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Parag Mahajan

University of Delaware

Dhiren Patki

Federal Reserve Bank of Boston

Heiko Stüber

UALS, IAB, FAU and IZA

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IZA – Institute of Labor Economics

Schaumburg-Lippe-Straße 5–9
53113 Bonn, Germany

Phone: +49-228-3894-0
Email: publications@iza.org

www.iza.org

ABSTRACT

Bad Times, Bad Jobs? How Recessions Affect Early Career Trajectories*

Workers who enter the labor market during recessions experience lasting earnings losses, but the role of non-pay amenities in exacerbating or counteracting these losses remains unknown. Using population-scale data from Germany, we find that labor market entry during recessions generates a 5 percent reduction in earnings cumulated over the first decade of experience. Implementing a revealed-preference estimator of employer quality that aggregates information from the universe of worker moves across employers, we find that 17 percent of recession-induced earnings losses are compensated by non-pay amenities. Purely pecuniary estimates can therefore overstate the welfare costs of labor market entry during recessions.

JEL Classification: E32, J24, J31, J32

Keywords: earnings inequality, recessions, non-pay amenities

Corresponding author:

Heiko Stüber
Friedrich-Alexander-Universität Erlangen-Nürnberg (FAU)
Schloßplatz 4
91054 Erlangen
Germany
E-mail: Heiko.Stueber@fau.de

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1 Introduction

Completing school and entering the labor market is a critical moment in the lives of young workers. It marks the start of a period of exploration when workers begin their first jobs, seek financial independence, and embark on new careers. It is also the moment that first exposes young workers to the large and unpredictable risk of business cycle fluctuations. Being lucky enough to start one's career in good times makes the transition to well-paying jobs and desirable careers easier, whereas the misfortune of entering during bad times makes well-paying jobs hard to find and stunts career development.¹

Previous studies find that exposure to adverse aggregate conditions at the time of labor market entry generates an earnings penalty that is both substantial and persistent (see, for example, [Kahn, 2010](#) and [Altonji et al., 2016](#) for evidence from the United States and [Oreopoulos et al., 2012](#) for evidence from Canada). These earnings penalties arise because the availability of high-wage jobs is strongly procyclical (see, for example, [Okun, 1973](#) and [McLaughlin and Bils, 2001](#)); however, the translation of short-term fluctuations in wages into long-term scarring effects is a product of several different factors. First, search frictions can hinder the movement of workers between employers, thereby extending the duration of recession-induced losses. Second, when these frictions increase with tenure, they can generate long-term human capital mismatch and reduced skill development if recession entrants do not find work in jobs, occupations, and industries in which they have already specialized (see, for example, [Oreopoulos et al., 2012](#) and [Arellano-Bover, 2022](#)). Third, employers may be slow to learn about the true quality of recession entrants relative to expansion entrants because of greater initial mismatch. Finally, the fact that much of the labor market is characterized by long-term wage setting rather than a spot market slows down the convergence between recession entrants and expansion entrants (see, for example, [Beaudry and DiNardo, 1991](#)). [Guvenen et al. \(2022\)](#) provide evidence of the far-reaching consequences of these shocks by showing that much of the cross-cohort lifetime earnings inequality for men in the United States can be explained by earnings losses experienced early on in workers' careers. Their findings imply that income inequality is partially rooted in early career events.

Despite the well-developed findings on the earnings consequences of labor market entry during bad times, almost nothing is known about how recessions affect job quality beyond purely pecuniary dimensions. Do recession-affected entrants work in jobs that are worse both in terms of pay and in terms of non-pay features? Or alternatively, if non-pay amenities are priced in the labor market as compensating differentials as in [Rosen \(1986\)](#), do they offset some of the pecuniary losses that recessionary entrants experience? Answering these questions is critical from a welfare perspective since pay is an incomplete proxy for utility. This is especially true in light of a new and growing body of research that documents the importance of non-pay amenities that workers value in lieu of

¹News headlines from different stages of the business cycle highlight this risk. For example, during the Great Recession: Catherine Rampell, "Many With New College Degree Find the Job Market Humbling," *New York Times*, May 8, 2011. During the strong labor market of 2016: Ben Casselman, "This Year's College Grads Are the Luckiest in a Decade," *FiveThirtyEight*, May 6, 2016. In the wake of the pandemic recession: Miguel Badia, "College Graduates Are Struggling to Make Up for the 'Lost Year' Created by the Coronavirus Pandemic," *CNBC*, July 26, 2021.

pay. For example, [Mas and Pallais \(2017\)](#) use a field experiment to study the value of scheduling flexibility, [Wiswall and Zafar \(2018\)](#) and [Maestas et al. \(2023\)](#) use survey data and stated preference experiments to study workers’ willingness to pay for a wide range of non-pay amenities, and [Sorkin \(2018\)](#) and [Taber and Vejlin \(2020\)](#) exploit worker flows in large-scale administrative data to infer the value of non-pay amenities.

We study the pecuniary and non-pecuniary impact of graduating into a recession in three steps. First, we estimate the utility posting job search model developed in [Sorkin \(2018\)](#) exploiting over 180 million worker moves in population-level German matched employer-employee administrative data. Our estimates reveal that compensating differentials account for a share of employer-specific pay variation in Germany comparable to the share that [Sorkin \(2018\)](#) finds for the United States. The parallels between German and U.S. data extend even to the pattern of inter-sector earnings differences attributable to compensating differentials, suggesting that the model captures important aspects of the nature of working conditions in advanced economies. Next, we exploit the regulated timing of the German vocational training system to identify the causal effect of the unemployment rate at labor market entry on the earnings trajectories of millions of new graduates. Finally, we use detailed measures of compensation to examine the role of different features of pay in the earnings of workers who enter in good versus bad times. We find that the typical recession causes entrants to experience a 5 percent loss in earnings cumulated over the first 10 years of their careers. The core result of our paper is that 17 percent of this loss is compensated for by employer-specific non-pay amenities, which indicates that purely pecuniary comparisons overstate the welfare cost of labor market entry during recessions. Looking beyond the early career stage, we find that recessionary entrants experience earnings losses that persist well into the second decade of labor market experience, although employers play a negligible role in these mid-career losses.

Our paper makes four contributions to the literature. First, we study the effect of cyclical shocks on early career earnings trajectories in Germany, which is a country with strong active labor market programs (ALMPs) for employment and re-training. Despite the broad availability and potentially useful role of these policy tools in mitigating the impact of cyclical shocks, our estimates of earnings penalties in Germany are comparable to those found by [Oreopoulos et al. \(2012\)](#) in the context of Canada, where ALMPs are less widespread.² Our estimates therefore suggest that worker-focused policy interventions do not easily neutralize the recession-induced earnings losses that labor market entrants experience.

While previous studies find that employer and occupational characteristics play a role in explaining recession-induced penalties (see, for example, [Oyer, 2006](#), [Oyer, 2008](#), [Oreopoulos et al., 2012](#), and [Rinz, 2020](#)), the precise magnitude of employer effects’ role in generating scars for recessionary entrants remains unknown. Our second contribution is to provide the first quantitative estimate

²[Oreopoulos et al. \(2012\)](#) also find that a typical recession results in a 5 percent loss of earnings cumulated over 10 years for Canadian college graduates. Estimates of recession-induced losses for labor market entrants in the United States are somewhat larger. [Kahn \(2010\)](#) predicts that the 1982 (recession) college graduate cohort earned 20 percent less than the 1989 (expansion) cohort in the first 17 years of their careers. We find that cumulated losses of 4.2 percent over the first 20 years of a career during a typical German recession.

of the relative importance of employer-specific factors in driving the recession-induced earnings penalty. To separate employer-specific factors from non-employer factors such as secular increases in search frictions, changes in the value of outside options, secular slowdowns in employer learning, and long-term wage-setting policies, we rely on the two-way fixed effect decomposition of earnings developed in [Abowd et al. \(1999\)](#) (AKM).³ Examining the impact of cyclical shocks on the AKM employer premia that workers obtain, we find that employers are responsible for about 31 percent of the overall earnings penalty in the first decade of a recession entrant’s career, while non-employer factors account for the remaining 69 percent.

Recession-induced losses in employer-specific pay can arise because workers match with employers that deliver smaller economic rents to their employees, because they match with employers that provide a larger share of compensation in the form of non-pay amenities in lieu of pay, or both. AKM employer effects conflate these two forms of compensation. Our third and most important contribution is to separately unravel the role of rents and compensating differentials in the recession-induced losses that accrue to labor market entrants. To do this, we estimate the utility associated with working for each employer using the revealed preference-based framework in [Sorkin \(2018\)](#). Within this framework, compensating differentials at the employer level are measured as residual variation in AKM employer effects that remains after conditioning on employer-level utility.⁴ In contrast, rents accruing to workers are measured as variation in AKM employer effects that is explained by utility. Using these rich measures of employer-specific compensation, we find that about 14 percent of the overall earnings loss faced by recessionary entrants in the first decade of their careers is explained by reductions in rents while 17 percent is compensated for by non-pay amenities. This finding implies a nontrivial downward adjustment to the welfare cost of labor market entry during recessions, at least in the German context.⁵

Our results are consistent with previous studies that posit industry-specific risks and location-specific differences in the quality of life as important determinants of compensating differentials (see, for example, [Rosen, 1986, 1979](#); [Roback, 1982](#)). Specifically, we find that industries, occupations, and locations explain the majority of the observed amenity gap between workers who enter during good versus bad times. Despite this finding, an important caveat is that our estimates on workplace-related utility may not capture other welfare-relevant consequences of young workers’ exposure to adverse aggregate conditions such as differences in health or future mortality (see, for example, [Macleay, 2013](#) and [Schwandt and von Wachter, 2019](#)).

Finally, we find that the positive amenity gap between recession entrants and expansion entrants arises from cyclical shifts in the source of hiring activity. In particular, our fourth contribution is

³[Oreopoulos et al. \(2012\)](#) use average employer-level pay as an outcome when examining the effects of cyclical shocks on labor market entrants. Although they find that this variable is an important determinant of earnings penalties in Canada, average employer-level pay is confounded by worker quality. The same concern is not applicable to AKM employer fixed effects.

⁴Under the revealed preference approach, employer-level utility combines all amenities associated with a workplace, including hours flexibility, job security, workplace injury and health risks, arduous working conditions, etc.

⁵In a finding related to our own, [Guvenen et al. \(2022\)](#) show that employer-provided benefits such as pensions and health insurance offset some of the increases in cross-cohort lifetime income inequality for men in the United States.

to establish a new empirical fact showing that job creation at low-pay, high-amenity employers is less pro-cyclical than it is at high-pay, low-amenity employers. This difference is primarily driven by increases in hiring as opposed to reductions in separations. Labor market entrants who are beholden to the job offer distribution they face at entry therefore flow toward high-amenity employers in recessions and toward low-amenity employers in expansions. Our finding that high- and low-amenity employers exhibit differential cyclical sensitivity in employment growth is related to research by Haltiwanger et al. (2018), who show that employment growth at high-pay employers is more cyclically sensitive than at low-pay employers. Relative to Haltiwanger et al. (2018), the novelty of our result is that it shows cyclical differences in employment growth have an amenity component, not just a pay component.

The remainder of this paper is organized as follows. Section 2 describes data provided by the Institute for Employment Research (IAB) of the German Federal Employment Agency (BA). Section 3 explains how we use the universe of matched employer-employee data to construct employer-specific measures of pay and non-pay compensation. Section 4 describes our identification strategy and sample construction. Section 5 presents our results on the impact of cyclical shocks on earnings and their sub-components. Section 6 explores mechanisms driving the changes in earnings and amenities. Section 7 examines heterogeneity across sub-groups of labor market entrants. Section 8 concludes.

2 Data

We use three different administrative data sets in our analyses. In this section, we provide details on the structure of each data set and describe how it features in subsequent empirical work.

2.1 Population-Level Employment Histories

Most of our empirical analyses rely on the 1997–2019 Employee History Files (*Beschäftigtenhistorik*, BeH), which are linked employer-employee histories provided by the IAB that cover the universe of employment subject to social security payroll tax contributions.⁶ Records in the BeH are organized in terms of spells, where each spell enumerates a match between a given worker and a given employer. Spell-level information about the worker includes the start and end dates of employment, earnings, full-time or part-time status, occupation, education, date of birth, gender, nationality, and place of residence.

Establishments in the BeH, which are assigned unique time-invariant identifiers, are either single-unit plants or groups of plants owned by the same firm that operate within the same municipality and industry. We refer to establishments in our data as employers. Employer-level information includes the place of business and industry classification. Spells associated with apprenticeships

⁶In Germany, social security payroll tax contributions cover unemployment insurance, health insurance, and old-age pensions. Civil service jobs, military service, and self-employment are not tracked in the BeH. Marginal part-time employment—or so-called mini-jobs—is tracked starting in April 1999.

in Germany’s vocational training system are separately tracked in the data, which allows us to infer information on training-specific occupations and the start and end dates of training. This information is critical for our research design because it allows us to track young workers through their vocational training activity and into regular employment with day-level granularity on labor market entry. In addition, the population-level scale of the BeH allows us to implement the data-hungry approaches to estimating employer-specific earnings premia as in [Abowd et al. \(1999\)](#) and employer-specific utility as in [Sorkin \(2018\)](#). We provide additional information about the BeH in [Appendix A](#).

2.2 Random Sample of Employment and Unemployment Benefit Histories

To calculate unemployment rates, we use the Sample of Integrated Labour Market Biographies (SIAB; for details, see [Frodermann et al., 2021](#)). These data comprise a 2-percent longitudinal random sample of all individuals in Germany who ever worked, claimed unemployment insurance benefits, or sought job-seeking assistance during the 1975–2019 period. In total, the sample describes the labor market histories of just over 1.9 million workers. Unlike the BeH, which is limited to only employment spells, the SIAB allows us to observe both employment and benefit receipt and contact with active labor market programs during nonemployment.⁷

Computing unemployment rates using the SIAB’s detailed microdata has two distinct advantages relative to relying on published statistics. First, and most importantly, the microdata allow us to estimate state-level unemployment rates specifically for workers with vocational training experience. Notably, these skill-specific unemployment rates are published at the national level, but not released at the state level. Second, as we will discuss in [Section 4.3](#), the microdata allow us to omit the entering cohort of trainees from the computation of the unemployment rate.

We construct our annual unemployment rates in three steps. After excluding the set of workers who complete their apprenticeships in a given year, we assign the remaining workers with vocational training experience a status of employed, unemployed, or out of the labor force on the 15th of each month.⁸ We then aggregate these observations into monthly unemployment rates by workers’ state of residence. Finally, we aggregate monthly unemployment rates into yearly unemployment rates by averaging across months and weighting monthly rates by the underlying number of observations associated with each month. The resulting state-year unemployment rate is our primary independent variable of interest throughout this paper.

2.3 Administrative Wage and Labor Market Flow Panel

To study the cyclical nature of employment growth, hiring, and separations at the employer-level, we use the IAB’s Administrative Wage and Labor Market Flow Panel (AWFP) (for details, see [Stüber and Seth, 2017, 2024](#)). Our analyses rely on the AWFPs measures of quarterly inflows and outflows

⁷Nonemployed individuals who neither receive benefits nor seek job-finding assistance are beyond the scope of the administrative data from which the SIAB is constructed.

⁸The SIAB provides detailed information about labor market status that we aggregate to three simple categories.

of full-time workers for each employer, as well as the end-of-quarter stock of full-time workers at each employer. Although it is a standalone employer-level data set, the AWFP is constructed using matched employer-employee histories from the BeH.

3 Estimating Employer-Specific Earnings Premia and Amenities

In this section, we describe how we estimate employer-specific earnings premia and employer-specific values. We explain the identification and estimation strategies involved in obtaining each of these employer-specific estimates. We then explain how we use earnings premia and values jointly to obtain employer-specific estimates of rents and compensating differentials.

3.1 AKM Earnings Decomposition

The AKM model decomposes log earnings for worker i in year t as

$$\log(y_{it}) = \alpha_i + \psi_{j(i,t)} + \mathbf{x}_{it}'\boldsymbol{\beta} + r_{it}. \quad (1)$$

In Equation (1), y_{it} is annual earnings. Person fixed effects, α_i , incorporate individual-specific skills that are rewarded equally across employers. $j(i,t)$ indexes the firm that employs worker i in year t , and the employer fixed effect, $\psi_{j(i,t)}$, is a proportional premium that is paid by employer j to all its employees. \mathbf{x}_{it} is a vector of unrestricted year dummies as well as age dummies fully interacted with educational attainment. These controls account for aggregate and life-cycle determinants of earnings. Consistency of the parameter estimates requires that the error term, r_{it} , is uncorrelated with α_i , $\psi_{j(i,t)}$, and the \mathbf{x}_{it} . [Card et al. \(2013\)](#) provide a detailed discussion about the validity of the identifying assumptions specifically based on the BeH data.

Estimation of the AKM decomposition requires three sample restrictions. First, when workers have multiple jobs—that is, spells with multiple employers in a given year—only the highest paying job is selected. Second, because unobserved variation in hours confounds the identification of the employer-specific earnings premium, the estimation is restricted to workers with full-time status. To remove confounding variation in earnings that arises from differences in job spell lengths, the AKM model is fit using average daily earnings in the BeH. The estimated employer-earnings premium therefore accrues proportionally per day of full-time work. Finally, person and employer fixed effects are identified only within a connected set of employers, that is, a set of employers that either hire from or lose workers to other employers in the set. Our main results are based on estimating the AKM decomposition using 3-year non-overlapping windows to cover our sample period, which spans 1998 to 2018. This approach allows us to measure employer pay premia at a relatively high frequency. As noted in [Andrews et al. \(2008\)](#), the employer effect estimates are subject to sampling errors, which are made worse when they are identified by a small number of worker moves. This “limited-mobility bias” is problematic when assessing the role of employers in explaining the variance of log earnings. We implement a split-sample approach that we describe in [Section 3.3](#) to mitigate

the impact of limited-mobility bias on our estimates. We also conduct robustness checks that rely on a wider 7-year bandwidth to estimate the AKM decomposition.

3.2 Estimating Employer-Specific Values

Much of the literature that builds on the AKM decomposition treats the employer earnings premium as a measure of economic rents shared by workers. However, a long tradition in economics posits that employer-specific aspects of pay can vary not only because of factors such as rent sharing or efficiency wages, but also because of amenities that are priced in the labor market as compensating differentials.⁹

Building on this tradition, [Sorkin \(2018\)](#) proposes a novel methodology to study labor market mobility on the basis of utility rather than just pay. In his model, the voluntary movement of workers across jobs provides information about the relative utility associated with those jobs that is composed of both pay and non-pay attributes. Implementing this revealed preference argument requires three assumptions. First, all workers have the same preferences over jobs, up to an idiosyncratic draw. Second, all jobs within an employer are deemed to be identical from the standpoint of non-pay characteristics. Finally, all workers—both employed and nonemployed—search randomly from the same offer distribution.

Taking these assumptions to linked employer-employee data, [Sorkin \(2018\)](#) develops an estimator that aggregates the voluntary movement of workers across employers into employer-level utility, which is also referred to as employer value. Intuitively, the estimator rewards employers for making more hires from other high-quality employers and penalizes them for voluntary departures. Akin to the connectedness requirement in AKM, values are calculable only within a *strongly connected* set of employers. Strong connectivity is defined as a set of employers who both gain *and* lose workers to other employers in the set.¹⁰

Because employer values are estimated using a revealed preference argument, a crucial step in the procedure is to infer the probability that a given worker move is voluntary. Appendix Figure B1 illustrates how these probabilities are estimated by exploiting the “hockey-stick” shape of the separation hazard across employer growth rates; that is, separations at growing employers are low and stable, whereas separations at shrinking employers rise sharply. The identifying assumption is that separations from growing employers are likely to represent “expected” turnover that is fueled by voluntary quitting. In contrast, excess separations at shrinking employers are likely driven by involuntary displacement. Then, the benchmark probability that *any* employer-to-employer (EE) transition is voluntary is the average EE transition probability for growing employers. Similarly, the benchmark probability that *any* employer-to-nonemployment (EN) transition is voluntary is the average EN transition for growing employers. At shrinking employers, the excess probability

⁹See, for example, [Rosen \(1974\)](#) and [Rosen \(1986\)](#) for theory, and [Lucas \(1977\)](#), [Freeman \(1978\)](#), and [Brown \(1980\)](#) for empirical evidence.

¹⁰Limited-mobility concerns associated with employer fixed effects in earnings are also relevant in the estimator proposed by [Sorkin \(2018\)](#) to infer employer utility. We discuss the impact of this source of estimation-induced noise in the next Section 3.3.

of an EE or EN transition, over and above the voluntary transition probability, is defined as the involuntary transition probability.¹¹

We implement the methodology developed in [Sorkin \(2018\)](#) using population-scale BeH data with some minor modifications to accommodate differences between U.S. employer-employee linked data to which the method was originally applied and the BeH. Appendix B provides details about the data, the estimation procedure, and model fit relative to targeted parameters. Analogous to the AKM effects, we estimate values separately for 3-year non-overlapping bandwidths that span our sample period of 1998–2018 for our main results and 7-year bandwidths as a robustness check.¹²

3.3 Estimating Employer-Specific Rents and Compensating Differentials

Idiosyncratic shocks in the utility posting job search model of [Sorkin \(2018\)](#) generate frictions that preclude free movement of workers between employers. Consequently, because utility is not equalized across employers, workers earn rents. In this framework, assume that workers’ utility functions can be written as

$$V_j = \omega(\psi_j + a_j), \quad (2)$$

where V_j is the forward-looking value of working at employer j , ω is utility per log euro, ψ_j is the employer-specific earnings premium, and a_j is the employer-specific non-pay amenity. Note that the non-pay amenity, a_j , has two potential roles. It can create additional dispersion in utility if it co-varies with the earnings premia, or, alternatively, it can reduce the dispersion in utility by compensating for differences in the earnings premia. One can rearrange Equation (2) to estimate

$$\psi_j = \pi V_j + \epsilon_j \quad (3)$$

and then obtain the residual terms $\hat{\epsilon}_j = \psi_j - \hat{\pi}V_j$. Because the residuals are orthogonal to V_j by construction, they only capture those components of a_j that generate variation in employer-level earnings, holding utility fixed. In the context of a profit maximizing employer, [Sorkin \(2018\)](#) shows that the residuals in Equation (3) arise from variation in the cost of amenity provision that is independent of V_j . The motive for employers to provide these non-pecuniary amenities is therefore analogous to the theory of compensating differentials in [Rosen \(1986\)](#); that is, they reduce the dispersion of utility by compensating for differences in pay. Notably, the types of amenities that co-vary with pay are not identified in this framework.¹³ The R^2 from estimating Equation (3) is the share of variance in employer fixed effects that is explained by value. The residual variance share, $1 - R^2$, captures components of the employer fixed effects that are orthogonal to value, that is, the

¹¹Note that unemployment and labor force non-participation are considered the same for the purposes of this estimation procedure. Nonemployment-to-employer transitions are always assumed to be voluntary.

¹²Data from 1997 and 2019 are used to determine the source and destination of worker inflows and outflows in 1998 and 2018, respectively. Consequently, employer effects are neither left nor right censored.

¹³[Sorkin \(2018\)](#) labels these amenities as “Mortensen” amenities following [Hwang et al. \(1998\)](#), [Mortensen \(2003\)](#), and [Lang and Majumdar \(2004\)](#) who write frictional models of the labor market where amenities and pay are positively correlated.

share attributable to compensating differentials. Finally, the fitted value, $\hat{\pi}V_j$, captures variation in employer fixed effects that is correlated with utility within a frictional labor market and therefore constitutes rents. This is different than much of the AKM-based literature, which treats all of ψ_j as rents.

It is worth noting that limited-mobility concerns associated with employer fixed effects in earnings are also relevant in the estimator used to infer employer value. Estimation-induced measurement error in employer values can induce attenuation bias in $\hat{\pi}$, which distorts the role of rents relative to compensating differentials. In addition, estimation-induced measurement error in the employer fixed effects adds noise to the residual term in Equation (3), thus lowering R^2 and inflating the importance of compensating differentials in overall pay variability. To account for the effects of these measurement errors, we implement a split-sample approach to compute bias-corrected estimates of both $\hat{\pi}$ and R^2 .

For each estimation window, we start by randomly splitting the set of workers into two subsamples. We then estimate the AKM decomposition and the structural model to recover employer fixed effects and employer values separately for each of the two random splits of the data. Denoting the (unobserved) true employer value by V_j^* , the employer value estimates derived from the two random splits of the data are:

$$V_j^1 = V_j^* + u_j^1 \quad (4)$$

$$V_j^2 = V_j^* + u_j^2, \quad (5)$$

where u_j^1 and u_j^2 are classical estimation-induced measurement errors (that is, uncorrelated with V_j^*). Because the data are randomly split into two independent samples, u_j^1 and u_j^2 are orthogonal by construction. We can therefore use V_j^1 as an instrument for V_j^2 when estimating Equation (3) to purge attenuation bias from the estimate of π .

Turning next to bias correcting R^2 , denote the (unobserved) true employer fixed effect by ψ_j^* . Then, the employer fixed effect estimates derived from the two random splits of the data are:

$$\psi_j^1 = \psi_j^* + v_j^1 \quad (6)$$

$$\psi_j^2 = \psi_j^* + v_j^2, \quad (7)$$

where v_j^1 and v_j^2 are classical estimation-induced measurement errors. Assuming that the measurement errors for the values are uncorrelated with the measurement errors for the employer fixed effects, the bias-corrected estimate of R^2 is given by

$$\frac{\left(\text{Cov}(\psi_j^1, V_j^1)\right)^2}{\text{Cov}(\psi_j^1, \psi_j^2)\text{Cov}(V_j^1, V_j^2)}. \quad (8)$$

In the top panel of Table 1 we show $\hat{\pi}$ and R^2 estimated with and without bias correction from the 3-year estimation windows (columns 1 and 2) and for the 7-year estimation windows (columns

3 and 4). For parsimonious presentation of parameter estimates, we construct a panel data set at the firm-window level to estimate Equation (3) with window fixed effects and report a single slope parameter and R^2 in each column. In the implementation of our main results, we rely on window-specific estimates of $\hat{\pi}$. For comparison, column (5) shows time-invariant estimates from [Sorkin \(2018\)](#), which are based on U.S. data from 2000 to 2008. Comparing column (1) to column (2), we see that measurement error in the values attenuates the slope coefficient while measurement error in both the employer fixed effects and the values attenuates R^2 . Comparing column (1) with column (3), we see that degree of attenuation in both the slope coefficient and R^2 is somewhat smaller in the 7-year estimation windows relative to the 3-year estimation windows, reflecting the fact that wider bandwidths mitigate limited-mobility bias by allowing for more worker moves per employer. Columns (2) and (4) show that once the estimates are corrected, the estimated slope coefficient and R^2 are very close regardless of bandwidth. From columns (2), (4), and (5), we see that the majority of the variance in employer fixed effects is attributable to compensating differentials. In Germany, the share is just under 75 percent whereas in the United States the share is about 70 percent.

The lower panel of Table 1 computes the share of variation in compensating differentials (that is, the estimated residuals, $\hat{\epsilon}_j$) explained by location and industry characteristics. For the estimates shown in columns (1) through (4), we interact the relevant industry or location fixed effects with window fixed effects to account for temporal changes. Column (5) reports equivalent shares from [Sorkin \(2018\)](#), which are time invariant. Focusing on bias-corrected estimates, the first row shows that state fixed effects explain 14 to 17 percent of compensating differentials in Germany, while county fixed effects explain 15 to 19 percent.¹⁴ These estimates are consistent with the theory developed in [Rosen \(1979\)](#) and [Roback \(1982\)](#) indicating that local differences in amenities and in the cost of living are priced into the labor market. The next two rows show that sector fixed effects explain 7 to 10 percent of compensating differentials in Germany, while the three-digit industry explains 14 to 18 percent. Comparing the estimates in columns (2) and (4) to those in column (5) reveals that location is a more important determinant of compensating differentials in Germany relative to the United States, while the converse is true for industry.¹⁵ The last row of the table shows that interacting county and industry fixed effects explains 43 to 50 percent of the variation in compensating differentials.

To illustrate the relationship in Equation (3) graphically, Figure 1 shows studentized employer values on the horizontal axis and studentized earnings premia on the vertical axis. The figure is based on estimates of employer earnings premia and values obtained from a firm-window data set spanning 1998–2018. Employer-level estimates in the panel are constructed using 3-year estimation windows after residualizing window fixed effects. Each dot in the figure averages over a 1 percentile bin of the employer value distribution and reveals a stable linear relationship between pay and values indicating that higher-value employers offer higher pay at approximately the same rate across the

¹⁴Counties in Germany are defined by administrative units known as Kreise, which are approximately equivalent to U.S. counties. There are 16 states and 401 Kreise in Germany.

¹⁵[Sorkin \(2018\)](#) measures industry heterogeneity at the four-digit level, whereas we are limited to the three-digit level in our data.

value distribution.¹⁶ The red lines show one standard deviation bands of the employer fixed effect distribution within each value percentile. Observing variation in employer fixed effects, holding value fixed, indicates that employers differ in pay premia even though they provide the same utility. These differences in employer-specific pay arise due to compensating differentials.

To elucidate the role of sector characteristics, Figure 2 averages employer values and earnings premia within sectors and plots those averages alongside the line of best fit, which is estimated on the underlying employer-level data. In this figure, compensating differentials appear as variation in sector-level earnings premia, holding sector-level value fixed. For example, employers in the mining sector (labeled B) pay a substantial premium relative to what would be predicted based on their average value. This pattern is consistent with the idea that employment in the mining sector is risky and unpleasant and therefore commands higher pay in the form of a compensating differential. In contrast, employers in the health and social work sector (labeled Q) pay a discount relative to what would be predicted based on their average value. This pattern indicates that employment in the health and social work sector is associated with amenities that workers value in lieu of pay. These amenities could arise, for example, because jobs in the health and social work sector are less sensitive to demand shocks and therefore carry lower employment risk. To restate the idea behind Equation (3), sectors that pay more than what is predicted by value are those with positive compensating differentials (dis-amenities), and sectors that pay less than what is predicted by value are those with negative compensating differentials (amenities). Notably, the inter-sector earnings differences presented in Figure 2 are strikingly similar to the ones shown in U.S. data by Sorkin (2018), indicating that the U.S. and Germany are similar in terms of the relative price of amenities in the labor market.

Taken together, the results in Table 1 and Figure 2 provide important qualitative evidence that the values we estimated have sensible economic meaning, and that the amenities obtained from the decomposition exercise in Equation (3) capture non-pay characteristics of workplaces.

4 Institutional Setting and Research Design

In this section, we provide institutional details about the German apprenticeship system, discuss how we define our estimation sample, and explain our identification strategy. We then provide empirical evidence that validates our research design.

4.1 The German “Dual-Training” Apprenticeship System

Apprenticeships are the most common form of higher education in Germany. During the sample period we analyze (1998–2018), vocational training graduates account for 59 percent of the annual

¹⁶To reflect the IV-based correction for measurement error in employer values, we rescale the values plotted on the horizontal axis by the ratio of estimated-value variance to true-value variance, $\frac{\hat{\sigma}_V^2}{\hat{\sigma}_{V^*}^2}$. As in the split-sample IV approach, $\hat{\sigma}_{V^*}^2$ is estimated by $\text{Cov}(V_j^1, V_j^2)$.

flow of labor market entrants with higher education degrees.¹⁷ Apprenticeship training in Germany is also known as dual vocational training because it combines workplace and classroom training in a roughly 60-40 split. The typical young worker begins their vocational training after secondary schooling by starting an employer-sponsored apprenticeship in one of approximately 350 officially recognized occupations. Employers, unions, and government agencies jointly regulate the course content and program length associated with training in each of the occupations to meet quality standards. The typical course takes about three years to complete and culminates in a qualifying examination. Trainee wages are set by collective bargaining agreements that vary by both state and occupation (Kuppe et al., eds, 2013).

The fact that training program duration is preset is crucial, because it makes it less likely that young workers can selectively enter the labor market when cyclical conditions are favorable. As described below, this fact plays an important role in our identifying assumption, which rules out manipulation of entry timing. In Section 4.4 we show empirically that trainees do not meaningfully manipulate their entry timing based on aggregate conditions.

4.2 Sample Construction

The BeH distinguishes between employment spells associated with apprenticeship training and those that are not. Using this information, we construct our sample of labor market entrants by first isolating all workers who are ever recorded as apprentices in the 1998–2018 BeH files. From this set of apprentices, we remove workers whose cumulative training duration was shorter than six months or longer than four years, as well as outlying workers whose last training spell occurred before age 18 or after age 28. We also eliminate apprentices who are recorded as having started training on January 1, 1998 itself—the first date of observation in our analysis—as training length is censored for these individuals. Finally, we limit our analysis sample to individuals whose pre-apprenticeship education level includes some form of secondary school degree but does not include a university degree.¹⁸ After imposing these restrictions, we define the date of labor market entry as the last day on which workers are classified as trainees—all spells subsequent to that day constitute post-entry labor market activity.

Our core analyses aim to quantify how employers contribute to early career recessionary scarring. Consequently, for all post-entry years, we assign each worker an annual dominant job—that is, a single employer—for each year. For workers with multiple job holding within a year, the dominant job is the one with the highest earnings in that calendar year. We then assign employer-level characteristics to each worker-year based on this dominant job. Because the decompositions represented by Equations (1) and (3) are only defined in this case, we restrict the sample for our core analyses to individuals in years when their dominant job is with an employer for which AKM effects and value

¹⁷Statistics are based on data provided by the Federal Institute for Vocational Training (Bundesinstitut für Berufsbildung) and the Federal Statistical Office (Statistisches Bundesamt). University graduates with Bachelor’s, Master’s, or Doctoral degrees account for the remaining 41 percent.

¹⁸We impose this restriction to focus our attention on the typical educational trajectory in Germany, where workers either move from secondary school to vocational training or from secondary school to university.

estimates are both available.¹⁹ Table 2 displays summary statistics of our main analyses sample. Panel A shows fixed characteristics, while Panel B shows post-entry outcomes for each potential experience year.

4.3 Identification Strategy

Using this sample and exploiting institutional features of the German apprenticeship training system, we estimate the effect of initial aggregate conditions on labor market outcomes using the following specification to model the outcome of individual i in year t :

$$y_{it} = \sum_{e=0}^{19} \beta_e [U_{s(i),c(i)} \times \mathbb{1}\{t - c(i) = e\}] + \mathbf{X}_i' \boldsymbol{\Gamma} + \theta_s + \theta_c + \theta_e + \theta_t + \nu_{it}. \quad (9)$$

In Equation (9), our independent variable of interest is the unemployment rate, $U_{s(i),c(i)}$, which captures the exposure of individuals entering the labor market in state s and year c to aggregate conditions at the time of entry. When computing $U_{s(i),c(i)}$, we exclude from the SIAB microdata all individuals who enter the labor market in year c . This leave-cohort-out approach removes the possibility that $U_{s(i),c(i)}$ directly reflects the $e = 0$ outcomes of cohort c . The coefficients of interest, β_e , trace out the impact of a 1 percentage point increase in $U_{s(i),c(i)}$ on labor market outcomes for workers with potential experience e . The vector \mathbf{X}_i includes a set of fixed effects that control for training occupation, gender, German citizenship, the level of educational attainment prior to training, age at the time of labor market entry, and the month of labor market entry.²⁰ Standard errors for estimates obtained using Equation (9) are clustered at the state (where the training took place) level. θ_s are state-of-training fixed effects, θ_c are year-of-entry fixed effects, θ_e are potential experience (year minus year of entry) fixed effects, and θ_t are calendar year fixed effects.²¹

Figure 3 plots U_{sc} for each state during our sample period in Panel A. It demonstrates a substantial amount of within-state variation in labor market slack faced by entrants during our study period. In particular, much of our identifying variation stems from the labor market overhang generated by the 2001–2003 recession. For comparison, Panel B of Figure 3 plots the survey-based unemployment rates for 15–64 year-old workers in German states, compiled by the OECD. While we prefer our measure due to its specificity to trainees and large underlying sample size, the two panels show largely similar patterns. A regression of the form $U_{st}^{\text{OECD}} = \beta U_{st} + \alpha_s + \varepsilon_{st}$ yields $\hat{\beta} = 1.27(0.03)$, with a within- R^2 of 0.75.

Our identifying assumption is that unemployment rates prevailing at the time of labor market entry are unrelated to unobserved factors that determine earnings, conditional on fixed effects and predetermined controls \mathbf{X}_i . This assumption encompasses two key points. First, workers with

¹⁹As noted in Sections 3.1 and 3.2, this restriction amounts to analyzing individuals when they work at full-time jobs within the largest strongly connected set of employers that each have 10 or more full-time workers per year.

²⁰Recall that career start, or labor market entry, is defined as the last day on which workers are observed as trainees in the BeH. Forty-five percent of trainees graduate in June or July (the usual exit exam period), while another 20 percent (comprising mostly those who take the exam early) graduate in January.

²¹As in Oreopoulos et al. (2012), we identify θ_c , θ_e , and θ_t by dropping an extra calendar year fixed effect.

particular unobserved characteristics do not manipulate their initial labor market conditions by retiming their entry, by switching training occupations, or by failing to complete their apprenticeships. Second, the introduction of a cohort that is particularly bad (good) in terms of unobserved characteristics is not responsible for adverse (benign) aggregate conditions. We present evidence consistent with these points in the next subsection.

4.4 Evidence Consistent with Identifying Assumption

We formally test whether workers’ training-specific outcomes vary based on aggregate labor market conditions at entry by estimating the following cross-sectional adaptation of Equation (9):

$$[\text{Placebo Outcome}]_i = \gamma U_{s(i),c(i)} + \theta_s + \theta_c + \mathbf{X}'_i \boldsymbol{\Gamma} + \nu_{it}. \quad (10)$$

We estimate this regression using three different placebo outcomes. We first consider the level and the log of training duration, which is measured in days. Sensitivity of this variable to the unemployment rate at entry captures whether workers selectively time their entry in response to cyclical conditions. We next consider an indicator variable that measures whether workers maintain the same occupation from the start through the end of training. This variable measures in-training stability in workers’ area of specialization. Sensitivity of this variable to the unemployment rate at entry captures whether workers respond to expected changes in the demand for specific skills by switching to a new occupation. Finally, we consider whether workers pass an end-of-training qualifying exam and obtain a diploma. Sensitivity of this variable to the unemployment rate at entry captures whether workers selectively drop out of their programs based on cyclical conditions. To the extent that testing standards remain fixed over time, this variable also proxies for differences in cohort quality. For each test, the parameter γ captures the effect of the unemployment rate at entry on the outcome of interest.

As shown in Table 3, we do not find evidence that training-related outcomes vary in a meaningful way based on aggregate conditions at the time of labor market entry, thereby lending credence to our identifying assumptions. Our 95 percent confidence interval excludes entry delays longer than 24 days due to a one standard deviation increase in U_{sc} , which is small relative to a mean of about 893 days. Thus, while our estimates on training duration are statistically significant, they imply an ability to delay that is highly unlikely to allow entrants to avoid slack labor markets. Similarly, we can rule out increases in the likelihood of switching training occupation of more than 0.8 percentage points. Trainees are at most 0.2 percent more likely or 0.8 percentage points less likely to obtain their training certificate when faced with a one standard deviation increase in U_{sc} . In summary, we do not detect meaningful differences in entry timing, occupation specialization, or successful program completion by trainees in response to their future labor market conditions at entry. We also show in Section 5.3 that including these training outcomes—log training duration, the indicator for maintaining occupation, and the indicator for having completed a certificate—as controls in Equation (9) does not alter the results.

5 The Effect of Cyclical Shocks on Early Career Outcomes

In this section, we study the effect of cyclical shocks on the early career trajectories of young workers. We show that labor market entry during recessions generates a loss in earnings of 5 percent cumulated over the first 10 years of individuals' careers. We find that 31 percent of this first-decade earnings penalty is attributable to employer effects. We further find that the loss in employer effects is roughly evenly split between rents and compensating differentials, with 17 percent of the earnings penalty compensated for by employer-specific amenities.

5.1 Long-Term Earnings Losses

Our first key result replicates findings of previous studies in a new setting by showing the effect of aggregate shocks on early career earnings trajectories. Figure 4 shows estimates of the β_e coefficients, which measure the effect of a 1 percentage point increase in the unemployment rate at entry on log earnings in subsequent years. Labor market slack at the time of entry generates an initial drop in pay that takes a full 20 years to overcome.²²

To assess the effect of a recession-sized shock on earnings trajectories, we rescale the change in the unemployment rate at entry to represent a one standard deviation increase in the state-level unemployment rate. We then consolidate these effects into two estimates that give the percentage loss in the present discounted value (PDV) of real earnings in the early-career stage represented by the first decade of potential experience and the mid-career stage represented by the second decade of potential experience, respectively. These calculations are written as:

$$\begin{aligned} \text{PDV Loss in First Decade of Career} &= 100 \times \left(1 - \frac{\sum_{e=0}^9 [\bar{y}_e(1 + \sigma_U \hat{\beta}_e)(1 + r)^{-e}]}{\sum_{e=0}^9 [\bar{y}_e(1 + r)^{-e}]} \right) \% \quad (11) \\ \text{PDV Loss in Next Decade of Career} &= 100 \times \left(1 - \frac{\sum_{e=10}^{19} [\bar{y}_e(1 + \sigma_U \hat{\beta}_e)(1 + r)^{-e}]}{\sum_{e=10}^{19} [\bar{y}_e(1 + r)^{-e}]} \right) \%. \end{aligned}$$

In Equation (11), \bar{y}_e is mean real earnings in potential experience year e , σ_U is the standard deviation in the state-level unemployment rate, and r is the discount rate. In our data, $\sigma_U = 3.77$, and we assume $r = 0.05$.

In our main analysis sample, the typical recession induces a 5.22 percent loss in the PDV of earnings over the first 10 years of an individual's career (Table 4, Column 1, Panel A). Although calculated in a different institutional setting, this estimate is quantitatively similar to the 5 percent earnings loss accrued over 10 years for the average Canadian recession graduate estimated in [Oreopoulos et al. \(2012\)](#). We also find that the earnings scar from graduating into a recession persists

²²While we focus on workers who are employed in their full-time dominant jobs and in the strongly connected set throughout the main text, Appendix D.3 examines total earnings, and Appendix Figure D2 shows the effect of recessionary entry on full-time employment propensity.

during the second decade of individuals' careers. In potential experience years 10-19, recession entrants earn 2.91 percent less in cumulated PDV terms than their luckier counterparts (Table 4, Column 1, Panel B).

5.2 Employer-Specific Versus Non-Employer Factors

To what extent are recession-induced earnings losses generated purely by employer factors, and to what extent are they generated by forces that are not specific to employers? To answer these questions, we use AKM employer fixed effects in earnings as the outcome variable in Equation (9).²³

Before interpreting the split between employer and non-employer factors, it is important to note that our estimates capture relatively high-frequency variation in employer-specific compensation because we re-estimate the AKM effects and values in 3-year windows. Furthermore, although the AKM effects contain estimation-induced measurement error, we assume that the error in this dependent variable is classical and therefore does not lead to any bias.²⁴ Figure 5 plots coefficients from estimating Equation (9) with AKM employer fixed effects as the outcome in blue alongside the log earnings outcomes. We see that employer effects dip at entry and then catch up steadily, indicating that workers who enter the labor market during recessions systematically match with lower-paying employers but subsequently take bigger steps to close the gap.

Further, the role of employers in determining the recession penalty faced by workers is distinct across the first two decades of workers' careers. In the first decade of potential experience, employers play a quantitatively important role: when we apply our coefficients to the formula in Equation (11) in Table 4, we find that employer effects explain about 31 percent of the recession-induced loss in the PDV of earnings.²⁵ In the second decade of potential experience, however, employer effects play a quantitatively negligible role, even as the earnings scar persists. The negligible role of employers in the mid-career stage likely stems from the fact that the employer-specific penalties have largely closed after the first decade of potential experience and because employer-to-employer movement declines as workers age.

The relatively large effect of employer-specific factors in early career earnings penalties could arise due to losses in rents or changes in the mix of pay and non-pay amenities received in lieu of pay. While valuable in ascertaining the role of employer-specific factors, the AKM employer effects on their own do not help to resolve this distinction. To break down these hidden features of employer-specific aspects of pay, we use the rent component of the AKM employer effect as an outcome variable in Equation (9) and plot the coefficients in orange in Figure 5. Notably, we see that the rent penalties are consistently smaller than the overall loss in employer-specific pay in the early part of workers' careers. This difference between rents and AKM employer effects reflects smaller compensating differentials, which indicates that recession-affected workers obtain more of

²³Our use of the AKM effects as dependent variables in this part of the analysis is in the vein of [Goldschmidt and Schmieder \(2017\)](#), who study the impact of domestic outsourcing on employer-level wage premia.

²⁴We provide evidence that supports this assumption in Appendix D.2.

²⁵Appendix C explains how we apply the PDV formula to infer the role of employer effects in the PDV of earnings losses.

their compensation in the form of employer-provided amenities. Appendix Figure D1 plots the estimated $\hat{\beta}$ from these outcomes separately, with standard errors.

The relative magnitude of the different features of pay penalties is summarized in Column 1 of Panel A in Table 4: of the 5.22 percent loss in the PDV of earnings over the first decade, employer-specific factors account for 1.67 percentage points, or about 31 percent. Reductions in rents account for about 0.75 percentage points or about 14 percent. Finally, gains in non-pay amenities account for 0.89 percentage points or about 17 percent. A novel and welfare-relevant conclusion that emerges from these estimates is that focusing purely on pecuniary losses and ignoring the role of compensating differentials overstates the cost of labor market entry during recessions by about 17 percent in the first decade of workers' careers. Meanwhile, the portion of the earnings scar that persists into the second decade of workers' careers is not compensated for by non-pay amenities. Indeed, even as (imprecise) estimates of employer effects rise above zero, our preferred measure of employer quality—rents—remains lower for recession entrants through the first 17 years of their careers.

In order to further understand our results, we examine the role of industry, occupation, and location characteristics in explaining the non-pay amenity gain obtained by recessionary labor market entrants over the first decade of their career. To do this, we first remove industry-by-county fixed effects from the employer-specific amenity estimates ($-\hat{\epsilon}_j$ from Equation (3)). We then use this residualized estimate of amenities as the outcome and re-estimate Equation (9). In this person-year-level regression, we also control for occupation fixed effects to absorb variation in amenities that is attributable to occupation-specific characteristics. Strikingly, we find that after we net out industry-by-county and occupation fixed effects, the non-pay amenity gains obtained by recessionary labor market entrants shrink by 82 percent. Industry effects alone shrink the amenity gap by 30 percent. Appendix Section D.4 provides a visual illustration of the results from this analysis.

These results generate two important takeaways. First, although amenities are measured as residuals in our framework, the fact that industry, occupation, and location explain 82 percent of the amenity gap between recessionary and expansionary entrants highlights that commonly posited sources of compensating differentials including industry- and occupation-related risks as well as local differences in the quality of life are priced in the labor market (see, for example, Rosen, 1986, 1979; Roback, 1982). Second, the quantitative importance of industry effects in explaining the amenity gap provides evidence that earnings penalties experienced by workers who enter low-paying industries during recessions are partly attributable to a compensating differential channel (and vice versa during expansions).²⁶

Having evaluated the impact of employer-driven changes in compensation, we finally turn to the impact of non-employer factors, which are shown in the last row of column 1 of Table 4, in each panel. Forces such as human capital mismatch, changes in outside offers, slow market-wide employer learning, and infrequent wage renegotiation account for 3.58 percentage points, or about 69 percent

²⁶In a related finding, Barlevy (2001) shows that some of the procyclical wage growth associated with switching industries in expansions is, in fact, a compensating differential for higher prospective unemployment risk in the industries to which workers move.

of the overall loss in the PDV of earnings in the first decade of trainees’ careers. From a policy perspective, it is notable that these forces are the dominant source of recession-induced earnings penalties even in an environment where workers have access to active labor market programs such as retraining and job-search assistance. These findings suggest that worker-focused policies do not easily neutralize the recession-induced earnings losses that young workers experience. We also note that non-employer factors are the only portion of the recession penalty that remain operative in the second decade of trainees’ careers. While infrequent wage renegotiation and slow market-wide employer learning are likely to have receded as non-employer explanations for the earnings penalty, skills mismatch could still be influential as a reason for the persistent earnings loss that we observe in the mid-career stage.

5.3 Robustness

Table 5 presents several robustness checks on our results. Column (1) duplicates column (1) of Table 4, our main results from the full analysis sample. column (2) allows for non-linearity in the relationship between ψ_j and V_j by including a quartic function of V_j in Equation (3), then re-estimating compensating differentials as the residual from this augmented model. Consistent with the linearity assumption implicit in Equation (3) and the visual evidence provided in Figure 1, our results are essentially unchanged when we use this alternate way of splitting employer fixed effects into rents and compensating differentials. In column (3), we include in the control set \mathbf{X}_i of Equation (9) the placebo outcomes from Section 4.4—log training duration, whether or not an individual obtained their training certificate, and whether or not the individual switched training occupations during training—given that we found small but statistically significant effects of our exposure variable on some of these placebo outcomes. Our results are once again quantitatively and qualitatively nearly identical. In column (4), we re-define our analysis sample by allowing those whose training duration was less than six months to remain in the data set. Our results are qualitatively unchanged. Finally, in column (5), we present results in which we use employer fixed effect and value estimates from 7-year windows instead of 3-year windows. As discussed in Section 3.3 and Table 1, longer windows may reduce estimation-induced error in values and fixed effects at the cost of missing cyclical changes to these characteristics. We find that compensating differentials play a smaller, but nonetheless non-trivial, role in explaining earnings losses in the first decade of trainees’ careers when we use 7-year windows.²⁷

²⁷The 7-year window estimation samples have fewer employers (and thus fewer total workers) relative to the 3-year window estimation samples because we impose a minimum employer size of 10 full-time workers across years in a given estimation window to reduce the effect of limited mobility bias. Marginal employers are more likely to be consistently seen above the 10-worker threshold over 3-year estimation windows than they are over 7-year estimation windows.

6 Mechanisms that Explain Our Findings

In this section, we examine mechanisms that explain our findings. We first show that recession-induced earnings losses are coincident with displacement from employers, occupations, industries, and locations where workers trained. We then show that high-amenity employers are more likely to grow in recessions relative to low-amenity employers, a shift driven primarily by hires rather than separations. This cyclical shift in the source of hiring activity pulls labor market entrants toward high-amenity employers in recessions and toward low-amenity employers in expansions, thereby generating the amenity gaps that we observe.

6.1 Mechanism 1: Displacement

Figure 6 shows that recession-affected entrants are displaced from employers, occupations, industries, and locations where they acquired specialized training as apprentices. We measure these displacement propensities by estimating Equation (9) using indicator variables for working for one’s training employer, within one’s two-digit training occupation, within one’s two-digit training industry, and within one’s state, respectively. In Panels A, B, and C, the rate of relative dislocation peaks at roughly 0.5 to 1 percentage point in workers’ second year of potential experience, which is coincident with the deepest loss in earnings.²⁸ In Panel D, relative dislocation continues throughout the window, peaking at roughly 1.5 percentage points by year 19 of potential experience. Notably, displacement propensities remain significantly elevated throughout the first decade of entry, indicating that many recession-affected workers experience a permanent shift away from the skill set in which they had invested during vocational training relative to workers who enter in expansions.

Because training employers are important contributors to earnings growth in the German context, the displacement we see in Panel A of Figure 6 suggests that there are strong parallels between labor market entry during adverse cyclical conditions and the unemployment scar that follows involuntary job loss (see, for example, [Jacobson et al., 1993](#), [von Wachter and Bender, 2006](#), and [Davis and von Wachter, 2011](#)). Furthermore, mismatch in occupation- and industry-specific human capital engendered by the displacement shown in Panels B and C further contributes to earnings penalties. This type of mismatch also has analogs in the literature that proposes mechanisms for earnings losses following job loss (see, for example, [Krolikowski, 2017](#), [Lachowska et al., 2020](#), [Helm et al., 2023](#), [Jarosch, 2023](#), and [Schmieder et al., 2023](#)).

6.2 Mechanism 2: Cyclical Changes in the Source of Employment Growth

Having shown the importance of displacement in driving earnings losses, we now turn to investigate why recessionary labor market entrants are more likely to match with high-amenity employers. We focus in particular on cyclical changes in the types of employers to which workers flow. To do this,

²⁸The initial dip, both in earnings and in all three displacement propensities, arises because recession-affected workers are slightly more likely to be retained by their training employers in the year of entry relative to the years immediately following entry.

we first construct estimates of annual employment growth, hiring rates, and separation rates using AWFPS data. As in [Davis and Haltiwanger \(1992\)](#), each rate is measured as changes in full-time employment flows normalized by mean employer size from year $t - 1$ through year t . For example, the growth rate of employer j in year t is given by

$$g_{jt} = \frac{\text{emp}_{j,t} - \text{emp}_{j,t-1}}{(\text{emp}_{j,t} + \text{emp}_{j,t-1})/2}, \quad (12)$$

where $\text{emp}_{j,t}$ is the number of full-time workers at the end of the fourth quarter of year t . We replace the numerator in Equation (12) with the change in worker inflows when computing the hiring rate and with the change in worker outflows when computing the separation rate. We then estimate the cyclical behavior of employment growth rates, hiring rates, and separation rates at high- and low-amenity employers using employer-level data with the following specification:

$$f_{jt} = \gamma_1 [\Delta U_{s(j),t}^{\text{all}}] + \gamma_2 [\text{High Amenity}_{jt}] + \gamma_3 [\Delta U_{s(j),t}^{\text{all}} \times \text{High Amenity}_{jt}] + \alpha_{k(j)} + \varepsilon_{jt}. \quad (13)$$

In Equation (13), f_{jt} measures the employer-level growth rate, hiring rate, or separation rate in year t ; $\Delta U_{st}^{\text{all}}$ is the change in the state-level unemployment rate from year $t - 1$ to year t ; *High Amenity* $_{jt}$ is a dummy variable that captures whether employer j has above-median amenities in year t ; and $\alpha_{k(j)}$ is an industry fixed effect.²⁹ We construct U_{st}^{all} from the SIAB using all worker types in order to capture employer-level exposure to cyclical conditions.³⁰ γ_1 measures the sensitivity of employment growth rates, hiring rates, and separation rates to fluctuations in the unemployment rate. γ_2 measures level differences in the same outcomes between low- and high-amenity employers. The key coefficient of interest is γ_3 , which measures cyclical differences in the sensitivity of employment growth, hiring rates, and separation rates at high-amenity employers relative to low-amenity employers. To interpret these coefficients in terms of aggregate changes, we weight each employer-year observation by mean employment size as measured by the denominator in Equation (12).

Table 6 shows coefficients estimated using Equation (13) over the 1998–2018 sample period. Column 1 shows that a 1 percentage point increase in the unemployment rate induces a 0.9 percentage point reduction in employment growth, which reflects the slowdown in job creation during recessions. This effect is markedly weaker among high-amenity employers, as is evident from the interaction term coefficient of 0.74. Another way to state this result is that high-amenity employers become more important sources of employment growth in recessions, whereas low-amenity employers become more important sources of employment growth in expansions. This finding is closely related to the finding by [Haltiwanger et al. \(2018\)](#), who show that high-paying employers exhibit more cyclically sensitive employment growth compared with low-paying employers. Our result is

²⁹Amenities are measured as $-\hat{\varepsilon}_{jt}$ from Equation (3) in each estimation window. The median level of amenities used to create *High Amenity* $_{jt}$ is therefore the median level of amenities in the AKM and employer value estimation window associated with year t .

³⁰Note that this is different from the unemployment rate used in Equation (9), which omits the entering cohort in each year and is specific to workers with vocational training experience. Results are nearly identical with the leave-cohort-out trainee unemployment rate, as seen in Table D3 of the Appendix, but use of the aggregate rate allows us to establish a more general empirical pattern.

also related to the finding by Moscarini and Postel-Vinay (2012), who show that small employers are more important sources of employment growth in recessions, whereas large employers are more important sources of employment growth in expansions. Notably, the correlation between employer size and amenities is only 0.07, which indicates that the differential cyclical sensitivity of employment growth between high- and low-amenity employers does not stem purely from size but instead captures a novel facet of employer heterogeneity in response to cyclical shocks.

Given that the growth rate in employment equals the hiring rate minus the separation rate, we separately investigate each of these margins in columns 2 and 3 of Table 6. Comparing coefficients in the first row of these two columns shows that cyclical reductions in employment growth rates are driven primarily by reduced hiring activity. This feature of employment dynamics likely reflects strong employment protections in Germany that encourage employers faced with adverse shocks to adjust labor demand either through attrition or by relying on short-term wage insurance (*Kurzarbeit*) as opposed to layoffs. The interaction term coefficients underscore the same pattern: differential growth rates at high-amenity employers arise largely from increased hiring activity as opposed to reductions in separation rates.³¹

Taken together, the estimates in Table 6 suggest that job creation shifts, in relative terms, toward high-amenity employers in recessions and toward low-amenity employers in expansions, and that these shifts occur primarily through the hiring margin. A consequence of this cyclical pattern is that labor market entrants, who are beholden to the offer distribution they face at entry, disproportionately flow toward high-amenity employers in recessions and toward low-amenity employers in expansions. This shift in the source of hiring activity gives rise to the observed positive amenity gap between recession and expansion entrants.

7 Heterogeneity Analysis

In this section, we study how our results vary across different groups of labor market entrants. For each of the sub-group analyses, which are shown as columns in Table 4, we report the PDV loss in earnings, its breakdown into employer and non-employer factors, and the breakdown of employer effects into rents and compensating differentials.

7.1 Comparing Skill Groups

To divide labor market entrants in our sample by skill level, we first regress log full-time dominant job earnings on calendar year and potential experience fixed effects to absorb aggregate effects as well as common, experience-related growth in earnings. We then average the residuals within occupations and classify occupations with above-median residuals as high-paying occupations and those with below-median residuals as low-paying occupations. Finally, we split entrants on the basis of training

³¹The coefficients are very similar in regressions estimated without industry fixed effects, as seen in Table D3 of the Appendix, which indicates that cyclical differences in job creation activity between high- and low-amenity employers is primarily a within-industry phenomenon.

occupations: Workers who complete their training in high-paying occupations are classified as high skill, while those who complete their training in low-paying occupations are classified as low skill.

Columns 2 and 3 of Table 4 present the decomposition of PDV earnings losses for low- and high-skilled workers, respectively. Comparing the first row in each column shows that low-skilled workers experience earnings losses that are about 0.37 percentage points greater than high-skilled workers' earnings losses in the first decade of their careers. That is, those trained in low-paying occupations earn 5.29 percent less in PDV terms over the first decade of their career when entering the labor market with a 3.77 (σ_U) higher unemployment rate than their luckier counterparts who also trained in low-paying occupations but who entered a more favorable labor market.

The breakdown of these losses into employer component and non-employer components, however, is largely similar. Employer-specific factors account for 29 percent of the earnings penalty for those trained in low-paying occupations and 32 percent of the earnings penalty for those trained in high-paying occupations. Further, 16 percent of the earnings penalty is compensated for by non-pay amenities for those trained in lower-paying occupations, as is the case for those trained in high-paying occupations. Taken together, these findings show that workers trained in lower-paying occupations experience deeper overall earnings losses while being no more compensated for their losses in the form of non-pay amenities, relative to those who train in higher-paying occupations.

7.2 Comparing Men and Women

Columns 4 and 5 of Table 4 further present the decomposition of PDV earnings losses for men and women, respectively. As we see from the first row of each column, male entrants experience markedly worse penalties in the face of recessionary shock relative to luckier males than do female entrants relative to luckier females. Specifically, men who enter the labor market with a 3.77 percentage point higher unemployment rate face PDV earnings losses that are about 1.4 percentage points larger than those experienced by female entrants.

While employer-specific pay premia account for a larger share of the overall earnings penalty for women than men, this masks a substantive difference in whether these pay premia reflect rents or compensating differentials. For women, 23 percent (1.02/4.36) of the overall earnings loss and 64 percent (1.02/1.59) of the employer-specific component are explained by compensating differentials. For men, only 15 percent of the overall earnings loss and 50 percent of the employer-specific component are explained by compensating differentials. We therefore find that 1) the labor market standing of men, relative to other men, is more dependent on business cycle luck than the labor market standing of women, relative to other women; and 2) this is especially true when accounting for compensating differentials. These findings on gender differences could arise because women select jobs with lower cyclical earnings risk or because women accrue skills that are better rewarded during downturns. Investigating these underlying mechanisms remains an important subject for future research.

8 Conclusion

In this paper we provide a new perspective on the costs that cyclical shocks impose on young workers. Using administrative employer-employee linked data from Germany, we first replicate the major finding of existing research showing that adverse cyclical conditions at labor market entry generate persistent earnings losses on affected workers. Using the AKM decomposition, we find that employer-specific pay premia account for 31 percent of the overall earnings loss faced in the first decade of a recession entrant’s career, which highlights that recessions substantially change the types of employers with which workers match. We then estimate a utility posting job search model by aggregating the universe of worker moves between employers. This revealed-preference-based approach to estimating employer-specific utility allows us to infer rents and non-pay amenities associated with each workplace in the data. With these rich measures of compensation in hand, we find that 14 percent of the recession-induced loss in earnings in the first decade of a recession entrant’s career is due to losses in employer-specific rents, whereas 17 percent is compensated for by non-pay amenities. Notably, the majority of the estimated amenity gap is explained by industry, occupation, and location characteristics. Our findings therefore show that focusing on earnings losses alone overstates the welfare consequences of labor market entry during recessionary periods.

An important qualifier is that our conclusions are specific to the labor market in Germany, which differs substantially in terms of employment protections, unemployment benefit generosity, job-search assistance, retraining programs, and health-care provision compared with the United States and Canada, the two other countries for which recession-induced earnings losses have been studied in detail. Furthermore, the welfare consequences that we focus on are employer-specific and do not account for other important factors such as health and well-being or consumption.

Caveats notwithstanding, the employer-specific amenities that we estimate using IAB data could be used to shed light on several broad questions. For example, our results suggest that the sully effect of recessions is not as severe for labor market entrants when we account for employer provision of non-pay amenities, but does this apply more broadly to other groups of workers, or is it a specific feature of early career employment trajectories? Moving beyond cyclical shocks, what is the relative importance of non-pay amenities over the life cycle, and do they correlate with labor supply decisions? Do the optimal designs of tax and transfer policies such as unemployment insurance change when one accounts for employer-provided amenities? How do employer- and worker-level survey reports of workplace quality relate to the amenities that workers appear to value through their revealed preference choices? These questions open promising avenues for future research.

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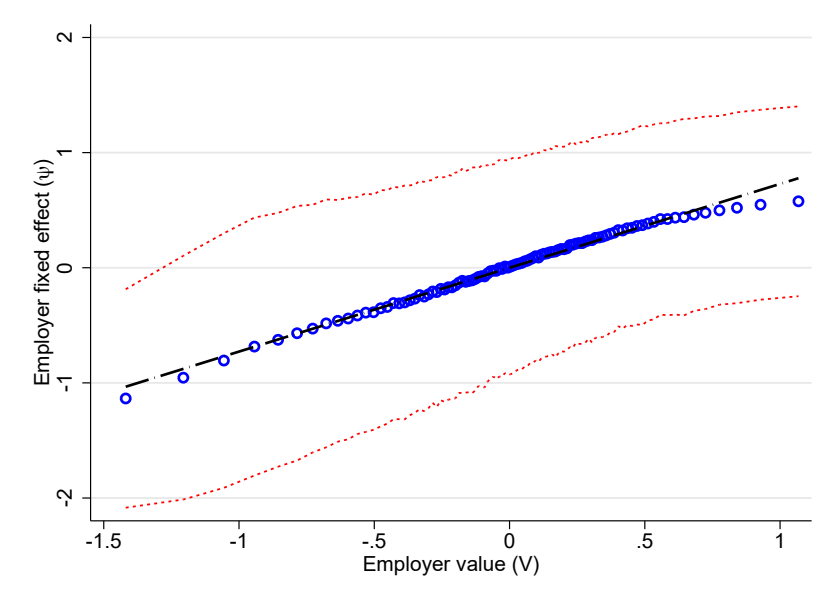
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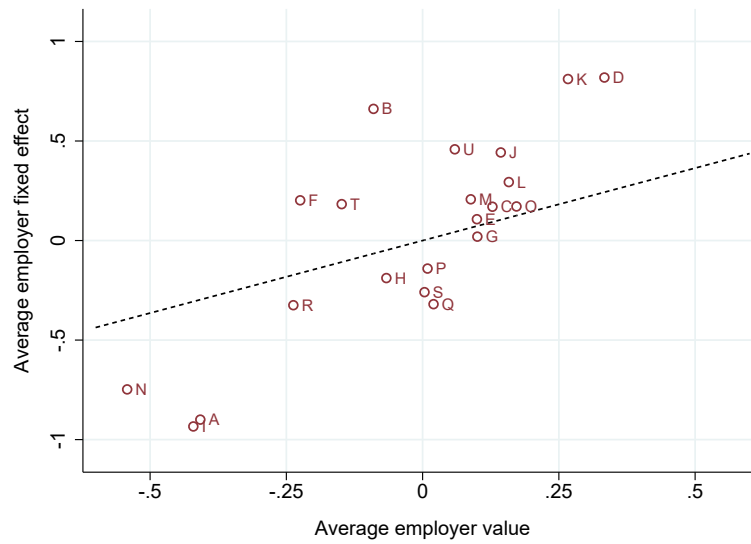
Figures and Tables

Figure 1: Relationship between Employer Fixed Effects and Employer Values

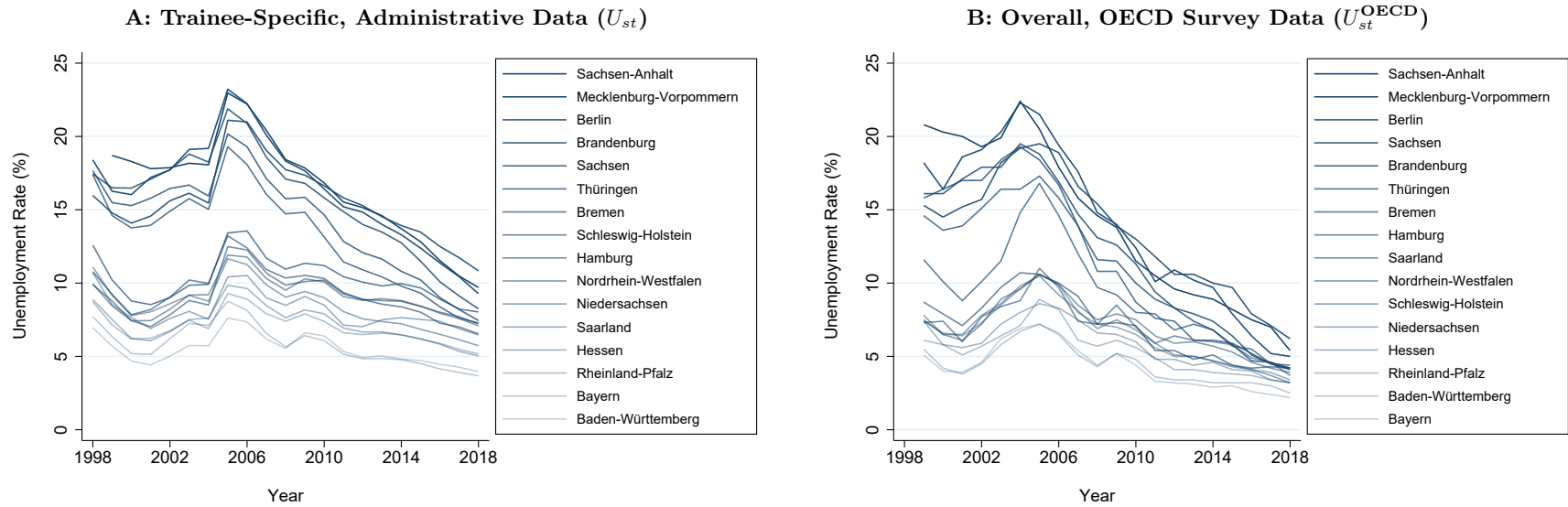


Notes: Each hollow circle in the figure shows employer fixed effects and values, residualized by window fixed effects and studentized, averaged within 1 percent bins of the employer value distribution. The line of best fit shows the slope of the relationship between employer fixed effects and values, estimated on employer-level data. The dotted lines show one standard deviation bands of the employer fixed effect distribution.

Figure 2: Average Employer Fixed Effects and Values by Sector

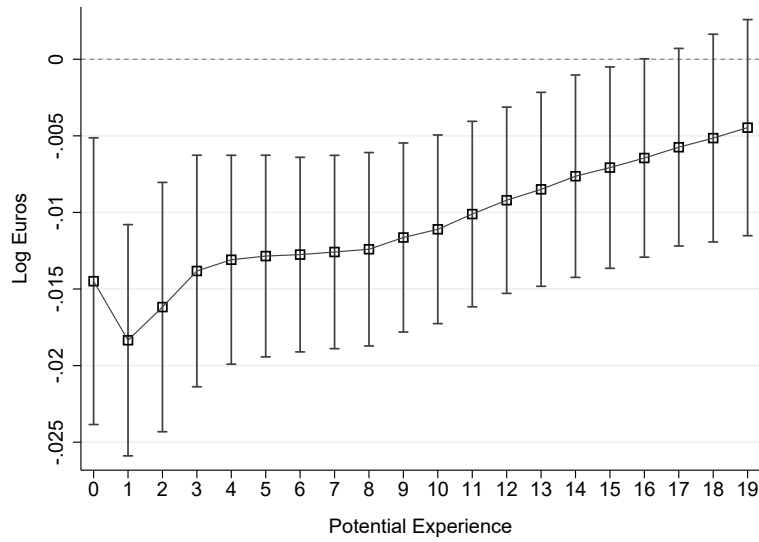


Notes: This figure plots mean employer fixed effects and values, residualized by window fixed effects and studentized, by sector. The line of best fit shows the slope of the relationship between employer fixed effects and values, estimated on employer-level data. The top three and the bottom three sectors by compensating differentials are, respectively, B: mining and quarrying; K: finance and insurance; D: electricity, gas, steam, and air conditioning supply; I: accommodation and food service; A: agriculture, forestry, fishing; N: administrative and support services. Table E1 contains exact values for average employer effects, values, rents, and compensating differentials plotted here along with all sector labels.

Figure 3: State Level Unemployment Rates

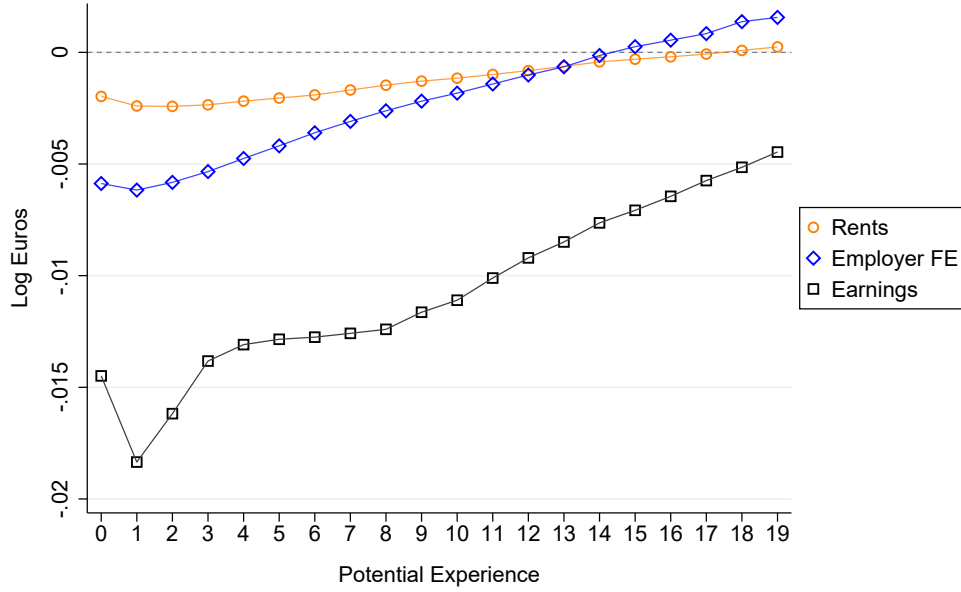
Notes: Legend in each panel is ordered based on average unemployment rate in a given state over the full time period. Panel A is constructed from the SIAB for workers with vocational training experience (see Section 2.2 for details). Panel B presents the survey-based overall unemployment rate for 15–64 year olds, compiled by the OECD, for comparison.

Figure 4: The Effect of Unemployment Rate at Entry on Early Career Earnings



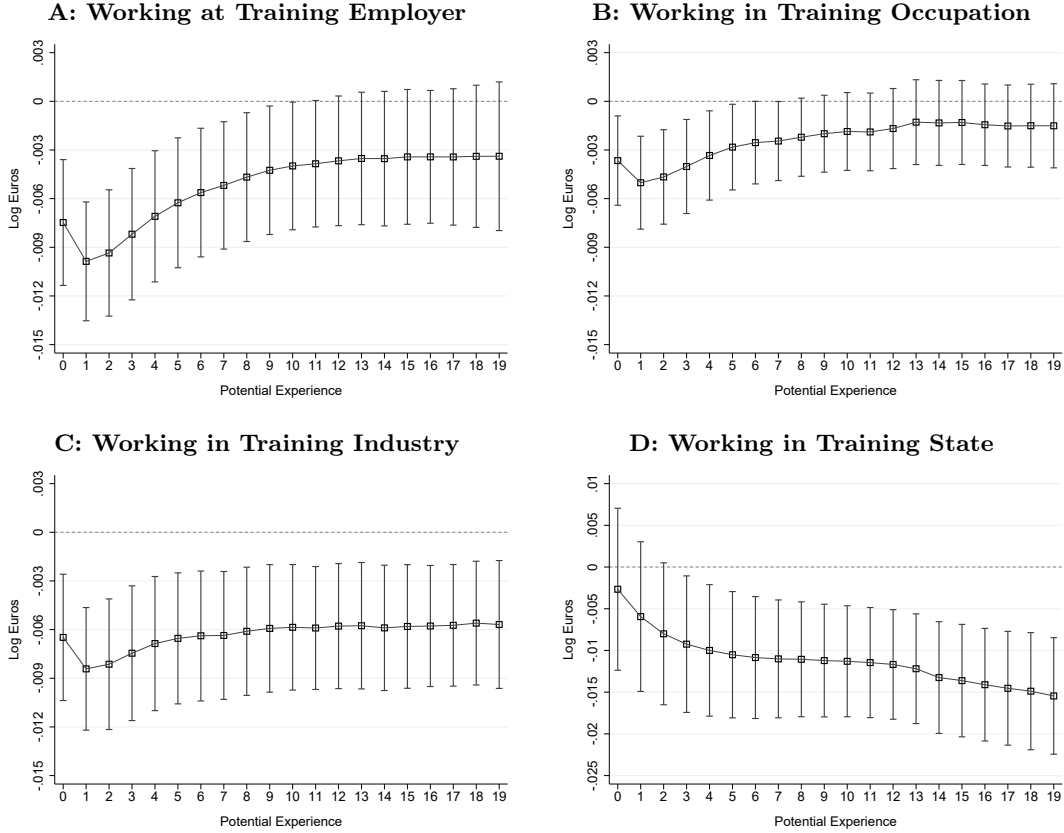
Notes: Lines connect coefficients $\hat{\beta}_e$, estimated by Equation (9). All estimated coefficients are in log earnings units. The specification is estimated on 6,845,618 individuals who are followed for all years in which they are working in a full-time annual dominant job for an employer that has an estimated fixed effect and value. The 95 percent confidence intervals are represented by capped spikes, with standard errors clustered at the state-of-entry level. Controls included are potential experience fixed effects, year fixed effects, year of entry fixed effects, month of entry fixed effects, training occupation fixed effects, state of training fixed effects, age at labor market entry fixed effects, level of pre-training education fixed effects, a German national indicator variable, and a female indicator variable.

Figure 5: Decomposing Early Career Earnings Losses



Notes: Lines connect coefficients $\hat{\beta}_e$, estimated by Equation (9). All estimated coefficients are in log earnings units. Each specification is estimated on 6,845,618 individuals who are followed for all years in which they are working in a full-time annual dominant job for an employer that has an estimated fixed effect and value. The black squares are coefficients from Equation (9) when log earnings is the outcome, and these are identical to coefficient estimates in Figure 4. The blue diamonds are coefficients from Equation (9) in which employer fixed effects ($\psi_{j(i,t)}$) is the outcome. The orange circles are coefficients from Equation (9) in which employer rents ($Rents_{j(i,t)}$) is the outcome. Separate estimates of each component in this figure that include standard errors can be seen in Figure D1 of the Appendix. Controls included are potential experience fixed effects, year fixed effects, year of entry fixed effects, month of entry fixed effects, training occupation fixed effects, state of training fixed effects, age at labor market entry fixed effects, level of pre-training education fixed effects, a German national indicator variable, and a female indicator variable.

Figure 6: The Effect of Unemployment Rate at Entry on Early Career Mobility



Notes: Lines connect coefficients $\hat{\beta}_e$, estimated by Equation (9). Each specification is estimated on 6,845,618 individuals who are followed for all years in which they are working in a full-time annual dominant job for an employer that has an estimated fixed effect and value. The 95 percent confidence intervals are represented by the shaded gray areas, with standard errors clustered at the state-of-entry level. Controls included are potential experience fixed effects, year fixed effects, year of entry fixed effects, month of entry fixed effects, training occupation fixed effects, state of training fixed effects, age at labor market entry fixed effects, level of pre-training education fixed effects, a German national indicator variable, and a female indicator variable. Note that the y-axis of Panel D is on a different scale than Panels A, B, and C.

Table 1: Compensating Differentials in Employer Earnings Premia

| | Germany (1998–2018) | | | | US (2000–2008) |
|---|---------------------------|------------------|---------------------------|------------------|-------------------------------|
| | (1) | (2) | (3) | (4) | (5) |
| | 3-year estimation windows | | 7-year estimation windows | | Time invariant |
| | Conventional | Bias corrected | Conventional | Bias corrected | Sorkin (2018) |
| $\hat{\pi}$ | 0.394 (0.001) | 0.639 (0.003) | 0.464 (0.002) | 0.685 (0.003) | - |
| $R^2(\psi_j, V_j)$ | 0.111 | 0.252 | 0.158 | 0.272 | 0.295 |
| Share of compensating differentials explained by observed factors | | | | | |
| State \times Window | 0.154 | 0.133 | 0.183 | 0.170 | 0.077 |
| County \times Window | 0.170 | 0.149 | 0.204 | 0.189 | 0.150 |
| Sector \times Window | 0.089 | 0.074 | 0.105 | 0.096 | 0.297 |
| Industry \times Window | 0.164 | 0.143 | 0.194 | 0.181 | 0.445 |
| County \times Industry \times Window | 0.461 | 0.429 | 0.525 | 0.504 | - |

Notes: The top panel shows the slope coefficient ($\hat{\pi}$), standard error, and R^2 from the regression of AKM employer fixed effects (ψ_j) on values (V_j) after residualizing both variables of window fixed effects. Conventional estimates rely on OLS, whereas bias-corrected estimates rely on split-sample IV. Bias-corrected R^2 is computed by plugging in sample moments (after residualizing window fixed effects) into Equation (8). $1 - R^2$ is the variation in AKM employer fixed effects attributable to compensating differentials. Employers are included in the sample if they are part of the largest strongly connected set in a given estimation window. Strong connectivity in columns (1) through (4) is defined inclusive of EN mobility, whereas strong connectivity in column (5) is defined based only on EE mobility. The lower panel regresses the estimated compensating differentials, which are residuals from Equation (3), on different sets of fixed effects and reports the R^2 from each of those regressions. In columns (1) through (4) of the lower panel, the different location and industry fixed effects are interacted with window fixed effects to account for temporal variation. In column (5), the employer effects are time invariant and reported share represents the explanatory effect of a given set of fixed effects without temporal interactions. Counties in Germany are defined by administrative units known as Kreise, which are approximately equivalent to U.S. counties. There are 16 states and 401 Kreise in Germany.

Table 2: Summary Statistics

| Variable | A: Entrant Characteristics | | | | |
|---|----------------------------|--------------------------------------|-------------------------------------|-----------------------------------|-----------------------------------|
| German national share | 0.95 | | | | |
| Female share | 0.40 | | | | |
| Age in years at labor market entry | 21.3 | | | | |
| Lower-level secondary school share | 0.81 | | | | |
| Higher-level secondary school share | 0.19 | | | | |
| Number of unique individuals | 6,845,618 | | | | |
| B: Post-Entry Outcomes, by Potential Experience | | | | | |
| Potential Experience Year | Log Earnings | Share Remaining at Training Employer | Share Retaining Training Occupation | Share Retaining Training Industry | Share Remaining in Training State |
| 0 | 9.47 | 0.64 | 0.77 | 0.78 | 0.94 |
| 1 | 9.95 | 0.51 | 0.70 | 0.67 | 0.91 |
| 2 | 10.04 | 0.42 | 0.64 | 0.60 | 0.89 |
| 3 | 10.08 | 0.35 | 0.59 | 0.55 | 0.87 |
| 4 | 10.12 | 0.30 | 0.54 | 0.50 | 0.85 |
| 5 | 10.16 | 0.27 | 0.50 | 0.46 | 0.84 |
| 6 | 10.21 | 0.24 | 0.46 | 0.43 | 0.83 |
| 7 | 10.24 | 0.21 | 0.43 | 0.40 | 0.82 |
| 8 | 10.28 | 0.19 | 0.41 | 0.38 | 0.81 |
| 9 | 10.30 | 0.18 | 0.39 | 0.36 | 0.80 |
| 10 | 10.33 | 0.17 | 0.37 | 0.35 | 0.79 |
| 11 | 10.35 | 0.16 | 0.35 | 0.34 | 0.79 |
| 12 | 10.38 | 0.15 | 0.33 | 0.33 | 0.78 |
| 13 | 10.40 | 0.14 | 0.32 | 0.31 | 0.78 |
| 14 | 10.42 | 0.13 | 0.31 | 0.31 | 0.78 |
| 15 | 10.44 | 0.12 | 0.30 | 0.30 | 0.77 |
| 16 | 10.46 | 0.12 | 0.29 | 0.29 | 0.77 |
| 17 | 10.48 | 0.11 | 0.29 | 0.28 | 0.77 |
| 18 | 10.50 | 0.10 | 0.28 | 0.28 | 0.77 |
| 19 | 10.51 | 0.10 | 0.28 | 0.27 | 0.76 |
| Mean across all years | 10.17 | 0.28 | 0.48 | 0.46 | 0.84 |
| Number of individual-years | 45,509,528 | | | | |

Notes: Schooling levels refer to educational attainment prior to training. Higher-level secondary schooling refers to the attainment of an *Abitur* degree. Earnings are based on employment in full-time dominant jobs and are measured in 2015 euros. The sample in Panel B tracks all individuals included in Panel A.

Table 3: Effect of Unemployment Rate at Entry on Placebo Training Outcomes

| | Training Duration (Days) | Log Training Duration | Maintained Training Occupation | Obtained Training Certificate |
|-------------------------|-----------------------------|--------------------------|-----------------------------------|----------------------------------|
| U_{sc} | 4.564 (1.646) | 0.008 (0.003) | -0.001 (0.002) | -0.0001 (0.0014) |
| Dependent variable mean | 893.291 | 6.657 | 0.854 | 0.915 |
| N | 6,845,618 | 6,845,618 | 6,845,618 | 6,845,618 |

Notes: This table shows the effect of the unemployment rate at entry on four different training outcomes. See Equation (10) for specification details. Standard errors shown in parentheses are clustered at the state-of-entry level. Training duration is measured in days. Workers are coded as having maintained their training occupation if their occupation at the start of training is the same as their occupation at the end of training. Workers are coded as having obtained a training certificate if, in spells subsequent to training, they are classified by employers as having successfully completed their apprenticeships.

Table 4: Percent PDV Earnings Losses from a One SD Increase in Unemployment Rate at Entry

| | (1) | (2) | (3) | (4) | (5) |
|---|----------------------------|---|--|------------|------------|
| | Full Analysis Sample | Trained in Low-Paying Occupations | Trained in High-Paying Occupations | Men | Women |
| Observations | 45,509,528 | 17,023,318 | 28,486,210 | 29,302,410 | 16,207,118 |
| Individuals | 6,845,618 | 2,877,234 | 3,968,384 | 4,080,448 | 2,765,170 |
| Employers | 421,547 | 352,610 | 369,595 | 390,541 | 322,792 |
| Panel A: First Decade of Career (0-9 Years of Potential Experience) | | | | | |
| Loss in earnings | 5.22 | 5.29 | 4.92 | 5.78 | 4.36 |
| | (1.22) | (1.23) | (1.15) | (1.38) | (0.82) |
| Due to employer-specific factors | 1.64 | 1.54 | 1.57 | 1.72 | 1.59 |
| | (0.60) | (0.59) | (0.59) | (0.66) | (0.52) |
| Due to rents | 0.75 | 0.67 | 0.77 | 0.86 | 0.57 |
| | (0.26) | (0.29) | (0.22) | (0.31) | (0.15) |
| Compensated for by non-pay amenities | 0.89 | 0.87 | 0.80 | 0.86 | 1.02 |
| | (0.37) | (0.35) | (0.40) | (0.38) | (0.42) |
| Due to non-employer factors | 3.58 | 3.75 | 3.35 | 4.06 | 2.77 |
| | (0.70) | (0.69) | (0.67) | (0.79) | (0.56) |
| Percent of Total Loss that is Compensated | 17% | 16% | 16% | 15% | 23% |
| Panel B: Second Decade of Career (10-19 Years of Potential Experience) | | | | | |
| Loss in earnings | 2.91 | 2.23 | 3.46 | 3.74 | 0.73 |
| | (1.13) | (1.10) | (1.12) | (1.32) | (0.83) |
| Due to employer-specific factors | 0.05 | 0.00 | 0.13 | -0.08 | 0.31 |
| | (0.43) | (0.45) | (0.42) | (0.48) | (0.37) |
| Due to rents | 0.18 | 0.15 | 0.30 | 0.15 | 0.21 |
| | (0.20) | (0.23) | (0.18) | (0.25) | (0.13) |
| Compensated for by non-pay amenities | -0.12 | -0.15 | -0.18 | -0.22 | 0.10 |
| | (0.26) | (0.27) | (0.26) | (0.26) | (0.30) |
| Due to non-employer factors | 2.86 | 2.22 | 3.33 | 3.82 | 0.43 |
| | (0.80) | (0.73) | (0.82) | (0.91) | (0.59) |
| Percent of Total Loss that is Compensated | < 0 | < 0 | < 0 | < 0 | 14% |

Notes: This table shows the breakdown of the present discounted value of earnings losses induced by a one standard deviation ($\sigma_U = 3.77$) increase in the unemployment rate at entry—cumulated over a given decade of potential experience. For example, in the primary sample, cohorts that face a one standard deviation higher unemployment rate at entry earn 5.22 percent less in their full-time dominant jobs over the first 10 years of their career relative to cohorts who do not face these adverse entry conditions, conditional on being employed by an employer with non-missing employer fixed effects and values (Panel A, Column 1, “Loss in Earnings”). See Section 5.1 for how overall losses are estimated and Appendix C for how overall losses are divided into specific sub-components. Standard errors shown in parentheses are computed based on the linear combination of $\hat{\beta}_e$ coefficients obtained from Equation (11). The earnings associated with each level of potential experience and the standard deviation of the unemployment rate are treated as non-random for the purposes of this computation. The underlying standard errors for the $\hat{\beta}_e$ coefficients are clustered at the state-of-entry level.

Table 5: Robustness—Percent PDV Earnings Losses from a One SD Increase in Unemployment Rate at Entry for Full Analysis Sample

| | (1) | (2) | (3) | (4) | (5) |
|---|------------|------------|------------|------------|------------|
| AKM/Value Window Length: | 3-Year | 3-Year | 3-Year | 3-Year | 7-Year |
| $\psi - V$ Functional Form: | Linear | Quartic | Linear | Linear | Linear |
| Include Additional Training Controls? | | | ✓ | | |
| Drop Individuals with < 6 mo. Training? | ✓ | ✓ | ✓ | | ✓ |
| Observations | 45,509,528 | 45,509,528 | 45,509,528 | 45,898,067 | 43,310,373 |
| Individuals | 6,845,618 | 6,845,618 | 6,845,618 | 6,952,870 | 6,610,449 |
| Employers | 421,547 | 421,547 | 421,547 | 423,039 | 340,440 |
| Panel A: First Decade of Career (0-9 Years of Potential Experience) | | | | | |
| Loss in earnings | 5.22 | 5.22 | 5.46 | 5.06 | 5.20 |
| | (1.22) | (1.22) | (1.20) | (1.11) | (1.21) |
| Due to employer-specific factors | 1.64 | 1.64 | 1.65 | 1.61 | 1.27 |
| | (0.60) | (0.60) | (0.60) | (0.58) | (0.52) |
| Due to rents | 0.75 | 0.74 | 0.76 | 0.73 | 0.72 |
| | (0.26) | (0.28) | (0.26) | (0.25) | (0.24) |
| Compensated for by non-pay amenities | 0.89 | 0.90 | 0.89 | 0.88 | 0.55 |
| | (0.37) | (0.36) | (0.37) | (0.37) | (0.32) |
| Due to non-employer factors | 3.58 | 3.58 | 3.81 | 3.45 | 3.93 |
| | (0.70) | (0.70) | (0.65) | (0.64) | (0.74) |
| Percent of Total Loss that is Compensated | 17% | 17% | 16% | 17% | 11% |
| Panel B: Second Decade of Career (10-19 Years of Potential Experience) | | | | | |
| Loss in earnings | 2.91 | 2.91 | 3.14 | 2.80 | 2.94 |
| | (1.13) | (1.13) | (1.09) | (1.09) | (1.11) |
| Due to employer-specific factors | 0.05 | 0.05 | 0.07 | 0.03 | -0.15 |
| | (0.43) | (0.43) | (0.43) | (0.42) | (0.36) |
| Due to rents | 0.18 | 0.19 | 0.19 | 0.16 | 0.18 |
| | (0.20) | (0.21) | (0.19) | (0.19) | (0.18) |
| Compensated for by non-pay amenities | -0.12 | -0.14 | -0.12 | -0.13 | -0.33 |
| | (0.26) | (0.25) | (0.26) | (0.25) | (0.21) |
| Due to non-employer factors | 2.86 | 2.86 | 3.07 | 2.77 | 3.09 |
| | (0.80) | (0.80) | (0.73) | (0.78) | (0.82) |
| Percent of Total Loss that is Compensated | < 0 | < 0 | < 0 | < 0 | < 0 |

Notes: This table shows the breakdown of the present discounted value of earnings losses induced by a one standard deviation ($\sigma_U = 3.77$) increase in the unemployment rate at entry—cumulated over a given decade of potential experience. For example, in the primary sample, cohorts that face a one standard deviation higher unemployment rate at entry earn 5.22 percent less in their full-time dominant jobs over the first 10 years of their career relative to cohorts who do not face these adverse entry conditions, conditional on being employed by an employer with non-missing employer fixed effects and values (Panel A, Column 1, “Loss in Earnings”). See Section 5.1 for how overall losses are estimated and Appendix C for how overall losses are divided into specific sub-components. Standard errors shown in parentheses are computed based on the linear combination of $\hat{\beta}_e$ coefficients obtained from Equation (11). The earnings associated with each level of potential experience and the standard deviation of the unemployment rate are treated as non-random for the purposes of this computation. The underlying standard errors for the $\hat{\beta}_e$ coefficients are clustered at the state-of-entry level.

Table 6: Cyclical Employment Dynamics at Low- and High-Amenity Employers

| | (1) | (2) | (3) |
|--|-------------------|-------------------|-------------------|
| | Growth Rate | Hiring Rate | Separation Rate |
| $\Delta U_{st}^{\text{all}}$ | -0.905 (0.113) | -0.960 (0.101) | -0.056 (0.064) |
| High amenity _{jt} | 0.023 (0.002) | 0.024 (0.002) | 0.002 (0.002) |
| $\Delta U_{st}^{\text{all}} \times \text{High amenity}_{jt}$ | 0.739 (0.122) | 0.660 (0.111) | -0.079 (0.081) |
| Dependent variable mean | 0.007 | 0.215 | 0.208 |
| Industry fixed effects | ✓ | ✓ | ✓ |
| <i>N</i> | 4,663,047 | 4,663,047 | 4,663,047 |

Notes: This table shows the effect of the change in the state-year unemployment rate on employer-level growth rates, hiring rates, and separation rates. See Equation (12) for definitions. All regressions are weighted by mean employer size from year t through year $t - 1$. Standard errors shown in parentheses are clustered at the state level. Table D3 in the Appendix presents several robustness checks surrounding these results.

Appendix for online publication

Appendix A Data

A.1 The Employee History File or Beschäftigtenhistorik (BeH)

The BeH data are linked employer-employee histories provided by the Institute for Employment Research (Institute für Arbeitsmarkt- und Berufsforschung, IAB). These data are based on the integrated notification procedure for unemployment insurance, health insurance, and old-age pensions (these three forms of social insurance are collectively considered social security in Germany), which came into effect on January 1, 1973, and was extended to cover eastern Germany as of January 1, 1991 (see, for example, [Wermter and Cramer, 1988](#); [Bender et al., 1996](#)). These data cover all workers, including apprentices, but exclude civil servants, those in military service, the self-employed, and regular students (see [Cramer, 1985](#)). Employees in marginal part-time employment and unpaid family workers are included as of April 1, 1999.

Establishments in the BeH, which are assigned unique time-invariant identifiers, are either single-unit plants or groups of plants owned by the same firm that operate within the same municipality and industry. We refer to establishments in our data as employers.

Employers report the exact start and ending dates of an employment spell and annually confirm an existing employment spell, making it possible to track workers' careers on a daily basis. In addition, employers report spell-level information about earnings, full-time or part-time status, occupation, education, date of birth, gender, nationality, and place of residence. Employer-level information includes the place of business and industry classification.

We use the universe of employment spells of “regular” (defined below) full- and part-time workers who were aged 15 to 66 in the years 1997 through 2019.

Regular workers belong to one of the following BeH persons-group codes:

1. (101) employees subject to social security with no special features
2. (102) trainees/apprentices with no special features
3. (140) seamen
4. (141) trainees/apprentices in seafaring occupations with no special features
5. (143) maritime pilots

Interns, marginal part-time employees, employees in partial retirement, and persons performing basic or voluntary military service are not considered regular workers.³² For an overview of all persons groups in the BeH, refer to [Ganzer et al. \(2021\)](#) Table 8, pp. 111–112.

³²Marginal part time workers are explicitly coded and hence easily excluded from data starting in 1999. For years prior to 1999, we code as marginal part time (and then drop) those employment spells which have real average daily earnings at or below the 1999 marginal part time earnings threshold.

Appendix B Estimating Employer Value

Using matched employer-employee data from the BeH, we replicate the methodology developed in [Sorkin \(2018\)](#) to estimate a revealed preference estimate of workplace quality. The procedure has four distinct steps, and we discuss each of them in turn below.

B.1 Recording Job Changes

The BeH records employer-employee matches as spells that are measured with daily precision. When a worker is employed at multiple employers at the same time, we retain the spell associated with the highest average daily earnings (spell-level earnings divided by spell duration in days). After imposing this restriction and limiting our sample to full-time workers, we track job changes as follows:³³

1. Employer-to-employer (EE) moves are recorded when a job spell ends and the worker starts a new employment spell at a different employer. Provided that fewer than 31 days elapse between two adjacent employment spells, we classify the transition as an EE move.
2. Employer-to-nonemployment (EN) moves are recorded when a job spell ends and the worker then spends more than 31 days out of work. To avoid classifying retirement or death as an EN move, we remove transitions where a job spell ends and the worker is never subsequently observed returning to work at any employer.
3. Nonemployment-to-employment (NE) moves are recorded when a worker spends more than 31 days out of work and then takes a job.

B.2 Identifying Voluntary Job Changes

When EE and EN transitions are voluntary, and therefore endogenous, they are informative about workers' preferences. However, job changes caused by displacement are involuntary or exogenous and are not informative about workers' preferences. Lacking direct information about the voluntary or involuntary nature of a job change in administrative data, we exploit variation in the employer-level growth rate to estimate the probability that a given job change is voluntary.

We begin by computing the quarterly growth rate of each employer. As in [Davis and Haltiwanger \(1992\)](#), the growth rate for employer j in calendar quarter t is defined as

$$r_{j,t} = \frac{(\text{emp}_{j,t} - \text{emp}_{j,t-1})}{(\text{emp}_{j,t} + \text{emp}_{j,t-1})/2} \quad (14)$$

where $\text{emp}_{j,t}$ is the count of full-time employees at the end of the quarter. By definition, new employers have a growth rate of 2, whereas closing employers have a growth rate of -2. Next, we

³³To address limited-mobility bias concerns, we impose a minimum employer size restriction of 10 full-time workers per year.

compute the EN and EE rate within 5 percentage point wide bins of the quarterly employer growth rate distribution. For illustration, figure B1 shows worker separation hazards across the employer growth rate distribution from the 2007–2009 estimation window. Notably, EE and EN rates are low and stable for growing employers. In contrast, EE and EN rates increase substantially at shrinking employers.

Following Sorkin (2018), we classify transitions out of growing employers as endogenous, up to an idiosyncratic shock, which we describe below. This assumption embeds the idea that, on average, workers who leave growing employers could have continued to work there if they wished to remain. Then, the benchmark probability that *any* EE transition is endogenous is the average EE transition probability for the set of growing employers. Similarly, the benchmark probability that *any* EN transition is endogenous is the average EN transition probability for the set of growing employers. At shrinking employers, the excess probability of an EE or EN transition over and above the voluntary transition probability is defined as the exogenous transition probability. NE transitions are always assumed to be endogenous.³⁴

B.3 Imposing Connectivity Restrictions

As we will discuss in Section B.4.1, the revealed preference measure of employer value is based on estimating equations that can be taken to the data only for the set of worker moves that occur within a strongly connected set of employers. An employer is part of a strongly connected set if it both hires workers from *and* loses workers to other employers in the set.³⁵ While Sorkin (2018) limits strong connectivity to employers linked only by EE flows, we expand the set to include employers linked by both EE flows and transitions of workers through nonemployment. This modification is fully consistent with the estimating equations in the Sorkin (2018) model that allow nonemployment to obtain its own relative value. Put differently, workers who make EN and NE transitions help to identify both the estimate of nonemployment value and estimates of employer values.

We treat nonemployment as a node in the graph of employers when obtaining the largest strongly connected set, which we do by using the graph algorithms provided by David Gleich in the Matlab `gaimc` library.³⁶

B.4 Estimating the Structural Model

In the utility posting random job search model of Sorkin (2018), employer j makes a share of offers f_j and employs a share of workers g_j . Employers differ in their exogenous separation probabilities, δ_j and ρ_j . δ_j is the probability of a job destruction shock that forces a worker from employment to

³⁴In Sorkin (2018), workers receive offers randomly. Consequently, NE flows are informative about the offer distribution made by an employer. Estimating employer value requires that we observe non-null NE flows and we drop employers for which this condition is not met.

³⁵This restriction is related to the connectedness requirement needed to estimate AKM fixed effects. Employers are part of a connected set if they hire workers from *or* lose workers to other employers in the set. We estimate the AKM model using the same strongly connected set of employers from which the structural model is estimated.

³⁶See <https://www.mathworks.com/matlabcentral/fileexchange/24134-gaimc-graph-algorithms-in-matlab-code>.

nonemployment (EN). ρ_j is the probability of a job reallocation shock that forces a worker from one employer to another (EE).³⁷ The forward-looking value of employer j is denoted by V_j^e and includes both pecuniary and non-pecuniary components. The forward-looking value of nonemployment is denoted by V^n .

In the model, workers draw an idiosyncratic shock from a type-I extreme value distribution in every period. This assumption allows movements between jobs to be driven by both the common component of employer value, V_j^e , and the idiosyncratic draw. The probability of receiving a job offer from another employer is λ_1 , while the probability of receiving a job offer from a particular employer, j , is f_j . The goal of the estimation procedure we describe below is to recover an estimate of the V_j^e and V^n that rationalizes the observed pattern of endogenous EE and EN moves.

B.4.1 Flow-Relevant Values

Let \mathbf{M} be a mobility matrix that records endogenous EE, EN, and NE moves. \mathbf{M} is an $N^\mathcal{E} + 1 \times N^\mathcal{E} + 1$ matrix where \mathcal{E} is the set of strongly connected employers of size $N^\mathcal{E}$, and nonemployment obtains its own row and column. m_{ij} represents the (i, j) entry in \mathbf{M} and records the number of endogenous flows to employer i from employer j . Recall that all moves from growing employers and from nonemployment are endogenous, whereas the probability of an endogenous move from a shrinking employer is based on the growth rate at the time of separation, as in Figure B1. Thus, when employer j is growing, m_{ij} is simply the count of flows to employer i . When employer j is shrinking, m_{ij} is the count of flows to employer i weighted by the probability that those moves are endogenous.

Define $\exp(\tilde{V}_j)$, the exponentiated flow-relevant employer-level value, as

$$\exp(\tilde{V}_j) \equiv \frac{f_j \exp(V_j^e)}{g_j(1 - \delta_j)(1 - \rho_j)}. \quad (15)$$

The flow-relevant value is a reduced-form object that combines differences in the structural value, V_j^e , along with differences in the effective size and offer rate. In particular, employers that make more offers (f_j) have a higher flow-relevant value since they attract more workers. Similarly, employers with a larger effective size ($g_j(1 - \delta_j)(1 - \rho_j)$) have more workers depart for endogenous reasons and therefore have lower flow-relevant value.

Sorkin (2018) shows that the endogenous moves in the mobility matrix \mathbf{M} can be reduced to a single linear restriction per employer. These employer-level restrictions can then be aggregated such that

$$\mathbf{S}^{-1} \mathbf{M} \exp(\tilde{\mathbf{V}}) = \exp(\tilde{\mathbf{V}}). \quad (16)$$

In Equation (16), \mathbf{S} is a diagonal matrix, with the i -th diagonal entry equal to $\sum_{j=1}^{N^\mathcal{E}+1} m_{ji}$, that is, the sum of all endogenous outflows from employer i . $\exp(\tilde{\mathbf{V}})$ is a vector of flow-relevant values for all employers and for nonemployment. This equation defines a high-dimensional function $\mathbf{S}^{-1} \mathbf{M}$

³⁷In estimation, these shock probabilities vary not at the employer level but only at the sector level.

whose fixed point is $\exp(\tilde{\mathbf{V}})$.³⁸ Note that the strong connectivity requirement for estimating values arises from Equation (16). If there are no inflows into an employer, its row sum in \mathbf{M} is zero and the flow-relevant value is mechanically zero. If there are no outflows from an employer, its column sum in \mathbf{M} is zero and the flow-relevant value requires division by zero.

B.4.2 Estimation Loop

With an estimate of the flow-relevant values, one can obtain the structural values using the definition in Equation (15). However, this calculation requires an estimate of the offer distribution f_j . To estimate the structural values, we proceed using a loop that is set up as follows:

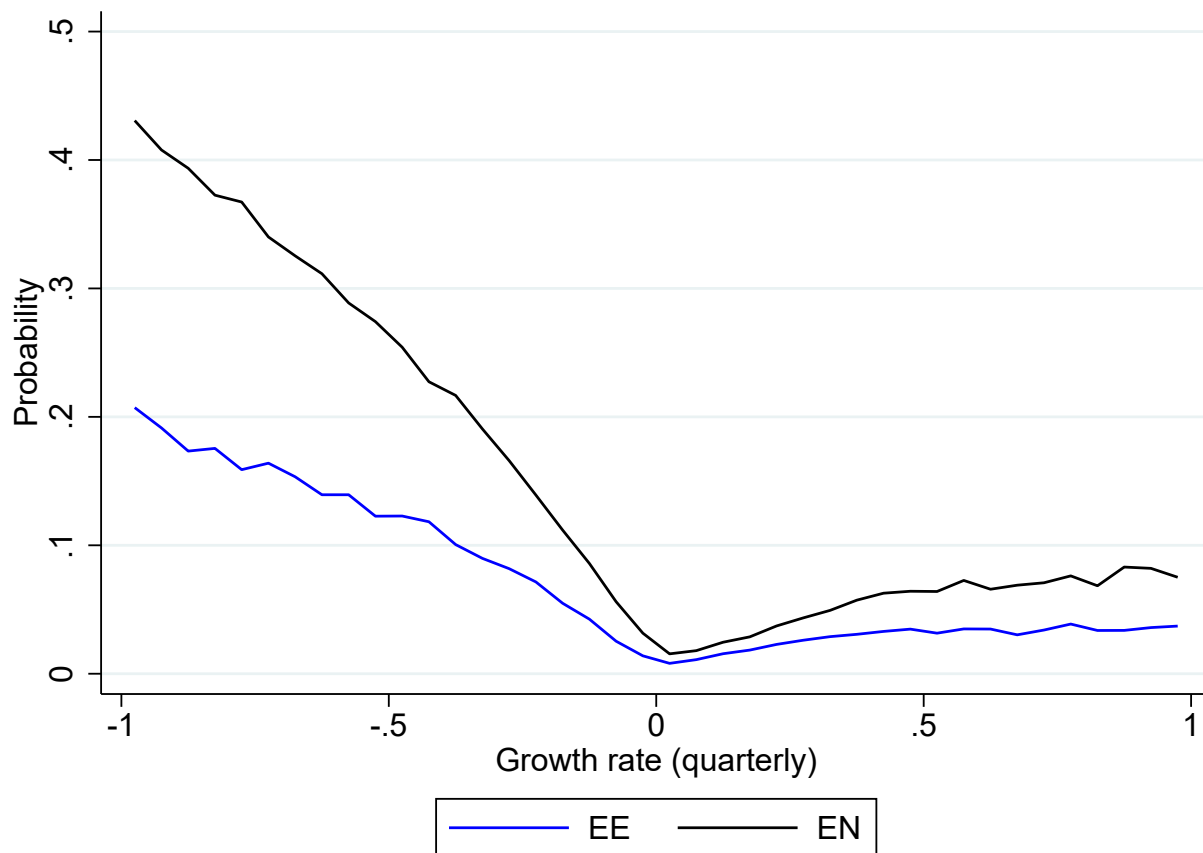
1. Initialize the model-implied endogenous EE and EN probabilities using observed EE and EN probabilities at expanding employers. Initialize $(V^n, \{V_j^e\}_{j=1}^{N^E})$ using a uniform random vector of length $E + 1$.
2. Construct \mathbf{M} by classifying all observed EE and EN flows out of growing employers as endogenous and weighting the observed flows out of shrinking employers by their endogenous EE or EN probability. Compute δ and ρ using the exogenous transition probabilities out of shrinking employers.
3. With \mathbf{M} and \mathbf{S} , obtain $\exp(\tilde{\mathbf{V}})$ by applying Equation (16) to an initial guess and iterating until convergence is attained.
4. Estimate f_j by doing a grid search for λ_1 to match the observed level of EE flows in the data. Details on the model equations used in these calculations are provided in Appendix G of Sorkin (2018). This step yields an estimate of $(V^n, \{V_j^e, f_j\}_{j=1}^{N^E}, \lambda_1)$.
5. Using $(V^n, \{V_j^e, f_j\}_{j=1}^{N^E}, \lambda_1)$ calculate the updated model-implied endogenous EE and EN probabilities. If the size-weighted correlation between the old and new estimates of $(V^n, \{V_j^e\}_{j=1}^{N^E})$ is less than 0.999, then return to step 1 of the loop using the new estimates of $(V^n, \{V_j^e\}_{j=1}^{N^E})$. If it is greater than or equal to 0.999, then stop.

We implement this estimation routine for each sample window (either 3 or 7 years wide) spanning our sample period of 1998–2018. Data from 1997 and 2019 are used to determine the source and destination of worker inflows and outflows in 1998 and 2018, respectively. Consequently, there is no flow-level censoring of values estimated in the first and last estimation windows.

For each non-overlapping 3-year estimation window, Table B1 shows model-based estimates of λ_1 (the probability of receiving a job offer), $\bar{\delta}$ (the average job displacement probability), and $\bar{\rho}$ (the average job reallocation probability) and compares the model-implied probabilities of endogenous EE and EN transitions with the same probabilities inferred from the data.

³⁸Sorkin (2018) shows that the top eigenvalue of $\mathbf{S}^{-1}\mathbf{M}$ is 1 and that $\exp(\tilde{\mathbf{V}})$ exists, is unique, and its elements all have the same sign.

Figure B1: Separation Hazard by Employer Growth Rate



Notes: This figure shows the separation hazard to nonemployment (EN) in black and the separation hazard to employment at a different employer (EE) in blue, averaged within 5 percentage point bins of the quarterly employer growth rate distribution. The separation hazards are censored at growth rates below -1 and above 1 . Estimates shown in the figure are based on 2007–2009 BeH data.

Table B1: Parameter Estimates and Selected Moments

| Parameter/moment | Estimation window | | | | | | |
|---|-------------------|-----------|-----------|-----------|-----------|-----------|-----------|
| | 1998–2000 | 2001–2003 | 2004–2006 | 2007–2009 | 2010–2012 | 2013–2015 | 2016–2018 |
| λ_1 | 0.080 | 0.080 | 0.070 | 0.080 | 0.090 | 0.080 | 0.100 |
| $\bar{\delta}$ | 0.037 | 0.048 | 0.050 | 0.034 | 0.025 | 0.023 | 0.015 |
| $\bar{\rho}$ | 0.096 | 0.092 | 0.093 | 0.085 | 0.069 | 0.076 | 0.053 |
| Probability of endogenous EE move - model | 0.037 | 0.034 | 0.029 | 0.036 | 0.040 | 0.037 | 0.047 |
| Probability of endogenous EE move - data | 0.037 | 0.032 | 0.028 | 0.035 | 0.040 | 0.038 | 0.047 |
| Probability of endogenous EN move - model | 0.129 | 0.129 | 0.115 | 0.118 | 0.112 | 0.115 | 0.118 |
| Probability of endogenous EN move - data | 0.113 | 0.112 | 0.099 | 0.104 | 0.100 | 0.102 | 0.108 |

Notes: This table shows parameter estimates and compares model-implied moments to moments estimated from the data. All probabilities are annual. λ_1 is the probability of receiving a job offer. $\bar{\delta}$ is the probability of a job destruction shock averaged over all employers. $\bar{\rho}$ is the probability of a job reallocation shock averaged over all employers. Separations to nonemployment are considered in the data only if they are followed by subsequent re-employment within the relevant estimation window.

Appendix C Decomposition of Recession-Induced Losses

This appendix explains how we decompose recession-induced wage losses into three different components.

Consider a hypothetical sample of individuals who enter the labor market when unemployment rates are at their average level. Using the AKM decomposition, individual-level earnings can be decomposed as:

$$\log(y_{it}) = \alpha_i + \psi_{j(i,t)} + \mathbf{x}'_{it}\boldsymbol{\beta} + r_{it}. \quad (17)$$

Taking expectations on both sides of Equation (1) in year t :

$$E[\log(y_{it})] = E[\alpha_i] + E[\psi_{j(i,t)}] + E[\mathbf{x}'_{it}\boldsymbol{\beta}] + E[r_{it}]. \quad (18)$$

The average residual in Equation (18) is not zero because the expectation operator is taken over a sample of entrants in a specific year, rather than the population of worker-years from which the AKM decomposition is estimated. Next, define average earnings in year t if exactly the same group of individuals experienced a 1 percentage point increase in the unemployment rate at entry as

$$E[\log(y_{it}^R)] = E[\alpha_i^R] + E[\psi_{j(i,t)}^R] + E[\mathbf{x}'_{it} \beta^R] + E[r_{it}^R]. \quad (19)$$

Relative to Equation (18), the person effects in Equation (19) are different because recession-induced earnings losses generate scarring effects that may not be fully removed in the eight-year time window over which the AKM decomposition is estimated. The employer effects are different due to recession-induced changes in worker-employer matching. The control variables and associated coefficients are different because they include calendar year effects, which capture the effect of aggregate shocks. The residual terms are different due to recession-induced changes in market-wide employer learning, unobserved human capital accumulation, and changes in the value of outside options (see, for example, the discussion about AKM residuals in [Card et al., 2013](#)).

The average cost of recessionary entry on earnings in year t , $E[\log(y_{it})] - E[\log(y_{it}^R)]$, can now be written as:

$$\underbrace{\beta_t^{\text{Earnings}}}_{\% \text{ Earnings differential}} = \underbrace{E[\alpha_i + \mathbf{x}'_{it}\boldsymbol{\beta} + r_{it}] - E[\alpha_i^R + \mathbf{x}'_{it} \beta^R + r_{it}^R]}_{\% \text{ due to non-employer factors}} + \underbrace{E[\psi_{j(i,t)}] - E[\psi_{j(i,t)}^R]}_{\% \text{ due to employer-specific factors}}. \quad (20)$$

Next, define

$$\beta_t^{\text{Non-employer}} = E[\alpha_i + \mathbf{x}'_{it}\boldsymbol{\beta} + r_{it}] - E[\alpha_i^R + \mathbf{x}'_{it} \beta^R + r_{it}^R], \quad (21)$$

$$\beta_t^{\text{Employer}} = E[\psi_{j(i,t)}] - E[\psi_{j(i,t)}^R], \quad (22)$$

as the non-employer and employer-specific components of recession-induced earnings differentials in year t . Estimating our main specification (Equation (9)) using employer fixed effects ($\psi_{j(i,t)}$) on the left-hand-side provides us with $\{\hat{\beta}_0^{\text{Employer}}, \dots, \hat{\beta}_T^{\text{Employer}}\}$.

Having defined recession-induced employer-specific losses, we now partition these losses into rents and compensating differentials using the decomposition in Equation (3). Recall that this decomposition splits employer fixed effects into rents, which are explained by employer value, and amenities, which are orthogonal to employer value. Taking expectations on both sides, year t employer effects for our hypothetical sample of individuals who enter the labor market when unemployment rates are at their average level are:

$$E[\psi_{j(i,t)}] = \pi E[V_{j(i,t)}] + E[\epsilon_{j(i,t)}]. \quad (23)$$

Employer effects for otherwise identical individuals who enter the labor market when unemployment rates are 1 percentage point higher are

$$E[\psi_{j(i,t)}^R] = \pi E[V_{j(i,t)}^R] + E[\epsilon_{j(i,t)}^R]. \quad (24)$$

Subtracting Equation (24) from Equation (23), the employer-specific pay reduction is

$$\beta_t^{\text{Employer}} = \underbrace{\pi \left(E[V_{j(i,t)}] - E[V_{j(i,t)}^R] \right)}_{\% \text{ due to rents}} - \underbrace{\left(E[\epsilon_{j(i,t)}] + E[\epsilon_{j(i,t)}^R] \right)}_{\% \text{ due to amenities}}. \quad (25)$$

Next, define

$$\beta_t^{\text{Rent}} = \pi \left(E[V_{j(i,t)}] - E[V_{j(i,t)}^R] \right), \quad (26)$$

$$\beta_t^{\text{Amenity}} = E[\epsilon_{j(i,t)}] - E[\epsilon_{j(i,t)}^R], \quad (27)$$

Combining Equations (20) and (25), we can write

$$\begin{aligned} \beta_t^{\text{Earnings}} &= \beta_t^{\text{Non-employer}} + \beta_t^{\text{Employer}} \\ &= \beta_t^{\text{Non-employer}} + \left(\beta_t^{\text{Rent}} - \beta_t^{\text{Amenity}} \right) \end{aligned} \quad (28)$$

Because $\beta_t^{\text{Earnings}}$, β_t^{Rent} , and β_t^{Amenity} are estimated directly, we can recover $\beta_t^{\text{Non-employer}}$ as a residual using Equation (28).

Finally, define the present value of earnings for the first $T + 1$ years of potential experience as

$$PDV = \bar{y}_0 + \frac{\bar{y}_1}{(1+r)} + \dots + \frac{\bar{y}_T}{(1+r)^T}, \quad (29)$$

where \bar{y}_e represents average annual earnings in potential experience year e . The PDV of earnings

for workers who face a 1 percentage point increase in the unemployment rate at entry is

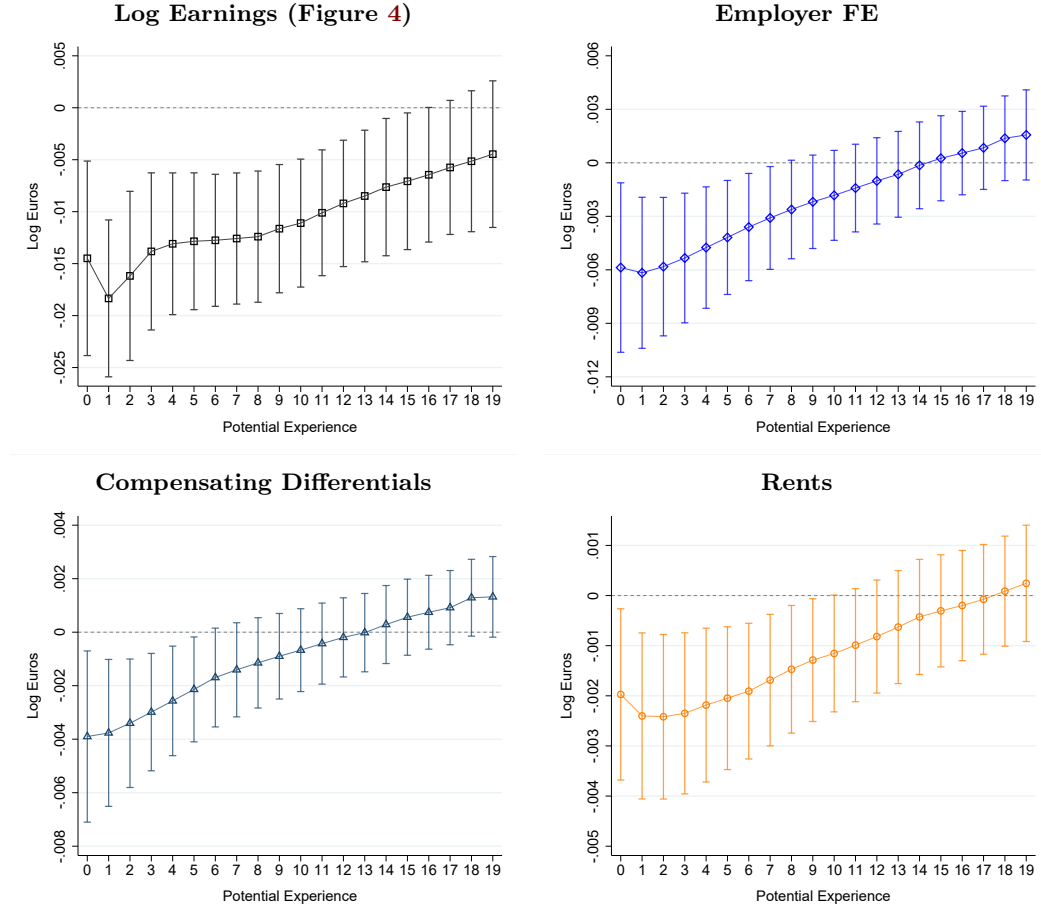
$$PDV^R = \bar{y}_0(1 + \beta_0^{\text{Earnings}}) + \frac{\bar{y}_1(1 + \beta_1^{\text{Earnings}})}{(1 + r)} + \dots + \frac{\bar{y}_{19}(1 + \beta_T^{\text{Earnings}})}{(1 + r)^T} \quad (30)$$

We use $\{\hat{\beta}_0^{\text{Earnings}}, \dots, \hat{\beta}_T^{\text{Earnings}}\}$ to quantify the loss in the present value of earnings attributable to a 1 percentage point change in the unemployment rate. We then scale the resulting estimate by a one standard deviation increase in the unemployment rate, which reflects a typical recession. Similar calculations with $\{\hat{\beta}_0^{\text{Employer}}, \dots, \hat{\beta}_T^{\text{Employer}}\}$, $\{\hat{\beta}_0^{\text{Rent}}, \dots, \hat{\beta}_T^{\text{Rent}}\}$, $\{\hat{\beta}_0^{\text{Amenity}}, \dots, \hat{\beta}_T^{\text{Amenity}}\}$, and $\{\hat{\beta}_0^{\text{Non-employer}}, \dots, \hat{\beta}_T^{\text{Non-employer}}\}$ yield estimates of the loss in the PDV of earnings attributable to employer-specific factors, rents, amenities, and non-employer factors.

Appendix D Supplemental Results

D.1 Separated Recession-Induced Changes to Employer-Specific Pay, Compensating Differentials, and Rents (with Standard Errors)

Figure D1: The Effect of Unemployment Rate at Entry on Early Career Mobility



Notes: Lines connect coefficients $\hat{\beta}_e$, estimated by Equation (9). Each specification is estimated on 6,930,829 individuals who are followed for all years in which they are working in a full-time annual dominant job for an employer that has an estimated fixed effect and value. The 95 percent confidence intervals are represented by the spiked caps, with standard errors clustered at the state-of-entry level. Controls included are potential experience fixed effects, year fixed effects, year of entry fixed effects, month of entry fixed effects, training occupation fixed effects, state of training fixed effects, age at labor market entry fixed effects, level of pre-training education fixed effects, a German national indicator variable, and a female indicator variable. Note the difference in scales. Estimated coefficients on Compensating Differentials and Rents add up to estimated coefficients on Employer FE for a given potential experience year.

D.2 Dispersion in AKM Estimation Errors and the Unemployment Rate

Our use of AKM employer effects and other employer-level variables as dependent variables in Equation (9) yields consistent estimates provided that estimation-induced error in these dependent variables is uncorrelated with the unemployment rate, conditional on controls. Although we cannot measure estimation error at the employer level, our split-sample exercise detailed in Section 3.3 allows us to estimate the variance of the estimation-induced error. In this Appendix, we examine whether the variance of the estimation-induced error in the AKM employer effects is correlated with the unemployment rate.

Using Equation (7), the variance of the estimation induced error can be computed using observed split-sample moments as:

$$\underbrace{V(u_j^1)}_{\text{Error variance}} = \underbrace{V(\psi_j^1)}_{\text{Variance of estimated AKM effects}} - \underbrace{V(\psi_j^*)}_{\text{Cov}(\psi_j^1, \psi_j^2)} . \quad (31)$$

We apply Equation (31) at the state-year level using our 3-year non-overlapping split-sample estimates of the AKM effects to obtain estimates of error variance, which we denote by $\sigma_{u,st}^2$. We then regress $\sigma_{u,st}^2$ on the unemployment rate (U_{st}) to test whether the error variance moves systematically with the key independent variable in our analyses. Coefficients from this regression are presented in Table D1 below. Without conditioning on state or year fixed effects, we see a statistically significant association in Column (1), although the point estimate is small relative to a mean error variance of 0.05. Conditioning on state fixed effects as shown in Column (2), and both state and year fixed effects (as we do for our main analyses) in Column (3), we see that the association between the variance of the estimation error and the unemployment rate becomes statistically insignificant. The absence of a meaningful relationship between the dispersion of estimation errors and the unemployment rate suggests that classical measurement error in the employer-level variables is a reasonable assumption.

Table D1: Relationship between $\sigma_{u,st}^2$ and Unemployment Rate (U_{st})

| | (1) | (2) | (3) |
|----------|---------------------|---------------------|--------------------|
| U_{st} | -0.0007 (0.0003) | -0.0002 (0.0003) | 0.0003 (0.0005) |
| State FE | | ✓ | ✓ |
| Year FE | | | ✓ |
| N | 112 | 112 | 112 |

Notes: Standard errors clustered at the state level.

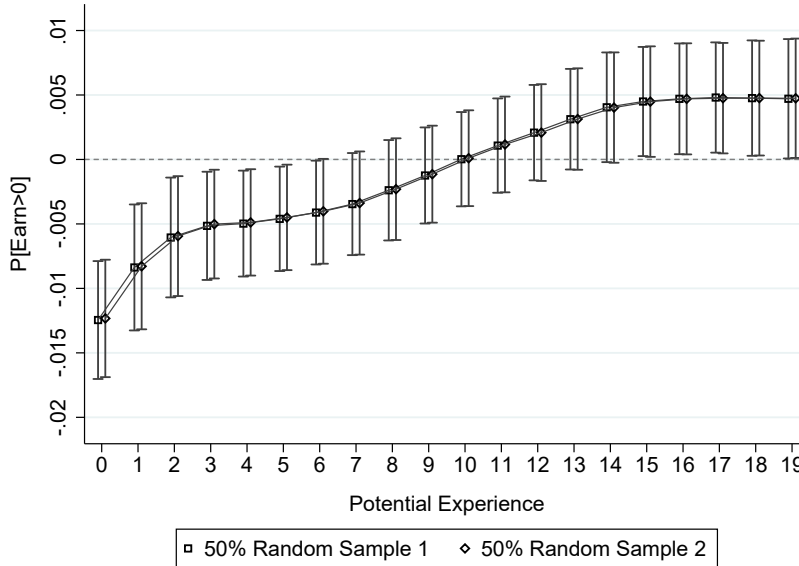
D.3 Accounting for Non-Employment and Total Earnings

Due to the sample restrictions required by the AKM decomposition and the [Sorkin \(2018\)](#) model, our main results are conditional on employment in full-time dominant jobs within the largest strongly connected set (henceforth, “FTDJSC earnings”). In this section, we address the effects of labor market conditions at entry on the probability of employment and on earnings outside of the largest strongly connected set.

We start by studying the impact of higher unemployment rates at entry on early-career employment propensities. To do this, we estimate Equation (9) using an outcome that indicates whether an individual had *any* earnings over the course of a calendar year.³⁹ Because this specification involves a significantly larger set of observations, computational constraints require that we estimate it separately in two, random 50% samples of our trainee entrants.

We plot the β_e coefficients from these regressions in Figure D2, which shows a pattern similar to the earnings trajectories. Workers are about 1.2 percentage points more likely to experience full-year nonemployment at entry. This effect slowly decays to zero by about 10 years of potential experience.

Figure D2: The Effect of Unemployment Rate at Entry on Employment



Notes: Lines connect coefficients $\hat{\beta}_e$, estimated by Equation (9). The 95 percent confidence intervals are represented by the spike caps, with standard errors clustered at the state level. Controls included are potential experience fixed effects, year fixed effects, year of entry fixed effects, month of entry fixed effects, training occupation fixed effects, state of training fixed effects, age at labor market entry fixed effects, level of pre-training education fixed effects, a German national indicator variable, and a female indicator variable.

An important takeaway from Figure D2 is that outcome trajectories *within* the largest strongly connected set do not fully capture the scarring effects of labor market entry during a recession.

³⁹In the year of entry, this variable is based on earnings in non-training employment spells.

Thus, while the primary purpose of this paper is to decompose log earnings losses into employer-specific components—something that can only be accomplished using FTDJSC earnings—we next aim here to assess the comprehensive impact of aggregate conditions at labor market entry on earnings potential.

To do so, we re-estimate Equation (9) using the *level* of total earnings and the level of FTDJSC earnings as outcomes. Using levels as outcomes allows us to include zeros when individuals are not employed and/or not in the largest strongly connected set, respectively. For comparison, we also estimate these levels regressions conditional on positive earnings, to assess how much of the full earnings penalty is missed by the conditioning necessitated by our use of the largest strongly connected set in the main results. As with the employment results shown above, we estimate these regressions on two random 50% samples of our trainee entrants due to computational constraints.

In order to make estimates from this exercise comparable with our main results, we once again rescale the earnings losses to represent the effect of a one standard deviation increase in the state-level unemployment rate and consolidate these effects by reporting a percentage loss in the PDV of real earnings over the first 10 years of an individual’s career:

$$\text{Levels-Based PDV Loss in First Decade of Career} = 100 \times \left(1 - \frac{\sum_{e=0}^9 [(\bar{y}_e + \sigma_U \hat{\beta}_e)(1+r)^{-e}]}{\sum_{e=0}^9 [\bar{y}_e(1+r)^{-e}]} \right) \%. \quad (32)$$

Table D2 presents the results of this exercise. The broad takeaway is that—in PDV percentage loss terms—restricting attention to the set of workers who are currently working in a FTDJ at a strongly connected employer misses a small but non-trivial portion of the scar imparted by graduating into a recession. We find that a one standard deviation increase in the unemployment rate at entry leads to a 4.44 or 4.24 percent loss in PDV earnings over the first decade of a trainee’s career, when we account for all earnings and include cases where earnings are 0 (Columns (1) and (2)). Losses are almost identical when focusing on the FTDJSC earnings and including cases where workers are either non-employed or employed outside of the largest strongly connected set as 0s (Columns (5) and (6)). However, losses appear smaller when we condition on FTDJSC earnings greater than 0 (observations with positive FTDJ earnings in the largest strongly connected set, Columns (7) and (8)), which is the same set of observations and outcome used in Figure 4 and the first row of Table 4, but just with the earnings outcome in levels instead of logs. Our best estimate, then, is that the conditioning required in our main results allows us to analyze and decompose a portion of the recession entry scar that accounts for around 80 percent of total earnings losses in percent PDV terms. We also note here that the levels outcome produces smaller percent PDV losses than do our log results. This likely owes to skewness in the levels earnings outcome.

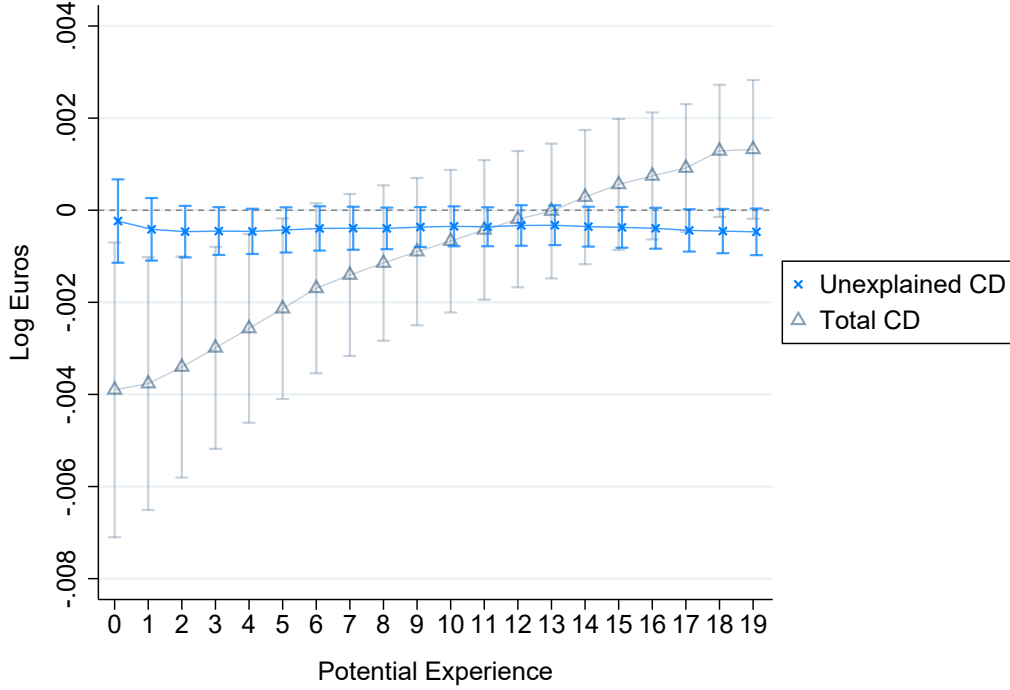
Table D2: Percent PDV Earnings Losses from a One SD Increase in Unemployment Rate at Entry in First Decade of Career, Based on Levels Regressions

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|----------------------------|--------------|------|--------------|------|-----------------|------|-----------------|------|
| Outcome: | All Earnings | | All Earnings | | FTDJSC Earnings | | FTDJSC Earnings | |
| Conditional on Outcome > 0 | | | ✓ | ✓ | | | ✓ | ✓ |
| 50% Random Sample | 1 | 2 | 1 | 2 | 1 | 2 | 1 | 2 |
| PDV Loss | 4.44 | 4.24 | 4.17 | 4.04 | 4.45 | 4.32 | 3.71 | 3.57 |

D.4 The Role of Industry, Occupation, and Location in Explaining Recession-Induced Compensating Differentials

As described in the main text, we assess how important industry, occupation, and location are in explaining our headline finding by regressing our compensating differential on industry by county fixed effects, then using the residual as the outcome in a version of Equation (9) that includes current occupation fixed effects. Figure D3, below, compares the estimated $\hat{\beta}_e$ from this regression with the estimated $\hat{\beta}_e$ from estimating Equation (9) with non-residualized compensating differentials as the outcome, without current-occupation fixed effects. It is both a visual illustration of our finding that a majority of the amenity buffer received by recessionary entrants is explained by industry, location, and occupation and an illustration that this buffer is most operative in the first ten years of their careers.

Figure D3: The Role of Industry, Occupation, and Location in Explaining Recession-Induced Compensating Differential Losses (Amenity Gains)



Notes: Lines connect coefficients $\hat{\beta}_e$, estimated by Equation (9). The specification is estimated on 6,845,618 individuals who are followed for all years in which they are working in a full-time annual dominant job for an employer that has an estimated fixed effect and value. Controls always included are potential experience fixed effects, year fixed effects, year of entry fixed effects, month of entry fixed effects, training occupation fixed effects, state of training fixed effects, age at labor market entry fixed effects, level of pre-training education fixed effects, a German national indicator variable, and a female indicator variable. The light blue exes represent $\hat{\beta}_e$ estimates where the outcome is compensating differentials residualized with respect to industry by county fixed effects and where controls also include current occupation fixed effects (“Unexplained CD”). The navy triangles represent $\hat{\beta}_e$ where the outcome is total compensating differentials (“Total CD” as in Figure 5 and Figure D1).

D.5 Robustness of Cyclical Employment Dynamics Results

Table D3: Cyclical Employment Dynamics at Low- and High-Amenity Employers

| | (1) | (2) | (3) | (4) | (5) |
|---|-----------------------|-----------------------|-----------------------|-----------------------|-------------------|
| Unemployment Rate (UR) | U_{st}^{all} | U_{st}^{all} | U_{st}^{all} | U_{st}^{all} | U_{st} |
| Industry FE | ✓ | ✓ | | ✓ | ✓ |
| Dropping Public Sector | | ✓ | | | |
| Weighted | ✓ | ✓ | ✓ | | ✓ |
| N | 4,663,047 | 4,429,353 | 4,663,097 | 4,663,047 | 4,663,047 |
| Outcome: Growth Rate | | | | | |
| Change in UR | -0.905 (0.113) | -0.945 (0.122) | -0.915 (0.112) | -1.359 (0.108) | -1.067 (0.111) |
| High amenity _{jt} | 0.023 (0.002) | 0.025 (0.002) | 0.024 (0.003) | 0.032 (0.003) | 0.023 (0.002) |
| Change in UR × High amenity _{jt} | 0.739 (0.122) | 0.776 (0.123) | 0.725 (0.121) | 1.252 (0.101) | 0.857 (0.139) |
| Outcome: Hiring Rate | | | | | |
| Change in UR | -0.960 (0.110) | -1.004 (0.114) | -1.059 (0.113) | -0.919 (0.133) | -1.012 (0.124) |
| High amenity _{jt} | 0.024 (0.002) | 0.027 (0.002) | 0.062 (0.005) | 0.022 (0.003) | 0.025 (0.002) |
| Change in UR × High amenity _{jt} | 0.660 (0.111) | 0.685 (0.114) | 0.712 (0.095) | 0.811 (0.090) | 0.757 (0.126) |
| Outcome: Separation Rate | | | | | |
| Change in UR | -0.056 (0.064) | -0.060 (0.067) | -0.144 (0.067) | 0.440 (0.066) | 0.055 (0.068) |
| High amenity _{jt} | 0.002 (0.002) | 0.002 (0.002) | 0.037 (0.003) | -0.011 (0.002) | 0.002 (0.002) |
| Change in UR × High amenity _{jt} | -0.079 (0.081) | -0.091 (0.089) | -0.013 (0.065) | -0.441 (0.079) | -0.101 (0.091) |

Notes: This table shows the effect of the change in the state-year unemployment rate on employer-level growth rates, hiring rates, and separation rates. See Equation (12) for definitions. Standard errors shown in parentheses are clustered at the state level. Where indicated with a ✓, regressions are weighted by mean employer size from year t through year $t - 1$. Column (1) replicates results from Table 6.

Appendix E Employer Effects Binned by Sector

Table E1: Employer Effects Binned within Sector

| Sector | Value | Employer Fixed Effect | Rent | Compensating Differential |
|---|-------|--------------------------|-------|------------------------------|
| B: Mining and quarrying | -0.09 | 0.66 | -0.07 | 0.73 |
| K: Financial and insurance activities | 0.27 | 0.81 | 0.19 | 0.62 |
| D: Electricity, gas, steam and air conditioning supply | 0.33 | 0.82 | 0.24 | 0.58 |
| U: Activities of extraterritorial organizations and bodies | 0.06 | 0.46 | 0.04 | 0.42 |
| F: Construction | -0.22 | 0.20 | -0.16 | 0.37 |
| J: Information and communication | 0.14 | 0.44 | 0.10 | 0.34 |
| T: Private households | -0.15 | 0.18 | -0.11 | 0.29 |
| L: Real estate activities | 0.16 | 0.29 | 0.12 | 0.18 |
| M: Professional, scientific and technical activities | 0.09 | 0.21 | 0.06 | 0.14 |
| C: Manufacturing | 0.13 | 0.17 | 0.09 | 0.08 |
| O: Public administration and defense; compulsory social security | 0.17 | 0.17 | 0.13 | 0.05 |
| E: Water supply; sewerage, waste management and remediation activities | 0.10 | 0.11 | 0.07 | 0.03 |
| G: Wholesale and retail trade; repair of motor vehicles and motorcycles | 0.10 | 0.02 | 0.07 | -0.05 |
| H: Transportation and storage | -0.07 | -0.19 | -0.05 | -0.14 |
| P: Education | 0.01 | -0.14 | 0.01 | -0.15 |
| R: Arts, entertainment and recreation | -0.24 | -0.32 | -0.17 | -0.15 |
| S: Other service activities | 0.00 | -0.26 | 0.00 | -0.26 |
| Q: Human health and social work activities | 0.02 | -0.32 | 0.01 | -0.33 |
| N: Administrative and support service activities | -0.54 | -0.75 | -0.39 | -0.35 |
| A: Agriculture, forestry and fishing | -0.41 | -0.90 | -0.30 | -0.60 |
| I: Accommodation and food service activities | -0.42 | -0.93 | -0.31 | -0.63 |

Notes: This table averages employer value, employer fixed effects, rents and compensating differentials—each residualized with respect to window fixed effects and then studentized—by sector. Sectors are sorted on the basis of compensating differentials. Sector classifications are based on the 2008 Industrial Classification of Economic Activities (WZ08) standard. Figure 2 is a visualization of this table.

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