-1)$ distribution, with G=44 (stars are reported accordingly).

‡ Bootstrap P-values are computed according to the wild bootstrap procedures explained in Appendix D for 999 repetitions.

†† Bootstrap F statistic is the F statistic corresponding to the bootstrap P-value of the t statistic of the instrument: we rely on the equivalence between F and t distribution: for $G = 44$, $t^2(G-1) = F(1, G-1)$.

§§ With clustered standard errors, this test is defined as the difference of two Sargan-Hansen statistics: one for the equation where y^0_{t1} is treated as endogenous and one for the equation where $y^0_{t_1}$ is treated as exogenous. Under the null that $y^0_{t_1}$ is exogenous, the statistic is distributed as $\chi^2(1)$.

5.1 Sensitivity Analysis for the Low Educated

As a first sensitivity analysis, we want to rule out that results in Table 1 are driven by multicollinearity, which may arise because in (2) we add many controls for the persistence of the unemployment rate (the current unemployment rate, the average unemployment rate since the end of the early period and the moment of evaluation as well as the minimum unemployment rate since graduation). Therefore, to rule this possibility we re-run the model including only the current unemployment rate (thereby excluding $\overline{UR}_p$ and $minUR_{pt}$): the results of this restricted specification are reported in Table 12 of Appendix F. Note that the effect of interest should be interpreted as an average effect between the scarring of early non-employment due to adverse labor market conditions at graduation and the persistence of the unemployment rate in the period before the moment of evaluation.[44] The stability of the results ensures that multicollinearity is not driving the results.

Next, we assess the impact of measurement error in the endogenous regressor, which arises from the combination of survey and administrative data. In fact, early non-employment is measured precisely for individuals graduating in the 1998-2002 period - by means of administrative data - but it is imputed for those graduating in the period 1994-1997, based on the Sonar database (see Appendix B for details). This allows us to maximize the variation of the instrument considering the entire graduation period 1994-2002, at the cost of introducing some measurement error in the endogenous regressor. Therefore, in this second sensitivity analysis we re-run the analysis for low educated restricting the sample to graduation period 1998-2002, so that the endogenous regressor is measured uniquely by administrative data. Of course, clusters are drastically reduced from 44 to 24.[45] This is problematic not only because it exacerbates the problem of few clusters, but also because equation (2) contains too many parameters ($k = 30$) compared to the number of clusters, and as a consequence the rank condition in 2SLS is not satisfied.[46] Therefore, we need to reduce the parameters in equation (2).

We decide to exclude some of the non-significant individual controls and rather include in the specification all the aggregate regressors, which are very important to ensure the validity of the exclusion restriction.[47] In particular, we drop the following controls that are jointly not significant at the 5% level according to an F test in the first stage regression: dummy for living with single parent, dummy for not living with parents, number of household members aged 12-17, 18-29, 30-64, 65+ (we keep the number of household members aged 0-11 since it is significant); plus, we aggregate all educational tracks different from general education (technical, vocational, part-time education or apprenticeship) and include a dummy for general education

[44] This is because $\overline{UR}_p$ is significant in (2) while $minUR_{pt}$ is not: see Table 8 and 9 Appendix E.

[45] In principle, 5 graduation years times 5 provinces, i.e. $G = 25$. However, $g2002p3$ is empty.

[46] The variance-covariance matrix of moment conditions has size (30×30) and rank=24 (Baum et al., 2003).

[47] Compared to the aggregate regressors individual controls play a minor role, as they alleviate the problem of omitted individual characteristics, which is anyway tackled by the IV approach.

instead.[48] Therefore, the new regression includes the following individual controls: father and mother education, repeated grades at age 17, dummy for general education at age 17, number of household members aged 0-11 when the individual is aged 17, birth cohort dummies. Table 11 shows that results are robust to this alternative specification, as the OLS estimate of the endogenous regressor is very stable in the full specification (column 1 and 4) and in the restricted specification (column 2 and 5); both specifications consider the graduation period 1994-2002.

Panel B of Table 10 reports the first stage regression for the 1998-2002 period. This table should be compared to Table 1. First, the bootstrap F statistic is 3.6 for the graduation period 1998-2002 compared to 9.2 for the period 1994-2002: therefore, the instrument becomes weak by restricting to the former period. As expected, the increased discrepancy between the original F and the bootstrap F statistic in Table 10 compared to Table 1 shows that the few-clusters bias worsens a lot by shifting from 44 to 24 clusters. Second, the direct effect of the instrument on early non-employment for the 1998-2002 period doubles compared to the period 1994-2002. An explanation is that the former period focuses on the dot-com recession, whose effects are mitigated in considering a larger span. Given the low F statistic, 2SLS are not reliable.

However, we can focus on the OLS results in Table 11 to shed lights on the importance of the measurement error in the endogenous regressor. In principle, this should bias the OLS estimate for the period 1994-2002 (Column 1 and 4) towards zero. At the same time, the OLS estimate in Column 3 and 6 should not be affected by measurement error "by construction", since the endogenous regressor is entirely measured by administrative data for the 1998-2002 period. We therefore compare the first row across columns (1 with 3 and 4 with 6): for each outcome, the estimate based on the graduation period 1994-2002 is slightly smaller than the corresponding estimate for the period 1998-2002. This is consistent with the presence of measurement error in the endogenous regressor for the graduation period 1994-2002. However, this difference is small (0.2 *pp*), which suggests that overall the OLS estimates are quite close in the 1998-2002 and 1994-2002 period: as a consequence, we conclude that measurement error in early non-employment is not a major issue in the main results.

Of course, another explanation could be that the combination of bias from various sources (ability, returns to search, liquidity constraints) may differ in the period 1998-2002 - when measurement error is absent - compared to the period 1994-2002 - when measurement error is present - and yet yield the same net effect: however, this requires that these (fixed) omitted factors affect individual labor market performances differently in the 1994-1997 and 1998-2002 period, which is peculiar. We think that this last story is more difficult to be argued.

[48]The reference is an aggregated category for technical, vocational, part-time education and apprenticeship.

5.2 Discussion on the validity of the IV approach

Under the assumption of homogenous effects, the 2SLS identifies the effect of interest under the Condition 1 and 2 illustrated in Section 4. This section discusses the limitations of this approach by assessing the validity of each assumption and the role played by each of them in the identification. Consistently with the notation used in Section 4, let Y be the outcome, Z the instrument, Y^0 the endogenous regressor. The IV estimator is $\beta_{IV} = cov(Y,Z)/cov(Y^0,Z)$. We start noting that it can be expressed as the ratio between the causal effect of Z on Y in the reduced form regression (RF) and the causal effect of Z on Y^0 in the first stage ($1^{st}stage$): $\beta_{IV} = \frac{cov(Y,Z)}{cov(Y^0,Z)} = \frac{cov(Y,Z)/Var(Z)}{cov(Y^0,Z)/Var(Z)} = \frac{RF}{1^{st}stage}$. Condition 1 (exogeneity) is required to interpret the numerator as causal. By virtue of the exclusion restriction, this causal effect can only result from Z effecting Y^0 and Y^0 in turn affecting Y. Condition 2 allows us to quantify the effect of interest (the effect of Y^0 on Y), dividing $cov(Y,Z)$ by $cov(Y^0,Z)$, which yields the IV estimator. At the extreme in which Z has no explanatory power in the first stage, $cov(Y^0,Z) = 0$ and the ratio would not exist. If condition 1 is not satisfied, the reduced form yields a biased estimate of the effect of Z on Y and, as a consequence, the IV estimator provides a biased estimate of the effect of Y^0 on Y. Furthermore, the bias of the IV estimator is magnified in presence of weakly correlated instruments.

In Section 4 we extensively discuss the validity of Condition 1. We formally test the exogeneity of the timing of graduation, and argue that the place of graduation is also exogenous since we hardly observe any inter-provincial mobility in the data. However, commuting across provinces is a source of endogeneity that we cannot control for. Moreover, we identify a number of factors that may represent a source of violation of the exclusion restriction and set up the specification in (2) so that the latter is most likely satisfied, conditional on the covariates. In our case, Condition 2 is the most problematic, as the instrument is not strong enough. As already mentioned, this may magnify problems related to even small violations of Condition 1.

Differently from the IV estimator, the effect of Z on Y in the reduced form equation only requires the exogeneity of Z to be given a causal interpretation. For this reason, we complement the IV approach with the estimates of the reduced form equation, which are reported in Table 13 together with the first stage:[49] for continuous outcomes, these effects are unconditional on being salaried employed. The effect of Z on the employment indicators evaluated 6 years after graduation are not significant (Columns 4-6 of Table 13). Instead, the unconditional effects of Z on the continuous variables are negative and significant: one *pp* increase in the unemployment rate at graduation decreases earnings by 54% and hours worked by 39%, 6 years after graduation. These effects are very big. The reason is that they are unconditional average effects, which include also the effects for those who are not salaried employed at the moment of evaluation.

[49] In the reduced form, $y^0_{igpt_1}$ in (2) is replaced by the unemployment rate at graduation; in the first stage, $y^0_{igpt_1}$ in (2) is replaced by the unemployment rate at graduation while at the same time y_{iT} is replaced by $y^0_{igpt_1}$.

Dividing these effects by the effect of Z on Y^0 in the first stage (Column 7) gives the 2SLS estimates of interest in Table 1.

So far the discussion has relied on the quite unrealistic assumption of homogenous effects. Allowing for heterogeneous effects, an IV estimator identifies an average causal effect for the sub-population that reacts to the instrument under two additional assumptions:[50]

- Stable Unit Treatment Value Assumption (SUTVA): no interference between units.
- Monotonicity: the effect of Z on Y^0 has the same sign for everybody.

In a simple framework with no covariates where both Z and Y^0 are discrete, the IV estimator identifies the local average treatment effect (LATE) for the compliers. In a more complicated framework as in our case, the IV estimator can be interpreted as a weighted average of local average treatment effects (LATEs) (Angrist and Pischke, 2008).[51]

The SUTVA assumption requires that the outcomes Y are independent across individuals. This is satisfied if one can rule out crowding-out effects: these effects occur if there are less job openings than new graduates so that the latter compete for the same jobs and, as a consequence, only some of them get good positions while some others remain unemployed, get lower-paying jobs, or work less hours. This is a quite strong assumption to make in the context of labor markets. The monotonicity assumption requires that a higher unemployment rate at graduation Z prolongs (shortens) early non-employment for every low educated person. The direction of the effect does not matter to the extent that it has the same sign for all individuals.[52] Also this assumption boils down to crowding out effects in the labor market, as it rules out the possibility that, by graduating in a downturns, some new graduates may experience longer early non-employment while other luckier ones work since the start of the career. As for the case of homogenous effects, the IV is the ratio between the effect of Z on Y in the reduced form equation and the effect of Z on Y^0 in the first stage. In the program evaluation literature, the numerator is the Intention-To-Treat (ITT) effect. The SUTVA ensures that the numerator is not biased, which typically occurs in case of externalities (Heckman et al., 1999). Monotonicity (together with the exclusion restriction) ensures that the IV estimator identifies the causal effect of interest for the compliers - i.e. in this case those who experience high incidence of non-employment because graduated with high unemployment rates. If either the exclusion restriction or the monotonicity

[50] Condition 1 is split into random assignment and exclusion restriction. Condition 2 remains invariant.

[51] With covariates X, the IV is a weighted average of covariates-specific LATEs (one for each value of X), where more weight is attributed to covariate values where Z creates more variation in the fitted values. If Y^0 is continuous, the IV is a weighted average of LATEs, where the weights depend on how the compliers are distributed over the range of Y^0. With multiple instruments, the IV is a weighted average of LATEs for instrument-specific compliant sub-populations, where the weights are proportional to the relative strength of each instrument in the first stage. Similarly, if Z is continuous, the IV is a weighted average of instrument values-specific LATEs, and bigger weights are given to the instrument values that contribute the most in explaining Y^0 in the first stage: here, bigger weights are given to the clusters gp whose unemployment rate variation is mostly correlated with early non-employment.

[52] Monotonicity is important given the heterogeneity of the effect, since the LATE is an average causal effect.

are violated, the IV estimates the LATE for the compliers plus a bias, which can be characterized depending on the assumption that is not satisfied. Clearly, allowing for heterogeneous effects comes at the cost of requiring more assumptions which, as already mentioned, may be restrictive in this specific case.

We conclude relating this analysis with the work of Cockx and Ghirelli (2014) which use the same sample (see Appendix A for details). In this section we estimate unconditional ITT effects of the instrument on alternative outcomes which are evaluated at potential experience 6. Given that the dependent variables are fixed at a given point in time, we exploit the cross-section variation. By contrast, Cockx and Ghirelli (2014) exploit the entire panel structure of the data and jointly estimate distinct ITT effects on the same outcomes - one for each year of potential experience. The effects on continuous variables are conditional on salaried employment. Consequently, these results cannot be directly compared since the two studies use different frameworks. Namely, in Cockx and Ghirelli (2014), the panel structure allows to control for calendar time FE in the specification (which accounts for common shocks affecting the entire economy),[53] and in general contain much more information, which yields more precise estimates. By excluding $minUR_{pt}$ and $\overline{UR}_p$ from (2) - which gives the most similar specification to the one estimated in Cockx and Ghirelli (2014) - we estimate a conditional ITT of -3.8% on earnings and -0.9% on hours worked (estimation output not reported).[54] The corresponding effects in Cockx and Ghirelli (2014) are -3.6% on earnings and -2.5% on hours worked (see Table B.1 in Cockx and Ghirelli, 2014). As expected, results from the panel estimation are more precise.[55] The point estimates of the effect on earnings are quite close across the two studies, as opposed to the ones referring to the effect on hours worked. However, the latter are not inconsistent, given the large confidence intervals of the conditional ITT resulting from our approach.

6 Conclusions

We consider a sample of low educated youth graduating in the period 1994-2002 in Flanders, the most prosperous of three Belgian regions. We study the impact of early non-employment on workers' subsequent career, measured 6 and 8 years after graduation. We deal with the endogeneity of unobserved individual characteristics with an IV approach, where the provincial unemployment rate at graduation is used as instrument for non-employment. The problem of few clusters is addressed by wild bootstrap methods.

Since in Belgium labor market institutions differ for blue and white collar workers, we focus

[53]There are additional differences in the specification: Cockx and Ghirelli (2014) include asymmetric effects for graduating in upturns/downturns, and interact the unemployment rate at graduation and the current one with experience splines. In this analysis, $minUR_{pt}$ and $\overline{UR}_p$ are included in (2).

[54]We focus on the continuous outcomes as they yield significant ITT effects, as opposed to the discrete ones.

[55]Standard errors of conditional ITT on earnings and hours worked are respectively 0.015 and 0.014 in Cockx and Ghirelli (2014), and 0.027 and 0.025 in this study.

on low educated new graduates. Cockx and Ghirelli (2014) provide evidence that Belgian low and high educated graduating in the period 1994-2004 are exposed differently to adverse labor market conditions because of these institutional differences: due to strict EPL for white collars, the high educated graduating in downturns are forced to downgrade and as a consequence are trapped in lower-paying jobs. The low educated instead tend to experience non-employment because of the flexible EPL for blue collars, and because wages are downwards-rigid due to the presence of minimum wages. We define the identification strategy in light of these results, so to ensure that the exclusion restriction is most likely satisfied: that is, we consider a measure of early non-employment as endogenous variable, since for the low educated the scar from graduating in downturns occurs through the loss of early work experience.[56]

In this study we have applied an IV approach to unveil the causality between early non-employment and subsequent labor market outcomes. Throughout the article we have discussed the assumptions required by the IV estimator, their validity and the role they play in the identification. Unfortunately, our IV estimators suffer from weak identification problem, which may magnify any small violation of the exclusion restriction.

We find that one *pp* increase in the time spent in non-employment in the first 2.5 years since graduation decreases annual earnings and hours worked from salaried employment by 10% and 7% respectively, 6 years after graduation. Provided that our identification strategy is correct, these effects are causal. They may originate by the foregone human capital that would have been accumulated in case of early work-experience, or because early non-employment is interpreted as a signal of low quality. From a policy perspective, it is therefore very important that low educated workers acquire work experience at the start of their career. This may be impeded in a rigid labor market where workers reallocation is costly and as a consequence exiting unemployment is harder and may have long-lasting consequences. Early work experience instead may be enhanced by a *flexicurity* system in which workers are reallocated easily while unemployed workers are protected by an unemployment insurance system. In this context, the majority of unemployment is temporary - to the extent that the hiring cost are low. Long-term unemployed, who are provided for by the unemployment insurance, may be additionally supported in their job search by active labor market policies.

The Belgian low educated workers face a number of rigidities which restrain workers reallocation: the short-term work compensation program, by anchoring the employees to the employers, is an example. A second rigidity is represented by the very high Belgian minimum wages, which limit the absorbtion of low educated new graduates for whom minimum wages are binding. A third one is the asymmetry between the flexible EPL for blue collars and a rigid EPL for white collars, which characterized the Belgian labor market until 2013: in adverse labor market conditions, low educated new graduates risked to be marginalized, if they had to face the additional

[56]For high educated we use early wage as endogenous regressor as accepting lower-paying jobs is the main channel to explain the scars of graduating in downturns. However, the instrument is weak (see Appendix H).

competition of higher educated new graduates, who in turn downgraded because of rigid EPL for white collar workers - which increased the hiring costs to fill white collar positions. Thus the low educated experienced serious difficulties to find stable jobs as a consequence of this asymmetry. Note that this controversial discrimination between blue and white collar workers has been removed since the beginning of 2014, as a single employment contract has been introduced, stipulating the same EPL for white and blue collar workers.

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APPENDIX

A Description of the Final Sample

In this study we consider almost the same sample as in Cockx and Ghirelli (2014). They consider 3514 individuals (comprising both low and high educated) while we consider 3586 individuals (see Table 3): i.e. we add 72 individuals (2% of the sample), including low educated graduating in 2002 and high educated graduating in 1995-96. For details in the construction of the control and outcome variables, see Section S.1, S.2 and S.3 of the Supplementary Online Appendix of Cockx and Ghirelli (2014): http://users.ugent.be/~bcockx/Ascars.pdf

Table 3: Dividing the Sample in Low and High Educated

education	low educated	high educated	Total
1	2		2
2	36		36
3	89		89
4	113		113
5	185		185
6	1,111		1,111
7	366	289	655
8		55	55
9		707	707
10		367	367
11		232	232
12		33	33
13		1	1
Total	1,902	1,684	3,586

Completed education refers to the number of years of education successfully attained from the beginning of secondary education, i.e. at age 12. Low educated are those who graduate with at most secondary education, which consists in 7 years of education in case of vocational track and 6 years for all other educational programs. High educated are those with higher than secondary education.

Table 4: Correspondence Between Low-High Educated and Blue-White Collar Workers

Function Undertaken 6 Years After Graduation†						
	Low educated			High educated		
	Freq.	Percent	Cum.	Freq.	Percent	Cum.
blue collar	1,193	62.72	62.72	184	10.93	10.93
white collar	390	20.5	83.23	1,193	70.84	81.77
functionary	68	3.58	86.8	105	6.24	88
missing	251	13.2	100	202	12	100
Total	1,902	100		1,684	100	

Prevalent Function Undertaken up to 6 Years After Graduation‡						
	Low educated			High educated		
	Freq.	Percent	Cum.	Freq.	Percent	Cum.
blue collar	1,346	70.77	70.77	235	13.95	13.95
white collar	401	21.08	91.85	1,337	79.39	93.35
functionary	53	2.79	94.64	36	2.14	95.49
missing	102	5.36	100	76	4.51	100
Total	1,902	100		1,684	100	

† It refers to the type of function undertaken at potential experience 6.

‡ It refers to the function that is undertaken more than 50% of the time from graduation up to potential experience 6. 70% (14%) of low (high) educated are prevalently employed as blue collars. Thus, there is clear correspondence between low (high) educated and blue (white) collars.

Table 5: Number of Individuals by Graduation Year and Province of Residence at Graduation

	Low educated					
grad_year	prov1	prov2	prov3	prov4	prov5	Total
1994	30	9	31	48	25	143
1995	47	22	44	48	48	209
1996	84	45	65	85	38	317
1997	78	41	65	67	36	287
1998	111	46	78	90	61	386
1999	99	42	47	64	47	299
2000	56	18	30	28	31	163
2001	26	8	11	17	18	80
2002	10	3	0	2	3	18
Total	541	234	371	449	307	1902

The analysis considers the graduation period 1994-2002 for the low educated. The combination g2002 & prov3 is excluded since empty. Provinces are in the following order from 1 to 5: Antwerp, Flemish Brabant, Western Flanders, Eastern Flanders, Limburg. Each combination of graduation year and province of residence at graduation represents a cluster *gp* in the main analysis.

Table 6: Descriptive Statistics of Individual Control Variables: Low Educated

Variable	Obs	Mean	Std. Dev.	Min	Max	label
birth cohort76	1902	0.330	0.470	0	1	1 if born in 1976
birth cohort78	1902	0.332	0.471	0	1	1 if born in 1978
birth cohort80	1902	0.338	0.473	0	1	1 if born in 1980
live in single-parent	1902	0.120	0.326	0	1	1 if live with single parent at age17(Dec)
not live with parents	1902	0.064	0.244	0	1	1 if not live with either parents at age17(Dec)
HH members aged 0-11	1902	0.248	0.625	0	7	nr of other HH members aged0-11 at age17(Dec)
HH members aged 12-17	1902	0.508	0.689	0	7	nr of other HH members aged12-17 at age17(Dec)
HH members aged 18-29	1902	0.521	0.731	0	8	nr of other HH members aged18-29 at age17(Dec)
HH members aged 30-64	1902	1.889	0.400	0	5	nr of other HH members aged30-64 at age17(Dec)
HH members aged 65+	1902	0.037	0.211	0	2	nr of other HH members aged65+ at age17(Dec)
father education	1902	4.586	3.201	0	13	father completed education since age12
mother education	1902	4.212	3.062	0	13	mother completed education since age12
years of delay in sec.edu	1902	0.828	0.840	-1	4	years of delay at age17(Aug)
general education	1902	0.110	0.313	0	1	1 if general edu at age17(Aug)
technical education	1902	0.379	0.485	0	1	1 if technical edu at age17(Aug)
vocational education	1902	0.410	0.492	0	1	1 if vocational edu at age17(Aug)
apprenticeship/PT voc.	1902	0.100	0.301	0	1	1 if apprenticeship/PT voc. edu at age17(Aug)

Descriptive statistics are computed on the sample used in the main analysis, in which the dependent variables are measured 6 years after graduation.

Table 7: Descriptive Statistics of Outcomes and Endogenous Regressor: Low Educated

Variable	Obs§	Mean	Std. Dev.	Min	Max	Label
						At potential experience 6
log earnings	1902	8.419	3.588	0	10.770	log annual gross earnings from salaried empl.
log hours	1902	6.202	2.669	0	7.725	log annual hours worked in salaried empl.
self-empl.	1902	0.122	0.327	0	1	1 if only pos. earnings from salaried (& not self-empl)
salaried empl.	1902	0.837	0.369	0	1	1 if registered as self-empl.
overall empl.	1902	0.959	0.198	0	1	1 if pos.earnings from salaried or registered as self-empl.
early non-empl.	1902	30.596	29.647	0	100	% hours not worked relative to FT salaried empl.
						At potential experience 8
log earnings	1894	8.615	3.461	0	10.943	log annual gross earnings from salaried empl.
log hours	1894	6.311	2.563	0	7.725	log annual hours worked in salaried empl.
self-empl.	1894	0.150	0.357	0	1	1 if only pos. earnings from salaried (& not self-empl)
salaried empl.	1894	0.822	0.383	0	1	1 if registered as self-empl.
overall empl.	1894	0.971	0.166	0	1	1 if pos.earnings from salaried or registered as self-empl.
early non-empl.	1894	30.553	29.634	0	100	% hours not worked relative to FT salaried empl.

Descriptive statistics is based on the sample studied in the main analysis, in which low educated graduate in the period 1994-2002. The employment indicators are related as follows: salaried+self=overall employment. For continuous outcomes we add value one before taking the log, so that non-salaried employed at the moment of evaluation are included with outcomes=0 after the logarithmic transformation.

§ Low educated are observed in both years of potential experience, except for 8 individuals who are observed in the labor market at potential experience 6 but not 2 years later.

B Construction of the Endogenous Variable

We define potential experience as a variable counting each calendar year since graduation. Potential experience 0 corresponds to the year of graduation and runs from the month after graduation until December of that calendar year; therefore, it potentially lasts less than 12 months (6 months for a student graduating in June). Potential experience 1 runs from January until December of the subsequent calendar year, thereby lasting 12 months. Subsequent potential experience years are defined similarly.

The regressor of interest is the percentage of hours spent in non-employment at potential experience 0-2, relative to potential total hours if one would work full-time during the whole period. We express everything in hours because this is the smallest unit of measurement used in the administrative data (it is used to measure time worked in part-time employment). The reference period is computed considering the entire calendar year for potential experience 1 and 2, and the part of the calendar year following the month of graduation for potential experience 0. That is, for one who graduated in June, the time spent working at potential experience 0-2 is divided by the total working hours in 30 months of full-time salaried employment. As already mentioned in Section 3 of the main text, for simplicity we refer to this reference period as "the first 2.5 years since graduation", i.e. the average reference period since most of the sample graduates in June.

Define a as the total hours worked (including self-employment) in the first 2.5 years from graduation and b the potential total hours if one wold work full-time during the whole period; then the regressor of interest is computed as $100 * (b - a)/b$. Below we explain in detail how its components are constructed.

1. Construct a according to the following steps.

   I. a is mostly based on the total hours worked in salaried employment and the date of registration and cancellation from the self-employment register from the Data Warehouse. Since hours worked are not available for self-employment, we assume that the latter work as much as a full-time salaried worker: i.e., 5 days per week and 8 hours per day until 2002, and 5 days per week and 7.6 hours per day from 2003 onwards. This is due to the introduction of a new law in Belgium that changed the daily working hours from 8 to 7.6 from the first of January 2003. Whenever one combines self-employment and salaried-employment in the same quarter we make the same assumption, so that the hours worked do not exceed the bounds.

   II. The construction of a requires an additional adjustment due to the limited availability of the administrative data, which cover the period 1998-2010. Since the sample contains 3 birth cohorts (1976, 1978, 1980) and that compulsory education ends at age 18 in Belgium, these data can be used in the following cases (68.5% of the final sample): all individuals born in 1980, those born in 1978 graduating at least at age 20 and those born in 1976 graduating at least at age 22. Figure 1 summarizes the availability of the data. To retain in the analysis also students born in 1978 (1976) graduating at age 18-29 (18-21),[57] we exploit the monthly working status from the Sonar database and impute the values of a following the procedure used for self-employed workers. That is, for each month in which individuals are working according to Sonar we attribute the working hours of a full-time salaried worker: i.e. 8 (7.6) working hours per day until (strictly after) 2002 and 21.6 working days per month (assuming 65 working days in a quarter gives 21.6 working days per month: $21.6 \times 3 = 65$).

2. Construct b. Recall that it is defined as the potential total hours if one wold work full-time during the first 2.5 years since graduation. As for a, we consider a full-time working regime of 5 days per week and 8 (7.6) hours per day until (strictly after) 2002. This gives a total of 2080 annual working hours until 2002 (8 hours/day $\times$ 65 days/quarter $\times$ 4 quarter/year) and 1976 annual working hours from 2003 onwards (7.6 hours/day $\times$ 65 days/quarter $\times$ 4 quarter/year).

3. The regressor of interest is computed as $(b - a)/b * 100$ and hence ranges between $[0, 100]$. In some cases (10% of the final sample) this percentage is negative because of overtime work. Therefore, it is censored at 0.

[57] These cases correspond to 31.5% of the final sample.

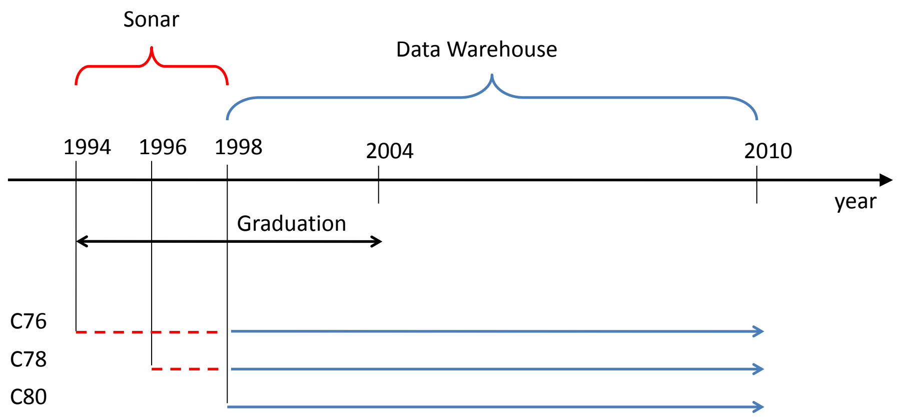

Figure 1: Availability of data for the construction of the main regressor. In 1998 birth cohorts 76, 78 and 80 are aged 22, 20 and 18, respectively. Birth cohort 76 (78) turns age 18 in 1994 (1996).

C LFS - Provincial unemployment rate

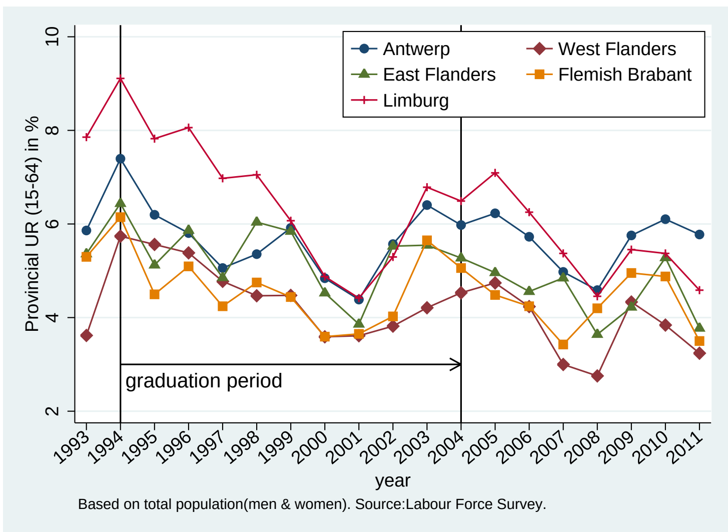

Figure 2: Provincial unemployment rates (15-64) for Flanders: graduation period 1994-2004, considering low and high educated together. For details, see Appendix S.1.5 of the Supplementary Online Appendix of Cockx and Ghirelli (2014).

D Bootstrap Procedure

The basic idea of bootstrap testing is to compare the observed value of some test statistic with the empirical distribution of B bootstrap test statistics computed on as many pseudo-samples, where B is the number of bootstrap replications. We use a *Wild Restricted Efficient Residual Bootstrap* (WRE Bootstrap) proposed by Davidson and MacKinnon (2010). It is the wild version of the Restricted Efficient Residual bootstrap designed for 2SLS by Davidson and MacKinnon (2008). Few words on the terminology (which will become clearer below): *Residual* means that the objects to sample in generating the pseudo-samples are the residuals.[58] *Wild* refers to a procedure that creates pseudo-samples based on $residuals * 1$ with probability 0.5 and $residuals * (-1)$ with probability 0.5, with this assignment at the cluster level. This allows to preserve the intra-cluster correlation. *Efficient* means that the first stage of 2SLS is efficiently estimated in case of weak instruments. *Restricted* means that the null hypothesis of interest is imposed on the data generating process (DGP): this enhances efficiency in the procedure. Consider the following system of equation, which is a simplified version of the 2SLS model of interest:

$$y_{igp} = \beta y^0_{igp} + x_i'\delta + e_{igp} \tag{A.1}$$

$$y^0_{igp} = \pi Z_{gp} + x_i'\delta + u_{igp} \tag{A.2}$$

(A.1) is the structural equation where individual labor market outcomes y_{igp} are regressed on early non-employment y^0_{igp} and individual controls (for simplicity we omit the grouped covariates and the time subscripts in (1)), and (A.2) is the first stage regression where the endogenous explanatory variable is regressed on the grouped instrument Z_{gp} and all exogenous regressors x_i. The fact that the instrument is grouped requires cluster robust standard errors in 2SLS. We are interested in bootstrapping the t statistic of y^0_{igp}, i.e. $t(\hat{\beta}, \beta_0) = \frac{(\hat{\beta}-\beta_0)}{se(\hat{\beta})}$. Call $\hat{\tau}$ the observed value of this statistic. The bootstrap procedure will generate an empirical distribution of B bootstrap test statistics τ^*, with B being the number of repetitions, and where these statistics are generated using the bootstrap DGP which imposes the null hypothesis that is tested. In practice this is implemented as follows:

1. Estimate the system in (A.1)-(A.2) by 2SLS with cluster robust standard errors and obtain the statistic $\hat{\tau}$.

2. Estimate the restricted version of (A.1) by OLS imposing the null hypothesis $\beta = 0$ (with conventional standard errors). Predict the residuals $\tilde{e}_{igp}$ and the fitted values $\tilde{y}_{igp}$. (*Restricted*)

3. Estimate (A.2) including $\tilde{e}_{igp}$ as additional control, i.e.:$y^0_{igp} = \pi Z_{gp} + x_i'\delta + \gamma\tilde{e}_{igp} + residuals$. Compute the residuals $\tilde{u}_{igp} = residuals + \hat{\gamma}\tilde{e}_{igp}$. This allows the residuals of the first stage

[58] Alternatively, one can sample pairs [y X] of data.

not to be too small in case of weak instrument (*Efficient*). Accordingly, compute the fitted values $\tilde{y}^0_{igp} = \hat{\pi} Z_{gp} + x_i' \hat{\delta}$.

4. At the cluster level, multiply the residuals $\tilde{u}_{igp}$ and $\tilde{e}_{igp}$ by a random variable ν^*, where $\nu^* = 1$ and $\nu^* = -1$ with probability 1/2, respectively. Note that the same ν^* is applied to both residuals: this preserves the correlation across (A.1) and (A.2).[59] (*Wild*)

5. Construct $y^*_{igp} = \tilde{y}_{igp} + \nu^* \tilde{e}_{igp}$ and $y^{0*}_{igp} = \tilde{y}^0_{igp} + \nu^* \tilde{u}_{igp}$.

6. Estimate (A.1)-(A.2) by 2SLS where y_{igp} is replaced by y^*_{igp} and y^0_{igp} by y^{0*}_{igp}, with cluster robust standard errors and obtain the t statistics τ^*.

7. Repeat steps 4-6 B times, where B is the number of repetitions, so to get an empirical distribution of τ^*_j for $j = 1, ..., B$.

8. Calculate the bootstrap P-value as $p^*(\hat{\tau}) = 2min(\frac{1}{B}\sum_1^B \mathbf{1}[\tau^*_j < \hat{\tau}], \frac{1}{B}\sum_1^B \mathbf{1}[\tau^*_j > \hat{\tau}])$.

Below we describe the simpler procedure to compute wild bootstrap in the OLS case. In the main analysis, we apply this to the OLS estimations of the structural equation and to the first stage. Below we take the first stage as example: we are interested in estimating (A.2) by OLS and then in bootstrapping the t statistic of the instrument, $t(\hat{\pi}, \pi_0) = \frac{(\hat{\pi} - \pi_0)}{se(\hat{\pi})}$. The procedure is reported below:

1. Estimate (A.2) by OLS with cluster robust standard errors and obtain the statistics $\hat{\tau}$.

2. Re-estimate (A.2) imposing the null hypothesis $\pi = 0$ (with conventional standard errors). Predict the residuals $\tilde{u}_{igp}$ and the fitted values $\tilde{y}^0_{igp}$. (*Restricted*)

3. At the cluster level, multiply the residuals $\tilde{u}_{igp}$ by a random variable ν^*, where $\nu^* = 1$ and $\nu^* = -1$ with probability 1/2, respectively.

4. Construct $y^{0*}_{igp} = \tilde{y}^0_{igp} + \nu^* \tilde{u}_{igp}$.

5. Estimate (A.2) by OLS where y^0_{igp} is replaced by y^{0*}_{igp}, with cluster robust standard errors and obtain the t statistics τ^*.

6. Repeat steps 3-5 B times, where B is the number of repetitions, so to get an empirical distribution of τ^*_j for $j = 1, ..., B$.

7. Calculate the bootstrap P-value as $p^*(\hat{\tau}) = 2min(\frac{1}{B}\sum_1^B \mathbf{1}[\tau^*_j < \hat{\tau}], \frac{1}{B}\sum_1^B \mathbf{1}[\tau^*_j > \hat{\tau}])$.

[59] Here we use the Rademacher weights, which have been shown to work well when the residuals are not too asymmetric. Other weights can be used.

E Complete Results for the Low Educated.

Table 8: Complete Estimations on Continuous Outcomes for the Low Educated

	second stage on continuous outcomes				first stage
Outcomes[‡]	log earnings		log hours worked		early non-empl.
	OLS	2SLS	OLS	2SLS	OLS
clustered standard errors:	$g*p$	$g*p$	$g*p$	$g*p$	$g*p$
	(1)	(2)	(3)	(4)	(5)
UR_grad					5.4615***
					(1.6848)
early non-empl.	-0.0269***	-0.1002***	-0.0203***	-0.0722***	
	(0.0040)	(0.0291)	(0.0029)	(0.0207)	
UR_pe6	0.4822**	0.4172**	0.3340**	0.2879**	0.3779
	(0.1904)	(0.1919)	(0.1382)	(0.1391)	(1.6071)
lin_grad_year	-0.1588	0.0469	-0.1140	0.0317	3.5329
	(0.2945)	(0.2995)	(0.2133)	(0.2190)	(2.2946)
lin_grad_year|trend>3	0.9033**	0.6407	0.6680**	0.4821	-4.4281
	(0.4002)	(0.4242)	(0.2914)	(0.3108)	(2.8479)
lin_grad_year|trend>6	-0.4604*	-0.4394	-0.3337*	-0.3188	3.9169
	(0.2582)	(0.2852)	(0.1866)	(0.2035)	(3.1378)
d_province2	-1.3812*	-1.2603	-0.9473*	-0.8617	3.6574
	(0.7107)	(0.9544)	(0.5039)	(0.6678)	(8.6769)
d_province3	-2.1752***	-2.9225***	-1.5767***	-2.1058***	-11.7452
	(0.6066)	(0.9315)	(0.4343)	(0.6701)	(8.9621)
d_province4	0.0841	0.1825	0.0743	0.1439	0.7495
	(0.4801)	(0.4983)	(0.3503)	(0.3567)	(4.7080)
d_province5	0.8991	0.9211	0.6684	0.6840	-15.1893**
	(0.6099)	(0.6721)	(0.4494)	(0.4864)	(6.6042)
lin_calend_year_prov2	0.0498	0.0410	0.0349	0.0287	0.0774
	(0.1386)	(0.1413)	(0.1018)	(0.1024)	(0.9931)
lin_calend_year_prov3	0.0723	0.1638	0.0587	0.1235	2.2624
	(0.1136)	(0.1441)	(0.0810)	(0.1039)	(1.7325)
lin_calend_year_prov4	-0.0946	-0.1524	-0.0685	-0.1094	-1.0198
	(0.1267)	(0.1477)	(0.0927)	(0.1051)	(1.2618)
lin_calend_year_prov5	-0.0813	-0.0760	-0.0595	-0.0557	1.8419
	(0.1067)	(0.1194)	(0.0795)	(0.0868)	(1.1761)
avg_UR_pe3-6	-1.2836***	-1.4348**	-0.9654***	-1.0725***	-0.3037
	(0.4509)	(0.5863)	(0.3261)	(0.4136)	(4.4775)
min_UR_pe0-6	-0.5718	-0.4848	-0.3121	-0.2505	-6.4742
	(0.6219)	(0.6613)	(0.4388)	(0.4659)	(7.1097)
birth cohort76	0.7043	-0.3299	0.5424	-0.1897	-13.2641***
	(0.5632)	(0.7686)	(0.4125)	(0.5581)	(3.6802)
birth cohort78	0.4261	-0.0504	0.3190	-0.0183	-5.9156**
	(0.3677)	(0.4687)	(0.2690)	(0.3408)	(2.5815)
live in single-parent	0.3453	0.7897	0.2736	0.5882	6.1392
	(0.4632)	(0.5207)	(0.3466)	(0.3838)	(4.1354)
not live with parents	0.4272*	0.5203	0.3483*	0.4142	1.0913
	(0.2431)	(0.3501)	(0.1792)	(0.2529)	(2.5611)
HH members aged 0-11	-0.0178	0.0845	-0.0478	0.0246	1.3007
	(0.1123)	(0.1472)	(0.0882)	(0.1069)	(1.1570)
HH members aged 12-17	0.1680	0.1467	0.1262	0.1111	-0.2429

Continued on next page

Table 8 – continued from previous page

Outcomes‡	log earnings		log hours worked		early non-empl.
	OLS	2SLS	OLS	2SLS	OLS
	(1)	(2)	(3)	(4)	(5)
	(0.1177)	(0.1473)	(0.0884)	(0.1087)	(0.9030)
HH members aged 18-29	0.0112	0.2054	0.0074	0.1449	2.6587**
	(0.1164)	(0.1561)	(0.0869)	(0.1133)	(1.0291)
HH members aged 30-64	-0.0333	-0.0800	-0.0109	-0.0439	-0.4836
	(0.4065)	(0.4287)	(0.3021)	(0.3128)	(3.2574)
HH members aged 65+	-0.0680	0.0308	-0.0288	0.0412	1.2468
	(0.3639)	(0.4196)	(0.2683)	(0.3078)	(3.1042)
father education	0.0011	0.0294	0.0000	0.0200	0.3816
	(0.0255)	(0.0330)	(0.0188)	(0.0240)	(0.2531)
mother education	-0.1053***	-0.0485	-0.0766***	-0.0364	0.7800**
	(0.0356)	(0.0432)	(0.0270)	(0.0317)	(0.2925)
years of delay in sec.edu	-0.0545	0.3485*	-0.0424	0.2429	5.4123***
	(0.1112)	(0.2094)	(0.0808)	(0.1509)	(1.1119)
technical edu	0.4431	-0.4556	0.3576	-0.2787	-11.8663***
	(0.3274)	(0.4733)	(0.2473)	(0.3399)	(2.9533)
vocational edu	0.4572	-0.3519	0.3683*	-0.2045	-10.6291***
	(0.2772)	(0.4516)	(0.2105)	(0.3232)	(3.3428)
apprenticeship/PT voc	-0.1879	-1.1339*	-0.1077	-0.7774*	-12.5300**
	(0.4530)	(0.6234)	(0.3375)	(0.4495)	(4.7050)
Constant	15.0563***	18.1333***	10.7753***	12.9534***	24.5592
	(3.7528)	(5.2570)	(2.6939)	(3.7253)	(49.3992)
Observations	1,902	1,902	1,902	1,902	1,902
R-squared	0.0895	-0.2401	0.0895	-0.2090	0.1070
F stat of first step§					10.51
Exogeneity test P-val†		0.00970		0.0113	

*** p<0.01, ** p<0.05, * p<0.1. Standard errors between parentheses. Columns 1-4 report the results from estimating (2) by OLS (odds columns) and 2SLS (even columns). Column 5 reports OLS results from estimating the first stage. All estimations report cluster robust standard errors by graduation year g and province of residence at graduation p ($G = 44$). Column 5 reports the F statistic of the first stage and even columns report the exogeneity test for early non-employment ($y^0_{igpt_1}$).

‡ Continuous outcomes are measured at potential experience 6; early non-emp-loyment is measured in the first 2.5 years after gradaution. For continuous outcomes we add value one before taking the logarithm, so that those who are not salaried employed at the moment of evaluation are included with outcomes=0 after the logarithmic transformation.

§ This statistic is not corrected for the problem of few clusters. The corrected value resulting from the bootstrap procedure is 9.25 (see table 1).

† With clustered standard errors, the exogeneity test is defined as the difference of two Sargan-Hansen statistics: one for the equation where y^0_{t1} is treated as endogenous and one for the equation where $y^0_{igpt_1}$ is treated as exogenous. Under the null that $y^0_{igpt_1}$ is exogenous, the statistic is distributed as $\chi^2(1)$. This statistic is not corrected for the problem of few clusters.

Table 9: Complete Estimations on Discrete Outcomes for the Low Educated

	second stage					
Outcomes:‡	salaried empl.		self-empl.		overall empl.	
	OLS	2SLS	OLS	2SLS	OLS	2SLS
clustered standard errors:	$g*p$	$g*p$	$g*p$	$g*p$	$g*p$	$g*p$
	(1)	(2)	(3)	(4)	(5)	(6)
early non-empl.	-0.00169***	-0.00256	0.00054	0.00248	-0.00115***	-0.00008
	(0.00041)	(0.00290)	(0.00041)	(0.00258)	(0.00025)	(0.00151)
UR_pe6	0.00481	0.00404	0.01081	0.01253	0.01562	0.01657*
	(0.02385)	(0.02209)	(0.01947)	(0.01765)	(0.01020)	(0.00999)
lin_grad_year	-0.00253	-0.00009	-0.01367	-0.01911	-0.01621	-0.01920
	(0.03120)	(0.03028)	(0.02544)	(0.02343)	(0.01397)	(0.01340)
lin_grad_year|trend>3	0.02993	0.02680	0.00338	0.01032	0.03331*	0.03712**
	(0.04020)	(0.04043)	(0.03029)	(0.02903)	(0.01832)	(0.01813)
lin_grad_year|trend>6	-0.01720	-0.01695	0.01884	0.01829	0.00164	0.00134
	(0.02568)	(0.02477)	(0.02503)	(0.02252)	(0.01457)	(0.01375)
d_province2	-0.22288**	-0.22144**	0.17666*	0.17347**	-0.04621	-0.04797
	(0.09844)	(0.09548)	(0.09049)	(0.08719)	(0.04174)	(0.03996)
d_province3	-0.17150***	-0.18038***	0.13845**	0.15820**	-0.03304	-0.02218
	(0.06193)	(0.06407)	(0.06588)	(0.06577)	(0.03720)	(0.04285)
d_province4	-0.11409**	-0.11292**	0.10736**	0.10477**	-0.00673	-0.00816
	(0.04892)	(0.04910)	(0.04459)	(0.04794)	(0.02450)	(0.02611)
d_province5	0.04132	0.04158	-0.06171	-0.06229	-0.02039	-0.02071
	(0.06183)	(0.06032)	(0.04557)	(0.04483)	(0.03418)	(0.03449)
lin_calend_year_prov2	0.01976	0.01966	-0.01544	-0.01521	0.00432	0.00445
	(0.01597)	(0.01524)	(0.01253)	(0.01214)	(0.00565)	(0.00651)
lin_calend_year_prov3	-0.00496	-0.00387	0.00501	0.00259	0.00005	-0.00128
	(0.01207)	(0.01140)	(0.01565)	(0.01376)	(0.00716)	(0.00690)
lin_calend_year_prov4	0.01299	0.01231	-0.01369	-0.01217	-0.00070	0.00014
	(0.01118)	(0.01157)	(0.00964)	(0.01055)	(0.00642)	(0.00679)
lin_calend_year_prov5	-0.00608	-0.00602	0.01656	0.01642*	0.01048**	0.01041**
	(0.01074)	(0.01027)	(0.01012)	(0.00967)	(0.00471)	(0.00491)
avg_UR_pe3-6	-0.08309**	-0.08489**	0.03333	0.03733	-0.04976*	-0.04756*
	(0.04062)	(0.04005)	(0.03418)	(0.03230)	(0.02622)	(0.02516)
min_UR_pe0-6	-0.01193	-0.01089	0.00452	0.00222	-0.00741	-0.00867
	(0.06247)	(0.05970)	(0.05849)	(0.05672)	(0.02769)	(0.02854)
birth cohort76	0.00488	-0.00741	0.01471	0.04204	0.01959	0.03462
	(0.04397)	(0.06402)	(0.03588)	(0.05434)	(0.02535)	(0.03517)
birth cohort78	0.02309	0.01743	-0.00086	0.01173	0.02223	0.02916
	(0.03666)	(0.04407)	(0.03059)	(0.03645)	(0.01745)	(0.02152)
live in single-parent	-0.00309	0.00220	-0.03561	-0.04736	-0.03870	-0.04516
	(0.05525)	(0.05471)	(0.05233)	(0.05472)	(0.03217)	(0.03371)
not live with parents	0.02521	0.02631	-0.01502	-0.01748	0.01019	0.00883
	(0.02900)	(0.02995)	(0.02753)	(0.03031)	(0.01581)	(0.01592)
HH members aged 0-11	-0.00246	-0.00124	0.00596	0.00325	0.00350	0.00201
	(0.01217)	(0.01251)	(0.01277)	(0.01265)	(0.00574)	(0.00627)
HH members aged 12-17	0.02291*	0.02265*	-0.01252	-0.01196	0.01039	0.01070
	(0.01185)	(0.01173)	(0.01041)	(0.01069)	(0.00709)	(0.00683)
HH members aged 18-29	0.00009	0.00240	0.00374	-0.00139	0.00384	0.00101
	(0.01152)	(0.01355)	(0.01034)	(0.01150)	(0.00604)	(0.00688)
HH members aged 30-64	-0.01697	-0.01752	-0.02276	-0.02152	-0.03972	-0.03904
	(0.05212)	(0.05087)	(0.04874)	(0.04896)	(0.02847)	(0.02961)

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Table 9 – continued from previous page

	salaried empl.		self-empl.		overall empl.	
	(1)	(2)	(3)	(4)	(5)	(6)
HH members aged 65+	-0.02001	-0.01884	0.00730	0.00469	-0.01271	-0.01414
	(0.04165)	(0.04136)	(0.03573)	(0.03540)	(0.02182)	(0.02205)
father education	-0.00132	-0.00099	0.00045	-0.00030	-0.00087	-0.00128
	(0.00198)	(0.00226)	(0.00202)	(0.00213)	(0.00160)	(0.00170)
mother education	-0.00971***	-0.00904**	0.00937***	0.00787**	-0.00034	-0.00116
	(0.00341)	(0.00417)	(0.00294)	(0.00327)	(0.00181)	(0.00210)
years of delay in sec.edu	0.00440	0.00919	-0.02391**	-0.03456*	-0.01952***	-0.02537*
	(0.01275)	(0.02067)	(0.01070)	(0.01764)	(0.00702)	(0.01381)
technical edu	0.01038	-0.00031	0.02957	0.05333	0.03995***	0.05301**
	(0.03694)	(0.04459)	(0.03180)	(0.04181)	(0.01452)	(0.02678)
vocational edu	0.00818	-0.00144	0.03776	0.05914	0.04594***	0.05770**
	(0.03164)	(0.04096)	(0.02700)	(0.04065)	(0.01546)	(0.02569)
apprenticeship/PT voc	-0.06240	-0.07366	0.08533*	0.11033**	0.02292	0.03667
	(0.05336)	(0.06364)	(0.04311)	(0.05434)	(0.02842)	(0.03967)
Constant	1.41749***	1.45409***	-0.15519	-0.23651	1.26230***	1.21757***
	(0.35242)	(0.37325)	(0.32471)	(0.34533)	(0.20910)	(0.22952)
Observations	1,902	1,902	1,902	1,902	1,902	1,902
R-squared	0.04170	0.03730	0.03098	0.00332	0.05818	0.03540
Exogeneity test P-val†		0.767		0.438		0.467

*** p<0.01, ** p<0.05, * p<0.1. Standard errors between parentheses. Columns 1-6 report the results from estimating (2) by OLS (odds columns) and 2SLS (even columns). The first stage regression is reported in Table 8 (Column 5). All estimations report cluster robust standard errors by graduation year g and province of residence at graduation p ($G = 44$). Even columns report the exogeneity test for early non-employment ($y^0_{igpt_1}$).
‡ The discrete outcomes are measured at potential experience 6.
† With clustered standard errors, the exogeneity test is defined as the difference of two Sargan-Hansen statistics: one for the equation where y^0_{t1} is treated as endogenous and one for the equation where $y^0_{igpt_1}$ is treated as exogenous. Under the null that $y^0_{igpt_1}$ is exogenous, the statistic is distributed as $\chi^2(1)$. This statistic is not corrected for the problem of few clusters.

F Sensitivity Analysis For the Low Educated

Table 10: Effect of Interest for the Low Educated: Graduation Period 1998-2002.

Panel A: Effect of early non-employment in the structural equation:					
		OLS		2SLS	
	standard errors†	robust	cluster $g*p$	robust	cluster $g*p$
Continuous outcomes:‡‡		(1)	(2)	(3)	(4)
log earnings	coeff	-0.0287***	-0.0287***	-0.1406**	-0.1406**
	se	(0.0045)	(0.0049)	(0.0666)	(0.0597)
	P-val§		6.09E-06		0.0273
	Bootstrap P-val‡		0		0.0821
	Exogeneity test P-val§§				0.0306
log hours worked	coeff	-0.0215***	-0.0215***	-0.1033**	-0.1033**
	se	(0.0034)	(0.0035)	(0.0492)	(0.0439)
	P-val		3.19E-06		0.0277
	Bootstrap P-val		0		0.0781
	Exogeneity test P-val				0.0318

Panel B: Effect of the instrument in the first stage : OLS			
outcome:	*standard errors:*	robust	cluster (g*p)
early non-empl.(% hours)	coeff	11.9484***	11.9484***
	se	(3.4994)	(3.4918)
	P-val	0.00233	
	Bootstrap P-val	0.07007	
	F stat	11.70921	
	Bootstrap F stat††	3.60923	

*** p<0.01, ** p<0.05, * p<0.1. Standard errors between parentheses. Panel A reports results from estimating β in (2) on continuous outcomes measured at potential experience 6. β is the effect of one *pp* increase in $y^0_{igpt_1}$, i.e. the % of hours spent in non-employment in the first 2.5 years after graduation relative to potential total hours if one would work full-time during the whole period. For clustered standard errors, we report the P-value and the wild bootstrap P-value. Column 1-2 (3-4) show OLS (2SLS). In 2SLS the provincial unemployment rate at graduation is used as instrument for $y^0_{igpt_1}$. Panel B shows the effect of the instrument on $y^0_{igpt_1}$ in the first stage and the corresponding F statistic.

‡‡ For continuous outcomes we add value one before taking the log, so that non-salaried employed at the moment of evaluation are included with outcomes=0 after the logarithmic transformation.

† Robust indicates heteroscedastic-robust standard errors; clusters are defined by year g and province of residence at graduation p (G=24 clusters).

§ The P-value from clustered standard errors is computed using the $t(G-1)$ distribution, with G=24 (stars are reported accordingly).

‡ Bootstrap P-values are computed according to the wild bootstrap procedures explained in Appendix D for 999 repetitions.

†† Bootstrap F statistic is the F statistic corresponding to the bootstrap P-value of the t statistic of the instrument: we rely on the equivalence between F and t distribution: $t^2(G-1) = F(1, G-1)$, with $G = 24$.

§§ With clustered standard errors, this test is defined as the difference of two Sargan-Hansen statistics: one for the equation where y^0_{t1} is treated as endogenous and one for the equation where $y^0_{igpt_1}$ is treated as exogenous. Under the null that $y^0_{igpt_1}$ is exogenous, the statistic is distributed as $\chi^2(1)$.

Table 11: Complete OLS Estimations for the Low Educated: Graduation Period 1994-2002 vs 1998-2002; Full vs Restricted Specification (excluding some individual controls).

Continuous outcomes‡‡:	log earnings			log hours worked		
	g94-02§		g98-02†	g94-02		g98-02
	full spec.§§	restricted spec.††		full spec.	restricted spec.	
	(1)	(2)	(3)	(4)	(5)	(6)
cluster	$g*p$	$g*p$	$g*p$	$g*p$	$g*p$	$g*p$
early non-empl	-0.027***	-0.027***	-0.029***	-0.020***	-0.020***	-0.022***
	(0.004)	(0.004)	(0.005)	(0.003)	(0.003)	(0.004)
UR_pe6	0.482**	0.617***	0.594	0.334**	0.432***	0.446
	(0.190)	(0.179)	(0.544)	(0.138)	(0.130)	(0.393)
lin_grad_year	-0.159	-0.114	0.812*	-0.114	-0.082	0.636*
	(0.294)	(0.284)	(0.460)	(0.213)	(0.205)	(0.333)
lin_grad_year|trend>3	0.903**	0.661*	-1.032	0.668**	0.492*	-0.797
	(0.400)	(0.360)	(0.701)	(0.291)	(0.260)	(0.503)
lin_grad_year|trend>6	-0.460*			-0.334*		
	(0.258)			(0.187)		
avg_UR_pe3-6	-1.284***	-0.826**	-1.879	-0.965***	-0.634**	-1.485
	(0.451)	(0.394)	(1.373)	(0.326)	(0.289)	(0.993)
min_UR_pe0-6	-0.572	-0.692	-3.139***	-0.312	-0.398	-2.203**
	(0.622)	(0.590)	(1.118)	(0.439)	(0.419)	(0.791)
d_province2	-1.381*	-0.913	-4.627***	-0.947*	-0.611	-3.214**
	(0.711)	(0.704)	(1.597)	(0.504)	(0.503)	(1.169)
d_province3	-2.175***	-1.756***	-4.127**	-1.577***	-1.275***	-3.026**
	(0.607)	(0.547)	(1.740)	(0.434)	(0.392)	(1.238)
d_province4	0.084	-0.109	-1.617	0.074	-0.069	-1.116
	(0.480)	(0.456)	(1.380)	(0.350)	(0.333)	(0.984)
d_province5	0.899	0.741	1.125	0.668	0.553	0.741
	(0.610)	(0.609)	(1.902)	(0.449)	(0.451)	(1.397)
lin_calend_year_prov2	0.050	0.075	0.126	0.035	0.055	0.064
	(0.139)	(0.144)	(0.307)	(0.102)	(0.106)	(0.228)
lin_calend_year_prov3	0.072	0.147	-0.135	0.059	0.114	-0.106
	(0.114)	(0.104)	(0.137)	(0.081)	(0.075)	(0.094)
lin_calend_year_prov4	-0.095	-0.007	-0.120	-0.069	-0.004	-0.102
	(0.127)	(0.109)	(0.304)	(0.093)	(0.080)	(0.219)
lin_calend_year_prov5	-0.081	-0.102	-0.078	-0.059	-0.075	-0.041
	(0.107)	(0.107)	(0.357)	(0.079)	(0.080)	(0.263)
birth cohort76	0.704	0.813	0.816	0.542	0.622	0.625
	(0.563)	(0.510)	(0.599)	(0.412)	(0.373)	(0.437)
birth cohort78	0.426	0.487	0.471	0.319	0.364	0.353
	(0.368)	(0.350)	(0.353)	(0.269)	(0.255)	(0.256)
HH members aged 0-11	-0.018	0.016	0.005	-0.048	-0.022	-0.028
	(0.112)	(0.116)	(0.136)	(0.088)	(0.091)	(0.103)
father education	0.001	-0.002	-0.017	0.000	-0.003	-0.014
	(0.026)	(0.025)	(0.041)	(0.019)	(0.019)	(0.031)
mother education	-0.105***	-0.100***	-0.103**	-0.077***	-0.073***	-0.074**
	(0.036)	(0.035)	(0.045)	(0.027)	(0.026)	(0.033)
years of delay in sec.edu	-0.055	-0.143	-0.190	-0.042	-0.106	-0.140
	(0.111)	(0.096)	(0.166)	(0.081)	(0.071)	(0.120)
general edu		-0.449	-0.161		-0.362	-0.145

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Continuous outcomes‡‡:	log earnings			log hours worked		
	g94-02§		g98-02†	g94-02		g98-02
	full spec.§§	restricted spec.††		full spec.	restricted spec.	
	(1)	(2)	(3)	(4)	(5)	(6)
		(0.299)	(0.353)		(0.227)	(0.266)
live in single-parent	0.345			0.274		
	(0.463)			(0.347)		
not live with parents	0.427*			0.348*		
	(0.243)			(0.179)		
HH members aged 12-17	0.168			0.126		
	(0.118)			(0.088)		
HH members aged 18-29	0.011			0.007		
	(0.116)			(0.087)		
HH members aged 30-64	-0.033			-0.011		
	(0.406)			(0.302)		
HH members aged 65+	-0.068			-0.029		
	(0.364)			(0.268)		
technical edu	0.443			0.358		
	(0.327)			(0.247)		
vocational edu	0.457			0.368*		
	(0.277)			(0.210)		
apprenticeship/PT voc	-0.188			-0.108		
	(0.453)			(0.337)		
Constant	15.056***	12.907***	29.988***	10.775***	9.279***	22.077***
	(3.753)	(3.432)	(7.716)	(2.694)	(2.453)	(5.519)
Observations	1,902	1,902	946	1,902	1,902	946
R-squared	0.090	0.084	0.097	0.090	0.084	0.098

*** p<0.01, ** p<0.05, * p<0.1. Standard errors between parentheses. Columns 1 and 4 estimate (2) by OLS considering the graduation period 1994-2002: they are equivalent to the estimations reported in columns 1 and 3 of Table 8. Columns 2 and 5 estimate the restricted specification discussed for the sensitivity exercise in Section 5.1, based on the graduation period 1994-2002. Columns 3 and 6 estimate the same restricted specification on the graduation period 1998-2002, which is used in the second sensitivity analysis. Standard errors are clustered by graduation year g and province of living at graduation p.

§ graduation period 1994-2002 considered.

† graduation period 1998-2002 considered: for this reason, the third graduation year spline $lin\_grad\_year|trend > 6$ is omitted.

‡‡ For continuous outcomes we add value one before taking the log, so that non-salaried employed at the moment of evaluation are included with outcomes=0 after the logarithmic transformation.

§§ The full specification corresponds to equation (2).

†† The restricted specification is the one used in the second sensitivity analysis in Section 5.1.

Table 12: Effect of Interest for the Low Educated Excluding $\overline{UR}_p$ and $minUR_{pt}$ from the Specification.

Panel A: Effect of early non-employment in the structural equation:					
		OLS		2SLS	
	standard errors†	robust	cluster $g*p$	robust	cluster $g*p$
outcomes:		(1)	(2)	(3)	(4)
salaried employment	coeff	-0.00169***	-0.00169***	-0.00199	-0.00199
	se	(0.00034)	(0.00041)	(0.00387)	(0.00348)
	P-val§		0.00019		0.57020
	Bootstrap P-val‡		0.00000		0.62462
	Exogeneity test P-val§§			0.937	0.931
self-employment	coeff	0.00054*	0.00054	0.00219	0.00219
	se	(0.00030)	(0.00041)	(0.00340)	(0.00313)
	P-val		0.19253		0.48668
	Bootstrap P-val		0.19219		0.52853
	Exogeneity test P-val		0.619		0.587
overall employment	coeff	-0.00115***	-0.00115***	0.00020	0.00020
	se	(0.00021)	(0.00025)	(0.00229)	(0.00164)
	P-val		0.00005		0.90232
	Bootstrap P-val		0.00000		0.91291
	Exogeneity test P-val			0.540	0.386
log earnings	coeff	-0.0269***	-0.0269***	-0.0947**	-0.0947***
	se	(0.0033)	(0.0040)	(0.0447)	(0.0354)
	P-val		2.78E-08		0.0105
	Bootstrap P-val		0.0000		0.0300
	Exogeneity test P-val			0.0796	0.0361
log hours worked	coeff	-0.0203***	-0.0203***	-0.0666**	-0.0666***
	se	(0.0024)	(0.0029)	(0.0326)	(0.0251)
	P-val		1.00E-08		0.0111
	Bootstrap P-val		0.0000		0.0340
	Exogeneity test P-val			0.108	0.0483

Panel B: Effect of the instrument from the first stage (OLS)			
outcome‡‡*:*	*standard errors:*	robust	cluster (g*p)
early non-empl.	coeff	5.0319***	5.0319***
	se	(1.6519)	(1.7139)
	P-val		0.0053
	Bootstrap P-val		0.0120
	F stat	9.279	8.620
	Bootstrap F stat††		5.84

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*** p<0.01, ** p<0.05, * p<0.1. Standard errors between parentheses. Panel A reports results from estimating β in (2) on outcomes measured at potential experience 6, excluding $\overline{UR}_p$ and $minUR_{pt}$. β is the effect of one *pp* increase in $y^0_{igpt_1}$, i.e. the % of hours spent in non-employment in the first 2.5 years after graduation relative to potential total hours if one would work full-time during the whole period. For clustered standard errors, we report the P-value and the wild bootstrap P-value. Column 1-2 (3-4) show OLS (2SLS). In 2SLS the provincial unemployment rate at graduation is used as instrument for $y^0_{igpt_1}$. Panel B reports the effect of the instrument on $y^0_{igpt_1}$ in the first stage and the F statistic. For continuous outcomes we add value one before taking the log, so that non-salaried employed at the moment of evaluation are included with outcomes=0 after the logarithmic transformation.

† Robust indicates heteroscedastic-robust standard errors; clusters are defined by year g and province of residence at graduation p (G=44 clusters).

§ The P-value from clustered standard errors is computed using the $t(G-1)$ distribution, with G=44 (stars are reported accordingly).

‡ Bootstrap P-values are computed according to the wild bootstrap procedures explained in Appendix D for 999 repetitions.

†† Bootstrap F statistic is the F statistic corresponding to the Bootstrap P-value of the t statistic of the instrument: we rely on the equivalence between F and t distribution: with $G = 44$, $t^2(G-1) = F(1, G-1)$.

§§ With clustered standard errors, this test is defined as the difference of two Sargan-Hansen statistics: one for the equation where y^0_{t1} is treated as endogenous and one for the equation where $y^0_{igpt_1}$ is treated as exogenous. Under the null that $y^0_{igpt_1}$ is exogenous, the statistic is distributed as $\chi^2(1)$.

G Reduced Form Estimations for the Low Educated:

Table 13: Complete Estimations of Reduced Form and First Stage for the Low Educated.

	Reduced Form (ITT effects)					1^{st} *stage*
Outcomes:§§	log earnings	log hours worked	salaried	self	overall empl.	early non-empl.
	(1)	(2)	(3)	(4)	(5)	(6)
clustered standard errors:	$g*p$	$g*p$	$g*p$	$g*p$	$g*p$	$g*p$
UR_grad	-0.5475***	-0.3945***	-0.01400	0.01355	-0.00045	5.4615***
	(0.1662)	(0.1223)	(0.01675)	(0.01365)	(0.00851)	(1.6848)
P-val§	0.00198	0.00239	0.40799	0.32666	0.95777	0.00230
Bootstrap P-val‡	0.00801	0.00601	0.45646	0.36036	0.96496	0.00400
UR_pe6	0.3793*	0.2606*	0.00307	0.01347	0.01654	0.3779
	(0.1898)	(0.1390)	(0.02343)	(0.01867)	(0.01025)	(1.6071)
lin_grad_year	-0.3072	-0.2235	-0.00914	-0.01035	-0.01949	3.5329
	(0.3349)	(0.2428)	(0.03309)	(0.02628)	(0.01455)	(2.2946)
lin_grad_year|trend>3	1.0846**	0.8020**	0.03816	-0.00066	0.03749*	-4.4281
	(0.4259)	(0.3118)	(0.04150)	(0.02987)	(0.01878)	(2.8479)
lin_grad_year|trend>6	-0.8321***	-0.6017***	-0.02699	0.02800	0.00101	3.9169
	(0.2976)	(0.2193)	(0.02719)	(0.02393)	(0.01559)	(3.1378)
d_province2	-1.6270*	-1.1259*	-0.23081**	0.18254*	-0.04827	3.6574
	(0.8282)	(0.5902)	(0.10284)	(0.09202)	(0.04254)	(8.6769)
d_province3	-1.7451*	-1.2573*	-0.15028**	0.12907*	-0.02120	-11.7452
	(0.8831)	(0.6340)	(0.06874)	(0.06645)	(0.03691)	(8.9621)
d_province4	0.1073	0.0898	-0.11484**	0.10662**	-0.00822	0.7495
	(0.5737)	(0.4196)	(0.05039)	(0.04570)	(0.02701)	(4.7080)
d_province5	2.4438**	1.7812**	0.08051	-0.09996	-0.01945	-15.1893**
	(0.9123)	(0.6716)	(0.09174)	(0.07501)	(0.03869)	(6.6042)
lin_calend_year_prov2	0.0333	0.0231	0.01946	-0.01501	0.00444	0.0774
	(0.1463)	(0.1076)	(0.01658)	(0.01269)	(0.00669)	(0.9931)
lin_calend_year_prov3	-0.0630	-0.0400	-0.00967	0.00821	-0.00147	2.2624
	(0.1586)	(0.1134)	(0.01455)	(0.01646)	(0.00668)	(1.7325)
lin_calend_year_prov4	-0.0502	-0.0358	0.01492	-0.01469	0.00023	-1.0198
	(0.1322)	(0.0980)	(0.01172)	(0.00997)	(0.00685)	(1.2618)
lin_calend_year_prov5	-0.2607*	-0.1888*	-0.01074	0.02099	0.01025*	1.8419
	(0.1436)	(0.1064)	(0.01490)	(0.01350)	(0.00532)	(1.1761)
avg_UR_pe3-6	-1.4044***	-1.0505***	-0.08411**	0.03657	-0.04753*	-0.3037
	(0.4377)	(0.3218)	(0.04124)	(0.03380)	(0.02560)	(4.4775)
min_UR_pe0-6	0.1642	0.2171	0.00570	-0.01383	-0.00813	-6.4742
	(0.8643)	(0.6086)	(0.06138)	(0.05224)	(0.02860)	(7.1097)
birth cohort76	0.9998	0.7684*	0.02659	0.00914	0.03572	-13.2641***
	(0.6088)	(0.4473)	(0.04683)	(0.03551)	(0.02615)	(3.6802)
birth cohort78	0.5426	0.4090	0.03259	-0.00294	0.02965	-5.9156**
	(0.4035)	(0.2949)	(0.03871)	(0.03095)	(0.01872)	(2.5815)
live in single-parent	0.1743	0.1447	-0.01354	-0.03213	-0.04567	6.1392
	(0.4770)	(0.3578)	(0.05554)	(0.05226)	(0.03403)	(4.1354)
not live with parents	0.4109*	0.3354**	0.02352	-0.01477	0.00874	1.0913
	(0.2252)	(0.1661)	(0.02742)	(0.02702)	(0.01600)	(2.5611)
HH members aged 0-11	-0.0459	-0.0693	-0.00457	0.00648	0.00191	1.3007
	(0.1194)	(0.0950)	(0.01213)	(0.01265)	(0.00581)	(1.1570)
HH members aged 12-17	0.1711	0.1287	0.02328**	-0.01256	0.01072	-0.2429
	(0.1152)	(0.0866)	(0.01143)	(0.01027)	(0.00697)	(0.9030)

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Table 13 – continued from previous page

	Reduced Form (ITT effects)					1^{st} stage
Outcomes:§§	log earnings	log hours worked	salaried	self	overall empl.	early non-empl.
	(1)	(2)	(3)	(4)	(5)	(6)
HH members aged 18-29	-0.0612	-0.0472	-0.00441	0.00521	0.00079	2.6587**
	(0.1209)	(0.0904)	(0.01253)	(0.01090)	(0.00622)	(1.0291)
HH members aged 30-64	-0.0316	-0.0090	-0.01628	-0.02272	-0.03900	-0.4836
	(0.4278)	(0.3192)	(0.05313)	(0.04852)	(0.03035)	(3.2574)
HH members aged 65+	-0.0942	-0.0489	-0.02203	0.00779	-0.01425	1.2468
	(0.3784)	(0.2784)	(0.04265)	(0.03599)	(0.02242)	(3.1042)
father education	-0.0089	-0.0075	-0.00196	0.00065	-0.00132	0.3816
	(0.0268)	(0.0198)	(0.00202)	(0.00203)	(0.00160)	(0.2531)
mother education	-0.1267***	-0.0927***	-0.01104***	0.00981***	-0.00123	0.7800**
	(0.0393)	(0.0297)	(0.00364)	(0.00299)	(0.00179)	(0.2925)
years of delay in sec.edu	-0.1941	-0.1481	-0.00468	-0.02114**	-0.02582***	5.4123***
	(0.1216)	(0.0888)	(0.01336)	(0.01037)	(0.00802)	(1.1119)
technical edu	0.7340**	0.5785**	0.03010	0.02389	0.05400***	-11.8663***
	(0.3559)	(0.2693)	(0.03912)	(0.03184)	(0.01708)	(2.9533)
vocational edu	0.7136**	0.5634**	0.02580	0.03278	0.05858***	-10.6291***
	(0.3132)	(0.2384)	(0.03306)	(0.02674)	(0.01792)	(3.3428)
apprenticeship/PT voc	0.1222	0.1277	-0.04154	0.07925*	0.03771	-12.5300**
	(0.4961)	(0.3700)	(0.05738)	(0.04296)	(0.03086)	(4.7050)
Constant	15.6713***	11.1794***	1.39113***	-0.17560	1.21553***	24.5592
	(4.5909)	(3.3431)	(0.38766)	(0.33394)	(0.22040)	(49.3992)
Observations	1,902	1,902	1,902	1,902	1,902	1,902
R-squared	0.0484	0.0469	0.02537	0.02905	0.03172	0.1070
F stat of first step						10.51
Bootstrap F stat†						9.25

*** p<0.01, ** p<0.05, * p<0.1. Standard errors between parentheses. Column 1-6 report the OLS result from estimating the reduced form equation of (2), i.e. where $y^0_{igpt_1}$ is replaced by the unemployment rate at graduation: the coefficients of the unemployment rate at graduation on the outcomes represent the Intention-To-Treat (ITT) effects. Column 7 reports the first stage regression, where y_{iT} is replaced by $y^0_{igpt_1}$ and $y^0_{igpt_1}$ is replaced by the unemployment rate at graduation. In all cases, for the coefficient of the unemployment rate at graduation we report the P-value and the wild bootstrap P-value.

§§ The outcomes of interest (Column 1-5) are measured at potential experience 6. The dependent variable in the first stage (Column 6) is measured in the first 2.5 years after graduation. For continuous outcomes (Column 1-2) we add value one before taking the log, so that non-salaried employed at the moment of evaluation are included with outcomes=0 after the logarithmic transformation.

§ The P-value from clustered standard errors is computed using the $t(G-1)$ distribution, with G=44 (stars are reported accordingly).

‡ Bootstrap P-values are computed according to the wild bootstrap procedures explained in Appendix D for 999 repetitions.

† Bootstrap F statistic is the F statistic corresponding to the Bootstrap P-value of the t statistic of the instrument: we rely on the equivalence between F and t distribution: $t^2(G-1) = F(1, G-1)$ with $G = 44$.

H The Analysis for the High Educated

For the high educated we use the average hourly wages earned in the first 2.5 years since graduation as endogenous regressor, since the high educated are persistently damaged by adverse labor market conditions at graduation because they accept lower-paying jobs (Cockx and Ghirelli, 2014). Accordingly, early wages should be the relevant channel through which the instrument affects the outcomes of interest (exclusion restriction). In this case we have to restrict the sample to the graduation period 1998-2004, since we do not observe early wages for those graduating before 1998: this leaves us with a sample of 35 clusters (7 graduation years times 5 provinces). The size of the clusters is reported in Table 14.

As outcome of interest we use the log of wage at potential experience 6: therefore, the dependent variable is missing for those who were not salaried employed at the time of the evaluation, as the log-transformation is not defined (13% of the sample). The endogenous regressor instead is expressed in level; [60] similarly as for the dependent variable, it is missing for individuals who never earned a wage in the first 2.5 years since graduation (6.5% of the sample). Hence, the estimates should be interpreted as semi-elasticities, conditional on being salaried employed both 6 years after graduation and in the early period. Of course, conditional effects may be biased if the sub-sample of salaried employed is selected: we do not tackle this problem. Descriptive statistics of the individual controls and the endogenous variables are reported in Table 15. Note that, since high educated graduate in the period 1998-2004, the last graduation cohort is followed until potential experience 6. Later than that this sample gets smaller as the last graduation cohorts progressively drop out from the sample.

For the high educated, we study the long-term effect of early wages on subsequent wages: therefore, we expect a positive β and the persistence of early wages on subsequent labor market performances will be considered as evidence of scarring. In this case, we do not expect measurement error since we restrict the sample to the graduation period 1998-2004 and measure early wage only exploiting administrative data. Ability and returns to job search are positively correlated with both early wages and the outcomes of interest, thereby causing a positive bias. Liquidity constraints are instead negatively correlated with both early wages and the outcomes of interest, which also lead to a positive bias. Accordingly, in any case we expect OLS to overestimate β.

As for the low educated, also here we tackle the endogeneity problem of early wages with an IV approach where the unemployment rate at graduation is used as instrument for the endogenous regressor. In this case the exclusion restriction imposes that the long-term penalties of graduating in downturns are uniquely explained by the acceptance of lower-paying jobs early in the career. In this case, persistence would arise by the accumulation of human capital specific to lower-paying jobs or by the foregone human capital that one would have accumulated in a

[60] Transforming $y^0_{igpt_1}$ as $log(y^0_{igpt_1})$ yielded a lower F statistic. Hence, we decided to express $y^0_{igpt_1}$ in level.

higher-paying job. Of course things can be a bit more blurry if we consider a wider definition of reservation wage which also includes non-pecuniary dimensions: then, high educated would be forced to accept lower quality jobs, such as temporary jobs or seasonal jobs, which may entail not only lower wages but also unemployment spells upon termination of the contract. Under heterogenous effects, the monotonicity assumption requires that a higher unemployment rate at graduation makes every high educated person earning a higher (or lower) early wage.

In table 16, the bootstrap F statistic of the first stage is 2 which warns against the problem of weak instrument. For one *pp* increase in the unemployment rate at graduation, the average wage in the first 2.5 years since graduation decreases by 0.22 Euros. In case of weak instrument, the 2SLS is biased towards the OLS. Both OLS and 2SLS estimates show the expected sign: a higher early wage is associated with better labor market outcomes 6 years after graduation. The estimate is highly significant for OLS, which in principle could be due to endogeneity or causality. Unfortunately, in this case our IV approach is not effective to identify the latter.

Table 14: Number of Individuals by graduation year and province of residence at graduation.

High educated						
grad_year	prov1	prov2	prov3	prov4	prov5	Total
1998	49	29	50	44	17	189
1999	55	54	61	63	37	270
2000	74	66	42	63	31	276
2001	61	39	44	48	33	225
2002	88	53	52	52	32	277
2003	53	35	24	32	26	170
2004	31	19	16	24	13	103
Total	457	319	327	361	220	1684

The analysis considers the graduation period 1998-2004 for the high educated. Provinces are in the following order from 1 to 5: Antwerp, Flemish Brabant, Western Flanders, Eastern Flanders, Limburg. Each combination of graduation year and province of residence at graduation represents a cluster *gp* in the main analysis.

Table 15: Descriptive Statistics for the High Educated

Individual Control Variables:						
Variable	Obs	Mean	Std. Dev.	Min	Max	label
birth cohort76	1684	0.357	0.479	0	1	1 if born in 1976
birth cohort78	1684	0.328	0.470	0	1	1 if born in 1978
birth cohort80	1684	0.315	0.465	0	1	1 if born in 1980
live in single-parent	1684	0.078	0.268	0	1	1 if live with single parent at age17(Dec)
not live with parents	1684	0.027	0.161	0	1	1 if not live with either parents at age17(Dec)
HH members aged 0-11	1684	0.156	0.427	0	3	nr of other HH members aged0-11 at age17(Dec)
HH members aged 12-17	1684	0.573	0.683	0	4	nr of other HH members aged12-17 at age17(Dec)
HH members aged 18-29	1684	0.582	0.715	0	4	nr of other HH members aged18-29 at age17(Dec)
HH members aged 30-64	1684	1.929	0.298	1	4	nr of other HH members aged30-64 at age17(Dec)
HH members aged 65+	1684	0.030	0.188	0	2	nr of other HH members aged65+ at age17(Dec)
father education	1684	6.935	3.273	0	13	father completed education since age12
mother education	1684	6.322	2.947	0	13	mother completed education since age12
years of delay in sec.edu	1684	0.260	0.539	-1	3	years of delay at age17(Aug)
general education	1684	0.633	0.482	0	1	1 if general edu at age17(Aug)
technical education	1684	0.357	0.479	0	1	1 if technical edu at age17(Aug)
vocational education	1684	0.009	0.094	0	1	1 if vocational edu at age17(Aug)
apprenticeship/PT voc.	1684	0.001	0.024	0	1	1 if apprenticeship/PT voc. edu at age17(Aug)
Endogenous Variables§:						
log hourly wage	1280	2.852	0.234	2.174	3.493	log hourly wage in salaried empl.
avg_early_wage	1280	13.566	2.560	8.014	31.375	average hourly wage in salaried empl.

Descriptive statistics are reported for the high educated graduating in the period 1998-2004.
§ "Log hourly wage" is measured at potential experience 6, whereas "*avg_early_wage*" is measured in the first 2.5 years since graduation. Both endogenous variables are conditional on being salaried employed at potential experience 6 and in the first 2.5 years after graduation. Hourly wages are computed as annual earnings divided by the annual hours worked in salaried employed. The outcome of interest ("Log hourly wage") is expressed in logarithm whereas the endogenous variable ("*avg_early_wage*") is expressed in level.

Table 16: Complete Estimations on Hourly Wages for the High Educated

	second stage		first stage
outcomes†	log hourly wage		avg_early_wage
	OLS	2SLS	OLS
clustered se	$g*p$	$g*p$	$g*p$
	(1)	(2)	(3)
UR_grad			-0.2239*
			(0.1131)
avg_early_wage	0.0397***	0.0626	
	(0.0040)	(0.0430)	
P-val‡	1.60E-11	0.1547	
Bootstrap P-val§	0	0.3784	
UR_pe6	-0.0096	-0.0061	-0.1125
	(0.0106)	(0.0105)	(0.1264)
lin_grad_year	0.0262	0.0200	0.1559
	(0.0166)	(0.0216)	(0.1757)
lin_grad_year|trend>3	0.0054	-0.0025	0.5491**
	(0.0170)	(0.0220)	(0.2493)
d_province2	-0.0244	0.0135	-1.8109**
	(0.0556)	(0.0835)	(0.7469)
d_province3	0.1924**	0.1816**	0.3435
	(0.0890)	(0.0902)	(0.6809)
d_province4	0.0388	0.0944	-2.1026***
	(0.0782)	(0.1182)	(0.7124)
d_province5	0.0980	0.1026	0.0096
	(0.0982)	(0.0924)	(0.4239)
lin_calend_year_prov2	-0.0069	-0.0091	0.0963
	(0.0088)	(0.0089)	(0.0853)
lin_calend_year_prov3	-0.0387**	-0.0335*	-0.2369*
	(0.0162)	(0.0192)	(0.1293)
lin_calend_year_prov4	-0.0127	-0.0187	0.2210**
	(0.0112)	(0.0143)	(0.0926)
lin_calend_year_prov5	-0.0113	-0.0106	-0.0520
	(0.0129)	(0.0118)	(0.0559)
avg_UR_pe3-6	-0.0124	-0.0242	0.4844
	(0.0306)	(0.0357)	(0.3852)
min_UR_pe0-6	-0.0570	-0.0285	-1.1750**
	(0.0547)	(0.0793)	(0.4599)
birth cohort76	0.1144***	0.0790	1.5638***
	(0.0301)	(0.0724)	(0.3043)
birth cohort78	0.0527***	0.0313	0.9167***
	(0.0154)	(0.0409)	(0.1876)
live in single-parent	0.0091	0.0241	-0.6719
	(0.0527)	(0.0537)	(0.5381)
not live with parents	0.0083	0.0175	-0.4004
	(0.0417)	(0.0458)	(0.2521)
HH members aged 0-11	-0.0031	-0.0032	0.0091
	(0.0154)	(0.0151)	(0.1056)
HH members aged 12-17	0.0030	-0.0003	0.1442
	(0.0096)	(0.0110)	(0.1028)
HH members aged 18-29	0.0080	0.0081	0.0048
	(0.0083)	(0.0084)	(0.1017)

Continued on next page

Table 16 – continued from previous page

	second stage log hourly wage		first stage avg_early_wage
	OLS	2SLS	OLS
	(1)	(2)	(3)
HH members aged 30-64	0.0014	0.0150	-0.6153
	(0.0397)	(0.0392)	(0.5187)
HH members aged 65+	0.0213	0.0257	-0.2119
	(0.0288)	(0.0293)	(0.2499)
father education	0.0060**	0.0055*	0.0231
	(0.0024)	(0.0030)	(0.0234)
mother education	-0.0001	-0.0002	0.0014
	(0.0028)	(0.0028)	(0.0279)
years of delay in sec.edu	-0.0412***	-0.0241	-0.7543***
	(0.0133)	(0.0332)	(0.1078)
technical edu	-0.0320**	-0.0252*	-0.2940
	(0.0131)	(0.0145)	(0.1808)
apprenticeship/PT voc	-0.1069**	-0.1115**	0.2022
	(0.0511)	(0.0494)	(0.5282)
Constant	2.5052***	2.1346***	17.3247***
	(0.2562)	(0.7360)	(3.0029)
Observations	1,280	1,280	1,280
R-squared	0.2979	0.2433	0.1244
F stat of first step			3.920
Bootstrap F stat of first step§§			2.0824
Exogeneity test P-val††		0.614	

*** p<0.01, ** p<0.05, * p<0.1. Standard errors between parentheses. Columns 1 and 2 report the results from estimating (2) by OLS and 2SLS, respectively; the outcome is measured at potential experience 6. Column 3 reports the first stage regression, where the dependent variable is measured in the first 2.5 years since graduation. All estimations report cluster robust standard errors by graduation year g and province of residence at graduation p ($G = 35$). The sample is restricted to individuals who are salaried employed 6 years after graduation and in the first 2.5 years after graduation. Hence, the estimation reports conditional effects.

† Hourly wages are computed as annual earnings divided by the annual hours worked in salaried employed. The outcome of interest is expressed in logarithm whereas the endogenous variable is expressed in level.

‡ The P-value from clustered standard errors is computed using the $t(G-1)$ distribution, with G=35 (stars of the corresponding coefficient are reported accordingly).

§ Bootstrap P-values are computed according to the wild bootstrap procedures explained in Appendix D for 999 repetitions.

§§ Bootstrap F statistic is the F statistic corresponding to the Bootstrap P-value of the t statistic of the instrument: we rely on the equivalence between F and t distribution: $t^2(G-1) = F(1, G-1)$, with $G = 35$.

†† With clustered standard errors, the exogeneity test is defined as the difference of two Sargan-Hansen statistics: one for the equation where y^0_{t1} is treated as endogenous, and one for the equation where $y^0_{igpt_1}$ is treated as exogenous. Under the null that $y^0_{igpt_1}$ is exogenous, the statistic is distributed as $\chi^2(1)$. This statistic is not corrected for the problem of few clusters.

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