Economica LSE

Economica (2021) **88**, 896–941
doi:10.1111/ecca.12384

The Wounds That Do Not Heal: The Lifetime Scar of Youth Unemployment

By GIANNI DE FRAJA*, SARA LEMOS† and JAMES ROCKEY‡

**University of Nottingham, University of Rome Tor Vergata, and CEPR*
†University of Leicester *‡University of Birmingham*

Final version received 27 May 2021.

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.

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 the analysis of Ruhm (1991) of the long-term effects of *past* displacement in the USA, confirmed by subsequent research, as summarized in Couch and Placzek (2010). A parallel literature has concentrated on the long-term effects of *youth* unemployment (see, for example, Lynch (1989) for the USA, Lynch (1985), Nickell *et al.* (2002) and 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.

© 2021 The London School of Economics and Political Science. Published by Blackwell Publishing, 9600 Garsington Road, Oxford OX4 2DQ, UK and 350 Main St, Malden, MA 02148, USA.

The aim of this paper is therefore to separate the effects of labour market shocks that 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 18–20 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 subperiods 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 that 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 the effects of the three separate subperiods. Averaging the effects across youth therefore misses the sharp difference between subperiods within youth: in later youth (ages 24–26), 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 onwards.[2]

The difference in the impact of the subperiods of youth, theoretical interest aside, has obvious implications for the design of policies aimed at lessening the long-term effects of youth unemployment, such as the European Youth Guarantee (ILO 2012). Our analysis suggests that 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 that we uncover is that experience affects differently individuals of different abilities (see Figure 5 below). Importantly, the severity of the scar effect increases as we move down the ability scale. Note that we exclude

© 2021 The London School of Economics and Political Science

14680335, 2021, 352, Downloaded from https://onlinelibrary.wiley.com/doi/10.1111/ecca.12384 by <Shibboleth>-staff@bham.ac.uk, Wiley Online Library on [09/08/2023]. See the Terms and Conditions (https://onlinelibrary.wiley.com/terms-and-conditions) on Wiley Online Library for rules of use; OA articles are governed by the applicable Creative Commons License

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 to be similar, but stronger in the early years (compare columns (3) and (4) of Table 1 in Section V). In comparison to men, the effect appears to be reversed between ages 24 and 26. This might be because other intervening variables are at play, information about which is not available in our dataset, 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, among others, Blundell *et al.* 2013). 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 anonymized 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 that is based on a binary variable measuring 'displacement' (see, for example, Jacobson *et al.* (1993, 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 that 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 the seminal contribution of Moretti (2004), we use detailed geographical information to control for endogenous geographical mobility. We separate the effect of unemployment shocks

© 2021 The London School of Economics and Political Science

14680335, 2021, 352, Downloaded from https://onlinelibrary.wiley.com/doi/10.1111/ecca.12384 by <Shibboleth>-staff@bham.ac.uk, Wiley Online Library on [09/08/2023]. See the Terms and Conditions (https://onlinelibrary.wiley.com/terms-and-conditions) on Wiley Online Library for rules of use; OA articles are governed by the applicable Creative Commons License

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 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 organized as follows. Section I summarizes the established theoretical background on the long-term effects of unemployment, and motivates the econometric specification discussed in Section II. Our identification strategy is illustrated in Section III, and Section IV presents the data. Our main results and some robustness tests are presented in Section V, and Section VI concludes in the light of the existing literature. An Appendix reports further results.

I. 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

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

© 2021 The London School of Economics and Political Science

14680335, 2021, 352, Downloaded from https://onlinelibrary.wiley.com/doi/10.1111/ecca.12384 by <Shibboleth>-staff@bham.ac.uk, Wiley Online Library on [09/08/2023]. See the Terms and Conditions (https://onlinelibrary.wiley.com/terms-and-conditions) on Wiley Online Library for rules of use; OA articles are governed by the applicable Creative Commons License

where w_i^t are the earnings of person i 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 the 'experience' of person i at time t; in general, we consider experience to be something that exerts a non-negative effect on earnings, so we posit $\partial f/\partial e_i^t \geq 0$. Finally, ε_i^t is a random shock affecting earnings.

Early theoretical models, such as that of Ben-Porath (1967), capture experience $\boldsymbol{e}_i^t$ as a single figure, typically as the total number of years that 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, then 'potential experience', given by the number of years not spent in formal education, is often used as a proxy, as in landmark studies by Mincer (1958, 1974), among others.

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. First, it is possible that recent occurrences may matter more than distant ones: negative events fade in importance, and conversely, skills acquired in the distant past become less relevant. Second, timing may matter because some periods in life, for example the years immediately after entry into the labour market, are more important than others.

To formalize these ideas, we replace $\boldsymbol{e}_i^t$ in equation (1) with a vector $(e_i^t, e_i^{t-1}, \ldots, e_i^2, e_i^1)$ that measures the experience in each of the years since the time of potential entry into the labour market, year 1. By convention, events that 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 $e_i^2 = e^2(e_i^1, \varepsilon_i^2)$, $e_i^3 = e^3(e_i^2, e_i^1, \varepsilon_i^3)$, 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 the experience of individual i. Thus equation (1) is replaced by

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

where the experience of individual i in period t is itself a function of previous experience:

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

In equation (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 equation (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 equation (3) as

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

© 2021 The London School of Economics and Political Science

14680335, 2021, 352, Downloaded from https://onlinelibrary.wiley.com/doi/10.1111/ecca.12384 by <Shibboleth>-staff@bham.ac.uk, Wiley Online Library on [09/08/2023]. See the Terms and Conditions (https://onlinelibrary.wiley.com/terms-and-conditions) on Wiley Online Library for rules of use; OA articles are governed by the applicable Creative Commons License

Substituting the above in equation (2), we have

$$w_i^t = f^t\big(Z_i,\, \lambda^t,\, e^t\big(e^{t-1}(\cdot),\, e^{t-2}(\cdot),\, \ldots,\, e^2(\cdot),\, e^1(u_i^1)\big),\, \ldots,\, e_i^2\big(e^1(u_i^1),\, u_i^2\big),\, e^1(u_i^1),\, \varepsilon_i^t\big). \tag{4}$$

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 separate conceptually a direct and an indirect effect. Taking the case $t=3$ as an illustrative example, we can write

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

So to determine the effect of a shock $\mathrm{d}u_i^1$ in period 1 on earnings in period 3, we would simply take

$$\mathrm{d}w_i^t = \frac{\mathrm{d}f^3}{\mathrm{d}e_i^1}\,\mathrm{d}u_i^1, \tag{6}$$

where the total effect $\mathrm{d}f^3/\mathrm{d}e_i^1$ on the right-hand side of equation (5) is decomposed into the direct effect of period 1 experience, $\partial f^3/\partial e_i^1$, and the indirect effect of experience in previous periods, given by its direct effect on period 2 experience, $\partial e^2/\partial e_i^1$, multiplied by the direct effect of period 2 experience on period 3 earnings, $\partial f^3/\partial e_i^2$. Similarly for the effect through the experience in period 3, given by the last term in equation (5). In the final subsection of Section V, 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 that occurred at time $t-\tau$ positively influence earnings at time t.

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

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

for some t^* and s^*, with $t^* > s^*$, and for a suitably small value of δ. According to inequality (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

© 2021 The London School of Economics and Political Science

14680335, 2021, 352, Downloaded from https://onlinelibrary.wiley.com/doi/10.1111/ecca.12384 by <Shibboleth>-staff@bham.ac.uk, Wiley Online Library on [09/08/2023]. See the Terms and Conditions (https://onlinelibrary.wiley.com/terms-and-conditions) on Wiley Online Library for rules of use; OA articles are governed by the applicable Creative Commons License

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 inequality (7) is replaced by the hypothesis that for some s^* and t^*, with $t^* > s^*$, and $M > 0$,

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

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 exist 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 $t \geq \tau_R$,

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

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

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

II. Empirical Specification

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

$$\log w_i^t = Z_i^t \alpha^t + \underbrace{\gamma^t e_i^t}_{\substack{\text{current} \\ \text{employment}}} + \underbrace{\beta_{t-1}^t e_i^{t-1} + \beta_{t-2}^t e_i^{t-2} + \ldots + \beta_{s+1}^t e_i^{s+1} + \beta_s^t e_i^s}_{\substack{\text{effects of past labour} \\ \text{market experience}}}, \tag{11}$$

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

© 2021 The London School of Economics and Political Science

14680335, 2021, 352, Downloaded from https://onlinelibrary.wiley.com/doi/10.1111/ecca.12384 by <Shibboleth>-staff@bham.ac.uk, Wiley Online Library on [09/08/2023]. See the Terms and Conditions (https://onlinelibrary.wiley.com/terms-and-conditions) on Wiley Online Library for rules of use; OA articles are governed by the applicable Creative Commons License

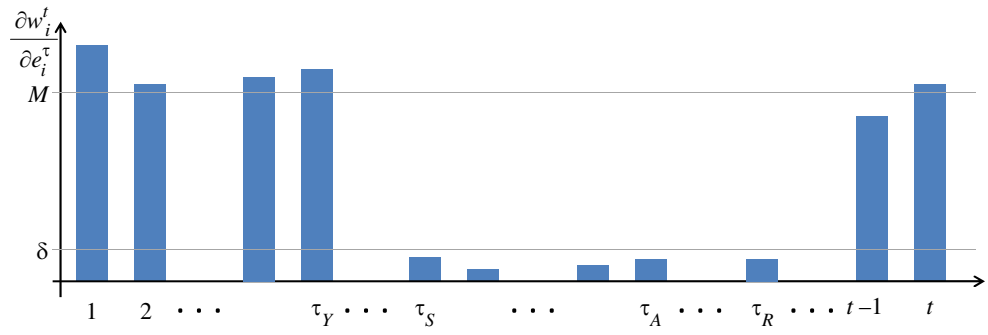


FIGURE 1. The partial derivatives of earnings at time t implied by Hypothesis 1.

where, as in equations (1) and (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 the labour market experience of individual i 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 USA, and Hijzen *et al.* (2010) for the UK—the focus has been on job losses due to 'displacement events' observed.[7] Our data are richer and more nuanced, allowing us to measure experience as the number of 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 rewrite (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:

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

© 2021 The London School of Economics and Political Science

14680335, 2021, 352, Downloaded from https://onlinelibrary.wiley.com/doi/10.1111/ecca.12384 by <Shibboleth>-staff@bham.ac.uk, Wiley Online Library on [09/08/2023]. See the Terms and Conditions (https://onlinelibrary.wiley.com/terms-and-conditions) on Wiley Online Library for rules of use; OA articles are governed by the applicable Creative Commons License

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 22×22 diagonal matrices, with $(\alpha_i^{40}, \ldots, \alpha_i^{19})$ and $(\gamma^{40}, \ldots, \gamma^{19})$ along the respective diagonals, and $\boldsymbol{\beta}$ is the 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

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

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 age 26 into three three-year intervals. *Entry* into the labour market, subscripted by the letter *E*, ages 18–20 inclusive; *Youth*, subscripted by *Y*, ages 21–23; and *early Adulthood*, subscripted by *A*, ages 24—26. We begin by imposing

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

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

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) and (17) are reported in column (1) of Table 1 (in Section V). 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, then 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.

© 2021 The London School of Economics and Political Science

14680335, 2021, 352, Downloaded from https://onlinelibrary.wiley.com/doi/10.1111/ecca.12384 by <Shibboleth>-staff@bham.ac.uk, Wiley Online Library on [09/08/2023]. See the Terms and Conditions (https://onlinelibrary.wiley.com/terms-and-conditions) on Wiley Online Library for rules of use; OA articles are governed by the applicable Creative Commons License

To decompose the direct effect from the indirect effect of being unemployed when young—that is, to evaluate the relative magnitude of the two terms on the right-hand side of equation (5)—we modify (17) to include experience gained in later periods of life as explanatory variables. We do so in two stages. First, 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

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

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

The results obtained with restrictions (16), (18) and (19) are reported in column (2) of Table 1. Finally, we add a third possible set of effects, that of experience in 'early adulthood', replacing (19) with

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

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

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, …, 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,…,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, …, 39.

III. 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 recognize a

© 2021 The London School of Economics and Political Science

14680335, 2021, 352, Downloaded from https://onlinelibrary.wiley.com/doi/10.1111/ecca.12384 by <Shibboleth>-staff@bham.ac.uk, Wiley Online Library on [09/08/2023]. See the Terms and Conditions (https://onlinelibrary.wiley.com/terms-and-conditions) on Wiley Online Library for rules of use; OA articles are governed by the applicable Creative Commons License

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. 349) 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 that we include in our specification (see equation (22) below) 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) of Table 1 (see Section V), 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 heterogeneous 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 valued, which makes location decisions endogenous. This potential problem is analogous to that convincingly addressed by the analysis of externalities in higher education by Moretti (2004). 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, 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 and Hendren (2018a,b), 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 being equal, the effects of labour market conditions in an area are 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 down this channel by controlling directly for any persistent effects of the particular labour market conditions that an individual encountered on entry, and of any variations over time in the labour market conditions faced by that individual's cohort.

© 2021 The London School of Economics and Political Science

14680335, 2021, 352, Downloaded from https://onlinelibrary.wiley.com/doi/10.1111/ecca.12384 by <Shibboleth>-staff@bham.ac.uk, Wiley Online Library on [09/08/2023]. See the Terms and Conditions (https://onlinelibrary.wiley.com/terms-and-conditions) on Wiley Online Library for rules of use; OA articles are governed by the applicable Creative Commons License

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) of Table 2 (in Section V), we also report clustering standard errors at the individual level. The estimates are slightly more precise but the inference is unaffected.

To summarize, with the restrictions on the β terms given in (16), (18), (20) and (21), and the assumptions on the fixed effects in (22), the regression specification (11) becomes

$$\begin{aligned}\log w_{iac}^t = {} & X_i^t\alpha^t + \gamma^t e_i^t + \underbrace{\beta_E^t \sum_{s=18}^{20} e_i^s}_{\substack{\text{scar effects of}\\ \text{unemployment}\\ \text{for Entrants}}} + \underbrace{\beta_Y^t \sum_{s=21}^{23} e_i^s}_{\substack{\text{scar effects of}\\ \text{unemployment}\\ \text{for Youths}}} + \underbrace{\beta_A^t \sum_{s=24}^{26} e_i^s}_{\substack{\text{scar effects of}\\ \text{unemployment}\\ \text{for early Adults}}} \\ & + \theta_i\mu_a + \eta_c^t + \mu_a^0 + \varepsilon_i^t, \quad t = 23{-}24,\ 25{-}26,\ \ldots,\ 39{-}40.\end{aligned} \tag{23}$$

As explained, the coefficients β_E^t, β_Y^t and β_A^t measure the effects of experience, and hence the scars of unemployment, for 'Entrant' (ages 18–20), 'Youth' (ages 21–23) and 'early Adult' (ages 24–26). The last four terms specify the error term, as described in detail in (22). Our identification assumption is

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

IV. 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 gender, and 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. First, as we have a precise 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. Second, 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, 2005) and Couch and Placzek (2010), which observe one cohort of individuals only.

© 2021 The London School of Economics and Political Science

14680335, 2021, 352, Downloaded from https://onlinelibrary.wiley.com/doi/10.1111/ecca.12384 by <Shibboleth>-staff@bham.ac.uk, Wiley Online Library on [09/08/2023]. See the Terms and Conditions (https://onlinelibrary.wiley.com/terms-and-conditions) on Wiley Online Library for rules of use; OA articles are governed by the applicable Creative Commons License

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 that they accumulate while at university—would, if anything, bias our estimate of the β coefficients *downwards*: they are not employed in their youth, but earn on average more than their peers when adults. They might, however, differ for other reasons, and given that during the period that 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 register, 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 per 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 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 employed or registered as unemployed only 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 (in Section V), 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. First, income from self-employment may not be recorded accurately. Second, the self-employed might have more opportunity to understate their employment and earnings, in order to reduce their tax liability. Third, 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 being self-employed in some years, still excluding the years in which they reported being 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 are constructed using tax records, and as such, are 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 contributions on their behalf. These payments determine the right to a state pension and other welfare payments, and can be regarded as accurate; for this

© 2021 The London School of Economics and Political Science

14680335, 2021, 352, Downloaded from https://onlinelibrary.wiley.com/doi/10.1111/ecca.12384 by <Shibboleth>-staff@bham.ac.uk, Wiley Online Library on [09/08/2023]. See the Terms and Conditions (https://onlinelibrary.wiley.com/terms-and-conditions) on Wiley Online Library for rules of use; OA articles are governed by the applicable Creative Commons License

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 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 contributions—this person is in fact not active in the labour market, and in particular is not employed, and so not gaining experience. This natural imputation ensures consistency between the 'unemployment' and '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 on the year considered. Figure 2 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 3 and in Table A1 of the Appendix. Figure 3 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 shades of the various lines move from dark to lighter 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 equation (23) includes an indicator variable that takes the value 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



FIGURE 2. Number of weeks in employment or unemployment (density). *Notes*: 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.

© 2021 The London School of Economics and Political Science

14680335, 2021, 352, Downloaded from https://onlinelibrary.wiley.com/doi/10.1111/ecca.12384 by <Shibboleth>-staff@bham.ac.uk, Wiley Online Library on [09/08/2023]. See the Terms and Conditions (https://onlinelibrary.wiley.com/terms-and-conditions) on Wiley Online Library for rules of use; OA articles are governed by the applicable Creative Commons License

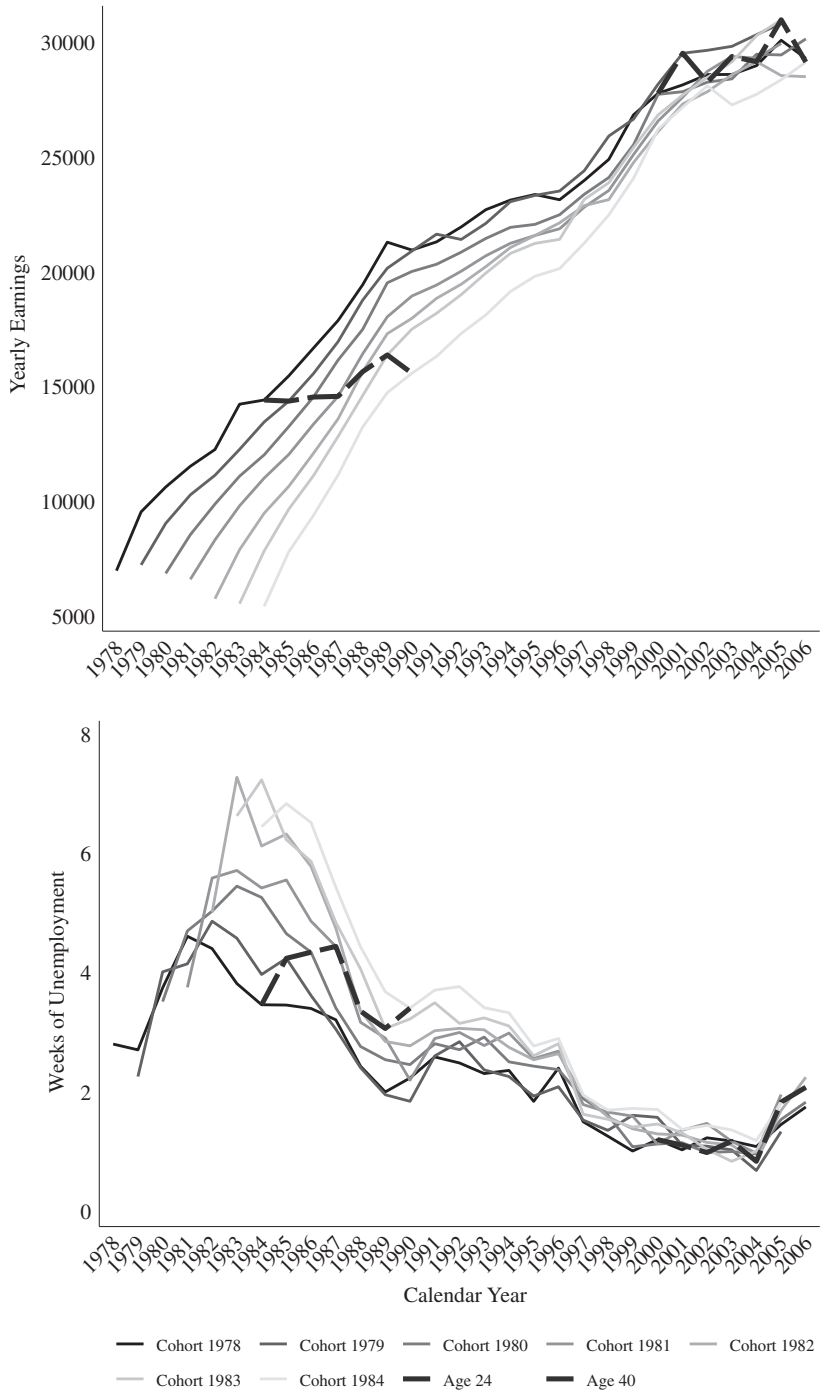


FIGURE 3. Yearly earnings and weeks of unemployment per year by cohort—men. *Notes*: Average year earnings, measured in 2004 pounds (top panel), and average number of weeks unemployed per year (bottom panel), for men born in each of the years 1978 and 1984 (dark lines for individuals born earlier), in the calendar year measured along the horizontal axis. The thick dashed lines highlight outcomes for different cohorts at ages 24 and 40.

© 2021 The London School of Economics and Political Science

14680335, 2021, 352, Downloaded from https://onlinelibrary.wiley.com/doi/10.1111/ecca.12384 by <Shibboleth>-staff@bham.ac.uk, Wiley Online Library on [09/08/2023]. See the Terms and Conditions (https://onlinelibrary.wiley.com/terms-and-conditions) on Wiley Online Library for rules of use; OA articles are governed by the applicable Creative Commons License

unemployed: the first is 1 in year t if individual i is unemployed for all 52 weeks of year t; the second is 1 if an individual is unemployed for two whole years, that is, for all 104 weeks of consecutive years $t-1$ and 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 (e.g. to health) rather than the negative impact of youth unemployment on long-run wages.[16]

The last terms in equation (23) are fixed effects interacted with one another. As explained in equation (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 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 specialized, 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 $a^0 \in \{1, \ldots, 409\}$. The location of individual i 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 midpoint between two 'naive' 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 Section V). In the second 'naive' 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) of Table 1 (also in Section V) and column (3) of Table 2 suggest that our results are not sensitive to alternative treatments of missing addresses.

V. RESULTS

Baseline specification

The results from our preferred specification are reported in Table 1. As discussed above, the regression in equation (23) is estimated for the subset of individuals who are in the sample from age 18 to age 40, pooled across cohorts. All coefficients are normalized to 100; they are therefore the effects, in percentage terms, of an additional week of experience in the relevant periods 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.

© 2021 The London School of Economics and Political Science

14680335, 2021, 352, Downloaded from https://onlinelibrary.wiley.com/doi/10.1111/ecca.12384 by <Shibboleth>-staff@bham.ac.uk, Wiley Online Library on [09/08/2023]. See the Terms and Conditions (https://onlinelibrary.wiley.com/terms-and-conditions) on Wiley Online Library for rules of use; OA articles are governed by the applicable Creative Commons License

TABLE 1
LONG-TERM EFFECTS OF EARLY LABOUR MARKET EXPERIENCE

	Entrant (1)	Entrant & Youth (2)	Whole period (3)	Women (4)	IV (5)
Weeks employed in year	2.355***	2.367***	2.365***	1.971***	2.232***
	(0.047)	(0.048)	(0.048)	(0.033)	(0.057)
Entrant experience (18–20)					
On earnings aged 23–24	0.224***	0.226***	0.226***	0.478***	0.237***
	(0.035)	(0.035)	(0.035)	(0.036)	(0.038)
On earnings aged 25–26	0.328***	0.323***	0.323***	0.550***	0.314***
	(0.037)	(0.045)	(0.045)	(0.045)	(0.049)
On earnings aged 27–28	0.410***	0.423***	0.423***	0.585***	0.349***
	(0.037)	(0.046)	(0.046)	(0.046)	(0.050)
On earnings aged 29–30	0.508***	0.451***	0.446***	0.567***	0.354***
	(0.040)	(0.047)	(0.047)	(0.049)	(0.052)
On earnings aged 31–32	0.501***	0.431***	0.430***	0.541***	0.276***
	(0.039)	(0.047)	(0.047)	(0.053)	(0.052)
On earnings aged 33–34	0.449***	0.379***	0.377***	0.539***	0.303***
	(0.040)	(0.047)	(0.047)	(0.054)	(0.052)
On earnings aged 35–36	0.441***	0.368***	0.369***	0.460***	0.290***
	(0.041)	(0.049)	(0.049)	(0.057)	(0.053)
On earnings aged 37–38	0.425***	0.364***	0.368***	0.436***	0.277***
	(0.043)	(0.050)	(0.050)	(0.057)	(0.054)
On earnings aged 39–40	0.412***	0.385***	0.391***	0.414***	0.269***
	(0.042)	(0.051)	(0.051)	(0.056)	(0.056)
Youth experience (21–23)					
On earnings aged 25–26		0.013	0.012	0.044	−0.024
		(0.041)	(0.041)	(0.043)	(0.042)
On earnings aged 27–28		−0.021	−0.021	0.064	0.046
		(0.042)	(0.042)	(0.047)	(0.046)
On earnings aged 29–30		0.109*	0.063	0.187***	0.073
		(0.044)	(0.049)	(0.048)	(0.054)
On earnings aged 31–32		0.135**	0.120*	0.162**	0.180***
		(0.045)	(0.050)	(0.052)	(0.054)
On earnings aged 33–34		0.134**	0.116*	0.126*	0.152**
		(0.045)	(0.050)	(0.054)	(0.054)
On earnings aged 35–36		0.137**	0.126*	0.157**	0.105*
		(0.047)	(0.051)	(0.056)	(0.054)
On earnings aged 37–38		0.117*	0.147**	0.189**	0.107
		(0.047)	(0.052)	(0.058)	(0.056)
On earnings aged 39–40		0.056	0.094	0.209***	0.070
		(0.048)	(0.053)	(0.058)	(0.057)
Early Adulthood experience (24–26)					
On earnings aged 29–30			0.090*	−0.164***	0.093
			(0.043)	(0.038)	(0.047)
On earnings aged 31–32			0.028	−0.171***	0.100*
			(0.042)	(0.042)	(0.049)
On earnings aged 33–34			0.036	−0.185***	0.139**
			(0.045)	(0.043)	(0.050)
On earnings aged 35–36			0.020	−0.235***	0.139**
			(0.045)	(0.045)	(0.050)

© 2021 The London School of Economics and Political Science

14680335, 2021, 352, Downloaded from https://onlinelibrary.wiley.com/doi/10.1111/ecca.12384 by <Shibboleth>-staff@bham.ac.uk, Wiley Online Library on [09/08/2023]. See the Terms and Conditions (https://onlinelibrary.wiley.com/terms-and-conditions) on Wiley Online Library for rules of use; OA articles are governed by the applicable Creative Commons License

TABLE 1
CONTINUED

	Entrant (1)	Entrant & Youth (2)	Whole period (3)	Women (4)	IV (5)
On earnings aged 37–38			−0.063	−0.282***	0.071
			(0.047)	(0.045)	(0.052)
On earnings aged 39–40			−0.080	−0.299***	0.033
			(0.047)	(0.046)	(0.054)
N	279,877	279,877	279,877	265,292	303,258
Individuals	13,495	13,495	13,495	12,778	13,684

Notes
*,**, *** indicate $p < 0.05$, $p < 0.01$, $p < 0.001$, respectively.
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 equation (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 equation (22).

The first coefficient in each column of Table 1 shows the effect of the number of weeks 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 Table 1 presents the long-term effect of experience on current earnings. In column (1), we report, for men, the effect of 'experience as Entrant' only. That is, we impose restrictions (16) and (17), namely $\beta^t_Y = \beta^t_A = 0$, in equation (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 'Youth experience' block report, in column (2), coefficients measuring the effect of an additional week not in work between ages 21 and 23: (18) and (19) replace (17), that is, β^t_Y is unrestricted in equation (23). Column (3) reports our benchmark specification, adding the period of 'early Adulthood', the ages between 24 and 26; in this column, the restrictions on the coefficient β terms 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, 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 ages 39–40 earnings by 0.39%.

There is a strong direct effect of experience at entry (ages 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 (see Figure 6 below). Recall that columns (2) and (3) of Table 1 add experience first between ages 21 and 23,

© 2021 The London School of Economics and Political Science

14680335, 2021, 352, Downloaded from https://onlinelibrary.wiley.com/doi/10.1111/ecca.12384 by <Shibboleth>-staff@bham.ac.uk, Wiley Online Library on [09/08/2023]. See the Terms and Conditions (https://onlinelibrary.wiley.com/terms-and-conditions) on Wiley Online Library for rules of use; OA articles are governed by the applicable Creative Commons License

and then between ages 24 and 26. The similarity of the coefficients in the first block in columns (1)–(3) 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 suggests that experience in Youth and early Adulthood does not have a long-term effect on earnings. We interpret this as strong evidence 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 the final subsection of this section.

Column (4) of Table 1 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 are larger than the effects for men, though as individuals become older, the coefficients become similar. Also larger are the effects of negative shocks in Youth (ages 21–23). Conversely, and counterintuitively, experience in early Adulthood (ages 24–26) 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 twenties (and thus accumulate more experience) are less likely in to have children in their twenties and are more likely to have them instead in their thirties, hence the negative correlation between early experience and labour market outcomes in their thirties. We are unable to verify that this is the case in our data, but this positive correlation is consistent with 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-thirties'.

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 in findings of a weaker persistence for women by Gregg (2001), 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 that 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), the experience of worker i 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 rewrite equations (23) and (24), replacing $e_i^t, \ldots, e_i^{18}$ with $\widehat{e}_i^{\,t}, \ldots, \widehat{e}_i^{\,18}$, where $\widehat{e}_i^{\,t}$ are the values of e_i^t instrumented with $\bar{e}_a^t$ the average unemployment in local market a at time t: $\widehat{e}_i^{\,r} = f(\bar{e}_a^{18}, \bar{e}_a^{19}, \ldots, \bar{e}_a^r)$, 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 that we consider, on the yearly earnings at the age window marked on the horizontal axis. The three age brackets that we consider are

© 2021 The London School of Economics and Political Science

14680335, 2021, 352, Downloaded from https://onlinelibrary.wiley.com/doi/10.1111/ecca.12384 by <Shibboleth>-staff@bham.ac.uk, Wiley Online Library on [09/08/2023]. See the Terms and Conditions (https://onlinelibrary.wiley.com/terms-and-conditions) on Wiley Online Library for rules of use; OA articles are governed by the applicable Creative Commons License

18–20 (β^t_E, shown as the solid line), 21–23 (β^t_Y, shown as the dashed line), and 24–26 (β^t_A, the dotted line). The thin dashed lines indicate the 95% confidence intervals around the estimated coefficients. The left-hand graph 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 (ages 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 while 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 the cross-sectional IV estimates of Gregg and Tominey (2005) for the UK, which are between 13% and 21%.

On the right-hand side of Figure 4 we depict, as the solid black line, the coefficients obtained when we impose the following restrictions on the β terms:

$$\beta^t_Y = \beta^t_s, \quad s = 18, \ldots, 26, \quad t > 22, \tag{25}$$

$$\beta^t_s = 0, \quad s \geq 27, \quad t > s; \tag{26}$$

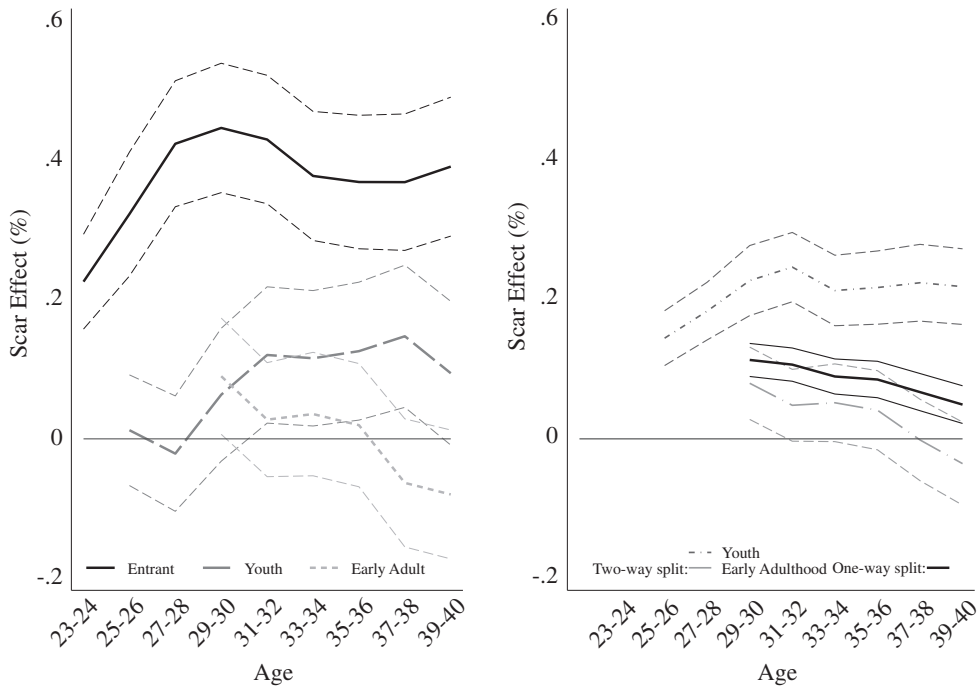


FIGURE 4. Long-term effects of early labour market experience. *Notes*: The left-hand panel reports estimated coefficients from equation (23) for the effect of experience for Entrants, Youths and early Adults, β^t_E (solid line), β^t_Y (dashed line), and β^t_A (dotted line), reported in column (3) of Table 1. The right-hand panel reports estimated coefficients from different restrictions on the β coefficients. The dash-dotted lines report the two-way split given by (27)–(29); the solid line reports the one-way split given by (25) and (26). The corresponding estimated coefficients are in columns (4) and (5) of Table A2 in the Appendix. In both panels, the thin dashed lines indicate the 95% confidence intervals.

© 2021 The London School of Economics and Political Science

14680335, 2021, 352, Downloaded from https://onlinelibrary.wiley.com/doi/10.1111/ecca.12384 by <Shibboleth>-staff@bham.ac.uk, Wiley Online Library on [09/08/2023]. See the Terms and Conditions (https://onlinelibrary.wiley.com/terms-and-conditions) on Wiley Online Library for rules of use; OA articles are governed by the applicable Creative Commons License

that is, when $\beta_E^t = \beta_Y^t = \beta_A^t$ in equation (23), so that no distinction is made between the ages of 18 and 26, reported in column (5) of Table A2 in the Appendix. These β terms give the overall effect of experience between ages 18 and 26 on future earnings. The thin dashed lines indicate the 95% confidence interval. This solid black line indicates a much lower and slightly decreasing effect of experience in the first nine years after entry into the labour market than the lines depicted on the left-hand side of Figure 4. The strong suggestion emerging from the comparison of the three lines on the left-hand side of Figure 4 and the solid black line on the right-hand side 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 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 right-hand side of Figure 4 also reports the coefficients obtained when Youth is split into two periods, that is, when the set of restrictions (16)–(19) is replaced by

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

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

$$\beta_s^t = 0, \quad s \geq 27, \quad t > s, \tag{29}$$

where the subscripts 1 and 2 label the first and second parts 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 18–22, the short dash-dot line on the right-hand side of Figure 4, with the thin dashed lines indicating the confidence interval), and no effect for the second (ages 23–27, the long dash-dot line).

To close this subsection, 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) of Table 1. The results 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, makes 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 of 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 seem 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.

© 2021 The London School of Economics and Political Science

14680335, 2021, 352, Downloaded from https://onlinelibrary.wiley.com/doi/10.1111/ecca.12384 by <Shibboleth>-staff@bham.ac.uk, Wiley Online Library on [09/08/2023]. See the Terms and Conditions (https://onlinelibrary.wiley.com/terms-and-conditions) on Wiley Online Library for rules of use; OA articles are governed by the applicable Creative Commons License

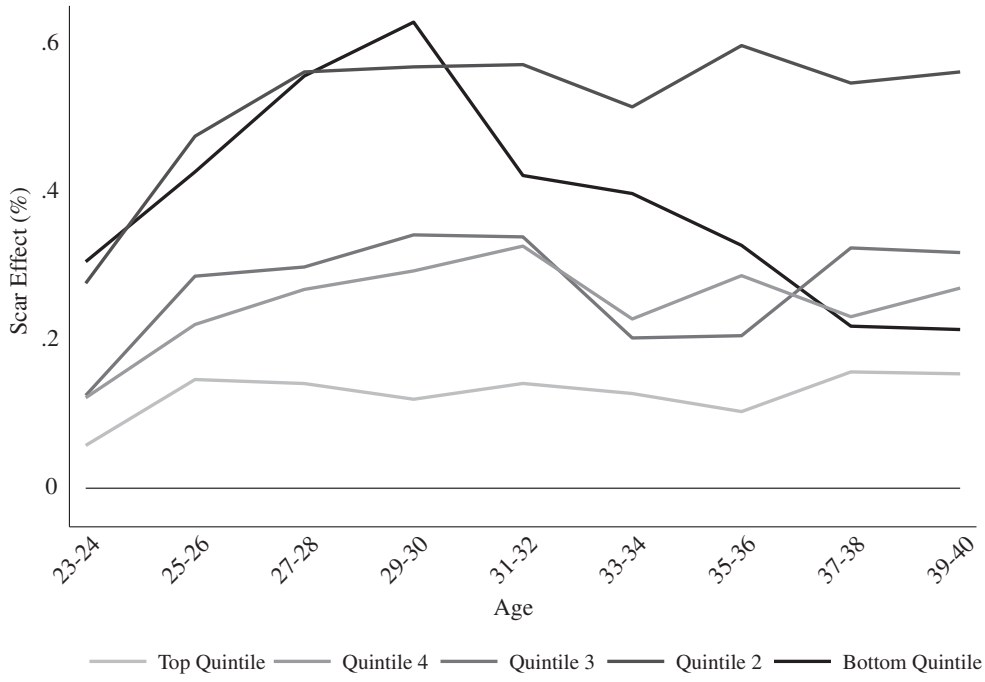


FIGURE 5. Long-term effects of experience on Entrants of different abilities. *Notes*: Coefficients β_E^t are calculated for subsamples of individuals for each quintile, membership of which is derived from the distribution of predicted earnings potentials. The values of the coefficients used to draw the lines are in Table A3 of the Appendix. We have not included confidence intervals.

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 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 (3) and (4) of Table 2, the 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 (ages 21–23) 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.

© 2021 The London School of Economics and Political Science

14680335, 2021, 352, Downloaded from https://onlinelibrary.wiley.com/doi/10.1111/ecca.12384 by <Shibboleth>-staff@bham.ac.uk, Wiley Online Library on [09/08/2023]. See the Terms and Conditions (https://onlinelibrary.wiley.com/terms-and-conditions) on Wiley Online Library for rules of use; OA articles are governed by the applicable Creative Commons License

TABLE 2
ROBUSTNESS CHECKS

	Students (1)	Self-employed (2)	National address (3)	First address (4)	Alternative clustering (5)
Weeks employed in year	2.318***	2.359***	2.339***	2.457***	2.365***
	(0.047)	(0.046)	(0.094)	(0.045)	(0.039)
Entrant experience (18–20)					
On earnings aged 23–24	0.321***	0.231***	0.250***	0.268***	0.226***
	(0.037)	(0.034)	(0.040)	(0.029)	(0.031)
On earnings aged 25–26	0.447***	0.320***	0.350***	0.391***	0.323***
	(0.046)	(0.044)	(0.053)	(0.036)	(0.039)
On earnings aged 27–28	0.537***	0.413***	0.435***	0.473***	0.423***
	(0.047)	(0.045)	(0.055)	(0.038)	(0.041)
On earnings aged 29–30	0.566***	0.443***	0.474***	0.507***	0.446***
	(0.049)	(0.046)	(0.068)	(0.038)	(0.041)
On earnings aged 31–32	0.552***	0.431***	0.441***	0.496***	0.430***
	(0.048)	(0.045)	(0.068)	(0.039)	(0.042)
On earnings aged 33–34	0.498***	0.383***	0.388***	0.464***	0.377***
	(0.048)	(0.046)	(0.048)	(0.038)	(0.042)
On earnings aged 35–36	0.492***	0.371***	0.382***	0.440***	0.369***
	(0.050)	(0.048)	(0.060)	(0.040)	(0.043)
On earnings aged 37–38	0.492***	0.367***	0.382***	0.430***	0.368***
	(0.051)	(0.049)	(0.055)	(0.039)	(0.044)
On earnings aged 39–40	0.506***	0.396***	0.405***	0.459***	0.391***
	(0.051)	(0.049)	(0.064)	(0.041)	(0.045)
Youth experience (21–23)					
On earnings aged 25–26	−0.013	0.019	−0.017	−0.007	0.012
	(0.041)	(0.039)	(0.037)	(0.033)	(0.036)
On earnings aged 27–28	−0.047	−0.008	−0.043	0.000	−0.021
	(0.042)	(0.041)	(0.048)	(0.034)	(0.038)
On earnings aged 29–30	0.042	0.070	0.021	0.091*	0.063
	(0.048)	(0.049)	(0.054)	(0.039)	(0.043)
On earnings aged 31–32	0.091	0.121*	0.106	0.128**	0.120**
	(0.050)	(0.049)	(0.071)	(0.040)	(0.043)
On earnings aged 33–34	0.091	0.114*	0.085	0.107**	0.116**
	(0.050)	(0.048)	(0.061)	(0.039)	(0.044)
On earnings aged 35–36	0.098	0.127*	0.098	0.126**	0.126**
	(0.051)	(0.049)	(0.064)	(0.040)	(0.044)
On earnings aged 37–38	0.110*	0.147**	0.121**	0.164***	0.147**
	(0.051)	(0.051)	(0.046)	(0.040)	(0.045)
On earnings aged 39–40	0.067	0.095	0.067	0.114**	0.094*
	(0.052)	(0.052)	(0.057)	(0.042)	(0.046)
Early Adulthood experience (24–26)					
On earnings aged 29–30	0.060	0.080	0.086	0.066	0.090*
	(0.042)	(0.041)	(0.047)	(0.037)	(0.037)
On earnings aged 31–32	0.007	0.028	0.031	0.020	0.028
	(0.042)	(0.041)	(0.032)	(0.036)	(0.038)
On earnings aged 33–34	0.016	0.038	0.042	0.037	0.036
	(0.045)	(0.044)	(0.049)	(0.037)	(0.041)

© 2021 The London School of Economics and Political Science

14680335, 2021, 352, Downloaded from https://onlinelibrary.wiley.com/doi/10.1111/ecca.12384 by <Shibboleth>-staff@bham.ac.uk, Wiley Online Library on [09/08/2023]. See the Terms and Conditions (https://onlinelibrary.wiley.com/terms-and-conditions) on Wiley Online Library for rules of use; OA articles are governed by the applicable Creative Commons License

TABLE 2
CONTINUED

	Students (1)	Self-employed (2)	National address (3)	First address (4)	Alternative clustering (5)
On earnings aged 35–36	0.000	0.020	0.025	0.047	0.020
	(0.045)	(0.044)	(0.050)	(0.037)	(0.041)
On earnings aged 37–38	−0.070	−0.063	−0.061	−0.042	−0.063
	(0.046)	(0.046)	(0.043)	(0.039)	(0.043)
On earnings aged 39–40	−0.092*	−0.084	−0.079	−0.058	−0.080
	(0.047)	(0.046)	(0.057)	(0.038)	(0.043)
Constant	10.094***	10.041***	10.033***	10.171***	10.033***
	(0.029)	(0.028)	(0.036)	(0.023)	(0.026)
N	294,725	291,136	289,608	294,705	279,877
Individuals	14,150	13,993	13,652	13,505	13,495

Notes
*,**,*** indicate $p < 0.05$, $p < 0.01$, $p < 0.001$, respectively.
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 equation (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 equation (22).

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 Entrant coefficients remain significant at all conventional levels, as shown in Table A2 of 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 allow 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 that enter the labour market at different stages in the business cycle? The results of this analysis are summarized 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 of the Appendix. The estimates of β_E for the 1983 and 1984 cohorts are less precisely calculated. 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 of 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.

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,

© 2021 The London School of Economics and Political Science

14680335, 2021, 352, Downloaded from https://onlinelibrary.wiley.com/doi/10.1111/ecca.12384 by <Shibboleth>-staff@bham.ac.uk, Wiley Online Library on [09/08/2023]. See the Terms and Conditions (https://onlinelibrary.wiley.com/terms-and-conditions) on Wiley Online Library for rules of use; OA articles are governed by the applicable Creative Commons License

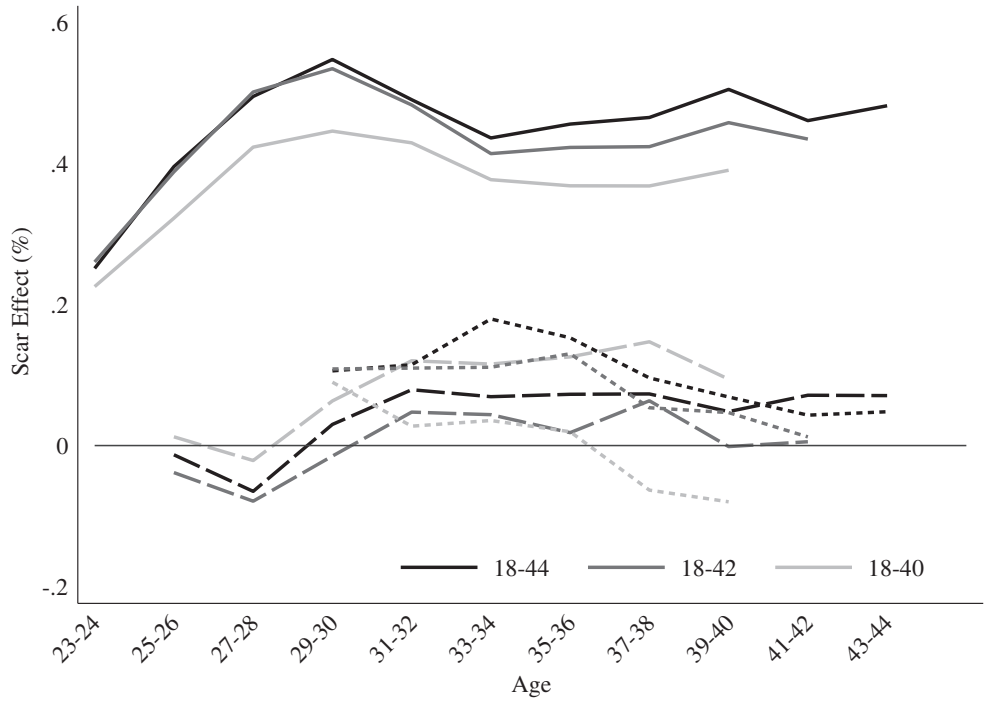


FIGURE 6. Scar effect of Youth unemployment for different cut-off ages. *Notes*: Estimated coefficients for the long-term effect of unemployment for different cut-off ages, estimated from the corresponding version of equation (23) for the effect of unemployment for Entrant, Youth and early Adult. The lightest 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 in the Appendix. We have not included confidence intervals.

19, reduces employability, so it increases the chances of unemployment and the opportunities to gain experience at age, say, 25.

We can rewrite the regression model (23) estimated in each of columns (1)–(3) of Table 1 with the set of restrictions (16) and (17) for column (1), (16), (18) and (19) for column (2), and (16), (18), (20) and (21) for column (3). To clarify the model that 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 earnings at age 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 a slight abuse of notation—the

© 2021 The London School of Economics and Political Science

14680335, 2021, 352, Downloaded from https://onlinelibrary.wiley.com/doi/10.1111/ecca.12384 by <Shibboleth>-staff@bham.ac.uk, Wiley Online Library on [09/08/2023]. See the Terms and Conditions (https://onlinelibrary.wiley.com/terms-and-conditions) on Wiley Online Library for rules of use; OA articles are governed by the applicable Creative Commons License

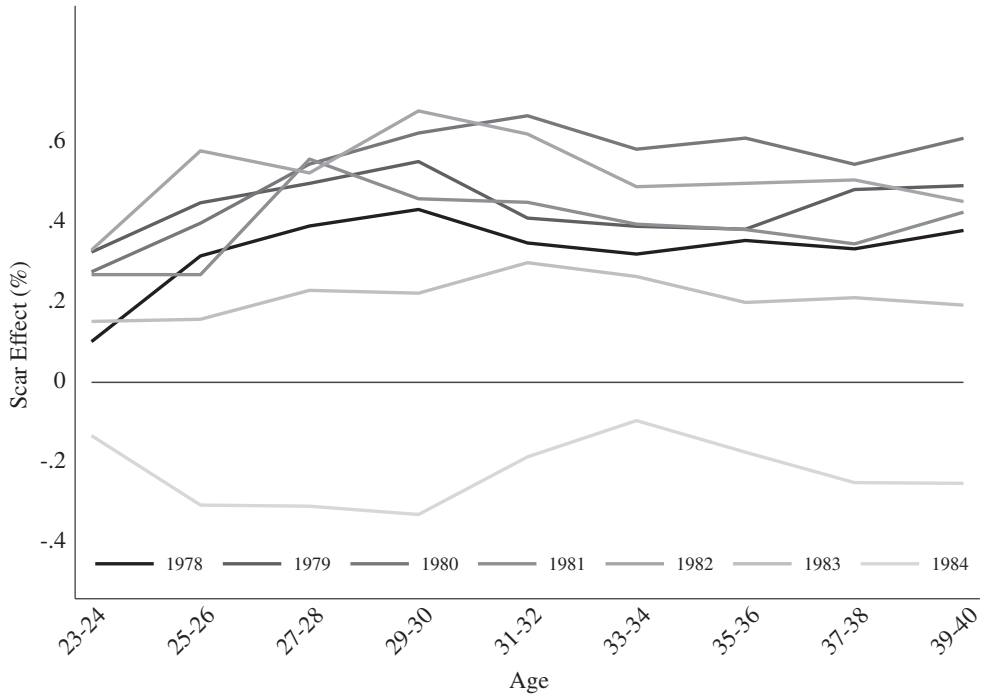


FIGURE 7. Scar effect of Entry unemployment for different cohorts. *Notes*: Coefficients β_E^t, are calculated separately for the subsample of individuals in each cohort who are in the sample used in our benchmark specification, column (3) of Table 1. The values of the coefficients used to draw the lines are in Table A4 of the Appendix. We have not included confidence intervals.

equation from which the partial derivative is derived, we obtain

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

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

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

Substituting now the values from equations (30)–(32), the above can be written as

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

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

© 2021 The London School of Economics and Political Science

14680335, 2021, 352, Downloaded from https://onlinelibrary.wiley.com/doi/10.1111/ecca.12384 by <Shibboleth>-staff@bham.ac.uk, Wiley Online Library on [09/08/2023]. See the Terms and Conditions (https://onlinelibrary.wiley.com/terms-and-conditions) on Wiley Online Library for rules of use; OA articles are governed by the applicable Creative Commons License

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

And from equating the right-hand sides of equations (30″) and (31″), we have

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

or

$$(33) \qquad \frac{\partial e_{2123}}{\partial e_{1820}} = \frac{\beta^t_{E,(1)} - \beta^t_{E,(2)}}{\beta^t_{Y,(2)}}.$$

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

$$(34) \qquad \frac{\partial e_{2426}}{\partial e_{2123}} = \frac{\beta^t_{Y,(2)} - \beta^t_{Y,(3)}}{\beta^t_{A,(3)}}.$$

Next, equating the right-hand sides of equations (31″) and (32″) gives

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

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

$$(35) \qquad \frac{\partial e_{2426}}{\partial e_{1820}} = \frac{\beta^t_{E,(2)} - \beta^t_{E,(3)}}{\beta^t_{A,(3)}}.$$

From equation (32′), we can write the total effect of experience at ages 18–20 on earnings at age t, with t = 29–30, …, 39–40, as

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

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, $\partial e_{2123}/\partial e_{1820}$ from equation (33), $\partial e_{2426}/\partial e_{2123}$ from equation (34), and $\partial e_{2426}/\partial e_{1820}$ from equation (35), gives

© 2021 The London School of Economics and Political Science

14680335, 2021, 352, Downloaded from https://onlinelibrary.wiley.com/doi/10.1111/ecca.12384 by <Shibboleth>-staff@bham.ac.uk, Wiley Online Library on [09/08/2023]. See the Terms and Conditions (https://onlinelibrary.wiley.com/terms-and-conditions) on Wiley Online Library for rules of use; OA articles are governed by the applicable Creative Commons License

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

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 of the middle block of column (3) in Table 1. Conversely, consider column (4) in Table 3: from equation (33), we see that this is the direct effect of experience at ages 18–20 on experience at ages 21–23. Its value is obtained as the ratio of 0.51, in the fourth row of the first block of column (1) in Table 1, minus 0.45, in the fourth row of the first block of column (2) in Table 1, and 0.11, in the third row of 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 ages 18–20 are associated with little more than one additional week out of work at ages 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) of Table 3, 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 ages 18–20 on earnings at age t, $t = 29$–$30, \ldots, 39$–40, via the negative impact of not being employed at ages 24–26. This is conceptually similar, but slightly more complex than the above. As shown in equation (36), it is the product of two terms: (i) the direct effect of experience at ages 24–26 on earnings at age t, reported in column (6), multiplied by the sum of the direct effect of experience at ages 18–20 on experience at ages 24–26 ($\partial e_{2426}/\partial e_{1820}$ reported in column (7)), and (ii) the effect of experience at ages 18–20 on experience at ages 24–26 via experience at ages 21–23. This, in turn, is the product of two terms, the effect of experience at ages 18–20 on experience at ages 21–23 ($\partial e_{2123}/\partial e_{1820}$ reported in column (4)) times the effect of experience at ages 21–23 on experience at ages 24–26 (reported in column (7)). Looking at column (5), we can see that the magnitude is similar to that in column (2) and again much smaller than in column (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 at ages 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,

© 2021 The London School of Economics and Political Science

14680335, 2021, 352, Downloaded from https://onlinelibrary.wiley.com/doi/10.1111/ecca.12384 by <Shibboleth>-staff@bham.ac.uk, Wiley Online Library on [09/08/2023]. See the Terms and Conditions (https://onlinelibrary.wiley.com/terms-and-conditions) on Wiley Online Library for rules of use; OA articles are governed by the applicable Creative Commons License

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

Notes
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, $\partial uw_{2123}/\partial uw_{1820}$, derived in equation (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, $\partial uw_{2426}/\partial uw_{2123}$, derived in equation (34). Column (8) is the effect on early Adult experience of Entrant experience, $\partial uw_{2426}/\partial uw_{1820}$; see equation (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, $\beta^t_{A,(3)} \times \left(\frac{\partial uw_{2426}}{\partial uw_{2123}}\frac{\partial uw_{2123}}{\partial uw_{1820}} + \frac{\partial uw_{2426}}{\partial uw_{1820}}\right)$.

interpretation of this result is that lower experience at ages 18–20 is particularly associated with reduced experimentation and learning as in Papageorgiou (2014) or Wee (2016), rather than stigma effects that might be expected to operate through subsequent periods of unemployment, as in Lockwood (1991).

VI. CONCLUDING REMARKS

Past unemployment lowers earnings for the rest of a worker's life. These long-term effects are extensively documented for the USA (Table 1 in Couch and Placzek (2010, p. 574) summarizes studies using both administrative and survey panel datasets), the UK (Arulampalam *et al.* (2001) review 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 US 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 become available in several countries, it will become more feasible to verify if the varying importance of shocks occurring in different periods of an individual's 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 subsidized 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

© 2021 The London School of Economics and Political Science

14680335, 2021, 352, Downloaded from https://onlinelibrary.wiley.com/doi/10.1111/ecca.12384 by <Shibboleth>-staff@bham.ac.uk, Wiley Online Library on [09/08/2023]. See the Terms and Conditions (https://onlinelibrary.wiley.com/terms-and-conditions) on Wiley Online Library for rules of use; OA articles are governed by the applicable Creative Commons License

economy, an understanding of where to direct scarce resources to maximize their medium-term impact is important. Evidence of the different responses of workers with different characteristics to unemployment shocks enhances this understanding.

ACKNOWLEDGMENTS

We would like to thank Martin Foreaux-Koppensteiner, Andrea Ichino, Jesse Matheson, Giovanni Pica, Till von Wachter, the editor Steve Machin, three anonymous referees and seminar participants at CHERE UTS Sydney, Leicester, UNNC Ningbo, York, and the ECB labour market conference, Frankfurt.

NOTES

1. Bell and Blanchflower (2011) analyse the effect of the Great Recession on young people in the USA 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 (accessed 30 May 2021) for data.
2. Schwandt and von Wachter (2019) study the role of the timing of graduation in the USA. In the period that 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.
3. A sensitivity test including students suggests that our main result is unchanged; see column (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 be qualitatively different from individuals who are never self-employed. In column (2) of 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.
4. Oreopoulos *et al.* (2012) find 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.
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).
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 and Stevens 1999, 2001). Hijzen *et al.* (2010) show that in the UK, the effects of displacement depend 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 it 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.
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.
9. A fresh cohort of individuals enters the data every year and is followed from then on. These administrative data are 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, 2014; Dickens and McKnight 2008b), and the link between unemployment and low pay (Gosling *et al.* 1997).
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 that 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, so it is adequately captured by individual fixed effects θ_i.
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.

© 2021 The London School of Economics and Political Science

14680335, 2021, 352, Downloaded from https://onlinelibrary.wiley.com/doi/10.1111/ecca.12384 by <Shibboleth>-staff@bham.ac.uk, Wiley Online Library on [09/08/2023]. See the Terms and Conditions (https://onlinelibrary.wiley.com/terms-and-conditions) on Wiley Online Library for rules of use; OA articles are governed by the applicable Creative Commons License

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.
15. The summary statistics reported in Figure 3 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 is the effect on lifetime wages, 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 http://geoportal.statistics.gov.uk/datasets/local-authority-districts-april-2019-names-and-codes-in-the-united-kingdom (accessed 31 May 2021). We use these, instead of 'travel to work areas' (TTWAs), 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.
18. We could also drop all observations with a missing address; this is an inferior alternative, however, 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.
19. Take as an example the coefficient 0.39 that measures the effect of one extra week of experience at ages 18–20 on earnings at ages 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 of unemployment reduces long-term earnings by $0.0039 \times 4 = 1.6\%$.
20. For example, if individual *i* from the cohort entering in 1978 is recorded as being self-employed in years 1994 and 1997, then we include the observations from all other years in the estimations reported in column (2) of Table 2.
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 and Miller (2015) for a discussion.

REFERENCES

ADDA, J., DUSTMANN, C. and STEVENS, K. (2017). The career costs of children. *Journal of Political Economy*, **125**(2), 293–337.

ARULAMPALAM, W., GREGG, P. and GREGORY, M. (2001). Unemployment scarring. *Economic Journal*, **111**, F577–F584.

BELL, D. N. F. and BLANCHFLOWER, D. G. (2011). Young people and the Great Recession. *Oxford Review of Economic Policy*, **27**, 241–67.

BEN-PORATH, Y. (1967). The production of human capital and the lifecycle of earnings. *Journal of Political Economy*, **75**, 352–65.

BIEWEN, M. and STEFFES, S. (2010). Unemployment persistence: is there evidence for stigma effects? *Economics Letters*, **106**, 188–90.

BLACKBURN, M. L., BLOOM, D. E. and NEUMARK, D. (1993). Fertility timing, wages, and human capital. *Journal of Population Economics*, **6**(1), 1–30.

BLUNDELL, R., BOZIO, A. and LAROQUE, G. (2013). Extensive and intensive margins of labour supply: work and working hours in the US, the UK and France. *Fiscal Studies*, **34**, 1–29.

BURGESS, S., PROPPER, C., REES, H. and SHEARER, A. (2003). The class of 1981: the effects of early career unemployment on subsequent unemployment experiences. *Labour Economics*, **10**, 291–309.

CAHUC, P., CARCILLO, S., RINNE, U. and ZIMMERMANN, K. F. (2013). Youth unemployment in old Europe: the polar cases of France and Germany. *IZA Journal of European Labor Studies*, **2**, 18.

CAMERON, A. C. and MILLER, D. L. (2015). A practitioner's guide to cluster-robust inference. *Journal of Human Resources*, **50**(2), 317–72.

CHAN, S. and STEVENS, A. H. (1999). Employment and retirement following a late-career job loss. *American Economic Review*, **89**(2), 211–16.

CHAN, S. and STEVENS, A. H. (2001). Job loss and employment patterns of older workers. *Journal of Labor Economics*, **19**(2), 484–521.

CHETTY, R. and HENDREN, N. (2018a). The impacts of neighborhoods on intergenerational mobility I: Childhood exposure effects. *Quarterly Journal of Economics*, **133**(3), 1107–62.

CHETTY, R. and HENDREN, N. (2018b). The impacts of neighborhoods on intergenerational mobility II: County-level estimates. *Quarterly Journal of Economics*, **133**(3), 1163–228.

© 2021 The London School of Economics and Political Science

14680335, 2021, 352, Downloaded from https://onlinelibrary.wiley.com/doi/10.1111/ecca.12384 by <Shibboleth>-staff@bham.ac.uk, Wiley Online Library on [09/08/2023]. See the Terms and Conditions (https://onlinelibrary.wiley.com/terms-and-conditions) on Wiley Online Library for rules of use; OA articles are governed by the applicable Creative Commons License

CLARK, A., GEORGELLIS, Y. and SANFEY, P. (2001). Scarring: the psychological impact of past unemployment. *Economica*, **68**, 221–41.
COOMBES, M. and OPENSHAW, S. (1982). The use and definition of travel-to-work areas in Great Britain: some comments. *Regional Studies*, **16**, 141–9.
CORNELISSEN, T., DUSTMANN, C. and SCHöNBERG, U. (2017). Peer effects in the workplace. *American Economic Review*, **107**, 425–56.
COUCH, K. A. and PLACZEK, D. W. (2010). Earnings losses of displaced workers revisited. *American Economic Review*, **100**, 572–89.
CUNHA, F., HECKMAN, J. J. and SCHENNACH, S. M. (2010). Estimating the technology of cognitive and noncognitive skill formation. *Econometrica*, **78**, 883–931.
DICKENS, R. and MCKNIGHT, A. (2008a). Changes in earnings inequality and mobility in Great Britain 1978/9–2005/6. LSE STICERD Research Paper no. CASE132.
DICKENS, R. and MCKNIGHT, A. (2008b). Assimilation of migrants into the British labour market. LSE STICERD Research Paper no. CASE133.
ELIASON, M. and STORRIE, D. (2006). Lasting or latent scars? Swedish evidence on the long-term effects of job displacement. *Journal of Labor Economics*, **24**(4), 831–56.
ELLWOOD, D. T. (1982). Teenage unemployment: permanent scars or temporary blemishes? In R. B. Freeman and D. A. Wise (eds), *The Youth Labor Market Problem: Its Nature, Causes, and Consequences*. Chicago, IL: University of Chicago Press, pp. 349–90.
GARDINER, K. and HILLS, J. (1999). Policy implications of new data on income mobility. *Economic Journal*, **109**, 91–111.
GENDA, Y., KONDO, A. and OHTA, S. (2010). Long-term effects of a recession at labor market entry in Japan and the United States. *Journal of Human Resources*, **45**, 157–96.
GOLDIN, C. (2014). A grand gender convergence: its last chapter. *American Economic Review*, **104**, 1091–119.
GOLLIN, D. (2002). Getting income shares right. *Journal of Political Economy*, **110**, 458–74.
GOSLING, A., JOHNSON, P., MCCRAE, J. and PAULL, G. (1997). The dynamics of low pay and unemployment in 1990s Britain. Institute for Fiscal Studies Report no. R54.
GREGG, P. (2001). The impact of youth unemployment on adult unemployment in the NCDS. *Economic Journal*, **111**, 626–53.
GREGG, P. and TOMINEY, E. (2005). The wage scar from male youth unemployment. *Labour Economics*, **12**, 487–509.
HARMON, C., WALKER, I. and WESTERGAARD-NIELSEN, N. (2001). Introduction. In C. Harmon, I. Walker and N. Westergaard-Nielsen (eds), *Education and Earnings in Europe: A Cross-Country Analysis of the Returns to Education*. Cheltenham: Edward Elgar.
HERSHBEIN, B. J. (2012). Graduating high school in a recession: work, education, and home production. *B. E. Journal of Economic Analysis & Policy*, **12**(1), 1–32.
HIJZEN, A., UPWARD, R. and WRIGHT, P. W. (2010). The income losses of displaced workers. *Journal of Human Resources*, **45**(1), 243–69.
ILO (2012). Youth Guarantees: A Response to the Youth Employment Crisis? Geneva: ILO.
JACOBSON, L. S., LALONDE, R. J. and SULLIVAN, D. G. (1993). Earnings losses of displaced workers. *American Economic Review*, **83**, 685–709.
JACOBSON, L. S., LALONDE, R. J. and SULLIVAN, D. G. (2005). Estimating the returns to community college schooling for displaced workers. *Journal of Econometrics*, **125**, 271–304.
KAHN, L. B. (2010). The long-term labor market consequences of graduating from college in a bad economy. *Labour Economics*, **17**, 303–16.
KÜBLER, D. and VON WEIZSäCKER, G. (2003). Information cascades in the labor market. *Journal of Economics*, **80**, 211–29.
LEMOS, S. (2013). Immigrant economic assimilation: evidence from UK longitudinal data between 1978 and 2006. *Labour Economics*, **24**, 339–53.
LEMOS, S. (2014). The immigrant–native earnings gap across the earnings distribution. *Applied Economics Letters*, **22**, 361–9.
LOCKWOOD, B. (1991). Information externalities in the labour market and the duration of unemployment. *Review of Economic Studies*, **58**, 733–53.
LOUGHRAN, D. S. and ZISSIMOPOULOS, J. M. (2009). Why wait? The effect of marriage and childbearing on the wages of men and women. *Journal of Human resources*, **44**(2), 326–49.
LYNCH, L. M. (1985). State dependency in youth unemployment. *Journal of Econometrics*, **28**, 71–84.
LYNCH, L. M. (1989). The youth labor market in the eighties: determinants of re-employment probabilities for young men and women. *Review of Economics and Statistics*, **71**, 37–45.

© 2021 The London School of Economics and Political Science

14680335, 2021, 352, Downloaded from https://onlinelibrary.wiley.com/doi/10.1111/ecca.12384 by <Shibboleth>-staff@bham.ac.uk, Wiley Online Library on [09/08/2023]. See the Terms and Conditions (https://onlinelibrary.wiley.com/terms-and-conditions) on Wiley Online Library for rules of use; OA articles are governed by the applicable Creative Commons License

MINCER, J. (1958). Investment in human capital and personal income distribution. *Journal of Political Economy*, **66**, 281–302.
MINCER, J. (1974). *Schooling, Experience and Earnings*. New York: Columbia University Press.
MORETTI, E. (2004). Estimating the social return to higher education: evidence from longitudinal and repeated cross-sectional data. *Journal of Econometrics*, **121**, 175–212.
MROZ, T. A. and SAVAGE, T. H. (2006). The long-term effects of youth unemployment. *Journal of Human Resources*, **41**, 259–93.
NEAL, D. (1995). Industry-specific human capital: evidence from displaced workers. *Journal of Labor Economics*, **13**, 653–77.
NICKELL, S., JONES, P. and QUINTINI, G. (2002). A picture of job insecurity facing British men. *Economic Journal*, **112**, 1–27.
OREOPOULOS, P., VON WACHTER, T. and HEISZ, A. (2012). The short- and long-term career effects of graduating in a recession. *American Economic Journal: Applied Economics*, **4**, 1–29.
PAPAGEORGIOU, T. (2014). Learning your comparative advantages. *Review of Economic Studies*, **81**, 1263–95.
PETRONGOLO, B. (2009). The long-term effects of job search requirements: evidence from the UK JSA reform. *Journal of Public Economics*, **93**, 1234–53.
PISSARIDES, C. A. (1992). Loss of skill during unemployment and the persistence of employment shocks. *Quarterly Journal of Economics*, **107**, 1371–91.
ROBERTSON, S. L. (2010). Globalising UK higher education. London: Centre for Learning and Life Chances in Knowledge Economies and Societies.
RUHM, C. J. (1991). Are workers permanently scarred by job displacements? *American Economic Review*, **81**, 319–24.
SCARPETTA, S., SONNET, A. and MANFREDI, T. (2010). Rising youth unemployment during the crisis. OECD Social, Employment and Migration Working Paper no. 106.
SCHMILLEN, A. and MÖLLER, J. (2012). Distribution and determinants of lifetime unemployment. *Labour Economics*, **19**, 33–47.
SCHMILLEN, A. and UMKEHRER, M. (2018). The scars of youth: effects of early-career unemployment on future unemployment experience. *International Labour Review*, **156**, 465–94.
SCHWANDT, H. and VON WACHTER, T. M. (2019). Unlucky cohorts: estimating the long-term effects of entering the labor market in a recession in large cross-sectional data sets. *Journal of Labor Economics*, **37**, S161–S198.
SPEER, J. D. (2016). Wages, hours, and the school-to-work transition: the consequences of leaving school in a recession for less-educated men. *B. E. Journal of Economic Analysis & Policy*, **16**(1), 97–124.
STEVENS, A. H. (1997). Persistent effects of job displacement: the importance of multiple job losses. *Journal of Labor Economics*, **15**, 165–88.
TATSIRAMOS, K. (2009). Unemployment insurance in Europe: unemployment duration and subsequent employment stability. *Journal of the European Economic Association*, **7**, 1225–60.
VISHWANATH, T. (1989). Job search, stigma effect, and escape rate from unemployment. *Journal of Labor Economics*, **7**, 487–502.
WEE, S. L. (2016). Delayed learning and human capital accumulation: the cost of entering the job market during a recession. Mimeo.

© 2021 The London School of Economics and Political Science

14680335, 2021, 352, Downloaded from https://onlinelibrary.wiley.com/doi/10.1111/ecca.12384 by <Shibboleth>-staff@bham.ac.uk, Wiley Online Library on [09/08/2023]. See the Terms and Conditions (https://onlinelibrary.wiley.com/terms-and-conditions) on Wiley Online Library for rules of use; OA articles are governed by the applicable Creative Commons License

APPENDIX: ADDITIONAL TABLES AND FIGURES

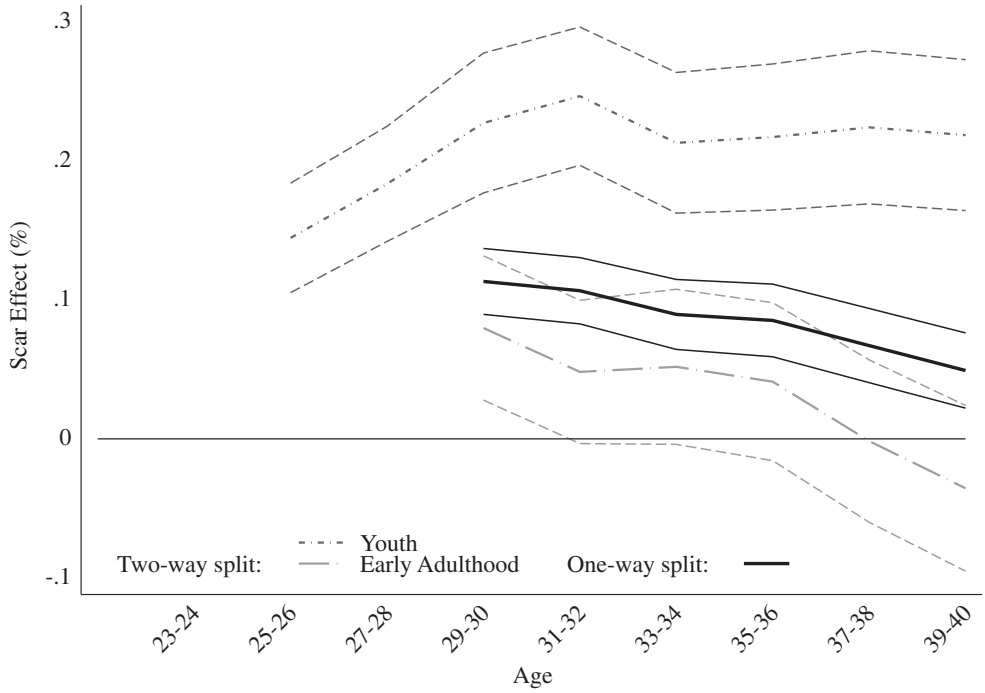


FIGURE A1. The scar effect of Youth unemployment. *Notes*: The dash-dotted lines are the coefficients of the two-way split, that is, when equation (23) is estimated with restrictions (27)–(29). The black solid line plots the coefficients imposing restrictions (27)–(29).

© 2021 The London School of Economics and Political Science

14680335, 2021, 352, Downloaded from https://onlinelibrary.wiley.com/doi/10.1111/ecca.12384 by <Shibboleth>-staff@bham.ac.uk, Wiley Online Library on [09/08/2023]. See the Terms and Conditions (https://onlinelibrary.wiley.com/terms-and-conditions) on Wiley Online Library for rules of use; OA articles are governed by the applicable Creative Commons License

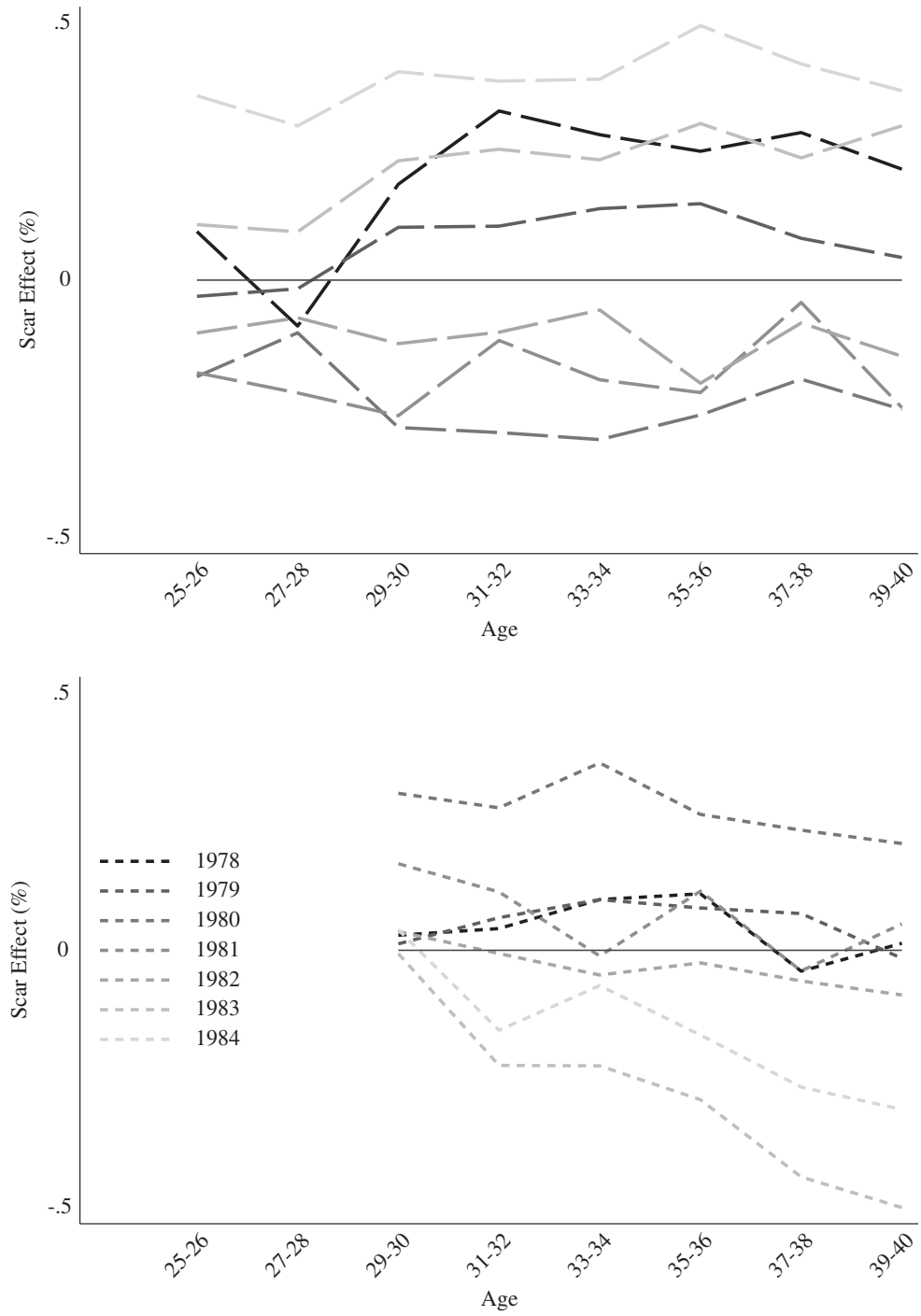


FIGURE A2. Scar effects of Youth and early Adult unemployment for different cohorts. *Notes*: The lines report the estimated coefficients from equation (23) for the effect of Youth and early Adulthood unemployment corresponding to those in Figure 7—that is, the coefficients β_Y^t and β_A^t, calculated from each subsample of individuals in the same cohort in the sample of the main regression, column (3) of Table 1.

© 2021 The London School of Economics and Political Science

14680335, 2021, 352, Downloaded from https://onlinelibrary.wiley.com/doi/10.1111/ecca.12384 by <Shibboleth>-staff@bham.ac.uk, Wiley Online Library on [09/08/2023]. See the Terms and Conditions (https://onlinelibrary.wiley.com/terms-and-conditions) on Wiley Online Library for rules of use; OA articles are governed by the applicable Creative Commons License

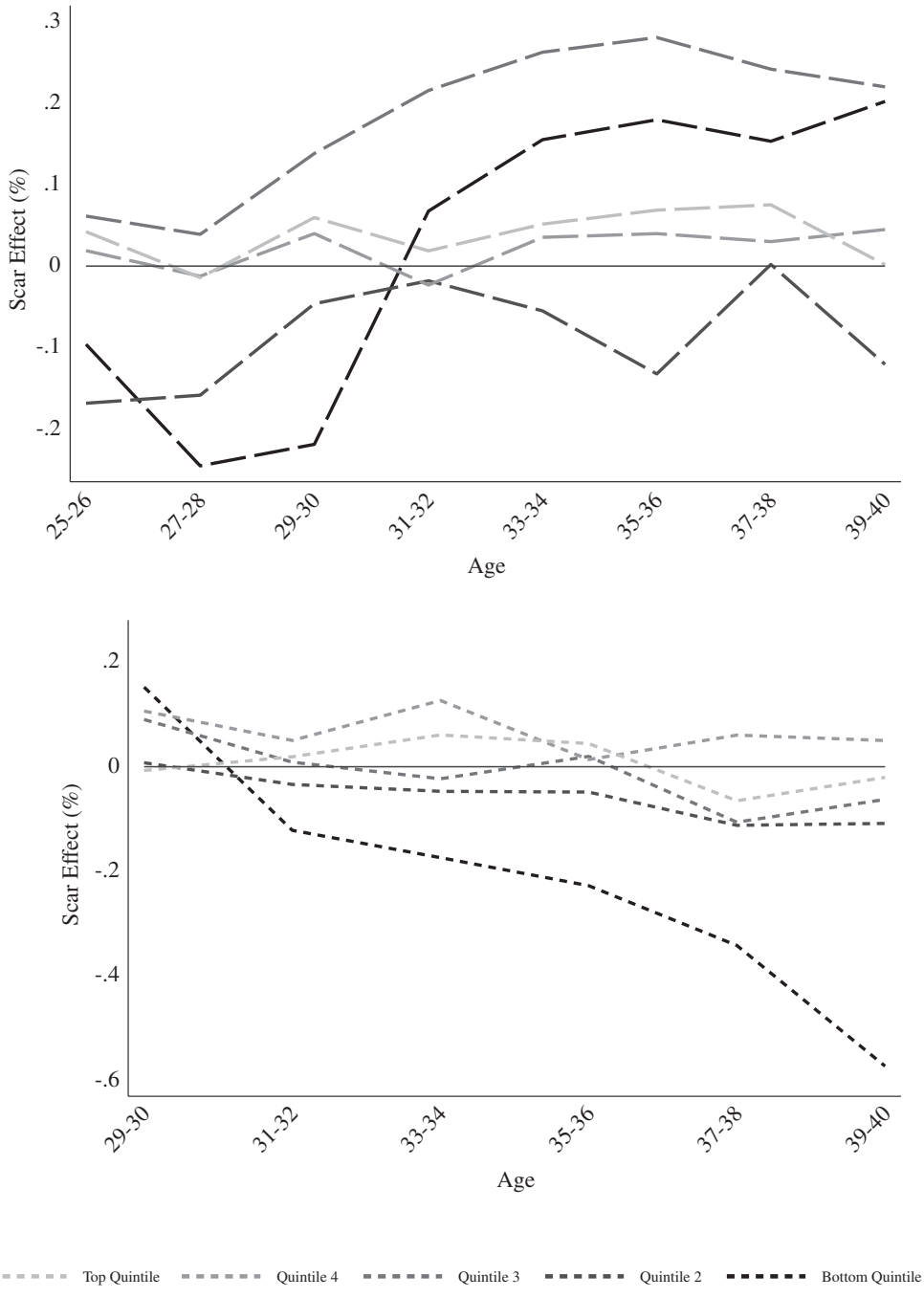


FIGURE A3. Scar effects of Youth and early Adult unemployment on individuals of different abilities. *Notes*: The lines report the estimated coefficients from equation (23) for the effect of Youth and early Adulthood unemployment corresponding to those in Figure 5—that is, the coefficients β_Y^t and β_A^t, calculated for the subsample of individuals of each ability quintile, membership of which is determined by individuals' predicted earnings.

© 2021 The London School of Economics and Political Science

14680335, 2021, 352, Downloaded from https://onlinelibrary.wiley.com/doi/10.1111/ecca.12384 by <Shibboleth>-staff@bham.ac.uk, Wiley Online Library on [09/08/2023]. See the Terms and Conditions (https://onlinelibrary.wiley.com/terms-and-conditions) on Wiley Online Library for rules of use; OA articles are governed by the applicable Creative Commons License

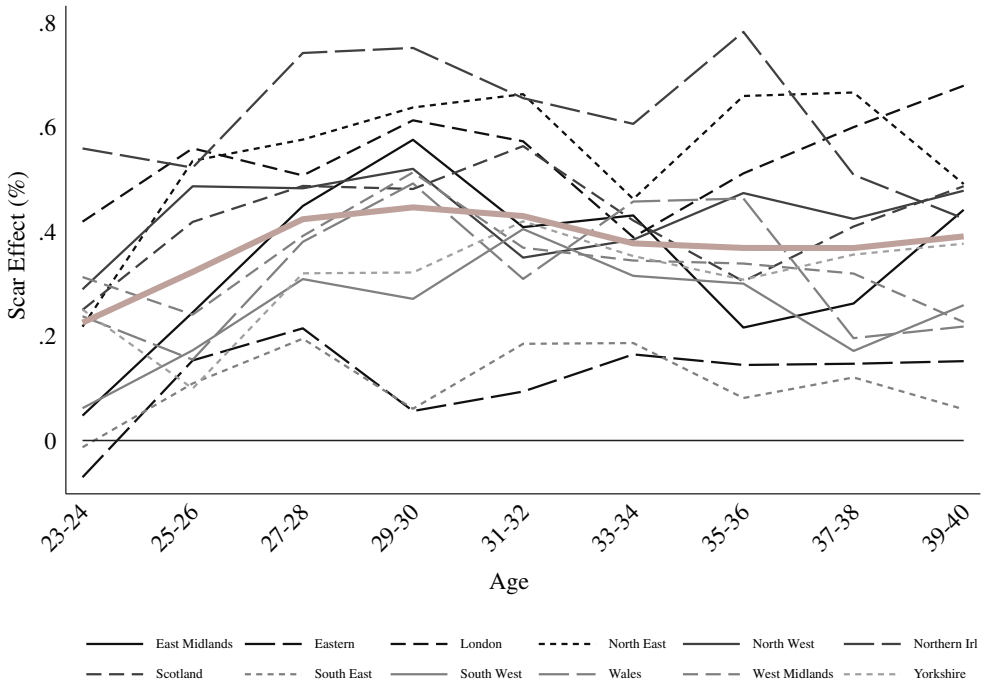


FIGURE A4. Scar effects of Entry unemployment in different regions. *Notes*: The lines report the estimated coefficients from equation (23) for the effect of Entry unemployment corresponding to those in Figure 5—that is, the coefficients β_E^t are calculated for each subsample of individuals in the same region at the time of entry into the labour market.

© 2021 The London School of Economics and Political Science

14680335, 2021, 352, Downloaded from https://onlinelibrary.wiley.com/doi/10.1111/ecca.12384 by <Shibboleth>-staff@bham.ac.uk, Wiley Online Library on [09/08/2023]. See the Terms and Conditions (https://onlinelibrary.wiley.com/terms-and-conditions) on Wiley Online Library for rules of use; OA articles are governed by the applicable Creative Commons License

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
S.D.	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
S.D.	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
S.D.	29.48	28.68	27.78	27.31	28.46	29.51	30.03	28.75
Earnings at age:								
23–24	12,229	11,998	11,936	11,932	12,454	13,114	13,313	12,409
S.D.	6875	7235	7929	7649	8163	8693	8221	7848
25–26	13,800	13,890	14,417	14,778	14,791	14,544	14,023	14,316
S.D.	8476	8687	9728	9040	9722	10,614	9544	9418
27–28	16,233	16,881	16,850	16,095	15,494	15,405	15,546	16,093
S.D.	10,624	10,783	11,446	11,370	11,400	12,040	11,883	11,376
29–30	18,306	18,208	17,337	16,631	16,737	16,869	16,508	17,242
S.D.	11,763	14,924	12,915	12,274	13,979	13,191	14,337	13,396
31–32	18,378	18,416	18,269	17,524	17,521	17,729	18,042	17,991
S.D.	12,441	12,905	13,557	14,306	15,170	15,640	16,952	14,463
33–34	19,205	19,441	18,795	17,972	18,529	19,914	20,876	19,236
S.D.	13,710	15,307	14,416	15,829	15,476	18,793	21,797	16,644
35–36	19,451	19,821	19,695	19,591	20,709	22,203	22,675	20,559
S.D.	14,389	17,442	16,460	18,019	18,760	23,908	23,121	19,094
37–38	20,461	21,659	21,830	21,857	22,041	23,414	22,401	21,936
S.D.	16,010	24,366	20,003	27,207	20,625	24,809	20,905	22,273
39–40	22,704	23,462	23,058	22,236	22,202	24,444	23,302	23,053
S.D.	22,904	25,356	20,691	26,052	23,458	32,200	26,050	25,398
41–42	23,638	23,877	23,394	22,036	22,075	0	0	23,019
S.D.	20,546	27,446	23,736	22,485	22,663	0	0	23,533
43–44	23,794	23,936	23,818	0	0	0	0	23,850
S.D.	21,963	22,492	26,957	0	0	0	0	23,935
45–46	24,348	0	0	0	0	0	0	24,348
S.D.	28,231	0	0	0	0	0	0	28,231
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	14,424

Notes
The upper block of the table reports the unemployment rate of UK male individuals born between 1960 and 1966 at ages 18–20, 21–23 and 24–26. The middle block reports their average yearly earnings, measured in 2004 pounds, at ages 23–24 to 45–46. The third block reports the proportion who claimed at least one benefit.

© 2021 The London School of Economics and Political Science

14680335, 2021, 352, Downloaded from https://onlinelibrary.wiley.com/doi/10.1111/ecca.12384 by <Shibboleth>-staff@bham.ac.uk, Wiley Online Library on [09/08/2023]. See the Terms and Conditions (https://onlinelibrary.wiley.com/terms-and-conditions) on Wiley Online Library for rules of use; OA articles are governed by the applicable Creative Commons License

TABLE A2
DIFFERENT CUT-OFF AGES AND DIFFERENT SPLITS OF THE PERIOD OF YOUTH

	Sample 18–40 (1)	Sample 18–42 (2)	Sample 18–44 (3)	Whole Youth (4)	Entrant + early Adult (5)
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 33–34	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 33–34	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)		

© 2021 The London School of Economics and Political Science

14680335, 2021, 352, Downloaded from https://onlinelibrary.wiley.com/doi/10.1111/ecca.12384 by <Shibboleth>-staff@bham.ac.uk, Wiley Online Library on [09/08/2023]. See the Terms and Conditions (https://onlinelibrary.wiley.com/terms-and-conditions) on Wiley Online Library for rules of use; OA articles are governed by the applicable Creative Commons License

TABLE A2
CONTINUED

	Sample 18–40 (1)	Sample 18–42 (2)	Sample 18–44 (3)	Whole Youth (4)	Entrant + early Adult (5)
On earnings aged 43–44			0.071		
			(0.089)		
Early Adulthood experience (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 33–34	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)		
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)	
Experience at age 18–22					
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)

© 2021 The London School of Economics and Political Science

14680335, 2021, 352, Downloaded from https://onlinelibrary.wiley.com/doi/10.1111/ecca.12384 by <Shibboleth>-staff@bham.ac.uk, Wiley Online Library on [09/08/2023]. See the Terms and Conditions (https://onlinelibrary.wiley.com/terms-and-conditions) on Wiley Online Library for rules of use; OA articles are governed by the applicable Creative Commons License

TABLE A2
CONTINUED

	Sample 18–40 (1)	Sample 18–42 (2)	Sample 18–44 (3)	Whole Youth (4)	Entrant + early Adult (5)
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)
Experience at age 23–26					
On earnings aged 29–30					0.080**
					(0.026)
On earnings aged 31–32					0.048
					(0.026)
On earnings aged 33–34					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)
279,877	220,218	146,294	279,877	279,877	
13,495	9782	5990	13,495	13,495	

Notes
*,**,*** indicate $p < 0.05$, $p < 0.01$, $p < 0.001$, respectively.
Columns (1)–(4) report estimated coefficients from equation (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 that 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 equation (23) is estimated with restrictions (25) and (26). These coefficients are depicted as the dashed line in the left-hand panel of 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 dash-dotted lines in Figure A1. Standard errors are given in parentheses.

© 2021 The London School of Economics and Political Science

14680335, 2021, 352, Downloaded from https://onlinelibrary.wiley.com/doi/10.1111/ecca.12384 by <Shibboleth>-staff@bham.ac.uk, Wiley Online Library on [09/08/2023]. See the Terms and Conditions (https://onlinelibrary.wiley.com/terms-and-conditions) on Wiley Online Library for rules of use; OA articles are governed by the applicable Creative Commons License

TABLE A3
RESULTS FOR DIFFERENT ABILITY QUINTILES

	Bottom quintile (1)	2nd quintile (2)	3rd quintile (3)	4th quintile (4)	Top quintile (5)
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 33–34	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 33–34	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 experience (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 33–34	−0.174	−0.047	−0.023	0.127	0.061
	(0.154)	(0.112)	(0.096)	(0.078)	(0.056)

© 2021 The London School of Economics and Political Science

14680335, 2021, 352, Downloaded from https://onlinelibrary.wiley.com/doi/10.1111/ecca.12384 by <Shibboleth>-staff@bham.ac.uk, Wiley Online Library on [09/08/2023]. See the Terms and Conditions (https://onlinelibrary.wiley.com/terms-and-conditions) on Wiley Online Library for rules of use; OA articles are governed by the applicable Creative Commons License

TABLE A3
CONTINUED

	Bottom quintile (1)	2nd quintile (2)	3rd quintile (3)	4th quintile (4)	Top quintile (5)
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	52,011	51,330	49,596	48,786	46,043
Individuals	2738	2493	2436	2445	2447

Notes
*,**,*** indicate $p < 0.05$, $p < 0.01$, $p < 0.001$, respectively.
Coefficients for the estimation of equation (23), the same as in the main regression, column (3) in Table 1, 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, and those in the other two blocks are plotted in Figure A3. Standard errors are given in parentheses.

© 2021 The London School of Economics and Political Science

14680335, 2021, 352, Downloaded from https://onlinelibrary.wiley.com/doi/10.1111/ecca.12384 by <Shibboleth>-staff@bham.ac.uk, Wiley Online Library on [09/08/2023]. See the Terms and Conditions (https://onlinelibrary.wiley.com/terms-and-conditions) on Wiley Online Library for rules of use; OA articles are governed by the applicable Creative Commons License

TABLE A4
RESULTS FOR INDIVIDUAL COHORTS

	1978 (1)	1979 (2)	1980 (3)	1981 (4)	1982 (5)	1983 (6)	1984 (7)
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 33–34	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)

© 2021 The London School of Economics and Political Science

14680335, 2021, 352, Downloaded from https://onlinelibrary.wiley.com/doi/10.1111/ecca.12384 by <Shibboleth>-staff@bham.ac.uk, Wiley Online Library on [09/08/2023]. See the Terms and Conditions (https://onlinelibrary.wiley.com/terms-and-conditions) on Wiley Online Library for rules of use; OA articles are governed by the applicable Creative Commons License

TABLE A4
CONTINUED

	1978 (1)	1979 (2)	1980 (3)	1981 (4)	1982 (5)	1983 (6)	1984 (7)
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 33–34	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 experience (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 33–34	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)

Economica
© 2021 The London School of Economics and Political Science

14680335, 2021, 352, Downloaded from https://onlinelibrary.wiley.com/doi/10.1111/ecca.12384 by <Shibboleth>-staff@bham.ac.uk, Wiley Online Library on [09/08/2023]. See the Terms and Conditions (https://onlinelibrary.wiley.com/terms-and-conditions) on Wiley Online Library for rules of use; OA articles are governed by the applicable Creative Commons License

TABLE A4
CONTINUED

	1978 (1)	1979 (2)	1980 (3)	1981 (4)	1982 (5)	1983 (6)	1984 (7)
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	41,584	42,578	42,196	40,630	37,399	37,316	38,174

Notes
*,**,*** indicate $p < 0.05$, $p < 0.01$, $p < 0.001$, respectively.
Coefficients for the estimation of equation (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 into the labour market. The coefficients in the top block are plotted in Figure 7, and those in the other two blocks are plotted in Figure A2. Standard errors are given in parentheses.

© 2021 The London School of Economics and Political Science

14680335, 2021, 352, Downloaded from https://onlinelibrary.wiley.com/doi/10.1111/ecca.12384 by <Shibboleth>-staff@bham.ac.uk, Wiley Online Library on [09/08/2023]. See the Terms and Conditions (https://onlinelibrary.wiley.com/terms-and-conditions) on Wiley Online Library for rules of use; OA articles are governed by the applicable Creative Commons License