The Wounds That Do Not Heal.
The Life-time Scar of Youth Unemployment

Gianni De Fraja

University of Nottingham & Università di Roma "Tor Vergata" & C.E.P.R.

Sara Lemos

University of Leicester

James Rockey

University of Leicester

2nd February 2017

Abstract: This paper finds that unemployment shocks affect young workers for the rest of their lives. This scar of youth unemployment is concentrated in the first few years after entry in the labour market: one month of unemployment at age 18-20 cause a permanent income loss of 2%. However, unemployment after that age has no long term effect.

Keywords: Youth unemployment, Lifetime earnings, Scarring effect.

JEL-Codes: J64, J31

We would like to thank, Martin Foreaux-Koppensteiner, Andrea Ichino, Jesse Matheson, Giovanni Pica, Till von Wachter, and seminar participants at Leicester and the ECB Labour market conference, Frankfurt November 2014. De Fraja: `gianni.defraja@nottingham.ac.uk` Lemos: `sl129@leicester.ac.uk` Rockey: `james.rockey@leicester.ac.uk`

Introduction

In many OECD countries the number of young people who are out of work is at unprecedented levels (oecd.org/youth.htm). Substantial economic and social losses are created by the idleness of so many otherwise productive workers. Concern about these losses is compounded by fears about their long-term consequences. These fears stem from the well established regularity that a person's past unemployment is a good predictor of their future labour market success. A seminal contribution is Ruhm's (1991) analysis of the long term effects of *past* displacement in the US, confirmed by subsequent research, summarised in Couch and Placzeck (2010). A parallel literature has concentrated on the long term effects of *youth* unemployment: Lynch (1989), or, for the UK, Lynch (1985), Nickell et al (2002), Gregg and Tominey (2005)).[1]

For an adult, of course, youth is in the past, and yet the qualifiers "youth" and "past" carry distinct connotations. This is not just a matter of semantics, but hints instead at an important lacuna in our understanding of how past fortune shapes the causal link between individuals' labour market history and their current outcomes. The term "past" emphasises *how distant* the shock is, whereas "youth" highlights shocks occurring in a *specific period* of a person's life, regardless of how long ago that was. This distinction, thus, begs the question: What matters more for today's labour market outcomes, the timing of an unemployment shock or how far back in time it occurred?

The aim of this paper is to separate the effects of youth unemployment qua youth unemployment from its effects as past unemployment. Our main finding is that labour market shocks at the time of a person's entry in the labour market are the most damaging. The punchline of our paper is thus that *wounds from youth unemployment permanently scar, wounds from past unemployment heal*. To be precise: *ceteris paribus*, for men, an additional month of unemployment in the first three years after entry into the labour market, between ages 18 and 20, permanently lowers earnings by around 2% per year. This is a very large effect, of the same order of magnitude as the estimated decrease in earnings attributable to the reduction of one year in formal education (Harmon et al 2001). Furthermore, this effect shows no sign of abating by the time the individual

[1] Bell and Blanchflower (2011) analyse the effect of the Great Recession on young people in the the US and in the UK, while Cahuc et al (2013) consider the effect on those living in France and Germany. Also see Scarpetta et al (2010).

reaches 40 (see Figure 3 below). We also find that the age 18 to 20 is crucial: a similar negative shock during the following six years, namely between ages 21 and 26, has no long-run effects as shown again in Figure 3.

There is therefore a substantial difference in the impact of unemployment in different sub-periods of youth: the damaging permanent effect is entirely concentrated in early youth. This qualitative difference across years is blurred to the point of not being recognisable when youth is treated as homogeneous period of eight years, ages 18 to 26, as dramatically illustrated in Figure 3. Averaging across effects therefore misses the sharp difference between subperiods within youth: for the late part of youth (ages 24 to 26), the reach of a shock is determined by *distance*: its effect fades as time passes.

The difference in the impact of the subperiods of youth, theoretical interest aside, has obvious implications for the design of policies aimed at lessening the long term effects of youth unemployment, such as the European Youth Guarantee (ILO 2012). Our analysis suggests that putting young people into work is likely to be more effective in the long term when precisely targeted at new entrants in the job market. It certainly casts profound doubts on the wisdom of institutional rules which harm younger workers for the sake of older ones.

A similar inference on the relative importance of different periods of unemployment is reached when the overall effect is separated into its direct and indirect component: in addition to its long term effect on earnings, a period of unemployment in youth may also have a shorter term effect on employment, which in turn may have long term effects on earnings. That is, being unemployed at 18 may increase the chance of being unemployed at 22; and being unemployed at 22 may reduce a person's future earnings. When we separate these effects, we find that only unemployment at entry has an indirect effect. One further and crucial dimension of variation we uncover is that individuals of different abilities are affected differently by the scarring effect (Figure 6). Importantly, the severity of the scar effect increases as we move down the ability scale. An unemployment shock hitting a given cohort of young workers will be weathered more effectively, in the long term, by high ability individuals. Therefore, such a shock exacerbates the long term inequality in earnings among individuals of that cohort over and above any pre-existing pre-shock inequality. Given the high level of youth unemployment in many advanced economies, this suggests that further increases in inequality in those countries are likely to occur.

For women, the negative effect of unemployment at entry in the labour market appears to be similar (compare columns (3) and (4) in Table 1). However, in comparison to men, the effect extends, albeit weakened, into the next three years. It also appears to be reversed at ages 24 to 26. This might be because other confounding factors are at play, which we cannot control for in our empirical model, as they are not available in our dataset. One such omitted variable is hours worked per week, which is more likely to be an important determinant of earnings for women than for men (Blundell et al 2013, among others), given their different decisions concerning labour market participation. This variable has also been shown to have a non-linear effect on earnings (Goldin 2014). As a result, it is harder to disentangle the effect of youth unemployment from the effect of other factors on earnings for females using out dataset, and, following most of the literature, we focus our analysis on men.

We exploit a sizeable and long longitudinal dataset that has so far been largely unused in the UK unemployment literature. The UK Lifetime Labour Market Database combines anonymised administrative tax records and social security records into a dataset that tracks a random sample of over 600,000 individuals between 1981 and 2006. We restrict our sample to those born between 1960 and 1967; we observe their unemployment spells, along with their periods of employment and earnings, between the ages of 18 and 40 (or 46 for the older individuals). This allows us to follow individuals for an extensive period of 23 to 29 years after their entry into the labour market. Observing unemployment spells, measured as the number of weeks unemployed within the year, constitutes a more precise and informative measure of unemployment shocks than most measures in the recent US literature, which are based on a binary variable measuring "displacement" (see for example Jacobson et al 1993 and 2005, who study long term "scarring" unemployment effects in Pennsylvania and Washington). Our dataset is also rich in geographical detail, since it records change of address within the year. This makes it particularly well suited to controlling for specific characteristics of the local labour market, which affect both youth unemployment and later earnings. More crucially, we are able to allow returns to unobserved ability to vary across local labour markets. This is important since individuals might move to labour markets where their individual skills are more valuable. Thus, an important feature of our model is that we are able to control for endogenous geographical mobility, following Moretti's (2004) seminal use of detailed geographical information. Separating the effect of unemployment shocks from the effect of unobserved individual ability, and from the effect of local labour market characteristics, and, more crucially, from the effect of the interaction of the two, is vital to ensure identification of the causal effect of early unemployment (and any associated permanent effects) on earnings. Another important feature of our model is that we are able to allow the effect of youth unemployment to vary with the time of entry in the labour market (Figure 5). To allow for this, we follow Oreopoulos et al (2012), who studied Canadian graduates and found that the regional conditions at the time of entry into the labour market matter more for earnings than contemporaneous regional unemployment.

The paper is organised as follows. Section 2, which summarises the established theoretical background on the long term effects of unemployment, motivates the econometric specification described in Section 3. Section 4 presents the data. Our main results and some robustness tests are in Section 5. Section 6 concludes in the light of the existing literature.

2 The model

The theoretical model is a straightforward Mincerian equation. In its most general form, we can write:

$$w_i^t = f\left(Z_i, \lambda^t, \boldsymbol{e}_i^t, \varepsilon_i^t\right), \tag{1}$$

where w_i^t are person i's earnings in period t, Z_i is a vector of personal characteristics such as years of education, innate ability, family background, and so on. λ^t measures the labour market conditions in period t, given for example by the local unemployment rates and other labour demand side variables, $\boldsymbol{e}_i^t$ is person i's "experience" at time t, and ε_i^t is a random shock affecting earnings.

Early theoretical models, such as Ben-Porath's (1967), captured experience $\boldsymbol{e}_i^t$ as a single figure, typically the total number of years individual i had spent in work at date t. This reflects the idea that, when employed, a person receives both formal training and "on-the-job" training.[2] If information on experience is not available, "potential experience", given by the number of years not spent in formal education, is often used as a proxy, as in Mincer's landmark studies (1958 and 1974) among others.

Specifically, one can think of at least two conceptual reasons why the importance of past events for present day outcomes depends on when these events were experienced, necessitating a more sophisticated approach. Firstly, it is possible that recent occurrences may matter more than distant ones: negative events fade in importance, and, conversely, work skills acquired in the distant past become less relevant. Secondly, time may matter because some periods in life are more important than others. It is by now firmly established that this is the case for the formation of cognitive and non-cognitive abilities. Heckman and his co-authors have convincingly demonstrated that people's early environment is substantially more important than their later environment in determining these abilities (Cunha et al 2010). Thus, in this paper, we investigate if the acquisition of labour market skills obeys a similar temporal pattern.

To formalise these ideas, replace e_i^t in (1) with a vector $\left(e_i^t, e_i^{t-1}, \ldots, e_i^2, e_i^1\right)$, which measures the experience in each of the years since the time of potential entry in the labour market, year 1. By convention, events which occurred before year 1 are captured by the time invariant individual characteristics term, Z_i. Experience in each period is influenced by a variety of factors, but to highlight the link between periods, we explicitly state it as a function of past experience and the period specific random component, by writing $e_i^2 = e^2\left(e_i^1, \varepsilon_i^2\right)$, $e_i^3 = e^3\left(e_i^2, e_i^1, \varepsilon_i^3\right)$, and so on. For example, the lack of experience of "entry level" jobs caused by an early unemployment shock hinders access to jobs higher up the jobs ladder, and, hence, reduces experience at this level. Thus (1) is replaced by:

$$w_i^t = f^t\Big(Z_i, \lambda^t, e^t\Big(e^{t-1}\left(\ldots\right), \ldots, e^2\left(e_i^1, \varepsilon_i^2\right), e_i^1, \varepsilon_i^t\Big), \ldots, e^2\left(e_i^1, \varepsilon_i^2\right), e_i^1, \varepsilon_i^t\Big). \tag{2}$$

Naturally, (2) includes e_i^t as an argument to account for the obvious direct effect of contemporaneous events on earnings. Since experience is a "good thing", in the sense that increasing it should enhance earnings, we measure it so that the sign of the derivative of w_i^t with respect to past experience $e_i^{t-\tau}$ should be non-negative. The partial derivative,

[2] Whether generic or job specific, training enhances a person's productivity, and, thus, future earnings. When formal training is unpaid, a further trade-off arises, as workers must choose between formal training and human capital accumulation while employed (Mroz and Savage 2006).

$\partial w_i^t / \partial e_i^{t-\tau}$, is the direct effect of date $t-\tau$ experience on date t earnings, whereas the total derivative, $\mathrm{d}w_i^t / \mathrm{d}e_i^{t-\tau}$, is its overall effect. From the latter, we can conceptually separate a direct and an indirect effect. Taking the case $t = 2$ as an illustrative example, we can write:

$$\frac{\mathrm{d}w_i^2}{\mathrm{d}e_i^1} = \frac{\partial w_i^2}{\partial e^2}\frac{\partial e^2}{\partial e_i^1} + \frac{\partial w_i^2}{\partial e_i^1} \tag{3}$$

where the total effect, $\mathrm{d}w_i^2/\mathrm{d}e_i^1$ on the RHS of (3), is decomposed into the indirect effect of period 1 experience shocks on period 2 earnings via period 2 experience and the direct effect of period 1 experience shocks on period 2 earnings. As we explain in the next section, our econometric strategy allows us to separate the direct from the indirect effect. To do so is important, as the direct effect sheds light on relative importance of the different links of the causal chain of transmission turning past shocks into present outcomes, while the total effect measures the relative importance of shocks occurring at different times. If

$$\frac{\partial w_i^t}{\partial e_i^{t-\tau}} > 0$$

for some values of $\tau > 0$ and t, then the effects of past experience are persistent: events which occurred at time $t-\tau$ influence positively earnings at time t.

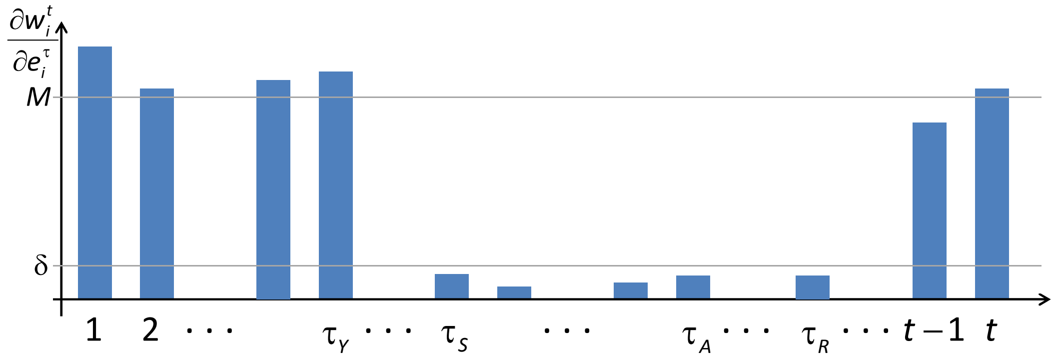
The long term importance of labour market experience is, of course well established, and the focus of our paper is on the details of these long term effects. Some events have only temporary effects and fade away with time. To express this hypothesis formally, we can write

$$\left|\frac{\partial w_i^t}{\partial e_i^s}\right| < \delta, \qquad \text{for } t > t^* \text{ and } s = 1, \ldots, s^*, \tag{4}$$

for some t^* and s^* with $t^* > s^*$ and for a suitably small value of δ. In words, (4) asserts that if an individual is old enough (has entered the labour market at least t^* years ago), then early events (those that occurred in the first s^* years after entry in the labour market) have a "small" (less than δ) direct effect on earnings in the years more recent than t^*. More succinctly, the effect of events experienced $t - s^*$ or more years ago *fades with time*.

If, instead, experience gained in some years had a permanent effect on earnings, in the way that formal education has, then (4) is replaced by the hypothesis that, for some

Figure 1:
The partial derivatives of the earnings at time t implied by Assumption 1.



s^* and t^* with $t^* > s^*$ and $M > 0$,

$$0 < M < \frac{\partial w_i^t}{\partial e_i^s}, \qquad \text{for } t > t^* \text{ and } s = 1, \ldots, s^*. \tag{5}$$

That is, experience acquired before year s^* has a "big" effect (larger than M) on earnings beyond time t^*, irrespective of the length of the period $t - s^*$: shocks occurring before date s^* *leave a permanent scar*.

We measure experience as the negative of unemployment, which is available in our data for each person in each year. The idea that youth unemployment leaves permanent "scars" but the effects of later unemployment "heals" can be cast formally in the following hypothesis..

Assumption 1 *There exist τ_Y, τ_S, τ_A, and τ_R, with $\tau_Y \leq \tau_A \leq \tau_S \leq \tau_R$, and positive constants M and δ, such that for $t \geqslant \tau_R$*

$$\textit{Scarring effect:} \qquad \frac{\partial w_i^t}{\partial e_i^\tau} > M, \qquad \tau = 1, \ldots, \tau_Y, \tag{6}$$

$$\textit{Healing effect:} \qquad 0 \leqslant \left| \frac{\partial w_i^t}{\partial e_i^\tau} \right| < \delta, \qquad \tau = \tau_S, \ldots, \tau_A. \tag{7}$$

Where $e_i^\tau < 0$ is the negative of the unemployment suffered in period τ by individual i. In words, (6) states that a shock suffered in the first τ_Y periods has a permanent effect on earnings, whereas according to (7), a shock suffered in periods τ_S to τ_A has a small effect on earnings, even though periods after τ_S are more recent. Figure 1 illustrates this: recent events, those happening later than τ_R, may have again a large impact, due to the very fact that they are recent. Testing the hypothesis stated in Assumption 1 is the aim of our empirical analysis

3 The econometric specification

If we assume equation (2) to be log-linear, and we consider earnings up to the age of 40, then its empirical counterpart may be written as follows.[3]

$$\log w_i^t = Z_i^t \alpha^t + \underbrace{\gamma^t e_i^t}_{\text{Current Unemployment}} + \underbrace{\beta_{t-1}^t e_i^{t-1} + \beta_{t-2}^t e_i^{t-2} + \ldots + \beta_{s+1}^t e_i^{s+1} + \beta_s^t e_i^s}_{\text{Past Unemployment Scarring Effects}}, \tag{8}$$

$$t = 19, \ldots, 40, \qquad s = 18, \ldots, t-1.$$

Where, as in (1) and in (2), w_i^t are the earnings of individual i in period t, Z_i^t is a vector of potentially time-varying individual characteristics, and $-e_i^t$ is the negative of the number of weeks that individual i was unemployed in year t. In most of the papers cited, for example in Jacobson et al (1993) and Couch and Placzek (2010), only a job loss, a "displacement event", is observed. Our data is much more nuanced, allowing us to measure experience as the number of weeks of unemployment in each year. This enables us to account for the fact that the loss of a job may affect future earnings differently when it is followed by a long spell of unemployment, and when a new job is found after a two week unemployment interval.

The vector of individual specific characteristics in (8), Z_i^t, may be decomposed into an observable component X_i^t, and an unobservable component V_i^t. As individual characteristics influence both earnings and early unemployment, unobserved heterogeneity is a common problem with this type of model, which makes it hard to disentangle the permanent negative effect of random unemployment shocks from the influence of an unobserved variable, such as "ability", or "earning potential", on both youth employment and future earnings. Intuitively, to the extent that employers recognise a relatively unproductive worker, they are less likely to employ him, and because he is relatively unproductive he also experiences lower earnings later in life. In his early contribution,

[3]The main regression sets 40 as the highest age; this cut-off age constitutes the best compromise between the number of observations and the number of scar coefficients. As Figure 4 below illustrates, the results are robust to considering the full sample to age 46, when only one cohort is included in the regression sample.

Ellwood (1982 p 346) remarks that this is likely to mar cross sectional studies. In some cases, the problem is alleviated by the inclusion of a rich set of observable individual characteristics, or by using local unemployment levels at the time of entry as an instrument (Gregg and Tominey 2005). In this paper, our strategy is to define a rich set of fixed effects with which to capture heterogeneity across individuals, labour markets, time, and cohorts: a strategy made possible by the fact that we observe a large number of individuals for well over 20 years, with detailed geographical information for each year. The vector of fixed effects we create allows us to filter out effectively individual, area, time and cohort characteristics and their interactions. This enables us to identify the coefficients in the vector β.

We formally present our treatment of the fixed effects by writing the unobserved individual characteristics as follows:

$$V_i^t = \theta_i \mu_a + \eta_{ac}^t + \varepsilon_i^t. \tag{9}$$

The error term (9) has three components. Let time-invariant individual unobserved determinants of earnings, such as innate ability or education, be denoted by θ_i. The return to these unobserved characteristics may well vary across different labour markets. These differences might motivate individuals to move to areas where their specific skills are more valued. This makes location decisions endogenous. This potential problem is analogous to that convincingly addressed by Moretti in his study of the externalities of higher education (2004). We follow his approach by including the interaction of the individual fixed effects θ_i with the area fixed effects, μ_a. This captures the differences in how particular individual characteristics, including education, are rewarded in different labour markets.[4] The first term in (9) thus allows the returns to individuals' characteristics to vary in an unrestricted way across labour markets.

The timing of entry into the labour market is also potentially important, as shown by Oreopoulos et al (2012) for Canadian graduates. In the second term in (9), we therefore include cohort fixed effects. These can vary across local labour markets and across time.

[4] In the UK, in the period we consider, formal education is normally completed by 23 years of age. This is the earliest that individual earnings are used in the regressions. It seems therefore reasonable to assume that the effects of education (which we do not observe in our data) on earnings are adequately captured by the fixed effect θ_i.

We exploit the detailed geographical information in our data and interact cohort effects with time fixed effects and with area fixed effects. Formally, η^t_{ac}, captures the shocks affecting cohort c in the local labour market in area a in period t.

The third term in (9), ε^t_i, is the individual specific transitory component of log wages which is allowed to be correlated with other individuals in the same cohort c entering a given local labour market a in a given year and otherwise independent across individuals. That is, we cluster by cohort and initial local labour market. Note that the inclusion of the interaction fixed effects encompasses the single fixed effects: formally, $\theta_i\mu_a$ includes θ_i, and the triple interaction term η^t_{ac} includes the area fixed effects, the cohort fixed effects, and the time fixed effects, as well as the "double" interaction fixed effect terms.

To sum up, our identification strategy is the following: for those who do not move, we control for any systematic variation in labour market opportunities, with time and cohort varying regional effects, η^t_{ac}. For those who do move, perhaps in search of local labour markets better suited to their specific skills, this is extended with the inclusion of a "Moretti" (2004) term, $\mu_a\theta_i$, which allows the returns to individual unobservable characteristics to vary across local labour markets. Formally, our identification assumption is,

$$E[\varepsilon^t_i e^t_i|\theta_i\mu_a, \eta^t_{ac}, X_{it}] = E[\varepsilon^t_i e^{t-1}_i|\theta_i\mu_a, \eta^t_{ac}, X_{it}] = \cdots = E[\varepsilon^t_i e^s_i|\theta_i\mu_a, \eta^t_{ac}, X_{it}] = 0. \tag{10}$$

If we re-write (8) long-hand, to focus on the effects of the previous labour market experience for individual i up to the age of 40, we obtain:

$$\begin{aligned}
\log w^{40}_i &= X^{40}_i\alpha^{40} + \underbrace{\gamma^{40}e^{40}_i} + \underbrace{\beta^{40}_{39}e^{39}_i + \beta^{40}_{38}e^{38}_i + \ldots + \beta^{40}_{19}e^{19}_i + \beta^{40}_{18}e^{18}_i} + V^{40}_i,\\
\log w^{39}_i &= X^{39}_i\alpha^{39} + \underbrace{\gamma^{39}e^{39}_i} + \underbrace{\beta^{39}_{38}e^{38}_i + \ldots + \beta^{39}_{19}e^{19}_i + \beta^{39}_{18}e^{18}_i} + V^{39}_i,\\
&\vdots\\
\log w^{20}_i &= X^{20}_i\alpha^{20} + \underbrace{\gamma^{20}e^{20}_i} + \underbrace{\beta^{20}_{19}e^{19}_i + \beta^{20}_{18}e^{18}_i} + V^{20}_i,\\
\log w^{19}_i &= X^{19}_i\alpha^{19} + \underbrace{\gamma^{19}e^{19}_i} + \underbrace{\beta^{19}_{18}e^{18}_i} + V^{19}_i,
\end{aligned} \tag{11}$$

where, as in (8), in each equation the first brace is the effect of current unemployment, the second the effects of past unemployment spells. We can write the above system compactly in matrix form:

$$\log \boldsymbol{w}_i = \boldsymbol{\alpha X}_i + \boldsymbol{\gamma E}_i + \boldsymbol{\beta E}_i^L + \boldsymbol{V}_i, \tag{12}$$

where $\boldsymbol{w}_i = \left(w_i^{40}, \ldots, w_i^{19}\right)$, $\boldsymbol{X}_i = \left(X_i^{40}, \ldots, X_i^{19}\right)$, $\boldsymbol{E}_i = \left(e_i^{40}, \ldots, e_i^{19}\right)$, $\boldsymbol{E}_i^L = \left(e_i^{39}, \ldots, e_i^{18}\right)$ and $\boldsymbol{V}_i = \left(V_i^{40}, \ldots, V_i^{19}\right)$ are 22-dimensional vectors, $\boldsymbol{\alpha}$ and $\boldsymbol{\gamma}$ are 22 by 22 diagonal matrices, with $\left(\alpha_i^{40}, \ldots, \alpha_i^{19}\right)$ and $\left(\gamma^{40}, \ldots, \gamma^{19}\right)$ along the diagonal, and $\boldsymbol{\beta}$ is the following upper triangular matrix:

$$\begin{bmatrix} \beta_{39}^{40} & \beta_{38}^{40} & \beta_{37}^{40} & \cdots & \beta_{19}^{40} & \beta_{18}^{40} \\ & \beta_{38}^{39} & \beta_{37}^{39} & \cdots & \beta_{19}^{39} & \beta_{18}^{39} \\ & & \beta_{37}^{38} & \cdots & \beta_{19}^{38} & \beta_{18}^{38} \\ & & & \ddots & \vdots & \vdots \\ & & & & \beta_{19}^{20} & \beta_{18}^{20} \\ & & & & & \beta_{18}^{19} \end{bmatrix}. \tag{13}$$

Writing (8) as (12) makes it clear that (8) is not identified: each equation has the same number of coefficients as the number of observation per individual. To achieve identification, we impose restrictions on the matrix $\boldsymbol{\beta}$. As a first step, we consider a two year interval for the effect of past unemployment on earnings. Formally, we set

$$\beta_s^t = \beta_s^{t+1}, \qquad t = 23, 25, 27, \ldots, 39. \tag{14}$$

This parallels the restriction imposed by Oreopoulos et al (2012), and reduces multicollinearity. Effectively, we study the effect on earnings measured over two years, rather than one.

With the next set of restrictions, we concentrate on isolating the effects of unemployment for entrants in the labour market. We split the period from age 18 to 26 into three three-year intervals. *Entry* into the labour market, indexed by the letter E, age 18 to 20 inclusive; *Youth*, letter Y, age 21 to 23; and *early Adulthood*, letter A, age 24 to 26. We begin by imposing

$$\beta_s^t = \beta_E^t, \quad \text{if} \quad s = 18, 19, 20 \quad \text{and} \quad t > 22; \tag{15}$$

$$\beta_s^t = 0, \qquad \text{otherwise.} \tag{16}$$

Restriction (15) posits that a spell of unemployment at 18 is equivalent to a spell of the same duration unemployment at 20. The coefficients β_E^t measure the effect of a labour market entrant's unemployment on their earnings from age 23 onwards. Unemployment at ages greater than 20 is restricted in (16) not to have any long term direct effect.

The results obtained with restrictions (15)-(16) are reported in the first column of Table 1. When we impose these restrictions, the estimated coefficients measure the *total* impact of being unemployed between ages 18 and 20. As noted earlier, however, if labour market outcomes at a given time are influenced by past experience, then unemployment between ages 18 and 20 harms labour market prospects at later ages: if someone is unemployed at 19, and if experience matters for labour market prospects at 25, they are also more likely to be unemployed at 25. As long as there is an independent effect of unemployment at 25 on labour market outcomes at 40, this exacerbates the direct negative effects of an entrant's unemployment on his or her labour market outcomes at 40.

To disentangle the direct and indirect effect of being unemployed when young, that is to evaluate the relative magnitude of the two terms on the RHS of (3), we modify (16) to include additional unemployment shocks as explanatory variables. We do so in two stages. First we add the coefficients that estimate the effects of "youth" unemployment, defined as the years between ages 21 and 23 inclusive: thus we replace (16) with

$$\beta_s^t = \beta_Y^t, \qquad \text{if} \qquad s = 21, 22, 23 \qquad \text{and} \qquad t > 24; \tag{17}$$

$$\beta_s^t = 0, \qquad \text{otherwise.} \tag{18}$$

The results obtained with restrictions (15), (17), and (18) are reported in the second column in Table 1. Finally, we add a third possible effect, "early adulthood" unemployment, replacing (18) with

$$\beta_s^t = \beta_A^t, \qquad \text{if} \qquad s = 24, 25, 26 \qquad \text{and} \qquad t > 28; \tag{19}$$

$$\beta_s^t = 0, \qquad \text{otherwise.} \tag{20}$$

Note that we follow Oreopoulos et al (2012) and, in (16), (18), and (20), we require that the coefficients capturing the effects of unemployment after a certain age are 0. Thus we disregard potential effects that are close in time to the current unemployment: time t unemployment affects earnings at time t, whereas unemployment at time $t-1$ does not. The estimated coefficients thus reflect any persistence in the unemployment process.

To sum up, with the assumptions on the fixed effects in (9) and the restrictions on the βs given in (15), (17), and (19)-(20), the regression specification (8) becomes:

$$\ln w_{iac}^t = X_i^t \alpha^t + \gamma^t e_i^t + \underbrace{\beta_E^t \sum_{s=18}^{20} e_i^s}_{\substack{\text{Scarring effects} \\ \text{of unemployment} \\ \text{for Entrants}}} + \underbrace{\beta_Y^t \sum_{s=21}^{23} e_i^s}_{\substack{\text{Scarring effects} \\ \text{of unemployment} \\ \text{for Youths}}} + \underbrace{\beta_A^t \sum_{s=24}^{26} e_i^s}_{\substack{\text{Scarring effects} \\ \text{of unemployment} \\ \text{for early Adults}}} + \mu_a \theta_i + \eta_{ac}^t + \varepsilon_i^t,$$

$$t = 23\text{-}24, 25\text{-}26, \ldots, 39\text{-}40. \tag{21}$$

The coefficients β_E^t, β_Y^t, and β_A^t measure the effects of unemployment when "Entrant" (age 18-20), "Youth" (age 21-23) or "Early Adult" (age 24-26). The last three terms are the error term, and are described in detail in (9).

4 The data

We use data from the Lifetime Labour Market Database (LLMDB), which combines tax and social security records into a dataset which follows a 1% random sample of National Insurance Number (NINo) holders, amounting to $647,068$ individuals between 1978 and 2006.[5] The LLMDB is, therefore, a rich, accurate, long and broad longitudinal dataset. It contains information on sex, and date and country of birth. For each year, it contains information on the address of residence, earnings, nature of employment (employee or self-employed), number of weeks of employment and unemployment in the year, and on benefits received. Similarly to many administrative datasets, the LLMDB does not contain information on education or family background. As they are time-invariant, these characteristics are controlled for with the inclusion of individual fixed effects, as

[5] A fresh cohort of individuals enters the data every year and is followed from then on. This administrative data is derived from a number of datasets linked by the unique individual identifier, the National Insurance number, which is allocated to British nationals just before they turn 16 years old and to foreign nationals who are eligible to work and/or claim benefits.

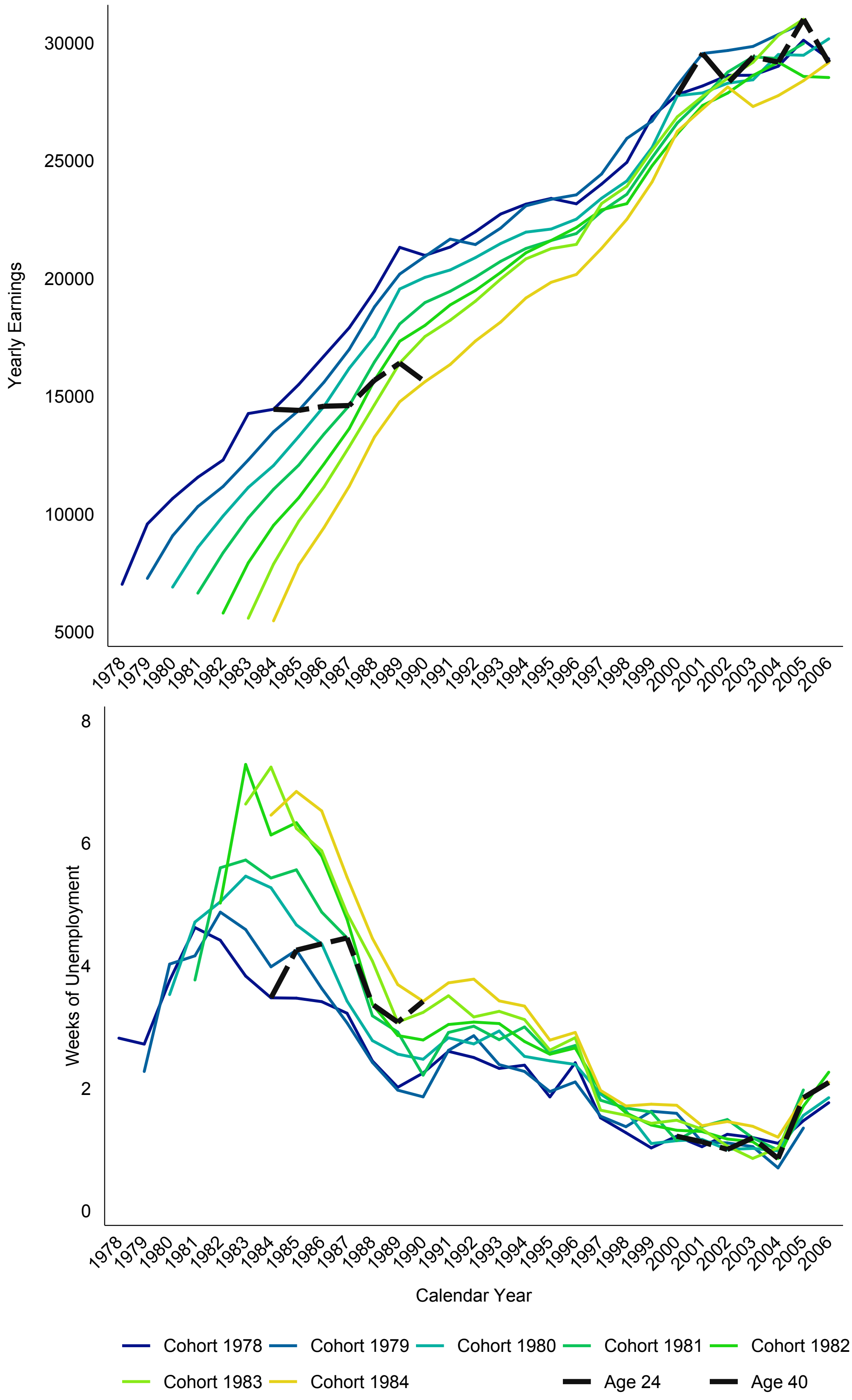
explained above. Unlike Orepopolous et al (2012), we can measure employment and unemployment at the beginning of individuals' careers, rather than having to proxy it with local labour averages. Given the importance of the time of entry into the labour market to this literature, our dataset also improves on other studies, such as Gregg and Tominey (2005), Jacobson et al (1993 and 2005) and Couch and Placzeck (2010), which observe one cohort only.[6]

We restrict our sample to UK nationals, for whom we observe earnings, benefits and employment and unemployment continuously between ages 18 to 40. The earliest cohort in our dataset comprises individuals born in 1960, who enter the labour market in 1978. The last cohort are those born in 1966, who enter in 1984. We exclude individuals who are recorded as ever being self-employed; these constitute 12.1% of the sample. We do so for several reasons. Firstly, income from self-employment may not be recorded accurately. Secondly, those self-employed might have more opportunity to understate their employment and earnings to reduce their tax liability. Thirdly, in the absence of information on balance sheets, we are unable to distinguish an individual's earnings from the return on the capital that the self-employed often own (Gollin 2002). Each of these reasons exacerbates measurement error in earnings, and in a way that is unlikely to be orthogonal to unemployment during the individual's youth. As a robustness check, we run the model excluding, for individuals who have ever reported being self-employed. Only the years when they report being self-employed. This increases the sample, whilst making the panel unbalanced, and the results for this case are reported in column (5) of Table 1.

The two main quantitative variables in our model are earnings and the number of weeks of unemployment in each year. While the data for those employed are very reliable and accurate, as is derived from the LLMDB tax records, the data for those unemployed are derived from the LLMDB benefit records. Benefit data, in any given year, will only capture those unemployed who meet the criteria for entitlement to a benefit. So, when it is recorded, the record is completely accurate, as these data form the basis for benefit payments. However, benefit data will often be missing, as it is simply not recorded for

[6]The LLMDB has been used in recent years to study income mobility and changes in inequality (Gardiner and Hills 1999, Dickens and McKnight 2008a), whether policy can affect the intensity of job search (Petrongolo 2009), the assimilation of immigrant workers into the UK labour market (Lemos 2013 and 2014, Dickens and McKnight 2008b), and the link between unemployment and low pay (Gosling et al 1997).

Figure 2:
Yearly Earnings and Weeks of Unemployment per Year by Cohort - Men



Note: Average year earnings, measured in 2004 pounds (top panel), and average number of weeks unemployed per year (bottom panel), for men born in the various years (dark lines for individuals born earlier), in the calendar year measured along the horizontal axis. The thick dashed lines joint the age 24 points and age 40 points for each cohort.

individuals who are not in receipt of benefits. In this case, it does not necessarily imply zero unemployment: it might mean that an individual is not entitled to benefits or that, even when entitled, is not claiming benefits.

We base the construction of our measure of unemployment on two variables recorded in the data: "number of employed weeks" and "number of unemployed weeks" in the year. For just over half of the observations in our sample, the number of employed and unemployed weeks in the year add up to 52, giving a consistent measure of unemployment in the year. When they do not, we determine unemployment for individual i in year t as the number of weeks individual i is recorded as unemployed. If this figure is missing, then individual i's unemployment in the year is defined as 52 minus the number of weeks individual i is recorded as employed. In other words, we turn to "employed weeks" only when "unemployed weeks" is missing, the idea being that when "unemployed weeks" is recorded, then it is recorded accurately, because it is the measure that forms the basis on which the benefits to be paid to claimants are calculated. This imputation is uncontroversial, and it populates the unemployment variable for over 97% of the observations in the LLMDB. Attributing a subordinate role to "employed weeks", which is used only when "unemployed weeks" is missing, is justified on the grounds that the calculation of a person's income tax liability, unlike the payment of benefits, is independent of the recorded number of employed weeks.[7] In the remaining observations, "employed weeks" and "unemployed weeks" do not add up to 52, and we calculate unemployment as the share of "unemployed weeks" in the sum of these two numbers.

The final sample for our main results comprises 14348 men and 13602 women observed for 23 years (ages 18 to 40 inclusive), a total of $645,656$ observations. Summary statistics for our dataset are presented in Figure 2 and in Table A1 in the Appendix. These report the cohort average rate of unemployment for each year for men entering the labour market between 1978 and 1984, and their average gross yearly real earnings, adjusted by the Retail Price Index and measured in 2006 pounds. The colour of the various lines move from blue to yellow for later cohorts. We have also plotted in dashed black the line

[7]And indeed the two measures come from data collect by two different government departments, the Department for Work and Pensions, and the Inland Revenue, now renamed HMRC, for different purposes, pensions and benefits, and income tax, respectively.

that joins the values for each cohort at age 24 and at age 40.[8]

The vector X_i^t in (21) includes an indicator variable that takes a value of 1 if the individual is in receipt of a benefit in the year. It also includes four indicator variables to identify those who are structurally or long-term unemployed: the first is 1 in year t if individual i is unemployed for all the 52 weeks of year t; the second is 1 if an individual is unemployed for two whole years, that is for all the 104 weeks of two consecutive years. And similarly, for three and five years. These indicator variables are meant to capture the non-linear effects of protracted periods of structural unemployment on long-run wages.

The last terms in (21) are the fixed effects interacted with one another, as explained in (9). These are essential features of our econometric specification, and allow us to separate the effects of unemployment shocks from the influence of individual characteristics, the conditions of the local labour market where individuals find themselves or move to, and the difference of the effects of the business cycle on different areas of the country. We use the administrative division of the country into 409 "local authority districts" to define area fixed effects, indexed by a.[9]

The information on individual addresses is complete from 1997 onwards. Prior to this, it is missing for 36% of the observations. Given that our identification strategy is based on the local authority district where each individual resides, care must be taken in dealing with missing addresses. While most moves in the UK are within a short distance and therefore subsequent local areas of residence are good predictors of previous ones, some people will have moved further and these individuals may be systematically different, potentially those whose labour market skills are more specialised. To account for this difference, we introduce an artificial second set of area fixed effects: the *inferred* area fixed effects are denoted by $a^0 \in \{1, \ldots, 409\}$. Individual i's location in year t is given by a if he is recorded as living in area a in year t; it is given by a^0 if his address is missing and his next recorded address is a, $t = 23, \ldots, 40$. This assumption is a mid-point

[8]The summary statistics reported in Figure 2 and Table A1 are adjusted to make them comparable over time, given changes in the underlying administrative processes generating the data. Our regression estimates use the unadjusted data and control for these differences using fixed effects.

[9]A full list of local authority districts is available at data.gov.uk/dataset/local-authority-districts-uk-2012-names-and-codes. We use these, instead of "travel to work areas" (TTWAs), which are not defined consistently for our entire sample period and have been identified as problematic, especially prior to recent revisions (Coombes and Openshaw 1982). They are also larger and hence identify location less accurately.

between the “naïve” alternatives of treating subsequent addresses as having correlation 1 or 0 with previous ones. In the former case we would impute a single “notional-national” address to observations where the information on location is missing.[10] The results for this treatment of missing observations are reported in Column (6) of Table 1. If instead we believed that current address had perfect predictive power of previous addresses we would not distinguish between a and a^0, implicitly assuming that the labour market consequences of individuals' moves are on average zero. The results for this case are reported in Column (7), of Table 1. These extremes can be seen as upper and lower bounds containing the true estimate, and comparisons between columns (3), (6) and (7) in Table 1 suggest that our results do not depend on our treatment of missing address observations.

5 Results

The results from our preferred specification are reported in Table 1: as we explained, the regression in (21) is estimated for the subset of the individuals who are in the sample continuously from age 18 to 40, and it is pooled across cohorts. All coefficients are multiplied by -100, to measure the “scar” effect in percentage terms.

The first coefficient in each column of Table 1 shows the effect of current unemployment on current earnings: being unemployed for an additional week at age 40, other things equal, brings about a reduction in earnings at age 40 of approximately 2.3%, for men and 2% for women, numbers which are very close to $\frac{1}{52}$, the proportionate earnings loss of a week of employment. We take this correspondence as a strong suggestion that our specification does capture all the other individual characteristics that may be determining earnings.

The rest of the table gives the long term effect of unemployment on current earnings. In the first column we report, for men, solely the effect of “unemployment as Entrant”. That is, with restrictions (15)-(16), which impose the restrictions $\beta_Y^t = \beta_A^t = 0$ in (21). If a man is unemployed for an additional week between ages 18 and 20, his earnings during two year intervals between ages 23 and 40 are lowered by the percentage amount

[10] We could also drop all observation with a missing address: this is an inferior option, as it would reduce the sample, make the panel unbalanced, and omit an individual's observations in a way likely to be correlated to that individual employment record.

Table 1: Long term effects of early unemployment

	Entrant	Entrant & Youth	Whole Period	Women	Self-Employed	National Address	First Address
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Current Unemployment	2.312***	2.321***	2.318***	1.949***	2.262***	2.423***	2.291***
	(0.051)	(0.051)	(0.051)	(0.035)	(0.046)	(0.047)	(0.044)
Entrant Unemployment (18-20)							
On Earnings Aged 23-24	0.320***	0.321***	0.321***	0.562***	0.334***	0.383***	0.340***
	(0.040)	(0.040)	(0.040)	(0.041)	(0.038)	(0.031)	(0.031)
On Earnings Aged 25-26	0.439***	0.447***	0.447***	0.621***	0.442***	0.534***	0.468***
	(0.041)	(0.051)	(0.051)	(0.051)	(0.046)	(0.039)	(0.042)
On Earnings Aged 27-28	0.511***	0.537***	0.537***	0.662***	0.510***	0.607***	0.537***
	(0.042)	(0.052)	(0.052)	(0.051)	(0.048)	(0.039)	(0.044)
On Earnings Aged 29-30	0.607***	0.570***	0.566***	0.633***	0.546***	0.641***	0.582***
	(0.045)	(0.054)	(0.054)	(0.055)	(0.050)	(0.040)	(0.046)
On Earnings Aged 31-32	0.602***	0.553***	0.552***	0.607***	0.525***	0.634***	0.548***
	(0.044)	(0.053)	(0.053)	(0.059)	(0.050)	(0.041)	(0.047)
On Earnings Aged 29-30	0.550***	0.498***	0.498***	0.604***	0.499***	0.602***	0.496***
	(0.044)	(0.053)	(0.053)	(0.059)	(0.050)	(0.041)	(0.046)
On Earnings Aged 35-36	0.542***	0.491***	0.492***	0.541***	0.502***	0.580***	0.494***
	(0.046)	(0.055)	(0.055)	(0.063)	(0.052)	(0.042)	(0.048)
On Earnings Aged 37-38	0.526***	0.487***	0.492***	0.512***	0.500***	0.568***	0.494***
	(0.047)	(0.056)	(0.056)	(0.062)	(0.052)	(0.041)	(0.049)
On Earnings Aged 39-40	0.510***	0.500***	0.506***	0.488***	0.518***	0.587***	0.509***
	(0.046)	(0.057)	(0.056)	(0.062)	(0.052)	(0.043)	(0.050)
Youth Unemployment (21-23)							
On Earnings Aged 25-26		−0.013	−0.013	0.056	−0.010	−0.029	−0.046
		(0.045)	(0.045)	(0.048)	(0.041)	(0.033)	(0.037)
On Earnings Aged 27-28		−0.047	−0.047	0.061	−0.033	−0.015	−0.064
		(0.046)	(0.046)	(0.052)	(0.044)	(0.034)	(0.040)
On Earnings Aged 29-30		0.073	0.042	0.179***	0.033	0.081*	0.003
		(0.048)	(0.053)	(0.053)	(0.050)	(0.039)	(0.047)
On Earnings Aged 31-32		0.096	0.091	0.151**	0.086	0.116**	0.084
		(0.050)	(0.055)	(0.058)	(0.051)	(0.040)	(0.049)
On Earnings Aged 29-30		0.100*	0.091	0.121*	0.076	0.099*	0.069
		(0.051)	(0.055)	(0.060)	(0.051)	(0.040)	(0.049)
On Earnings Aged 35-36		0.100	0.098	0.143*	0.082	0.115**	0.075
		(0.052)	(0.056)	(0.062)	(0.052)	(0.041)	(0.050)
On Earnings Aged 37-38		0.077	0.110	0.178**	0.078	0.145***	0.089
		(0.052)	(0.056)	(0.064)	(0.053)	(0.040)	(0.051)
On Earnings Aged 39-40		0.024	0.067	0.200**	0.040	0.104*	0.045
		(0.053)	(0.057)	(0.064)	(0.054)	(0.041)	(0.052)
Early Adulthood Unemployment (24-26)							
On Earnings Aged 29-30			0.060	−0.137**	0.051	0.043	0.057
			(0.047)	(0.042)	(0.043)	(0.037)	(0.043)
On Earnings Aged 31-32			0.007	−0.141**	0.005	−0.001	0.011
			(0.046)	(0.046)	(0.044)	(0.035)	(0.042)
On Earnings Aged 29-30			0.016	−0.158***	0.009	0.013	0.019
			(0.049)	(0.047)	(0.046)	(0.038)	(0.046)
On Earnings Aged 35-36			0.000	−0.206***	−0.020	0.023	0.005
			(0.050)	(0.050)	(0.046)	(0.037)	(0.046)
On Earnings Aged 37-38			−0.070	−0.259***	−0.089	−0.057	−0.069
			(0.051)	(0.050)	(0.048)	(0.037)	(0.047)
On Earnings Aged 39-40			−0.092	−0.275***	−0.114*	−0.073*	−0.092
			(0.051)	(0.051)	(0.048)	(0.037)	(0.048)
N	319481	319481	319481	302328	358864	319481	319481
Number of Individuals	14348	14348	14348	13602	16082	14348	14348

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. The dependent variable is the (log of) total annual earnings. The estimation uses felsdvreg (Cornelissen 2008). Standard errors are clustered by η_{ac}, i.e. cohorts defined by initial local area and are below the associated coefficients. Reported coefficients are obtained using the estimator described in (21) and measure the percentage effect of an increased week of unemployment in the three age brackets, 18-20, 21-23, and 24-26, on earnings in different subsequent periods. All specifications also include dummy variables recording the receipt of benefits and long-term unemployment in during a year, as well as individual, local labour market, cohort, and time fixed effects and their interactions as explained in the discussion of (9). Columns (1)-(3) report results for men from our prefered treatment of missing addresses and exclude those ever self-employed. Column (4) reports results for women. Columns (5)-(7) present robustness tests. In Column (5) individuals who were self-employed in some years between the ages of 18-40 are included, though not for those years when they were self-employed. In Column (6) missing addresses are allocated to a single address, the same for every observation; in Column (7) missing addresses are assumed to be identical to the earliest recorded address.

in the corresponding row of the table. Thus, for example, the coefficient "On earnings aged 35-36" says that one additional week unemployment suffered between ages 18 and 20 (inclusive) decreases annual earnings received between ages 35 and 36 (inclusive) by 0.54%. Similarly, the rows in the second column, labelled "Youth", report coefficients measuring the effect of an additional week of unemployment between ages 21 and 23: (17)-(18) replace (16), that is β_Y^t is unrestricted in (21). The third column adds the period of "Early adulthood", ages between 24 and 26. The fourth column is the specification of column 3 for the women in the sample.

We see a strong direct effect of unemployment at entry (age 18-20) on lifetime earnings, in this table up to the age of 40, although when we consider a longer time frame there is no indication that this effect is dampened (Figure 4). Columns (2) and (3) add first unemployment between ages 21 and 23, and then between 24 and 26. The similarity between the coefficients in the first block in the first three columns is due to the lack of statistical significance of the coefficients in the second and third block. This suggests that the long term effects of unemployment for an entrant are fully captured by β_E, and that unemployment in Youth and Early Adulthood does not have a long term effect on earnings.

The fourth column reports the results for women. They are similar to those for men: the effect of Entrants' unemployment and the effect of current unemployment on earnings are similar in sign and size. Unlike for men, there are some significant coefficients in Youth (age 21 to 23). Counterintuitively, moreover, unemployment in early Adulthood (age 24 to 26) seems to have a positive effect on future earnings. As discussed in the introduction, we feel less confident about the results for women, as we lack information on two important determinants of earnings: the number of hours worked per week and childbearing decisions. This might also explain the difference with Gregg's findings (2001) of a weaker persistence for women, since they can control for a large array of individual characteristics, including, of course, those related to childbearing.

The results reported in column (3) of Table 1 are illustrated in Figure 3. In the diagram, the horizontal axis gives two year age windows, and the vertical axis, the coefficients which measure the scar, namely the effect of an additional week of unemployment, at the three age brackets we consider, 18-20 ($-\beta^t_E$, shown as the solid line), 21-23 ($-\beta^t_Y$, shown as the dashed line), and 24-26 ($-\beta^t_A$, the dotted line), on the yearly earnings at the age window marked on the horizontal axis. The thin lines are the 95% confidence intervals around the estimated coefficients. Figure 3 shows how the scarring effect of unemployment for labour market Entrants increases with time, settling by around age 30 at around 0.5%. Conversely, the effect of an extra week's unemployment in Youth and Early Adulthood (age 21-26) is not significantly different from 0. To get a handle on the magnitude of the scarring effect, the coefficients indicate that one year of unemployment between ages 18 and 20 determines a long term permanent earnings loss exceeding 22% per year. This is a large effect, and is in the upper end of the US estimates of the earnings loss due to displacement, which range from 7% (Stevens 1997) to over 20% (Jacobson et al 1993), and with Gregg and Tominey's (2005) cross sectional IV estimates for the UK, which are between 13% and 21%.

The light green solid line in Figure 3 depicts the estimated coefficients when the restrictions on the βs are given by:

$$\beta^t_Y = \beta^t_s, \qquad s = 18, \ldots, 26, \qquad t > 22; \tag{22}$$

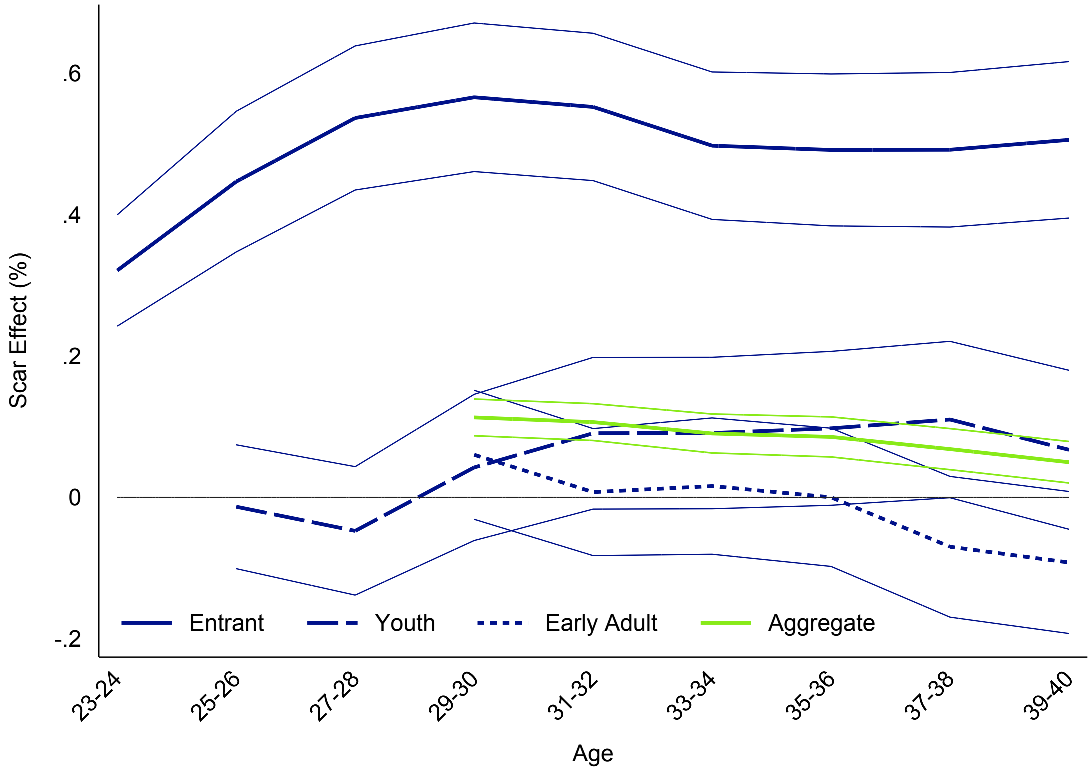
$$\beta^t_s = 0, \qquad s \geqslant 27, \qquad t > s, \tag{23}$$

that is when $\beta^t_V = \beta^t_Y = \beta^A_Y$ in (21), so that no distinction is made among the ages between 18 and 26: these βs give the overall effect of unemployment experienced between ages 18 and 26 on future earnings. The thin lines include the 95% confidence interval.

The coefficients for this case are reported in Column (5) of Table A2, and they indicate a much lower and slightly decreasing effect of unemployment in the first nine years after entry in the labour market. The strong suggestion emerging from the comparison of the three blue lines and the single light green line is that lumping together all the spells of unemployment between 18 and 26 is misleading as it misses the large difference between Entrants, Youth and Early Adulthood unemployment highlighted by our regressions and shown in Figure 3. Indeed, averaging large and statistically significant coefficients with those close to 0 for later years might conceal altogether the long run effect of youth unemployment.[11]

[11]Column (6) of Table A2 in the appendix reports the results of a robustness check using two alternative

Figure 3:
The scar effect of youth unemployment



Estimated coefficients from equation (21) for the effect of Entrant, Youth, and early Adulthood unemployment, β_E^t (solid line), β_Y^t (dashed line), and β_A^t (dotted line); the dashed lines include the 95% confidence intervals.

We consider yet another split of the youth period. Column (6) in Table A2 and Figure A.1 in the Appendix report the coefficients estimated in the case when this nine year period is split in two sub-periods, that is when the set of restriction (15)-(18) is replaced by

$$\beta_1^t = \beta_s^t, \qquad s = 18, \dots, 22, \qquad t > 24; \tag{24}$$

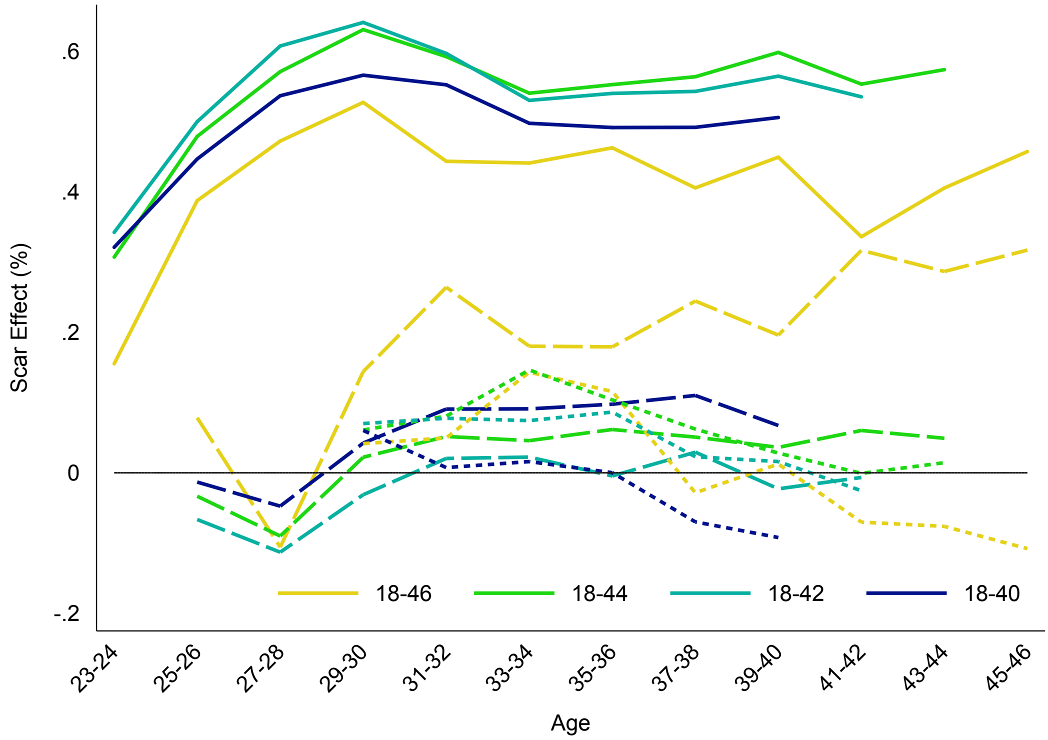
$$\beta_2^t = \beta_s^t, \qquad s = 23, \dots, 26, \qquad t > 27; \tag{25}$$

$$\beta_s^t = 0, \qquad s \geqslant 27, \qquad t > s. \tag{26}$$

Where the subscripts 1 and 2 label the first and the second part of Youth and Early Adulthood. These are depicted by the aquamarine lines in Figure A.1, also in the

periods. We find, unsurprisingly given our other main results, a precisely estimated, but smaller effect for the first period (ages 18 to 22), and no effect for the second (ages 23 to 27).

Figure 4:
Scar Effect of Youth Unemployment for Different Cut-off Ages



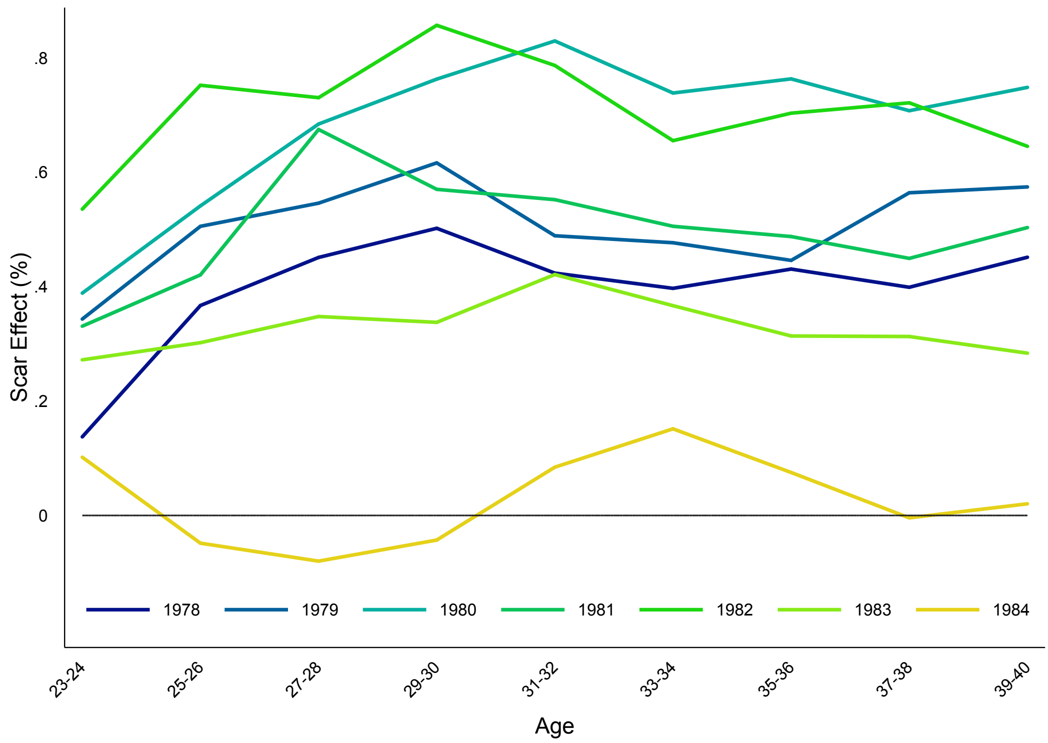
Note: Estimated coefficients for the long term effect of unemployment for different cut-off ages, estimated from the corresponding version of equation (21) for the effect of unemployment for Entrant, Youth, and early Adult. The black curves are the same as in Figure 3, and, for each age group, solid, dashed, and dotted lines denote the coefficients for Entrant, Youth, and early Adult, respectively. The values of the coefficients used to draw the lines are in first four columns of Table A2. We have not included confidence intervals.

Appendix. These confirm our main finding: early shocks scar, later ones do not.

Columns (5)-(7) of Table 1 confirm that the main message of our paper is robust to changes in the empirical specification. Column (5) shows that our estimates change only marginally when, for individuals who report ever being self-employed, we include observations for years in which they are not self-employed.[12] In columns (6) and (7), we report estimates obtained when missing address information is not inferred as explained above, by filling missing information with a "fictitious" local area given by their earliest known address, controlling for the fact that the address is missing. Column (6) simply replaces all missing addresses with an amorphous "national" address, which captures the "average national labour market", with the implicit assumption that everyone had the same chance to have been at any given location. Column (7) replaces a missing address

[12]For example, if individual i from cohort entering in 1978 is recorded as self-employed in years 1994 and 1997, we drop only these observation from the estimations reported in column (5).

Figure 5:
Scar Effect of Entry Unemployment for Different Cohorts



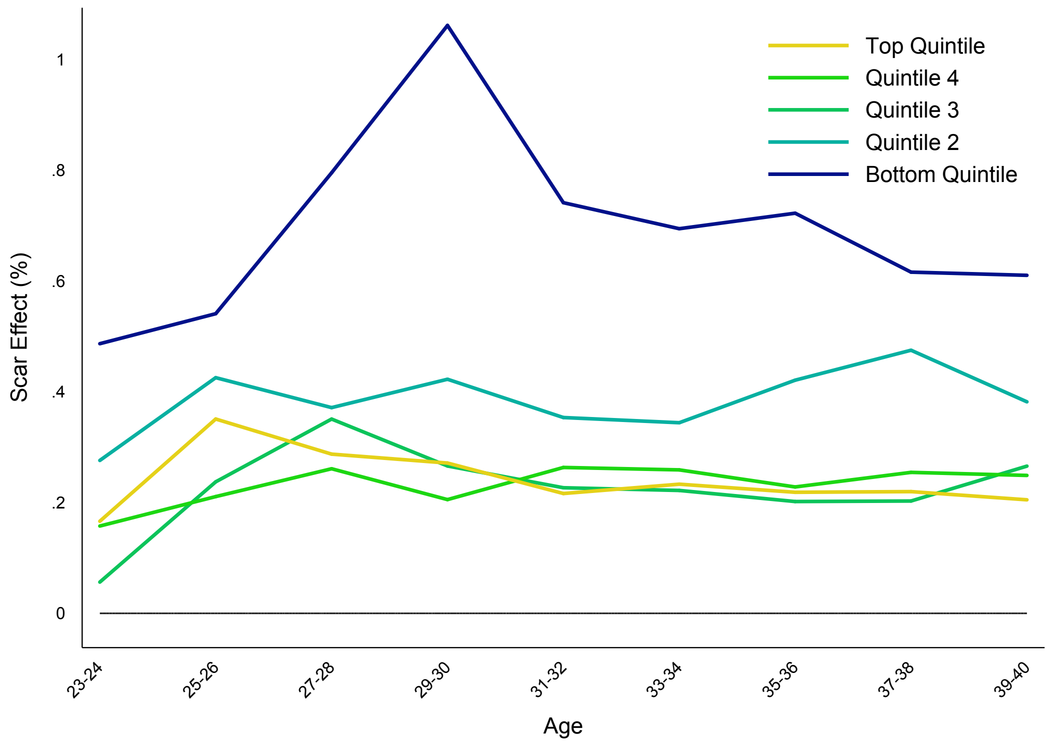
Note: Coefficients β_E^t, calculated from each subsample of individuals in the same cohort in the sample of the main regression, column (3) in Table 1. The values of the coefficients used to draw the lines are in Table A4. We have not included confidence intervals.

with the next recorded address, without accounting for the fact that the information is in fact missing. In both columns results are very similar to those in column (3), perhaps with some evidence of a small scar effect in the Youth period (21 to 23) in column (6) only. This suggests that the results are not sensitive to our treatment of missing addresses.

Figure 4 illustrates the results obtained when we allow the reach of bad shocks ot extend beyond age 40. The results are once again robust: those observed until 46, for whom unemployment at age 21-23 has some long term effect. The smaller sample size (only one cohort is observed until age 46) reduces the precision of the estimates, as shown in Table A2 in the Appendix.

Oreopoulos et al (2012) study the effect of the business cycle on the importance of the scarring effect for Canadian graduates. Similar analyses for American and Japanese men are carried out by Kahn (2010) and by Genda et al (2010). Our data allows us to

Figure 6:
Scar Effect of Entry Unemployment on Individuals of Different Abilities.



Note: Coefficients β_E^t, calculated from the subsamples of individuals made from the quintile according to the rank determined by their earnings potential. The values of the coefficients used to draw the lines are in Table A4. We have not included confidence intervals.

ask the same question for different types of workers in a different country. Are there systematic differences in the effects for different cohorts which enter the labour market at different stages in the business cycle? The results of this analysis are summarised in Figure 5, where, for Entrants only, we break down by cohort the pattern shown in Figure 3. The coefficients for Youth and Early Adulthood are depicted in Figure A.2 in the Appendix. The estimates of β_E for the 1984 cohort are less precisely estimated, and appear to be the only cohort to diverge from the overall pattern derived in the pooled regression.

Apart from the possible effects of the recovery from the deep recession of the early 1980s, it is difficult to ascertain other possible causes for this difference in this cohort.

To close the paper, we reprise one of the questions addressed by Oreopoulos et al (2012), namely whether the scarring effect is different for individuals with different abilities. Following their strategy, we split the sample into five “ability quintiles”, and allocate individuals to quintiles according to their predicted permanent income estimated on the basis of the permanent incomes of women who have lived in the same areas at the same time.[13] The results of this exercise for those in the 18-40 age range are presented in Figure 6, again only for the shocks in the Entry period. Individuals in the lowest ability group suffer the severest scar. Furthermore, Table A3 in the appendix shows that the scar effect is not significantly different from 0 for workers in the upper half of the ability distribution. The worsening of the scar effect for less able workers is a worrying aspect of our results: youth unemployment seems to reduce most the income of the least paid workers. This exacerbates the inequality of lifetime incomes.

As we discussed in Section 3, we do not observe whether an individual is a university student. To the extent that students experience low unemployment when aged 18 to 22, and high earnings later in life, not being able to identify students would bias downward the scar effect. Given that university that students are probably over-represented in the higher quartiles of the ability distribution we might be concerned that the lower scar effect for these quartiles, in part, reflects this bias. However, there are two reasons to doubt that this is the case. Firstly, we note that only approximately 10% of 18 year olds went to university in the late 1970s and early 1980s. Secondly, if anything, the scar effect is larger for more recent cohorts, against a trend of increases in the proportion of school leavers entering higher education.

6 Concluding remarks

Past unemployment lowers earnings for the rest of a worker's life. These long term effects are extensively documented for the US (Table 1 in Couch and Placzeck 2010, p 574, summarises studies using both administrative and survey panel dataset), the UK (Arulampalam et al 2001 reviews a number of papers of the UK, all of which report evidence of scarring), and in Japan (Genda et al 2010), among other countries. The paper by Schmillen and Möller (2012), which follows cohorts of American men born between 1950 and 1954, also highlights the importance of early shocks for lifetime labour

[13]Using the income of equivalent women to categorise men into quintiles overlooks the importance of child-bearing and part time work as determinants of women's incomes. It seems however preferable to using the same data and model both to categorise individuals into quintiles and to estimate the effects for each quintile.

market outcomes. Our paper contributes to this literature by documenting that not all unemployment is equal. We report the distinct, permanent effects of being unemployed at the very beginning of one's working life.

Knowing that there is a marked difference between the effect of unemployment at the time of entry and the effect of later unemployment on earnings is different from knowing why this should be the case. The literature has suggested several possible causes of a permanent effect of unemployment, ranging from the decay of human capital (Pissarides 1992), to psychological discouragement or habituation effects (Clark et al 2001), to stigma effects (Vishwanath 1989, Lockwood 1991, Kübler and von Weizsäcker 2003, Biewen and Steffes 2010), to the nature of the search technology (Tatsiramos 2009). Neal (1995) studies the scar effect for workers who subsequently find a new job in the same sector to identify the extent to which the loss of earnings is due to the sector specific loss of human capital. Given the large differences in long term effects between Youth and Early Adulthood unemployment we uncover, it would be unsatisfactory if the only explanation for this difference were that the young are more vulnerable to these long term effects.

Understanding the causes of these differences would assist the design of policies specifically directed at relieving youth unemployment. It would also have implications for macroeconomic policy more generally, given the potentially large difference in the long term costs and benefits of tackling the unemployment of individuals at different ages. A promising explanation is the importance of experimentation and learning (Papageorgiou 2014). Wee (2016) argues that those entering the labour market during a recession may suffer a wage scar. This is because of reduced early career mobility limiting learning and the accumulation of human capital. This would be in line with our results, which show that these effects are particularly pronounced for those at the very beginning of their careers.

References

Arulampalam, Wiji, Paul Gregg, and Mary Gregory (2001), "Unemployment scarring." *Economic Journal*, 111, F577–F584.

Bell, D. N. F. and D. G. Blanchflower (2011), "Young people and the Great Recession." *Oxford Review of Economic Policy*, 27, 241–267,

Ben-Porath, Yoram (1967), "The production of human capital and the lifecycle of earnings." *Journal of Political Economy*, 75, 352–365.

Biewen, Martin and Susanne Steffes (2010), "Unemployment persistence: Is there evidence for stigma effects?" *Economics Letters*, 106, 188–190.

Blundell, Richard, Antoine Bozio, and Guy Laroque (2013), "Extensive and intensive margins of labour supply: Work and working hours in the US, the UK and France*." *Fiscal Studies*, 34, 1–29,

Cahuc, Pierre, Stéphane Carcillo, Ulf Rinne, and Klaus F Zimmermann (2013), "Youth unemployment in old Europe: the polar cases of France and Germany." *IZA Journal of European Labor Studies*, 2, 18,

Clark, Andrew, Yannis Georgellis, and Peter Sanfey (2001), "Scarring: The psychological impact of past unemployment." *Economica*, 68, 221–241.

Cornelissen, Thomas (2008), "The stata command felsdvreg to fit a linear model with two high-dimensional fixed effects." *The Stata Journal*, 8, 170–189.

Couch, Kenneth A and Dana W Placzek (2010), "Earnings losses of displaced workers revisited." *American Economic Review*, 100, 572–589.

Cunha, Flavio, James J. Heckman, and Susanne M. Schennach (2010), "Estimating the technology of cognitive and noncognitive skill formation ." *Econometrica*, 78, 883–931,

Dickens, Richard and Abigail McKnight (2008a), "Changes in earnings inequality and mobility in Great Britain 1978/9-2005/6."

Dickens, Richard and Abigail McKnight (2008b), "Assimilation of migrants into the British labour market."

Ellwood, David T (1982), "Teenage unemployment: Permanent scars or temporary blemishes?" In *The Youth Labor Market Problem: Its Nature, Causes, and Consequences* (Richard B Freeman and David A Wise, eds.), 349–390, University of Chicago Press, Chicago.

Gardiner, Karen and John Hills (1999), "Policy implications of new data on income mobility ." *The Economic Journal*, 109, 91–111,

Genda, Yuji, Ayako Kondo, and Souichi Ohta (2010), "Long-term effects of a recession at labor market entry in Japan and the United States." *Journal of Human Resources*, 45, 157–196,

Goldin, Claudia (2014), "A grand gender convergence: Its last chapter ." *American Economic Review*, 104, 1091 – 1119.

Gollin, Douglas (2002), "Getting income shares right." *Journal of Political Economy*, 110, 458–474.

Gosling, Amanda, Paul Johnson, Julian McCrae, and Gillian Paull (1997), "The dynamics of low pay and unemployment in 1990s Britain." Technical report, Institute for Fiscal Studies Working Paper.

Gregg, Paul (2001), "The impact of youth unemployment on adult unemployment in the NCDS." *Economic Journal*, 111, 626–653.

Gregg, Paul and Emma Tominey (2005), "The wage scar from male youth unemployment." *Labour Economics*, 12, 487–509.

Harmon, Colm, Ian Walker, and Niels Westergaard-Nielsen (2001), "Introduction." In *Education and Earnings in Europe: A Cross-Country Analysis of the Returns to Education* (Colm Harmon, Ian Walker, and Niels Westergaard-Nielsen, eds.), Edward Elgar.

ILO (2012), "Youth Guarantees: A Response to the Youth Employment Crisis?" ILO, Geneva.

Jacobson, Louis S, Robert J LaLonde, and Daniel G Sullivan (1993), "Earnings losses of displaced workers." *American Economic Review*, 83, 685–709.

Jacobson, Louis S, Robert J LaLonde, and Daniel G Sullivan (2005), "Estimating the returns to community college schooling for displaced workers." *Journal of Econometrics*, 125, 271–304.

Kahn, Lisa B. (2010), "The long-term labor market consequences of graduating from college in a bad economy." *Labour Economics*, 17, 303–316,

Kübler, Dorothea and Georg von Weizsäcker (2003), "Information cascades in the labor market." *Journal of Economics*, 80, 211–229.

Lemos, Sara (2013), "Immigrant economic assimilation: Evidence from UK longitudinal data between 1978 and 2006." *Labour Economics*, 24, 339–353, Lemos, Sara (2014), "The immigrant-native earnings gap across the earnings distribution." *Applied Economics Letters*, 22, 361–369,

Lockwood, Ben (1991), "Information externalities in the labour market and the duration of unemployment." *Review of Economic Studies*, 58, 733–753.

Lynch, Lisa M. (1985), "State dependency in youth unemployment." *Journal of Econometrics*, 28, 71–84,

Lynch, Lisa M (1989), "The youth labor market in the eighties: Determinants of re-employment probabilities for young men and women." *Review of Economics and Statistics*, 71, 37–45.

Mincer, Jacob (1958), "Investment in human capital and personal income distribution." *Journal of Political Economy*, 66, 281–302.

Mincer, Jacob (1974), *Schooling, Experience and Earnings*. Columbia University Press, New York.

Moretti, Enrico (2004), "Estimating the social return to higher education: Evidence from longitudinal and repeated cross-sectional data." *Journal of Econometrics*, 121, 175–212.

Mroz, Thomas A and Timothy H Savage (2006), "The long-term effects of youth unemployment." *Journal of Human Resources*, 41, 259–293.

Neal, Derek (1995), "Industry-specific human capital: Evidence from displaced workers." *Journal of Labor Economics*, 13, 653–677.

Nickell, Stephen, Patricia Jones, and Glenda Quintini (2002), "A picture of job insecurity facing British men." *The Economic Journal*, 112, 1–27,

Oreopoulos, Philip, Till von Wachter, and Andrew Heisz (2012), "The short- and long-term career effects of graduating in a recession." *American Economic Journal: Applied Economics*, 4, 1–29.

Papageorgiou, Theodore (2014), "Learning your comparative advantages." *Review of Economic Studies*, 81, 1263–1295.

Petrongolo, Barbara (2009), "The long-term effects of job search requirements: Evidence from the UK JSA reform." *Journal of Public Economics*, 93, 1234–1253,

Pissarides, Christopher A (1992), "Loss of skill during unemployment and the persistence of employment shocks." *Quarterly Journal of Economics*, 107, 1371–1391.

Ruhm, Christopher J (1991), "Are workers permanently scarred by job displacements?" *American Economic Review*, 81, 319–324.

Scarpetta, Stefano, Anne Sonnet, and Thomas Manfredi (2010), "Rising Youth Unemployment During The Crisis " OECD Social, Employment and Migration Working Papers, 106, OECD Publishing.

Schmillen, Achim and Joachim Möller (2012), "Distribution and determinants of lifetime unemployment." *Labour Economics*, 19, 33–47.

Stevens, Ann Huff (1997), "Persistent effects of job displacement: The importance of multiple job losses." *Journal of Labor Economics*, 15, 165–188.

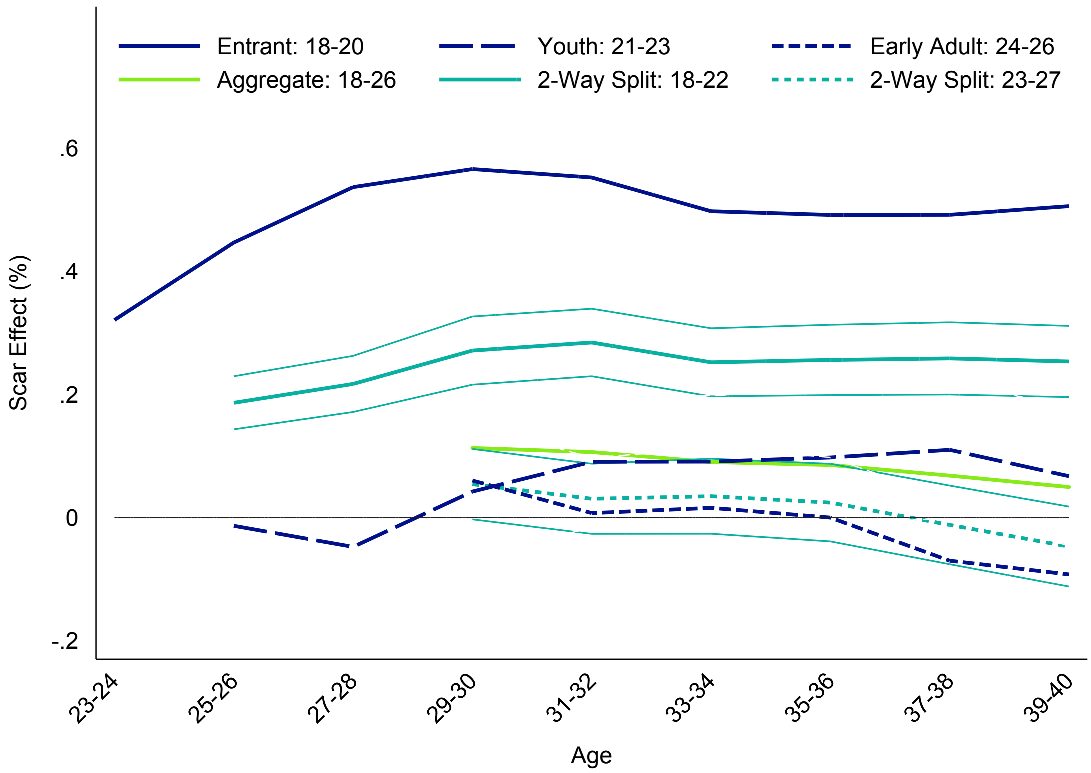
Tatsiramos, Konstantinos (2009), "Unemployment insurance in Europe: Unemployment duration and subsequent employment stability." *Journal of the European Economic Association*, 7, 1225–1260.

Vishwanath, Tara (1989), "Job search, stigma effect, and escape rate from unemployment." *Journal of Labor Economics*, 7, 487–502.

Wee, Shu Lin (2010), "Delayed learning and human capital accumulation: The cost of entering the job market during a recession " mimeo.

Appendix: Additional Tables and Figures

Figure A.1:
The scar effect of youth unemployment



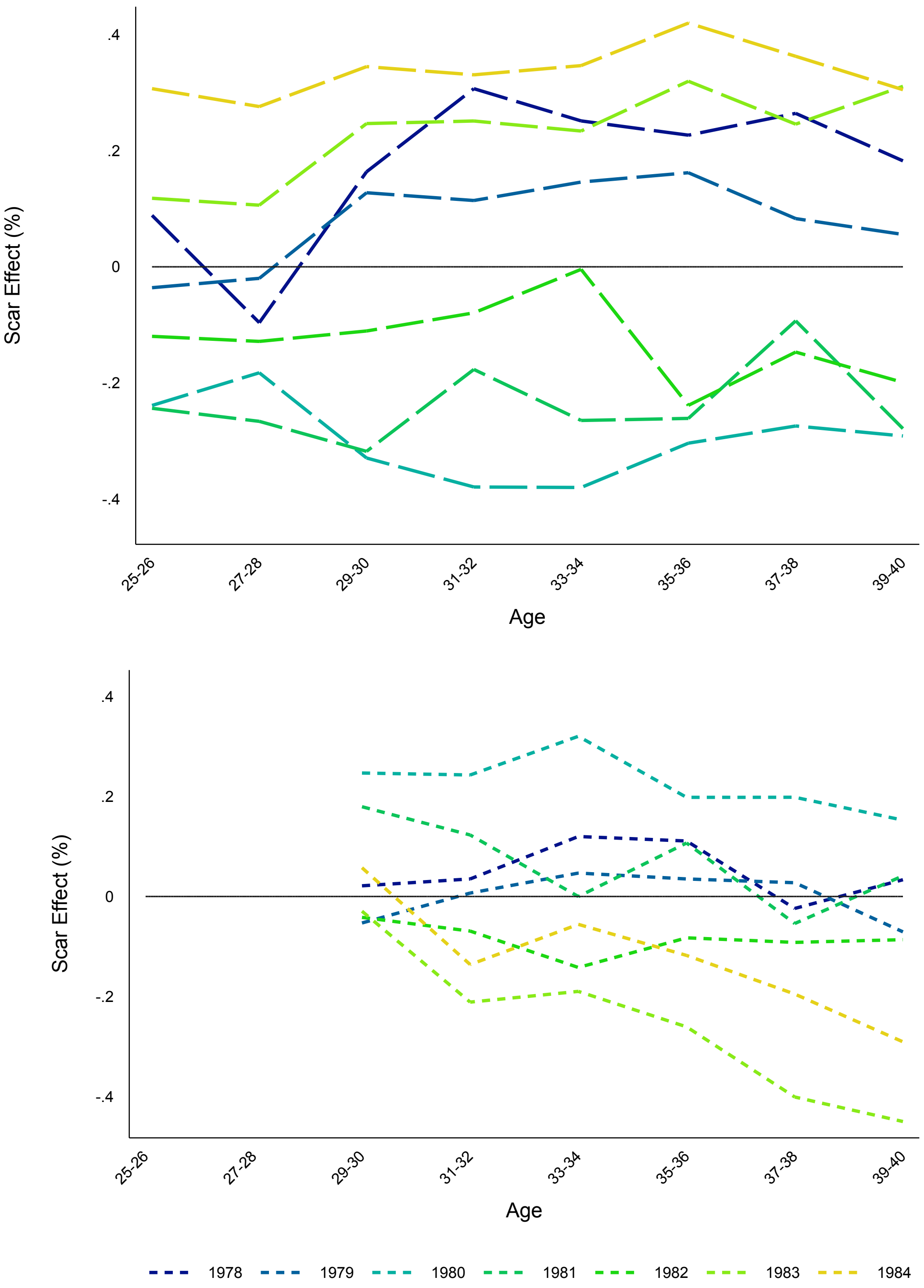
Note: The blue lines report the estimated coefficients from equation (21) for the effect of Entrant, Youth, and early Adulthood unemployment, β_E^t (solid line), β_Y^t (dashed line), and β_A^t (dotted line). The light gren line depicts the coefficients when (21) is estimated with the restrictions (22)-(23). These are the same lines as in Figure 3, without the confidence intervals. The aquamarine lines are the coefficients when (21) is estimated with the restrictions (24)-(26). For all lines, the dashed lines include the 10% confidence intervals.

Table A1:
Summary Statistics for individuals in Sample for Table A1.

	1960	1961	1962	1963	1964	1965	1966	**Total**
u Age 18-20	9.66	13.25	18.59	22.11	25.50	25.78	23.10	**19.58**
sd	19.76	23.73	28.91	31.74	32.78	32.07	29.81	**29.24**
21-23	16.60	19.02	20.36	20.40	20.70	18.93	16.02	**18.88**
sd	29.17	30.90	31.72	31.46	30.63	29.89	27.81	**30.32**
24-26	15.08	15.00	14.25	13.77	14.29	15.85	16.31	**14.92**
sd	29.48	28.68	27.78	27.31	28.46	29.51	30.03	**28.75**
Earn 23-24	12229	11998	11936	11932	12454	13114	13313	**12409**
sd	6875	7235	7929	7649	8163	8693	8221	**7848**
25-26	13800	13890	14417	14778	14791	14544	14023	**14316**
sd	8476	8687	9728	9040	9722	10614	9544	**9418**
27-28	16233	16881	16850	16095	15494	15405	15546	**16093**
sd	10624	10783	11446	11370	11400	12040	11883	**11376**
29-30	18306	18208	17337	16631	16737	16869	16508	**17242**
sd	11763	14924	12915	12274	13979	13191	14337	**13396**
31-32	18378	18416	18269	17524	17521	17729	18042	**17991**
sd	12441	12905	13557	14306	15170	15640	16952	**14463**
33-34	19205	19441	18795	17972	18529	19914	20876	**19236**
sd	13710	15307	14416	15829	15476	18793	21797	**16644**
35-36	19451	19821	19695	19591	20709	22203	22675	**20559**
sd	14389	17442	16460	18019	18760	23908	23121	**19094**
37-38	20461	21659	21830	21857	22041	23414	22401	**21936**
sd	16010	24366	20003	27207	20625	24809	20905	**22273**
39-40	22704	23462	23058	22236	22202	24444	23302	**23053**
sd	22904	25356	20691	26052	23458	32200	26050	**25398**
41-42	23638	23877	23394	22036	22075	0	0	**23019**
sd	20546	27446	23736	22485	22663	0	0	**23533**
43-44	23794	23936	23818	0	0	0	0	**23850**
sd	21963	22492	26957	0	0	0	0	**23935**
45-46	24348	0	0	0	0	0	0	**24348**
sd	28231	0	0	0	0	0	0	**28231**
Benef 23-24	0.29	0.31	0.33	0.33	0.33	0.30	0.29	**0.31**
25-26	0.26	0.27	0.26	0.25	0.28	0.31	0.31	**0.28**
27-28	0.22	0.21	0.23	0.26	0.29	0.31	0.30	**0.26**
29-30	0.20	0.23	0.27	0.28	0.30	0.30	0.30	**0.26**
31-32	0.24	0.25	0.27	0.30	0.29	0.28	0.26	**0.27**
33-34	0.26	0.26	0.28	0.28	0.28	0.26	0.26	**0.27**
35-36	0.26	0.25	0.25	0.28	0.28	0.28	0.27	**0.27**
37-38	0.24	0.24	0.25	0.29	0.29	0.27	0.25	**0.26**
39-40	0.25	0.25	0.26	0.28	0.27	0.24	0.23	**0.26**
41-42	0.26	0.25	0.24	0.25	0.25	0	0	**0.25**
43-44	0.24	0.22	0.24	0	0	0	0	**0.23**
45-46	0.22	0	0	0	0	0	0	**0.22**
N	2065	2154	2164	2089	1985	1966	2001	**14424**

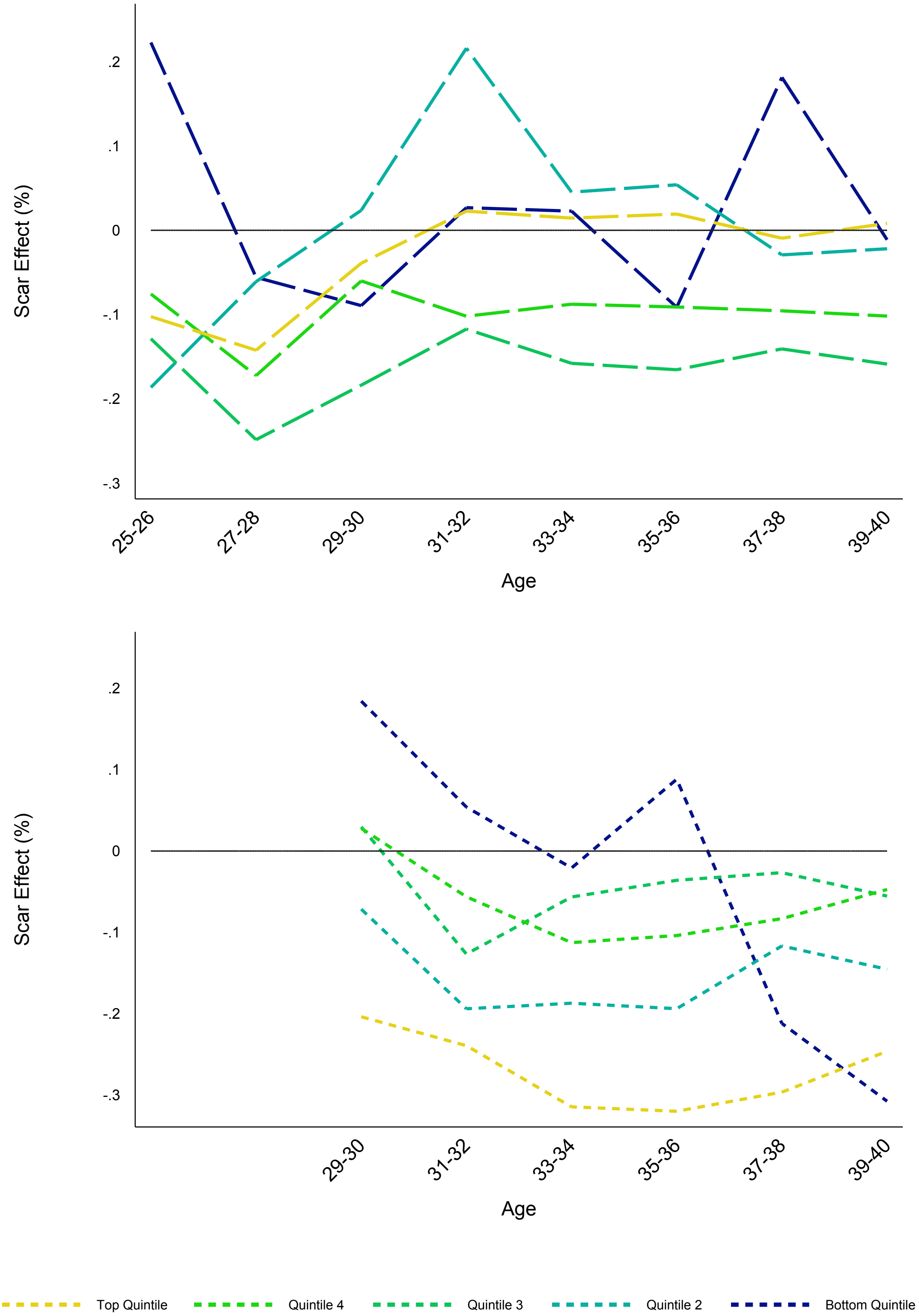
Note: The upper part reports the unemployment rate of UK male individuals born in 1960-1966 at age 18-20, 21-23 and 24-26. the middle part their average yearly earnings at ages 23-24 to 45-46. The third part is the proportion who claimed some of the benefits available

Figure A.2:
Scar Effects of Youth and Early Adult Unemployment for Different Cohorts



Note: The lines report the estimated coefficients from (21) for the effect of Youth, and Early Adulthood unemployment corresponding to those in Figure 5. That is the coefficients β_Y^t and β_A^t, calculated from each subsample of individuals in the same cohort in the sample of the main regression, column 3 in Table 1.

Figure A.3:
Scar Effects of Youth and Early Adult Unemployment on
Individuals of Different Abilities.



Note: The lines report the estimated coefficients from (21) for the effect of Youth, and Early Adulthood unemployment corresponding to those in Figure 6. That is the coefficients β^t_Y and β^t_A, calculated for each subsample of individuals in the same ability quintile according to the rank determined by the earnings potentials of women who have lived in the same places at the same time.

Table A2:
Different cut-off ages; different splits of the period of youth.

	(1)	(2)	(3)	(4)	(5)	(6)
	Sample 18-40	Sample 18-42	Sample 18-44	Sample 18-46	Whole Youth	Entrant + Early Adult
Current Unemployment	2.318***	2.288***	2.235***	2.067***	2.222***	2.279***
	(0.051)	(0.057)	(0.069)	(0.115)	(0.051)	(0.051)
Entrant Unemployment (18-20)						
On Earnings Aged 23-24	0.321***	0.342***	0.307***	0.155		
	(0.040)	(0.043)	(0.053)	(0.087)		
On Earnings Aged 25-26	0.447***	0.500***	0.479***	0.388***		
	(0.051)	(0.056)	(0.071)	(0.103)		
On Earnings Aged 27-28	0.537***	0.608***	0.571***	0.472***		
	(0.052)	(0.057)	(0.069)	(0.109)		
On Earnings Aged 29-30	0.566***	0.641***	0.631***	0.527***		
	(0.054)	(0.058)	(0.073)	(0.100)		
On Earnings Aged 31-32	0.552***	0.597***	0.593***	0.444***		
	(0.053)	(0.059)	(0.071)	(0.106)		
On Earnings Aged 29-30	0.498***	0.530***	0.540***	0.441***		
	(0.053)	(0.058)	(0.073)	(0.114)		
On Earnings Aged 35-36	0.492***	0.540***	0.553***	0.463***		
	(0.055)	(0.060)	(0.073)	(0.121)		
On Earnings Aged 37-38	0.492***	0.543***	0.564***	0.406***		
	(0.056)	(0.060)	(0.074)	(0.110)		
On Earnings Aged 39-40	0.506***	0.565***	0.599***	0.449***		
	(0.056)	(0.061)	(0.074)	(0.115)		
On Earnings Aged 41-42		0.535***	0.553***	0.336***		
		(0.053)	(0.065)	(0.100)		
On Earnings Aged 43-44			0.574***	0.406***		
			(0.065)	(0.102)		
On Earnings Aged 45-46				0.457***		
				(0.102)		
Youth Unemployment (21-23)						
On Earnings Aged 25-26	−0.013	−0.067	−0.033	0.078		
	(0.045)	(0.052)	(0.063)	(0.097)		
On Earnings Aged 27-28	−0.047	−0.113*	−0.090	−0.106		
	(0.046)	(0.054)	(0.061)	(0.100)		
On Earnings Aged 29-30	0.042	−0.031	0.022	0.144		
	(0.053)	(0.062)	(0.075)	(0.115)		
On Earnings Aged 31-32	0.091	0.020	0.052	0.264*		
	(0.055)	(0.064)	(0.076)	(0.125)		
On Earnings Aged 29-30	0.091	0.022	0.046	0.180		
	(0.055)	(0.065)	(0.077)	(0.120)		
On Earnings Aged 35-36	0.098	−0.005	0.062	0.179		
	(0.056)	(0.065)	(0.076)	(0.129)		
On Earnings Aged 37-38	0.110	0.029	0.051	0.244*		
	(0.056)	(0.066)	(0.077)	(0.121)		
On Earnings Aged 39-40	0.067	−0.023	0.036	0.196		
	(0.057)	(0.066)	(0.078)	(0.126)		
On Earnings Aged 41-42		−0.007	0.060	0.317**		
		(0.056)	(0.066)	(0.107)		
On Earnings Aged 43-44			0.049	0.287**		
			(0.066)	(0.108)		
On Earnings Aged 45-46				0.317**		
				(0.109)		
Early Adulthood Unempl. (24-26)						
On Earnings Aged 29-30	0.060	0.070	0.061	0.042		
	(0.047)	(0.055)	(0.073)	(0.127)		
On Earnings Aged 31-32	0.007	0.078	0.081	0.050		
	(0.046)	(0.052)	(0.068)	(0.134)		
On Earnings Aged 29-30	0.016	0.074	0.146*	0.143		
	(0.049)	(0.056)	(0.071)	(0.129)		
On Earnings Aged 35-36	0.000	0.086	0.104	0.116		
	(0.050)	(0.056)	(0.074)	(0.153)		
On Earnings Aged 37-38	−0.070	0.023	0.062	−0.028		
	(0.051)	(0.058)	(0.075)	(0.139)		
On Earnings Aged 39-40	−0.092	0.016	0.028	0.012		
	(0.051)	(0.057)	(0.074)	(0.142)		

Continued on next page

Table A2 – continued from previous page

	(1)	(2)	(3)	(4)	(5)	(6)
	Sample 18-40	Sample 18-42	Sample 18-44	Sample 18-46	Whole Youth	Entrant + Early Adult
On Earnings Aged 41-42		-0.026	-0.001	-0.070		
		(0.044)	(0.058)	(0.115)		
On Earnings Aged 43-44			0.014	-0.076		
			(0.058)	(0.116)		
On Earnings Aged 45-46				-0.108		
				(0.116)		
Whole Youth Unemployment (18-26)						
On Earnings Aged 29-30					0.113^{***}	
					(0.013)	
On Earnings Aged 31-32					0.106^{***}	
					(0.013)	
On Earnings Aged 33-34					0.090^{***}	
					(0.014)	
On Earnings Aged 35-36					0.086^{***}	
					(0.014)	
On Earnings Aged 37-38					0.068^{***}	
					(0.015)	
On Earnings Aged 39-40					0.050^{***}	
					(0.015)	
Entrant (18-22) Unemployment						
On Earnings Aged 25-26						0.187^{***}
						(0.022)
On Earnings Aged 27-28						0.217^{***}
						(0.023)
On Earnings Aged 29-30						0.271^{***}
						(0.028)
On Earnings Aged 31-32						0.285^{***}
						(0.028)
On Earnings Aged 33-34						0.252^{***}
						(0.028)
On Earnings Aged 35-36						0.256^{***}
						(0.029)
On Earnings Aged 37-38						0.259^{***}
						(0.030)
On Earnings Aged 39-40						0.254^{***}
						(0.029)
Youth Unemployment (23-27)						
On Earnings Aged 29-30						0.054
						(0.029)
On Earnings Aged 31-32						0.031
						(0.029)
On Earnings Aged 29-30						0.035
						(0.031)
On Earnings Aged 35-36						0.024
						(0.032)
On Earnings Aged 37-38						-0.012
						(0.033)
On Earnings Aged 39-40						-0.047
						(0.033)
N	319481	249465	163725	56621	319481	319481
Number of Individuals	14348	10326	6264	2017	14348	14348

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Columns (1)-(4) report estimated coefficients from (21) for different samples. Therefore Column (1) is the same as column (3) in Table 1, and is the regression run on a sample which contains seven cohorts. The next three columns are for smaller samples, containing respectively five, three and one cohort only. Column (5) contains the estimated coefficients when the entire period from age 18 to age 26 is treated as homogeneous, that is when (21) is estimated with the restrictions (22)-(23). These coefficients are drawn as the light green line in Figure 3, and in Figure A.1. Finally, Column (6) reports the estimated coefficients when youth is split into two periods, with restrictions (24)-(26). The resulting coefficients are drawn as the aquamarine lines in Figure A.1.

Table A3:
Results For Different Ability Quintiles

	(1) Bottom Quartile	(2) 2nd Quartile	(3) 3rd Quartile	(4) 4th Quartile	(5) Top Quartile
Current Unemployment	4.453***	1.822***	0.887***	0.742***	0.381**
	(0.205)	(0.153)	(0.136)	(0.137)	(0.124)
Entrant Unemployment (18-20)					
On Earnings Aged 23-24	0.487*	0.276	0.056	0.158	0.166
	(0.225)	(0.186)	(0.167)	(0.145)	(0.145)
On Earnings Aged 25-26	0.541	0.425*	0.237	0.211	0.351*
	(0.289)	(0.208)	(0.215)	(0.181)	(0.177)
On Earnings Aged 27-28	0.795**	0.371	0.351	0.261	0.287
	(0.289)	(0.220)	(0.235)	(0.163)	(0.173)
On Earnings Aged 29-30	1.061***	0.423	0.266	0.205	0.271
	(0.318)	(0.216)	(0.204)	(0.169)	(0.173)
On Earnings Aged 31-32	0.741*	0.353	0.227	0.263	0.216
	(0.331)	(0.234)	(0.206)	(0.161)	(0.168)
On Earnings Aged 29-30	0.694*	0.344	0.222	0.259	0.233
	(0.337)	(0.222)	(0.200)	(0.158)	(0.168)
On Earnings Aged 35-36	0.722*	0.421	0.202	0.228	0.219
	(0.338)	(0.224)	(0.197)	(0.155)	(0.169)
On Earnings Aged 37-38	0.616	0.475*	0.203	0.254	0.220
	(0.356)	(0.219)	(0.197)	(0.155)	(0.165)
On Earnings Aged 39-40	0.610	0.382	0.266	0.249	0.205
	(0.357)	(0.231)	(0.202)	(0.157)	(0.164)
Youth Unemployment (21-23)					
On Earnings Aged 25-26	0.223	−0.186	−0.129	−0.076	−0.102
	(0.230)	(0.173)	(0.188)	(0.172)	(0.142)
On Earnings Aged 27-28	−0.056	−0.061	−0.248	−0.172	−0.142
	(0.249)	(0.182)	(0.198)	(0.152)	(0.144)
On Earnings Aged 29-30	−0.089	0.024	−0.184	−0.060	−0.039
	(0.340)	(0.194)	(0.184)	(0.174)	(0.144)
On Earnings Aged 31-32	0.027	0.216	−0.117	−0.102	0.023
	(0.347)	(0.204)	(0.166)	(0.166)	(0.139)
On Earnings Aged 29-30	0.023	0.045	−0.158	−0.088	0.014
	(0.359)	(0.189)	(0.167)	(0.160)	(0.137)
On Earnings Aged 35-36	−0.091	0.054	−0.165	−0.091	0.019
	(0.384)	(0.189)	(0.167)	(0.164)	(0.140)
On Earnings Aged 37-38	0.181	−0.029	−0.141	−0.096	−0.009
	(0.402)	(0.191)	(0.169)	(0.167)	(0.139)
On Earnings Aged 39-40	−0.011	−0.022	−0.159	−0.102	0.008
	(0.381)	(0.211)	(0.173)	(0.164)	(0.144)
Early Adulthood Unemployment (24-26)					
On Earnings Aged 29-30	0.184	−0.071	0.030	0.028	−0.204
	(0.264)	(0.160)	(0.133)	(0.132)	(0.138)
On Earnings Aged 31-32	0.054	−0.194	−0.127	−0.056	−0.239
	(0.262)	(0.159)	(0.121)	(0.134)	(0.139)
On Earnings Aged 29-30	−0.021	−0.187	−0.056	−0.113	−0.314*
	(0.289)	(0.146)	(0.118)	(0.133)	(0.136)
On Earnings Aged 35-36	0.088	−0.194	−0.036	−0.104	−0.320*
	(0.303)	(0.145)	(0.117)	(0.136)	(0.136)
On Earnings Aged 37-38	−0.212	−0.117	−0.027	−0.083	−0.296*
	(0.316)	(0.149)	(0.125)	(0.138)	(0.136)
On Earnings Aged 39-40	−0.308	−0.145	−0.055	−0.047	−0.246
	(0.326)	(0.163)	(0.121)	(0.128)	(0.132)
Observations	64,009	63,832	63,952	63,852	63,835
Number of Individuals	3,059	2,839	2,827	2,811	2,812

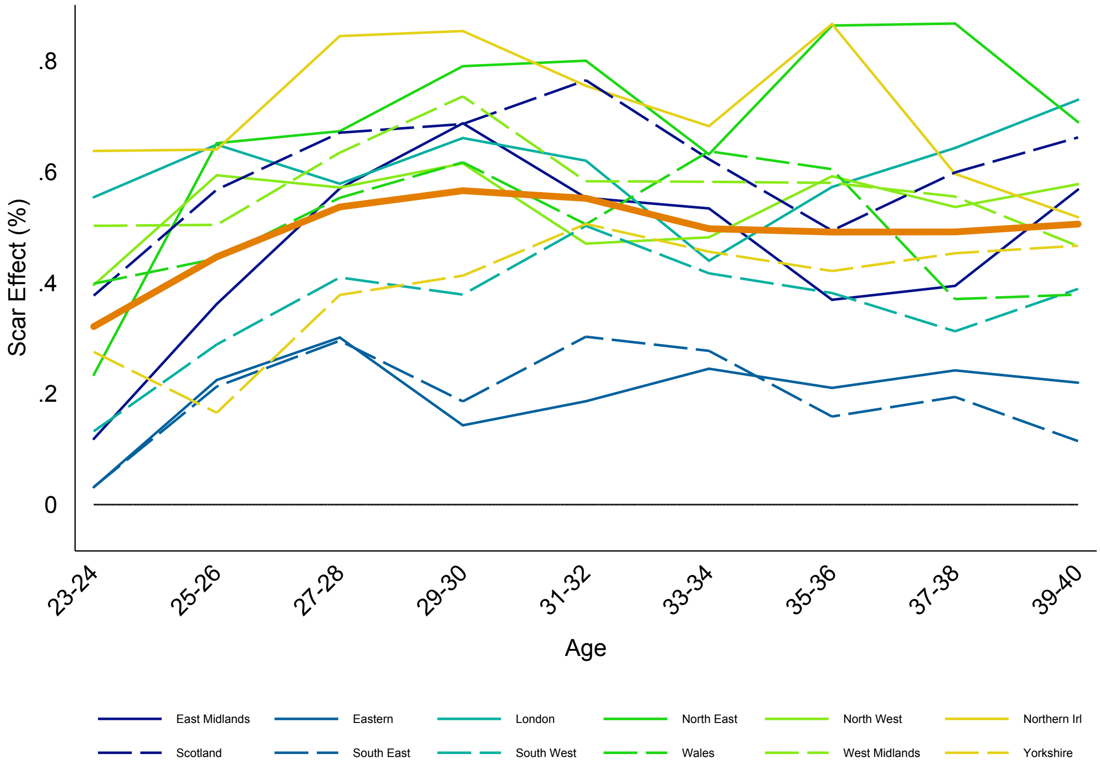
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Coefficients for the estimation of (21), the same as the main regression, column (3) in Table 1, estimated from each subsample of individuals in the same ability quintile according to the rank determined by their earnings potential. The coefficients in the top block are plotted in Figure 6, those in the other two blocks in Figure A.3.

Table A4:
Results For Individual Cohorts

	(1) 1978	(2) 1979	(3) 1980	(4) 1981	(5) 1982	(6) 1983	(7) 1984
Current Unemployment	2.050^{***}	2.124^{***}	2.388^{***}	2.381^{***}	2.439^{***}	2.489^{***}	2.557^{***}
	(0.131)	(0.118)	(0.142)	(0.141)	(0.139)	(0.149)	(0.125)
Entrant Unemployment (18-20)							
On Earnings Aged 23-24	0.137	0.343^{***}	0.389^{***}	0.331^{***}	0.536^{***}	0.272	0.102
	(0.085)	(0.085)	(0.102)	(0.095)	(0.114)	(0.143)	(0.182)
On Earnings Aged 25-26	0.367^{***}	0.506^{***}	0.541^{***}	0.421^{***}	0.752^{***}	0.302	-0.048
	(0.100)	(0.127)	(0.141)	(0.119)	(0.132)	(0.170)	(0.204)
On Earnings Aged 27-28	0.451^{***}	0.546^{***}	0.684^{***}	0.675^{***}	0.731^{***}	0.348^{*}	-0.080
	(0.112)	(0.116)	(0.138)	(0.136)	(0.146)	(0.167)	(0.212)
On Earnings Aged 29-30	0.502^{***}	0.617^{***}	0.763^{***}	0.570^{***}	0.857^{***}	0.338	-0.043
	(0.103)	(0.118)	(0.154)	(0.137)	(0.140)	(0.181)	(0.210)
On Earnings Aged 31-32	0.424^{***}	0.489^{***}	0.830^{***}	0.552^{***}	0.787^{***}	0.421^{*}	0.084
	(0.113)	(0.114)	(0.148)	(0.144)	(0.139)	(0.169)	(0.210)
On Earnings Aged 29-30	0.397^{**}	0.477^{***}	0.739^{***}	0.506^{***}	0.655^{***}	0.367^{*}	0.151
	(0.124)	(0.120)	(0.144)	(0.137)	(0.135)	(0.166)	(0.216)
On Earnings Aged 35-36	0.431^{***}	0.446^{***}	0.763^{***}	0.488^{***}	0.703^{***}	0.314	0.075
	(0.130)	(0.125)	(0.136)	(0.145)	(0.139)	(0.177)	(0.215)
On Earnings Aged 37-38	0.399^{***}	0.564^{***}	0.708^{***}	0.450^{**}	0.722^{***}	0.313	-0.004
	(0.120)	(0.129)	(0.151)	(0.151)	(0.141)	(0.173)	(0.223)
On Earnings Aged 39-40	0.452^{***}	0.574^{***}	0.749^{***}	0.503^{***}	0.646^{***}	0.284	0.020
	(0.125)	(0.130)	(0.152)	(0.152)	(0.145)	(0.182)	(0.223)
Youth Unemployment (21-23)							
On Earnings Aged 25-26	0.089	-0.036	-0.239	-0.244^{*}	-0.120	0.118	0.307^{**}
	(0.095)	(0.104)	(0.130)	(0.122)	(0.152)	(0.127)	(0.119)
On Earnings Aged 27-28	-0.096	-0.020	-0.183	-0.266	-0.129	0.106	0.276^{*}
	(0.099)	(0.098)	(0.126)	(0.155)	(0.163)	(0.127)	(0.120)
On Earnings Aged 29-30	0.164	0.128	-0.329^{*}	-0.318	-0.111	0.247	0.345^{**}
	(0.112)	(0.123)	(0.163)	(0.180)	(0.167)	(0.158)	(0.116)
On Earnings Aged 31-32	0.307^{*}	0.114	-0.379^{*}	-0.177	-0.079	0.251	0.331^{*}
	(0.133)	(0.121)	(0.162)	(0.183)	(0.189)	(0.152)	(0.129)
On Earnings Aged 29-30	0.251	0.146	-0.380^{*}	-0.265	-0.004	0.234	0.347^{**}
	(0.129)	(0.129)	(0.159)	(0.184)	(0.176)	(0.155)	(0.123)
On Earnings Aged 35-36	0.227	0.162	-0.304	-0.261	-0.239	0.320^{*}	0.420^{**}
	(0.137)	(0.123)	(0.159)	(0.180)	(0.178)	(0.149)	(0.131)
On Earnings Aged 37-38	0.264^{*}	0.083	-0.274	-0.093	-0.147	0.246	0.363^{**}
	(0.131)	(0.130)	(0.170)	(0.192)	(0.176)	(0.154)	(0.126)
On Earnings Aged 39-40	0.183	0.056	-0.291	-0.279	-0.199	0.311^{*}	0.305^{*}
	(0.133)	(0.129)	(0.180)	(0.189)	(0.182)	(0.155)	(0.134)
Early Adulthood Unemployment (24-26)							
On Earnings Aged 29-30	0.021	-0.053	0.247^{*}	0.179	-0.042	-0.029	0.057
	(0.127)	(0.135)	(0.121)	(0.103)	(0.126)	(0.143)	(0.120)
On Earnings Aged 31-32	0.035	0.007	0.243^{*}	0.123	-0.069	-0.211	-0.136
	(0.146)	(0.115)	(0.112)	(0.097)	(0.137)	(0.143)	(0.125)
On Earnings Aged 29-30	0.120	0.047	0.320^{**}	-0.000	-0.142	-0.190	-0.056
	(0.139)	(0.132)	(0.117)	(0.116)	(0.150)	(0.144)	(0.132)
On Earnings Aged 35-36	0.111	0.035	0.198	0.107	-0.083	-0.261	-0.118
	(0.165)	(0.127)	(0.119)	(0.111)	(0.137)	(0.143)	(0.139)
On Earnings Aged 37-38	-0.024	0.027	0.198	-0.054	-0.092	-0.401^{**}	-0.195
	(0.151)	(0.138)	(0.133)	(0.117)	(0.142)	(0.136)	(0.138)
On Earnings Aged 39-40	0.033	-0.071	0.152	0.041	-0.086	-0.450^{**}	-0.291^{*}
	(0.154)	(0.137)	(0.128)	(0.113)	(0.153)	(0.138)	(0.143)
N	46222	48013	48222	46073	43583	43203	44165
Number of Individuals	2057	2143	2152	2076	1972	1954	1994

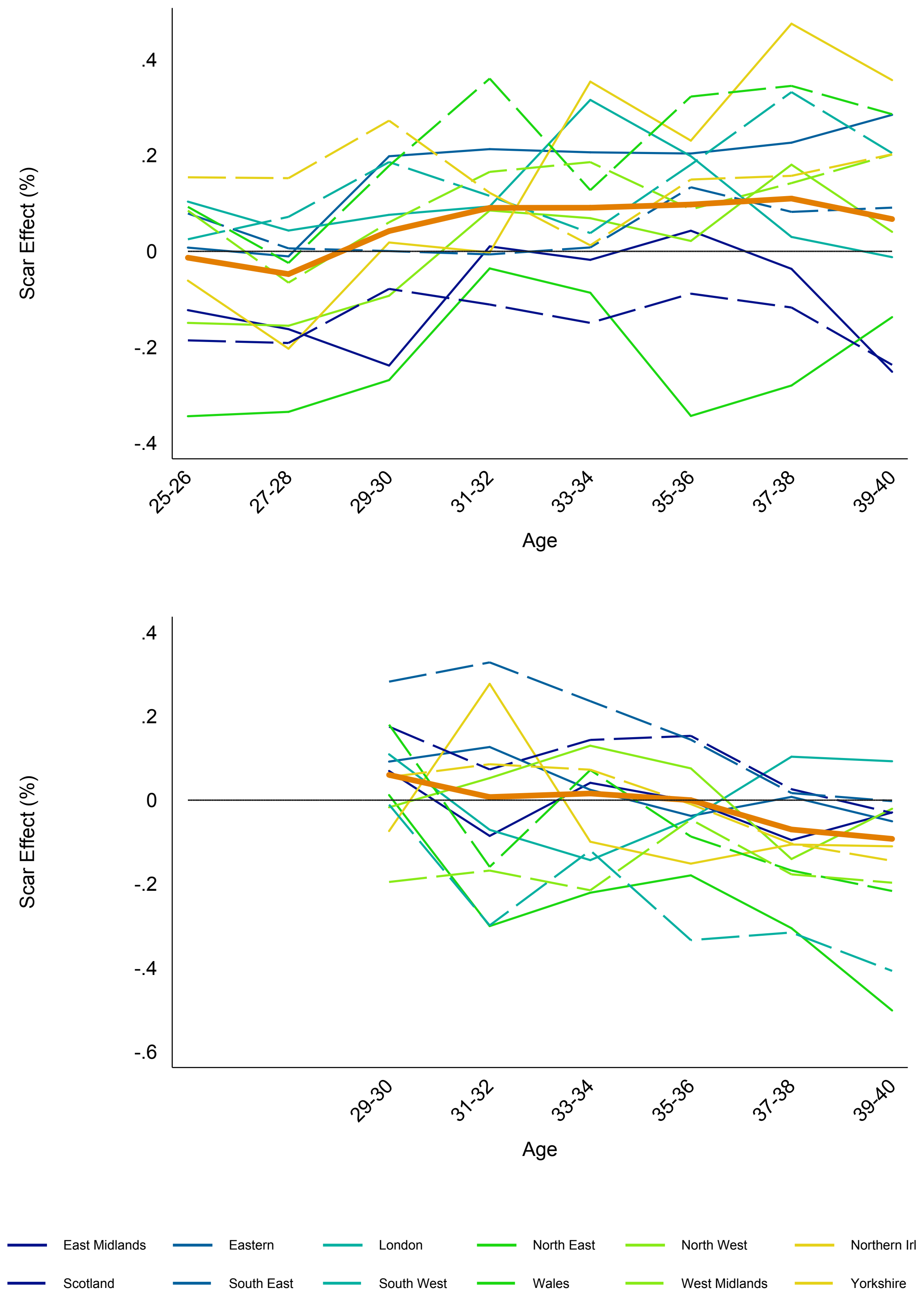
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Coefficients for the estimation of (21), the same as the main regression, column (3) in Table 1, estimated from each subsample of individuals according to the year of entry in the labour market. The coefficients in the top block are plotted in Figure 5, those in the other two blocks in Figure A.2.

Figure A.4:
Scar Effects of Entry Unemployment in Different Regions.



Note: The lines report the estimated coefficients from equation(21) for the effect of Entry unemployment corresponding to those in Figure 6. That is the coefficients β_E^t, calculated for each subsample of individuals in the same region at the time of entry in the labour market.

Figure A.5:
Scar Effects of Youth and Early Adult Unemployment in Different Regions.



Note: The lines report the estimated coefficients from equation(21) for the effect of Youth, and Early Adulthood unemployment corresponding to those in Figure 6. That is the coefficients β_Y^t and β_A^t, calculated from each subsample of individuals in the same region at the time of entry in the labour market.