Essays in the Macroeconomics of the Labour Market

by

Frank Jacobus Jacoba Leenders

A thesis submitted in conformity with the requirements
for the degree of Doctor of Philosophy

Department of Economics
University of Toronto

Essays in the Macroeconomics of the Labour Market

Frank Jacobus Jacoba Leenders
Doctor of Philosophy

Department of Economics
University of Toronto
2022

Abstract

In this thesis, I combine three essays that examine earnings consequences of labour market transitions, using detailed data and search-theoretical frameworks. In the first chapter, I use German employer-employee data to explore long-run earnings consequences of mass layoffs. I find that workers who return to the employer that laid them off experience larger earnings losses, both in the short and in the long run. These larger earnings losses for these so-called recalled workers are driven by employment in the short run and by wages in the long run. Furthermore, recalled workers are more likely to lose their job again compared to workers who were re-employed at different employers. In the second chapter, I propose a job search model that explains these findings. In the model, I distinguish between workers who may be recalled soon and regularly unemployed workers. The model replicates the larger earnings losses experienced by recalled workers, despite these workers optimally choosing for this recall, suggesting that the recall was not a bad outcome. Indeed, decomposing the difference in earnings loss between recalled and non-recalled workers shows that the short-run difference is largely explained by selection, whereas the long-run effect is driven by recalled workers being more likely to lose their job again. The third chapter investigates the cyclicality of occupational mobility and its consequences for subsequent earnings and wages, distinguishing between workers who switch occupations through unemployment and workers who switch during a job-to-job transition. Using data from the Survey of Income and Program Participation, I find that the fraction of occupational switchers switching through unemployment is countercyclical, and while these workers generally do worse in terms of earnings than workers who make a job-to-job transition, their earnings and wage patterns may slightly improve in recessions. I then propose a job search model of occupational mobility in which I incorporate both types of occupational switches and show that the overall deterioration of earnings outcomes for occupational switchers in recessions is driven by a composition change towards switching through unemployment, dampened by the outcomes experienced by workers who switch occupations without switching employers.

Acknowledgements

Hi Mom

Contents

List of Tables

List of Figures

Chapter 1

Recall and the scarring effects of job displacement: Evidence from Germany

1.1 Introduction

Workers who lose their job are exposed to a large and persistent loss in earnings compared to similar workers who did not lose their job (see e.g. Jacobson et al., 1993). The estimation of this "scarring" effect of job loss generally focuses on workers who lose their job through a mass layoff – I will refer to these workers as "displaced workers" – and do so permanently. However, it has also been documented that a large fraction of workers who lose their job will return to their former employer rather than move to a new employer.[1]

In this chapter, I explore the long-run effects of displacement on earnings, estimating and explaining the effects separately for workers who are recalled to their previous employer and those who were not. As recalled workers account for up to 10% of displaced workers, and these workers are generally abstracted from in existing literature, exploring the long-run effects of displacement by recall status contributes to a more complete understanding of the long-run effects of job loss.

To estimate the scarring effects of displacement by recall status, I use administrative (employer-employee) data from Germany. These data allow me to reliably identify individuals who are recalled to their former employing establishment. By separating the group of displaced workers into a recalled and non-recalled group, I can then separately estimate the scarring effect of displacement, rather than restricting the sample to omit recalls.[2] My baseline estimation uses the interaction-weighted estimator, as proposed by Sun and Abraham (2020), which allows me to account for the fact that workers displaced in different years may face different effects compared to the control group of

[1]For example, Fujita and Moscarini (2017) found that these workers account for over 40% of U.S. employed workers who move into unemployment upon losing their job (in the United States). For German workers, Mavromaras and Rudolph (1998) find similar albeit slightly lower numbers. In section 1.3.1, I show that I find recall rates ranging from 30 to 50% in my data (before conditioning on displacement).

[2]In the existing literature, recalled workers are often omitted from the sample. For example, Lachowska et al. (2020) focused on "permanent separations", and Schmieder et al. (2020) omitted any worker who returns to work for the same employer in the first 10 years after displacement.

never-displaced workers. The results on the average scarring effect of displacement (on earnings) are contrasted with those from a "standard" (two-way fixed effects) approach, revealing in particular that while this standard approach estimates a full recovery of employment, the interaction-weighted estimator suggests that the loss in employment is persistent.

I estimate that recalled workers, who account for close to 10% of the sample of displaced workers, experience larger earnings losses than displaced workers who are not recalled, both compared to a control group of never-displaced workers. As observed in figure 1.1, this difference appears both in the short- and long-run earnings losses. In particular, 1 year after displacement the recalled workers earns approximately 56% less than a worker in the control group, whereas a non-recalled worker earns approximately 39% less than a worker in that same control group. This difference persists, as 15 years after displacement the recalled worker still earns 38% less than a worker in the control group, whereas a non-recalled worker earns only 29% less. Moreover, estimating the effect on the employment fraction (fraction of the year spent in an employment relation) reveals that this larger earnings loss is fully driven by employment in the short run, but by wages in the long run. Finally, I find that a recalled worker is 6 to 10 percentage points more likely than a non-recalled worker to be separated from their job again in the first few years following the initial displacement, thereby providing a first step towards an explanation that I will further explore in the next chapter.

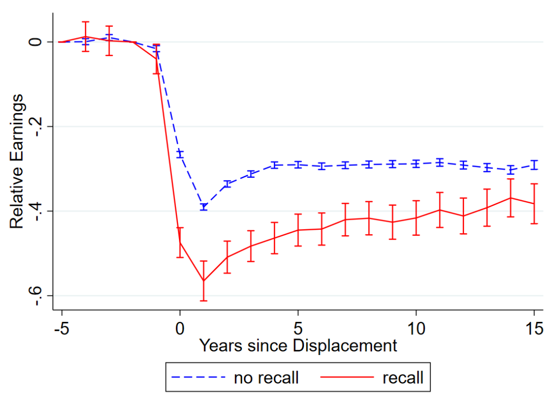


Figure 1.1: *The estimated effect of displacement on earnings by recall status (observed ex-post), relative to the control group of never-displaced workers, generated using the interaction-weighted estimator from Sun and Abraham (2020). The error bars correspond to 95% pointwise confidence intervals. The details of the estimation procedure are described in sections 1.2 and 1.3.3 of the paper.*

The rest of this chapter is organized as follows: After a brief overview of the related literature in the next subsection, section 1.2 describes the data and methodology used to generate the empirical results. These empirical results are then presented and discussed in section 1.3, after which section 1.4 concludes.

1.1.1 Related Literature

In empirically investigating the impact of recalls on the long-term consequences of job loss (and mass layoffs in particular), I contribute to a substantial existing literature. This literature goes back to Jacobson et al. (1993), who used quarterly administrative data from Pennsylvania and found that workers who were displaced in 1982 suffered an immediate earnings loss of more than 50% (relative to comparable workers who were not laid off), and still earned roughly 25% less 5 years later. This paper sparked a rich empirical literature, which either built on the result of Jacobson et al. (1993) in other U.S. settings[3], highlighting the important role of working hours in the short run and wages in the long run for explaining these losses (Lachowska et al., 2020), or showed that the results also hold in other countries.[4]

Until recently, most of the empirical discussion contained in the existing displacement literature abstracted from heterogeneity or only briefly touched upon it. One of the first exceptions to this is Guvenen et al. (2017), who documented how the scarring effects of job loss differ depending on where the worker is situated in the earnings distribution before being laid off. In a recent paper, Gulyas and Pytka (2020) used a machine learning approach to investigate which of the observable variables are most important in explaining heterogeneity in the earnings decline after job loss (using administrative data from Austria), finding an especially large role for firm characteristics. By focusing on ex-post recall status (and education), this chapter enriches the literature investigating heterogeneity in the scarring effects of displacement.

Aside from contributing to the investigation of heterogeneity in the scarring effects of displacement, this chapter also contributes to the existing displacement literature by analyzing the impact of using recently proposed methods that aim to correct for treatment effect heterogeneity (by cohort) on the empirical results. The majority of existing papers use a very similar method, based on an event-study framework with staggered treatment implementation, and the effects are generally estimated using two-way fixed effects. These methods are subject to the recent criticism of estimating (dynamic) treatment effects using two-way fixed effects specifications, most notably by Callaway and Sant'Anna (2020), Sun and Abraham (2020), and Borusyak et al. (2021).[5] In its essence, the issue is that the two-way fixed effects setup as described above does not allow for treatment effect heterogeneity, which is unfortunately quite likely to exist in the case of the long-run effects of job displacement.[6] Furthermore, because of the staggered timing of the treatment, the estimates obtained using two-way fixed effects can be shown to no longer exclusively reflect the effect of displacement in a certain period of interest. Rather, the estimate becomes a weighted average of (cohort-specific) treatment effects for several periods, where the weight can potentially even be negative.[7] Throughout the empirical section, I will primarily use the method from Sun and Abraham (2020), further discussed in section 1.2, which is shown to avoid these issues. I show how results on the long-run effect of displacement on earnings obtained using this method differ from those obtained using the

[3] For example, Kletzer (1998), Couch and Placzek (2010), and Von Wachter et al. (2009).

[4] Examples include Bonikowska and Morissette (2012) for Canada, Hijzen et al. (2010) for the United Kingdom, Deelen et al. (2018) for the Netherlands, Raposo et al. (2019) for Portugal, and Burda and Mertens (2001), Nedelkoska et al. (2015), and Schmieder et al. (2020) for Germany.

[5] There are many other papers highlighting the issues with these estimations, generally focusing on a setting where the researcher is interested in estimating a single (static) treatment effect, rather than a dynamic treatment effect.

[6] To give an example, it has been shown in the literature that the effects of job loss on earnings are more severe if a worker is laid of during a recession (see Davis and Von Wachter, 2011, for example). In other words, it would not be reasonable to assume that the dynamic treatment effect is homogeneous across cohorts, if we define cohorts as groups of workers displaced within the same year.

[7] Both Callaway and Sant'Anna (2020) and Sun and Abraham (2020) contain a more detailed description of the issue, and Sun and Abraham (2020) explicitly derived the decomposition of the two-way fixed effects estimate into cohort-specific estimated treatment effects. See these papers for a more elaborate discussion. The issue is also briefly discussed in Borusyak et al. (2021), who also discuss other issues such as the collinearity between the set of treatment leads and lags and the time fixed effects in the standard two-way fixed effects setup.

"traditional" methods. As a robustness exercise (in the appendix), I also show that similar results can be obtained using the method from Borusyak et al. (2021) instead. As I show in section 1.3, the differences between results obtained using the interaction-weighted method from Sun and Abraham (2020) and those obtained using a traditional two-way fixed effects method are quite striking: in the periods leading up to the event, the pre-trend that often appears under the traditional method no longer appear when using the interaction-weighted method. Furthermore, the recovery in both earnings and employment over time is much milder under the interaction-weighted estimator, thus suggesting that some of the recovery observed using the traditional two-way fixed effects methods reflect developments taking place in the control group instead.[8]

A final contribution of this chapter is to the growing literature analyzing the incidence and consequences of recalls, to which I contribute by examining subsequent earnings and employment outcomes of recalled workers. The topic of recall has been studied quite extensively, going back to studies such as Feldstein (1976) and Katz (1986). More recently, Nekoei and Weber (2015), as well as Nekoei and Weber (2020) have used detailed administrative data from Austria to shed more light on the topic of recall, in particular distinguishing between the expectation of recall and the actual materialization of recall. Similarly, studies like Hall and Kudlyak (2020) and Forsythe et al. (2020) have highlighted the unusually large role recalls play in labour market dynamics during the Covid-19 pandemic, especially in the early months. However, while the literature on the impact of recall on labour market flows is quite sizeable, the existing research generally does not comment on how recalled workers differ from non-recalled workers in terms of their subsequent earnings. In this chapter, I contribute to the research on this topic by investigating this dimension as well.

1.2 Data and Estimation Methods

Throughout this chapter (as well as the next chapter) I use two administrative datasets from the German Federal Employment Agency's (BA) Institute for Employment Research (IAB). In particular, I use the Linked-Employer-Employee Dataset (LIAB) and the Sample of Integrated Labour Market Biographies (SIAB). While these datasets mostly use data from a common source, and both contain information on the individual as well the (linked) establishment, the two datasets differ slightly in their length and sampling method.[9] The SIAB dataset draws a 2% random sample of the individuals from the Integrated Employment Biographies (IEB), after which the observations are matched with the relevant establishment data. The LIAB dataset, on the other hand, samples from the Establishment Panel and matches these establishments to individuals employed at these establishments (any time between 2002 and 2012). For all these individuals, the complete individual history is available (from the Integrated Employment Biographies). In other words, the SIAB dataset samples individuals whereas the LIAB dataset samples establishments. Furthermore, the SIAB dataset covers the period from 1975 to 2017, while the version of the LIAB dataset used in this chapter only covers the period from 1993 to 2014. Thus, while the two datasets are quite similar, it is valuable to use both as their respective sampling methods naturally lead to a different sample

[8] In particular, such a development would be workers in the control group being displaced in later years. In such a case, the recovery observed in the two-way fixed effects methods reflect earnings losses in the control group rather than recovery of the treatment group. Using the interaction-weighted method, such developments would not affect the results as these "later-displaced" workers would be in a different cohort of the treatment group rather than in the control group.

[9] In the data, an establishment is defined as all locations of a firm within a Kreis (municipality).

(especially of establishments). For example, looking at the summary statistics for establishments in appendix A.2, it becomes clear that SIAB dataset contains a relatively larger sample of large establishments.

Each observation in the original data (for both datasets) represents one spell of employment or non-employment, and is marked by a start and end date. These start and end dates are the dates at which the establishment (or social security administration) submits social security notifications, which either act as a yearly notification or signal a changed or ended employment relation. Using the establishment ID, as well as the observed reason for the social security notification, I then construct a yearly and quarterly linked employer-employee dataset, in which the establishment information is used from the establishment at which the individual was employed on the first day of the year/quarter.[10] Further restricting observations to those aged between 25 and 60 leads to a large dataset which nevertheless has some gaps in some workers' time series. These gaps occur because not all forms of employment or non-employment are recorded in the dataset. Among others, individuals are not observed if they are employed for the government, self-employed, or not receiving any social security benefits during nonemployment.[11] When constructing my main dataset, I fill these gaps for variables that can reasonably be interpolated (such as age and location), while leaving key information (such as earnings) missing, thus leading to these observation being omitted from estimation procedures. Table 1.1 summarizes the number of observations and individuals observed in the original data and the main analysis dataset (both quarterly and yearly).[12] Further summary statistics on both workers and establishments are presented in appendix A.1 and A.2.

	SIAB data		LIAB data	
Frequency	Yearly	Quarterly	Yearly	Quarterly
Raw observations	66,961,520	66,961,520	53,433,114	53,433,114
Observations (Age restricted)	52,162,319	52,162,319	43,001,421	43,001,421
Main Panel, Observations	24,183,133	79,771,399	25,848,195	76,886,425
Main Panel, Individuals	1,601,849	1,197,965	1,797,764	1,160,841

Table 1.1: *Number of observations and individuals in the raw dataset and main analysis datasets, using either LIAB or SIAB. The raw number of observations and age-restricted observations refer to the number of spells. In the main panel, these spells are collapsed to form yearly (or quarterly) observations.*

In order to analyze the consequences of displacement, I first need to be specific on how exactly I define displacement. For the purpose of estimating the specification described below, I define a worker as separated in some period t if this worker's employment spell with their establishment ends in period t. This means that the worker either no longer works for the same establishment in period $t + 1$ or returned to the establishment after being away for more than 31 days. Throughout, I drop workers who are trainees, casual workers, or partially retired workers, and further focus in particular on workers whose social security notification indicates that employment at the establishment was ended for a reason that could point to displacement.[13] I then define such a worker as displaced if the

[10] If the individual is non-employed at the start of the year/quarter (or employed at multiple establishments), the information is used for the establishment from which the individual has the highest earning in that period.

[11] Other reasons for not observing an individual include working (and moving) abroad.

[12] Note that the results discussed in the empirical section of this chapter are based on the yearly dataset only. In contrast, the quarterly dataset is the primary source used to calculate the moments that I use to calibrate the model in the next chapter.

[13] This way, I exclude apparent separations that are caused by paternity or maternity leave, disease, or seasonal patterns in employment.

establishment either closes or experiences a mass layoff.[14] Following the literature, an establishment is defined to experience a mass layoff if the employment at the establishment in the next period is at most 80% of the establishment's maximum employment over the previous five years, and the establishment has a net outflow of at least 20% of its workforce in the displacement year.[15] Finally, in order to determine whether a worker was recalled to their previous establishment, I look ahead at most 5 years after displacement. If the worker's first employing establishment after being displaced is the same as her employing establishment before displacement, I define the worker as recalled.[16]

The empirical results presented in the next section are largely based on the two specifications. The first of these specifications resembles the event study specification that was estimated in Davis and Von Wachter (2011):

$$e_{it}^y = \alpha_i^y + \gamma_t^y + \bar{e}_i^y \lambda_t^y + \beta^y X_{it} + \sum_{\substack{k=-5 \\ k \neq -1}}^{K} \delta_k^y D_{it}^k + u_{it}^y \tag{1.1}$$

In the equation above, i refers to the individual and t refers to the year (unless indicated otherwise). The dependent variable in this specification, e_{it} refers to the outcome variable of interest for individual i in period t. In most cases, this outcome variable is the individuals yearly earnings or the fraction of the year the individual spent in an employment relationship. Other outcome variables considered include the (yearly) job loss rate and the (yearly) average daily wage. The explanatory variables include an individual fixed effect α_i and a time fixed effect γ_t, as well as a quadratic polynomial in age X_{it} and an error term u_{it}. The variable $\bar{e}_i^y$ denotes the average earnings of individual i between years $y-5$ and $y-1$, and I will generally refer to this as recent earnings. When deriving these recent earnings, I condition of the individual having earnings available in the data for at least three of the years between $y-5$ and $y-1$, which must include year $y-1$.[17] The coefficients of interest are a series of coefficients on dummy variables D_{it}^k. These variables equal 1 if individual i was displaced in period $t-k$ (where the dummy variable for $k=-1$ is omitted). As these dummy variables always equal 0 for workers who did not get displaced, the coefficients represent the effect of displacement on earnings (relative to the earnings of non-displaced workers), k periods after displacement. The maximum number of future periods, K, is either 10 (when using LIAB) or 20

[14] I use an extension file that clarifies the reason for an establishment leaving the sample. In particular, I do not consider an establishment to be closed if a large portion of the workers at the establishment finds employment at a common establishment after the closure. After all, these events point towards a merger or the closure of a firm in one municipality only. See appendix A.2 for more details.

[15] For establishments with up to 20 employees, I use a threshold of 50% for both these conditions. However, as explained later in this section, these mass layoffs are generally not used for estimation purposes.

[16] Note that due to my definition of separation, I will miss unemployment spells of less than 31 days. As workers with such a spell would not be marked as separated, they can also not be defined as displaced (or recalled). Furthermore, a worker who is displaced from a closing establishment will always be in the non-recalled group of displaced workers. As I show in section 1.3.1, however, excluding these workers does not substantially alter the recall rate. Indeed, as I show in appendix A.3.6, excluding workers whose establishment closes from the estimation altogether does not change the results.

[17] I also use these recent earnings to generate the recent earnings distribution, which is generated separately for each year, age group, gender, and location. Here, the two age groups are prime-age (35 to 60) and young (below 35), and the two locations considered are East and West, corresponding to the locations formerly belonging to East and West Germany (with the exception of Berlin, which is classified as East in its entirety).

(when using SIAB).[18] The estimation is done separately for each sample year y.[19] Within each such estimation, only displacements that took place in year y are taken into account, thus implying that the dummy variable D_{it}^{k} will only equal to 1 if the individual i was displaced in period $t-k$ and this period $t-k$ corresponds to year y. Furthermore, only observations that correspond to years $y-5$ to $y+K$ are used. To enhance the interpretation of the estimated value, I then divide the estimated coefficient δ_k^y by the control group's average of the dependent variable (usually earnings) in year $y+k$, obtaining relative coefficient $\tilde{\delta}_k^y$. The standard displacement graph then plots the resulting relative coefficient $\tilde{\delta}_k$ over k (where $\tilde{\delta}_k$ is the average of $\tilde{\delta}_k^y$ over base years y), thus revealing an earnings path from 5 periods before to K periods after the displacement event.[20]

Recently, a number of papers have stressed the shortcomings of event study settings such as the one described above, in particular stressing that the estimates of δ_k^y may be contaminated by effects from earlier and later periods, as well as by subsequent and prior treatments that are ignored in this specification.[21] In fact, in the specification above individuals who are displaced in years $y+1$ and later, as well as individuals displaced before year y who are re-employed again (and satisfy other sample requirements) are likely to be placed in the control group. While the estimation described above is informative for the purpose of comparing my estimates with those found in previous work, I will therefore primarily focus on results based on a specification that takes these issues into account. As the specification (equation 1.2 below) does not allow for covariates, I will first estimate a trimmed version of specification (1.1), where the recent earnings and quadratic polynomial in age do not appear:[22]

$$e_{it}^{y} = \alpha_i^y + \gamma_t^y + \sum_{\substack{k=-5 \\ k\neq -1}}^{K} \delta_k^y D_{it}^k + u_{it}^y \tag{1.1'}$$

In order to take into account potential contamination of the estimate of δ_k^y (and consequentially of the average $\tilde{\delta}_k$), I use the interaction-weighted estimator from Sun and Abraham (2020). In practice, this means that I am estimating the following equation:

$$e_{it} = \alpha_i + \gamma_t + \sum_{C\neq 0} \sum_{\substack{k=-4 \\ k\neq -2}}^{K} \delta_k^C D_{it}^{C,k} + u_{it} \tag{1.2}$$

In the equation above, α_i and γ_t represent the person- and time fixed effects, and u_{it} is an error

[18] I chose a different value for K in the two datasets because setting $K = 20$ means that I require 25 years of data for every y. When using LIAB, this substantially restricts the number of years for which the estimation can be run. An alternative way of dealing with this would be to let K decrease as y increases. This would allow for more estimation years, but also could introduce some bias in the estimates for high values of k if the years with a lower K are also years where the long-run effect of displacement is stronger or weaker.

[19] Note that the similar estimation in Jacobson et al. (1993) and Couch and Placzek (2010) is not done separately by sample year because these papers focused on the effect of displacement in a specific year.

[20] An alternative to this method of estimating the relative earnings path is to estimate equation (1.1) using log earnings instead. I decided against this, as the data includes many observations with zero earnings, which I would need to omit in order to run this alternative estimation.

[21] See section 1.1.1 for a brief overview of these papers.

[22] Note that the method proposed in Callaway and Sant'Anna (2020) allows for covariates that do not vary over time, which would allow me to use the person's year of birth (but not age) to come closer to specification (1.1). This would be especially useful if the covariate in question (year of birth) is expected to influence not only the outcome, but also the probability of being displaced. If the only concern is the effect on the outcome (earnings), this effect would likely be absorbed by the individual fixed effect.

term. Similarly, the dependent variable, e_{it}, corresponds to the value of the outcome variable of interest for individual i in period t, like before. The main difference with equation 1.1' is that rather than estimating the equation for each base year separately, the above specification is only estimated once. However, the specification still allows for a different treatment effect (and different dynamics of this treatment effect) depending on which treatment cohort C the individual belongs to. In my estimation, the definition of the cohort C is equivalent to the base year y in which the individual is displaced, with $C = 0$ corresponding to the cohort of individuals who I do not observe being displaced at all. This "never-treated" group acts as the control group.[23] Furthermore, note that rather than omitting one value of k, I follow the discussion in Borusyak et al. (2021) by omitting two values of k. This is because generally the set of relative time indicators D_{it}^C is collinear with itself as well as with the time fixed effect. In order to allow for anticipation one period ahead, the first period I omit is $k = -2$ (rather than $k = -1$). The second omitted period is the earliest period, $k = -5$ (as reflected by the summation over k starting at $k = -4$). This period is chosen to maximize the distance between the two omitted periods, thereby making the resulting estimate less sensitive to any possible fluctuations (or trend) between these two periods.[24]

Estimation of equation (1.2) above will yield a set of estimates $\hat{\delta}_k^C$ for all $C \neq 0$ and $k \neq \{-5, -2\}$. These are then be averaged over C, using a weighted average that assigns to each pair (C, k) a weight equal to the number of observations with (C, k) divided by the number of observations of relative time period k (across cohorts). Since all coefficients δ_k^C are estimated in a single estimation procedure, I can then also form corresponding (pointwise) confidence intervals for the resulting weighted averages $\hat{\delta}_k$.[25]

When estimating the equations discussed above I partially follow the literature by restricting my sample to individuals with an establishment tenure (prior to displacement, if applicable) of at least 6 years (to ensure reasonable attachment to the labour force), and working at an establishment with at least 50 employees (to avoid classifying a job loss as a mass layoff when only a limited amount of workers loses their job). However, in my estimation I combine the data of male and female workers. In the appendix, I show how the results presented below are affected when changing one of these restrictions.

1.3 Empirical Results

In this section, I present the results generated from the data. In particular, I start by describing the incidence of separation, displacement, and subsequent recall, and how this differs by a number of observable characteristics of the worker. Then, I present the results for the average scarring effect of separation and displacement on earnings, using the specifications presented in section 1.2. Finally, I document heterogeneity in the scarring effect of displacement, focusing in particular on the importance of education level and (ex-post) recall status. All results in this section are generated using the SIAB dataset. However, the same analysis is also done using the LIAB dataset, and these

[23] In the appendix, I show how results are affected by instead using the "last-treated" group as the control group.

[24] Note that Borusyak et al. (2021) also propose an alternative estimation themselves. I show in appendix A.3.6 that using their method yields the same results as using the interaction-weighted estimator from Sun and Abraham (2020).

[25] In principle, one could construct confidence intervals for the coefficients that follow from specification (1.1) or (1.1') as well. However, in order to do so one would need to make a number of strong assumptions. For example, one would assume that there is no covariance between the estimates of δ_k^y for different values of y.

results can be found in the appendix. The conclusions made below hold for either dataset, although the results using the LIAB dataset are sometimes less convincing, potentially due to the smaller time period spanned by this dataset and its different sampling method.

1.3.1 The Incidence of Displacement and Recall

Before analyzing the detrimental effect displacement can have on a worker's earnings, and how this effect differs by observable characteristics, it is worth investigating how common a separation or displacement event (as well as subsequent recall) is. In order to do so, this subsection presents separation, displacement, and recall rates for the entire sample as well as several subsets of the sample.[26]

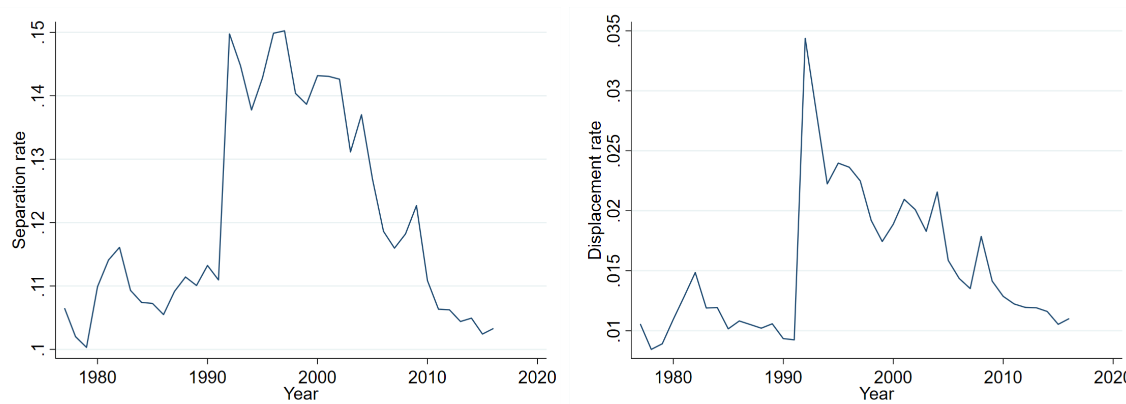


Figure 1.2: *The incidence of separation (left) and displacement (right) over time.*

First of all, figure 1.2 displays the separation and displacement rates over time. As can be seen in this figure, the separation averages roughly 12% whereas the displacement rate is roughly 1.5% on average. All rates display substantial variation over time, and in particular the aftermath of the German reunification in 1990 is quite clearly visible.[27] While separation and displacement rates tend to peak around recessions, the magnitude of these peaks are relatively small. For example, it can be seen that during the Great Recession, the separation and displacement rates increased but still remained below pre-2005 levels.

In figure 1.3, I plot the separation and displacement rates over time by education group, where education level is defined as (1) Non-University (low) or (2) University (high). As can be seen in the graph, workers with a relatively low education level tend to be more vulnerable to separation and displacement. Furthermore, with roughly 80% of the workers being categorized in the first group, the overall fluctuations of the separation and displacement rates observed in figure 1.2 primarily follow those of workers with a low education level.

[26]All graphs in this subsection are generated using the complete sample. In other words, I do not apply the restrictions on pre-displacement establishment tenure and establishment size used to generate the restricted sample on which I estimate equations (1.1) to (1.2). The corresponding graphs for this "restricted sample" can be found in appendix A.3.2.

[27]Note that workers from East Germany are generally not included in the data before the reunification, so therefore the jump in separation and displacement rates can also partially be explained as a composition effect. In appendix A.3.1, I show the separation and displacement rates over time for East and West Germany separately.

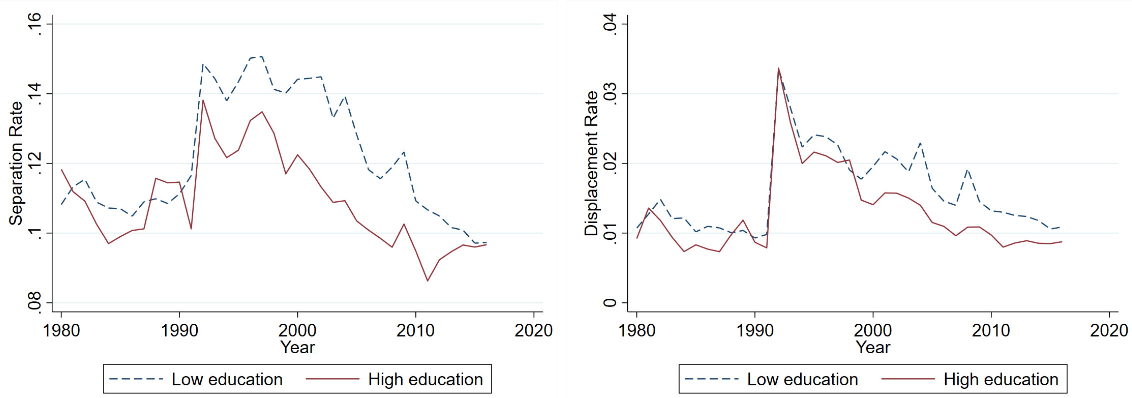


Figure 1.3: *The incidence of separation (left) and displacement (right) over time, by education level.*

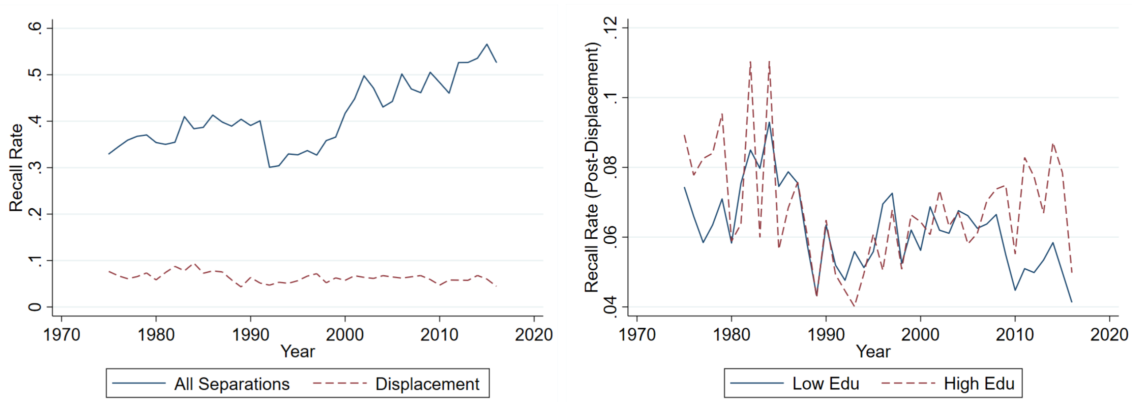


Figure 1.4: *Left: The incidence of recall within 5 years of job loss over time, unconditionally and conditional on displacement. Right: The incidence of recall within 5 years of job loss, conditional on displacement, over time and by education level.*

Next, figure 1.4 shows the incidence of recall (within 5 years), unconditionally or conditional on displacement. As can be seen in the figure, the unconditional incidence of recall is fairly high, and takes values between 30% and 55%, in line with observations from existing papers such as Fujita and Moscarini (2017) and Mavromaras and Rudolph (1998). The recall rate conditional on displacement is much lower, and fluctuates between 4.5% and 7% in recent decades. This indicates that generally roughly 6% of the workers who are displaced (notably including workers who are displaced as a consequence of their employing establishment shutting down) return to their previous employer. As can be seen in the right panel of figure 1.4, the recall rates (conditional on displacement) are fairly similar for the two education levels.

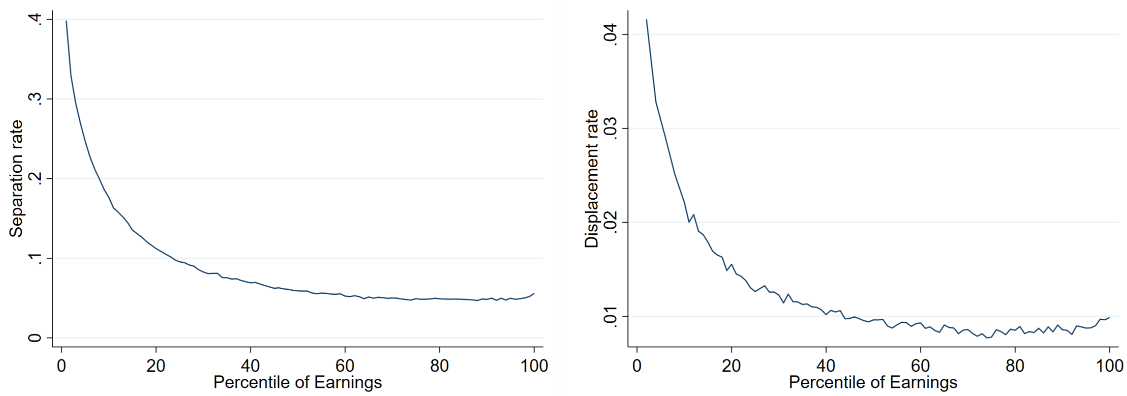


Figure 1.5: *The incidence of separation (left) and displacement (right) over the recent earnings distribution.*

As shown in figure 1.5, the separation and displacement rates in general tend to be higher for individuals located lower on the (recent) earnings distribution. This corresponds with the statement that higher quality matches in terms of productivity also tend to be more stable, as posited in Jarosch (2021), and therefore seems to support his idea of a job ladder with slippery bottom rungs (which I will use in my model in chapter 2 as well). However, it should be noted that the pattern in the data is not quite monotonic throughout the distribution: above the 80th percentile of the distribution, the displacement rates are slightly increasing again.

Figure 1.6 shows the incidence of recall (within 5 years) after displacement, over the recent earnings distribution. As can be seen in the figure, the recall rate (conditional on displacement) is consistently above 2.5% across the recent earnings distribution, and much higher towards the bottom of the distribution. This indicates that while recall is more prevalent for low earning workers, it is not a phenomenon exclusive to these workers. The recall rate itself may seem like a relatively low fraction, but given that workers likely follow a very different path after job loss if they expect to be recalled (as shown in the analysis below), it is important to consider these workers separately.

While the above analysis has highlighted some of the different worker characteristics that are associated with different rates of job loss that will appear throughout the remainder of the empirical section as well as in the model, it is likely that job loss rates also differ by other worker characteristics

Figure 1.6: *The incidence of recall within 5 years of displacement (as a fraction of total displacements), by percentile of the recent earnings distribution.*

or establishment characteristics. In appendix A.3.1, I further discuss the incidence of separation and displacement along some other dimensions of interest, whereas appendix A.3.2 discusses what happens to the displacement, separation, and recall rates if the sample is restricted in the same way as I restrict the sample for the estimation in the next sections and appendix A.3.3 shows incidence rates using the LIAB data instead of the SIAB data.

1.3.2 The Average Scarring Effect of Job Loss

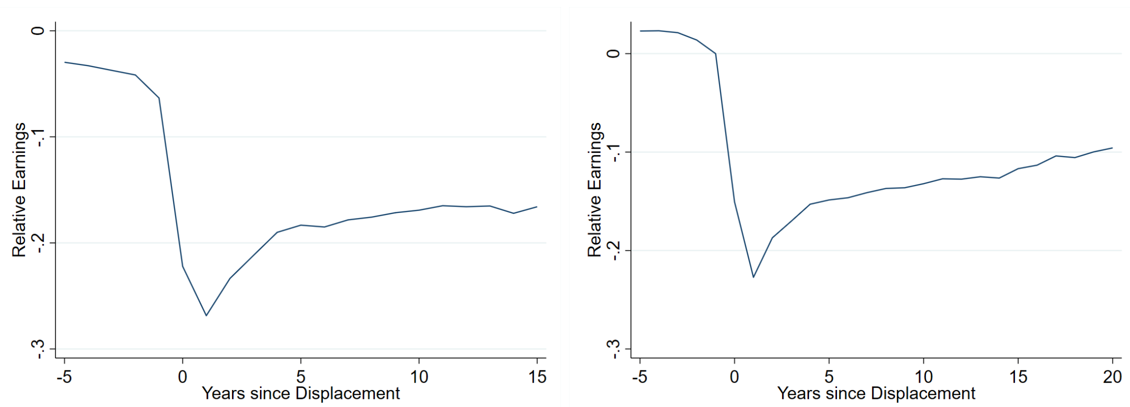


Figure 1.7: *Raw (left) or regression-based (right, using specification 1.1) average effect of displacement on earnings, relative to the control group of never-displaced workers.*

Having investigated the incidence of job loss across the sample, I will now move towards assessing the effects of displacement on earnings.[28] Before moving to the results of estimating equations (1.1) and (1.2), however, it is worth looking at the raw earnings losses first. The left panel of figure 1.7 presents these raw earnings losses. The differences shown in the graph are generated by calculating the difference between average earnings of the treatment and control group (from 5 years before to 15 years after the event), relative to the average earnings of the control group, separately for each

[28]The results presented here focus on displacement only. As shown in appendix A.3.4, the results continue to hold if I focus on separation instead.

base year, and averaging these differences over base years. As can be seen in the figure, the effect of job loss on earnings is quite substantial.[29] Further, it is worth noting that while there is some recovery over time, earnings remain substantially lower for the treatment group 10 to 15 years after the job loss event.

Of course, the raw comparison of earnings between displaced and non-displaced ignores many possible confounding factors, some of which may be unobserved. The right panel of figure 1.7 shows the results of estimating equation (1.1), again defining the treatment as displacement. In particular, it can be seen that in the short-run, workers who are displaced lose roughly 20% of their earnings.[30] This earnings loss is shown to be quite persistent, with these displaced workers still earning 10% less 20 years after the job loss took place.[31] These conclusions are in line with what has been observed in the literature, and confirm the observation of a large average scarring effect of displacement on earnings (though what I find here is on the lower end of the estimates found in existing work).

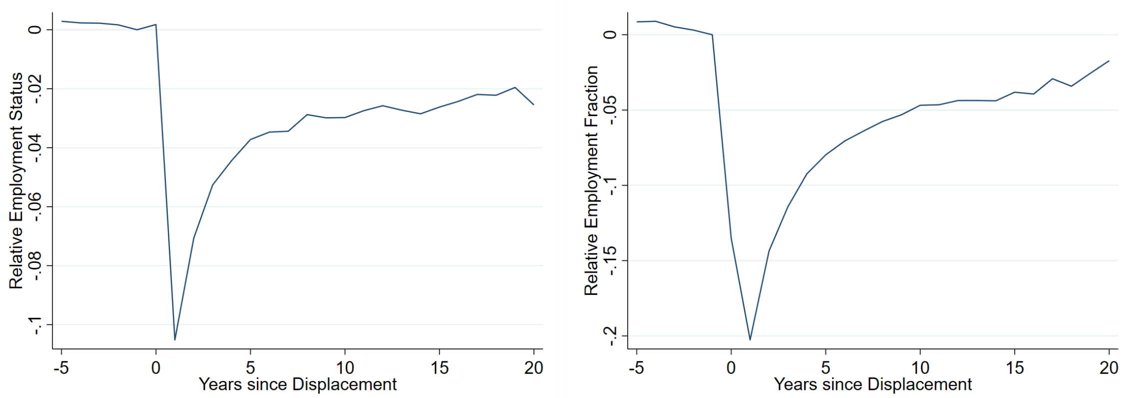


Figure 1.8: *The effect of displacement on employment status (left) and employment fraction (right), relative to the control group of never-displaced workers, using estimated coefficients from equation* (1.1).

As the left panel of figure 1.8 shows, the employment status of the displaced workers recovers much faster than earnings (though it does not recover completely), thus suggesting that a large proportion of the earnings loss may be explained by wages and intensive margin employment choices (working hours).[32] In particular, while the likelihood of being employed (at any point in the year) drops by 10% in the year after displacement, the decrease recovers to roughly 5% after only 3 years, and further recovers to roughly 3% in less than 10 years. If I look at the fraction of the year in which

[29]Note that in the setting of estimating earnings losses from equations (1.1) and (1.1'), as well as when calculating earnings losses directly from the raw data, the control group consists of workers who were employed and not displaced in the specific year for which the losses are calculated. This is slightly different from the control group used when estimating equation (1.2), which consists of all never-displaced workers (who are employed at least once).

[30]To be more precise, the numbers in the graph should be interpreted as earnings loss relative to the expected earnings the worker would have followed if they would not have been displaced (which is based on the trend of the control group). Since this trend is generally positive, the absolute earnings loss is likely larger than indicated in the graph.

[31]It should also be noted that the earnings start declining before the job loss actually takes place. This so-called decline appears in many of my estimates using specification (1.1), including those where I restrict workers in the control group to those who were working in the same establishments as the treated workers. Some of this could be explained by anticipation, or a so-called "Ashenfelter's dip", but for earlier years this is more likely to be a result of contamination of the estimates by other cohorts and years.

[32]The number of hours worked are not observed in the data beyond an indicator for full-time work, but evidence provided elsewhere in the literature, such as in Lachowska et al. (2020), suggests that the long-term earnings loss is mostly explained by wages.

the worker is employed, as done in the right panel of figure 1.8, a similar picture arises, though it should be noted that the effect of displacement on employment fraction is estimated to be stronger and more persistent than the effect on employment status. One potential explanation of this is that these displaced workers are more likely (than the control group) to be separated from their job in subsequent years, as illustrated by Jarosch (2021). If such a subsequent nonemployment spell is less than a year in duration, it would count towards the employment fraction but not toward the employment status (as the worker would still be employed at some point in the year).

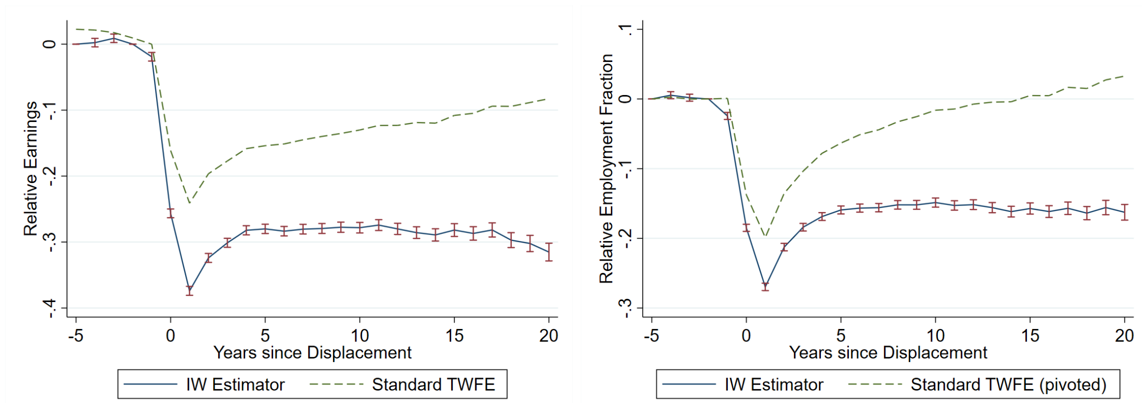


Figure 1.9: *The effect of displacement on earnings (left) and employment fraction (right), relative to the control group of never-displaced workers, using estimated coefficients from equation* (1.2) *(solid) or* (1.1') *(dashed). The error bars on the solid line correspond to 95% pointwise confidence intervals*

Finally, figure 1.9 shows how estimates of the average scarring effect of displacement on earnings and employment (fraction of the year employed) change when using the interaction-weighted estimator from Sun and Abraham (2020), as described in section 1.2, instead.[33] Comparing the estimates using that method to those obtained using equation (1.1'), included as dashed lines in the figures, reveals that the impact is especially large when estimating the long run effect, for both earnings and employment. As seen in the the figure, the estimates of the long-run effect are quite different: while the estimation of equation (1.1') suggests that employment fully recovers after roughly 15 years and earnings recover substantially, the interaction-weighted estimator reveals that the recovery stagnates after roughly 5 years, and employment remains roughly 15% below that of the control group while earnings losses remain at roughly 30%. This is quite a striking difference, and seems to suggest a larger role for employment in explaining the long-run effects of displacement than traditionally proposed in the literature, even if there is still a substantial role for wages as well.

When it comes to earnings, the left panel also shows a difference between the two estimators in the short run and before the event time. Especially the changed estimate in the years prior to displacement is encouraging, as it suggests that the pre-trend that is visible when estimating equations (1.1) and (1.1') may not be a genuine pre-trend, but rather an artifact of contamination by other cohorts and time periods, as discussed in section 1.2.

[33] In appendix A.3.4, I show that the results are similar when using the alternative method from Borusyak et al. (2021).

1.3.3 Heterogeneity in the Scarring Effect of Displacement

Unfortunately, the average effects in the previous subsection are not necessarily a good indicator for the earning losses a randomly chosen displaced worker can expect over the next number of years, as the average effects likely mask a substantial amount of heterogeneity. In order to improve such an indicator, one first needs to have a clearer view of how these average effects differ by a number of observable characteristics of the worker or the establishment they are displaced from. In this subsection, I will focus on two dimensions in particular, which inform the setup of the model: education level and ex-post recall status. However, the data allows me to look at many other characteristics of the individual as well as their (former) employer. In appendix A.3.6, I show that the results presented below are robust to considering some of these other characteristics.

1.3.3.1 Education Level

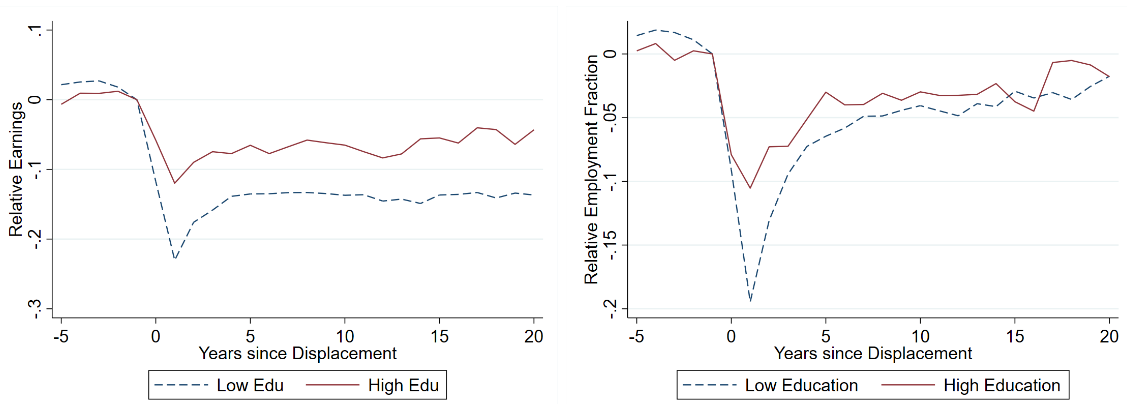


Figure 1.10: *The effect of displacement on earnings (left) and employment fraction (right), relative to the control group of never-displaced workers (by education group), using estimated coefficients from equation* (1.1).

One factor that one might argue to be important for an individual's earnings loss after displacement is the individual's educational background. In figure 1.10, I plot the results of estimating equation (1.1) when splitting the sample by education (non-University and University).[34] Comparing the two educational groups, it can be observed that workers with a relatively low education tend to suffer from higher earnings losses, both in the short and the long run. In the short run, this is likely partially driven by a larger initial effect on employment fraction, which suggests that workers with a high education level find a new job faster (on average). Indeed, comparing the two figures reveals that the recovery in the first few years following displacement is slightly faster for workers with a lower education, although this faster initial recovery only makes a minor difference for the differences between the two education groups in the long run.[35]

In figure 1.11, I show the results obtained by using the interaction-weighted estimator from

[34] Note that I split the sample by education group for both the treatment and control group. In other words, the effects in figure 1.10 are relative to workers in the same education group. In appendix A.3.6, I show how the results change if I don't restrict the control group to have the same education level.

[35] The result that workers with a lower education level suffer from larger earnings losses is consistent with what has been found in other work using similar data, such as Schmieder et al. (2020) and Burdett et al. (2020). Note that Burdett et al. (2020) split the sample into three education groups, and my "low education" group can be thought of as a combination of their "low" and "medium" education groups.

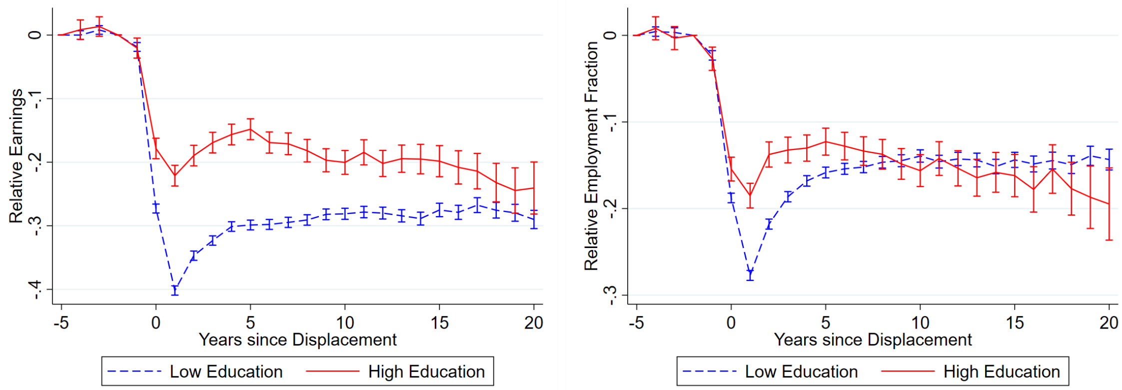


Figure 1.11: *The effect of displacement on earnings (left) and employment fraction (right) by education level, relative to the control group of never-displaced workers, using estimated coefficients from equation* (1.2)*. The error bars correspond to 95% pointwise confidence intervals.*

specification (1.2) instead. As can be seen, it still holds that the worker with a lower education level experiences higher earnings losses in the short run, but the two groups slowly converge, such that the difference earnings losses 20 years after the displacement event is much smaller than the initial difference. Furthermore, the magnitude of the losses is generally larger in figure 1.11 than in figure 1.10, consistent with what I found earlier for the average losses. In terms of employment, the results are also quite different from those in figure 1.10, although the comparison between the two education groups is similar: while workers with a lower education level still do worse in the short run, they exhibit some recovery over time, while the figure does not reveal much recovery for the highly educated workers (especially more than 5 years after the event). Indeed, 20 years after the displacement event, the worker with the lower education level seems to be doing better in terms of employment than the worker with the high education level.

1.3.3.2 Recalled Workers

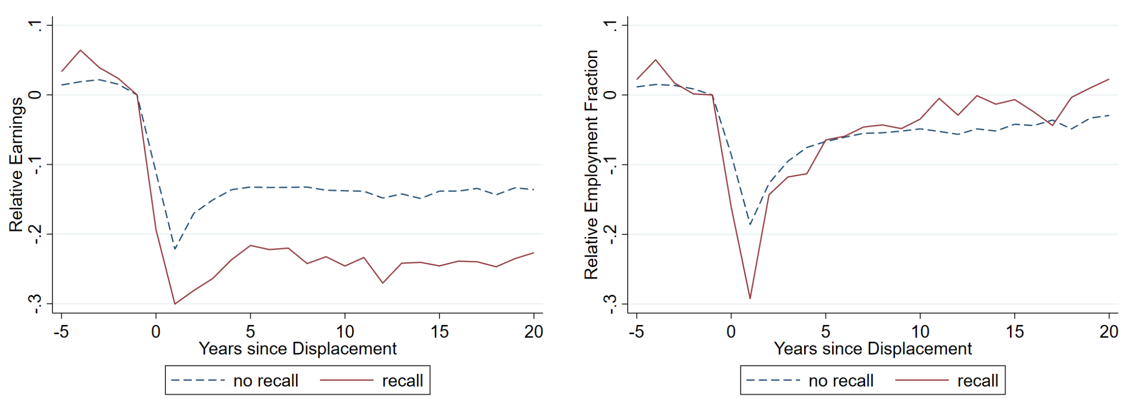


Figure 1.12: *The effect of displacement on earnings (left) and employment fraction (right) relative to the control group of never-displaced workers, by ex-post recall status (materialization of recall within 5 years), using estimated coefficients from equation* (1.1)*.*

One factor that the existing literature on the scarring effect of displacement (on earnings) generally abstracts from is the possibility of workers being recalled to their former employer. This makes sense for the (small portion of) displacement events where the establishment closes down, but for displacement in general a fairly sizeable fraction of workers ends up returning to their former employer, as shown in figures 1.4 and 1.6.

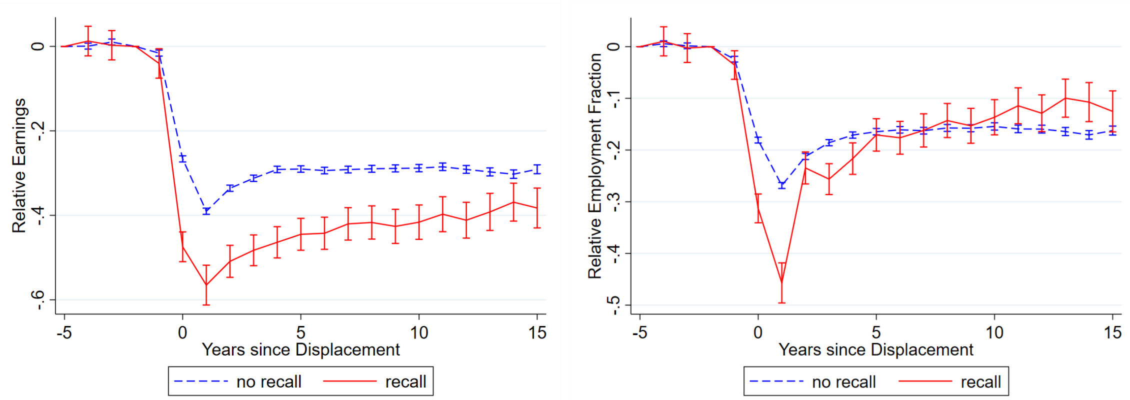


Figure 1.13: *The effect of displacement on earnings (left) and employment fraction (right) by ex-post recall status, relative to the control group of never-displaced workers, using estimated coefficients from equation* (1.2). *The error bars correspond to 95% pointwise confidence intervals.*

In figure 1.12, I show how the effects of displacement on employment and earnings differs by ex-post recall status.[36] As can be seen in the figure, workers who are recalled suffer from larger earnings losses (both in the short and in the long run), and do worse in the short run when it comes to days employed in the year, compared to the non-recalled (but still displaced) worker.[37] As shown in figure 1.13, this result also appears when using the interaction-weighted estimator from specification (1.2), although the difference between recalled and non-recalled is slightly larger in the short run and smaller in the long run, and once again the apparent pretrends (especially visible for recalled workers in figure 1.12) disappear.

Figure 1.14 provides a first step towards an explanation. As can be seen in the figure, recalled workers are much more likely to be separated again shortly after being recalled: while non-recalled workers are roughly 14 percentage points more likely to be separated than the control group one year after their initial displacement (and 11 percentage points more likely two and three years after displacement), recalled workers are more than 18 percentage points more likely to be separated again (compared to the control group) in the first three years after displacement. Thus, while recalled workers generally are re-employed faster after their initial displacement, they are also very likely to be separated again shortly, thus leading to less days worked in the year overall. This seems to indicate that workers who are recalled return to an unstable job, and I will use this insight in the next section to inform the setup of the model. Note that the result strengthens when I allow the estimation to also use workers who are displaced more than once according to my definition (i.e. they are displaced from high-tenure positions more than once), as shown in the right panel of figure

[36] As I do not observe whether a worker expects to be recalled, I divide workers according to whether or not a recall materializes within 5 years of the job loss. This may not exactly line up with whether a worker expected to be recalled, but given the correlation between the recall rate and the recall expectations (see e.g. Nekoei and Weber, 2015) it serves as a good proxy.

[37] At the same time, the recalled worker tends to be re-employed faster (not visible in the graphs), which may seem to generate a contradiction, but can be explained by the higher incidence of subsequent separation for recalled workers.

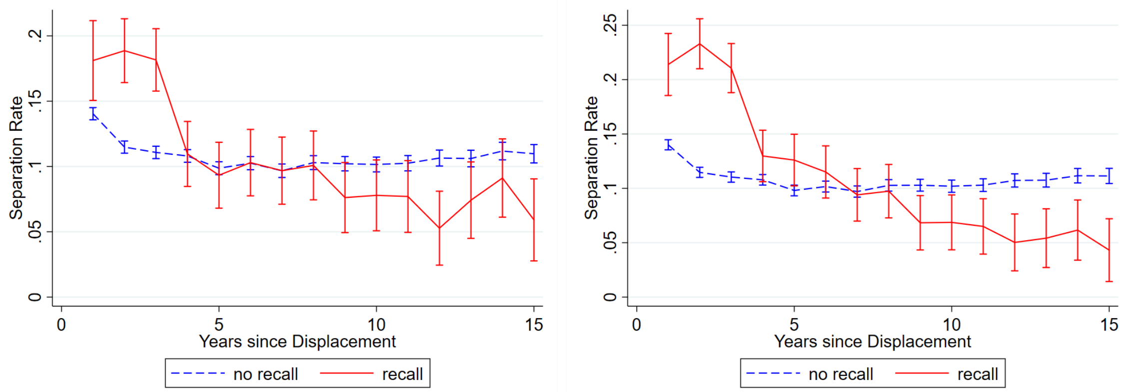


Figure 1.14: *The effect of displacement on separation rates by ex-post recall status, relative to the control group of never-displaced workers, using estimated coefficients from equation* (1.2). *The error bars correspond to 95% pointwise confidence intervals. Left: estimation allowing for only one displacement per individual; Right: estimation allowing for multiple displacements per individual (classifying the worker according to their first displacement). Only results from period* $k = 1$ *onwards are displayed here. The full graphs (starting from* $k = -5$*) are included in appendix* A.3.6.

1.14. This is also the case for the results on earnings and employment fraction, as illustrated in appendix A.3.6. Additionally, I show in appendix A.3.6 that these results generally continue to hold when the data is further restricted along observable dimensions such as traditionally seasonal industries, gender, and age group.

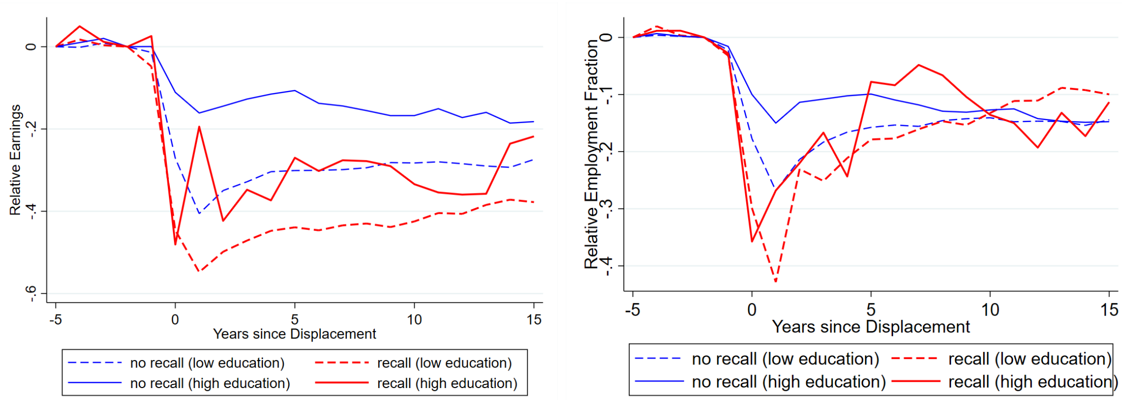


Figure 1.15: *The effect of displacement on earnings (left) and employment fraction (right) by ex-post recall status and education level, relative to the control group of never-displaced workers, using estimated coefficients from equation* (1.2). *The error bars corresponding to 95% pointwise confidence intervals are omitted and available upon request.*

As the left panel of figure 1.15 shows, the observation that recalled workers do worse in terms of earnings after displacement holds across the education levels considered earlier. However, it is worth noting that the difference in earnings loss between recalled and non-recalled workers is more volatile for the high education group. This is primarily because the highly educated recall group is fairly small in the SIAB data. These differences also arise when looking at the fraction of the years spent in employment, as shown in the right panel of the figure. Nevertheless, it can be seen that non-recalled workers with a low education level do slightly worse than their highly educated counterparts, but overall the comparison between recalled and non-recalled workers looks fairly similar for the two education levels.

1.4 Conclusion

In this chapter, I study the scarring effect of displacement on earnings and employment, focusing in particular on how these effects differ by whether workers are recalled to their previous employer. Using detailed administrative data from Germany, I find that while recalled workers tend to be re-employed faster than non-recalled workers, they suffer more in terms of their earnings. In the short run, this is driven by the recalled workers losing more days of employment (per year), due to a higher probability of subsequent job loss. In the long run, however, the difference between the recalled and non-recalled workers is driven by recalled workers experiencing a larger negative effect on wages (even though recalled workers do better than non-recalled workers in this dimension in the short run). Furthermore, I find that earnings losses tend to be higher for workers with a low education level.

Based on the results of this chapter, one can think of various avenues for future research, and I will highlight a few of those possibilities here. First of all, while this chapter's investigation is purely empirical in nature, the patterns unveiled in this chapter are not quite sufficient to provide a clear explanation of the seemingly larger scarring effect experienced by recalled workers. In order to provide such an explanation, a model of the labour market is needed which accounts for differences between recalled and non-recalled worker. In the next chapter, I propose such a model, and decompose the earnings loss observed in this chapter into a number of different channels. Secondly, this chapter focuses in particular on the dimension of ex-post recall status, but given the right data it would be interesting to further look into the differences between recall expectations and recall materialization (as emphasized by Nekoei and Weber, 2015), and its consequences for worker's earning paths after job loss.[38] Finally, there are several other dimensions of observable heterogeneity that show promising results and may be key to further improving the understanding of the heterogeneity in the scarring effects of job loss. One particular dimension that comes to mind is that of the industry in which the worker was (formerly) employed. In particular, one may think about what drives workers to switch industries after displacement and how closely this is related to patterns of structural change.

[38] As highlighted in the next chapter, I can use a model to generate a simulation-based analysis of this dimension, but I cannot verify this analysis in the data since I do not observe recall expectations in the data.

Chapter 2

Job displacement scars after a recall: A model-based decomposition

2.1 Introduction

In the previous chapter, I showed that recalled workers experience larger earnings losses than displaced workers who are not recalled, both compared to a control group of never-displaced workers, and that these larger earnings losses can be explained by employment in the short run and wages in the long run. In this chapter, I build on these results and offer a model-based explanation for the observations made in the German data.

In particular, I develop a job search model that is able to explain the larger earnings losses experienced by recalled workers, while still also matching the average scarring effect of displacement. In order to do so, I introduce a fixed worker type (which I interpret as the education level), and explicitly distinguish between workers who are expecting to be recalled and workers who are not (interpreting them as two different states). This is in addition to elements that have been used in existing models to explain the average scarring effect of displacement[1], such as human capital (which increases or decreases over time depending on employment status) and heterogeneity of firms by productivity and separation rates as in Jarosch (2021). As I allow recalled workers to follow a different path than non-recalled workers while non-employed (as well as afterwards), I can capture several possible explanations for the observed severity of the scarring effect of displacement for recalled workers, such as different transition rates and human capital depreciation rates.

I calibrate the model using the German administrative data that I also used in the previous chapter, though I do not explicitly target the estimation results from the previous chapter. I then use model simulations to decompose the difference in earnings loss between recalled and non-recalled displaced workers. I find that the differences in the long run are primarily driven by the instability of the job to which the worker is recalled, thus corresponding to my observation in the previous chapter that recalled workers are 6 to 10 percentage points more likely than a non-recalled worker to be

[1] See section 2.1.1 for a brief discussion of these models.

separated from their job again in the first few years following the initial displacement. Furthermore, I find that recalled workers experience a lower human capital depreciation rate and a higher job finding rate, which partially offsets the negative impact of their subsequent job losses.

When comparing the decomposition of a recalled worker's earnings loss to that of a non-recalled worker's earnings loss, I find that while human capital depreciation may be important to explain the long-run earnings loss for non-recalled workers, this is not the case for recalled workers (who experience a probability of human capital depreciation close to 0 during their initial nonemployment spell). This, in turn, suggests that while a policy targeting a worker's loss of human capital during nonemployment (such as a retraining program) may be successful in bringing down the long-term earnings losses for non-recalled workers, it will not be very effective in alleviating recalled workers' earnings losses. Indeed, I show that fully eliminating human capital depreciation in the model leads to a deterioration of the earnings losses faced by the recalled workers, while the earnings losses of non-recalled workers are slightly alleviated, thus suggesting that a policy achieving this would hurt the recalled workers. Since these recalled workers already face a larger relative earnings loss on average than the non-recalled workers, it can therefore be concluded that such a policy would not be desirable.

The rest of this chapter is organized as follows: After a brief overview of the related literature in the next subsection, section 2.2 then presents the model. Section 2.3 focuses on the estimation of the model, discussing the moments used for the estimation as well as the resulting estimates of the parameter values. Section 2.4 contains the quantitative analysis of the model, and shows that it recovers the heterogeneity observed in the data, and studies the drivers and implications of these observations. Finally, section 2.5 concludes.

2.1.1 Related Literature

This chapter contributes to the literature providing theoretical analysis of the long-term consequences of displacement, in particular by distinguishing between recalled and non-recalled workers. The theoretical analysis of the long-term consequences of displacement has only recently started gaining more attention, after Pries (2004) and Davis and Von Wachter (2011) noted that a standard job search model cannot generate the large losses observed in the data, even when expanding it with on-the-job search.[2] Some recent work has attempted to resolve this issue with some success. The paper closest to this chapter in terms of the model is Jarosch (2021), who proposed a model in which firms differ not only in terms of productivity, but also in the separation rate, thereby allowing for workers to experience several subsequent displacements after the initial one (as observed earlier by Stevens, 1997). This, combined with the presence of human capital which depreciates during unemployment (and increases while employed), enables him to reproduce the average earnings loss after displacement, both in the short and in the long run. Other models that have been successful in replicating the average earnings loss after displacement include Krolikowski (2017), Huckfeldt (2016), Jung and Kuhn (2019), Burdett et al. (2020), and Gregory et al. (2021).[3]

[2] See Pissarides (2000) for an example of such a standard job search model.

[3] The result in Huckfeldt (2016) that workers who switch occupations suffer from larger losses than workers who stay in their former occupation may seem to contradict my result that recalled workers do worse than non-recalled workers, but this is not quite the case. In particular, for workers to be considered recalled in my setting, they do not necessarily have to return in the same occupation. Indeed, throughout this chapter, as well as the previous chapter, I do not explicitly consider occupational switching after displacement, which can therefore be considered as one of the dimensions of heterogeneity that are still masked by some of my estimated effects (including those by ex-post recall

Similarly, I contribute to the theoretical analysis of recalls by explicitly distinguishing between nonemployed workers expecting a recall and nonemployed workers not expecting a recall. In my model, these two workers are in a separate state, thereby allowing for these workers to follow different paths in all outcome variables of interest (both during and after nonemployment). There does already exist a rather large body of literature that builds the possibility of recall into a model. Specifically, this strand of literature goes back to early work such as Feldstein (1976), Pissarides (1982), and Katz and Meyer (1990). More recently, recall has been explicitly modeled in Fujita and Moscarini (2017).[4] However, what all these papers have in common is that they focus exclusively on the impact of recall on labour market flows. As such, they refrain from commenting on how workers' earnings are affected by this possibility. Furthermore, the way most existing papers model recall is by considering the current job to be "paused" while the worker is unemployed. Until the recall materializes, they then make the same choices (such as search effort and accepting potential offers) as any other unemployed worker. In my model, this is not quite the case, as I make a sharp distinction between workers expecting to be recalled and other unemployed workers.

2.2 Model

In this section, I develop a search model of the labour market, with the aim of explaining some of the key heterogeneity I observed in chapter 1. In this discrete-time model, both firms and workers are heterogeneous along two dimensions.[5] Further, the model explicitly features the possibility of recall, as a separate state, reflecting my observation that workers who expect to be recalled face a substantially different earnings path. By allowing workers who are expecting a recall to be in a different state, I can account for differences between workers who are expecting or not expecting a recall, both after their nonemployment spell and during their nonemployment spell.

2.2.1 Environment

The economy is populated by workers and firms, both of which differ in two dimensions. Firms differ in their productivity y and separation risk δ, which will be summarized using a vector $\theta = [y, \delta]$. Workers differ in their human capital s and type ε, and can be either employed, unemployed, or nonemployed while expecting to be recalled. The type ε is fixed over time, whereas the human capital s can evolve over time. I will interpret the type ε as the worker's education when calibrating the model in section 2.3, but the way it is implemented in the model does not prevent it from being interpreted as some other fixed characteristic. The human capital increases by $\Delta_s(\varepsilon)$ (with probability ψ_e) when the worker is employed, and decreases by $\Delta_s(\varepsilon)$ when the worker is nonemployed (with probability ψ_u if unemployed or $\psi_r \psi_u$ when expecting a recall). This human capital can therefore be interpreted as being closely related to a worker's market experience.[6]

status).

[4] In the context of the Covid-19 pandemic, the possibility of recall is also explicitly modeled in Gregory et al. (2020) and Gallant et al. (2020).

[5] In particular, the model resembles Jarosch (2021) in that firms are heterogeneous with respect to their productivity and separation rate. However, in contrast to that model, workers are heterogeneous in two dimensions as well (rather than one), and the possibility of recall is explicitly featured in the model.

[6] The worker's human capital cannot go below s_{min}, so technically the probability ψ_u depends on s: If $s = s_{min}$, then $\psi_u = 0$. However, in practice s_{min} is set sufficiently low such that workers will only reach s_{min} in very rare instances (see appendix B.1).

2.2.1.1 Firms

Each firm can hire at most one worker.[7] If a firm is matched to a worker, production takes place according to the log-linear production function $p(s, y) = e^{s+y}$, and the firm pays a wage w to the worker, the determination of which is discussed in subsection 2.2.1.3. With (match-specific) probability δ, the match faces a separation shock. If this shock materializes, the match is destroyed, and with probability $(1 - \phi_{\varepsilon}^{f})$ the destruction shock is permanent, in which case the worker and firm return to an unmatched and unemployed status. However, with probability ϕ_{ε}^{f} the job destruction is potentially only temporary and the worker can choose to potentially be recalled.[8] Upon recall, nevertheless, the productivity of the match is reduced by c^f, such that the recalled match produces $p(s, y') = p(s, y) - c^f$ (where y' is restricted to be in the range of y). Furthermore, the separation rate attached to the firm (and therefore to the match) is increased by c^{δ}. The intuition behind the recall productivity penalty is that the firm is likely to incur costs for firing and re-hiring the worker as well as possible restructuring to survive the circumstances that lead to the layoff in the first place, which it will prefer to earn back (e.g. by lowering the worker's wage).[9] Furthermore, it can be seen in the data that recalled workers are more likely to be separated again within a year of being re-employed (see figure 1.14), thus reflecting that the worker returns to an unstable job. The penalty on the separation rate aims to reflect this directly. Finally, I assume that firms that are unmatched do not produce anything and also don't face any costs, thus setting the current period value of an unmatched firm equal to 0.

2.2.1.2 Workers

Workers are assumed to be infinitely-lived, and unable to transfer resources between periods. Further, their utility function is assumed to be logarithmic, and they discount future utility at a rate β. Each worker enters the market as unemployed and with the human capital s_{ε}. Their education type is determined prior to entering the labour market, corresponding to the sample restriction in the data where I did not consider workers below the age of 25 and/or workers who are still in school. An unemployed worker meets a firm with probability $\lambda_{\varepsilon}^{u}$, and this firm is drawn from the distribution $G_{\varepsilon}(\theta)$, where ε changes the marginal distributions of δ and y (see section 2.3), thus enabling different worker types to meet firms with different characteristics on average, but not restricting the range of δ to certain worker types.[10] If the worker meets a firm, the worker decides whether or not to accept the job. If the worker accepts, she becomes employed and receives wage w. If the worker does not accept, or does not receive an offer, the worker receives $b(s)$, which can be interpreted as the value of being unemployed (and is related to the unemployment benefit). It is a function of the worker's human capital as I set it equal to a fraction of the lowest possible production a worker could produce in a match: $b(s) = bp(s, y^{min})$. In doing so I proxy a setting in which the unemployment benefit depends on the last earned wage, while also not ruling out the scenario where unemployed

[7] Because the firm can only hire one worker, the model does not differentiate between firms, establishments, or jobs. In order to stay consistent with the literature, I will refer to the production entity as a firm, but when making the link with the data these entities can be thought of as establishments.

[8] With probability $\phi_{\varepsilon}^{rg}\phi_{\varepsilon}^{r}$, this recall takes place in the same model period as the initial displacement.

[9] Instead of explicitly lowering the wage, I chose to lower the productivity of the firm. In practice, this does not affect the wage in any different way.

[10] Additionally, a separated worker who moves into regular unemployment (regardless of whether this is by choice or not) finds a new job in the same period with probability $\lambda_{\varepsilon}^{ug}$.

workers reject some job offers.[11] Finally, it should be noted here that I do not explicitly model how the unemployment benefit is financed, though I can do so when I introduce counterfactual policies such as those suggested in section 2.4.3. Thus, I essentially assume that the government has exactly enough revenues to pay for the unemployment benefits and obtained this revenues from some outside source.

Naturally, an employed worker faces the same job destruction and recall shocks as the firm, and receives the wage w. Additionally, an employed worker meets another firm with probability λ_ε^e, and if she does the offer is again drawn from distribution $G_\varepsilon(\theta)$. Upon receiving such an offer, the employed worker can decide to switch to the new firm or to reject the offer. However, upon deciding to reject the offer, it can be used to re-bargain with the current employer.

Finally, if a match is temporarily destroyed and the worker is expecting to be recalled, she will receive $b(s)$ (just like the regular unemployed worker). While she is nonemployed and expecting a recall, the worker's human capital decreases by $\Delta_s(\varepsilon)$ with probability $\psi_r\psi_u$, reflecting that a worker expecting a recall may either experience faster or slower depreciation of human capital. In particular, one could argue that the depreciation is faster because the worker does not have to invest in knowledge needed to match with a new employer. However, it could also be argued that the depreciation is slower, since the worker already knows whom she might be employed by in the future, and therefore can keep her job-specific knowledge from depreciating.[12] The worker is recalled to her previous match with probability ϕ_ε^r every period. When the recall materializes, the wage is re-determined as if the worker is using the value of nonemployment as the outside option, and the firm characteristics change as described in subsection 2.2.1.1.[13] Furthermore, I allow the worker coming back from recall to face a slightly different wage setting process, as described in the next subsection. If the worker is not recalled in a period, she meets a new employer with probability $\lambda^r\lambda_\varepsilon^u$, where λ^r is expected to be below 1 (but not restricted as such). If the worker meets a new employer, this employer is again drawn from distribution $G_\varepsilon(\theta)$, and the worker can decide whether to accept the offer (leading to a wage w). Finally, if the worker does not get recalled and also does not meet a new firm (or rejects the offer from the new firm), she can decide move to the regular state of unemployment, thus giving up the potential recall.

2.2.1.3 Wage Setting

In determining the wages, I follow a similar procedure to Bagger et al. (2014). At the time of bargaining the worker and firm agree on a piece-rate $R = e^r$, and the worker receives a wage of $w = Rp(s, y) = e^{r+s+y}$ until either the match is destroyed (because of separation or because the worker switches firms) or until the worker receives an offer that triggers re-bargaining.

When the worker and the firm meet, the piece rate is determined using the maximum surplus a worker could extract from the match and the maximum surplus that could be extracted from the outside option. In practice, this maximum surplus equals the value function of the worker if the

[11] In the case where $b = 1$, this unemployment benefit is very similar to the one seen in Bagger et al. (2014). In particular, the lower the value of the parameter b is, the lower the value of being unemployed is, and therefore the more job offers will be accepted. In particular, there exists a threshold b, which depends on job offer rates λ_ε^u and λ_ε^e, such that the unemployed worker accepts any job offer, as in the model in Bagger et al. (2014).

[12] As she is not physically in the workplace, it is likely that she will not be able to increase her knowledge like she would if she were to be employed (as she cannot accumulate any experience in practice).

[13] The loss of the outside option is a simplifying assumption, but is justified by the fact that the worker did not exercise this outside option upon being displaced, so that the firm may no longer consider the threat of leaving to accept this outside offer to be credible.

piece-rate R is set equal to 1 (or $r = 0$), and I denote this value as W^{max}. The piece-rate is set such that the surplus extracted by the worker (W) equals the maximum surplus she could extract from her outside option, plus a constant fraction of the excess maximum surplus of the pending match. This fraction, κ, is interpreted as the bargaining power of the worker. Denoting the maximum surplus from the outside option by W^{oo}:

$$W_\varepsilon(s, s, \theta, \hat{\theta}) = W^{oo} + \kappa\left(W_\varepsilon^{max}(s, \theta) - W^{oo}\right) \tag{2.1}$$

Here, it is explicitly taken into account that in general the match value for the worker, W, depends on the value of the firm characteristics θ, the outside option firm characteristics $\hat{\theta}$, and the worker's human capital, both current (s) and when the worker and firm last bargained ($\hat{s}$).[14] Note that equation (2.1) can take four distinct forms. First, if the worker is coming out of (regular) unemployment, the outside option value W^{oo} equals the value of unemployment, $U_\varepsilon(s)$ and $\hat{\theta} = u$. Then, denoting by x the firm characteristics of the worker's new firm, equation (2.1) can be rewritten as equation (2.2).

$$W_\varepsilon(s, s, x, u) = U_\varepsilon(s) + \kappa\left(W_\varepsilon^{max}(s, x) - U_\varepsilon(s)\right) \tag{2.2}$$

$$W_\varepsilon(s, s, x, \theta) = W_\varepsilon^{max}(s, \theta) + \kappa\left(W_\varepsilon^{max}(s, x) - W_\varepsilon^{max}(s, \theta)\right) \tag{2.3}$$

$$W_\varepsilon(s, s, \theta, x) = W_\varepsilon^{max}(s, x) + \kappa\left(W_\varepsilon^{max}(s, \theta) - W_\varepsilon^{max}(s, x)\right) \tag{2.4}$$

$$W_\varepsilon(s, s, \theta, r) = \max\{U_\varepsilon(s), T_\varepsilon(s, \theta)\} + \kappa^r\left(W_\varepsilon^{max}(s, \theta') - \max\{U_\varepsilon(s), T_\varepsilon(s, \theta)\}\right) \tag{2.5}$$

If the worker is moving between two jobs, from a firm with characteristics θ to a firm with characteristics x, the outside option W^{oo} equals the maximum surplus that could have been obtained at her previous job, $W_\varepsilon^{max}(s, \theta)$, so that equation (2.1) can be rewritten as equation (2.3). If the worker is using a job offer from a firm with characteristics x to extract more value from her current employer, the outside option W^{oo} equals the maximum surplus that could have been obtained from this job offer, $W^{max}(s, x)$, and equation (2.1) can be rewritten as equation (2.4). Finally, if the worker is being recalled, the determination of the worker's surplus is very similar to that of a worker being hired from unemployment (equation 2.2), but the recalled worker uses a different bargaining weight κ^r, and uses the maximum of the value of unemployment $U(s)$ and the value of nonemployment while expecting a recall, $T(s, \theta)$, thus reflecting that upon rejecting the offer, the worker can choose to give up the potential recall and move to regular unemployment.[15] Furthermore, since the maximum value obtained from the match changed due to the penalties on production and separation rate, the firm characteristic that is relevant for the determination of the maximal surplus obtained from the recall is not quite the same as the previous characteristic (as denoted by using θ' rather than θ).

2.2.2 Timing and Value Functions

To summarize the setup of the model, every model period can be divided into 4 stages. At the start of the period, in the first stage, the human capital level of the workers is updated. Then, in the

[14] At the time of bargaining, the human capital "when the worker and firm last bargained" ($\hat{s}$) is set equal to the current human capital (s), so in equations (2.1) to (2.4) I set $\hat{s} = s$.

[15] The value of the recalled worker's bargaining weight, κ^r, is expected to be lower than that of other workers (κ), reflecting that this worker may not be able or willing to find a different employer and thus does not have a very strong bargaining position when entering wage bargaining with the recalling firm. This may strengthen the negative effect of losing the outside offer.

second stage, recall materialization, separation, and recall choice takes place.[16] Then, in the third stage, workers who started the period as unemployed or employed (and are still in the same state) may receive an offer from a firm, after which they choose to accept or reject it, (re-)bargaining takes place, and workers expecting a recall may choose to move to permanent unemployment. Finally, at the end of the period, production takes place and wages (and unemployment benefits) are paid out. Using the above description, I can write out the value functions of the worker and the firm. In particular, I write out these value functions from the viewpoint of a worker/firm at the end of the period (before the start of the production stage). First, the value of unemployment U for a worker of type ε with human capital s can be written out as follows:

$$\begin{aligned} U_\varepsilon(s) = \ln(b_\varepsilon(s)) + \beta \mathbb{E}_{s'|s,u,\varepsilon} \Bigg\{ & \lambda^u_\varepsilon \int_{x \in \Theta^u_\varepsilon(s')} W_\varepsilon(s', s', x, u) dG_\varepsilon(x) \\ & + \left(1 - \lambda^u_\varepsilon \int_{x \in \Theta^u_\varepsilon(s')} dG_\varepsilon(x)\right) U_\varepsilon(s') \Bigg\} \end{aligned} \tag{2.6}$$

Here, the set $\Theta^u_\varepsilon(s)$ is the set of firm characteristics of the firms from whom the worker of type ε would accept an job offer if her current human capital level is s. Using equation (2.2), this set can be specified as $\Theta^u_\varepsilon(s) = \{x \in [0,1] \times \mathbb{R}_+ : W^{max}_\varepsilon(s,x) \geq U_\varepsilon(s)\}$.

As shown in appendix B.2, equation (2.6) can be rewritten in terms of W^{max}, U, and parameters only:

$$U_\varepsilon(s) = \ln(b_\varepsilon(s)) + \beta \mathbb{E}_{s'|s,u,\varepsilon} \left\{ \lambda^u_\varepsilon \int_{x \in \Theta^u_\varepsilon(s')} \kappa \left(W^{max}_\varepsilon(s', x) - U_\varepsilon(s') \right) dG_\varepsilon(x) + U_\varepsilon(s') \right\} \tag{2.7}$$

Similarly, the value function T for a worker of type ε with human capital s, expecting to be recalled to a job of (former) type $\theta = [\delta, y]$, is as follows:

$$\begin{aligned} T_\varepsilon(s, \theta) = \ln(b_\varepsilon(s)) + \beta \mathbb{E}_{s'|s,r,\varepsilon} \Bigg\{ & \phi^r_\varepsilon W_\varepsilon(s', s', \theta', r) + (1 - \phi^r_\varepsilon) \lambda^r \lambda^u_\varepsilon \int_{x \in \Theta^r_\varepsilon(s', \theta)} W_\varepsilon(s', s', x, f) dG_\varepsilon(x) \\ & + (1 - \phi^r_\varepsilon) \left(1 - \lambda^r \lambda^u_\varepsilon \int_{x \in \Theta^r_\varepsilon(s', \theta)} dG_\varepsilon(x)\right) \max\{T_\varepsilon(s', \theta), U_\varepsilon(s')\} \Bigg\} \end{aligned} \tag{2.8}$$

Here, $W_\varepsilon(s', s', \theta', r)$ is as defined above, and $W_\varepsilon(s', s', x, f)$ denotes that a worker finding a new job while expecting to be recalled may use either the value of unemployment or the value of nonemployment while expecting a recall as their outside option, thereby also allowing for the set of accepted offers $\Theta^r_\varepsilon(s', \theta)$ to be slightly different from the corresponding set for an unemployed worker ($\Theta^u_\varepsilon(s')$). Note that since the worker loses her outside option upon separating (even if the separation is temporary), the value function T does not depend on $\hat{s}$ or $\hat{\theta}$. Further, note that $\theta' = [\delta', y']$, where $\delta' = \delta + c^\delta$ and y' is the maximum of y^{min} (the lower bound of the range of y) and y' such that $p(s, y') = p(s, y) - c^f$. Finally, I allow for the depreciation rate of human capital to be different for the worker expecting to be recalled. However, I do not make any assumption on whether the human

[16] Note that by recall choice, I mean only the choice a worker faces when confronted with a separation shock that may not be permanent. As I assume that the worker cannot choose to transition to permanent unemployment from the temporary unemployment state until the recall materialization shock ϕ^r_ε and job search is realized, this second type of recall choice does not take place until the end of the third stage.

capital depreciation occurs faster or slower for a worker expecting a recall.
Just like value function $U_\varepsilon(s)$, this value function $T_\varepsilon(s,\theta)$ can be rewritten using the bargaining equations (2.2) and (2.5):

$$T_\varepsilon(s,\theta) = \ln(b_\varepsilon(s)) + \beta\mathbb{E}_{s'|s,r,\varepsilon}\Bigg\{\phi^r_\varepsilon\kappa^r W^{max}_\varepsilon(s',\theta') + \phi^r_\varepsilon(1-\kappa^r)\max\{T_\varepsilon(s',\theta),U_\varepsilon(s')\}$$
$$+(1-\phi^r_\varepsilon)\left(\lambda^r\lambda^u_\varepsilon\int_{x\in\Theta^r_\varepsilon(s',\theta)}\kappa\left(W^{max}_\varepsilon(s',x)-\max\{T_\varepsilon(s',\theta),U_\varepsilon(s')\}\right)dG_\varepsilon(x)+\max\{T_\varepsilon(s',\theta),U_\varepsilon(s')\}\right)\Bigg\} \tag{2.9}$$

The value of employment W for a worker of type ε with human capital s, matched with a firm of type θ, is as specified below:

$$\begin{aligned} W_\varepsilon(s,\hat{s},\theta,\hat{\theta}) &= \ln(R_\varepsilon(\hat{s},\theta,\hat{\theta})p(s,y)) + \beta\mathbb{E}_{s'|s,e,\varepsilon}\Bigg\{\delta\left[\phi^f_\varepsilon\max\left\{\hat{T}_\varepsilon(s',\theta),\hat{U}_\varepsilon(s')\right\}+(1-\phi^f_\varepsilon)\hat{U}_\varepsilon(s')\right] \\ &+(1-\delta)\Bigg[\lambda^e_\varepsilon\left(\int_{x\in\Theta^1_\varepsilon(s',\theta)}W_\varepsilon(s',s',x,\theta)dG_\varepsilon(x)+\int_{x\in\Theta^2_\varepsilon(s',\hat{s},\theta,\hat{\theta})}W_\varepsilon(s',s',\theta,x)dG_\varepsilon(x)\right) \\ &+\left(1-\lambda^e_\varepsilon\int_{x\in\Theta^1_\varepsilon(s',\theta)\cup\Theta^2_\varepsilon(s',\hat{s},\theta,\hat{\theta})}dG_\varepsilon(x)\right)W_\varepsilon(s',\hat{s},\theta,\hat{\theta})\Bigg]\Bigg\} \end{aligned} \tag{2.10}$$

Here, I denote by $\hat{s}$ the value of human capital at the time of the most recent bargaining. Similarly, $\hat{\theta} \in \{[0,1]\times\mathbb{R}_+, u, r, f\}$ represents the firm characteristics corresponding to the job offer that was used for bargaining.[17] The set $\Theta^1_\varepsilon(s,\theta)$ is the set of firm characteristics of the firms from whom the worker (of type ε and with human capital s) would accept an job offer if she is currently employed at a firm with characteristics θ, and $\Theta^2_\varepsilon(s,\hat{s},\theta,\hat{\theta})$ is the set of firm characteristics of the firms whose offers this worker would use to trigger re-bargaining at her current match. Using equations (2.3) and (2.4), these sets can be specified as $\Theta^1_\varepsilon(s,\theta) = \{[0,1]\times\mathbb{R}_+ : W^{max}_\varepsilon(s,x) \geq W^{max}_\varepsilon(s,\theta)\}$ and $\Theta^2_\varepsilon(s,\theta) = \{x\in[0,1]\times\mathbb{R}_+ : W^{max}_\varepsilon(s,\theta) > W^{max}_\varepsilon(s,x) \geq W^{max}_\varepsilon(\hat{s},\hat{\theta})\}$.[18] Note that the values $\hat{T}$ and $\hat{U}$ correspond to the value of a newly separated worker who chose to either potentially be recalled or move into unemployment. These values reflect the possibility of these workers being re-employed in the same period, and therefore relate to value functions (2.9) and (2.7) above as follows:

$$\hat{T}_\varepsilon(s',\theta) = \phi^{rg}_\varepsilon\phi^r_\varepsilon W_\varepsilon(s',s',\theta',r) + (1-\phi^{rg}_\varepsilon\phi^r_\varepsilon)T_\varepsilon(s',\theta) \tag{2.11}$$
$$\hat{U}_\varepsilon(s') = \lambda^{ug}_\varepsilon\int_{x\in\Theta^u_\varepsilon(s')}W_\varepsilon(s',s',x,u)dG_\varepsilon(x) + \left(1-\lambda^{ug}_\varepsilon\int_{x\in\Theta^u_\varepsilon(s')}dG_\varepsilon(x)\right)U_\varepsilon(s') \tag{2.12}$$

Using equation (2.10), the value for W^{max} can be deduced for every combination of ε, s and θ, by setting $R_\varepsilon(\hat{s},\theta,\hat{\theta}) = 1$. The resulting expression, which is derived in appendix B.2.3, no longer depends on the bargaining benchmark, as the outcome of the bargaining (which is the piece-rate) is

[17] If a worker comes out of unemployment, she does not have such a job offer to use for bargaining, and uses the value of unemployment instead. With some abuse of notation, I denote this by setting $\hat{\theta} = u$. Similarly, I denote the setting for workers being recalled as $\hat{\theta} = r$ and workers finding a new job while expecting a recall as $\hat{\theta} = f$.

[18] Note that the two sets $\Theta^1_\varepsilon(s,\theta)$ and $\Theta^2_\varepsilon(s,\hat{s},\theta,\hat{\theta})$ do not overlap. Further, together they do not cover all possible values of $x \in [0,1]\times\mathbb{R}_+$, revealing the third possible result of receiving an outside offer: if the offer is not good enough for the worker to use to trigger re-bargaining, the worker discards the offer and remains employed under her previously bargained piece-rate.

already known:

$$W_\varepsilon^{max}(s,\theta) = \ln(p(s,y)) + \beta \mathbb{E}_{s'|s,e,\varepsilon}\left\{\delta\left[\phi_\varepsilon^f \max\left\{\hat{F}_\varepsilon(s',\theta), \hat{U}_\varepsilon(s')\right\} + (1-\phi_\varepsilon^f)\hat{U}_\varepsilon(s')\right]\right.$$
$$\left. +(1-\delta)\left[\lambda_\varepsilon^e \int_{x\in\Theta_\varepsilon^1(s',\theta)} \kappa\left(W_\varepsilon^{max}(s',x) - W_\varepsilon^{max}(s',\theta)\right) dG_\varepsilon(x) + W_\varepsilon^{max}(s',\theta)\right]\right\} \tag{2.13}$$

On the firm side, one could also set up a value function of a producing firm. However, since the above equations are sufficient to solve the model (for a given set of parameters), these value functions as well as the flow equations are deferred to the appendix (see appendix B.2.1 and B.2.2).

2.2.3 Equilibrium

In this model economy, an equilibrium consists of value functions $U_\varepsilon(s)$, $W_\varepsilon(s,\hat{s},\theta,\hat{\theta})$, $T_\varepsilon(s,\theta)$, $J_\varepsilon(s,\hat{s},\theta,\hat{\theta})$, and a piece-rate function $R_\varepsilon(\hat{s},\theta,\hat{\theta})$, such that, given distribution $G_\varepsilon(\theta)$ and parameters, the value functions $W_\varepsilon(s,\hat{s},\theta,\hat{\theta})$ and $U_\varepsilon(s)$ satisfy equations (2.2) to (2.5), the value functions and the piece-rate function satisfy equations (2.6) to (2.13) and equation (B.7), and the distribution of workers across different states evolves according to equations (B.10) to (B.15).

2.3 Calibration

For the purpose of the calibration, I set up the distribution of firms $G_\varepsilon(\theta)$ as a combination of marginal distributions of productivity y and separation rate δ, and I make parametric assumptions on these marginal distributions. In particular, I assume that the marginal distribution of δ is a Beta distribution with parameters η_δ and $\mu_{\delta,\varepsilon}$, reshaped to the $[0, 0.25]$ interval (rather than $[0, 1]$), whereas the marginal distribution of y is a Pareto distribution with scale parameter $\mu_{y,\varepsilon}$ and shape parameter η_y. I then follow Jarosch (2021) in combining the two marginal distributions into the bivariate distribution $G_\varepsilon(\theta)$ using Frank's copula with parameter ρ (thereby allowing for correlation between the two variables). Finally, as alluded to earlier in this paper, I will interpret the worker type ε as the education level. In line with the discussion in chapter 1, I therefore allow for two worker types.

As table 2.1 shows, these assumptions lead me to a total of 35 parameters that need to be identified. Of these 35 parameters, I will set 5 parameters exogenously, leaving the remaining 30 parameters to be estimated using the indirect inference method from Gourieroux et al. (1993).[19] In the next two subsections, I describe how I set the 5 exogenous parameters, and which moments I use to identify the remaining 30 parameters. The discussion in these two subsections is summarized in tables 2.2 and 2.3, and a more detailed description of the estimation of these moments (both in the data and in the model simulation) can be found in appendix B.1.

[19]Note that most of the elements of the calibration method are reminiscent of a simulated method of moments approach, which is nested in the indirect inference approach from Gourieroux et al. (1993). However, given the use of an auxiliary regression estimation for one of the moments, it is more appropriate to classify it as the more general indirect inference method.

Parameter	**Meaning**
β	discount factor
ϵ_ε	distribution of worker types ε
κ	worker's bargaining power
κ_r	worker's bargaining power upon recall
b	unemployment benefit, fraction of minimum production
ψ_e	human capital transition, employment
ψ_u	human capital transition, non-employment
ψ_r	human capital transition, recall relative to non-employment
s_ε	starting value of human capital
$\Delta_s(\varepsilon)$	human capital transition size
$\mu_{\delta,\varepsilon}$	1st shape parameter, marginal distribution of δ
η_δ	2nd shape parameter, marginal distribution of δ
η_y	shape parameter, marginal distribution of y
$\mu_{y,\varepsilon}$	scale parameters, marginal distribution of y
ρ	copula parameter
λ_ε^u	meeting probabilities, unemployed workers
λ^r	relative meeting probability, workers expecting a recall
λ_ε^{ug}	meeting probabilities, newly unemployed workers
λ_ε^e	meeting probabilities, employed workers
ϕ_ε^f	probability of recall
ϕ_ε^r	recall materialization probability
ϕ_ε^{rg}	immediate recall materialization probability
c^f	production penalty of recall
c^δ	stability penalty of recall

Table 2.1: *A summary of all parameters in the model to be set exogenously or to be calibrated. Note that any notation with a subscript ε represents two parameters: one for each worker type ε.*

2.3.1 Exogenously Set Parameters

As I interpret ε to correspond to the worker's education level, it makes sense to set the distribution of ε so that the fraction of workers in each education group corresponds to the accompanying fractions found in the data. As such, following the definitions of the education groups used in chapter 1, I set the fraction of workers with education levels 1 and 2 to equal 0.84 and 0.16 respectively.

Parameter(s)	**Value(s)**	**Source**
β	0.98726	5% annual interest rate
s_1	0	normalization
$\Delta_s(1)$	0.1	normalization
ϵ_1	0.84	fraction of workers with education level 1
ϵ_2	0.16	fraction of workers with education level 2

Table 2.2: *A summary of all exogenously set parameters*

Furthermore, as one model period corresponds to one quarter, I set the discount rate $\beta = 0.95^{1/4}$ to reflect an annual interest rate of 5%, and I set $s_1 = 0$ and $\Delta_s(1) = 0.1$ as a normalization, so that the values of human capital coming out of the simulation can be interpreted as relative to the human capital of a worker with education level 1 entering the labour market (s_1), and step-sizes in this human capital can be interpreted as relative to the step-size of a worker with low education ($\Delta_s(1)$). Table 2.2 summarizes the values of the exogenously set parameters, and the sources used to set these values.

2.3.2 Calibration Moments

Using that I interpret ε to correspond to education levels, I next identify 45 moments that together identify the values of the 30 parameters that I calibrate using the indirect inference method from Gourieroux et al. (1993). While the parameters are estimated simultaneously, I divide the parameters into six groups, and I argue that each of these groups are identified by a corresponding group of moments.[20]

The first set of moments contains information on employment rates and transition rates from employment to non-employment, and these moments are used to calibrate parameters governing the marginal distribution of δ and the separation penalty of recall c^{δ}. To identify the second shape parameter of the marginal distribution of δ, η_{δ} (which is common across education levels), I use the average separation rate into non-employment for workers with an establishment tenure of 1-3.5, 3.5-6, 6-9, and 9+ years respectively. Then, to discipline the education-specific first shape parameter of this distribution, I use the average job loss rate (by education level). Finally, the subsequent separation rate after re-employment following a recall or a displacement (including those resulting in recalls) aids in identifying the separation penalty of recall.

The second set of moments is informative about the average wage level (by education level) and its variance. Using that there is a direct link between production and wages in the model, I use these moments to identify the marginal distribution of firm productivity y, as well as the starting level of human capital that was not normalized, s_2 (education level 2). In particular, I use the average educational wage premium for education level 2 (compared to education level 1), both overall and upon labour market entry (identified as a market tenure between 3 and 5 years). As the model generates these wage differences primarily through differences in productivity y and human capital s, these moments help to identify initial human capital levels for education level 2 (s_1 is normalized to 0) as well as the education-specific scale parameter $\mu_{y,\varepsilon}$ of the marginal distribution of y. The median-p25 and p75-p25 ratio of wages (by education level) are then used to complete the identification of the shape parameter η_y and education-specific scale parameter $\mu_{y,\varepsilon}$ of the marginal distribution of y.

The third set of moments provides information regarding job finding probabilities, both on-the-job and from nonemployment. In particular, the job-to-job transition rate upon displacement (by education level) helps to identify the meeting probability for newly unemployed workers ($\lambda_{\varepsilon}^{ug}$). Note that such a direct transition of a worker to a new job will be observed as a job-to-job transition. The overall quarterly job-to-job transition rate (by education level) therefore also contributes to identifying this parameter, while also informing the value of the on-the-job meeting rate $\lambda_{\varepsilon}^{e}$. Similarly, the average job finding rates (by education level) closely correspond to the job finding rate of unemployed workers, $\lambda_{\varepsilon}^{u}$, while the average education-specific employment rate connects all these different flows into employment (as well as the flows out of unemployment from the first set of moments).

The next set of moments focuses on wage growth within and between job spells, thereby helping to identify human capital transition rates and stepsizes, among others. The specific moments used here include the net replacement rate in unemployment, which closely relates to the parameter b

[20] Dividing the parameters and moments in groups is an exercise I purely do for exposition purposes. In reality, all parameters directly or indirectly affect all moments, but dividing the parameters and moments into groups clarifies the main considerations leading to the choice of certain moments.

included in the expression for the instantaneous value of non-employment $b(s)$.[21] Next, the average yearly wage growth (by education level), conditional on full-year full-time employment, helps to identify the human capital stepsize that was not normalized, $\Delta_s(2)$, and human capital on-the-job transition rate ψ_e, while also providing more information on λ^e_ε (as on-the-job offers may lead to re-bargaining and therefore a wage change). To identify the human capital transition rates during unemployment and while expecting a recall (ψ_u and ψ_r) as well as the production penalty associated with recall c^f, I then use the average difference between pre- and post-layoff wages, conditional on education level and non-employment duration (up to 0.5, 0.5 to 1, or 1 to 2 years). As laid out in appendix B.1.3, this moment closely resembles a difference-in-difference estimation. Similarly, to identify the human capital transition rates during unemployment and while expecting a recall (ψ_u and ψ_r), as well as the production penalty associated with recall c^f, I use the average difference between pre- and post-recall wages, conditional on education level and non-employment duration (0.25 to 0.5, and 0.5 to 1 year). These last two sets of moments also relate directly to the human capital step-size $\Delta_s(2)$ and therefore aid in its identification.

As the model allows for a choice between unemployment and potential recall upon separation, the recall probability ϕ^f_ε and the recall materialization probabilities ϕ^r_ε and $\phi^{rg}_\varepsilon\phi^r_\varepsilon$ are likely to be different from the observed recall and recall materialization probabilities. However, given the close relation between the two, I can use the observed probabilities as targets in the calibration. In particular, I use two sets of recall materialization rate, derived from the observed recall materialization rate within two years and within one year, in order to tease out the difference between ϕ^r_ε and ϕ^{rg}_ε, noting that I simplify the estimation by setting $\phi^{rg}_1 = \phi^{rg}_2 = \phi^{rg}$.[22] Similarly, I can use information on the fraction of workers expecting a recall who find a new job instead to inform the probability of meeting a new employer, $\lambda^r\lambda^u_\varepsilon$, and in particular the parameter λ^r.[23]

The final group consists of all remaining parameters (κ, κ^r, and ρ), which are identified using information on workers' starting wages and the observed correlation between wages and separation rates. In particular, I use the average wage of a new worker (hired out of unemployment) relative to the average wage to identify the bargaining power κ, and the average wage of a newly recalled worker (relative to the average wage) to identify the bargaining worker of the recalled worker κ^r. Finally, for the identification of the copula parameter ρ, I follow Jarosch (2021) in targeting the regression coefficient γ in the estimation equation (2.14) below:

$$D^\delta_{i,t} = \alpha_i + \gamma \log(w_{it}) + u_{i,t} \tag{2.14}$$

In equation (2.14), the variable $D^\delta_{i,t}$ is a dummy variable that is only filled if the worker i is employed in period t and still observed in period $t+1$. It equals 1 if the worker is separated from their job between t and $t+1$. The explanatory variables include an individual fixed effect α_i and the natural logarithm of the worker's wage in period t, $w_{i,t}$.

[21] The net replacement rate is the not derived from the IAB data used in chapter 1. Rather, I follow Gregory (2021) in taking this moment from OECD (2020).

[22] I choose to simplify the estimation as the fairly low number of highly educated recalled workers implies that the recall materialization probability for highly educated workers is fairly noisy.

[23] As explained in appendix B.1.3, this moment cannot be estimated from my data, so the data equivalent of this moment is based on results in Nekoei and Weber (2015). Analogously, I restrict the estimation in the model to workers expecting to be recalled who are re-employed within a year of displacement.

2.3.3 Calibration Results and Model Fit

Section under Construction

The moments described above add up to a total of 45 moments used to identify 30 parameters. Further details of the procedure used to estimate these moments can be found in appendix B.1.3. Table 2.3 summarizes the estimated moment values and their model counterparts. As can be seen in the table, the model fits the moments quite well. Nevertheless, it can be observed that the model has trouble matching a few moments, an example being the level of the separation rate. Similarly, it can be observed that the model tends to exacerbate differences between education levels compared to the data. Given that many of the parameters are already education-specific, one might therefore wonder whether it would be worth splitting some of the remaining parameters into education-specific parameters as well (especially the human capital transition probabilities).

When looking at the parameter estimates in table 2.3, and comparing these with closely related models such as those calibrated in Jarosch (2021) and Gregory (2021), it can be seen that the parameters estimated in both models generally yield very comparable estimates.[24] In general, however, it can be said that a few values stand out. In particular, the estimated value for the worker's bargaining power, κ, is very high. This is not particularly uncommon in models like the one proposed in this paper, and may be a consequence of the calibration attempting to match in particular the measures of wage dispersion (the p75-p25 and median-p25 wage ratios) by alleviating the impact of changing outside options.[25] After all, an increase in κ would lead the wage to be less dependent on the outside option, thus alleviating the impact of the loss of negotiation capital upon layoff or gain of negotiation capital through on-the-job search. This also makes it more notable that the bargaining weight of a recalled worker is substantially lower, at 0.64, reflecting that omitting this distinction and allocating all workers with the same bargaining weight could lead to a substantial loss of explanatory power.

It is also worth noting that the recall rates ϕ_1^f and especially ϕ_2^f are substantially higher than the observed recall rates in the data and model simulation. As this set of calibrated parameters implies that everyone chooses in favour of a potential recall when offered to do so, this implies that the role of allowing workers to find new jobs despite expecting to be recalled is quite large. Indeed, this meeting probability is slightly higher (104%) than the corresponding meeting probabilities for unemployed workers (as illustrated by the value of λ^r). For a similar reason, it can be seen that the recall materialization rates ϕ_1^r and ϕ_2^r do not quite line up with the rates found in the data and model simulation. In general, it can be observed that a nonemployed worker is much less likely to lose human capital while expecting to be recalled. In fact, the human capital depreciation rate $(\psi_r \psi_u)$ is equal to zero for the worker expecting a recall.[26] However, the recall itself also comes with substantial negative consequences in addition to the aforementioned lower bargaining weight, in the form of a production penalty c^f (that is relatively mild)[27] and a substantial penalty on the separation

[24] One major exception to this is the on-the-job meeting probabilities, which are very close to (or equal to) 0 in my calibration. This difference can be explained by the fact that I allow for displaced workers to find a job in the same period as being displaced, and this happens with quite high probability, as indicated by the calibrated values of λ_ε^{ug}.

[25] For comparison, Jarosch (2021) and Gregory (2021), who calibrate models that are fairly similar to the one I propose in this paper, find bargaining weights of 0.96 and 0.66 respectively.

[26] Note that in the estimation, I do not allow for the worker to accumulate human capital while nonemployed, but the fact that the estimate of ψ_r hits the boundary indicates that it might be worth allowing this for the worker expecting a recall.

[27] For context, note that the value of the lowest possible value of production for a worker of education level 1 with the starting level of human capital, $p(s_1, y^{min})$ equals $e^{0+1.71} \approx 5.52$

Description of Moment(s)	**Data**	**Model**	**Parameters**
Average rate of job loss, tenure 1-3.5y	0.0312	0.045	$\eta_\delta = 3.22$
Average rate of job loss, tenure 3.5-6y	0.0191	0.038	$\mu_{\delta,1} = 12.84$
Average rate of job loss, tenure 6-9y	0.0133	0.033	$\mu_{\delta,2} = 76.94$
Average rate of job loss, tenure>9y	0.0086	0.023	$c^\delta = 0.09$
Average rate of job loss, by education	0.0252	0.042	
	0.0224	0.009	
Subsequent separation, displacement	0.0728	0.057	
Subsequent separation, recall	0.1263	0.129	
p75-p25 ratio of wages	1.5678	1.69	$\eta_y = 7.67$
	1.0562	1.56	$\mu_{y,1} = 1.71$
median-p25 ratio of wages	1.2605	1.30	$\mu_{y,2} = 1.81$
	1.3063	1.41	$s_2 = 0.45$
Educational wage premium (all)	1.4656	1.47	
Educational wage premium (entry)	1.4639	1.46	
Job-to-job transition rate	0.0261	0.026	$\lambda_1^e = 0$
	0.0284	0.028	$\lambda_2^e = 0.002$
Job-to-job transition upon displacement	0.616	0.64	$\lambda_1^{ug} = 0.72$
	0.774	0.79	$\lambda_2^{ug} = 0.84$
Average job finding rate	0.2463	0.33	$\lambda_1^u = 0.41$
	0.2364	0.19	$\lambda_2^u = 0.2$
Average employment rate	0.867	0.96	
	0.8708	0.99	
Replacement rate	0.6	0.64	$b = 0.77$
Yearly wage growth	0.0144	0.031	$\Delta_s(2) = 0.13$
	0.019	0.04	$\psi_e = 0.073$
Pre- to post-layoff wage, duration<0.5y	-0.0366	-0.008	$\psi_u = 0.25$
	0.0175	-0.024	$\psi_r = 0$
Pre- to post-layoff wage, duration 0.5-1y	-0.0826	-0.009	$c^f = 0.34$
	-0.0358	-0.027	
Pre- to post-layoff wage, duration 1-2y	-0.101	-0.095	
	-0.1143	-0.149	
Pre- to post-recall wage, duration 0.25-0.5y	-0.0488	-0.045	
	-0.0543	-0.062	
Pre- to post-recall wage, duration 0.5-1y	-0.066	-0.08	
	-0.0186	-0.068	
Recall rate	0.109	0.11	$\phi_1^f = 0.14$
	0.0572	0.06	$\phi_2^f = 0.38$
Recall materialization rate	0.3251	0.28	$\phi_1^r = 0.14$
(Based on materialization in 2 years)	0.2623	0.23	$\phi_2^r = 0.12$
Recall materialization rate	0.287	0.32	$\phi_1^{rg} = 0.91$
(Based on materialization in 1 year)	0.2182	0.24	$\phi_2^{rg} = 0.91$
New job finding rate, workers expecting a recall	0.2927	0.3	$\lambda^r = 1.04$
Wage of newly hired worker	0.6463	0.62	$\kappa = 0.99$
Wage of newly recalled worker	0.7734	0.7	$\kappa^r = 0.64$
Coefficient $\hat{\gamma}$ in equation (2.14)	-0.0304	-0.02	$\rho = -23.28$

Table 2.3: *A summary of calibration moments, their values in the data and in the calibrated model, and corresponding parameter values* Pending Updated Model Moments and Parameters.

rate c^{δ}, which implies that after recall the worker's separation rate increases by 9 percentage points.

Moving to the differences between the two education levels, it can be noted that workers with a low education level are usually less likely to obtain an offer, regardless of whether they are unemployed ($\lambda_1^u < \lambda_2^u$ and $\lambda_1^e < \lambda_2^e$), the exception being the newly unemployed workers ($\lambda_1^{ug} > \lambda_2^{ug}$). In fact, the on-the-job meeting probability for workers with a low education level is 0, indicating that the model generates all job-to-job transitions through immediate transition after displacement. Furthermore, compared to the worker with a low education level, a highly educated worker starts with a higher level of human capital $s_2 = 0.45 > 0$ (which is more than four low education step-sizes higher than the starting level of a worker with a low education level, which was normalized to 0), while they also experience a bigger change every time they are hit with an appreciation or depreciation shock $(\psi_e, \psi_u, \psi_r\psi_u)$, $\Delta_s(2) = 0.13 > 0.1$. When it comes to the firm distributions

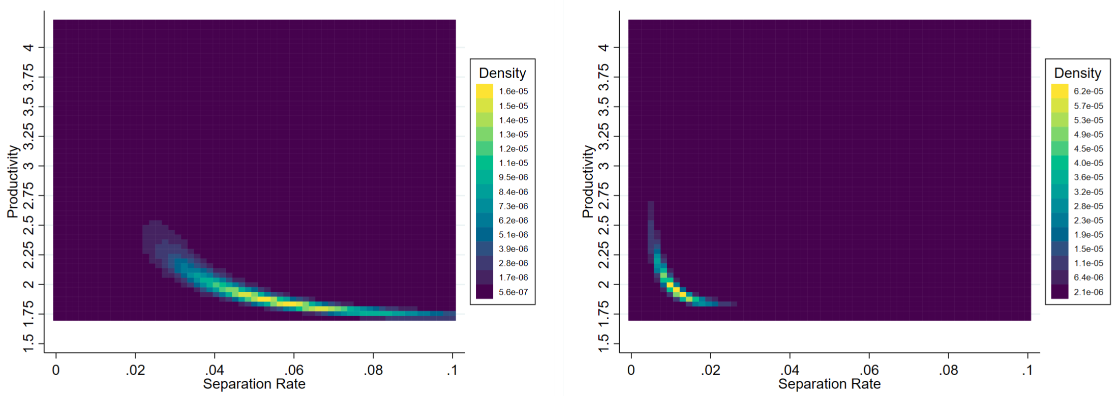


Figure 2.1: *The joint distribution of firm types faced by workers with a low education level (left) and a high education level (right). A lighter colour in this chart corresponds to a higher density.* Pending Updated Graphs.

the workers draw from upon receiving an offer, these are best illustrated in a diagram. Figure 2.1 visualizes the joint distribution of firms $G_{\varepsilon}(\theta)$ for the two education groups. For both education groups, the bulk of the density is located in the bottom left corner of the graph (which corresponds to low productivity and low separation rates), thus illustrating that both marginal distributions of δ and y are quite heavily right-skewed. When comparing the two distributions, the first thing that can be noted is that the low education level's minimum productivity is slightly lower than that of the high education level. This is due to $\mu_{y,1} < \mu_{y,2}$, as seen in table 2.3. Furthermore, the marginal distribution of the separation rate is much more right-skewed for the high education level (due to $\mu_{\delta,1} < \mu_{\delta,2}$), thus implying that on average low education workers are more likely to draw a higher separation rate and thus are more likely to be separated once they accept the offer.

2.4 Simulation Results

Section under Construction

In this section, I present the results of the simulation of the model, using the parameters that were calibrated in the previous section. In particular, I will start in subsection 2.4.1 by comparing the predictions of the model regarding the scarring effects of displacement to the observations I made in the data (in chapter 1). As none of these patterns were explicitly targeted in the calibration, this can be thought of as a test of the model's performance in achieving its aim. Then, in subsection 2.4.2, I use the model to illustrate the importance of taking into account the possibility of recall, by simulating a temporary shutdown of 50% of the economy. Finally, in subsection 2.4.3, I use the model to comment on a number of policies that have been proposed (and in some cases implemented) to alleviate the scarring effects of displacement.

2.4.1 Heterogeneity in the scarring effects of displacement

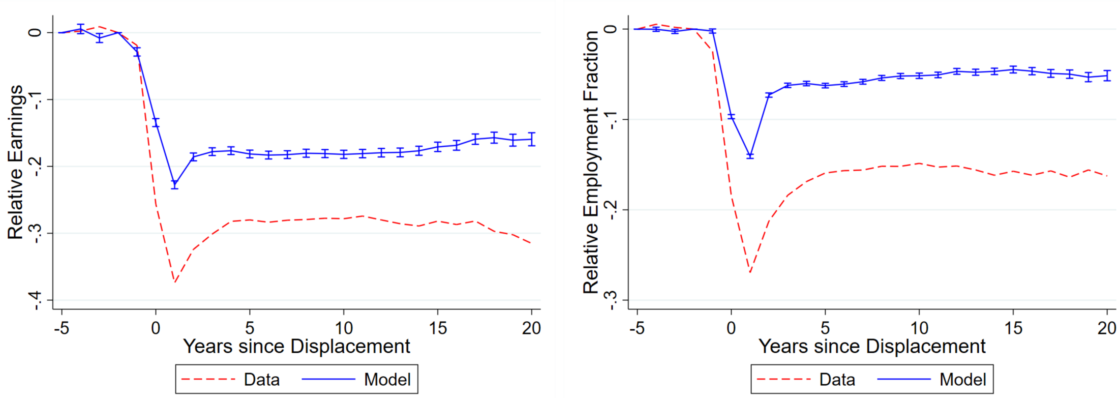


Figure 2.2: *The effect of displacement on earnings (left) and employment (fraction of the year spent in an employment spell, right), relative to the control group, using model simulation data (solid) and using the data (dashed, corresponding to figure 1.9).* Pending Updated Graphs.

Before moving to the dimensions of heterogeneity highlighted in section 1.3.3, figure 2.2 displays the average effect of displacement on earnings and employment status (defined as the fraction of the year spent in an employment spell). Just like in section 1.3.2, the effect is estimated by estimating equation (1.2), and thus the results can be compared to figure 1.9. For this purpose, I have included the results from figure 1.9 in figure 2.2 as dashed lines. Making this comparison, it can be seen that while the general shapes of the graph are matched quite well, the model underestimates the level of the scarring effect. This is likely to be caused in part by the model overshooting the job finding probability, especially for the worker with a lower education level (as seen in table 2.3). Indeed, it can be seen in figure 2.2 that the differences in magnitude between the earnings effect roughly line up with the differences in magnitude for the employment effect.

Figure 2.3 shows the predicted effect of displacement on earnings and employment status (defined as the fraction of the year spent in an employment spell) by education level, compared to the results in figure 1.10. It can be seen that while the model matches the fact that the workers with a low education level suffer more in terms of earnings, the simulated differences are much less severe than those seen in the data. This reflects the observations from the average effects above in figure 2.2. Conditional on the level differences, however, the model is well able to match the observed differences between the workers with a low and a high education level. The exception to this is that the two groups of workers are not converging in terms of their subsequent employment paths in the model, whereas in the data in section 1.3.3.1 it was observed that the employment effects are fairly similar for the two groups in the long run (as indicated by the dashed lines in the right panel of figure 2.3).

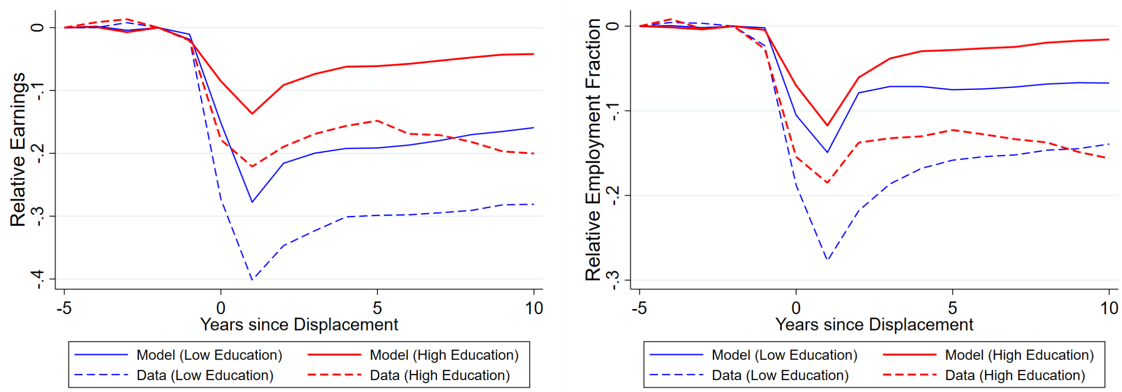


Figure 2.3: *The effect of displacement on earnings (left) and employment (fraction of the year spent in an employment spell, right), relative to the control group (by education group), using model simulation data (solid) and using the data (dashed, corresponding to figure 1.10).* Pending Updated Graphs.

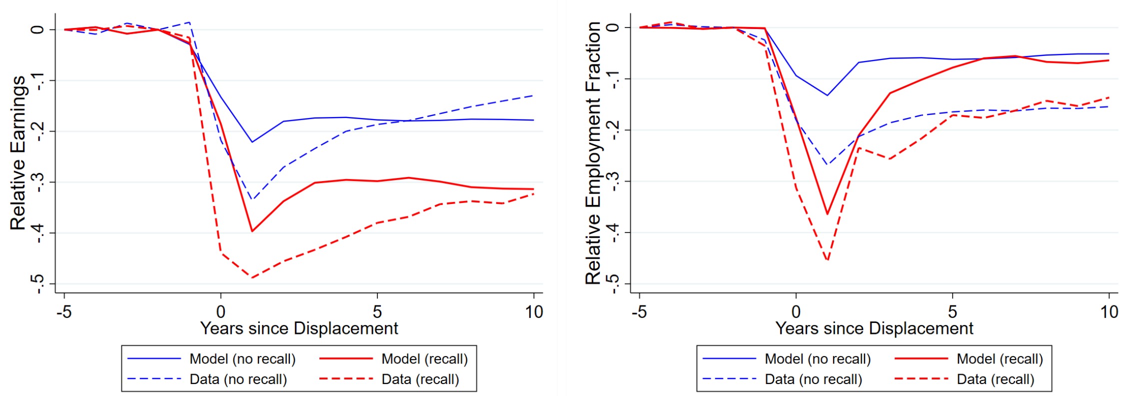


Figure 2.4: *The effect of displacement on earnings (left) and employment (fraction of the year spent in an employment spell, right) relative to the control group, by ex-post recall status (materialization of recall within 5 years), using model simulation data (solid) and using the data (dashed, corresponding to figure 1.13).* Pending Updated Graphs.

In figure 2.4, I show the estimated effect of displacement on earnings and employment fraction (defined as the fraction of the year spent in an employment spell) by ex-post recall status, compared to the results in figure 1.13. As can be seen from figure 2.4, the model matches the observation made in section 1.3.3.2 that recalled workers do worse than non-recalled workers after displacement (in terms of their earnings) in the short and long run, even though the effect on employment is fairly similar in the long run. In particular, while the effect on recalled workers in the short run falls short of the short-run effect found in the data, the long-run gap between the scarring effects for recalled and non-recalled workers is very similar to the gap found in the data. As can be seen in the right panel, this continues to hold when looking at employment fraction instead, even though both groups fall short of the effect estimated from the data.

Given that the model is successful in generating the observed differences between recalled and

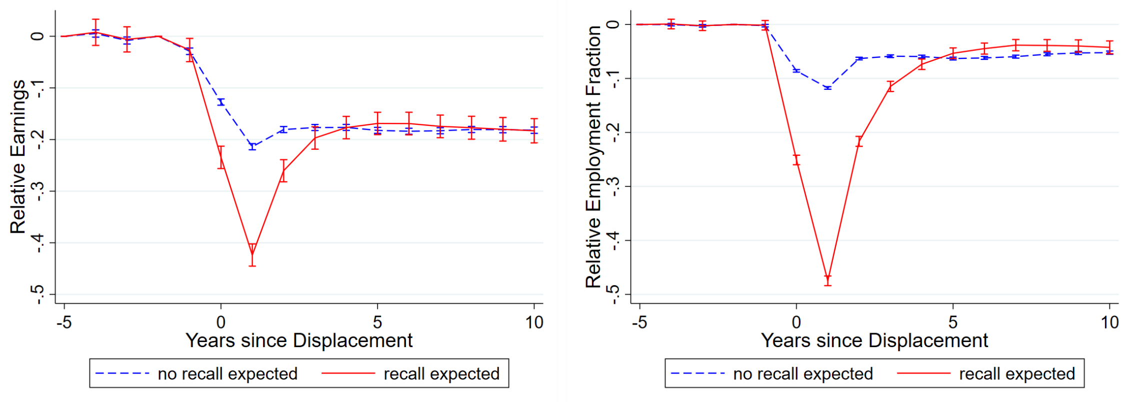


Figure 2.5: *The effect of displacement on earnings (left) and employment (fraction of the year spent in an employment spell, right) relative to the control group, by post-displacement state, using model simulation data. The red solid line corresponds to workers expecting a recall, and the blue dashed line corresponds to workers moving into unemployment (not necessarily by choice).* Pending Updated Graphs.

non-recalled workers after displacement, a natural next question to ask is what is driving these differences. In particular, given that recalled workers end up doing worse than non-recalled workers, why would a displaced worker choose a potential recall? Figure 2.5 provides an indication of how one could answer the second question. The figure repeats the estimation from figure 2.4, but uses worker states immediately after displacement (i.e. whether they expecting a recall or not) rather than the ex-post recall status. In other words, figure 2.5 indicates what the expected earnings path is for someone choosing for a potential recall (red) and someone moving to unemployment (blue). As can be seen from comparing figure 2.5 to the solid lines in figure 2.4, the choice of whether to accept a potential recall is not as simple as the ex-post differences between recall and non-recall suggest. In particular, when splitting the sample by post-displacement state rather than ex-post recall status, it can be seen that the worker expecting a recall is expected to experience similar earnings losses in the long run, despite being worse off in the short run. The faster recovery in the short run can be attributed to the worker expecting to be recalled having a higher probability of transitioning back to employment than the unemployed worker.[28] Given that workers are generally quite patient, as reflected by β, a worker will prefer to move into the nonemployment state with a potential for a recall. Indeed, under the parameter values resulting from the calibration, all workers choose for a potential recall when given the option.

In figure 2.6, I fully decompose the differences in estimated post-displacement earnings between recalled and non-recalled workers (as shown in the left panel of figure 2.4). In particular, I consider all channels (discussed below) through which the ex-post recalled worker is (potentially) different than a non-recalled worker in my model, and switch these channels off one by one in order to generate counterfactual earnings differences between recalled and non-recalled workers. As can be observed from figure 2.6, I find that the differences between recalled and non-recalled workers are primarily driven by the post-recall match characteristics. In particular, while the impact of the productivity penalty c^f is fairly small in the long run (and positive), the negative difference is primarily driven by

[28] To be specific, a worker with a low education level expecting to be recalled becomes employed in a model period with probability $\phi_1^r + (1-\phi_1^r)\lambda^r\lambda_1^u = 0.507$, whereas for an unemployed worker this probability equals $\lambda_1^u = 0.41$. Similarly, for a worker with a high education level these probabilities equal 0.303 and 0.2 respectively.

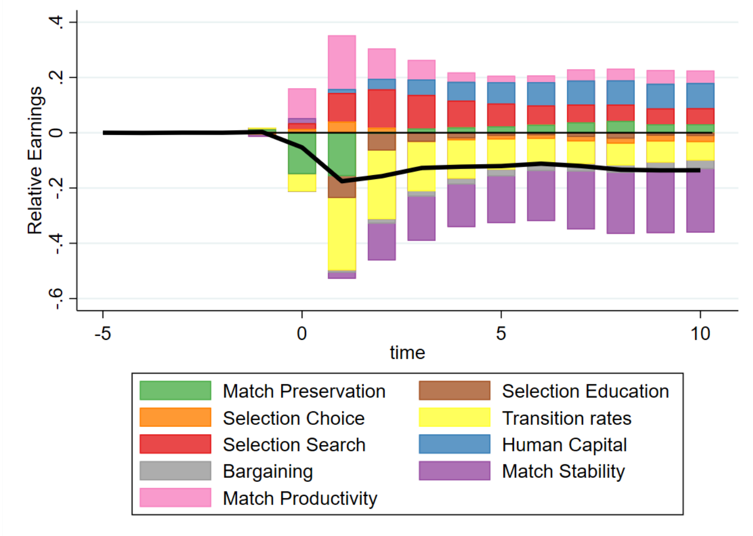


Figure 2.6: *A decomposition of the difference in the scarring effect of displacement on earnings between (ex-post) recalled and non-recalled workers. The black line represents the total difference, calculated as the difference between the solid red and blue lines in the left panel of figure 2.4. The decomposition is generated by turning off the indicated channels one by one (starting with those depicted on the outside), thus generating counterfactuals. Corresponding numerical values for selected time periods (0,1,3,5, and 10 years after displacement) can be found in the appendix in table B.1.* Pending Updated Graph.

the worker going back to an unstable job (as represented in the model by the separation rate penalty c^{δ}). Essentially, the fact that the worker has a much higher probability of being separated again shortly after being re-employed implies that the worker is likely to be set back in her development multiple times, both in terms of human capital and in terms of repeated loss of outside option.[29]

It is further worth noting what the impact is of all the other channels. The "Selection Choice" channel reflects the impact of allowing the worker to choose between the regular unemployment state and the state of nonemployment while expecting a recall. As expected, this has a (small) positive impact. Similarly, allowing the worker to search while expecting to be recalled (as indicated by $\lambda^r > 0$) has a positive impact, denoted "Selection Search" in the figure. While the bargaining power is much lower for a recalled worker, as observed in section 2.3.3, the negative impact of this difference ("Bargaining") turns out to be quite minor compared to other channels. On the other hand, the finding that the worker expecting to be recalled is much less likely to lose any human capital ("Human Capital") has quite a large positive effect, especially in the long run. The effect of different transition rates is negative throughout the simulation, reflecting that the higher transition probabilities of the worker expecting a recall after that initial period are not necessarily a positive outcome for the worker, given that they are returning to a low-productivity match ("Transition rates"). It is also worth noting that the differences between the two education levels also plays a small (negative) role here ("Selection Education"), although this is likely to be a consequence of higher transition rates from unemployment for the lower educated worker rather than differences by education level for workers expecting to be recalled. Finally, the residual element named "Match

[29]Note that the productivity and separation rate penalty is only applied once, so this penalty does not compound if the worker is separated and recalled a second time.

preservation", which reflects the difference between the two states if all parameters would be the same (and therefore the only difference between the two states is whether they find a new employer or move back to their previous employer), can be observed to be quite large and negative in the short run. This reflects that the displaced workers are negatively selected towards workers who are in a worse match (in terms of productivity and separation rate) than the match they would expect to find when drawing a (random) new employer from the joint distribution $G_\varepsilon(\theta)$.

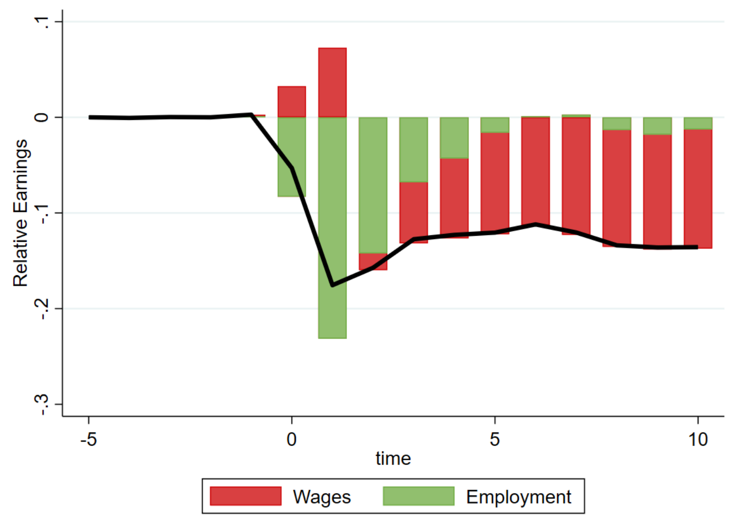


Figure 2.7: *A decomposition of the difference in the scarring effect of displacement on earnings between (ex-post) recalled and non-recalled workers, into earnings and employment. The black line represents the total difference, calculated as the difference between the solid red and blue lines in the left panel of figure 2.4. The decomposition is generated by using the estimation for employment (the difference between the solid red and blue lines in the right panel of figure 2.4) , and backing out the effect on wages as a residual.* Pending Updated Graph.

While the discussion above focuses on the decomposition of the (difference in) earnings losses for recalled and non-recalled workers, the setup of the model and the fact that it is able to match the difference in employment fraction as well (as shown in figure 2.4) also allows me to further decompose earnings losses into employment and wage components. In figure 2.7, I use the results from figure 2.4 to decompose the earnings loss into employment and wages. Corresponding to my findings in the data (in section 1.3.3.2), I find that the short-term difference is entirely driven by the employment margin. In fact, the wage margin goes in the opposite direction, suggesting that conditional on employment the recalled workers earn more.

In figure 2.8, I further decompose the wage and employment differences into the same 9 channels used for the earnings decomposition above. As can be seen in the left panel, the main positive difference for wages is the human capital margin. This reflects that the worker expecting a recall barely loses any human capital at all, while the general unemployed worker faces a substantial loss of human capital. As a result, it can be concluded that if the human capital depreciation would be the only channel at work, the recalled worker would return to their job with the same wage as before, whereas the non-recalled worker would lose some wage due to human capital depreciation, thus generating the positive difference as depicted in the left panel of figure 2.8. In the long run, this positive difference is increasingly offset by the negative influence of the higher separation rate

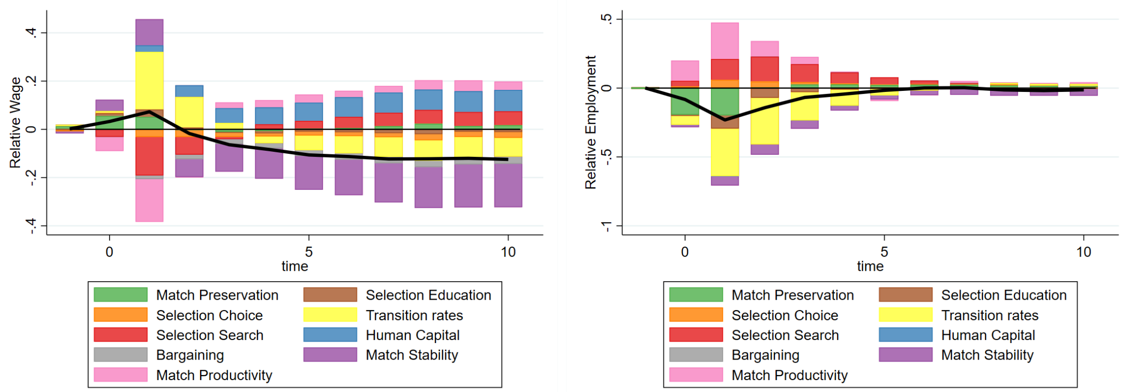


Figure 2.8: *A decomposition of the difference in the scarring effect of displacement on wages (left) and employment (right) between (ex-post) recalled and non-recalled workers. The black line represents the total difference, as depicted in figure 2.7. The decomposition is generated by turning off the indicated channels one by one (starting with those depicted on the outside), thus generating counterfactuals. Corresponding numerical values for selected time periods (0,1,3,5, and 10 years after displacement) can be found in the appendix in tables B.3 and B.2.* Pending Updated Graphs.

faced by the recalled worker, thus eventually leading to a negative wage difference.

In the right panel of figure 2.8, it can be seen that this human capital channel does not play a role when it comes to employment. For employment, the main drivers are related to the higher separation rate faced by the recalled worker. In principle, the fact that the separation rate faced by the recalled worker is higher than the separation rate faced by the non-recalled worker can be attributed to two channels, interacted with the faster transition back into employment after the initial displacement. As stressed before, the recalled worker returns to a job that has a higher separation rate than it had before the layoff. This is due to the penalty on the separation rate, and this channel is represented in the figure by the "Match Stability" elements. The second channel playing a role here is the fact that displaced workers in general come from matches of lower quality than the average match quality in the economy. In other words, the displaced worker tends to come from a match with a lower productivity and higher separation rate than the average. This also means that even without the additional penalty on the separation rate, the recalled worker would still be more likely to be separated again compared to the non-recalled worker (who on average will draw a lower separation rate when finding a new job). This is important especially in the short run, and is reflected by the "Match preservation" channel in the decomposition, as well as the "Transition rates" channel, which reflects that the worker returns to this lower quality match quickly.

2.4.2 A Shutdown Simulation

In this section, I use the calibrated model to simulate a temporary shutdown of the model economy. Using this simulation, and comparing its implied worker recovery patterns to the baseline simulation, I then highlight the importance of explicitly taking into account that workers may expect a potential return to their previous employer after the shutdown ends.

In order to simulate the temporary shutdown of the economy, I simulate the model twice, using the same realizations of random variables in both simulations so that I can directly calculate the effect of the shutdown on an individual level. I randomly select 50% of the workers, who (unexpectedly) move into nonemployment at the start of the shutdown.[30] The worker stays in this state of nonemployment for 4 quarters, after which the economy starts to re-open again. After reopening, I (initially) assume that the probability of moving back into employment is higher than usual for two quarters, after which the economy resumes operating as it did before the shutdown.[31]

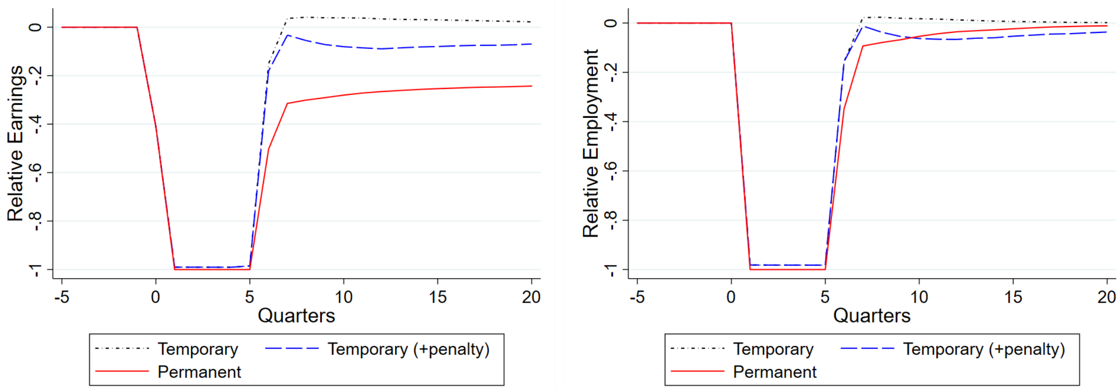


Figure 2.9: *The effect of a temporary shutdown on the earnings and employment status of affected workers. During the shutdown, workers are assumed to be either in the permanent unemployment state (red, solid) or in the temporary unemployment state with the associated penalties (blue, dashed) or without penalties (black, short-dashed).* Pending Updated Graphs.

In figure 2.9, I show how the effect of the shutdown on earnings and employment of the affected worker depends on the type of nonemployment experienced by the affected worker. In the left panel, it can be seen that the worker who moves into the temporary unemployment state (expecting a recall) rather than the permanent ("regular") unemployment state is able to recover much faster, regardless of whether the recall penalties c^δ and c^f are imposed or not.[32] As shown in the right panel, this is not necessarily the case when looking at employment, in which a worker forced into temporary unemployment may be worse off in the long run if the recall comes with the usual penalties. As figure 2.10 shows, these conclusions on the shutdown's effect on earnings and employment continue to hold when focusing only on workers with a high education level.[33]

In figure 2.11, I show the importance of the assumed transition probabilities in the periods immediately following the lifting of the shutdown (assuming that workers were temporarily unemployed without the associated penalties). As can be seen in the left panel of the figure, assuming an immediate transition back to the worker's former employer slightly improves the worker's outcome compared to the baseline "faster" transition (which corresponds to the simulation illustrated in figure 2.9), which in turn substantially improves the recovery compared to a simulation in which I assume the

[30]Since the workers do not interact, the size of the shutdown does not affect the results of the simulation. In the baseline simulation shown in this section, the shutdown occurs in the 15th quarter of the simulation. In appendix B.3.2 I show that the timing (and duration) of the shutdown does not substantially affect results.

[31]To be specific, I assume that this higher transition probability equals the average of 1 and the "usual" transition probability. In appendix B.3.2, I show that the conclusions are very similar if I assume that the transition rates return to the usual rates immediately after the shutdown ends.

[32]Given that the shutdown occurs randomly, one could argue that the recall penalties may not be as large as in the baseline model. After all, it may no longer be the case that the worker returns to an unstable job if the reason for the shutdown was in no way related to the job itself (as I'm assuming here by randomly selecting the affected matches).

[33]In appendix B.3.2, I show that these conclusions also hold when focusing only on workers with a low education level instead.

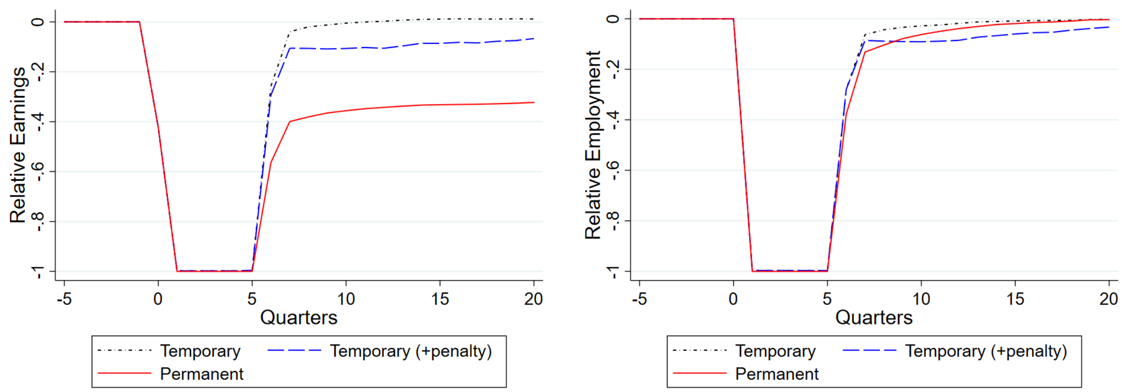


Figure 2.10: *The effect of a temporary shutdown on the earnings and employment status of affected workers with a high education level. During the shutdown, workers are assumed to be either in the permanent unemployment state (red, solid) or in the temporary unemployment state with the associated penalties (blue, dashed) or without penalties (black, short-dashed).* Pending Updated Graphs.

transition rates to return to the rates in the baseline economy immediately after the shutdown ends. Notably, in the two simulations with higher transition probabilities, the recovery initially overshoots the counterfactual outcome (in which the worker did not experience the shutdown). This is primarily due to workers returning to a job they would have lost in the counterfactual simulation, and as can be seen in the figure this is gradually corrected over time.

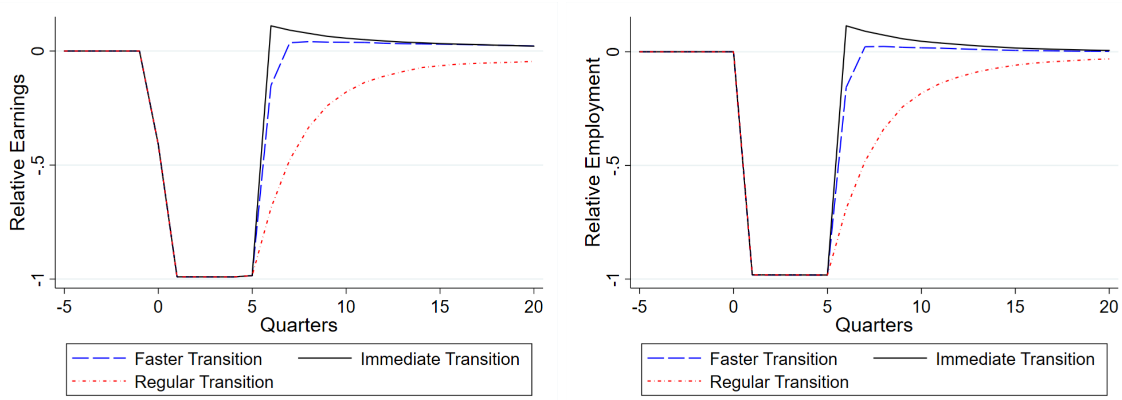


Figure 2.11: *The effect of a temporary shutdown on the earnings and employment status of affected workers. During the shutdown, workers are assumed to be in the temporary unemployment state without the associated penalties. After the shutdown, workers transition back to employment either immediately (blue, dashed), at a faster rate than usual (black, solid), or at the usual rate (red, short-dashed).* Pending Updated Graphs.

Overall, this simulation exercise serves to emphasize two points. First of all, contrary to what the figures in section 1.3.3 may suggest, being recalled by their former employer may not necessarily be bad for the worker's outcomes. Indeed, as stressed in the previous section, a substantial part of the apparent negative effect of recall is caused by selection: the workers that are recalled in reality tend to be matched to an employer with lower productivity and higher separation rate. If separation would occur at the same probability across all matches, as assumed in this simulation exercise, the prospect of recall may be better than the prospect of permanent unemployment, as the lost matches are of higher productivity (on average) than they are in the baseline model.

Secondly, the large difference between the recovery paths under temporary and permanent unemployment serves to re-emphasize the importance of explicitly including the possibility of recall in a model of the labour market, especially in a situation where workers are likely to return to their previous employer. Using a "standard" model of the labour market, in which this possibility is not included, would likely lead to an overestimation of the negative effects of the shutdown on the affected workers, and therefore potentially to policies that target to alleviate more losses than actually experienced by the worker.

2.4.3 Policy Implications

Throughout this paper, I have illustrated that workers who return to their employer after being laid off tend to do worse in terms of earnings than their non-recalled (but still laid off) counterparts. In the previous subsections, I have illustrated that this can be explained using the model I developed in section 2.2. Given that I have highlighted the importance of accounting for these two sets of workers (recalled and non-recalled) in a model, this might leave one wondering about the policy implications of this result. Unfortunately, the setup of the model is such that it is not necessarily informative to think of a social planner. After all, the model stresses the viewpoint of the worker, and thereby abstracts from other elements that a social planner may wish to take into account.[34] Nevertheless, a number of lessons can still be drawn from the results in the previous subsections, and in this discussion I highlight some of these lessons.

The decomposition of the differences (between recalled and non-recalled workers) in earnings loss after displacement in section 2.4.1 highlighted the depreciation of human capital as a channel that works in favour of the recalled worker. Indeed, as pointed out in section 2.3.3, the probability of human capital depreciation for a worker expecting a recall is equal to zero, so while a regular unemployed worker loses some of their human capital in 25% of the periods they spend in unemployment, this is not the case for the worker expecting a recall. As shown in appendix B.3.3, this implies that while human capital depreciation (relative to a continuously employed worker) accounts for a large portion of long-run earnings losses for the non-recalled worker (83.7% in year $k = 10$ after displacement), it plays a negative role in explaining the long-run earnings losses for a recalled worker (-13.3% in year $k = 10$ after displacement, i.e. it offsets some of the factors that drive the earnings losses). This is in contrast with the decomposition of the average scarring effect of displacement, which largely follows the non-recalled worker and therefore yields a large role for human capital depreciation, in line with what the existing literature has found. Naturally, a response to the decomposition of the average scarring effect of displacement might be to suggest a policy that would help the nonemployed worker prevent human capital depreciation. However, as this depreciation plays a minor role for the recalled worker, this would not help the recalled worker. Indeed, as I show in appendix B.3.3, an unintended consequence of such a policy would be that it increases the gap between recalled and non-recalled workers. In other words, given that the recalled worker tends to do worse, such a policy would not help the workers that have been shown to suffer the most from

[34] Two prominent examples of such elements include the firm choosing who to (potentially) recall, as well as the potential congestion externalities in the labour market (which I abstract from by setting exogenous offer arrival rates).

displacement.

Finally, while the model in this paper was calibrated to data from Germany, it is worth exploring how results might change when using data from a labour market that is associated with less generous unemployment benefits, such as the United States. As a final simulation exercise, in appendix B.3.3, I re-calibrate the nonemployment benefit parameter b to the United States equivalent of the replacement rate, keeping all other model parameters constant at their values calibrated using the German data. Given that the replacement rate in the US is lower than in Germany (0.45 versus 0.6), I find a lower value of b (0.52 instead of 0.7). In order to keep the government budget balanced, I also calibrate a proportional subsidy ($-\tau = 0.0114$) on output such that the costs of this subsidy equals the savings generated by lowering b. As I show in appendix B.3.3, the resulting counterfactual simulation shows a widened gap between recalled and non-recalled workers compared to the baseline model. In other words, if the unemployment benefit were to be the only difference between the two countries, I would expect the recalled worker in the United States to do even worse than his counterpart in Germany. Given the availability of appropriate data to fully calibrate a US version of the model, it would be interesting to see whether this prediction holds up. I leave this as an exercise for future work.

2.5 Conclusion

In this chapter, I further explore the scarring effect of displacement on earnings and employment by ex-post recall status, offering a model-based explanation for the previous chapter's observation of larger scarring effects experienced by recalled workers. As the existing theoretical models cannot account for these observations, I develop a search model of the labour market in which I explicitly allow for recall by dividing newly nonemployed workers into two separate states, according to whether or not the workers are expecting to be recalled. Furthermore, I distinguish between two fixed worker types, which I interpret as education levels. Further adding elements that have been successful in explaining the average scarring effect of displacement, such as human capital which evolves over time according to the worker's employment status, I find that this model, calibrated to the German data, is able to generate the heterogeneity I observed in the data in the previous chapter.

I then use the calibrated model to study the main drivers of the heterogeneity in the scarring effects of displacement. In particular, I explain the observation that recalled workers do worse than non-recalled workers after displacement as a combination of a selection and job stability effect. In the model, there is a negative correlation between the separation rate and the job productivity (which is directly related to the wage). Because of this negative correlation, the jobs that workers are displaced from are less productive and less stable than the average job in the economy. This implies that if workers return to such a job, they return to a job with a relatively low productivity and low stability, whereas workers moving to a new job will draw from the overall distribution of jobs and find a more productive and more stable job on average. This selection into displacement translates into the observation that recalled workers do worse than non-recalled workers in the short run. Furthermore, the higher separation rate (and, less importantly, lower productivity) plays a large role in explaining the differences between (ex-post) recalled and non-recalled workers in the long run as well, as a subsequent separation sets back the worker in their path of recovery, especially if such a subsequent separation no longer comes with (the expectation of) a future recall.

When decomposing the long-run effect of displacement on earnings for recalled and non-recalled workers separately, I find that whereas human capital depreciation plays a large role for non-recalled workers, its role for recalled workers is much smaller. This implies that a policy designed to dampen the depreciation of human capital (traditionally identified as one of the key drivers of the long-run effects of displacement on earnings) will likely be much less effective for recalled workers, thus further widening the long-run gap between recalled and non-recalled workers. For a recalled worker, a more beneficial policy might be one that aids the rehiring firm after the recall takes place, thus reducing the probability that the worker will be displaced again shortly after being rehired.

Based on the results of the model-based decomposition laid out in this chapter, one can think of various avenues for future research, and I will highlight a few of those possibilities here. First of all, the selection channels operating in the model in this chapter are partially exogenous, and it would be worth exploring models in which these channels are endogenous instead. The main extension that would be important in order to reach this would be to more explicitly incorporate decision making on the firm side of the model, which I largely abstract from in this chapter. Going a step further, one might consider exploring an environment with multi-worker firms, where the firm not only decides *whether* the recall any worker, but also decides *which worker* to recall. Finally, when extending the framework in this chapter to one with cyclical variation, and especially when doing so in the context of the German labour market, it will be important to explicitly add in the possibility of using short-time employment rather than an explicit layoff when facing an economic downturn. This will be particularly important given the wide usage of short-time employment policies throughout Europe during the Covid-19 pandemic.

Chapter 3

Business cycle patterns of occupational mobility and subsequent earnings

3.1 Introduction

In recent decades, the increased use of outsourcing by large businesses is one indication of the ongoing process of globalization. This trend, together with an accelerating rate of technological change, causes rapid changes in the demand for certain occupations that are particularly sensitive to these factors.[1] These fluctuations in demand have caused some workers to change occupations, while others remained, even with a rapidly decreasing demand for their occupation. Understanding why some workers decide to move and other workers do not, especially given that workers on average suffer from substantial wage and earnings losses when making such a move (see e.g. Forsythe, 2020), is important in order to make accurate predictions about a worker's behaviour as the process continues.

It is important to gain a deeper understanding of occupational mobility, broadly defined as the fraction of employed workers whose occupation is different from their occupation one year ago (resembling the definition in Kambourov and Manovskii, 2009b), and its consequences in terms of wages and earnings.[2] Understanding why workers decide to move occupations is important not only in this specific context, but also in the context of general labour markets. After all, understanding occupational mobility may be an important factor in understanding increasing wage inequality over the last few decades (see Kambourov and Manovskii, 2009a), lifetime wage inequality (see Gyetvai, 2021), and cyclicality of the tails of the distribution of earnings changes (see Carrillo-Tudela et al., 2021). When thinking about the switchers' labor market outcomes after the occupational switch has been made, earlier research finds that the links between occupational experience and wages may even be much more important than the link between either employer-specific or industry-specific

[1] A large literature on the influence of technological change argues that technological change is biased towards certain occupations. For an overview of this discussion, see for example Acemoglu and Autor (2011).

[2] Most empirical investigations of occupational mobility focus on the one-year rate but it is possible to change the time period under consideration, for example focusing on a 4-month rate like I do in this paper.

experience and wages (Kambourov and Manovskii, 2009b).[3] This finding raises the question of why workers change occupations when there is such a strong link between wages and occupational tenure. In this chapter, I contribute to the literature on this topic by explicitly distinguishing between workers who switch occupations through unemployment and workers who switch as part of a job-to-job transition, arguing that this distinction helps to explain a substantial share of the cyclicality in the estimated earnings and wage losses experienced by occupational switchers after making the switch.

The topic of occupational mobility has not been studied extensively in the economic literature until fairly recently. This apparent lack of research is due largely to the potential impact of measurement errors when trying to obtain empirical estimates of the occupational mobility rate. These measurement errors are caused by the fact that the occupational categories are at times very close to each other, and certain job descriptions may therefore not clearly correspond to a single occupational category, especially if the survey respondent's description of their job changes slightly between interviews. Therefore, the same job description may be coded as a different occupation in different years, thus falsely suggesting an occupational switch. In order to prevent this measurement error from contaminating the results, the general approach taken in the literature, as suggested in Kambourov and Manovskii (2009b), is to identify true occupational switches by looking at simultaneous labour market changes of the kind that often occur together with occupational switches, such as a switch of employer. In my data section (Section 3.2) I will use a similar set of conditions to prevent the aforementioned measurement error from contaminating my results.

My first set of observations from the data focuses on the level and cyclicality of the occupational mobility rate. The total occupational mobility rate itself is widely documented. For example, using the method above, Kambourov and Manovskii (2008) measure occupational mobility rates in the US between 1968 and 1996, finding an average (gross[4]) mobility rate of 18% at the 3-digit occupation level, which declines to approximately 13% at the 1-digit occupation level.[5]. This observation is roughly consistent with the 4-month rate of 4% I find in Section 3.2. When it comes to the cyclicality of the occupational mobility rates, a mild procyclical pattern is generally found in the literature (Kambourov and Manovskii, 2008). After I take into account the downward trend in occupational mobility rates over the observed time period, as observed in (among others) Xu (2017) and Lalé (2017), I confirm such a mild procyclical pattern.

When differentiating occupational switches by whether or not workers go through unemployment, some interesting patterns arise. First of all, I find that 60-70% of occupational switches do not involve unemployment. This result is consistent with the findings in Xiong (2008), who reaches a similar conclusion using the same dataset as I use in this paper (the Survey of Income and Program Participation), but for a different time period. Similarly, when focusing only on those workers who did not go through unemployment, I find that a large fraction of them also stay with the same

[3]Note the difference between an occupation and an industry: An occupation (e.g. postmaster) is defined using the tasks performed by a worker, whereas an industry (e.g. hospitals) is defined using the products produced by the firm.

[4]Gross mobility rates do not take into account that worker flows may go in both directions (e.g. from occupation A to occupation B but also the other way), whereas net mobility rates take these directions into account by cancelling out worker flows going in opposite directions.

[5]The 3-digit occupation code is generally the most disaggregated occupation code available. These 3-digit codes can easily be aggregated to 2- and 1-digit levels. Aggregating to higher levels necessarily decreases the resulting occupational mobility rate, but will also decrease the likelihood of measurement errors of the earlier-described kind affecting the results.

employer, a conclusion that was reached earlier in Papageorgiou (2018).[6] I find that the switches through unemployment exhibit a different pattern than the direct job-to-job switches. In particular, I find that the fraction of occupational switchers going through unemployment is countercyclical, which seemingly contradicts results from Carrillo-Tudela et al. (2014) and Carrillo-Tudela et al. (2016) (both of whom find no differences in these groups' cyclical patterns).

The second set of observations from the data focuses on earnings and wage paths experienced by occupational switchers after their switch is complete. Generally, I find that job-to-job occupational switchers tend to do better both in terms of earnings and wages. This result is consistent with the results obtained in Longhi and Taylor (2013), who use UK data to stress that occupational switches do not necessarily have negative consequences on earnings and wages. Indeed, while focusing on all switchers simultaneously reveals a wage and earnings loss, consistent with results from Forsythe (2020), these averages mask large differences between switchers through unemployment and job-to-job switchers, the latter of which do not necessarily face any losses in earnings and wages at all.

The main empirical contribution of this paper lies in distinguishing between the cyclical patterns of post-switch earnings and wage paths of job-to-job switchers and occupational switchers who experienced an intervening unemployment spell. The result above (on job-to-job switchers generally doing better) continues to hold regardless of the economic conditions at the time of the switch. However, when focusing on the cyclicality of earnings and wage losses for job-to-job occupational switchers and switchers going through unemployment I find that the two groups experience different patterns. In particular, while job-to-job switchers generally tend to do worse in recessions (compared to job-to-job switchers in booms), this is not necessarily true for switchers going through unemployment. For switchers who go through unemployment, I instead find that they do slightly better in recessions than in booms. This result, combined with the aforementioned result that in a recession more switchers make their change through unemployment, leads me to conclude that the cyclical patterns in overall earnings and wage patterns after occupational mobility are largely shaped by offsetting composition effects and cyclicality of the outcomes themselves. In particular, it may be the case that the cyclicality of average earnings changes after occupational mobility is primarily due to more workers switching through unemployment, and therefore experiencing worse outcomes than they would have in a boom (as their transition may have been a job-to-job transition in a boom). However, this is not something I can directly confirm in the data, and therefore I turn to a model instead.

There are a number of recent papers that provide a theoretical model of occupational mobility. Many of these models are in the spirit of the Islands model from Lucas and Prescott (1974), and interpret these "islands" as occupations. In particular, Kambourov and Manovskii (2009a) take into account occupational human capital (as suggested in Kambourov and Manovskii (2009b)) to set up a model of occupational mobility that performs very well in explaining increasing wage inequality. A similar model is used in Lalé (2017), who uses his model to explicitly estimate mobility costs. He finds a substantial increase in mobility costs in the last decades, an observation that was linked to the decreasing trend in occupational mobility in Xu (2017).

Though models of occupational mobility often use the Lucas Islands model, there are also other

[6] To be specific, Papageorgiou (2018) uses the 1996 SIPP panel to find that, annually, 8% of employed workers switch occupations within the firm. I do not find a rate this large, which is not necessarily surprising given my focus on the 1-digit level.

types of models. The model I present in this chapter uses a DMP-style search model, which makes an explicit distinction between unemployed and employed workers while also incorporating search frictions on the labour market. In particular, the model in this chapter is fairly closely related to the model in Carrillo-Tudela and Visschers (2021). They extend the standard DMP model (see for example Pissarides, 2000) to analyze how the (unemployed) worker's decision to switch occupations changes with individual and aggregate occupations. Their model generates productivity cutoffs for both separation and reallocation, and the relative positioning of these cutoff functions imply that workers may be unwilling to reallocate even though they face a zero job-finding probability. Such a worker is referred to as rest unemployed, a concept that appears earlier in Alvarez and Shimer (2011), Shimer (2007), and Coles and Smith (1998), although the latter of these three is a model not specific to the labour market. This type of unemployment also appears in the model I propose in this chapter, where similar productivity cutoffs appear (with an additional cutoff for job-to-job switches).

Generally, the existing models of occupational mobility do not allow for occupation switching as part of a job-to-job transition. One exception to this is the model in Carrillo-Tudela et al. (2021), who use a model that allows for both types of occupational switches in order to explain cyclicality in the tails of the earnings change distribution. My model is fairly similar to theirs in that it allows for both on-the-job search in different occupations and occupational switches through unemployment. Additionally, my model allows for workers to switch occupations without switching employers. On the other hand, as I focus on gross occupational mobility patterns, which I find to be fairly symmetric in the data, I abstract from occupation-specific elements that would lead a worker to target specific occupations in their search, such as in Carrillo-Tudela and Visschers (2021), Carrillo-Tudela et al. (2021), and Pilossoph (2022). Instead, I make the simplifying assumption that workers move to a randomly drawn new occupation, thereby essentially interpreting occupations as "islands", which are ex-ante identical.

Following the discussion above, the main contribution of this chapter to the theoretical literature lies in the distinction between occupational changes by unemployed and employed workers, and further distinction of job-to-job switches between switches with and without an accompanying employer change. By separating these types of mobility, I can explain the cyclical patterns in the incidence of occupational mobility as well as its consequences that I found in the empirical section of the chapter. Indeed, I find that the overall cyclicality of the earnings and wage consequences of occupational mobility are dampened by two opposing forces. On the one hand, there are the earnings and wage consequences conditional on the type of switch, which worsen in recessions for job-to-job switchers especially. Furthermore, the fraction of switchers through unemployment increases in recessions, which further increases the procyclicality of the average earnings profile since switchers through unemployment generally do worse than job-to-job switchers. On the other hand, however, the job-to-job switching rate within the same employer is constant across the business cycle, and this dampens the procyclicality induces by the first force. This is because while the productivities drawn by these within-employer job-to-job switchers do not change over the business cycle, the aggregate productivity at the time of bargaining does still affect the wages obtained by these workers in their new job. Therefore, the cyclicality of the endogenous separation threshold still plays a role, leading to more workers rejecting this within-employer job-to-job switch and the resulting average effect being better than in a boom.

The rest of this chapter is organized as follows: Section 3.2 describes some of the patterns found in the data, using the 1996 to 2008 panels of the Survey for Income and Program Participation (SIPP). Section 3.3 then presents the model. The quantitative analysis of the model is split into two sections: Section 3.4 focuses on the calibration of the model and the resulting parameter values; Section 3.5 on the implications of the model for the question of interest. Finally, Section 3.6 concludes and provides some directions for potential future research.

3.2 Observations from the Data

In order to motivate the setup of the model in the next section, this section presents some observations from the data. These results are obtained using data from the Survey of Income and Program Participation (SIPP). The SIPP is one of three U.S. datasets that are often used in the existing literature to empirically investigate occupational mobility. The other two datasets are the Current Population Survey (CPS) and the Panel Study of Income Dynamics (PSID). For my purposes, the SIPP is the most appropriate given its design as a sequence of rotating panels.[7] Specifically, each panel of the SIPP tracks a representative (multistage stratified) sample of the civilian non-institutionalized population of the United States.[8] This sample is divided into four "rotation groups". In every four-month period (called a "wave"), these rotation groups are then interviewed in their corresponding month.[9] As every interview asks the respondent about the prior four months, the resulting panel contains monthly information for every respondent.[10] For these months, the SIPP contains information on income, labour market participation, program participation and general demographics.[11] Most importantly, the SIPP contains monthly data on the respondent's occupation on a 3-digit level, which allows me to calculate occupational mobility rates up to the 3-digit level.

All results below are obtained using the 1996, 2001, 2004 and 2008 panels of the SIPP. I use only these panels because they are fairly consistent in terms of how variables are measured across individuals and across time. For the purpose of this section, I restrict the sample to respondents between the age of 23 and 61, who participated in the first interview of the panel, are not self- or dual-employed, and do not work for the government. These restrictions largely follow Xiong (2008), although he also restricts respondents to be male.[12] Furthermore, I follow Kambourov and Manovskii (2008) and define occupational mobility as the fraction of employed individuals who

[7] The way the CPS is structured makes that dataset more appropriate when one is interested in cross-sections given that each respondent is only interviewed twice with an 8-month gap in between, whereas the SIPP allows me to track the same individual over an extended period of time. Furthermore, the SIPP tracks a larger sample than the PSID does (although the subjects of the PSID are tracked for a longer period), which makes the observations more reliable when it comes to fairly low-frequency events such as an occupational switch at the 1-digit level. The SIPP also interviews the subjects more frequently than the PSID does (every four months instead of annually).

[8] Generally, respondents are not paid for their participation in the survey. However, there are exceptions to this rule. For example, in the first wave of the 1996 panel, a random selection of the respondents were given a small incentive payment of \$10 or \$20, in order to assess the effect of these payments on the response rate and the consistency of responses. A further discussion of this experiment can be found in James (1997) and Davern et al. (2003), among others.

[9] The first rotation group is interviewed in the first month, the second rotation group is interviewed in the second month, and so on.

[10] The nature of the data collection implies that the first month of data from the fourth rotation group coincides with the fourth month of data from the first rotation group so that the monthly information is not complete for the first three and last three months in the data.

[11] These collected variables are collected to serve the main purpose of the SIPP, which is "to provide accurate and comprehensive information about the income and program participation of individuals and households in the United States, and about the principal determinants of income and program participation" (U.S. Census Bureau, 2001).

[12] In appendix C.1.2 I show how this restriction influences the results obtained in this section.

report an occupation different from their most recent previous reported occupation. In order to obtain consistency between the 1996 and 2001 panels on the one hand (which use the SOC 1990 system), and the 2004 and 2008 panels on the other hand (which use the SOC 2000 system), I convert the reported occupational codes using the occupation system from Dorn (2009). Throughout, I use the reported occupation in the same month of the previous wave as the previous report, thus avoiding the seam bias that occurs in the SIPP due to respondents often reporting the same value for many variables for all months they are asked about (which creates a disproportionate amount of changes between the last month of a certain wave and the first month of the next wave). More information on the construction of the dataset used to obtain the observations below, and in particular the measures of occupational mobility, can be found in Appendix C.1.1.

3.2.1 The Cyclicality of Occupational Mobility Rates

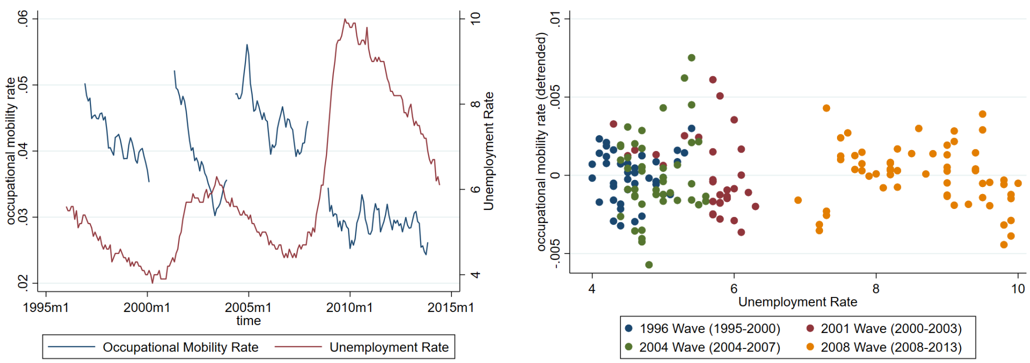


Figure 3.1: *The 1-digit occupational mobility rate and the corresponding month's unemployment rate from the BLS, plotted over time (left) and against each other in a scatter plot (right).*

As the model in Section 3.3 focuses on the 1-digit occupational mobility rate rather than its 2- or 3-digit counterpart, this section focuses on the occupational mobility on a 1-digit level.[13] First, the left panel of figure 3.1 plots the 1-digit (4 month) occupational mobility rate over time. As can be seen in the figure, rates range from 2% to 5.5%, with an average rate of approximately 4%. This observation is roughly consistent with the results in Kambourov and Manovskii (2008), who find a 1-digit (yearly) occupational mobility rate of approximately 13%. Both panels also compare the occupational mobility rate to the unemployment rate, in order to provide an indication of the cyclical behaviour of the mobility rate. However, in order to properly assess the cyclicality of the occupational mobility rate, the trend should first be removed. In the left panel of Figure 3.1, a clear negative trend is visible for each SIPP panel, one that was observed in the existing literature as well (see e.g. Xu, 2017).[14] The right panel therefore uses the detrended data instead.[15] From the right panel of Figure 3.1, there is no clear cyclical pattern visible. However, a naive regression reveals

[13] In appendix C.1.3, I show how results in this section change when considering the 2-digit counterpart, while also providing the equivalent of figure 3.1 for the 3-digit counterpart.

[14] It is worth noting that this visible negative trend for each panel is not observed when I omit the validation exercise for occupation switches described in appendix C.1.1, thus implying that it is likely to be a consequence of the validation I impose to check for "true" occupational switches.

[15] In order to detrend the occupational mobility rates, I apply a HP filter with smoothing parameter 14,400 for each SIPP panel separately.

a slope of -0.00004, albeit not statistically significant.[16] This leads me to the conclusion that the occupational mobility rate is (if anything) mildly procyclical. This conclusion is in line with the existing literature discussed in Section 3.1. Similarly, I find a mildly procyclical (but not statistically significant) pattern when only considering occupational mobility through unemployment, in line with Carrillo-Tudela and Visschers (2021).[17]

Occupation	1	2	3	4	5	6
Observations	1238030	735782	439620	111905	233481	564869
Inflow	32968	33147	22128	5640	11627	22527
Outflow	30934	33770	23344	5887	12258	21844
Net Inflow	2034	-623	-1216	-247	-631	683

Table 3.1: *Total number of incoming and outgoing switches found in the data for every 1-digit occupation, and number of times I observe a worker in each of these occupations in the data.*[18]

Figure 3.1 focuses on the gross occupational mobility rate. However, one might expect that there may be net flows of workers into or out of specific occupations, thus leading to a substantial net occupational mobility rate as well.[19] In order to investigate this net rate, Table 3.1 lists the total inflow and outflow for each 1-digit occupation, as well as the total number of times I observe a worker in these occupation.[20] As can be seen in Table 3.1, the net inflow for each occupation is fairly low compared to the gross worker flows. Thus, I infer that there does not seem to be a specific occupation that expels or attracts workers. This observation is confirmed by Table C.2 in Appendix C.1.2, which repeats the analysis but is specific to the occupation of origin and destination for all observed flows. As there is no specific occupation that expels or attracts workers, I assume in the model in Section 3.3 that workers who change occupations are assigned a random new occupation.

Since the focus of this chapter is on the distinction between those who change occupations with and without going through an unemployment spell (referred to as U-switchers and E-switchers respectively), it is important to confirm whether these two groups follow different cyclical patterns. As can be seen in both panels of Figure 3.2, the fraction of switches that goes through unemployment shows a clear countercyclical pattern. This result continues to hold when looking at the state level instead of the national level, as shown in Appendix C.1.2. Note that this result holds despite the occupational mobility rate through unemployment (as well as its counterpart through employment) being mildly procyclical, as shown in Appendix C.1.2, thus suggesting that it is primarily a mechan-

[16] Naive regressions by SIPP panel reveal a slope of 0.0006 for the 1996 panel, -0.0020 for the 2001 panel, 0.0012 for the 2004 panel, and -0.0004 for the 2008 panel, with only the coefficient for the 2001 panel being statistically significant at the 5% level.

[17] If I only consider reallocation through unemployment, the naive regression coefficient on the unemployment rate becomes -0.00003, with the SIPP panel-specific coefficients being 0.0005 for the 1996 panel, -0.0004 for the 2001 panel, -0.0004 for the 2004 panel, and -0.0003 for the 2008 panel.

[18] The 1-digit occupations listed are "(1) Management, Professional, Technical, Financial Sales, and Public Security Occupations", "(2) Administrative Support and Retail Sales Occupations", "(3) Low-skill Services", "(4) Precision Production and Craft Occupations", "(5) Machine Operators, Assemblers and Inspectors", and "(6) Transportation, Construction, Mechanics, Mining, and Agricultural Occupations".

[19] Recall that in calculating the net occupational mobility rate, flows between two occupations are cancelled out against each other. So, a switch from occupation A to occupation B cancels out a switch from occupation B to occupation A, whereas in the gross occupational mobility rate these two switches would both add to the total.

[20] Note that all numbers in Tables 3.1 and C.2 are totals over the entire sample period (and do not use the sample weights). As such, one cannot easily convert the totals in these tables into fractions of workers in these occupations, as the number of workers in these occupations is fluctuating over time. Nevertheless, to give an idea of what this fraction would look like, I include the number of times I observe a worker in each occupation. Furthermore, in Appendix C.1.2, when I show panel-specific equivalents on table 3.1, I include the number of times a worker is observed in the occupation in the first complete month of the panel.

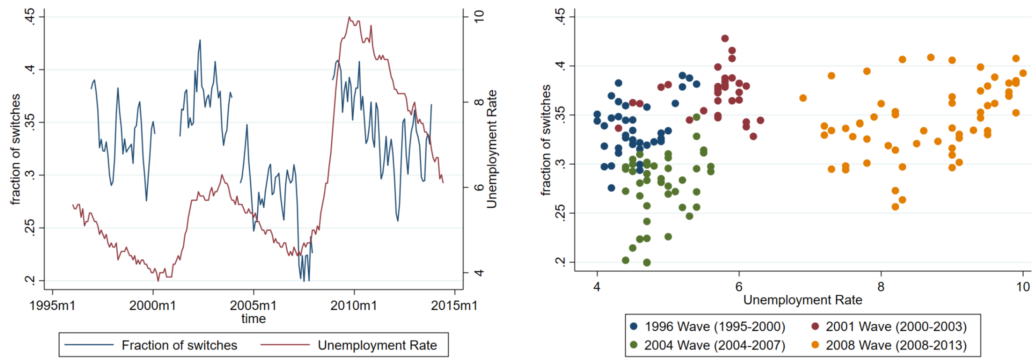


Figure 3.2: *The fraction of occupational switchers (1-digit) going through unemployment and the corresponding month's unemployment rate from the BLS, over time (left) and plotted against each other in a scatter plot (right).*

ical result, driven by the base number (of unemployed workers) being highly countercyclical, and therefore the procyclicality of the occupational mobility rate through unemployment being slightly weaker. However, given that the two types of occupational mobility show very different cyclical patterns in terms of their subsequent earnings, as stressed in the next subsection, it is important to model the two groups separately, as I do in Section 3.3.

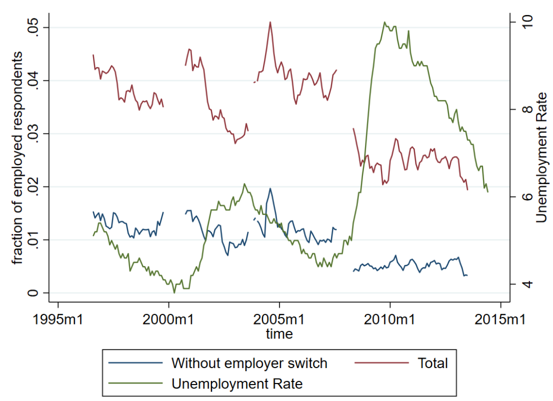


Figure 3.3: *The fraction of employed workers switching occupations in the next 4 months and the fraction of employed workers doing so without switching employer, plotted over time together with the corresponding month's unemployment rate from the BLS.*

Before moving to the earnings pattern experienced by occupational switchers, it is worth taking a closer look at the occupational switches that occur without an intervening unemployment spell. In Figure 3.3, I plot the occupational mobility rate considering only switches through employment.[21] Inspecting the graph, it can be seen that approximately a quarter to a third of the employed workers who switch occupations do so without changing employers. This observation is noteworthy given that an employer switch is one of the events I use to verify an occupational switch, but it is nevertheless consistent with the findings in Papageorgiou (2018). Thus, job-to-job occupational mobility is not

[21] Note that the rate shown in Figure 3.3 looks at the next 4 months instead of the previous 4 months. This restriction is necessary as I am interested in the fraction of previously employed workers rather than the fraction of currently employed workers.

always a result of on-the-job search for a better match with a new employer. Therefore, I make an explicit distinction between job-to-job occupational mobility with and without an employer change when modeling job-to-job occupational mobility in Section 3.3.

3.2.2 The Cyclicality of Earnings and Wage Gains after Occupational Mobility

Having established the cyclical patterns of (1-digit) occupational mobility, both overall and specific to switches with and without intervening unemployment spells (U-switchers and E-switchers), I now proceed to examine patterns in the affected workers' real wages and real earnings after such switches.

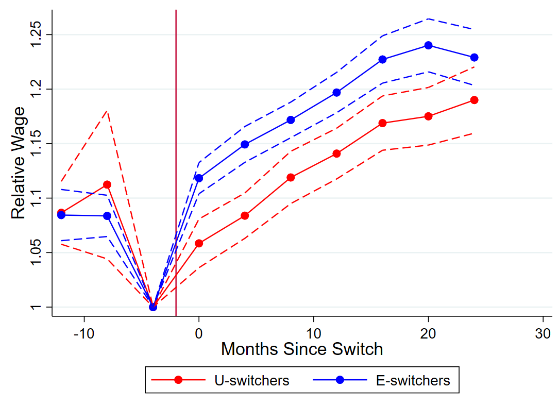


Figure 3.4: *Real wage paths over time for occupational U-switchers and E-switchers. The switch takes place between time -4 and 0, as represented by the vertical line at -2. The dashed lines correspond to the (pointwise) 95% confidence interval.*

In Figure 3.4, I plot the average wage for U-switchers and E-switchers, from 12 months before I observe the switch until 24 months after I observe the switch and relative to the last observed wage before the switch takes place.[22] The main restriction I make in creating this figure is that I only use respondents for whom I observe the wage in at least 5 of the relevant 4-month periods, including times 0 and 4, at least one of times 8 and 12, at least one of times 16, 20, or 24, and at least one of the pre-event periods.[23] Furthermore, I remove observations where the ratio of wages between two successive waves is more than 2 or less than 0.5. As can be seen in the figure, the jump in the average wage is much larger for E-switchers, and this difference is persistent over time.

One way to investigate how the pattern from Figure 3.4 changes over the business cycle is to calculate these relative wages separately for each wave and rotation group, and plot the resulting numbers against the unemployment rate at the time of observation. In Figure 3.5, I do this for the wages observed immediately after the occupational switch (corresponding to month 0 in Figure 3.4), conditioning on each data point in the graph summarizing the wage differential of at least 20 individuals.[24] As can be seen in the left panel of Figure 3.5, the overall wage differentials for occupational

[22]For U-switchers, the wages before the switch refer to the wages earned in their previous job(s), thus implying that these wages are further back than 4, 8, and 12 months before the switch.

[23]In Appendix C.1.2, I show that the result is not substantially affected if I remove this restriction. Furthermore, I show where the relative wages of non-switchers would appear in this graph.

[24]Similar figures for different points in time (e.g. 4 months after the switch) are available upon request. Note that I

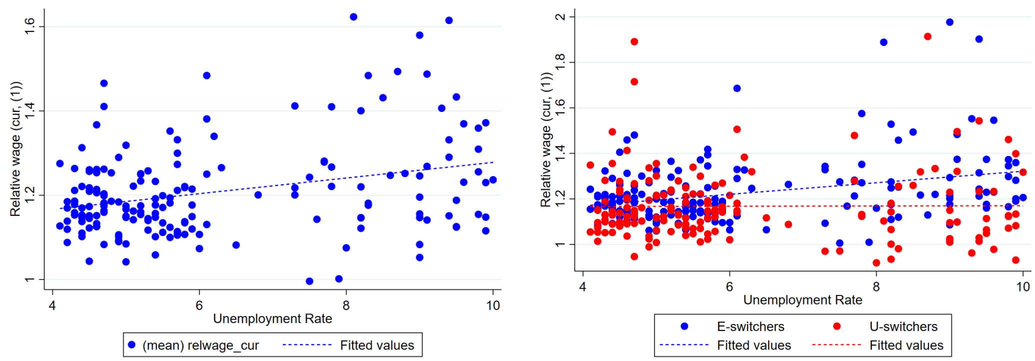


Figure 3.5: *Real wages immediately after the occupational switch, relative to the real wage before the switch, for all occupational switchers (left) or for occupational U-switchers and E-switchers (right), plotted against the unemployment rate at the time of the switch. Dashed lines show fitted values corresponding to a simple OLS regression.*

switchers are mildly countercyclical. However, this primarily reflects the countercyclicality of the wage differentials for E-switchers, as shown in the right panel of Figure 3.5. The wage differentials for U-switchers, on the other hand, appear to be acyclical. In Appendix C.1.2, I show that this conclusion is robust to removing validation checks on occupational switches and to removing all female workers from the sample.

While the analysis above is based on raw wage differentials, one can imagine that the groups of occupational U-switchers and E-switchers are not necessarily comparable in terms of their pre-switch wage (against which the relative wage above is measured), and may be different in terms of other characteristics as well.[25] Therefore, I now proceed to estimate the wage and earnings patterns experienced by occupational switchers in regression frameworks. The first of these frameworks is an event study setup, where I define the event of interest to be the occupational switch, with the time of the event corresponding to the time at which the worker starts their job in their new occupation. In practice, the estimation is set up as a two-way fixed effects (TWFE) estimation, estimating equation (3.1) below:

$$w_{it} = \alpha_i^C + \gamma_t^C + \bar{e}_i^C \lambda_t^C + \beta^C X_{it} + \sum_{\substack{k=-3 \\ k \neq -2}}^{K} \delta_k^C D_{it}^{C,k} + \varepsilon_{it}^C \tag{3.1}$$

Equation (3.1) is estimated separately for each sample wave C. Within each such estimation, only occupational switches that took place in wave C are considered to be treatments. In equation (3.1), i refers to the individual and t refers to the wave. The dependent variable in this specification, w_{it} refers to the wage (or earnings) of individual i in period t. The explanatory variables include an individual fixed effect α_i^C and a time fixed effect γ_t^C (both of which are allowed to differ between estimations, as denoted by superscript C), as well as a quadratic polynomial in age X_{it} and an (estimation-specific) error term ε_{it}^C. The variable $\bar{e}_i^C$ denotes the average earnings of individual i between waves $C-5$ and $C-1$, and I will generally refer to this as recent earnings. When deriving

no longer impose Figure 3.4's restriction on having observed the individual in at least 5 periods when creating Figure 3.5, as now only the wages in period -4 and 0 are relevant.

[25]Indeed, as demonstrated in Appendix C.1.2, these two groups differ substantially in their age and education

these recent earnings, I condition on the individual having earnings available in the data for at least three of the waves between $C-5$ and $C-1$, one of which should be wave $C-1$. The coefficients of interest are a series of coefficients on dummy variables $D_{it}^{C,k}$. These variables equal 1 if individual i was displaced in period $t-k$ (where the dummy variable for $k=-1$ is omitted), and where period $t-k$ corresponds to wave C. As these dummy variables always equal 0 for workers who did not switch occupations (in wave C), the coefficients represent the effect of switching occupations on wages (relative to the wage of non-switching workers), k periods after the occupational switch. The maximum number of future periods equals $K=5$, reflecting the relatively short panels of the SIPP. As the estimation is done separately for each sample wave C, only observations that correspond to waves $C-3$ to $C+5$ are used for the estimation. To enhance the interpretation of the estimated value, I then divide the estimated coefficient δ_k^C by the control group's average wage in wave $C+k$, obtaining relative coefficient $\tilde{\delta}_k^C$. The graphs below then plot the resulting relative coefficient $\tilde{\delta}_k$ over k (where $\tilde{\delta}_k$ is the average of $\tilde{\delta}_k^C$ over base waves C), thus revealing a wage (or earnings) path from 3 periods before to 5 periods after the occupational switch event.

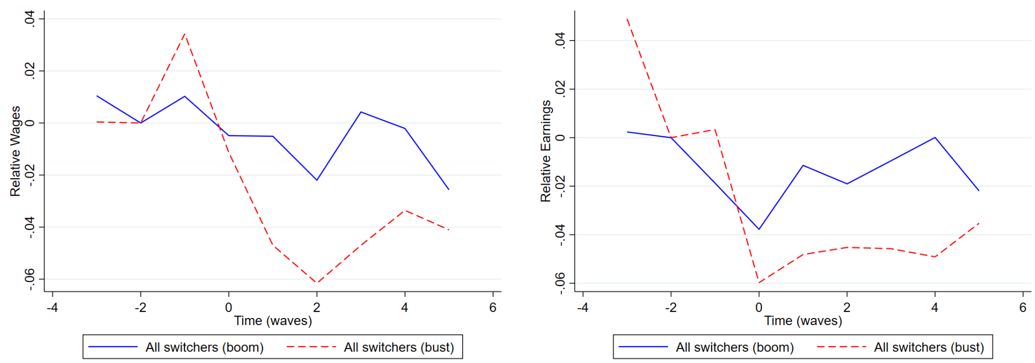


Figure 3.6: *The effect of occupational switches on real wages (left) and real earnings (right), relative to the control group of never-switched workers, using estimated coefficients from equation* (3.1)*,specific to switches that materialized in booms or busts.*

In Figure 3.6, I plot the results of an estimation of equation (3.1) where I do not distinguish between E-switchers and U-switchers. As can be seen in the figure, the subsequent wage and earnings patterns of occupational switchers are subject to some degree of cyclicality. In particular, the figure suggests that the wage and earnings outlook is worse for workers who switch occupations during a recession.

Since the conclusions drawn from figure 3.6 abstract from the distinction between E-switchers and U-switchers, they do not consider my earlier observation that during recessions a larger fraction of occupational switchers goes through an intervening unemployment spell. In Figure 3.7, I estimate equation (3.1) while allowing for the treatment effect to be different for the two types of switches.[26] As can be seen in Figure 3.7, workers who switch through unemployment are generally worse off than workers who switch occupations without going through unemployment (both in terms of wages and earnings), which is consistent with the observations from the raw data in figures 3.4 and 3.5.

[26]Note that I do not estimate the effects for the two groups separately. Rather, I allow for two types of (mutually exclusive) treatments, such that the fifth term in equation (3.1) becomes $\sum_{U=0}^{1}\sum_{\substack{k=-3\\k\neq-2}}^{K}\delta_k^{C,U}D_{it}^{C,U,k}$ instead of $\sum_{\substack{k=-3\\k\neq-2}}^{K}\delta_k^{C}D_{it}^{C,k}$, where U corresponds to the type of switch.

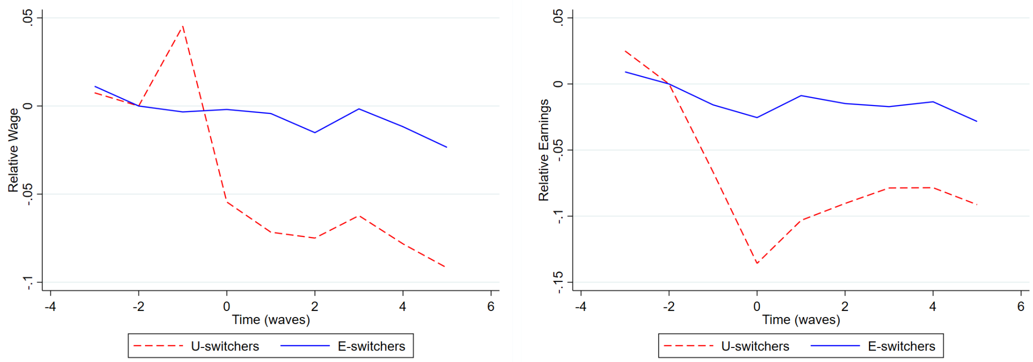


Figure 3.7: *The effect of occupational switches on real wages (left) and real earnings (right), relative to the control group of never-switched workers, by type of switch, using estimated coefficients from equation* (3.1)*.*

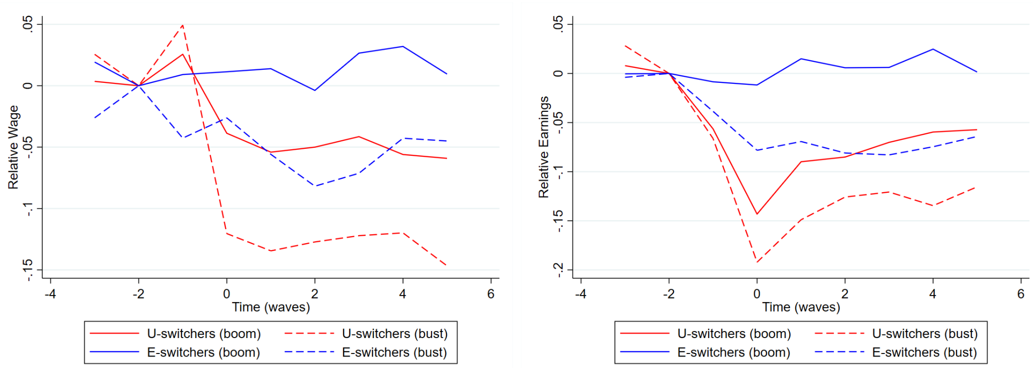


Figure 3.8: *The effect of occupational switches on real wages (left) and real earnings (right), relative to the control group of never-switched workers, by type of switch, using estimated coefficients from equation* (3.1)*, specific to switches that materialized in booms or busts (right panel).*

In Figure 3.8, I show how the wage and earnings patterns of E-switchers and U-switchers change over the business cycle. Contrary to Figure 3.7, Figure 3.8 is not consistent with the observations from the raw data, as it suggests that wage (and earnings) differentials are procyclical for both U-switchers and E-switchers. One potential explanation for this inconsistency could be that the workers who switch occupations in booms or busts are different in more dimensions than just the type of switch they make. Such differences would not be accounted for in the figures generated using raw data, but in a two-way fixed effects specification some of those differences would be accounted for through the individual fixed effect.

Recently, a number of papers have stressed the shortcomings of event study settings such as the used above, in particular stressing that the estimates of δ_k^C in equation (3.1) may be contaminated by effects from earlier and later periods, as well as by subsequent and prior treatments that are ignored in this specification.[27] In the specification above, this occurs because individuals who switch in waves $C+1$ and later, as well as individuals who switched before wave C who are re-employed again (and satisfy other sample requirements) are likely to be placed in the control group when estimating the effect for workers switching in wave C. In order to take into account potential contamination of the estimate of δ_k^C (and consequentially of the average $\tilde{\delta}_k$), I use the three-step estimation method from Borusyak et al. (2021).

The method proposed in Borusyak et al. (2021) proceeds in three steps. In the first step, the method aims to directly estimate the counterfactual implicitly used in a difference-in-differences estimation procedure. This is done by estimating a standard two-way fixed effects model (without the leads and lags for treatment) on all not-yet-treated and never-treated workers in the sample. Following the notation in equation (3.1) , this means that I estimate the following equation:

$$w_{it} = \alpha_i + \gamma_t + u_{it} \tag{3.2}$$

Note that specification (3.2) no longer includes control variables $\bar{e}_{it}$ (recent earnings) and X_{it} (the quadratic polynomial in age). The estimates of the individual and time fixed effects in equation (3.2) are then used to estimate the untreated (counterfactual) outcome for all treated observations as well. In other words, the estimated counterfactual outcome combines the estimated individual fixed effect (estimated using the individual's observations before treatment) and the estimated time fixed effect (estimated using other individuals, who were not treated at the time period of interest).

In the second step, these counterfactual untreated outcomes are compared to the (observed) treated outcome to form an estimate of the individual- and time-specific treatment effect (which is thus the difference between the estimated untreated outcome from step 1 and the observed outcome). In the third and final step, the target aggregation is then estimated using a weighted average of the individual and time-specific estimated treatment effects from step 2.

In Figure 3.9, I show the results of an estimation using the three-step estimation method, where I do not distinguish between E-switchers and U-switchers. Compared to Figure 3.6, it can be concluded from this figure that the cyclicality of the wage and earnings paths after occupational switches may be milder than initially thought. While the cyclicality of the post-switch wage path remains, the differences are much smaller than those observed in Figure 3.6. When it comes to earnings, the cyclicality observed in Figure 3.6 is no longer visible in the right panel of 3.9, thus suggesting that

[27] See, for example, Callaway and Sant'Anna (2020), Sun and Abraham (2020), and Borusyak et al. (2021).

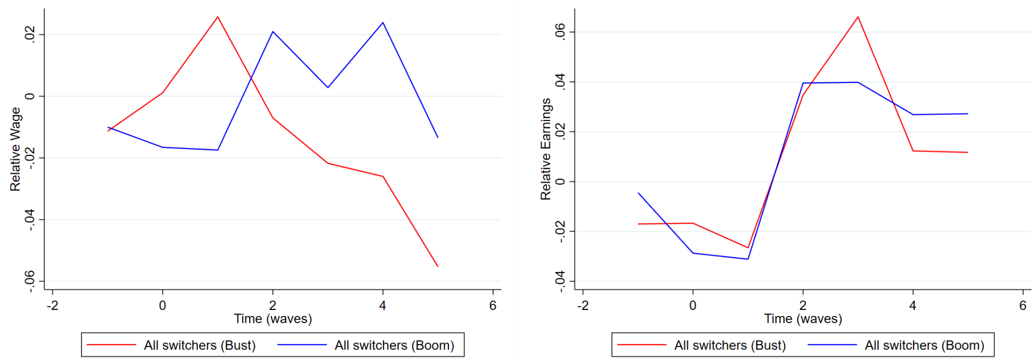


Figure 3.9: *The effect of occupational switches on real wages (left) and real earnings (right), relative to the counterfactual of never switching, using estimated coefficients obtained using the three-step estimation method. The plots show separate estimates of the effect for switches materializing during a boom or a bust.*

the aforementioned conclusion on procyclicality of the estimated earnings path may be a result of the contamination discussed in the recent literature (and in the text above).

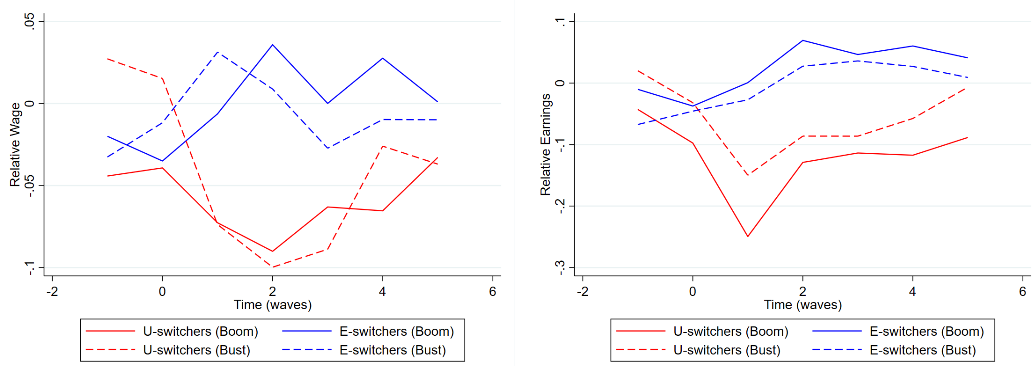


Figure 3.10: *The effect of occupational switches on real wages (left) and real earnings (right), relative to the counterfactual of never switching, by type of switch, using estimated coefficients obtained using the three-step estimation method. The plots show separate estimates of the effect for switches materializing during a boom or a bust.*

Figure 3.10 displays the results of repeating this estimation while allowing for the treatment effect to be different for the two types of switches. As can be seen in the left panel, the difference between E-switchers and U-switchers observed earlier (in Figure 3.7) remains intact, and even strengthens slightly for earnings. However, when it comes to the cyclicality of these wage paths, Figure 3.10 suggests a milder cyclical pattern for wage. For earnings, the cyclical pattern for U-switches even clearly reverses: the right panel of Figure 3.10 suggests that post-switch earnings paths are countercyclical for U-switchers, whereas the post-switch earnings paths for E-switchers are procyclical.

Having investigated the cyclicality of the post-switch real wage and real earnings patterns experienced by occupational switchers in three different ways, it can be said that the overall conclusion is not particularly clear. Generally, all methods agree that U-switchers tend to do worse than E-switchers, and the regression-based methods agree that subsequent real wage and real earnings pats of E-switchers are procyclical. However,when it comes to the cyclicality of these paths for U-switchers, the methods disagree. Unfortunately, while one could argue that regression-based methods should be expected to provide a more accurate picture than raw numbers, it is not necessarily clear that one of the two regression-based methods dominates. In particular, one could argue that the estimates obtained using three-step estimation method may dominate because they should avoid some of the bias stressed in the recent event study literature. However, one also has to keep in mind a second impactful difference between the two specifications, namely the fact that the three-step estimation method requires me to take a stance on when the impact of the switch starts. In the estimation results above, I allow for the effects to start in the wave before the switch materializes, but one could argue that this may not be far enough back for U-switchers, who may be switching at the end of an unemployment spell of several months. This is especially true when considering that U-switchers in recessions may take longer to find a new job and are therefore more likely to be unemployed in the second wave prior to the switch materializing. The conclusion would therefore be that the true effect is likely to fall in between the two estimated effects, thus suggesting procyclicality in the real wage and real earnings patterns for E-switchers, whereas the real wage and real earnings patterns for U-switchers are ambiguous (acyclical or mildly countercyclical).

3.3 The Model

Motivated by the observations made in Section 3.2, this section presents the model used for obtaining the results in Sections 3.4 and 3.5. The model is heavily based on the model Carrillo-Tudela and Visschers (2021). In particular, I use a version of their model where workers cannot direct their search to an occupation of their choice, and add the possibility of switching while staying employed. Given the similarity of the two models, it is natural that the presentation in this section is in many ways very similar. In particular, to enhance the comparability of the two models, I choose to use the same notation wherever possible. Furthermore, note that while most variables (but not the parameters) change over time, I drop the time subscript in the equations to enhance readability. To nevertheless stress the fact that these variables change over time, I do use the time subscripts when discussing these variables in the text.

3.3.1 Environment

3.3.1.1 Firms

The model economy is divided into O occupations, each of which is home to a continuum (of measure one) of risk-neutral workers and firms. Each firm has room for only one worker, hired on a frictional labour market. As labour is the only input in production, and it is equal to 1 if the firm is producing, the production of the firm (y_t) depends only on productivity variables. Throughout this chapter, the production function is assumed to exhibit constant returns to scale and depend on three types of productivity (p_t, x_t, and z_t), so that $y = f(p, x, z)$. The first productivity type, p_t, is aggregate productivity. This productivity, which takes the same value for all workers (regardless of occupation), can take a value between $\underline{p} > 0$ and $\bar{p} < \infty$ and follows a first-order stationary Markov process. It can be interpreted as the state of the economy as a whole: a low value for p_t corresponds to the economy being in a recession, and a high value corresponds to the economy experiencing a boom. The other two types of productivity are idiosyncratic productivity z_t and occupational human capital of the worker x_t. As both of these are specific to the worker (and occupation), I postpone the discussion of these productivity types to the next subsection.

It is assumed in the model that when the firm is not currently matched with a worker, it will post a vacancy at a (time-invariant) cost $k > 0$. In principle, the firm chooses which occupation to operate in, but since the value of posting a vacancy will be zero in all markets in equilibrium, this choice is not explicitly modeled. When being matched with a worker, the firm generally does not switch occupations, unless the match is hit with an exogenous occupational transfer shock (which is further discussed in the subsection on occupational transfers). However, even when matched with a worker, the firm always has the possibility to end the match at the start of each period. This separation decision is the choice variable for the firm, and it will be influenced by the three worker productivity variables p_t, z_t and x_t. Generally, this decision will be denoted by $\sigma(p, z, x) \in \{\delta, 1\}$, reflecting that regardless of its decision the firm will always face an exogenous probability $\delta \in [0, 1]$ of being separated from the worker.[28]

3.3.1.2 Workers

From the previous subsection, it can be deduced that a firm can be in one of two states: it can be producing or it can be posting a vacancy. Similarly, the worker can also be in two states: she can either be employed or unemployed. In either state, the worker faces a probability of death ϕ. However, this probability is not important when it comes to the decisions of the worker, and can be though of as embedded in the discount rate β.[29] When the worker is unemployed, she receives b every period, and has the choice of either searching in her current occupation or switching to a different occupation (this alternative is further discussed in subsection 3.3.1.4).

If the worker is matched with a firm, she receives a wage $w(p, z, x)$ from the firm every period. Just like the firm, the worker also always has the choice of terminating the match, a decision which is denoted by $d(p, z, x) \in \{\delta, 1\}$.[30] Furthermore, the worker also has the choice to search for a match in a different occupation, a choice that will be further discussed in subsection 3.3.1.4. As stressed by the notation, these two decisions both depend on all three types of productivity. As mentioned earlier, two of these productivities are specific to the worker-occupation pair. The idiosyncratic productivity z_t is in many ways similar to p_t: It can take values between $\underline{z} > 0$ and $\bar{z} < \infty$, and follows the same first-order stationary Markov process for all workers, represented by $F(z_{t+1}|z_t)$, the probability of the idiosyncratic productivity being at most z_{t+1} next period conditional on the current value being z_t.

The third and last type of productivity, x_t, is interpreted as occupational human capital. It can take H values, ranging from $x_1 > 0$ to $x_H < \infty$. When a worker starts working in a new occupation, she starts with the lowest value x_1. After that, the occupational human capital increases to the next level with probability $\chi(x_{h+1}|x_h)$ in every period in which the worker is employed in the occupation. While this occupational human capital does not depreciate over time (not even when the worker

[28]It is assumed here that when the firm is indifferent between separating and not separate, the firm will always decide not to separate. Thus, the firm will never decide to follow a mixed strategy.

[29]Throughout the model, I use the same β for workers and firms, even though the firm does not face this probability of death. However, for the producing firm the death of its worker is identical to separation, thus justifying using β for these firms as well. For the firms that have a vacancy, this inconsistency will not influence the solution, as β will be multiplying a term with value zero.

[30]Mirroring the assumption made for firms, it is assumed that a worker never follows a mixed strategy. Thus, if the worker is indifferent between separating and not separating, she will decide not to separate.

is unemployed), the worker can still lose her accumulated human capital. This loss occurs when a worker chooses to change occupations. Whenever a worker changes occupations, all the accumulated human capital in her former occupation is completely destroyed, and she starts over in her new occupation with $x_t = x_1$.[31] Arguably, a complete loss of human capital is not necessarily a realistic assumption to make, but the assumption greatly simplifies the model as there is no need to keep track of human capital in occupations other than the worker's current occupation.

3.3.1.3 Labour Markets

In this model, there is a separate labour market for each combination of occupation and the two worker-occupation specific productivities. In other words, it is assumed that the firm can observe the productivity values of the worker, and can thus aim a vacancy at a specific level of productivities x_t and z_t. In each of these labour markets, matches are formed according to a matching function. Letting $\theta(p, z, x)$ be the labour market tightness, this matching function can be rewritten such that $q(\theta(\cdot))$ is the matching probability for the firm and $\lambda(\theta(\cdot)) = \theta(\cdot)q(\theta(\cdot))$ is the matching probability for the worker.

When a worker and a firm match, the wage for the worker is determined as a solution to a standard Nash bargaining problem, where the bargaining power of the firm is denoted by η. The wage thus depends on both the value of being employed $W^E(p, z, x)$ for the worker and the value of producing $J(p, z, x)$ for the firm, as well as the outside options for both parties: the value of being unemployed $W^U(p, z, x)$ for the worker and the value of setting a vacancy $V(p, z, x)$ for the firm. The explicit expressions for these value functions are presented in Subsection 3.3.2. In short, the wage rate solves the following equation:

$$\eta \left(W^E(p, z, x) - W^U(p, z, x)\right) = (1 - \eta) \left(J(p, z, x) - V(p, z, x)\right) \tag{3.3}$$

Finally, note since the elements of this equation change over time, it follows that the wage rate earned by the worker also changes over time. Thus, rebargaining takes place every period.

3.3.1.4 Occupational Transfers

There are three ways in which a worker could switch occupations. The first opportunity for a worker to switch occupations occurs when the worker is unemployed. When a worker is unemployed, she has the option of choosing to switch occupations every period (before the matching takes place), at a cost $c^u(p)$, which is allowed to vary over the business cycle. This decision is captured by the variable $\rho^u(p, z, x) \in \{0, 1\}$.[32] If the worker decides not to switch ($\rho^u(p, z, x) = 0$), she will search for a match in her current occupation this period. If the worker decides to switch occupations ($\rho^u(p, z, x) = 1$), she will randomly select one of the $O - 1$ other occupations.[33] In that occupation, she will start with occupational human capital of the lowest level (x_1). She will also have a new

[31] Note that this loss also occurs in situations in which an unemployed worker chooses to switch from occupation A to occupation B while unemployed, and decides to switch back one period later (without having been employed in occupation B). In this situation, the worker will have $x_t = x_1$ in occupation A after she returns, regardless of how much human capital she had accumulated before switching to occupation B.

[32] It is assumed here that when the worker is indifferent between switching and not switching, she will always decide not to switch. Thus, the worker will never decide on a mixed strategy.

[33] The assumption of random search instead of directed search when it comes to changing occupations is motivated by the observation made in Section 3.2 that occupational mobility flows seem to be fairly symmetric.

value for the idiosyncratic productivity z_t, drawn from the stationary distribution $F(z)$ associated with the first-order Markov process that z_t follows. Finally, the worker will have to sit out the rest of the period unemployed. Thus, she will not be allowed to search for a match until the next period.

The second channel through which a worker can switch occupations is similar to the first, with the exception that it concerns workers who are already in a match. A worker who is matched to a firm at the beginning of a period has the option to search in a different occupation, a decision which is denoted by $\rho^e(p, z, x) \in \{0, 1\}$.[34] If she decides to do so, she pays a cost $c^e(p)$, which is allowed to vary over the business cycle, after which she searches in her new (randomly drawn) occupation. If she matches with a firm in that new occupation, which occurs with probability $\lambda(\theta(p, \tilde{z}, x_1))$ (where $\tilde{z}$ is the level of z_t drawn in the new occupation), she quits her current job and switches to that new occupation.[35] If she does not match with a firm in the new occupation, she remains in her current occupation without losing her accumulated human capital x_h.

Finally, it is possible for a worker (and firm) to switch occupations as the consequence of an exogenous shock. This shock, which hits with probability ψ, forces the worker to switch occupations, while not switching employers or losing any human capital. This type of switch corresponds to workers switching occupations without leaving the firm. In principle, it is not always a beneficial switch for the worker, as she will draw a new value for z which may be lower than her value at the start of the period, and a firm and worker may decide to subsequently destroy their match if the new value of z is sufficiently low. Thus, interpreting this exogenous shock as a promotion shock does not fully capture the effect of this shock. Rather, one might interpret the shock as a reorganization of the firm. After all, if the worker switches occupations without switching employers, the model implies that the firm switched occupation as well. As it was shown in Section 3.2 that these types of switches are quite common among job-to-job occupational switches, it is important to include these types of switches in the model explicitly. After all, while the other two reallocation decisions are a choice of the worker, these switches are imposed on the worker, without the worker having any say in it. As such, the consequences for the worker may be very different.

3.3.1.5 Timing

Having described all the elements of the model, it may be worth reviewing the order of events and decisions in a single period. After all, the order in which these take place has a substantial influence on the value functions in the next subsection. In short, a model period can be divided in 6 subperiods. In the first of these subperiods, the new values for p_t, z_t, and x_t are revealed to all surviving workers and firms (the death shock occurs before the start of the period), and a value of z_t is drawn from $F(z)$ for all newborn workers.[36] Thus, when making decisions later in the period, the firm (and worker) is assumed to know the value of production (and wages) if the match remains intact.

In the second subperiod, the occupational transfer shock ψ is realized, after which the workers

[34] It is once again assumed that when the worker is indifferent between switching and not switching, she will decide not to switch.

[35] Specifically, it is assumed that the worker does not know her value of $\tilde{z}$ until she enters the bargaining process with a new firm. As a worker needs to quit her current job before entering a bargaining process with a new firm, a worker will always decide to do so (after all, this decision will be the same as the decision captured by ρ^e, without the switching cost c^e). Note that this order of events also implies that the outside option of the worker when bargaining with the new firm will be the value of being unemployed.

[36] It is assumed here that a new worker is born whenever a worker dies. This newborn worker is allocated to a random occupation where they will be unemployed with occupational human capital at its lowest level x_1.

and firms who are currently in a match make their separation decisions (and thus set $d(p,z,x)$ and $\sigma(p,z,x)$) in the third subperiod. It should be noted that workers and firms who are hit by the occupational transfer shock or decided to destroy their match do not make any of the decisions in the remaining subperiods.

Next, in the fourth subperiod, the occupational transfer decisions are made by workers who were unemployed in the first subperiod and workers who are employed and were not hit by the occupational transfer shock in the second subperiod. Thus, in the fourth subperiod, these workers set $\rho^u(p,z,x)$ or $\rho^e(p,z,x)$ (whichever applies to them) and pay the associated cost.

In the fifth subperiod, the search and matching process takes place, conditional on the workers and firms being allowed to search in this period.[37] Finally, production takes place in the sixth and last subperiod.

3.3.2 Value Functions

Following the above description of the model and its corresponding timing of events and decisions, one can now provide an expression for the value functions of the worker and firm. First, the value of being unemployed at the start of the last (production) subperiod, $W^U(p,z,x)$, can be expressed as follows:

$$\begin{aligned} W^U(p,z,x_h) = b + \beta \mathbb{E}_{p',z'} \Bigg[\max_{\rho^u(\cdot)} \Bigg\{ \rho^u(p',z',x_h) \left[\int_{\underline{z}}^{\bar{z}} W^U(p',\tilde{z},x_1) dF(\tilde{z}) - c^u(p') \right] \\ + (1-\rho^u(p',z',x_h)) \Big[\lambda(\theta(p',z',x_h)) W^E(p',z',x_h) \\ + (1-\lambda(\theta(p',z',x_h))) W^U(p',z',x_h) \Big] \Bigg\} \Bigg] \end{aligned} \tag{3.4}$$

This value function reflects that an unemployed worker only has one decision to make: the decision of whether or not to change occupations. If she decides to change occupations ($\rho^u(p,z,x_h) = 1$) next period, she pays the cost $c^u(p')$ and will be unemployed for the remainder of the next period at the new values for p_t (p') and z_t ($\tilde{z}$), and the lowest level of occupational human capital x_1. If she decides not to change occupations, she will be searching for a job, and she will match with a firm with probability $\lambda(\theta(p',z',x_h))$.[38]

An employed worker has two decisions to make: her occupational transfer decisions $\rho^e(p,z,x)$ and her separation decision $d(p,z,x)$. Denoting the value of searching in a different occupation by $R^E(p,z,x)$, the value of being employed at the start of the last (production) subperiod $W^E(p,z,x)$ can be expressed as follows:

$$\begin{aligned} W^E(p,z,x_h) = w(p,z,x_h) + \beta \mathbb{E}_{p',z',x'} \Bigg[\max_{d(\cdot),\rho^e(\cdot)} \Bigg\{ \psi \int_{\underline{z}}^{\bar{z}} \max \Big\{ W^E(p',\tilde{z},x'), W^U(p',\tilde{z},x') \Big\} W^E(p',\tilde{z},x') dF(\tilde{z}) \\ + (1-\psi) \Big[d(p',z',x') W^U(p',z',x') + (1-d(p',z',x')) \Big[(1-\rho^e(p',z',x')) W^E(p',z',x') \\ + \rho^e(p',z',x')(-c^e(p') + R^E(p',z',x')) \Big] \Big] \Bigg\} \Bigg] \end{aligned} \tag{3.5}$$

[37] As mentioned earlier, workers are not allowed to search in a period if they have destroyed their previous match in the same period, or if they have decided to (or were forced to) switch occupations while unemployed in the same period.

[38] Note that the value of her occupational human capital in the next period is the same as in the current period (x_h), reflecting that the occupational human capital does not depreciate when the worker is unemployed.

$$R^E(p', z', x') = \int_{\underline{z}}^{\bar{z}} \Big[(1 - \lambda(\theta(p', \tilde{z}, x_1)))W^E(p', z', x')$$
$$+\lambda(\theta(p', \tilde{z}, x_1))W^E(p', \tilde{z}, x_1)\Big] dF(\tilde{z}) \tag{3.6}$$

Here, the inclusion of the exogenous occupational transfer shock has caused the inclusion of ψ and the integral on the first line of equation (3.5), the latter of which reflects the expected value for the worker who receives the occupational transfer shock (next period). The inclusion of the occupational transfer decision has in turn caused the inclusion of $\rho^e(\cdot)$ as well as the term $R^E(\cdot)$.

Firms that are currently not matched to a worker are not making an explicit decision in this model, as they are assumed to be posting a vacancy. Therefore, the value of posting a vacancy in a market with productivity pair (z, x_h) at the start of the fifth (matching) subperiod is rather simple[39]:

$$V(p, z, x) = -k + q(\theta(p, z, x))J(p, z, x) + (1 - q(\theta(p, z, x)))\beta\mathbb{E}_{p'}\left[V(p', z, x)\right] \tag{3.7}$$

Finally, firms that are currently in a match with a worker only make the separation decision $\sigma(p, z, x)$. However, since they are also subject to the occupational transfer shock, their value function includes an additional term similar to the one seen earlier in equation (3.5), the value function for employed workers. As a consequence, the value function for producing firms at the start of the last (production) subperiod can be expressed as follows:

$$J(p, z, x_h) = y(p, z, x_h) - w(p, z, x_h) + \beta\mathbb{E}_{p',z',x'}\Bigg[\max_{\sigma(\cdot)}\Bigg\{\psi\int_{\underline{z}}^{\bar{z}} \max\left\{J(p', \tilde{z}, x'), V(p', \tilde{z}, x')\right\} dF(\tilde{z})$$
$$+\ (1 - \psi)\left[(1 - \sigma(\cdot))(1 - \hat{\rho}(p', z', x'))J(p', z', x')\right.$$
$$\left.+\ (1 - \sigma(\cdot))\hat{\rho}(p', z', x')\beta\mathbb{E}_{p''}[V(p'', z', x')] + \sigma(\cdot)V(p', z', x')\right]\Bigg\}\Bigg] \tag{3.8}$$

Here, $\hat{\rho}(p', z', x') = \rho^e(p', z', x')\int_{\underline{z}}^{\bar{z}} \lambda(p', \tilde{z}, x_1)dF(\tilde{z})$ represents the probability that the worker will decide to search in a different occupation and match there (and thus destroy her current match). As these are both events outside of the control of the firm, the firm will take the function $\hat{\rho}(p', z', x')$ as given.

3.3.3 Transition Equations

So far, the model description has mostly focused on the agent's decisions within a single period. However, it will also be important to keep track of the mass of workers flowing in and out of unemployment across periods. These transitions can be summarized by two equations: one for the mass of unemployed and one for the mass of employed workers, both specific to a combination of productivities $(z_t, x_t) = (z, x_h)$ as well as the occupation o. The following equation provides an expression for the mass of unemployed workers next period in occupation o, with idiosyncratic

[39]Note that the timing of this value function is slightly different than the others. This inconsistency is implemented to avoid expectations in this value function, as firms decide on vacancies after separation subperiod.

productivity $z_t = z$ and occupational human capital $x_t = x_h$:

$$
\begin{aligned}
u_o'(z, x_h) = &\int_{\underline{z}}^{\bar{z}} (1 - \lambda(\theta(p, \tilde{z}, x_h)))(1 - \rho^u(p, \tilde{z}, x_h))(1 - \phi)u_o(\tilde{z}, x_h) dF(z|\tilde{z}) d\tilde{z} \\
&+ \int_{\underline{z}}^{\bar{z}} \hat{d}(p, \tilde{z}, x_h)(1 - \phi)e_o(\tilde{z}, x_h) dF(z|\tilde{z}) d\tilde{z} \\
&+ (\mathbb{1}_{h=1}) \left[\sum_{\tilde{o} \neq o} \sum_{h=1}^{H} \left[\int_{\underline{z}}^{\bar{z}} \rho^u(p, \tilde{z}, \tilde{x}_h)(1 - \phi)u_{\tilde{o}}(\tilde{z}, \tilde{x}_h) d\tilde{z} \right] \right] \frac{dF(z)}{O - 1} \\
&+ (\mathbb{1}_{h=1}) \frac{\phi}{O} dF(z)
\end{aligned}
\tag{3.9}
$$

From the equation, it can be seen that unemployed workers with this combination of o, $z_t = z$, and $x_t = x_h$ (next period) can be divided into four categories. The first term corresponds to surviving workers currently unemployed in the same occupation o with occupational human capital $x_t = x_h$, who decide not to change occupations. The second term in equation (3.9) corresponds to surviving workers who were employed in the same occupation o with occupational human capital x and did not receive the occupational transfer shock, but had their match destroyed, or received the transfer shock and chose to destroy their match. Here, the inclusion of $\hat{d}(p, \tilde{z}, x_h)$ rather than $d(p, \tilde{z}, x_h)(1 - \psi)$ reflects that this term reflects both channels. In particular, one could think of this as representing $\hat{d}(p, \tilde{z}, x_h) = d(p, \tilde{z}, x_h)(1 - \psi) + \psi \int_{\underline{z}}^{\bar{z}} \max \mathbb{1}_{W^E(p', \tilde{z}, x') < W^U(p', \tilde{z}, x')} dF(\tilde{z})$. Finally, the third term corresponds to those who are unemployed in a different occupation ($\tilde{o} \neq o$) and decide to switch, and the fourth term corresponds to newborn workers. As these workers move to a random occupation, they will go to occupation o with probability $1/(O - 1)$ if they came from a different occupation and with probability $1/O$ if they are newborn. Furthermore, as these workers will have the lowest level of occupational human capital (x_1) in their new occupation, these terms only apply if $x_h = x_1$.

For employed workers, there are five channels through which a worker can be employed next period in occupation o, with idiosyncratic productivity $z_t = z$ and occupational human capital $x_t = x_h$. These five ways are reflected by the five different terms in equation (3.10):

$$
\begin{aligned}
\frac{e_o'(z, x_h)}{1 - \phi} = &\int_{\underline{z}}^{\bar{z}} \lambda(\theta(p, \tilde{z}, x_h))(1 - \rho^u(p, \tilde{z}, x_h))u_o(\tilde{z}, x_h) dF(z|\tilde{z}) d\tilde{z} \\
&+ \chi(x_h|x_h) \int_{\underline{z}}^{\bar{z}} (1 - \hat{\rho}(p, \tilde{z}, x_h))(1 - d(p, \tilde{z}, x_h))(1 - \psi)e_o(\tilde{z}, x_h) dF(z|\tilde{z}) d\tilde{z} \\
&+ \mathbb{1}_{h>1} \left[\chi(x_h|x_{\tilde{h}}) \int_{\underline{z}}^{\bar{z}} (1 - \hat{\rho}(p, \tilde{z}, x_{\tilde{h}}))(1 - d(p, \tilde{z}, x_{\tilde{h}}))(1 - \psi)e_o(\tilde{z}, x_{\tilde{h}}) dF(z|\tilde{z}) d\tilde{z} \right] \\
&+ \left[(\mathbb{1}_{h=1}) \sum_{\tilde{o} \neq o} \sum_{h=1}^{H} \int_{\underline{z}}^{\bar{z}} [1 - d(p, \tilde{z}, \tilde{x}_h)] \rho^e(p, \tilde{z}, \tilde{x}_h) \lambda(\theta(p, z, x_1))(1 - \psi) e_{\tilde{o}}(\tilde{z}, \tilde{x}_h) d\tilde{z} \right. \\
&\left. + \sum_{\tilde{o} \neq o} \left[\int_{\underline{z}}^{\bar{z}} \mathbb{1}_{W^E(p', \tilde{z}, x_h) \geq W^U(p', \tilde{z}, x_h)} \psi e_{\tilde{o}}(\tilde{z}, x_h) d\tilde{z} \right] \right] \frac{dF(z)}{O - 1}
\end{aligned}
\tag{3.10}
$$

Here, the first term corresponds to workers who are currently unemployed in the occupation of interest o, did not decide to change occupations, and subsequently matched with a firm. Similarly, the second and third term in equation (3.10) correspond to workers who are currently employed in this occupation, did not match with a firm in a different occupation, and either remained in the occupational human capital level of interest x_h (second term) or moved up to x_h from the previous level $x_{\tilde{h}} = x_{h-1}$ (third term), defining $\hat{\rho}(p', z', x')$ to equal $\rho^e(p', z', x') \int_{\underline{z}}^{\tilde{z}} \lambda(p', \tilde{z}, x_1) dF(\tilde{z})$. The fourth term corresponds to workers who are employed in a different occupation, but switched occupations by searching while on the job (and matching with a firm in occupation o), and the fifth term corresponds to those who are employed in a different occupation and switched occupations due to the occupational transfer shock. Finally, note that the entire expression is divided by $1 - \phi$ (the probability of staying alive) to account for the fact that newborn workers always start unemployed.

3.3.4 Equilibrium

In this paper, the equilibrium of interest will be a block-recursive equilibrium (BRE). This type of equilibrium has the advantage of allowing me to solve for the agents' decisions without taking into account the distribution of workers and firms across occupations, and productivity levels. This feature greatly reduces the computational costs of solving (and simulating) the model. The BRE is defined as follows:

Definition 1. *A block-recursive equilibrium (BRE) consist of a set of value functions* $W^U(p, z, x)$, $W^E(p, z, x)$, $J(p, z, x)$, $V(p, z, x)$, *policy functions* $d(p, z, x)$, $\rho^u(p, z, x)$, $\rho^e(p, z, x)$, $\sigma(p, z, x)$, *labour market tightness function* $\theta(p, z, x)$, *wage function* $w(p, z, x)$, *and laws of motion for* p, z, x, u_o, *and* e_o *such that:*

1. *The value functions and policy functions solve the worker's and firm's problems as described by equations* (3.4), (3.5), (3.7), *and* (3.8)

2. *Free entry in labour markets:* $V(p, z, x) \leq 0$, *and* $\theta(p, z, x) = 0$ *if* $V(p, z, x) < 0$.

3. *Wages* $w(p, z, x)$ *solve the Nash bargaining problem in equation* (3.3)

4. *The laws of motion for* u_o *and* e_o *satisfy equations* (3.9) *and* (3.10)

Proposition 1. *The model has a unique block-recursive equilibrium.*

Proof. See appendix C.2.1 □

The proposition justifies a complete focus on block-recursive equilibria when it comes to solving the model in order to obtain the quantitative results in Section 3.5. However, when comparing the resulting equilibrium functions with those obtained by solving the social planner's problem, it can be noticed that the two sets of functions do not (necessarily) coincide, as stated in the following proposition:

Proposition 2. *Unless* c^e *is prohibitively high for all* p *or* $\lambda(p, z, x_1) = 0$ *for all* (p, z), *the block-recursive equilibrium is not constrained efficient.*

Proof. See appendix C.2.2 □

As can be seen in the proof, the inefficiency of the decentralized solution is caused by the reallocation decision of the employed worker, ρ^e. When deciding whether to search in a new occupation, the worker takes into account only her own value of the current match when considering the value lost upon reallocation. Thus, the worker fails to take into account that if she reallocates to another occupation, the firm that currently employs her will also lose its value of the match, J. Thus, it can be expected that the worker will decide to search in another occupation more often than the social planner would allow her to. Note that this result deviates from the efficiency results obtained in other papers, such as Carrillo-Tudela and Visschers (2021) and Menzio and Shi (2011). In the case of Carrillo-Tudela and Visschers (2021) this deviation is not surprising, as the element of my model that causes the departure from constrained efficiency is not present in their model. On the other hand, the model in Menzio and Shi (2011) does include on-the-job search, and would thus be subject to the same issue. The difference between my model and the model in Menzio and Shi (2011) is that in the latter model it is assumed that the contract between the worker and firm specifies when exactly the worker is allowed to search (and where she would search in that case). In that case, as the contract is negotiated between the worker and the firm, the decision to search does take into account the lost value for the firm when the worker reallocates, thus getting around the issue that leads to the departure from constrained efficiency in my model. In principle, my result implies that one cannot use the social planner's problem to solve the model instead of solving the worker and firm problem. However, the result does provide me with the opportunity to evaluate how important this inefficiency is, by solving the social planner's problem and comparing the solution to the solution of the worker and firm problems.

3.4 Calibration

For the purpose of the estimation of the model in the previous section, I assume that the production function takes the simple form $y = pzx$ and the matching function takes the form $M(u,v) = \frac{uv}{u+v}$. Finally, I slightly simplify the model by assuming that the cost of changing occupations does not change over the business cycle, i.e. $c^e(p) = \bar{c^e}$ and $c^u(p) = \bar{c^u}$.[40] The model presented in the previous section is then characterized by a total of $17 + H$ parameters[41], where the distribution of each of the productivity variables p and z is characterized by three parameters, which govern the mean value, persistence, and volatility of the corresponding variable. As I follow Carrillo-Tudela and Visschers (2021) in setting $H = 3$, there leaves a total of 20 parameters to be calibrated. For the purpose of the simulation, I divide these parameters into two groups. One of these groups contains the parameters whose values are determined in the calibration, and the parameters in the other group are set directly.

The set of parameters that are determined outside of the calibration is $\{O, x_1, \mu_p, \phi, \chi, \beta, b, \eta\}$. The number of occupations O is set to 6, consistent with the number of 1-digit occupations considered in section 3.2. Both parameters x_1 (the lowest level of occupational human capital) and μ_p (the mean value of aggregate productivity p) are furthermore normalized to 1. The probability of death, ϕ, is set such that an individual "lives" for 40 years on average. Given that the model period corresponds to approximately a week (so that four periods correspond to a month), this average survival rate yields a value of $\phi = 1/1920 \approx 0.00052$. Similarly, χ (the probability of moving to the next level of occupational human capital) is set so that employed workers on average reach the next level after 5 year, so that the associated level of $\chi = 1/240 \approx 0.0042$. In order to set the discount rate β, I then use the value of ϕ, together with a yearly interest rate of 4% (so that $r = 1.04^{1/48}$), to set

[40] In appendix C.4, I show how the estimation results change when allowing these costs to vary by the aggregate productivity p, i.e. either $c^e(p) = p \cdot \bar{c^e}$ and $c^u(p) = p \cdot \bar{c^u}$ or $c^e(p) = \bar{c^e} + p \cdot \bar{c^e}$ and $c^u(p) = \bar{c^u} + p \cdot \bar{c^u}$.

[41] Recall that H stands for the number of values the occupational human capital variable x_h can take.

the discount rate $\beta = (1-\phi)/(1+r) \approx 0.9987$. Similarly, the value of unemployment benefit b is set to 0.738, corresponding to the value found when estimating the model from Carrillo-Tudela and Visschers (2021) without occupation targeting,[42] while the firm's bargaining weight η is set to 0.5, corresponding to symmetric bargaining.

Given the above parameters, the remaining 12 parameters $\{\sigma_p, \sigma_z, \rho_p, \rho_z, \mu_z, k, c^u, c^e, x_2, x_3, \delta, \psi\}$ are estimated in the calibration to match a set of 26 moments as close as possible.[43] As the model is overidentified, it is not surprising that I am unable to match these moment exactly. The model values for these moments (which are discussed briefly below) and their data counterparts can be found in Table 3.2. The calculation method for these moments can be found in appendix C.3. While the calibration does not restrict certain moments to inform a specific parameter (rather, the calibration minimizes the sum of squared distances to all targets), most of the moments are chosen with a certain parameter in mind, as discussed below

Moment	Source	Data	Model
Average job-finding rate	SIPP	0.468	0.8358
Average proportion of employed workers experiencing 1+ unemployment spell in the next year	SIPP	0.051	0.0767
Average aggregate productivity	Normalization	1	1.7205
Persistence of aggregate productivity	BLS	0.719	0.8878
Volatility of aggregate productivity	BLS	0.009	0.0562
Returns to occupational experience (5 years)	KM09	0.1616	0.138
Returns to occupational experience (10 years)	KM09	0.2526	0.2951
Unemployment rate of unexperienced workers	SIPP	0.072	0.0443
Unemployment rate of experienced workers	SIPP	0.049	0.0049
Unemployment survival rate (4 months)	SIPP	0.560	0.1647
Unemployment survival rate (8 months)	SIPP	0.387	0.0307
Unemployment survival rate (12 months)	SIPP	0.295	0.0064
Occupational mobility rate for workers unemployed for at least 1 month	SIPP	0.431	0.5285
Occupational mobility rate for workers unemployed for at least 3 months	SIPP	0.473	0.8207
Occupational mobility rate for workers unemployed for at least 6 months	SIPP	0.474	0.8475
Occupational mobility rate for workers unemployed for at least 9 months	SIPP	0.473	0.8659
Occupational mobility rate for workers unemployed for at least 12 months	SIPP	0.470	0.875
Subsequent mobility rate	SIPP	0.741	0.946
Relative occupational mobility rate of unexperienced workers	SIPP	1.077	1.9808
Occupational mobility rate for employed workers	SIPP	0.036	0.0349
Relative occupational mobility rate for unexperienced employed workers	SIPP	2.156	1.3295
Occupational mobility rate for employed workers without employer change	SIPP	0.011	0.029
Fraction of occupational transfers going through unemployment	SIPP	0.175	0.1161
Coefficient $\hat{\gamma}$ in equation (3.11)	SIPP	2.13	2.0414
Coefficient $\hat{\gamma}$ in equation (3.12), E-switchers	SIPP	-0.001	0.3139
Coefficient $\hat{\gamma}$ in equation (3.12), U-switchers	SIPP	0.015	0.3546

Table 3.2: *The moments targeted in the calibration. The second column names the source of the data counterpart of the moment. KM09 refers to Kambourov and Manovskii (2009b), and "SIPP"/"BLS" means that the data counterpart of the moment was calculated using the dataset created from the SIPP or using data on productivity from BLS.*[44]

[42]Note that this value of b is fairly close to the value of 0.716 found in Menzio and Shi (2011).

[43]To be specific, I set the parameters to minimize the sum of square differences between model and data moments.

The first set of moments is selected to inform the aggregate productivity process and the occupational human capital grid values. Some of the productivity parameters have a clear counterpart in the data. For example, the data counterparts parameters σ_p and ρ_p (the standard deviation and persistence of aggregate productivity) are the persistence and volatility of aggregate productivity. To obtain the corresponding persistence and volatility of aggregate productivity in the data, I apply an HP filter with smoothing parameter on the quarterly "Real Output Per Person" data from the Bureau of Labor Statistics (BLS). Similarly, since the parameters x_2 and x_3 are related to the additional wage that an individual might receive if his occupational human capital is higher, these parameters can be calibrated to match the returns to occupational experience. Specifically, the moments used here are the returns to 5 and 10 years of occupational experience since the parameter χ is set so that an individual reaches the next level after 5 years (on average). The data counterparts of these moments calculated using the results in Kambourov and Manovskii (2009b).

The next set of moments corresponds to the idiosyncratic productivity process and the cost of reallocation for an unemployed worker (c^u). The average value of the idiosyncratic productivity is set such that the average productivity equals 1. This normalization is made so that it is easier to interpret the parameters and equilibrium objects that have a monetary interpretation, such as c^u and the wage $w(p, z, x)$. In order to estimate the remaining parameters of the idiosyncratic productivity process as well as the reallocation cost c^u, I include the 4-, 8-, and 12-month unemployment survival rate, the occupational mobility rate for workers who are unemployed either 1, 3, 6, 9, or 12 months, the subsequent mobility rate, and the relative occupational mobility rate of unexperienced workers.[45]

The third set of moments targets the parameters that govern the worker's job-to-job occupational mobility decision (in particular cost c^e) as well as the occupational transfer shock ψ. In order to set these parameters, I include the average occupational mobility rate for employed workers (also displayed in figure 3.3), the occupational mobility rate for unexperienced employed workers (relative to experienced workers), and the occupational mobility rate for employed workers specific to switches made without an employer change.

A number of parameters also appear in the basic search and matching model.[46] The moments corresponding to these parameters are generally moments that can also be calculated with the basic model. Specifically, the moments corresponding to the parameters δ (exogenous separation rate) and k (vacancy cost) are the proportion of employed workers experiencing at least one unemployment spell in the next year, the unemployment rates of the unexperienced and experienced worker, and the average job finding rate.

Finally, in order to reasonably match the patterns uncovered in section 3.2 of this chapter, I target the average fraction of occupational switches going through unemployment, as well as a set of results from simple regression estimations. In particular, I regress the aforementioned fraction of occupational switches going through unemployment on the unemployment rate at the time of the materialization of the shock, as displayed in equation (3.11), thus essentially fitting a line through

[44] In particular, the data I use to calculate the persistence and volatility of aggregate productivity is the "Real Output per Hour of All Persons" time series for the Nonfarm Business Sector. I use only the time period corresponding to the sample period of the SIPP.

[45] As I do not observe experience in my data, I define an individual to be experienced if he is aged between 35 and 55 and I define an individual to be unexperienced if he is aged between 20 and 30. Of course, the worker's age does not map directly into the worker's experience in his occupation, especially if the worker changes occupation relatively late in his working life. However, it is not unreasonable to expect age and experience to be strongly correlated, thus making age a good proxy for experience.

[46] For an overview of the basic search and matching model, see for example Pissarides (2000).

the scatter plot displayed in the right panel of figure 3.2. Finally, I also regress the wage differentials experienced by either occupational U-switchers or E-switchers on this same unemployment rate at the time of the materialization of the shock, as displayed in equation (3.12):

$$\text{Frac}_t^u = \alpha + \gamma U_t + \varepsilon_t \tag{3.11}$$

$$\Delta w_t = \alpha + \gamma U_t + \epsilon_t \tag{3.12}$$

O	x_1	μ_p	ϕ	χ	β	b	η
6	1	1	0.0005	0.0042	0.9987	0.738	0.500

σ_p	σ_z	ρ_p	ρ_z	μ_z	k	c^u	c^e	x_2	x_3	ψ	δ
0.037	0.051	0.986	0.987	1.036	1.579	-0.691	0.258	1.39	1.883	0.002	0.0037

Table 3.3: *Values of parameters used to obtain the results in Section 3.5. Parameters are either be determined by calibration (bottom table) or set outside of the calibration (top table).*

Table 3.3 lists the parameter values for both groups of parameters. The parameter value that stands out here is the cost of changing occupations through unemployment c^u, which is calibrated to be negative. Naturally, this negative cost results in newly unemployed workers being very likely to choose to switch occupations, especially when they did not accumulate any occupational human capital, as reflected in the results in section 3.5. Table 3.3 also shows that the calibrated values for k is fairly high, reflecting that the cost of posting a vacancy is close to one period's worth of output. On the other hand, the cost of switching occupations on the job is fairly low, while still positive, with the cost c^e corresponding to less than a quarter of a period's worth of output.

The model counterparts of the moments used in the calibration exercise can be found in the fourth column of Table 3.2. As can be seen by comparing these moments generated by the model with those generated by the data (in the third column of Table 3.2), the model has trouble matching several features of the data. In particular, the model generates very high job finding rates, leading to excessively low unemployment survival rates. As shown in section 3.5, this is primarily driven by newly unemployed workers often immediately choosing to switch occupations. Despite this, the model generates too many job-to-job occupational switches compared to switches through unemployment, although it is worth noting that the cyclical pattern of the fraction of of occupational transfers going through unemployment, a key moment for the purpose of this chapter, is matched quite well. Furthermore, the model does reasonably well when it comes to the returns to occupational experience, and the occupational mobility rate for employed workers.

3.5 Results

3.5.1 Model Fit

From the discussion in the previous section it can be concluded that the fit of the model in terms of the targeted moments is not perfect. However, despite these imperfections, the model does seem to be able to match the distinctive patterns of U- and E-switching over the business cycle. In this subsection, I will further explore these patterns in the model, as well as their implications for the switchers' subsequent earnings.

3.5.1.1 Mobility Rates

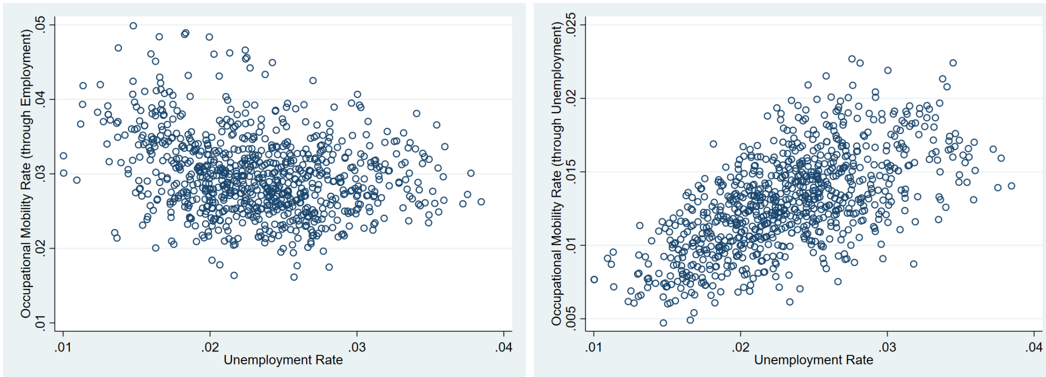


Figure 3.11: *The occupational U-mobility rate (counting only U-switchers, left) or E-mobility rate (counting only E-switchers, right), plotted against the corresponding month's unemployment rate, using model-generated data.*

In figure 3.11, I show the cyclical patterns of the occupational U-mobility (left panel) and E-mobility rates (right panel). In both panels, the scale of the horizontal axis clearly reflects the fact that the model implies an excessively low unemployment rate throughout the panels. As can be seen in the left panel of the figure, the model predicts the occupational E-mobility rate to be slightly procyclical, which is in line with the observations I made from the SIPP in section 3.2 and appendix C.1.2. The occupational U-mobility rate, on the other hand, is found to be countercyclical, as seen in the right panel of figure 3.11. The model needs this countercyclical U-mobility rate in order to generate the cyclical pattern in the fraction of occupational switchers going through unemployment, which was targeted more directly in the estimation and therefore matches quite well with the pattern found in the data, as can be seen in figure 3.12.

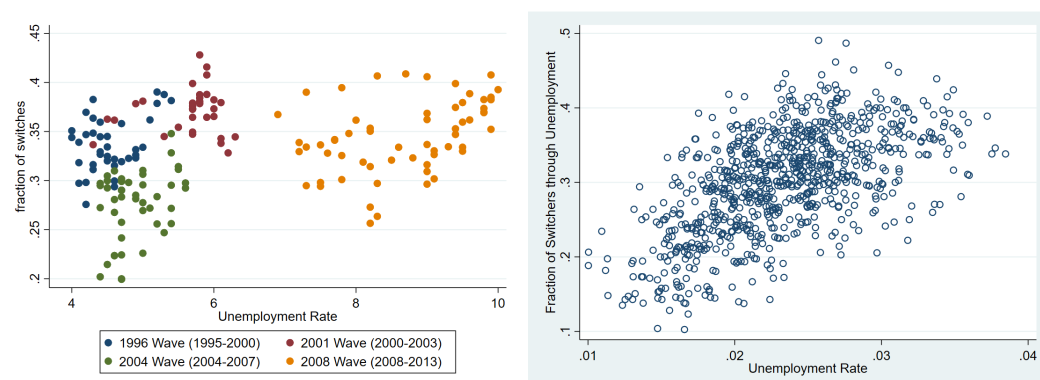


Figure 3.12: *The fraction of occupational switchers going through unemployment and the corresponding month's unemployment rate, as found in the data (left) and as generated by the model (right).*

Focusing on the underlying mechanisms that generate the patterns discussed above, it should be recognized that there are three decisions made in the model (the separation decision and the reallocation decision for both unemployed and employed workers). Essentially, all three of these decisions are binary decisions: either the worker decides to separate/reallocate or she decides not to. In fact, the structure of the model is such that for every combination of p and x, one can find a threshold value of z below which the worker decides to separate/reallocate, and above which the worker decides not to do so. These threshold functions are plotted in the left panel of figure 3.13. As can be seen in the figure, all the action in the model is taking place at the low occupational human capital level x_1: once the worker progresses to the second of the three levels of occupational human capital, she is no longer willing to (voluntarily) switch occupations or destroy her match with the firm.

It is also worth noting that in the left panel of figure 3.13, the reallocation threshold for unemployed workers lies above the separation threshold at all values of aggregate productivity p. This implies that any worker who decides to destroy her match with her employer will also decide to switch occupations in the next period, as long as she does not receive a large positive shock in her idiosyncratic productivity in the next period. This also implies that the model does not feature any rest unemployment, as workers who are situated below the separation threshold, and therefore face a zero probability of matching in the current period, decide to switch occupations instead of waiting for conditions to improve. This is confirmed in appendix C.4.1, where I decompose the model-generated unemployment into rest, reallocation, and search unemployment.

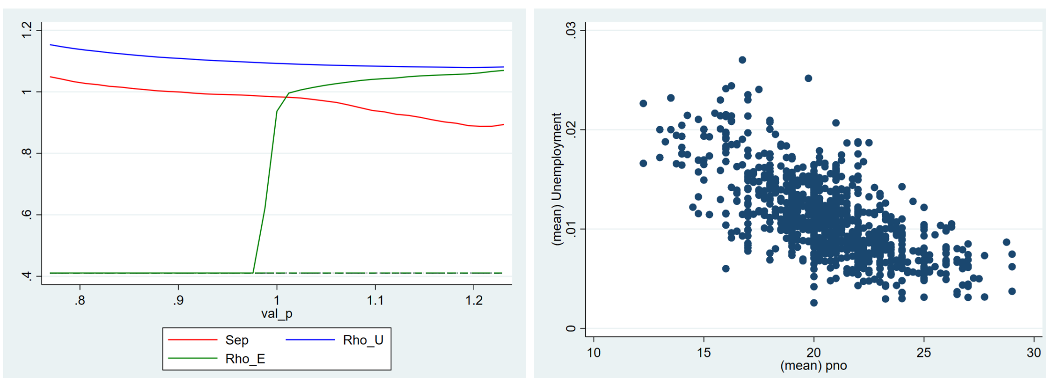


Figure 3.13: *Thresholds, helps for explanation; with pno-unemp The fraction of occupational switchers (1-digit) going through unemployment and the corresponding month's unemployment rate from the BLS, over time (left) and plotted against each other in a scatter plot (right).*

Shifting the focus to cyclical patterns, it can be observed in the left panel of figure 3.13 that the thresholds for on-the-job reallocation are generally increasing in aggregate productivity, while the thresholds for separation and reallocation during unemployment are (mildly) decreasing in aggregate productivity. Keeping everything else constant, these patterns cause a countercyclical U-mobility rate, a countercyclical unemployment rate, and a procyclical E-mobility rate. For the unemployment rate, this pattern is confirmed in the right panel of figure 3.13, which plots the model-generated unemployment rate against the aggregate productivity, generating a noisy but clearly downward sloping pattern.

The fact that the decision thresholds shown in the left panel of figure 3.13 are not all constant in aggregate productivity p is also visible in Figure 3.14, which shows the distribution of unemployed (left) and employed (right) workers over different combinations of values of idiosyncratic productivity z and aggregate productivity p. Looking at the left panel, for unemployed workers, the threshold for reallocation (while unemployed at the lowest level of occupational human capital) is clearly visible. The high concentration below this threshold also indicates that a majority of the workers in the simulation have a higher level of occupational human capital. This is also reflected in the right panel, which shows the distribution of employed workers, and does not show a clear threshold.

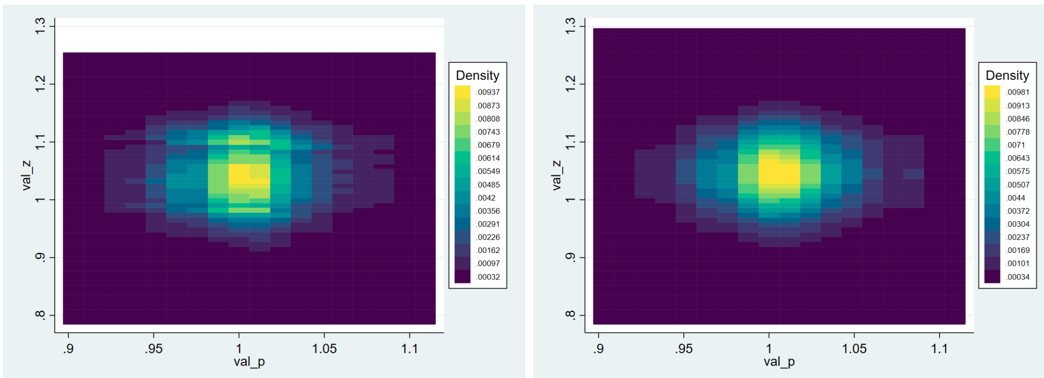


Figure 3.14: *The distribution of unemployed (left) or employed (right) workers over different combinations of aggregate productivity p and idiosyncratic productivity z, generated from the model simulation. The heatmaps are generated separately for each value of p, so the values are relative to the total number of (un)employed workers with the same value of p.*

3.5.1.2 Subsequent Earnings

While the cyclical patterns of the mobility rates, and especially the cyclicality of the fraction of occupational switchers going through unemployment, were in part explicitly targeted when estimating the model in section 3.4, this is not the case for the subsequent earnings paths experienced by occupational switchers. When it comes to these patterns, the only directly related moments targeted in the estimation are the regression coefficients on equation (3.12) (specific to either U-switchers or E-switchers). These moments, however, only take into consideration the wage earned by the switcher directly after the switch materialized, and is therefore not informative on the subsequent earnings path experienced by these workers. In this subsection, I explore how the model performs in matching these subsequent earnings paths, as discussed in section 3.2.

In figure 3.15, I show the results of an estimation using the three-step estimation method from Borusyak et al. (2021) on real earnings, where I do not distinguish between E-switchers and U-switchers. The left panel shows the results obtained from the data, as shown earlier in the right panel of figure 3.9, whereas the right panel repeats the same estimation on model-generated simulation data. Comparing the two figures, it can be seen that the model overshoots the earnings lossed observed in the data, regardless of the economic conditions at the time of the occupational switch. However, it is worth mentioning that the model is consistent with the data in not identifying a clear cyclical pattern in the earnings paths experienced by occupational switchers after the materialization of their switch.

In figure 3.16 I display the results of repeating the estimation while allowing for different treatment effects for U-switchers and E-switchers. Here, the left panel repeats the observation from the data, and corresponds to the right panel of figure 3.10. As can be seen by comparing the findings in the

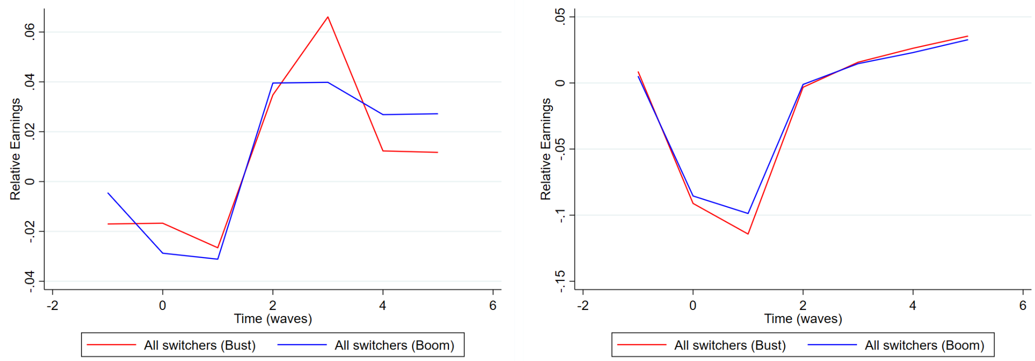


Figure 3.15: *The effect of occupational switches on real earnings, relative to the counterfactual of never switching, using estimated coefficients obtained using the three-step estimation method. The plots show separate estimates of the effect for switches materializing during a boom or a bust, and compare the estimates from the data (left) and the estimates obtained from model-generated simulation data (right).*

left panel to those obtained from model-generated simulation data, as displayed in the right panel of figure 3.16, the difference between E-switchers and U-switchers is fairly similar between the data and the model. However, it is worth noting that the model does not match the countercyclicality of the post-switch earnings paths for U-switchers as observed in the data, and furthermore suggests a faster recovery in earnings than I found in the data. Especially the latter of these two observations is likely to be a consequence of the excessively high job finding rates generated by the model, which were pointed out earlier in section 3.4.

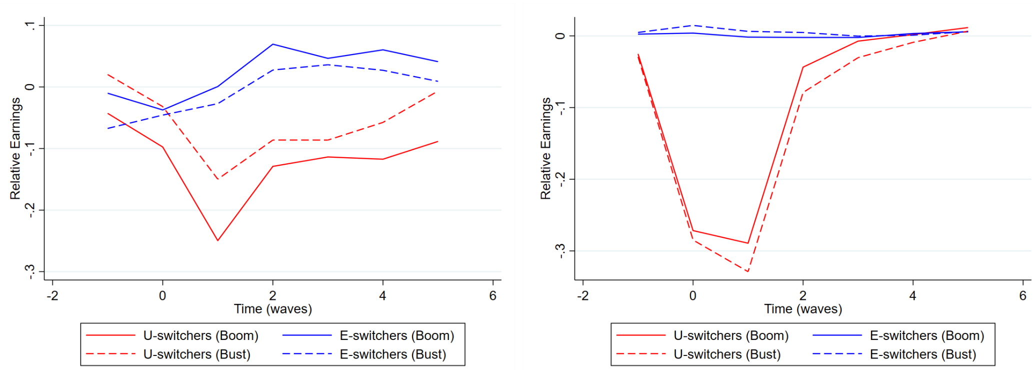


Figure 3.16: *The effect of occupational switches on real earnings, relative to the counterfactual of never switching, by type of switch, using estimated coefficients obtained using the three-step estimation method. The plots show separate estimates of the effect for switches materializing during a boom or a bust, and compare the estimates from the data (left) and the estimates obtained from model-generated simulation data (right).*

3.5.2 Model Implications

In the previous subsections, I showed that the model is able to generate the countercyclical fraction of occupational switchers going through unemployment that I observed in the data in section 3.2, and furthermore generates a fairly acyclical pattern in terms of earnings experienced by (all) occupational switchers after the materialization of their switch. Earlier in this chapter, I hypothesized that this acyclicality of the earnings path likely masks the effects of a composition change, where many workers do worse after an occupational change simply because they switched through unemployment rather than through a job-to-job transition. As the model is able to generate such compositional changes, I will now proceed to analyze the extent to which these compositional changes drive the cyclicality of the post-switch earnings path.

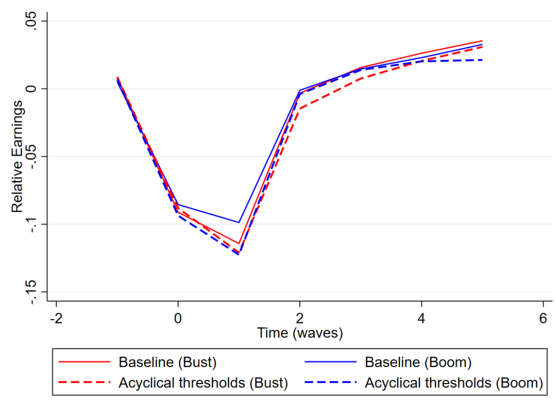


Figure 3.17: *The impact of cyclical reallocation thresholds on the model-generated (BJS) effect of occupational switches on real earnings, relative to the counterfactual of never switching, and aggregated separately for switches materializing during a boom or a bust. The figure combines the estimates obtained from the baseline model (solid) and a model in which separation and reallocation thresholds are forced to be constant in aggregate productivity (dashed).*

In figure 3.17, I show how the model-based estimation of the cyclicality of the post-switch real earnings paths changes if I force the reallocation thresholds to be constant over the business cycle. In particular, I generate this figure by forcing workers to base their separation and their reallocation decision for both unemployed and employed workers on a comparison of values at the average aggregate productivity level, $p = 1$, rather than the current aggregate productivity level. The result of this change is that the thresholds in the left panel of figure 3.13 would be horizontal at their level for $p = 1$ rather than decreasing in p (for separation and reallocation through unemployment) or increasing in p (for job-to-job reallocation).

As can be seen in figure 3.17, taking out this cyclical component has a fairly mild impact on the estimated post-switch real earnings path. This is because, as shown in figure 3.13, the reallocation and separation thresholds are fairly flat in this estimation, and thereby did not change much when forcing it to be completely horizontal. The exception to this is the job-to-job reallocation threshold, which is now forced to be positive in a recession whereas it previously dropped off at values of p slightly below 1. This may explain why the average earnings losses after an occupational switch in a bust have slightly worsened for all years after the materialization takes place. Furthermore, it is worth noticing that removing the cyclicality of the thresholds has reversed the overall pattern from very mildly countercyclical to very mildly procyclical.

One important way in which the model in this chapter differs from most models in the existing literature is by including the possibility of switching occupations without having to go through

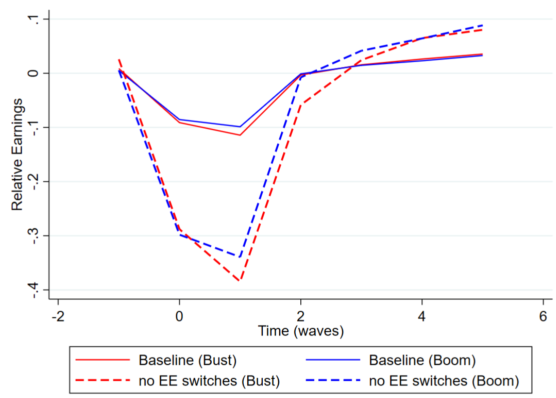


Figure 3.18: *The impact of including job-to-job transitions on the model-generated (BJS) effect of occupational switches on real earnings, relative to the counterfactual of never switching, and aggregated separately for switches materializing during a boom or a bust. The figure combines the estimates obtained from the baseline model (solid) and a model without either type of job-to-job occupational transfers (dashed).*

unemployment. As seen in the data as well as in the model (in figure 3.16), workers who switch through unemployment tend to experience much larger earnings losses than job-to-job switchers, who in most cases do not face any losses at all. Naturally, including these E-switchers therefore has a large impact on the earnings losses after an occupational switch as implied by the model. This is stressed in figure 3.18, which plots estimates of the average earnings loss after an occupational switch in a version of the model without job-to-job reallocation, compared to the baseline model. As can be seen in the figure, only allowing for switches through unemployment implies that the model will predict very large consequences of an occupational switch on subsequent earnings, which is not representative in practice for workers who decided to switch while staying on the job.
In this chapter's model, I allow for two distinct ways of switching occupations while staying on the job. The first channel is an (exogenously imposed) reallocation shock which forces the worker to reallocate but not separate from her employer, whereas the second channel is driven by the worker's (endogenous) choice to search for a job in a different occupation and with a different employer. In figure 3.19, I decompose the cyclicality of the average earnings consequences of an occupational change into these two channels, as well as a residual channel, which reflects the remaining cyclicality after taking out all job-to-job transitions and therefore reflects the difference between the two dashed lines in figure 3.18. In order to accurately assign contributions to each of the job-to-job transition channels, I use a Shapley-Shorrocks decomposition (see Shorrocks, 2013), which aims to calculate contributions that are independent of the order in which the channels are switched off.[47]

As can be seen in figure 3.19, the exogenous reallocation shock has a large impact on the cyclicality of the average earnings consequences of an occupational change, and this impact is consistently negative, thus indicating that this reallocation shock pushes the earnings losses to be countercyclical.

[47] In practice, this means that I do the decomposition for each possible order of channels separately, and then take the average across decompositions for each channels. For a decomposition such as this one, in which I am only interested in two channels, this is fairly straightforward and only requires one additional simulation. However, in a decomposition with more channels, such as the one in chapter 2, this becomes infeasible as the number of required simulations explodes.

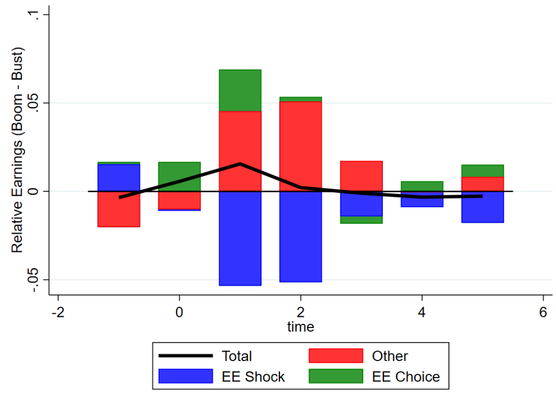


Figure 3.19: *The impact of including job-to-job transitions on the model-generated (BJS) cyclicality in the effect of occupational switches on real earnings. The figure displays the separate impact of the occupational transfer shock and job-to-job occupational transfer choices, obtained using a Shapley-Shorrocks decomposition.*

The endogenous choice channel as well as the residual channel, however, are generally pushing the earnings consequences to be procyclical. In other words, while this decomposition suggests that the composition effect that ensures that the fraction of switchers through unemployment is larger in a recession has a procyclical effect on average earnings losses after an occupational switch (as evidenced by the positive impact of including endogenous job-to-job transitions on the cyclicality), this is offset by the acyclical reallocation shock, which goes in the other direction. This result therefore points to a need to further explore these transitions that occur on-the-job and within a firm in order to fully understand all the cyclical properties as implied by this model.

3.6 Conclusion

In this chapter, I study occupational mobility and its effect on subsequent earnings and wages, focusing in particular on the business cycle properties of these effects. In doing so, I distinguish between three types of occupational transitions: transitions that move through unemployment, job-to-job occupational transitions that also involve an employer change, and job-to-job occupational transitions without such an employer change. Using data from the Survey of Income and Program Participation, I find that the fraction of occupational switchers who switch through unemployment is countercyclical, and while these workers generally do worse in terms of earnings than workers who make a job-to-job transition, their earnings and wage patterns may slightly improve in recessions, whereas the patterns slightly deteriorate for occupational switchers on average. I argue that this reflects a composition effect, where deterioration of the average patterns are driven by a larger fraction of occupational switchers doing so through unemployment, and occupational transfers through unemployment generally being associated with large earnings losses.

In order to quantify the effect of this composition effect on the earnings patterns of occupational switchers, I propose a DMP-style job search model of occupational mobility, which includes each of the aforementioned three types of occupational mobility. In this model, the endogenous separation probability increases in a recession, while for a given separation probability workers are also more willing to search in a different occupation while unemployed. The willingness to search in a different occupation decreases for job-to-job transitions in a recession, thus generating the countercyclical fraction of occupational switchers who switch through unemployment as observed in the data. Quantifying the effect of this changing composition on the average earnings consequences of occupational mobility, I find that this composition channel pushes the earnings consequences to be more procyclical, but is offset by the (acyclical) within-firm occupational changes which drive the earnings consequences to be countercyclical.

While the model is fairly successful in generating a changing composition of occupational switchers over the business cycle, the fit of the model is fairly weak in some other dimensions. In particular, the model generates job finding rates that are too large, therefore leading to low unemployment rates. In future work, I plan to further investigate and address this apparent shortcoming of the model.

Even though this chapter extends the existing theoretical literature in a promising way, the model in this paper still has a number of limitations that can be addressed in future work. For example, in the model presented in this paper it is possible to search on-the-job in other occupations, but not in the worker's current occupation, which seems unrealistic. Furthermore, while the assumption of random reallocation to a new occupation seems supported by the data, it may be interesting to see whether results change when workers can direct their reallocation to a specific occupation, like in Carrillo-Tudela et al. (2021). Especially when one is interested in a more detailed occupational classification system (such as the 3-digit classification), directed search seems more realistic. Finally, the reallocation within the firm is currently taken as an exogenous shock. It would be interesting to further explore the within-firm occupational mobility of workers, especially given it's important role in explaining the cyclicality of the earnings patterns after occupational transfers. Future work that further investigates this channel could build on some existing work in this area, such as Papageorgiou (2018).

Other limitations of the model come from the assumptions that are made in the model. Most of these assumptions are common in the theoretical literature, but have been questioned or rejected in the empirical literature. One example is the assumption that wage determination takes place through Nash bargaining, where the value of unemployment is used by the worker as the outside option. This assumption has been subject to some discussion in some recent work, such as Moscarini and Postel-Vinay (2017), whose results suggest that rather than using the value of unemployment, the workers use a credible threat to quit once an alternative offer has arrived. Another simplifying assumption made in my paper is that everyone who searches for a job searches with the same intensity. However, several empirical papers have already indicated otherwise. In fact, Faberman and Kudlyak (2017) find that it seems to be those with a lower search intensity who find a job in a shorter time, suggesting that the matching probability depends on more than just the search intensity and labour market tightness. Finally, my model does not allow a worker to become inactive. However, when investigating the explanatory power of a search model for labour market outcomes during and after the Great Recession, Kroft et al. (2016) find an important role for transitions from inactivity to unemployed and back, while also suggesting a role for duration dependence in job-finding rates. I chose not to include these extensions in the model presented in this chapter, as they present a substantial complication to the model, without a clear benefit in terms of the goal of the model. However, given the empirical importance of several of these channels, and the model's current challenges in matching transitions out of unemployment, these are interesting extensions that should be explored in future work.

Finally, it is worth mentioning that while the Survey of Income and Program Participation is a fairly commonly used dataset to investigate occupational mobility, it comes with the limitation of fairly short panels, which might hamper the ability of the empirical methods to accurately estimate the earnings and wage paths experienced by occupational switchers. Future work could seek to further improve on these estimates by using rich administrative data (such as the one used in chapter 1) that allows one to follow an individual over an extended period of time.

Appendix A

Appendices to *Recall and the scarring effects of job displacement: Evidence from Germany*

A.1 Individual Summary Statistics

Section under Construction

	SIAB		LIAB	
Frequency	Mean	Std.Dev.	Mean	Std.Dev.
Age	41.29	9.897	40.57	9.53
Primeage (aged 35–60)	0.6922	0.462	0.6790	0.467
Gender (female)	0.4634	0.499	0.4037	0.491
Location (east)	0.1897	0.392	0.3647	0.481
Self-employed	0.0059	0.077	0.0021	0.046
Establishment Size	1,143.6	4,606.5	3,529.1	10,668.7
Establishment Tenure (days)	2,222.9	2,260.5	2,678.8	2,769.1
Job Tenure (days)	2,102.5	2,209.4	2,446.1	2,655.0
Yearly earnings (2015 Euros)[1]	17,848.7	15,796.3	31,764.98	19,599.34
Separation	0.1253	0.331	0.1192	0.324
Displacement	0.016	0.126	0.0216	0.145

Table A.1: *Summary statistics using the yearly sample. The table shows the estimated mean and standard deviation of a number of important variables, using the main sample from either LIAB or SIAB (as defined in section 1.2, without any of the further restrictions imposed for the estimation).* Pending Updated SIAB Numbers.

Table A.1 presents summary statistics on a number of worker-related variables used in the main analysis. In particular, it presents summary statistics on all important continuous and binary variables (categorical variables are discussed below). A few observations can be made from these

[1]In these yearly earnings, only earnings from employment are taken into account.

summary statistics, including some that were already mentioned in the main text. First, both datasets likely substantially undersample self-employed workers. This is because the structure of the social security system is such that self-employed workers would often not be recorded in the administrative data my datasets are based on. Second, female workers and workers residing in West Germany are underrepresented in both the LIAB and SIAB sample. Further, the LIAB has a much larger mean establishment size, which is an artifact of its sampling method (based on sampling establishments rather than individuals), larger mean yearly earnings, and a slightly higher displacement rate.

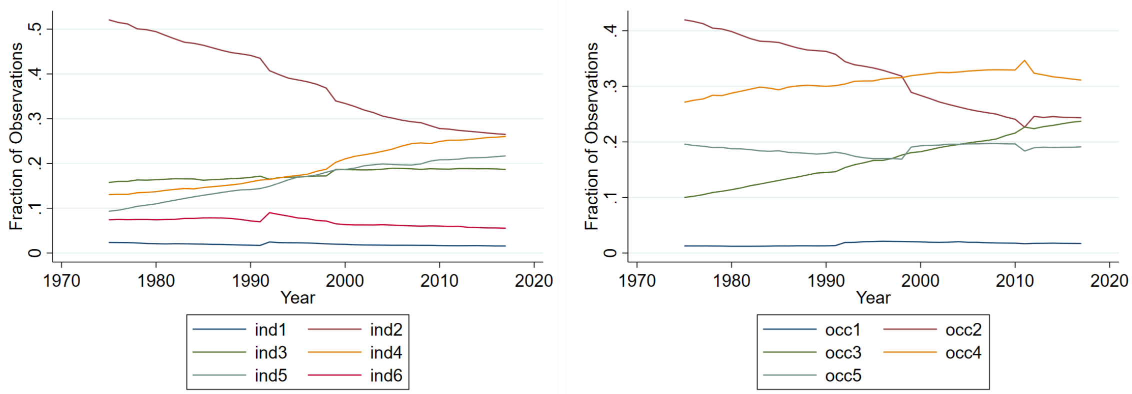


Figure A.1: *The fraction of observations by industry (left) and occupation (right) over time. The labels corresponding to the industry and occupation numbers can be found in footnote 2.*

Figure A.1 shows the fraction of observations accounted for by each major industry and occupation.[2] Looking at how the breakdown of industries and occupations evolves over time, it can be seen that industries and occupations related to manufacturing and construction (industry and occupation 2) seem to be declining over time, while most other industries and occupations are increasing their share of the total over time (with the exception of the “other” industries, category 6, and the occupation and industry related to agriculture). This could potentially be used in future work, comparing the scarring effect of separation and displacement by the industry or occupation of origin, and comparing the declining industry/occupation with the largest clearly growing industry/occupation.

As figure A.2 shows, it is not necessarily the case that the workers who switch industry or occupation after being displaced are primarily coming from industries/occupations in decline. After all, while the manufacturing-related industries and occupations (industry and occupation category 2) exhibited the strongest decline in terms of their relative size in the sample (as seen in figure A.1), these are generally not the industries and occupations associated with the highest mobility rate.

Figure A.3 shows the fraction of observations accounted for by each level of education and

[2] The major industries are defined as (1) Agriculture, Fishing, Mining, (2) Manufacturing, Utilities, and Construction, (3) Wholesale and Retail Trade, Hospitality, (4) Business Service Activities, (5) Education, Health, and other Community Services, and (6) Industries not otherwise classified (Public Administration, Private Households, Extra-Territorial).
The major occupations are defined as (1) Agriculture, Forestry, and Horticulture, (2) Manufacturing, Production Technology, and Construction, (3) Personal Services, (4) Business Related Services, and (5) Other Service Occupations.

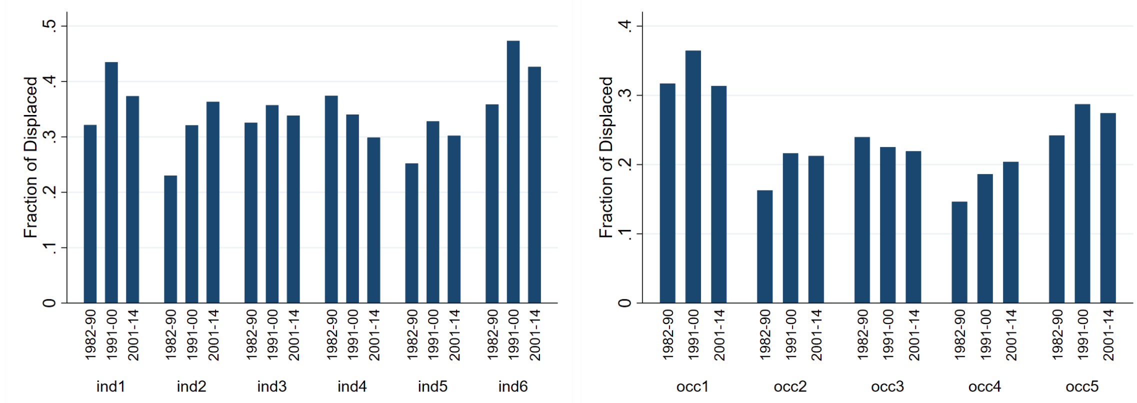


Figure A.2: *The fraction of displaced workers moving industry (left) and occupation (right), by former employing industry/occupation. The labels corresponding to the industry and occupation numbers can be found in footnote 2.*

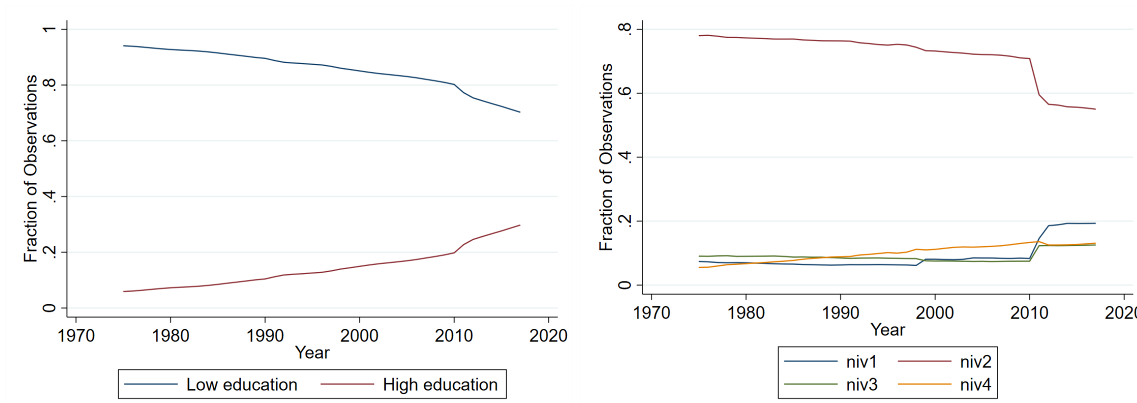


Figure A.3: *The fraction of observations by education (left) and occupational complexity (right) over time. For occupational complexity, a higher category corresponds to a more complex occupation.*

occupational complexity, measured in the data by the fifth digit of the occupational code. Here, it can be seen that the fraction of highly educated workers is increasing over time, although the increase is generally very gradual (prior to 2010). When it comes to occupational complexity, a similar trend can be found. However, as more than 60% of all jobs in any given year is of the second complexity level, and the lowest complexity level is also showing an increasing trend, it is fair to note here that this trend is much less pronounced.

It should be noted that the notion of occupation group and occupational complexity, though seemingly related, represent two distinct features of a job. In particular, it can be argued a worker can potentially move to an occupation within the same occupation group with a higher or lower complexity with relatively low associated costs. In fact, many job changes that one would consider to be promotions would likely show up in the data as a worker moving to a higher complexity level. At the same time, occupational mobility and occupational complexity switching often go together. Therefore, it seems natural to find the occupational complexity moving rate to be higher than the occupational mobility rate.[3]

A.2 Establishments in the Sample

As I classify workers as displaced if the establishment at which they were employed exits (and conditions on the worker are satisfied), it is worth summarizing what these exiting establishments look like. Below, I describe the exiting establishments in the SIAB sample, in terms of industry, age, size, and exit type. I also include a similar description of exiting establishments in the LIAB sample, to stress the similarity between the two datasets in these dimensions, despite the differences highlighted in the previous section.

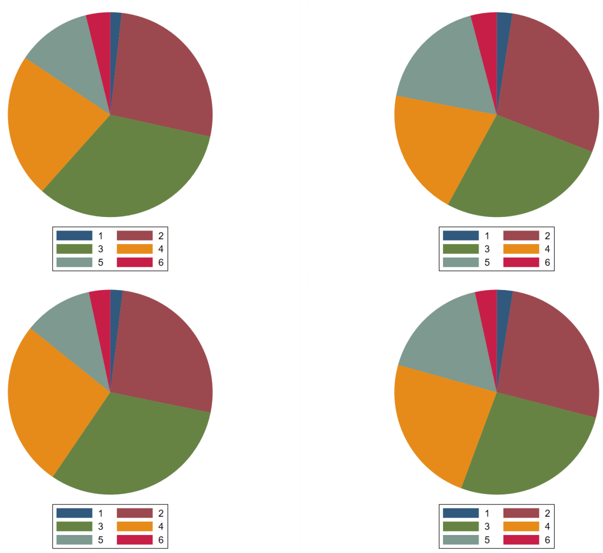


Figure A.4: *Breakdown of exiting establishments (left) and all establishments in the data (right) by major industry*[4]*, using data from the SIAB (top row) and LIAB (bottom row).*

[3] Conditional on displacement, the occupational complexity moving rate (in the LIAB) is 11%, whereas the corresponding occupational mobility rate is 7.9%. Furthermore, among the workers that switched occupation groups after displacement, the occupational complexity moving rate is 52.7%, and among workers that move between complexity levels after displacement the occupational mobility rate is 38%.

As shown in figure A.4, splitting out the exiting establishments by major industry and comparing this with the breakdown of all establishments in the data by major industry does not reveal any striking differences. Comparing the two charts, it can be said that industry 3 (Wholesale and Retail Trade, Hospitality) is slightly over-represented in the pool of exiting establishments, whereas industries 5 (Education, Health, and other Community Services) and 6 (Education, Health, and other related services) are slightly under-represented, but the two charts look similar enough to conclude that in general the pool of exiting establishments includes reasonable representation from all major industries.[5]

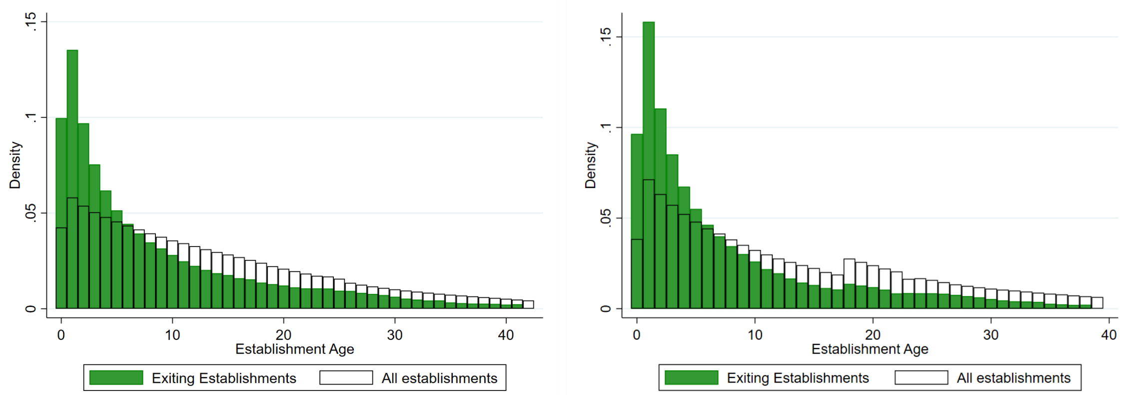


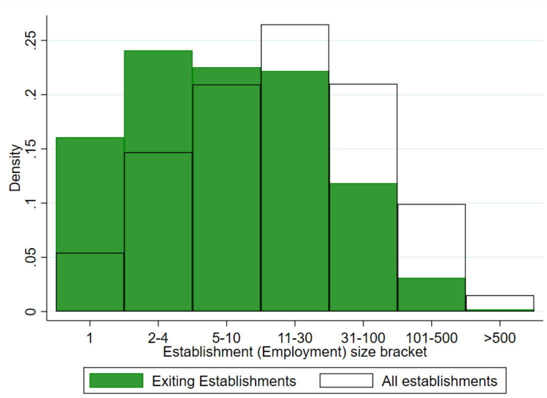
Figure A.5: *The distribution of the exiting establishments in SIAB (left) and LIAB (right) by establishment age.*

Figure A.5 shows how old exiting establishments tend to be when they exit. The figure shows that exiting establishment tend to be relatively young. This corresponds to observations made in the literature discussing firm exits (see for example Haltiwanger et al. (2013), who find that age and exits are important when considering the role of small business in accounting for job creation in the U.S.), where the consensus is that young firms tend to have a relatively low survival rate. Similarly, it can be concluded from figure A.6 that the exiting firms are disproportionately small in size, which also corresponds to existing evidence on the topic (discussed in Haltiwanger et al. (2013), among many others). In general, there are relatively few large establishments in the data, and this is true for both SIAB and LIAB. Note that this does not contradict the observation (made in section A.1) that individuals in the LIAB have a much larger mean establishment size, as the sampling method of the LIAB is such that even though not many large establishments are included, all workers employed at these establishments (in the sample period) are included in the dataset.

Since the dataset provides information on what happens to the majority of an establishment's former employees after an establishment exits, it is possible to distinguish between several exit types. Using the definitions from Hethey and Schmieder (2010), I define three exit types. Type A exits are interpreted to be a consequence of an establishment ID change, a takeover, or a spinoff. In practice,

[4] Just like in appendix A.1, major industries include (1) Agriculture, Fishing, Mining, (2) Manufacturing, Utilities, and Construction, (3) Wholesale and Retail Trade, Hospitality, (4) Business Service Activities, (5) Education, Health, and other Community Services, and (6) Industries not otherwise classified (Public Administration, Private Households, Extra-Territorial).

[5] The underrepresentation of Manufacturing seems to contradict the notion of automation causing manufacturing firms to lay off many workers, but should not be interpreted as such. After all, an establishment only appears in this chart if it completely exits (rather than laying off many, but not all, workers).



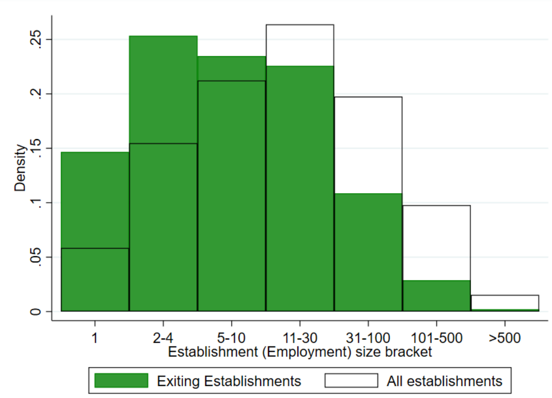


Figure A.6: *The distribution of exiting establishments (green) and all establishments in the data (black) over establishment size groups (defined as the number of employees at an establishment), using the SIAB (left) and LIAB (right).*

this means that the exiting establishment had at least 4 employees, and either at least 80% of the (newly entered) establishment at which the majority of workers are re-employed consists of workers from the exiting establishment, or at least 80% of the workers from the exiting establishment find work at the same (previously existing) establishment but do not make up more than 80% of the employment at their new establishment. An exit is classified as type B (establishment death) if either the exiting establishment had 3 employees or less, or no more than 30% of the former employees of the establishment find employment at the same establishment (and if that establishment is an entrant, the former employees of the exiting establishment do not make up more than 80% of the entering establishment's employment). Finally, an exit is classified as type C if it does not satisfy the conditions for type A and B. Therefore, these are exiting establishments with at least 4 employees where more than 30% of the former employees find a job at a common establishment. Further, type C exits do not include cases where that common establishment is an entrant and the former employees make up more than 80% of the entrant's employment, or cases where the common establishment is not an entrant, more than 80% of the exiting establishment's employees is re-employed at that establishment, and these employees make up less than 80% of their common establishment's total employment. Figure A.7 shows how the exiting establishments across all establishment size groups are divided over these three types. Due to the definition of exit types, it mechanically holds that all of the exits of one-person establishments, and the majority of establishments with 2 to 4 employees are classified as type B exits. However, conditioning on establishments having at least 5 employees, it can also be seen that larger exiting establishments are less likely to be classified as exit type B. This may be a consequence of large layoffs often resulting from selling off parts of the company or establishments making arrangements for laid off workers to gain employment elsewhere before laying off the worker.

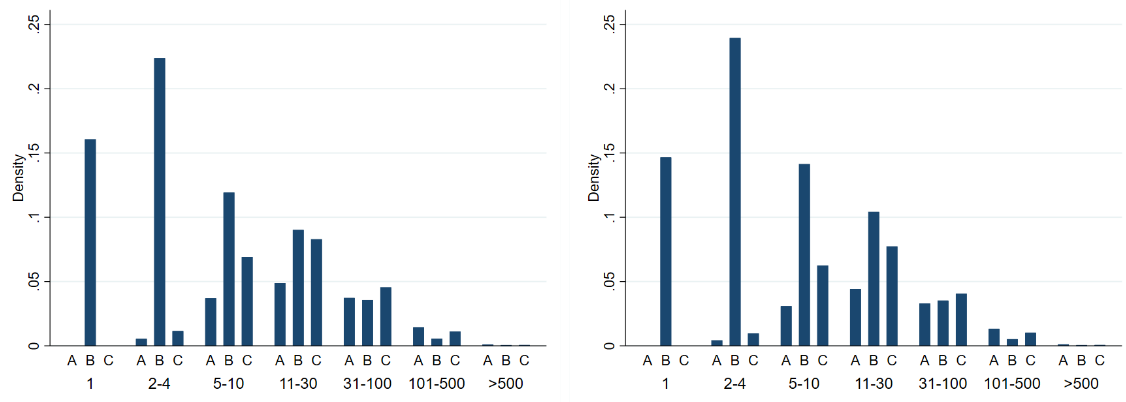


Figure A.7: *The distribution of exiting establishments over exit types A, B, and C (as defined in the text), and size group (defined by the number of employees at an establishment), generated using the SIAB (left) and LIAB (right).*

A.3 Further Empirical Results

A.3.1 Further observations on the incidence of displacement

In this subsection, I provide some further observations of the incidence of separation and displacement, beyond those that were displayed in section 1.3.1. In particular, this section focuses on the incidence of job loss by worker and establishment characteristics that are not further investigated in the remainder of the chapter and do not appear in the model in chapter 2.

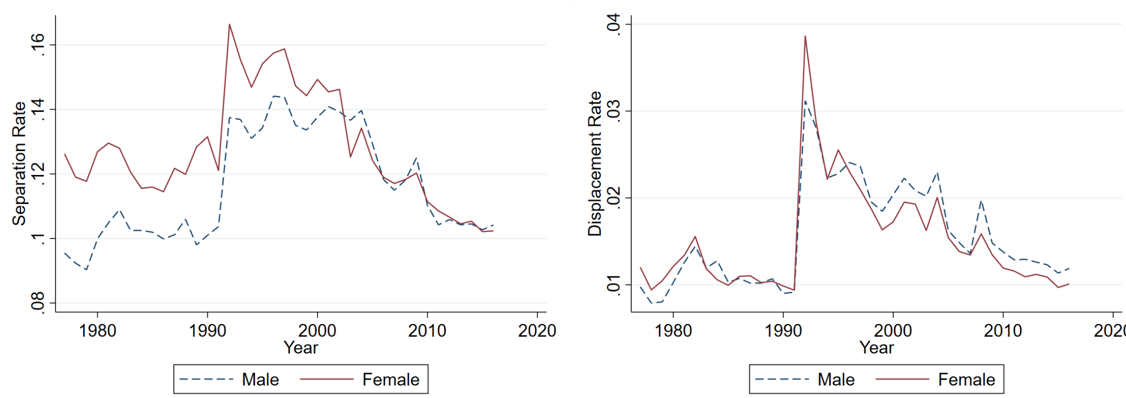


Figure A.8: *The incidence of separation (left) and displacement (right) by gender, over time.*

In figure A.8, the separation and displacement rates over time are plotted separately for male and female workers. While the patterns are more erratic than those seen in figure 1.2, it can be concluded that until recently the separation rate tended to be higher for female workers, but this was not the case for the displacement rate, thereby implying that female workers do not seem to be disproportionately hit by mass layoffs (as defined in section 1.2). Furthermore, looking at the more recent years, it no longer seems to be the case that female workers face higher separation rates, and displacement rates are now slightly higher for male workers than for female workers.

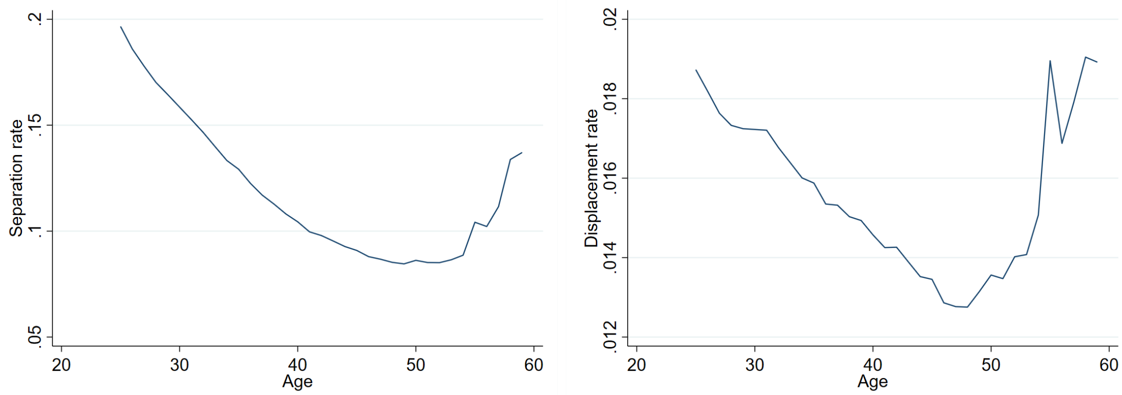


Figure A.9: *The incidence of separation (left) and displacement (right) by age.*

In figure A.9, the separation and displacement rates are displayed by age of the worker at the time of the job loss event. As can be seen here, the separation rate tends to be higher during early years, but this pattern is not as extreme for the displacement rate, which corresponds to the narrative in the literature (see e.g. Topel and Ward, 1992) that stresses the prevalence of job hopping early in the life cycle.[6] Notably, both the separation and displacement rates increase substantially around the age of 55, which can be explained using the regulations surrounding early (partial) retirement in Germany (ATZ), which can be used by workers aged 55 and above.[7]

Figure A.10 plots the separation and displacement rates by worker age groups over time, thereby

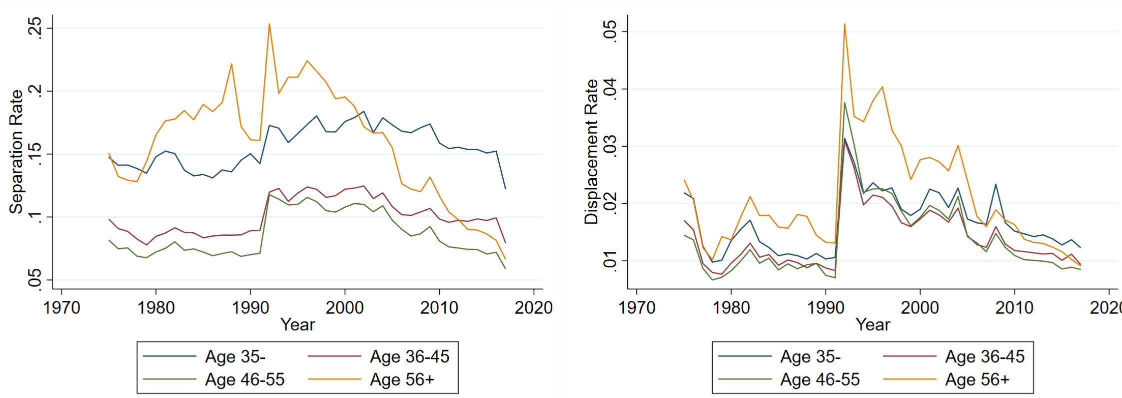


Figure A.10: *The incidence of separation (left) and displacement (right) by age group, over time.*

complementing the observations from figure A.9 (which were averaged across years). Looking at figure A.10, it can be seen that the differences between age groups are quite persistent over time. One exception to this is the 56+ age group, which used to have higher separation and displacement rates in the earlier decades of the data, but now no longer experiences the highest separation and displacement rates across the age groups.[8]

[6]Note that the fact that the peak early in the life cycle (for the separation rate) largely disappears when I impose sample restrictions, requiring (for example) a tenure of at least 6 years.

[7]See Berg et al. (2015) for a more extensive description of this policy, implemented in 1996.

[8]A partial explanation for the declining separation of the workers aged 56 and above can once again be found in

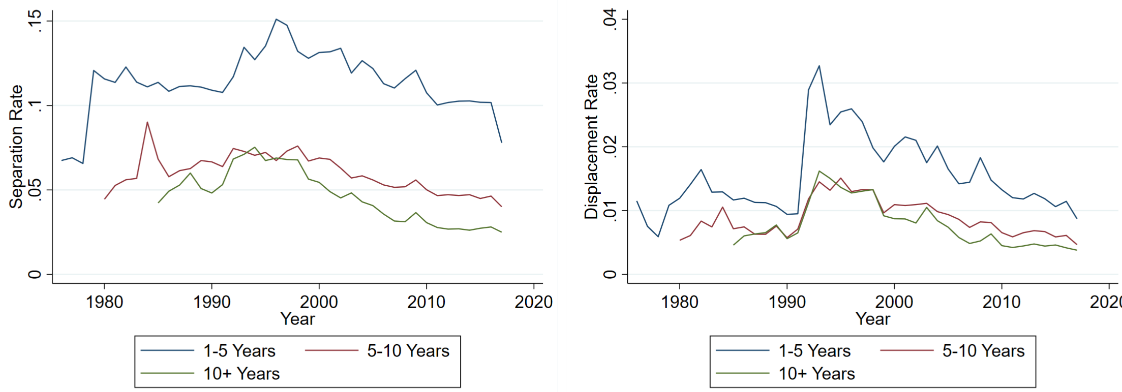


Figure A.11: *The incidence of separation (left) and displacement (right) by job tenure, over time.*

A similar pattern can be discovered by plotting the separation and displacement rate by job tenure, as done in figure A.11.[9] As one might expect, this figure reveals that the separation and displacement rates are generally higher for workers with a lower job tenure, and a similar conclusion can be reached by looking at establishment tenure instead.[10] As workers with a higher job and establishment tenure are mechanically expected to be older (on average), this figure, combined with figure A.9 supports a narrative of separation being more prevalent early in the lifecycle, while also not ruling out an alternative narrative of workers being more likely to be laid off if they have lower tenure (regardless of their age).

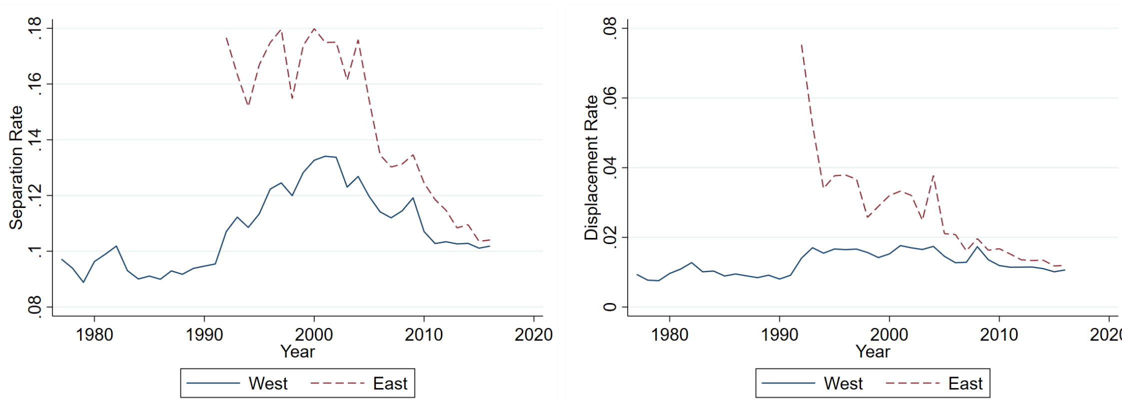


Figure A.12: *The incidence of separation (left) and displacement (right) by location, over time.*

An additional benefit of using data from Germany starting before 1990 is that it allows me to look at a situation specific to Germany: a comparison of the provinces formerly part of West Germany

early retirement, which is excluded from the definition of separation and displacement. If more workers choose to retire (early), these workers would be excluded from the calculation of the separation and displacement rates. If some of these workers in fact decided to retire early as a consequence of an impending job separation, this would drive down the overall separation and displacement rates in figure A.10.

[9]Note that job (or establishment) tenure in the data is measured from the start of the data in 1975. As such, it is not possible for a person to have more than 5 (10) years of tenure in the data until 1980 (1985), as is reflected by the lines for these higher tenure groups starting from that year.

[10]The figures for establishment tenure are available upon request. They are not included here as they are very similar to the ones for job tenure.

and those formerly part of East Germany. Figure A.12 compares the two regions in terms of their separation and displacement rates. As can be seen in the figure, there is clear convergence between the two regions, but in the final years covered by the data both the separation and displacement rates are still slightly higher in Eastern Germany.

It is likely that job loss rates also differ by establishment characteristics such as establishment size. The left panel of figure A.13 shows the displacement rate by establishment size group. As can be seen in this figure, the displacement rate tends to be higher for smaller establishments, especially if I remove the restrictions on worker tenure.

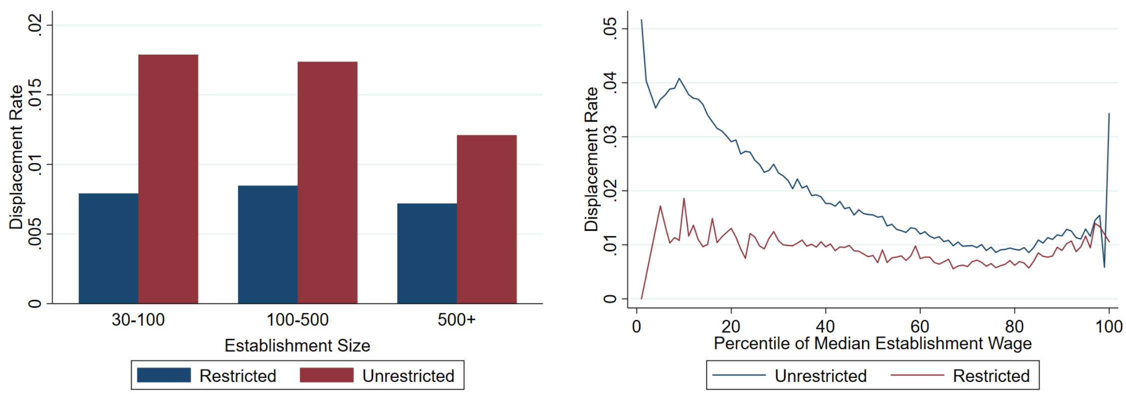


Figure A.13: *The incidence of displacement by establishment size (left) and median wage (right), with and without restrictions on worker tenure.*

The right panel of figure A.13 shows how the displacement rate differs according to how high the median establishment wage is. The pattern here is similar to the one seen earlier in figure 1.5: the displacement rate tends to be high especially in establishments that have a low median establishment wage (especially in the sample without restrictions on worker tenure). This resemblance makes sense, as the median establishment wage and an individual's recent earnings are likely to be highly (though not perfectly) correlated.

The left panel of figure A.14 shows how the incidence of recall (within 5 years), conditional on displacement, changes if establishment closures are excluded from the definition of a mass layoff. As workers who are laid off from a closing establishment can not be recalled, these closures mechanically drive down the recall rate. Indeed, as can be observed in the figure, excluding these establishment closures leads to an increase in the recall rate of approximately 2 percentage points (from an average of 6% to an average of 8%).

Finally, the right panel of figure A.14 shows that recall rates are fairly similar for male and female workers, although the recall rate for male workers does appear to be slightly more volatile.

A.3.2 The Incidence of Displacement, using a Restricted (SIAB) Sample

While most results in sections 1.3.2 and 1.3.3 are based on a sample that is restricted to workers with a pre-displacement tenure of at least 6 years, this is not the case for most results in section 1.3.1. In this section, I show that the results from that section continue to hold when using the

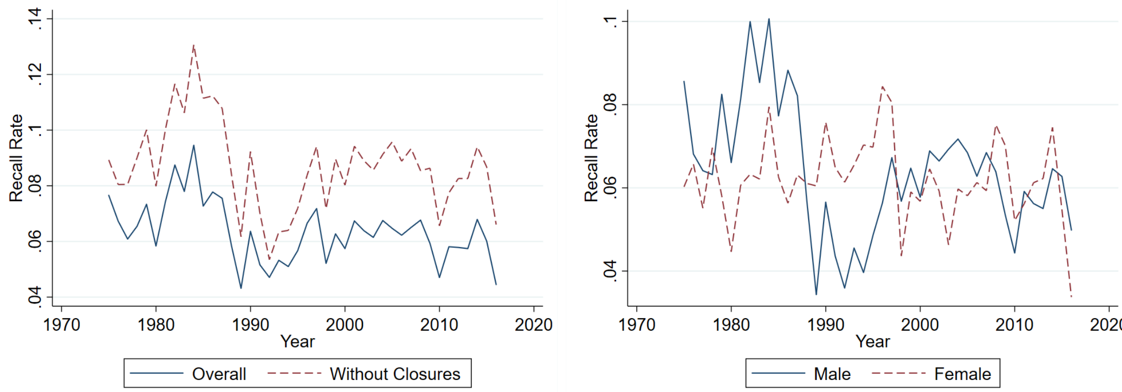


Figure A.14: *Left: The incidence of recall within 5 years of job loss over time, conditional on displacement, with and without including establishment closures in the definition of a displacement. Right: The incidence of recall within 5 years of job loss, conditional on displacement, over time and by gender.*

sample restrictions from sections 1.3.2 and 1.3.3.

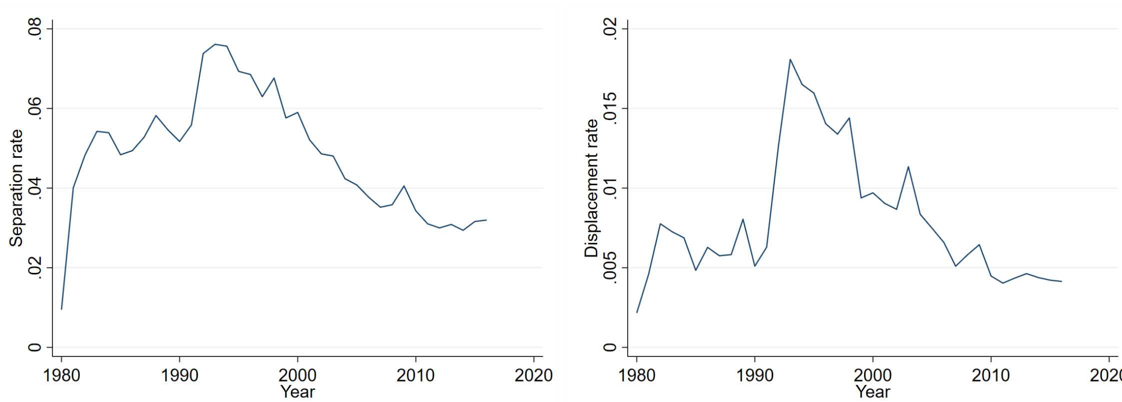


Figure A.15: *The incidence of separation (left) and displacement (right) over time, with restrictions on worker tenure.*

First of all, figure A.15 displays the separation and displacement rates over time, for the tenure-restricted sample. As can be seen in this figure, the separation rate averages at roughly 4% for this restricted sample whereas the displacement rate is roughly 0.7% on average. This is substantially lower than the rates found in the main text for the unrestricted sample, reflecting that incidence differs by job tenure (as observed in section A.3.1). As before, the aftermath of the German reunification is quite clearly visible in the graph.

Figure A.16 displays the restricted separation and displacement rates over time by education level, thus mirroring figure 1.3 from the main text (which does not impose restrictions on worker tenure). As can be observed by comparing the two figures, imposing restrictions on worker tenure dampens the differences between the low and high educated workers in terms of their separation and displacement rates: while the workers with low educational attainment still have a slightly higher

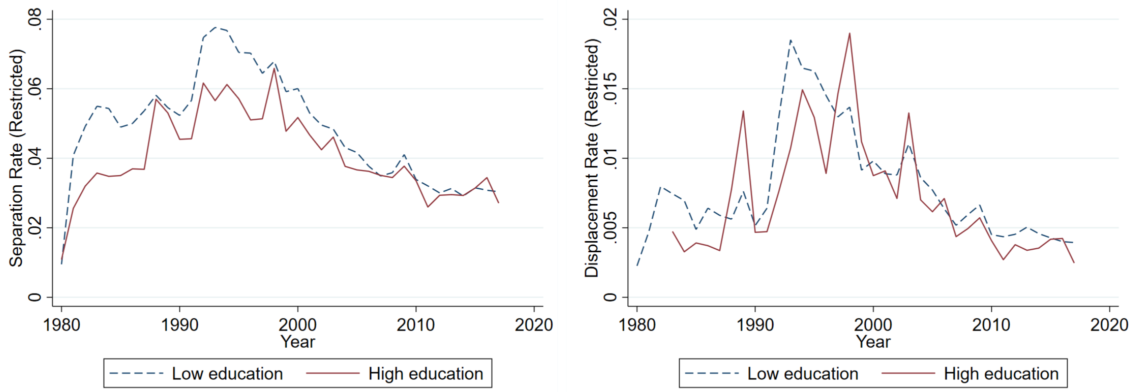


Figure A.16: *The incidence of separation (left) and displacement (right) by education level, over time, with restrictions on worker tenure.*

separation and displacement rate on average, the difference is very small.

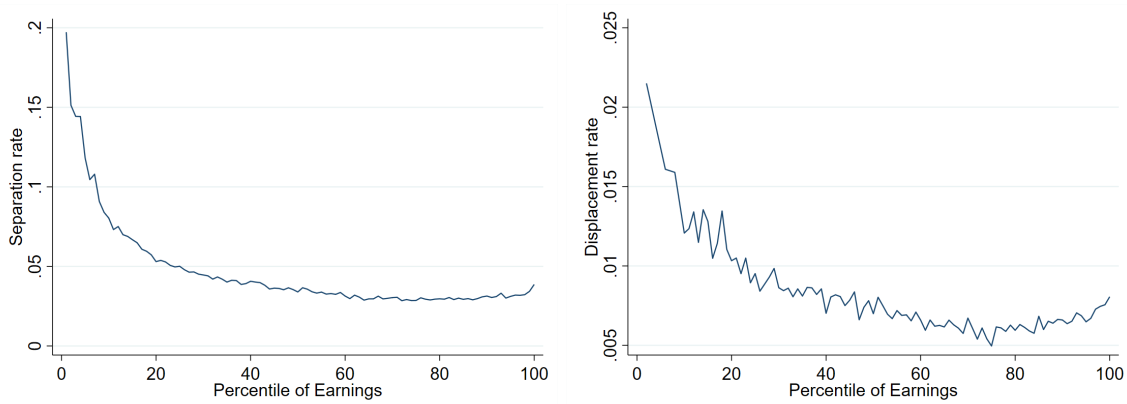


Figure A.17: *The incidence of separation (left) and displacement (right) over the earnings distribution, with restrictions on worker tenure.*

As shown in figure A.17, the conclusion that the separation and displacement rates in general tend to be higher for individuals located lower on the (recent) earnings distribution continues to hold when worker tenure restrictions are imposed.

Finally, figure A.18 shows how the incidence of recall (within 5 years), conditional on displacement, changes when I impose the further sample restrictions for the purpose of the regression estimations in section 1.3.2 and 1.3.3. In particular, this means that I require the worker to have at least 6 years of establishment tenure prior to displacement and I require the establishment to contain at least 50 workers (prior to displacement). As can be seen in the figure, this further decreases the recall rate to an average of approximately 4%.

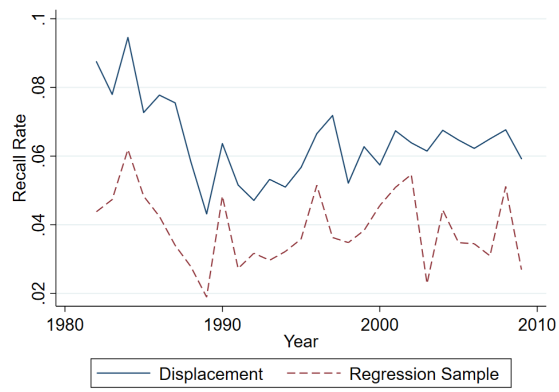


Figure A.18: *The incidence of recall within 5 years of job loss over time, conditional on displacement, with and without further sample restrictions (as imposed for regression estimation).*

A.3.3 The Incidence of Displacement, using LIAB

Section under Construction

In this subsection I repeat the analysis of the incidence of displacement, as seen in section 1.3.1 in the main text, using data from the LIAB instead of the SIAB.[11]

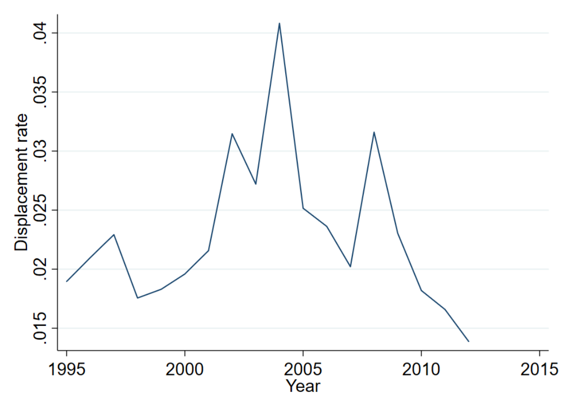


Figure A.19: *The incidence of separation (left) and displacement (right) over time, using LIAB.*

First of all, figure A.19 displays the separation and displacement rates over time. It can be seen that the average separation and displacement rates are roughly in line with those seen in the main text (though the displacement rates are higher), at 13% and 2.5% respectively. Just like seen in the main text, all rates display substantial variation over time, with the peaks generally lining up with recessions in Germany. Because the LIAB sample begins after the German reunification, the jump that was observed in the early 1990s in the SIAB is not visible here.

Figure A.20 splits out the incidence rates seen in figure A.19 by education level. Therefore, it can be seen as the LIAB version of figure 1.3. Comparing these two figures reveals that the LIAB seems

[11]Note that I only repeat the analysis done in the main text in this section. The analysis displayed in sections A.3.1 and A.3.2 is omitted here and is available upon request.

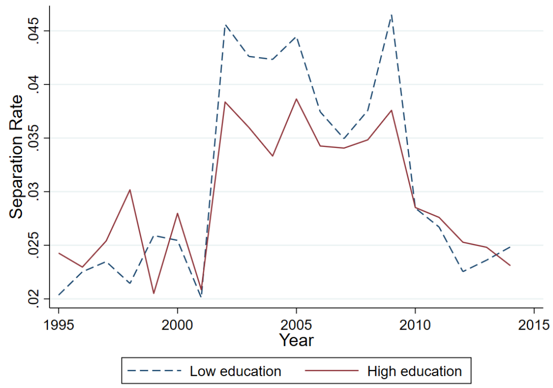


Figure A.20: *The incidence of separation (left) and displacement (right) over time, by education level, using LIAB.* Pending Unrestricted Graphs.

to suggest much lower displacement and separation rates prior to 2000 than the SIAB. However, when focusing on the years after 2000, the conclusion from the main text as well as section A.3.2 seems to continue to hold: Generally, the workers with low educational attainment are slightly more likely to be displaced and separated.

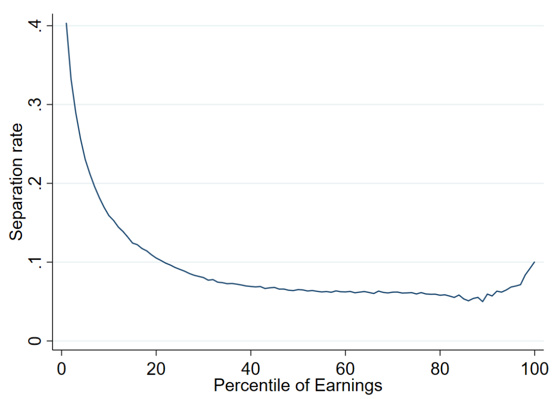


Figure A.21: *The incidence of separation (left) and displacement (right) over the earnings distribution, using LIAB.*

As shown in figure A.21, the separation and displacement rates over the recent earnings distribution display the same pattern as in the SIAB: they tend to be higher for individuals located lower on the (recent) earnings distribution and increase again above the 80th percentile of the distribution.

Finally, when it comes to ex-post recall status, figure A.22 shows that the incidence of recall (within 5 years) is especially high for separation, but even for displacement consistently above 3.5% across the distribution, and much higher towards the bottom of the distribution, in line with the conclusions from figure 1.6.

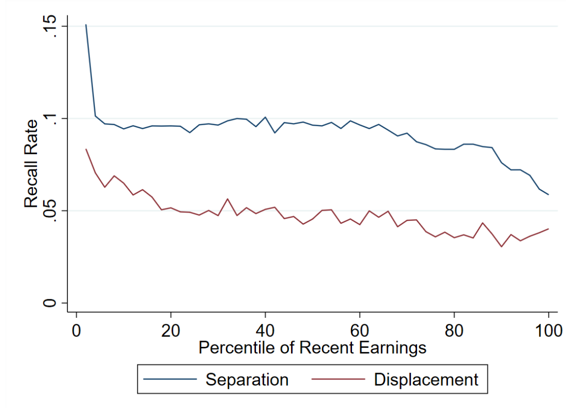


Figure A.22: *The incidence of recall within 5 years of separation or displacement, by percentile of the recent earnings distribution, using LIAB.*

A.3.4 Further Observations on the Average Scarring Effect of Displacement

Section under Construction

In this subsection, I will provide some further results to illustrate the robustness of the results in section 1.3.2 of the main text.

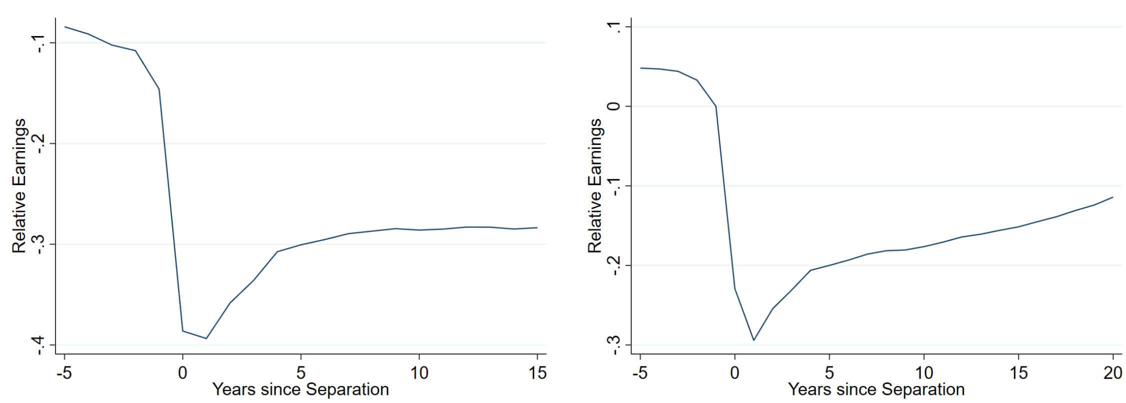


Figure A.23: *Raw (left) or regression-based (right, using specification 1.1) average effect of separation on earnings, relative to the control group.*

First, figure A.23 presents the counterpart of figure 1.7 using separations rather than displacements. As can be seen in the left panel of the figure, the raw effect of separation on earnings is quite substantial, and quite comparable to the estimated effects of displacement seen in the main text. In particular, the change in relative earnings is fairly similar, though the initial level is lower than it was for displacement. The right panel of the figure confirms the similarity between the results for displacement and separation using specification (1.1), with the earnings loss after separation seeming to be only slightly higher than that for displacement. However, the slightly increased magnitude of the earnings loss as well as the lower initial level in the raw differences can be (partially) attributed to selection of separated workers on their (potential) productivity, which is often cited as the main reason for focusing on displacements rather than all separations in the literature.

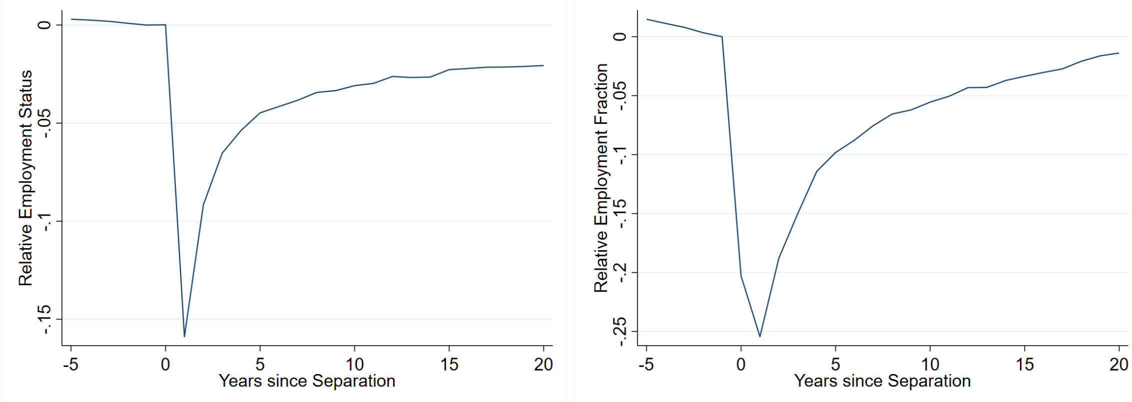


Figure A.24: *The effect of separation on employment status (left) and employment fraction (right), relative to the control group, using estimated coefficients from equation 1.1.*

As figure A.24 shows, the employment status of the separated workers also follows a similar pattern to that of the displaced workers (as shown in figure 1.8), with a slightly larger effect in the short run and a slightly faster recovery.

As mentioned in the main text, the conclusions regarding the scarring effect of displacement are in line with the literature, though slightly conservative in some cases. In what follows, I change some of the restrictions on the sample used in obtaining the estimation results in the main text, to show how sensitive results are to these restrictions. First of all, I did not include any observation where the earnings for the individual are missing when creating my estimation sample used in the main text. I chose not to include these observations, as these missing values may not in fact be zero given that there are many possible reasons for the earnings to be missing (including self-employment and employment in the public sector, as indicated in section 1.2). If I instead include these missing values as zeros in the estimation, the estimated effect (using specification 1.1) becomes slightly larger, as seen in figure A.25.

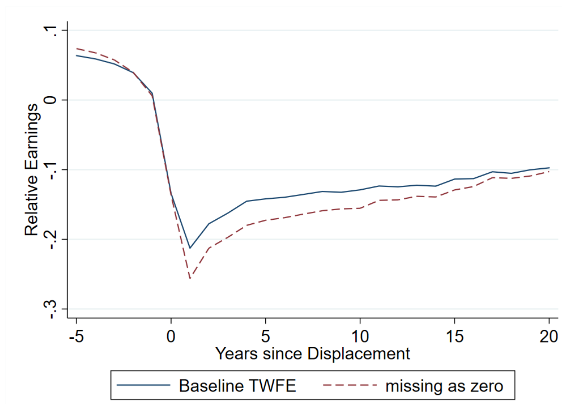


Figure A.25: *The effect of displacement on earnings, relative to the control group, using estimated coefficients from equation 1.1. The solid line mirrors the estimate from the right panel of figure 1.7, whereas the dashed line estimates the effect including missing values (interpreted as zero earnings).* Pending Updated Graph.

Second of all, the estimation of the interaction-weighted estimator (described in section 1.2 and summarized by equation (1.2)) only allows for an individual to be assigned to a single cohort. In the estimation discussed in the main text, I accounted for this by excluding any individual that experienced multiple displacements. An alternative method would be to assign these individuals to the cohort corresponding to their first observed displacement. The estimation results using this alternative sampling method can be seen in figure A.26, where the original result (as seen in the main text) is included for the reader's convenience. Comparing the two lines in figure A.26, it can be observed that including individuals who experience multiple displacements leads to a slightly larger estimate of the scarring effect of displacement (in the long run). However, the difference is very small and almost indistinguishable on the graph, and the difference between the point estimates is not statistically significant, thus showing that the decision to omit individuals who experience multiple displacements does not seem to affect the result.

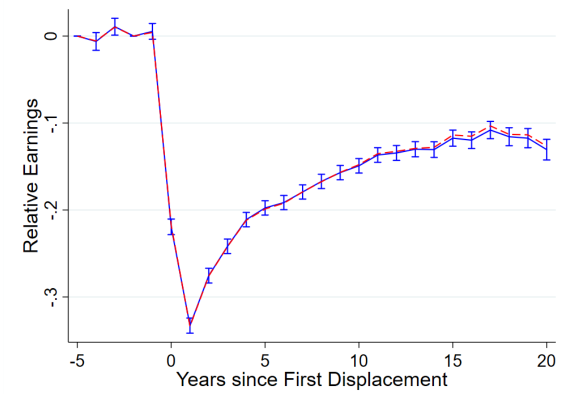


Figure A.26: *The effect of displacement on earnings, relative to the control group, using estimated coefficients from equation 1.2, and using either the main sample (dashed, red), or using a sample that additionally includes individuals who experience multiple displacements (solid, blue).* Pending Updated Graph.

As I mention in section 1.3.2, the estimated scarring effects of displacement on earnings and employment fraction seem to suggest that the daily wage of the displaced worker plays a large role (though slightly smaller than that of employment). In figure A.27, I confirm this hypothesis by showing the results of estimating equation 1.2 using the (daily) wage as the dependent variable. As can be seen in the left panel, using the full sample gives rather inconclusive results when it comes to daily wages, likely due to some workers earning very high daily wages in the data.[12] If I instead omit the top and bottom 5% of the observed wages, the result becomes much clearer, as seen in the right panel. Here it can be seen that it is indeed the case that the displaced workers tend to experience a wage loss of roughly 8% in the long run. This wage loss does not materialize immediately following the displacement but rather takes a few years to materialize, thus suggesting that this wage loss may be primarily driven by the displaced workers that are unemployed for an extended period of time before finding a new job.

The richness of the data allows me estimate the scarring effect of displacement for several sub-samples. In the main text, I already stressed the difference by educational attainment and ex-post

[12]In the original data, these observations are censored, and I use a program from the FDZ at the IAB to impute values for these wages. Omitting the top 5% of daily wages leads to me omitting many of these imputed values, thereby also likely yielding a more reliable estimate.

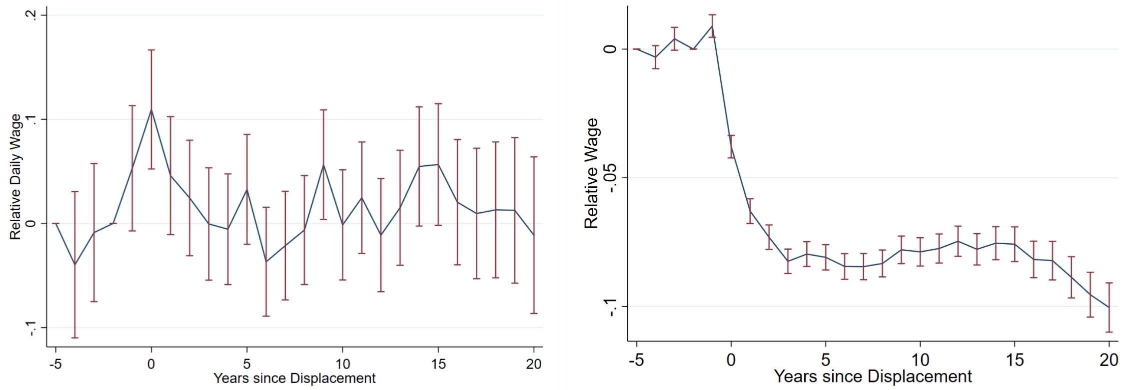


Figure A.27: *The effect of displacement on daily wages, relative to the control group, using estimated coefficients from equation 1.2, and using either the full sample (left), or using a sample that excludes the top and bottom 5% of the daily wages (right).* Pending Updated Graph (left).

recall status. Some other dimensions that are often investigated are that of the worker's gender and age, as well as the economic conditions at the time of displacement.

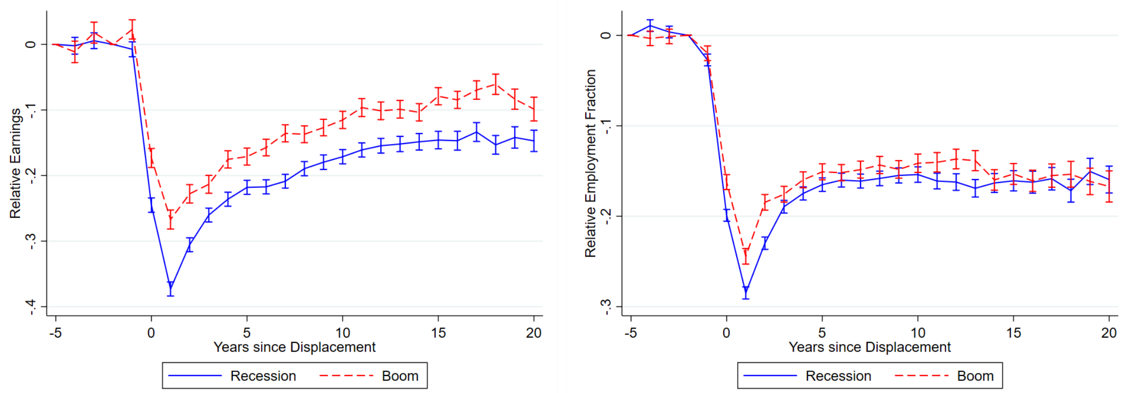


Figure A.28: *The effect of displacement on earnings (left) and employment fraction (right), relative to the control group, using estimated coefficients from equation 1.2, and averaging the resulting coefficients separately by economic conditions at the time of displacement.* Pending Updated Graphs.

In figure A.28, I show how the estimated scarring effect of displacement changes depending on the economic conditions at the time of displacement. For this estimation, I divide the cohorts used in the estimation of equation (1.2) into two groups, according to the unemployment rate at the time of displacement. Notably, the results for earnings (in the left panel) are very similar to the results found by Davis and Von Wachter (2011), who use data from the United States. Just like in that paper, I find that workers who are displaced during booms experience a lower earnings loss than workers who were displaced during recessions. As can be seen in the right panel, the difference in the effect on employment fraction is much smaller.

In figure A.29, I show how the results differ by the worker's gender. As can be seen in the left panel, the results suggest that female workers suffer from much larger earnings losses after displacement than male workers. Nevertheless, the general shape of the earnings loss graph is very similar between the two subsamples. Again, the difference is much smaller when looking at the effect on

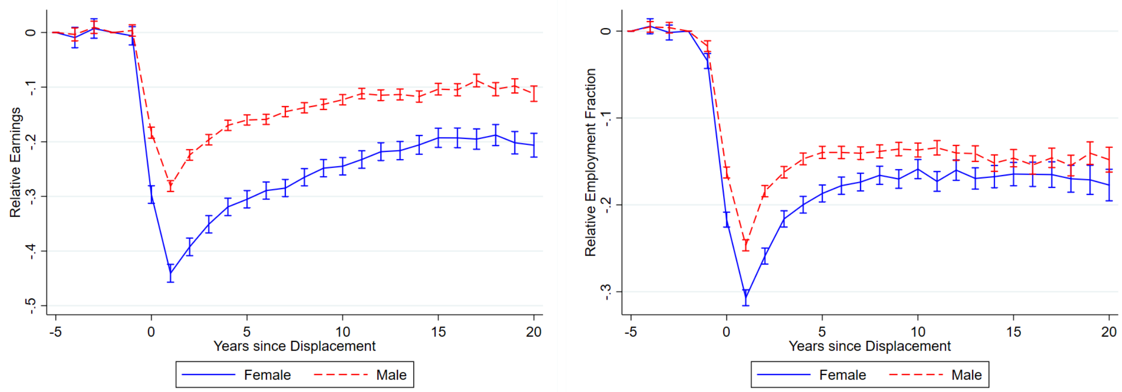


Figure A.29: *The effect of displacement on earnings (left) and employment fraction (right), relative to the control group, using estimated coefficients from equation 1.2, estimated separately for male and female workers.* Pending Updated Graphs.

employment fraction, as shown in the right panel of figure A.29.

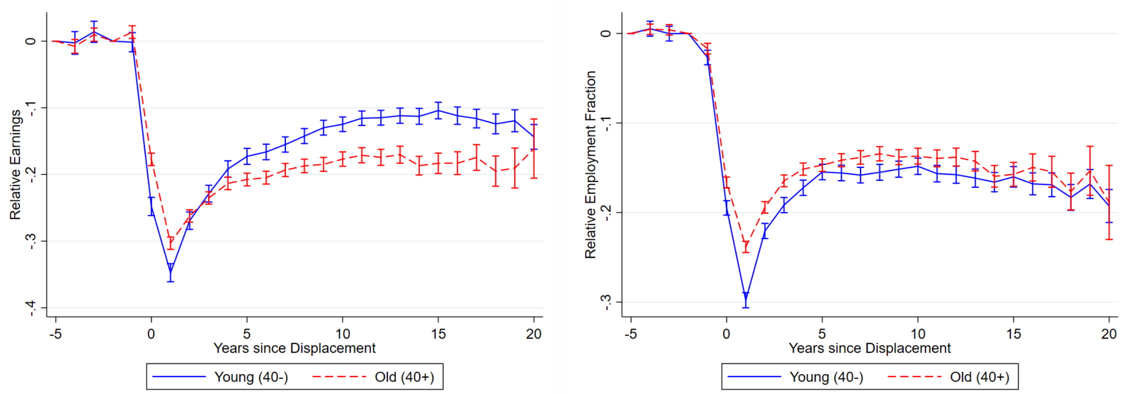


Figure A.30: *The effect of displacement on earnings (left) and employment fraction (right), relative to the control group, using estimated coefficients from equation 1.2, estimated separately for workers who are "young" (below 40) or "old" (40 or older) at the time of displacement.* Pending Updated Graphs.

Figure A.30 plots the results of estimating equation 1.2 separately by age groups. For the purpose of this estimation, I split the sample by age at the time of displacement, where the "young" sample includes those aged below 40 at the time of displacement, and the "old" cohort includes workers aged 40 or higher at the time of displacement. For the control group, a similar restriction is put in place to ensure that they cover a similar age span. This means that for the estimation on the "young" sample, control observations with an age above 50 are omitted, whereas for the estimation on the "old" sample I omit control observations for workers aged below 35. As can be observed in figure A.30, the young workers tend to experience a slightly larger effect on both earnings and employment fraction upon impact. However, where the difference between young and old for the effect on employment fraction is fairly constant over time, the young workers recover much faster than the old workers in terms of their earnings.

Finally, when discussing the interaction-weighted estimator from Sun and Abraham (2020) in section 1.2, I mentioned that it is not the only estimator that aims to correct for contamination of the coefficients estimated a two-way fixed effects model such as equation (1.1) by other time periods and other cohorts. An alternative method is proposed in Borusyak et al. (2021), and I have estimated the main results from sections 1.3.2 and 1.3.3 using this alternative method as well.

The method proposed in Borusyak et al. (2021) proceeds in three steps. In the first step, the method aims to directly estimate the counterfactual implicitly used in a difference-in-differences estimation procedure. This is done by estimating a standard two-way fixed effects model (without the leads and lags for treatment) on all not-yet-treated and never-treated workers in the sample. Following the notation in section 1.2, this means that I estimate the following equation:

$$e_{it} = \alpha_i + \gamma_t + u_{it} \tag{A.1}$$

The estimates of the individual and time fixed effects in equation (A.1) are then used to estimate the untreated (counterfactual) outcome for all treated observations as well. In other words, the estimated counterfactual outcome combines the estimated individual fixed effect (estimated using the individual's observations before treatment) and the estimated time fixed effect (estimated using other individuals, who were not treated at the time period of interest).

In the second step, these counterfactual untreated outcomes are compared to the (observed) treated outcome to form an estimate of the individual- and time-specific treatment effect (which is thus the difference between the estimated untreated outcome from step 1 and the observed outcome). In the third and final step, the target aggregation is then estimated using a weighted average of the individual and time-specific estimated treatment effects from step 2.

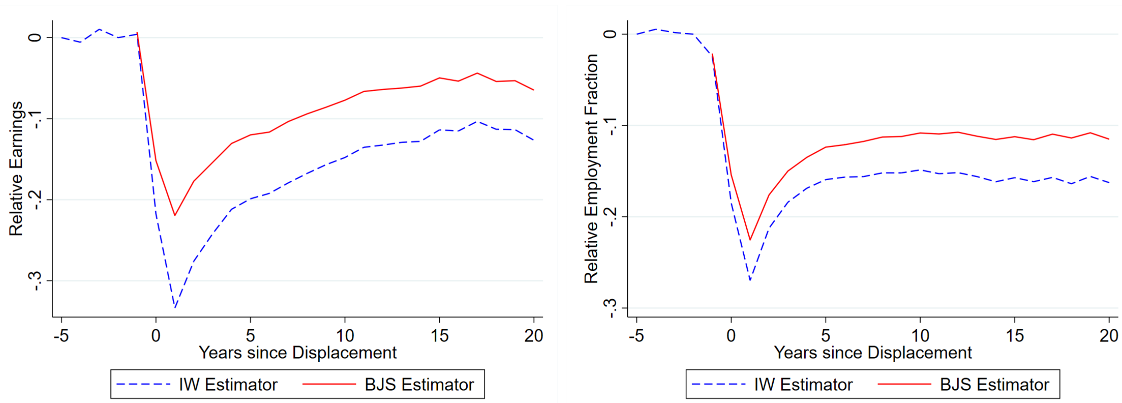


Figure A.31: *The effect of displacement on earnings (left) and employment fraction (right), relative to the control group, using estimated coefficients from either equation 1.2 (blue) or the three-step method from Borusyak et al. (2021) (red).* Pending Updated Graphs.

In figure A.31, I show how the estimated scarring effect of displacement differs by the used method. As can be seen in the figure, the results for the two methods are very similar, noting that the three-step method from Borusyak et al. (2021) yields slightly smaller earnings and employment losses. This in turn means that the contrast between the standard two-way fixed effects estimation and these alternative methods (as shown in figure 1.9 for the interaction-weighted estimator) would have been slightly weaker if I had used the the three-step method from Borusyak et al. (2021) instead.

A.3.5 The Average Scarring Effect of Displacement, using LIAB

In this subsection, I repeat the analysis of the average scarring effect of displacement (and separation) as done in section 1.3.2 of the main text, using the LIAB data instead.

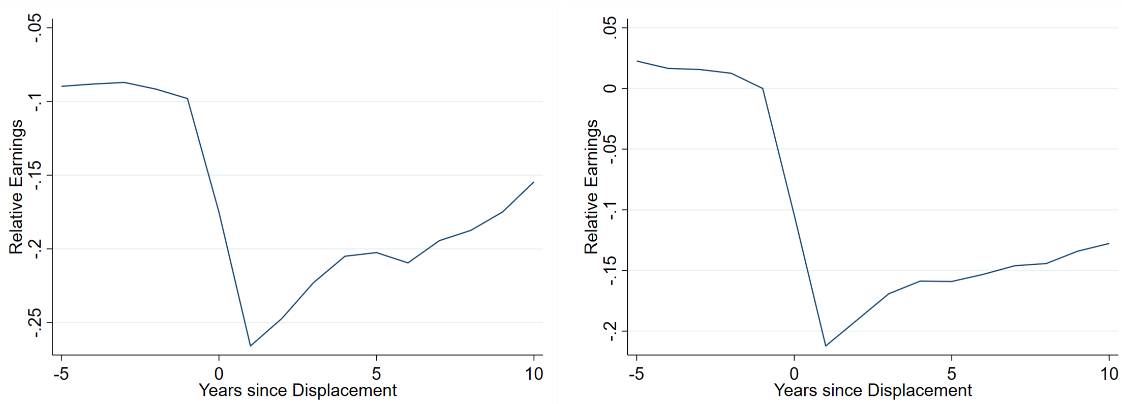


Figure A.32: *Raw (left) or regression-based (right, using specification 1.1) average effect of displacement on earnings, relative to the control group, and using data from LIAB.*

First, the left panel of figure A.32 displays the raw earnings differences after displacement (from 5 years before to 10 years after the event). Just like in the main text (the left panel of figures 1.7), the effect of job loss on earnings is quite substantial. There is some partial recovery in earnings, but earnings have not fully recovered 10 years after job loss.

The right panel of figure A.32 shows the results of estimating equation (1.1) using the LIAB dataset.[13] In particular, consistent with the findings in the main text, it can be seen that in the short-run, workers who are displaced earn roughly 20% less on average than a worker in the control group. This earnings loss is shown to be quite persistent, with these displaced workers still earning 12% less than workers in the control group 10 years after the job loss took place.

Similarly, when using employment status as the dependent variable, as seen in figure A.33 (which is roughly the equivalent of figure 1.8), the results from the main text continue to hold. Using data from the LIAB instead of the SIAB, I now find that the likelihood of being employed (at any point in the year) drops by 8% in the year after displacement, this likelihood decreases recovers to roughly 4.5% after only 3 years, and further recovers to less than 2% by year 8. Similarly, in terms of the fraction of the years spent in employment, I find in the LIAB that this is roughly 17% lower in the short run (for displaced workers, compared to the control group), but almost fully recovers in 10 years.

Finally, I show in figure A.34 that the estimates of the average scarring effect of displacement

[13] The right panel of figure A.32 is roughly the equivalent of the right panel of figure 1.7 in the main text which uses SIAB data. However, it should be noted that due to the shorter timespan covered by the LIAB, I estimate a slightly altered version of (1.1), where I estimate the effects of job loss up 10 years (rather than 20 years) after the event. While in principle this should not matter for the estimates, following the argument made in Sun and Abraham (2020) would suggest that one might find different estimates since omitting observations more than 10 years out would also imply that these observations can no longer contaminate the estimates of the scarring effect less than 10 years after the displacement.

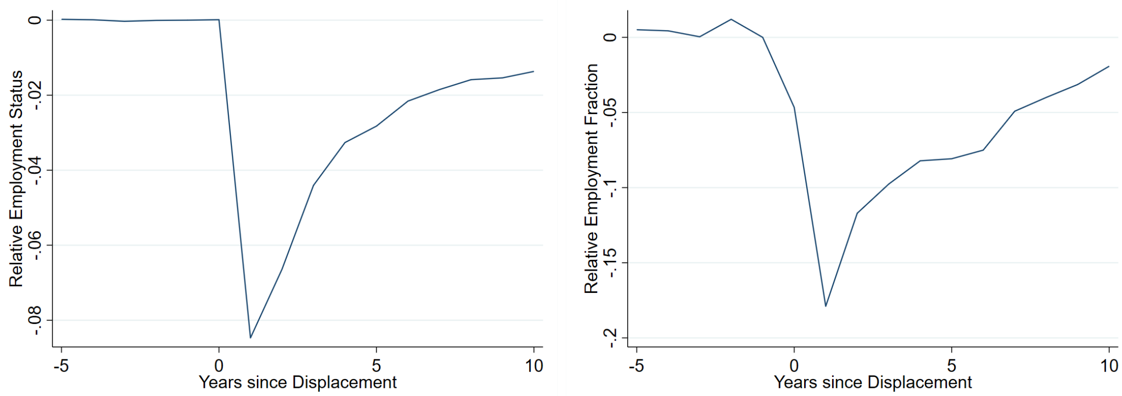


Figure A.33: *The effect of displacement on employment status (left) and employment fraction (right), relative to the control group, using estimated coefficients from equation 1.1 and using data from LIAB.*

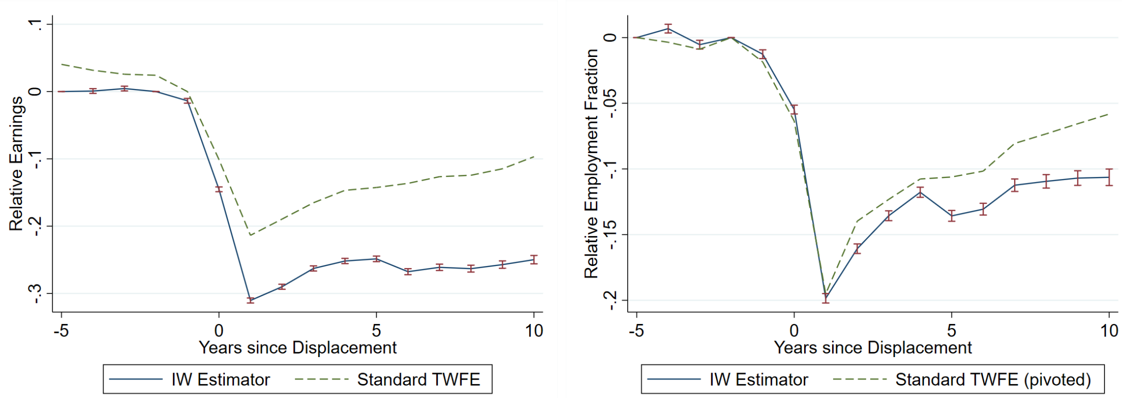


Figure A.34: *The effect of displacement on earnings (left) and employment fraction (right), relative to the control group, using estimated coefficients from equation* (1.2) *(solid) or* (1.1') *(dashed) and using data from LIAB. The error bars on the solid line correspond to 95% pointwise confidence intervals* (on earnings and employment fraction) change in a similar way as seen in the main text (figure 1.9) when using the interaction-weighted estimator from Sun and Abraham (2020). In particular, just like in the main text, while the estimation of equation (1.1') suggests that employment and earnings both recover substantially, the interaction-weighted estimator reveals that the recovery stagnates after roughly 5 years, with employment remaining roughly 10% below that of the control group and earnings losses remaining at roughly 25%.

A.3.6 Further Heterogeneity in the Scarring Effect of Displacement

Section under Construction

In the main text, in section 1.3.3, I showed that the scarring effect of displacement on both earnings and (in the short run) employment tends to be worse for workers with a lower education level and for workers who are recalled to their previous employer. In this section, I highlight the robustness of the results discussed in section 1.3.3 of the main text, by showing how these results change when conditioning the sample on some other observable variable, or when using alternative estimation methods. Since the main focus in the main text (in both chapters 1 and 2) is on the dimension of ex-post recall, most of this section focuses on the robustness of those results.

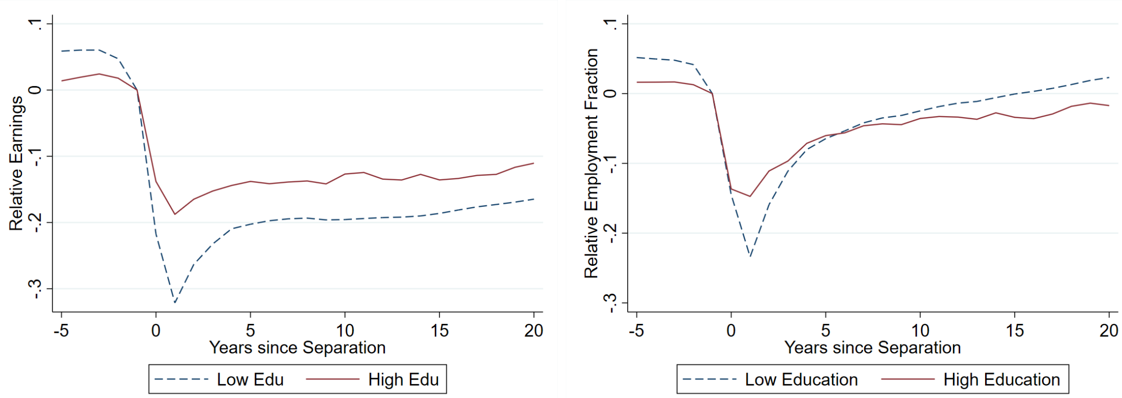


Figure A.35: *The effect of separation on earnings (left) and employment fraction (right), relative to the control group of never-separated workers (by education group), using estimated coefficients from equation* (1.1).

In figure A.35, I repeat the analysis using specification (1.1), estimating the effects of separation instead. Comparing figure A.35 to figure 1.10 in the main text, it can be observed that the effects of separation tend to be worse than the effects of displacement in the short run, and this holds for both education levels. When it comes to employment, workers with a low education suffer from a larger loss in the short run. In the long run, however, the different between the two education levels mostly disappears, and in this case the workers with a low education level even do slightly better in the long run than workers with a high education level.

In figure A.36, I repeat the analysis of the education-specific effect of displacement using an alternative definition of the control group. Contrary to the analysis that generated figure 1.10 in the main text, I now use the same control group for both education groups. That is, the control group contains workers of both education levels. In general, the results from the main text continue

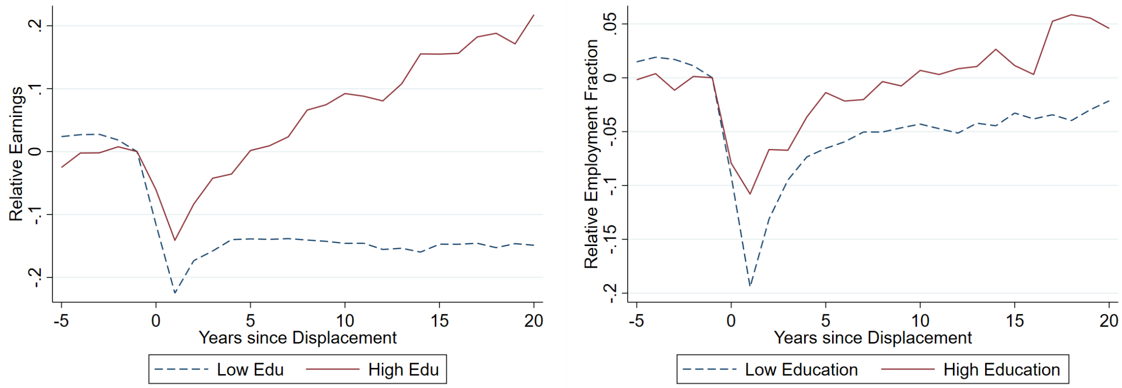


Figure A.36: *The effect of displacement on earnings (left) and employment fraction (right), relative to the control group of never-displaced workers (by education group) and using estimated coefficients from equation* (1.1)*, where the control group contains individuals in either education group (rather than individuals from the same education group).*

to hold. However, it can be noted that in the years following (and preceding) displacement, the relative earnings of the highly educated group grows faster than the relative earnings of the lower educated group. This trend eventually leads the relative earnings difference for the highly educated group of displaced workers (compared to the control group) to become positive. This does not fully reflect a scarring effect of displacement. Rather, it is an artifact of the treatment group consisting of only highly educated workers, whereas the control group also includes workers with a low education level. In other words, this figure (and especially the left panel) serves as a reminder that the choice of the correct control group is crucial.

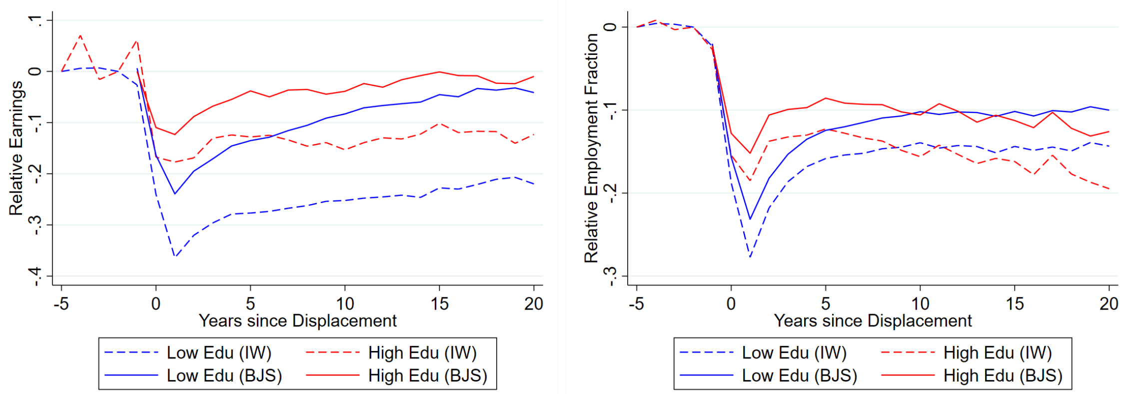


Figure A.37: *The effect of displacement on earnings (left) and employment fraction (right) by education level, relative to the control group, using either estimated coefficients from equation 1.2 (dashed) or estimated coefficients from the three-step method in Borusyak et al. (2021) (solid).* Pending Updated Graphs.

In figure 1.11 in the main text, I show results obtained using the interaction-weighted estimator from Sun and Abraham (2020), which is one of the proposed alternatives to the "standard" two-way fixed effects estimator commonly used. However, as mentioned in section 1.1.1, several of these alternative estimators have been proposed. In figure A.37, I show how the results from the interaction-weighted estimator differ from those obtained using the three-step estimation method from Borusyak et al. (2021), outlined in appendix A.3.4. As can be seen in the figure, using this method yields very comparable results, though the resulting estimates are not quite identical. In particular, the three-step method yields smaller estimates of the scarring effect for both education groups, and the difference between the two groups is slightly smaller when using the three-step estimation method, although the sign of the difference is consistent between the two methods.

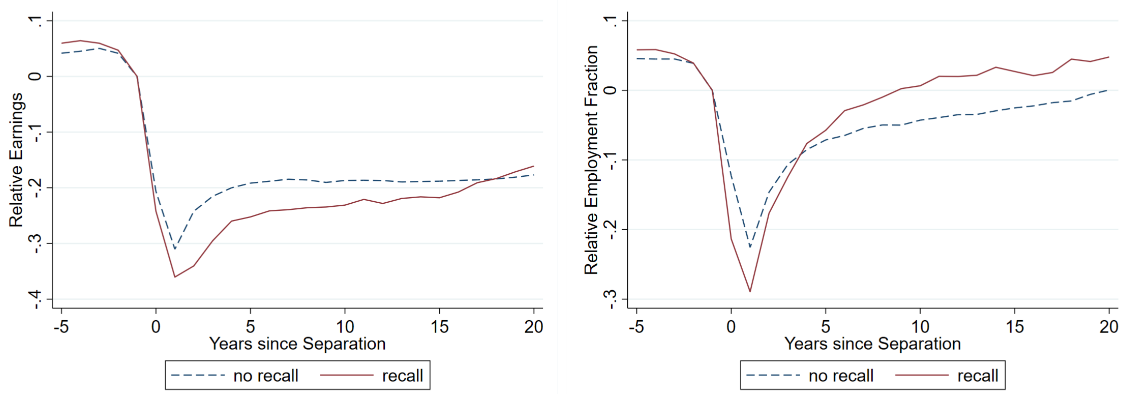


Figure A.38: *The effect of separation on earnings (left) and employment fraction (right) relative to the control group, by ex-post recall status (materialization of recall within 5 years), using specification (1.1).*

In section 1.3.3.2 of the main text, I show how displacement effects differ by ex-post recall status. In that section, I found that recalled workers tend to do worse than non-recalled (but displaced) workers in terms of earnings, and do worse in terms of employment in the short run. In the remainder of this section I will examine the robustness of these findings. In figure A.38, I repeat the analysis, estimating the effects of separation instead. Comparing figure A.38 to figure 1.12 in the main text, it can be seen that the effects after separation are more severe than the effects of displacement. However, while this is true for both recalled and non-recalled workers, the deterioration is worse for non-recalled workers, so that the result from the main text does not hold in the long run: compared to non-recalled workers, recalled workers do worse in terms of employment and earnings in the short run only, and in fact do slightly better in the long run in terms of employment.

Moving on to the estimation using the interaction-weighted estimator from specification (1.2), figure A.39 repeats the analysis of figure 1.13 allowing for workers that experience multiple displacement spells.[14] As can be seen by comparing figure A.39 and figure 1.13, allowing for multiple displacements slightly strengthens the result as the earnings and employment loss of recalled workers becomes slightly larger in magnitude.

In figure A.40, I show how the results obtained using the interaction-weighted estimator compare to those obtained using the three-step estimation method from Borusyak et al. (2021), which was introduced in appendix A.3.4. In figure A.40, it can be observed that using this three-step method

[14]Recall that in order for a separation to be considered a displacement according to my definition, the workers needs to have a tenure of at least 6 years in the establishment from which they are displaced. As the tenure counter simply resumes counting after returning to a firm, allowing for multiple displacements will primarily affect the group of recalled workers who are displaced again from the same firm.

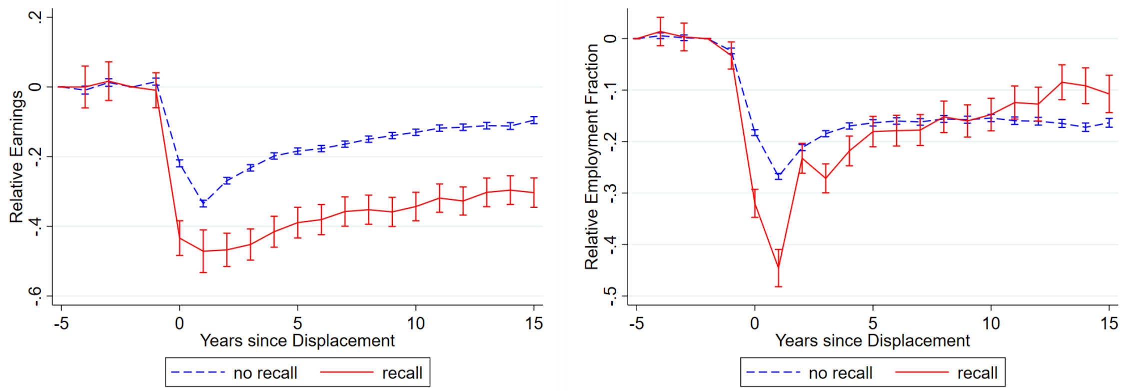


Figure A.39: *The effect of displacement on earnings (left) and employment fraction (right) by ex-post recall status, relative to the control group, using estimated coefficients from equation* (1.2)*. The error bars correspond to 95% pointwise confidence intervals. Compared to figure 1.13 in the main text, the estimation here allows for multiple displacements per individual (classifying the worker according to their first displacement).* Pending Updated Graphs.

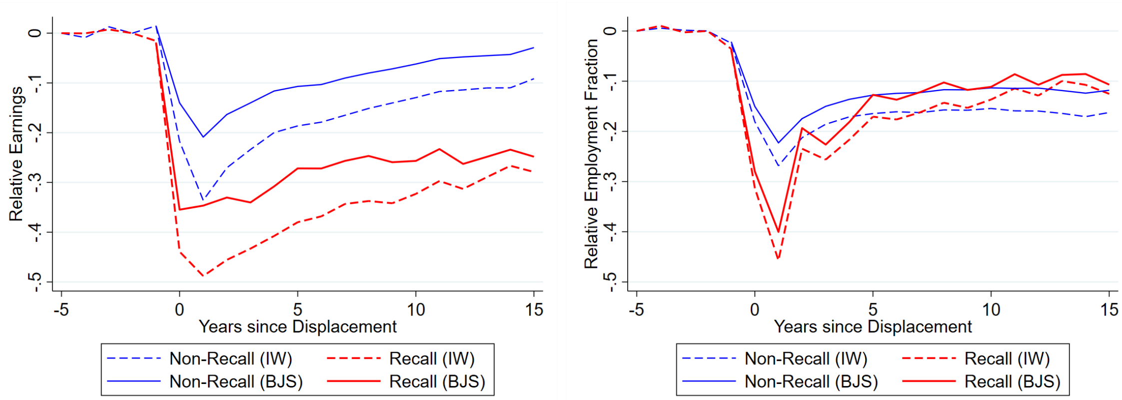


Figure A.40: *The effect of displacement on earnings (left) and employment fraction (right) by ex-post recall status, relative to the control group, using either estimated coefficients from equation 1.2 (dashed) or estimated coefficients from the three-step method in Borusyak et al. (2021) (solid).* Pending Updated Graphs.

produces slightly smaller estimates of the scarring effect of displacement by ex-post recall status. However, this is the case for both the recalled and non-recalled groups, so that the difference between the two groups is similar to the difference found in the main text. As shown in figure A.41, this also holds when focusing on wages and job loss probabilities instead. The full results for the effect on job loss probabilities obtained using the interaction-weighted estimator can be found in figure A.42.

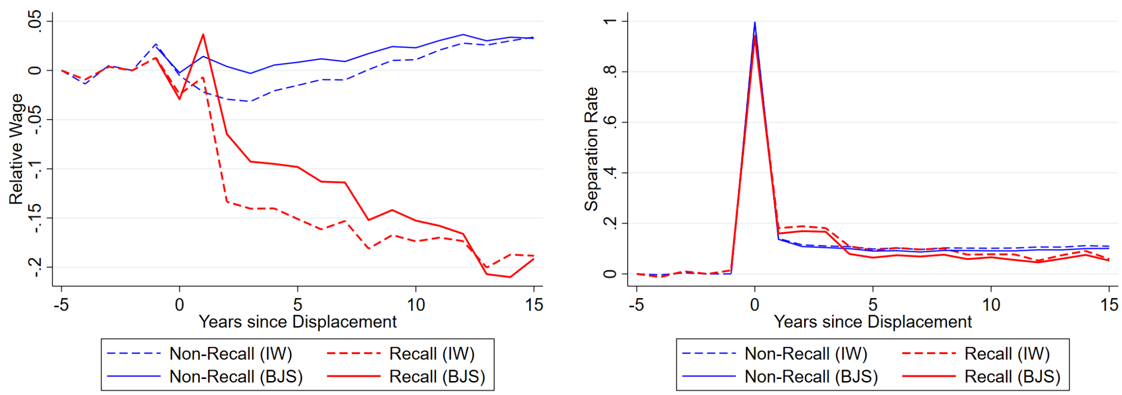


Figure A.41: *The effect of displacement on wages (left) and job loss probabilities (right) by ex-post recall status, relative to the control group, using either estimated coefficients from equation 1.2 (dashed) or estimated coefficients from the three-step method in Borusyak et al. (2021) (solid).* Pending Updated Graphs.

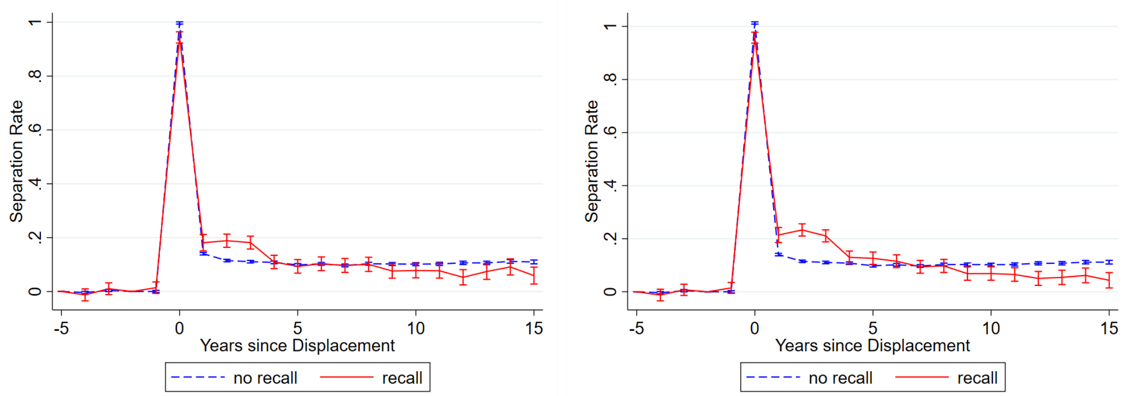


Figure A.42: *The effect of displacement on separation rates by ex-post recall status, relative to the control group, using estimated coefficients from equation (1.2). The error bars correspond to 95% pointwise confidence intervals. Left: estimation allowing for only one displacement per individual; Right: estimation allowing for multiple displacements per individual (classifying the worker according to their first displacement). Compared to figure 1.14 in the main text, this figure shows the full graph (starting from $k = -5$) rather than only the results from period $k = 1$ onwards.* Pending Updated Graphs.

In figure A.43, I repeat the analysis from figure 1.13 using only employment (and earnings) in full-time jobs, addressing possible concerns of earnings losses being driven by workers transitioning from full-time to part-time jobs after displacement. As can be seen in the figure, the results on the difference between recalled and non-recalled workers remain intact.

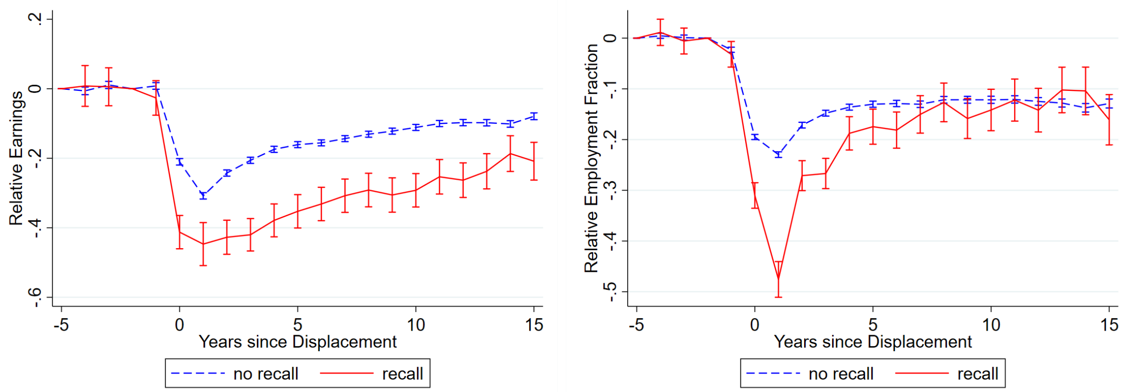


Figure A.43: *The effect of displacement on earnings (left) and employment fraction (right) by ex-post recall status, relative to the control group, using estimated coefficients from equation* (1.2)*. The error bars correspond to 95% pointwise confidence intervals. Compared to figure 1.13 in the main text, the estimation here only uses earnings (and employment) from full-time jobs.* Pending Updated Graphs.

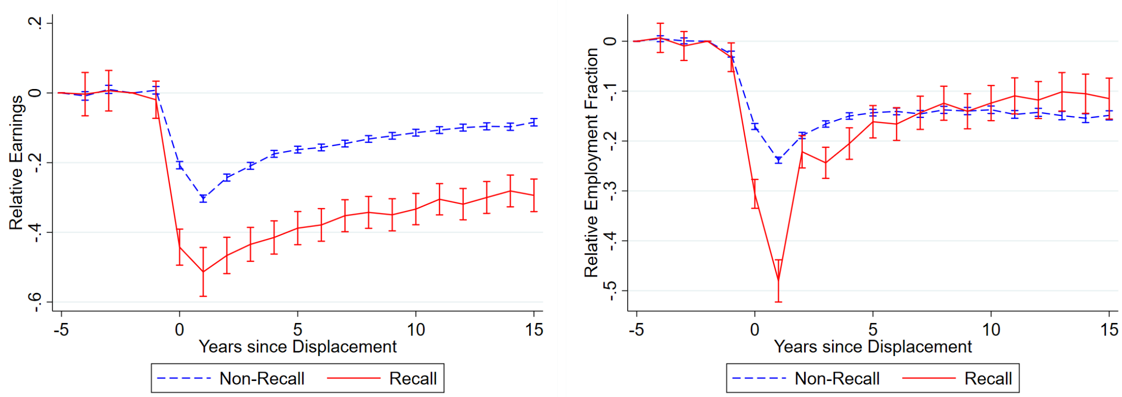


Figure A.44: *The effect of displacement on earnings, by ex-post recall status and relative to the control group, using estimated coefficients from equation* (1.2)*. The error bars correspond to 95% pointwise confidence intervals. The estimation here only includes workers who were not displaced (control group) or workers who were displaced from an establishment that did not close.* Pending Updated Graphs.

Another potential concern with the comparison between recalled and non-recalled workers is that the group of non-recalled workers also includes workers that were displaced from an establishment that closed, thus leaving no room for a recall choice. As can be seen in figure A.44, however, omitting the workers that were displaced from a closing establishment does not substantially affect the conclusion made in the main text (based on figure 1.13): in fact, the estimated earnings and employment loss for non-recalled workers is slightly lower, thus widening the gap between recalled and non-recalled workers.

Figure A.45 shows that the larger earnings loss experienced by recalled workers cannot be explained solely by lower earnings at the recalling establishment. In the figure, I show how the

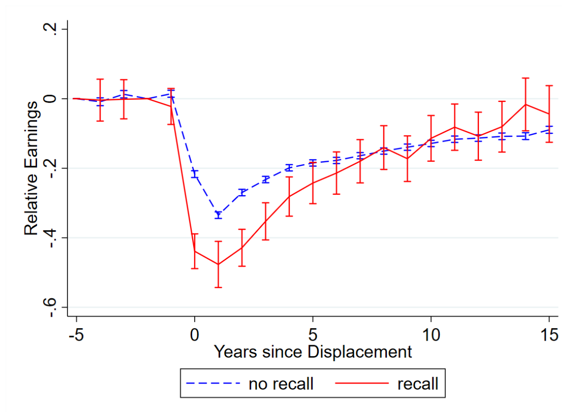


Figure A.45: *The effect of displacement on earnings, by ex-post recall status and relative to the control group, using estimated coefficients from equation* (1.2)*. The error bars correspond to 95% pointwise confidence intervals. Compared to the left panel of figure 1.13 in the main text, the estimation here only uses earnings from the establishment to which the recalled worker is recalled.* Pending Updated Graphs.

estimation changes when I use only earnings at the recalling establishment (for the recall group, leaving the control and non-recall group unchanged). As can be seen in the figure, the short-term earnings loss remains largely the same, but it is no longer the case that the recalled worker also does worse in the long run. This indicates that part of the long-run persistence of the larger earnings losses for recalled workers is driven by subsequent employer changes.

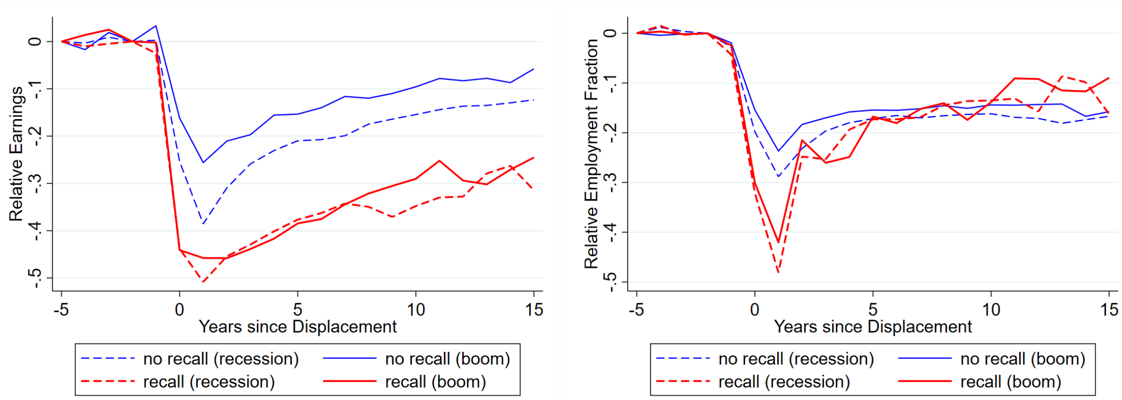


Figure A.46: *The effect of displacement on earnings (left) and employment fraction (right), by ex-post recall status and relative to the control group, using estimated coefficients from equation* (1.2)*. The error bars are omitted for convenience (but available upon request). Compared to figure 1.13 in the main text, the estimation splits out the effect by economic conditions at the time of displacement, roughly categorized as recession (dashed) and boom (solid).* Pending Updated Graphs.

In figure A.46, I show that the gap in the earnings loss experienced by recalled and non-recalled workers is slightly smaller if the worker is displaced in a recession. This is primarily due to the relative earnings loss experienced by the non-recalled worker being higher in recessions, possibly reflecting less opportunities to move into a new job quickly. Nevertheless, it should be noted that the general result that recalled workers experience larger earnings losses continues to hold in recessions.

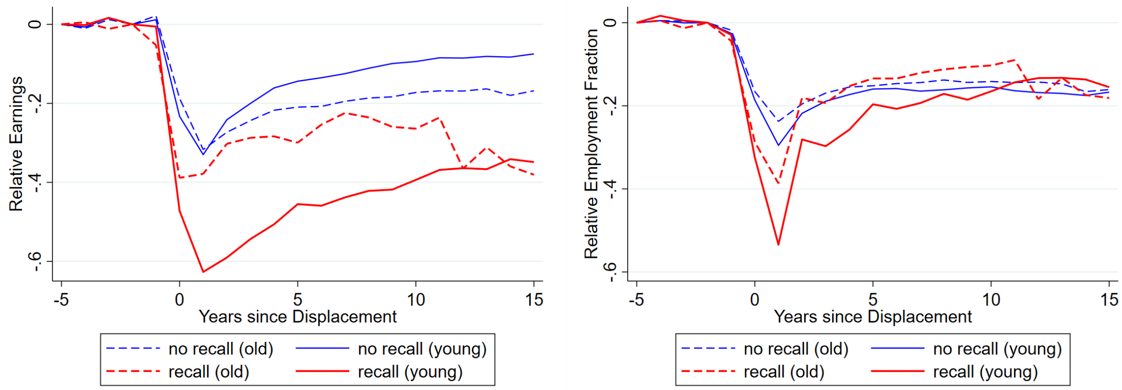


Figure A.47: *The effect of displacement on earnings (left) and employment fraction (right), by ex-post recall status and relative to the control group, using estimated coefficients from equation* (1.2). *The error bars are omitted for convenience (but available upon request). Compared to figure 1.13 in the main text, the estimation splits out the effect by age of the worker at the time of displacement, separately estimating the effect for workers aged below 40 (solid) and workers aged 40 and up (dashed).* Pending Updated Graphs.

In figure A.47, I show how the effects of displacement by ex-post recall status differ by age group. In particular, it can be observed that for workers aged above 40, the difference between recalled and non-recalled workers is much smaller than for younger workers, even if the effect on employment fraction is fairly comparable between the two age groups. Part of the explanation for this observation may lie in older workers likely having worked for their previous employer for a longer time, and therefore having built up more firm-specific knowledge, which may drive up their earnings relative to the (counterfactual) earnings they would have had if they were to move to a new firm instead.

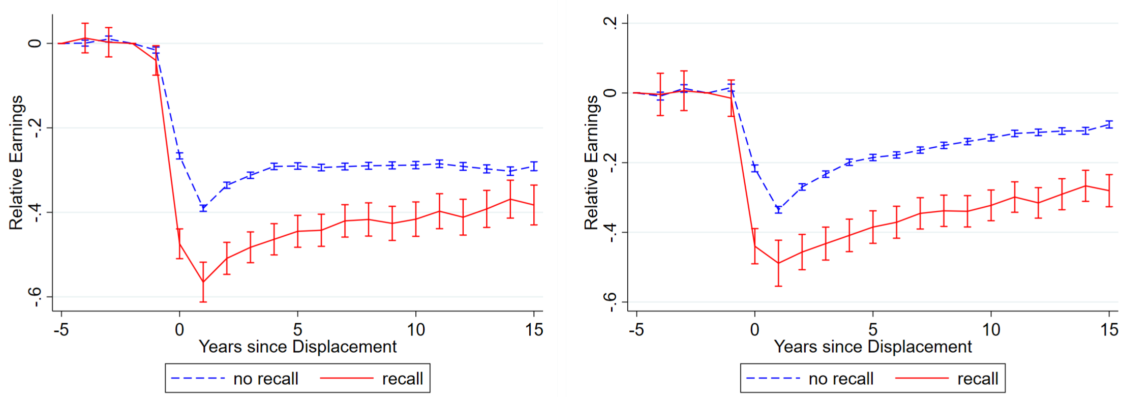


Figure A.48: *The effect of displacement on earnings, by ex-post recall status and relative to the control group, using estimated coefficients from equation* (1.2). *The error bars correspond to 95% pointwise confidence intervals. The left panel replicates the left panel of figure 1.13 in the main text, whereas the estimation in the right panel excludes workers from traditionally seasonal industries (agriculture and hospitality).* Pending Updated Graphs.

Figure A.48 shows that the comparison between recalled and non-recalled workers in figure 1.13 of the main text is not likely to be driven by seasonal workers. After all, omitting workers in industries that traditionally show strong seasonal patterns (agriculture and hospitality) does not substantially alter the results compared to figure 1.13 in the main text (and in fact yields a stronger result).[15]

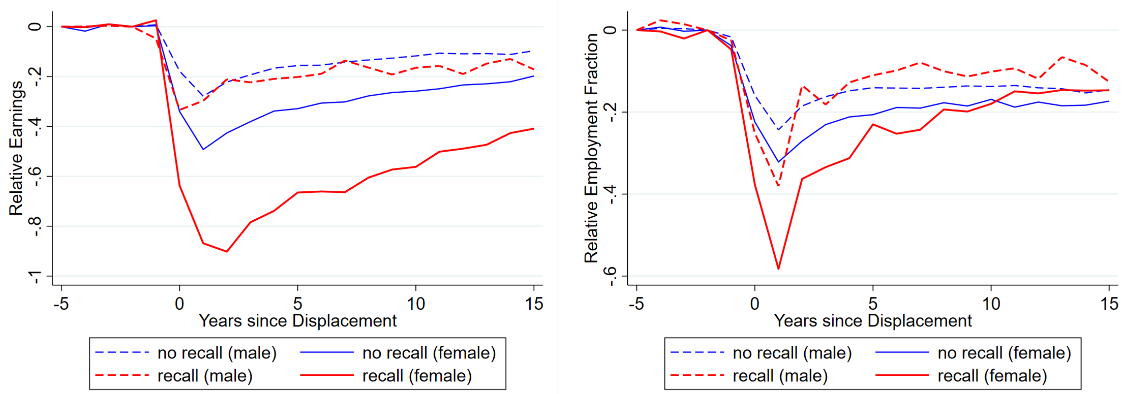


Figure A.49: *The effect of displacement on earnings (left) and employment fraction (right), by ex-post recall status and relative to the control group, using estimated coefficients from equation* (1.2)*. The error bars are omitted for convenience (but available upon request). Compared to figure 1.13 in the main text, the estimation splits out the effect by gender, separately illustrating the effect for female workers (solid) and male workers (dashed).* Pending Updated Graphs.

In figure A.49, I show that the difference between recalled and non-recalled workers identified in the main text tends to be higher for female workers than for male workers. This is partially a composition effect: male workers are more likely to work in industries in which the difference between recalled and non-recalled workers is small (manufacturing and construction) and the observations from male workers are also more likely to come from the earlier years in the data. Indeed, as figure A.50 shows, focusing on male workers aged below 40 for whom displacement is observed after 1991 partially restores the clear difference between recalled and non-recalled workers.

As I mention in the main text, in section 1.3.3, the data allows me to look at how the scarring effects of displacement differ along many dimensions of observable heterogeneity. Therefore, the results above are merely a small selection of the dimensions along which I can split out the effect of displacement by ex-post recall status. Among others, I can also show that the difference between the recalled and non-recalled worker tends to be smaller (but the difference is still clearly present) when focusing on Eastern Germany, the years prior to the Hartz reforms, low complexity occupations, or larger establishments. These results are available upon request.

A.3.7 Heterogeneity in the Scarring Effect of Displacement, using LIAB

In this section, I will use the LIAB to re-affirm the main conclusions from section 1.3.3 of the main text.

[15]In principle, one could estimate the result separately by industry. Doing this exercise reveals that none of the industries (for which there are enough observations) the recalled worker is strictly better off than the non-recalled worker when it comes to their relative earnings after displacement. However, for the manufacturing and construction industries it should be noted that the recalled workers are also not clearly strictly worse off. The corresponding results are available upon request.

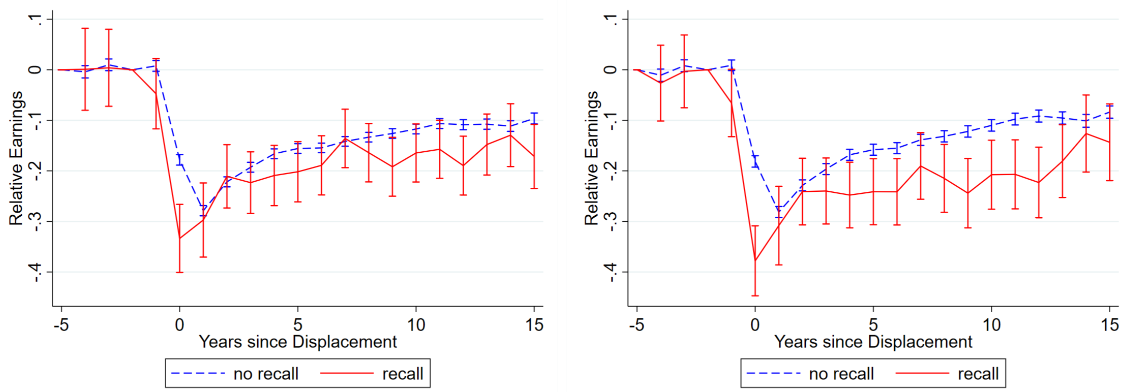


Figure A.50: *The effect of displacement on earnings, by ex-post recall status and relative to the control group, using estimated coefficients from equation* (1.2)*, and using male workers only. The error bars correspond to 95% pointwise confidence intervals. The left panel repeats the corresponding estimates figure A.49 above, whereas the right panel uses only observations made after 1991.* Pending Updated Graphs.

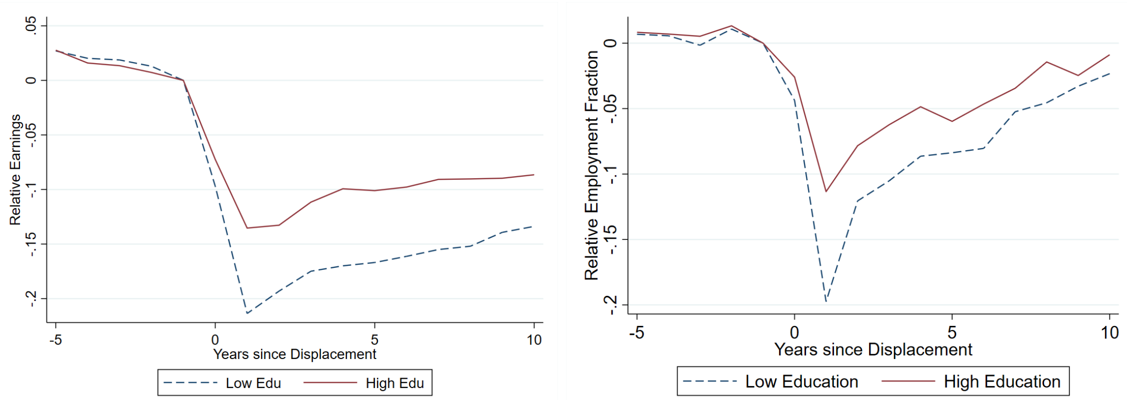


Figure A.51: *The effect of displacement on earnings (left) and employment fraction (right), relative to the control group (by education group), using LIAB and using estimated coefficients from equation* (1.1).

The first factor that I investigated, in subsection 1.3.3.1, was the individual's educational background. In figure A.51, I plot the results of the estimation when splitting the sample by education (non-University and University), using LIAB and the "standard TWFE" specification (1.1). As seen in the main text, workers with a relatively low education tend to suffer from higher earnings losses, both in the short- and long term, as well as a larger initial (and long-run) effect on employment status.

In figure A.52, I show the results obtained by using the interaction-weighted estimator from specification (1.2) instead. As can be seen, it still holds that the worker with a lower education level experiences higher earnings losses in the short run, and the two groups converge towards the end of the sample (noting that this is after 10 years here, rather than after 20 years as in the corresponding

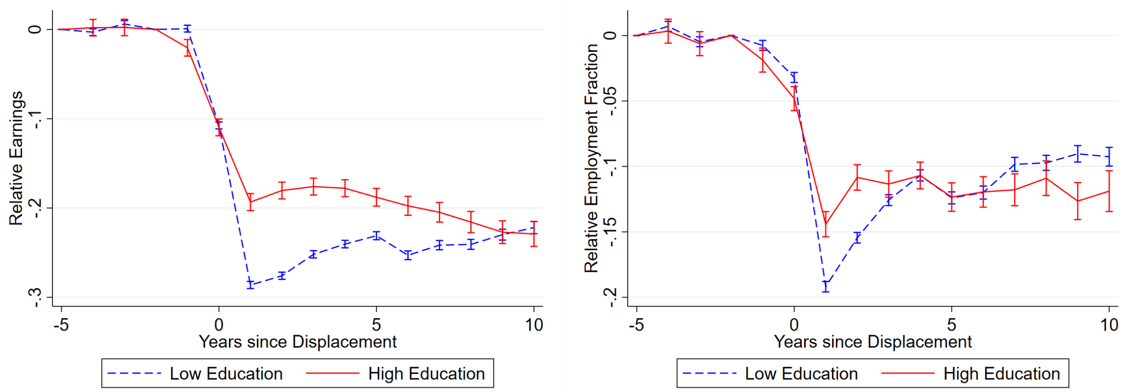


Figure A.52: *The effect of displacement on earnings (left) and employment fraction (right) by education level, relative to the control group, using estimated coefficients from equation* (1.2)*, and using LIAB. The error bars correspond to 95% pointwise confidence intervals.*

figure 1.11). In terms of employment, the results are very similar to those in figure 1.11 in the main text: workers with a lower education level do worse than workers with a high education level in the short run. However, workers with a low education level exhibit some recovery over time, while the highly educated workers do not show much recovery, leading to the lower educated worker to do better than the highly educated worker (in terms of employment) 10 years after displacement.

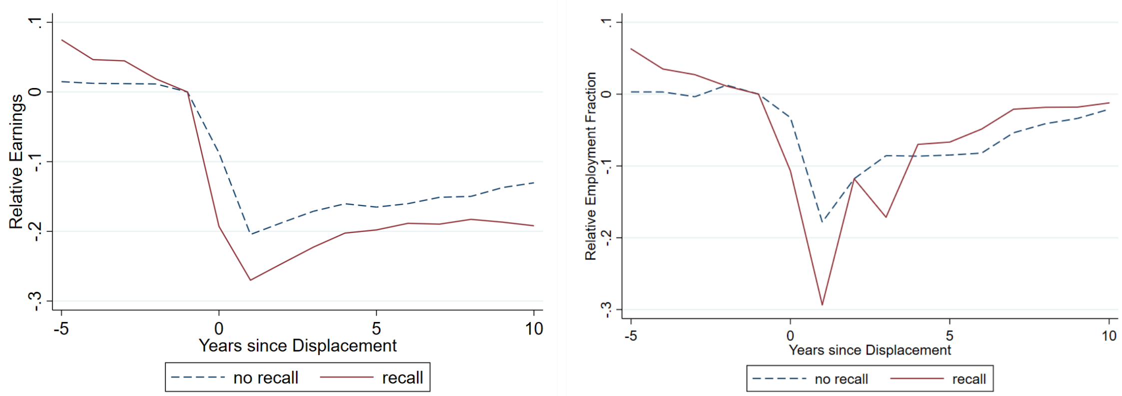


Figure A.53: *The effect of displacement on earnings (left) and employment fraction (right) relative to the control group, by ex-post recall status (materialization of recall within 5 years), using estimated coefficients from equation* (1.1) *and using data from LIAB.*

In figure A.53, I show how the effects of displacement on employment and earnings differs by ex-post recall status using LIAB (the counterpart of figure 1.12 in the main text). Just like in the main text, I find that workers who are recalled suffer from larger earnings losses, and do worse in the short run only when it comes to days employed in the year. As shown in figure A.54, this result once again continues to hold when using the interaction-weighted estimator from specification (1.2), although the difference between recalled and non-recalled is slightly smaller here than in the corresponding figure 1.13 in the main text. The estimation for employment fraction shows a clear “double-dip” for recalled workers. This double dip is partially due to the structure of the data, which samples on establishments. In this case, as seen in figure A.55, taking out the first (1998) cohort from the estimation results in the double dip disappearing, thus suggesting that it may have been caused by disproportionally large number of people being laid off and recalled in that year.

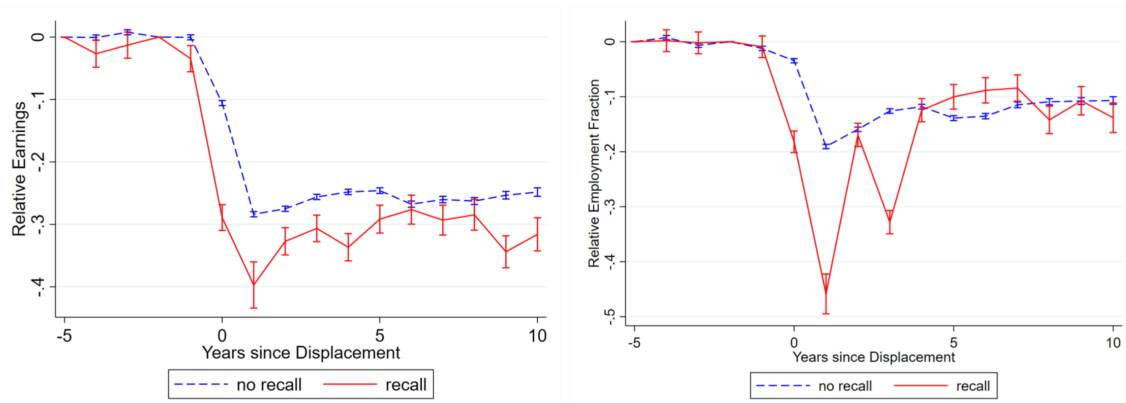


Figure A.54: *The effect of displacement on earnings (left) and employment fraction (right) by ex-post recall status, relative to the control group, using estimated coefficients from equation* (1.2) *and using data from LIAB. The error bars correspond to 95% pointwise confidence intervals.*

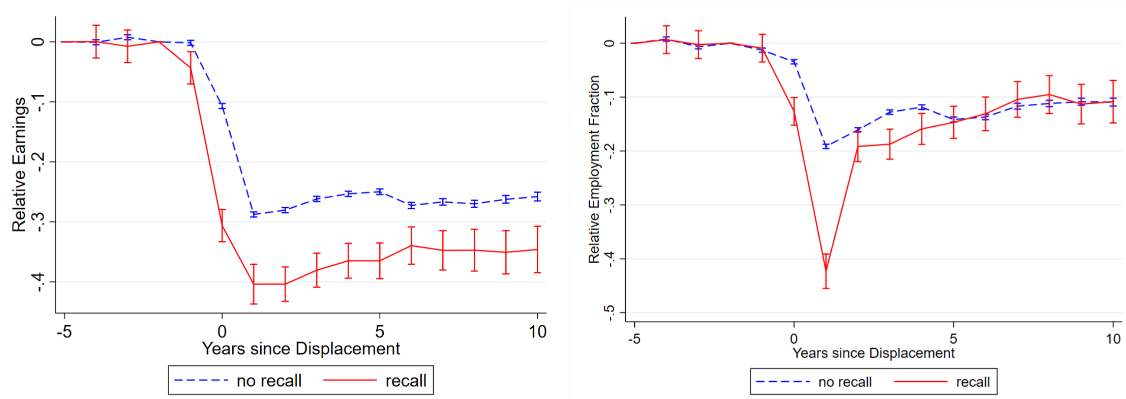


Figure A.55: *The effect of displacement on earnings (left) and employment fraction (right) by ex-post recall status, relative to the control group, using estimated coefficients from equation* (1.2) *corresponding to post-1998 cohorts, and using data from LIAB. The error bars correspond to 95% pointwise confidence intervals.*

Figure A.56 confirms (using LIAB) that recalled workers are much more likely to be separated again shortly after being recalled. Compared to the corresponding figure 1.14 in the main text, the difference is only clear in the first year (rather than the first three years) after displacement, where non-recalled workers are roughly 18 percentage points more likely to be separated than the control group, and recalled workers are more than 30 percentage points more likely to be separated again (compared to the control group). Once again, the result strengthens when I allow the estimation to also use workers who are displaced more than once according to my definition (i.e. they are displaced from high-tenure positions more than once), as shown in the right panel of figure A.56.

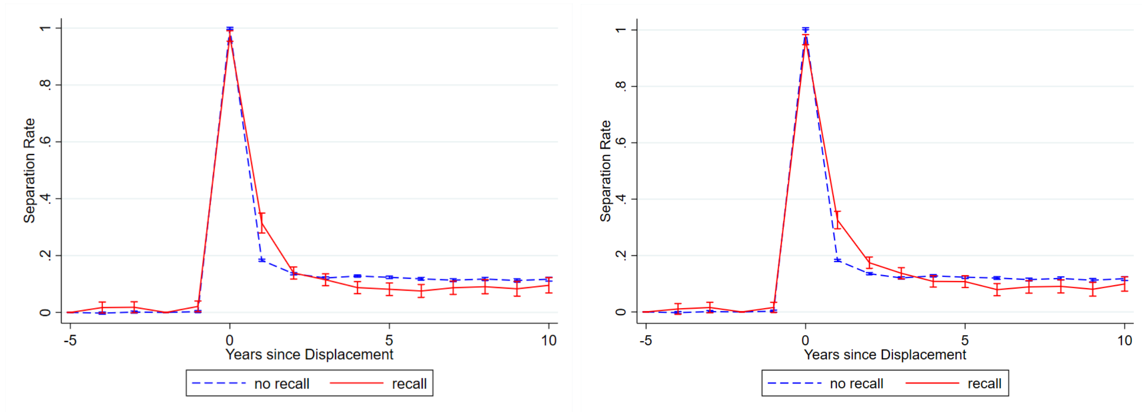


Figure A.56: *The effect of displacement on separation rates by ex-post recall status, relative to the control group, using estimated coefficients from equation 1.2 and using data from LIAB. The error bars correspond to 95% pointwise confidence intervals. Left: estimation allowing for only one displacement per individual; Right: estimation allowing for multiple displacements per individual (classifying the worker according to their first displacement).*

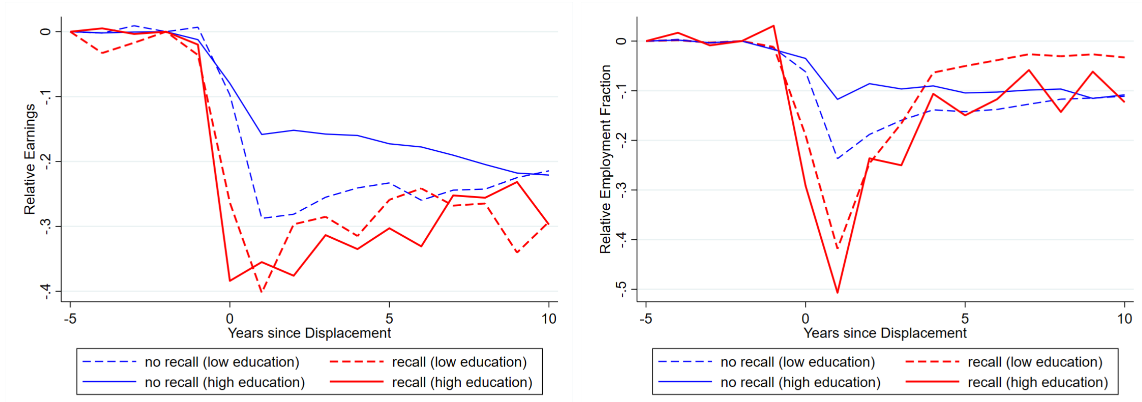


Figure A.57: *The effect of displacement on earnings (left) and employment fraction (right) by ex-post recall status and education level, relative to the control group, using estimated coefficients from equation* (1.2) *and using data from LIAB. The error bars corresponding to 95% pointwise confidence intervals are omitted and available upon request.*

In figure A.57, I show that the observations on the scarring effects experienced by recalled workers (as opposed to non-recalled displaced workers) hold across the two education levels. However, it is worth noting that the difference in earnings loss between recalled and non-recalled workers in the LIAB is larger for the high education group. This is primarily because the non-recall group does better for the highly educated workers, consistent with the observations made in figure A.51, although it can also be observed that the recall group for highly educated workers does slightly worse. Note that these differences also arise (though not as strongly) when looking at the fraction of the years spent in employment, as shown in the right panel of the figure.

Appendix B

Appendices to *Job displacement scars after a recall: A model-based decomposition*

B.1 Numerical Methods

B.1.1 Solution Method

Due to its size and structure, the model presented in Section 2.2 is not analytically solvable. Instead, in order to obtain the results in Section 2.4, I solve the model numerically. The step-by-step procedure followed to obtain the model solution in this paper is described below. It takes as given the values for all parameters.

1. Set up the grid for worker fixed effect ε, using the proportions found in the data (ϵ_1, ϵ_2)

2. Set up the grid for s (over which the model will be solved). In particular, let the maximum grid point be such that 99.9% of workers would expect to stay below it even if they were employed at all times for 30 years. Remaining grid-points above the middle are set by dividing the max location by 3 and using integer arithmetic, so that the majority of the grid-points is near the middle (where workers will start). A similar approach is used for grid points below the middle, though the gridpoints between the mid and the min are set by dividing the difference by 4 rather than 3. Note that while the number of steps between the grid points is constant between worker types, the value of s at those grid points depends on ε through both the stepsize $\Delta_s(\varepsilon)$ and initial value s_ε (which determines the value at the middle point of the grid), so there is a different grid for each ε

3. Set up the grids for y and δ (for each ε). In particular:

    - For y: Divide the unit interval into $N_y - 3$ intervals, and let the midpoint for each of these intervals be i. The first $N_y - 3$ grid-points then correspond to the value of y for which the cdf equals i. The final 3 grid-points correspond to the values of y for which the cdf equals 0.95, 0.99, and 0.999 respectively (noting that the grid-points are subsequently sorted, in case $N_y - 3$ is higher than 20 and therefore the highest of the $N_y - 3$ first grid-points is higher than at least one of the three extra grid-points).

- For δ: Divide the unit interval into N_δ equally-sized intervals, and let the lower and upper bound for these intervals be i_{down} and i_{up}. Now, invert these bounds such that B_{down} and B_{up} are the values for δ for which the cdf equals i_{down} and i_{up}. The values of the grid points then equals the expected value of δ, conditional on δ being between B_{down} and B_{up}.

4. Set up the cdf of θ (for each ε), using Frank's copula, so that if u_1 is the probability that $y \leq y_1$ and u_2 is the probability that $\delta \leq \delta_1$, then the probability for both $y \leq y_1$ and $\delta \leq \delta_1$ to hold is
$$G(y_1, \delta_1, \rho) = -\frac{1}{\rho} \ln \left[1 + \frac{\exp(-\rho u_1 - 1)\exp(-\rho u_2 - 1)}{\exp(-\rho) - 1}\right]$$
   - Once the joint cdf is calculated using the formula above, the probability matrix for θ can be retrieved,defined on a discrete grid.

5. (From this step, loop over ε) Since equations (2.13) and (2.7) only depend on functions W^{max} and U and known functions and parameters, use an iterative loop to solve for functions W^{max} and U. In particular:
   (a) Guess an initial matrix for W^{max} (N_y by N_δ by N_s by 2) and U (1 by N_s).[1]
   (b) Using initial guesses W^{max} and U, calculate an updated $U(s)$ for all s and call this $U^*(s)$. For the next step, set the new guess for U as $\hat{U}(s) = \omega_u U^*(s) + (1 - \omega_u)U(s)$ (with some $\omega_u \in (0, 1]$)
   (c) Now, using initial guess W^{max} and updated $\hat{U}$, calculate the implied value for value function T. To do this, first using its recursive structure to solve directly, assuming that a worker expecting to be recalled cannot search for a new job. Then, in a second iteration, add the search option, using the previously calculated T as the possible outside option, and re-calculate T.
   (d) Using initial guess W^{max}, updated $\hat{U}$, and implied value T, calculate an updated $W^{max}(s, \theta)$ for all combinations of s and θ and call this $W^{max*}(s, \theta)$. For the next step, set the new guess for W^{max} as $\hat{W}^{max}(s, \theta) = \omega_s W^{max*}(s, \theta) + (1 - \omega_s)W^{max}(s, \theta)$ (with some $\omega_s \in (0, 1]$)
   (e) Calculate the distance between the initial W^{max} and the updated W^{max}. If this distance is not close enough to zero, return to step b, setting $U = \hat{U}$ and $W^{max} = \hat{W}^{max}$.
   (f) Calculate the distance between the initial U and the updated U. If this distance is not close enough to zero, return to step b, setting $U = \hat{U}$ and $W^{max} = \hat{W}^{max}$.

6. Using the calculated value for $W^{max}(s, \theta)$ and $U(s)$, calculate the value for T by using the same procedure as in step 5c, but now repeatedly executing the second step until the value for T converges.

[1] The fourth dimension of the matrix W^{max} is used to distinguish whether the match carries a separation rate penalty from a previous recall. In the remainder of the description of this solution method I ignore this for notational convenience. In practice, the two 3-dimensional matrices are closely linked together, using only a single matrix for the value of T and U, and further linking through on-the-job search (as an EE transition will lead the worker to transition to a job that does not carry this penalty).

7. Now that $W^{max}(s,\theta)$ and $U(s)$ are known for all s and θ, and noting that $W^{max}(s,u) = U(s)$, we can use the bargaining condition to calculate $W(s,s,\theta,\hat{\theta}) = W^{max}(s,\hat{\theta}) + \kappa(W^{max}(s,\theta) - W^{max}(s,\hat{\theta}))$. In other words, since we know that at the time of bargaining the extended version of equation 3 holds, but not necessarily if $s \neq \hat{s}$ (note that since s can only go up during employment $s \neq \hat{s}$ implies $s > \hat{s}$), we now know the diagonal elements of $W(s,\hat{s},\cdot)$ only.

8. Solve for the wage: See section B.1.2 below.

B.1.2 Derivation of the wage

To derive the wage (or rather the piece-rate), I use value function W (again omitting the ε):

$$\begin{aligned} W(s,\hat{s},\theta,\hat{\theta}) &= \ln(R(\hat{s},\theta,\hat{\theta})p(s,y)) + \beta\mathbb{E}_{s'|s,e}\Bigg\{\delta\left[\phi_f \max\{\hat{T}(s',\theta),\hat{U}(s')\} + (1-\phi_f)\hat{U}(s')\right] \\ &+(1-\delta)\Bigg[\lambda^e\left(\int_{x\in\Theta^1(s',\theta)} W(s',s',x,\theta)dG(x) + \int_{x\in\Theta^2(s',\hat{s},\theta,\hat{\theta})} W(s',s',\theta,x)dG(x)\right) \\ &+\left(1-\lambda^e\int_{x\in\Theta^1(s',\theta)\cup\Theta^2(s',\hat{s},\theta,\hat{\theta})} dG(x)\right) W(s',\hat{s},\theta,\hat{\theta})\Bigg]\Bigg\} \end{aligned}$$

Further, note that given a known value for W^{max} and U (for every s and θ), the value $T(s,\theta)$ is known:[2]

$$\begin{aligned} T(s,\theta) &= \ln(b(s)) + \beta\mathbb{E}_{s'|s,r}\Bigg\{\phi^r\kappa^r W^{max}(s',\theta') + \phi^r(1-\kappa^r)\max\{T(s',\theta),U(s')\} \\ &+(1-\phi^r)\left(\lambda^r\int_{x\in\Theta^r(s',\theta)} \kappa\left(W^{max}(s',x) - \max\{T(s',\theta),U(s')\}\right)dG(x) + \max\{T(s',\theta),U(s')\}\right)\Bigg\} \end{aligned}$$

Similarly, for given values W^{max} and U, and T, the values $\hat{U}$ and $\hat{T}$ can be directly calculated using equations (2.11) and (2.12). Throughout the derivation, I will therefore denote the value of a newly nonemployed worker expecting a recall by $\bar{T}$, denoting that since this value is known I consider it to be a constant:

$$\begin{aligned} W(s,\hat{s},\theta,\hat{\theta}) &= \ln(R(\hat{s},\theta,\hat{\theta})p(s,y)) + \beta\mathbb{E}_{s'|s,e}\Bigg\{\delta\left[\phi_f \max\{\bar{T}(s',\theta),\hat{U}(s')\} + (1-\phi_f)\hat{U}(s')\right] \\ &+(1-\delta)\Bigg[\lambda^e\left(\int_{x\in\Theta^1(s',\theta)} W(s',s',x,\theta)dG(x) + \int_{x\in\Theta^2(s',\hat{s},\theta,\hat{\theta})} W(s',s',\theta,x)dG(x)\right) \\ &+\left(1-\lambda^e\int_{x\in\Theta^1(s',\theta)\cup\Theta^2(s',\hat{s},\theta,\hat{\theta})} dG(x)\right) W(s',\hat{s},\theta,\hat{\theta})\Bigg]\Bigg\} \end{aligned}$$

Further, note that:

$$\begin{aligned} &x \in \Theta^1(s',\theta) \iff W^{max}(s',x) \geq W^{max}(s',\theta) \\ &x \in \Theta^2(s',\hat{s},\theta,\hat{\theta}) \iff W^{max}(s',\theta) > W^{max}(s',x) \geq W^{max}(\hat{s},\hat{\theta}) \\ &W(s,s,x,\theta) = W^{max}(s,\theta) + \kappa\left(W^{max}(s,x) - W^{max}(s,\theta)\right) \end{aligned}$$

[2]To be specific, I solve for the value $T(s,\theta)$ before solving for the wage, as noted in the previous subsection.

Since I know the value of W^{max}, U, $\bar{T}$, and p for a given combination of s and θ, this implies that the only unknowns in the value function are $W(s, \hat{s}, \theta, \hat{\theta})$, $R(\hat{s}, \theta, \hat{\theta})$, and $W(s', \hat{s}, \theta, \hat{\theta})$.

As these are all using the same value for $\hat{s}$, θ and $\hat{\theta}$, this equation can be greatly simplified, by defining the following constants (where the subscript denotes current human capital level s, i.e. the first variable in the value function):

$$
\begin{aligned}
C_{s'} &= \beta(1-\delta)\lambda^e \left(\int_{x \in \Theta^1(s',\theta)} W(s', s', x, \theta) dG(x) + \int_{x \in \Theta^2(s', \hat{s}, \theta, \hat{\theta})} W(s', s', \theta, x) dG(x) \right) \\
&\quad + \beta\delta(1-\phi_f)\hat{U}(s') + \beta\delta\phi_f \max\{\bar{T}(s', \theta), \hat{U}(s')\} \\
a_{s'} &= \beta(1-\delta) \left(1 - \lambda^e \int_{x \in \Theta^1(s',\theta) \cup \Theta^2(s', \hat{s}, \theta, \hat{\theta})} dG(x) \right)
\end{aligned}
$$

We can use this notation to rewrite the value function W as follows:

$$
W(s, \hat{s}, \theta, \hat{\theta}) = \ln(R(\hat{s}, \theta, \hat{\theta}) p(s, y)) + \mathbb{E}_{s'|s,e} \left\{ C_{s'} + a_{s'} W(s', \hat{s}, \theta, \hat{\theta}) \right\}
$$

The expression above can be simplified further by using the simple structure of the expectations operator. If the match is formed (as denoted by the subscript e), there are only two options for the future level of s, s': With probability ψ_e, $s' = s + 1$ (i.e. the previous level plus 1 stepsize, which may not necessarily be the next grid point) and with probability $1 - \psi_e$, $s' = s$. The one exception to this is that if the worker is at the maximum value of s, in which case $\psi_e = 0$.[3] Below, I rewrite the value function using this structure. In what follows, I use $\psi = \psi_e$ (for ease of notation):

$$
W(s, \hat{s}, \theta, \hat{\theta}) = \ln(R(\hat{s}, \theta, \hat{\theta}) p(s, y)) + \psi \left\{ C_{s+1} + a_{s+1} W(s+1, \hat{s}, \theta, \hat{\theta}) \right\} + (1-\psi) \left\{ C_s + a_s W(s, \hat{s}, \theta, \hat{\theta}) \right\}
$$

In what follows, I will drop the elements $\hat{s}$, $\hat{\theta}$ and θ, so that this equation becomes:

$$
\begin{aligned}
W_s &= \ln(Rp(s, y)) + \psi \left\{ C_{s+1} + a_{s+1} W_{s+1} \right\} + (1-\psi) \left\{ C_s + a_s W_s \right\} \\
W_s \left[1 - (1-\psi) a_s \right] &= r + \ln(p(s, y)) + \psi \left\{ C_{s+1} + a_{s+1} W_{s+1} \right\} + (1-\psi) C_s
\end{aligned}
$$

This is a system of equations for each value of $\hat{s}$ on the grid. Since $s \geq \hat{s}$, there are (with slight abuse of notation) $N_s - \hat{s} + 1$ equations, one for each $s \geq \hat{s}$, and $N_s - \hat{s} + 2$ unknowns, one for each value W_s and the piecerate R. However, one additional equation can be added, which does not add any unknowns: $W_{\hat{s}} = W^{max}(\hat{s}, \hat{\theta}) + \kappa \left(W^{max}(\hat{s}, \theta) - W^{max}(\hat{s}, \hat{\theta}) \right)$

The resulting system of equations has $N_s - \hat{s} + 2$ equations and $N_s - \hat{s} + 2$ unknowns and can thus be solved. In order to do so, I set up matrix A and vector B, such that the system is represented as $Ax = B$, where x is a vector containing the unknowns. These matrices will be $N_s - \hat{s} + 2$ by $N_s - \hat{s} + 2$, but take an easily generalizeable form. For example, for $\hat{s} = N - 2$, the vectors and

[3]Note that technically there is no maximum value of s, but I do solve the model on a limited number of grid points for s. Later in this section, I briefly comment on how I reconcile this.

matrices will look as follows (denoting $p_s = p(s, y)$ and $r = \ln(R)$):

$$Ax = \begin{pmatrix} 1-a_N & 0 & 0 & -1 \\ -\psi a_N & 1-(1-\psi)a_{N-1} & 0 & -1 \\ 0 & -\psi a_{N-1} & 1-(1-\psi)a_{N-2} & -1 \\ 0 & 0 & 1 & 0 \end{pmatrix} \cdot \begin{pmatrix} W_N \\ W_{N-1} \\ W_{N-2} \\ r \end{pmatrix}$$

$$B = \begin{pmatrix} C_N + \ln(p_N) \\ \psi C_N + (1-\psi)C_{N-1} + \ln(p_{N-1}) \\ \psi C_{N-1} + (1-\psi)C_{N-2} + \ln(p_{N-2}) \\ W^{max}(\hat{s}, \hat{\theta}) + \kappa\left(W^{max}(\hat{s}, \theta) - W^{max}(\hat{s}, \hat{\theta})\right) \end{pmatrix}$$

Unfortunately, there is one small complication: the method above is based on the assumption that there is a maximum level of human capital. However, given that workers in the model are infinitely-lived, workers could in principle accumulate an infinite amount of human capital if I would run the simulation for an infinite number of periods. Furthermore, as the workers can infinitely accumulate human capital, there are an infinite number of possible values for s and $\hat{s}$.

I get around this issue by using an approximation. In particular, I solve the model (and therefore also the wage) only for a limited number of human capital grid-points, and interpolate and extrapolate the solution for all other grid-points. These grid-points for the solution are heavily concentrated near the lowest possible level, as every worker starts at this low level, and therefore many workers will pass through these grid-points. As mentioned in the previous subsection, I select the maximum grid-point by calculating the grid-point that is achieved only by the top 0.1% of the workers after 30 years.

Of course, solving the model on a limited grid also has consequences for some of the equations discussed above (and explicitly so where I explicitly use the structure of the expectations operator). In practice, I therefore use a slightly adjusted formulation of the matrix A and vector B above. In the matrix A, there are two changes. First in every row except for the first and last row of matrices A and B, I replace ψ by $\psi\frac{\Delta_s}{(N)-(N-1)}$ (for the second row, and similarly for other rows using other values of N), where Δ_s is the actual jump in human capital upon ψ materializing, and N and $N-1$ are the values of s on the Nth and (N-1)st grid-point. This reflects the interpolation between grid points. For the top row, the extrapolation implies that the top left element of A becomes $1-(1+\bar{\psi})a_N$, where $\bar{\psi} = \psi\frac{\Delta_s}{(N)-(N-1)}$. The second element of the first row becomes $\bar{\psi}a_{N-1}$. Finally, the top row of vector B becomes $(1+\bar{\psi})C_N - \bar{\psi}C_{N-1} + \ln(p_N)$. To be explicit, this means that the vectors and matrices will look as follows in practice:

$$A = \begin{pmatrix} 1-\left(1+\psi\frac{\Delta_s}{(N)-(N-1)}\right)a_N & \psi\frac{\Delta_s}{(N)-(N-1)}a_{N-1} & 0 & -1 \\ -\psi\frac{\Delta_s}{(N)-(N-1)}a_N & 1-\left(1-\psi\frac{\Delta_s}{(N)-(N-1)}\right)a_{N-1} & 0 & -1 \\ 0 & -\psi\frac{\Delta_s}{(N-1)-(N-2)}a_{N-1} & 1-\left(1-\psi\frac{\Delta_s}{(N-1)-(N-2)}\right)a_{N-2} & -1 \\ 0 & 0 & 1 & 0 \end{pmatrix}$$

$$B = \begin{pmatrix} \left(1+\psi\frac{\Delta_s}{(N)-(N-1)}\right)C_N - \psi\frac{\Delta_s}{(N)-(N-1)}C_{N-1} + \ln(p_N) \\ \psi\frac{\Delta_s}{(N)-(N-1)}C_N + \left(1-\psi\frac{\Delta_s}{(N)-(N-1)}\right)C_{N-1} + \ln(p_{N-1}) \\ \psi\frac{\Delta_s}{(N-1)-(N-2)}C_{N-1} + \left(1-\psi\frac{\Delta_s}{(N-1)-(N-2)}\right)C_{N-2} + \ln(p_{N-2}) \\ W^{max}(\hat{s},\hat{\theta}) + \kappa\left(W^{max}(\hat{s},\theta) - W^{max}(\hat{s},\hat{\theta})\right) \end{pmatrix}$$

Note that x is still the same as specified above, but using only the value function W on the grid points (along with the piece-rate). The matrix equation $Ax = B$ is then solved for x, using LU decomposition, and the solution will yield the piece-rate $R = e^r$ for this particular value of $\hat{s}$, θ, and $\hat{\theta}$, and solving this system of equations for every combination of $\hat{s}$ (on the grid), θ, and $\hat{\theta}$ (including u) will complete the solution.

B.1.3 Calibration Method

In this subsection, I will describe in more detail how I estimate the moments used for the calibration of the model (see section 2.3), both in the data and in the model simulation. When estimating these moments in the data, I restrict the data such that I only consider workers with a market tenure of at least 3 years. This is to avoid biased estimates due to traineeships.[4] With the exception of the yearly wage growth, all moments are estimated using the quarterly data set.

B.1.3.1 Employment Rate and Transition Rates

As argued in section 2.3.2, the transition rates of workers between employment and unemployment and between employment at different establishments aids primarily in the identification of the job offer rates, λ_ε^e, λ_ε^u, and λ_ε^{ug}, and the marginal distribution of δ. The estimation of these moments described below.

For the average rate of job loss, I create a variable that is only filled if the worker is employed in the current quarter and still observed in the next quarter. I set this variable to 0 if the worker is still employed next quarter and 1 if the worker is unemployed in the next quarter.[5] The job loss rate by tenure is then estimated by taking a simple average over all workers with an establishment tenure of 1 to 3.5 years (i.e. more than exactly 1 year, less than exactly 3.5 years), 3.5 to 6 years, 6 to 9 years, and more than 9 years. Similarly, I take the simple average over all workers with a low and high education level to find the education-specific unconditional rates of job loss. Finally, I take the average over all workers who returned from nonemployment to find the rates of subsequent separation for displaced, and take the average over all workers who returned from nonemployment through a recall to find the rate of subsequent separation for recalled workers.

When estimating the job-to-job transition rate, a similar variable is created (and filled under the same conditions). Now, the variable equals 1 if the worker is employed for a different establishment next quarter. In the data, this can be tracked using the establishment id number. In the model, the firm productivity y can be used for this. After all, since the marginal distribution of y has a continuous support, the probability that two different establishments in the model have the exact

[4] In principle, workers can be flagged as a trainee in the data, and these observations were omitted when estimating the empirical results, and further do not count towards the measure of market tenure. Thus, this restriction is merely a safety measure to avoid bias arising because certain trainees may not be coded as such.

[5] Note that in the model I consider workers that are expecting a recall to be unemployed for this purpose, reflecting that in the data I do not see whether a worker is expecting to be recalled.

same productivity is negligible.[6] In order to construct the moment, I then take the average by education group. Similarly, I calculate the job-to-job transition rate upon displacement (by education group) by following the same procedure, but conditioning the filling of the variable of interest on the worker experiencing a displacement event in the (current) quarter.

In order to estimate the average job finding rate, a similar procedure is followed. However, for this moment, only nonemployed workers (including those expecting a recall) are considered, and the variable equals 1 if they are employed in the next quarter. To compute the moment value, the average is taken by education group.

Finally, the estimation of the employment rate in the data is fairly simple, and merely requires the average of a variable that equals the fraction of the quarter the worker spent in an employment relation. In the model, I do not need to keep track of this explicitly, as employed workers will always be employed for the full quarter, which will be marked by having a strictly positive firm productivity y. In other words, the value of this fraction in the model will always be either 0 or 1.

B.1.3.2 p75-p25 and median-p25 Ratios of Wages

In order to estimate the p75-p25 and median-p25 ratios of wages (by education group) in the data, I restrict the sample to full-time workers only, along with the aforementioned restriction on market tenure. Furthermore, I restrict the observations to those who are (full-time) employed for the entire quarter. In the data, I can then directly summarize the wage by education group, which will yield the 25th percentile, median, and 75th percentile wage. Once these are retrieved, the p75-p25 and median-p25 ratio can be calculated directly.

In the model, the simulation is set up such that workers are ordered by education group, making the separation of individuals by education straightforward. For each education group, I isolate all wages of employed workers.[7] The 25th percentile, median, and 75th percentile wage can then be calculated directly by sorting the resulting vector of wages and taking out the middle observation and the observation at the 25th and 75th percentile. The ratios of interest can then be directly calculated.

B.1.3.3 Replacement Rate, and Average Wage of New Hires

In order to calculate the replacement rate in the model, I need to calculate the average wage and the average unemployment benefit in the simulation. As I track the quarterly wage throughout the entire simulation, this is straightforward to do, and it only requires restricting the sample to employed workers (for the average wage) and non-employed workers (for the average unemployment benefit). Denoting this average wage by $\bar{w}$ and the average unemployment benefit by $\bar{b}$, the replacement rate then equals $\bar{b}/\bar{w}$. As the data counterpart is taken directly from OECD (2020), no further estimation is necessary in the data.

The average wage calculated in order to calculate the replacement rate is also used when calculating the average (relative) wage of new hires and newly recalled workers. Denoting the average wage of new hires (or new recalls) by $\bar{w}_N$, this moment equals $\bar{w}_N/\bar{w}$. In order to calculate $\bar{w}_N$, I

[6] An important note to make here is that in the case where a displacement and recall take place within the same quarter, a worker can change firm productivity y while not switching establishments in the model. I account for this by explicitly keeping track of recall events in the simulation.

[7] In the model, a restriction to full-time workers is not necessary, since the model does not allow for part-time work.

restrict the sample to workers with an establishment tenure of more than a quarter, and less than a year, who are (full-time) employed for the entire quarter, and were unemployed before starting at their current establishment. The average wage of newly recalled workers is calculated in an identical way, restricting the average wage of new hires to those of workers for whom their current establishment is the same as the establishment they worked for prior to their preceding unemployment spell. Calculating the data counterpart of the average wage $\bar{w}$ uses the data equivalent of the procedure outlined above for the replacement rate, again restricting the sample to full-time workers who are employed for the entire quarter. Note that when I estimate this moment in the data, I omit the top and bottom 5% of observations when calculating $\bar{w}_N$ and $\bar{w}$. This is to avoid an extreme influence by some of the outliers I see in the data.

B.1.3.4 Average Educational Wage Premium, Overall and Upon Entry

In order to estimate the educational wage premium, the same dataset of wages is used as in the previous subsection (though the dataset is separated by education group). In order to estimate the educational wage premium, I estimate the average wage of each education group (again omitting the top and bottom 5%). Denoting this average by $\bar{w}_\varepsilon$, the educational wage premium then equals $\bar{w}_2/\bar{w}_1$. When estimating this educational wage premium upon entry, the same procedure is followed, further restricting the sample to workers with a market tenure of 3 to 5 years (i.e. more than exactly 3 years, and less than exactly 6 years).

B.1.3.5 Average Yearly Wage Growth

As mentioned earlier, these moments are the only ones for which the yearly dataset is used. In particular, I restrict the sample in the yearly dataset to workers with a market tenure of at least 3 years who were full-time employed for the entire year as well as the entire next year. For each worker-year combination for which this holds, I then calculate the yearly wage growth as $w_{t+1}/w_t - 1$, after which the average yearly wage growth is a simple average over workers of the same education group (omitting the top and bottom 5%).

B.1.3.6 Recall and Recall Materialization Rates

In order to estimate the recall rate and the recall materialization rate in the data, I look forward up to 5 years from the point of separation. If the worker's main employing establishment in her first quarter at full employment is the same establishment as the one she was displaced from, I count it as a recall materialization. Further, I record whether or not the recall occurred within 1 or 2 years of displacement. From the resulting variable, I then calculate the recall rate as the fraction of displaced workers that are recalled within 5 years. The recall materialization rates are calculated by first obtaining the fraction of recalled workers that were recalled within 1 or 2 years. The recall materialization rates are then calculated as the constant quarterly materialization rate such that this fraction would indeed be recalled within 1 or 2 years.

In the model, it is much easier to detect recalls, as the worker can only have one employer, and I keep track of that employer's productivity for the purpose of the simulation. Beyond that, calculating the recall rate and recall materialization is done using the same method as used in the data.

In order to calculate the new job finding rate for temporarily unemployed workers that are re-employed within a year, I need to keep track of the exact nonemployment state of the worker as well (to disentangle job finding from unemployment and new job finding from temporary unemployment). I do this in the model simulation by assigning workers who are expecting to be recalled a productivity equal to -1 times the productivity of their former employer. I can then calculate the moment of interest by taking all such workers who are re-employed from a state of temporary unemployment (as indicated by the productivity in the period before re-entering employment) and were in that state for at most a year, and calculating the fraction of these workers who moved to a new employer rather than being recalled. As I cannot see in the data whether workers are expecting to be recalled, I base the data equivalent of this model on findings in Nekoei and Weber (2015), who find in their Austrian data that 58% of workers who report to expect being recalled are in fact recalled within a year, while 24% of these workers who are expecting a recall find a new job within a year. Translating this to a new job finding rate conditional on being re-employed from a state of temporary unemployment within a year then yields a data equivalent of 29.27%.

B.1.3.7 Pre- to Post-layoff Wage Differentials

In order to calculate the average pre- to post-layoff wage differential, I first identify all individuals who were working full-time at the job from which they were laid off (this is true by definition in the model). The resulting sample is split into 16 subsamples: by education group, and according to unemployment duration in quarters (ranging from 1 quarter to 8 quarters). The pre-layoff wage is then equal to the wage in the quarter before the layoff, provided that the worker worked full-time at this same establishment for this entire previous quarter. Further restricting the sample to workers whose next job after re-employment is also full-time, the post-layoff wage is equal to the average wage in the first four full quarters after starting this job (conditional on being full-time employed for that entire quarter). The resulting wage differential is the difference between this pre- and post layoff wage. The same procedure is then followed for a control group of non-displaced workers (looking forward the same amount of time as for the corresponding treatment group), after which the moment of interest is the average of the differences in these differences across duration quarters that fall within each group of interest (1 quarter to 0.5 year, 0.5 to 1 year, and 1 to 2 years). Thus, the moment is essentially an average of coefficients of difference-in-difference estimations, where a separate estimation is done for each education level and quarter of nonemployment duration. It should be noted that this calculation excludes workers who found a new job immediately or within a quarter. Further, I exclude workers with an unemployment duration of more than 2 years, due to a low number of observations with a higher duration in the data (especially for the high education level).

In a separate set of moments, I calculate these same wage differentials, restricting the sample to workers who are recalled (using only workers with a nonemployment duration of 1 to 3 quarters). In the model, these workers are relatively straightforward to pick out, but in the data this involves looking forward from the moment of separation to see whether the worker will eventually be recalled (as described in the previous subsubsection). Restricting the sample to workers who will be recalled, these moments are nevertheless calculated in the exact same way for each education group, separately for those with a nonemployment duration of 1 quarter and those with a duration of 2 or 3 quarters.

B.1.3.8 Correlation between Wages and Separation

The final moment to be estimated in the baseline calibration is the regression coefficient $\hat{\gamma}$ in equation (B.1):

$$D^{\delta}_{i,t} = \alpha_i + \gamma \log(w_{it}) + u_{i,t} \tag{B.1}$$

In the data, this equation can be estimated using a standard fixed effects estimation. Given the number of individuals in the simulation (and therefore the number of individual fixed effects), however, this is a quite computationally intensive estimation to estimate in each iteration of the calibration. Therefore, I use the fact that the individual fixed effect is constant over time to greatly simplify the estimation, while not throwing out any observations. In particular, I calculate the average log wage for each individual, restricting the calculation in the data to wages in full-quarter full-time employment. Similarly, I calculate the average value of the separation indicator (which was created earlier to calculate the average rate of job loss) over all the periods for which it is filled. Then, I rewrite the equation by subtracting the average from both sides:

$$D^{\delta}_{i,t} - \bar{D}^{\delta}_{i,t} = \alpha_i - \bar{\alpha}_i + \gamma \log(w_{it}) - \gamma\overline{\log(w_{it})} + u_{i,t} - \bar{u}_{i,t} \tag{B.2}$$

$$\left(D^{\delta} - \bar{D}^{\delta}\right)_{i,t} = \gamma \left(\log(w) - \overline{\log(w)}\right)_{it} + u_{i,t} \tag{B.3}$$

As can be seen in equation (B.3), all elements on both sides of the equation now depend on both i and t, thus allowing for a simple OLS estimation, yielding coefficient $\hat{\gamma}$.

B.1.3.9 Explicit Estimation of the Scarring Effect of Displacement

In an alternative calibration of the model, I could estimate the model by directly targeting the scarring effects of displacement by (ex-post) recall status that were estimated in section 1.3.3.2 of the main text. In other words, I target the outcome of the estimation of the following equation:

$$e_{it} = \alpha_i + \gamma_t + \sum_{C\neq 0} \sum_{\substack{k=-4 \\ k\neq -2}}^{10} \delta^C_k D^{C,k}_{it} + u_{it} \tag{B.4}$$

Given the presence of individual and time fixed effects in equation (B.4), this estimation yields similar issues as those pointed out in the previous subsubsection. However, the structure of the model and its simulation allow me to make several simplifications. First, note that while different cohorts in the data pick up effects of (among others) differences in economic conditions at the time of displacement, there are no such differences in the model. Therefore, I do not allow for different estimates by cohort in the model equivalent, thus reducing the equation as follows:

$$e_{it} = \alpha_i + \gamma_t + \sum_{\substack{k=-4 \\ k\neq -2}}^{10} \delta_k D^{k}_{it} + u_{it} \tag{B.5}$$

Then, to get around having to estimate the fixed effects explicitly, I interpret the equation above as a two-way error component model, and use the two-way within transformation from Hansen (2021). In particular, this means that for both the dependent and independent variables in equation

(B.5), I calculate $\ddot{X}_{it} = X_{it} - \bar{X}_i - \tilde{X}_t + \bar{X}$, where $\bar{X}$ is the average variable over all individuals and time periods, $\tilde{X}_t$ is the average over individuals within a time period t, and $\bar{X}_i$ is the average over all time periods for an individual i. Using this transformation, the equation to be estimated reduces to the following equation:

$$\ddot{e}_{it} = \sum_{\substack{k=-4 \\ k\neq -2}}^{10} \delta_k \ddot{D}_{it}^k + \ddot{u}_{it} \tag{B.6}$$

The above equation can be estimated fairly easily using OLS, which thus yields the model equivalent of the moments (with one moment for every k). Note that the model estimation is not exactly identical to the data equivalent, because the panel in the simulation is not completely balanced (for example, because I omit simulation data from individuals above the age of 62). Therefore, the targeting of the scarring effect is not as precise as it would be if I were to estimate (B.4) directly, but the transformation does make this (imperfect) targeting feasible, and is therefore allows me to use this for an alternative calibration.

B.2 Model Appendix

B.2.1 Further Value Functions

As mentioned in section 2.2, the model can be solved using value functions from the worker side only. However, it could still be valuable to consider what the value function for a (producing) firm looks like. The value function J for a firm of type θ, employing a worker of type ε with human capital s, is as follows:

$$\begin{aligned} J_\varepsilon(s,\hat{s},\theta,\hat{\theta}) = \left(1 - R_\varepsilon(\hat{s},\theta,\hat{\theta})\right) p(s,y) + \beta \mathbb{E}_{s'|s,e,\varepsilon} \Bigg\{ (1-\delta) \Bigg[\lambda_\varepsilon^e \int_{x\in\Theta_\varepsilon^2(s',\hat{s},\theta,\hat{\theta})} J_\varepsilon(s',s',\theta,x) dG_\varepsilon(x) \\ + \left(1 - \lambda_\varepsilon^e \int_{x\in\Theta_\varepsilon^1(s',\theta)\cup\Theta_\varepsilon^2(s',\hat{s},\theta,\hat{\theta})} dG_\varepsilon(x)\right) J_\varepsilon(s',\hat{s},\theta,\hat{\theta}) \Bigg] + \delta \bar{\phi}_\varepsilon^f(s,\theta) \hat{J}_\varepsilon^f(s',\theta) \Bigg\} \end{aligned} \tag{B.7}$$

Here, $\bar{\phi}_\varepsilon^f(s,\theta) = \phi_\varepsilon^f \mathbb{1}_{T_\varepsilon(s,\theta)>U_\varepsilon(s)}$, capturing that the worker may choose to forego the option of recall. As mentioned before, the value of an unmatched firm is $V = 0$. Finally, $\hat{J}_\varepsilon^f(s,\theta)$ is the value for a firm newly expecting to recall, which can be decomposed into the separation period-specific part and a general value for a firm expecting to recall:

$$\hat{J}_\varepsilon^f(s',\theta) = \phi_\varepsilon^{rg} J_\varepsilon(s',s',\theta',r) + (1-\phi_\varepsilon^{rg}) J_\varepsilon^f(s',\theta) \tag{B.8}$$

$$J_\varepsilon^f(s,\theta) = \beta \mathbb{E}_{s'|s,r,\varepsilon} \left\{ \phi_\varepsilon^r J_\varepsilon(s',s',\theta',r) + (1-\phi_\varepsilon^r) \left(1 - \lambda_\varepsilon^r \int_{x\in\Theta_\varepsilon^r(s',\theta)} dG_\varepsilon(x)\right) \mathbb{1}_{T_\varepsilon(s',\theta)>U_\varepsilon(s')} J_\varepsilon^f(s',\theta) \right\} \tag{B.9}$$

B.2.2 Worker Flows

The description of the model in the main text (section 2.2) can be used to construct a number of worker flow equations. In particular, denote by $d_\varepsilon(s,\hat{s},\theta,\hat{\theta})$ the density of employed workers of type ε with current human capital s, negotiation benchmark human capital $\hat{s}$, matched to a firm with characteristics $\theta \in [0,1] \times \mathbb{R}_+$, and benchmark characteristics $\hat{\theta} \in [0,1] \times \mathbb{R}_+$, and denote by $d_\varepsilon(s,\hat{s},\theta,u)$, $d_\varepsilon(s,\hat{s},\theta,r)$, and $d_\varepsilon(s,\hat{s},\theta,f)$ the equivalents if this worker used unemployment as the outside option at the time of bargaining, was recently recalled to their current job, or found the current job while expecting to be recalled. Further, let $d^f_\varepsilon(s,\theta)$ be the density of workers with current human capital s expecting to be recalled to a firm with (pre-recall) characteristics θ, and let $u_\varepsilon(s)$ be the density of unemployed workers of type ε with human capital s. First, define the following densities, defined after human capital accumulation (or depreciation) takes place:

$$
\begin{aligned}
\bar{d}_\varepsilon(s,\hat{s},\theta,\cdot) &= (1-\psi_e)d_\varepsilon(s,\hat{s},\theta,\cdot) + \psi_e d_\varepsilon(s-\Delta_s(\varepsilon),\hat{s},\theta,\cdot)\\
\bar{d}^f_\varepsilon(s,\theta) &= (1-\psi_r\psi_u)d^f_\varepsilon(s,\theta) + \psi_r\psi_u d^f_\varepsilon(s+\Delta_s(\varepsilon),\theta)\\
\bar{u}_\varepsilon(s) &= (1-\psi_u)u_\varepsilon(s) + \psi_u u_\varepsilon(s+\Delta_s(\varepsilon))
\end{aligned}
$$

The flow equations are then as follows:[8]

$$
\begin{aligned}
d'_\varepsilon(s,\hat{s},\theta,\hat{\theta}) = (1-\delta)&\left(1-\lambda^e_\varepsilon\int_{x\in\Theta^1_\varepsilon(s,\theta)\cup\Theta^2_\varepsilon(s',\hat{s},\theta,\hat{\theta})} dG_\varepsilon(x)\right)\bar{d}_\varepsilon(s,\hat{s},\theta,\hat{\theta})\\
&+\mathbb{1}_{s=\hat{s}}\lambda^e_\varepsilon g_\varepsilon(\theta)\left[\iint(1-\hat{\delta})\left(\mathbb{1}_{\theta\in\Theta^1_\varepsilon(s,\hat{\theta})}\bar{d}_\varepsilon(s,x,\hat{\theta},y)\right)dxdy\right]\\
&+\lambda^e_\varepsilon\left[g_\varepsilon(\hat{\theta})\iint(1-\hat{\delta})\left(\mathbb{1}_{\hat{\theta}\in\Theta^2_\varepsilon(s,x,\theta,y)}\bar{d}_\varepsilon(s,x,\theta,y)\right)dxdy\right]\Bigg\}
\end{aligned}
\tag{B.10}
$$

$$
\begin{aligned}
d'_\varepsilon(s,\hat{s},\theta,u) = (1-\delta)&\left(1-\lambda^e_\varepsilon\int_{x\in\Theta^1_\varepsilon(s,\theta)\cup\Theta^2_\varepsilon(s',\hat{s},\theta,u)} dG_\varepsilon(x)\right)\bar{d}_\varepsilon(s,\hat{s},\theta,u)\\
&+g_\varepsilon(\theta)\mathbb{1}_{s=\hat{s}}\mathbb{1}_{\theta\in\Theta^u_\varepsilon(s)}\left(\lambda^u_\varepsilon\bar{u}_\varepsilon(s)+\lambda^{ug}_\varepsilon\iiint\delta(1-\bar{\phi}^f_\varepsilon(s,x))\bar{d}_\varepsilon(s,\tilde{s},x,\hat{x})d\tilde{s}dxd\hat{x}\right)
\end{aligned}
\tag{B.11}
$$

$$
\begin{aligned}
d'_\varepsilon(s,\hat{s},\theta,r) = (1-\delta)&\left(1-\lambda^e_\varepsilon\int_{x\in\Theta^1_\varepsilon(s,\theta)\cup\Theta^2_\varepsilon(s',\hat{s},\theta,r)} dG_\varepsilon(x)\right)\bar{d}_\varepsilon(s,\hat{s},\theta,r)\\
&+\mathbb{1}_{s=\hat{s}}\int\mathbb{1}_{\theta'\in\Theta^f_\varepsilon(\theta)}\left(\phi^r_\varepsilon\bar{d}^f_\varepsilon(s,\theta')+\bar{\phi}^f_\varepsilon(s,\theta')\phi^{rg}_\varepsilon\iint\bar{d}_\varepsilon(s,\hat{s},\theta',\hat{x})d\hat{s}d\hat{x}\right)d\theta'
\end{aligned}
\tag{B.12}
$$

$$
\begin{aligned}
d'_\varepsilon(s,\hat{s},\theta,f) = (1-\delta)&\left(1-\lambda^e_\varepsilon\int_{x\in\Theta^1_\varepsilon(s,\theta)\cup\Theta^2_\varepsilon(s',\hat{s},\theta,f)} dG_\varepsilon(x)\right)\bar{d}_\varepsilon(s,\hat{s},\theta,f)\\
&+\mathbb{1}_{s=\hat{s}}\int\mathbb{1}_{\theta\in\Theta^r_\varepsilon(s,x)}(1-\phi^r_\varepsilon)\lambda^r\bar{d}^f_\varepsilon(s,x)dx
\end{aligned}
\tag{B.13}
$$

$$
\begin{aligned}
d^{f\prime}_\varepsilon(s,\theta) = (1-\phi^r_\varepsilon)&\left(1-\lambda^r\int_{x\in\Theta^r_\varepsilon(s,\theta)} dG_\varepsilon(x)\right)\mathbb{1}_{F_\varepsilon(s,\theta)>U_\varepsilon(s)}\bar{d}^f_\varepsilon(s,\theta)\\
&+\iint\delta\bar{\phi}^f_\varepsilon(s,\theta)(1-\phi^{rg}_\varepsilon)\bar{d}_\varepsilon(s,\hat{s},\theta,\hat{x})d\hat{s}d\hat{x}
\end{aligned}
\tag{B.14}
$$

$$
\begin{aligned}
u'_\varepsilon(s) = &\left(1-\lambda^u_\varepsilon\int_{x\in\Theta^u_\varepsilon(s)} dG_\varepsilon(x)\right)\bar{u}_\varepsilon(s)\\
&+\int\delta(1-\bar{\phi}^f_\varepsilon(s,\theta))\left(1-\lambda^{ug}_\varepsilon\int_{x\in\Theta^u_\varepsilon(s)} dG_\varepsilon(x)\right)\iint\bar{d}_\varepsilon(s,\hat{s},\theta,\hat{x})d\hat{x}d\hat{s}d\theta
\end{aligned}
\tag{B.15}
$$

[8]Note that when I integrate over y in equation (B.10), I include all possible values for $\hat{\theta}$, including u, r, and f, in this integration. The same holds for the integration over $\hat{x}$ in equations (B.11), (B.12), (B.14), and (B.15).

where

$$\Theta^f_\varepsilon(\theta) = \left\{[\delta', y'] \in [0,1] \times \mathbb{R}_+ : y = \max(y^{min}_\varepsilon, \hat{y}); \quad \hat{y} : p(s, \hat{y}) = p(s, y') - c^f; \quad \delta = \delta' + c^\delta\right\}$$

Alternatively, once could display the flows through a diagram, as is done in figure B.1, although it should be noted that this figure focuses primarily on the transition between the three main states, and abstracts from the evolution of human capital and the outside option.

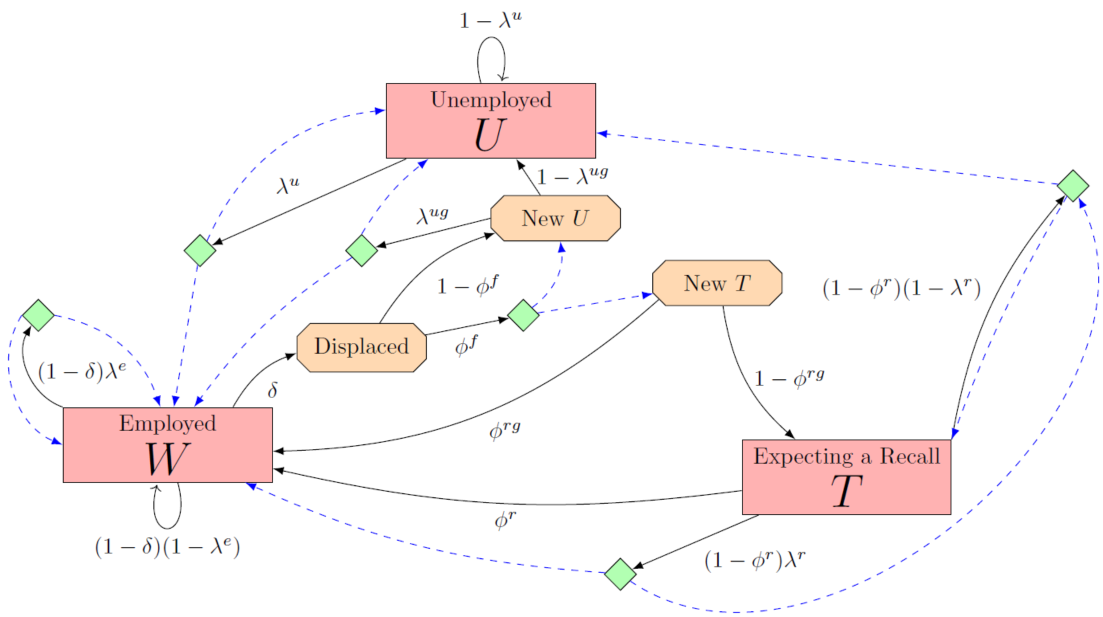


Figure B.1: *A flow chart depicting the flows between the three main states for a worker. Solid arrows depict an exogenous flow, caused by materialization of some probability. Dashed (blue) lines are flows that follow from a decision of the worker, and these decisions are made at decision points (which are denoted by green diamonds).*

B.2.3 Derivation of $W^{max}(s, \theta)$ and $U(s)$

Below, I derive the function $W^{max}(s, \theta)$, which is interpreted as the value the worker would derive from a match if they were to receive the entire surplus (i.e. $w(s, \hat{s}, \theta, \hat{\theta}) = p(s, \theta_y)$). In other words, I rewrite equation (2.10), ignoring the fixed worker types (since the model can be solved separately for each type ε), and setting $R(\hat{s}, \theta, \hat{\theta}) = 1$. First, note that one can rewrite the value of expecting a recall, equation (2.8) in terms of W^{max} and U only. In order to do so, I use the bargaining equations (2.2) and (2.5), leading to equation (2.9), mentioned in the main text (in section 2.2). Given a guess for W^{max}, one can solve the above equation (2.9) for the corresponding T, thus essentially eliminating the need for a separate value function. Furthermore, given that the values for T and U are then known (for a given value of W^{max} and U), I can also directly calculate the corresponding values for a newly nonemployed worker (either expecting a recall or not expecting a recall), $\hat{T}(s, \theta)$ and $\hat{U}(s)$.

Using these calculations (and leaving in $\hat{T}$ and $\hat{U}$), I can then start to rewrite equation (2.10), by plugging in $R(\hat{s}, \theta, \hat{\theta}) = 1$ and rewriting:

$$
\begin{aligned}
W^{max}(s,\theta) &= \ln(p(s,y)) + \beta \mathbb{E}_{s'|s,e} \Bigg\{ \delta \left[\phi^f \max\{\hat{T}(s',\theta), \hat{U}(s')\} + (1-\phi^f)\hat{U}(s') \right] \\
&\quad + (1-\delta) \Bigg[\lambda^e \Bigg(\int_{x \in \Theta^1(s',\theta)} W(s',s',x,\theta) dG(x) + \int_{x \in \Theta^2(s',\hat{s},\theta,\hat{\theta})} W(s',s',\theta,x) dG(x) \Bigg) \\
&\quad + \Bigg(1 - \lambda^e \int_{x \in \Theta^1(s',\theta) \cup \Theta^2(s',\hat{s},\theta,\hat{\theta})} dG(x) \Bigg) W(s',\hat{s},\theta,\hat{\theta}) \Bigg] \Bigg\} \\
&= \ln(p(s,y)) + \beta \mathbb{E}_{s'|s,e} \Bigg\{ \delta \left[\phi^f \max\{\hat{T}(s',\theta), \hat{U}(s')\} + (1-\phi^f)\hat{U}(s') \right] \\
&\quad + (1-\delta) \Bigg[\lambda^e \int_{x \in \Theta^1(s',\theta)} \left(W(s',s',x,\theta) - W(s',\hat{s},\theta,\hat{\theta}) \right) dG(x) \\
&\quad + \lambda^e \int_{x \in \Theta^2(s',\hat{s},\theta,\hat{\theta})} \left(W(s',s',\theta,x) - W(s',\hat{s},\theta,\hat{\theta}) \right) dG(x) + W(s',\hat{s},\theta,\hat{\theta}) \Bigg] \Bigg\}
\end{aligned}
$$

To simplify the equation above, use that if the worker gets all the surplus, $W(s', \hat{s}, \theta, \hat{\theta}) = W^{max}(s', \theta)$. Further, note that if the worker already is in the position of receiving all the surplus, there is no more room to re-bargain the piece-rate at the current employer. As such, the re-bargaining set $\Theta^2(s', \hat{s}, \theta, \hat{\theta})$ is an empty set and the corresponding integral cancels out:

$$
\begin{aligned}
W^{max}(s,\theta) &= \ln(p(s,y)) + \beta \mathbb{E}_{s'|s,e} \Bigg\{ \delta \left[\phi^f \max\{\hat{T}(s',\theta), \hat{U}(s')\} + (1-\phi^f)\hat{U}(s') \right] \\
&\quad + (1-\delta) \Bigg[\lambda^e \int_{x \in \Theta^1(s',\theta)} \left(W(s',s',x,\theta) - W^{max}(s',\theta) \right) dG(x) + W^{max}(s',\theta) \Bigg] \Bigg\}
\end{aligned}
$$

Finally, to arrive at equation (2.13), simplify the term inside of the integral by using the bargaining equation $W_\varepsilon(s, s, x, \theta) = W_\varepsilon^{max}(s, \theta) + \kappa \left(W_\varepsilon^{max}(s, x) - W_\varepsilon^{max}(s, \theta) \right)$:

$$
\begin{aligned}
W^{max}(s,\theta) &= \ln(p(s,y)) + \beta \mathbb{E}_{s'|s,e} \Bigg\{ \delta \left[\phi^f \max\{\hat{T}(s',\theta), \hat{U}(s')\} + (1-\phi^f)\hat{U}(s') \right] \\
&\quad + (1-\delta) \Bigg[\lambda^e \int_{x \in \Theta^1(s',\theta)} \left(W^{max}(s',\theta) + \kappa \left(W^{max}(s',x) - W^{max}(s',\theta) \right) - W^{max}(s',\theta) \right) dG(x) \\
&\quad + W^{max}(s',\theta) \Bigg] \Bigg\} \\
&= \ln(p(s,y)) + \beta \mathbb{E}_{s'|s,e} \Bigg\{ \delta \left[\phi^f \max\{\hat{T}(s',\theta), \hat{U}(s')\} + (1-\phi^f)\hat{U}(s') \right] \\
&\quad + (1-\delta) \Bigg[\lambda^e \int_{x \in \Theta^1(s',\theta)} \kappa \left(W^{max}(s',x) - W^{max}(s',\theta) \right) dG(x) + W^{max}(s',\theta) \Bigg] \Bigg\}
\end{aligned} \tag{B.16}
$$

In order to solve for both W^{max} and U, I still need to remove the value function W from the value function U, equation (2.6). To do this, I use the bargaining equation (2.2):

$$U(s) = \ln(b(s)) + \beta\mathbb{E}_{s'|s,u}\left\{\lambda^u \int_{x\in\Theta^u(s')} W(s',s',x,u)dG(x) + \left(1-\lambda^u \int_{x\in\Theta^u(s')} dG(x)\right)U(s')\right\}$$

$$U(s) = \ln(b(s)) + \beta\mathbb{E}_{s'|s,u}\left\{\lambda^u \int_{x\in\Theta^u(s')} \left(W(s',s',x,u) - U(s')\right)dG(x) + U(s')\right\}$$

$$U(s) = \ln(b(s)) + \beta\mathbb{E}_{s'|s,u}\left\{\lambda^u \int_{x\in\Theta^u(s')} \kappa\left(W^{max}(s',x) - U(s')\right)dG(x) + U(s')\right\} \tag{B.17}$$

B.3 Additional Simulation Results

B.3.1 Further Decomposition Results

Section under Construction

In the main text, in section 2.4.1, I used the calibrated model to decompose the difference in the scarring effects of displacement (on earnings, employment, and wages) between recalled and non-recalled workers into the channels through which the two groups are potentially different according to the model. In tables B.1 (earnings), B.2 (employment), and B.3 (wages) I show the numerical values used to construct a selection of the bars in the corresponding figures 2.6 and 2.7.

Channel	$k=0$	$k=1$	$k=3$	$k=5$	$k=10$
Match Productivity Penalty	0.108	0.195	0.071	0.023	0.046
Match Stability Penalty	0.019	−0.022	−0.16	−0.169	−0.23
Bargaining	−0.001	−0.009	−0.019	−0.024	−0.031
Human Capital	−0.001	0.014	0.056	0.077	0.091
Selection Search	0.021	0.104	0.122	0.083	0.058
Transition Rates	−0.063	−0.262	−0.178	−0.11	−0.067
Selection Choice	0.013	0.04	−0.001	−0.012	−0.022
Selection Education	0.001	−0.079	−0.033	−0.011	−0.012
Match Preservation	−0.149	−0.157	0.015	0.023	0.031
Total	−0.053	−0.175	−0.127	−0.121	−0.136

Table B.1: *Summary of the decomposition of the difference in the scarring effect of displacement on earnings between (ex-post) recalled and non-recalled workers. The total difference is calculated as the difference between the solid red and blue lines in the left panel of figure 2.4. The decomposition is generated by turning off the indicated channels one by one (in the order in which they are presented in the table), thus generating counterfactuals. The numbers reflect the contribution of each channel to the difference in the scarring effect of displacement on earnings, k years after displacement, and reflect the corresponding decomposition depicted in figure 2.6.* Pending Updated Results.

In these tables, I separate the differential effects on earnings, employment, and wages (denoted “Total”) into the 9 channels that could potentially drive these differences in the model. For clarification purposes, I will discuss here again how these channels are incorporated into the model that was presented in section 2.2 of the main text.

The first two channels listed in the tables correspond directly to the two penalty parameters c^f (“Match Productivity Penalty”) and c^δ (Match Stability Penalty). In other words, the contribution of these channels is calculated by setting these parameters to zero instead of their calibrated value. Next, the “Bargaining” channel corresponds to the difference between bargaining power κ and κ^r. This contribution is therefore calculated by setting κ^r to the same value as the calibrated value for κ. Similarly, the “Human Capital” channel corresponds directly to the difference between human capital depreciation rates ψ_u and $\psi_r\psi_u$, and is therefore calculated by setting $\psi_r = 1$.

Channel	$k=0$	$k=1$	$k=3$	$k=5$	$k=10$
Match Productivity Penalty	0.148	0.265	0.053	−0.011	0.013
Match Stability Penalty	−0.016	−0.067	−0.058	−0.029	−0.053
Bargaining	0.0	0.0	0.0	0.0	0.0
Human Capital	−0.001	−0.001	−0.002	−0.001	−0.0001
Selection Search	0.042	0.151	0.131	0.053	−0.002
Transition Rates	−0.065	−0.345	−0.206	−0.052	0.014
Selection Choice	0.011	0.06	0.017	0.004	0.002
Selection Education	−0.008	−0.096	−0.029	−0.001	0.0
Match Preservation	−0.194	−0.198	0.026	0.022	0.014
Total	−0.083	−0.231	−0.068	−0.016	−0.013

Table B.2: *Summary of the decomposition of the difference in the scarring effect of displacement on employment fraction between (ex-post) recalled and non-recalled workers. The decomposition is generated by turning off the indicated channels one by one (in the order in which they are presented in the table), thus generating counterfactuals. The numbers reflect the contribution of each channel to the difference in the scarring effect of displacement on employment fraction, k years after displacement, and reflect the corresponding decomposition depicted in figure 2.8.* Pending Updated Results.

As mentioned in the main text, a worker expecting to be recalled generally transitions back into employment faster than other unemployed workers. In order to calculate the impact of these "Transition Rates", I set the transition rates from the two states equal by setting $\phi^r_\varepsilon = \lambda^u_\varepsilon$, and $\phi^{rg}_\varepsilon = \lambda^{ug}_\varepsilon$. In other words, the recall materialization rates are set equal to the job finding rates of the general unemployed worker.[9]

Finally, the model contains four explicit selection channels, which can be referred to as selection into displacement, selection into recall expectation, selection out of recall expectation, and selection by education. The last of these channels corresponds to "Selection Education" in the tables and figures, and is calculated by setting all education-specific parameters equal to their value for education level 1. The selection into recall expectation, "Selection Choice", refers to the worker being able to choose whether or not move into the state of expecting a recall upon being offered as such (which happens at rate ϕ^f_ε). In order to calculate the contribution of this channel, I remove this choice, thus forcing a worker into the recall expectation state with probability ϕ^f_ε. The selection out of recall expectation, "Selection Search", is incorporated into the model by allowing the worker expecting a recall to search for a new job, which arrives at a rate $\lambda^r\lambda^u_\varepsilon$. In order to calculate the contribution of this channel, I shut down this model element by setting $\lambda^r = 0$. Finally, the selection into displacement, "Match Preservation", refers to the fact that (in the model) displaced workers are coming from jobs with lower productivity and higher separation rates, due to the negative correlation between those two job characteristics. The pure contribution of this final channel is calculated as a residual. After all, if all other channels are shut down, the only difference between the two states that remains is that the workers expecting a recall move back to their previous job, whereas the workers not expecting a recall draw a new job from the distribution $G_\varepsilon(\theta)$ (which at this point of the decomposition no longer depends on ε).

[9] Note that I shut down this "Transition Rates" channel after shutting down the "Selection Search" channel, so at this point I already have $\lambda^r = 0$.

Channel	$k = 0$	$k = 1$	$k = 3$	$k = 5$	$k = 10$
Match Productivity Penalty	−0.058	−0.177	0.024	0.034	0.035
Match Stability Penalty	0.046	0.108	−0.114	−0.14	−0.18
Bargaining	−0.001	−0.015	−0.021	−0.024	−0.03
Human Capital	0.001	0.027	0.061	0.076	0.089
Selection Search	−0.031	−0.159	−0.009	0.034	0.058
Transition Rates	0.008	0.239	0.026	−0.061	−0.078
Selection Choice	0.002	−0.032	−0.017	0.016	0.024
Selection Education	0.011	0.031	−0.004	−0.01	−0.011
Match Preservation	0.055	0.052	−0.01	0.001	0.017
Total	0.033	0.073	−0.064	−0.106	−0.125

Table B.3: *Summary of the decomposition of the difference in the scarring effect of displacement on wages between (ex-post) recalled and non-recalled workers. The decomposition is generated by turning off the indicated channels one by one (in the order in which they are presented in the table), thus generating counterfactuals. The numbers reflect the contribution of each channel to the difference in the scarring effect of displacement on wages, k years after displacement, and reflect the corresponding decomposition depicted in figure 2.8.* Pending Updated Results.

B.3.2 A Shutdown Simulation

Section under Construction

In section 2.4.2, I showed the importance of taking into account the possibility of recall using

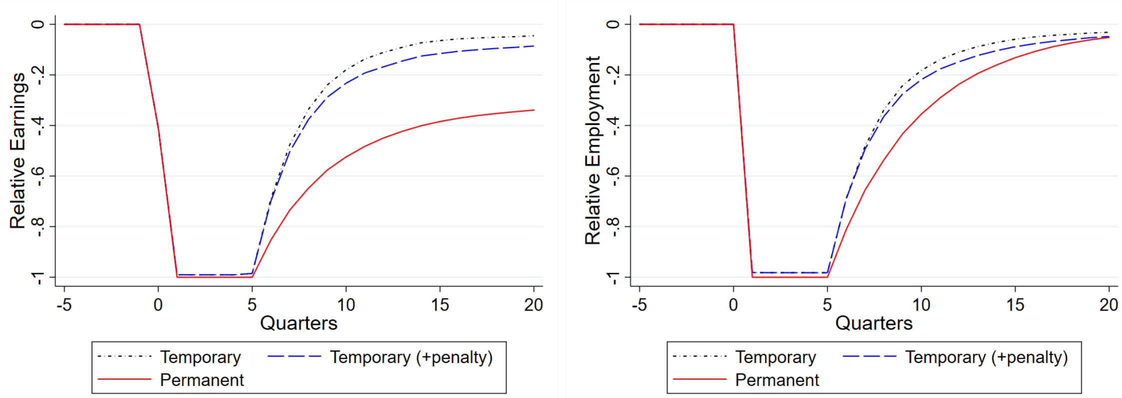


Figure B.2: *The effect of a temporary shutdown on the earnings and employment status of affected workers, without imposing two quarters of subsequent faster transitions. During the shutdown, workers are assumed to be either in the permanent unemployment state (red, solid) or in the temporary unemployment state with the associated penalties (blue, dashed) or without penalties (black, short-dashed).* Pending Updated Graphs.

a simulation of a temporary shutdown of 50% of the economy, taking place in quarter 15 of the simulation and lasting for 4 quarters. In the main text, I used the results from a simulation in which I assume that transition rates back into employment are higher than usual in the first two quarters after the shutdown ends. However, as can be seen in figure B.2, the results continue to hold if I assume that transition rates immediately go back to normal, although in this case the effects on employment are slightly more distinct between the temporary and permanently unemployed workers. Furthermore, I showed in the main text that the results continue to hold when focusing on workers with a high education level only. As can be seen in figure B.3, this remains true when focusing on the low education level only. In fact, the results for the low education level closely mirror those of the full simulation shown in figure 2.9.

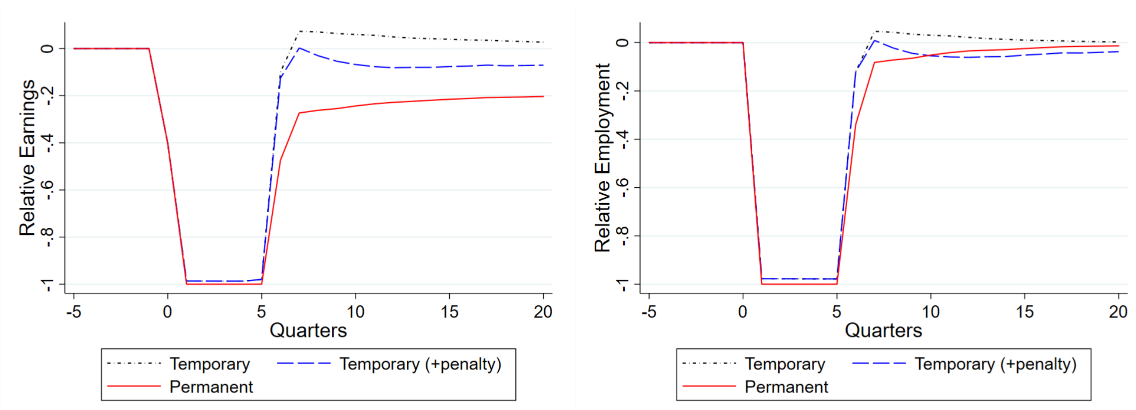


Figure B.3: *The effect of a temporary shutdown on the earnings and employment status of affected workers with a low education level. During the shutdown, workers are assumed to be either in the permanent unemployment state (red, solid) or in the temporary unemployment state with the associated penalties (blue, dashed) or without penalties (black, short-dashed).* Pending Updated Graphs.

In the main text, I also highlighted that the stark difference between temporary and permanent unemployment as a result of the shutdown does not necessarily hold for employment status. As can be seen in the left panel of figure B.4, the result similarly does not necessarily hold when focusing on the productivity of the worker. This productivity, calculated as the value of the production function (and thus taking into account both firm and worker productivity), follows a fairly similar pattern to that of the employment status (as seen in figure 2.9 in the main text).

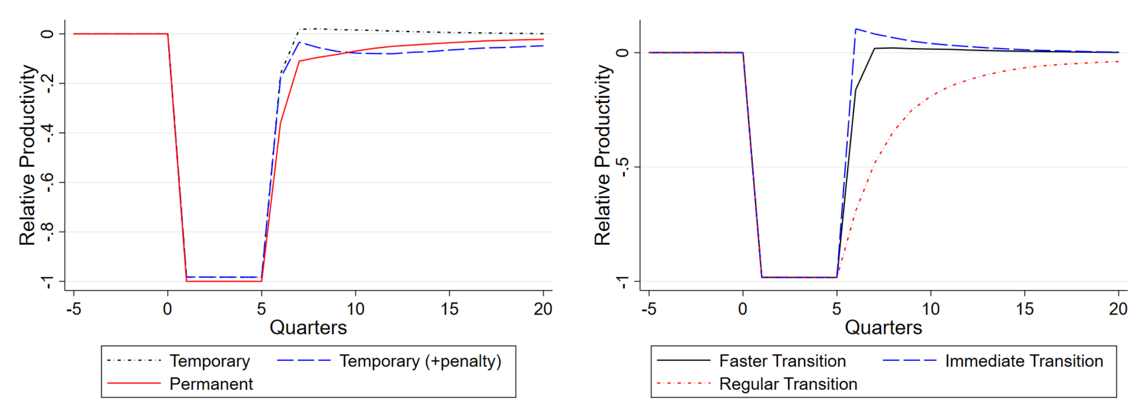


Figure B.4: *The effect of a temporary shutdown on the productivity of affected workers. Left panel: during the shutdown, workers are assumed to be either in the permanent unemployment state (red, solid) or in the temporary unemployment state with the associated penalties (blue, dashed) or without penalties (black, short-dashed), and after the shutdown workers transition back to employment at a faster rate than usual. Right panel: during the shutdown workers are assumed to be in the temporary unemployment state without the associated penalties, and after the shutdown workers transition back to employment either immediately (blue, dashed), at a faster rate than usual (black, solid), or at the usual rate (red, short-dashed).* Pending Updated Graphs.

In figure B.5, I show that the results discussed in the main text do not depend on the timing of the shutdown. Letting the shutdown take place in quarter 55 or 105 leads to slightly worse recovery paths for the affected worker, primarily due to the average worker being in a more stable match at that time and therefore the counterfactual simulation being less likely to include a separation for the affected worker. However, as can be seen in the figure, the difference is fairly small and therefore does not alter any of the aforementioned results.

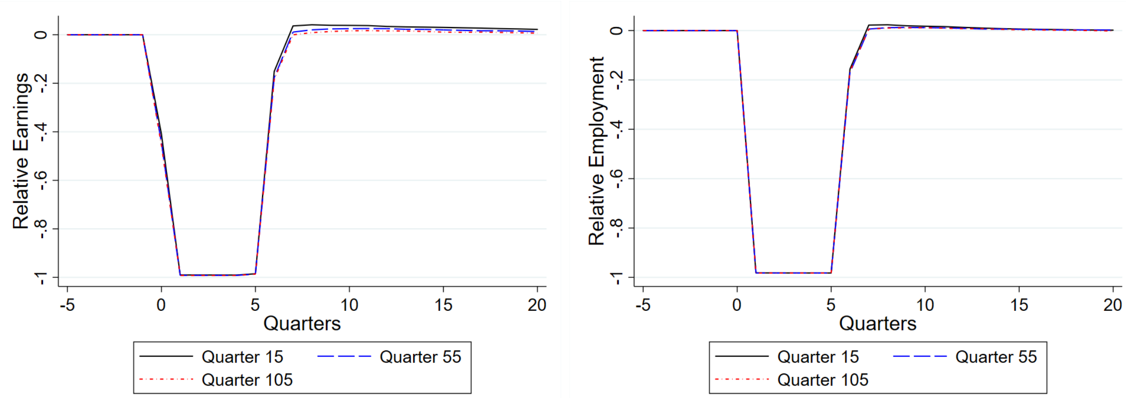


Figure B.5: *The effect of a temporary shutdown on the earnings and employment status of affected workers. During the shutdown, workers are assumed to be either in the temporary unemployment state without the associated penalties, and after the shutdown workers transition back to employment at a faster rate than usual. The shutdown starts in either period 15 (black, solid), period 55 (blue, dashed), or period 105 (red, short-dashed) of the simulation.* Pending Updated Graphs.

Finally, I show in figure B.6 that the results are similarly not majorly affected by the length of the shutdown (aside from the periods of the shutdown itself). As can be seen in the left panel of figure B.6, a longer shutdown slightly worsens the affected workers' earnings in subsequent periods. This is due to the human capital depreciation during the shutdown. However, as the depreciation probability in the temporary unemployment state is fairly small (as shown in section 2.3.3 of the main text), the difference is small and it does not affect the main results of the simulation exercise.

B.3.3 Policy Implications

Section under Construction

In section 2.4.3, I briefly discussed two counterfactual exercises that illustrate the policy relevance of the findings in this paper. In this subsection, I provide some of the underlying details (and illustrations).

First of all, figure B.7 and accompanying tables B.4 and B.5 illustrate my statement in section 2.4.3 that while human capital depreciation (relative to the continuously employed worker) accounts for a large portion of long-run earnings losses for the non-recalled worker, it only plays a minor role in explaining the long-run earnings losses for a recalled worker. This is clearly visible in the figure, where the human capital (relative) depreciation elements are represented by the blue, pink and dark

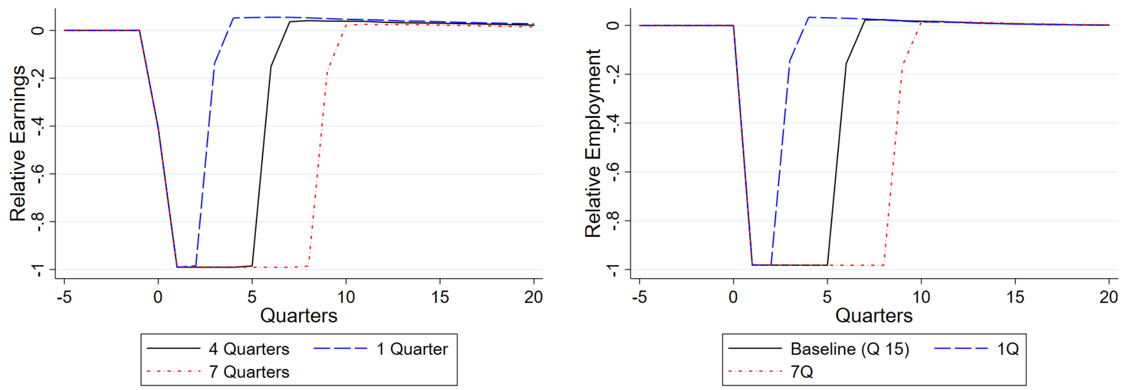


Figure B.6: *The effect of a temporary shutdown on the earnings and employment status of affected workers. During the shutdown, workers are assumed to be either in the temporary unemployment state without the associated penalties, and after the shutdown workers transition back to employment at a faster rate than usual. The shutdown lasts for either 1 period (blue, dashed), 4 periods (black, solid), or 7 periods (red, short-dashed) of the simulation.* Pending Updated Graphs.

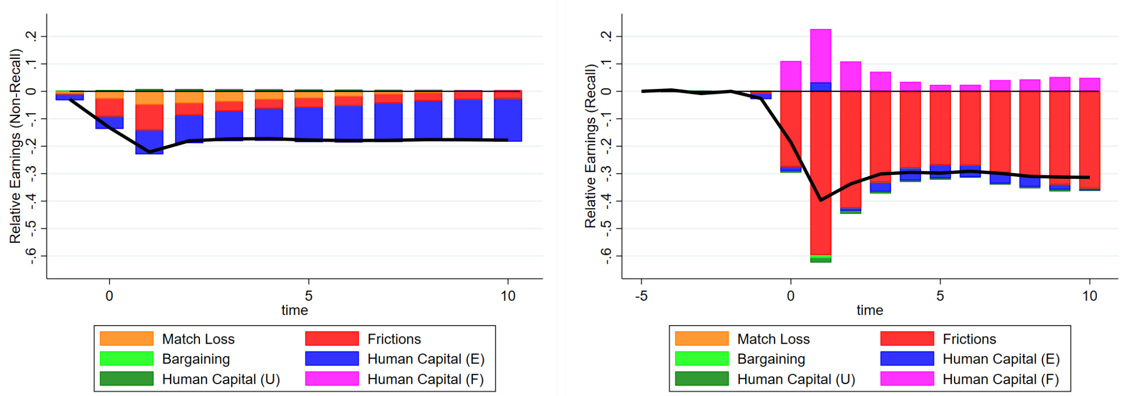


Figure B.7: *A decomposition of the scarring effect of displacement on earnings for (ex-post) non-recalled workers (left) and recalled workers (right).The black line represents the total earnings loss, and corresponds to the solid blue and red lines in the left panel of figure 2.4. The decomposition is generated by turning off the indicated channels one by one (starting with those depicted on the outside), thus generating counterfactuals. Corresponding numerical values for selected time periods (0,1,3,5, and 10 years after displacement) can be found in the appendix in tables B.4 and B.5.* Pending Updated Results.

green areas.

Channel	$k = 0$	$k = 1$	$k = 3$	$k = 5$	$k = 10$
Match Loss	0	0	0	0	0
Frictions	-0.275	-0.6	-0.332	-0.269	-0.354
Bargaining	0.0002	-0.011	-0.001	-0.0004	-0.001
Human Capital (E)	-0.015	0.032	-0.032	-0.048	-0.005
Human Capital (U)	-0.006	-0.016	-0.007	-0.005	-0.002
Human Capital (T)	0.11	0.195	0.071	0.024	0.049
Total	-0.186	-0.4	-0.301	-0.3	-0.314

Table B.4: *Summary of the decomposition of the scarring effect of displacement on earnings for (ex-post) recalled workers. The total difference corresponds to the solid red line in the left panel of figure 2.4. The decomposition is generated by turning off the indicated channels one by one (presented here in reversed order), thus generating counterfactuals. The numbers reflect the contribution of each channel to the difference in the scarring effect of displacement on earnings, k years after displacement, and reflect the corresponding decomposition depicted in figure B.7.* Pending Updated Results.

As can be observed in the figure, the human capital depreciation for workers expecting a recall (in pink) is generally has a positive impact (for recalled workers), due to the fact that this depreciation rate is so close to zero. Similarly, because the recalled worker does not lose their match, the match loss element is essentially zero for them. As a result, the bulk (more than 85%) of the earnings losses experienced by the recalled worker is explained by "Frictions", which encompasses losses that occur because the worker spends time in nonemployment (anything other than human capital loss).

Channel	$k = 0$	$k = 1$	$k = 3$	$k = 5$	$k = 10$
Match Loss	-0.027	-0.049	-0.038	-0.025	0.001
Frictions	-0.065	-0.093	-0.035	-0.035	-0.027
Bargaining	0	-0.0001	-0.0001	-0.0001	-0.0002
Human Capital (E)	-0.044	-0.087	-0.109	-0.125	-0.156
Human Capital (U)	0.005	0.008	0.007	0.005	0.002
Human Capital (T)	-0.001	-0.0004	0.001	0.002	0.002
Total	-0.133	-0.221	-0.174	-0.177	-0.178

Table B.5: *Summary of the decomposition of the scarring effect of displacement on earnings for (ex-post) non-recalled workers. The total difference corresponds to the solid blue line in the left panel of figure 2.4. The decomposition is generated by turning off the indicated channels one by one (presented here in reversed order), thus generating counterfactuals. The numbers reflect the contribution of each channel to the difference in the scarring effect of displacement on earnings, k years after displacement, and reflect the corresponding decomposition depicted in figure B.7.* Pending Updated Results.

Indeed, as can be seen in figure B.8, the decomposition of the average scarring effect of displacement looks fairly similar to the decomposition for non-recalled workers only, reflecting that the group of non-recalled workers is much larger than the group of recalled workers.

Since the decomposition of the average scarring effect of displacement (on earnings) points towards human capital depreciation as one of the major reasons for the large losses in the long run, it is natural to expect that a policy aimed at helping displaced workers may be targeted at bringing down the depreciation rate of human capital for nonemployed workers. In figure B.9, I consider the

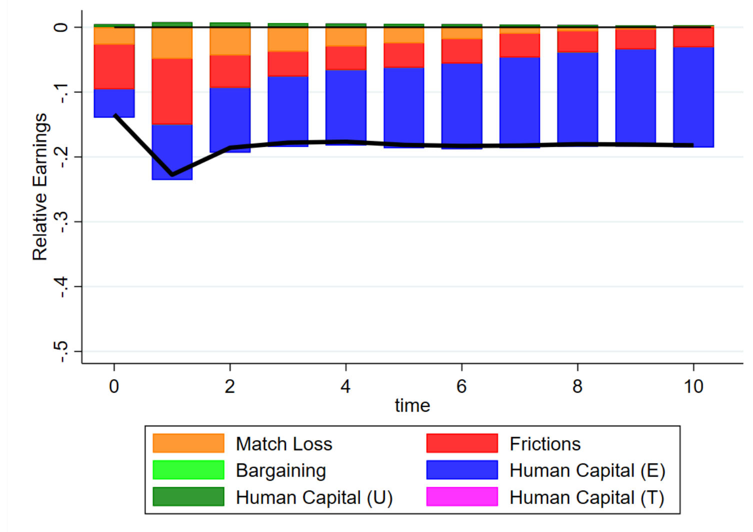


Figure B.8: *A decomposition of the average scarring effect of displacement on earnings.The black line represents the total earnings loss, and corresponds to the solid line in the left panel of figure 2.2. The decomposition is generated by turning off the indicated channels one by one (starting with those depicted on the outside), thus generating counterfactuals.* Pending Updated Results.

extreme case where human capital depreciation is zero (i.e. $\psi_u = 0$, and therefore also $\psi_r \psi_u = 0$). As can be seen in the figure, such a policy would hurt both the non-recalled and recalled worker, but while the impact on the non-recalled worker is fairly small, the recalled worker is hurt much more. This reflects the observations from B.7 that the earnings loss for non-recalled workers is primarily coming from the counterfactual employed worker accumulating human capital (and this channel was not shut down here), rather than the non-recalled workers themselves losing human capital, whereas for the recalled worker their limited human capital depreciation enters as a positive channel (which is now shut down).

Finally, the last paragraph of section 2.4.3 discussed how the difference between recalled and non-recalled workers would change if I were to calibrate the nonemployment benefit fraction b to the US replacement rate rather than the German replacement rate. As mentioned in the main text, such an exercise yields a value of $b = 0.52$ (rather than $b = 0.7$ as found in section 2.3.3 using the German replacement rate). In order to account for the fact that with a lower nonemployment benefit, the government will also have lower expenses, I additionally calibrate a proportional subsidy on the output, such that the total cost faced by the government stays the same. As mentioned in the main text, the resulting value of $-\tau = 0.0114$ is fairly mild.

In figure B.10, I compare the results of estimating the scarring effect of displacement (on earnings) by ex-post recall status for this alternative parametrization of the model (with and without the proportional subsidy) to those obtained in the baseline specification. As can be seen in the figure, the non-recalled workers face very similar earnings losses, but the recalled worker now suffers from much larger earnings losses. As indicated by the difference between the dashed and dotted lines, the addition of the proportional subsidy does not change this result. Therefore, I conclude that

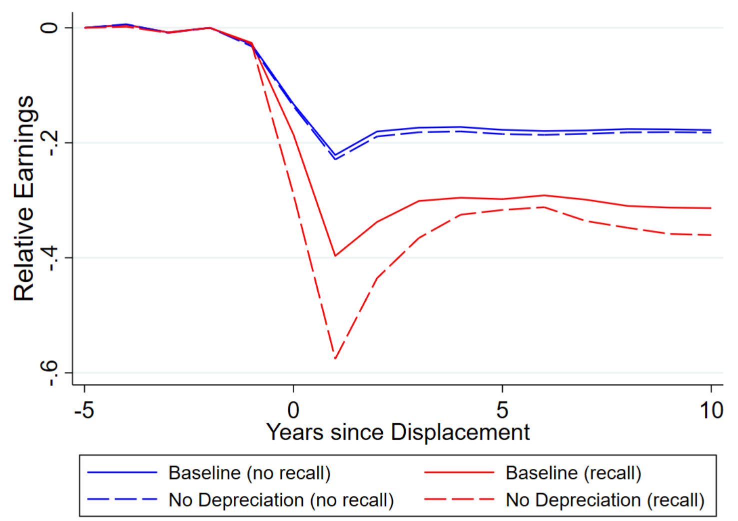


Figure B.9: *The effect of displacement on earnings relative to the control group, by ex-post recall status (materialization of recall within 5 years), using model simulation data from the baseline model (solid, corresponding to figure 2.4) and a counterfactual in which nonemployed workers do not lose human capital during nonemployment ($\psi_u = \psi_r \psi_u = 0$, dashed).* Pending Updated Graph.

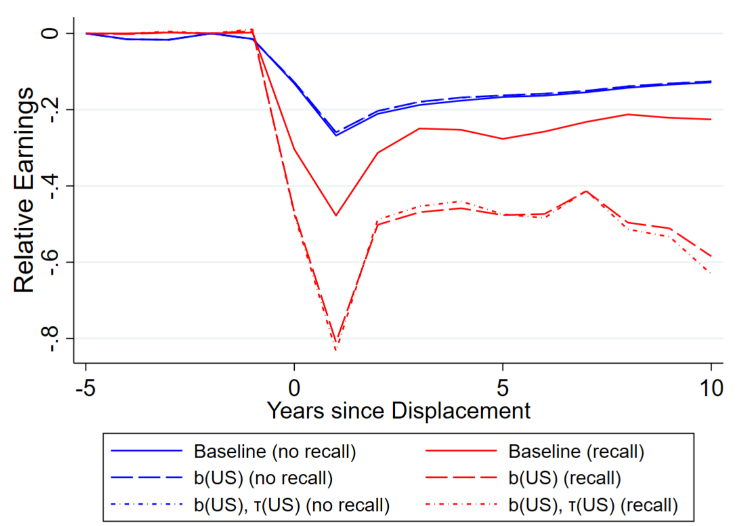


Figure B.10: *The effect of displacement on earnings relative to the control group, by ex-post recall status (materialization of recall within 5 years), using model simulation data from the baseline model (solid, corresponding to figure 2.4) and alternative parametrizations with a lower value of b (dashed) or a lower value of b and a proportional output subsidy (dotted), where the lower value of b is calibrated to match the US replacement rate.* Pending Updated Graph.

conditional on all other parameters being the same in both economies, I would expect the seemingly negative effect of recall to be much stronger in an economy with less generous nonemployment benefits.

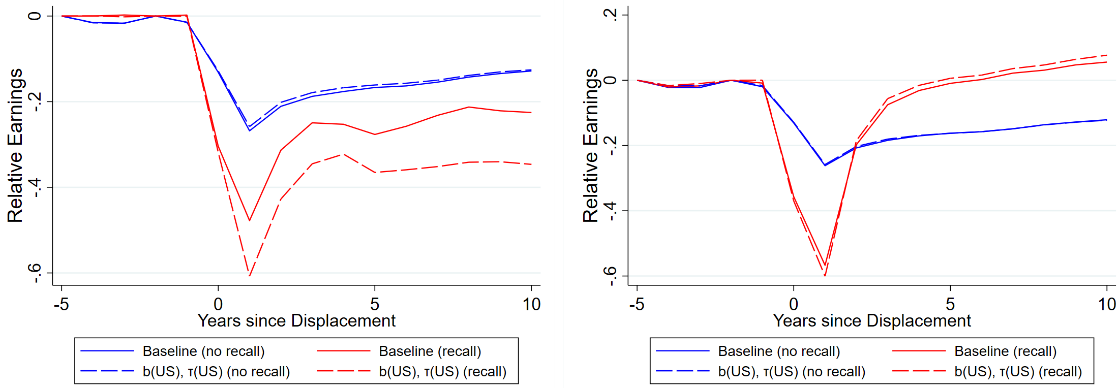


Figure B.11: *The effect of displacement on earnings relative to the control group, by ex-post recall status (materialization of recall within 5 years), using model simulation data from the baseline model (solid) and alternative parametrizations with a lower value of b and a proportional output subsidy (dotted), where the lower value of b is calibrated to match the US replacement rate. The figures are generated using model versions with either $\kappa^r = \kappa$ (left) or $c^\delta = 0$ (right).* Pending Updated Graphs.

Given that the nonemployment benefit plays a large role in determining the value of both states of nonemployment, one potential reason for this disparity in the effects of lowering b on recalled and non-recalled workers could be that the value of nonemployment is more important in determining the next wage for recalled workers (compared to non-recalled workers), because they re-bargain their wage using a bargaining power of $\kappa^r = 0.309$ rather than $\kappa = 0.883$. However, the left panel of figure B.11 shows that this does not fully explain the disparity. If I set the value of κ^r equal to the calibrated value of κ, the lower value of b still impacts the recalled worker much more than the non-recalled worker. Rather, the larger impact is driven by the fact that the recalled worker is much more likely to lose their job again. Therefore, the recalled worker is exposed to the lower value of b multiple times. If I disable the separation rate penalty c^δ (by setting it to 0), the large difference no longer arises, as shown in the right panel of figure B.11.

B.3.4 Alternative Calibrations

Section under Construction

In this section, I consider the robustness of the simulation results in section 2.4 to alternative calibrations of the model, using either different data to estimate the moments, or using an alternative version of the model in which I shut down the channel through which the worker is recalled in the same period as the initial displacement.

Given that the empirical analysis in chapter 1 uses data from the SIAB, calibrating the model in chapter 2 using this same SIAB data seems like a natural choice. However, it is worth considering how the main conclusions from chapter 2 would change if I calibrate the model using moments estimated from the LIAB data instead. In table B.6, I present the equivalent of table 2.3 for the calibration using moments from LIAB.

Comparing the estimated parameters in table B.6 to those in the main calibration (in table 2.3), a few parameters stand out as being quite different. The parameter that stands out the most is that of the post-recall penalty on the depreciation rate, c^{δ}, which is much larger when estimating the model using LIAB. Indeed, the LIAB calibration suggests increasing the separation rate by more than 20 percentage points after a recall materializes, which seems fairly extreme given that I observed in the data (in section 1.3.3.2) that recalled worker face an increase in their separation rate of up to 7 percentage points more than displaced workers.This gap in separation rates is larger when estimated using LIAB, as pointed out in appendix A.3.7, but only lasts for one period. Nevertheless, this larger gap in the first period after displacement may partially explain why the estimated value for c^{δ} is so large in LIAB.
Other differences between the LIAB calibration and the baseline calibration from chapter 2 worth pointing out include the probabilities of being offered a potential recall, ϕ^{f}_{ε}, which are lower in the LIAB than in the SIAB, as well as the human capital depreciation and appreciation rates, which are generally lower in LIAB. Finally, the LIAB allows for a slightly higher job-to-job transition rate than the SIAB, but the corresponding rates $\lambda^{e}_{\varepsilon}$ are still very low.

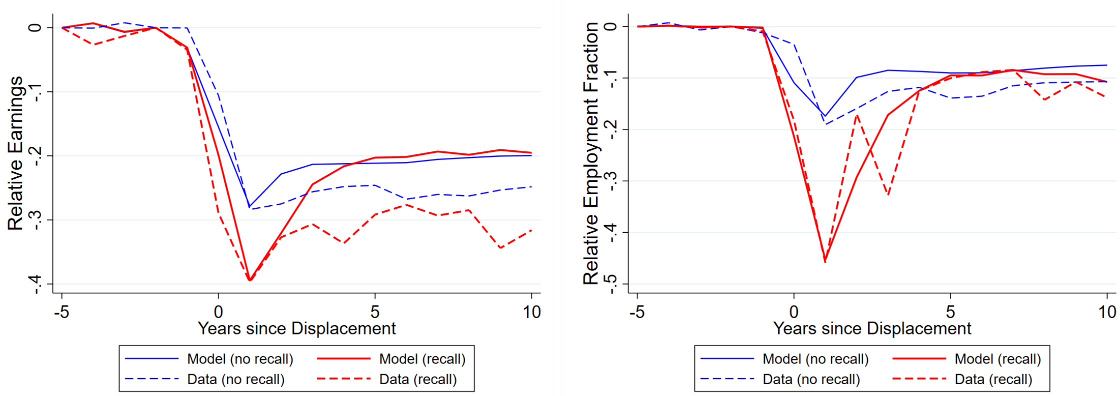


Figure B.12: *The effect of displacement on earnings (left) and employment (fraction of the year spent in an employment spell, right) relative to the control group, by ex-post recall status (materialization of recall within 5 years), using model simulation data from the LIAB-calibrated model (solid) and using the data (dashed, corresponding to figure A.54).*

In figure B.12, I show the estimated effect of displacement on earnings and employment fraction (defined as the fraction of the year spent in an employment spell) by ex-post recall status, compared to the results in figure A.54. As can be seen from figure B.12, the model matches the observation made in section A.3.7 that recalled workers do worse than non-recalled workers after displacement (in terms of their earnings) in the short run, but unlike the calibration in the main text, the calibration using LIAB data has trouble matching the differences in the long run, even though the effect on employment is fairly well matched in the short and long run.

In figure B.13, I fully decompose the differences in estimated post-displacement earnings between recalled and non-recalled workers (as shown in the left panel of figure B.12), just like I did for the

Description of Moment(s)	**Data**	**Model**	**Parameters**
Average rate of job loss, tenure 1-3.5y	0.0461	0.048	$\eta_\delta = 3.08$
Average rate of job loss, tenure 3.5-6y	0.0261	0.04	$\mu_{\delta,1} = 10.14$
Average rate of job loss, tenure 6-9y	0.0172	0.034	$\mu_{\delta,2} = 44.72$
Average rate of job loss, tenure>9y	0.0088	0.023	$c^\delta = 0.214$
Average rate of job loss, by education	0.0386	0.027	
	0.03	0.023	
Subsequent separation, displacement	0.1085	0.068	
Subsequent separation, recall	0.2042	0.125	
p75-p25 ratio of wages	1.5791	1.45	$\eta_y = 6.97$
	1.5719	1.74	$\mu_{y,1} = 1.61$
median-p25 ratio of wages	1.277	1.19	$\mu_{y,2} = 1.7$
	1.3112	1.25	$s_2 = 0.35$
Educational wage premium (all)	1.4295	1.41	
Educational wage premium (entry)	1.5535	1.55	
Job-to-job transition rate	0.0377	0.032	$\lambda_1^e = 0.005$
	0.0352	0.02	$\lambda_2^e = 0.05$
Job-to-job transition upon displacement	0.6348	0.64	$\lambda_1^{ug} = 0.69$
	0.7558	0.73	$\lambda_2^{ug} = 0.77$
Average job finding rate	0.2583	0.276	$\lambda_1^u = 0.3$
	0.2596	0.278	$\lambda_2^u = 0.3$
Average employment rate	0.8704	0.94	
	0.8887	0.99	
Replacement rate	0.6	0.7	$b = 0.88$
Yearly wage growth	0.0138	0.007	$\Delta_s(2) = 0.16$
	0.019	0.01	$\psi_e = 0.016$
Pre- to post-layoff wage, duration<0.5y	−0.0513	−0.035	$\psi_u = 0.126$
	0.0114	0.023	$\psi_r = 0.115$
Pre- to post-layoff wage, duration 0.5-1y	−0.1052	−0.078	$c^f = 0.29$
	−0.0595	−0.045	
Pre- to post-layoff wage, duration 1-2y	−0.1738	−0.117	
	−0.1481	−0.181	
Pre- to post-recall wage, duration 0.25-0.5y	−0.0234	−0.052	
	−0.0199	−0.051	
Pre- to post-recall wage, duration 0.5-1y	−0.0344	−0.061	
	−0.0456	−0.022	
Recall rate	0.0725	0.085	$\phi_1^f = 0.098$
	0.052	0.05	$\phi_2^f = 0.06$
Recall materialization rate	0.3049	0.265	$\phi_1^r = 0.151$
(Based on materialization in 2 years)	0.267	0.252	$\phi_2^r = 0.145$
Recall materialization rate	0.2927	0.302	$\phi_1^{rg} = 0.99$
(Based on materialization in 1 year)	0.2294	0.275	$\phi_2^{rg} = 0.99$
New job finding rate, workers expecting a recall	0.2927	0.235	$\lambda^r = 1.06$
Wage of newly hired worker	0.5878	0.605	$\kappa = 0.865$
Wage of newly recalled worker	0.6351	0.629	$\kappa^r = 0.766$
Coefficient $\hat{\gamma}$ in equation (2.14)	−0.0524	−0.05	$\rho = -22.24$

Table B.6: *A summary of calibration moments, their values in the data and in the calibrated model, and corresponding parameter values, from an alternative calibration that uses moments from the LIAB.*

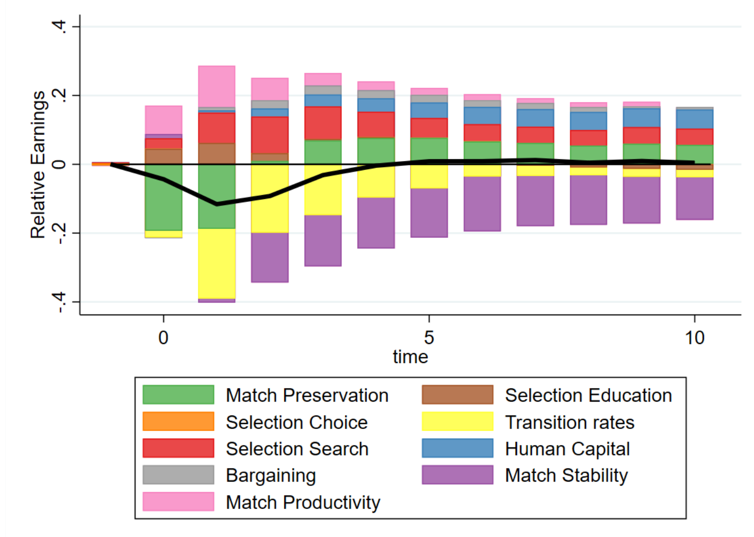


Figure B.13: *A decomposition of the difference in the scarring effect of displacement on earnings between (ex-post) recalled and non-recalled workers, using the LIAB-calibrated model. The black line represents the total difference, calculated as the difference between the solid red and blue lines in the left panel of figure B.12. The decomposition is generated by turning off the indicated channels one by one (starting with those depicted on the outside), thus generating counterfactuals.*

main calibration in figure 2.6. Comparing the decomposition in figure B.13 to the decomposition in figure 2.6 reveals a very similar picture. Indeed, it still holds that the main negative drivers for the recalled worker are the higher probability of subsequent separation and the higher transition rates back into employment. However, despite the value of c^{δ} being much higher in the LIAB-calibrated model, as noted above, the "Match Stability" channel has a smaller contribution. This, together with a slightly smaller contribution of the "Transition Rates" channel, seems to explain the difference between the two calibrations when it comes to the long-run earnings losses for recalled workers.

In figure B.14, I further decompose the wage and employment differences into the same 9 channels used for the earnings decomposition above. As can be seen by comparing the left panel to the left panel of figure 2.8, the inability of the LIAB-calibrated model to match the larger long-run earnings losses experienced by recalled workers is driven primarily by the smaller contributions of the "Match Stability" and "Transition Rates" channels. Because these two channels provide a smaller contribution in the long run, the wage difference does not turn negative in the LIAB-calibrated model as it did in the main text calibration. The other channels, on the other hand, provide contributions that are very similar to those observed in the main calibration. Similarly, the decomposition of the employment differences, shown in the right panel of figure B.14, is very similar to the one laid out in the main text (in the right panel of figure 2.8).

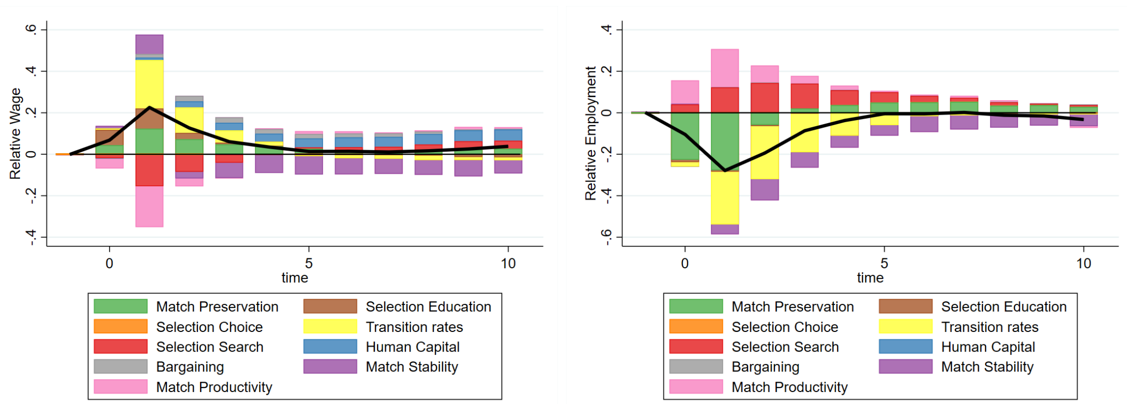


Figure B.14: *A decomposition of the difference in the scarring effect of displacement on wages (left) and employment (right) between (ex-post) recalled and non-recalled workers, using the LIAB-calibrated model. The black line represents the total difference, as depicted in figure 2.7. The decomposition is generated by turning off the indicated channels one by one (starting with those depicted on the outside), thus generating counterfactuals.*

Appendix C

Appendices to *Business cycle patterns of occupational mobility and subsequent earnings*

C.1 Data Appendix

C.1.1 Data Construction

In this section, I will provide some more details on the construction of the dataset I use to generate the figures and tables in Sections 3.2, C.1.2, and C.1.3, and to obtain the data counterparts of the moments used in Section 3.4 to estimate the model.

The dataset of the Survey of Income and Program Participation contains many more variables than the occupation and employment status variables that I use, and because of the size of the dataset the data is usually delivered separately by wave (thus meaning that each panel will consist of more than 10 separate datasets). The first step is thus to combine all these files and clean all the relevant variables. For this purpose, the Center for Economic and Policy Research (CEPR) has made a number of programs available, separating the cleaning process according to theme.[1] After running these programs, one can start using the data to create the dataset of interest. At this point I impose the sample restrictions, thus dropping any observations belonging to individuals who did not participate in the first wave (interview) of the panel, are aged below 23 or over 61, are self- or dual-employed, or work for the government.

In order to create the measure for occupational mobility, I compare the respondent's occupation in a certain month to their reported occupation 4 months ago (which is the same reference month one interview earlier). Of course, this previous occupation is not always available. For example, the occupation variable is not always filled when the respondent is unemployed. In those cases, I look further back up to a maximum of 8 months. As these occupation variables are the 3-digit occupations, it is easy to track the 1-digit occupation as well. To do so, I first assign all respondents to their 1-digit occupation group, after which I follow the same procedure as described above.

[1]These programs are available at http://ceprdata.org/sipp-uniform-data-extracts/sipp-recoding-programs/

Unfortunately, the 1996 and 2001 panels of the SIPP use a different occupation classification system than the 2004 and 2008 panels (SOC 1990 instead of SOC 2000), so that the procedure to create the measure for occupational mobility creates a discontinuity between the end of the 2001 panel and the start of the 2004 panel. In order to avoid creating such a discontinuity, I recode all occupations using the consistent panel of occupations from Dorn (2009). Table C.1 lists all the 1-digit and 2-digit occupational codes that the tables in the main text and in the next sections refer to.

	1-digit occupations	**2-digit occupations**
1	Management, Professional, Technical, Financial Sales, and Public Security Occupations	Executive, Administrative and Managerial Occupations
2	Administrative Support and Retail Sales Occupations	Management Related Occupations
3	Low-skill Services Support Occupations	Professional Specialty Occupations
4	Precision Production and Craft Occupations	Technicians and Related Support Occupations
5	Machine Operators, Assemblers, and Inspectors	Financial Sales and Related Occupations
6	Transportation, Construction, Mechanics, Mining, and Agricultural Occupations	Fire Fighting, Police, and Correctional Institutions
7		Retail Sales Occupations
8		Administrative Support Occupations
9		Housekeeping, Cleaning, Laundry
10		Supervisors of Guards; Guards
11		Food Preparation and Service Occupations
12		Health Service Occupations
13		Building and Grounds Cleaning and Maintenance Occupations
14		Personal Appearance Occupations
15		Recreation and Hospitality Occupations
16		Child Care Workers
17		Misc. Personal Care and Service Occupations
18		Precision Production Occupations
19		Machine Operators, Assemblers, and Inspectors
20		Transportation and Material Moving Occupations
21		Construction Trades
22		Mechanics and Repairers
23		Extractive Occupations
24		Farm Operators and Managers
25		Other Agricultural and Related Occupations

Table C.1: *1-digit (left) and 2-digit (right) occupation codes according to the system from Dorn (2009).*

Of course, the procedure above will also pick up the occupation changes in the data that were caused by measurement errors, as discussed in Section 3.1. Therefore, I check whether the respondent also changed either their employer, industry, working hours, or hourly wage. If either of these changes occur, I conclude that the respondent genuinely changed occupations and record it accordingly. If none of these changes occur, I conclude that the occupation change in the data may be caused by a measurement error. If that is the case, I set the occupational change variable to missing, essentially deleting this observation for the purpose of measuring the mobility rate.[2]

[2] In appendix C.1.2, I show that the overall occupational mobility as well as the fraction of occupational switchers

In order to identify whether respondents went through an unemployment spell, I use the SIPP's employment status recode variable, which can take 8 different values. I define respondents to be unemployed if they are reported to be (3) "*With a job all month, absent from work without pay 1+ weeks, absence due to layoff*", (5) "*With a job at least 1 but not all weeks, some weeks on layoff or looking for work*", (6) "*No job all month, on layoff or looking for work all weeks*", (7) "*No job all month, at least one but not all weeks on layoff or looking for work*", or (8) "*No job all month, no time on layoff and no time looking for work*".
As a result, I define respondents to be employed if they are reported to be (1) "*With a job entire month, worked all weeks*", (2) "*With a job all month, absent from work without pay 1+ weeks, absence not due to layoff*", or (4) "*With a job at least 1 but not all weeks, no time of layoff and no time looking for work*".

For the construction of the data counterparts of the moments in Section 3.4 of the main text, I also need to keep track of the length of an unemployment spell. In general, I can keep track of unemployment spells immediately once I have defined a respondent to be employed or unemployed. However, some respondents have missing information for a month or for one or multiple interviews. For these respondents, I assume that during these months their employment status remains the same. For example, if a respondent was unemployed when he last gave information, and five months later (after missing 4 months) he reports being employed, I assume this person was unemployed for all 4 months. In order to find previous reports of employment status, I look back up to a maximum of 13 months. Then, using the constructed variable of occupational changes, I identify the data counterparts of the moments using the same procedure as used on the simulated data from the model. This specific procedure is outlined for each moment separately in Appendix C.3. Using the measure created for the occupational mobility rate for employed workers, I then create the Figure 3.3 in the main text.

In the main text I explained that I use occupational changes at a 4 month rate rather than a 1 month rate because of so-called seam bias. This bias occurs because respondents will often report the same value 4 months in a row (those 4 months being the months the respondent is interviewed about in a single interview). One way to see how severe this seam bias might be is to look at the months in which respondents change their employment status. Figure C.1 reports the fraction of respondents who changed employment status compared to the previous month, by reference month.[3] As can be seen in the figure, the fraction of employment status changes is substantially higher in the first reference month. This observation indicates that there may be seam bias arising, and while the rotating panel design makes sure that each month is a first reference month for one group of respondents, the observation makes me conclude that in order to avoid biased estimates it is better to assess the data on a 4 month basis.

C.1.2 Further Results on 1-digit occupational mobility

In this section, I present some additional observations on 1-digit occupational mobility, made from the SIPP. These observations mainly serve to strengthen the points made in Section 3.2, although

going through unemployment exhibits similar but clearer patterns as those identified in the main text if I do not impose these checks for "genuine" switches.

[3]Recall that the reference months are the months the respondent is asked about in an interview. For example, if the interview asks about the months of May, June, July, and August, then the month of May would be the first reference month.

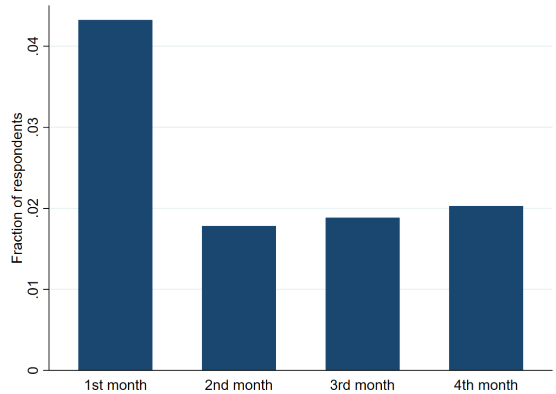


Figure C.1: *The fraction of respondents who change employment status compared to the previous month, by interview reference month.*

the observations in this section are not critical to the conclusions made there.

C.1.2.1 Mobility Rates

	1	2	3	4	5	6
1	0	16465	6222	1049	1830	5368
2	17540	0	7783	903	2108	5436
3	6921	8271	0	784	2016	5352
4	1249	860	868	0	1237	1673
5	1762	2311	2104	1383	0	4698
6	5496	5240	5151	1521	4436	0

Table C.2: *Number of switches found in the data for every combination of 1-digit occupations. Rows correspond to the previous occupations, and columns correspond to new occupations. For a list of the occupations corresponding to these codes, see Appendix C.1.1.*

In order to further investigate the net occupational mobility, Table C.2 lists the number of occupational changes observed in the data for every possible combination of 1-digit occupations.[4] At first sight, the table looks fairly symmetric: for every pair (A,B) of occupations, the number of workers switching from A to B is roughly similar to that who switch from B to A. This symmetry confirms the observation made in Section 3.2, where I observed that there does not seem to be a specific occupation that expels or attracts workers.

The analysis of Table 3.1 in the main text is repeated in Tables C.3 to C.10 for subsets of the data. In particular, Tables C.3 to C.6 look at the 1996, 2001, 2004, or 2008 panel only, Tables C.7 and C.8 count only U-switches, and Tables C.9 and C.10 count only E-switches (where Tables C.7 and C.9 are the equivalents of Table C.2 above for U- or E-switchers only) . Looking at these tables, it seems clear that the conclusions drawn from Tables C.2 and 3.1 regarding the direction of occupational changes continues to hold. There is only one example for which it does not seem to hold, namely occupational switches in and out of occupation 1 (*Management, Professional, Technical, Financial Sales, and Public Security Occupations*). For this occupation, the inflow seems much larger than the outflow when it comes to E-switches and the outflow is much larger than the inflow

[4]Note that Table 3.1 and C.2 do not use the sample weights. If the tables are tabulated using sample weights, the conclusions remain unchanged. These results are available upon request

for U-switches, which makes sense as one could imagine many within-firm promotions going into managerial positions.[5]

Occupation	1	2	3	4	5	6
Observations	333379	209511	108962	31914	85566	171214
Observations (wave 1)	9754	6231	3474	956	2615	5224
Inflow	10931	10918	6285	1921	4479	7843
Outflow	10112	11312	6955	1849	4763	7386
Net Inflow	819	-394	-670	72	-284	457

Table C.3: *Total number of incoming and outgoing switches found in the data for every 1-digit occupations, and number of times I observe a worker in each of these occupations in the data (in the first wave only and in the entire panel), for the 1996 panel only. For a list of the occupations corresponding to these codes, see Appendix C.1.1.*

Occupation	1	2	3	4	5	6
Observations	224018	121972	74030	17902	42079	106756
Observations (wave 1)	10386	5701	3543	886	2078	5059
Inflow	5886	5786	3806	874	1934	4301
Outflow	5593	5699	4052	953	2250	4040
Net Inflow	293	87	-246	-79	-316	261

Table C.4: *Total number of incoming and outgoing switches found in the data for every 1-digit occupations, and number of times I observe a worker in each of these occupations in the data (in the first wave only and in the entire panel), for the 2001 panel only. For a list of the occupations corresponding to these codes, see Appendix C.1.1.*

Occupation	1	2	3	4	5	6
Observations	290365	179748	106275	30468	52491	130816
Observations (wave 1)	11004	6954	4209	1167	2098	5176
Inflow	9202	9033	6402	1812	3024	5823
Outflow	8464	9507	6663	1869	3181	5612
Net Inflow	738	-474	-261	-57	-157	211

Table C.5: *Total number of incoming and outgoing switches found in the data for every 1-digit occupations, and number of times I observe a worker in each of these occupations in the data (in the first wave only and in the entire panel), for the 2004 panel only. For a list of the occupations corresponding to these codes, see Appendix C.1.1.*

In Figure C.2, I plot the 1-digit occupational mobility rate over time. Compared to Figure 3.1 in the main text, this figure also includes occupational switches that could not be verified as coinciding with a change in employer, industry, working hours, or hourly wage. This implies that some of the occupational switches included in Figure C.2 may be caused by a measurement error. Nevertheless, it can be observed that additionally including the non-verified switches leads the within-panel negative trend in the occupational mobility rate (visible in Figure 3.1, among others) to disappear, thereby revealing a seemingly countercyclical trend. Nevertheless, when plotting the rates against the corresponding unemployment rates reveals a negative relation, thus confirming the fact that the overall occupational mobility rate is procyclical, as concluded in the main text after detrending the occupational mobility rate based on verified switches only.

[5]It should be noted while that occupation code 1 includes many managerial occupations, many of the supervisory occupations are included in the occupation codes that are closest to the type of work (of their team).

Occupation	1	2	3	4	5	6
Observations	390268	224551	150353	31621	53345	156083
Observations (wave 1)	10698	6125	4243	931	1546	4920
Inflow	6949	7410	5635	1033	2190	4560
Outflow	6765	7252	5674	1216	2064	4806
Net Inflow	184	158	-39	-183	126	-246

Table C.6: *Total number of incoming and outgoing switches found in the data for every 1-digit occupations, and number of times I observe a worker in each of these occupations in the data (in the first wave only and in the entire panel), for the 2008 panel only. For a list of the occupations corresponding to these codes, see Appendix C.1.1.*

	1	2	3	4	5	6
1	0	5118	1918	237	561	1598
2	4236	0	3102	268	811	1969
3	1732	3100	0	217	887	2246
4	206	271	315	0	392	586
5	517	996	931	363	0	1914
6	1518	1922	2202	520	1952	0

Table C.7: *Number of switches found in the data for every combination of 1-digit occupations, for U-switchers only. Rows correspond to the previous occupations, and columns correspond to new occupations. For a list of the occupations corresponding to these codes, see Appendix C.1.1.*

Occupation	1	2	3	4	5	6
Observations	1238030	735782	439620	111905	233481	564869
Inflow	8209	11407	8468	1605	4603	8313
Outflow	9432	10386	8182	1770	4721	8114
Net Inflow	-1223	1021	286	-165	-118	199

Table C.8: *Total number of incoming and outgoing switches found in the data for every 1-digit occupations, and number of times I observe a worker in each of these occupations in the data, for U-switchers only. For a list of the occupations corresponding to these codes, see Appendix C.1.1.*

	1	2	3	4	5	6
1	0	11347	4304	812	1269	3770
2	13304	0	4681	635	1297	3467
3	5189	5171	0	567	1129	3106
4	1043	589	553	0	845	1087
5	1245	1315	1173	1020	0	2784
6	3978	3318	2949	1001	2484	0

Table C.9: *Number of switches found in the data for every combination of 1-digit occupations, for E-switchers only. Rows correspond to the previous occupations, and columns correspond to new occupations. For a list of the occupations corresponding to these codes, see Appendix C.1.1.*

Occupation	1	2	3	4	5	6
Observations	1238030	735782	439620	111905	233481	564869
Inflow	24759	21740	13660	4035	7024	14214
Outflow	21502	23384	15162	4117	7537	13730
Net Inflow	3257	-1644	-1502	-82	-513	484

Table C.10: *Total number of incoming and outgoing switches found in the data for every 1-digit occupations, and number of times I observe a worker in each of these occupations in the data, for E-switchers only. For a list of the occupations corresponding to these codes, see Appendix C.1.1.*

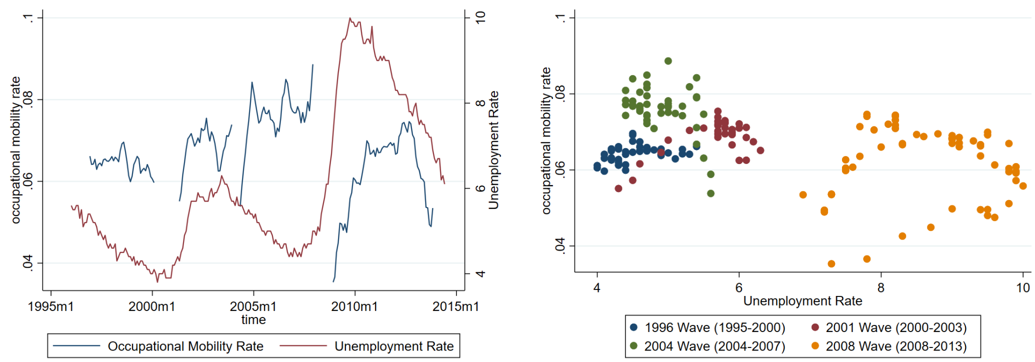


Figure C.2: *The 1-digit occupational mobility rate and the corresponding month's unemployment rate from the BLS, plotted over time (left) and against each other in a scatter plot (right), including both verified and unverified occupational switches.*

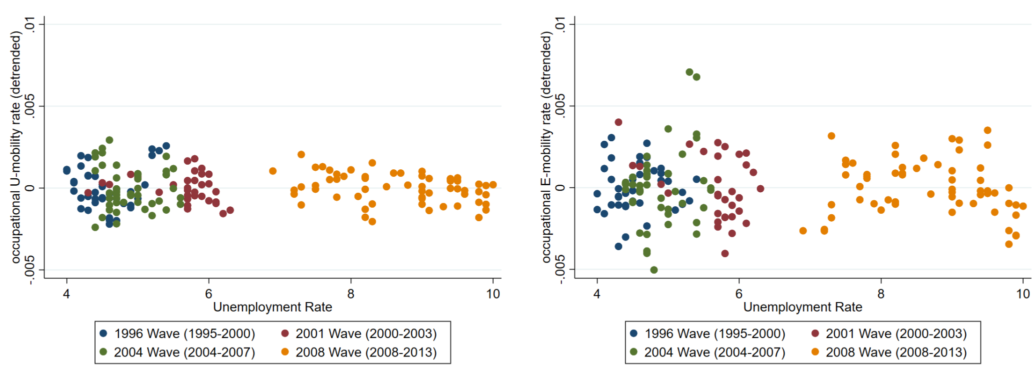


Figure C.3: *The detrended 1-digit occupational U-mobility rate (counting only U-switchers, left) or E-mobility rate (counting only E-switchers, right), plotted against the corresponding month's unemployment rate from the BLS.*

Figure C.3 plots the unemployment rate as well as the occupational mobility rate separately for workers who change occupations with and without going through an unemployment spell (U-switchers and E-switchers). As can be seen by comparing the two panels of Figure C.3, the occupational U-mobility rate, which counts only those going through unemployment, seems much less volatile than the occupational E-mobility rate. In terms of cyclicality, the two panels look fairly similar. Indeed, a simple (naive) OLS regression (of the mobility rate on the unemployment rate) gives a coefficient of -0.00003 for the U-switchers and -0.00002 for the E-switchers, thus suggesting both rates to be mildly procyclical. The fact that the fraction of occupational switchers going through unemployment is so strongly countercyclical, as shown in Figure 3.2 in the main text, therefore does not necessarily seem to be due to the occupational U-mobility rate itself being countercyclical. However, it should be noted here that it is likely that this result is partially influenced by the detrending of the mobility rates being too aggressive and taking out some of the variation that is driving the result.[6] Indeed, looking at Figure C.4, which is the non-detrended version of Figure C.3, both mobility rates still seem procyclical, but now it is clear that the procyclicality of the occupational U-mobility rate is much milder than that of the occupational E-mobility rate.

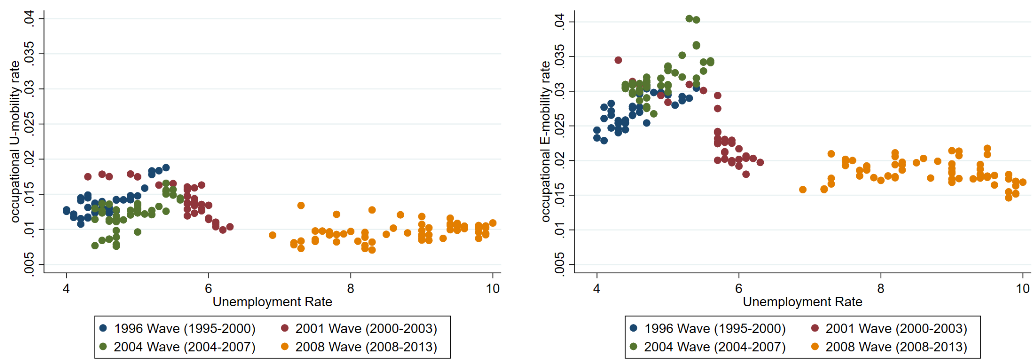


Figure C.4: *The (non-detrended) 1-digit occupational U-mobility rate (counting only U-switchers, left) or E-mobility rate (counting only E-switchers, right), plotted against the corresponding month's unemployment rate from the BLS.*

To show that the results discussed in Section 3.2 still hold on a state-level, Figure C.5 re-plots the right panel of Figures 3.1 and 3.2 for the state of New York only. As can be seen in the figures, the cyclicality of the total occupational mobility rate is still weak but procyclical (left), and the fraction of the switches that goes through unemployment still shows a countercyclical pattern, although the pattern becomes substantially noisier. A similar conclusion can be reached when considering only male respondents, as done in Figure C.6, although the pattern of the fraction of switches that goes through unemployment (right panel) shows a much stronger countercyclical pattern than the one specific to the state of New York (but still including female respondents).

In Figure C.7, I expand the sample by additionally including occupational switches that could not be verified as coinciding with a change in employer, industry, working hours, or hourly wage. The left panel, which corresponds to Figure C.2, confirms the procyclicality of the total occupational mobility

[6] This complication is likely to be the result of the fact that most of the SIPP panels under consideration in this paper cover only a single (upward or downward) movement in the general business cycle.

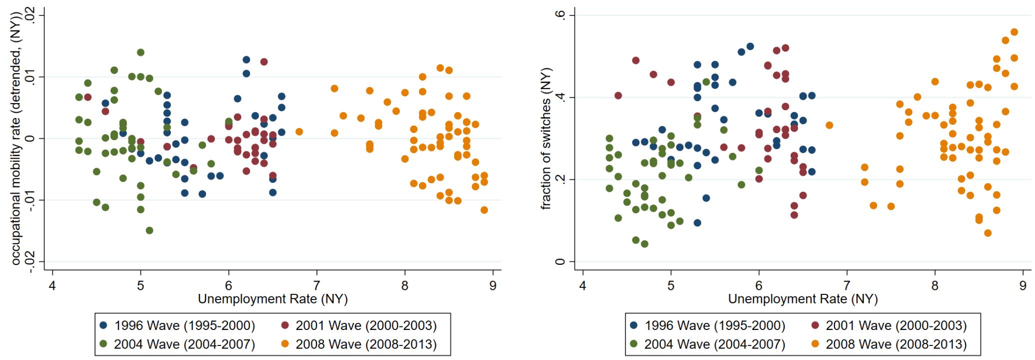


Figure C.5: *The total 1-digit occupational mobility rate (left) and the fraction of occupational switchers (1-digit) going through unemployment (right) for New York, plotted against the unemployment rate in a scatter plot.*

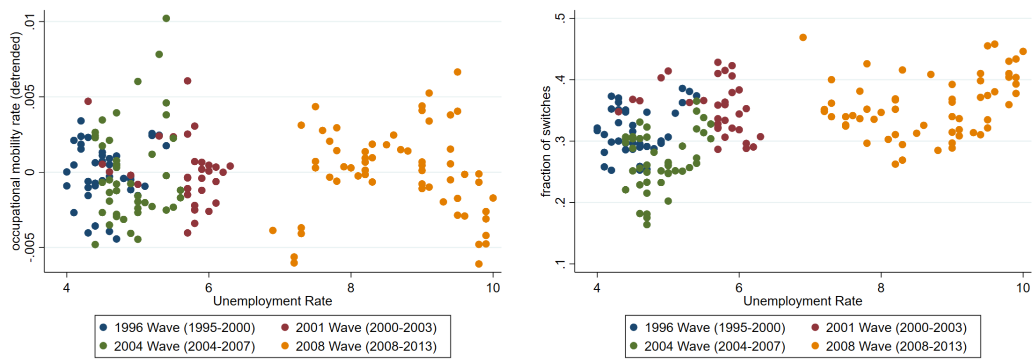


Figure C.6: *The total 1-digit occupational mobility rate (left) and the fraction of occupational switchers (1-digit) going through unemployment (right) for male respondents only, plotted against the unemployment rate in a scatter plot.*

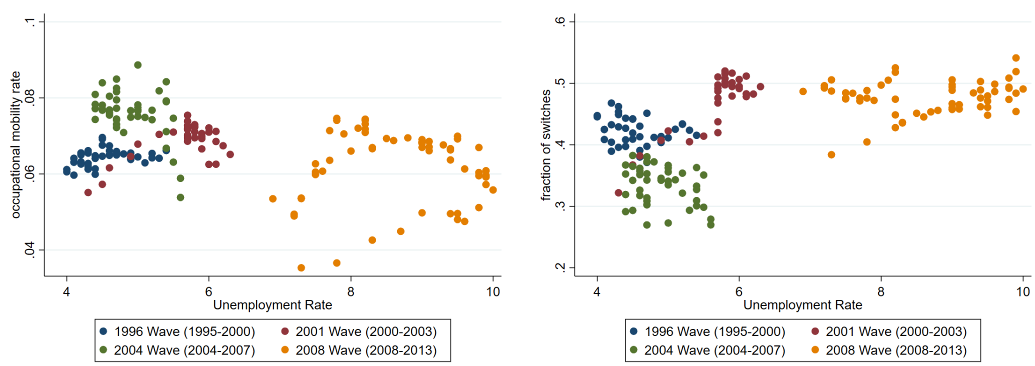


Figure C.7: *The total 1-digit occupational mobility rate (left) and the fraction of occupational switchers (1-digit) going through unemployment (right), plotted against the unemployment rate in a scatter plot, including both verified and unverified occupational switches.*

rate.[7] The right panel confirms the countercyclicality of the fraction of occupational switchers going through unemployment, although it should be noted that including the non-verified switches has slightly increased the level of this fraction across all panels.

Similarly, Figure C.8 plots occupational mobility rates (including non-verified switches) including only switches through employment, thus corresponding to Figure 3.3 in the main text. As can be seen in the figure, including the non-verified switches does not change my conclusion that roughly a quarter to a third of employed workers switching occupations do so without changing employers.

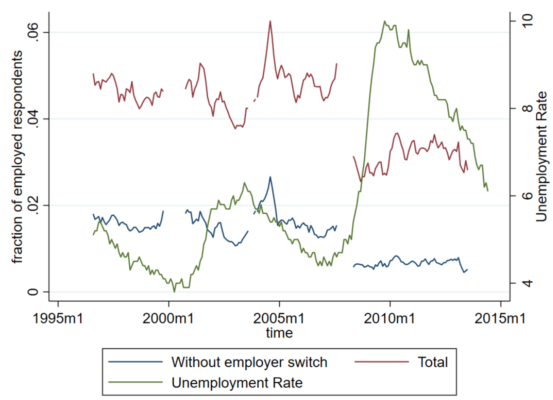


Figure C.8: *The fraction of employed workers switching occupations in the next 4 months, and the fraction of employed workers doing so without switching employer, including both verified and unverified occupational switches, plotted over time together with the corresponding month's unemployment rate from the BLS.*

C.1.2.2 Subsequent Earnings and Wages

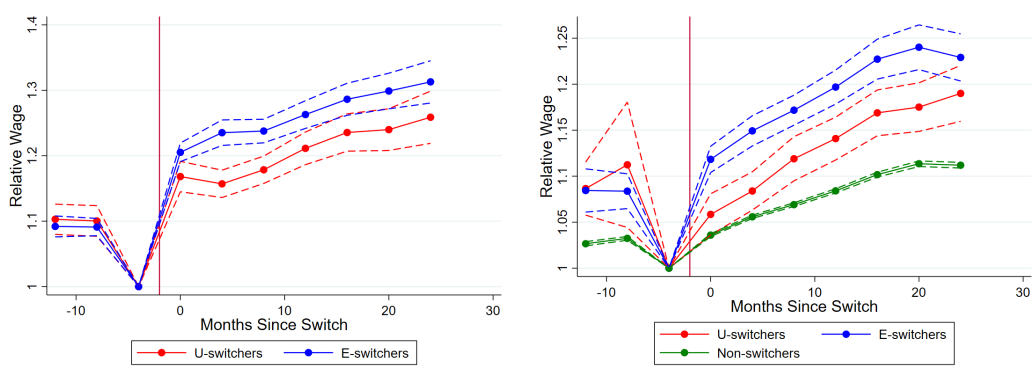


Figure C.9: *Real wage paths over time for occupational U-switchers and E-switchers, with (right) and without (left) removal of outlier wages. The switch takes place between time -4 and 0, as represented by the vertical line at -2. The right panel additionally includes the relative real wage path for non-switchers.*

[7] Note that in order to create the left panel of Figure C.7, I did not detrend the occupational mobility rates. I chose not to do this because the original reason for detrending in the main text (the within-panel negative trends) are no longer visible when including the non-verified switches.

In Figure C.9, I plot the average real wage for occupational U- and E-switchers, from 12 months before I observe the switch until 24 months after I observe the switch and relative to the last observed wage before the switch takes place (thus not taking into account the wage difference shown in Table C.11).[8] In the right panel (which corresponds to Figure 3.4 in the main text) I restrict the respondents to have observations in all periods in the time frame, whereas the left panel does not make that restriction. Furthermore, to put the observations from the main text in context, I added the corresponding wage paths for non-switchers to the figure in the right panel. As can be seen in the figure, the restriction to respondents with sufficient observations does not substantially alter the conclusions, although the real wage paths for both types of switchers are situated slightly higher when estimating without these restrictions. In the right panel, it is worth noticing that the real wage paths for non-switchers lie below those for either type of occupational switcher. This is likely to be a consequence of the baseline level of the real wage being much higher for non-switchers, as pointed out later in this section when discussing Table C.11.
In Figure C.10, I repeat the analysis from Figure 3.4 and the right panel of Figure C.9, additionally including switchers whose occupational switch could not be verified with a coinciding change in employer, industry, working hours, or hourly wage. As can be observed when comparing Figure C.10 with either Figure 3.4 or the right panel of Figure C.9, additionally including these unverified occupational switches in the sample does not change the conclusion.

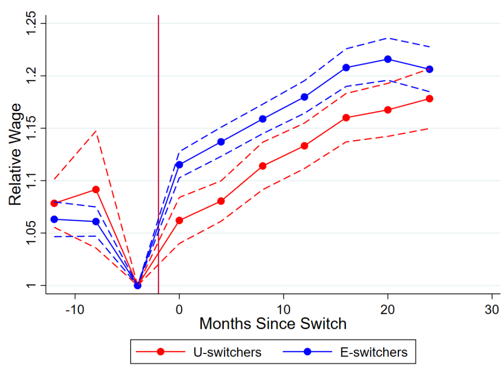


Figure C.10: *Real wage paths over time for occupational U-switchers and E-switchers, including both verified and unverified occupational switches. The switch takes place between time -4 and 0, as represented by the vertical line at -2.*

In Figures C.11 and C.12, I repeat the analysis of Figure 3.5 by plotting the real wage immediately after the switch (relative to the pre-switch real wage) against the unemployment rate at the time of the switch. Compared to the figure in the main text, Figure C.11 additionally includes switchers whose occupational switch could not be verified with a coinciding change in employer, industry, working hours, or hourly wage. Figure C.12, on the other hand, considers only male (verified) occupational switchers. Despite creating these graphs with different samples than the one used in the main text, it can be seen that the conclusion from the graph is unchanged: overall wage differentials for occupational switchers are mildly countercyclical (left panel), and this primarily reflects the countercyclicality of the relative real wages for E-switchers whereas the relative real

[8]As in the main text, the wage for U-switchers before the switch refers to the wage in their previous job.

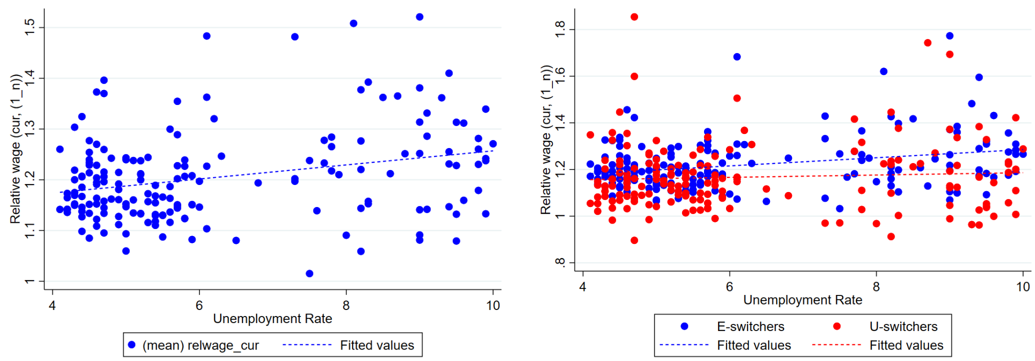


Figure C.11: *Real wages immediately after the occupational switch, relative to the real wage before the switch, for all occupational switchers (left) or for occupational U-switchers and E-switchers (right), plotted against the unemployment rate at the time of the switch, including both verified and unverified occupational switches. Dashed lines show fitted values corresponding to a simple OLS regression.*

wages for U-switchers appear to be acyclical (right panel).

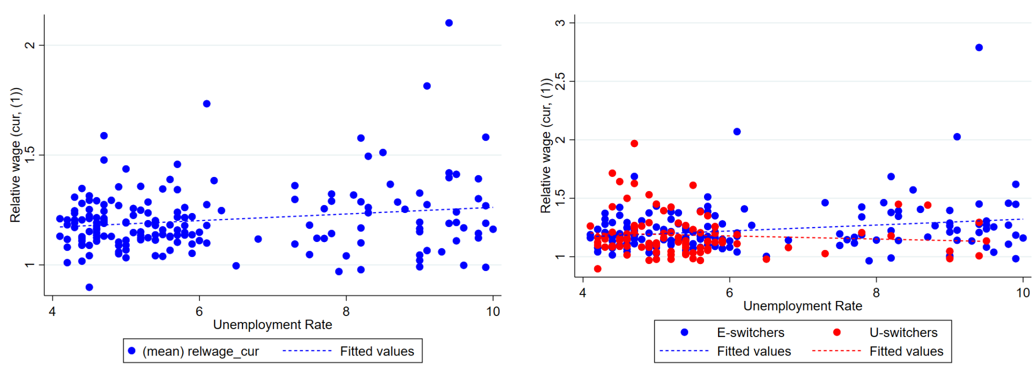


Figure C.12: *Real wages immediately after the occupational switch, relative to the real wage before the switch, for all occupational switchers (left) or for occupational U-switchers and E-switchers (right), plotted against the unemployment rate at the time of the switch and using male respondents only. Dashed lines show fitted values corresponding to a simple OLS regression.*

To stress the point made that workers who switch occupations while unemployed (U-switchers) and job-to-job occupational switchers (E-switchers) are different, Table C.11 provides some descriptive statistics on a number of parameters of interest, separately for the two groups of occupational switchers as well as for non-switchers. As can be seen in the table, non-switchers, U-switchers and E-switchers differ substantially in especially their age and education (which is measured on a 5-point scale). This difference points towards low-educated and younger workers mainly switching through unemployment and highly educated and older (but still younger than average) workers switching on the job. Furthermore, it is true for both groups that their wage tends to be lower than the average, and for U-switchers the wage both before and after the switch is lower than that of E-switchers. When considering how the composition of U- and E-switchers changes over the business cycle, it can be seen in Table C.12 that while the description of the average U- and E-switcher does not change much in terms of their age (slightly lower in recessions), education (slightly higher in recessions) or

	Non-Switchers	U-Switchers	E-Switchers	t
Age	41.079 (0.006)	36.338 (0.052)	37.579 (0.036)	19.402
Education	2.888 (0.001)	2.544 (0.005)	2.735 (0.003)	31.278
Gender (Female)	0.486 (0.000)	0.504 (0.003)	0.482 (0.002)	-7.030
Wage (Before)	-	12.637 (0.140)	13.499 (0.051)	7.153
Wage (After)	-	11.244 (0.059)	13.981 (0.051)	31.891
Wage	18.878 (0.012)	-	-	-
Observations	3133472	36704	81364	

Table C.11: *Descriptive statistics for Non-switchers, U-switchers, and E-switchers, with standard errors in parentheses. The t-statistic reported in the last column refers to a t-test testing for equality of means between U-switchers and E-switchers.*

	U-Switchers		E-Switchers	
	Boom	Recession	Boom	Recession
Age	36.508 (0.147)	36.313 (0.056)	38.007 (0.112)	37.530 (0.038)
Education	2.498 (0.014)	2.551 (0.005)	2.719 (0.011)	2.736 (0.004)
Gender (Female)	0.480 (0.007)	0.507 (0.003)	0.485 (0.005)	0.481 (0.002)
Wage (Before)	14.276 (0.461)	12.397 (0.145)	14.408 (0.160)	13.394 (0.054)
Wage (After)	12.650 (0.225)	11.038 (0.059)	15.008 (0.218)	13.863 (0.051)
Observations	4684	32020	8432	72932

Table C.12: *Descriptive statistics for U-switchers and E-switchers, by economic conditions at the time of (materialization of) the switch, with standard errors in parentheses.*

gender, both the pre-switch and post-switch wage are substantially lower in recessions. Furthermore, this decrease is generally more pronounced for U-switchers than for E-switchers, both in absolute and in relative terms, and it can be noted that the majority of switches of both types take place in recessions.

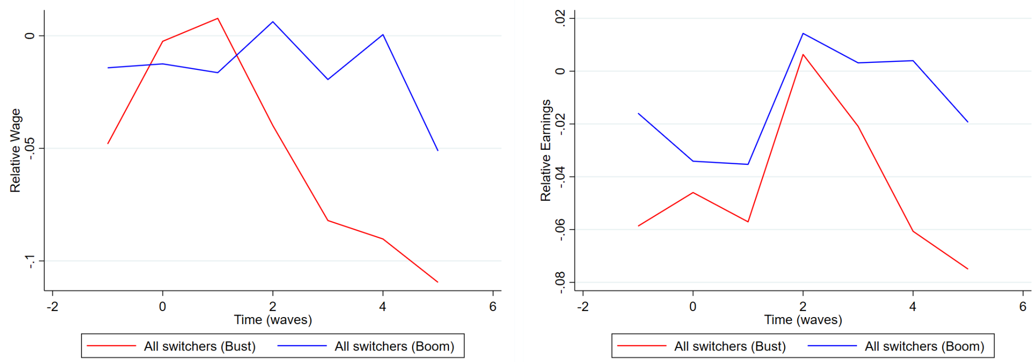


Figure C.13: *The effect of occupational switches on real wages (left) and real earnings (right), relative to the counterfactual of never switching, using estimated coefficients obtained using the three-step estimation method, and including both verified and unverified occupational switches. The plots show separate estimates of the effect for switches materializing during a boom or a bust.*

In Figures C.13 and C.14 I show the results of a regression-based estimation of the relative (real) earnings and wage paths after an occupational switch of either type. As these results are obtained using the three-step estimation method from Borusyak et al. (2021), these figures are similar to Figure 3.9 in the main text. Compared to the figure in the main text, however, Figure C.14 shrinks the sample by considering only male workers, whereas Figure C.13 expands the sample by also considering switchers whose switch could not be verified with a coinciding change in employer, industry, working hours, or hourly wage. As can be seen by comparing Figures C.13 to Figure 3.9, expanding the sample with unverified switches negatively affects the post-switch wage and earnings paths, especially for switches that materialized during a bust. Thereby, the procyclicality of the wage losses strengthened, whereas the earnings path is now also observed to be mildly procyclical. Similarly, restricting the sample to male workers only, as in Figure C.14 lead the results to weaken to such an extent that neither the real wage path nor the real earnings path is clearly procyclical anymore.

In Figures C.15 to C.18 I repeat the estimation from Figure 3.10 in the main text, thus estimating (using the three-step estimation method) how the difference in the post-switch real wage and real earnings paths changes over the business cycle. Each of the Figures C.15 to C.18 imposes a change in the sample on which the estimation is performed. In particular, in Figure C.15 I only consider occupational changes that materialize in the first (reference) month of the wave. Given the prevalence of the seam bias, as discussed in Appendix C.1.1, which is stronger for occupational changes than it is for employment status changes, this does not change the sample substantially. In Figure C.16, I expand the sample by additionally including unverified occupational switches, whereas in Figure C.17 I further restrict the sample by only considering (verified) switched made by male respondents. Finally, Figure C.18 considers only occupational switches that coincide with an employer change. As can be seen by comparing each of Figures C.15 to C.18 to the corresponding figure in the main text

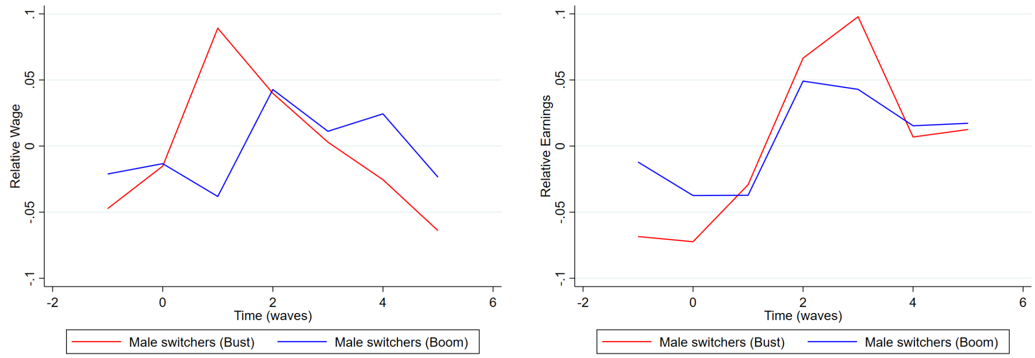


Figure C.14: *The effect of occupational switches on real wages (left) and real earnings (right), relative to the counterfactual of never switching, using estimated coefficients obtained using the three-step estimation method, and including male switchers only. The plots show separate estimates of the effect for switches materializing during a boom or a bust.*

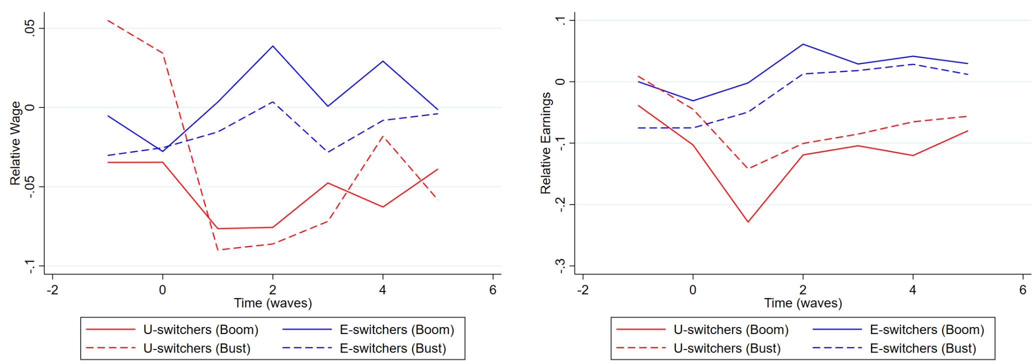


Figure C.15: *The effect of occupational switches on real wages (left) and real earnings (right), relative to the counterfactual of never switching, by type of switch (including only occupational switches materializing in the first month of the wave), using estimated coefficients obtained using the three-step estimation method. The plots show separate estimates of the effect for switches materializing during a boom or a bust.*

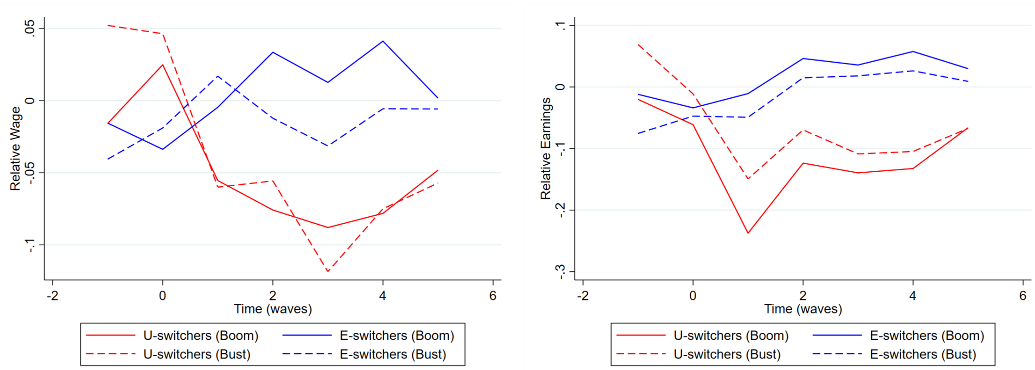


Figure C.16: *The effect of occupational switches on real wages (left) and real earnings (right), relative to the counterfactual of never switching, by type of switch (including both verified and unverified occupational switches), using estimated coefficients obtained using the three-step estimation method. The plots show separate estimates of the effect for switches materializing during a boom or a bust.*

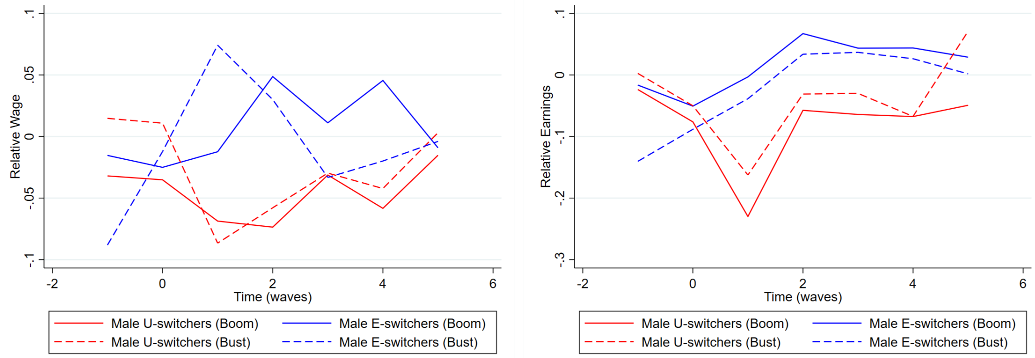


Figure C.17: *The effect of occupational switches on real wages (left) and real earnings (right), relative to the counterfactual of never switching, by type of switch, using estimated coefficients obtained using the three-step estimation method, and including male switchers only. The plots show separate estimates of the effect for switches materializing during a boom or a bust.*

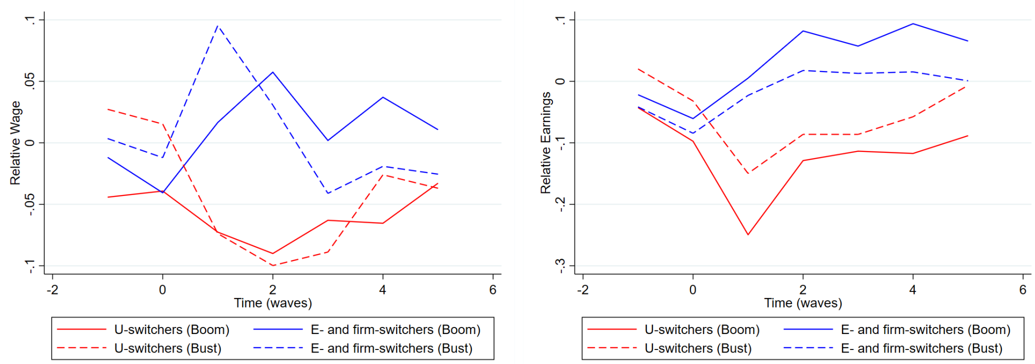


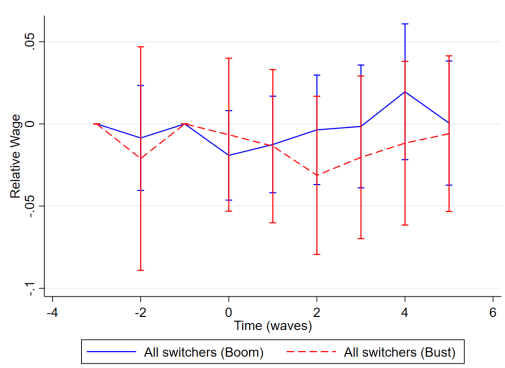
Figure C.18: *The effect of occupational switches on real wages (left) and real earnings (right), relative to the counterfactual of never switching, by type of switch (using only switches that coincide with a change in employer), using estimated coefficients obtained using the three-step estimation method. The plots show separate estimates of the effect for switches materializing during a boom or a bust.*

(Figure 3.10), none of these changes in the sample affect the conclusion. In each of these figures, the real wage and especially the real earnings path is procyclical for E-switchers, whereas the real earnings path for U-switchers is countercyclical (and the real wage path is fairly inconclusive).

Finally, I mentioned in the section 3.2 that the three-step method from Borusyak et al. (2021) is not the only method proposed to take into account potential contamination of the event study estimates by effects from earlier and later periods or subsequent and prior treatments. Below, I use the interaction-weighted estimator from Sun and Abraham (2020) instead. In practice, this means that I am estimating the following equation:

$$w_{it} = \alpha_i + \gamma_t + \sum_{C \neq 0} \sum_{\substack{k=-2 \\ k \neq -1}}^{K} \delta_k^C D_{it}^{C,k} + u_{it} \tag{C.1}$$

Equation (C.1) follows the same notation as equation (3.1) in the main text. As such, α_i and γ_t represent the person- and time fixed effects, and u_{it} is an error term. Similarly, the dependent variable, w_{it}, corresponds to individual i's wage in period t, like before. The main difference with equation 3.1 is that rather than estimating the equation for each base period separately, the above specification is only estimated once. However, the specification still allows for a different treatment effect (and different dynamics of this treatment effect) depending on which treatment cohort C the individual belongs to, with $C = 0$ corresponding to the cohort of individuals who I do not observe switching occupations at all. This "never-treated" group acts as the control group. Furthermore, note that rather than omitting one value of k, I follow the discussion in Borusyak et al. (2021) by omitting two values of k. This is because generally the set of relative time indicators D_{it}^C is collinear with itself as well as with the time fixed effect. The first period I omit is $k = -1$, and the second omitted period is the earliest period, $k = -3$ (as reflected by the summation over k starting at $k = -2$). These periods are chosen to maximize the distance between the two omitted periods, thereby making the resulting estimate less sensitive to any possible fluctuations (or trend) between these two periods. Finally, note that specification (C.1) no longer allows for the inclusion of control variables $\bar{e}_{it}$ (recent earnings) and X_{it} (the quadratic polynomial in age).



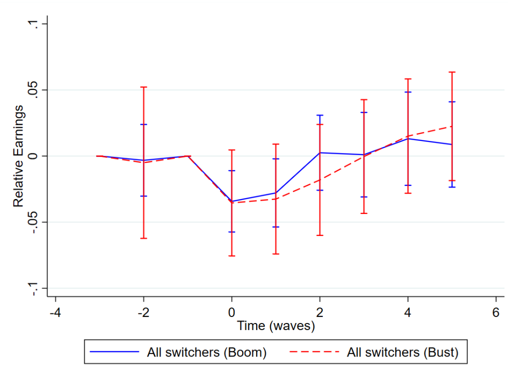


Figure C.19: *The effect of occupational switches on wages (left) and earnings (right), relative to the control group of never-switching workers, using estimated coefficients from equation (C.1). The plots show separate estimates of the effect for switches materializing during a boom or a bust. The error bars correspond to 95% pointwise confidence intervals.*

In Figure C.19, I show the results of an estimation of equation (C.1) where I do not distinguish between E-switchers and U-switchers. Compared to Figure 3.9, the estimation results are very similar: the real wage paths are very mildly procyclical (and not statistically significant), whereas no clear cyclical pattern is visible for the real earnings.

Figure C.20 displays the results of estimating specification (C.1) while allowing for the treatment

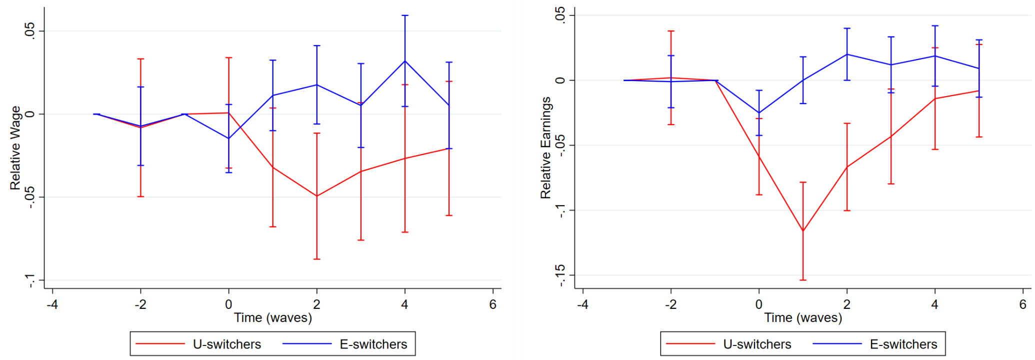


Figure C.20: *The effect of occupational switches on wages (left) and earnings (right), relative to the control group of never-switching workers, by type of switch, using estimated coefficients from equation* (C.1)*. The error bars correspond to 95% pointwise confidence intervals.*

effect to be different for the two types of switches. As can be seen in the left panel, the difference between E-switchers and U-switchers observed earlier (in Figure 3.7 in the main text) remains intact, and even strengthens slightly for earnings. Similarly, when it comes to the cyclicality of these real wage paths, the results from estimating specification (C.1) confirm the observations made from Figure 3.10 in the main text: there is a procyclical pattern for E-switchers and no clear pattern for U-switchers (as seen in the left panel of Figure C.21). On the other hand, the conclusions on the real earnings path are weakened in Figure C.21 (right panel) compared to Figure 3.10: while there is still some procyclicality for E-switchers and countercyclicality for U-switchers, the pattern is much weaker than the pattern observed in the main text.[9]

C.1.3 Results on 2-digit and 3-digit occupational mobility

In this section, I repeat the analysis of the data from Section 3.2 and parts of Section C.1.2 using (primarily) 2-digit and (in one case) 3-digit occupations instead of 1-digit occupations. To avoid a tedious repetition of the same discussion as in the main text, I will keep the discussion of the figures rather brief. It suffices to note that all tables and figures can be interpreted in exactly the same way as the corresponding tables and figures in the main text (which are identified in the discussion of each table and figure below).

C.1.3.1 Mobility Rates

In Figure C.22, I plot the 2-digit (4 month) occupational mobility rate over time (left panel) and against the unemployment rate (right panel). As can be seen by comparing Figure C.22 with it's

[9]It should be noted that for both panels of Figure C.21, all changes between the estimates for switches materializing during a boom or a bust are not statistically significant.

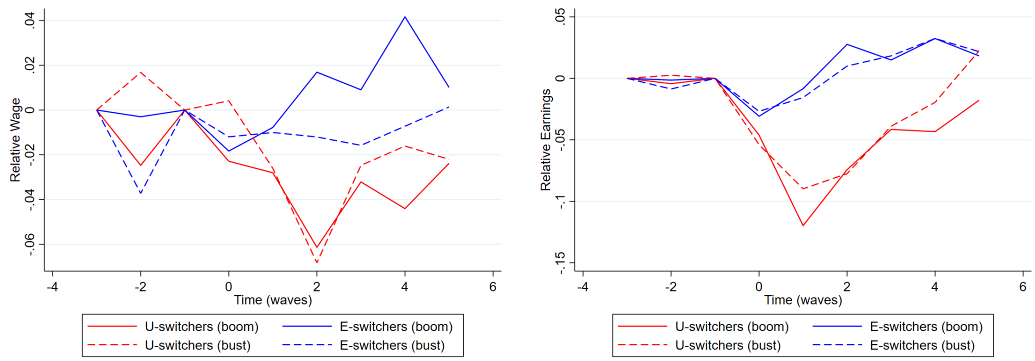


Figure C.21: *The effect of occupational switches on wages (left) and earnings (right), relative to the control group of never-switching workers, by type of switch, using estimated coefficients from equation (C.1). The plots show separate estimates of the effect for switches materializing during a boom or a bust. The 95% pointwise confidence intervals for the are available upon request.*

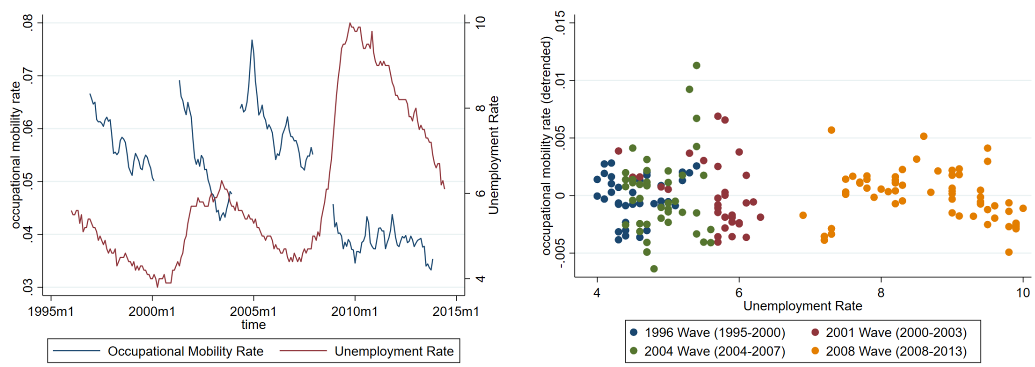


Figure C.22: *The 2-digit occupational mobility rate and the corresponding month's unemployment rate from the BLS, plotted over time (left) and against each other in a scatter plot (right).*

counterpart in the main text (Figure 3.1), the occupational mobility rates on the 2-digit level are slightly higher than on the 1-digit level, with rates ranging from 3.5% to 7.5%. However, when it comes to the cyclical patterns of the occupational mobility rates, the conclusion is similar: the left panel of Figure C.22 does not show a clear pattern, although a naive OLS regression yields the conclusion that the occupational mobility rate is mildly procyclical.
Looking at the left panel of Figure C.22, it can be seen that just like the 1-digit rate, the 2-digit occupational mobility rate seems to exhibit negative within-panel trends. Just like with the 1-digit rate, this is likely to be a consequence of the validation exercise. Indeed, re-plotting Figure C.22 with the unverified switches included, as I do in Figure C.23 (which is therefore the 2-digit equivalent of Figure C.2), again reveals a seemingly clearer countercyclical pattern of the (2-digit) occupational mobility rate, with the scatter plot in the right panel clarifies that the 2-digit occupational mobility rate is in fact procyclical.

In Figure C.24, I go one step further and plot the 3-digit occupational mobility rate, which is the rate that is most commonly reported in the literature. Not surprisingly, the 3-digit mobility rate is substantially higher than the 1-digit rate reported in Section 3.2. As can be seen in the

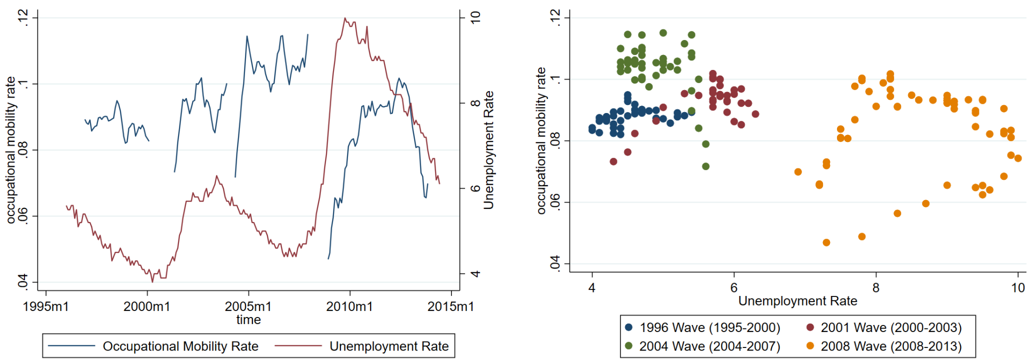


Figure C.23: *The 2-digit occupational mobility rate and the corresponding month's unemployment rate from the BLS, plotted over time, including both verified and unverified occupational switches.*

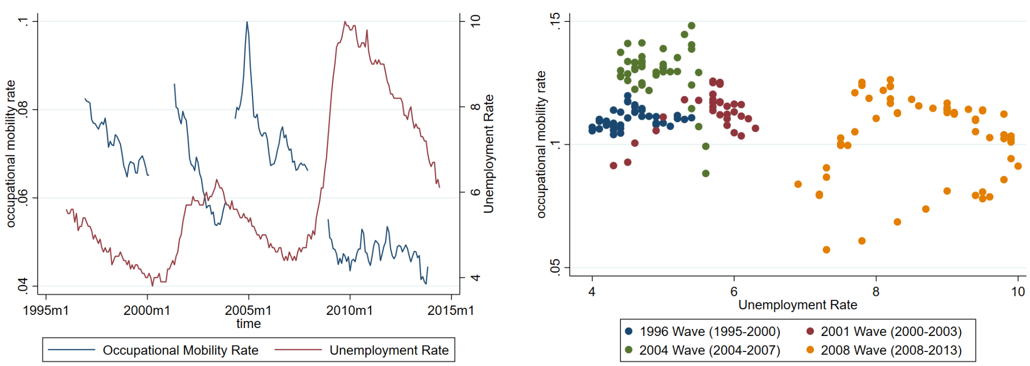


Figure C.24: *The 3-digit occupational mobility rate, plotted over time together with the corresponding month's unemployment rate from the BLS. The left panel only considers verified occupational switches (and plots the rates against the unemployment rate rather than time), whereas the right panel additionally includes unverified occupational switches.*

figure, the occupational mobility rates found in the data range from 4 to 10% with an average rate of approximately 6 to 7%. Keeping in mind that the rates in the figures are 4-month rates instead of yearly rates, this rate seems roughly consistent with the 18% yearly rate found in Kambourov and Manovskii (2008). Further including the occupational switches which could not be verified as coinciding with a change in employer, industry, working hours, or hourly wage, as I do in the right panel of Figure C.24, again reveals a clear procyclical pattern of the occupational mobility rate.

Occupation	1	2	3	4	5	6	7	8	9
Observations	344138	138122	456500	145415	151122	2733	217221	518561	39179
Inflow	15840	7556	15627	5971	8282	383	15590	24943	2607
Outflow	14856	7244	14494	6313	8405	313	16547	24609	2682
Net Inflow	984	312	1133	-342	-123	70	-957	334	-75

Occupation	10	11	12	13	14	15	16	17
Observations	23586	142496	99857	80911	15663	11642	14784	11502
Inflow	1541	9331	5483	5945	587	1043	1229	876
Outflow	1683	10427	5228	5761	694	1083	1389	911
Net Inflow	-142	-1096	255	184	-107	-40	-160	-35

Occupation	18	19	20	21	22	23	24	25
Observations	111905	233481	268968	126894	132884	3636	4697	27790
Inflow	5640	11627	16836	6599	5963	264	342	1911
Outflow	5887	12258	16482	6387	5813	243	339	1968
Net Inflow	-247	-631	354	212	150	21	3	-57

Table C.13: *Total number of incoming and outgoing switches found in the data for every 2-digit occupation, and number of times I observe a worker in each of these occupations in the data. For a list of the occupations corresponding to these codes, see Appendix C.1.1.*

Tables C.14 and C.13 list the number of occupational changes observed in the data for every possible combination of 2-digit occupations, and the corresponding total in- and outflow for each occupation. The observations that can be drawn from these tables are identical to those made for 1-digit occupations (from Tables C.2 and 3.1): it does not seem like there is specific occupation from which workers are changing or a specific occupation that workers are changing to.

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25
1	0	1437	2564	569	1420	30	1594	3123	71	87	830	225	221	22	109	71	84	312	521	814	344	338	16	24	30
2	1512	0	957	182	622	4	541	2208	51	28	170	120	57	8	29	27	20	67	174	256	63	99	4	12	33
3	2809	926	0	1511	471	40	1212	2850	91	97	532	970	240	67	129	240	71	334	506	582	258	463	7	33	55
4	617	320	1770	0	176	12	331	1115	32	19	149	387	78	8	20	17	23	143	338	320	147	271	8	4	8
5	1456	476	568	163	0	0	1735	1709	34	56	356	108	87	28	46	30	12	177	279	614	122	299	4	16	30
6	4	12	42	13	8	0	20	27	4	33	16	8	4	0	0	0	0	16	12	55	16	15	0	0	8
7	1597	572	1415	409	1823	24	0	4019	290	169	1461	556	405	72	93	152	96	344	791	1372	339	415	8	32	93
8	3292	2484	3175	1145	1568	36	3367	0	420	270	1516	1070	479	138	183	266	147	559	1317	2206	371	458	7	20	115
9	91	31	101	31	28	0	266	321	0	23	345	281	443	12	8	36	24	49	250	228	47	12	8	0	47
10	90	42	122	27	69	15	181	264	20	0	93	55	70	4	20	24	7	51	107	276	63	80	0	0	3
11	990	215	743	230	325	20	1700	1699	286	76	0	495	489	82	77	82	78	301	707	1261	271	185	8	15	92
12	254	116	1141	363	113	16	482	1002	255	43	420	0	127	24	60	97	19	103	241	233	31	39	4	4	41
13	231	70	203	78	83	12	446	527	365	87	564	167	0	4	54	23	28	212	557	1098	423	335	3	12	179
14	38	35	43	0	23	0	102	168	4	4	46	56	15	0	4	16	43	24	27	39	0	0	0	3	4
15	91	18	131	20	39	0	122	233	12	32	62	60	53	10	0	8	32	12	16	72	22	35	0	0	3
16	69	44	242	17	31	0	202	326	47	13	99	95	29	10	23	0	8	16	59	46	0	5	4	0	4
17	95	28	63	16	28	0	92	138	16	8	107	40	59	12	16	8	0	16	52	62	19	8	0	8	20
18	443	101	327	142	220	16	332	528	69	52	277	161	257	16	11	18	7	0	1237	789	400	358	16	12	98
19	498	168	505	317	259	15	860	1451	228	120	721	279	611	31	19	57	38	1383	0	2893	746	765	20	12	262
20	802	242	668	308	553	68	1254	2128	192	227	1057	253	1208	31	103	41	71	753	2656	0	2068	1097	93	56	553
21	422	91	266	117	160	40	268	428	50	58	246	32	440	0	6	0	12	324	728	1977	0	589	38	1	94
22	384	84	452	286	226	23	361	505	16	23	173	37	359	4	23	4	20	333	737	1048	622	0	12	12	69
23	4	4	12	0	0	4	0	7	4	0	0	0	4	0	0	4	0	24	12	91	46	20	0	0	7
24	24	16	8	4	4	4	30	44	0	0	8	0	8	0	0	0	12	8	16	70	8	12	0	0	63
25	27	24	109	23	33	4	92	123	50	16	83	28	202	4	10	8	24	79	287	434	173	65	4	66	0

Table C.14: *Number of switches found in the data for every combination of 2-digit occupations. Rows correspond to the previous occupations, and columns correspond to new occupations. Horizontal and vertical lines group 2-digit occupations belonging to the same 1-digit occupation. For a list of the occupations corresponding to these codes, see Appendix C.1.1.*

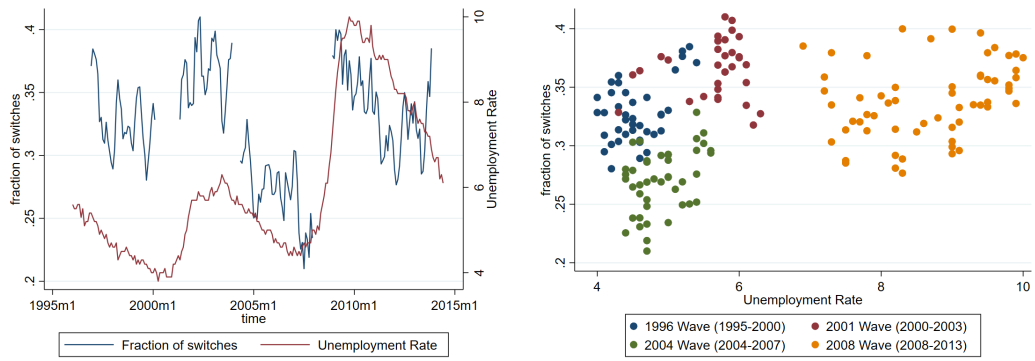


Figure C.25: *The fraction of occupational switchers (2-digit) going through unemployment and the corresponding month's unemployment rate from the BLS, over time (left) and plotted against each other in a scatter plot (right).*

In Figure C.25, I analyze the fraction of 2-digit occupational switchers going through an unemployment spell. Similar to my findings for 1-digit occupations (in Figure 3.2), I find that the fraction of switches that goes through unemployment shows a clear countercyclical pattern, even if the average fraction is slightly lower than it was for 1-digit switchers.

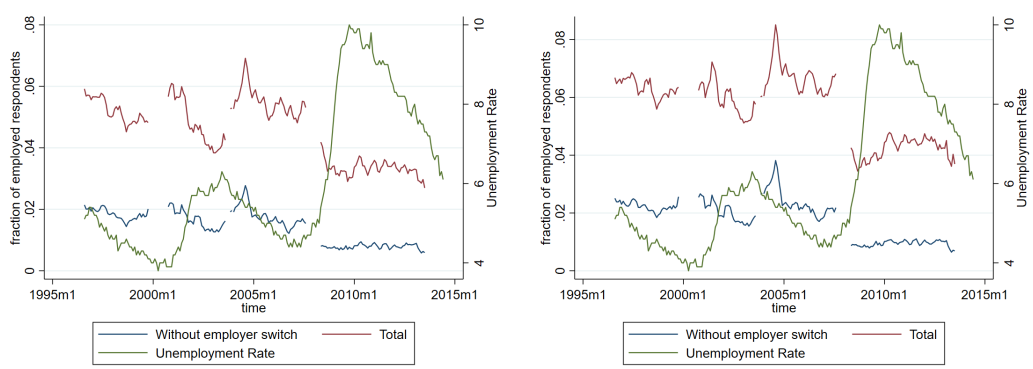


Figure C.26: *The fraction of employed workers switching occupations (2-digit) in the next 4 months, the fraction of employed workers doing so without switching employer, plotted over time together with the corresponding month's unemployment rate from the BLS. The left panel only considers verified occupational switches, whereas the right panel additionally includes unverified occupational switches.*

Finally, Figure C.26 plots the 2-digit occupational mobility rate considering only switches through employment (similar to what Figure 3.3 did for 1-digit occupational switches). Comparing Figures C.26 and 3.3, it can be seen that slightly more,but still roughly a third, of the employed workers who switch occupations do so without changing employers, thereby re-confirming the importance of considering job-to-job occupational mobility without employment changes when modeling occupational mobility.

C.1.3.2 Subsequent Earnings and Wages

In Figure C.27, I plot the equivalent of Figure 3.4 from the main text, using 2-digit occupational switchers instead. Thus, it plots the average real wage for U- and E-switchers, from 12 months before until 24 months after the occupational switch materializes, relative to the last observed real

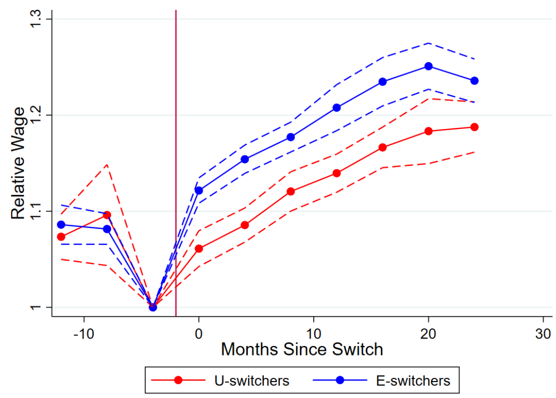


Figure C.27: *Real wage paths over time for (2-digit) occupational U-switchers and E-switchers. The switch takes place between time -4 and 0, as represented by the vertical line at -2. The dashed lines correspond to the (pointwise) 95% confidence interval.*

wage before the switch. As can be seen by comparing Figures C.27 and it's 1-digit equivalent 3.4, using 2-digit instead of 1-digit occupational switchers does not make a substantial difference in terms of the subsequent relative real wage paths. Just like in the main text, it can be concluded that U-switchers tend to do worse than E-switchers.

In order to analyze whether the pattern changes over the business cycle, I plot the 2-digit equivalent of Figure 3.5 in Figure C.28, which thus show how cyclical the relative real wages observed immediately after the switch are. Once again, the conclusions from the figure are almost identical to those obtained in the main text: Overall, wage differentials seem to be countercyclical, and this is primarily coming from the countercyclicality of the wage differentials for E-switchers, while the wage differentials for U-switchers appear to be acyclical.

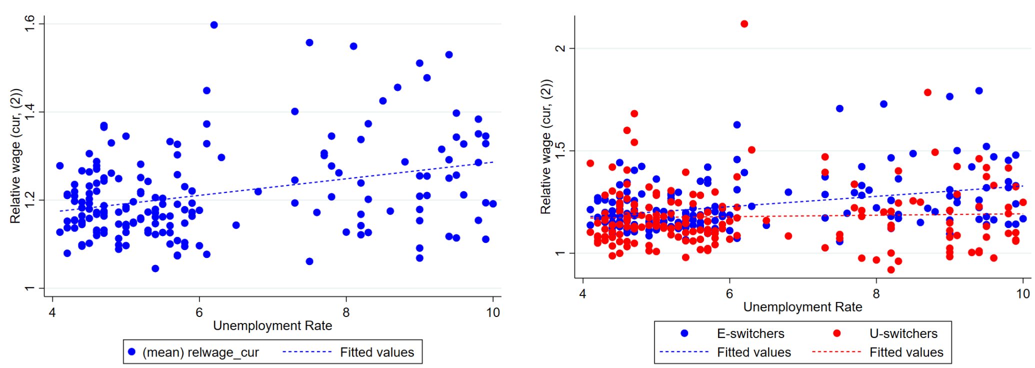


Figure C.28: *Real wages immediately after the occupational switch, relative to the real wage before the switch, for all (2-digit) occupational switchers (left) or for occupational U-switchers and E-switchers (right), plotted against the unemployment rate at the time of the switch. Dashed lines show fitted values corresponding to a simple OLS regression.*

In Figure C.29, I plot the results of an estimation of equation (3.1) where I do not distinguish between E-switchers and U-switchers, using 2-digit occupational switchers (with the 1-digit equivalent being Figure 3.6 in the main text). Similar to what was found for 1-digit occupational switchers,

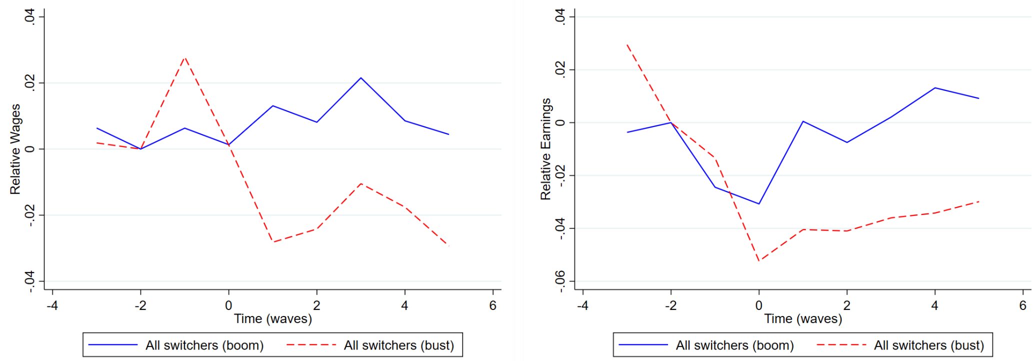


Figure C.29: *The effect of (2-digit) occupational switches on real wages (left) and real earnings (right), relative to the control group of never-switched workers, using estimated coefficients from equation* (3.1)*,specific to switches that materialized in booms or busts.*

the subsequent wage and earnings outlook of 2-digit occupational switchers is worse for workers who switch occupations during a recession.

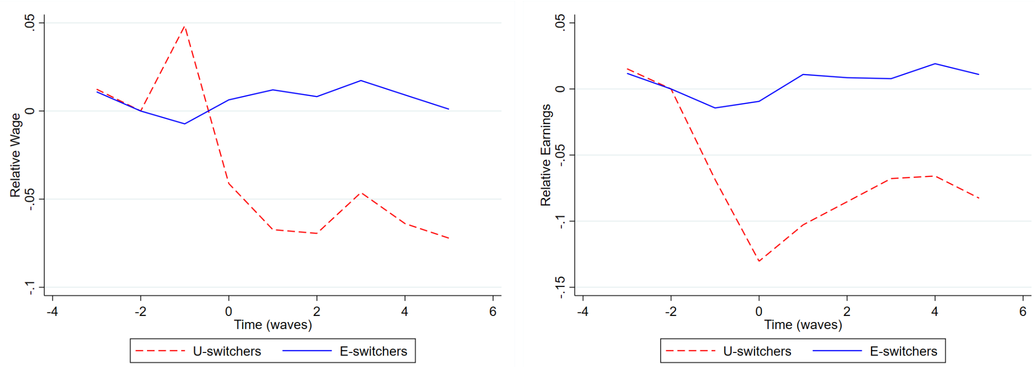


Figure C.30: *The effect of (2-digit) occupational switches on real wages (left) and real earnings (right), relative to the control group of never-switched workers, by type of switch, using estimated coefficients from equation* (3.1)*.*

Moving to the distinction between E-switchers and U-switchers, Figure C.30 shows the results of estimating equation (3.1) while allowing the treatment effects to be different for E- and U-switchers at the 2-digit level. Similar to what was observed for the 1-digit level in Figure 3.7, U-switchers are observed to do substantially worse than E-switchers, both in terms of real wages and real earnings. When considering how these separate real wage and real earnings paths for E- and U-switchers change over the business cycle (for 2-digit switchers), Figure C.31 yields a similar conclusion as its 1-digit equivalent in the main text (Figure 3.8): Following this estimation, the paths seem to be procyclical for both E-switchers and U-switchers.

Finally, using the three-step estimation method from Borusyak et al. (2021) instead of the standard two-way fixed effects approach yields similar results on the 2-digit level compared to the 1-digit level. When estimating a single treatment effect for all switcher types, it can be seen in Figure C.32 (which corresponds to Figure 3.9 in the main text) that the subsequent real wage path seems clearly procyclical, whereas the real earnings path is only very mildly procyclical (as opposed to acyclical as seen in the main text). Further splitting out these results by switcher types, as done in Figure

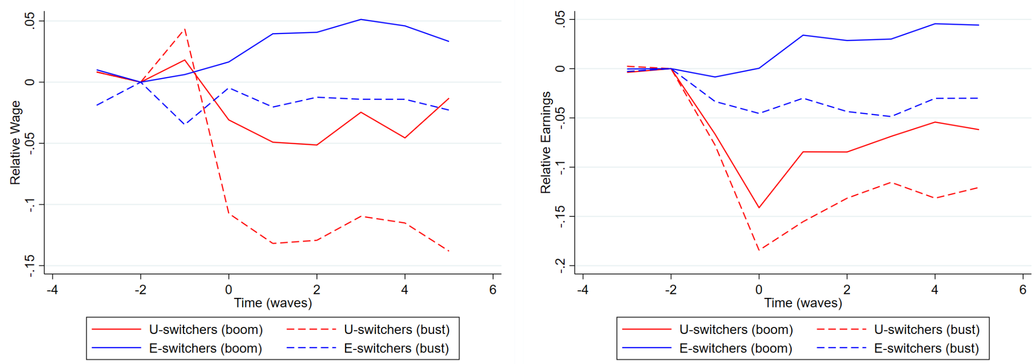


Figure C.31: *The effect of (2-digit) occupational switches on real wages (left) and real earnings (right), relative to the control group of never-switched workers, by type of switch, using estimated coefficients from equation (3.1), specific to switches that materialized in booms or busts (right panel).*

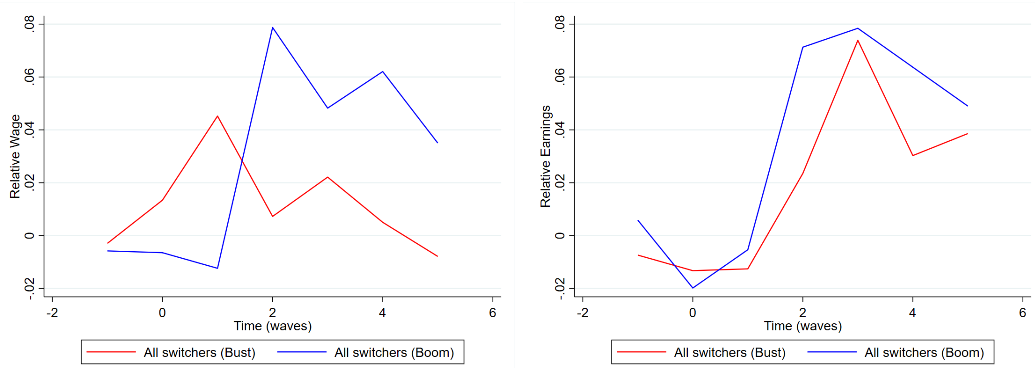


Figure C.32: *The effect of (2-digit) occupational switches on real wages (left) and real earnings (right), relative to the counterfactual of never switching, using estimated coefficients obtained using the three-step estimation method. The plots show separate estimates of the effect for switches materializing during a boom or a bust.*

C.33 (the 2-digit equivalent of Figure 3.10), slightly strengthens the results from the main text: A clear countercyclical pattern is visible for U-switchers, especially when it comes to real earnings, but also (albeit much milder) for real wages.

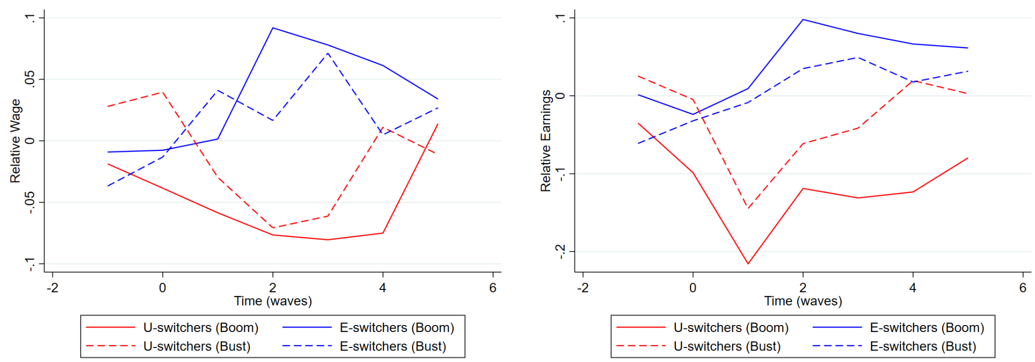


Figure C.33: *The effect of (2-digit) occupational switches on real wages (left) and real earnings (right), relative to the counterfactual of never switching, by type of switch, using estimated coefficients obtained using the three-step estimation method. The plots show separate estimates of the effect for switches materializing during a boom or a bust.*

C.2 Omitted Proofs

C.2.1 Proposition 1

Proposition 1: The model has a unique block-recursive equilibrium.

Proof. Let $M(p,z,x_h) = W^E(p,z,x_h) + J(p,z,x_h)$ be the value of a match, and let T be an operator that maps $M(p,z,x_h)$ and $W^U(p,z,x_h)$ into the same function space. In order to do so, define Γ such that $\Gamma(p,z,x_h,0) = W^E(p,z,x_h) + J(p,z,x_h)$ and $\Gamma(p,z,x_h,1) = W^U(p,z,x_h)$. Using $\sigma(p,z,x_h) = d(p,z,x_h) = \hat{\sigma}(p,z,x_h)$, equations (3.5) and (3.8), and the free entry condition, one can rewrite $T(\Gamma(p,z,x_h,0))$ as follows (dropping the subscript h from x_h throughout, and using $(\cdot)$ instead of (p',z',x')):

$$
\begin{aligned}
T(\Gamma(p,z,x,0)) = w(p,z,x) + \beta\mathbb{E}_{p',z',x'} & \Bigg[\max_{\hat{\sigma}(\cdot),\rho^e(\cdot)} \Bigg\{\psi \int_{\underline{z}}^{\bar{z}} \max\Big\{W^E(p',\tilde{z},x'), W^U(p',\tilde{z},x')\Big\}\, dF(\tilde{z}) \\
&+(1-\psi)\Big[\hat{\sigma}(\cdot)W^U(\cdot) + (1-\hat{\sigma}(\cdot))\Big[(1-\rho^e(\cdot))W^E(\cdot) \\
&+\rho^e(\cdot)\Big(-c^e(p') + R^E(\cdot)\Big)\Big]\Big]\Bigg\}\Bigg] + y(p,z,x) - w(p,z,x) \\
&+\beta\mathbb{E}_{p',z',x'}\Bigg[\max_{\hat{\sigma}(\cdot)}\Bigg\{\psi\int_{\underline{z}}^{\bar{z}} \max\left\{J(p',\tilde{z},x'), V(p',\tilde{z},x')\right\} dF(\tilde{z}) \\
&+(1-\psi)\Big((1-\hat{\sigma}(\cdot))\Big(1-\rho^e(\cdot)\int_{\underline{z}}^{\bar{z}} \lambda(\theta(p',\tilde{z},x_1))dF(\tilde{z})\Big) J(\cdot)\Big)\Bigg\}\Bigg]
\end{aligned}
$$

$$
\begin{aligned}
&= y(p,z,x) + \beta \mathbb{E}_{p',z',x'} \left[\max_{\hat{\sigma}(\cdot)} \left\{ \psi \int_{\underline{z}}^{\bar{z}} \max\left\{ M(p',\tilde{z},x'), W^U(p',\tilde{z},x') \right\} dF(\tilde{z}) \right.\right. \\
&\quad + (1-\psi) \left[\hat{\sigma}(\cdot) W^U(\cdot) + (1-\hat{\sigma}(\cdot)) \left[\max_{\rho^e(\cdot)} \left\{ (1-\rho^e(\cdot)) W^E(\cdot) + \rho^e(\cdot) \left(-c^e(p') \right.\right.\right.\right. \\
&\quad \left. + \int_{\underline{z}}^{\bar{z}} [(1-\lambda(\theta(p',\tilde{z},x_1))) W^E(\cdot) + \lambda(\theta(p',\tilde{z},x_1)) W^E(p',\tilde{z},x_1)] dF(\tilde{z}) \right) \Bigg\} \\
&\quad + \left(1 - \rho^e(\cdot) \int_{\underline{z}}^{\bar{z}} \lambda(\theta(p',\tilde{z},x_1)) dF(\tilde{z}) \right) J(\cdot) \Bigg] \Bigg] \Bigg\} \Bigg] \\
&= y(p,z,x) + \beta \mathbb{E}_{p',z',x'} \left[\max_{\hat{\sigma}(\cdot)} \left\{ \psi \int_{\underline{z}}^{\bar{z}} \max\left\{ M(p',\tilde{z},x'), W^U(p',\tilde{z},x') \right\} dF(\tilde{z}) + (1-\psi) \left[\hat{\sigma}(\cdot) W^U(\cdot) \right.\right.\right. \\
&\quad + (1-\hat{\sigma}(\cdot)) \left[\max_{\rho^e(\cdot)} \left\{ \left(1 - \rho^e(\cdot) \int_{\underline{z}}^{\bar{z}} \lambda(\theta(p',\tilde{z},x_1)) dF(\tilde{z}) \right) W^E(\cdot) \right.\right. \\
&\quad \left. + \rho^e(\cdot) \left(-c^e(p') + \int_{\underline{z}}^{\bar{z}} \lambda(\theta(p',\tilde{z},x_1)) W^E(p',\tilde{z},x_1) dF(\tilde{z}) \right) \right\} \\
&\quad + \left(1 - \rho^e(\cdot) \int_{\underline{z}}^{\bar{z}} \lambda(\theta(p',\tilde{z},x_1)) dF(\tilde{z}) \right) J(\cdot) \Bigg] \Bigg] \Bigg\} \Bigg] \\
&= y(p,z,x) + \beta \mathbb{E}_{p',z',x'} \left[\max_{\hat{\sigma}(\cdot)} \left\{ \psi \int_{\underline{z}}^{\bar{z}} \max\left\{ M(p',\tilde{z},x'), W^U(p',\tilde{z},x') \right\} dF(\tilde{z}) + (1-\psi) \left[\hat{\sigma}(\cdot) W^U(\cdot) \right.\right.\right. \\
&\quad + (1-\hat{\sigma}(\cdot)) \left[\max_{\rho^e(\cdot)} \left\{ \left(1 - \rho^e(\cdot) \int_{\underline{z}}^{\bar{z}} \lambda(\theta(p',\tilde{z},x_1)) dF(\tilde{z}) \right) \left[M(\cdot) - \frac{k\theta(\cdot)}{\lambda(\theta(\cdot))} \right] \right.\right. \\
&\quad \left. + \rho^e(\cdot) \left(-c^e(p') + \int_{\underline{z}}^{\bar{z}} [\lambda(\theta(p',\tilde{z},x_1)) M(p',\tilde{z},x_1) - k\theta(p',\tilde{z},x_1)] dF(\tilde{z}) \right) \right\} \\
&\quad + \left(1 - \rho^e(\cdot) \int_{\underline{z}}^{\bar{z}} \lambda(\theta(p',\tilde{z},x_1)) dF(\tilde{z}) \right) \frac{k\theta(\cdot)}{\lambda(\theta(\cdot))} \Bigg] \Bigg] \Bigg\} \Bigg]
\end{aligned} \tag{C.2}
$$

Here, the second equality uses that by Nash bargaining, $W^E > W^U$ implies $J > V$ and vice versa, so that $\max\left\{W^E(p',\tilde{z},x'), W^U(p',\tilde{z},x')\right\}$ and $\max\left\{J(p',\tilde{z},x'), V(p',\tilde{z},x')\right\}$ can be combined to $\max\left\{M(p',\tilde{z},x'), W^U(p',\tilde{z},x')\right\}$ (using that $V=0$ in equilibrium). Furthermore, the last equality uses that (using the free entry condition and the definition of M) $J(p,z,x) = k\theta(p,z,x)/\lambda(\theta(p,z,x))$ and $W^E(p,z,x) = M(p,z,x) - k\theta(p,z,x)/\lambda(\theta(p,z,x))$.

Similarly, one can rewrite $T(\Gamma(p,z,x_h,1))$ as follows (dropping the subscript h from x_h throughout, and using $(\cdot)$ instead of (p',z',x_h)):

$$
\begin{aligned}
T(\Gamma(p,z,x,1)) &= b + \beta \mathbb{E}_{p',z'} \left[\max_{\rho^u(\cdot)} \left\{ \rho^u(\cdot) \left[-c^u(p') + \int_{\underline{z}}^{\bar{z}} W^U(p',\tilde{z},x_1) dF(\tilde{z}) \right] \right.\right. \\
&\quad \left.\left. + (1-\rho^u(\cdot)) \left[\lambda(\theta(\cdot)) W^E(\cdot) + (1-\lambda(\theta(\cdot))) W^U(\cdot) \right] \right\} \right] \\
&= b + \beta \mathbb{E}_{p',z'} \left[\max_{\rho^u(\cdot)} \left\{ \rho^u(\cdot) \left[-c^u(p') + \int_{\underline{z}}^{\bar{z}} W^U(p',\tilde{z},x_1) dF(\tilde{z}) \right] \right.\right. \\
&\quad \left.\left. + (1-\rho^u(\cdot)) \left[\lambda(\theta(\cdot)) M(\cdot) - k\theta(\cdot) + (1-\lambda(\theta(\cdot))) W^U(\cdot) \right] \right\} \right]
\end{aligned} \tag{C.3}
$$

Throughout the rest of this proof, I will further simplify notation by not acknowledging the arguments of functions. However, in order to still acknowledge that arguments of the function differ throughout the equation, I introduce some additional notation. So, if f is a function (e.g. θ), then $f = f(p',z',x')$ (noting that $x' = x$ if the worker is unemployed), $\bar{f} = f(p,z,x)$, and $\tilde{f} = f(p',\tilde{z},x')$.

Applying this notation, equations (C.2) and (C.3) reduce to equations (C.4) and (C.5) below:

$$\begin{aligned} T(\Gamma(p,z,x,0)) = \bar{y} + \beta\mathbb{E}\Bigg[\max_{\hat{\sigma}}\Bigg\{\psi\int_{\underline{z}}^{\bar{z}}\max\{\tilde{M},\tilde{W^U}\}dF(\tilde{z}) + (1-\psi)\Bigg[\hat{\sigma}W^U \\ +(1-\hat{\sigma})\Bigg[\max_{\rho^e}\Bigg\{\rho^e\left(-c^{e\prime}+\int_{\underline{z}}^{\bar{z}}[\tilde{\lambda}\tilde{M}-k\tilde{\theta}]dF(\tilde{z})\right) \\ +\left(1-\rho^e\int_{\underline{z}}^{\bar{z}}\tilde{\lambda}dF(\tilde{z})\right)[M-k\theta/\lambda]\Bigg\} + \left(1-\rho^e\int_{\underline{z}}^{\bar{z}}\tilde{\lambda}dF(\tilde{z})\right)\frac{k\theta}{\lambda}\Bigg]\Bigg]\Bigg\}\Bigg] \end{aligned} \tag{C.4}$$

$$\begin{aligned} T(\Gamma(p,z,x,1)) = b + \beta\mathbb{E}\Bigg[\max_{\rho^u}\Bigg\{\rho^u\left[-c^{u\prime}+\int_{\underline{z}}^{\bar{z}}\tilde{W^U}dF(\tilde{z})\right] \\ +(1-\rho^u)\left[\lambda M - k\theta + (1-\lambda)W^U\right]\Bigg\}\Bigg] \end{aligned} \tag{C.5}$$

It can be shown that T maps continuous functions into continuous functions: After all, W^E, J, W^U, λ and y are all continuous. Because the choice ρ^u comes down to selecting $\max\{-c^u + \int_{\underline{z}}^{\bar{z}}\tilde{W^U}dF(\tilde{z}),$ $\lambda M - k\theta + (1-\lambda)W^U\}$, and both these elements are continuous, so is $T(\Gamma(p,z,x,1))$. A similar argument holds for ρ^e, which comes down to selecting $\max\{W^E, \int_{\underline{z}}^{\bar{z}}[(1-\tilde{\lambda})W^E + \tilde{\lambda}\tilde{W^E}]dF(\tilde{z})\}$ and $\hat{\sigma}$, which comes down to selecting
$\max\left\{W^U, \rho^e\left(-c^e + \int_{\underline{z}}^{\bar{z}}[\tilde{\lambda}\tilde{M}-k\tilde{\theta}]dF(\tilde{z})\right) + \left(1-\rho^e\int_{\underline{z}}^{\bar{z}}\tilde{\lambda}dF(\tilde{z})\right)M\right\}$ (using the optimal ρ^e from the previous max operator) and the choice between $\tilde{W^U}$ and $\tilde{M}$ after the occupational transfer shock ψ. Therefore, $T(\Gamma(p,z,x,0))$ is also continuous. As a result, it can be concluded that T maps bounded continuous functions into bounded continuous functions, where the boundedness follows from the boundedness of p, z, and x.

To show that in fact T is a contraction mapping, one can use Blackwell's sufficiency conditions (see Stokey et al. (1989), Theorem 3.3). The monotonicity condition requires that for all $f, g \in \Gamma$ for which $f(p,z,x,n) \leq g(p,z,x,n)\ \forall p,z,x$ (on the grid) and $n = 0,1$, it must be that $T(f(p,z,x,n)) \leq T(g(p,z,x,n))$. In this case, checking this condition entails assuming $W^U(p,z,x) \leq \hat{W^U}(p,z,x)$ and $M(p,z,x) \leq \hat{M}(p,z,x)$, and showing that this assumption implies
$T(\Gamma(p,z,x,n)) \leq T(\hat{\Gamma}(p,z,x,n))$ for both $n=0$ and $n=1$ (and for all p,z,x on the grid). For $n=1$, this implication follows immediately: If $W^U(p,z,x) \leq \hat{W^U}(p,z,x)$ for all (p,z,x), then it must be that $\int_{\underline{z}}^{\bar{z}} W^U dF(z) \leq \int_{\underline{z}}^{\bar{z}}\hat{W^U}dF(z)$. As it furthermore holds that $\beta \in (0,1)$, $\rho^u \in [0,1]$, and $\lambda \in [0,1]$, it follows from equation (C.5) that $T(\Gamma(p,z,x,1)) \leq T(\hat{\Gamma}(p,z,x,1))$. To show that $T(\Gamma(p,z,x,0)) \leq T(\hat{\Gamma}(p,z,x,0))$, a similar reasoning can be used, which leads to the conclusion that this condition will also hold if $\psi \in [0,1]$, $\hat{\sigma} \in [0,1]$, $\rho^e \in [0,1]$, and $\lambda \in [0,1]$, all of which hold by assumption. Therefore, it can be concluded that the monotonicity condition is satisfied.

The discounting condition requires that $\exists\beta \in (0,1)$ such that $T(f+a) \leq T(f) + \beta a\ \forall f \in \Gamma$, $\forall a \geq 0$, and $\forall(p,z,x)$. To show that this condition also holds, one can replace all M and W^U in equations (C.4) and (C.5) by $M+a$ and W^U+a:

$$\begin{aligned} T(\Gamma(p,z,x,0)+a) = \bar{y} + \beta\mathbb{E}\Bigg[\max_{\hat{\sigma}}\Bigg\{\psi\int_{\underline{z}}^{\bar{z}}(\max\{\tilde{M},\tilde{W^U}\}+a)dF(\tilde{z}) + (1-\psi)\Bigg[\hat{\sigma}(W^U+a) \\ +(1-\hat{\sigma})\Bigg[\max_{\rho^e}\Bigg\{\rho^e\left(-c^{e\prime}+\int_{\underline{z}}^{\bar{z}}[\tilde{\lambda}(\tilde{M}+a)-k\tilde{\theta}]dF(\tilde{z})\right) \\ +\left(1-\rho^e\int_{\underline{z}}^{\bar{z}}\tilde{\lambda}dF(\tilde{z})\right)[M+a-k\theta/\lambda]\Bigg\} + \left(1-\rho^e\int_{\underline{z}}^{\bar{z}}\tilde{\lambda}dF(\tilde{z})\right)\frac{k\theta}{\lambda}\Bigg]\Bigg]\Bigg\}\Bigg] \end{aligned}$$

$$T(\Gamma(p,z,x,0)+a) = \bar{y} + \beta\mathbb{E}\left[\max_{\hat{\sigma}}\left\{\psi\int_{\underline{z}}^{\bar{z}}\max\{\tilde{M},\tilde{W^U}\}dF(\tilde{z}) + \psi\int_{\underline{z}}^{\bar{z}} adF(\tilde{z})\right.\right.$$
$$+(1-\psi)\left[\hat{\sigma}W^U + \hat{\sigma}a + (1-\hat{\sigma})\left[\max_{\rho^e}\left\{\left(1-\rho^e\int_{\underline{z}}^{\bar{z}}\tilde{\lambda}dF(\tilde{z})\right)\left[M-\frac{k\theta}{\lambda}\right]\right.\right.\right.$$
$$+\left(1-\rho^e\int_{\underline{z}}^{\bar{z}}\tilde{\lambda}dF(\tilde{z})\right)a + \rho^e\left(-c^{e\prime} + \int_{\underline{z}}^{\bar{z}}[\tilde{\lambda}\tilde{M}-k\tilde{\theta}]dF(\tilde{z}) + \int_{\underline{z}}^{\bar{z}}\tilde{\lambda}adF(\tilde{z})\right)\Bigg\}$$
$$\left.\left.\left.\left.+\left(1-\rho^e\int_{\underline{z}}^{\bar{z}}\tilde{\lambda}dF(\tilde{z})\right)\frac{k\theta}{\lambda}\right]\right]\right\}\right]$$
$$T(\Gamma(p,z,x,0)+a) = \bar{y} + \beta\mathbb{E}\left[\max_{\hat{\sigma}}\left\{\psi\int_{\underline{z}}^{\bar{z}}\max\{\tilde{M},\tilde{W^U}\}dF(\tilde{z}) + \psi a + (1-\psi)\left[\hat{\sigma}W^U + \hat{\sigma}a\right.\right.\right.$$
$$+(1-\hat{\sigma})\left[a + \max_{\rho^e}\left\{\rho^e\left(-c^{e\prime} + \int_{\underline{z}}^{\bar{z}}[\tilde{\lambda}\tilde{M}-k\tilde{\theta}]dF(\tilde{z})\right)\right.\right.$$
$$\left.\left.\left.\left.\left.+\left(1-\rho^e\int_{\underline{z}}^{\bar{z}}\tilde{\lambda}dF(\tilde{z})\right)\left[M-\frac{k\theta}{\lambda}\right]\right\} + \left(1-\rho^e\int_{\underline{z}}^{\bar{z}}\tilde{\lambda}dF(\tilde{z})\right)\frac{k\theta}{\lambda}\right]\right]\right\}\right]$$
$$T(\Gamma(p,z,x,0)+a) = \bar{y} + \beta\mathbb{E}\left[\max_{\hat{\sigma}}\left\{\psi\int_{\underline{z}}^{\bar{z}}\max\{\tilde{M},\tilde{W^U}\}dF(\tilde{z}) + (1-\psi)\left[\hat{\sigma}W^U\right.\right.\right.$$
$$+(1-\hat{\sigma})\left[\max_{\rho^e}\left\{\rho^e\left(-c^{e\prime} + \int_{\underline{z}}^{\bar{z}}[\tilde{\lambda}\tilde{M}-k\tilde{\theta}]dF(\tilde{z})\right)\right.\right.$$
$$\left.\left.\left.\left.\left.+\left(1-\rho^e\int_{\underline{z}}^{\bar{z}}\tilde{\lambda}dF(\tilde{z})\right)[M-k\theta/\lambda]\right\} + \left(1-\rho^e\int_{\underline{z}}^{\bar{z}}\tilde{\lambda}dF(\tilde{z})\right)\frac{k\theta}{\lambda}\right]\right]\right\}\right] + \beta a$$
$$T(\Gamma(p,z,x,0)+a) = T(\Gamma(p,z,x,0)) + \beta a \tag{C.6}$$
$$T(\Gamma(p,z,x,1)) = b + \beta\mathbb{E}\left[\max_{\rho^u}\left\{\rho^u\left[-c^{u\prime} + \int_{\underline{z}}^{\bar{z}}(\tilde{W^U}+a)dF(\tilde{z})\right]\right.\right.$$
$$\left.\left.+(1-\rho^u)\left[\lambda(M+a) - k\theta + (1-\lambda)(W^U+a)\right]\right\}\right]$$
$$T(\Gamma(p,z,x,1)) = b + \beta\mathbb{E}\left[\max_{\rho^u}\left\{\rho^u\left[-c^{u\prime} + \int_{\underline{z}}^{\bar{z}}\tilde{W^U}dF(\tilde{z}) + \int_{\underline{z}}^{\bar{z}}adF(\tilde{z})\right]\right.\right.$$
$$\left.\left.+(1-\rho^u)\left[\lambda M - k\theta + (1-\lambda)W^U + a\right]\right\}\right]$$
$$T(\Gamma(p,z,x,1)) = b + \beta\mathbb{E}\left[\max_{\rho^u}\left\{\rho^u a + \rho^u\left[-c^{u\prime} + \int_{\underline{z}}^{\bar{z}}\tilde{W^U}dF(\tilde{z})\right]\right.\right.$$
$$\left.\left.+(1-\rho^u)a + (1-\rho^u)\left[\lambda M - k\theta + (1-\lambda)W^U\right]\right\}\right]$$
$$T(\Gamma(p,z,x,1)) = b + \beta\mathbb{E}\left[\max_{\rho^u}\left\{\rho^u\left[-c^{u\prime} + \int_{\underline{z}}^{\bar{z}}\tilde{W^U}dF(\tilde{z})\right]\right.\right.$$
$$\left.\left.+(1-\rho^u)\left[\lambda M - k\theta + (1-\lambda)W^U\right]\right\}\right] + \beta a$$
$$T(\Gamma(p,z,x,1)) = T(\Gamma(p,z,x,1)) + \beta a \tag{C.7}$$

From equations (C.6) and (C.7) it can be concluded that for both $n = 0$ and $n = 1$ it holds that $T(\Gamma(p,z,x,n)+a) \leq T(\hat{\Gamma}(p,z,x,n)) + \beta a$. As the derivation above does not rely on the actual value taken by W^U, M or a, and since $\beta \in (0,1)$ by assumption, it can thus be concluded that the discounting condition is also satisfied. Therefore, it can be stated that T is a contraction mapping, which by the contraction mapping theorem (see Stokey et al. (1989), Theorem 3.2) has a unique fixed point. This fixed point is a candidate for a BRE.

The fixed point of contraction mapping T contains functions
$M(p,z,x)$ and $W^U(p,z,x)$. As W^E can be calculated using these two objects using $W^E(p,z,x) =$

$(1-\eta)M(p,z,x)+\eta W^U(p,z,x)$, which follows from the Nash bargaining condition (equation 3.3), and $V(p,z,x)$ follows from the free entry condition, it can be concluded that all value functions can be obtained from the fixed point. One can then use the free entry condition to calculate $\theta(p,z,x)$, after which functions d,σ,ρ^u, and ρ^e follows from the inequality conditions above (in the paragraph following equation C.5), and $w(p,z,x)$ can be calculated from the expression for $J(p,z,x)$ (equation 3.8). Finally, the expression for $W^E(p,z,x)$ (equation 3.5) is satisfied by construction, given that the expression for $J(p,z,x)$ holds and $M(p,z,x)$ is a combination of the expressions for $J(p,z,x)$ and $W^E(p,z,x)$. Therefore, it can be concluded that this fixed point of T satisfies all the equilibrium conditions, thus completing the proof of existence of the BRE.

In order to prove uniqueness, first suppose that the BRE constructed above is not the unique BRE as a function of p, z, and x. Then, there must be a second set of functions $W^U, W^E, J, V, \theta, w, d, \sigma, \rho^u, \rho^e$ that satisfies the equilibrium conditions. Using W^U, W^E, and J from that second set of functions, one can then construct a corresponding $\Gamma(p,z,x,0)$ and $\Gamma(p,z,x,1)$, which must be a fixed point of T. After all, if this set $\Gamma(p,z,x,0)$ and $\Gamma(p,z,x,1)$ would not be a fixed point of T, the equilibrium conditions (specifically at least one of equations 3.4, 3.5, and 3.8) are not satisfied. However, this conclusion contradicts the uniqueness of the fixed point of T, thus contradicting the existence of this second set of equilibrium functions. As there is no reason to believe the equilibrium functions depend on anything other than the three productivity variables p, z, and x, given that no other variables enter in any of the equilibrium conditions, this contradiction completes the proof of uniqueness.

□

C.2.2 Proposition 2

Proposition (Proposition 2)**.** *Unless c^e is prohibitively high for all p or $\lambda(p,z,x_1)=0$ for all (p,z), the block-recursive equilibrium is not constrained efficient.*

Proof. Throughout this proof, denote by $\mathcal{E}_t^j$ the distribution of unemployed and employed workers over all occupations at the start of subperiod j or period t. Similarly, let $\Omega_t^j=\{n_t,o_t,p_t,z_t,x_h,\mathcal{E}_t^j\}$ be the state space for a worker at the start of subperiod j of period t. Here, n_t denotes the worker's employment status, and o_t denotes the worker's current occupation. In order to evaluate the (constrained) efficiency of the BRE, I will compare the social planner's problem (in recursive form) with the operator T defined in the proof of Proposition 1 (Section C.2.1).In general, one can write down the social planner's problem as follows:

$$\max_{d(\cdot),\rho^u(\cdot),\rho^e(\cdot),v(\cdot)} \mathbb{E}\left[\sum_{t=0}^{\infty}\beta^t\sum_{o=1}^{O}\sum_{h=1}^{H}\int_{\underline{z}}^{\bar{z}}\Big[u_{o,t}(z,x_h)b+e_{o,t}(z,x_h)y(p_t,z,x_h)\right.$$
$$\left. -\Big(c^u(p_t)\rho^u(\cdot)u_{o,t}(z,x_h)+c^e(p_t)\rho^e(\cdot)(1-\psi)(1-d(\cdot))e_{o,t}(z,x_h)+kv_{o,t}(\cdot)\Big)\Big]\,dz\right]$$
$$\text{s.t. equations (3.9) and (3.10), and initial } p_0 \text{ and } \mathcal{E}_0 \tag{C.8}$$

Note that the functions d, ρ^u, and ρ^e represent the same decision as in the decentralized economy, but now are also a function of $\mathcal{E}_t^j$. Specifically, as these decisions are made at different subperiods, they are functions of $\mathcal{E}_t^{sep}$, $\mathcal{E}_t^{re}$, and $\mathcal{E}_t^{re}$ respectively (where “sep” stands for the third (separation) subperiod, and ”re” stands for the fourth (reallocation) subperiod). The function $v_{o,t}(p_t,z_t,x_h,\mathcal{E}_t^{mat})$ denotes the number of vacancies posted at time t in a market for occupation o that is characterized by productivity parameters z_t and x_h. As the decision to set a vacancy is made in the matching (fifth) subperiod, the relevant distribution is $\mathcal{E}_t^{mat}$.

The social planner's problem can be written in recursive form. To do so, define operator T^{SP} as follows:

$$\begin{aligned}T^{SP}W^{SP}(\Omega^{prod}) = &\max_{d(\cdot),\rho^u(\cdot),\rho^e(\cdot),v(\cdot)} \Bigg\{ \sum_{o=1}^{O}\sum_{h=1}^{H}\int_{\underline{z}}^{\bar{z}} \Big(e_{o,t}(z,x_h)y(p_t,z,x_h) \\ &+u_{o,t}(z,x_h)b\Big)\, dz + \beta\mathbb{E}_{p',z',x'}\Bigg[-\Bigg(k\sum_{o=1}^{O}\sum_{h=1}^{H}\int_{\underline{z}}^{\bar{z}} v_o(p',z',x'_h,\mathcal{E}^{mat\prime})dz' \\ &+c^e(p')\sum_{o=1}^{O}\sum_{h=1}^{H}\int_{\underline{z}}^{\bar{z}} \rho^e(p',z',x'_h,\mathcal{E}^{re\prime})(1-\psi)(1-d(p',z',x'_h,\mathcal{E}^{sep\prime}))e_o(z',x'_h)dz' \\ &+c^u(p')\sum_{o=1}^{O}\sum_{h=1}^{H}\int_{\underline{z}}^{\bar{z}} \rho^u(p',z',x'_h,\mathcal{E}^{re\prime})u_o(z',x'_h)dz' \Bigg) + W^{SP}(\Omega^{prod\prime}) \Bigg] \Bigg\} \end{aligned} \tag{C.9}$$

Note that the maximization here is still subject to the flow equations (3.9) and (3.10) and the initial conditions for p and $\mathcal{E}$. Furthermore, note that the problem is defined in the last (production) subperiod. As at that point all the reallocation for the period has already taken place, the terms in the expectation refer to u_o and e_o rather than u'_o and e'_o. Finally, it should be noted that one could replace the maximization with respect to $v_o(p,z,x_h,\mathcal{E}^{mat})$ with a maximization with respect to labour market tightness $\theta_o(p,z,x_h,\mathcal{E}^{mat})$, with:

$$\begin{aligned} &v(\cdot) = \theta(\cdot)(1-\rho^u(p,z,x_h,\mathcal{E}^{re}))u_o(z,x_h) = \Psi \text{ for } h \neq 1 \\ &v(\cdot) = \theta(\cdot)\sum_{h=1}^{H}\int_{\underline{z}}^{\bar{z}}(1-d(p,\tilde{z},x_h,\mathcal{E}^{sep}))\rho^e(p,\tilde{z},x_h,\mathcal{E}^{re})(1-\psi)e_o(\tilde{z},x_h)d\tilde{z}dF(z) \\ &\quad + \Psi \text{ for } h = 1 \end{aligned}$$

Defining $u(z,x_h) = \sum_{o=1}^{O} u_o(z,x_h)$ and $e(z,x_h) = \sum_{o=1}^{O} e_o(z,x_h)$, one can now use the fact that the social planner's problem in equation (C.9) is linear in both $u_o(z,x_h)$ and $e_o(z,x_h)$ to argue that then the functions $d(\cdot)$, $\rho^u(\cdot)$, $\rho^e(\cdot)$ and $\theta(\cdot)$ should be independent of $\mathcal{E}_t^j$. The linearity of W^{SP} (and therefore of $T^{SP}W^{SP}$) furthermore implies that the problem can be separated into two parts: one relevant to unemployed workers and one relevant to employed workers. Defining the corresponding values $U(p,z,x_h)$ and $S(p,z,x_h)$, one could thus write

$$W^{SP}(\Omega^{prod}) = \sum_{h=1}^{H}\int_{\underline{z}}^{\bar{z}} \Big[U(p,z,x_h)u(z,x_h) + S(p,z,x_h)e(z,x_h) \Big]\, dz \tag{C.10}$$

Plugging this equation into the recursive formulation of the social planner's problem, equation (C.9), then gives:

$$
\begin{aligned}
T^{SP}W^{SP}(\Omega^{prod}) = & \max_{d(\cdot),\rho^u(\cdot),\rho^e(\cdot),\theta(\cdot)} \Bigg\{ \sum_{h=1}^{H} \int_{\underline{z}}^{\bar{z}} \left(u(z,x_h)b + e(z,x_h)y(p,z,x_h)\right) dz \\
& +\beta \mathbb{E}_{p',z',x'} \Bigg[-c^u(p') \sum_{h=1}^{H} \int_{\underline{z}}^{\bar{z}} \rho^u(p',\tilde{z},x'_h) u(\tilde{z},x_h) d\tilde{z} \\
& -c^e(p') \sum_{h=1}^{H} \int_{\underline{z}}^{\bar{z}} \rho^e(p',\tilde{z},x'_h)(1-\psi)(1-d(p',\tilde{z},x'_h)) e(\tilde{z},x'_h) d\tilde{z} \\
& -k \sum_{h=1}^{H} \int_{\underline{z}}^{\bar{z}} \theta(p',\tilde{z},x'_h) \Bigg[(1-\rho^u(p',\tilde{z},x'_h)) u(\tilde{z},x'_h) \\
& +\mathbb{1}_{h=1} \sum_{\tilde{h}=1}^{H} \int_{\underline{z}}^{\bar{z}} (1-d(p',\ddot{z},x'_{\tilde{h}}))\rho^e(p',\ddot{z},x'_{\tilde{h}})(1-\psi) e(\ddot{z},x'_{\tilde{h}}) d\ddot{z} dF(\tilde{z}) \Bigg] d\tilde{z} \\
& + \sum_{h=1}^{H} \int_{\underline{z}}^{\bar{z}} U(p',\tilde{z},x'_h) u(\tilde{z},x'_h) d\tilde{z} + \sum_{h=1}^{H} \int_{\underline{z}}^{\bar{z}} S(p',\tilde{z},x'_h) e(\tilde{z},x'_h) d\tilde{z} \Bigg] \Bigg\}
\end{aligned}
\tag{C.11}
$$

Again, the maximization here is still subject to the flow equations (3.9) and (3.10) and the initial conditions for p and $\mathcal{E}$. However, I can now use these flow equations so that the equation is only in terms of current values $u(z,x_h)$ and $e(z,x_h)$ (thus essentially plugging in the flow equations for the terms $e(\tilde{z},x'_h)$ and $u(\tilde{z},x'_h)$ on the last line of equation (C.11)). Then, and with some further rearrangement, the equation can be rewritten as the following recursive problem, only subject to initial conditions for p and $\mathcal{E}$:

$$
\begin{aligned}
T^{SP}W^{SP}(\Omega^{prod}) = & \max_{d(\cdot),\rho^u(\cdot),\rho^e(\cdot),\theta(\cdot)} \Bigg\{ \sum_{h=1}^{H} \int_{\underline{z}}^{\bar{z}} b u(z,x_h) dz \\
& + \sum_{h=1}^{H} \int_{\underline{z}}^{\bar{z}} \beta \mathbb{E}_{p',z'} u(z,x_h) \Big[-k\theta(p',z',x_h)(1-\rho^u(p',z',x_h)) \\
& -c^u(p')\rho^u(p',z',x_h) \Big] dz + \sum_{h=1}^{H} \int_{\underline{z}}^{\bar{z}} y(p,z,x_h) e(z,x_h) dz \\
& + \sum_{h=1}^{H} \int_{\underline{z}}^{\bar{z}} \beta \mathbb{E}_{p',z',x'} \Big[-c^e(p')\rho^e(p',z',x'_h)(1-d(p',z',x'_h)) \Big] (1-\psi) e(z,x_h) dz \\
& + \int_{\underline{z}}^{\bar{z}} \beta \mathbb{E}_{p',z'} \Bigg[-k\theta(p',z',x_1) \sum_{\tilde{h}=1}^{H} \int_{\underline{z}}^{\bar{z}} (1-d(p',\tilde{z},x'_{\tilde{h}}))\rho^e(p',\tilde{z},x'_{\tilde{h}})(1-\psi) \\
& \times e(\tilde{z},x'_{\tilde{h}}) d\tilde{z} dF(z) \Bigg] dz + \sum_{h=1}^{H} \int_{\underline{z}}^{\bar{z}} \beta \mathbb{E}_{p',z'} \Bigg[\rho^u(p',z',x_h) \int_{\underline{z}}^{\bar{z}} U(p',\tilde{z},x_1) dF(\tilde{z}) \\
& +(1-\rho^u(p',z',x_h)) \Big[\lambda(\theta(p',z',x_h)) S(p',z',x_h) \\
& +(1-\lambda(\theta(p',z',x_h))) U(p',z',x_h) \Big] \Bigg] u(z,x_h) dz
\end{aligned}
$$

$$
\begin{aligned}
&+\sum_{h=1}^{H}\int_{\underline{z}}^{\bar{z}} \beta\mathbb{E}_{p',z',x'}\left[\psi\int_{\underline{z}}^{\bar{z}}\max\left\{S(p',\tilde{z},x'_h),U(p',\tilde{z},x'_h)\right\}dF(\tilde{z})\right.\\
&+(1-\psi)\Bigg(d(p',z',x'_h)U(p',z',x'_h)\\
&+(1-d(p',z',x'_h))\left[\rho^e(p',z',x')\int_{\underline{z}}^{\bar{z}}\lambda(\theta(p',\tilde{z},x_1))S(p',\tilde{z},x_1)dF(\tilde{z})\right.\\
&\left.\left.+\left(1-\rho^e(p',z',x')\int_{\underline{z}}^{\bar{z}}\lambda(\theta(p',\tilde{z},x_1))dF(\tilde{z})\right)S(p',z',x')\right]\Bigg)\right]e(z,x)dz\Bigg\}
\end{aligned}
\tag{C.12}
$$

Once again using $(\cdot)$ instead of (p', z', x'_h) (where $x'_h = x_h$ for unemployed workers), this equation simplifies as follows:

$$
\begin{aligned}
T^{SP}W^{SP}(\Omega^{prod}) = &\max_{d(\cdot),\rho^u(\cdot),\rho^e(\cdot),\theta(\cdot)}\left\{\sum_{h=1}^{H}\int_{\underline{z}}^{\bar{z}}u(z,x_h)\,(b\right.\\
&+\beta\mathbb{E}_{p',z'}\left[\left(\int_{\underline{z}}^{\bar{z}}U(p',\tilde{z},x_1)dF(\tilde{z})-c^u(p')\right)\rho^u(\cdot)\right.\\
&\left.\left.+(1-\rho^u(\cdot))\left(\lambda(\theta(\cdot))S(\cdot)+(1-\lambda(\cdot))U(\cdot)-k\theta(\cdot)\right)\right]\right)dz\\
&+\sum_{h=1}^{H}\int_{\underline{z}}^{\bar{z}}e(z,x_h)\left(y(p,z,x_h)+\beta\mathbb{E}_{p',z',x'}\left[\psi\int_{\underline{z}}^{\bar{z}}\max\left\{S(p',\tilde{z},x'),U(p',\tilde{z},x')\right\}dF(\tilde{z})\right.\right.\\
&+(1-\psi)\left[d(\cdot)U(\cdot)+(1-d(\cdot))\left[\left(1-\rho^e(\cdot)\int_{\underline{z}}^{\bar{z}}\lambda(\theta(p',\tilde{z},x_1))dF(\tilde{z})\right)S(\cdot)\right.\right.\\
&\left.\left.\left.\left.\left.+\rho^e(\cdot)\left(\int_{\underline{z}}^{\bar{z}}\left[\lambda(\theta(p',\tilde{z},x_1))S(p',\tilde{z},x_1)-k\theta(p',\tilde{z},x_1)\right]dF(\tilde{z})-c^e(p')\right)\right]\right]\right]\right)dz\right\}
\end{aligned}
\tag{C.13}
$$

Given that most functions that are being chosen in this problem only appear in part of the rewritten problem in equation (C.13), this equation can be rewritten as follows:

$$
\begin{aligned}
&T^{SP}W^{SP}(\Omega^{prod}) =\\
&\quad\max_{\theta(\cdot)}\left\{\sum_{h=1}^{H}\int_{\underline{z}}^{\bar{z}}\left[U_{\max}(p,z,x_h)u(z,x_h)+S_{\max}(p,z,x_h)e(z,x_h)\right]dz\right\}
\end{aligned}
\tag{C.14}
$$

Here, $U_{\max}(p, z, x_h)$ and $S_{\max}(p, z, x_h)$ are defined as follows:

$$
\begin{aligned}
U_{\max}(p,z,x_h) = &\max_{d(\cdot),\rho^u(\cdot)}\left\{b+\beta\mathbb{E}_{p',z'}\left[\left(\int_{\underline{z}}^{\bar{z}}U(p',\tilde{z},x_1)dF(\tilde{z})-c^u(p')\right)\rho^u(\cdot)\right.\right.\\
&\left.\left.+(1-\rho^u(\cdot))\left(\lambda(\theta(\cdot))S(\cdot)+(1-\lambda(\cdot))U(\cdot)-k\theta(\cdot)\right)\right]\right\}
\end{aligned}
\tag{C.15}
$$

$$
\begin{aligned}
S_{\max}(p,z,x_h) = &\max_{\rho^e(\cdot)}\left\{y(p,z,x_h)+\beta\mathbb{E}_{p',z',x'}\left[\psi\int_{\underline{z}}^{\bar{z}}\max\left\{S(p',\tilde{z},x'),U(p',\tilde{z},x')\right\}dF(\tilde{z})\right.\right.\\
&+(1-\psi)\left[d(\cdot)U(\cdot)+(1-d(\cdot))\left[\left(1-\rho^e(\cdot)\int_{\underline{z}}^{\bar{z}}\lambda(\theta(p',\tilde{z},x_1))dF(\tilde{z})\right)S(\cdot)\right.\right.\\
&\left.\left.\left.\left.+\rho^e(\cdot)\left(\int_{\underline{z}}^{\bar{z}}\left[\lambda(\theta(p',\tilde{z},x_1))S(p',\tilde{z},x_1)-k\theta(p',\tilde{z},x_1)\right]dF(\tilde{z})-c^e(p')\right)\right]\right]\right]\right\}
\end{aligned}
\tag{C.16}
$$

Note the resemblance between $S_{\max}(p, z, x_h)$ and $T(\Gamma(p, z, x_h, 0))$ (in equation C.2) and between $U_{\max}(p, z, x_h)$ and $T(\Gamma(p, z, x_h, 1))$ (in equation C.3), taking $\hat{\sigma}(\cdot) = d(\cdot)$, and replacing S and U with M and W^U respectively. Nevertheless, if one looks closely at $S_{\max}(p, z, x_h)$ in equation (C.16) and

$T(\Gamma(p, z, x_h, 0))$ in equation (C.2), it can be seen that the two are not exactly identical. After all, in equation (C.2), the maximization for function $\rho^e(\cdot)$ does not take into account all terms in which this function enters. Specifically, this difference means that in a competitive market, the term that is not taken into account when the employed worker makes his reallocation decision is as follows:

$$\left(1 - \rho^e(\cdot) \int_{\underline{z}}^{\bar{z}} \lambda(\theta(p', \tilde{z}, x_1)) dF(\tilde{z})\right) \frac{k\theta(\cdot)}{\lambda(\theta(\cdot))}$$

This term specifically represents the value lost by the current employer if the worker decides to quit his job and change occupations. Looking at equation (C.2), a similar term appears negatively inside the maximization, meaning that if one were to expand the maximization to cover this extra term at the end, it would cancel out and reduce equation (C.2) to equation (C.16). Therefore, the only way to ensure that the social planner's problem (which takes this term into account) and the decentralized problem give the same solution (thus implying that the decentralized problem's solution is constrained efficient) is to place restrictions on the parameters in the extra term that will make it cancel out. Specifically, the extra term can be cancelled out by setting $\lambda(\theta(p', \tilde{z}, x_1))$ for all p and $\tilde{z}$, so that the integral evaluates to zero, or by setting reallocation cost c^e prohibitively high, so that the worker would always set $\rho^e(p, z, x_h) = 0$. □

C.3 Simulation Method

C.3.1 Solution Method

Due to its size and structure, the model presented in Section 3.3 is not analytically solvable. Instead, in order to obtain the results in Section 3.5, I solve the model numerically. The step-by-step procedure followed to obtain the model solution in this paper is described below. It takes as given the values for the parameters $(\beta, O, \chi, \psi, \phi, b, c^u, c^e, \eta, \delta, k, \rho_p, \sigma_p, \sigma_z, z_0, x_2, x_3)$, and results in the equilibrium values for all equilibrium objects (W^U,W^E,J,V,d,ρ^u,ρ^e,σ,θ,w, all functions of p,z, and x). Below, I refer to the number of grid points for p_t, z_t, and x_t as N_p, N_z and N_x. The algorithm closely follows the proof of Proposition 1:

1. In order to obtain the grid for (x, p, z), the process for p_t and z_t needs to be discretized. I do so using Rouwenhorst method, which requires (besides the number of desired grid points for each of these two productivity variables) a value for σ_p (σ_z for z_t), ρ_p (ρ_z for z_t), and μ_p (μ_z for z_t). Of these parameters, μ_p is set equal to 1, and all the others are parameters set in the calibration (with $\mu_z = z_0$), just like the values for x_h ($h \neq 1$).

2. Defining $M \equiv J + W^E$, guess a value for M and W^U, for every triple (p, z, x) on the grid.

3. Using equation (3.3) and the guess for $M(p, z, x)$ and $W^U(p, z, x)$, calculate the value of $J(p, z, x) = \eta M(p, z, x) - \eta W^U(p, z, x)$ and $W^E(p, z, x) = M(p, z, x) - J(p, z, x)$

4. Using the expression for the value function V (equation (3.7)), and using that by the free entry condition $\mathbb{E}_{p'}[V(p', z, x_h)] = 0$, solve for $q(\theta(p, x, z))$, $\theta(p, z, x)$, and $\lambda(\theta(p, z, x)$, using the value of $J(p, x, z)$. Specifically, using the value for $J(p, z, x)$, calculate $q(\theta(p, z, x))$ that will make equation (3.7) equal to zero if $J(p, z, x) > k$, or set $q(\theta(p, z, x)) = 0$ if $J(p, z, x) \leq k$ (Using

k instead of 0 as a value for J in between would imply $q(\theta) > 1$). Then, use the matching function $q(\theta(p,z,x)) = \frac{1}{1+\theta(p,z,x)}$ to back out $\theta(p,z,x)$ and calculate $\lambda(\theta(p,z,x)) = \frac{\theta(p,z,x)}{1+\theta(p,z,x)}$, or set both of these equal to zero (if $J(p,z,x) < k$).

5. Using equation (3.4), and using the obtained values for $W^E(p,z,x)$, $W^U(p,z,x)$ and $\lambda(\theta(p,z,x))$, solve for $\rho^u(p,z,x)$: From equation (3.4), it can be concluded that $\rho^u(p,z,x) = 1$ if $W^U(p,z,x_h) + \lambda(\theta(p,z,x_h))(W^E(p,z,x_h) - W^U(p,z,x_h)) < -c^u + \int_{\underline{z}}^{\bar{z}} W^U(p,\tilde{z},x_1)dF(\tilde{z})$ and $\rho^u(p,z,x) = 0$ otherwise.

6. Use the expression for the value function W^E (equation (3.5)) to solve for $\rho^e(p,z,x)$. From the equation, it can be concluded that $\rho^e(p,z,x) = 1$ if $-c^e + R^E(p,z,x) > W^E(p,z,x)$ (where $R^E(p,z,x)$ is evaluated using equation (3.6)) and $\rho^e(p,z,x) = 0$ otherwise. Simply evaluating this condition will give the desired solution.

7. Using the expression for the value function for J (equation (3.8)) and the values obtained above, solve for $d(p,z,x)$ and $\sigma(p,z,x)$: From equation (3.8), it can be concluded that $\sigma(p,z,x) = 1$ if $J(p,z,x) < V(p,z,x)$ and $\sigma(p,z,x) = \delta$ otherwise. Using this condition (and the free entry condition), conclude that $\sigma(p,z,x)$ in the equation will equal $\delta + (1-\delta)\cdot \mathbb{1}\{J(p,z,x) < 0\}$, which can be calculated given the current guess of $J(p,z,x)$. As it can be argued that the firm will always decide to separate if the worker does (but not necessarily the other way around), set $d(p,z,x) = \sigma(p,z,x)$.[10]

8. Now update $M(p,z,x)$ and $W^U(p,z,x)$ for all triples (p,z,x), using equations (C.2) and (C.3) (from Appendix C.2.1). Unless convergence has been reached, return to step 4.

9. Once convergence is reached, one can obtain the wage $w(p,z,x)$ implied by the solution through either equation (3.5) or (3.8). As the solutions will differ very slightly, I take the average of the two wages for the purpose of the simulation. One can also simplify the laws of motion of u_o and e_o (if desired), by using all the values obtained for the other equilibrium objects and plugging them into the corresponding equations (3.9) and (3.10).

C.3.2 Calibration Method

As mentioned in the main text, the calibration of most parameters involves the estimation of the model counterparts of a set of 26 moments. These parameters are then set to minimize the distance between these moments and the corresponding moments from the data, weighted by the inverse of the standard deviation of the estimated values in the data. In this section, I will describe how the model counterparts of the moments are estimated.

The model counterparts of the targeted moments are all estimated from the simulation that is obtained after solving the model (the steps for which were described in the previous subsection). Specifically, I use the equilibrium solutions for all the relevant equilibrium objects $(d,\rho^u,\rho^e,\sigma,\theta,w)$, as well as parameters $(O,\chi,\psi,\phi,\delta,x_2,x_3)$ and transition matrices (for z and p) to create multiple

[10] This argument holds because the separation decision of the firm is based on the inequality $J(p,z,x) < 0$, while the worker's separation decision is is based on the inequality $W^U > (1-\rho^e(p,z,x))W^E + \rho^e(p,z,x)(-c^e + R^E(p,z,x)) \geq W^E$, where the last inequality is strict if $\rho^e(p,z,x) = 1$. This statement implies that there can be a situation where $J(p,z,x) < 0$ and thus (by Nash bargaining) $W^E(p,z,x) - W^U(p,z,x) < 0$, and yet the worker decides not to separate.

time series that mimic the SIPP in terms of their age and employment distribution in the first period[11] and in terms of their length (which is set to 5 years). In each of these simulations, I follow the timing of the model (described in Section 3.3) in recording the decisions.

As I create a total of $N_{sim} = 15$ such time series, each with a length of $T_{sim} = 240$ periods and $I_{sim} = 2000$ individuals, and each period corresponds to approximately one week[12], I end up with a panel of 30000 individuals, followed over 5 years. This panel will contain information on each individual's age, wage, employment status, occupation, production (if employed), labour market tightness (if unemployed), as well as information on the individual's employment (and occupation) history. Most of the tracked variables follow directly from the model variables, combined with the specific values for p, z, and x that an individual is faced with in a certain period. The only variable that does not immediately follow from variables in the model is the age of the worker, which is tracked starting from the initial age by simply adding on a year every 48 periods, assuming that the group of individuals of a certain age in the first period was spread evenly among weeks (for example, the number of agents aged 23 years and 4 weeks is roughly the same as the number of agents aged 23 years and 32 weeks). Similarly, whenever an agent dies, the newborn agent is assumed to be exactly 23 years old (which was the minimum age in the SIPP). Once an agent turns 62 the simulation of the remainder of her life is no longer relevant to the estimation of the moments (as the estimation from the SIPP had a maximum age of 61). However, the simulation is continued as it is used to determine when a new agent enters the sample.

After obtaining the simulation data, the model counterparts of all 26 moments are estimated. The procedure for each of these moments is described below:

- Average job-finding rate: This particular moment is rather straightforward to estimate from the simulated data. For each month in the simulation (so once every four periods), I take all workers who are unemployed, where workers are unemployed if they spend the entire month in unemployment. The proportion of those workers who are employed again 4 months later is the job-finding rate for that particular period, thus mimicking the procedure followed with the SIPP data, where I compare employment status across waves. The average job-finding rate is then simply the average of all the job-finding rates. Note that this moment thus ignores workers who are employed but decided to search for a job in another occupation. I ignore these workers because this search pattern would not have been observed in the SIPP data either.

- Average proportion of employed workers experiencing at least one unemployment spell in the next year: Just like the moment described above, this moment is straightforward to estimate, using a similar procedure. For each month in the simulation, I take all workers who are employed (for the entire moonth). Then, mimicking the wave structure of the SIPP, I look at the employment status 4, 8, and 12 months ahead. The proportion of interest will then be the proportion of those workers who are unemployed in either of those three periods. The average proportion is then once again simply the average of these period-specific proportions.

[11] The initial distribution used here is that of the fourth month of the combined SIPP of 2004 and 2008 (so that each rotation group of the original SIPP is included). Furthermore, I base the initial distribution of agents over x_1, x_2 and x_3 on age, setting $x = x_1$ if the agent is 35 years old or younger and setting $x = x_3$ if the agent is 49 years old or older.

[12] To be precize, the model period is set to one quarter of a month, so that it is easy to generate monthly statistics, while still keeping the relatively short periods. Furthermore, the simulation used to generate is actually substantially longer than T_{sim} periods, in order to account for differences caused by the initial distribution I impose.

- Persistence and volatility of aggregate productivity: In order to estimate the model counterpart of this moment, I follow the exact same procedure as the one used to obtain this moment from the data. First, I calculate the total output in the economy by summing up the value of $y = pxz$ for all employed workers. Then, the aggregate productivity will be this total, divided by the number of employed workers. Then, to mimic the quarterly data structure of the BLS data, I average this number for every quarter. The persistence and volatility is then obtained by estimating an AR(1) process from the resulting time series.

- Returns to occupational experience (5 and 10 years): In order to estimate the model counterpart, I use a simple OLS regression of the log of the wage of all employed agents in the simulation on their years of occupational experience (counting only years of employment as attributing to experience), thereby mimicking the OLS regressions in Kambourov and Manovskii (2009b) without the additional variables that were included there (such as marital status and age).

- Unemployment rate of unexperienced and experienced workers: I define a worker as unexperienced in this context if her age is 30 or lower, whereas a worker falls in the category of experienced workers if her age is between 35 and 55.[13] The model counterpart of the moment is calculated by first calculating the unemployment rate for these two groups separately each month. Taking the average of these two unemployment rates then gives the two numbers of interest. Note that for purposes of the model, a worker is only defined as unemployed for a month if she is without a job in all 4 weeks, similar to the definition used to calculate the average job finding rate.

- Unemployment survival rates (for 4, 8, and 12 months): The model counterpart of these moments are estimated by taking all workers in the model simulation that are newly unemployed at the start of a certain month of the simulation. The moment of interest is then the proportion of these workers that is still unemployed (for the entire month) after the specified time (4, 8, or 12 months) has passed.

- Occupational mobility rate for unemployed workers (at durations of 1, 3, 6, 9, and 12 months): For these moments, I define a switch to take place once the worker matched with a firm in a different occupation than the one she worked in last. Thus, while switching back and forth between occupations while being unemployed will destroy the agent's human capital in the model, it will not count as occupational switches for the purpose of this moment. After all, I would not have observed these switches in the data either. Specifically, the model counterpart of these moments will be the proportion of workers who were unemployed for at least the specified number of months (1, 3, 6, 9, and 12) and eventually found a job in a different occupation than her previous occupation of employment.

- Subsequent mobility rate: The model counterpart of this model is the reason why I need to keep track of the occupational mobility in the previous (complete) unemployment spell. Following Carrillo-Tudela and Visschers (2021), I estimate the model counterpart directly as the proportion of occupational stayers who again do not switch occupations in their next

[13] Note that this ignores the workers between the age of 56 and 61. However, as one may see workers of this age retiring in the SIPP and my model does not include early retirement, I do not believe this to be a problem.

unemployment spell. Here, "occupational stayers" thus refers to agents who did not switch occupations while being unemployed. Thus, the estimation of the model counterpart of this moment does not take into account any switches that the worker may have made while being employed.

- Relative occupational mobility rate of unexperienced workers (relative to experienced workers): Once again, the occupational mobility here is defined as the proportion of unemployed workers who eventually find a job in a different occupation, thus ignoring occupational transitions without an unemployment spell. Mimicking the structure of the SIPP, the observations of interest will be the first period of a certain month and the period 4 months (16 periods) later.

- Occupational mobility rate for employed workers: Finally, the model counterpart of this moment is estimated by determining the proportion of employed workers who are still employed, but in a different occupation, 4 months later (thus again mimicking the wave structure of the SIPP). This can also be done separately for unexperienced and experienced workers, thus yielding the relative rate for unexperienced workers. Furthermore, using the additional criterion that the worker is still working for the same employer yields the occupational mobility rate for employed workers without an employer change.

- Fraction of occupational transfers going through unemployment: Using the above definitions of occupational transfers for unemployed and employed workers, I can calculate the month-specific fraction of occupational transfers going through unemployment by collecting the number of occupational transfers of each type by simulation month. Taking the average of the resulting fraction across all simulation months then yields the moment value.

- Regressions (3.11) and (3.12): For these regressions, I calculate the monthly unemployment by counting the fraction of workers who are unemployed for the entire month. For equation (3.11), the dependent variable is taken directly from the calculations of the previous moment. For equation (3.12), I calculate the wage differences (which are used as the dependent variable) by comparing the last observed wage before the materialization of the occupational switch to the first wage observed after this materialization. The wage differential used as the dependent variable is then calculated by dividing the new wage by the old wage and averaging the resulting value across all workers whose occupational switch materialized in the simulation month.

C.4 Additional Simulation Results

C.4.1 Further results using the baseline estimation

In this subsection I discuss some further results using the baseline estimation as discussed in sections 3.4 and 3.5 of the main text.

Types of unemployment As mentioned in the main text, all unemployed workers in the model are either rest, search, or reallocation unemployed, depending on where they are located relative to the reallocation and separation thresholds. Therefore, it is possible to split out the (total) unemployment rate into an unemployment rate specific to these types of unemployment. This decomposition is shown in Figure C.34. As can be seen in this figure, no unemployed workers are rest unemployed any value of aggregate productivity p, reflecting that the reallocation threshold for unemployed

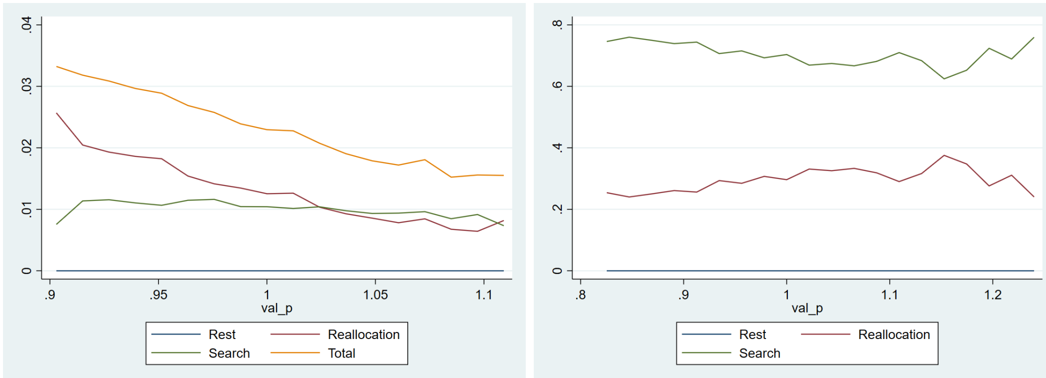


Figure C.34: *The decomposition of the unemployment rate into search, rest, and reallocation unemployment, for different values of aggregate productivity p. Different types of unemployment are plotted either as a fraction of the total population (left) or as a fraction of unemployed workers (right).*

workers is always above the separation threshold. Furthermore, the fraction of unemployed workers classified as reallocation unemployed slightly increases in aggregate productivity. As can be deduced from the left panel, however, this increase is largely caused by a decrease in the number of search unemployed workers as p increases, which in turn is a consequence of the fact that the separation threshold in Figure 3.13 is decreasing in p.

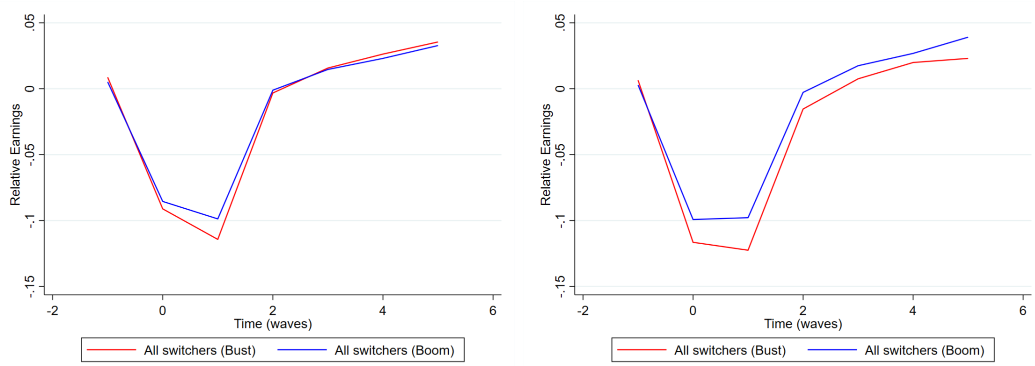


Figure C.35: *The effect of occupational switches on real earnings, relative to the counterfactual of never switching, using estimated coefficients obtained using the three-step estimation method. The plots show separate estimates of the effect for switches materializing during a boom or a bust, and compare the model-based estimates from the data-based definition (left) and a model-based definition of occupational mobility(right).*

When it comes to analyzing the earnings consequences of occupational mobility, both on average and by switcher type, I followed the SIPP-based definition of occupational mobility throughout my analysis in the main text. In other words, I look four months back in the simulation in order to identify occupational switches as well as the type of occupational switch. However, given that the model-based simulation allows me to precisely identify the moment of the transfer at the period (weekly) level, it is not strictly necessary to follow this definition in the model. In figures C.35 and C.36 I show how using a model-based (weekly) definition of occupational mobility alters the results on the earnings consequences of occupational mobility. Comparing the results using the model-based definition (in the right panel of each figure) to the results presented in the main text (replicated in the left panel of each figure), it can be seen that the model-based definition predicts a mildly procyclical earnings loss after occupational mobility, a result which is primarily driven by the level of the earnings loss after an occupational U-switch being lower, while the cyclicality by switch type has not changed substantially. In other words, using the model-based definition seems to strengthen the composition effect in influencing the cyclicality of the average earnings losses after an occupational transfer.

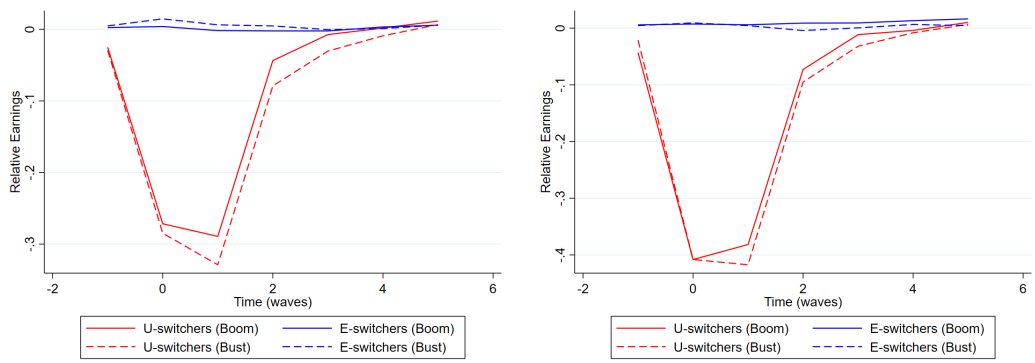


Figure C.36: *The effect of occupational switches on real earnings, relative to the counterfactual of never switching, by type of switch, using estimated coefficients obtained using the three-step estimation method. The plots show separate estimates of the effect for switches materializing during a boom or a bust, and compare the model-based estimates from the data-based definition (left) and a model-based definition of occupational mobility(right).*

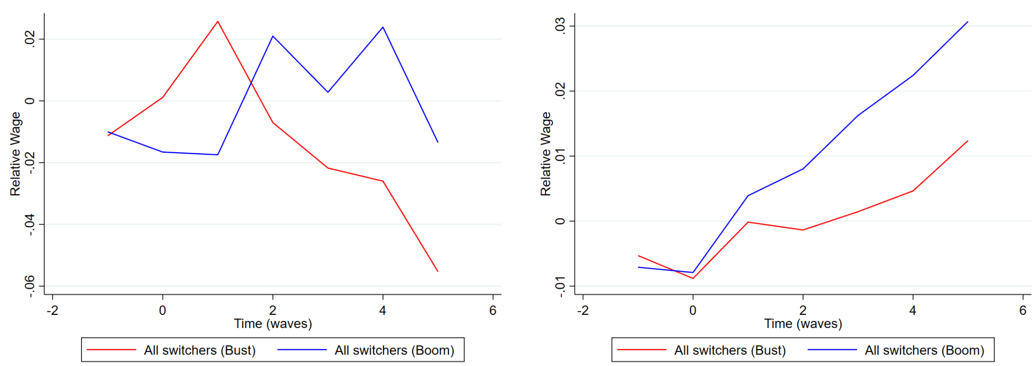


Figure C.37: *The effect of occupational switches on real wages, relative to the counterfactual of never switching, using estimated coefficients obtained using the three-step estimation method. The plots show separate estimates of the effect for switches materializing during a boom or a bust, and compare the estimates from the data (left) and the estimates obtained from model-generated simulation data (right).*

While most of the discussion in section 3.5 focuses on earnings, it is worth noting that section 3.2 also contains results on wages rather than earnings. I did not focus on these results in the model, as the model simplifies this analysis substantially by assuming away the intensive margin of employment. Therefore, the wages in the model are essentially equivalent to the earnings per week, conditional on being employed. Indeed, if I repeat the analysis from figures C.35 and C.36 using wages instead, the results are not very encouraging, as shown in figures C.37 and C.38: Although the cyclicality of the wage paths is not that far off from what I found in the data, the model-based simulation shows a clear upward trend, which is not present in the data.

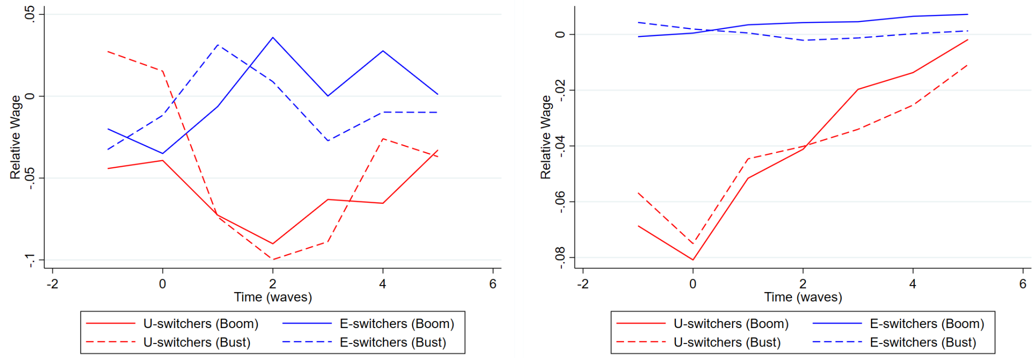


Figure C.38: *The effect of occupational switches on real wages, relative to the counterfactual of never switching, by type of switch, using estimated coefficients obtained using the three-step estimation method. The plots show separate estimates of the effect for switches materializing during a boom or a bust, and compare the estimates from the data (left) and the estimates obtained from model-generated simulation data (right).*

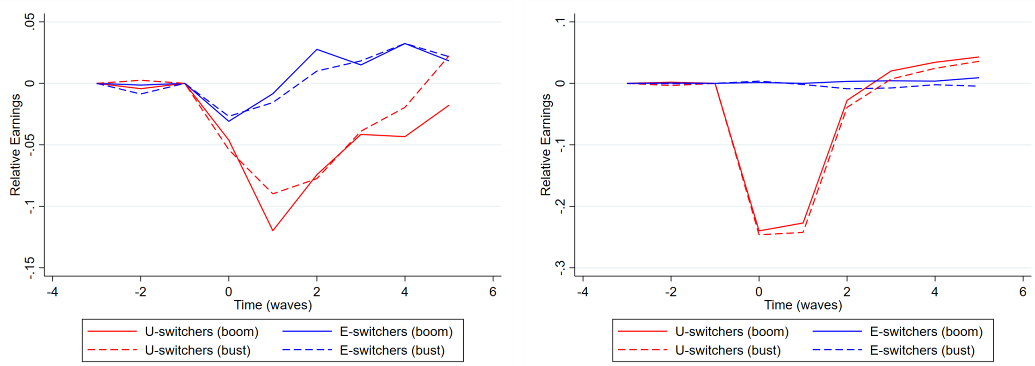


Figure C.39: *The effect of occupational switches on earnings, relative to the control group of never-switching workers, by type of switch, using estimated coefficients from equation* (C.1)*, and comparing the estimates from the data (left) and the estimates obtained from model-generated simulation data (right).*

Finally, figure C.39 repeats the analysis from figure 3.16 using an alternative method. In particular, figure C.39 uses the method from Sun and Abraham (2020) rather than the three-step estimation method from Borusyak et al. (2021), both for the data result and the model-based result. As can be seen in the figure, the data result looks slightly different from it's equivalent in the main text (as also discussed in appendix C.1.2), but the model-based estimation is almost identical to that obtained using the three-step method, thus indicating that the estimation method does not seem to substantially influence the model-based results.

C.4.2 Results using cyclical mobility costs

Moment	**Data**	**Baseline**	$C = p \cdot c$	$C = \bar{c} + p \cdot c$
Average job-finding rate	0.468	0.8358	0.9322	0.915
Average proportion of employed workers experiencing 1+ unemployment spell in the next year	0.051	0.0767	0.05	0.017
Average aggregate productivity	1	1.7205	1.591	1.185
Persistence of aggregate productivity	0.719	0.8878	0.7709	0.9869
Volatility of aggregate productivity	0.009	0.0562	0.0537	0.1039
Returns to occupational experience (5 years)	0.1616	0.138	0.1487	0.0868
Returns to occupational experience (10 years)	0.2526	0.2951	0.3194	0.1811
Unemployment rate of unexperienced workers	0.072	0.0443	0.0273	0.021
Unemployment rate of experienced workers	0.049	0.0049	0.0025	0.0017
Unemployment survival rate (4 months)	0.560	0.1647	0.0676	0.0852
Unemployment survival rate (8 months)	0.387	0.0307	0.0056	0.008
Unemployment survival rate (12 months)	0.295	0.0064	0.0007	0.0008
Occupational mobility rate for workers unemployed for at least 1 month	0.431	0.5285	0.7484	0.5561
Occupational mobility rate for workers unemployed for at least 3 months	0.473	0.8207	0.8613	0.6557
Occupational mobility rate for workers unemployed for at least 6 months	0.474	0.8475	0.875	0.6818
Occupational mobility rate for workers unemployed for at least 9 months	0.473	0.8659	0.7857	0.5625
Occupational mobility rate for workers unemployed for at least 12 months	0.470	0.875	0.5	0.75
Subsequent mobility rate	0.741	0.946	0.8667	0.7465
Relative occupational mobility rate of unexperienced workers	1.077	1.9808	1.2527	1.8726
Occupational mobility rate for employed workers	0.036	0.0349	0.0373	0.0543
Relative occupational mobility rate for unexperienced employed workers	2.156	1.3295	1.3276	1.0924
Occupational mobility rate for employed workers without employer change	0.011	0.029	0.0319	0.0511
Fraction of occupational transfers going through unemployment	0.175	0.1161	0.1828	0.0336
Coefficient $\hat{\gamma}$ in equation (3.11)	2.13	2.0414	2.0452	1.979
Coefficient $\hat{\gamma}$ in equation (3.12), E-switchers	-0.001	0.3139	0.0957	0.136
Coefficient $\hat{\gamma}$ in equation (3.12), U-switchers	0.015	0.3546	0.0693	-0.113

Table C.15: *The moments targeted in the calibration, and their model counterparts in the baseline model from the main text, as well as two alternative calibrations that allow for dependence of the reallocation costs on aggregate productivity p.*

In the baseline estimation of the model in the main text of chapter 3, I assumed the cost of reallocation, either through unemployment or while staying on the job, to be constant in aggregate productivity. This assumption was driven by the fact that there does not exist a lot of evidence in the literature on the magnitude of such costs. The little evidence that has been provided to date, such as the calculations in Lalé (2017), are generally focusing on estimating an average cost of reallocation, and do not make any statements regarding the cyclicality of such costs. However, it is not unreasonable to expect that these costs could have a cyclical component to them, especially if they are driven by foregone earnings. For this reason, I allowed the costs in the model section 3.3 to depend on aggregate productivity. In this subsection, I will investigate how the estimation of the model in section 3.4 as well as the subsequent results in section 3.5 change when allowing for this cyclicality in costs.

In table C.15, I show the results of two alternative estimations, compared to the baseline estimation. In particular, I consider two simple functions for the reallocation costs. In both of these functions, the costs depend linearly on the aggregate productivity, with the slope being one of the parameters to be estimated in the calibration exercise. However, in one version I additionally allow for a acyclical component of the reallocation cost. Note that I do not add any additional moments to the calibration exercise, and therefore it is not entirely unexpected that this second alternative calibration does not perform as well, since it adds two additional parameters to the model.

By comparing the columns in table C.15, it can be seen that both alternative calibrations are performing worse than the baseline model when it comes to the transitions out of unemployment. Both alternative calibrations severely overshoot the job finding rate, and as a result underestimate the unemployment rate more than the baseline model did. The second alternative calibration does better than the baseline when it comes to the rate of occupational mobility through unemployment, but does worse than the baseline (and the first alternative) for occupational mobility through employment. Nevertheless, both alternative calibrations are able to match the countercyclical fraction of occupational switches going through unemployment, just like the baseline model.

Parameter	**Baseline**	$C = p \cdot c$	$C = \bar{c} + p \cdot c$
σ_p	0.037	0.037	0.067
σ_z	0.051	0.058	0.041
ρ_p	0.986	0.967	0.996
ρ_z	0.987	0.981	0.995
μ_z	1.036	0.951	0.716
k	1.579	0.913	1.937
$\bar{c^u}$	-0.691	-	0.57
c^u	-	-0.536	0.224
$\bar{c^e}$	0.258	-	0.26
c^e	-	1.148	0.149
x_2	1.39	1.258	1.522
x_3	1.883	1.954	1.856
ψ	0.002	0.0005	0.0034
δ	0.0037	0.0017	0.0004

Table C.16: *Values of parameters used to obtain the results in Section 3.5, and their counterparts from the two alternative calibrations that allow for the reallocation costs to depend on aggregate productivity p.*

Table C.16 compares the parameter estimates from both alternative calibrations to the baseline estimates used to obtain the results in section 3.5. Naturally, the first set of parameters of interest when comparing the three calibrations concern the reallocation costs. As mentioned in section 3.4, one value that stood out in the baseline calibration was the cost of reallocation through unemployment, which was estimated to be negative. This is also the case in the first alternative calibration, which furthermore estimates the cost to be more negative in booms than in recessions. In the second alternative calibration, however, the pattern reverses, with the cost generally being positive and increasing in aggregate productivity. Furthermore, this second alternative calibration estimates the cost of reallocation through unemployment to be higher than the cost of job-to-job occupational transfers. Aside from these key differences, it can be noted that the second alternative calibration seems to suggest that aggregate productivity is more volatile than idiosyncratic productivity, and both alternatives (as well as the baseline) suggest a very low value for the exogenous separation rate δ.

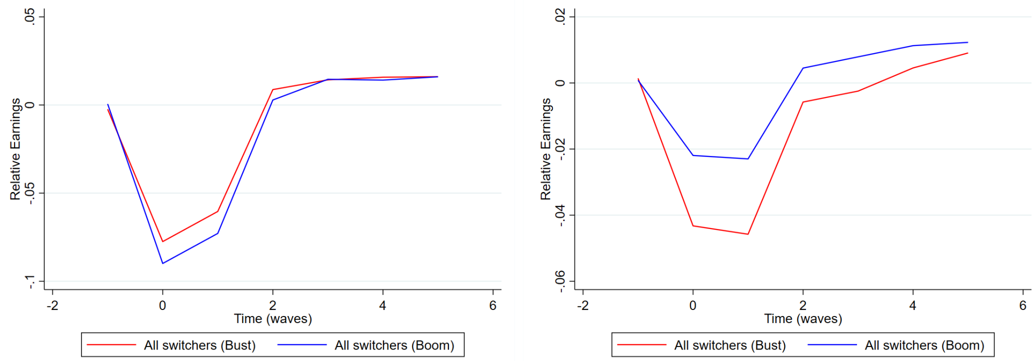


Figure C.40: *The effect of occupational switches on real earnings, relative to the counterfactual of never switching, using estimated coefficients obtained using the three-step estimation method. The plots show separate estimates of the effect for switches materializing during a boom or a bust, and compare the estimates from two alternative calibrations, using* $C = p \cdot c$ *(left) or* $C = \bar{c} + p \cdot c$ *(right).*

In figure C.40, I show how the two alternative calibrations perform when it comes to the average earnings consequences of occupational mobility. Comparing the figure to figure 3.15 in the main text, it can be seen that the second alternative calibration does not match the data very well, suggesting a strongly procyclical pattern (though performing better than the baseline model when it comes to the levels), whereas the first alternative calibration performs fairly similar to the baseline model.
A similar conclusion can be reached when comparing the two alternative calibrations and the baseline model for earnings consequences specific to E-switchers and U-switchers. As figure C.41 shows, the second alternative calibration suggests strongly countercyclical earnings consequences of U-switches, whereas the first alternative calibration stays closer to the baseline model in terms of cyclicality, and both alternative calibrations match the levels of the earnings losses to a similar extent as the baseline model.

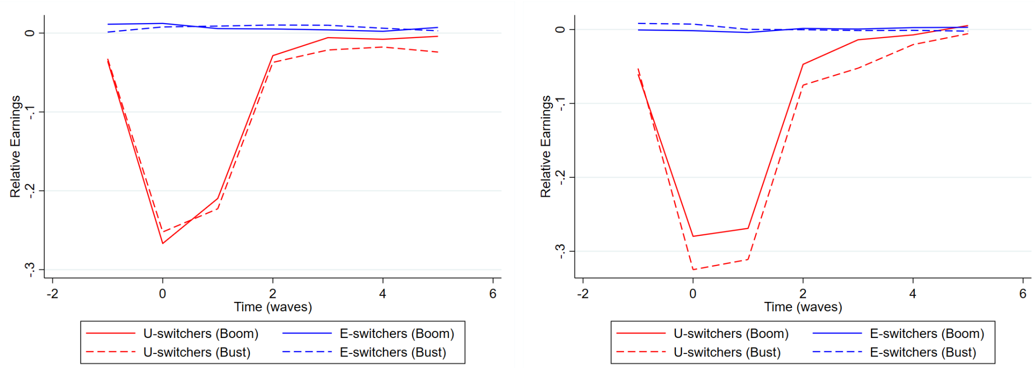


Figure C.41: *The effect of occupational switches on real earnings, relative to the counterfactual of never switching, by type of switch, using estimated coefficients obtained using the three-step estimation method. The plots show separate estimates of the effect for switches materializing during a boom or a bust, and compare the estimates from two alternative calibrations, using* $C = p \cdot c$ *(left) or* $C = \bar{c} + p \cdot c$ *(right).*

In figure C.42, the counterpart of figure 3.17 in the main text, I show the impact of forcing

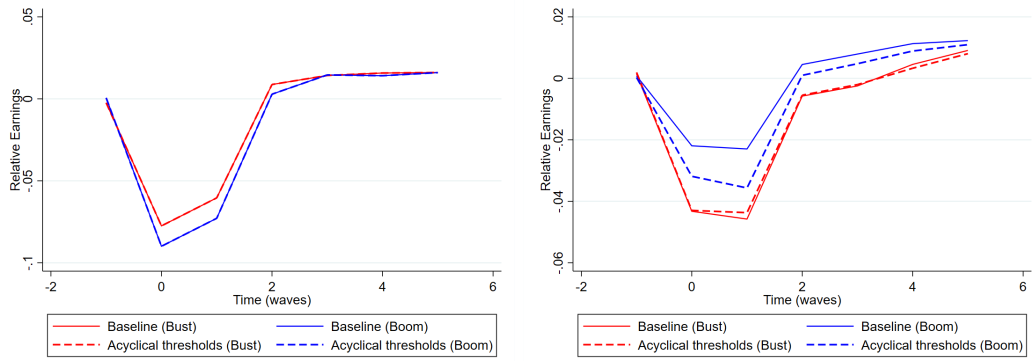


Figure C.42: *The impact of cyclical reallocation thresholds on the model-generated (BJS) effect of occupational switches on real earnings, relative to the counterfactual of never switching, and aggregated separately for switches materializing during a boom or a bust. The figure combines the estimates obtained from the baseline model (solid) and a model in which separation and reallocation thresholds are forced to be constant in aggregate productivity (dashed), and compare the estimates from two alternative calibrations, using $C = p \cdot c$ (left) or $C = \bar{c} + p \cdot c$ (right).*

reallocation and separation thresholds to be acyclical on the average earnings consequences of occupational mobility in the alternative calibrations. As can be seen in the left panel of figure C.42, the effect of taking out the cyclical reallocation and separation thresholds completely disappears under the first alternative calibration. This is because, as mentioned before, the job-to-job reallocation in this version of the model is coming purely from the exogenous reallocation shock, which was already acyclical, whereas the separation threshold is very flat in this estimation, and thereby did not change much when forcing it to be completely horizontal. The right panel of figure C.42, on the other hand, shows that shutting down the cyclicality of these thresholds in the second alternative calibration deteriorates the outcomes for workers who switch occupations in booms, but not for workers who switch in a bust. In many ways, the reason for this is similar to the reasons mentioned in the main text for the baseline model: in this second alternative calibration, the threshold for job-to-job occupational mobility jumps up at a value of p slightly above 1 (rather than slightly below 1 as in the baseline model).
In figure C.43, I repeat the analysis from figure 3.18 in the main text for the two alternative calibrations. Just like in the main text, it can be observed that not including either type of job-to-job mobility leads to a much larger average earnings loss after an occupational transfer.

Finally, figure C.44 shows the results of the decomposition of the cyclicality of the average earnings consequences of occupational transitions into the two types of job-to-job mobility and a residual component. As mentioned above, all job-to-job transitions in the first alternative calibration occur through the exogenous reallocation shock, thus leaving no contribution whatsoever to the endogenous choice of job-to-job mobility in this alternative. The decomposition of the second calibration, on the other hand, is fairly close to the one discussed in the main text, with a generally positive role for the endogenous choice of job-to-job occupational mobility, and a negative (countercyclical) force coming from the exogenous reallocation shock. This therefore reinforces the need for better understanding these reallocations within the firm, as concluded in the main text.

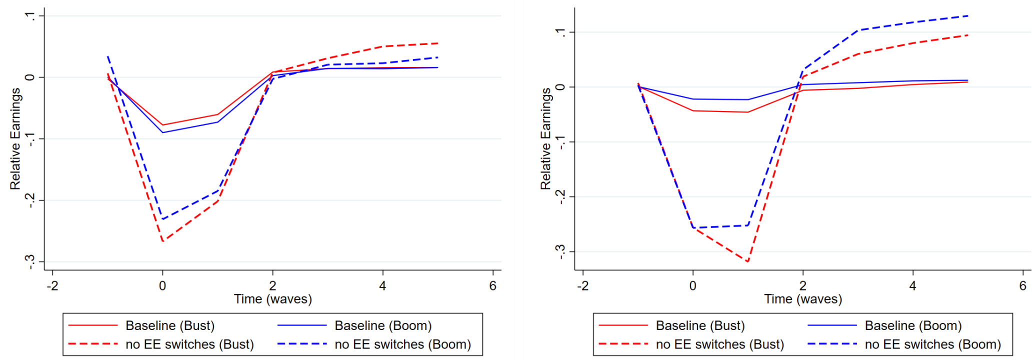


Figure C.43: *The impact of including job-to-job transitions on the model-generated (BJS) effect of occupational switches on real earnings, relative to the counterfactual of never switching, and aggregated separately for switches materializing during a boom or a bust. The figure combines the estimates obtained from the baseline model (solid) and a model without either type of job-to-job occupational transfers (dashed), and compare the estimates from two alternative calibrations, using $C = p \cdot c$ (left) or $C = \bar{c} + p \cdot c$ (right).*

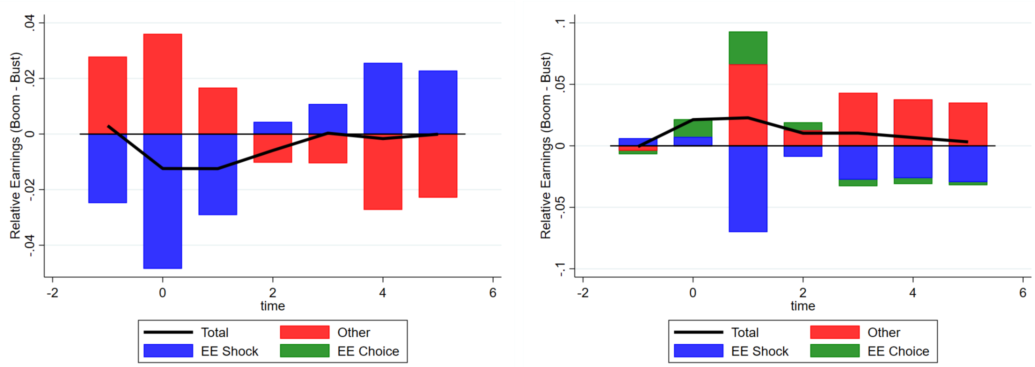


Figure C.44: *The impact of including job-to-job transitions on the model-generated (BJS) cyclicality in the effect of occupational switches on real earnings. The figure displays the separate impact of the occupational transfer shock and job-to-job occupational transfer choices, obtained using a Shapley-Shorrocks decomposition, using alternative calibrations with either $C = p \cdot c$ (left) or $C = \bar{c} + p \cdot c$ (right).*

Bibliography

Acemoglu, D. and Autor, D. (2011). Skills, tasks and technologies: Implications for employment and earnings. In Ashenfelter, O. and Card, D., editors, *Handbook of Labor Economics*, chapter 12, pages 1043–1171. Elsevier.

Alvarez, F. and Shimer, R. (2011). Search and rest unemployment. *Econometrica*, 79(1):75–122.

Bagger, J., Fontaine, F., Postel-Vinay, F., and Robin, J.-M. (2014). Tenure, experience, human capital, and wages: A tractable equilibrium search model of wage dynamics. *American Economic Review*, 104(6):1551–1596.

Berg, P., Hamman, M. K., Piszczek, M., and Ruhm, C. J. (2015). Can policy facilitate partial retirement? Evidence from Germany. Working Paper 21478, National Bureau of Economic Research.

Bonikowska, A. and Morissette, R. (2012). Earnings losses of displaced workers with stable labour market attachment: Recent evidence from Canada. Research Paper 346, Statistics Canada.

Borusyak, K., Jaravel, X., and Spiess, J. (2021). Revisiting event study designs: Robust and efficient estimation. Working Paper.

Burda, M. C. and Mertens, A. (2001). Estimating wage losses of displaced workers in Germany. *Labour Economics*, 8:15–41.

Burdett, K., Carrillo-Tudela, C., and Coles, M. (2020). The cost of job loss. *Review of Economic Studies*, 87:1757–1798.

Callaway, B. and Sant'Anna, P. H. C. (2020). Difference-in-differences with multiple time periods. Working Paper.

Carrillo-Tudela, C., Hobijn, B., She, P., and Visschers, L. (2016). The extent and cyclicality of career changes: Evidence for the U.K. *European Economic Review*, 84:18–41.

Carrillo-Tudela, C., Hobijn, B., and Visschers, L. (2014). Career changes decline during recessions. Economic Letter 2014-09, Federal Reserve Bank of San Francisco.

Carrillo-Tudela, C. and Visschers, L. (2021). Unemployment and endogenous reallocation over the business cycle. Working Paper.

Carrillo-Tudela, C., Visschers, L., and Wiczer, D. (2021). Cyclical earnings and employment transitions. Working Paper.

Coles, M. G. and Smith, E. (1998). Marketplaces and matching. *International Economic Review*, 39(1):239–254.

Couch, K. A. and Placzek, D. W. (2010). Earnings losses of displaced workers revisited. *The American Economic Review*, 100(1):572–589.

Davern, M., Rockwood, T. H., Sherrod, R., and Campbell, S. (2003). Prepaid monetary incentives and data quality in face-to-face interviews: Data from the 1996 survey of income and program participation incentive experiment. *The Public Opinion Quarterly*, 67(1):139–147.

Davis, S. J. and Von Wachter, T. (2011). Recessions and the costs of job loss. Brookings Papers on Economic Activity.

Deelen, A., De Graaf-Zijl, M., and Van Den Berge, W. (2018). Labour market effects of job displacement for prime-age and older workers. *IZA Journal of Labor Economics*, 7(3):1–30.

Dorn, D. (2009). Essays on inequality, spatial interaction, and the demand for skills. Dissertation no. 3613, University of St. Gallen.

Faberman, R. J. and Kudlyak, M. (2017). The intensity of job search and search duration. Working Paper.

Feldstein, M. (1976). Temporary layoffs in the theory of unemployment. *Journal of Political Economy*, 84(5):937–958.

Forsythe, E. (2020). Occupational job ladders and displaced workers. Working Paper.

Forsythe, E., Kahn, L. B., Lange, F., and Wiczer, D. G. (2020). Searching, recalls, and tightness: An interim report on the covid labor market. Working Paper 28083, National Bureau of Economic Research.

Fujita, S. and Moscarini, G. (2017). Recall and unemployment. *American Economic Review*, 107(12):3875–3916.

Gallant, J., Kroft, K., Lange, F., and Notowidigdo, M. J. (2020). Temporary unemployment and labor market dynamics during the covid-19 recession. Working Paper 27924, National Bureau of Economic Research.

Gourieroux, C., Montfort, A., and Renault, E. (1993). Indirect inference. *Journal of Applied Econometrics*, 8(S1):S85–S118.

Gregory, V. (2021). Firms as learning environments: Implications for earnings dynamics and job search. Working Paper.

Gregory, V., Menzio, G., and Wiczer, D. G. (2020). Pandemic recession: L or v-shaped? *Quarterly Review*, 40(1).

Gregory, V., Menzio, G., and Wiczer, D. G. (2021). The alpha beta gamma of the labor market. Working Paper 2021-003, Federal Reserve Bank of St. Louis.

Gulyas, A. and Pytka, K. (2020). Understanding the sources of earnings losses after job displacement: A machine-learning approach. Working Paper.

Guvenen, F., Karahan, F., Ozkan, S., and Song, J. (2017). Heterogeneous scarring effects of full-year nonemployment. *American Economic Review: Papers & Proceedings*, 107(5):369–373.

Gyetvai, A. (2021). Job mobility within and across occupations. Working Paper.

Hall, R. E. and Kudlyak, M. (2020). Unemployed with jobs and without jobs. Working Paper 27886, National Bureau of Economic Research.

Haltiwanger, J., Jarmin, R. S., and Miranda, J. (2013). Who creates jobs? Small versus large versus young. *The Review of Economics and Statistis*, 95(2):347–361.

Hansen, B. E. (2021). *Econometrics*. Manuscript, pages 626-627.

Hethey, T. and Schmieder, J. F. (2010). Using worker flows in the analysis of establishment turover - evidence from German administrative data. FDZ-Methodenreport 06/2010 EN, Research Data Centre (FDZ) of the German Federal Employment Agency (BA) at the Institute for Employment Research (IAB).

Hijzen, A., Upward, R., and Wright, P. W. (2010). The income losses of displaced workers. *The Journal of Human Resources*, 45(1):243–269.

Huckfeldt, C. (2016). Understanding the scarring effect of recessions. Working Paper.

Jacobson, L. S., LaLonde, R. J., and Sullivan, D. G. (1993). Earnings losses of displaced workers. *The American Economic Review*, 83(4):685–709.

James, T. (1997). Results of the wave 1 incentive experiment in the 1996 survey of income and program participation. *Proceedings of the Survey Research Section of the American Statistical Association*, pages 834–39.

Jarosch, G. (2021). Searching for job security and the consequences of job loss. Working Paper 28481, National Bureau of Economic Research.

Jung, P. and Kuhn, M. (2019). Earnings losses and labor mobility over the life cycle. *Journal of the European Economic Association*, 17(3):678–724.

Kambourov, G. and Manovskii, I. (2008). Rising occupational and industry mobility in the United States: 1968-97. *International Economic Review*, 49(1):41–79.

Kambourov, G. and Manovskii, I. (2009a). Occupational mobility and wage inequality. *Review of Economic Studies*, 76(2):731–759.

Kambourov, G. and Manovskii, I. (2009b). Occupational specificity of human capital. *International Economic Review*, 50(1):63–114.

Katz, L. F. (1986). Layoffs, recall and the duration of unemployment. Working Paper 1825, National Bureau of Economic Research.

Katz, L. F. and Meyer, B. D. (1990). Unemployment insurance, recall expectations, and unemployment outcomes. *The Quarterly Journal of Economics*, 105(4):973–1002.

Kletzer, L. G. (1998). Job displacement. *Journal of Economic Perspectives*, 12(1):115–136.

Kroft, K., Lange, F., Notowidigdo, M. J., and Katz, L. F. (2016). Long-term unemployment and the great recession: The role of composition, duration dependence, and nonparticipation. *Journal of Labor Economics*, 34(S1):S7–S54.

Krolikowski, P. (2017). Job ladders and earnings of displaced workers. *American Economic Journal: Macroeconomics*, 9(2):1–31.

Lachowska, M., Mas, A., and Woodbury, S. A. (2020). Sources of displaced workers' long-term earnings losses. *American Economic Review*, 110(10):3231–3266.

Lalé, E. (2017). Worker reallocation across occupations: Confronting data with theory. *Labour Economics*, 44(1):51–68.

Longhi, S. and Taylor, M. (2013). Occupational change and mobility among employed and unemployed job seekers. *Scottish Journal of Political Economy*, 60(1):71–100.

Lucas, R. E. J. and Prescott, E. C. (1974). Equilibrium search and unemployment. *Journal of Economic Theory*, 7:188–209.

Mavromaras, K. G. and Rudolph, H. (1998). Temporary separations and firm size in the german labour market. *Oxford Bulletin of Economics and Statistics*, 60:215–225.

Menzio, G. and Shi, S. (2011). Efficient search on the job and the business cycle. *Journal of Political Economy*, 119:468–510.

Moscarini, G. and Postel-Vinay, F. (2017). The relative power of employment-to-employment reallocation and unemployment exits in predicting wage growth. *American Economic Review Papers and Proceedings*, 107(5):364–368.

Nedelkoska, L., Neffke, F., and Wiederhold, S. (2015). Skill mismatch and the costs of job displacement. Conference paper, CESifo.

Nekoei, A. and Weber, A. (2015). Recall expectations and duration dependence. *American Economic Review: Papers & Proceedings*, 105(5):142–146.

Nekoei, A. and Weber, A. (2020). Seven facts about temporary layoffs. Working Paper.

OECD (2020). Net replacement rates in unemployment. https://stats.oecd.org/Index.aspx?DataSetCode=NRR (accessed August 18, 2020).

Papageorgiou, T. (2018). Large firms and within firm occupational reallocation. *Journal of Economic Theory*, 174:184–223.

Pilossoph, L. (2022). Sectoral shocks and mismatch unemployment. Working Paper.

Pissarides, C. A. (1982). Job search and the duration of layoff unemployment. *The Quarterly Journal of Economics*, 97:595–612.

Pissarides, C. A. (2000). *Equilibrium Unemployment Theory*. Oxford University Press.

Pries, M. J. (2004). Persistence of employment fluctuations: A model of recurring job loss. *The Review of Economic Studies*, 71(1):193–215.

Raposo, P. S., Portugal, P., and Carneiro, A. (2019). The sources of the wage losses of displaced workers: the role of the reallocation of workers into firms, matches, and job titles. *Journal of Human Resources*, pages 0317–8667R3.

Schmieder, J. F., von Wachter, T., and Heining, J. (2020). The costs of job displacement over the business cycle and its sources: Evidence from Germany. Working Paper.

Shimer, R. (2007). Mismatch. *The American Economic Review*, 97(4):1074–1101.

Shorrocks, A. F. (2013). Decomposition procedures for distributional analysis: a unified framework based on the shapley value. *The Journal of Economic Inequality*, 11:99–126.

Stevens, A. H. (1997). Persistent effects of job displacement: The importance of multiple job losses. *Journal of Labor Economics*, 15(1):165–188.

Stokey, N., Lucas, R. E., and Prescott, E. C. (1989). *Recursive Methods in Economic Dynamics*. Harvard University Press.

Sun, L. and Abraham, S. (2020). Estimating dynamic treatment effects in event studies with heterogeneous treatment effects. Working Paper.

Topel, R. H. and Ward, M. P. (1992). Job mobility and the careers of young men. *The Quarterly Journal of Economics*, 107(2):439–479.

U.S. Census Bureau (2001). *Survey of Income and Program Participation Users' Guide*, 3 edition.

Von Wachter, T., Song, J., and Manchester, J. (2009). Long-term earnings losses due to mass layoffs during the 1982 recession: An analysis using u.s. administrative data from 1974 to 2004. IZA/CEPR 11th European Summer Symposium in Labour Economics.

Xiong, H. (2008). The U.S. occupational mobility from 1988 to 2003: Evidence from SIPP. Working Paper.

Xu, M. (2017). Understanding the decline in occupational mobility. Working Paper, University of Minnesota.