Empirical Economics
https://doi.org/10.1007/s00181-025-02735-y

Off to a bad start: youth nonemployment and labor market outcomes later in life

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Received: 29 January 2024 / Accepted: 8 March 2025

Abstract
We estimate the effect of nonemployment experienced by Italian youth after secondary school exit on subsequent labor market outcomes. We focus on the impact on earnings and labor market participation both in the short term and in the long term. By estimating a factor-analytic model that controls for time-varying unobserved heterogeneity, we find that the negative effect of nonemployment on earnings is persistent, being sizeable and statistically significant up to 25 years after school completion. Penalties in terms of participation last instead shorter. Hence, early nonemployment operates by persistently locking the youth who get off to a bad start into low-wage jobs.

Keywords Youth nonemployment · Scarring effects · Earnings · Labor market participation · Factor analytic model

JEL Classification J01 · J08 · J31 · J64

1 Introduction

Since the 1980s, many labor economists have studied the consequences of early nonemployment (or unemployment) on subsequent labor market performances. They have especially sought to understand whether they are temporary, persistent or even permanent. The empirical literature provides several findings about the so-called scarring effects of nonemployment on both wages and employability in the future. In addition to the immediate loss in terms of forgone earnings and accumulation of human

We wish to thank the Ministry of Economics and Finance and the Giacomo Brodolini Foundation for providing us with the permission to use the dataset AD-SILC. Any opinions expressed in this paper are those of the authors and not those of the Ministry of Economics and Finance, the European Commission, the Italian National Institute of Statistics (ISTAT), or the Italian National Social Insurance Agency (INPS). We are grateful to Andrea Albanese, Chiara Mussida, two anonymous reviewers, the associate editor and the participants at the AIEL workshop (Genoa, 2022), XXXVII AIEL conference (Salerno, 2022), and seminars held at the University of Cagliari and Rotterdam Erasmus School of Economics (2022) for their comments. M. Filomena acknowledges financial support from the Cariverona Foundation Ph.D. research scholarship.

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Published online: 08 April 2025

capital, nonemployment episodes may also have longer-term or permanent effects by increasing the likelihood of experiencing future joblessness and lower subsequent wages (Arulampalam et al. 2001; Gregg and Tominey 2005).

Filomena (2024) provided an up-to-date survey of the empirical literature on scarring effects of nonemployment on subsequent labor market outcomes. The empirical findings are unambiguous in detecting significant, and often persistent, wage penalties and lower employment probabilities after episodes of joblessness, despite different datasets used, countries considered, time span covered and identification strategies of the causal effect. Some finding heterogeneity concerns the magnitude of the scarring effects: for instance, unemployment episodes experienced by school-leavers or by laid-off workers are particularly penalizing (see, e.g., Burda and Mertens 2001; Jacobson et al. 1993; Mroz and Savage 2006; Spivey 2005), while the negative effect is less stigmatizing in cases of plant closures (Gibbons and Katz 1991) or during economic downturns (Omori 1997).[1]

We study the impact of nonemployment events after secondary school completion on subsequent labor market performances in Italy. More in detail, we try to give credible answers to the following research questions: i) What is the causal impact of early nonemployment on subsequent earnings and labor market participation for Italian school-leavers? ii) If penalties are detected, how long do they take to fade away?

The contribution of our analysis is twofold. First, we shed further light into the scarring effects of early nonemployment on both labor earnings and participation by estimating short-, medium- and long-term impacts, measured up to 25 years after school completion. Experiencing joblessness at the start of the professional career is not unusual, as youth are typically searching for an adequate job match in this phase, and it is not per se worrying, because the difficulty to have a stable career may dissolve automatically without any intervention. Moreover, even if it causes penalties in terms of subsequent employment and wages, these penalties may fade away. If instead early nonemployment generates long-lasting scarring effects, effective preventive and curative policies should be designed to deal with the school-to-work transition phase. Our work, with its in-depth analysis of the dynamic of the effect of early nonemployment on labor earnings and participation up to 25 years after school completion, provides valuable information from this viewpoint. Second, we focus on the Italian case, which is particularly interesting. Understanding the scarring effects of early nonemployment in Italy is of crucial relevance from both a socioeconomic point of view and a policy perspective. Indeed, new labor market entrants face significant difficulties in Italy, where the average duration of the school-to-work transition, 2.88 years for those aged 18–34, is the highest in Europe, discouraging young people from investing in tertiary education (Pastore et al. 2021a, b). To the best of our knowledge, the stigma effects of nonemployment for Italian youth have not been investigated in sufficient detail for a proper understanding of the rigidity of the youth labor market. Lupi et al. (2002) examined the effect of unemployment events on re-employment wages only, without a clear identification strategy for the endogeneity of the occurrence of unemployment

[1] A related strand of the recent literature focused on the effect of adverse labor market conditions at graduation (see, e.g., Kahn 2010; Oreopoulos et al. 2012; Altonji et al. 2016; Cockx 2016; von Wachter 2020).

events. They found that they are scarring only in the North, where the aggregate unemployment rate is lower than in the rest of the country. Tanzi (2023) obtained similar findings by looking at the impact on future labor market participation at maximum after 6 years; the negative effects of early nonemployment on the propensity to experience further nonemployment events are smaller during recession or in regions with high unemployment rates.

To answer our research questions, we used the AD-SILC database, which is the result of the match between the IT-SILC database and administrative labor market data from the National Social Insurance Agency (INPS). For each interviewee of the IT-SILC, the dataset contains and allows us to reconstruct all the working history as an employee up to the end of 2013. We modeled and estimate the sequence of labor market experiences as of school completion. We started modeling the fraction of time the youth spent in nonemployment in the first three years after the secondary school diploma, which is the treatment. Then, we related this treatment intensity to the realization of earnings and fraction of time spent in employment 5 years after school completion and, every 5 years, until 25 years since the secondary school diploma.

The treatment intensity is very likely to be an endogenous variable as there will be both time-constant and time-varying characteristics, which we cannot observe or measure but which could nonetheless affect the likelihood of experiencing long nonemployment events after school exit and future labor market performances. Persistent or time-changing latent variables like ability, motivation, search intensity, family/social/economic background, household duties are typical examples of such crucial latent variables. This makes it difficult to credibly identify the causal effect of early nonemployment on subsequent labor market outcomes. Our identification approach of the causal effect fits into the factor-analytic dynamic models (FADMs) (Carneiro et al. 2003; Heckman and Navarro 2007), which has been more recently exploited by Fruehwirth et al. (2016) and Cockx et al. (2019) to study the impact of grade retention on subsequent school performances or by Picchio et al. (2021) to investigate the effect of fertility on subsequent labor market outcomes. Our model is a simplified version of the one in Fruehwirth et al. (2016), because we stick to a one-loading-factor specification of the unobserved heterogeneity. Like in Fruehwirth et al. (2016), we integrated *essential heterogeneity* (Heckman et al. 2006), i.e., we allowed the treatment effect also to depend on unobserved characteristics.[2] The assumptions in Fruehwirth et al. (2016) are therefore sufficient to attain the nonparametric identification of the treatment effect in our framework. This is based on three main ingredients. First, we imposed a loading factor structure on the unobserved determinants. Second, since we can rebuild all the working history, we can take advantage of the longitudinal information in our data and observe multiple realizations over time of the endogenous variables. Third, as in Picchio et al. (2021), we exploited measures of the latent factor which are free of selection into treatment (Carneiro et al. 2003), like the work experience before school completion and the number of siblings when the individual was 14. Because these measures are realized before the treatment occurs, i.e., before school completion and eventual accumulation of nonemployment events, they are free

[2] See also Cockx et al. (2019) for another application in which the treatment effect is allowed to depend on observed and unobserved characteristics.

of selection into treatment and convey information on the distribution of the latent factor, as they may be related to social, economic and family background.

This paper is organized as follows. Section 2 summarizes the empirical literature. Section 3 describes data and sample. Section 4 illustrates the econometric strategy for the identification of the causal effect of early nonemployment on subsequent labor market outcomes. We report the estimation results and comment on them in Sect. 5. Section 6 concludes.

2 Literature review

Theoretical predictions on the scarring effects of unemployment or nonemployment events can be derived from two main strands of the economic theory: the human capital theory and the signaling theory. According to the former, scarring effects are related to the depreciation of workers' general skills and knowledge during the nonemployment spell and to the lack of accumulation of human capital (Mincer 1974; Becker 1975; Pissarides 1992). Following the signaling theory, employers may use past nonemployment events of a worker as a signal of low productivity, with the magnitude of the stigma effect on worker's subsequent labor market outcomes, which may depend on the cause of previous nonemployment spells (Spence 1973; Vishwanath 1989; Lockwood 1991).

The empirical literature on the scarring effects of joblessness has adopted different perspectives and displayed a special interest to the impact of job displacements, finding large and permanent wage scars (see, e.g., Jacobson et al. 1993; Arulampalam et al. 2000; Arulampalam 2001; Burda and Mertens 2001; Eliason and Storrie 2006; Böheim and Taylor 2002; Deelen et al. 2018).

Regarding the impact of early unemployment, Corcoran (1982) and Ellwood (1982) found that it causes lower future earnings also 10 years after school completion. Similarly, Mroz and Savage (2006) detected that early unemployment experienced as long ago as ten years continues to negatively affect earnings, although they provide evidence of a relevant catch-up response. Doiron and Gørgens (2008) found that the occurrence of unemployment increases the probability of being unemployed in the future for young low-skilled Australians, but its duration is not relevant, i.e., they did not detect evidence of lagged duration dependence. Gartell (2009) and Nordström Skans (2011) studied the impact of early unemployment for Swedish youth, concluding that the longer the unemployment spell upon graduation the more substantial are subsequent individual earning losses and higher the unemployment probability after five years. A similar result is in Ghirelli (2015) for Belgium; one percentage point increase in the fraction of time spent in nonemployment in the first two and a half years since graduation decreases annual earnings by 10% and hours worked by 7% six years later. According to Cockx and Picchio (2013), in Belgium the job finding probability decreases from 60 to 16% for men and from 47 to 13% for women in the subsequent two years if the labor market entry is delayed by one year. In Germany, early unemployment is found to increase the probability of future unemployment by 3.4 percentage points (Manzoni and Mooi-Reci 2011), with relevant and persistent effects (Schmillen and Umkehrer 2017). For the UK, Gregg (2001) detected strong evidence of structural dependence induced by the early experience of unemployment for men but only a minor persistence for women, while Gregg and Tominey (2005) estimated long-term, significant and substantial wage penalties caused by youth unemployment in the magnitude of 13–21% at age 42.

Some studies sought to determine whether the scarring effects of nonemployment may be heterogeneous across some dimensions. Burgess et al. (2003) found that early unemployment has a negative effect on later employment prospects for the unskilled and a small beneficial effect for the more skilled. Möller and Umkehrer (2015) detected wage penalties, which are different across the distribution of earnings; an increase in early-career unemployment by one standard deviation (about 11 months) causes persistent earning losses of about 56% for workers at the bottom and 7% for workers at the top of the earnings distribution. Finally, Tanzi (2023) revealed that the size of the scarring effect of early unemployment in Italy depends on regional labor market characteristics; as suggested by the signaling theory, the higher the regional unemployment rate or the worse the regional business cycle, the smaller the scarring effect.

Our work is especially related to Tanzi's (2023), with respect to which it was developed independently. We both analyze the same country with a similar definition of the treatment. However, whereas in our work, the year of diploma is observed, it is unknown in Tanzi (2023) and she approximates it with the theoretical age of diploma. In addition, while Tanzi (2023) analyses a more recent time period but she can look at the impact of early nonemployment at maximum after 6 years, we are able to carry out a more in-depth analysis of the dynamic of the effect, by looking at the impact of the treatment on labor earnings and participation up to 25 years after school completion. Finally, our identification strategy does not rely only on an instrumental variable based on regional variation in the youth unemployment rate as in Tanzi (2023). Our FADM, after imposing a one-loading-factor specification of the unobserved heterogeneity, exploits measures of the latent factor, which are predetermined with respect to the treatment (work experience before school completion and number of siblings when the individual was 14), and allows us to let the treatment effect also to depend on unobserved characteristics (essential heterogeneity).

3 Data and sample

Our empirical analysis was based on the AD-SILC database, which is the result of the match between two data sources: i) the IT-SILC database covering the period 2004–2012 gathered by the Italian National Institute of Statistics (ISTAT); ii) the administrative data on labor market contracts from the National Social Insurance Agency (INPS). The latter allows, for each individual interviewed in the IT-SILC survey, to rebuild her/his working history as an employee up to the end of 2013. It contains gross earnings and the number of working days for each working episode and for each year. We further enriched the database with the regional time series of unemployment, employment, and real GDP growth rates retrieved from ISTAT. We used these variables as time-varying controls in the specification of the equations for the outcome variables.

From all the waves of the IT-SILC, we kept only individuals interviewed in 2005 and 2011. We limited our sample to individuals in these two waves because they are the only ones with the *ad hoc* module on intergenerational transmission of poverty and disadvantages, which provides information on the family situation when the respondents were 14 years old. We used indeed the number of siblings at 14 as a measure of the family and social background which is free of selection into treatment, i.e., predetermined with respect to the realization of nonemployment after school completion. As such, it contains predetermined information, which may proxy distribution on unobserved heterogeneity affecting labor market outcomes. As we explain later, we also have a second measure, i.e., the fraction of time spent in employment during the year before obtaining the secondary school diploma. Our factor structure model would be identified even without these measures, but only on the basis of exclusion restrictions and normalizations, which may be viewed as too arbitrary. As pointed out by Carneiro et al. (2003), having measures, which may proxy the unobserved determinants of the treatment and outcomes reduce the degree of "arbitrariness and render greater interpretability to estimates obtained from our model".

This IT-SILC sample was then merged with the administrative data from INPS. There were 1,558 individuals who responded to the IT-SILC survey, but they did not appear in the latter database. This may happen, for example, when an individual has never had a payroll employment position up to the moment of the IT-SILC interview. We deleted them.[3] Moreover, we decided then to only focus on individuals who exited school with a secondary school diploma.[4] We removed those with a lower degree to have a more homogeneous sample in terms of skills. We deleted individuals with a tertiary diploma because, although we know the year in which they graduated from the replies to the IT-SILC questionnaire, the month of graduation is unknown.[5] Hence, we do not know the month in which individuals with a tertiary degree exited formal education. For individuals with a secondary school diploma, we know instead the month in which they obtained the diploma, because the final examination takes place between the second half of June and the first half of July, and the results are known around mid of July. We set to 1 September the moment of the labor market entry. It is from this date that we started counting the time spent in nonemployment.

In Online appendix A, Table OA.1 reports in detail all the adopted sample selection criteria, including those which only marginally affected the sample size and were not discussed in the paragraphs above. The final sample was made up of 10,295 individuals: 5396 men and 4899 women. This sample is composed only by individuals who obtained the secondary school diploma more than 3 years before the IT-SILC interview. Since the administrative data contain information of job episodes up to the end of 2013, we can observe for each individual in our sample, even for those interviewed in 2011 as they completed the secondary school in 2008, their labor market outcomes up to 5 years after school completion. The number of individuals

[3] These dropped individuals are likely not a random sample, which renders our sample non-random and makes our estimated effect of nonemployment inconsistent. For example, they may be missing from the administrative data as a result of the negative effect of nonemployment after school exit. We expect a bias toward zero, as our analysis excludes individuals who are absent from the administrative database due to never having been officially employed.

[4] According to the microdata of the Italian National Institute of Statistics (ISTAT), 41% of the Italian population aged between 35 and 64 years in 2020 (about the cohorts in our final sample) had a secondary school diploma as the highest educational outcome. This figure is available online at http://dati.istat.it.

[5] In Italy, the final examination (the thesis discussion) for obtaining the tertiary degree is spread over the year.

whose labor market histories are observed for longer time spans is decreasing with the size of the time window since the secondary school diploma. In our empirical analysis, we look at the effect of early nonemployment on labor market outcomes until at most 25 years after school completion. The number of women(men) for whom we can observe the 25th year since the secondary school diploma amounts to 2423 (2792).

Table OA.2 shows the number of observations from 5 to 25 years after school completion grouped by periods of 5 years. It also provides descriptive statistics about the treatment variable, i.e., the fraction of days in nonemployment during the first 3 years after school completion, and other time-invariant characteristics predetermined with respect to the treatment. Table OA.3 shows summary statistics of our outcome variables, yearly labor earnings and yearly fraction of days spent in employment from 5 to 25 years after school completion. These descriptive statistics and the econometric analysis for the estimation of the effect of nonemployment on subsequent labor market outcomes are presented by separating men from women. The labor market functioning may indeed be gender sensitive, especially in Italy, where the labor force participation rate is traditionally quite low among women[6] and the gender roles and duties in the family still follow the patriarchal model consisting of the male breadwinner and the mother caretaker (Saraceno 1994; Giuliani 2022) in the period under analysis. If so, men and women experiencing randomly a nonemployment event could be differently affected. On the one hand, women may be more likely to react by permanently withdrawing from the labor market. On the other hand, since nonemployment is more common among women, an early nonemployment event experienced by a woman may generate a weaker signal and less adverse effects on future labor market performances. Furthermore, men and women are very likely to be differently affected by parenthood which, in our econometric approach, will end up into a time-varying unobserved factor. By keeping the female and the male sample separated, we identify gender different distributions of the time-varying unobservables and accommodate for gender differences in unobservables determining both early nonemployment and later labor market performances.

4 Econometric model

4.1 Estimation framework and the effect of interest

We denote by Y_{it}^{j} the j-th labor market outcome, with $i = 1, ..., n$ being the index for individuals, $j = 1, 2$ being the index for our two labor market outcomes, yearly earnings and yearly fraction of time in employment, and $t = 5, ..., T_i$ being the index for the time elapsed since school completion. The observable time elapsed since school completion (T_i) differs across individuals. As mentioned in Sect. 3, whereas we observe for all the sample the labor market outcomes measured 5 years after school completion ($t = 5$), we do not observe for everybody the same time span after

[6] In 2005, the employment rate from 20 to 64 years was 49% for women and 75% of men (Eurostat, Labour Force Survey).

the secondary school diploma. Since administrative data on salaried employment are available up to 2013, the labor market history of individuals of older cohorts, who therefore got the diploma in earlier calendar years, is observed for a longer time span and, eventually, up to 25 years after school completion.[7] Moreover, to keep the model tractable and have a limited number of equations, we restricted the set of time index t to {5, 10, 15, 20, 25}, so that the labor market outcomes are measured in 5-year intervals since school completion, up to 25 years at maximum (or the closest multiple of 5 for individuals with shorter observed labor market histories after the diploma). We specify the labor market outcome j of individual i at time t since school completion as

$$Y_{it}^{j} = \beta_t^j TR_i + \mu_t^{j\prime} X_{it}^{j} + \epsilon_{it}^{j}, \tag{1}$$

where β_t^j is the effect of the treatment variable TR on outcome j at time t, X_{it}^j is a vector of observed covariates, μ_t^j is the corresponding parameter vector and ϵ_{it}^j collects the individual- and time-varying unobservables. The treatment intensity TR is the fraction of days spent in nonemployment in the three first years after the diploma. More in detail, the starting date of the three-year time window over which we computed the fraction of time spent in nonemployment is 1 September of the year of the diploma, which is officially obtained in Italy around the mid of July, after a set of final exams taking place between the second half of June and the beginning of July. The treatment intensity is continuously distributed from a minimum of 0, for those who spent 0 days out of salaried employment during the first three years after school completion, to 1, for those who have never been in salaried employment in the same three years. The average treatment effect of going from full employment to full nonemployment on labor market outcome j at time t since school completion is simply given by β_t^j. The intensity of the treatment TR_i, i.e., the fraction of the first three years after school completion spent in nonemployment, is specified as follows:

$$TR_i = \nu' Z_i + u_i \tag{2}$$

where ν is of a vector parameters for covariates Z_i, which are realized either before the end of secondary school (for example mother's highest education) or in the three years after school exit (like number of kids, labor market status or GDP growth at regional level) and u_i is individual unobserved heterogeneity.

4.2 Identification strategy

Identifying the effect of the intensity of nonemployment after school completion on future labor market outcomes requires to properly account for unobserved heterogeneity across individuals, which might affect the occurrence of early nonemployment events after school exit and subsequent labor market outcomes. For example, school-leavers with high labor force attachment, ability, motivation, liquidity constraints and

[7] Hence, the decreasing number of observations is almost entirely due to exogenous right-censoring, i.e., to the fact that we can observe individuals in the administrative dataset until the end of 2013: the younger the cohorts, the shorter the observed time horizon.

job search intensity may be less likely to experience early nonemployment events. A large value of these unobservables may also generate better career opportunities and, therefore, better labor market outcomes later in life. Furthermore, these unobserved characteristics may change over time. For instance, the liquidity constraints of those individuals who experience more intensively longer nonemployment events may become tighter and more relevant over time, increasing the job search intensity, lowering the reservation wages and having therefore an impact on labor market outcomes that may be varying over time. As a further example of the relevance of time-varying heterogeneity, one may refer to the labor force attachment or the career orientation of an individual. The experience of a nonemployment event at the start of the career may be related to the labor force attachment. At the same time, early nonemployment may address an individual toward a lower career track, lower levels of job satisfaction and, henceforth, a decreasing profile of the labor force attachment. Finally, some determinants of early nonemployment, like preferences for family formation or parenthood, may also change over time. At some point after school exit, individuals may form a family and have kids, modifying the preference toward the work–family balance, which is a time-varying unobservable very likely to matter for future labor market outcomes.

To account for time-varying unobserved heterogeneity, we set up a factor-analytic model (Carneiro et al. 2003; Heckman and Navarro 2007; Fruehwirth et al. 2016; Cockx et al. 2019; Picchio et al. 2021).[8]

Assumption 1. Factor structure: the unobserved terms of the equations of the outcomes and the treatment intensity are composed of a latent factor $\boldsymbol{\theta}$, which collects the time-varying unobserved differences among individuals and error terms that are conditionally independent given the latent factor:

$$\epsilon_{it}^{j} = \alpha_t^j \theta_{it} + \varepsilon_{it}^{j} \tag{3}$$

$$u_i = \lambda \theta_{i5} + v_i, \tag{4}$$

where θ_{it} is the latent factor at time t in $\boldsymbol{\theta}_i = (\theta_{i5}, \theta_{i10}, ..., \theta_{i25})$, with a multivariate distribution characterized by $\mathrm{Cov}(\theta_{it}, \theta_{it'}) \neq 0, \forall t \neq t'$. The error terms ε_{it}^{j} and v_i are independent of θ_{is} and mutually independent for all j, t, s.

Unobserved heterogeneity varies over time because of the factor distribution and a linear combination of the latent factor with time-varying coefficients α_t^j, the so-called *factor loadings*.[9] As in Picchio et al. (2021), we adopted a one-loading-factor specification, i.e., we allow only for a single-dimensional time-varying unobserved determinant of the treatment intensity and of the outcomes. Our specification of the

[8] Carneiro et al. (2003) study the impact of different schooling levels on future returns; Fruehwirth et al. (2016) and Cockx et al. (2019) estimate how grade retention affects subsequent school performances; Picchio et al. (2021) study the effect of childbirth and its timing on female labor market outcomes in Italy.

[9] Up-to-scale normalizations of the latent components are required to identify the distribution of $\boldsymbol{\theta}$. We apply the normalization suggested in Carneiro et al. (2003) and also used in Picchio et al. (2021), i.e., $\alpha_t^1 = 1$ for all t.

factor structure is therefore encompassed in the more general specification in Fruehwirth et al. (2016), who instead differentiated among several sources of unobserved heterogeneity (multidimensional factor structure).

Carneiro et al. (2003) showed that having a set of selection-free measurements related to the unobservables that jointly determine the treatment intensity and the outcomes reduces the degree of arbitrariness of factor analysis. For this reason, we take advantage of the next assumption.

Assumption 2. Free of selection measurements: we have measures, which are predetermined with respect to the moment of school completion and, therefore, to the realization of the treatment intensity. We specify these selection-free measurements as

$$M_i^l = \omega^{l\prime} S_i^l + \xi^l \theta_{i5} + e_i^l, \quad l = 1, \ldots, L, \tag{5}$$

where S_i^l collects observed covariates, ω^l is the corresponding parameter vector, ξ^l is the factor loading and e_i^l is a zero-mean error term independent of S_i^l, θ_{i5}, v_i and ε_{it}^j.

Free-of-selection measures can identify the model because they are informative about the latent factor before treatment occurrence and, therefore, they allow to pin down the distribution of the latent factor in the initial period and "the additional assumption of valid exclusion restrictions is not necessary but rather aids in identification" (Fruehwirth et al. 2016). In our empirical analysis, we use $L = 2$ selection-free measurements, although one would have been enough without invoking the use of exclusion restrictions. Indeed, as proved by Carneiro et al. (2003), in a one-loading-factor specification without exclusion restrictions, at least three outcomes that depend on the latent factor are necessary. Given that the treatment intensity variable and the labor market outcome depend on the latent factor, by having one free-of-selection measure, we obtain three outcomes depending on the latent factor. Alternatively, if valid exclusion restrictions are available, they can be employed to address selection, eliminating the need for selection-free measures to achieve identification (Fruehwirth et al. 2016).

The first measure M_i^1 is a variable which corresponds to the fraction of days spent at work during the year before the school completion. It is likely to be determined by a set of unobserved traits which include labor force attachment, motivation, ability, job search strategies, liquidity constraints and family, social or cultural background.

The second measure M_i^2 is the number of siblings when the individual was 14 years old. There is a strand of the literature investigating the relation between family size, investments in human capital and labor market outcomes later in life. On the one hand, the number of siblings in a household may be negatively correlated to the opportunity to study longer or the quality of the attended schools, because of the dilution of parents' material resources (see Steelman et al. 2002, for a review). On the other hand, a larger number of siblings may increase the need for other liquidity entries, hence determining an earlier and more active participation in the labor market. Blake (1981) suggested that the number of siblings have an important detrimental impact on a child's educational attainment and college plans, while families with fewer siblings provide more resources for the child and support the development of better educational outcomes.[10] Olneck and Bills (1979) focused on the effects of birth order and family size on individuals' adult level of wages and occupation, revealing negative effects of family size on occupation only. Kessler (1991) found that family size is a significant determinant of employment status for women. As such, the second measure M_i^2 may encompass information on childhood household environment shaping the likelihood of success in the labor market in adulthood.

Under Assumption 1 and Assumption 2, our factor structure and model are special cases of both Fruehwirth et al. (2016) and Picchio et al. (2021). Their identification results related to the factor analysis can be invoked. To invoke Fruehwirth et al.'s (2016) nonparametric identification result, one of the following two assumptions is further needed[11]:

> Assumption 3a. Asymmetric factor distribution: the third moment of the latent factor distribution exists and is different from zero.
> Assumption 3b. Symmetric factor distribution: the third moment of the latent factor distribution is equal to zero, the fourth moment exists, and the second and fourth moments do not have the same relationship as that of the normal distribution.

In the model specification, we use exclusion restrictions, i.e., we include some continuous variables among the set of observed determinants of one outcome, but we exclude them from the others. Exclusion restrictions are not necessary for identification when using free-of-selection measures, as free-of-selection measures can pin down the distribution of the latent factor in the initial period. However, exclusion restrictions aid in identification and improve efficiency. These variables are the regional employment rate, the regional unemployment rate and the regional GDP growth rate, measured in different moments depending on the outcome. They are measured at the time when each individual was born in S_i^l, for $l = 1, 2$, because we assume that, for the number of siblings when the individual was 14 and for the participation to the labor market before the end of secondary school, this is what matters for family formation and to shape the attachment to the job market and to affect liquidity constraints that lead the individual to work even before school exit. To model the treatment intensity, which is an indicator of the labor market participation in the first three years after secondary school completion, in Z_i we use the regional rates averaged over those three years. Hence, we assume that, conditional on the measures and the average labor market state and business cycle in the three years after school exit, past regional rates when the individual was still in school do not matter. Finally, in the labor market outcome equations, in X_{it}^j, we use regional rates at the time t in which the labor market outcome is evaluated, for all j and t. Therefore, we assume that once we control for the current state of the labor market and of the business cycle, any lagged regional rate has no

[10] See also Blake (1989), Åslund and Grönqvist (2010), Wijanarko and Wisana (2019) and Li and Hiwatari (2020) for studies on the relation between the number of siblings and the risk that individuals stop their education earlier than they should.

[11] See Sections 1 and 3 of the Online Appendix B of Fruehwirth et al. (2016).

direct impact on current labor market outcomes, except through the time-varying unobserved heterogeneity component in the current year and conditional on the other covariates. Both Bhargava (1991) and Mroz and Savage (2006) clarified why the variation of exogenous variables, like these regional rates, is of help to identify the causal effects of endogenous variables in a dynamic discrete time panel data model. In fact, these covariates implicitly provide additional identification conditions, resulting in significantly more degrees of freedom to control for endogenous determinants. Every lag of the exogenous time-varying regressor may indeed determine a separate effect on the current realization of the outcome. Table OA.7 clarifies in detail the exclusions across all the equations.

Albeit the factor model places restrictions on the covariances between unobservables in the treatment intensity and outcome equations, with the unobserved traits affecting treatment intensity and labor market outcomes with a constrained structure, it has the advantage of allowing for essential heterogeneity and time-varying unobservables. These cannot be dealt with in standard econometric panel data models with linear outcome regression, like in canonical fixed effects models, where the unobserved heterogeneity is allowed to be correlated with the treatment variable but it is imposed to be time-constant. Additionally, they cannot be tackled in propensity score approaches or other methods estimating causal effects using different covariate balancing techniques on the basis of a rich set of observables (e.g., entropy balancing). With the dataset in hand, we cannot rely on a sufficient amount of individual characteristics to credibly base our identification strategy on selection on observables only. Therefore, in our framework, we condition not only on observed covariates but also on the unobserved latent factor. This is an alternative way to obtain the conditional independence assumption used in matching methods, which is based on the idea of matching not only on the variables that are observed to the analyst, but also on unobservable factors (Carneiro et al. 2003; Fruehwirth et al. 2016).

4.3 Likelihood function

We estimated the model by maximum likelihood. Hence, we needed to specify the implicit functions in Eqs. (1), (2) and (5) as dependent on a finite set of parameters. We adopted the usual linear index specification for the deterministic parts.[12] Let include all the parameters for our measurement, treatment and outcome equations in $\boldsymbol{\phi} = (\boldsymbol{\tau}^1, \boldsymbol{\tau}^2, \boldsymbol{\varphi}, \boldsymbol{\psi})$, with $\boldsymbol{\tau}^1$ and $\boldsymbol{\tau}^2$ being the parameters of our measurement equations, $\boldsymbol{\varphi}$ of the treatment intensity equation and $\boldsymbol{\psi}$ of the equations of the labor market outcomes. The likelihood for individual i is the joint density of $(M_i^1, M_i^2, TR_i, \mathbf{Y}_i)$, with $\mathbf{Y}_i = (Y_5^1, \ldots, Y_{25}^1, Y_5^2, \ldots, Y_{25}^2)$. Using the chain rule, the individual contribution to the

[12] To reduce the number of parameters to be estimated, we followed Fruehwirth et al. (2016) and Picchio et al. (2021) and imposed that, in the labor market outcome equations, the coefficients of the covariates do not vary with t.

likelihood function conditional on observable and unobservable characteristics can be written:

$$\mathcal{L}_i(\boldsymbol{\phi} \mid M_i^1, M_i^2, TR_i, \mathbf{Y}_i, S_i^1, S_i^2, Z_i, \mathbf{X}_i, \boldsymbol{\theta}_i) = \prod_{l=1,2} g^l(M_i^l \mid S_i^l, \theta_{i5}; \boldsymbol{\tau}^l) h(TR_i \mid Z_i, \theta_{i5}; \boldsymbol{\varphi}) \prod_{j=1,2} \prod_{t=5,10,...,25} f(Y_{it}^j \mid TR_i, X_{it}, \theta_{it}; \boldsymbol{\psi})^{d_{it}}, \tag{6}$$

where all the sets of covariates contain the constant, g^l, h and f are normal density functions, and d_{it} is a dummy equal to 1 if individual i is still in our sample t years after school completion.

In order to account for the presence of individual time-varying unobserved heterogeneity, we assumed that the vector of latent factors $\boldsymbol{\theta}_i = (\theta_{i5}, ..., \theta_{i25})$ has a multivariate discrete distribution with H support points. Thus, $\boldsymbol{\theta}_i$ takes values $\boldsymbol{\theta}^h$, $h = 1, ..., H$, following a multinomial logit parametrization

$$p^h = Pr(\boldsymbol{\theta}_i = \boldsymbol{\theta}^h) = \frac{\exp(\rho^h)}{\sum_{r=1}^H \exp(\rho^r)} \tag{7}$$

with innocuous normalization $\boldsymbol{\theta}^1 = \mathbf{0}$ and $\rho^H = 0$.[13] Moreover, we constrained the time-varying latent factor to be constant from 20 to 25 years after school completion ($\theta_{20}^h = \theta_{25}^h$ for all h). We imposed this restriction to avoid identification issues related to the fact the sample is halved when approaching $t = 25$. It should not be a too strict assumption because the latent factor determining labor market outcomes may stabilize over time. The i-th contribution to the likelihood becomes

$$\mathcal{L}_i(\boldsymbol{\phi}, \boldsymbol{\rho}, \boldsymbol{\Theta} \mid M_i^l, TR_i, \mathbf{Y}_i, S_i^l, Z_i, \mathbf{X}_i) = \sum_{h=1}^H p^h \mathcal{L}_{ih}(\boldsymbol{\phi} \mid M_i^l, TR_i, \mathbf{Y}_i, S_i^l, Z_i, \mathbf{X}_i, \boldsymbol{\theta}_i = \boldsymbol{\theta}^h) \tag{8}$$

where $\mathcal{L}_{ih}$ is the likelihood in Eq. (6), conditional on $\boldsymbol{\theta}_i$ taking value $\boldsymbol{\theta}^h$, the matrix $\boldsymbol{\Theta}$ contains the vectors of support points $(\boldsymbol{\theta}^1, ..., \boldsymbol{\theta}^H)$ and the vector $\boldsymbol{\rho}$ collects the weights determining the H masses of probabilities. The sample log-likelihood is the sum across the natural logarithm of the individuals contributions in Eq. (8), i.e.

$$\ln(\mathcal{L}) = \sum_{i=1}^N \ln\left[\mathcal{L}_i(\boldsymbol{\phi}, \boldsymbol{\rho}, \boldsymbol{\Theta} \mid M_i^l, TR_i, \mathbf{Y}_i, S_i^l, Z_i, \mathbf{X}_i)\right]. \tag{9}$$

We maximized Eq. (9) with respect to its parameters using analytical derivatives.

[13] The former normalization arises because the mean of the latent factor cannot be identified when all equations include a constant term. The latter ensures that the mass probabilities sum to one.

5 Estimation results

We estimated three different models, with three different assumptions about the presence of unobserved heterogeneity: i) without unobserved heterogeneity; ii) with a time-constant latent factor with discrete distribution; iii) with a time-varying latent factor with discrete distribution.

The simulations in Gaure et al. (2007) suggest to choose the number of support points which minimizes the Akaike information criterion (AIC). Following this advice, we stopped at $H = 5$ when we assumed the latent factor to be time constant. With the time-varying latent factor, we increased the number of support points until 10, experiencing a continuous improvement in the AIC. We then stopped at 10 for the sake of model specification parsimony and because we realized that the estimated coefficients of the treatment had become very stable and unaffected by the last increases in the number of support points.

The time-varying specification of the latent factor yielded the best results in terms of information criteria, both for men and for women. Table 1 shows post-estimates statistics. In the next subsections, we report and comment on the effects of early nonemployment across the three different assumptions on the latent factor. Sections B and C of the Online Appendix report the full set of estimation results for all the models.

5.1 Main findings

The core question of the analysis is whether experiencing nonemployment after school completion inflicts a scar on future labor market outcomes as measured by labor earnings and yearly fraction of days spent in employment.[14] Tables 2 and Fig. 1 display the impact of the fraction of time spent in nonemployment in the first 3 years after school completion on yearly labor earnings evaluated at $t \in \{5, 10, 15, 20, 25\}$ after the diploma, along the three different latent factor structures.

Shifting from panel (a) to panel (c) of Table 2 or from graph (a) to graph (c) of Fig. 1, it clearly emerges that if time-varying unobservables were not accounted for, the negative impact of early nonemployment on subsequent earnings would be largely overestimated. Even if the early nonemployment penalty is much smaller when we control for time-varying unobserved heterogeneity, it is statistically significant up to 25 years since school completion; the scarring effect of early nonemployment is long lasting for both men and women. The estimates reported in Table 2 and Fig. 1 are the impact of the fraction of time spent in nonemployment in the first three years since school completion going from 0 to 1. Hence, if the time spent in nonemployment just after school completion increases by 10 percentage points (pp), male (female) yearly earnings decrease by €382 (€492) 5 years after school completion. This penalty for men (women) is reduced to €225 (€140) 25 years after the diploma. Figure 1 visually shows that men and women experience a similar nonemployment penalty in the short run ($t = 5$ and $t = 10$). However, men suffer larger penalties in subsequent years.

[14] In Table OE.10 in Online appendix D we report the estimated effects if we use daily earnings as outcome variable instead of yearly earnings.

Table 1 Summary statistics on the estimated models across different assumptions on the latent factor

	Without unobserved heterogeneity	Time-constant unobserved heterogeneity	Time-varying unobserved heterogeneity
(a) Men			
Number of parameters	160	180	212
Log-likelihood	53,673.34	48,731.48	39,755.52
AIC	107,666.68	97,822.96	79,935.03
BIC	108,721.63	99,009.78	81,332.83
Distribution of the latent factor	–	Discrete	Discrete
Number of support points of the latent factor	–	5	10
(b) Women			
Number of parameters	160	180	212
Log-likelihood	44,541.40	39,831.57	32,911.81
AIC	89,402.80	80,023.15	66,247.62
BIC	90,442.29	81,192.57	67,624.94
Distribution of the latent factor	–	Discrete	Discrete
Number of support points of the latent factor	–	5	10

AIC = Akaike information criterion; BIC = Bayesian information criterion

Table 2 Impact of early nonemployment on yearly labor earnings (€)

Treatment intensity fraction of time in nonemployment during the first 3 years after school completion	Years since school completion				
	$t=5$	$t=10$	$t=15$	$t=20$	$t=25$
(a) Without unobserved heterogeneity					
Men	− 12,382.93***	− 8230.58***	− 6699.58***	− 6220.41***	− 4041.89***
	(1431.78)	(1019.47)	(940.95)	(892.95)	(828.87)
Women	− 12,424.21***	− 8856.36***	− 5161.12***	− 5170.76***	− 4134.63***
	(1099.92)	(8220.13)	(736.22)	(755.33)	(711.54)
(b) With time-constant unobserved heterogeneity					

Table 2 continued

Treatment intensity fraction of time in nonemployment during the first 3 years after school completion

	Years since school completion				
	$t = 5$	$t = 10$	$t = 15$	$t = 20$	$t = 25$
Men	− 11,006.14***	− 5957.12***	− 3537.09***	− 1842.11**	502.03
	(814.19)	(654.60)	(707.99)	(802.19)	(762.80)
Women	− 10,422.62***	− 5954.38***	− 1474.79**	−935.41	245.864
	(663.50)	(593.01)	(633.67)	(669.56)	(690.10)
(c) With time-varying unobserved heterogeneity					
Men	− 3815.09***	− 4125.82***	− 3935.38***	− 4448.10***	− 2254.04***
	(1048.98)	(757.46)	(701.46)	(646.82)	(616.17)
Women	− 4919.67***	− 3610.87***	− 1880.88***	− 2765.47***	− 1399.38***
	(722.63)	(547.32)	(487.52)	(489.42)	(471.35)
Observations (men)	5396	5310	4864	3947	2792
Observations (women)	4899	4722	4235	3383	2423

Labor earnings are in 2014 prices and deflated by the ISTAT consumer price index. The effect of one more year in nonemployment is equal to the estimated coefficients divided by three * Significant at 10%, **significant at 5%, ***significant at 1%. Standard errors are in parentheses

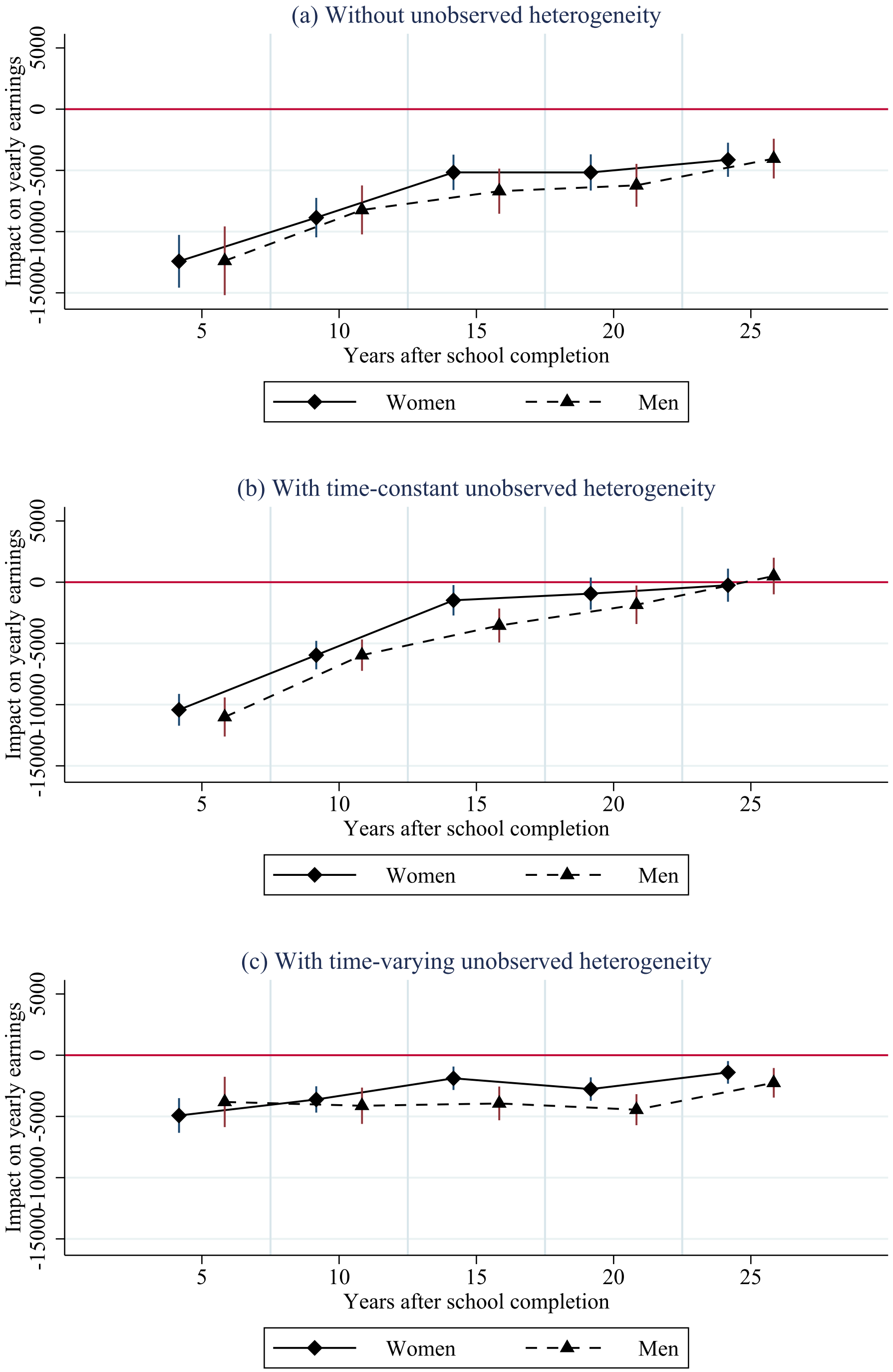

Fig. 1 Impact of early nonemployment on yearly labor earnings (€). *Notes*: Labor earnings are in 2014 prices and deflated by the ISTAT consumer price index. The vertical segments are 95% confidence intervals

Table 3 and Fig. 2 display the estimated impact of early nonemployment on the yearly fraction of days spent in salaried employment in the future. Also in this case, not controlling for time-varying unobserved heterogeneity generates a large overestimation of the scarring effect of early nonemployment, both in size and in duration. Once controlling for time-varying unobserved heterogeneity, we found that early nonemployment negatively affects the labor market participation only in the short term; a 10-pp increase in the time spent in nonemployment after school completion reduced the fraction of days spent in employment 5 years after the diploma by 0.65 (0.99) pp for men (women). This penalty becomes very close to zero and not significantly different from zero by the 10th year after school completion for both men and women. Finally, in the last year of observation ($t = 25$), individuals who experienced longer nonemployment events after school completion spend more time in the labor market, although the effect is small; an increase by 10 pp in the time spent in nonemployment after the diploma generates an increase by 0.43 (0.31) pp in the fraction of days spent at work 25 years later.

For the model accounting for time-varying unobserved heterogeneity, Table 4 presents the estimated impacts of early nonemployment on labor market outcomes, compared to the average outcomes of individuals who did not experience early nonemployment. Therefore, Table 4 highlights the scale of these estimated effects, considering the changing sample composition over time t due to exogenous right-censoring, as individuals are observed only until the end of 2013. A 10-pp increase in early nonemployment leads to a reduction in male (female) annual earnings by 2.6% (3%) 5 years after school completion, relative to those who did not experience early nonemployment. This reduction decreases over time, reaching around 0.9% (0.7%) 25 years after graduation. The negative impact on the annual fraction of days worked is around 0.9% (1.2%) for men (women) 5 years after school completion and is nearly negligible in later years.

When we control for time-varying unobserved heterogeneity, the negative effect of early nonemployment on labor earnings becomes smaller in magnitude, whereas the penalties in terms of labor participation are present only up to 5 years after school completion. This suggests that when we include in the model the time-varying latent factor, we capture those latent traits which affect both selection into early nonemployment and future labor market performances. As an example, career-oriented individuals with higher abilities and motivations are more likely to have success in the labor market and therefore the negative impact of nonemployment on labor market outcomes is subject to upward bias if these characteristics were not accounted for. Moreover, differences in the estimated penalties between the model with time-constant and the model with time-varying unobserved heterogeneity indicate that the latent factor is subject to relevant variations over time. For instance, the influence of the family background may diminish as a person ages (Gregg 2001); further, liquidity constraints may change over time and individuals may reduce their reservation wages as they experience longer nonemployment spells, accepting therefore low-quality jobs and translating into worse labor earning profiles throughout the reminder of their working career (Ghirelli 2015).

In summary, the main findings on the impact of early nonemployment on future labor market outcomes are the following. First, both men and women suffer sizable earnings

Table 3 Impact of early nonemployment on yearly fraction of days spent at work

Treatment intensity fraction of time in nonemployment during the first 3 years after school completion	Years since school completion				
	$t = 5$	$t = 10$	$t = 15$	$t = 20$	$t = 25$
(a) Without unobserved heterogeneity					
Men	−0.556***	−0.202***	−0.114***	−0.050*	−0.002
	(0.024)	(0.022)	(0.024)	(0.026)	(0.030)
Women	−0.567***	−0.300***	−0.177***	−0.099***	0.069**
	(0.029)	(0.026)	(0.025)	(0.028)	(0.033)
(b) With time-constant unobserved heterogeneity					
Men	−0.513***	−0.149***	−0.062***	−0.001	0.039
	(0.021)	(0.021)	(0.021)	(0.023)	(0.026)
Women	−0.500***	−0.211***	−0.079***	−0.008	0.007
	(0.023)	(0.023)	(0.023)	(0.024)	(0.027)

Table 3 continued

Treatment intensity fraction of time in nonemployment during the first 3 years after school completion		Years since school completion			
	$t = 5$	$t = 10$	$t = 15$	$t = 20$	$t = 25$
(c) With time-varying unobserved heterogeneity					
Men	−0.065***	−0.012	−0.002	−0.006	0.043***
	(0.010)	(0.009)	(0.013)	(0.008)	(0.009)
Women	−0.099***	−0.003	−0.004	−0.014	0.031***
	(0.011)	(0.016)	(0.014)	(0.008)	(0.009)
Observations (men)	5396	5310	4864	3947	2792
Observations (women)	4899	4722	4235	3383	2423

The effect of one more year in nonemployment is equal to the estimated coefficients divided by three. * Significant at 10%, ** significant at 5%, *** significant at 1%. Standard errors are in parentheses

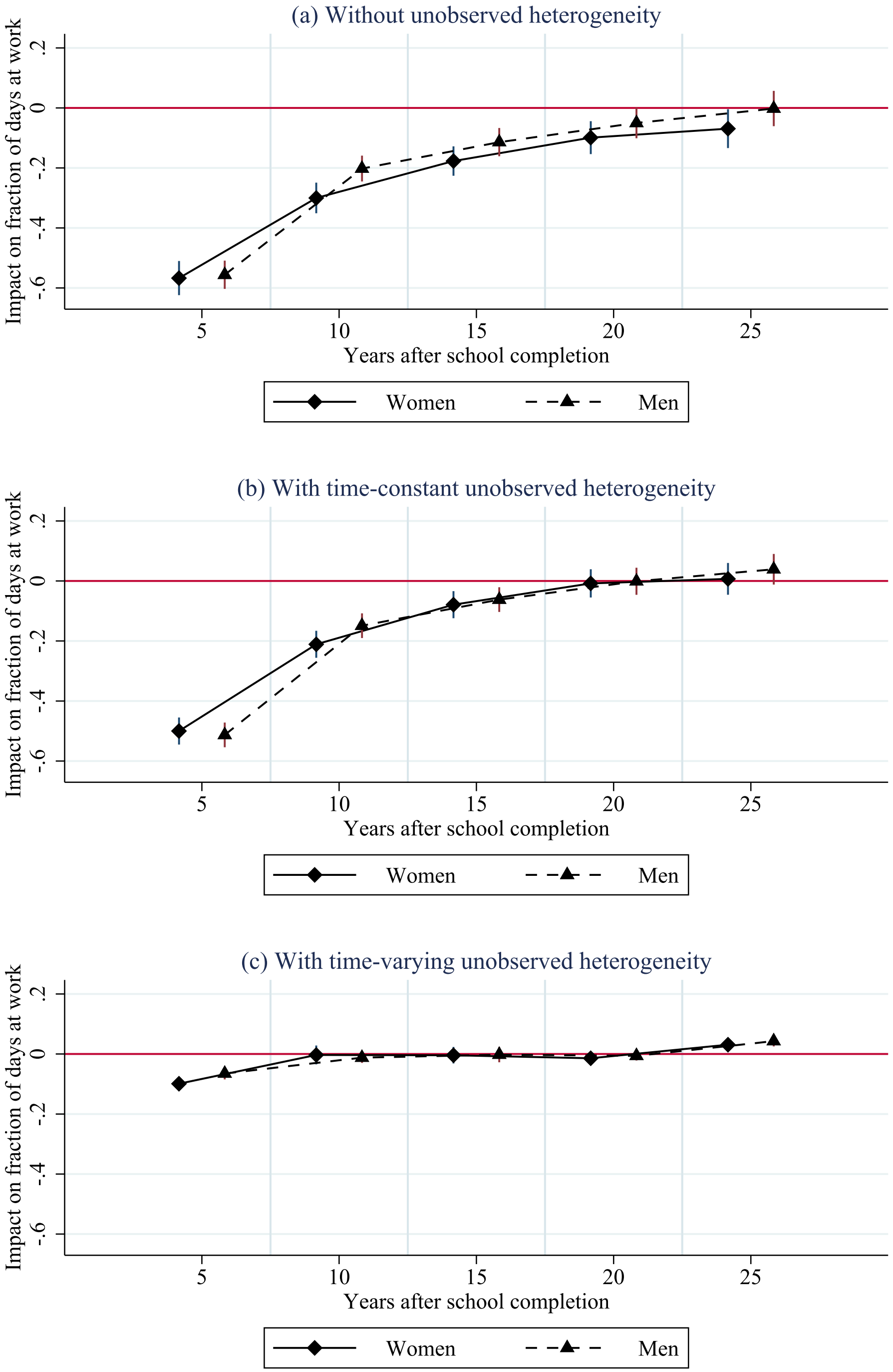

Fig. 2 Impact of early nonemployment on yearly fraction of days spent at work. *Notes*: The vertical segments are 95% confidence intervals

Table 4 Estimated impacts of early nonemployment on future labor market outcomes, compared to the average outcomes at t for individuals who did not experience early nonemployment

Years since school completion	$t = 5$	$t = 10$	$t = 15$	$t = 20$	$t = 25$
(a) Yearly labor earnings					
Men	−26.19%	−19.95%	−17.69%	−18.36%	−9.12%
Women	−29.88%	−20.13%	−10.72%	−14.67%	−6.89%
(b) Yearly fraction of days at work					
Men	−8.67%	−1.43%	−0.23%	−0.68%	4.94%
Women	−11.65%	−0.34%	−0.47%	−1.69%	3.48%

These figures are computed by evaluating the impact on the labor market outcomes t years after school exit as implied by the estimated coefficients reported in Tables 2 and 3 of our manuscript, and comparing them to the average labor market outcomes at time t for individuals who did not experience nonemployment after diploma penalties, which are persistent up to 25 years after the secondary school diploma. Second, experiencing early nonemployment causes a lower participation in the labor market only in the short term for both men and women. Our results on labor earnings are consistent with those in Gregg and Tominey (2005), who used the local unemployment rate at age 16 as an instrument for youth unemployment, while controlling for a large set of variables related to family background and individual ability. In their study, wage scarring effects persisted up to the age of 42. In addition, our estimates of the effects on earnings and labor market participation align with those reported by Mroz and Savage (2006), who jointly modeled individual outcomes on an annual basis. Like our approach, they addressed potential self-selection biases by incorporating both time-constant and time-varying unobserved factors. Mroz and Savage (2006) found that the effect of unemployment on hourly earnings is long-lived, whereas only a short-lived persistence of about 4 years in terms of future unemployment was detected. As suggested by Ellwood (1982), early work experience may have a large and positive earnings effect and, therefore, the biggest costs of being nonemployed during the first years after school completion are wage penalties and lower earning power.

Our findings are not fully in line with the predictions of the signaling theory. Early nonemployment events may be used as a signal of low productivity and employers may penalize those individuals who experienced them (Spence 1973; Vishwanath 1989; Lockwood 1991). However, individuals incurring in random early nonemployment events, once hired, will show greater productivity than expected and the initial penalties should disappear after a while. Only our findings on labor market participation are in line with the signaling theory. This is not the case in terms of earnings, because we find that the earnings penalties persist up to 25 years after school completion. A potential explanation of the persistent scars on earnings may come from the job search theory. Given that people experiencing early nonemployment send a worse signal, accumulate less human capital relative to their employed peers, and are more likely to face liquidity constraints, they could lower their reservation wage and be more likely to accept worse jobs, characterized by a career track of lower profile, which traps them in lower wages and lower chances of subsequent promotions.

5.2 Heterogeneity analysis

We questioned if our main findings were heterogeneous across two main dimensions: among individuals with different unobserved traits; the moment in which the nonemployment events are experienced.

Individuals may be heterogeneous in several aspects, many of which may be unobserved. In our framework, relevant unobserved traits are likely to be factors like ability, motivation and labor force attachment. People endowed with different level of the latent factor may differently react to and be affected by nonemployment events. On the one hand, individuals who are more able, motivated and attached to the labor market may respond by investing in their own human capital and catch up soon with their peers after the experience of a random nonemployment event. Moreover, more able graduates may enjoy a higher probability of finding a job and therefore they may prefer to invest time searching for a better job match, rather than to accept the first job. On the other hand, people endowed with a low level of the latent factor may already suffer very low probabilities of finding good jobs and having a stable career, so that early nonemployment events are not able to worsen further their labor market performances. In order to investigate the heterogeneity in the nonemployment effect due to unobserved traits, we estimated the main model with essential heterogeneity (Heckman et al. 2006), i.e., by including the interaction between the treatment and the latent factor.[15] The results are reported in Table OD.1 in Online appendix D. We found that for men essential heterogeneity is not important, i.e., the effect is homogeneous across different levels of the latent factor. For women, we instead found that the smaller the unobserved traits, like ability or labor force attachment, the smaller the earning penalties induced by early nonemployment. Therefore, women who are less able or less attached to the labor market lose less from early nonemployment events.

A relevant source of effect heterogeneity may also concern the timing with which the nonemployment spells occur. The experience of nonemployment immediately after the diploma, when the youth are still looking for their first job, may have different effects on future labor market performances than nonemployment events occurring in the subsequent years, in particular after a job loss, because they may send different signals. Potential employers may see a period of nonemployment right after school leave as physiological. Instead, a nonemployment spell later may signal the inability to retain a job and have more damaging effects on future labor market opportunities. To test whether the scarring effect of nonemployment differs by the timing with which it is experienced, we included in the outcome equations three treatment intensity variables, measuring the fraction of days spent in nonemployment in the first, second, and third year after school completion. Table OD.2 shows their estimated coefficients. As hypothesized on the basis of the signaling mechanism, we found that nonemployment in the first year after school completion does not generate penalties on future labor market outcomes, while the negative effect on labor earnings and, in the short-term, on labor market participation is instead driven by the intensity of nonemployment in the third year after diploma.

5.3 Sensitivity analysis

We ran some sensitivity checks to assess the robustness of our findings in several directions. We started by modifying the definition of nonemployment. In the benchmark model, experiences like volunteer work, internships and stages are considered as a form of employment and do not contribute to the computation of the fraction of days spent in nonemployment after the diploma. We modified this definition by considering as nonemployment also all the forms of unpaid work, for example volunteer work and

[15] We modified Eq. (1) by including the interaction between the treatment intensity and the unobserved heterogeneity term, i.e.

$$Y_{it}^{j} = \beta_t^j TR_i + \delta_t^j \alpha_t^j \theta_{it} TR_i + \mu_t^{j\prime} X_{it}^{j} + \alpha_t^j \theta_{it} + \varepsilon_{it}^{j},$$

where δ_t^j captures the heterogeneity effect across the unobserved traits $\alpha_t^j \theta_{it}$. This modified version is identified without further assumptions. It is indeed a special case of the model in Fruehwirth et al. (2016), which they proved to be identified with essential heterogeneity under Assumptions 1, 2 and 3a or 3b.

unpaid internships, stages and training. Indeed, volunteer work, stages, internships and training are non-standard and so unstable positions in the labor market that one may wonder if they could be viewed as proper employment in terms of building a career, accumulating human capital, generating a network, etc. Table OE.1 in Online appendix E displays the results, which are in line with the benchmark ones.

Second, we changed the definition of the treatment intensity by using, instead of the fraction of days spent in nonemployment in the first 3 years after school completion, the fraction of days spent in nonemployment during the first 2 or 4 years. The choice of measuring the intensity of early nonemployment by looking at the first 3 years after the diploma may indeed be viewed as arbitrary. Tables OE.2 and OE.3 display the effects of the fraction of days spent in nonemployment during the first 2 and 4 years after the diploma, respectively. They are in line with those obtained using the benchmark definition of treatment intensity. The only difference is that the penalties are somewhat: i) smaller if early nonemployment is computed in the first 2 years after the diploma; ii) larger if early nonemployment is defined in the first 4 years after school completion.

Third, one may wonder if the linearity with which the fraction of time spent in nonemployment affects labor market outcomes may be too restrictive. In order to allow for a more flexible impact of the treatment intensity, we specified the relations between early nonemployment and the labor market outcomes by using continuous spline functions with knots at 1 year and 2 years of cumulated early nonemployment.

The estimated coefficients are shown in Table OE.4 and reveal that there are no statistically significant changes in the slope of the function, supporting the linear specification of the benchmark model.

Fourth, we used different combinations of exclusion restrictions to test whether they play a relevant role in determining the findings. For example, one may wonder whether geographical area or local labor market conditions at birth or just after school exit may, not only affect the predetermined outcomes (the measures) and early nonemployment, but also determine future labor market outcomes. In our baseline specification, as Table OA.7 clarifies, we indeed include these controls measured at birth in the measurement equations, measured just after school completion in the early nonemployment equation and measured at time t for the labor market equation at time t. These exclusion restrictions would not be supported by the data if, for instance, being born and growing up in more disadvantaged regions or in areas characterized by worse economic conditions increases future penalties in terms of labor market success, conditional on the current status of the economy and labor market. More in detail, we proceeded by checking the main findings with two different combinations of the exclusion restrictions: (i) we included both the dummies for geographical area at birth and the regional employment, unemployment and GDP growth rates at birth in the labor market outcome equations and in the treatment equation; (ii) we further added in the specification of the labor market equations also the regional rates in the first 3 years after school completion which, in the baseline model, are only included in the treatment equation. The findings from these alternative specifications are all in line with the benchmark results and are reported Tables OE.5 and OE.6 in Online appendix E.

We ran a final check with the aim of understanding whether the findings are driven by cohort effects. We divided the sample in individuals born in the 1960s and those born later (see Table OA.4 for summary statistics). For both groups, the results are very similar to those obtained in the benchmark model and the main conclusions hold for both those born in the 1960s and those born later (see Tables OE.7 and OE.8). However, the point estimates suggest that the latter suffered larger earning penalties. We also estimated the benchmark model using only those individuals we can follow up to 25 years after school completion. Even in this case the main results are confirmed (see Table OE.9).

6 Conclusions

We estimated the impact of early nonemployment on subsequent labor market outcomes in Italian youth who exited formal education with a secondary school diploma. We traced the impact up to 25 years since school completion and evaluated it in terms of yearly labor earnings and participation in the labor market. We carried out the empirical analysis separately for men and women.

Using a factor-analytic model, we took into account time-varying unobserved heterogeneity jointly affecting the treatment intensity, i.e., the exposure to nonemployment in the three years after school completion, and subsequent labor market outcomes. Once time-varying unobserved characteristics are accounted for, we provided evidence that early nonemployment generates relevant labor market penalties for both men and women. The negative effects are very persistent in terms of earnings: they are still sizable and statistically significant 25 years after school completion. Labor market participation, measured as the fraction of days spent at work in a year, is negatively affected by early nonemployment for a shorter span, as it disappears for both men and women by the 10th year after the school completion. The early nonemployment effect on labor market participation turns to be positive and significant 25 years after school completion, suggesting those who were exposed to early nonemployment in the long run suffer smaller earnings and try to compensate with a larger participation in the labor market. While for men the effect of nonemployment is homogeneous across different levels of the unobserved traits, the labor market penalties are larger for women who are more able or more attached to the labor market. Finally, we found that the negative effects on labor market outcomes are driven by the time spent in nonemployment during the third year after diploma.

Our findings imply relevant policy recommendations. First, given that the exposure to early nonemployment generates persistent earnings scars and participation penalties shorter-lasting but still present, favoring work experience after school completion is a very urgent socioeconomic goal. The policy-maker could confine these negative consequences operating at different levels and following the general advice coming from the meta-analysis by Kluve et al. (2019) that youth policies based on profiling systems and individualized follow-up are very effective. This is a general and apparently obvious advice, which may be, however, complemented by a second peculiarity of our findings. The fact that earnings are persistently and negatively affected, while participation at the intensive margins is able to catch up after a bunch of years, suggests that those individuals who randomly experienced nonemployment after school completion were able to get reintegrated after a while, but in a downgraded track. Individuals suffering early nonemployment could have experienced the depreciation of their human capital (or they could have lost the opportunity to accumulate general human capital) and, under tighter liquidity constraints, could have been forced to lower their reservation wages and accept worse job conditions, limiting the transition to better career profiles. The policy-maker could confine these negative consequences operating at different levels. First, the policy-maker could favor training programs and apprenticeships for those who were exposed to early nonemployment, so as to facilitate their recoup of general human capital. For example, as shown by Picchio and Staffolani (2019), apprenticeships are effective ways for Italian workers to increase the probability of promotion to an open-ended contract. Second, the policy-maker could intervene facilitating the match between employers and the youth who suffered early nonemployment, for example by *ad hoc* subsidies for hiring school-leavers with difficulties in making the school-to-work transition. Moreover, relying on our results on the heterogeneous effects of youth nonemployment, these policy interventions might be particularly targeted to avoid further nonemployment spells between the second and the third years after diploma and to facilitate labor market entries for more able women. Finally, to limit the lowering of the reservation wage and the acceptance of bad jobs in downgraded tracks, the welfare state could play a role: benefits and, to circumscribe moral hazard, monitoring job search behaviors, so as to guide the school-leavers exposed to nonemployment toward more efficient and better quality job matches.

Supplementary Information The online version contains supplementary material available at https://doi.org/10.1007/s00181-025-02735-y.

Funding Open access funding provided by Università degli studi di Bergamo within the CRUI-CARE Agreement.

Declarations

Conflict of interest: None.

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