The Wounds That Do Not Heal.

The Life-time Scar of Youth Unemployment.

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Abstract: This paper uses UK administrative data to study the long-term effects of unemployment on earnings. It is the first paper to pinpoint accurately the relative importance of the timing of employment shocks within workers' lives. We find a strong effect of events in the first few years after entry into the labour market: each month of unemployment between ages 18 and 20 causes a permanent income loss of 1.2% per year. This scar effect of youth unemployment is lower when it happens when the worker's age is between 21 and 23, and it disappears altogether in the next three-year age period. The scar effect is most severe for individuals at the lower end of the ability distribution.

Keywords: Youth unemployment, Lifetime earnings, Scarring effect.

JEL-Codes: J64, J31

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Introduction

Following the 2008 financial crisis, the number of young people out of work reached unprecedented levels in many OECD countries and has remained stubbornly high since. The virtual halt of production in many industries caused by Covid-19 will swell the hiring backlog, and exacerbate this problem still further for some time. Substantial economic and social losses are created by the idleness of so many otherwise productive workers. Concerns about such losses are compounded by fears about the associated long-term consequences. These fears stem from the well established regularity that a person's past employment history 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, as summarised in Couch and Placzeck (2010). A parallel literature has concentrated on the long term effects of *youth* unemployment (see, for example, Lynch 1989 for the US; Lynch 1985, Nickell et al. 2002, Gregg and Tominey 2005 for the UK; and Schmillen and Umkehrer 2018 for Germany).[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 the causal link between individuals' labour market experience and their current outcomes. The term "past" focuses on *how distant* in time the shock was, whereas "youth" highlights that the shocks occur in a *specific period* of a person's life, regardless of how long ago that was. This distinction, thus, raises 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? It is by now firmly established that different periods in a person's formative years have different impacts on their future 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). Correspondingly, in this paper, we show that the acquisition of labour market skills obeys an analogous temporal pattern, with early shocks more damaging than later ones.

The aim of this paper is, therefore, to separate the effects of labour market shocks which

[1]Bell and Blanchflower (2011) analyse the effect of the Great Recession on young people in the US and in the UK, while Cahuc et al. (2013) consider the effect on those living in France and Germany, Genda et al. (2010) in Japan, and Eliason and Storrie (2006) in Sweden. See Scarpetta et al. (2010) for a policy oriented perspective, and oecd.org/youth.htm for data.

occur during youth from those of equally distant shocks that happened when the worker's age was different. Our main finding is that shocks at the time of a person's entry into the labour market are the most influential. The punchline of our paper is thus, that *wounds from youth unemployment scar permanently, while wounds from past unemployment heal with time.* To be precise: *ceteris paribus*, for men, an additional month of unemployment between ages 18 and 20, that is, in the first three years after entry into the labour market, permanently lowers earnings by around 1.5% per year. This *scar* effect is large, slightly higher than the estimated decrease in earnings attributable to a reduction of one year in formal education (Harmon et al. 2001). Furthermore, this effect shows no sign of abating by the time individuals reach age 40 (see Figure 4 below). We also find that the age interval 18 to 20 is crucial: a similar shock during the following three years, namely between ages 21 and 23, has a considerably weaker long-term effect, and any effect becomes undetectable for shocks received in the further three years between the ages of 24 and 26, as also shown in Figure 4.

There is, therefore, a substantial difference in the impact of labour market experience in different sub-periods of youth: the permanent long-term effect is concentrated in early youth. To the best of our knowledge, this paper is the first that separates the period of youth into subperiods. This is important, as not doing so carries the risk of underestimating the importance of unemployment shocks which occur in youth: when youth is treated as a homogeneous period of nine years, from 18 to 26, the impact of a shock, while statistically significant throughout the period, is much smaller, and is also decreasing with age. This is illustrated in Figure 4, where the scar effect of the aggregate period is shown alongside those of the three separate subperiods. Averaging the effects across youth, therefore, misses the sharp difference between sub-periods within youth: in later youth (ages 24 to 26), the impact of a shock is determined by time *distance* and its effect fades as time passes. The extent of the difference in the effect of spells of unemployment between ages 18 and 20 relative to later ages suggests that the correlation between low employment in youth and low earnings in adulthood is not solely due to unobservable variables that determine an individual's attachment to the labour market, as these should arguably also cause low employment from age 21 on.[2]

The difference in the impact of the sub-periods of youth, theoretical interest aside, has

[2]Schwandt and von Wachter (2019) study the role of the timing of graduation in the US. In the period we study, there was no such flexibility for UK workers other than for those who went to university, which are a small proportion of the total, and whom we exclude from our analysis.

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 measures intended to assuage youth unemployment are likely to be more effective in the long term when they are specifically targeted at new entrants into the labour market. It certainly casts doubts on the wisdom of institutional rules whose effect is to favour older workers to the detriment of younger ones.

A similar inference on the relative importance of experience at different ages can be drawn when the overall effect is separated into its direct and indirect components. In addition to its long term effect on earnings, a period of unemployment in youth may also have a short term effect on employment, which, in turn, may have long term effects on earnings. That is, being unemployed at age 18 may increase the chance of being unemployed at age 22, and being unemployed at age 22 may reduce a person's future earnings. When we separate out these effects, we find that it is the direct effect of experience at entry that is most important.

One further crucial dimension of variation we uncover is that experience affects differently individuals of different abilities (Figure 5). Importantly, the severity of the scar effect increases as we move down the ability scale. Note that we exclude individuals who, based on their pattern of employment and unemployment in the relevant age range, are likely to have gone to university or further education. At the time, this was a small proportion of the relevant cohort, and again, there might be reasons to suspect that their experience of the labour market was fundamentally different from that of their less educated peers.[3] Thus these differences are unlikely to be attributed to different educational and professional backgrounds. An employment shock hitting a given cohort of young workers will, in the long term, be weathered more effectively by high ability individuals. It follows that an aggregate negative labour market shock affecting a cohort would exacerbate the long term inequality in earnings among individuals of that cohort over and above any existing pre-shock inequality. Given the high level of youth unemployment in many advanced economies, the difference in the strength of the long term effect between the better-offs and the worse-offs may cause more inequality in the long term than would occur with lower youth unemployment.

For women, the negative effect of unemployment at entry into the labour market appears

[3] A sensitivity test including students suggests that our main result is unchanged, see Columns (1) in Table 2 below. Similarly for the self-employed: we exclude from the main regression all those that are self-employed at any time during the period, as they might differ in important respects from individuals who are never self-employed. In Column (2) in Table 2, we show anyway that including them in the years when they are employed does not alter the results. For the same reason, we also exclude individuals who were born outside the UK.

to be similar, but stronger in the early years (compare Columns (3) and (4) in Table 1). In comparison to men, the effect appears to be reversed between ages 24 to 26. This might be because other intervening variables are at play, information about which is not available in our dataset, and so we cannot control for them in our empirical model. Among these omitted variables are fertility choices and hours worked per week. Both are plausibly endogenous and likely to be more important determinant of earnings for women than for men (see Blundell et al. 2013, among others). The number of hours worked per week 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 in our dataset. Thus, we follow most of the literature and focus our analysis on men.

We use the UK Lifetime Labour Market Database. This is sizeable and long, and it combines anonymised administrative tax and social security records into a dataset that tracks a random sample of almost 650,000 individuals, corresponding to 1% of the holders of the UK social security identifier, between 1978 and 2006. We restrict our sample to those born between 1960 and 1967 and we observe their periods of employment and unemployment, along with their earnings, between the ages of 18 and 40 (we extend this range for some robustness tests). This allows us to follow individuals for an extensive period of 23 years after their entry into the labour market. Observing employment periods, measured as the number of weeks employed within the year, provides a valuable alternative to much of the recent US literature which is based on a binary variable measuring "displacement" (see, for example, Jacobson et al. 1993 and 2005, who study long term "scar" unemployment effects in Pennsylvania and Washington). One advantage of our approach is that we are able to contrast, for the same individual, the effects of a period of unemployment earlier or later in their career. This is difficult to do with the displacement approach.

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. This geographic detail also means 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, following Moretti's (2004) seminal contribution, we use detailed geographical information to control for endogenous geographical mobility. We separate the effect of unemployment shocks from the effect of unobserved individual ability and from the effect of local labour market characteristics, as well as, crucially, the effect of the interaction of the two. That is, we control for individual heterogeneity and local labour market heterogeneity and the resulting sorting. To do so is important to rule out a wide-range of alternative causal mechanisms. Similarly, another important feature of our model is that we are able to allow the effect of labour market experience to vary with the time of entry in the labour market, in line with some recent literature.[4] In other words, our identification strategy relies on precise geographic detail and on the length of the panel to capture other unobservable sources of heterogeneity by including individual times labour market fixed effects and cohort fixed effects.

As discussed above, we uncover a hitherto unnoticed important difference between the effect on lifetime earnings of experience at the time of entry and the effect of experience in the following few years. The next important task is to explain why this should be so. Even if part of the link between youth unemployment and adult earning potential may be due to weak attachment to the labour market, in the form, for example, of lower determination to hold a job, the key question remains as to why people are more vulnerable in the long term when they are younger. 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 sector specific loss of human capital.

Understanding the causes of the differences between the effects of shocks sustained at different stages of a person's youth 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 unemployment for individuals at different ages. A promising explanation is the importance of experimentation and learning (Papageorgiou 2014). Wee (2016) argues that

[4] Oreopoulos et al. (2012) found that the local conditions at the time of entry into the labour market for Canadian graduates matter more for earnings than contemporaneous regional unemployment. Analogous results are obtained by Kahn (2010), for US graduates, and Hershbein (2012) and Speer (2016), for high school graduates.

those entering the labour market during a recession may suffer a wage scar: this is because reduced early career mobility limits 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, when human capital formation is most important.

The paper is organised as follows. Section 2 summarises the established theoretical background on the long term effects of unemployment and motivates the econometric specification discussed in Section 3. Our identification strategy is illustrated in Section 4 and Section 5 presents the data. Our main results and some robustness tests are presented in Section 6 and Section 7 concludes in the light of the existing literature. An appendix reports further results.

2 The model

The theoretical model is a straightforward Mincerian equation, in which periods of employment are assumed to increase experience, and periods of unemployment are not. 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 local unemployment rates and other labour demand side variables; $\boldsymbol{e}_i^t$ is person i's "experience" at time t; in general, we consider experience something that exerts a non-negative effect on earnings and so we posit $\frac{\partial f}{\partial e_i^t} \geqslant 0$. Finally, 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 as 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.[5] 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 (1958 and 1974) landmark studies, among others.

[5]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).

The long term importance of labour market experience is, of course, well established, and the focus of our paper is on the description of the role of experience in more detail. To this end, one can think of at least two conceptual reasons why the importance of past events for present day outcomes depends on the timing of these events. Firstly, it is possible that recent occurrences may matter more than distant ones: negative events fade in importance and work, and, conversely, skills acquired in the distant past become less relevant. Secondly, timing may matter because some periods in life, for example the years immediately after entry in the labour market, are more important than others.

To formalise these ideas, we replace $\boldsymbol{e}_i^t$ 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 into 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. Note that e without a subscript is a function, the same for every individual, while e_i with subscript i is the actual value of individual i's experience. Thus, (1) is replaced by:

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

where individual i's experience in period t is itself a function of previous experience:

$$e_i^t = \tilde{e}^t\left(Z_i, \lambda^t, e_i^{t-1}, \ldots, e_i^2, e_i^1, u_i^t\right). \tag{3}$$

In (3), u_i^t is an idiosyncratic random error, which affects experience and may be correlated to the direct shock on earnings, ε_i^t. The dependency, explicit in (3), of e_i^t on Z_i and λ^t, can be factored out into the shape of the function f^t, and left implicit, writing (3) as:

$$e_i^\tau = e^\tau\left(e_i^{\tau-1}, \ldots, e_i^2, e_i^1, u_i^\tau\right), \qquad \tau = t, t-1, \ldots, 2, 1.$$

Substituting the above in (2), we have:

$$w_i^t = f^t\Big(Z_i, \lambda^t, e^t\Big(e^{t-1}\left(\cdot\right), e^{t-2}\left(\cdot\right), \ldots, e^2\left(\cdot\right), e^1\left(u_i^1\right)\Big), \ldots, e_i^2\Big(e^1\left(u_i^1\right), u_i^2\Big), e^1\left(u_i^1\right), \varepsilon_i^t\Big) \quad (4)$$

The partial derivative $\partial w_i^t/\partial e_i^{t-\tau}$ 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 $t=3$ as an illustrative example, we can write:

$$\frac{\mathrm{d}f^3}{\mathrm{d}e_i^1} = \frac{\partial f^3}{\partial e_i^1} + \frac{\partial f^3}{\partial e_i^2}\frac{\partial e^2}{\partial e_i^1} + \frac{\partial f^3}{\partial e_i^3}\left(\frac{\partial e^3}{\partial e_i^2}\frac{\partial e^2}{\partial e_i^1} + \frac{\partial e^3}{\partial e_i^1}\right), \quad (5)$$

and so to determine the effect of a shock du_i^1 in period 1 on earnings in period 3, we would simply take:

$$dw_i^t = \frac{\mathrm{d}f^3}{\mathrm{d}e_i^1} du_i^1 \quad (6)$$

where the total effect $\mathrm{d}f^3/\mathrm{d}e_i^1$ on the RHS of (5) is decomposed into the direct effect of period 1 experience, $\frac{\partial f^3}{\partial e_i^1}$, and the indirect effect of experience in previous periods, given by its direct effect on period 2 experience, $\frac{\partial e^2}{\partial e_i^1}$, multiplied by the direct effect of period 2 experience on period 3 earnings, $\frac{\partial f^3}{\partial e_i^2}$. Similarly for the effect through the experience in period 3, given by the last term in (5). In Section 6.3, we illustrate how our econometric strategy allows us to separate the direct from the indirect effects. To do so is important, as the direct effect sheds light on the 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

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

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

In practice, some events have only temporary effects and fade away with time. To express this possibility formally, we can write:

$$\frac{\partial w_i^t}{\partial e_i^s} < \delta, \qquad \text{for } t > t^* \text{ and } s = 1, \ldots, s^*, \quad (7)$$

for some t^* and s^*, with $t^* > s^*$, and for a suitably small value of δ. According to (7), 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 into 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 (7) is replaced by the hypothesis that, for some s^* and t^*, with $t^* > s^*$, and $M > 0$,

$$0 < M < \frac{\partial w_i^t}{\partial e_i^s}, \qquad \text{for } t > t^* \text{ and } s = 1, \ldots, s^*. \tag{8}$$

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

We measure experience in year t as 52 minus the number of weeks of unemployment in year t. So experience can take any value in $\{0, 1, , \ldots, 52\}$. This is available in our data for each person in each year. The idea that the loss of experience due to a period of youth unemployment leaves permanent "scars" whereas the effect of later unemployment "heals" can be cast formally in the following hypothesis.

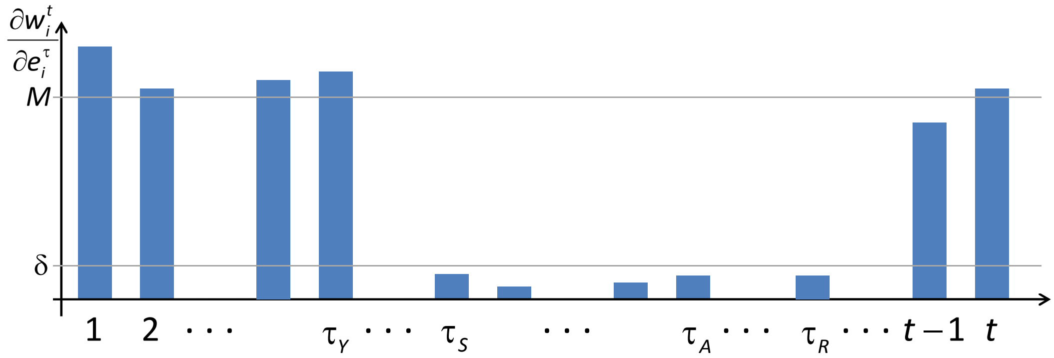
Hypothesis 1 *There exists τ_Y, τ_S, τ_A and τ_R, with $\tau_Y < \tau_S \leq \tau_A \leq \tau_R$, and positive constants M and δ, with $M > \delta$, such that for $t \geqslant \tau_R$*

$$\textit{Scar effect:} \qquad \frac{\partial w_i^t}{\partial e_i^\tau} > M, \qquad \tau = 1, \ldots, \tau_Y, \tag{9}$$

$$\textit{Healing effect:} \qquad 0 \leqslant \frac{\partial w_i^t}{\partial e_i^\tau} < \delta, \qquad \tau = \tau_S, \ldots, \tau_A. \tag{10}$$

A spell of unemployment or non-participation in the labour market in period τ corresponds to a reduction in $e_i^\tau \geq 0$. Thus (9) states that a shock suffered in the first τ_Y periods has a permanent effect on earnings, whereas according to (10), lower experience in periods τ_S to τ_A, such as that caused by a spell of unemployment, has a small effect on earnings, even though periods after τ_S are more recent than those before τ_Y. Figure 1 sketches this: recent events, those happening later than τ_R, may again have a large impact, due to the very fact

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



that they are recent. Testing Hypothesis 1 is the aim of our empirical analysis.

3 Empirical specification

If we assume equation (4) to be log-linear and consider earnings up to the age of 40,[6] then its empirical counterpart may be written as follows.

$$\log w_i^t = Z_i^t \alpha^t + \underbrace{\gamma^t e_i^t}_{\substack{\text{Current}\\ \text{Employment}}} + \underbrace{\beta_{t-1}^t e_i^{t-1} + \beta_{t-2}^t e_i^{t-2} + \ldots + \beta_{s+1}^t e_i^{s+1} + \beta_s^t e_i^s}_{\substack{\text{Effects of Past Labour}\\ \text{Market Experience}}}, \tag{11}$$

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

where, as in (1) and in (4), w_i^t are the earnings of individual i in period t and s indexes each of the $t-1$ prior years in the labour market; Z_i^t is a vector of potentially time-varying individual characteristics, and e_i^t is individual i's labour market experience in year t. A spell of unemployment for this individual is therefore a reduction in e_i^t. In most of the literature, for example in Jacobson et al. (1993) and Couch and Placzek (2010) for the US, and Hijzen et al. (2010) for the UK, the focus has been on job losses due to "displacement events" observed.[7] Our data is richer and more nuanced, allowing us to measure experience as the number of

[6] As Figure 6 below illustrates, the results are robust to the cut-off age of 44, though this significantly reduces the sample size.

[7] The displacement literature has previously highlighted that older workers may suffer more from mass layoffs (Chan et al. 1999 and 2001). Hijzen et al. (2010) show that in the UK, the effects of displacement depends on age: larger and longer lasting impacts are found for those laid-off after the age of 40. As such, their analysis is not comparable to ours, but does suggest that workers may suffer most from unemployment, due to scar effects, at the beginning of their careers and, perhaps due to the loss of firm-specific human capital, at the end of their careers.

weeks of employment in each year. This enables us to account for the fact that a job loss may affect future earnings differently when it is followed by a long spell of unemployment than when a new job is found after a short period.

Each of the equations in (11) has the same number of coefficients as observations per individual, and therefore they are not identified. To see this, we can re-write (11) long-hand (to work with a concrete example, we consider the effects of the previous labour market experience for individual i up to the age of 40) to obtain:

$$
\begin{aligned}
\log w_i^{40} &= Z_i^{40}\alpha^{40} + \underbrace{\gamma^{40} e_i^{40}} + \underbrace{\beta_{39}^{40} e_i^{39} + \beta_{38}^{40} e_i^{38} + \ldots + \beta_{19}^{40} e_i^{19} + \beta_{18}^{40} e_i^{18}}, \\
\log w_i^{39} &= Z_i^{39}\alpha^{39} + \underbrace{\gamma^{39} e_i^{39}} + \underbrace{\beta_{38}^{39} e_i^{38} + \ldots + \beta_{19}^{39} e_i^{19} + \beta_{18}^{39} e_i^{18}}, \\
&\vdots \\
\log w_i^{20} &= Z_i^{20}\alpha^{20} + \underbrace{\gamma^{20} e_i^{20}} + \underbrace{\beta_{19}^{20} e_i^{19} + \beta_{18}^{20} e_i^{18}}, \\
\log w_i^{19} &= Z_i^{19}\alpha^{19} + \underbrace{\gamma^{19} e_i^{19}} + \underbrace{\beta_{18}^{19} e_i^{18}},
\end{aligned}
\tag{12}
$$

where, as in (11), in each equation, the first brace is the effect of current experience, measured as weeks of current employment, which obviously increases current earnings, and the second brace is the effect of past experience, also measured as weeks of employment. We can write the above system compactly in matrix form:

$$
\log \boldsymbol{w}_i = \boldsymbol{\alpha} \boldsymbol{Z}_i + \boldsymbol{\gamma} \boldsymbol{E}_i + \boldsymbol{\beta} \boldsymbol{E}_i^L, \tag{13}
$$

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

$$
\boldsymbol{\beta} = \begin{bmatrix}
\beta_{39}^{40} & \beta_{38}^{40} & \beta_{37}^{40} & \cdots & \beta_{19}^{40} & \beta_{18}^{40} \\
 & \beta_{38}^{39} & \beta_{37}^{39} & \cdots & \beta_{19}^{39} & \beta_{18}^{39} \\
 & & \beta_{37}^{38} & \cdots & \beta_{19}^{38} & \beta_{18}^{38} \\
 & & & \ddots & \vdots & \vdots \\
 & & & & \beta_{19}^{20} & \beta_{18}^{20} \\
 & & & & & \beta_{18}^{19}
\end{bmatrix}. \tag{14}
$$

Writing (11) as (13) makes it clear that (11) is not identified: in order to identify (11) we impose restrictions on the matrix $\boldsymbol{\beta}$. As a first step, we consider a two year interval for the effect of past experience on earnings. Formally, we set:

$$\beta_s^t = \beta_s^{t+1}, \qquad t = 23, 25, 27, \ldots, 39. \tag{15}$$

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

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

$$\beta_s^t = \beta_E^t, \quad \text{if} \quad s = 18, 19, 20 \quad \text{and} \quad t > 22; \tag{16}$$

$$\beta_s^t = 0, \quad \text{otherwise}. \tag{17}$$

Restriction (16) posits that experience gained at age 18 is equivalent to experience gained at age 19 and at age 20. The coefficients β_E^t measure the effect of a labour market entrant's unemployment on their earnings from age 23 onwards. Experience gained when a person is older than 20 is restricted in (17) to have no long term direct effect.

The results obtained with restrictions (16)-(17) are reported in the first column of Table 1. When we impose these restrictions, the estimated coefficients measure the *total* impact of experience between ages 18 and 20. As noted earlier, however, if labour market outcomes at a given time are influenced by past experience, then the loss of experience caused by unemployment between ages 18 and 20 harms labour market prospects at later ages. That is, if someone is unemployed at 19, and if experience matters for labour market prospects at 25, they are also less likely to be gaining experience at 25. As long as there is an independent effect of experience at 25 on labour market outcomes at 40, this exacerbates the direct negative effects of an entrant's loss of experience through unemployment on his or her labour market outcomes at 40.

To decompose the direct from the indirect effect of being unemployed when young, that is, to evaluate the relative magnitude of the two terms on the RHS of (5), we modify (17) to include experience gained in later periods of life as explanatory variables. We do so in two stages. Firstly, we add the coefficients that estimate the effects of experience in "youth" defined as the years between ages 21 and 23 inclusive. Thus, we replace (17) with:

$$\beta_s^t = \beta_Y^t, \qquad \text{if} \qquad s = 21, 22, 23 \qquad \text{and} \qquad t > 24; \tag{18}$$

$$\beta_s^t = 0, \qquad \text{otherwise.} \tag{19}$$

The results obtained with restrictions (16), (18), and (19) are reported in the second column in Table 1. Finally, we add a third possible set of effects, that of experience in "early adulthood", replacing (19) with:

$$\beta_s^t = \beta_A^t, \qquad \text{if} \qquad s = 24, 25, 26 \qquad \text{and} \qquad t > 28; \tag{20}$$

$$\beta_s^t = 0, \qquad \text{otherwise.} \tag{21}$$

Note that we follow Oreopoulos et al. (2012) and, in (17), (19) and (21), we require that the coefficients capturing the effects of experience after a certain age are 0. Thus, we disregard potential effects that are close in time to the current period: time t employment obviously directly affects earnings at time t, whereas the experience obtained with employment at time $t-1$ does not indirectly affect earnings.

To sum up, with the impositions of all these restrictions we have reduced the number of different β coefficients in (11) and (14) from 220 to 23, which can be divided into three groups:

- β_E^t measures the effects of experience of "Entrants" in the labour market, that is at ages 18-20, on the earnings of the two year periods beginning at $t = 23, \ldots, 39$: these are the scars inflicted by the experience loss due to being unemployed when entering the labour market;

- β_Y^t measures the "Youth" unemployment scar, that is the effects of experience at ages 21-23 on the earnings of the two year periods beginning at $t = 25, \ldots, 39$;

- β_A^t measures the "early Adulthood" unemployment scar, given by the effects of experience at ages 24-26 on the earnings of the two year periods beginning at $t = 29, \ldots, 39$.

4 Identification

The vector of individual specific characteristics in (11), 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 experience, unobserved heterogeneity is a common problem with this type of model. This makes it hard to disentangle the permanent effect of random experience shocks from the influence of an unobserved variable, such as "ability" or "earning potential", on both youth employment (i.e. experience) 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, 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 (Gregg 2001, Burgess et al. 2003). The individual fixed effect we include in our specification, see (22), accounts for potential endogeneity due to unobservable ability. In addition, we also report the regression results of an instrumental variable specification, where we separate the effects of individual ability from those of (early) labour market shocks by exploiting the granular geographic detail in our data. In Column (5) in Table 1, we report the results obtained by instrumenting individual experience, measured as the number of weeks in work, in period t, e_i^t, with the number of weeks worked by the representative worker in their cohort in their local labour market at that time, $\bar{e}_a^t$.[8]

A second concern, following the literature on graduating in a recession (Oreopoulous et al. 2012, Kahn 2010, Hershbein 2012, Speer 2016), particularly since we follow individuals for over twenty years, is that the impact of shocks may be heterogenous across time, place, and cohort. Thus, we write unobserved individual characteristics as:

$$V_i^t = \theta_i \mu_a^t + \eta_c^t + \mu_a^0 + \varepsilon_i^t. \tag{22}$$

The error term (22) has four 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

[8]We prefer the OLS estimates, as regression diagnostics and inference are hard to interpret with a large number of endogenous variables, 24 our case.

valued, which makes location decisions endogenous. This potential problem is analogous to that convincingly addressed by Moretti's (2004) analysis of externalities in higher education. We follow his approach by including the interaction of individual fixed effects θ_i and area fixed effects μ_a. These capture the differences in how particular individual characteristics, including education, are rewarded in different labour markets. Given that each individual lives at only one location at any given time, we do not include a t superscript. The first term in (22), 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 (22), therefore, we include cohort fixed effects. These can vary across local labour markets and across time, and so we interact cohort fixed effects with time and area fixed effects. Formally, η_c^t captures the shocks affecting cohort c in the labour market in period t. Similarly, given the importance of a local area identified by Chetty (2018a and 2018b), we include a third term, μ_a^0, which captures any persistent impact of where the individual lived when they entered the labour market.

A further concern is that local labour market conditions may have a direct impact on an individual's future earnings. Other things equal, the effects of labour market conditions in an area is expected to be similar across all those entering the labour market in that area. Thus, the inclusion of μ_a^0 and η_c^t shuts this channel down by controlling directly for any persistent effects of the particular labour market conditions an individual encountered on entry, and of any variations over time in the labour market conditions faced by that individual's cohort.

The last term in (22), ε_i^t, is the individual specific transitory component of log wages. We allow this component to be correlated across the group of individuals who entered the labour market in the same locality, but this component is independent across individuals in different labour markets. That is, we cluster by initial local labour market μ_a^0. Alternatively, in Column (5) in Table 2 we also report clustering standard errors at the individual level. The estimates are slightly more precise but the inference is unaffected.

To summarise, with the restrictions on the βs given in (16), (18) and (20)-(21) and the assumptions on the fixed effects in (22), the regression specification (11) becomes:

$$\log w_{iac}^t = X_i^t\alpha^t + \gamma^t e_i^t + \underbrace{\beta_E^t \sum_{s=18}^{20} e_i^s}_{\substack{\text{Scar effects of}\\ \text{unemployment}\\ \text{for Entrants}}} + \underbrace{\beta_Y^t \sum_{s=21}^{23} e_i^s}_{\substack{\text{Scar effects of}\\ \text{unemployment}\\ \text{for Youths}}} + \underbrace{\beta_A^t \sum_{s=24}^{26} e_i^s}_{\substack{\text{Scar effects of}\\ \text{unemployment}\\ \text{for early Adults}}} + \theta_i\mu_a + \eta_c^t + \mu_a^0 + \varepsilon_i^t,$$

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

As explained, the coefficients β_E^t, β_Y^t, and β_A^t measure the effects of experience, and hence the scar of unemployment, for "Entrant" (age 18-20), "Youth" (age 21-23) and "early Adult" (age 24-26). The last four terms specify the error term, as described in detail in (22). Our identification assumption is:

$$E[\varepsilon_i^t e_i^t|\theta_i\mu_a, \eta_c^t, \mu_a^0, X_{it}] = E[\varepsilon_i^t e_i^{t-1}|\theta_i\mu_a, \eta_c^t, \mu_a^0, X_{it}] = \cdots = E[\varepsilon_i^t e_i^{18}|\theta_i\mu_a, \eta_c^t, \mu_a^0, X_{it}] = 0. \tag{24}$$

5 The data

We use data from the Lifetime Labour Market Database (LLMDB). The LLMDB combines tax and social security records into a dataset that follows a 1% random sample of the universe of those holding a UK social security number, amounting to 647,068 individuals between 1978 and 2006.[9] The LLMDB contains individual information on sex, date and country of birth, and for each year, address of residence, earnings, nature of employment (employee or self-employed), number of weeks of employment and unemployment in the year, and benefits received. Similarly to most administrative datasets, the LLMDB does not contain information on education or family background.[10] As these are time-invariant individual characteristics, they are controlled for with the inclusion of individual fixed effects, as explained above. The LLMDB has two advantages relative to data used previously. Firstly, as we have a precise

[9] 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. This is allocated to British nationals automatically just before they turn 16 years old, and to foreign nationals if they are eligible and apply to work or claim benefits in the UK.

[10] The LLMDB has been used to study income mobility and changes in inequality (Gardiner and Hills 1999, Dickens and McKnight 2008a), 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).

measure of employment at the beginning of individuals' working lives we do not need to proxy for it, such as with local unemployment rates as Oreopoulous et al. (2012) do. Secondly, given the importance, documented by this literature, of the time of entry into the labour market, we are able to work with multiple cohorts and thus improve on other studies, such as Gregg and Tominey (2005), Jacobson et al. (1993 and 2005) and Couch and Placzeck (2010), which observe one cohort of individuals only.

We restrict our sample to UK nationals, for whom we observe earnings, benefits, employment and unemployment between ages 18 and 40.[11] The first cohort in our dataset comprises individuals born in 1960, who therefore entered the labour market in 1978. The last cohort are those born in 1966, who entered in 1984. Importantly, we do not observe if individuals are currently in education or training. According to our measure of experience, someone who is a student, and hence who is not employed, is not accumulating experience. This therefore ignores the fact that students are accumulating human capital, an obvious substitute for experience. Treating individuals who are in full time education as out of work, that is, ignoring the human capital they accumulate while at university, would, if anything, bias our estimate of the β coefficients *downwards*: they are not employed in their youth, but earning on average more than their peers when adults. They might however differ for other reasons, and given that during the period we consider, only little more than 10% of each UK cohort went to university (Robertson, 2010, p. 19), we therefore try to identify those who were in further or higher education then, and exclude them from the sample.[12] To do this we exploit an important institutional information detail. At the time, students were permitted to register for unemployment benefits during university vacations. Those who did not gain temporary employment typically did so, at least in the summer vacation. Thus, a student would be recorded as neither in work nor unemployed during term time, and as one or the other for around 14 weeks a year, for two years in their youth. This proxy is likely to be conservative, since we would expect *only* students to fit this pattern of repeated temporary engagement with the labour market between the ages of 18 and 22, but some students may

[11] One limitation of our data is that we do not have reliable data (and are not aware of any) on labour market activity before age 18. This means we do not have data on who participated in the Youth Opportunities Programme (1978-83) or the Youth Training Scheme that replaced it. Thus, we start our analysis in all cases from age 18, and rely on the individual fixed-effect to capture any effects of pre-18 training programmes.

[12] Moreover, for virtually all of them, formal education was completed by age 23, as very few people spent more than four years at university. Since this is the earliest age when we include individual earnings in the regression, a person's educational achievement does not vary with time, and so it is adequately captured by individual fixed effects θ_i.

have different patterns. Everyone else would be either in work or unemployed and eligible to claim unemployment benefits. We thus identify, and drop from our sample, those individuals who were only employed or registered as unemployed for between 9 and 18 weeks, for at least two years between ages 18 and 22.[13] The results when we include these individuals are reported in Column (1) of Table 2, which shows that our results are not sensitive to including these likely students. We exclude also individuals who are recorded as ever being self-employed, who constitute 12.1% of the sample. We do so for several reasons. Firstly, income from self-employment may not be recorded accurately. Secondly, the self-employed might have more opportunity to understate their employment and earnings, in order 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 further sensitivity check, we also run the model *including* individuals who report to be self-employed in some years, still excluding the years in which they reported to be self-employed. This increases the sample and makes the panel unbalanced. The results for this case are reported in Column (2) of Table 2.

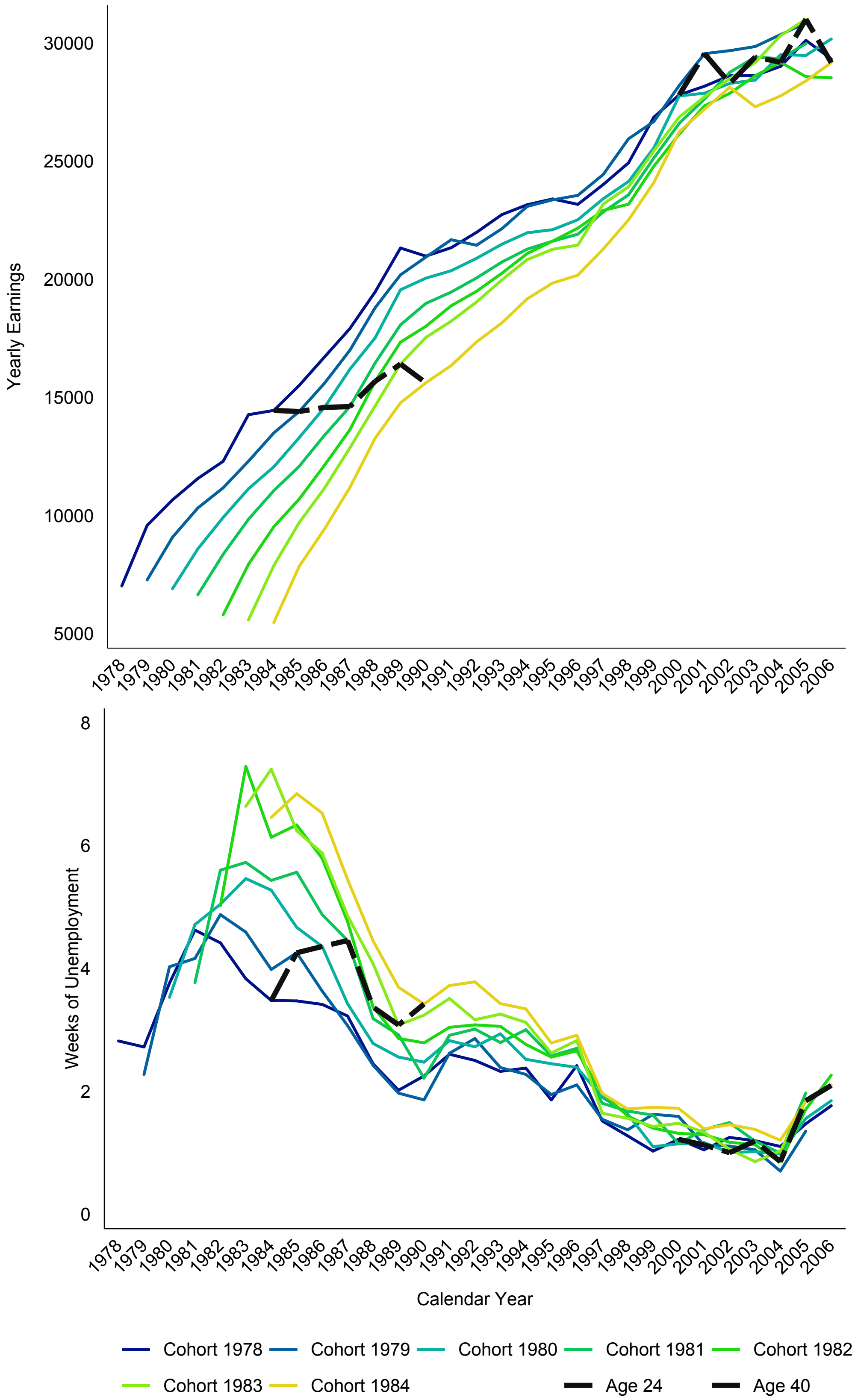
The two main quantitative variables in our model are earnings and experience. Data on earnings for those employed is constructed using tax records, and as such, is very reliable and accurate. Because national insurance, the UK payroll tax, is levied only on labour income, and because we exclude the self-employed, all earnings are wage payments. We add 1 to all earnings before taking the logarithm.

We measure experience as weeks of employment. This is derived from the LLMDB benefit records.[14] Whenever a person was employed, their employer had to collect and pay national insurance contribution on their behalf. These payments determine the right to a state pension and other welfare payments, and can be regarded as accurate: for this reason, we begin by recording individual i as employed in a given year for the number of weeks when this payment is made. Similarly, to receive unemployment benefit or other related welfare payments, a

[13] This strategy excludes around 5% of our sample. The results are robust to varying the number of weeks, to varying the number of years, and to considering only consecutive years.

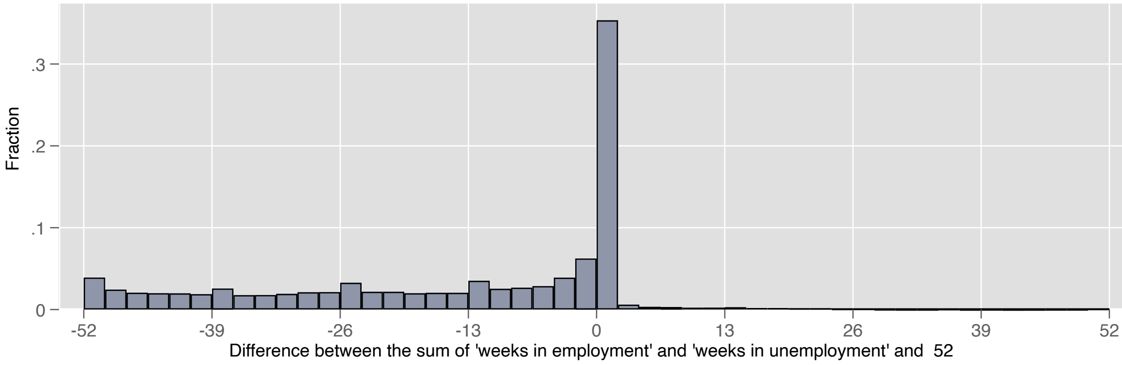
[14] These records are maintained by two separate government departments for different purposes: the Inland Revenue, now renamed HMRC, for tax purposes, and the Department for Work and Pensions, for social security purposes.

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 each of the years 1978 and 1984 (dark lines for individuals born earlier), in the calendar year measured along the horizontal axis. The thick dashed lines highlight outcomes for different cohorts at ages 24 and 40.

Figure 3:
Number of weeks in employment or unemployment (density)



Note: The histogram reports the number of observations with a given difference between 52 and the sum of weeks in employment and weeks in unemployment: a positive number indicates a likely error, a negative number indicates someone not in work and not claiming benefits.

person had to be recorded as unemployed, and precise and accurate record keeping was important for both the government and the recipient.

For just over half of the observations in our sample, the numbers of employed and unemployed weeks in the year add up to 52. If the two numbers, "weeks employed" and "weeks unemployed" add up to less than 52 for individual i, then i was neither working nor claiming benefits in a certain week. This would be the case because they were not entitled to receive benefits or because they omitted to claim benefits for whatever reason. So we make the assumption that when a person is recorded as neither employed nor unemployed, that is when they are neither claiming benefits nor paying national insurance contribution, then this person is in fact not active in the labour market, and in particular not employed, and so not gaining experience. This natural imputation insures consistency between the "unemployment" and the "employment" variables for over 97% of the observations in our dataset. For the remaining 3% of observations, "employed weeks" and "unemployed weeks" add up to more than 52: this is likely to constitute an error. In these cases, we calculate the number of week in employment using information from the ratio of reported "employed weeks" and reported "unemployed weeks" in the three year window centred in the year considered. Figure 3 reports the distribution of the number of weeks recorded as employed or unemployed, with the centre at 0, for someone recorded as either employed or unemployed in every week of the year.

Summary statistics for our dataset are presented in Figure 2 and in Table A1 in the

Appendix. Figure 2 reports 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 2004 pounds. The colour of the various lines move from blue to yellow for later cohorts. We have also plotted, in dashed black, the line that joins the values for each cohort at age 24 and at age 40.[15]

The vector X_i^t in (23) includes an indicator variable that takes a value of 1 if the individual is in receipt of a benefit other than unemployment benefit in that 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 consecutive year $t-1$ and year t. Similarly, for three and five years. These indicator variables capture the non-linear effects of protracted periods of structural unemployment, the cause of which is likely to be severe shocks, for example to health, rather than the negative impact of youth unemployment on long-run wages.[16]

The last terms in (23) are fixed effects interacted with one another. As explained in (22), these are essential features of our econometric specification. They allow us to separate the effects of labour market shocks from the influence of individual characteristics, the conditions of the local labour market where individuals find themselves or move to, and 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.[17]

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 of a short distance and, therefore,

[15] 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. In our regressions, we use the unadjusted data and control for these changes in processes using time fixed effects.

[16] One way to think about the inclusion of these effects is to ask what the effect on life-time wages is, conditional on being in the labour market. Whether or not an individual is in the labour market may, in some cases, be due to their early employment experience, and thus an endogenous outcome. We argue that these will be a very small number, and, even in such cases, given that our interest is specifically on the conditional effect, these controls are not 'bad controls'.

[17] A full list of local authority districts is available at geoportal.statistics.gov.uk/datasets/local-authority-districts-april-2019-names-and-codes-in-the-united-kingdom. We use these, instead of "travel to work areas" (TTWA), as the latter 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.

subsequent local areas of residence are good predictors of previous ones, some people will have moved further and these individuals may be those whose labour market skills are more specialised, which implies that they may be systematically different from the rest. To account for this difference, we introduce an artificial second set of area fixed effects: the *inferred* area fixed effects, 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 between two "naïve" alternatives. In the first of these alternatives, we assume that all locations in the country are equivalent, implicitly arguing that the labour market consequences of individuals' moves are on average zero; in practice, we impute a single "notional-national" address to observations where the information on location is missing.[18] The results for this treatment of missing observations are reported in Column (3) of Table 2. In the second "naïve" alternative, the assumption is that the current address has perfect predictive power for previous addresses, that is, we do not distinguish between a and a^0. The results for this case are reported in Column (4) of Table 2. These extremes can be seen as upper and lower bounds containing the true estimate, and comparisons between Column (3) in Table 1 and Column (3) in Table 2 suggest that our results are not sensitive to alternative treatments of missing address.

6 Results

6.1 Baseline specification

The results from our preferred specification are reported in Table 1. As discussed above, the regression in (23) is estimated for the subset of individuals who are in the sample from age 18 to 40, pooled across cohorts. All coefficients are normalised to 100: they are therefore the effect, in percentage terms, of an additional week of experience in the relevant period of youth on earnings at the age given in the corresponding row. That is, they may be interpreted in line with the literature, as the "scar" effect, in percentage terms, the loss of a week of work on earnings in the relevant period of youth, at the age on the corresponding row.

The first coefficient in each column of Table 1 shows the effect of the number of weeks

[18]We could also drop all observation with a missing address: this is an inferior alternative, though, as it would reduce the sample size, make the panel unbalanced, and omit individuals' observations in a way likely to be correlated to their employment record.

employed in that year on earnings in that year: being unemployed for an additional week at age 40, other things equal, brings on a reduction in annual earnings at age 40 of about 2.37% for men and 1.97% for women. These figures are very close to $\frac{1}{52}$, the proportionate earnings loss of a week of employment. We see this as a reassuring sign that our results are not driven by omitted variable bias.

The rest of the table presents the long term effect of experience on current earnings. In the first column, we report, for men, the effect of "experience as Entrant" only. That is, we impose restrictions (16)-(17), namely $\beta_Y^t = \beta_A^t = 0$, in (23). If a man works for a week less between ages 18 and 20, his earnings during two year intervals, between ages 23 and 40, are lowered by the percentage amount in the corresponding row of the table. Thus, for example, the coefficient "On earnings aged 35-36" means that one fewer week in work between ages 18 and 20 (inclusive) decreases annual earnings received between ages 35 and 36 (inclusive) by 0.4%. Similarly, the rows in the second column, labelled "Youth", report coefficients measuring the effect of an additional week not in work between ages 21 and 23: (18)-(19) replace (17), that is, β_Y^t is unrestricted in (23). The third column reports our benchmark specification, adding the period of "early Adulthood", the ages between 24 and 26: in this column, the restrictions on the coefficient βs are (16), (18), (20), and (21). For example, consider the last coefficient in Column (3) in the row labelled "On earnings aged 35-36" in the first "block", labelled "Entrant Experience (18-20)". This is estimated to be 0.39: this says that had the average man experienced an extra week of work when he was aged between 18 and 20, then his earnings at ages 39 and 40 would have been 0.39% higher. Correspondingly an extra week of unemployment (or being out of the labour market) would have lowered his age 39-40 earnings by 0.39%.

There is a strong direct effect of experience at entry (age 18-20) on lifetime earnings, up to the age of 40, and, although in a necessarily smaller sample, there is no indication that this effect is dampened when we consider a longer time frame (Figure 6). Recall that Columns (2) and (3) add first experience between ages 21 and 23, and then between ages 24 and 26. The similarity of the coefficients in the first block in the first three columns suggests that, for men, the long term effects of experience for an entrant are fully captured by β_E. The lack of statistical significance for these coefficients suggest that experience in Youth and early Adulthood does not have a long term effect on earnings. We interpret this as strong evidence

Table 1: Long term effects of early labour market experience

	Entrant	Entrant & Youth	Whole Period	Women	IV
	(1)	(2)	(3)	(4)	(5)
Weeks Employed in Year	2.36***	2.37***	2.37***	1.97***	2.23***
	(0.05)	(0.05)	(0.05)	(0.03)	(0.06)
Entrant Experience (18-20)					
On Earnings Aged 23-24	0.22***	0.23***	0.23***	0.48***	0.24***
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
On Earnings Aged 25-26	0.33***	0.32***	0.32***	0.55***	0.31***
	(0.04)	(0.05)	(0.05)	(0.05)	(0.05)
On Earnings Aged 27-28	0.41***	0.42***	0.42***	0.59***	0.35***
	(0.04)	(0.05)	(0.05)	(0.05)	(0.05)
On Earnings Aged 29-30	0.51***	0.45***	0.45***	0.57***	0.35***
	(0.04)	(0.05)	(0.05)	(0.05)	(0.05)
On Earnings Aged 31-32	0.50***	0.43***	0.43***	0.54***	0.28***
	(0.04)	(0.05)	(0.05)	(0.05)	(0.05)
On Earnings Aged 33-34	0.45***	0.38***	0.38***	0.54***	0.30***
	(0.04)	(0.05)	(0.05)	(0.05)	(0.05)
On Earnings Aged 35-36	0.44***	0.37***	0.37***	0.46***	0.29***
	(0.04)	(0.05)	(0.05)	(0.06)	(0.05)
On Earnings Aged 37-38	0.43***	0.36***	0.37***	0.44***	0.28***
	(0.04)	(0.05)	(0.05)	(0.06)	(0.05)
On Earnings Aged 39-40	0.41***	0.39***	0.39***	0.41***	0.27***
	(0.04)	(0.05)	(0.05)	(0.06)	(0.06)
Youth Experience (21-23)					
On Earnings Aged 25-26		0.01	0.01	0.04	-0.02
		(0.04)	(0.04)	(0.04)	(0.04)
On Earnings Aged 27-28		-0.02	-0.02	0.06	0.05
		(0.04)	(0.04)	(0.05)	(0.05)
On Earnings Aged 29-30		0.11*	0.06	0.19***	0.07
		(0.04)	(0.05)	(0.05)	(0.05)
On Earnings Aged 31-32		0.14**	0.12*	0.16**	0.18***
		(0.05)	(0.05)	(0.05)	(0.05)
On Earnings Aged 33-34		0.13**	0.12*	0.13*	0.15**
		(0.05)	(0.05)	(0.05)	(0.05)
On Earnings Aged 35-36		0.14**	0.13*	0.16**	0.11*
		(0.05)	(0.05)	(0.06)	(0.05)
On Earnings Aged 37-38		0.12*	0.15**	0.19**	0.11
		(0.05)	(0.05)	(0.06)	(0.06)
On Earnings Aged 39-40		0.06	0.09	0.21***	0.07
		(0.05)	(0.05)	(0.06)	(0.06)
Early Adulthood Exper. (24-26)					
On Earnings Aged 29-30			0.09*	-0.16***	0.09
			(0.04)	(0.04)	(0.05)
On Earnings Aged 31-32			0.03	-0.17***	0.10*
			(0.04)	(0.04)	(0.05)
On Earnings Aged 33-34			0.04	-0.19***	0.14**
			(0.05)	(0.04)	(0.05)
On Earnings Aged 35-36			0.02	-0.24***	0.14**
			(0.05)	(0.05)	(0.05)
On Earnings Aged 37-38			-0.06	-0.28***	0.07
			(0.05)	(0.05)	(0.05)
On Earnings Aged 39-40			-0.08	-0.30***	0.03
			(0.05)	(0.05)	(0.05)
N	279877	279877	279877	265292	303258
Individuals	13495	13495	13495	12778	13684

Note: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. The dependent variable is the log total annual earnings. Standard errors are clustered by local labour market age 18 and are below the associated coefficients. Reported coefficients are OLS estimates of (23) in columns 1–4 and 2SLS estimates in column 5. The coefficients measure the percentage effect of an increased week in employment in the three age brackets, 18-20, 21-23, and 24-26, on annual earnings in different subsequent periods. We include dummies for receipt of benefits and long-term unemployment in the year. We also include individual, local labour market, cohort, and time fixed effects and their interactions, as explained in the discussion of (22).

that the long term “damage” of youth unemployment is concentrated in the earlier years. The possible channels through which this effect operates are explored further in Section 6.3.

The fourth column is again our benchmark specification but now estimated for women rather than men. There are some qualitative differences relative to men: the effect of entrants' experience and the effect of current unemployment on earnings is larger than that for men, though as individuals become older, the coefficients become similar. Also larger are the effects of negative shocks in Youth (age 21 to 23). Conversely, and counter-intuitively, experience in early Adulthood (age 24 to 26) seems to have a negative effect on future earnings. This is in contrast to the results for men for whom there was no detectable effect.

A potential explanation for this finding is that some women who work more during their 20s (and thus accumulate more experience) are less likely in to have children in their 20s and are more likely to have them instead in their 30s, hence the negative correlation between early experience and labour market outcomes in their 30s. We are unable to verify that this is the case in our data, but this positive correlation is consistent with the findings of Blackburn et al. (1993) and Loughran and Zissimopoulos (2009), who find that having children later correlates with higher lifetime earnings. Adda et al. (2017) build a dynamic model of career, and “find that fertility explains an important part of the gender wage gap, especially for women in their mid-30s”.

As discussed in the Introduction, we feel less confident about the results for women because we lack information on two important determinants of earnings: the number of hours worked per week and childbearing choices. This might also explain the difference to Gregg's (2001) findings of a weaker persistence for women, since he can control for a large array of individual characteristics, including, of course, those related to childbearing.

Column (5) of Table 1, reports IV estimates which allow for the possibility that an omitted variable, such as labour market attachment, correlated with both the current employability of a person and their future earnings may be driving our results. In the spirit of Oreopoulous et al. (2012) worker i's experience at entry in the labour market is instrumented with average local experience in the corresponding year, that is 52 minus the average weeks of unemployment of the Entrants in the local authority district where they were resident at the time of entry in the labour market. Formally, we re-write (23)-(24) replacing $e_i^t \dots e_i^{18}$ with $\hat{e}_i^t \dots \hat{e}_i^{18}$ where $\hat{e}_i^t$ are the values of e_i^t instrumented with $\overline{e}_a^t$ the average unemployment in local market a at time t: $\hat{e}_i^r = f\left(\overline{e}_a^{18}, \overline{e}_a^{19}, \ldots, \overline{e}_a^r\right)$, for $r = 18, \ldots, t$, and $t = 18, \ldots, 26$. The similarity of Column (5), which reports the estimates obtained in this way, and the main regression in Column (3) suggests that we can be confident that our results are not driven by omitted variable bias.

Our results are illustrated in Figure 4. In the diagrams, the horizontal axis gives two-year age windows, and the vertical axis gives the coefficients reported in Column (3) of Table 1. These coefficients measure the positive effect of an additional week of experience, and therefore the *scar* effect of a additional week spent in unemployment rather than work, in the three age brackets we consider, on the yearly earnings at the age window marked on the horizontal axis. The three age brackets we consider are 18-20 (β_E^t, shown as the solid line), 21-23 (β_Y^t, shown as the dashed line), and 24-26 (β_A^t, the dotted line). The thin lines are the 95% confidence intervals around the estimated coefficients. The LHS in Figure 4 shows how the effect of experience for labour market entrants increases with age when experience is gained, settling around age 30 at around 0.3%. Conversely, the effect of an extra week's experience in Youth and early Adulthood (age 21-26) is not significantly different from 0. To get a handle on the magnitude of these effects, the coefficients indicate that missing one year of experience between ages 18 and 20 leads to a long term permanent earnings loss of around 20% per year.[19] This is the scar effect of unemployment, and whilst this estimate implies a large effect, it is in line with previous estimates. Specifically, it is towards the upper end of US estimates of earnings losses due to displacement, which range from 7% (Stevens 1997) to over 20% (Jacobson et al. 1993), and is similar to Gregg and Tominey's (2005) cross sectional IV estimates for the UK, which are between 13% and 21%.

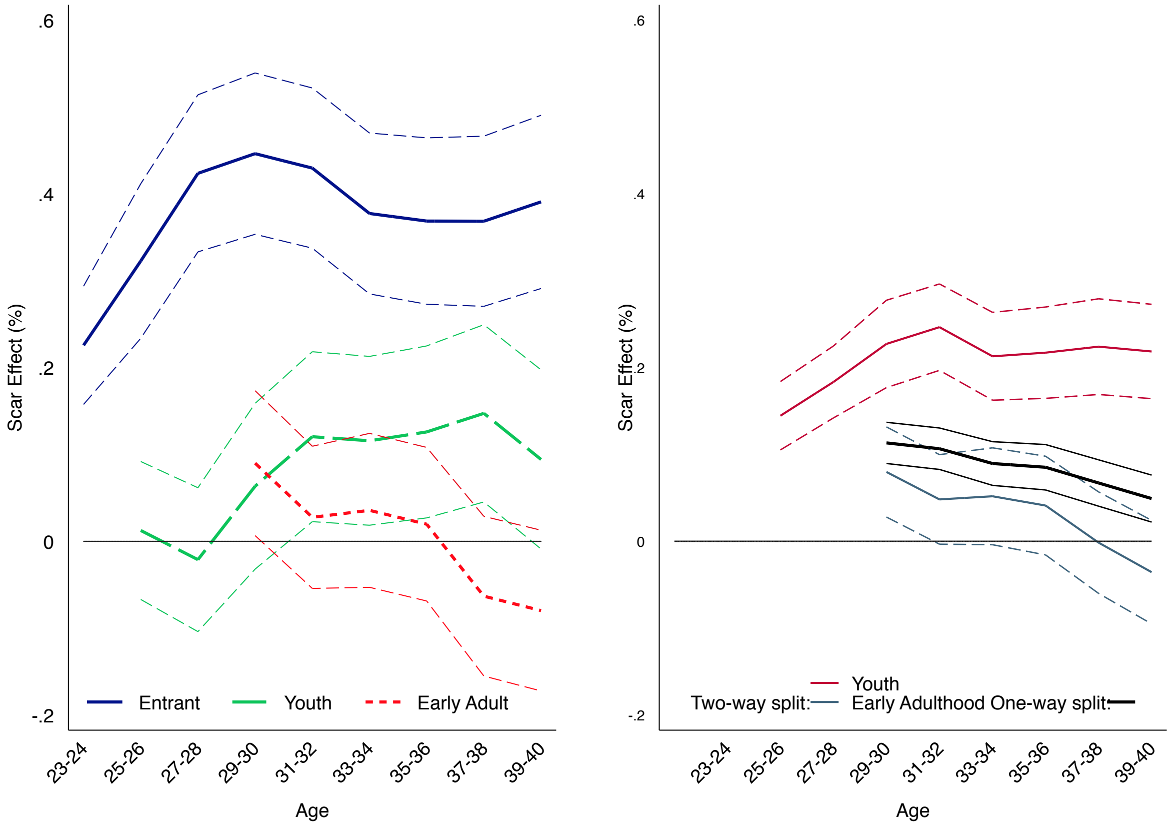
On the RHS of Figure 4 we depict, as the solid black line, the coefficients obtained when we impose the following restrictions on the βs:

$$\beta_Y^t = \beta_s^t, \qquad s = 18, \ldots, 26, \qquad t > 22; \tag{25}$$

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

[19] Take as an example the coefficient of 0.39, which measures the effect of one extra week of experience at ages 18-20 on the earnings at age 39-40. As earnings are measured in logs, this says that one week of unemployment reduces earning by 0.39%, that is, if we consider 52 weeks, the total effect is $0.0039 \times 52 = 20.2\%$. Of course this calculation assumes a linear effect, which is unlikely to apply over this long a period. A better interpretation is that a month extra unemployment reduces long term earnings by $0.0039 \times 4 = 1.6\%$.

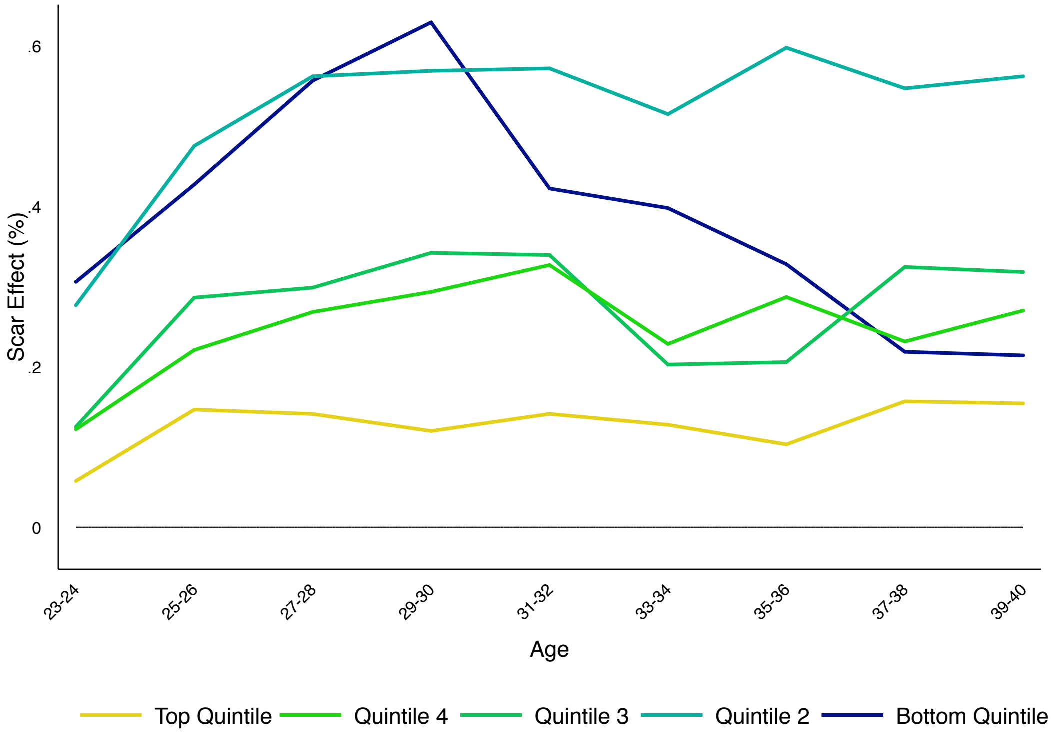
Figure 4:
Long term effects of early labour market experience



Note: The left-hand side panel reports estimated coefficients from equation (23) for the effect of experience for Entrants, Youths, and early Adults, β_E^t (solid line), β_Y^t (dashed line), and β_A^t (dotted line); reported in Column (3) of Table 1. The right-hand side panel reports estimated coefficients from different restrictions on the β coefficients. The red and blue lines report the two-way split given by (27)-(29), the black line the one-way split given by (25)-(26). The corresponding estimated coefficients are in Columns (4) and (5) of Table A2. In both panels, the dashed lines include the 95% confidence intervals.

that is, when $\beta_E^t = \beta_Y^t = \beta_A^t$ in (23), so that no distinction is made between the ages of 18 and 26, reported in Column (5) of Table A2. These βs give the overall effect of experience between ages 18 and 26 on future earnings. The thin lines include the 95% confidence interval. This line indicates a much lower and slightly decreasing effect of experience in the first nine years after entry into the labour market than those depicted on the LHS of Figure 4. The strong suggestion emerging from the comparison of the three lines on the LHS in Figure 4 and the solid black line on the RHS is that lumping together all the labour market shocks that occur between 18 and 26 is misleading, as it misses the large difference between Entrant, Youth and early Adulthood experience highlighted by our findings. Averaging large and

Figure 5:
Long term effects of experience on entrants of different abilities



Note: Coefficients β_E^t are calculated for subsamples of individuals for each quintile, membership of which is derived from the distribution of predicted earnings potentials. The values of the coefficients used to draw the lines are in Table A3. We have not included confidence intervals.

statistically significant coefficients for ages between 18 and 20 with those close to 0 for later years almost completely conceals the long run effect of labour market shocks experienced in early youth.

The RHS of the figure also reports the coefficients obtained when youth is split into two periods, that is, when the set of restrictions (16)-(19) is replaced by:

$$\beta_1^t = \beta_s^t, \qquad s = 18, \ldots, 22, \qquad t > 24; \tag{27}$$

$$\beta_2^t = \beta_s^t, \qquad s = 23, \ldots, 26, \qquad t > 27; \tag{28}$$

$$\beta_s^t = 0, \qquad s \geqslant 27, \qquad t > s. \tag{29}$$

Where the subscripts 1 and 2 label the first and the second part of the whole period. The coefficients are reported in Column (5) of Table A2 in the Appendix. We find, unsurprisingly given our other main results, a precisely estimated, but smaller effect for the first period (ages

18 to 22, the solid red line on the RHS of Figure 4, with the dashed lines as the confidence interval) and no effect for the second (ages 23 to 27, the blue lines).

To close this section, we reprise one of the questions addressed by Oreopoulos et al. (2012), namely, whether the effect of experience is different for individuals with different abilities. Following their strategy, and similarly to Cornelissen et al. (2017), we split the sample into five "ability quintiles", and allocate individuals to quintiles according to their estimated fixed effect from the main regression in Column (3) in Table 1. The results, in of this exercise are presented in Figure 5, again only for the shocks in the Entry period. The lack of a direct measure of ability or earning potential, as well as the period covered make the analysis only suggestive, yet the figure indicates a plausible conclusion: individuals in the lower ability groups suffer more severe scars from early unemployment. Indeed, Table A3 in the Appendix shows that for the top quintile the scar effect is not significantly different from 0. The worsening of the scar effect for less able workers is a worrying aspect of our results: negative labour market shocks seems to reduce the income of the lowest paid workers the most. The decade of very high youth unemployment in many developed countries following the 2008 financial crisis seems therefore bound to cast a long lasting shadow on the future by exacerbating the inequality of lifetime incomes for these cohorts. The picture is likely even bleaker in the face of the Covid-19 crisis.

6.2 Robustness analysis

The results in Table 2 confirm that our results are robust to changes in the empirical specification. Column (1) shows that our estimates change only marginally when we include the individuals who are likely to be university students. Similarly, Column (2) shows this to be the case also when, instead of excluding altogether individuals who report ever being self-employed, as we do in Table 1, we exclude only the observations for the years in which they report being self-employed.[20] In Columns (3) and (4), we report estimates handling missing addresses in two alternative ways. Column (3) 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 (4) replaces a missing address with the next recorded address, treating

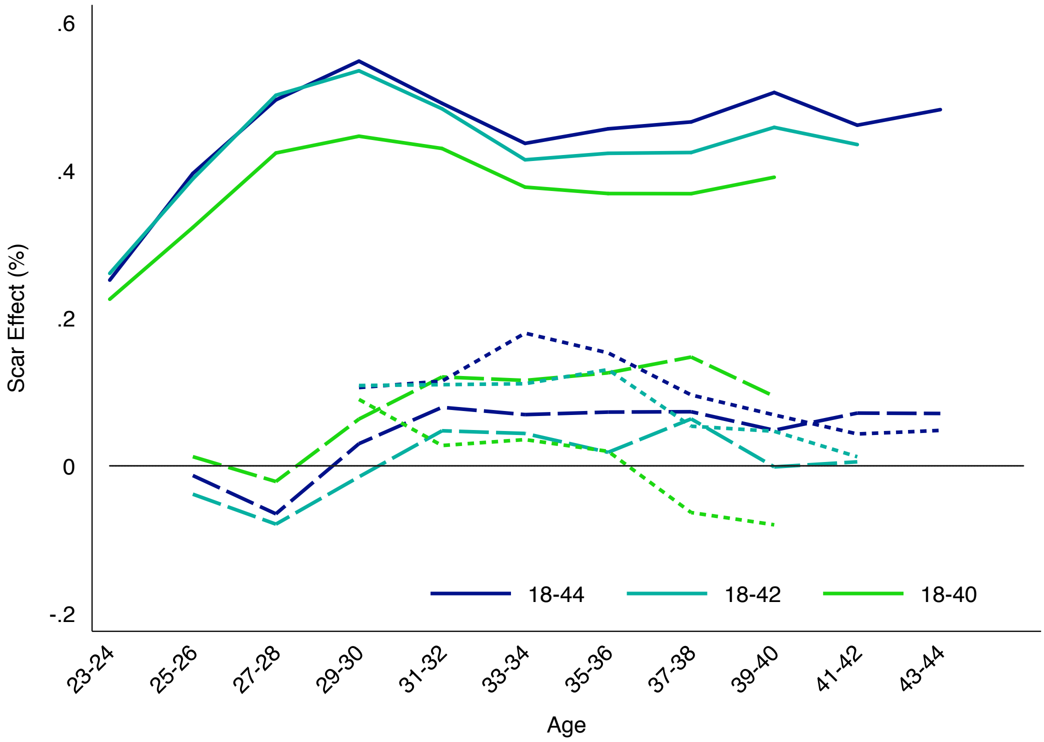
[20] For example, if individual i from the cohort entering in 1978 is recorded as being self-employed in years 1994 and 1997, we include the observations from all other years in the estimations reported in Column (2).

Table 2: Robustness Checks

	Students	Self-Employed	National Address	First Address	Alternative Clustering
	(1)	(2)	(3)	(4)	(5)
Weeks Employed in Year	2.32***	2.36***	2.34***	2.46***	2.37***
	(0.05)	(0.05)	(0.09)	(0.05)	(0.04)
Entrant Experience (18-20)					
On Earnings Aged 23-24	0.32***	0.23***	0.25***	0.27***	0.23***
	(0.04)	(0.03)	(0.04)	(0.03)	(0.03)
On Earnings Aged 25-26	0.45***	0.32***	0.35***	0.39***	0.32***
	(0.05)	(0.04)	(0.05)	(0.04)	(0.04)
On Earnings Aged 27-28	0.54***	0.41***	0.44***	0.47***	0.42***
	(0.05)	(0.05)	(0.06)	(0.04)	(0.04)
On Earnings Aged 29-30	0.57***	0.44***	0.47***	0.51***	0.45***
	(0.05)	(0.05)	(0.07)	(0.04)	(0.04)
On Earnings Aged 31-32	0.55***	0.43***	0.44***	0.50***	0.43***
	(0.05)	(0.05)	(0.07)	(0.04)	(0.04)
On Earnings Aged 33-34	0.50***	0.38***	0.39***	0.46***	0.38***
	(0.05)	(0.05)	(0.05)	(0.04)	(0.04)
On Earnings Aged 35-36	0.49***	0.37***	0.38***	0.44***	0.37***
	(0.05)	(0.05)	(0.06)	(0.04)	(0.04)
On Earnings Aged 37-38	0.49***	0.37***	0.38***	0.43***	0.37***
	(0.05)	(0.05)	(0.06)	(0.04)	(0.04)
On Earnings Aged 39-40	0.51***	0.40***	0.41***	0.46***	0.39***
	(0.05)	(0.05)	(0.06)	(0.04)	(0.05)
Youth Experience (21-23)					
On Earnings Aged 25-26	-0.01	0.02	-0.02	-0.01	0.01
	(0.04)	(0.04)	(0.04)	(0.03)	(0.04)
On Earnings Aged 27-28	-0.05	-0.01	-0.04	0.00	-0.02
	(0.04)	(0.04)	(0.05)	(0.03)	(0.04)
On Earnings Aged 29-30	0.04	0.07	0.02	0.09*	0.06
	(0.05)	(0.05)	(0.05)	(0.04)	(0.04)
On Earnings Aged 31-32	0.09	0.12*	0.11	0.13**	0.12**
	(0.05)	(0.05)	(0.07)	(0.04)	(0.04)
On Earnings Aged 29-30	0.09	0.11*	0.09	0.11**	0.12**
	(0.05)	(0.05)	(0.06)	(0.04)	(0.04)
On Earnings Aged 35-36	0.10	0.13*	0.10	0.13**	0.13**
	(0.05)	(0.05)	(0.06)	(0.04)	(0.04)
On Earnings Aged 37-38	0.11*	0.15**	0.12**	0.16***	0.15**
	(0.05)	(0.05)	(0.05)	(0.04)	(0.05)
On Earnings Aged 39-40	0.07	0.10	0.07	0.11**	0.09*
	(0.05)	(0.05)	(0.06)	(0.04)	(0.05)
Early Adulthood Exper. (24-26)					
On Earnings Aged 29-30	0.06	0.08	0.09	0.07	0.09*
	(0.04)	(0.04)	(0.05)	(0.04)	(0.04)
On Earnings Aged 31-32	0.01	0.03	0.03	0.02	0.03
	(0.04)	(0.04)	(0.03)	(0.04)	(0.04)
On Earnings Aged 29-30	0.02	0.04	0.04	0.04	0.04
	(0.05)	(0.04)	(0.05)	(0.04)	(0.04)
On Earnings Aged 35-36	0.00	0.02	0.03	0.05	0.02
	(0.05)	(0.04)	(0.05)	(0.04)	(0.04)
On Earnings Aged 37-38	-0.07	-0.06	-0.06	-0.04	-0.06
	(0.05)	(0.05)	(0.04)	(0.04)	(0.04)
On Earnings Aged 39-40	-0.09*	-0.08	-0.08	-0.06	-0.08
	(0.05)	(0.05)	(0.06)	(0.04)	(0.04)
Constant	10.09***	10.04***	10.03***	10.17***	10.03***
	(0.03)	(0.03)	(0.04)	(0.02)	(0.03)
N	294725	291136	289608	294705	279877
Individuals	14150	13993	13652	13505	13495

Note: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. The dependent variable is the log total annual earnings. Standard errors are clustered by local labour market age 18 and are below the associated coefficients. Reported coefficients are OLS estimates of (23) and measure the percentage effect of an increased week in employment in the three age brackets, 18-20, 21-23, and 24-26, on annual earnings in different subsequent periods. We include dummies for receipt of benefits and long-term unemployment in the year. We also include individual, local labour market, cohort, and time fixed effects and their interactions, as explained in the discussion of (22).

Figure 6:
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 (23) for the effect of unemployment for Entrant, Youth, and early Adult. The dark blue curves are the same as in Figure 4, 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 the first four columns of Table A2. We have not included confidence intervals.

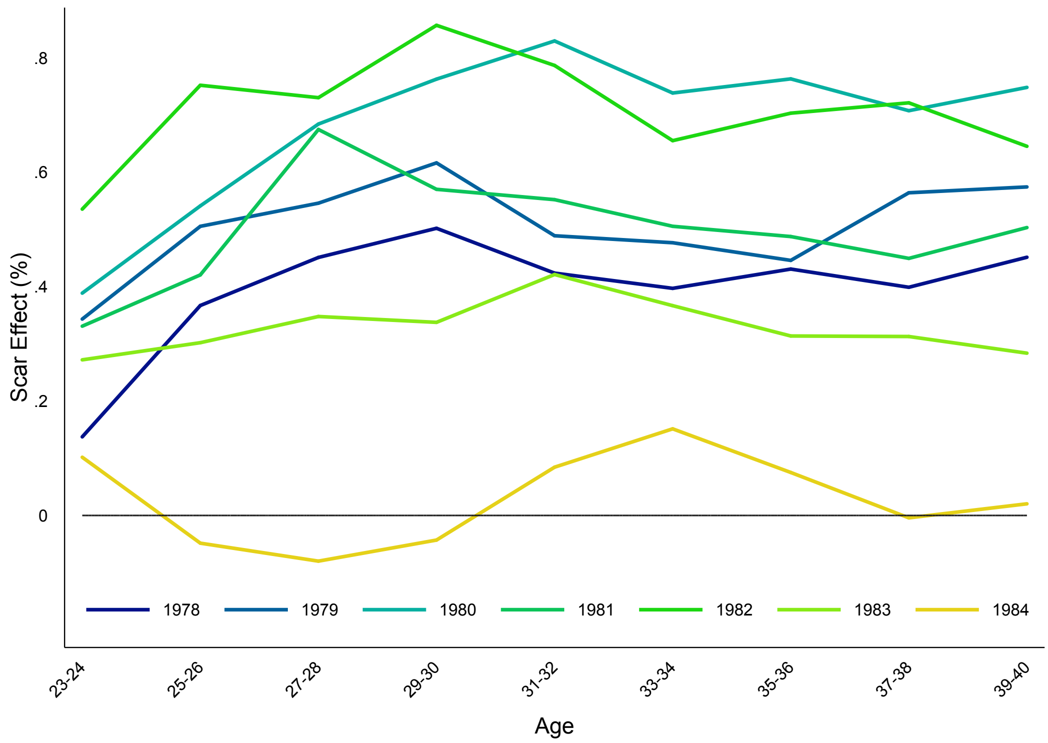
such imputed observations as equivalent to observed addresses and without accounting for the fact that the information is, in fact, missing.[21]

In both columns, results are very similar to those in Column (3) of Table 1, perhaps with some evidence of a small scar effect in the Youth period (21 to 23) in Column (4) only. This suggests that the results are not sensitive to alternative treatments of missing addresses. Column (5) reports results for our benchmark specification but now clustering standard errors at the level of the individual rather than the local labour market. The estimates are a little more precise but qualitatively unchanged.

Figure 6 illustrates the results obtained when we allow the reach of negative shocks to extend beyond age 40. The results are, once again, robust. The smaller sample size (fewer cohorts are observed at older ages) reduces the precision of the estimates a little, but all

[21]The sample size differs from that in Column (3) of Table 1 since the number of singleton groups will differ with different treatments of missing addresses. See Cameron et al. (2015) for a discussion.

Figure 7:
Scar Effect of Entry Unemployment for Different Cohorts



Note: Coefficients β_E^t, are calculated separately for the subsample of individuals in each cohort who are in the sample used in our benchmark specification, 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.

Entrant coefficients remain significant at all conventional levels, as shown in Table A2 in the Appendix.

Oreopoulos et al. (2012) study the effect of the business cycle on the importance of the scar 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 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 7, where, for Entrants only, we break down the pattern shown in Figure 4 by cohort. The coefficients for Youth and early Adulthood are depicted in Figure A2 in the Appendix. The estimates of β_E for the 1983 and 1984 cohorts are less precisely estimated. The 1984 cohort appears to be the only one 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 conceive other possible causes for the difference in this cohort. It seems unlikely that it reflects the overall performance of the economy given that we find a clear scar effect for the 1978 and 1979 cohorts when GDP growth was 4.2% and 3.7% respectively.

6.3 Decomposition of the effect of unemployment at entry

As argued above, in the discussion of Equation (3), the overall effect of experience at entry on later earnings can be decomposed into the direct effect of experience at entry, and its indirect effect, the fact that lower experience due to a spell of unemployment at age, say 19, reduces employability and so it increases the chances of unemployment and the opportunities to gain experience at age, say, 25.

We can re-write the regression model (23) estimated in each of Columns (1)-(3) in Table 1 with the set of restrictions (16)-(17), Column (1), (16) and (18)-(19), Column (2), and (16), (18), and (20)-(21), Column (3). To clarify the model we refer to, we add to the β coefficients a subscript, in parentheses, corresponding to the column of Table 1 from which it is obtained. To lighten notation, we also omit the subscript i in the experience variables e_i^s, and replace the first summation term with $e_{1820} = \sum_{s=18}^{20} e_i^s$. We replace in the same way the other summations with e_{2123}, and e_{2426} respectively. Thus, we can write, for the earnings at ages t, where $t = $ 23-24, ..., 39-40:

$$\log w_i^t = \beta_{E,(1)}^t e_{1820} + \lambda_1(X) + \varepsilon_1, \tag{30}$$

$$\log w_i^t = \beta_{E,(2)}^t e_{1820} + \beta_{Y,(2)}^t e_{2123} + \lambda_2(X) + \varepsilon_2, \tag{31}$$

$$\log w_i^t = \beta_{E,(3)}^t e_{1820} + \beta_{Y,(3)}^t e_{2123} + \beta_{A,(3)}^t e_{2426} + \lambda_3(X) + \varepsilon_3. \tag{32}$$

For some functions, λ_j, of the controls, X, and with error terms, ε_j, $j = 1, 2, 3$. Differentiating, and indicating with a subscript, with slight abuse of notation, the equation from which the partial derivative is derived, we obtain:

$$\frac{\mathrm{d}w_i^t}{\mathrm{d}e_{1820}} = \left.\frac{\partial w_i^t}{\partial e_{1820}}\right|_{(30)}, \tag{30$'$}$$

$$\frac{\mathrm{d}w_i^t}{\mathrm{d}e_{1820}} = \left.\frac{\partial w_i^t}{\partial e_{1820}}\right|_{(31)} + \left.\frac{\partial w_i^t}{\partial e_{2123}}\right|_{(31)} \frac{\partial e_{2123}}{\partial e_{1820}}, \tag{31$'$}$$

$$\frac{\mathrm{d}w_i^t}{\mathrm{d}e_{1820}} = \left.\frac{\partial w_i^t}{\partial e_{1820}}\right|_{(32)} + \left.\frac{\partial w_i^t}{\partial e_{2123}}\right|_{(32)} \frac{\partial e_{2123}}{\partial e_{1820}} + \left.\frac{\partial w_i^t}{\partial e_{2426}}\right|_{(32)} \left(\frac{\partial e_{2426}}{\partial e_{2123}}\frac{\partial e_{2123}}{\partial e_{1820}} + \frac{\partial e_{2426}}{\partial e_{1820}}\right). \tag{32$'$}$$

Substituting now the values from (30)-(32), the above can be written as:

$$\frac{\mathrm{d}w_i^t}{\mathrm{d}e_{1820}} = \beta_{E,(1)}^t, \tag{30$''$}$$

$$\frac{\mathrm{d}w_i^t}{\mathrm{d}e_{1820}} = \beta_{E,(2)}^t + \beta_{Y,(2)}^t \frac{\partial e_{2123}}{\partial e_{1820}}, \tag{31$''$}$$

$$\frac{\mathrm{d}w_i^t}{\mathrm{d}e_{1820}} = \beta_{E,(3)}^t + \beta_{Y,(3)}^t \frac{\partial e_{2123}}{\partial e_{1820}} + \beta_{A,(3)}^t \left(\frac{\partial e_{2426}}{\partial e_{2123}}\frac{\partial e_{2123}}{\partial e_{1820}} + \frac{\partial e_{2426}}{\partial e_{1820}}\right). \tag{32$''$}$$

And, from equating the RHS of (30$''$) and (31$''$):

$$\beta_{E,(1)}^t = \beta_{E,(2)}^t + \beta_{Y,(2)}^t \frac{\partial e_{2123}}{\partial e_{1820}},$$

$$\frac{\partial e_{2123}}{\partial e_{1820}} = \frac{\beta_{E,(1)}^t - \beta_{E,(2)}^t}{\beta_{Y,(2)}^t}. \tag{33}$$

An analogous equation to the above, for the successive period, can be used similarly to derive:

$$\frac{\partial e_{2426}}{\partial e_{2123}} = \frac{\beta_{Y,(2)}^t - \beta_{Y,(3)}^t}{\beta_{A,(3)}^t}. \tag{34}$$

Next, equating the RHS of (31$''$) and (32$''$):

$$\beta_{E,(2)}^t + \beta_{Y,(2)}^t \frac{\partial e_{2123}}{\partial e_{1820}} = \beta_{E,(3)}^t + \beta_{Y,(3)}^t \frac{\partial e_{2123}}{\partial e_{1820}} + \beta_{A,(3)}^t \left(\frac{\partial e_{2426}}{\partial e_{2123}}\frac{\partial e_{2123}}{\partial e_{1820}} + \frac{\partial e_{2426}}{\partial e_{1820}}\right),$$

from which, after some rearrangement, we derive the effect of experience as an Entrant on employment as a young Adult:

$$\frac{\partial e_{2426}}{\partial e_{1820}} = \frac{\beta_{E,(2)}^t - \beta_{E,(3)}^t}{\beta_{A,(3)}^t}. \tag{35}$$

From equation (32$'$), we can write the total effect of experience at age 18-20 on earnings at age t, with $t = 29\text{-}30, \ldots, 39\text{-}40$, as:

$$\frac{\mathrm{d}w_i^t}{\mathrm{d}e_{1820}} = \overbrace{\beta^t_{E,(3)}}^{(1)} + \overbrace{\underbrace{\beta^t_{Y,(3)}}_{(3)} \underbrace{\frac{\partial e_{2123}}{\partial e_{1820}}}_{(4)}}^{(2)} + \overbrace{\underbrace{\beta^t_{A,(3)}}_{(6)} \left(\underbrace{\frac{\partial e_{2426}}{\partial e_{2123}}}_{(7)} \underbrace{\frac{\partial e_{2123}}{\partial e_{1820}}}_{(4)} + \underbrace{\frac{\partial e_{2426}}{\partial e_{1820}}}_{(8)} \right)}^{(5)}, \tag{36}$$

where the braces group the terms that are calculated empirically from the regression in Table 1, and reported in Table 3. Substituting the values of the effects of past experience on current experience, that is $\frac{\partial e_{2123}}{\partial e_{1820}}$ from (33), $\frac{\partial e_{2426}}{\partial e_{2123}}$ from (34), and $\frac{\partial e_{2426}}{\partial e_{1820}}$ from (35):

$$\frac{\mathrm{d}w_i^t}{\mathrm{d}e_{1820}} = \overbrace{\beta^t_{E,(3)}}^{(1)} + \overbrace{\underbrace{\beta^t_{Y,(3)}}_{(3)} \underbrace{\frac{\beta^t_{E,(1)} - \beta^t_{E,(2)}}{\beta^t_{Y,(2)}}}_{(4)}}^{(2)} + \overbrace{\underbrace{\beta^t_{A,(3)}}_{(6)} \left(\underbrace{\frac{\beta^t_{Y,(2)} - \beta^t_{Y,(3)}}{\beta^t_{A,(3)}}}_{(7)} \underbrace{\frac{\beta^t_{E,(1)} - \beta^t_{E,(2)}}{\beta^t_{Y,(2)}}}_{(4)} + \underbrace{\frac{\beta^t_{E,(2)} - \beta^t_{E,(3)}}{\beta^t_{A,(3)}}}_{(8)} \right)}^{(5)}.$$

These values can be obtained by calculation from the estimations reported in Table 1. Table 3 collects the values obtained from these calculations, for each of the year pairs from 29-30 to 39-40. Thus for example, the value 0.063 in the first row of Column (3) in Table 3 corresponds to the value, rounded to 0.06 reported in the third row in the middle block of Column (3) in Table 1. Conversely, consider Column (4) in Table 3: from (33), we see that this is the direct effect of experience at age 18-20 on experience at age 21-23. Its value is obtained as the ratio of 0.51, the fourth row in the first block of Column (1) in Table 1, minus 0.45, the fourth row in the first block of Column (2) in Table 1, and 0.11, the third row in the middle block of Column (2) in Table 1. The value of this ratio is 0.524, suggesting that two additional weeks out of work at age 18-20 are associated with little more than one additional week out of work at age 21-23. And similarly for the remaining coefficients.

While these calculations are only suggestive, they indicate that the indirect effect of Entrant experience via Youth experience, calculated in Column (2), is small compared to the direct effect reported in Column (1), suggesting that the scar effect is not mediated via higher subsequent unemployment. Comparison of Columns (3) and (4) suggests that this is not because Entrant experience does not affect Youth experience, but rather because Youth experience is not an important determinant of later earnings. Column (5) shows the effect of experience at age 18-20 on earnings at age t, $t = 29\text{-}30, \ldots, 39\text{-}40$, via the negative impact of not being employed at age 24-26. This is conceptually similar, but slightly more complex

Table 3:
Decomposition of the Scar Effect

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
On Earnings Aged 29-30	0.446	0.033	0.063	0.524	0.029	0.090	0.503	0.058
On Earnings Aged 31-32	0.430	0.063	0.120	0.525	0.009	0.028	0.529	0.036
On Earnings Aged 33-34	0.377	0.061	0.116	0.527	0.011	0.036	0.527	0.040
On Earnings Aged 35-36	0.369	0.066	0.126	0.528	0.006	0.020	0.577	-0.006
On Earnings Aged 37-38	0.368	0.077	0.147	0.523	-0.021	-0.063	0.481	0.077
On Earnings Aged 39-40	0.391	0.046	0.094	0.491	-0.024	-0.080	0.483	0.069

Note: The table reports the decomposition of scar effect into direct and indirect components: its columns correspond to the terms of equation (36). Column (1) is the direct effect of Entrant experience, $\beta^t_{E,(3)}$. Column (3) is the effect of Entrant experience, $\beta^t_{Y,(3)}$, and Column (4) is the effect of Entrant experience on Youth experience, $\frac{\partial uw_{2123}}{\partial uw_{1820}}$, derived in (33). Column (2) is the product of Columns (3) and (4). Column (6) is $\beta^t_{A,(3)}$, the effect of early Adult experience on future earnings. Column (7) is the effect of Youth experience on early Adult experience $\frac{\partial uw_{2426}}{\partial uw_{2123}}$, derived in (34). Column (8) is the effect on early Adult experience of Entrant experience $\frac{\partial uw_{2426}}{\partial uw_{1820}}$, see (35). Column (5) is given by the sum of Column (8) and the product of Column (7) and Column (4) all multiplied by Column (6). That is, $\beta^t_{A,(3)} \times \left(\frac{\partial uw_{2426}}{\partial uw_{2123}}\frac{\partial uw_{2123}}{\partial uw_{1820}} + \frac{\partial uw_{2426}}{\partial uw_{1820}}\right)$.

than the above. As shown in (36), it is the product of two terms: (i) the direct effect of experience at age 24-26 on earnings at age t, reported in Column (6), multiplied by the sum of the direct effect of experience at age 18-20 on experience at age 24-26 ($\frac{\partial e_{2426}}{\partial e_{1820}}$ reported in Column (7)) and (ii) the effect of experience at age 18-20 on experience at age 24-26 via experience at age 21-23. This, in turn, is the product of two terms, the effect of experience at age 18-20 on the experience at age 21-23 ($\frac{\partial e_{2123}}{\partial e_{1820}}$ reported in Column (4)) times the effect of experience at age 21-23 on the experience at age 24-26 (reported in Column (7)). Looking at Column (5), we can see that the magnitude is similar to that of Column (2) and again much smaller than (1). This, in turn, can be seen to reflect the limited importance of adult experience in Column (6).

Taken together, the results reported in Table 3 suggest that the scar effect associated with a negative labour market shock aged 18-20 is driven by its direct effect on subsequent earnings and, while it is also associated with lower subsequent employment, this indirect effect has little effect on future wages. One possible, non-exclusive, interpretation of this result is that lower experience at age 18-20 is particularly associated with reduced experimentation and learning as in Papageorgiou (2014) or Wee (2016), rather than stigma effects which might be expected to operate through subsequent periods of unemployment, as in Lockwood (1991).

7 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, all of which report evidence of a scar), Japan (Genda et al. 2010), and Sweden (Eliason and Storrie 2006), among other countries. The paper by Schmillen and Möller (2012), which follows cohorts of American men born between 1950 and 1954, highlights the importance of early shocks for lifetime labour market outcomes. Our paper contributes to this literature by confirming the UK studies with a different dataset. It underlines the difference between "past" experience and "youth" experience, and it is also the first paper to document convincingly that not all labour market shocks are equal. Mirroring the well established finding for the development of cognitive abilities in children, we report the distinct, much more severe, effects of being unemployed at the very beginning of one's working life. A period of unemployment at this stage causes a permanent large loss of earnings, while an unemployment shock later is less harmful.

As more high quality administrative data becomes available in several countries, it will become more feasible to verify if the different importance of shock occurring in different periods of individuals' life is a feature of labour markets in different periods and in different countries. Should this prove to be a consistent pattern in different conditions and environments, it would pave the way for more targeted active labour market intervention, which can be made more effective by concentrating policies such as subsidised internships, hiring incentives, or vocational training, towards workers at the earlier stages of their labour market engagement. To the extent that a rapid recovery by some groups of workers from the Covid-19 shock may have a stronger multiplier effect on the remainder of the economy, then an understanding of where to direct scarce resources to maximise their medium term impact is important. Evidence of the different response of workers with different characteristics to unemployment shocks enhances this understanding.

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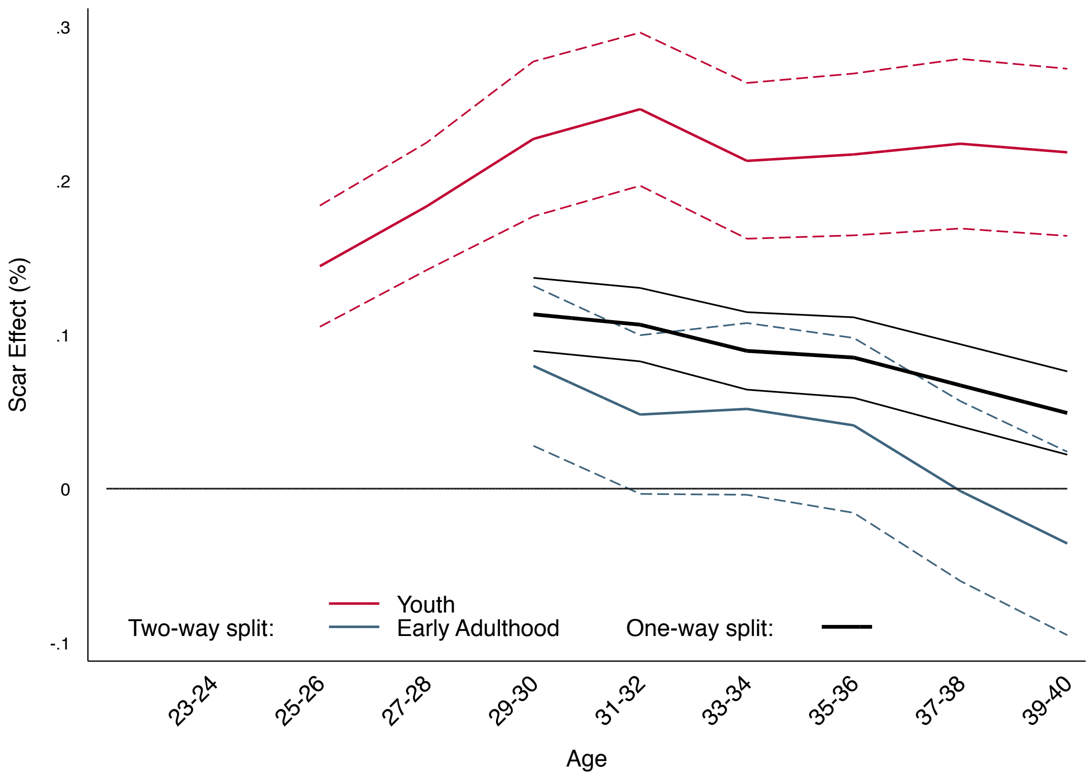
Appendix: Additional Tables and Figures

Figure A1:
The scar effect of youth unemployment



Note: The aquamarine lines are the coefficients of the 2-way split, that is, when (23) is estimated with the restrictions (27)-(29). The black line plots the coefficients imposing restrictions (27)-(29).

Table A1:
Summary statistics for individuals in sample for Table 1

	1960	1961	1962	1963	1964	1965	1966	**Total**
Unemployment at 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**
Earnings at age								
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**
Receiving benefits at age								
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, measured in 2004 pounds, at ages 23-24 to 45-46. The third part is the proportion who claimed at least one benefit.

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

	(1)	(2)	(3)	(4)	(5)
	Sample 18-40	Sample 18-42	Sample 18-44	Whole Youth	Entrant + early Adult
Current Unemployment	2.365***	2.337***	2.269***	2.289***	2.336***
	(0.048)	(0.095)	(0.122)	(0.047)	(0.048)
Entrant Experience (18-20)					
On Earnings Aged 23-24	0.226***	0.261***	0.251***		
	(0.035)	(0.043)	(0.059)		
On Earnings Aged 25-26	0.323***	0.388***	0.396***		
	(0.045)	(0.053)	(0.045)		
On Earnings Aged 27-28	0.423***	0.502***	0.495***		
	(0.046)	(0.036)	(0.050)		
On Earnings Aged 29-30	0.446***	0.535***	0.548***		
	(0.047)	(0.056)	(0.070)		
On Earnings Aged 31-32	0.430***	0.483***	0.491***		
	(0.047)	(0.056)	(0.074)		
On Earnings Aged 29-30	0.377***	0.414***	0.436***		
	(0.047)	(0.045)	(0.062)		
On Earnings Aged 35-36	0.369***	0.423***	0.456***		
	(0.049)	(0.056)	(0.077)		
On Earnings Aged 37-38	0.368***	0.424***	0.466***		
	(0.050)	(0.058)	(0.073)		
On Earnings Aged 39-40	0.391***	0.458***	0.505***		
	(0.051)	(0.060)	(0.083)		
On Earnings Aged 41-42		0.435***	0.461***		
		(0.071)	(0.103)		
On Earnings Aged 43-44			0.482***		
			(0.080)		
Youth Experience (21-23)					
On Earnings Aged 25-26	0.012	−0.038	−0.013		
	(0.041)	(0.034)	(0.040)		
On Earnings Aged 27-28	−0.021	−0.079*	−0.065		
	(0.042)	(0.038)	(0.046)		
On Earnings Aged 29-30	0.063	−0.015	0.030		
	(0.049)	(0.070)	(0.082)		
On Earnings Aged 31-32	0.120*	0.047	0.079		
	(0.050)	(0.078)	(0.096)		
On Earnings Aged 29-30	0.116*	0.044	0.069		
	(0.050)	(0.085)	(0.103)		
On Earnings Aged 35-36	0.126*	0.018	0.073		
	(0.051)	(0.071)	(0.083)		
On Earnings Aged 37-38	0.147**	0.064	0.073		
	(0.052)	(0.055)	(0.073)		
On Earnings Aged 39-40	0.094	−0.001	0.048		
	(0.053)	(0.069)	(0.082)		
On Earnings Aged 41-42		0.005	0.071		
		(0.084)	(0.100)		
On Earnings Aged 43-44			0.071		
			(0.089)		
Early Adulthood Exper. (24-26)					
On Earnings Aged 29-30	0.090*	0.109*	0.106		
	(0.043)	(0.050)	(0.069)		
On Earnings Aged 31-32	0.028	0.110**	0.115*		
	(0.042)	(0.040)	(0.052)		
On Earnings Aged 29-30	0.036	0.111	0.180*		
	(0.045)	(0.066)	(0.082)		
On Earnings Aged 35-36	0.020	0.130*	0.153*		
	(0.045)	(0.052)	(0.067)		
On Earnings Aged 37-38	−0.063	0.054	0.096		
	(0.047)	(0.053)	(0.071)		
On Earnings Aged 39-40	−0.080	0.047	0.069		
	(0.047)	(0.062)	(0.074)		
On Earnings Aged 41-42		0.012	0.043		
		(0.066)	(0.069)		
On Earnings Aged 43-44			0.048		
			(0.058)		

Continued on next page

Table A2 – continued from previous page

	(1)	(2)	(3)	(4)	(5)
	Sample 18-40	Sample 18-42	Sample 18-44	Whole Youth	Entrant + early Adult
On Earnings Aged 29-30				0.113***	
				(0.012)	
On Earnings Aged 31-32				0.107***	
				(0.012)	
On Earnings Aged 33-34				0.089***	
				(0.013)	
On Earnings Aged 35-36				0.085***	
				(0.013)	
On Earnings Aged 37-38				0.067***	
				(0.014)	
On Earnings Aged 39-40				0.049***	
				(0.014)	
On Earnings Aged 25-26					0.144***
					(0.020)
On Earnings Aged 27-28					0.183***
					(0.021)
On Earnings Aged 29-30					0.227***
					(0.026)
On Earnings Aged 31-32					0.246***
					(0.025)
On Earnings Aged 33-34					0.213***
					(0.026)
On Earnings Aged 35-36					0.217***
					(0.027)
On Earnings Aged 37-38					0.224***
					(0.028)
On Earnings Aged 39-40					0.218***
					(0.028)
On Earnings Aged 29-30					0.080**
					(0.026)
On Earnings Aged 31-32					0.048
					(0.026)
On Earnings Aged 29-30					0.052
					(0.028)
On Earnings Aged 35-36					0.041
					(0.029)
On Earnings Aged 37-38					−0.002
					(0.030)
On Earnings Aged 39-40					−0.036
					(0.030)
Constant	10.033***	10.144***	10.280***	9.804***	9.937***
	(0.029)	(0.035)	(0.038)	(0.016)	(0.024)
N	279877	220218	146294	279877	279877
Individuals	13495	9782	5990	13495	13495

Note: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Columns (1)-(4) report estimated coefficients from (23) for different samples. Therefore Column (1) here is the same as Column (3) in Table 1, that is the regression run on a sample which contains seven cohorts. The next two columns are for smaller samples, containing respectively five and three cohorts only. Column (4) contains the estimated coefficients when the entire period from age 18 to age 26 is treated as homogeneous, that is, when (23) is estimated with the restrictions (25)-(26). These coefficients are depicted as the light green line in Figure 4 and in Figure A1. Finally, Column (5) reports the estimated coefficients when youth is split into two periods, with restrictions (27)-(29). The resulting coefficients are depicted as the aquamarine lines in Figure A1.

Table A3:
Results For Different Ability Quintiles

	(1) Bottom Quartile	(2) 2nd Quartile	(3) 3rd Quartile	(4) 4th Quartile	(5) Top Quartile
Current Unemployment	3.637***	2.498***	1.961***	1.340***	0.874***
	(0.118)	(0.102)	(0.095)	(0.090)	(0.067)
Entrant Experience (18-20)					
On Earnings Aged 23-24	0.306**	0.277**	0.125*	0.122*	0.058
	(0.114)	(0.094)	(0.062)	(0.060)	(0.059)
On Earnings Aged 25-26	0.427**	0.475***	0.286**	0.221**	0.147
	(0.133)	(0.119)	(0.089)	(0.072)	(0.076)
On Earnings Aged 27-28	0.556***	0.562***	0.299***	0.268***	0.141*
	(0.145)	(0.118)	(0.081)	(0.077)	(0.065)
On Earnings Aged 29-30	0.629***	0.569***	0.342***	0.293***	0.120
	(0.159)	(0.126)	(0.091)	(0.078)	(0.068)
On Earnings Aged 31-32	0.422*	0.572***	0.339***	0.327***	0.141*
	(0.164)	(0.125)	(0.097)	(0.081)	(0.067)
On Earnings Aged 29-30	0.398*	0.515***	0.203*	0.228**	0.128
	(0.174)	(0.129)	(0.091)	(0.073)	(0.071)
On Earnings Aged 35-36	0.328	0.597***	0.206*	0.287***	0.103
	(0.174)	(0.141)	(0.095)	(0.078)	(0.068)
On Earnings Aged 37-38	0.219	0.547***	0.324***	0.232**	0.157*
	(0.184)	(0.141)	(0.097)	(0.079)	(0.074)
On Earnings Aged 39-40	0.214	0.562***	0.318**	0.270***	0.154*
	(0.190)	(0.136)	(0.107)	(0.077)	(0.074)
Youth Experience (21-23)					
On Earnings Aged 25-26	−0.096	−0.168	0.061	0.019	0.042
	(0.122)	(0.101)	(0.089)	(0.069)	(0.070)
On Earnings Aged 27-28	−0.245	−0.158	0.039	−0.012	−0.014
	(0.126)	(0.111)	(0.086)	(0.075)	(0.064)
On Earnings Aged 29-30	−0.219	−0.046	0.138	0.040	0.060
	(0.171)	(0.113)	(0.099)	(0.080)	(0.065)
On Earnings Aged 31-32	0.068	−0.018	0.215*	−0.023	0.019
	(0.174)	(0.123)	(0.106)	(0.081)	(0.071)
On Earnings Aged 29-30	0.155	−0.055	0.262**	0.035	0.051
	(0.178)	(0.134)	(0.097)	(0.080)	(0.072)
On Earnings Aged 35-36	0.179	−0.132	0.280**	0.040	0.069
	(0.180)	(0.135)	(0.103)	(0.081)	(0.071)
On Earnings Aged 37-38	0.153	0.002	0.242*	0.030	0.075
	(0.191)	(0.133)	(0.104)	(0.089)	(0.075)
On Earnings Aged 39-40	0.202	−0.121	0.220*	0.045	0.002
	(0.195)	(0.132)	(0.110)	(0.090)	(0.077)
Early Adulthood Exper. (24-26)					
On Earnings Aged 29-30	0.152	0.008	0.090	0.106	−0.007
	(0.139)	(0.104)	(0.091)	(0.073)	(0.051)
On Earnings Aged 31-32	−0.122	−0.034	0.009	0.050	0.019
	(0.142)	(0.111)	(0.089)	(0.072)	(0.055)
On Earnings Aged 29-30	−0.174	−0.047	−0.023	0.127	0.061
	(0.154)	(0.112)	(0.096)	(0.078)	(0.056)
On Earnings Aged 35-36	−0.228	−0.048	0.020	0.014	0.044
	(0.147)	(0.120)	(0.104)	(0.079)	(0.055)
On Earnings Aged 37-38	−0.342*	−0.112	−0.106	0.060	−0.065
	(0.160)	(0.118)	(0.096)	(0.077)	(0.060)
On Earnings Aged 39-40	−0.573***	−0.109	−0.063	0.050	−0.020
	(0.162)	(0.116)	(0.106)	(0.074)	(0.059)
Constant	9.445***	9.946***	10.067***	10.083***	10.134***
	(0.094)	(0.075)	(0.055)	(0.048)	(0.048)
N	52011	51330	49596	48786	46043
Individuals	2738	2493	2436	2445	2447

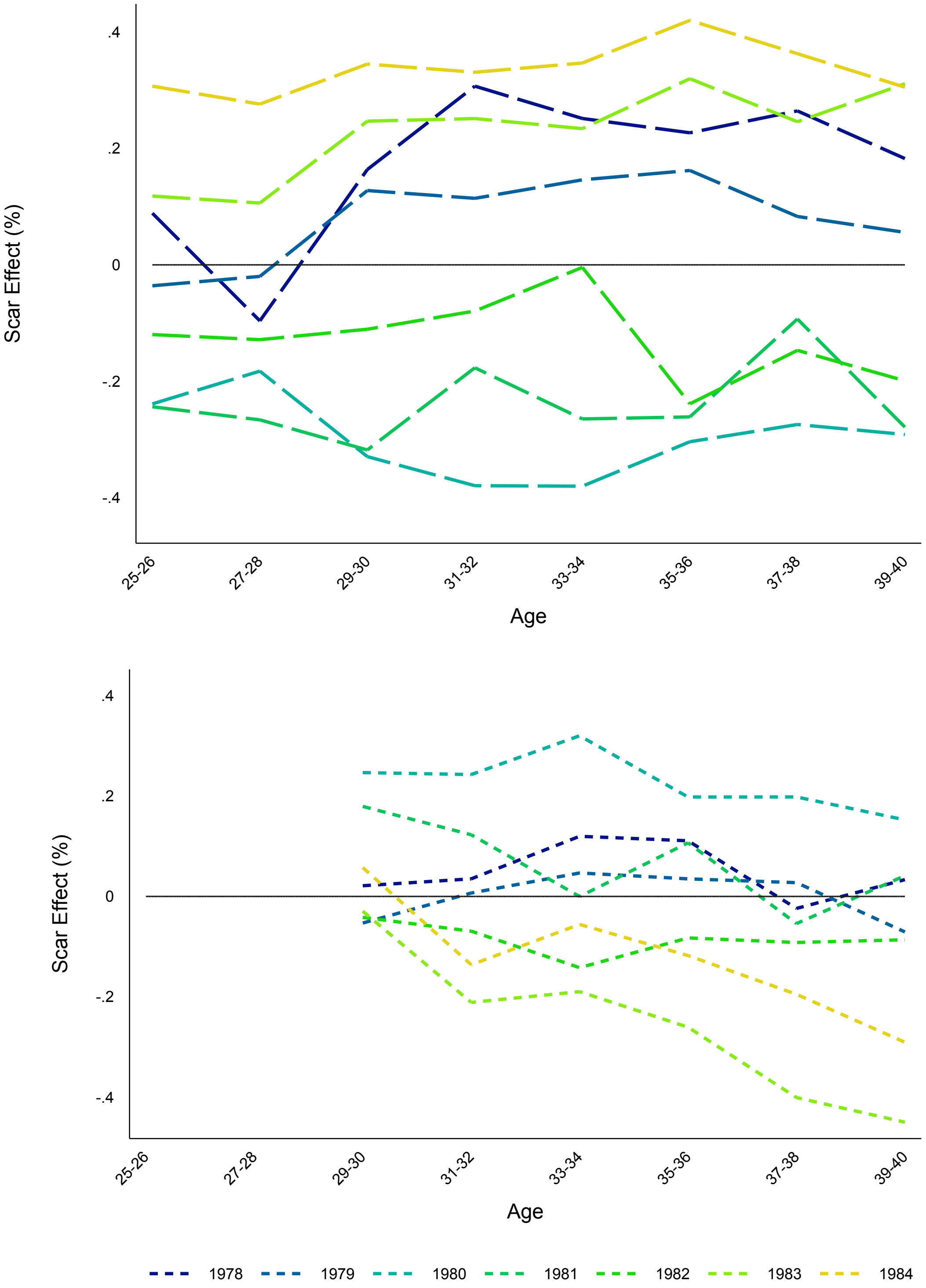
Note: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Coefficients for the estimation of (23), the same as the main regression, Column (3) in Table 1. These are estimated from each subsample of individuals in the same ability quintile, defined on the basis of their fixed effect in Column (3) of Table 1. The coefficients in the top block are plotted in Figure 5, those in the other two blocks, in Figure A3.

Table A4:
Results For Individual Cohorts

	(1) 1978	(2) 1979	(3) 1980	(4) 1981	(5) 1982	(6) 1983	(7) 1984
Weeks Employed in Year	2.080***	2.141***	2.444***	2.431***	2.541***	2.553***	2.615***
	(0.122)	(0.109)	(0.128)	(0.132)	(0.133)	(0.139)	(0.119)
Entrant Experience (18-20)							
On Earnings Aged 23-24	0.102	0.326***	0.276**	0.269**	0.331***	0.152	−0.133
	(0.075)	(0.081)	(0.087)	(0.082)	(0.087)	(0.125)	(0.163)
On Earnings Aged 25-26	0.316***	0.449***	0.398**	0.269**	0.579***	0.158	−0.307
	(0.095)	(0.117)	(0.121)	(0.103)	(0.118)	(0.153)	(0.186)
On Earnings Aged 27-28	0.391***	0.497***	0.545***	0.558***	0.523***	0.230	−0.310
	(0.103)	(0.102)	(0.118)	(0.120)	(0.135)	(0.149)	(0.186)
On Earnings Aged 29-30	0.432***	0.552***	0.623***	0.459***	0.678***	0.223	−0.330
	(0.094)	(0.104)	(0.137)	(0.121)	(0.122)	(0.162)	(0.189)
On Earnings Aged 31-32	0.348***	0.411***	0.666***	0.450***	0.621***	0.299*	−0.186
	(0.105)	(0.096)	(0.129)	(0.129)	(0.126)	(0.152)	(0.179)
On Earnings Aged 29-30	0.321**	0.390***	0.583***	0.395**	0.489***	0.264	−0.096
	(0.113)	(0.104)	(0.126)	(0.124)	(0.122)	(0.146)	(0.187)
On Earnings Aged 35-36	0.355**	0.383***	0.610***	0.382**	0.497***	0.200	−0.175
	(0.118)	(0.106)	(0.119)	(0.130)	(0.130)	(0.154)	(0.191)
On Earnings Aged 37-38	0.334**	0.482***	0.545***	0.346*	0.506***	0.212	−0.251
	(0.108)	(0.111)	(0.132)	(0.136)	(0.136)	(0.155)	(0.198)
On Earnings Aged 39-40	0.380***	0.491***	0.610***	0.425**	0.452**	0.193	−0.252
	(0.114)	(0.110)	(0.132)	(0.140)	(0.140)	(0.166)	(0.203)
Youth Experience (21-23)							
On Earnings Aged 25-26	0.094	−0.032	−0.188	−0.180	−0.103	0.108	0.358***
	(0.087)	(0.095)	(0.114)	(0.111)	(0.144)	(0.117)	(0.104)
On Earnings Aged 27-28	−0.089	−0.017	−0.102	−0.219	−0.073	0.094	0.300**
	(0.093)	(0.087)	(0.111)	(0.143)	(0.154)	(0.118)	(0.104)
On Earnings Aged 29-30	0.186	0.103	−0.287	−0.264	−0.124	0.232	0.405***
	(0.098)	(0.112)	(0.154)	(0.162)	(0.159)	(0.151)	(0.109)
On Earnings Aged 31-32	0.329**	0.105	−0.296*	−0.117	−0.101	0.254	0.387***
	(0.118)	(0.107)	(0.145)	(0.167)	(0.173)	(0.147)	(0.116)
On Earnings Aged 29-30	0.283*	0.139	−0.310*	−0.194	−0.058	0.234	0.391***
	(0.115)	(0.110)	(0.142)	(0.169)	(0.162)	(0.147)	(0.113)
On Earnings Aged 35-36	0.251*	0.149	−0.262	−0.219	−0.200	0.304*	0.495***
	(0.116)	(0.110)	(0.145)	(0.165)	(0.165)	(0.138)	(0.116)
On Earnings Aged 37-38	0.287*	0.081	−0.193	−0.043	−0.083	0.238	0.420***
	(0.115)	(0.116)	(0.156)	(0.179)	(0.169)	(0.146)	(0.113)
On Earnings Aged 39-40	0.216	0.044	−0.251	−0.248	−0.148	0.300*	0.368**
	(0.116)	(0.114)	(0.162)	(0.177)	(0.175)	(0.147)	(0.123)
Early Adulthood Exper. (24-26)							
On Earnings Aged 29-30	0.029	0.013	0.305**	0.168	0.036	−0.006	0.040
	(0.105)	(0.126)	(0.115)	(0.089)	(0.109)	(0.136)	(0.113)
On Earnings Aged 31-32	0.042	0.063	0.277**	0.113	−0.006	−0.224	−0.155
	(0.124)	(0.103)	(0.104)	(0.086)	(0.122)	(0.135)	(0.116)
On Earnings Aged 29-30	0.099	0.099	0.364***	−0.011	−0.048	−0.225	−0.069
	(0.122)	(0.119)	(0.107)	(0.102)	(0.132)	(0.139)	(0.125)
On Earnings Aged 35-36	0.110	0.082	0.264*	0.115	−0.024	−0.291*	−0.165
	(0.137)	(0.111)	(0.106)	(0.099)	(0.122)	(0.135)	(0.131)
On Earnings Aged 37-38	−0.041	0.072	0.234*	−0.040	−0.060	−0.441***	−0.266*
	(0.130)	(0.124)	(0.119)	(0.109)	(0.128)	(0.132)	(0.128)
On Earnings Aged 39-40	0.014	−0.015	0.208	0.051	−0.087	−0.500***	−0.309*
	(0.132)	(0.123)	(0.114)	(0.104)	(0.140)	(0.135)	(0.134)
Constant	10.239***	10.232***	10.127***	9.915***	9.929***	9.790***	9.688***
	(0.065)	(0.071)	(0.077)	(0.079)	(0.084)	(0.099)	(0.097)
N	41584	42578	42196	40630	37399	37316	38174

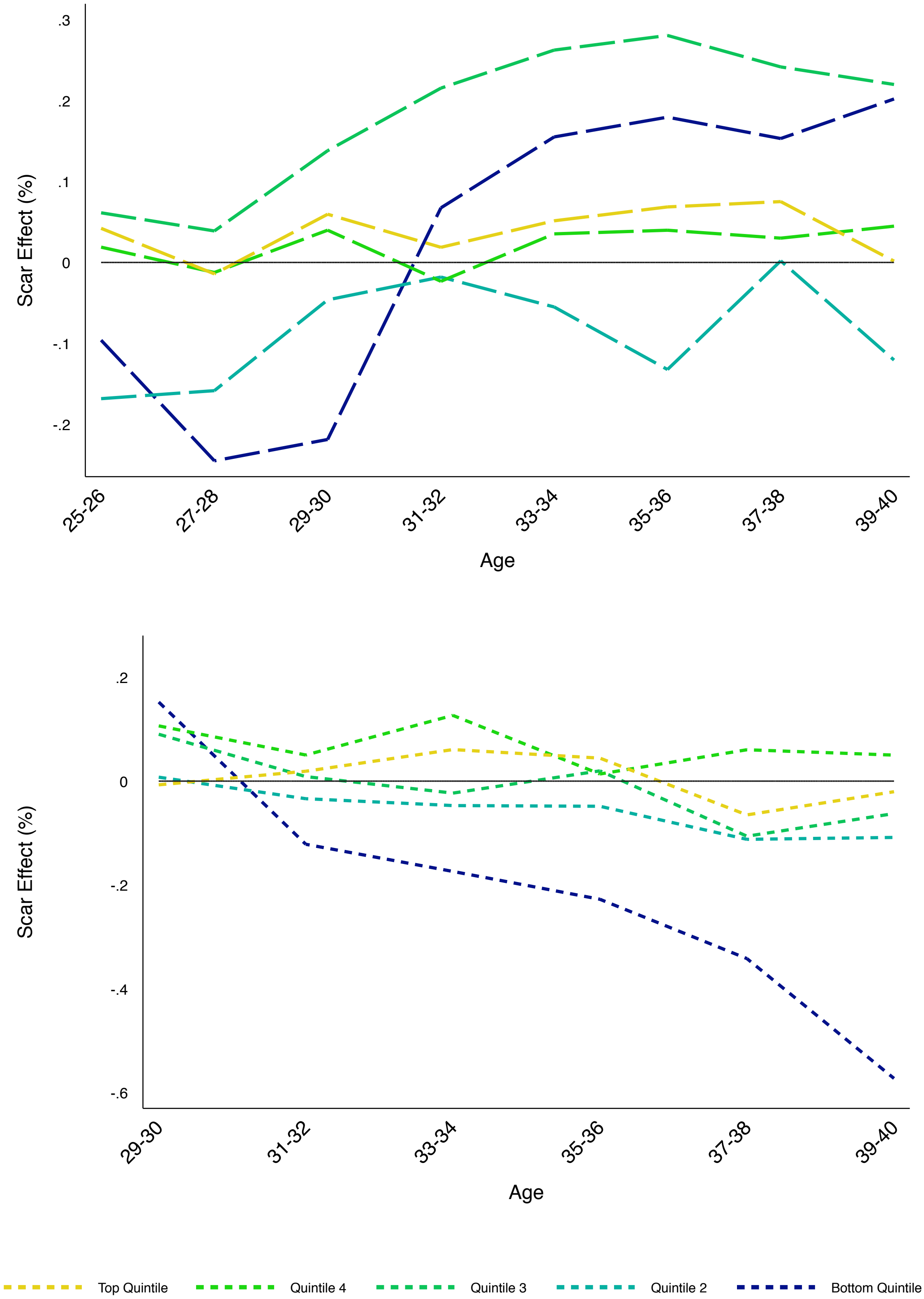
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Coefficients for the estimation of (23), the same as in the main regression, Column (3) in Table 1, estimated from each subsample of individuals according to the year of entry in the labour market. The coefficients in the top block are plotted in Figure 7, those in the other two blocks in Figure A2.

Figure A2:
Scar Effects of Youth and early Adult Unemployment for Different Cohorts



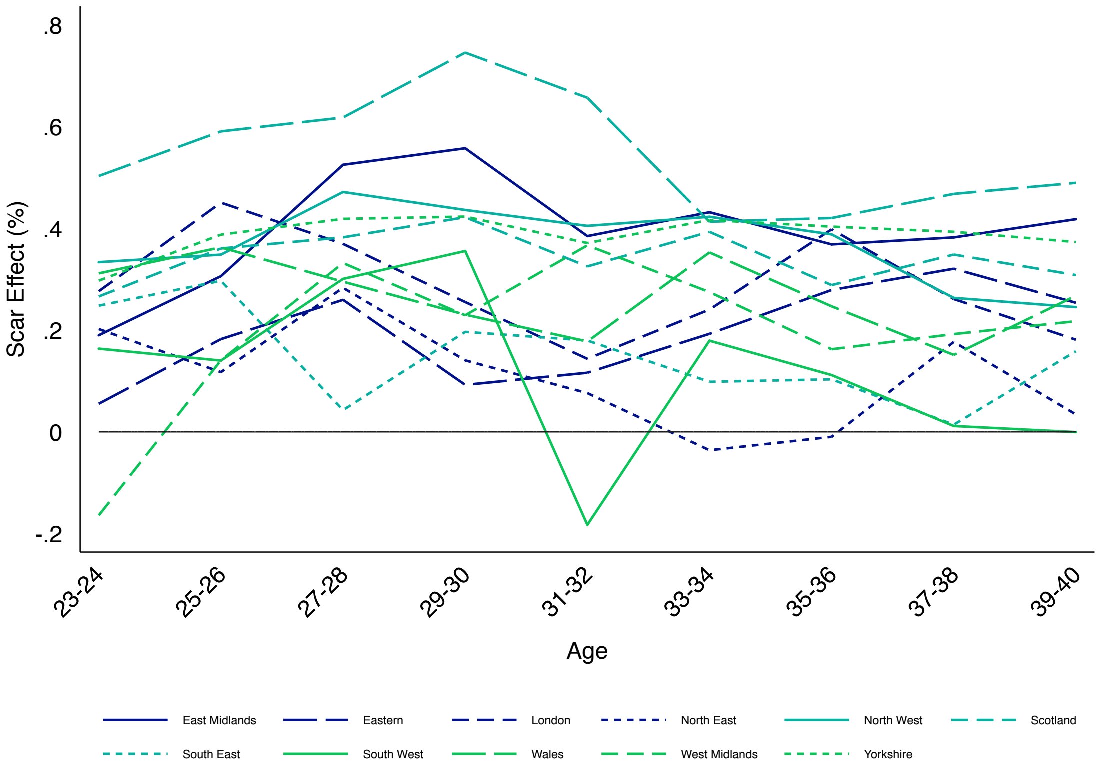
Note: The lines report the estimated coefficients from (23) for the effect of Youth, and early Adulthood unemployment corresponding to those in Figure 7. That is the coefficients β^t_Y and β^t_A, calculated from each subsample of individuals in the same cohort in the sample of the main regression, Column (3) in Table 1.

Figure A3:
Scar Effects of Youth and early Adult Unemployment on
Individuals of Different Abilities



Note: The lines report the estimated coefficients from (23) 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 for the subsample of individuals of each ability quintile, membership of which is determined by individuals' predicted earnings.

Figure A4:
Scar Effects of Entry Unemployment in Different Regions



Note: The lines report the estimated coefficients from (23) for the effect of Entry unemployment corresponding to those in Figure 5. That is, the coefficients β_E^t are calculated for each subsample of individuals in the same region at the time of entry in the labour market.