Unemployment insurance reforms and labor market dynamics*

Benjamin Hartung† Philip Jung‡ Moritz Kuhn§

May 2024

Abstract

A key question in labor market research is how the unemployment insurance system affects unemployment rates and labor market dynamics. We provide new answers to this old question by studying one of the largest unemployment insurance reforms in recent decades, the German *Hartz reforms*. On average, lower separation rates into unemployment account for 76% of declining unemployment after the reform, a fact unexplained by existing research focusing on job-finding rates. Exploiting institutional changes by age, employment duration, and wages, we establish a causal link between the reform and changes in labor market dynamics. Relying on labor market theory, we generalize our empirical findings beyond the German case and establish separation rate changes as an important macroeconomic adjustment channel after UI reforms. We derive analytically that the change of separation rates increases in proportion to average unemployment duration suggesting an equally important role for most other European labor markets.

JEL-Classification: E24, J63, J64

Keywords: Unemployment insurance, labor market flows, endogenous separations

*We thank Jieun Kim and Leonardo Lievano for outstanding research assistance and Rudi Bachmann, Christian Bayer, Marcus Hagedorn, Simon Jäger, Leo Kaas, Tom Krebs, Iourii Manovskii, Kurt Mitman, Thijs van Rens, Joost Roettgers, Benjamin Schöfer, and Harald Uhlig for their comments and remarks. We thank Philipp vom Berge for help regarding the IAB data. We also thank seminar and conference participants at various places for their helpful comments and remarks. The usual disclaimer applies. Hartung, Jung, and Kuhn thank the German Research Foundation (*Deutsche Forschungsgemeinschaft*) for financial support of this research project as part of the priority program SPP 1764. Jung thanks the DFG SFB 823 for financial support. Kuhn thanks the German Research Foundation for funding under Germany´s Excellence Strategy – EXC 2126/1– 390838866. This paper should not be reported as representing the views of the European Central Bank (ECB). The views expressed are those of the authors and do not necessarily reflect those of the ECB.

†European Central Bank, Benjamin.Hartung@ecb.europa.eu

‡TU Dortmund and IZA, Philip.Jung@tu-dortmund.de

§University of Bonn, ECONtribute, CEPR, and IZA, mokuhn@uni-bonn.de

1 Introduction

A key question in labor market research is how the unemployment insurance (UI) system affects unemployment rates and labor market dynamics. We revisit this old question and provide new answers based on an analysis of one of the largest UI reforms in industrialized countries in recent decades: the German *Hartz reforms*. Economists have extensively studied how changes in the UI system affect job-finding rates either through their incentive effects on unemployed workers when searching for new jobs (Katz and Meyer (1990) and Schmieder and Von Wachter (2016)) or through their incentive effects on firms when posting new vacancies (Millard and Mortensen (1997), Krause and Uhlig (2012), Hagedorn et al. (2019)).[1] In this paper, we scrutinize the existing focus on job-finding rates (unemployment outflows) and draw attention to separation rates into unemployment (unemployment inflows). While the link between separation rates and the UI system is known in theory, little is known about its quantitative importance for the macroeconomy (Tuit and van Ours (2010)). In this paper, we establish separation rate changes as an important macroeconomic adjustment channel after UI reforms and discuss the conditions for its quantitative importance.

The Hartz reforms in Germany took place in the mid-2000s. In the decade after the reform, unemployment rates were cut in half. At the heart of the reform was an overhaul of the UI system that abolished long-term, wage-dependent unemployment assistance benefits and that also reduced maximum benefit duration for older, long-term employed workers. Using social security microdata, we document that three-quarters of the large decline in unemployment rates after the reform resulted from lower separation rates into unemployment, while the increase in job-finding rates only accounts for the remainder. We document large heterogeneity in the changes of separation rates across worker groups, with the largest reduction for long-term employed, older workers.

To establish a causal link between the UI reform and changes in separation rates, we exploit exogenous variation in treatment intensities of the reform across worker groups in a difference-in-difference approach. Our empirical estimates imply that separation rates and also wages of workers more affected by the reform decline more strongly. Our estimates therefore support a trade-off between wages and job-stability after the reduction in UI generosity. To generalize from these estimates of reform effects for treated subgroups to an estimate of the macroeconomic consequences of the UI reform, we rely on economic theory and develop a labor search model. The calibrated model is quantitatively consistent with the estimated reform effects for different worker groups and matches the documented macroeconomic changes in labor market dynamics and unemployment rates over time. The model also allows us to extend our results beyond the

[1] The existing literature on job search incentives builds on theoretical grounds in the large body of literature studying the (optimal) design of UI systems. This literature focuses on the trade-off between providing insurance and the cost of additional unemployment due to reduced search effort (Baily (1978), Shavell and Weiss (1979), Hopenhayn and Nicolini (1997), and Chetty (2006)). Recently, researchers have shown renewed interest in quantifying the incentive effects for firms' vacancy postings in relation to changes in UI benefits during the Great Recession in the United States (Hagedorn et al. (2019), Hagedorn et al. (2015), Chodorow-Reich and Karabarbounis (2019)) and Sweden (Fredriksson and Söderström, 2020).

Germany case. We derive analytically that a quantitatively important response of separation rates to a UI reform depends on the average unemployment duration in a labor market. This result rationalizes the existing focus of the literature on job-finding rates for the United States where unemployment duration is short. However, our paper highlights that for most European labor markets that are characterized by long average unemployment duration a prominent role of separation rate changes ought to be expected in response to UI reforms.

The main data source for our empirical analysis is the social security microdata of individual employment histories in West Germany from the Sample of Integrated Labour Market Biographies (SIAB). We construct worker-flow rates for one decade before and after the UI reform and find that separation rates declined by 28% after the reform, while job-finding rates increased by only 13%. As a consequence, changes in separation rates account for 76% of the decline in unemployment rates. This stylized fact is robust to a wide range of sensitivity checks and in alternative data sources. The average decline in separation rates hides a lot of heterogeneity that we exploit to establish a causal relationship between the UI reform and changes in labor market dynamics. The first dimension of heterogeneity that we exploit are heterogeneous changes in maximum benefit duration by age and employment duration. On average, we find that separation rates of long-term employed workers fell by up to 60%, while short-term employed workers show a comparatively modest decline of 20% in their separation rates. Using a difference-in-difference regression, we document a statistically significant effect of maximum benefit duration on separation rates. As a second dimension of heterogeneity, we exploit that social assistance benefits constitute a lower bound for benefits that remained unchanged by the reform. The existence of this constant lower bound turns the group of low-wage workers into a natural control group for the impact of the reform as their benefit level remained unaffected. Accordingly, we document that for the control group of low-wage workers there has been no change in separation rates after the reform but we estimate 30% lower separation rates for workers with higher wages and cuts in their expected benefit levels. As we restrict the sample for this regression to workers without changes in maximum benefit duration, we can rule out any confounding effects from changes in maximum benefit duration. These partial reform effects establish causality of the UI reform for separation rate changes. In a further step of our empirical analysis, we use the methodology in Elsby et al. (2013) to extend estimates for separation rates and job-finding rates for a large group of OECD countries. The estimates for Germany based on this independent data source corroborate our findings about the relative importance of separation and job-finding rates based on social security data. We use the OECD data to estimate a synthetic control no-reform counterfactual for Germany using the methodology of Abadie et al. (2010). The synthetic-control estimate provides additional evidence for a strong fall of German separation rates and a modest increase of job-finding rates after the UI reform relative to the estimated no-reform counterfactual. In a final step, we explore wage effects of the reform by exploiting differences in treatment intensity of the UI reform by age and find that wages of older, more affected workers, declined by 0.9% to 1.5% as a result of the reform. Together with the evidence on separation rates, this result supports the trade off between wages and job stability.

In the second part, we generalize our empirical results by demonstrating that the documented effects on separation rates after the UI reform are qualitatively and quantitatively consistent with economic theory. We develop a general equilibrium labor market search model with worker heterogeneity, aggregate fluctuations, and endogenous separation decisions. Workers in the model differ in their employment status, skills, job duration, wages, and UI benefit eligibility. Our model incorporates key institutional features of Germany's UI benefit eligibility rules with respect to the dependence on employment duration and wages, as in Krause and Uhlig (2012).[2] Our model also incorporates all three channels from the literature on how UI reforms affect labor market dynamics: workers' incentives to search and accept job offers, firms' incentives to post vacancies, and the decision of workers and firms to separate. Endogenous separation decisions lead to falling separation rates after a reduction in UI generosity (Pissarides, 2000, Ch.2). We calibrate the model to the pre-reform period and introduce the UI reform by abolishing long-term, wage-dependent benefits and shortening the benefit duration for long-term employed workers. After the reform, the model closely matches the observed time series for average separation and job-finding rates. The model also matches the empirically documented heterogeneous responses. In the model, as in the data, the long-term employed, high-wage workers are most adversely affected, and the model-implied elasticity of separation rates with respect to maximum benefit duration aligns well with our empirical estimates. We use the model to perform counterfactual simulations of the German labor market in the absence of the UI reform. Absent the reform, the model predicts unemployment rates that would have been 50% higher by 2014 than what has been observed in the data. This counterfactual model prediction closely tracks our synthetic control estimates for German unemployment rates absent the UI reform.

In the model, the UI reform affects workers' search incentives, firms' incentives to post vacancies, and separation decisions. The model structure imposes no predetermined relative importance on the different channels, so the question arises on how to discipline the relative importance of these three adjustment channels. In theory, there is a tight link between aggregate labor market fluctuations from productivity fluctuations and the responsiveness to changes in UI benefits (Costain and Reiter (2008a)). Through the lens of the model, productivity changes and benefit changes both directly affect the value of employment relative to the outside option so that pre-reform business-cycle fluctuations inform the key reform elasticity of separation rates with respect to changes in UI benefits. Based on this insight, we calibrate the model to be consistent with business-cycle moments for separation rates and job-finding rates before the UI reform. For the responsiveness of workers' search behavior, we target existing estimates on the elasticity of the search intensity to changes in UI benefits from the empirical literature.[3]

[2] We share several modeling choices with Krause and Uhlig (2012) but differ in our focus. Their findings and calibration strategy focus on changes in job-finding rates through the effects on vacancy postings, rendering separation rates effectively exogenous in their quantitative analysis. Their model also does not include aggregate fluctuations to impose discipline on the elasticity of separation and job-finding rates, which we exploit for the calibration as described below.

[3] A broad empirical consensus has emerged suggesting that this effect is modest. Typical estimates find that granting one additional month of UI benefits leads to 0.15 more months of unemployment (Chetty (2006),

Our calibration only targets unconditional moments of worker-flow rates but closely matches the time series dynamics of labor market flows before the reform, thereby providing support for the model mechanism. After the reform, the model still matches the time series of labor market flow rates very closely, lending support to the independently calibrated elasticities.

Using an analytically tractable version of the model, we derive the relationship between UI reforms and business-cycle elasticities that underlies our calibration strategy. We also identify low average job-finding rates as the key condition of a strong reaction of separation rates to UI reforms. Intuitively, separation decisions trade off staying at the current job against a separation and its associated costs from unemployment. How costly a separation into unemployment from an employed worker's perspective is depends on the average unemployment duration during which the worker expects to receive benefits instead of a wage. A UI reform that reduces benefit generosity increases the costs of a separation and it increases them by more, the lower the job-finding rate and the longer the average unemployment duration after a separation is. A stronger change of separation costs leads then to a stronger change of separation decisions. In short, a UI reform leads to stronger separation rate changes the longer the expected unemployment duration is. Long unemployment spells as in Germany and many other European countries therefore amplify the effect on the separation decision after a UI reform compared to the United States where job-finding rates are high and unemployment duration is short. This latter result reconciles our findings with results on the U.S. labor market that highlight the important role of changes in job-finding rates, for example, in Hagedorn et al. (2019).

We also use our microfounded framework to quantify the welfare effects of the reform for different labor market participants. We consider welfare effects abstracting from compensating transfers that the government could finance because of the lower spending on UI benefits after the reform. We find that losses amount to 2.1% in terms of consumption-equivalent variation for the recipients of unemployment assistance benefits that have been abolished by the reform. Among the employed, we find the largest welfare losses of 0.64% among the long-term employed, high-wage workers. Long-term employed workers account for almost two-thirds of the German labor market, and the fact that their separation rates are the lowest suggests that these workers are very detached from any changes in the UI system. Yet, we show that this is not the case and that in hindsight, their large welfare costs might explain the widespread discontent in the population with the reform.

Two potentially important policy implications arise from our findings. The first relates to UI reform proposals taking the German UI reform as a role model. Regarding the political feasibility of such reforms, our findings imply that appropriate compensation schemes have to be designed to avoid discontent in large parts of the electorate, as we show that a quantitatively important role for changes in separation rates should be expected in most European countries. Second, the strong reaction of separation rates after changes in UI generosity suggests that similar reactions ought to be expected when implementing other social security reforms such as early retirement programs or disability insurance programs, especially as job-finding rates out

Schmieder and Von Wachter (2016)).

of these programs are low or even zero.

This implication for early retirement reforms is also supported by the existing empirical literature that looks at separation-rate effects for older workers after changes in UI generosity. Our findings align with the empirical results in Jäger et al. (2023), Kyyrä and Wilke (2007), and Dlugosz et al. (2014) that support a causal relationship between separation rates and UI generosity. Jäger et al. (2023) explore an extension of maximum UI benefit duration by age on older male workers in Austria and find large increases in separation rates after an increase in benefit generosity. Kyyrä and Wilke (2007) also look at older workers and their transition to early retirement in Finland. They find strong effects on separation rates from postponing the option for early retirement. Dlugosz et al. (2014) look at a similar variation in maximum UI benefit duration for older, long-term employed workers in Germany in 2006. They provide bounds on the long-run effect on separation rates in line with our results. Schmieder et al. (2012) focus on job-finding rates but also report separation flows of prime-age workers with strong labor market attachment in Germany. They find flat age profiles of separation flows and statistically significant but small changes at the age thresholds where UI generosity increases. In theory, such age-specific extensions of UI generosity will lead to a flattening of the slope of an otherwise falling separation-rate age profile. A falling age profile is observed in labor markets without age-specific extensions of UI generosity, for example, the United States (Jung and Kuhn, 2018). The flatting of the profile follows from the forward-looking separation decisions of agents anticipating the policy discontinuity by age. Their empirical finding of a flat age profile is therefore what ought to be expected if there are sizable effects of increasing UI generosity on separation rates. In the model, we capture these general equilibrium feedbacks from an increase in UI generosity. While most microeconometric studies focus on a selective group on typically older workers with long tenure, the flow analysis in Carrillo-Tudela et al. (2021) has a macroeconomic focus but restricts the data on employment histories in a way that leads to time-varying selection effects and additional flows to and from non-participation. Their findings still support a strong decline of the separation rate in the decade after the UI reform. Our work also relates to the growing literature that explores unemployment dynamics in Germany after the Hartz reforms for which some observers have coined the term *German labor market miracle* (Burda and Seele, 2016). What distinguishes our work from the existing literature is the focus on changes in separation rates into unemployment. Existing research focuses on job-finding rates as the key margin of adjustment by highlighting changes in search effort (Krebs and Scheffel (2013)), changes in matching efficiency (Launov and Wälde (2013), Hertweck and Sigrist (2015), and Klinger and Weber (2016)), changes in employer hiring standards (Hochmuth et al., 2021), or changes in vacancy posting behavior (Krause and Uhlig (2012)).

The remainder of the paper is structured as follows. We provide in Section 2 a detailed description of the UI reform part of the Hartz reforms and explain the institutional variation that induces the differences in treatment intensities that we exploit in our empirical analysis. In Section 3, we describe our data and present the empirical results. We describe the labor market search model in Section 4. Section 5 shows the model results and discusses the counterfactual analysis. We conclude in Section 6. An appendix with additional results and a wide range of robustness and sensitivity checks follows.

2 The UI reform

In 2002, the German government entrusted an expert commission consisting of various representatives from business, unions, and academia with the task of working out reforms for the German labor market. The chairman was Peter Hartz, at that time director of human resources at Volkswagen. The subsequent reforms are commonly referred to as the *Hartz reforms*.[4] The reforms were enacted in four separate legislative packages commonly referred to as *Hartz I* to *Hartz IV* between 2003 and 2005 and consisted of measures from subsidies for self-employment to the restructuring of the federal employment agency and an overhaul of the unemployment insurance system.[5]

We focus our analysis on the fourth step of the reform package (*Hartz IV*) that constituted a reform of the German UI system. The reform changed the former German three-tier system of unemployment benefits, unemployment assistance, and subsistence benefits into a two-tier system of unemployment and subsistence benefits. The reform implied a drastic cut in UI generosity for long-term employed and older workers. Before the reform, long-term employed workers were after their unemployment benefits expired eligible to long-term, wage-dependent unemployment assistance. After the reform, workers received instead of unemployment assistance benefits subsistence benefits once unemployment benefits expired. In addition, the maximum unemployment benefit duration was reduced for workers 45 years and older. Benefit levels in the first and third tier, unemployment benefits and social assistance, remained however unaffected. Hence, all workers received after the reform lower benefits after their unemployment benefits expired and for older, long-term employed workers unemployment benefits also expired earlier.

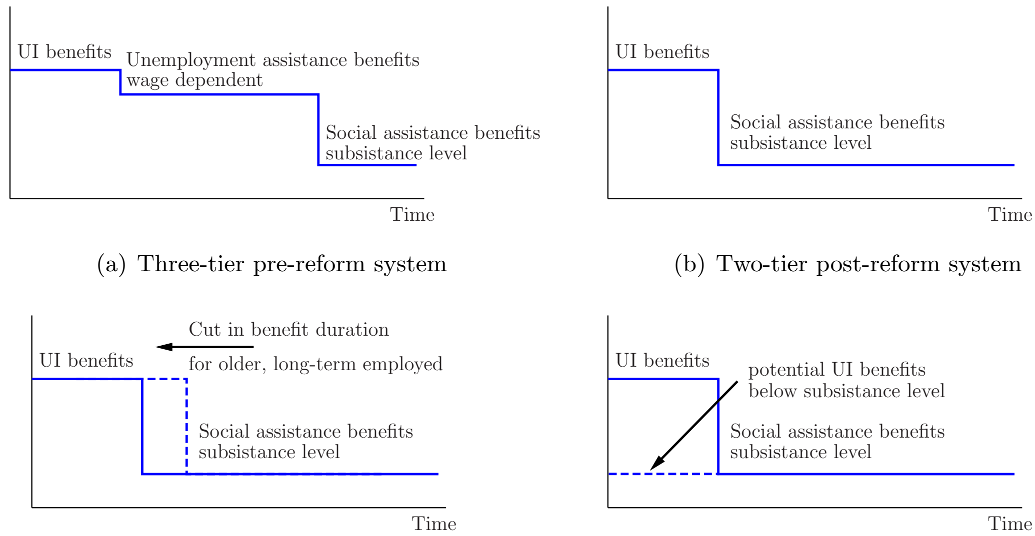
Figure 1(a) sketches the three-tier UI system *before* the reform with UI benefits that are tied to the last wage, unemployment assistance benefits that long-term unemployed workers receive after their unemployment benefits expired, and as the third tier social assistance benefits that are need based at subsistence level and independent of the last wage. Figure 1(b) sketches the UI system *after* the UI reform that abolished the second tier of unemployment assistance benefits. After the reform, workers for whom UI benefits expire receive social assistance benefits at the subsistence level. Generally, this change applied to all workers but institutional differences of benefit eligibility led to heterogeneity in the impact of these changes.

The first dimension of heterogeneity stems from the maximum duration of benefit eligibility. This maximum benefit duration depends on previous employment duration and age and was cut differentially by age and employment duration. Figure 1(c) sketches how changes in maximum benefit eligibility led to heterogeneity in treatment intensity. Older, long-term employed workers

[4]The official title of the commission was the *Commission for Modern Labor Market Services*.

[5]The official title of the acts were *First, Second, Third,* and *Fourth Act for Modern Labor Market Services*. Steffen (2008) provides a detailed chronicle of the German social security system. We provide further details in Appendix A.

Figure 1: Stylized pre- and post-reform UI system and heterogeneous treatment effects



(a) Three-tier pre-reform system

(b) Two-tier post-reform system

(c) Variation in treatment intensity by age and employment duration

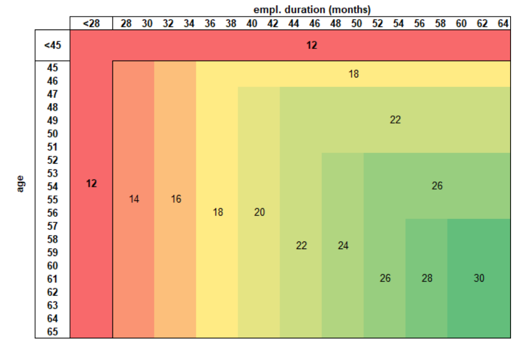
(d) Treatment heterogeneity along the wage distribution

Notes: Stylized pre- and post-reform UI system. The vertical axis shows the qualitative level differences in replacement rates for the average worker. The horizontal axis shows unemployment benefit duration. Top left panel shows three-tier pre-reform UI system. Top right panel shows the two-tier post-reform UI system. Bottom left panel shows the heterogeneity in treatment intensity by age and employment duration arising from a reduction in maximum benefit duration. Bottom right panel shows heterogeneity in treatment intensity along the wage distribution from subsistence benefits for low-wage workers.

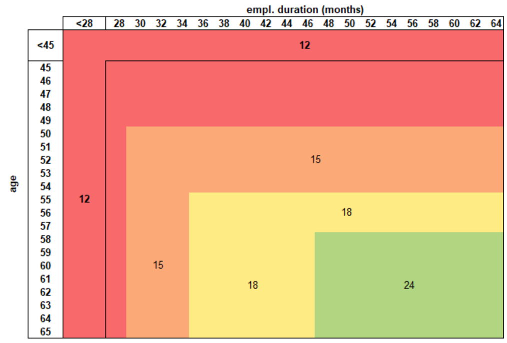
received, in addition to the abolition of unemployment assistance benefits, a cut in maximum benefit duration, implying a larger treatment intensity for these workers. This cut in maximum benefit duration became effective in 2006. Such institutional variation by age is also exploited to generate variation among the unemployed when estimating the effect of UI generosity on search behavior (e.g., Schmieder et al. (2012); Price (2019)).

Figure 2 shows the maximum unemployment benefit duration by employment duration and age before and after the reform. Changes in maximum benefit duration only affected long-term employed older workers, specifically, workers age 45 and older with at least 28 months of previous employment duration. There were no changes in maximum benefit duration for younger and short-term employed workers. Looking at the pre-reform situation in Figure 2(a), we see that for workers younger than 45 years, the maximum benefit duration was 12 months. For older workers, we find a steep gradient in employment duration from 14 months after 30 months of previous employment to up to 30 months after 60 months of previous employment. Comparing this pattern to the post-reform regulation in Figure 2(b), we see that there is much less variation and that especially older, long-term employed workers see a strong decline in their maximum benefit duration. For example, a 49-year-old worker with four years of previous employment receives after the reform UI benefits for up to 12 months, while before the reform she received UI benefits for up to 22 months. Figure 2(c) shows the relative changes in UI benefit duration

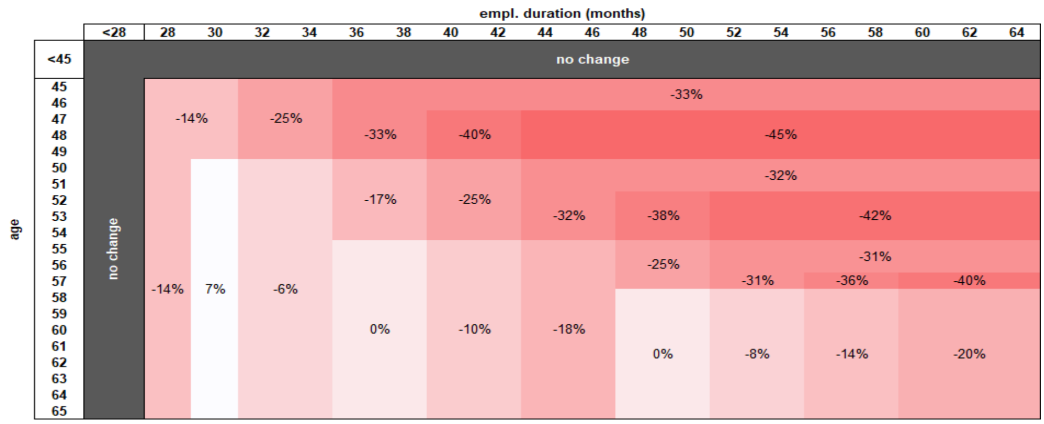
Figure 2: Changes in maximum benefit duration by age and employment duration



(a) Maximum benefit duration pre-reform



(b) Maximum benefit duration post-reform



(c) Change in maximum benefit duration

Notes: Maximum benefit duration for unemployment benefits in months by age and employment duration. Employment duration refers to the reference period prior to the unemployment spell. Panel (a) shows maximum benefit duration before the reform. Panel (b) shows maximum benefit duration after the reform in 2008. Panel (c) shows the relative change in maximum duration in percentage for each combination of age and employment duration. Each panel shows in rows age at the time of unemployment and in columns previous employment duration in months.

for the different groups from the pre- to the post-reform period. We see that the largest decline happened for workers with more than three years of previous employment duration between ages 45 and 55. We will exploit this variation in the changes of maximum benefit duration in a difference-in-difference design in our empirical analysis in Section 3.

Figure 1(d) sketches the second dimension along which the institutional design of the UI reform created heterogeneity in treatment effects. Benefits at subsistence level existed before and after the UI reform and are a traditional property of the German social security system. The fixed subsistence benefit level remained unaffected by the UI reform what implies that any cut in unemployment benefit levels affected only workers who are eligible to unemployment benefits above this subsistence level. Workers for whom UI benefits are below subsistence level are eligible for supplementary benefits ("Aufstocker") both before and after the reform. For these workers, abolishing wage-dependent unemployment assistance benefits had no effect on their potential UI benefit level because their potential benefit level stayed at the subsistence level and remained unaffected by the reform. This variation along the wage distribution provides us with a second dimension of heterogeneity to exploit differential cuts in expected benefit levels from the reform relative to a control group of low-wage workers. In the data, we cannot directly identify these control-group workers because need-based subsistence benefits depend on household characteristics that remain unobserved in the social security data. For the period starting in 2008, there is data on UI benefit recipients who receive supplementary social security benefits to match the subsistence level. We show the data in Appendix Figure A.1 and find that these are about 10% of benefit recipients. To be conservative, we therefore take the bottom 10% of the wage distribution as our control group that remained unaffected by abolishing unemployment assistance benefits.

A final institutional detail of the reform is important for the setup of our empirical analysis. We consider the years from 2005 to 2008 respectively 2011 as a transition period after the reform. The reason is that to cushion the cut in benefit generosity in the aftermath of the UI reform, the government introduced in §24 SGB II additional supplement benefits for newly unemployed workers. Specifically, former UI recipients transiting from unemployment to subsistence benefits were for 24 months after their UI benefits expired eligible to supplementary benefits equal to two-thirds of the difference of their previous UI benefits and their new benefit level with a maximum of 160 Euros for singles, 320 Euros for couples, and 60 Euros per dependent child (Steffen, 2008).[6] These benefits were cut in half after 12 months and expired completely after 24 months. The regulation was abolished by the end of 2010.[7] Appendix Figure A.2 shows the number of recipients of these supplementary benefits and that it declined strongly between 2005 and 2008 when it leveled off. We therefore take the period from 2005 to 2008 as a transition period after the reform. Alternatively, we also always report results for a post-reform period starting in 2011 when all supplementary benefits expired but that also excludes the financial crisis. Appendix A provides further details on the German unemployment insurance system and its reform.

3 Data and empirical results

Our main data source to study the consequences of the UI reform on labor market dynamics is the microdata on individual employment histories from the Sample of Integrated Labour Market Biographies (SIAB) provided by the Institute for Employment Research (IAB) for the period from 1975 to 2014.[8] The SIAB is a 2% representative sample of administrative data on all workers who are subject to social security contributions and on all unemployed workers in Germany. It excludes self-employed and civil servants, thus covering approximately 80% of Germany's labor force. Apart from its large size (1.8 million individuals) and its long panel

[6]Price (2019) who studies the search behavior of the unemployed abstracts from such a transition period but also notes that *"(...)some long-term beneficiaries were also eligible for temporary supplemental payments (...)"* (p.7).

[7]This was part of the *Haushaltsbegleitgesetz* 2011.

[8]We use the weakly anonymous Sample of Integrated Labour Market Biographies (SIAB), 1975-2014. The data were accessed on-site at the Research Data Centre (FDZ) of the Federal Employment Agency (BA) at the Institute for Employment Research (IAB) and via remote data access at the FDZ.

dimension (up to 40 years), one further advantage of the administrative data is that they are virtually free of measurement error for the variables of interest in this paper. The data contain the exact start and end dates of each employment and unemployment spell and comprise in total almost 60 million individual spells. See Antoni et al. (2016) for further details on the data.

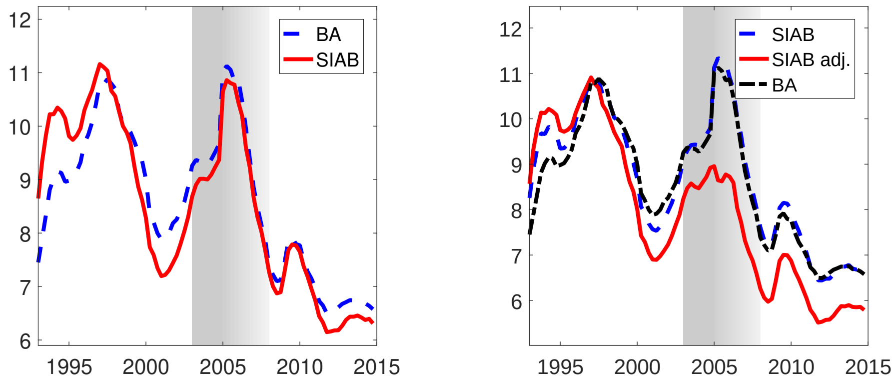
3.1 Sample selection, construction of worker-flow rates, and inflow correction

We restrict our sample to workers in West Germany and exclude marginal employment in our benchmark sample. We drop a few individuals with missing information on employment status or missing geographic information, and all individuals who only receive social assistance benefits while in the sample. We consider the effect of including marginal employment and results for East Germany in our sensitivity analysis (Appendix D).

The data contain daily employment histories, and we follow Jung and Kuhn (2014) to aggregate daily labor market histories to histories at a monthly frequency. We assign monthly employment spells based on a reference week within each month. We report as the separation rate the share of employed workers entering into unemployment from one month to the next (unemployment inflows) and as the job-finding rate the share of unemployed workers entering into employment between months (unemployment outflows). We assign the employment state in the reference week following a hierarchical ordering where employment supersedes unemployment and unemployment supersedes out of the labor force. This approach closely follows labor force surveys such as the Current Population Survey (CPS) for the United States. We count workers as employed if they are employed full- or part-time or work as apprentices. We count workers as unemployed if they are registered as unemployed at the employment agency, which requires that they are actively looking for a job. Registration is required to be eligible for unemployment benefits. The German unemployment insurance system distinguishes between unemployed workers and benefit recipients. In the microdata, reliable information on the registered unemployment status is available from 2000 onward. We use this information to assign employment states. We assign employment states for earlier periods based on records of benefit-recipient status and compute worker-flow rates based on benefit-recipient status before 2000. We construct growth rates of these worker-flow rates and use these growth rates to extend the registration-based flow rates starting in the year 2000 backward. This leaves the dynamics of the flow rates unaffected but removes the level differences between the two definitions. We provide further details on the construction of monthly employment states and transition rates in Appendix B. For our empirical analysis, we focus on the decade from 1993 to 2002 to document worker flows before the first reform steps were implemented. We report the entire time series of worker flows for the period after the reform and take the periods from 2008 respectively 2011 to 2014 as the post-reform period when the transition period after the UI reform was completed (Section 2).

The goal of our empirical analysis is to study the changes in labor market dynamics that determine the evolution of the unemployment rate. We therefore demonstrate first that the microdata match the macroeconomic trends of the unemployment rate. The microdata do not include public servants (*Beamte*), and hence, for the microdata to be consistent with the reported unemployment rates by the German employment office, public servants have to be included. Figure 3(a) shows the unemployment rate for West Germany as reported by the German federal employment agency and the unemployment rate constructed from the SIAB microdata for the period between 1993 and 2014.[9] Both unemployment rates track each other closely in trend and level, so we rely on them to study the underlying changes in labor market dynamics. In Appendix B, we demonstrate that using only the constructed worker-flow rates between employment and unemployment in a two-state stock-flow model matches the dynamics of the unemployment rate over time very well. We also consider a three-state model of unemployment with flows in and out of the labor force but find no notable improvement in accounting for the dynamics of the unemployment rate compared to the two-state model. We therefore focus on the two-state model for our analysis.

Figure 3: West German unemployment rates (1993-2014)



(a) BA and SIAB (b) SIAB with inflow correction

Notes: Left panel shows unemployment rate as reported by the employment agency (BA) (blue dashed line) and unemployment rate constructed from the SIAB microdata including imputed numbers for public servants not covered by the microdata (red solid line). Right panel shows unemployment rate from SIAB microdata and employment agency as in the left panel (dashed blue and black lines) and unemployment rate from SIAB microdata after inflow correction (solid red line). The grey area marks the reform period and the fading out indicates the transition period after the reform. Data are quarterly averages of seasonally adjusted monthly rates.

The data in Figure 3(a) show a large spike in unemployment in January 2005. The spike reflects regulatory changes in the UI system that became effective in January 2005. These regulatory changes required all nonemployed who are able to work to register as unemployed to remain eligible for UI benefits. This change caused a large inflow of former social assistance recipients and spouses of unemployed into the unemployment pool and poses a challenge to obtaining a consistent measurement of worker flows over time. To account for this effect, we propose an *inflow correction* for constructing comparable and consistent transition and unemployment rates for this period.

The key challenge for this adjustment is that we cannot directly observe workers who were forced

[9] The German employment office reports two unemployment rates. The unemployment rate for dependent employment that we rely on excludes self-employed workers. The employment office also reports an unemployment rate including all employees.

Table 1: Worker characteristics of entrants into unemployment

	entrants from N			other U	
	2004-01	2005-01	2005-01 (corr.)	2004-01	2005-01
female	43.3%	60.9%	45.8%	41.1%	42.1%
age	36.9	37.3	36.0	40.9	40.9
high school	23.2%	44.2%	32.6%	18.2%	18.8%
vocational training	70.3%	52.9%	62.9%	76.4%	76.0%
college	6.5%	2.9%	4.6%	5.4%	5.3%

Notes: Demographic characteristics of workers who transit to unemployment from out of the labor force (*entrants from N*) or all other states (*other U*) in January 2004 and January 2005. The column for the entrants from N labeled *corr.* shows characteristics after applying the inflow correction. Row *female* shows the share of females in inflows, row *age* shows average age, and the bottom three rows show the shares of workers with at most high school education, vocational training, and a college education.

to register as unemployed to retain their unemployment benefit eligibility. We therefore exclude persons who simultaneously satisfy three conditions: (1) entered unemployment in the first six months of 2005,[10] (2) had a nonemployment spell before registering as unemployed, and (3) did not work for at least one month until the end of 2006. We compare in Table 1 the characteristics of new entrants into unemployment from out of the labor force in January 2004 and January 2005.[11] We find large differences across the two years. Comparing columns 1 and 2 of Table 1, we observe that in January 2005, new entrants are slightly older, substantially more female (61% versus 43%), and less educated (44% versus 23% with high school or less). When looking at all other entrants into unemployment (columns *other U*), we find that worker characteristics do not differ notably for this other group of workers in January 2004 and 2005. Our inflow correction excludes entrants into the unemployment pool in early 2005 who are very detached from the labor market and are likely to have registered as unemployed solely because of the new registration requirements in 2005. Column 3 of Table 1, *entrants from N*, reports worker characteristics for entrants after the inflow correction. After the inflow correction, the worker characteristics of entrants in 2005 resemble those of the entrants in 2004 much more closely, although some differences still remain. We refer to the sample after excluding these persons as the *inflow-corrected sample* (column 3), and we will use this sample as our benchmark sample for the rest of the paper.

Figure 3(b) shows the unemployment rate of the inflow-corrected sample (solid red line) and the full sample (dashed blue line). The spike in January 2005 disappears almost completely in the inflow-corrected sample. The persistently lower level of the unemployment rate in the inflow-

[10] There is evidence that administrative problems and incomplete data records during the transition period make the records for the affected group in the first months after the reform less reliable.

[11] Out of the labor force is not directly observed in the data, and we assign out of the labor force as a residual employment state to nonemployed workers who have intermittent nonemployment spells that are not unemployment spells.

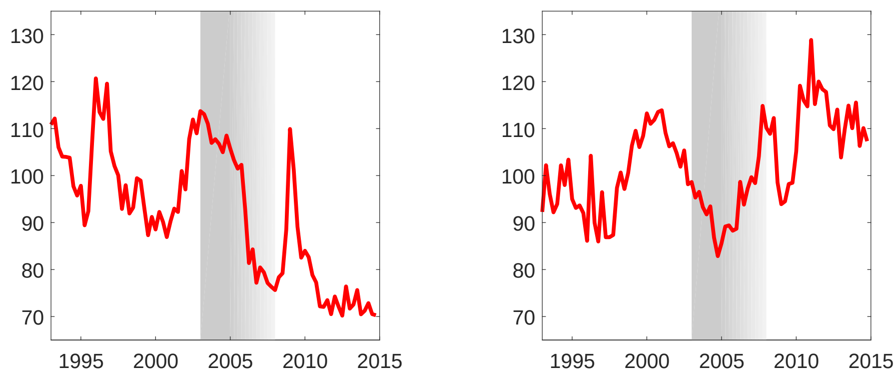
corrected sample shows that the inflow of formerly nonemployed persons into the unemployment pool in early 2005 changed the composition toward persons who are less attached to the labor market. Given that we remove these workers completely from the sample, we also change unemployment rates before 2005, but this change is small. In 2014, unemployment rates in the inflow-corrected sample are about 0.75 percentage points lower. Looking at relative changes, we find that the inflow correction reduces the decline in unemployment rates after 2005 from roughly 40% to 30%.

In Appendix D.1, we provide a sensitivity analysis for skipping the inflow correction. We find that our key empirical result of a stronger separation rate change is reinforced if we skip the inflow correction because the unemployed workers who enter in 2005 and whom we exclude using the inflow correction have on average lower job-finding rates. Additionally, we provide in Sections 3.4 and 5.1 independent evidence on labor market dynamics in Germany based on OECD data that were not subject to these regulatory changes. These independent estimates on transition rates and the evolution of the unemployment rate corroborate the empirical results from our benchmark sample of the inflow-corrected social security data.

3.2 Descriptive results

For our analysis, we consider the years 2003 and 2004 as the period of the reforms, the years from 1993 to 2002 as representative of the labor market situation before the reform, and the years from 2008 to 2014 as representative of the labor market situation after the reform. Throughout the paper, we indicate in all figures the reform period as gray shaded area and the transition period with a fading gray shade. We also report results with 2011 to 2014 as the post-reform period when all supplementary benefits expired and excluding the Great Recession. In total, the sample period includes three recessions and, in particular, the Great Recession.

Figure 4: Separation and job-finding rates (1993-2014)



(a) Separation rate (indexed) (b) job-finding rate (indexed)

Notes: Left panel shows separation rate and right panel job-finding rate for West Germany from 1993 to 2014. Both series have been indexed to their pre-reform level (1993-2002 = 100). The grey area marks the reform period and the fading out indicates the transition period after the reform. Data are quarterly averages of seasonally adjusted monthly rates.

Figure 4(a) shows the relative change in the separation rate for the period from 1993 to 2014.

The separation rate is indexed to its average pre-reform level (1993-2002 = 100). The level of the separation rate is low in the German labor market even before the reform, only 0.6% of workers transit from their employer to unemployment each month (Table 2). We focus in our analysis on percentage changes of transition rates rather than level changes because relative (percentage) changes of transition rates directly translate into relative changes of the unemployment rate.[12] For the separation rate, we find a substantial 28% decline between the pre-reform average and the separation rate during the post-reform period. When we consider the post-reform average including the Great Recession, the decline is smaller but still at 22%. It is interesting to note that separation rates spiked during the Great Recession, with an increase of about 40% relative to their 2007 level. We will return to the experience during the Great Recession when discussing counterfactual labor market dynamics for the post-reform period (Section 5.1).

Table 2: Pre- and post-reform unemployment rates, transition rates, and steady-state decomposition

				2008-2014		2011-2014	
	1993-2002	2008-2014	2011-2014	Δ	$\frac{\Delta\pi}{\Delta\bar{u}}$	Δ	$\frac{\Delta\pi}{\Delta\bar{u}}$
unemployment rate	10.5%	7.6%	7.2%	-28%		-32%	
separation rate	0.6%	0.5%	0.5%	-22%	75%	-28%	76%
job-finding rate	5.2%	5.7%	5.9%	10%	31%	13%	32%

Notes: Columns 2-4 show the level of the unemployment rate, separation rate, and job-finding rate before the UI reform (1993-2002), after the UI reform including the Great Recession (2008-2014), and excluding the Great Recession (2011-2014). Columns labeled Δ report the percentage change in rates from before to after the reform. Columns labeled $\frac{\Delta\pi}{\Delta\bar{u}}$ show the relative contribution to changes in the steady-state unemployment rate from changes in separation and job-finding rates. $\Delta\bar{u}$ indicates the change in the steady-state unemployment rate from before to after the reform based on average transition rates before and after the reform.

Figure 4(b) shows the relative change in the job-finding rate over time, again indexed to its average pre-reform level. Job-finding rates are before the reform slightly above 5% and increase to slightly below 6% after the reform (Table 2). In relative terms, the increase until 2014 constitutes a 13% increase in the job-finding rate. If we include the Great Recession in the post-reform average, the increase amounts to only 10%. During the Great Recession, job-finding rates declined by 20%, which is a modest decline given the size of the shock, and job-finding rates also recovered quickly compared to previous recessions (Jung and Kuhn, 2014). Compared to the 28% decline in the separation rates, the 13% increase in job-finding rates suggests that declining separation rates were the main driver behind the decline in unemployment rates over the decade following the UI reform. The relative differences in changes remain largely unaffected

[12]Using a two-state model of the unemployment rate with π_{eu} denoting the separation rate and π_{ue} the job-finding rate, the steady state unemployment rate is $u = \frac{\pi_{eu}}{\pi_{eu}+\pi_{ue}} \approx \frac{\pi_{eu}}{\pi_{ue}}$ with the approximation being valid because the job-finding rate is an order of magnitude larger than the separation rate. From this expression, the mapping from relative changes of transition rates to relative changes of the unemployment rate becomes immediately apparent.

when we include the Great Recession (22% versus 10%). In both cases, the decline in separation rates is more than twice as large as the increase in job-finding rates.

Table 2 uses a steady-state decomposition based on a two-state stock-flow model to quantify the relative contribution of separation rates and job-finding rates in explaining the 32% decline in the unemployment rate until 2014.[13] The columns labelled $\frac{\Delta\pi}{\Delta\bar{u}}$ of Table 2 report the relative contributions of changes in the separation rate and the job-finding rate to changes of the unemployment rate over time. The declining separation rate accounts for 75% respectively 76% of the decline in the unemployment rate depending on the start of the post-reform period. This large contribution of changes in the separation rate to changes in the unemployment rate implies that explanations that focus on the job-finding rate, either from changes in search effort or from changes in contact rates for unemployed workers from more vacancy postings, fall short in explaining the German experience.

3.3 Heterogeneity and causal evidence

The average decline in separation rates hides a lot of the heterogeneity. We trace this heterogeneity back to the institutional variation of the UI reform (Section 2) and use it to establish a causal link from the UI reform to the observed changes.

We proceed in two steps. In a first step, we provide descriptive evidence for heterogeneous changes in separation rates by age and employment duration. In the second step, we rely on a difference-in-difference analysis to establish a statistically significant and causal impact of the UI reform on separation rates.

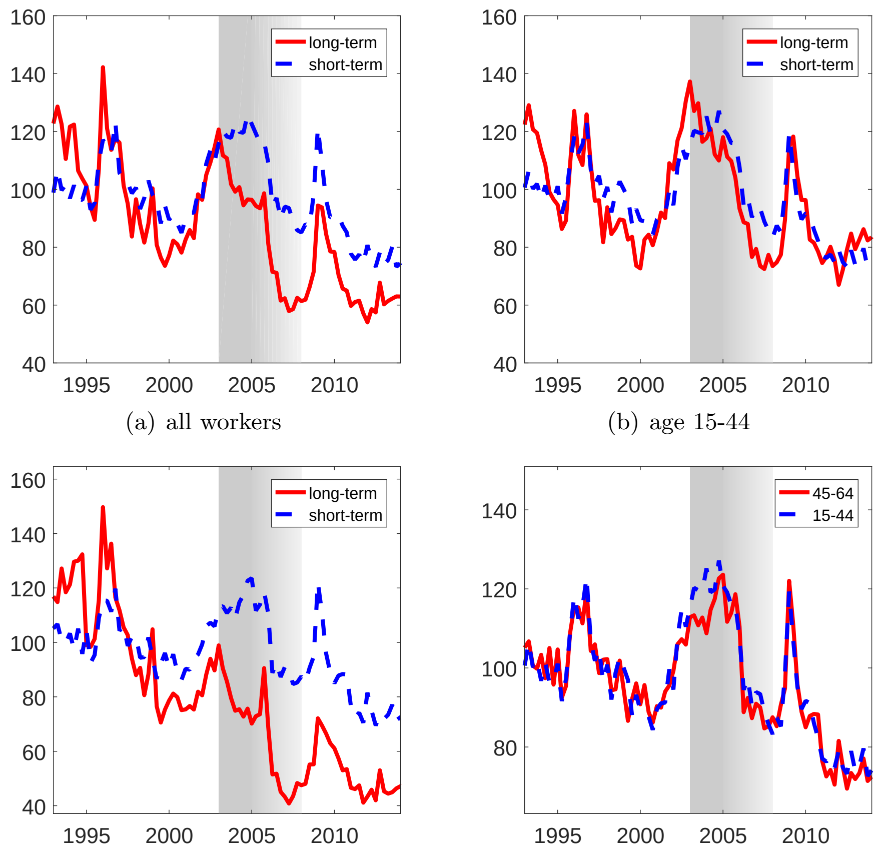
3.3.1 Descriptive results on heterogeneity by employment duration and age

We first consider heterogeneous effects on workers with different employment duration and age. For employment duration, we split employed workers into two groups. The first group comprises short-term employed workers with at most three years of employment duration, and the second group is long-term employed workers with more than three years of employment duration. This threshold cuts the sample roughly into a first group of workers (short-term employed) who are only affected by abolishing unemployment assistance benefits and a second group of workers (long-term employed) where older workers also experienced an additional effect from the cut in maximum benefit duration. Figure 5 shows the relative changes of separation rates compared to the pre-reform period. Appendix Table 1 shows the corresponding numerical results. Figure 5(a) shows the indexed time series of separation rates for short-term and long-term employed workers. After the UI reform, we observe a strong divergence in the time series of separation rates. The divergence persists over the entire post-reform period so that separation rates of long-term employed workers decline twice as much as those of short-term employed workers.

In a second step, we further dissect the data in Figures 5(b) and 5(c) by looking at younger (44 years and younger) and older workers with different employment duration. Young workers

[13]Here, we use a two-state model so that the steady-state unemployment rate is $\bar{u} = \frac{\bar{\pi}_{eu}}{\bar{\pi}_{eu}+\bar{\pi}_{ue}}$ where $\bar{\pi}_{eu}$ denotes the steady-state separation rate (unemployment inflow) and $\bar{\pi}_{ue}$ denotes the steady-state job-finding rate (unemployment outflow). In Appendix B.3, we demonstrate that two-state and three-state models deliver very similar dynamics of the unemployment rate over time.

Figure 5: Separation rates by age and employment duration (1993-2014)



(a) all workers (b) age 15-44

(c) age 45-64 (d) short-term employed

Notes: Separation rates by age and employment duration. All rates are indexed to their pre-reform level (1993-2002 = 100). Panels (a)-(c) show short- and long-term employed workers of different age groups. The solid red lines in panels (a)-(c) mark the separation rate for long-term employed workers ($\geq$ 3 years). The dashed blue lines mark the separation rate for short-term employed workers (< 3 years). Panel (d) shows the separation rate for short-term employed workers separately for young (age 15-44, dashed blue line) and old (age 45-64, solid red line) workers. The grey area indicates the reform period and the fading out shows the transition period after the reform.

in Figure 5(b) are only affected by the abolition of unemployment benefit assistance but not by the cut in maximum benefit duration. In line with such a homogeneous treatment effect by the reform, we find no differential changes between short-term and long-term employed young workers, and separation rates decline in lockstep. By contrast, we observe differential treatment effects from changes in UI benefit duration in Figure 5(c) when we consider older long-term employed and short-term employed workers. We find the strongest reduction in separation rates for long-term employed, older workers with almost 60% lower separation rates after the reform compared to their pre-reform average. By contrast, the reduction for older short-term employed workers is only about half as large. Finally, looking at short-term employed workers across age groups in Figure 5(d), we find a strikingly close tracking of separation rate changes for short-term employed young (age 15-44) and short-term employed old workers (age 45-64) in line with a homogeneous treatment by the UI reform as both groups are only affected by the abolition of unemployment assistance benefits and do not experience differential cuts in maximum benefit duration. These differences in the evolution of separation rates align closely with the variation of the institutional changes described in Section 2 and provide first evidence for a causal link of separation rate changes to the UI reform.

In Appendix C.2, we provide additional results on differences by age groups. One finding from this analysis is that workers closer to retirement show an even stronger decline in separation rates. Their decline in separation rates follows a longer-run trend that accelerated during the 2000s so that, over time, unemployment rates for older workers decreased more than those of younger workers. This trend was accompanied by a strongly rising labor force participation rate of workers close to retirement age (Carrillo-Tudela et al., 2021). We abstract from this fact of independent interest as it is beyond the scope of this paper.[14]

3.3.2 Regression results on heterogeneity by employment duration and age

In the next step, we provide regression evidence to support a causal relationship of the UI reform and changes of separation rates. First, we exploit the age variation in treatment intensities from the cut in maximum benefit duration in an event study. We consider workers younger than 45 and workers 45 to 49. We assign each age group the reform-induced log changes of maximum unemployment benefit duration from the pre- to the post-reform period $\Delta \log(D^{max})$, so that we get two groups with different treatment intensities, workers age 45 to 47 and workers age 48 and 49 (Figure 2). Workers age 44 and younger constitute the control group with a treatment intensity of zero. We run the following event-study regression

$$\log(\pi_{i,t}) = \gamma_i + \alpha_t + \sum_{t=1996}^{2014} \beta_t \Delta \log(D_{i,t}^{max}) + \varepsilon_{i,t} \tag{1}$$

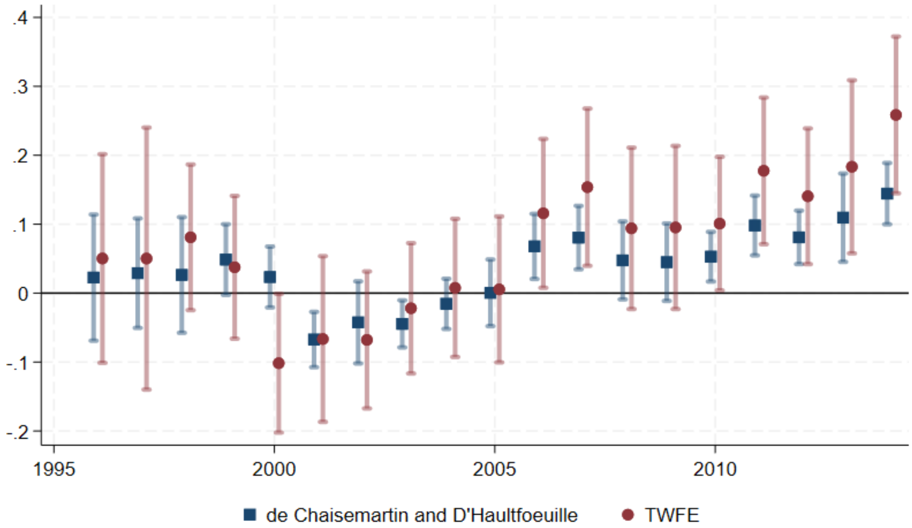
where γ_i denotes an age-group fixed effect, α_t denotes a time fixed effect, and β_t is the coefficient on the treatment intensity $\Delta \log(D_{i,t}^{max})$ that is the log difference of the maximum benefit duration of group i in period t. We implement this regression using a two-way fixed effect estimator and the estimator proposed by De Chaisemartin and d'Haultfoeuille (2024) that applies for non-binary treatments.[15,16] The dependent variable is the log separation rate $\log(\pi_{i,t})$ of group i in period t. Figure 6 shows the estimated coefficient $\hat{\beta}_t$ with 90% confidence bounds. For the period after 2005, we find positive and significant treatment effects from the cut in the maximum benefit duration under the TWFE estimator and the estimator by De Chaisemartin and

[14] Jäger et al. (2023) and Kyyrä and Wilke (2007) provide detailed investigations of separation rates of older workers. Kyyrä and Wilke (2007) consider the case of Finland and Jäger et al. (2023) the case of Austria. In line with our empirical results, they document large changes in separation rates for workers close to retirement after changes in UI generosity. A strong change of separation rates for old workers is in line with economic theory if these workers have low job-finding rates (Section 5.2). Low or even zero job-finding rates after separation are typical for older workers especially when retiring early.

[15] There is a large recent literature on difference-in-difference estimators. Roth et al. (2023) and De Chaisemartin and d'Haultfoeuille (2023) provide excellent surveys. We follow the discussion in De Chaisemartin and d'Haultfoeuille (2023) for the implementation of the estimator from De Chaisemartin and d'Haultfoeuille (2024).

[16] For the estimator in De Chaisemartin and d'Haultfoeuille (2024), the pre-treatment period cannot be longer than the treatment period so that we only consider the regression period in 1996.

Figure 6: Event-study estimate of reduction in maximum benefit duration on separation rates



Notes: Coefficient estimates from event study regression of separation rate on change in maximum unemployment benefit duration (equation 1). Treatment intensity of changes in maximum benefit duration is assigned based on age. One set of coefficients (red dots) show the standard two-way fixed-effect estimator (TWFE). Second set of coefficients (blue squares) show the estimator by De Chaisemartin and d'Haultfoeuille (2024). Confidence bounds for both estimators are for a 90% confidence level. See text for further details.

d'Haultfoeuille (2024).[17] Treatment effects become strongly positive in 2006 when the cut in maximum benefit duration became effective. As treatment effects $\Delta \log(D^{max})$ are negative, a positive coefficient implies a lower separation rate in that year for the treated group. Treatment effects are statistically significant at the 10% level starting in 2006 and are significant starting in 2010 and thus for the years after the transition period.

The specification in equation (1) focuses on workers around the age discontinuity for changes in maximum UI benefit duration at age 45. The treatment only exploits age variation abstracting from within age-group variation from differences in employment duration. In a second step, we exploit all variation in treatment intensity from Figure 2(c) using a difference-in-difference design. We use as before $\Delta \log(D^{max})$ and pool separation rate data by age-employment-duration treatment cell from Figure 2(c) over the pre- and post-reform period and use the log-difference in separation rates $\Delta \log(\pi)$ as our outcome variable. We include a constant in the regression that captures the baseline effect for workers with a treatment intensity of zero. Specifically, we run the regression

$$\Delta \log(\pi_i) = \beta_0 + \beta_1 \Delta \log(D_i^{max}) + \varepsilon_i, \tag{2}$$

where i identifies the different treatment groups. Note that we run the specification in first differences taking out fixed characteristics across treatment cells. As before, we expect separation rates to decline on average (negative β_0) and to fall in treatment intensity (positive treatment

[17]Note that the treatment effect in period zero (2005) is close to but numerically different from zero.

coefficient β_1). Table 3 reports the estimated regression coefficients for four specifications that differ with respect to the post-reform period either including or excluding the Great Recession and if treatment groups are weighted by their average employment size.

Table 3: Estimation of separation rate change on change in maximum benefit duration

	(1)	(2)	(3)	(4)
$\hat{\beta}_0$	-0.211	-0.296	-0.216	-0.324
	(0.002)	(0.000)	(0.038)	(0.003)
$\hat{\beta}_1$	0.545	0.583	0.513	0.523
	(0.029)	(0.026)	(0.134)	(0.127)
post-reform	2008 - 2014	2011 - 2014	2008 - 2014	2011 - 2014
weighted	yes	yes	no	no
obs.	28	28	28	28

Notes: Estimated regression coefficients from regression of the (log) separation rate change on the (log) change in maximum benefit duration for different regression specifications. Coefficient estimates $\hat{\beta}_0$ and $\hat{\beta}_1$ for constant and slope coefficient, p-values below coefficients in parentheses. Row *post-reform* indicates the data years used for the post-reform period. Row *weighted* indicates if observations have been weighted by pre-reform employment in cell. Row *obs.* shows number of observations (duration-age cells) in regression.

Looking at the regression results in columns 1 and 3, we find a negative β_0 coefficient that is slightly larger than 0.2 in absolute value. This implies that, on average, separation rates of workers without cuts in maximum benefit duration declined by approximately 20%, in line with the effects in Figures 5(b) and 5(d). If we exclude the Great Recession in columns 2 and 4, the coefficients decrease by about 9 percentage points, consistent with the descriptive analysis. Weighting observations by employment has a negligible effect on the estimated coefficients as the unweighted regression results in columns 3 and 4 show. We find β_0 to be always statistically significant at a 5% level. For the weighted regressions, the estimated treatment effect β_1 has the expected positive sign and is statistically significant at the 5% level. The estimated coefficients imply an elasticity of separation rates with respect to unemployment benefit duration that is slightly larger than 0.5. Evaluated at the average cut in benefit duration of 33% ($\Delta\log(D^{max}) = -0.42$), we get a treatment effect that lowers separation rates of all treated workers by 23% in addition to the baseline effect of 20% across all workers.

These point estimates that exploit variation in treatment intensity by age and employment duration are larger than the event study coefficients. The lower coefficients in the event study regression that only uses variation in age are in line with the robustness analysis in Appendix D.8 that also only relies on variation in age for the regression in equation (2). This robustness analysis addresses the concern that employment duration is endogenous and affected by changing separation rates. The robustness analysis therefore exploits as the event-study regression treatment only variation of treatment intensity by age. We find the estimated effects from different treatment intensities on separation rates to be highly significant but smaller in line with the event study results.

3.3.3 Heterogeneity by wage levels

In a further step, we exploit heterogeneity along the wage distribution from the reduction in expected benefit levels following the abolition of unemployment assistance benefits. We consider the effect on separation rates for workers 44 years and younger who we group year by year into wage deciles. As we only consider workers age 44 and younger, the estimated effect on separation rates stems only from abolishing unemployment assistance benefits as for workers younger than 45 there is no additional treatment effect from a cut in maximum benefit duration.[18] We pool data at an annual frequency to get precise estimates of transition rates especially for high-wage workers who have low average transition rates into unemployment. The bottom decile of the wage distribution forms our control group for the abolition of unemployment assistance benefits (Section 2). This choice is also supported by the descriptive statistics. We find that separation rates in the bottom wage decile did not change over time when comparing the pre-reform to the post-reform period starting in 2008. Also for the post-reform period starting in 2011, separation rates in the lowest wage decile hardly changed and declined by only 4%. We run the following regression

$$\log(\pi_{eu,i,t}) = \alpha_i + \beta_1 \mathbf{1}_{post,2-4} + \beta_2 \mathbf{1}_{post,5+} + \varepsilon_{i,t}$$

where $\pi_{eu,i,t}$ denotes the separation rate in wage decile i in year t, α_i is a group fixed effect, $\mathbf{1}_{post,2-4}$ is an indicator for the post-reform period for the 2nd to 4th wage decile, and $\mathbf{1}_{post,5+}$ is the corresponding indicator for wage deciles starting at the median. Columns (1) and (2) of Table 4 report the coefficient estimates of interest for two post-reform periods starting in 2008 and 2011.

Table 4: Estimation results for separation rate change by wage decile

	(1)	(2)	(3)	(4)	(5)	(6)
post-reform period	2008-2014	2011-2014	2008-2014	2008-2014	2011-2014	2011-2014
$\hat{\beta}_1$	-0.335	-0.413	-0.231	-0.308	-0.375	-0.347
p-value	(0.000)	(0.000)	(0.015)	(0.000)	(0.001)	(0.001)
$\hat{\beta}_2$	-0.364	-0.435	-0.199	-0.297	-0.331	-0.300
p-value	(0.000)	(0.000)	(0.003)	(0.000)	(0.000)	(0.000)
ind. trend	no	no	yes	yes	yes	yes
business-cycle	no	no	no	yes	no	yes

Notes: Regression coefficients for change in separation rates by wage decile for different post-reform periods (column headers) and regression specifications (bottom rows). Regression of (log) separation rate on wage-decile-group fixed effects ($\hat{\beta}_1$ for deciles 2-4, $\hat{\beta}_2$ for deciles 5 and higher). Row *ind. trend* indicates if wage-decile-specific time trends are included and row *business-cycle* indicates if wage-decile-specific business-cycle controls have been included. The sample includes only full-time employed West German workers age 44 and younger. See text for details.

[18] Treatment intensity is $\Delta \log(D^{max}) = 0$ for all workers age 45 and younger.

Looking at columns (1) and (2), we find economically and statistically highly significant treatment effects $\hat{\beta}_1$ and $\hat{\beta}_2$ from the abolition of unemployment assistance benefits relative to the control group of low-wage workers. The point estimate of the treatment effect $\hat{\beta}_1$ is slightly smaller than for high-wage workers $\hat{\beta}_2$. The point estimates imply that the cut in long-term benefits led at least to a 30% decline in separation rates relative to the control group of low-wage workers.

One concern when exploiting variation along the wage distribution is that secular trends such as skill-biased technological change or business-cycle fluctuations induce different trends in separation rates along the wage distribution unrelated to the UI reform. To address such concerns, we use two different specifications controlling for additional business-cycle and trend heterogeneity. First, we add to the variables from the previous regression a wage-decile-specific time trend to allow for group-specific trends in separation rates. Second, to control for differences in business-cycle sensitivity, we also include the log-deviation of GDP per capita from a linear time trend interacted with the group fixed effect $\alpha_i \times \widehat{GDP}_t$. Columns (3) to (6) of Table 4 report the estimated coefficients of interest for the extended regression. All specifications include the group-specific time trends and the last line indicates if also controls for heterogeneity in business-cycle sensitivity are included.

We find that controlling for group-specific time trends in separation rates and business-cycle sensitivity reduces the reform effect on separation rates. The decline in separation rates remains however highly statistically and economically significant. Comparing post-reform periods, we find that as in the descriptive analysis the decline is larger in absolute value for the post-reform period starting in 2011 (columns (5) and (6)). When we include business-cycle controls for the post-reform period starting in 2008, we find that this closes the gap between the 2008 and 2011 coefficients highlighting the effect of the Great Recession (column (4)). There is only a small effect of including business-cycle controls for the post-reform period starting in 2011.

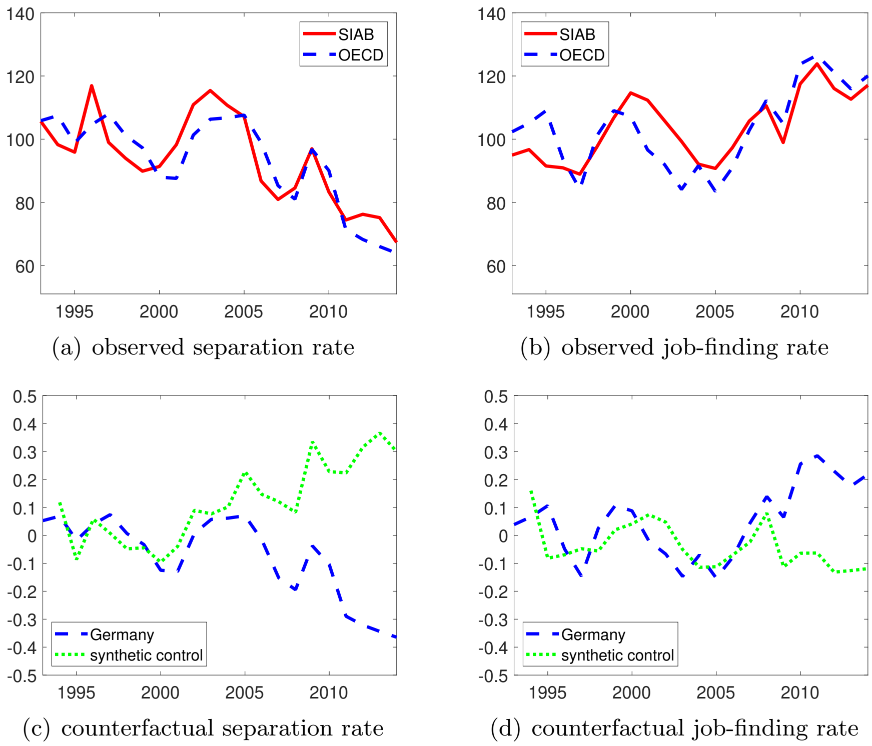
3.4 OECD data and synthetic control

As a complementary approach to establish a causal link from the UI reform to the changes in separation rates, we also exploit cross-country variation. We apply the synthetic-control approach from Abadie et al. (2010) and use as source of variation that Germany was the only OECD country that implemented a large UI reform in 2005. We rely on data from the largest set of OECD countries for which we can construct worker-flow rates following the methodology in Elsby et al. (2013). For our analysis, we extend their estimates from 2009 to today.[19] In a first step, we compare the newly constructed alternative and independent estimates of transition rates for Germany to the transition rates based on social security data. The first row of Figure 7 shows the estimated separation and job-finding rates based on the OECD data together with

[19]These are 15 countries Australia, Austria, Canada, Finland, France, Ireland, Italy, Japan, New Zealand, Norway, Portugal, Spain, Sweden, United Kingdom, and United States. We cannot construct transition rates for Austria in 1993 but kept Austria in the sample because of its expected similarity to the German labor market. As Austria is part of the synthetic control group, this will lead to missing data for 1993 for the synthetic-control estimate. We smooth an outlier in the German job-finding rate for 2005 using a centered three-year average.

our estimates based on social security data (annual averages). Reassuringly, we find that the OECD data that were not subject to regulatory changes in 2005 follow our estimates using the inflow-corrected social security data very closely.

Figure 7: worker-flow rates based on OECD data



(a) observed separation rate

(b) observed job-finding rate

(c) counterfactual separation rate

(d) counterfactual job-finding rate

Notes: Estimates of separation and job-finding rates from OECD and SIAB data and estimated synthetic-control counterfactual of these rates. Top row: Estimates for the separation and the job-finding rate for Germany based on SIAB microdata and from OECD data using the methodology in Elsby et al. (2013). Top left panel shows separation rates and top right job-finding rate. Bottom row shows separation and job-finding rates for Gemany based on OECD data as in the top row and the synthetic-control counterfactual for these rates. All series have been indexed to the pre-reform period (1993-2002 = 100).

In a second step, we apply the synthetic-control approach to estimate a counterfactual evolution of transition rates absent the UI reform. The synthetic control estimation constructs synthetic German labor market outcomes absent the UI reform as a weighted average from a pool of candidate countries. To determine the weights, we consider the German unemployment rate as the outcome variable to be matched in the pre-reform period from 1993 to 2002. We provide further details in Appendix E. We get a synthetic control group for Germany that is composed of Austria, France, Japan, and Portugal with the largest weight on Austria (56%). We then use the estimated control-group weights to construct the counterfactual separation and job-finding rates for Germany absent the UI reform. The bottom row of Figure 7 shows the synthetic separation rate and job-finding rate together with the observed German transition rates based on the OECD data. Comparing counterfactual and observed rates, we find that the separation rates start deviating in 2005 from the synthetic control group that shows even increasing separation rates after 2005. Similarly, we find that synthetic job-finding rates remain constant or slightly decline in the control group so that we observe diverging trends starting at the end of the transition period in 2008. We find in line with the results from the social security data that the decline in the separation rate relative to the control group is twice as large as the corresponding increase in the job-finding rate.[20] The relative changes of separation and job-finding rates corroborate our key empirical finding of a dominant role of the separation rate for the declining unemployment rate after the UI reform.

3.5 Evidence on wages

Our empirical analysis established a causal effect of the UI reform on separation rates. In a final step, we consider the effect of the UI reform on wages to explore if lower separation rates and hence more stable jobs were also associated with lower wages. Put differently, if there has been a trade-off between job stability and wages. To get evidence on wages, we exploit again the different treatment intensities of the UI reform by age. As hours worked are not observed in the social security data, we restrict the sample to employed workers who are employed under a full-time contract for the entire year.[21] We focus on prime-age workers age 25 to 54 and estimate a flexible wage equation that we augment with controls for the impact of the cut in UI generosity. We denote log (daily) wages of individual i in year t by $w_{t,i}$ and specify the (log) wage equation

$$w_{t,i} = \gamma_i + \alpha_t + \sum_{a=25}^{54} \beta_a \mathbf{1}^a_{i,t} + F(a_{i,t}) \times post + \varepsilon_{t,i} \tag{3}$$

where γ_i denotes an individual fixed effect, α_t denotes a time effect to control for aggregate conditions, $\mathbf{1}^a_{i,t}$ is an indicator function if individual i is in year t of age a, and $\varepsilon_{t,i}$ denotes the error term. The function $F(a_{i,t})$ is interacted with a post-reform dummy and contains the age effect with $a_{i,t}$ denoting worker i's age in period t. We use two specifications for the post-reform age effect $F(a_{i,t})$. First, we exploit the age discontinuity at age 45 and implement a linear specification that is normalized to zero at age 44, i.e., $F(a_{i,t}) = \delta_{linear} \times max(a_{i,t} - 44, 0)$. We report the coefficient δ_{linear} as regression result. The second specification uses flexible post-reform age effects with two dummy variables for the age group 45 to 49 years and 50 to 54 years, i.e., $F(a_{i,t}) = \delta_{4549} \times \mathbf{1}(a_{i,t} \in [45, 49]) + \delta_{5054} \times \mathbf{1}(a_{i,t} \in [50, 54])$. In this case, we report the estimated coefficients δ_{4549} and δ_{5054} for each age group. Our preferred specification excludes the transition period after the reform but we also report results for the case when including the transition period.

We show the estimated coefficients for the different specifications in Table 5. Columns (1) and (2) report the results for the linear specification of the age effect for the case with and without the transition period. The point estimates are highly statistically significant (P-values < 0.001) and show a stronger wage decline for older workers. Evaluated at age 49, the average

[20]Note that with the social security data, we compare job-finding and separation rates relative to their pre-reform average. The counterfactual synthetic-control estimates point for the post-reform period to a decline of job-finding rates and an increase of separation rates absent the UI reform. The trend in the separation rate results mainly from Portugal that saw a doubling of its separation rates after 2010.

[21]We define full-year employment as employment spells of at least 360 days.

Table 5: Regression results for wages

	(1)	(2)	(3)	(4)
δ_{linear} ($\times$100)	-0.196	-0.136		
standard error ($\times$100)	(0.016)	(0.012)		
δ_{4549} ($\times$100)			-0.873	-0.351
standard error ($\times$100)			(0.093)	(0.066)
δ_{5054} ($\times$100)			-1.548	-0.982
standard error ($\times$100)			(0.126)	(0.091)
Including transition	no	yes	no	yes

Notes: Estimated coefficients for the post-reform effect from benefit extensions on (log) wages. Coefficients for two specifications of post-reform age effect from change in maximum benefit duration. Coefficient δ_{linear} shows estimated effect for linear specification normalized to zero for age 44 and younger $F(a_{i,t}) = \delta_{linear} \times max(a_{i,t} - 44, 0)$. Coefficients δ_{4549} and δ_{5054} show estimated effect for a non-linear specification using age-group dummies $F(a_{i,t}) = \delta_{4549} \times \mathbf{1}(a_{i,t} \in [45, 49]) + \delta_{5054} \times \mathbf{1}(a_{i,t} \in [50, 54])$. Standard errors are reported in parentheses below coefficient estimates clustered at the worker level. All coefficients and standard errors have been multiplied by 100. Bottom row shows if the post-reform period is dropped from the estimation sample. See text for further details.

wage decline relative to workers age 44 and younger is 1.0%. This estimate is in the range of the estimated wage effects in Hagedorn et al. (2019) in response to maximum benefit duration extensions in the United States. Using their coefficient estimate for job stayers, a decline of maximum benefit duration for a 49-year-old worker from 22 to 12 months implies a reduction in wages of 1.4%. Using the coefficient for all workers, their estimate implies a reduction of 0.5%.[22,23] The more flexible specification in columns (3) and (4) corroborates increasing wage reductions with age. We find a highly significant wage reduction of 0.9% for the age group 45 to 49 years (δ_{4549}) and 1.5% for the older age group (δ_{5054}). Including the transition period yields again highly significant coefficients and consistently slightly smaller wage declines.

Hence, the empirical evidence provides support for the idea of a trade off between wages and job stability. Workers with a stronger reduction of UI generosity had a stronger reduction in separation rates and an increase in job stability (Table 3), but also a stronger decline in wages (Table 5).

3.6 Sensitivity analysis

Finally, we summarize the results of our extensive sensitivity analysis and relegate the details to Appendix D. In a first step of our sensitivity analysis, we demonstrate that skipping the inflow

[22]We use regression coefficients for job stayers and all workers from columns (1) and (5) of their Table 4. For $\beta = 0.0232(0.0090)$, we get $\exp(\beta \times (\log(12) - \log(22))) = 0.986(0.995)$.

[23]Recently, Dahl and Knepper (2022) estimate wage effects of reductions in maximum UI benefit duration from 26 to 20 weeks across U.S. states. They find average wage reductions of 1.35% which is consistent with our estimates for older workers (Dahl and Knepper, 2022, Table 5).

correction mainly leads to lower job-finding rates after the reform (Appendix D.1). Second, we address in detail in Appendix D.2 the robustness of the results for worker-flow rates with respect to the change in registration regulation in 2005. In addition to the corroborating evidence on worker-flow rates based on OECD data in Section 3.4, we provide worker-flow rates based on unemployment insurance claims and alternative estimates for worker-flow rates based on the German Microcensus. We corroborate our finding of a strongly declining separation rate after 2005. Furthermore, we report estimates for worker-flow rates from employment to out of the labor force (EN flows) based on SIAB data and find that, if anything, these flows also declined after 2005 compared to the pre-reform period. Relatedly, we compare in Section D.3 our results to Carrillo-Tudela et al. (2021) who find an important role for flows to out of the labor force over the 2000s. We demonstrate that their sample selection that restricts employment information to a subset of the available information in the SIAB leads to additional and an increasing number out of the labor force spells over time and consequently additional flows to out of the labor force that account for the differences to our results. In a third step, we control for changes in the composition of the employed in terms of worker characteristics and employment duration using a linear regression model (Appendix D.4). Fixing the composition of the employed at the level in 2000, we find that compositional changes alone are too small to account for changes in separation rates over time.[24] To explore the sensitivity with respect to sample selection, we provide results for East Germany (Appendix D.5), results when including marginally employed workers who are registered as unemployed as employed (Appendix D.6), and results when counting workers in active labor market programs among the employed (Appendix D.7).[25] Finally, Appendix D.8 provides robustness results for the regression analysis on separation rate changes by age and employment duration. We find all results to be robust.

4 Model

This section generalizes our empirical findings for Germany based on economic theory. We develop a labor market search model with aggregate fluctuations, endogenous separations, and worker heterogeneity to demonstrate that the observed changes in labor market dynamics and the unemployment rate are the consequence of the UI reform.

In the model, time is discrete and there is a continuum of workers of measure one and a positive measure of firms. Workers and firms are risk neutral and discount the future at rate $\tilde{\beta}$. Each period there is a positive probability that a worker leaves the labor force for good. We denote this probability by ω and the product of the time discount factor and the probability of remaining in the labor market by $\beta = \tilde{\beta}(1 - \omega)$. A worker who leaves the labor force is immediately replaced by a newborn worker so that there is always a constant mass of workers. Workers in the model are either employed or unemployed. We consider single-worker firms and refer to a worker-firm pair as a match.

[24] Controlling for employment duration also addresses concerns regarding the regression analysis about shifts in the distribution of employment duration after the reform.

[25] After the reform, workers who participate in active labor market programs were no longer counted as unemployed.

Employed workers have one of two skill levels x_1 or x_2 with $x_1 < x_2$. We refer to workers with skill level x_1 as low-skill workers and workers with skill level x_2 as high-skill workers. Workers who enter the labor force start as low skill. While working, workers accumulate skills by learning-by-doing. An employed low-skilled worker stochastically gains skills at rate α. The accumulated skills are lost upon separation. We denote the share of employed workers in state x_1 by e_1 and the share of employed workers in state x_2 by e_2.

The state of unemployed workers can take three values b_j with $j = 1, 2, 3$. The different states describe the current benefit level of the unemployed: social assistance (b_1), unemployment assistance (b_2), and unemployment benefits (b_3). It holds that $b_1 \leq b_2 < b_3$. Benefit eligibility depends on employment duration. Since the accumulation of skills and benefit eligibility both depend on employment duration, we economize on the state space and assume that eligibility and skill level are perfectly correlated so that all high-skill workers are eligible for unemployment benefits.[26,27] Hence, high-skill workers are upon entering unemployment eligible for unemployment benefits b_3. If low-skill workers become unemployed, they enter in state b_3 with probability γ, and with probability $1 - \gamma$, they enter unemployment in state b_1. Stochastic eligibility for low-skill workers captures in a parsimonious way the more complex eligibility rules of the actual system.[28] During unemployment, the eligibility state stochastically changes. Workers in state b_3, receiving unemployment benefits, transit to state b_2, receiving unemployment assistance, with probability δ_3. Workers who are in state b_2 transit to state b_1, receiving social assistance, with probability δ_2. We denote the mass of workers in each state by u_j for $j = 1, 2, 3$.

The aggregate state of the economy s comprises an aggregate productivity state a and the distribution of workers over states $s = \{a, e_1, e_2, u_1, u_2\}$ where we dropped u_3 because of the identity $e_1 + e_2 + u_1 + u_2 + u_3 = 1$. The aggregate productivity state a follows an AR(1) process with autocorrelation ρ and variance σ_a^2. The state of a match at the beginning of the period is described by the tuple (x, s) of the idiosyncratic state x and the aggregate state s. The state of an unemployed worker is (b, s), where the idiosyncratic state b is the current benefit level.

Each period consists of two stages. The first stage is the separation stage when each match decides about separating into unemployment or entering the production stage. The second stage is the production stage for the employed and the search stage for the unemployed. Search happens simultaneously with production. We refer to this stage, respectively, as the search or production stage depending on whether the unemployed or the employed are considered. We

[26]We abstract from age heterogeneity that would lead to the introduction of an additional state variable. The underlying economic mechanism would be identical to the mechanism that works along the employment duration dimension. Krause and Uhlig (2012) follow the same modeling approach.

[27]In general, experience and skill accumulation need not be perfectly correlated. The empirical evidence on wage growth for the German labor market finds strong returns to experience in the first two years (Dustmann and Meghir (2005)). This suggests that productivity gains and eligibility in the data are also highly correlated, so we are confident that our assumption to economize on the state space is of minor importance.

[28]Two main reasons account for the misalignment of employment duration and eligibility. First, employees with more than one year of employment duration are already eligible for UI benefits for a period of 6 months, which then gradually increases to 12 months the longer a person has been working. Second, employment duration in the legislation does not refer to the latest continuous employment spell but the accumulated duration in a reference period that varied between 2 and 7 years.

abstract from on-the-job search. Labor market exit happens with probability ω at the end of the period. At the separation stage, each match draws an idiosyncratic productivity shock ε and decides given its state (x, s) whether to enter the production stage. For analytical tractability, we assume that the shock ε is independently and identically distributed across matches and time and is drawn from a logistic distribution F with mean zero and variance $\sigma_\varepsilon^2 = \pi^2 \frac{\psi_\varepsilon^2}{3}$. Optimal behavior follows a threshold rule where separations happen when the idiosyncratic productivity shock ε is below the state-specific threshold $\varepsilon^u(x, s)$. This threshold is determined as part of the bargaining process between the worker and the firm so that separation decisions will be jointly efficient. A match that does not separate produces $y = \exp(a + x) + \varepsilon$ units of output depending on skill level x, aggregate productivity a, and idiosyncratic productivity ε. We account for capital costs of firms by assuming that firms have to pay a fixed cost of production k for capital.

We denote the value of a firm matched to a worker of skill type x before the realization of the idiosyncratic shock ε by $J(x, s)$. The value $J(x, s)$ expressed recursively is

$$J(x,s) = \int_{\varepsilon^u(x,s)}^{\infty} \Bigg(\exp(a+x) + \varepsilon - w(x,s) - k + \beta \mathbb{E}[J(x', s')|x, s] \Bigg) dF(\varepsilon), \tag{4}$$

where $w(x, s)$ denotes the wage for the worker, k the cost of capital, and expectations are taken over the realization of the idiosyncratic and aggregate state next period (x', s') conditional on the current state (x, s). Because of free entry of firms, the continuation value of the firm after separation is zero in equilibrium. The properties of the logistic distribution imply

$$\Psi_\varepsilon(\pi_{eu}) = \int_{\varepsilon^u}^{\infty} \varepsilon dF(\varepsilon) = -\psi_\varepsilon \Bigg((1 - \pi_{eu}) \log(1 - \pi_{eu}) + \pi_{eu} \log(\pi_{eu}) \Bigg),$$

with $\pi_{eu} = F(\varepsilon^u)$ denoting the separation probability given the threshold value ε^u. The firm value simplifies to

$$J(x,s) = (1 - \pi_{eu}(x,s)) \Bigg(\exp(a+x) - w(x,s) - k + \beta \mathbb{E}[J(x', s')|x, s] \Bigg) + \Psi_\varepsilon(\pi_{eu}(x,s)). \tag{5}$$

Unemployed workers at the search stage consist of unemployed workers from last period who did not find a job and newly unemployed workers who separated at the separation stage. The worker's flow utility in unemployment is $b + h$, where h is the utility value of leisure relative to working (the disutility of working is normalized to zero). Search is random, and all workers receive job offers with the same probability $\lambda(s)$ that only depends on the aggregate state of the economy. We assume that each job offer is associated with an idiosyncratic stochastic utility component ν capturing the personal valuation of workers for jobs. This stochastic non-pecuniary job component comprises, among other things, commuting time, workplace atmosphere, and working schedules of the offered job. It captures in a parsimonious way endogenous search behavior of the unemployed. Unemployed workers optimally follow a reservation utility rule and accept all job offers with ν larger than a state-dependent threshold $\nu^u(b,s)$. We assume ν is independently and identically distributed and is drawn from a logistic distribution G with state-specific mean $\overline{\nu}(b)$ and variance $\sigma_\nu^2 = \pi\frac{\psi_\nu^2}{3}$. The average acceptance probability of an unemployed worker in state (b,s) is $q(b,s) = 1 - G(\nu^u(b,s))$, and the transition rate into employment is $\pi_{ue}(b,s) = \lambda(s)q(b,s)$ combining contact rate $\lambda(s)$ and acceptance rate $q(b,s)$. The recursive formulation of the value of an unemployed worker in state (b,s) is

$$
\begin{aligned}
V_u(b,s) &= b + h + \beta\Bigg(\lambda(s)\int\limits_{\nu^u(b,s)}^{\infty}\Big(\mathbb{E}[V_e(x',s')|b,s] - \nu\Big)dG(\nu) \\
&+ \lambda(s)\int\limits_{-\infty}^{\nu^u(b,s)}\mathbb{E}[V_u(b',s')|b,s]dG(\nu) + (1-\lambda(s))\mathbb{E}[V_u(b',s')|b,s]\Bigg) \\
&= b + h + \beta\Big(\pi_{ue}(b,s)\mathbb{E}[V_e(x',s')|b,s] + (1-\pi_{ue}(b,s))\mathbb{E}[V_u(b',s')|b,s] + \lambda(s)\Psi_\nu(q(b,s))\Big)
\end{aligned} \tag{6}
$$

where $V_e(x,s)$ denotes the value of being employed in state (x,s) and the last line again exploits the properties of the logistic distribution with $\Psi_\nu(q) = -q\overline{\nu}(b) - \psi_\nu((1-q)\log(1-q) + q\log(q))$. The state-specific means $\overline{\nu}(b)$ allow us to obtain job-finding rates that are falling with unemployment duration. Such changing utilities capture, for example, decreasing motivation to apply for jobs, more effort to prepare for job interviews, and more effort to be up-to-date with job requirements.

An employed worker who does not separate at the separation stage receives her wage at the production stage. At the end of the production stage, the stochastic skill accumulation takes place. The recursive representation of the value function of employed workers is

$$
V_e(x,s) = (1-\pi_{eu}(x,s))\Big(w(x,s) + \beta\mathbb{E}[V_e(x',s')|x,s]\Big) + \pi_{eu}(x,s)\mathbb{E}[V_u(b',s)|x]. \tag{7}
$$

Note that in the case of separation, expectations are only over the idiosyncratic benefit state b, as the worker becomes unemployed in the current period. In an abuse of notation, we denote the stochastic benefit level for low-skill workers by b'.

A Cobb-Douglas matching function $m = \varkappa v^{1-\varrho}u^{\varrho}$ determines the number of matches m between vacancies v and unemployed workers $u = u_1 + u_2 + u_3$ during the search stage of each period. The contact rate from a worker's perspective is $\lambda = \frac{m}{u} = \varkappa\theta^{1-\varrho}$ and from a firm's perspective is $\lambda_v = \frac{m}{v} = \varkappa\theta^{-\varrho}$ with labor market tightness $\theta = \frac{v}{u}$. The number of vacancies at the search stage of each period is determined by a free-entry condition

$$
\kappa = \lambda_v(s)\beta\sum_{j=1}^{3} q(b_j,s)\frac{u_j}{u}\mathbb{E}[J(x_1,s')|s], \tag{8}
$$

where κ denotes the per-period cost to post a vacancy. Firms posting vacancies take into account the acceptance rates $q(b_j,s)$ of workers with different unemployment benefit eligibility. Recall that all newly hired workers start with x_1 so there is only uncertainty regarding the aggregate state s' for the next period when posting a vacancy.

Wages and threshold values for separation decisions $\varepsilon^u(x,s)$, equivalently separation probabilities $\pi_{eu}(x,s)$, are determined by state-contingent Nash bargaining between the worker and firm over the joint surplus of the match $S(x,s) = J(x,s) + V_e(x,s) - \mathbb{E}[V_u(b',s)] \equiv J(x,s) + \Delta(x,s)$, as in Pissarides (2000, Ch. 2). We denote the bargaining power of the worker by μ so that the Nash bargaining problem reads $\max_{\{w,\varepsilon^u\}} J(x,s)^{1-\mu}\Delta(x,s)^{\mu}$. The first-order condition with respect to the wage delivers the standard surplus-sharing rule $\mu J(x,s) = (1-\mu)\Delta(x,s)$. The first-order condition with respect to the separation cutoff ε^u characterizes the cutoff value in terms of the separation rate $\pi_{eu} = 1 - F(\varepsilon^u)$ as

$$\pi_{eu}(x,s) = \left(1 + \exp\left(\psi_{\varepsilon}^{-1}\left(\exp(a+x) - k + \tilde{S}(x,s)\right)\right)\right)^{-1}, \tag{9}$$

with $\tilde{S}(x,s) = \beta\mathbb{E}[S(x',s')|x,s] + \beta\mathbb{E}[V_u(b',s')|x,s] - \mathbb{E}[V_u(b',s)|x]$ where $\mathbb{E}[V_u(b',s)|x]$ denotes the expected value from unemployment in the current period taking into account stochastic eligibility (equation (7)). Note that the resulting separation decision is equivalent to the unilateral case where only the firm decides about state-contingent separations (Pissarides, 2000, Ch. 2) and wages are bargained conditional on the realization of the idiosyncratic productivity shock. In both cases, a reduction in UI generosity leads to an increase in the firm value and a decline in the separation cutoff of the match. With less generous UI benefits, the firm will also produce at some of the negative realizations of idiosyncratic productivity that would have led to separations with more generous UI benefits otherwise. Workers in turn are willing to accept lower wages in exchange for more stable jobs. We describe the determination of the separation cutoff as the outcome of a joint bargaining over wages and separation decisions to highlight this trade off between wages and job stability.

4.1 Calibration

For the calibration, we take a model period to be one month. We set a first group of parameters outside the model to standard values. We set the discount factor $\tilde{\beta}$ to match an annual interest rate of 4% so that $\tilde{\beta} = 0.997$. We set the parameter ϱ of the matching function to 0.5 and the bargaining power of the worker μ to the Hosios condition $\varrho = \mu = 0.5$. The persistence of productivity is $\rho = 0.97$. We show calibrated parameters in Table 6. When we simulate the model, we linearize it around its deterministic steady state using only aggregate productivity shocks a to generate model dynamics over time.

The remaining model parameters are calibrated inside the model to match the pre-reform dynamics of the German labor market. Hence, the key elasticities of job-finding and separation rates with respect to changes in the UI system are only informed by pre-reform data. This calibration strategy relies on the insight from Costain and Reiter (2008a) that time series variation of transition rates is informative about the effects from structural changes in labor market institutions. To see this, recall that a 1% change in the surplus of the match from a change in

Table 6: Model Parameters

	Parameter	Value	Description
search and matching	ϱ	0.5	elasticity of the matching function
	$\varkappa$	0.164	efficiency of the matching function
	κ	1.33	vacancy posting costs
	μ	0.5	worker's bargaining power
	γ	0.6	eligibility rate of low-skill workers
	ω	0.01	labor market exit rate
preferences	$\tilde{\beta}$	0.997	time discount factor
	h	0.24	flow leisure utility
	$\bar{\nu}(b_1) = \bar{\nu}(b_2)$	1.9	means of non-pecuniary shock
	$\bar{\nu}(b_3)$	0.53	
	ψ_ν	0.14	dispersion of non-pecuniary shock
skills and productivity	k	0.4	average capital costs per match
	ψ_ε	1.15	dispersion of productivity shocks
	α	0.028	probability of skill accumulation
	Δx	0.12	skill level difference $x_2 - x_1$
	ρ	0.97	autocorrelation of aggregate shock

productivity works similarly to a 1% change in the surplus from a change in the outside option. Hence, the reform effect on labor market dynamics and in particular the relative importance of changes in the separation rate and the job-finding rate after the UI reform are informed exclusively by pre-reform labor market dynamics.

In the internal calibration, we target the level of the job-finding, vacancy filling, and separation rate. Following the idea of our calibration strategy, we also target for job-finding and separation rates their pre-reform business-cycle volatility. In terms of heterogeneity, we target differences in the level of separation rates across skill groups and differences in job-finding rates by duration. For the remaining model parameters, we target a capital share in production of 40%, a share of 60% of UI recipients among all new UI claimants, an average employment duration of three years for short-term employment, and the share of long-term employment. We also target directly the elasticity of job acceptance with respect to changes in UI benefits of 0.53 from Schmieder and Von Wachter (2016) so that our model is by construction consistent with the evidence of changes in job search behavior after UI changes. All targets are matched exactly and jointly determine parameters. We provide an intuitive discussion of the relationship between model parameters and data targets in Appendix F.

4.2 UI system and UI reform

We calibrate parameters of the unemployment insurance system to independent evidence on replacement rates from the OECD and benefit duration from Figure 2. Parameters for the period before and after the reform are shown in Table 7. According to the OECD, a single worker with the average wage received before 2004 unemployment insurance benefits corresponding to 61% of the previous wage during the first year of unemployment and 55% of the previous wage for the following four years.[29] We use these replacement rates to pin down b_3 and b_2. We set δ_3 for the duration of UI benefits to 16.2 months, in line with the average duration in Figure 2 when using the underlying employment distribution for the pre-reform period. We set δ_2 to match an average duration of receiving unemployment assistance of 36 months. For the subsistence level b_1, we match the average ratio of subsistence benefits to unemployment benefits over the period 1996 to 2002 based on data from the German Statistical Office (earlier data not available). The average ratio corresponds to $\frac{b_1}{b_3}$ in the model, and we fit it to be 67% as in the data ($\frac{b_1}{b_3} = 0.67$).[30]

Table 7: Parameters of the unemployment insurance system

pre-reform		post-reform	
b_1	0.253	b_1	0.253
b_2	0.341	b_2	**0.253**
b_3	0.379	b_3	0.379
δ_2	0.028	δ_2	0.028
δ_3	0.062	δ_3	**0.072**

The UI reform abolished long-term unemployment assistance benefits and cut the maximum benefit duration for long-term employed workers. As in Krause and Uhlig (2012), we implement the first part of the reform by setting long-term unemployment assistance benefits b_2 to the level of subsistence social security benefits b_1 (i.e., we set $b_1 = b_2$). The duration parameter δ_2 becomes irrelevant because transitions happen between states with the same benefit levels, and mean utility shocks $\bar{\nu}(b_1)$ and $\bar{\nu}(b_2)$ are set identical across the two states in the calibration. For the change in maximum benefit duration, we decrease the expected benefit duration of UI benefits b_3 from 16.2 months to 13.9 months by increasing the probability that they expire δ_3 (column "post-reform" in Table 7). We obtain the post-reform duration again by averaging the weighted maximum benefit duration after the reform in Figure 2.

Dynamics in the model are driven by two sources: aggregate productivity fluctuations and the structural change of the UI system. To simulate the model, we linearize around its deterministic steady state and use a Kalman filter on GDP growth per capita to determine the time series of aggregate productivity shocks a building on Jung and Kuhn (2014) and Murtin and Robin

[29]OECD data on *Net Replacement Rate in Unemployment* for single worker with average wage and without children in 2004. See `https://stats.oecd.org/Index.aspx?DataSetCode=NRR`.

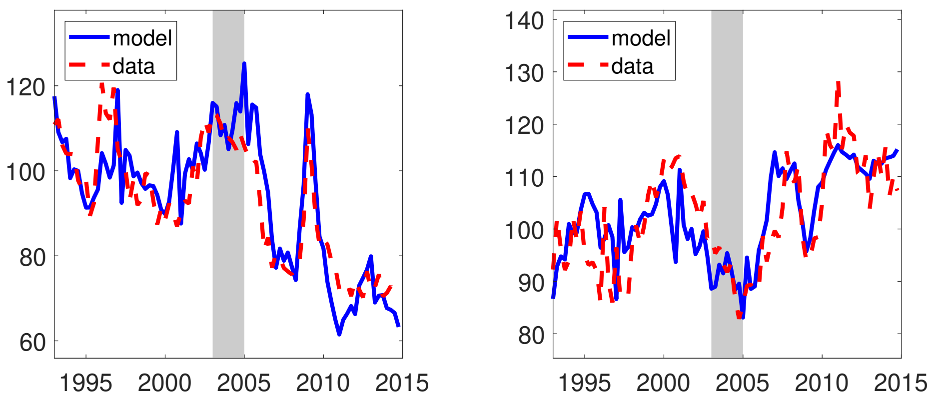
[30]Data on average social assistance benefits from statistical yearbook (*Statistische Jahrbücher*). Data on average UI benefits from statistical reports of the employment office (*Amtliche Nachrichten der Bundesagentur für Arbeit*).

(2016).[31] In the model, the UI benefit changes become effective in January 2006 as described in Section 2. We implement the complex and detailed legislation of the transition period by gradually increasing the impact of the reform on labor market dynamics. Specifically, we use different policy functions based on a linear approximation of the steady-state systems before and after the reform. We assume a linear weighting that spreads the implementation over four years so that the reform is fully effective in January 2010.[32,33] When implementing the UI reform in the model, we keep *all* other parameters except for the UI system constant over time.

5 Results

In this section, we demonstrate the model's ability to match observed labor market flows over time. The model is calibrated to the pre-reform period and the UI reform constitutes a parsimonious parameter change of the model's UI system. Figure 8 shows simulated times series of separation and job-finding rates from the model together with the data counterparts of these series. We index all series to the pre-reform steady state that we match as part of the calibration.

Figure 8: Fit for average labor market mobility (1993-2014)



(a) separation rates (b) job finding rates

Notes: Model fit for average separation and job-finding rates. Left panel shows separation rates and right panel job-finding rates. Solid blue lines show in both panels the model prediction and dashed red lines show the respective flow rate from the SIAB microdata. All rates are indexed to the pre-reform period (1993-2002 = 100). The grey area marks the reform period and the fading out indicates the transition period after the reform.

Figure 8(a) shows the close fit to the separation rate from the data. The empirical and simulated time series largely lie on top of each other. This is true both before and after the reform. Except for a short period around 2010, the model matches the dynamics of the separation rate closely, notably, also during the financial crisis of 2008.

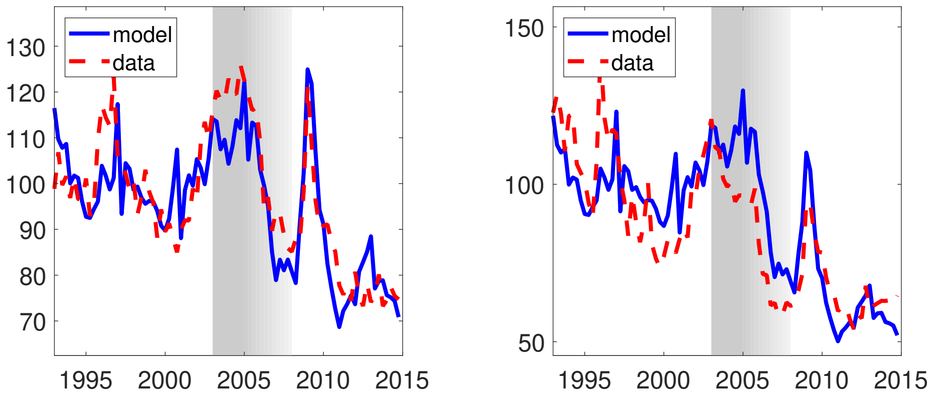
Figure 8(b) shows the simulated job-finding rate together with the data counterpart. Job-finding rates before 2005 are again matched very closely. After the UI reform, the model predicts an

[31] We use GDP per capita for Germany as data on West German GDP are not available at a quarterly frequency.
[32] Supplementary benefits according to §24 SGB II were abolished in 2010.
[33] We also tried implementing the reform directly, with the only difference that the dynamics during the transition period are matched less closely. Obviously, this assumption does not affect changes in steady states but only the behavior of the model during the transition phase. Hence, our key results do not depend on the specific implementation of the transition period.

increase in job-finding rates by 13%, in line with the data. The model also matches closely the dynamics and level changes during the post-reform period.

Our empirical analysis emphasizes the heterogeneity of changes in separation rates after the reform. While the heterogeneity in the model remains stylized, we demonstrate in Figure 9 the key dimension of heterogeneity by employment duration and the model's ability to match heterogeneous changes of separation rates. As for the average separation rate, levels and level differences between short-term and long-term employed workers before the reform have been calibrated so that they are matched by construction. Heterogeneity in changes is untargeted and provides a check of the empirically documented relationship against the model prediction.

Figure 9: Fit for heterogeneity in separation rate changes (1993-2014)



(a) separation rates (≤ 3 years) (b) separation rates (> 3 years)

Notes: Model fit for separation rates for workers with short (≤ 3 years, left panel) and long (> 3 years, right panel) employment duration. The solid blue lines in both panels show the model prediction and the dashed red lines show the respective flow rate from the SIAB microdata. All rates are indexed to the pre-reform period (1993-2002 = 100). The grey area marks the reform period and the fading out indicates the transition period after the reform.

Figure 9(a) shows the simulated and empirical separation rates for short-term employed workers. The model matches the time series, including the volatility, very closely. Unlike for the average separation rate, heterogeneous volatilities of separation rates for short-term and long-term employed workers have not been part of the calibration but are an endogenous prediction of the model. Over the long run, the model predicts a decline in separation rates for short-term employed workers by 23%, in line with the data, and importantly a substantially smaller decline in separation rates for short-term employed workers relative to the average in Figure 8(a) and relative to long-term employed workers in Figure 9(b).

Figure 9(b) compares the separation rates of long-term employed workers between the model and data. We find again that the time series for the long-term employed workers are matched closely in both level and volatility. For long-term employed workers, the model predicts a decline in separation rates by 41%, matching also the empirical decline of separation rates for this group closely.

Separation rates of long-term employed workers are affected by the abolition of unemployment assistance benefits b_2 but also by the reduction of the maximum benefit duration δ_3. In Table

3, we estimate elasticities between 0.51 and 0.58 of separation rates with respect to changes in maximum benefit duration. In the model, we derive the corresponding elasticity by varying δ_3 at post-reform benefit levels. The implied elasticity of separation rates with respect to changes in maximum benefit duration is 0.62, which is just slightly outside the range of empirical point estimates but well within their confidence intervals. This close alignment of model and data for this untargeted elasticity lends further support for the underlying calibrated elasticities of our quantitative model.

In Section 3.5, we provide evidence for a stronger wage decline for older more affected workers, supporting a wage job-stability trade off from theory. We find empirically that wages decline for workers aged 50 to 54 relative to workers aged 44 and younger by 1.5%. The model equivalent to the estimated wage effect is the differences in the wage decline of long-term employed relative to short-term employed workers. In the model, this decline is 2.4%, so the model matches quantitatively the empirical wage job-stability trade off. This result highlights that a small wage response and a large change in the separation rate after a UI reform are not only a feature of the data but also consistent with economic theory (Jäger et al., 2020).[34]

Overall, our parsimonious model of labor market dynamics aligns closely with the key empirical pattern for the changes in separation rates, job-finding rates, and the wage job-stability trade off. The causal mechanism in the model is the UI reform. Our quantitative results thereby demonstrate that the observed empirical pattern are consistent with economic theory and that they generalize as a theory-based prediction beyond the German case. To further validate this prediction, we provide in the next step counterfactual simulations for labor market dynamics absent the UI reform and contrast them to an estimated synthetic-control counterfactual.

5.1 German unemployment without the UI reform

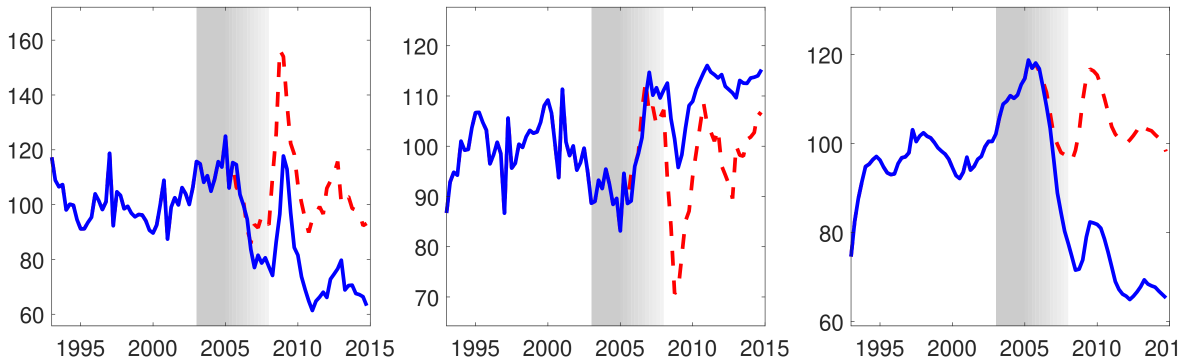
Simulating the German labor market in the model absent the UI reform delivers labor market dynamics that are strongly at odds with the data. For the counterfactual simulation, we keep all model parameters constant over time, including the parameters of the UI system. We also keep the aggregate shock series identical and feed in the previously estimated productivity shocks. This counterfactual simulation provides time series of separation rates, job-finding rates, and unemployment rates in the absence of the UI reform. Figure 10 shows the counterfactual simulation results as dashed red lines and the simulation with the UI reform as solid blue lines.

By construction, the time series from the baseline and the counterfactual in the period before the implementation of the UI reform lie exactly on top of each other as we rule out any anticipation effects.[35] After the implementation of the reform, the two simulated time series strongly diverge. Separation rates of the counterfactual remain high and fluctuate around their pre-reform level, as shown by the dashed red line in Figure 10(a). Separation rates of the counterfactual simulation

[34] In Appendix G, we derive for a simplified model without worker heterogeneity $(1-\mu)u$ as a special case for the equilibrium wage response to changes in UI generosity. The unemployment rate u constitutes in this case an upper bound for the wage response.

[35] Anticipation effects are likely small as the implementation of the reform happened on short notice. Parliament approved the law that became effective in January 2005 only in June 2004. See Hochmuth et al. (2021) for additional discussion supporting the assumption of no or very small anticipation effects.

Figure 10: Counterfactual model simulation absent UI reform (1993-2014)



(a) Separation rates (b) Job-finding rates (c) Unemployment rates

Notes: Model simulations for separation, job-finding, and unemployment rates with and without UI reform. The solid blue lines show the model with the reform and the dashed red lines show the counterfactual rates without UI reform. All rates are indexed to the pre-reform period (1993-2002 = 100). The grey area marks the reform period and the fading out indicates the transition period after the reform.

strongly spike during the financial crisis of 2008, to almost 160% of their steady-state level. In the case of the reform, the separation rate still spikes but increases only to 120% of the old steady-state level.

Job-finding rates in Figure 10(b) also evolve identically between baseline and counterfactual up to the implementation of the reform, when the two series start to diverge. During the transition period, the divergence is still modest and we only observe a strong divergence during the financial crisis as in the estimated synthetic-control counterfactual in Section 3.4. In the new steady state after the reform, the job-finding rates increase permanently by 10%, whereas by construction, they fluctuate around the old steady-state level in the absence of the reform. The divergence is strongest during the financial crisis when job-finding rates absent the reform plummet to around 70% of their steady-state level. In the case with the UI reform, the job-finding rate still decreases, but only to a level slightly below its old steady-state level. The divergence of the separation and job-finding rates manifests itself in very different dynamics of the unemployment rate. While unemployment in the baseline simulation with the UI reform declines by 30% relative to the pre-reform steady state, the unemployment rate, by construction, stays put at its pre-reform level absent the reform.

We also find a marked difference in the evolution of unemployment rates between the simulations with and without the UI reform during the financial crisis. The counterfactual simulation shows an increase in the unemployment rate of almost 30% over its long-run average. Such sharply and strongly rising unemployment rates are reminiscent of the typical European country and the United States during these years. In the case of the implementation of the UI reform, the rise in unemployment rates is substantially smaller. Unemployment rates increase only about 10% over their new steady-state level, which itself is 30% below the pre-reform level. The reason for the modest increase in unemployment after the reform is that while separation rates spike in both simulations, the relative decline in the job-finding rate is much smaller after the UI reform.[36]

To evaluate this counterfactual model prediction, we apply the synthetic control approach (Abadie et al., 2010) to estimate a counterfactual to the German unemployment rate absent the UI reform. We rely on a slightly larger sample than in Section 3.4 and use quarterly data. We target the registered unemployment rate consistent with the model calibration. In Appendix E.1, we report in addition results when focusing on the OECD unemployment rate for Germany and compare the OECD worker-flow rates to the counterfactual model simulation. All results show a close alignment between theory and empirical counterfactual. The most notable deviation is observed for separation rates with the synthetic control approach pointing towards an even stronger UI reform effect on separation rates.

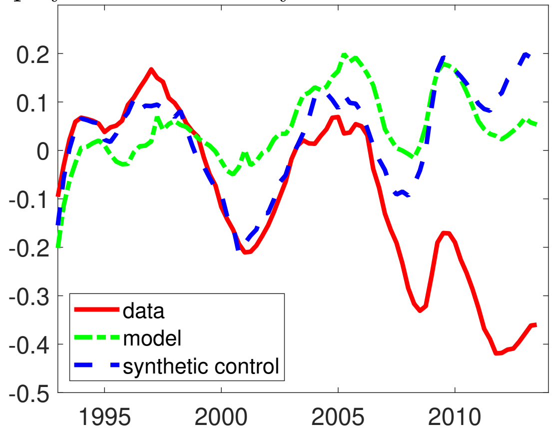
Figure 11 shows the unemployment rate and the close fit between the estimated synthetic control group (dashed blue line) and the German unemployment rate (solid red line) over the pre-reform period (1993-2002). The countries in the synthetic control group did not implement the UI reform and provide an empirical estimate for what would have happened to the German unemployment rate had the UI reform not been implemented. The dashed green line shows our counterfactual model prediction in case the UI reform is not implemented. Before the UI reform, the model shows a smaller decline in unemployment in 2001 but matches well the dynamics of the unemployment rate. Over the post-reform period, the model and the estimated synthetic-control counterfactual show a close comovement. Both counterfactual unemployment rates go down after 2005 and increase strongly during the financial crisis. The increase is slightly stronger for the synthetic-control estimate. For 2014, the model predicts an unemployment rate that is 50% higher than what we observe in the data. This estimate is more conservative compared to that of the synthetic control group, which predicts a 60% higher unemployment rate in Germany absent the reform. This analysis provides two important conclusions. First, the model is consistent with empirically observed elasticities of the UI reform, and second, unemployment rates today would be at least 50% higher than observed in the data had the UI reform not been implemented.[37,38]

[36] Germany's reliance on short-time work is oftentimes suggested as an explanation for the low rise in unemployment rates during the Great Recession. Balleer et al. (2016) find that short-term work reduces the increase in unemployment rates by around 20%, so unemployment rates would have gone up by 36% rather than 30% without short-term work. Balleer et al. (2016) also find that the smaller reaction results mostly from lower separation rates, whereas we explain the small reaction by a smaller decline in job-finding rates in comparison with the counterfactual simulation. We abstract from a detailed investigation of short-time work, but we acknowledge that such an investigation is important but beyond the scope of the current paper.

[37] As a further check to corroborate the model-implied elasticities, we explore the consequences of the Austrian UI reform considered in Jäger et al. (2023) where maximum UI benefit duration increased by 36 months from 16 months to 52 months. If we implement this Austrian reform in our calibrated model of the German labor market, we find an increase of separation rates of 32% and Jäger et al. (2023) report a 27% increase very close to our estimate for the consequences in the German labor market. We consider this close alignment as providing further support for our calibration strategy.

[38] In Appendix E.2, we also rely on the synthetic control estimate to provide an additional validity check for our inflow correction.

Figure 11: Unemployment rate from synthetic control and model prediction



Notes: Solid red line shows the empirical unemployment rate with inflow correction. The dashed-dotted green line shows the model counterfactual for the unemployment rate without the UI reform. The dashed blue line shows the synthetic-control estimate for the unemployment rate absent the UI reform. All unemployment rates are expressed as log deviations from their pre-reform average.

5.2 Relative importance of the separation rate channel

Falling separation rates are the key driver of declining unemployment rates after the UI reform in Germany. This finding is in contrast to most existing labor market research that focuses on changes in job-finding rates after changes in the UI system, for example, the recent work by Hagedorn et al. (2019) and Chodorow-Reich and Karabarbounis (2019). At first glance, these results suggest a tension between our findings and the focus of existing research. We show that the divergence of our results from the existing focus of the literature is not only consistent with economic theory but ought to be expected. For our discussion, we focus on a static version of the model from Section 4 that allows us to derive the reaction of the separation rate to a UI reform in closed form.

The economic environment is as follows. All workers are employed at the beginning of the period. Each worker-firm match has stochastic output y that is composed of an aggregate (deterministic) productivity component A and an idiosyncratic productivity shock ε, so that output is $y = A + \varepsilon$. Idiosyncratic productivity shocks ε have a distribution function $F(\varepsilon)$ and density function $f(\varepsilon)$. Separation decisions are taken at the beginning of the period after having received the idiosyncratic shock. If the match does not separate, the worker receives the bargained wage w. In case of separation, the worker becomes unemployed and receives UI benefits b for a fraction of the period $1 - \pi_{ue}$ (unemployment duration). For the remaining fraction of time π_{ue}, the worker will work in a new job with average productivity A and $\varepsilon = 0$. The resulting value of being unemployed is $V_u = \pi_{ue}A + (1 - \pi_{ue})b$, the value of employment is $V_e = w$, and the value of a filled job is $J = y - w$. The worker surplus is $\Delta = w - V_u$, and the total surplus is $S = J + \Delta = y - V_u$. Nash bargaining over wages and separation decisions delivers $w = \mu y + (1 - \mu)V_u$ with μ denoting the worker's bargaining power. The separation cutoff for productivity shocks ε^u is $-(1-\pi_{ue})(A-b)$ and the separation probability (separation rate) is $\pi_{eu} = F(\varepsilon^u)$. Separation decisions are as before jointly efficient and occur if $S < 0$. Using this result, the elasticity of separations with respect to a change in UI generosity (b) is

$$\frac{\partial \pi_{eu}}{\partial b}\frac{b}{\pi_{eu}} = \frac{f(\varepsilon^u)}{F(\varepsilon^u)} b(1 - \pi_{ue}). \tag{10}$$

As the elasticity depends negatively on the job-finding rate π_{ue} and positively on unemployment duration $1 - \pi_{ue}$, economic theory predicts a high elasticity if the job-finding rate is low and the average unemployment duration is long. A long unemployment duration is a characteristic of the German and most European labor markets. By contrast, the unemployment duration in the United States is short, so that economic theory predicts a low separation rate elasticity with respect to UI reforms in the United States rationalizing the focus on job-finding rates.[39]

Intuitively, the reason for the high elasticity is that low job-finding rates and long unemployment duration amplify the consequences of UI reforms for employed workers.[40] To see this, recall that the separation decision weighs off producing at low productivity against the cost of match separation, that is, receiving UI benefits instead of a wage. This trade off determines the productivity threshold at which separations take place. If a UI reform reduces benefit generosity, the costs of a separation increase more, the lower the job-finding rate is because a lower job-finding rate implies that the reduced UI benefits are received for longer. This amplification effect of low job-finding rates leads to the stronger reduction in the separation cutoff and separation rates. To see this, consider the following example: if job-finding rates differ by a factor of two across countries, so that unemployment duration in one country is one period and in the other country it is two periods, then a reform-induced cut in UI benefits will apply twice as long to the separating worker in one country compared to the other. The following stronger increase in the costs of unemployment from the perspective of an existing match in the low job-finding country makes more negative productivity shocks acceptable and let the separation rate decline by more in this country. In short, low job-finding rates amplify the costs of UI reforms for the employed because unemployment is more persistent and therefore lead to a stronger adjustments of separation decisions.

This intuition easily reconciles what at first glance appeared to be a tension in the cross-country analysis. It also corroborates the focus on job-finding rates when analyzing the United States, and it provides the argument for a focus on the separation rate response in Germany and other countries characterized by low job-finding rates.

The elasticity formula in equation (10) also connects our results to insights in Costain and Reiter (2008b) that underlie our calibration strategy in Section 4.1. The elasticity can be reformulated

[39] Our argument ignores that the job-finding rate itself is endogenous and a function of the UI system and other labor market institutions. In Jung and Kuhn (2014), we study differences in labor market institutions as source of cross-country differences in job-finding and separation rates in a general equilibrium model. We find that the reduced-form relationship that we rely on here constitutes a sufficient statistic for the separation rate elasticity.

[40] Surplus sharing make the worker surplus proportional to the match surplus, so that it is sufficient to look at the consequences for the worker surplus of UI reforms.

as an elasticity with respect to changes in aggregate productivity A (business-cycle shocks):

$$\frac{\partial \pi_{eu}}{\partial b}\frac{b}{\pi_{eu}} = -\underbrace{\frac{\partial \pi_{eu}}{\partial A}\frac{A}{\pi_{eu}}}_{\text{business-cycle elasticity}}\frac{b}{A}. \tag{11}$$

This result highlights the tight connection between the business-cycle volatility of separation rates and their reaction to changes in the UI system. Using data from 1980 to 2004, Jung and Kuhn (2014) document that the business-cycle volatility of separation rates is three times higher in Germany than in the United States. Adding further data from Elsby et al. (2013), they show that such higher volatility in separation rates is a common feature across European labor markets and correlates strongly with lower job-finding rates (see their Figure 2). Indeed, Jung and Kuhn (2014) document that the United States has the lowest separation rate volatility across all OECD countries, scrutinizing the transferability of results on the consequences of UI reforms on labor market dynamics from the United States to the typical European country. Our results imply that the differences in the volatility of separation rates over the business cycle rather point to a quantitatively important role of separation rates after UI reforms for most European countries.

5.3 Welfare effects

Our empirical and theoretical analysis demonstrates that changes in separation rates have been the key driver of the decline of unemployment rates after 2005. We document and explain why the decline in separation rates has not been uniform in the population and that long-term employed workers saw the strongest decline in their separation rates in reaction to the reform. Job-finding rates increased and thereby increased the probability that both short- and long-term unemployed can find jobs and enter into employment more quickly. Our structural model allows us to investigate the welfare consequences of these changes for the different worker groups. We derive welfare consequences as the consumption-equivalent variation in steady-state consumption for a worker (i.e., we quantify a worker's willingness to pay to avoid the reform). We compute welfare consequences by relying on a steady-state comparison for all worker types: short- and long-term employed workers and workers in each of the three tiers of the unemployment insurance system.[41] Note that this equivalent variation is uncompensated in the sense that because of lower unemployment after the reform, the government could redistribute gains from the reform.

Table 8 shows the welfare effects for the different groups of workers. We find the largest welfare losses for former recipients of unemployment assistance benefits, with a consumption-equivalent variation larger than 2%. Unemployment assistance benefits were abolished by the reform and such a large welfare loss likely provides an explanation for the existence of the transition rules that accompanied the reform. Note that here we compare steady states so

[41] The assumption of risk neutrality leads to simple formulas for the consumption-equivalent variation. Denoting the value function before the reform by V_0 and after the reform by V_1, the consumption-equivalent variation is $\Delta = \frac{V_0 - V_1}{V_1}$.

Table 8: Welfare effects from the unemployment insurance reform

employed		unemployed		
short-term employed	long-term employed	social assistance	unemployment assistance	unemployment benefits
0.2%	0.7%	0.1%	2.3%	1.4%

Notes: Welfare effects of the UI reform for different worker groups expressed as consumption-equivalent variation for avoiding the reform implementation.

that, even in our model with the staggered implementation, the welfare effects including the transition would be lower. The group with the second-largest welfare loss are the unemployed, with an equivalent variation of 1.4%. Unemployed workers receiving social assistance benefits experience hardly any welfare change because their benefits remain unchanged by the reform. Turning to the employed, we find much larger effects for the long-term employed compared to short-term employed workers. The group of long-term employed workers with low separation rates experiences a welfare loss corresponding to a consumption-equivalent variation of 0.7%. This group corresponds to more than 60% of all employed workers in the German labor market. Their low separation rates might suggest that this group is the least affected by the reform, yet we find large welfare losses for them. The reason is highly intuitive and closely connected to the causal mechanism of this paper. Welfare effects are large because the outside option for these workers deteriorates most strongly with the abolition of long-term, wage-dependent unemployment assistance benefits and the cut in maximum benefit duration. Hence, a group of almost two-thirds of the German labor market experienced large welfare losses from the reform. These losses remained largely uncompensated in the aftermath of the reform and might therefore explain the discontent with the reform by large parts of the population.

These results have important implications beyond the specific case of Germany for UI reform proposals in other European countries. The results suggest that the political feasibility of UI reforms might critically depend on the compensation of the large group of long-term employed workers with stable jobs who, at first glance, might appear very detached from the topic of UI reforms. Yet, we show that they are at the center of the adjustment process in countries with long average unemployment duration.

6 Conclusions

A key question in labor market research is how UI reforms affect unemployment rates and labor market dynamics. We revisit this old question by studying the German UI reform as part of the Hartz reforms in the mid-2000s, one of the largest UI reforms in industrialized countries in recent decades. By combining empirical analyses of worker flows with economic theory, we establish a dominant role of separation rates after the German UI reform. Separation rate changes in the decade after the reform have been the major macroeconomic adjustment channel for bringing down German unemployment rates.

Specifically, we provide evidence that a decrease in separation rates after the reform accounts for 76% of declining unemployment. The reduction in separation rates is heterogeneous, with long-term employed, high-wage workers being most affected. We exploit this heterogeneity in combination with differences in treatment intensities by the reform to establish a causal link from the reform to changes in separation rates. We also provide empirical evidence for a wage job-stability trade off in response to the UI reform. We use economic theory to support and generalize this empirical relationship qualitatively and quantitatively. Using the calibrated labor market model, we provide a counterfactual simulation of the German labor market absent the reform and find that unemployment rates would be 50% higher one decade after the reform. We derive theoretically that such a strong response of separation rates to the UI reform ought to be expected because of low job-finding rates and long unemployment duration in Germany. Low job-finding rates make unemployment from an employed worker's perspective particularly costly so that separation decisions adjust more strongly in case of a reform reducing UI generosity.

Data Availability Statement: Some of the data underlying this article cannot be shared publicly but can be obtained from the respective research data centers (FDZ). All other data and all code underlying this research is available on Zenodo at https://doi.org/10.5281/zenodo.14898687.

References

Abadie, Alberto, Alexis Diamond, and Jens Hainmueller, "Synthetic control methods for comparative case studies: Estimating the effect of California's tobacco control program," *Journal of the American statistical Association*, 2010, *105* (490), 493–505.

Antoni, Manfred, Andreas Ganzer, and Philipp vom Berge, "Sample of Integrated Labour Market Biographies (SIAB) 1975-2014," Technical Report, Data report 04/2016, Institute for Labor Market Research (IAB), Nuremberg 2016.

Baily, Martin, "Some aspects of optimal unemployment insurance," *Journal of Public Economics*, 1978, *10*, 379–402.

Balleer, Almut, Britta Gehrke, Wolfgang Lechthaler, and Christian Merkl, "Does short-time work save jobs? A business cycle analysis," *European Economic Review*, 2016, *84*, 99 – 122. European Labor Market Issues.

Burda, Michael and Stefanie Seele, "No Role for the Hartz Reforms? Demand and Supply Factors in the German Labor Market 1993-2014," *SFB 649 Discussion Paper*, 2016.

Carrillo-Tudela, Carlos, Andrey Launov, and Jean-Marc Robin, "The fall in German unemployment: A flow analysis," *European Economic Review*, 2021, *132*, 103658.

Chaisemartin, Clément De and Xavier d'Haultfoeuille, "Difference-in-differences estimators of intertemporal treatment effects," *Review of Economics and Statistics*, 2024, pp. 1–45.

_ **and Xavier d'Haultfoeuille**, "Two-way fixed effects and differences-in-differences with heterogeneous treatment effects: A survey," *The Econometrics Journal*, 2023, *26* (3), C1–C30.

Chetty, Raj, "A general formula for the optimal level of social insurance," *Journal of Public Economics*, 2006, *90*, 1879–1901.

Chodorow-Reich, Gabriel and Loukas Karabarbounis, "The macro effects of unemployment benefit extensions: a measurement error approach," *The Quarterly Journal of Economics*, 2019, *134* (1), 227–279.

Costain, James and Michael Reiter, "Business Cycles, Unemployment Insurance, and the Calibration of Matching Models," *Journal of Economic Dynamics and Control*, 2008, *32* (4), 1120 – 1155.

Costain, James S and Michael Reiter, "Business cycles, unemployment insurance, and the calibration of matching models," *Journal of Economic Dynamics and control*, 2008, *32* (4), 1120–1155.

Dahl, Gordon and Matthew M Knepper, "Unemployment Insurance, Starting Salaries, and Jobs," Technical Report, National Bureau of Economic Research 2022.

Dlugosz, Stephan, Ralf A Wilke, and Gesine Stephan, "Fixing the leak: unemployment incidence before and after a major reform of unemployment benefits in Germany," *German Economic Review*, 2014, *15* (3), 329–352.

Dustmann, Christian and Costas Meghir, "Wages, Experience and Seniority," *Review of Economic Studies*, 2005, *72* (1), 77–108.

Elsby, Michael WL, Bart Hobijn, and Ayşegül Şahin, "Unemployment dynamics in the OECD," *Review of Economics and Statistics*, 2013, *95* (2), 530–548.

Fredriksson, Peter and Martin Söderström, "The equilibrium impact of unemployment insurance on unemployment: Evidence from a non-linear policy rule," *Journal of Public Economics*, 2020, *187*, 104199.

Hagedorn, Marcus, Fatih Karahan, Iourii Manovskii, and Kurt Mitman, "Unemployment benefits and unemployment in the great recession: the role of equilibrium effects," Technical Report, working paper 2019.

_ , Iourii Manovskii, and Kurt Mitman, "The impact of unemployment benefit extensions on employment: the 2014 employment miracle?," Technical Report, National Bureau of Economic Research 2015.

Hertweck, M. and O. Sigrist, "The Ins and Outs of German Unemployment: A Transatlantic Perspective," *Oxford Economic Papers*, 2015, *67* (4), 1078–1095.

Hochmuth, Brigitte, Britta Kohlbrecher, Christian Merkl, and Hermann Gartner, "Hartz IV and the decline of German unemployment: A macroeconomic evaluation," *Journal of Economic Dynamics and Control*, 2021, *127*, 104114.

Hopenhayn, Hugo and Juan Pablo Nicolini, "Optimal Unemployment Insurance," *Journal of Political Economy*, 1997, *105* (2).

Jäger, Simon, Benjamin Schoefer, and Josef Zweimüller, "Marginal jobs and job surplus: a test of the efficiency of separations," *The Review of Economic Studies*, 2023, *90* (3), 1265–1303.

_ , _ , Samuel Young, and Josef Zweimüller, "Wages and the Value of Nonemployment," *The Quarterly Journal of Economics*, 2020, *135* (4), 1905–1963.

Jung, Philip and Moritz Kuhn, "Labour market institutions and worker flows: comparing Germany and the US," *The Economic Journal*, 2014, *124* (581), 1317–1342.

_ and _ , "Earnings Losses and Labor Mobility Over the Life Cycle," *The Journal of the European Economic Association*, 2018.

Katz, Lawrence F and Bruce D Meyer, "The impact of the potential duration of unemployment benefits on the duration of unemployment," *Journal of Public Economics*, 1990, *41* (1), 45–72.

Klinger, Sabine and Enzo Weber, "Decomposing Beveridge Curve Dynamics by Correlated Unobserved Components," *Oxford Bulletin of Economics and Statistics*, 2016, *78* (6), 877–894.

Krause, Michael U and Harald Uhlig, "Transitions in the German labor market: Structure and crisis," *Journal of Monetary Economics*, 2012, *59* (1), 64–79.

Krebs, Tom and Martin Scheffel, "Macroeconomic evaluation of labor market reform in Germany," *IMF Economic Review*, 2013, *61* (4), 664–701.

Kyyrä, Tomi and Ralf A Wilke, "Reduction in the long-term unemployment of the elderly: A success story from Finland," *Journal of the European Economic Association*, 2007, *5* (1), 154–182.

Launov, Andrey and Klaus Wälde, "Estimating incentive and welfare effects of nonstationary unemployment benefits," *International Economic Review*, 2013, *54* (4), 1159–1198.

Millard, Stephen P. and Dale T. Mortensen, "The unemployment and welfare effects of labour market policy: A comparison of the USA and the UK," in Dennis J. Snower and Guillermo de la Dehesa, eds., *Unemployment Policy: Government Options for the Labour Market*, Cambridge University Press, 1997, pp. 545–572.

Murtin, Fabrice and Jean-Marc Robin, "Labor market reforms and unemployment dynamics," *Labour Economics*, 2016.

Pissarides, Christopher A, *Equilibrium Unemployment Theory*, MIT press, 2000.

Price, Brendan, "The duration and wage effects of long-term unemployment benefits: Evidence from Germany's Hartz IV reform," in "MIT Working Paper" 2019.

Roth, Jonathan, Pedro H.C. Sant'Anna, Alyssa Bilinski, and John Poe, "What's trending in difference-in-differences? A synthesis of the recent econometrics literature," *Journal of Econometrics*, 2023, *235* (2), 2218–2244.

Schmieder, Johannes F and Till Von Wachter, "The effects of unemployment insurance benefits: New evidence and interpretation," *Annual Review of Economics*, 2016, *8*, 547–581.

_ , _ , and Stefan Bender, "The effects of extended unemployment insurance over the business cycle: Evidence from regression discontinuity estimates over 20 years," *The Quarterly Journal of Economics*, 2012, *127* (2), 701–752.

Shavell, Steven and Laurence Weiss, "The Optimal Payment of Unemployment Insurance Benefits over Time," *Journal of Political Economy*, 1979, *87* (6), 1347–1362.

Steffen, Johannes, "Sozialpolitische Chronik," *Arbeitslosenversicherung (seit 1969). Arbeitnehmerkammer Bremen. Bremen*, 2008.

Tuit, Sanders and Jan van Ours, "How changes in unemployment benefit duration affect the inflow into unemployment," *Economic Letters*, 2010, *109* (2), 105–107.