Hartz IV and the Decline of German Unemployment: A Macroeconomic Evaluation*

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March 2020

This paper proposes a new approach to evaluate the macroeconomic effects of the unemployment benefits reform in Germany ("Hartz IV"). In a model with different unemployment durations, the reform initiates both a partial effect and an equilibrium effect. The relative importance of these two effects and the size of the partial effect are estimated based on the IAB Job Vacancy Survey. Our novel methodology provides a solution for the existing disagreement on the unemployment effects of Hartz IV. We find that Hartz IV was a major driver for the decline of Germany's steady state unemployment and that partial and equilibrium effect were nearly of equal importance. In addition, we are the first to provide direct empirical evidence on labor selection, one potential dimension of recruiting intensity.

JEL classification: E24, E00, E60

Keywords: Unemployment benefits reform, search and matching, Hartz reforms

*We are grateful for feedback at the following conferences: CESifo Conference on Macroeconomics and Survey Data (Munich), Ensuring Economics and Employment Stability (Nuremberg), T2M 2018 (Paris), Verein für Socialpolitik 2019 (Leipzig) and the Austrian Labor Market Workshop 2019 (Vienna). We have obtained very useful feedback at seminars at Australian National University, Humboldt Universität, IAB, International Monetary Fund, IWH Halle, Université de Strasbourg, University of Adelaide, University of Bamberg, University of Trier and the University of Queensland. We would like to thank Vincent Sterk for an insightful discussion of our paper and are grateful for feedback from Leo Kaas, Andrey Launov, Kurt Mitman, Mathias Trabandt, and Coen Teulings. We are grateful for funding from the German Research Foundation (DFG) and the Hans-Frisch-Stiftung.

1. Introduction

From the beginning of the 1970s to the mid-2000s Germany's unemployment rate had been on a rising trend, reaching its maximum in 2005. Since then, it has dropped by roughly 50% (see Figure 1). At the beginning of this steep decline in 2005, Germany implemented a major reform of its unemployment benefit system. Before the reform, long-term unemployed received benefits proportional to their prior net earnings. These proportional long-term benefits were abolished in 2005 and replaced by a means-tested transfer (dubbed as "Hartz IV") that is independent of prior earnings and employment history. The Hartz IV reform was the last step in a series of structural labor market reforms (Hartz I - Hartz IV, see Appendix A) implemented between 2003 and 2006.

Figure 1: Registered unemployment rate in West Germany, 1970-2017. Note that a long time series is only available for West Germany.

While the effects of Hartz IV have been studied in the literature before, up to date no clear consensus has emerged on the quantitative importance of the reform for the decline of unemployment. Several papers look at the effects of the Hartz reforms in a reduced-form or descriptive way (Klinger and Rothe, 2012; Hertweck and Sigrist, 2013; Burda and Seele, forthcoming; Carrillo-Tudela et al., 2018) . Generally, these studies find significant changes of labor market stocks and flows around the time of the Hartz reforms (e.g. a large increase of matching efficiency). However, based on these approaches, it is difficult to discriminate between the effects of different reform steps or to establish a causal link. By contrast, a recent paper by Price (2018) exploits the rich German administrative data to identify causal effects of the Hartz IV reform. He finds statistically significant and economically meaningful employment effects. However, this approach can only identify the partial effect of the reform. If Hartz IV induced further equilibrium effects, e.g. because it affected firms' vacancy posting behavior, macroeconomic tools are required.

The quantitative results from early simulation studies on the macroeconomic effects of

Hartz IV differ substantially and range from a decline in unemployment of 0.1 percentage points (Launov and Wälde, 2013) to 2.8 percentage points (Krause and Uhlig, 2012). The key reason for these large discrepancies are different assumptions about the decline of the replacement rate for long-term unemployed caused by the Hartz IV reform. In practice, due to the heterogeneous effect of the reform, it has turned out to be very difficult to assign a number to that variable suited for macro models (see Section 2 for details and a discussion). In fact, estimates of the fall of the replacement rate for long-term unemployed range from just 7% (Launov and Wälde, 2013) to nearly 70% (upper bound for one skill group in Krause and Uhlig, 2012). In a recent study, Hartung et al. (2018) focus on the role of the separation rate and attribute a large role for separations in the context of the Hartz IV reform.[1]

Against the background of diverging views on the quantitative effects of the reform, we propose a novel methodology based on a newly created and innovative dataset. Our macroeconomic model of the labor market distinguishes between a partial and an equilibrium effect of the reform. Instead of directly assuming a certain reduction of the replacement rate, we empirically estimate and then target the partial effect of the reform.

The contribution of our paper is threefold. First, our paper contributes to the stream of the literature that evaluates the role of Hartz IV for the decline of German unemployment. Second, we contribute to the discussion on the relative importance of microeconomic versus macroeconomic effects of unemployment benefit changes. Third, we provide evidence on the behavior of labor selection over the business cycle.

In our model, workers have to search and firms have to post vacancies in order to get in contact with one another (with a standard contact function). New worker-firm contacts draw an idiosyncratic training cost shock. Only workers below a certain training cost threshold will be selected (see e.g. Chugh and Merkl, 2016; Kohlbrecher et al., 2016; Sedláček, 2014). When benefits for long-term unemployed workers are reduced, the value of unemployment decreases which leads to lower bargained wages. In reality, besides lower wages, this could also be reflected in an increased willingness to accept certain jobs or to commute longer. Indeed, descriptive evidence from the IAB Job Vacancy Survey shows that workers were willing to make concessions in terms of wages and other job-characteristics in response to the reform (see Appendix E for descriptive empirical evidence). In our model, this decrease in wages initiates two effects. First, firms post more vacancies and the contact rate of all unemployed workers increases. This represents the equilibrium effect. Second, because of lower wages, firms are willing to hire workers with larger idiosyncratic training costs. Thus, the selection rate increases. This increased hiring probability upon contact represents the partial effect in our model.

Similar to Hagedorn et al. (2019) and Karahan et al. (2019), our framework allows us to decompose the job-finding rate into a market-level (equilibrium) and an individual

[1] Bauer and King (2018) analyze the effects of the Hartz reform on labor reallocation.

level (partial) effect:

$$\text{job-finding rate}_t = \underbrace{p_t(\theta_t)}_{\text{contact rate}} \times \underbrace{\eta_t}_{\text{selectivity}}$$

The probability of finding a job is the product of the contact rate, p_t, which depends on the aggregate labor market tightness, θ_t, and the selection rate, η_t, which is determined at the worker-firm level. The contact rate represents the equilibrium effect, as it varies with aggregate market tightness. Upon contact, the selectivity (share of selected workers) matters. As this is a decision at the worker-firm level which happens independently of aggregate market tightness movements,[2] we refer to the latter as partial effect.

Our novel evaluation strategy consists of directly determining the partial effect of the reform by estimating the response of the selection rate in the data. For this purpose, we construct time series for the selection rate using the IAB Job Vacancy Survey, which is a representative survey among up to 14,000 establishments.[3] To our knowledge, we are the first to i) construct an empirical measure of firms' selection rate (i.e. hiring standards) over time, ii) thereby providing empirical evidence on the importance of the selection margin, and iii) use this to evaluate a labor market reform. The aggregate selection rate increased from 46 percent before the Hartz IV reform (1992-2004) to 53 percent after the Hartz IV reform (from 2005-2015).

Thus, our paper contributes to the debate on the size of microeconomic and macroeconomic effects of benefit changes on unemployment, which goes beyond the German case. Many papers estimate the microeconomic effects of changing unemployment benefit generosity (see Krueger and Meyer, 2002 for a survey or Card et al., 2015a,b for more recent examples) these may only capture part of the overall effect. This argument is stressed in Hagedorn et al. (2019) who estimate the macroeconomic elasticity based on policy discontinuities at state borders in the United States. A similar quasi-experimental approach is taken by Karahan et al. (2019) for a cut in unemployment benefits in Missouri.

Our empirical approach is very different and complementary to theirs. Hagedorn et al. (2019) and Karahan et al. (2019) decompose the response of the job-finding rate into a microeconomic (individual worker-level) and a macroeconomic (market-level) response. Our approach features this distinction as well. We directly measure the microeconomic effect in the data. In our setting, this corresponds to a change in firms' selectivity in hiring. Although we call our partial equilibrium variable selection rate, jobs are created in our model whenever there is a joint surplus at the worker-firm level. Thus, we could also call the variable workers' job acceptance rate, as both worker and firm accept job creation whenever there is a positive joint surplus.

Furthermore, we use the business cycle variation of the selection rate and the job-finding rate to determine the relative importance of partial and equilibrium effects. In our model, this pins down the relative size of these two effects in response to unemployment benefit changes.

[2]If we had benefit changes for one atomistic individual, the selection rate for this individual would change (and could be detected econometrically), despite no change in aggregate market tightness.

[3]For expositional simplicity, we refer to firms throughout the paper.

Our results suggest that the overall aggregate effects of the German Hartz IV reform were about twice as large as the micro effects.[4] To our knowledge, we are the first to use data on firms' selectivity to pin down the partial and equilibrium effects over the business cycle.

As a further contribution, we document the time-series behavior of labor selection, i.e. time-varying hiring standards over the business cycle. The selection rate moves procyclically and accounts for more than one half of the movement of the job-finding rate over the business cycle and thus constitutes an important adjustment margin. Our findings close an important research gap as up to date only cross-sectional evidence on the selection margin had been available. Based on the Employment Opportunity Pilot Project (EOPP), Barron et al. (1985, p. 50) document for the United States that "(...) most employment is the outcome of an employer selecting from a pool of job applicants (...)." More recently, Faberman et al. (2017) show based on a supplement to the Survey of Consumer Expectations that only a fraction of worker-firm contacts translate to job offers.

Our empirical results on the business cycle movements of the selection rate are closely related to Davis et al. (2013) and Gavazza et al. (2018), who argue that the collapse of recruiting intensity played an important role during the Great Recession in the United States.[5] Our measure of labor selection can be interpreted as one dimension of recruiting intensity. We show that employers hire a larger fraction of applicants in a boom and thereby provide direct evidence that this dimension of recruiting intensity matters over the business cycle.

Overall, our calibrated model suggests that the Hartz IV reform caused the German steady state unemployment rate to drop by 2 percentage points. Partial and equilibrium effect are of similar importance for the initial increase of the job-finding rate. Importantly, our partial effect is in a similar order of magnitude as Price's (2018) results, although he uses a completely different methodology. While we estimate the response of the selection rate to Hartz IV from establishment-survey data, Price (2018) obtains causal estimates based on administrative worker-level data.[6] The decline of the replacement rate required to generate the targeted partial effect in our model depends on the choice of the bargaining regime (individual Nash or collective bargaining). In both cases, the required decline is within plausible ranges (11% and 23%). This highlights another important advantage of our approach: By directly targeting the partial effect in our model, our results are robust to different wage formation mechanisms. Our results further demonstrate that the additional equilibrium effect is substantial. Aggregate policy statements that are only based on the partial effect miss an important part of the story. Finally, our model

[4]Further contributions on the role of macroeconomic effects in evaluating unemployment benefit changes are Hagedorn et al. (2015) and Mitman and Rabinovich (2015), and Landais et al. (2018).

[5]While our paper focuses on the time series dimension of the data, Baydur (2017) shows that a selection model can also replicate important cross-sectional dimensions of the data (e.g. the cross-sectional behavior of vacancy yields, as outlined by Davis et al. (2013)).

[6]Given the similar order of magnitude, we could also calibrate the partial effect based on Price (2018) in our macroeconomic model. The advantage of our approach is that the empirical measures we use directly correspond to our model.

generates a quantitatively similar shift of the Beveridge curve during the three years after the reform as observed in the data from 2005 to 2007. This confirms that our model generates plausible results and that the Hartz IV reform was an important driver of the observed aggregate labor market dynamics in the aftermath of the labor market reforms.

The rest of the paper proceeds as follows. Section 2 briefly outlines the institutional background on Hartz IV and the consequences for the replacement rate of different population groups. Section 3 derives a suitable search and matching model with labor selection, which allows us to look at the data in a structural way. Section 4 explains our identification strategy for the partial and equilibrium effects and provides empirical results. Section 5 explains the calibration of the contact function and the selection mechanism. Section 6 shows the aggregate partial and equilibrium effects of Hartz IV, performs several numerical exercises and puts the results in perspective to the existing literature. Section 7 concludes.

2. The Unemployment Benefit Reform

Prior to the Hartz IV reform, the German unemployment system consisted of three layers. Upon beginning a new unemployment spell, workers received short-term unemployment benefits ("Arbeitslosengeld"), which amounted to 60-67% of the previous net earnings[7] and was usually paid for 12 months.[8] After the expiration of short-term benefits, the unemployed received long-term unemployment benefits ("Arbeitslosenhilfe"), which replaced 53-57% of the prior net earnings and could be awarded until retirement. If unemployed workers did not qualify for these transfers (e.g. because they did not have a sufficiently long employment history), they could apply for means-tested social assistance ("Sozialhilfe"). As part of the reform, the proportional long-term unemployment benefits and social assistance were merged to "Arbeitslosengeld II" (ALG II), which is purely means tested based on household income and wealth. The standard rate in 2005 for a single household was 345 Euro (plus a limited reimbursement for rent). Thus, the system was merged into two pillars, switching to a means-tested system for the long-term unemployed. In addition, as a second component of Hartz IV, the maximum duration of short-term unemployment benefits for older workers, in particular, those above 57, was reduced significantly in 2006. See Figure 2 for an illustration.

As a rule of thumb, the cut of benefits for long-term unemployed was larger for high income and high wealth households. The former faced a large drop because the new system switched from a system that was proportional to prior earnings to a fixed low-level transfer or because of ineligibility due to high spousal income. The latter faced a large drop because they, too, may have been ineligible for benefits before running down their wealth.

This institutional setting explains why it is difficult to quantify the decline of the replacement rate due to Hartz IV. Some groups faced a strong reduction of the replace-

[7] The higher rate was awarded to recipients with children.

[8] The maximum duration of unemployment benefit receipt gradually increased by age for workers older than 45 years.

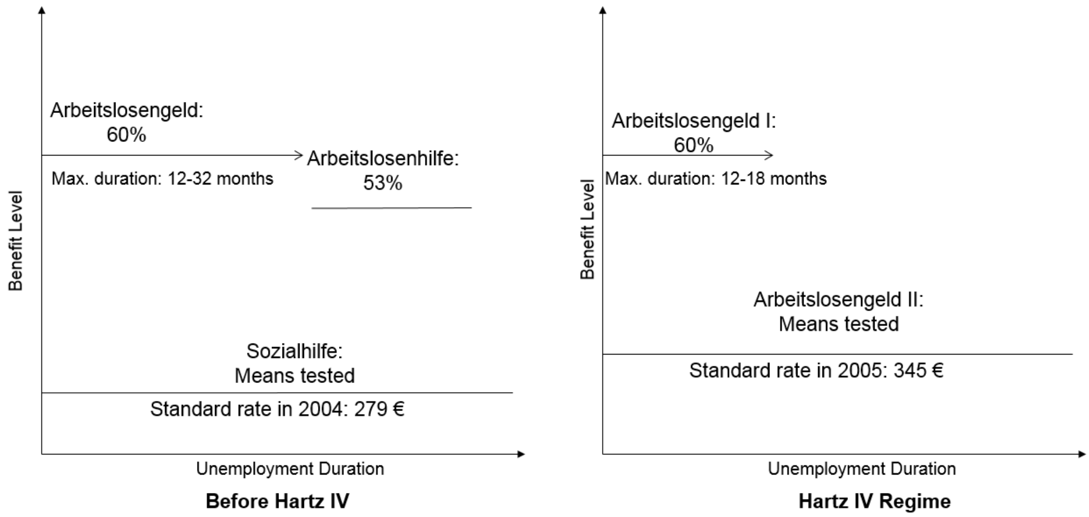


Figure 2: Illustration of the Hartz IV Reform for single households.

ment rate. A single median-income earner faced a drop of 69% according to the OECD tax-benefit calculator (Seeleib-Kaiser, 2016). By contrast, some low-income households (without wealth) saw a slight increase of their replacement rate. It is very difficult to weigh these groups properly because the low-skilled workers are overrepresented in the pool of long-term unemployed and their benefits changed the least with the reform. On the other hand, many high-income workers who have on average short unemployment spells never touch the pool of long-term unemployed. Even if they do, they might not claim benefits because they would not pass the means-testing. If those type of workers de-registered with the Federal Employment Agency because of the reform, they were not counted as registered unemployed any more. Finally, measuring the average decline of the replacement rate is further complicated by the cut in maximum entitlement duration for older workers.

It is therefore not surprising that one of the key reasons for the diverging results in existing macroeconomic studies are different values for the decline of the replacement rate. Launov and Wälde (2013) use a decline of the replacement rate for long-term unemployed of 7%. Krebs and Scheffel (2013) use a decline of 20% for the replacement rate, while in Krause and Uhlig (2012) the reduction is around 24% for low-skilled workers and around 67% for high-skilled workers. As a result, unemployment declines by 0.1 percentage points in Launov and Wälde (2013), by 1.4 percentage points in Krebs and Scheffel (2013) and by 2.8 percentage points in Krause and Uhlig (2012). Given the mentioned difficulties in quantifying the decline of the replacement rate and the resulting consequences for the effects on unemployment, we use a choice variable of firms that is directly affected by a decline of the present value of unemployment, namely the share of workers that is selected by firms upon contact.

3. The Model

The economy consists of a unit mass of infinitely lived, risk neutral, atomistic multi-worker firms and infinitely lived, risk neutral workers. We use an enhanced version of the Diamond-Mortensen-Pissarides (DMP) model (e.g. Pissarides, 2000, Ch.1) in discrete time. The model is enriched in two dimensions: First, we assume that the hiring process consists of two stages. At the first stage, workers and firms get in contact with one another via a contact function and each worker-firm pair is hit by an idiosyncratic training costs shock. In the second stage, firms only select a fraction of workers and not all contacts turn into matches. The selection rate depends on the aggregate state of the economy and on unemployment benefits. Second, we add a rich unemployment duration structure for unemployed workers with different contact efficiencies and fixed hiring costs. Workers can either be employed or unemployed. Unemployed workers randomly search for jobs on a single labor market. Unemployed workers differ in their unemployment duration. They are indexed by the letter d, where $d \in \{0, 1, \ldots, 12\}$ denotes the time left in months that a worker is still eligible for short-term unemployment benefits b^s, while receiving long-term benefits b^l afterwards. Therefore, a worker who has just lost a job receives the index 12, while a worker indexed by 0 is long-term unemployed.

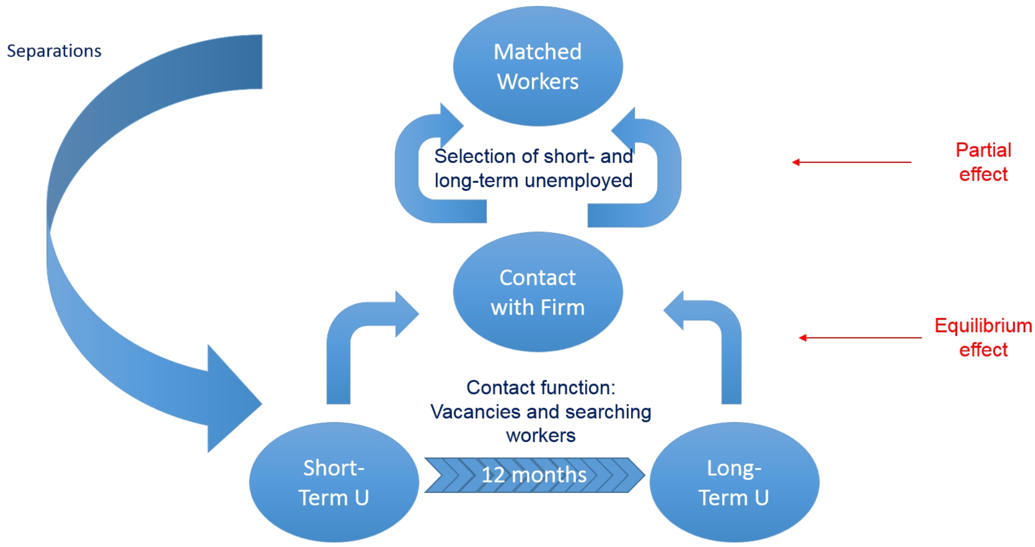


Figure 3: Graphical model description

Figure 3 illustrates the main features of the model. Our model is similar to that in Kohlbrecher et al. (2016), to the stochastic job matching model (Pissarides, 2000, chapter 6) or many of the endogenous separation models (e.g. Krause and Lubik, 2007). Chugh and Merkl (2016), Lechthaler et al. (2010), and Sedláček (2014) are further examples of labor selection models. Except for different unemployment durations, which are essential for the reform, we do not model further heterogeneities in our theoretical framework (e.g. permanent skill differentials or wealth differentials among unemployed workers).

The reason is that the IAB Job Vacancy Survey does not provide any guidance on the selection rate in these dimensions. Thereby, the results across groups would be driven by modeling and parametrization choices instead of being disciplined by the data.

3.1. Firms

There is a continuum of atomistic multi-worker firms indexed by i on the unit interval. Firms produce with a constant returns technology with labor as the only input. Employed workers lose their job with constant exogenous probability ϕ. In order to hire new workers, firms have to use two instruments. First, firms have to post a vacancy to get in contact with a random worker from the unemployment pool. Searching workers and vacancies are connected with a contact function.

Second, firms select a certain fraction of those workers they got in contact with. Technically, firms and workers draw a match-specific realization ε_{it} from an idiosyncratic training costs distribution with stable density $f(\varepsilon)$ and cumulative density $F(\varepsilon)$. Further, we assume a fixed training cost component tc^d that reflects that the average training required upon reemployment depends on the duration of the prior unemployment spell. This is consistent with the idea that human capital may depreciate during unemployment.

Firms are intertemporal profit maximizers. Firms' revenues consists of aggregate productivity, a_t, multiplied with firm-specific employment, n_{it}. Their costs consist of wages for incumbent workers, w_{it}^I, that are retained from the previous period plus wages and recruiting costs for newly hired workers. The latter consist of linear vacancy posting costs, κ, for each vacancy, v_{it}, average wage costs for these new workers, $\bar{w}_{it}^d$, and average training costs that contain a stochastic and a fixed part $(\bar{H}_{it}^d + tc^d)$. Firms post vacancies in an undirected search market. Once they have posted a vacancy, they randomly get in contact with an unemployed worker of any of the duration groups d. The probability for a firm of hiring an unemployed worker indexed by duration d depends on three factors: the share of unemployed workers indexed by d among all the searching workers s_t^d, the contact probability within this duration group q_t^d (which depends on aggregate market tightness), and the firm's selection rate, $\eta_{it}^d = \eta\left(\tilde{\varepsilon}_{it}^d\right)$, which depends on the firm's hiring cutoff $\tilde{\varepsilon}_{it}^d$.

Representative atomistic firms maximize the net present value of profits (discounting with δ) with respect to employment n_{it}, vacancies v_{it} and hiring cutoffs $\tilde{\varepsilon}_{it}^d$ for all duration groups.

$$E_0 \left\{ \sum_{t=0}^{\infty} \delta^t \left[\; a_t n_{it} - w_{it}^I (1-\phi) n_{i,t-1} - \kappa v_{it} - v_{it} \textstyle\sum_{d=0}^{12} s_t^d q_t^d \eta_{it}^d (\bar{w}_{it}^d + \bar{H}_{it}^d + tc^d) \; \right] \right\}, \quad (1)$$

subject to the evolution of the firm-specific employment stock, and the definitions of selection rates, the average idiosyncratic training costs and entrant wages (both conditional on being hired) for each duration group:

$$n_{it} = (1-\phi)n_{i,t-1} + v_{it}\sum_{d=0}^{12} s_t^d q_t^d \eta_{it}^d, \tag{2}$$

$$\eta_{it}^d = \int_{-\infty}^{\tilde{\varepsilon}_{it}^d} f(\varepsilon)d\varepsilon \quad \forall d, \tag{3}$$

$$\bar{H}_{it}^d = \frac{\int_{-\infty}^{\tilde{\varepsilon}_{it}^d} \varepsilon f(\varepsilon)d\varepsilon}{\eta_{it}^d} \quad \forall d. \tag{4}$$

$$\bar{w}_{it}^d = \frac{\int_{-\infty}^{\tilde{\varepsilon}_{it}^d} w\left(\varepsilon\right) f(\varepsilon)d\varepsilon}{\eta_{it}^d} \quad \forall d. \tag{5}$$

Maximization of the intertemporal profit function yields the optimal cutoff points and optimal number of vacancies posted by firms:

$$\tilde{\varepsilon}_{it}^d = a_t - \tilde{w}_{it}^d - tc^d + \delta(1-\phi)E_t\pi_{it+1}^I \quad \forall d, \tag{6}$$

and

$$\kappa = \sum_{d=0}^{12} s_t^d q_t^d \eta_{it}^d \bar{\pi}_{it}^d, \tag{7}$$

where π_{it}^I and π_{it}^d denote the firm's discounted profit at time t for an incumbent worker (indexed by I) and a newly hired worker in duration group d:

$$\pi_{it}^I = a_t - w_{it}^I + \delta(1-\phi)E_t\pi_{it+1}^I, \tag{8}$$

$$\bar{\pi}_{it}^d = a_t - \bar{w}_{it}^d - \bar{H}_{it}^d - tc^d + \delta(1-\phi)E_t\pi_{it+1}^I. \tag{9}$$

Note that all variables with a "tilde" sign are evaluated at the cutoff training costs $\tilde{\varepsilon}_{it}^d$, while variables with a "bar" (such as $\bar{H}_{it}^d$) correspond to the expectation of the respective variable conditional on hiring (i.e. the evaluation of the variable at the conditional mean of idiosyncratic training costs and/or wages). Intuitively, equation (6) shows that a firm selects workers in each duration group up to the point where the expected discounted present value of profits for the marginal worker is equal to his training costs. Only contacts with sufficiently low training costs, $\varepsilon_{it} \leq \tilde{\varepsilon}_{it}^d$, will result in a hire, where $\tilde{\varepsilon}_{it}^d$ is firm i's hiring cutoff and $\eta_{it}^d = \eta(\tilde{\varepsilon}_{it}^d)$ is the firm's selection rate (i.e. the hiring probability for a contact in a specific duration group). Although we call η_{it}^d the selection rate, it is important to emphasize that decisions are based on the joint worker-firm surplus of a given contact. Workers will be selected whenever there is a positive joint surplus, i.e. in this case workers are also willing to accept the job.

Equation (7) shows that a firm posts vacancies up to the point where the costs, κ, are equal to the expected returns from posting a vacancy. As search is undirected in our model, the firm may get in contact with workers of different duration groups, depending on their share in the unemployment pool s_t^d and the contact probability. The contact probability for each duration group, q_t^d, is driven by a Cobb-Douglas, constant returns to scale (CRS) contact function

$$c_t^d = \mu_t^d v_t^\gamma us_t^{1-\gamma} \quad \forall d, \tag{10}$$

where us_t are the number of searching workers at the beginning of period t, v_t is the vacancy stock, c_t^d is the number of contacts in period t made with unemployed with duration d, and μ_t^d is the contact efficiency that depends on the duration of unemployment. The contact probability for a firm and a worker in a given duration group are therefore:

$$q_t^d(\theta_t) = \mu_t^d \theta_t^{\gamma-1}, \tag{11}$$

and

$$p_t^d(\theta_t) = \mu_t^d \theta_t^\gamma \quad \forall d, \tag{12}$$

with aggregate market tightness defined as

$$\theta_t = \frac{v_t}{us_t}. \tag{13}$$

3.2. Workers

Workers have linear utility over consumption and discount the future with discount factor δ. Once separated from a job, a worker is entitled to 12 months of short-term unemployment benefits b^s and long-term unemployment benefits b_t^l afterwards, with $b^s > b_t^l$.

The value of unemployment therefore depends on the remaining months a worker is eligible for short-term unemployment benefits. For a short-term unemployed (i.e. $d \in \{1, \ldots, 12\}$) the value of unemployment is given by:

$$U_t^d = b^s + \delta E_t \left[p_{t+1}^{d-1} \eta_{t+1}^{d-1} \bar{V}_{t+1}^{d-1} + (1 - p_{t+1}^{d-1} \eta_{t+1}^{d-1}) U_{t+1}^{d-1} \right]. \tag{14}$$

In the current period, the short-term unemployed receives benefits b^s. In the next period, she either finds a job or remains unemployed. In the latter case the time left in short-term unemployment d is reduced by a month. The probability of finding employment in the next period will depend on the next period's contact probability and selection rate, which both depend on unemployment duration at that point.

After 12 months of unemployment the worker receives the lower long-term unemployment benefits b_t^l indefinitely or until she finds a job:

$$U_t^0 = b_t^l + \delta E_t \left[p_{t+1}^0 \eta_{t+1}^0 \bar{V}_{t+1}^0 + (1 - p_{t+1}^0 \eta_{t+1}^0) U_{t+1}^0 \right]. \tag{15}$$

The value of work for an entrant depends through the wage on the remaining months she is eligible for short-term benefits and on the realization of the idiosyncratic training cost:

$$V_t^d(\varepsilon_t) = w_t^d(\varepsilon_t) + \delta E_t\left[(1-\phi)V_{t+1}^I + \phi U_{t+1}^I\right] \quad \forall d. \tag{16}$$

Following our previous notation, $\bar{V}_t^d$ corresponds to the evaluation of $V_t^d(\varepsilon_t)$ at the conditional expectation of ε_t. We allow for the possibility of immediate rehiring. The resulting value of work for an incumbent worker I is:

$$V_t^I = w_t^I + \delta E_t\left[(1-\phi)V_{t+1}^I + \phi U_{t+1}^I\right], \tag{17}$$

where U_t^I denotes the outside option for an incumbent worker, in case that wage negotiations fail or she is exogenously separated from her job:

$$U_t^I = p_t^{12}\eta_t^{12}\bar{V}_t^{12} + (1 - p_t^{12}\eta_t^{12})U_t^{12}. \tag{18}$$

3.3. Wages

In the main part of the paper, we assume individual Nash bargaining for both new and existing matches. Workers and firms bargain over the joint surplus of a match, where workers' bargaining power is α and firms' bargaining power is $(1-\alpha)$. The Nash bargained wages therefore solve the following problems:

$$w_t^d(\varepsilon_t) \in \arg\max\left(V_t^d(\varepsilon_t) - U_t^d\right)^\alpha \left(\pi_t^d(\varepsilon_t)\right)^{1-\alpha} \quad \forall d \tag{19}$$

for newly hired workers with prior duration index d and

$$w_t^I \in \arg\max\left(V_t^I - U_t^I\right)^\alpha \left(\pi_t^I\right)^{1-\alpha} \tag{20}$$

for an incumbent worker.

Maximizing the Nash product yields the following explicit wage equations for incumbents

$$w_t^I = \alpha\left[a_t + \delta p_t^{12}\eta_t^{12}E_t\pi_{t+1}^I\right] + (1-\alpha)\begin{bmatrix} -p_t^{12}\eta_t^{12}E_t\left[(\delta V_{t+1}^I - \bar{V}_t^{12})\right] \\ -(1-p_t^{12}\eta_t^{12})E_t\left[(\delta U_{t+1}^I - U_t^{12})\right]\end{bmatrix}, \tag{21}$$

and different duration groups of entries:

$$\begin{aligned} w_t^d(\varepsilon_t) =& \alpha\left[a_t - \varepsilon_t - tc^d + \delta E_t\left[p_{t+1}^{d-1}\eta_{t+1}^{d-1}\pi_{t+1}^I\right]\right] \\ &+ (1-\alpha)\begin{bmatrix} b^S - \delta E_t p_{t+1}^{d-1}\eta_{t+1}^{d-1}(V_{t+1}^I - \bar{V}_{t+1}^{d-1}) \\ -\delta E_t\left[(1-p_{t+1}^{d-1}\eta_{t+1}^{d-1})(U_{t+1}^I - U_{t+1}^{d-1})\right]\end{bmatrix} \quad \forall d \in \{1,\ldots,12\} \end{aligned} \tag{22}$$

$$\begin{aligned} w_t^0(\varepsilon_t) =& \alpha\left[a_t - \varepsilon_t - tc^0 + \delta E_t\left[p_{t+1}^0\eta_{t+1}^0\pi_{t+1}^I\right]\right] \\ &+ (1-\alpha)\begin{bmatrix} b_t^L - \delta E_t p_{t+1}^0\eta_{t+1}^0(V_{t+1}^I - \bar{V}_{t+1}^0) \\ -\delta E_t\left[(1-p_{t+1}^0\eta_{t+1}^0)(U_{t+1}^I - U_{t+1}^0)\right]\end{bmatrix}. \end{aligned} \tag{23}$$

Note that these equations would collapse to the standard Nash bargaining solution if we just had one duration group. Several things are worth emphasizing: The benefits for long-term unemployed directly affect the wage for long-term unemployed, as they are part of their outside option. They also affect the wage of short-term unemployed. However, this happens in an indirect way through the expected value of long-term unemployment $E_t U^0_{t+1}$. Obviously, the outside option and thereby the wage will be affected more by changes of b^L, the closer workers are to long-term unemployment

The individually bargained wage will depend on the idiosyncratic training cost component as the latter enters firms' profits $\pi^d_t(\varepsilon)$.[9] In Appendix C.2 we show results for the case when wages are bargained collectively.

3.4. Unemployment Dynamics

As the total labor force is normalized to one, the total number of unemployment (and also the unemployment rate) in period t after matching has taken place is the sum over all unemployment states ($d \in \{0, ..., 12\}$):

$$u_t = \sum_{d=0}^{12} u^d_t. \tag{24}$$

Employment in period t is thus given by

$$u_t = 1 - n_t. \tag{25}$$

The number of unemployed with 12 remaining months of short-term benefits is given by the workers that have been separated at the end of last period and were not immediately rehired:

$$u^{12}_t = \phi(1 - p^{12}_t \eta^{12}_t) n_{t-1}. \tag{26}$$

The law of motion for unemployment with remaining eligibility of short-term unemployment benefits $d \in \{1, \dots, 11\}$ is:

$$u^d_t = (1 - p^d_t \eta^d_t) u^{d+1}_{t-1}, \tag{27}$$

and the pool of long-term unemployed consists of the unemployed whose short-term benefit eligibility has just expired as well as previous period's long-term unemployed that have not been matched:

$$u^0_t = (1 - p^0_t \eta^0_t)(u^1_{t-1} + u^0_{t-1}). \tag{28}$$

We can now define the number of searching workers at the beginning of period t (before matching has taken place):

[9] Due to the training costs in the first period, the wage for entrants is smaller than the wage for incumbents in our bargaining setup. However, the net present value of the match for workers and firms at the time of hiring is equivalent to a wage contract where the training costs in the wage are spread over the entire employment spell. Results are available on request.

$$us_t = \phi n_{t-1} + u_{t-1}. \quad (29)$$

The share of searching workers with remaining short-term unemployment eligibility of d months among all searchers is therefore:

$$s_t^{12} = \frac{\phi n_{t-1}}{us_t}, \quad (30)$$

for newly separated workers,

$$s_t^d = \frac{u_{t-1}^{d+1}}{us_t}, \quad (31)$$

for $d \in \{1, \ldots, 11\}$ and

$$s_t^0 = \frac{u_{t-1}^1 + u_{t-1}^0}{us_t} \quad (32)$$

for long-term unemployed.

3.5. Labor Market Equilibrium

Given initial values for all states of unemployment u_{t-1}^d with $d \in \{0, ..., 12\}$ and employment n_{t-1} as well as processes for productivity, long-term unemployment benefits, and the spell-dependent contact efficiency $\left\{a_t, b_t^l, \mu_t^d\right\}_{t=0}^{+\infty}$, the labor market equilibrium is a sequence of allocations
$\left\{n_t, u_t, us_t, v_t, \theta_t, u_t^d, s_t^d, p_t^d, q_t^d, \tilde{\varepsilon}_t^d, \eta_t^d, \bar{H}_t^d, \pi_t^I, \bar{\pi}_t^d, V_t^I, \bar{V}_t^d, \tilde{V}_t^d, U_t^I, U_t^d, w_t^I, \bar{w}_t^d, \tilde{w}_t^d\right\}_{t=0}^{+\infty}$
for all durations $d \in \{0, ..., 12\}$ that satisfy the following equations: the definition of employment (25), unemployment (24) and searching workers (29), market tightness (13), unemployment (26) - (28), the shares of searching workers (30) - (32), the contact rates for workers (12) and firms (11), the free-entry condition for vacancies (7), the hiring cutoffs (6), selection rates (3), conditional expectation of idiosyncratic hiring costs (4) and wages (5)[10], the definition of profits for entrants (9) and incumbents (8), the value of a job for entrants (16) and incumbents (17), the value of unemployment (14), (15), and (18), and wages (21), (22) and (23).

4. Empirical Strategy

The German Hartz IV reform reduced the replacement rate for long-term unemployed. Less generous unemployment benefits decrease workers' fallback option in our model. The closer unemployed workers get to the expiration of short-term benefits, the lower will be the value of unemployment and the lower will be their reservation wage. This leads to lower bargained wages. One of the key differences of our model relative to the plain vanilla search and matching model (e.g. Pissarides, 2000, Ch.1) is that matching has two components and that two effects are initiated due to a decline in benefits. First,

[10] In equilibrium, all atomistic firms behave symmetrically. Therefore, we can drop the firm indices i in the previous five equations.

workers and firms have to get in contact with one another, where p_t denotes workers' contact rate. Lower unemployment benefits lead to more vacancy posting by all firms due to higher expected profits. In equilibrium, this leads to a higher tightness in the market and thus a higher contact rate for workers. We call this mechanism the equilibrium effect. Second, upon contact a certain fraction of workers is selected at the firm level, where η_t denotes the selection rate. With lower unemployment benefits and hence lower wages, firms are willing to select workers with higher idiosyncratic training costs at the margin (as these worker-firm pairs now have a positive bilateral surplus from employment relative to non-employment). Thus, the selection rate increases. As this effect takes place at the worker-firm level, we call this mechanism the partial effect.

As the aggregate contact and selection rate are roughly multiplicative in our model ($jfr_t \approx p_t \eta_t$),[11] we can express the job-finding rate as the sum of the contact rate and the selection rate in terms of log-deviations (denoted with hats):

$$\hat{jfr}_t \approx \hat{p}_t + \hat{\eta}_t, \tag{33}$$

where $\hat{p}_t$ corresponds to the equilibrium effect and $\hat{\eta}$ corresponds to the partial effect. For the sake of simplicity, we will refer to the response of the selection rate to a benefit change as the partial effect. This is slightly inaccurate wording, as there is a small, negative feedback effect from tightness on the selection rate.

This section proceeds in four steps. We will first describe the construction of an empirical measure of the selection rate. We will then demonstrate how the combination of model and data informs us about the partial and equilibrium effect of a benefit reform. In steps three and four, we will describe the empirical estimation and implementation for partial and equilibrium effect.

4.1. Measuring Selection

A core innovation of our paper is the construction of an empirical time series for the selection rate. Our approach is closely related to Davis et al. (2013) and Gavazza et al. (2018). Davis et al. (2013) show that firms use additional margins to vacancy posting to adjust hiring over the business cycle. Labor selection represents one of these. We are the first to provide direct evidence on this channel.

As our choice of measurement is informed by the model, it is useful to think about the role of selection through the lens of the model. The selection rate corresponds to the share of workers that is hired upon meeting a firm or, put differently, the probability that a worker that gets in contact with a firm, is hired. Therefore, the selection rate corresponds to the inverse of the average number of contacts a firm makes until it realizes a hire. Figure 4 shows the response of the total number of contacts in the model economy and the number of contacts for the last hire in response to a negative benefit shock in a model

[11] Note that this connection holds with equality for each duration group $jfr_t^d = p_t^d \eta_t^d$. In aggregate, it only holds with equality on impact when the shares of unemployed workers in different duration groups are equal to the steady state shares. During the adjustment dynamics, composition effects start playing a role. See discussion in Section 6.

simulation.[12] The total number of contacts first goes up after the decline of benefits because workers' contact rate increases due to more vacancy posting (the equilibrium effect). In the medium run, it converges to a new steady state below the initial level because the pool of unemployed declines over time. Very importantly, the number of contacts per hire, which equals the inverse of the selection rate, has completely different dynamics. It drops on impact and stays at a permanently lower level. For example, when a multi-worker firm has a selection probability of 50%, it has on average two contacts per hire. When the firm selects only 33% of workers, it requires on average three contacts until hiring. As firms select a larger fraction of contacts when benefits - and hence workers' outside option - fall, the number of contacts per hire goes down. Note that if the selection rate was constant, as standard in many search and matching models, the number of matches and the number of contacts in the economy would rise in equal proportion and the number of contacts per hire would not change. With an increased selection rate, however, hires rise more than proportionally which is reflected in a lower number of contacts per hire.

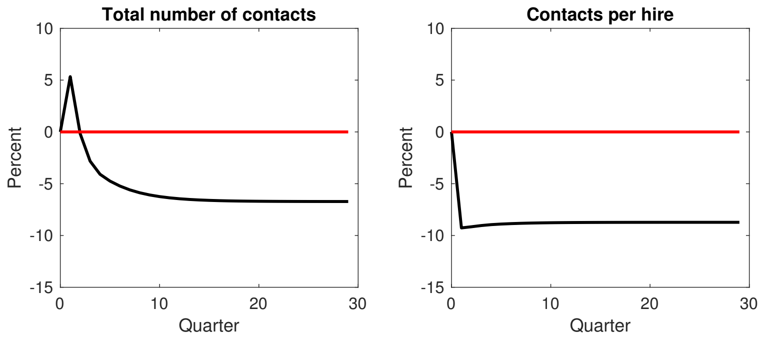


Figure 4: Response of the total number of contacts and contacts per hire in response to a reduction of the replacement rate for long-term unemployed in the baseline calibration.

Based on these insights, we are the first to construct a time series for selection over the business cycle. For this purpose, we use the IAB Job Vacancy Survey which is an annual representative survey of up to 14,000 German establishments (for more information on the dataset, see Appendix B.1). Firms are asked about the number of suitable applicants for their last realized hire. The question is well in line with our model. Given that firms are asked about the number of *suitable* applicants, firms must have screened these candidates in some way (e.g. by checking the application package or by inviting the applicant for an interview). The number of suitable applicants, therefore, is a natural proxy for the number of contacts a firm has made for the last hire. Thus, we can calculate the average probability of a worker (who got in contact with a firm) to be selected as the inverse of the number of suitable applicants for the last hire. Note that the IAB Job Vacancy

[12]Note that the IRFs are based on the calibration as described below. At this stage, we show them for illustration purposes and only discuss the qualitative response.

Survey is a repeated cross section and hence does not allow to perform firm-level panel regressions. Thus, we can only run regressions at aggregated levels. Using representative survey weights for the last hire, we, therefore, construct annual selection rate time series on the national (West Germany as a baseline and entire Germany as robustness[13]), state and industry level from 1992 to 2015.

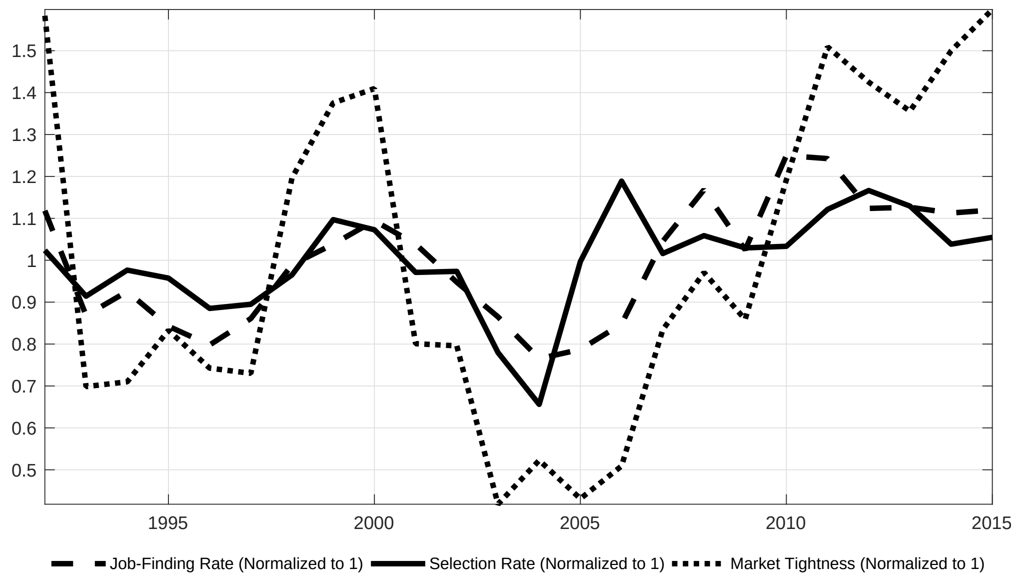
Figure 5 shows the movement of the job-finding rate (defined as matches over unemployment), selection rate and market tightness (defined as vacancies over unemployment) from 1992 to 2015 (for details on the data, see appendix B). We normalized all three time series to a mean of one to improve the visibility of relative movements (i.e. we divided each time series by its sample mean). As predicted by theory, both the job-finding rate and the selection rate move procyclically with market tightness, although the latter shows much stronger fluctuations. This is well in line with our model. Kohlbrecher et al. (2016) show that the selection rate comoves procyclically (but less than proportionally) with market tightness over the business cycle in a selection model. Note that in the standard model without selection, the share of selected applicants would be constant over the business cycle as the number of contacts and the number of hires would comove one to one.

In addition, the descriptive evidence is well in line with the idea of endogenous recruiting intensity by Davis et al. (2013) and Gavazza et al. (2018). Labor selection may be one important dimension of recruiting intensity. Figure 5 shows that firms increase their labor selection rate in labor market upswings and reduce it in labor market downswings. This illustrates that they do not only use the vacancy margin over the business cycle. They also become more or less selective in terms of their hiring behavior.

4.2. Linking the Model to the Data

How are these time series helpful for our identification? Ideally, we would be able to identify the reaction of the job-finding rate with respect to benefit changes directly, namely $\partial \hat{jfr}_t / \partial \hat{b}_t$. Besides the usual econometric issues, this is particularly complicated for the Hartz IV reform. First, several other labor market reforms (namely, Hartz I to III) were implemented in 2003 and 2004 briefly before the Hartz IV reform. These may have affected the job-finding rate through increases in contact efficiency (see e.g. Launov and Wälde, 2016). Thus, based on time series for the job-finding rate it is very difficult to disentangle the effects of the different reform steps. Second, there is a severe structural break in the unemployment series in 2005 as a direct consequence of the reform. In order to be eligible for benefits after 2005, former recipients of social assistance who were able to work had to register as unemployed. Those people had not been counted as unemployed under the old system. There is both a shift in the level as well as the composition of the unemployment pool. We correct for the structural break in terms of

[13]We restrict the baseline analysis to West Germany for two reasons. First, the conditions in East Germany were driven by the transformation to a market economy in the 1990s. Labor market turnover rates in East Germany have converged to those of West Germany only by 2008 (see Fuchs et al., 2018). Second, the number of establishments in the sample is very small in the early 1990s.



Note: For visibility, we normalized all three time series to mean one. The job-finding rate (matches over unemployment) and market tightness (vacancies over unemployment) are constructed with the corrected unemployment series (as described in appendix D.2).

Figure 5: German labor market dynamics, 1992-2015.

the level shift (for details, see appendix D.2)[14] and use the corrected series for estimating dynamics over the business cycle. In a robustness check in appendix D.2, we show that the estimated business cycle dynamics for the uncorrected series are very similar. Finally, a third and related reason for not relying on the job-finding rate around the reform is that unemployment in Germany is based on registration with the Federal Employment Office and not self-reported job search. Unemployed workers not eligible for long-term benefits after the reform may have decided not to register while still searching. Therefore, only looking at unemployment to employment transitions might miss some of the employment effects if formerly unemployed workers found jobs after deregistering. Recent evidence on labor market flows around the time of the Hartz reforms by Carrillo-Tudela et al. (2018) suggests that this channel indeed played an important role.

To circumvent these problems, we apply a novel empirical strategy that combines insights from our theoretical model with our unique time series and panel data. We start with the observation that we can decompose the reaction of the job-finding rate to benefit changes into changes of the contact rate and selection rate:

$$\frac{\partial \hat{jfr}_t}{\partial \hat{b}_t} \approx \frac{\partial \hat{p}_t}{\partial \hat{b}_t} + \frac{\partial \hat{\eta}_t}{\partial \hat{b}_t}. \tag{34}$$

[14]We use the corrected unemployment series for the construction of the job-finding rate and market tightness

Unfortunately, we cannot provide any estimates for the reaction of the contact rate, $\partial\hat{p}_t/\partial\hat{b}_t$, because there is no direct and independent measure available. Our identification therefore consists of two steps. First, we will directly estimate the partial effect $(\partial\hat{\eta}_t/\partial\hat{b}_t)$ based on the reaction of the selection rate at the time of the Hartz IV reform. As the selection rate is based on an establishment survey, it is not affected by the structural measurement break in the unemployment series in 2005.

Second, we use an indirect inference method to estimate the equilibrium effect, i.e. the response of the contact rate $\partial\hat{p}_t/\partial\hat{b}_t$. With an estimate of the partial effect (i.e. selection) with respect to benefits $(\partial\hat{\eta}_t/\partial\hat{b}_t)$, all we need to know is how important the response of the contact rate is relative to the response of the selection rate to a benefit shock. Through the lens of our model, the relative contributions of the contact rate and the selection rate to the transmission of aggregate shocks (in our case, an aggregate productivity shock) and benefit changes are equivalent. We show this analytically in the context of a simplified model in Appendix C.1. We can also numerically show that this feature holds approximately in our full quantitative model. More precisely, in our model the following relation is true:

$$\frac{\partial\hat{p}_t/\partial\hat{b}_t}{\partial\hat{\eta}_t/\partial\hat{b}_t} \approx \frac{\partial\hat{p}_t/\partial\hat{\theta}_t}{\partial\hat{\eta}_t/\partial\hat{\theta}_t}. \tag{35}$$

We can therefore use the business cycle behavior of the job-finding rate and the selection rate to infer the relative importance of the equilibrium effect. To be more precise, we will use the following decomposition:

$$\frac{\partial \hat{jfr}_t}{\partial\hat{\theta}_t} \approx \frac{\partial\hat{p}_t}{\partial\hat{\theta}_t} + \frac{\partial\hat{\eta}_t}{\partial\hat{\theta}_t}. \tag{36}$$

The job-finding rate over the business cycle is a function of market tightness and it can be decomposed into the comovement of the contact rate and the selection with respect to market tightness. Thus, in order to identify the relative importance of the equilibrium effect over the business cycle $\left[\partial\hat{p}_t/\partial\hat{\theta}_t\right] / \left[\partial\hat{\eta}_t/\partial\hat{\theta}_t\right]$, we estimate the elasticity of the job-finding rate with respect to market tightness and the elasticity of the selection rate with respect to market tightness. By equation (36), these two measures pin down the elasticity of the contact rate with respect to market tightness, which in the model is given by the parameter γ (see equations (10) and (12)).

Note that our model predicts a positive elasticity of the selection rate with respect to market tightness. This does not reflect a direct link from tightness to selection[15] but a joint comovement in response to business cycle shocks (productivity shocks in our case). Kohlbrecher et al. (2016) show in a similar model structure that this joint comovement has an analytical expression in the steady state, namely:[16]

$$\frac{\partial\ln\eta}{\partial\ln\theta} = \frac{f(\tilde{\varepsilon})}{\eta}\left(\tilde{\varepsilon} - \frac{\int_{-\infty}^{\tilde{\varepsilon}} \varepsilon f(\varepsilon)\, d\varepsilon}{\eta}\right) > 0. \tag{37}$$

[15] As argued, the direct effect of tightness on selection is negative although quantitatively small.

[16] This expression is only exactly true in the steady state of a model with a constant contact rate.

Two implications follow: First, the elasticity of the selection rate with respect to market tightness is directly tied to the shape of the training cost distribution at the hiring cutoff. Indeed, this target jointly with a target for the mean selection rate will uniquely pin down the parameters of the training cost distribution. Second, if both the selection rate and the contact rate move procyclically over the business cycle, the elasticity of each variable with respect to market tightness must be strictly smaller than the estimated elasticity of the job-finding rate with respect to market tightness.[17]

4.3. Determining the Partial Effect

Our new time series for the selection rate allows us to estimate the partial effect for the Hartz IV reform. Visual inspection of Figure 5 shows that the selection rate - in line with our model prediction - increased substantially in 2005 and 2006 when the two steps of the Hartz IV reform were implemented. We have argued before that it is very difficult to estimate the effects of Hartz IV based on the time-series data on unemployment and the job-finding rate. In the following, we provide several arguments why these caveats do not apply to the selection rate. First, the selection rate is derived from the IAB Job Vacancy Survey and is, therefore, not affected by the change of registration requirements/incentives and the resulting structural break in the unemployment series in the administrative data. In addition, the selection rate is not directly affected by labor market reforms that improve matching efficiency. Launov and Wälde (2016) argue that the reform of the Federal Employment Agency (Hartz III reform in 2004) has increased the matching efficiency in Germany substantially and was therefore a key contributor for the decline of unemployment in Germany. In our model, however, improved matching efficiency does not directly impact the selection rate, as selection takes place after contacts between workers and firms have been established. While there is an indirect effect though bargained wages, it is negative and small (see Figure C.2 in the Appendix). In this case, we obtain a lower bound when we estimate the partial effect. One might also object that the selection rate could be affected by changes in workers' search behavior caused by the reform. A common perception is that under the new and stricter benefit regime workers are required to document search effort, e.g. in forms of written applications. Two scenarios are possible: Either, these forced applications are not meaningful and do not make it into the pool of suitable applicants. In this case the selection rate would be unaffected. Alternatively, the number of suitable applicants increases which could lower the measured selection rate. In this case, the estimate for the selection rate would be downward biased, i.e. we would obtain a lower bound. Finally, it is worthwhile pointing out that the reform introduced a permanent change in policy. For all these reasons, it is a valid strategy to estimate the reform effect with a simple shift dummy. Of course, while the selection rate is not immediately affected by changes in tightness, there is a small feedback effect through the influence of contact rates on wages. We show that the change of the selection rate is barely affected if we control for tightness in our estimations.

[17] We refer the reader to Kohlbrecher et al. (2016) for analytical proofs and a more detailed discussion of the above decomposition.

In order to obtain the partial effect of Hartz IV on the selection rate, we run versions of the following regression:

$$\ln \eta_t = \beta_0 + \beta_1 D_t^{Hartz\ IV} + \beta_2 BI_t + \beta_3 X_t + \nu_t. \tag{38}$$

The dependent variable $\ln \eta_t$ is the logarithm of the selection rate, which is regressed on a shift dummy that takes the value of one from 2005 onward ($D_t^{Hartz\ IV}$) to measure the differences in the selection rate before and after Hartz IV. In order to disentangle the Hartz IV effect from the business cycle, our regression contains business cycle indicators (BI). In the first specification, we use value added growth for West Germany. We use value added because we have constrained ourselves to West Germany where GDP is not readily available. Using GDP growth for entire Germany as an alternative business cycle indicator leaves our results unaffected. In the second specification, we add market tightness θ_t as an additional business cycle indicator. Furthermore, X_t denotes controls which we add in robustness checks, and ν_t is the error term. Due to data availability, we perform the baseline estimation on an annual basis for the sample range 1992 to 2015. In a robustness check, we also perform a fixed-effects panel estimation at West German state and industry level as well as for entire Germany (including East Germany, see appendix D.1), which yields very similar results.

Table 1 shows that conditional on value added growth the aggregate selection rate has increased by 14% after the Hartz IV reform. If we additionally control for market tightness, the coefficient is slightly smaller (12%). The estimated coefficients are statistically significant at the 1% level. Given that the specification with value added growth and market tightness (column 2 of Table 1) results in the most conservative effect on the Hartz IV reform, we take this estimation as a target for our model simulation.

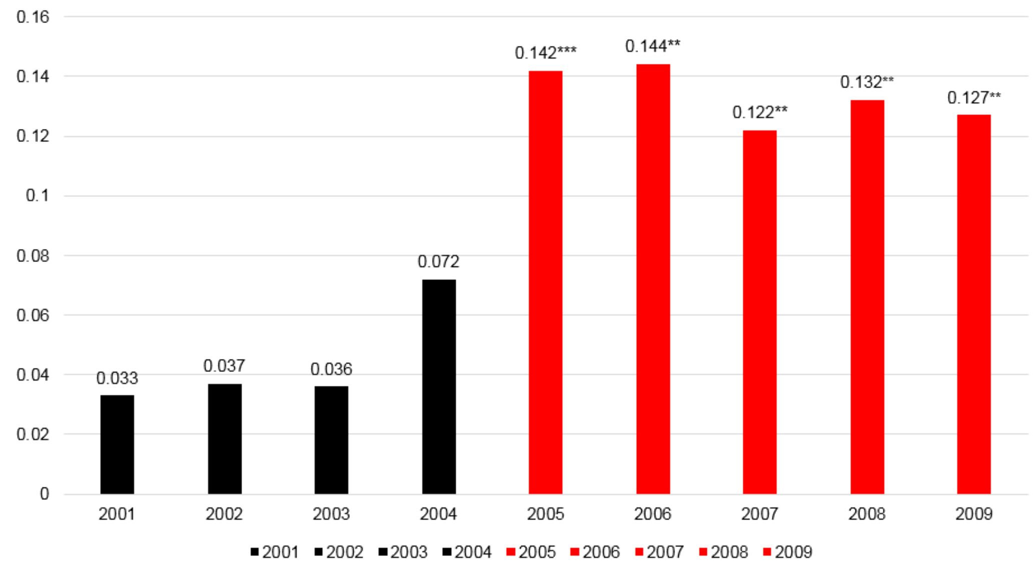
Of course, our estimation does not allow us to causally link this effect to the reform but documents a strong positive comovement. In the above discussion, we have already ruled out several alternative explanations for this sharp increase of the selection rate. In addition, a number of robustness checks support our result that the observed increase of the selection rate is indeed linked to the benefit reform. In columns 3 and 4 of Table 1, we show that the results are very similar if we disaggregate by state and industry and use a panel fixed-effects estimator. Moreover, our results are robust if we control for the share of vacancies for low-qualification jobs and the share of long-term unemployed in the pool of unemployment (see columns 5 and 6 in Table 1).

Table 1: Estimates of the partial effect, results for West Germany, 1992-2015.

	Dependent variable:					
	log(selection rate)					
	OLS		*panel linear FE*		*OLS*	
	Aggregate West Germany	Aggregate West Germany	State Level	Industry Level	Aggr.: Low Qualification	Aggr.: Long-term U
	(1)	(2)	(3)	(4)	(5)	(6)
Hartz IV dummy	0.14***	0.12***	0.12***	0.12***	0.19***	0.15**
	(0.04)	(0.04)	(0.03)	(0.02)	(0.04)	(0.07)
log(market tightness)		0.14**				
		(0.07)				
value added growth	1.11*	0.51	0.16	2.00*	0.75	0.65
	(0.60)	(0.75)	(0.72)	(1.07)	(0.63)	(0.74)
log(low qualification)					0.46**	
					(0.19)	
log(share long-term u)						0.23
						(0.31)
Constant	−0.79***	−0.58***			0.15	−0.56*
	(0.04)	(0.09)			(0.38)	(0.34)
Observations	24	24	120	192	24	18
R^2	0.37	0.55	0.14	0.13	0.62	0.36
Adjusted R^2	0.31	0.48	0.09	0.09	0.56	0.23
Residual Std. Error	0.11 (df = 21)	0.09 (df = 20)			0.09 (df = 20)	0.13 (df = 14)
F Statistic	6.07*** (df = 2; 21)	8.18*** (df = 3; 20)	8.87*** (df = 2; 113)	14.16*** (df = 2; 182)	10.94*** (df = 3; 20)	2.66* (df = 3; 14)

Note: Aggregate estimation by OLS with robust standard errors; Panel estimation with fixed effects (state and industry fixed effects respectively) and robust standard errors clustered at group level. Due to data availability, the regression for long-term unemployed covers the shorter time span 1998 - 2015. *p<0.1; **p<0.05; ***p<0.01

Could the increase in the selection rate capture some general trend in the economy unrelated to the labor market reform? In order to illustrate that our Hartz IV-dummy effect is no coincidence, we perform several regressions with placebo shift-dummies in the years before and after the reform. Figure 6 clearly supports our view that the jump of the selection rate took place from 2005 onwards, the time when the first step of the Hartz IV reform was implemented. The shift dummy starts being statistically different from zero from 2005 onwards. This is completely in line with our theoretical framework, which predicts that the selection rate increases on a permanent basis once the Hartz IV labor market reform was implemented. In addition, the significance of the shift dummy from 2005 onwards shows that the increase of the selection rate cannot be attributed to earlier Hartz reforms (I to III) or the wage moderation starting in the early 2000s.



Note: The red bars denote significant dummy estimates at the 1 percent (***) and 5 percent (**) significance level.

Figure 6: Alternative shift-dummies starting in each respective year (controlling for value added growth).

Figure E.4 in Appendix E shows further that the increase of the selection rate between 2004 and 2005 was largest for workers in the middle of the skill distribution.[18] This is in line with our expectations. Compared to workers in the middle of the skill distribution, low-skilled workers faced a moderate decline of the replacement rate due to the Hartz IV reform (see Section 2), while high-skilled workers usually face short unemployment spells and therefore a lower risk of becoming long-term unemployed. Thus, medium-skilled workers were hit hardest by the Hartz IV reform and thereby reacted most in terms of the selection rate. While we agree that it would be desirable to study the cross-sectional

[18]The question on skills is only available from 2004 onwards. Therefore, we can only provide anecdotal evidence for the upward shift between 2004 and 2005.

response of the selection rate in more detail, we are limited by information provided in the data. Information on skills (for the selection rate) is for example not available for a longer time series.

Before we continue our discussion, let us briefly comment on the estimated results for the business cycle indicators. The selection rate comoves positively with value added growth. However, the estimated coefficient on value added growth are not very precisely estimated in Table 1. Two comments are in order: First, real GDP dropped by around 5% during the Great Recession, while the labor market barely reacted. This phenomenon is known as the German "labor market miracle" (see Burda and Hunt, 2011). Not surprisingly, this phenomenon reduces the statistical significance of value added growth in our estimations. Second, the comovement between the selection rate and market tightness is statistically significant at the 1% level because market tightness represents the state of the labor market much better than value added. This is very much in line with our model that predicts a positive comovement of the selection rate with market tightness (see equation (37)). Controlling for tightness in addition to value added growth (see column 2) does not change the estimated coefficient on the Hartz IV dummy by much.

Finally, although we cannot establish a causal relationship between Hartz IV and the partial effect on the selection rate in a microeconometric sense, our approach has two virtues: (i) we are the first to create (semi-)aggregated time series that correspond directly to the partial effect in our model, (ii) we show that the selection rate has shifted upwards in an economically and statistically significant way from 2005 onwards. We believe that reverse causality is not an issue in our regressions because the Hartz IV reform was an exogenous event that was certainly not affected by the selection rate. The parliamentary discussions about the Hartz IV reform started in 2003. The Hartz IV law passed both chambers in 2004. To the extent that employers and employees anticipated the reform, our model predicts that the selection rate would already increase before the actual implementation. In this case, we would expect a significant placebo shift dummy for 2004, which we do not find (Figure 6). Even if anticipation effects played a role, our dummy estimate for 2005 would constitute a lower bound.

As further reassuring evidence, we can compare ourselves to Price (2018) who uses German administrative worker-level data to analyze the partial effect of Hartz IV. He estimates the causal microeconomtric effects of Hartz IV. We show below that our and Price's (2018) partial effects are of a similar order of magnitude despite a different data source and very different methodologies. Our partial effects are even more conservative. Finally, while the similarity of the partial effects of our and Price's (2018) approach is very reassuring, the benefit of our framework is that we provide additional and independent evidence and can make statements on the size of the equilibrium effect on top of the partial effect.

4.4. Determining the Importance of Partial and Equilibrium Effect

We target the estimated partial effect of Section 4.3 in our calibration. In different words, in our simulation exercise, we reduce the long-term unemployment benefits by the amount necessary to obtain a 12% increase of the aggregate selection rate in a partial equilibrium version of our model, i.e. keeping contact rates constant.[19] This corresponds to the coefficient obtained from column (2) of Table 1 in which we control for both value added growth and tightness and which is the lower bound of our estimates. Our approach distinguishes us from Krause and Uhlig (2012), Krebs and Scheffel (2013), and Launov and Wälde (2013) who all impose a certain decline of the replacement rate based on exogenous sources. Instead, we target an outcome variable of our model observable in the data.

Now, given an estimate for the partial effect, we need to determine the relative importance of partial and equilibrium effect. As explained in Section 4.2, we make use of two insights from our model: The relative importance of partial and equilibrium effects are the same for benefit and business cycle shocks (see Appendix C.1) and the elasticity of the job-finding rate with respect to market tightness is determined by the elasticity of the selection and contact rate with respect to market tightness:

$$\frac{\partial \hat{p}_t / \partial \hat{b}_t}{\partial \hat{\eta}_t / \partial \hat{b}_t} \approx \frac{\partial \hat{p}_t / \partial \hat{\theta}_t}{\partial \hat{\eta}_t / \partial \hat{\theta}_t} \tag{39}$$

$$\frac{\partial \hat{jfr}_t}{\partial \hat{\theta}_t} \approx \frac{\partial \hat{p}_t}{\partial \hat{\theta}_t} + \frac{\partial \hat{\eta}_t}{\partial \hat{\theta}_t}. \tag{40}$$

Our newly constructed time series allows us to estimate both the elasticity of the job-finding rate and the elasticity of the selection rate with respect to market tightness. We run the following regressions:

$$\ln jfr_t = \varphi_0 + \varphi_1 \ln \theta_t + \varphi_2 D_t^{Hartz\ IV} + \zeta_t, \tag{41}$$

$$\ln \eta_t = \beta_0 + \beta_1 \ln \theta_t + \beta_2 D_t^{Hartz\ IV} + \nu_t, \tag{42}$$

In analogy with equation (38), we include a shift dummy from 2005 onwards.[20]

The estimated elasticities are equal to 0.27 for the job-finding rate and 0.15 for the selection rate (see Table 2). We are the first to estimate the elasticity of the selection rate from time series data and thereby to quantify the contribution of the selection margin for the behavior of the job-finding rate over the business cycle. By targeting both estimated elasticities in a dynamic business cycle simulation of our model, we can uniquely determine the contact elasticity, in turn.

Two things are worth pointing out in this context. First, the estimated elasticity of the job-finding rate with respect to market tightness is well in line with other matching function estimations for Germany such as Hertweck and Sigrist (2013) (based on data from the German Socio-Economic Panel) and Kohlbrecher et al. (2016) (based on detailed administrative data). Second, in line with our model prediction, the elasticity of the

[19]We could also target the increase of the selection rate in the full equilibrium model. However, as we condition on tightness in the estimation, we do the same in our calibration. In addition, the small feedback effect from tightness on wages lowers the response of the selection rate. We therefore choose the more conservative strategy.

[20]While we corrected the series for the level shift in unemployment, there is still a structural break due to the change in the composition of the unemployment pool.

Table 2: Regression results for West Germany, 1992-2015.

	Dependent variable:	
	log(selection rate)	log(job-finding rate)
	(1)	(2)
Hartz IV dummy	0.12***	0.09***
	(0.04)	(0.03)
log(market tightness)	0.15**	0.27***
	(0.06)	(0.04)
Constant	−0.56***	−2.61***
	(0.08)	(0.05)
Observations	24	24
R^2	0.54	0.78
Adjusted R^2	0.50	0.76
Residual Std. Error (df = 21)	0.09	0.07
F Statistic (df = 2; 21)	12.50***	38.02***

*Note: Estimation by OLS with robust standard errors; *p<0.1; **p<0.05; ***p<0.01* selection rate with respect to market tightness is smaller than the elasticity of the job-finding rate. Thus, the dynamics of the job-finding rate is both driven by contact and selection. The partial effect and the equilibrium effects are of roughly similar size.[21]

5. Calibration

We calibrate our model to West-German data.[22] We choose a monthly frequency with a discount factor of $0.99^{\frac{1}{3}}$ and normalize aggregate productivity to 1. Furthermore, we assume that firms and households have equal bargaining power (i.e. $\alpha = 0.5$). The short-term unemployed in Germany receive unemployment benefits that amount to 60% or 67% of the last net wage, the long-term unemployed received 53% or 57% prior to the Hartz IV reform. As the unemployed may also enjoy some home production or utility from leisure, we choose the upper bound of the legal replacement rates for our calibration. We set the replacement rates to 67% and 57% of the steady state incumbent wage in our model. We set the monthly separation rate to 1.6% to target a steady state unemployment rate of 9%. This corresponds to the average unemployment rate in our sample prior to the reform. Likewise, we target the steady state market tightness to its pre-reform empirical average of 0.25, which pins down the value of the vacancy posting costs.

The rest of the parameters are pinned down by six additional targets that we can measure in the data: The exit rates out of short-term and long-term unemployment, the aggregate selection rate, the relative contact rates of long-term versus short-term unemployed, as well as the elasticity of both the selection rate and the job-finding rate with respect to market tightness.

Using the data provided by Klinger and Rothe (2012), the pre-reform exit rates out of unemployment are 16% and 6.5% for short-term and long-term unemployed. In our model, this could be driven by both lower contact rates and lower selection rates over time. How can we differentiate between the two? We observe the average pre-reform selection rate form the Job Vacancy Survey, which is 46%, and take that as given. Unfortunately, we cannot differentiate selection rates for long-term and short-term unemployed with our firm dataset. We therefore use the information contained in the IAB PASS survey (see Appendix B for details). In this survey, respondents are asked whether they have had a job interview during the last four weeks. We compute the contact rate as the share of respondents who answer this question affirmatively. It turns out that the contact rate for ALG II recipients (i.e. long-term unemployed) is 45% of the contact rate for ALG I recipients (i.e. short-term unemployed). We accordingly set the relative contact efficiency of the long-term unemployed to 45%. Together with the targeted aggregate selection rate and the exit rates for long- and short-term unemployed this pins down all the contact, selection, and job-finding rates in the economy. Note that while we assume

[21]Kohlbrecher et al. (2016) determine the relative importance of the two effects based on microeconomic administrative residual wage data. Their exercise yields similar results in this regard.

[22]We restrict our analysis to West Germany, as we do not want our regressions to be distorted by labor market transition effects in East Germany at the beginning and middle of the 1990s. Note, however, that we obtain a similar partial Hartz IV effect when we estimate the effects for Germany as a whole (see appendix D.1).

Table 3: Parameters and targets for calibration.

Parameter/Target	Value	Source
Aggr. productivity	1	Normalization
Discount factor	$0.99^{\frac{1}{3}}$	Standard value
Short-term replacement rate	0.67	Legal replacement rate
Long-term replacement rate (pre-reform)	0.57	Legal replacement rate
Bargaining power	0.5	Standard value
Separation rate	0.016	Unemployment rate of 9%
Short-term job-finding rate	0.16	Klinger and Rothe (2012)
Long-term job-finding rate	0.07	Klinger and Rothe (2012)
Relative contact rate of long-term unemp.	0.45	PASS survey
Market tightness	0.25	IEB and Job Vacancy Survey
Selection rate	0.46	Job Vacancy Survey
$\partial \ln \eta / \partial \ln \theta$	0.15	IEB and Job Vacancy Survey
$\partial \ln jfr / \partial \ln \theta$	0.27	IEB and Job Vacancy Survey

that all short-term unemployed face the same contact, selection, and job-finding rate,[23] our calibration implies that the fixed training costs component increases every month with the duration of unemployment.[24]

We assume that idiosyncratic productivity follows a lognormal distribution. As shown by Kohlbrecher et al. (2016), in a selection model the elasticity of the selection rate with respect to market tightness is determined by the shape of the idiosyncratic productivity distribution at the cutoff point. Given the distribution, the cutoff point is in turn determined by the selection rate, which we have already targeted. We can, therefore, pin down the parameters of the distribution by targeting the elasticity of the selection rate with respect to market tightness, which is 0.15 in our data.[25] The elasticity of the contact rate with respect to market tightness (i.e. the weight on vacancies in the contact function) is finally set to target the overall elasticity of the job-finding rate with respect to market tightness, which is 0.27 in the data. The resulting weight on vacancies in the contact function is 0.11. Thus, the selection mechanism accounts for more than one half of the elasticity of the job-finding rate with respect to market tightness in our model.

[23]While we observe different job-finding rates per month of short-term unemployment duration in the data, we cannot compute the corresponding contact rates.

[24]As the reservation wage falls with the duration of unemployment, average training costs have to increase if we want to keep the steady state job-finding rates fixed.

[25]The resulting scale parameter of the distribution is 4.1. Note that we fix the location parameter of the distribution at 0 and instead allow the fixed training costs component to adjust. This allows us to vary the mean of the training costs for different groups while preserving the shape of the distribution.

6. The Effects of Hartz IV

This section proceeds in three steps. First, we show the partial effects in our model. Although we target the aggregate partial effect in our estimation, we can make statements on the response of the selection rate for each unemployment duration group. Second, we switch on equilibrium effects and analyze how aggregate unemployment and vacancies changed due to Hartz IV. Third, we put our quantitative results in perspective to other papers on the German Hartz reforms.

6.1. Partial Effects

Our empirical estimation in Section 4.3 has shown that the reform resulted in a 12% increase in the selection rate (controlling for business cycle effects). We, therefore, target the same increase of the average selection rate in the quantitative model, while keeping equilibrium effects switched off (i.e. a constant contact rate).

For this purpose, we require a decline of unemployment benefits for long-term unemployed of 11%. Under collective bargaining the required drop is 23% (see Appendix C.2). For a given drop of workers' reservation wages initiated by the fall of the replacement rate, firms are willing to extend hiring by more when wages are bargained individually. The reason is that part of the increase in training costs for the marginal worker is directly offset by her wage. Conversely, if wages are bargained collectively, the increase in training costs for the marginal worker is only indirectly reflected in her wage, i.e. through its effect on average training costs. We, therefore, require a larger fall of the replacement rate to achieve the same response of the selection rate under collective bargaining. Nonetheless, both values are within the range used by Launov and Wälde (2013), Krause and Uhlig (2012), and Krebs and Scheffel (2013). Apart from the size of the replacement rate change, our results under collective bargaining are virtually unchanged (see Appendix C.2).

These results stress another important aspect of our evaluation strategy. By directly targeting the empirical increase of the selection rate (i.e. the partial effect), our results for the effects of the Hartz IV reform on the job-finding rate are robust to a number of modelling choices. This does not only apply to the wage bargaining regime. In a recent paper, Hartung et al. (2018) argue that separation rates declined sharply as a result of the Hartz IV reform which contributed to the decline of the unemployment rate. If separation rates indeed fell due to the reform, this would further boost the response of the selection rate in our model because longer employment spells increase the present value. Given that the empirical target for the increase of the selection rate is unchanged, we would require a lower drop of the replacement rate in this scenario. Overall, adding a separation channel to our model would, of course, further increase the effects on the unemployment rate.

Figure 7 shows the impulse responses of the selection rate in reaction to this permanent decline of the replacement rate for long-term unemployed. The selection rate immediately increases on impact for all groups of searching workers due to a lower outside option. However, the effect is larger, the closer the unemployed get to the expiration of the

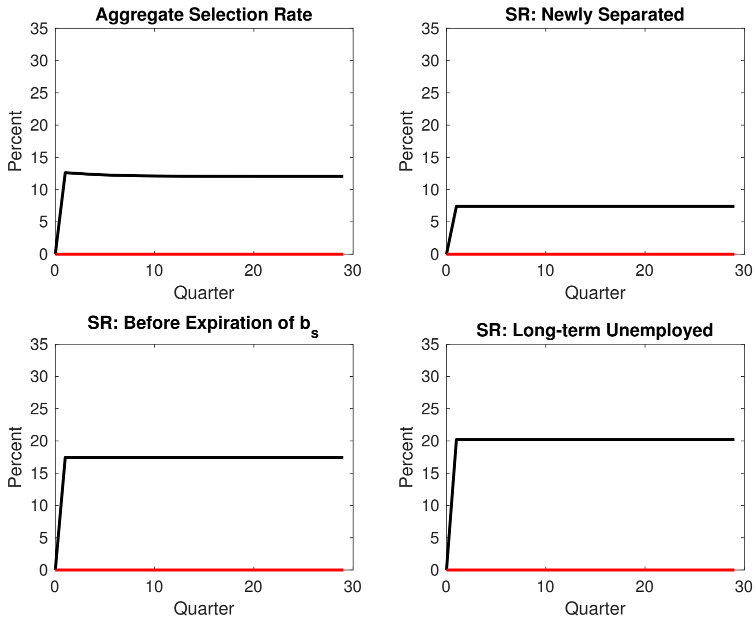


Figure 7: Selection rate (SR): impulse responses to a 11% decline in long-term unemployment benefits.

more generous short-term benefits. For workers who have just been separated from a job (upper right panel in Figure 7), the reduction of long-term unemployment benefits affects their present value of unemployment by the least because they will only feel the reduction if they are not matched within the next twelve months. Still, their outside option falls, which increases the joint surplus of a match. The selection rate for workers who still have a full year of short-term benefits increases by around 7%. For workers who switch to the long-term benefit scheme in the next period, the reduction in long-term benefits has a larger effect on their outside option. Their selection rate increases by 17%. This is in line with Price (2018) who finds that unemployed workers' job-finding rates increase sharply before the expiration of benefits. Finally, the impact is largest for the long-term unemployed who are immediately affected by the reduction of long-term benefits. Their selection rate increases by 20%. While the individual selection rates all adjust on impact, the aggregate rate, which is a weighted average, slightly overshoots at the beginning. The reason is a composition effect. Initially, there are more long-term unemployed for whom the effect is largest. However, the difference between the initial response and the steady state response is small.

Figure 8 shows the impact responses of the selection rate in response to a decline in long-term unemployment benefits for all duration groups of our model. The x-axis indicates the time remaining until short-term benefits expire. We see that the response increases gradually with the expiration of short-term benefits coming nearer and kinks at the expiration threshold.

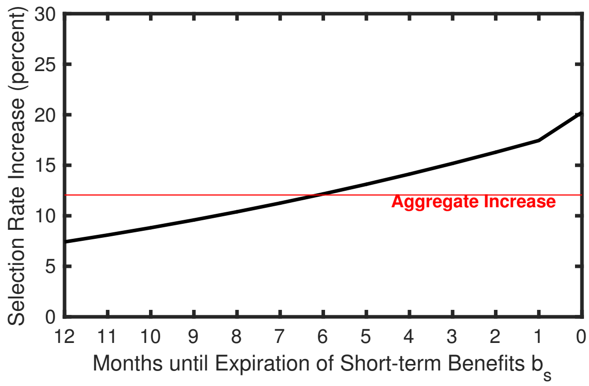


Figure 8: Impact responses of selection rate to a decline of long-term unemployment benefits by remaining months of short-term benefit entitlement.

How do our results compare to other recent microeconometric studies of the Hartz IV reform? Price (2018) uses the German administrative data to estimate the causal effects of Hartz IV from the worker side. He finds that the probability of being reemployed within 12 months of beginning a claim increases by 4.7 percentage points. We find an increase of the reemployment hazard of 3 percentage points, which is smaller but close to Price's (2018) results. Furthermore, the magnitudes of the wage effects in our model are quite comparable. In our model, the average wage over the employment spell for a reemployed worker who exhausted short-term benefits falls by 2.5% due to Hartz IV. Price (2018) finds that those workers accept 4% - 8% lower wages on reemployment after the reform and conditional on jobless duration.[26] The wage effect is much smaller if we average over all unemployment durations. In this case, the average reemployment wage drops by only 1.1%. Note that the results in this subsection all rule out equilibrium effects, as we fixed the contact rate at its pre-reform level. In the full equilibrium model, average reemployment wages (again measured as average wages over the employment spell) drop by only 0.7%. As wage effects are crucial for the transmission of the benefit shock in our model, it is reassuring that these are well in line with empirical estimates. This is particularly important in light of the debate in the empirical literature as to whether benefits actually influence reemployment wages once controlling for unemployment duration. Schmieder et al. (2016), for example, find for the pre-Hartz period in Germany that the effect of benefit duration on wages is at best very small. However, they study a different time period and identify their effects based on age-related differences in the maximum duration of short-term benefits. In the pre-Hartz period, however, upon exhaustion of short-term benefits, workers still received relatively generous long-term benefits. The Hartz IV reform, however, meant that entering long-term unemployment became a lot

[26] We cannot make this distinction in our model as there is a one to one relationship between duration and benefit eligibility.

more painful which might explain why Price (2018) finds larger effects on wages for those workers close to the expiration of short-term benefits. Finally, it is important to stress that the similarity in results between our study and Price (2018) is quite remarkable, given that we derive our partial effects based on completely different methodologies and data sources: the administrative worker data (in the case of Price (2018)) and firm survey data (this study).

6.2. Equilibrium Effects

One of the key advantages of our approach relative to pure microeconometric estimations is that we can quantify the equilibrium effect. We now present results for the full model, i.e. we allow contact rates to adjust. Keep in mind that we have disciplined the relative magnitude of the equilibrium effect by our estimations in Section 4.4.

As firms' expected surplus rises, they post more vacancies. More vacancies increase the market tightness and thereby increase the probability of workers to get in contact with a firm (through the contact function). This is illustrated in the lower left panel of Figure 9. The contact rate for unemployed workers rises by 8% on impact.[27] The overall aggregate job-finding rate, which is the product of both the contact and the selection rates, increases by 19% on impact (lower right panel of Figure 9). Therefore, somewhat less than one half of the initial response of the job-finding rate is due to the equilibrium effect (around 41%). Note that the response of the selection rate (11% increase on impact, 10.5% higher in the new steady state) is a bit smaller in the full model compared to the model with the equilibrium effect switched off. The reason is a small negative feedback effect from increased contact rates on the wage level and hence the selection rate. Overall, the unemployment rate falls by 22%. This corresponds to a decrease of the steady state unemployment rate by 2 percentage points in our calibration.

When the economy adjusts to a new steady state, the composition of the pool of unemployed changes. This can be seen in the adjustment dynamics of the contact and job-finding rate, which increase quite sluggishly. The aggregate contact and job-finding rates are a weighted average for all duration groups. Due to the reform, the duration of unemployment is shortened. The share of the searching workers with long unemployment duration declines over time.[28] When we control for the composition effect,[29] the unemployment rate falls by 17% or 1.6 percentage points (instead of 2 percentage points). The increase of the selection rate (partial effect) and the contact rate (equilibrium effect) are similarly important.

In the long run, the job-finding rate in our simulated model increases by almost 26 percent. On average, the (corrected) job-finding rate in our data is 15% higher in the post-Hartz compared to pre-Hartz period. However, the comparison is quite sensitive

[27] As all workers search on the same labor market, the relative response of the contact rate to the reform is the same for short and long-term unemployed.

[28] In principle, the composition effect could also be driven by selection. However, in our calibration, most of the differences in job-finding rates between long- and short-term unemployed are accounted for by lower contact efficiencies, which was guided by the PASS survey.

[29] We assume counterfactually that the shares of each unemployment duration group stay constant.

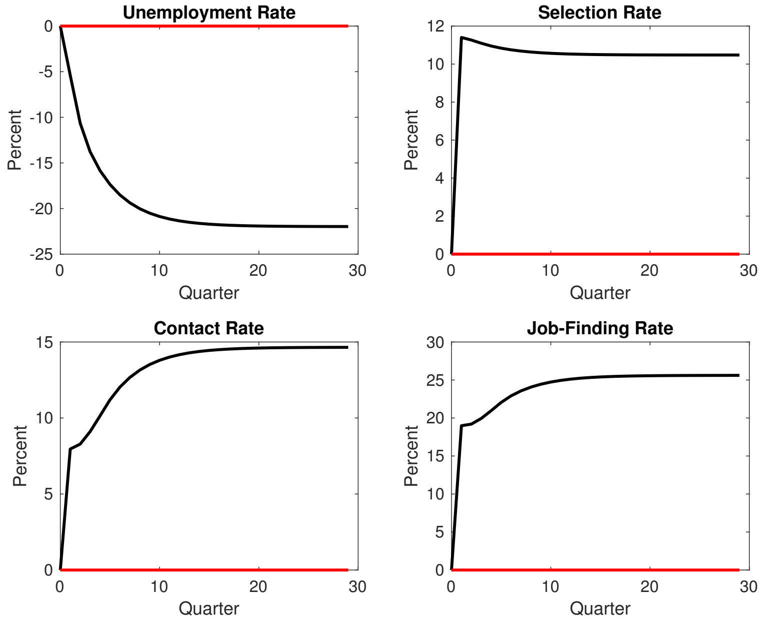


Figure 9: Impulse responses to a decline in long-term unemployment benefits.

to the chosen time period. If we choose the same time period as Hartung et al. (2018) (i.e. 1993-2002 and 2008-2014), we obtain an increase of around 22%. If we compare the two recessions in 2005 and 2009, we obtain an increase of around 30%. Second, it has to be emphasized that many workers may have left registered unemployment after 2005 (as they were not eligible for long-term benefits due to Hartz IV any more) before reentering employment. This is a caveat of the German unemployment definition, which is based on registration instead of active search. These transitions out of non-registration are not captured by the empirical job-finding rate but are included in our model response. Recent evidence on labor market flows between employment, non-registration and registered unemployment by Carrillo-Tudela et al. (2018) suggest that this was indeed an important adjustment channel.

Our results show that there are quantitative important equilibrium effects at work. This is consistent with findings of Karahan et al. (2019) and Hagedorn et al. (2019) who also find sizeable effects operating via aggregate labor market tightness. Nonetheless, it appears to stand in contradiction to a literature that finds small macro effects for the extension of unemployment benefits in the aftermath of the Great Recession in the United States (e.g. Chodorow-Reich et al., 2019). From a theoretical perspective, the overall aggregate elasticity may even be smaller than the micro elasticity. This is typically the result of search and matching models with decreasing marginal returns to labor (e.g. Landais et al., 2018). In the short run, where capital is sluggish to adjust, these modeling choices may be adequate. However, in a setting, where we aim at analyzing the long-run effects of a permanent change in the generosity of unemployment benefits, constant returns appear to be more realistic. Hence, overall aggregate effects can be expected to be larger than micro effects.

How would our results change if our model contained an additional endogenous search effort margin in the spirit of Costain and Reiter (2008)? A reduction of long-term unemployment benefits increases the difference between the value of employment and unemployment. This raises workers' incentives to increase their search effort. For a given parametrization, this would lead to a larger increase of the job-finding rate compared to a scenario with exogenous search effort. As a consequence, the unemployment rate would decline more than in our baseline scenario. However, the quantitative effects would strongly depend on the parametrization of the search effort function. Due to the lack of empirical evidence that allows us to pin down the curvature of individual search effort,[30] we abstain from extending our framework in this respect. Hence, we regard the effects of our baseline model as a lower bound.

Finally, it is interesting to study the trajectory of the Beveridge curve in the data and in the model. Figure 10 shows the simulated Beveridge Curve in response to the decline of the replacement rate for long-term unemployed workers in our model. Vacancies increase, overshoot and end up at a level above the initial steady state. Unemployment sequentially declines to a lower long-run level. We contrast our simulation results with the actual movement of the Beveridge Curve from the first quarter of 2005 to the fourth quarter of 2007 (Figure 11). Similar to the simulation, vacancies increase, overshoot somewhat and end up at a higher level.[31] Unemployment sequentially declines to a permanently lower level in the data. The movements are not only qualitatively comparable, but the quantitative reactions (as percent deviations) are also similar.

While the comparison of our simulation and the data is purely descriptive, given the similarities between the two, the exercise provides suggestive evidence for the importance of the Hartz IV reform for German labor market dynamics in the years after the reform. Overall, our work points to an important role of the reform of the benefit system for the decline of German unemployment. Other reforms (such as Hartz III) may also have contributed (e.g. Launov and Wälde, 2016). However, our methodology does not allow us to quantify these contributions.

6.3. Broader Perspective

How do our results compare to recent empirical studies on labor market stocks and flows around the time of the Hartz reforms? In recent papers, Carrillo-Tudela et al. (2018) and Rothe and Wälde (2017) document that relatively few unemployed workers

[30]There are several microeconometric studies that quantify the partial effects of unemployment benefit changes on individual labor market transitions. However, these studies would both capture search effort effects and selection effects. To our knowledge, there is no established method to isolate the pure search effort effects.

[31]The overshooting behavior takes place later in the data and is somewhat less pronounced. Vacancies are a purely forward-looking variable in our model, while there may be reasons why they are more persistent in the data.

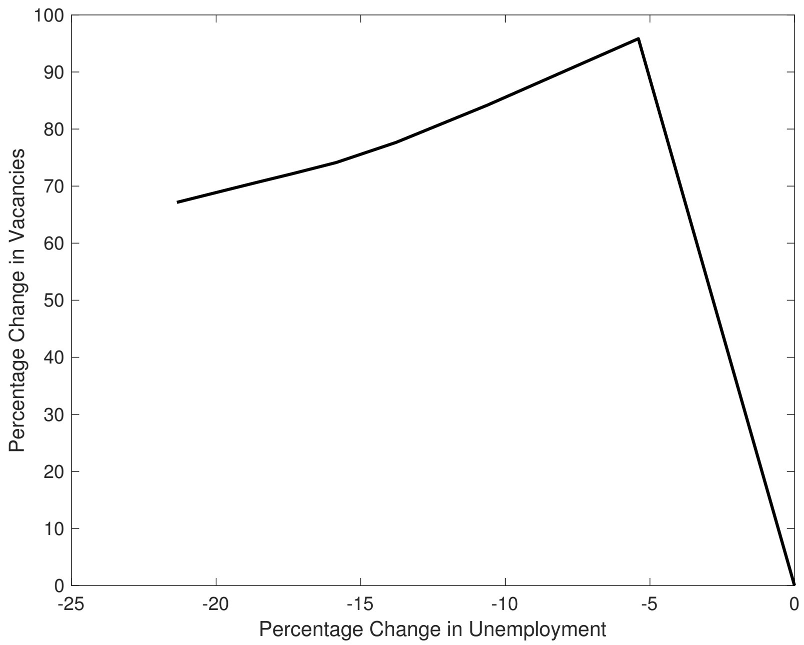
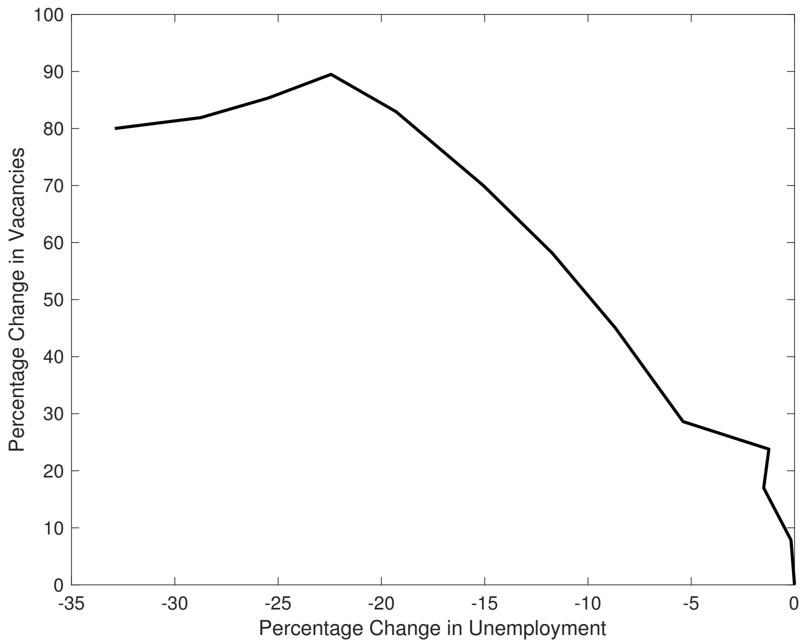


Figure 10: Beveridge curve generated by the model during first three years after the shock.



Note: We use the corrected unemployment series (as described in appendix D.2).

Figure 11: West German Beveridge curve from 2005-2007.

directly transitioned to regular (full-time) employment after Hartz IV. Indeed, Carrillo-Tudela et al. (2018) show that flows into non-participation account for a large part of the outflows from unemployment in the aftermath of the Hartz IV reform but that labor force participation actually increased. Apparently, the reform induced many unemployed to deregister with the Federal Employment Office, but then - out of non-participation - to take up jobs often in the form of part-time employment[32] or mini-jobs. The latter then often served as stepping stones into contributing employment. Besides the known measurement issues with the unemployment rate in 2005, these dynamics provide an additional argument why focusing on direct unemployment to employment transitions when assessing the Hartz IV reform would only show part of the picture.

Interestingly, these results contrast with the findings by Price (2018). He documents that the net-employment effects from his causal identification - which are comparable in magnitude to our partial effects - are driven by full-time employment. Still, against the background Carrillo-Tudela et al.'s (2018) and Rothe and Wälde's (2017) work, it may be the case that labor market transitions took place in a more complex and sluggish way than in our simulated model (e.g. via non-participation and irregular types of unemployment, which served as stepping stones). To the extent that stepping stones played an important role, our model overestimates the speed at which the equilibrium effect generates full-time jobs. Finally, understanding better the dynamics of participation decisions and their interaction with the benefit reform would certainly be desirable. However, besides the usual challenges of modelling participation decisions in quantitative models, the work by Carrillo-Tudela et al. (2018) shows that the German case is even more challenging as incentives to be registered as unemployed might be completely unrelated to search behavior. Against this background, we have proposed a novel approach how to measure the partial effect of Hartz IV with a data source (IAB Job Vacancy Survey) that is completely unrelated to the definition of registered unemployment. Our new selection measure is robust to changed definitions of labor market states (related to unemployment registration) and resulting spurious labor market flows.

7. Conclusion

This paper proposes a novel approach how to evaluate the reform of the German unemployment benefits system in 2005. For this purpose, we construct a measure of labor selection over the business cycle. In contrast to existing literature, our strategy does not hinge on an external source for the quantitative decline of the replacement rate for long-term unemployed, for which the literature provides a wide range of estimates. Instead, we provide direct empirical evidence on firms' hiring behavior from the IAB Job Vacancy Survey and show that their selection rates increased following the Hartz IV reform. In addition, we estimate the relative importance of partial and equilibrium effects over the business cycle and impose it on our model. Our simulation shows that the reform had important equilibrium effects. Our simulated model can match important facts, such as

[32]The rise in part-time employment around the time of the Hartz reforms is also documented in a recent paper by Burda and Seele (forthcoming).

the trajectory of the Beveridge Curve after the reform and the larger increase of the job-finding rate for unemployed with longer unemployment durations. Overall, our results show that two percentage points of the decline in steady state unemployment since 2005 can be attributed to the Hartz IV reforms. It was thus a major driver of the decline of unemployment in Germany.

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A. Details on the Hartz Reforms

In response to rising unemployment in the early 2000s, the Hartz commission developed recommendations for the German labor market. These proposals were implemented gradually between 2003 (Hartz I and Hartz II) and 2005 (Hartz IV). According to Jacobi and Kluve (2006), the Hartz reforms had three main goals: (1) increasing the effectiveness and efficiency of labor market services, (2) activating the unemployed and (3) boosting labor demand by deregulating labor markets. Under the concept of *"demanding and supporting" (Fordern und Fördern)*, these four reforms radically restructured the German labor market:

Hartz I (in action since 01/01/2003): This reform facilitated the employment of temporary workers. Additionally, vouchers for on-the-job training were introduced.

Hartz II (in action since 01/01/2003): Introduction of new types of marginal employment with low income such as *Minijobs* (up to 450 euros per month, exempted from the income tax) and *Midijobs* (income up to 850 euros per month, reduced social security contributions). Furthermore subsidies for business start ups of unemployed were introduced.

Hartz III (in action since 01/01/2004): The core element of Hartz III was the restructuring of the Federal Employment Agency. The Federal Employment Agency was divided into a headquarter, regional directorates and local job center. Those local job center are now managed via a target agreement. Since Hartz III, all claims of an unemployed person are processed by the same case worker (support from a single source) and an upper limit on the number of cases handled was introduced. Furthermore, a special focus was put on long-term unemployed and unemployed who are older than fifty years. In addition, market elements for private placement services and providers of training measures were introduced.

Hartz IV (in action since 01/01/2005): The last step was the most widely discussed reform since it caused a substantial cut in long-term unemployment benefits for several groups. Prior to the reform, unemployed workers who had exhausted their short-term unemployment benefits received unemployment assistance (Arbeitslosenhilfe) which amounted to 53% of previous net earnings (57% with children). In addition, unemployed workers not eligible for unemployment assistance (Arbeitslosenhilfe) received means-tested social assistance. Both forms of long-term unemployment benefits were abolished in 2005 and replaced by the purely means-tested Arbeitslosengeld (ALG) II (commonly called "Hartz IV"). This constituted a severe cut in long-term unemployment benefits for most former recipients of Arbeitslosenhilfe. Eligibility for ALG II depends on savings and the partner's income. In addition, a sanctioning system was introduced which allowed cuts in the fixed unemployment benefits if the unemployed person breaks an agreement with the Public Employment Agency (e.g. in terms of writing applications, reachability, responsible economic behavior). In addition, the Hartz IV law also includes a reduction of the maximum entitlement duration of short-term unemployment benefits for workers older than 45 years by 6 to 14 months. This reform step became effective on February 1, 2006. For a more detailed description of the Hartz reforms, see Jacobi and Kluve (2006) or Launov and Wälde (2016).

B. Data

We use West German annual data on the number of suitable applicants for the most recent hire in the last 12 months and the number of total vacancies of the IAB Job Vacancy Survey. Information on the IAB Job Vacancy Survey can be found in Moczall et al. (2015). Note that since the IAB Job Vacancy survey corresponds to the third quarter of a year, we use third quarter data in our estimations except for the value added measure. Regarding value added growth, we are restricted to use annual data as disaggregated national accounts data is only available at an annual frequency. The value added measure for West Germany is constructed by aggregating value added at state level and taking growth rates. In addition, data on unemployment and transitions from unemployment into employment (matches) were taken from register data of the federal labor office, the "Integrated Labour Market Biographies (IEB)" (vom Berge et al., 2013).[33] We define the job-finding rate as matches over unemployment, where matches are transitions from unemployment into employment. Furthermore, a person is counted as unemployed if he or she does not have a job which is subject to the payment of social security contributions, is registered to be actively looking for a job or receives unemployment benefits.

Data for calculating the contact rate for short-term and long-term unemployed stems from the IAB PASS Survey. Furthermore, we take values on the job-finding rates for ALG I (short-term unemployed) and ALG II recipients (long-term unemployed) from (Klinger and Rothe, 2012). They calculated these job-finding rates based on German administrative data. We use the average job-finding rate by duration of unemployment for the time span 1998-2004.[34]

B.1. Details on the IAB Job Vacancy Survey

The Job Vacancy Survey was first carried out in 1989 in West Germany and was extended to East Germany in 1992. Note that due to the small number of East German establishments in the early 1990s in the sample and due to the different behavior of labor market turnover rates (see Fuchs et al., 2018), we restrict our sample to West Germany. The survey is conducted via a written questionnaire every fourth quarter of the year. Yearly, a stratified random sample of establishments is drawn according to industries, regions as well as size classes. The number of establishments participating ranges from 4,000 in the first years to about 14,000 in the recent years. The data set includes weights to extrapolate the data for the whole economy. Weights for the most recent case of hiring ensure representativeness for all hires.

As the number of suitable applicants for Germany is available from 1992 onwards, we restrict our sample range from 1992 to 2015. Since the aggregate sample range is quite short to conduct time series analysis, we additionally calculate the time series at the federal state and industry level. We aggregate the inverse of the number of suitable applicants by taking mean values. Following Klinger and Rothe (2012, p.17), we add the city state Bremen to the neighboring state Lower Saxony to avoid spatial correlation.

[33]Status quo of the data as of January 2016.
[34]This corresponds to the available pre-Hartz period.

The Job Vacancy Survey contains too few observations for small federal states in order to be representative. Therefore, we restrict our sample to federal states with at least 6 million inhabitants.[35]

B.2. Details on the IAB PASS Survey

Furthermore, we use data of the IAB Panel Study Labor Market and Social Security (PASS)[36] to calculate the relative contact rates of long- and short-term unemployed workers. This annual Panel Survey was first carried out in 2007 and consists currently of ten waves. Each wave consists of approximately 10,000 households. Its focus lies on the circumstances and characteristics of recipients of Unemployment Benefit II (ALG II). Interview units are both households as well as individuals (15,000 each year). The Panel consists of two equally large subsamples, (a) recipients of unemployment benefits II (ALG II) and (b) a sample of the German population in which low-income households are overrepresented.[37] In addition, the PASS survey includes several questions on the job search behavior of unemployed workers. These questions regard job search channels, the number of applications as well as the number of job search interviews attended. We measure the contact rate in our model by calculating the share of unemployed workers who attended at least one job interview in the past four weeks. Furthermore, we split unemployed workers by short-term unemployed (ALG I recipients) and long-term unemployed (ALG II recipients). The number of unemployed workers in our sample is 1,806 for ALG I recipients and 23,103 for ALG II recipients. For a detailed description of the IAB PASS survey, see Trappmann et al. (2013).

C. Model Robustness

C.1. Equivalence of Productivity and Business Cycle Shocks

For quantifying the relative importance of the contact margin versus the selection margin to the reform, we exploit the relative importance of the two margins over the business cycle. We now show in a simplified version of our model that productivity shocks and benefit shocks indeed affect the selection and contact rate in a symmetric way.

We assume that the unemployed are a homogenous group, i.e. we drop the distinction between long- and short-term unemployed. All unemployed receive unemployment compensation b. In steady state and under Nash bargaining[38] the model can be described by the following four equations:

[35]As of December 2014. Hence, we include Baden-Wuerttemberg, Bavaria, North-Rhine Westphalia, Lower Saxony plus Bremen and Hessen.

[36]Data access was provided via a Scientific Use File supplied by the Research Data Centre (FDZ) of the German Federal Employment Agency (BA) at the Institute for Employment Research (IAB), project no. 101752.

[37]For details, see `http://www.iab.de/en/befragungen/iab-haushaltspanel-pass.aspx`.

[38]In steady state, the wage for incumbents is given by $w^I = \alpha(a + \delta\kappa\theta) + (1-\alpha)b$ and the wage for newly hired at the hiring cutoff is $w^I - \alpha\tilde{\varepsilon}$.

$$\tilde{\varepsilon} = \frac{a - b - \frac{\alpha}{1-\alpha}\delta\kappa\theta}{1 - \delta(1 - \phi)} \tag{43}$$

$$\theta = (1 - \alpha)\frac{p\eta}{\kappa}\left(\tilde{\varepsilon} - \frac{\int_{-\infty}^{\tilde{\varepsilon}} \varepsilon f(\varepsilon) d\varepsilon}{\eta}\right) \tag{44}$$

$$\eta = \int_{-\infty}^{\tilde{\varepsilon}} f(\varepsilon) d\varepsilon \tag{45}$$

$$p = \mu\theta^{\gamma} \tag{46}$$

Using the implicit function theorem, it can be shown that

$$\frac{\partial \eta}{\partial b} = -\frac{\partial \eta}{\partial a} \tag{47}$$

and

$$\frac{\partial p}{\partial b} = -\frac{\partial p}{\partial a}. \tag{48}$$

It follows that:

$$\frac{\partial \hat{p}/\partial \hat{b}}{\partial \hat{\eta}/\partial \hat{b}} = \frac{\partial \hat{p}/\partial \hat{a}}{\partial \hat{\eta}/\partial \hat{a}} \tag{49}$$

C.2. Collective Bargaining

In the main part of the paper we assume that all wages are determined by individual Nash bargaining. However, a significant share of German wages are still set under collective bargaining arrangements. In 2010, 53% of all West German employees in the private sector were covered by collective bargaining (see e.g. Hirsch et al., 2014). We therefore present results for the polar opposite case, i.e. wages are bargained collectively. We assume that all workers within a duration group earn the same wage but still allow for differences between groups. We assume that the union represents the average worker that is hired in every group. The union wage is then again determined by Nash bargaining. We apply the exact same calibration strategy as in the main part of the paper (i.e. we keep the same targets). The only major difference between the collective and the individual Nash bargaining case is that the reduction of the replacement rate that is required for the targeted increase of the selection rate is higher under collective bargaining. More precisely, we require a 23% drop of long-term unemployment benefits to achieve a 13% increase of the selection rate in partial equilibrium (i.e. keeping the contact rate fixed). While this is more than double the amount required under individual Nash bargaining, it is similar to the reduction used by Krebs and Scheffel (2013) and Krause and Uhlig (2012) (for low-skilled workers). Figure C.1 shows that otherwise the model reaction is virtually unchanged.

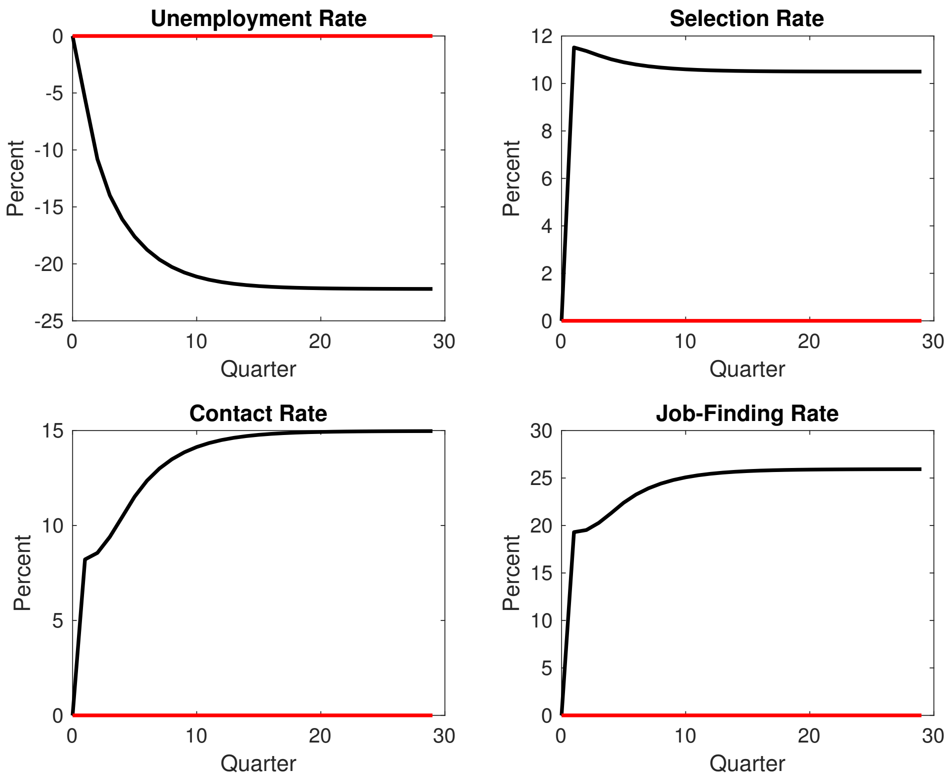


Figure C.1: Impulse responses to a 23% drop of long-term unemployment benefits under collective bargaining.

C.3. Contact Efficiency Shock

Figure C.2 shows the response of the selection rate to a positive shock to the contact efficiency. A one percent increase of contact efficiency leads to a drop in the selection rate of around 0.1%. Thus, the effect is extremely small and - if any - would bias our results downward.

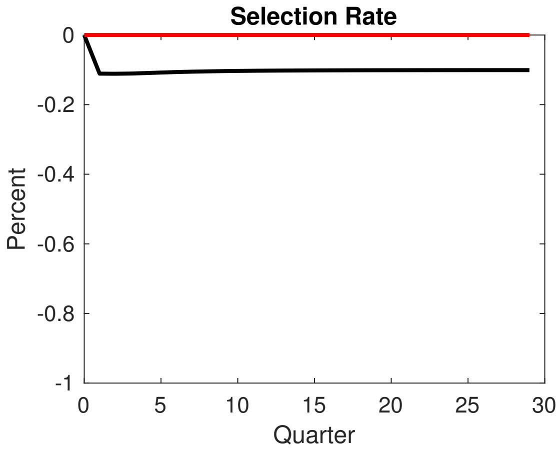


Figure C.2: Response of the selection rate to a 1% positive shock to the contact efficiency.

D. Robustness of Empirical Strategy

D.1. Including East Germany

In this robustness check, we extend our sample to entire Germany (including East Germany). Table D.1 shows the estimation results. The coefficient on the Hartz IV dummy in the specification with GDP growth and market tightness (column 2) is unaffected when including East Germany (0.12). However, the elasticity of the selection rate and the job-finding rate with respect to market tightness is smaller compared to the specification with West Germany. This can be explained by the different labor market dynamics in East Germany in the years after the German reunification. Moreover, East German firms were underrepresented in the first years of the IAB Job Vacancy Survey.

D.2. Correction of Unemployment Series

As part of the transition from the old system of unemployment and social assistance to the new system of "Arbeitslosengeld II" ("Hartz IV"), the number of registered unemployed rose significantly in the first quarter of 2005. This increase has two reasons: First, former social assistance ("Sozialhilfe") recipients able to work were counted as unemployed (which was not the case before the reform). Second, family members of unemployment benefit recipients also had to register as unemployed under certain conditions (see Statistik der

Table D.1: Results for entire Germany (including East Germany)

	Dependent variable:			
	log(selection rate)	log(selection rate)	log(selection rate)	log(job-finding rate)
	(1)	(2)	(3)	(4)
Hartz IV dummy	0.15***	0.12***	0.12***	0.10***
	(0.03)	(0.03)	(0.03)	(0.03)
log(market tightness)		0.09*	0.10**	0.24***
		(0.05)	(0.05)	(0.04)
GDP growth	0.84	0.55		
	(0.52)	(0.59)		
Constant	−0.77***	−0.60***	−0.58***	−2.62***
	(0.03)	(0.09)	(0.08)	(0.06)
Observations	24	24	24	24
R^2	0.50	0.62	0.61	0.77
Adjusted R^2	0.46	0.56	0.57	0.75
Residual Std. Error	0.08 (df = 21)	0.07 (df = 20)	0.07 (df = 21)	0.07 (df = 21)
F Statistic	10.61*** (df = 2; 21)	10.91*** (df = 3; 20)	16.38*** (df = 2; 21)	35.87*** (df = 2; 21)

Note: Estimation by OLS with robust standard errors. *p<0.1; **p<0.05; ***p<0.01

Bundesagentur für Arbeit, 2005). Low-skilled workers and women were overrepresented among the newly registered workers. Given that unemployment increased for purely statistical reasons in 2005, this reduces the job-finding rate (which is defined as matches divided by unemployment) ands it reduces the market tightness (which is defined as vacancies divided by unemployment).

In order to clear the break in the unemployment series, we estimate the growth rate of (seasonally) adjusted unemployment from the fourth quarter of 2004 to the first quarter 2005. We correct the aggregate unemployment time series by the corresponding level difference. We compare this correcting method with a second approach that uses the number of additional unemployed published by the Federal Employment Agency.[39] Figure D.3 illustrates the original unemployment series (solid line, data based on the IAB Integrated Employment Biographies), the corrected series using the dummy approach (dashed line) and the corrected series using the number of the Federal Employment agency (dotted line). The two correction methodologies deliver very similar series.[40]

[39] According to the Federal Employment Agency, the number of unemployed rose by 380.000 recipients (entire Germany) due to the new requirements to register as unemployed until March 2005 (Statistik der Bundesagentur für Arbeit, 2005, p. 10). We weight the number of the additional unemployed for Germany by the share of unemployed in West Germany in the year 2005 (66.8%). This results in an overall number of approximately 254.000 unemployed which we deduct from 2005 onward. Given that we have the number of additional unemployed for entire Germany only (and not on a disaggregated level), we rely on the purely statistical correction approach for our baseline estimation.

[40] The estimation results with the second correction method are also very similar and are available upon request.

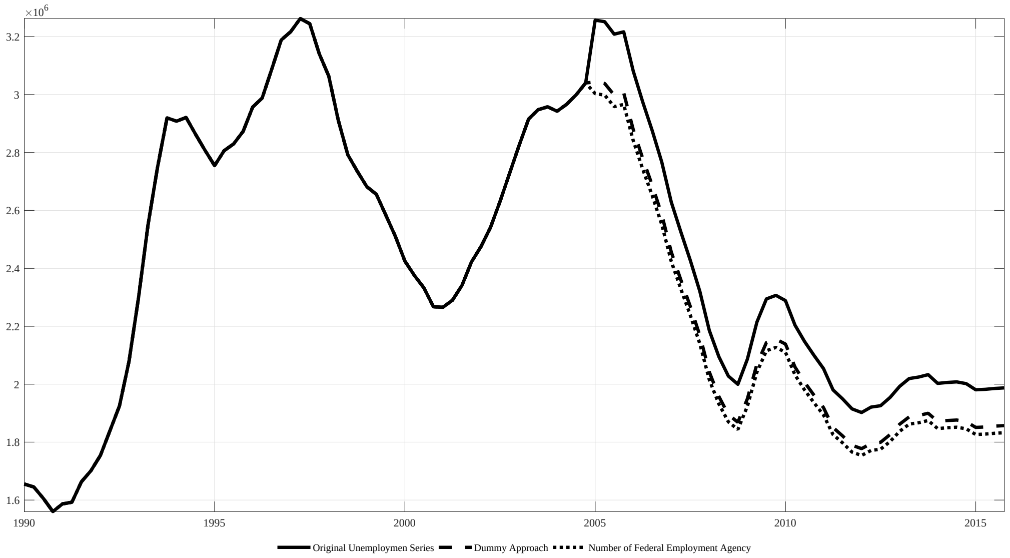


Figure D.3: Clearing for the break in the unemployment series, 1990Q1-2015Q4.

It is also noteworthy that the estimated elasticities of the selection rate with respect to market tightness in Table D.2 are identical to our baseline estimation (see Table 2). The estimated elasticity of the job-finding rate with respect to market tightness is slightly higher (0.27 vs. 0.31).

Table D.2: Estimation results without unemployment correction, 92-15

	Dependent variable:		
	log(selection rate)		log(job-finding rate)
	(1)	(2)	(3)
Hartz IV dummy	0.13***	0.13***	0.03
	(0.04)	(0.04)	(0.04)
log(market tightness)	0.14***	0.15***	0.31***
	(0.05)	(0.05)	(0.05)
value added growth	0.51		
	(0.89)		
Constant	−0.58***	−0.56***	−2.54***
	(0.08)	(0.07)	(0.08)
Observations	24	24	24
R^2	0.55	0.54	0.68
Adjusted R^2	0.48	0.50	0.65
Residual Std. Error	0.09 (df = 20)	0.09 (df = 21)	0.10 (df = 21)
F Statistic	8.18*** (df = 3; 20)	12.50*** (df = 2; 21)	22.52*** (df = 2; 21)

*Note: Estimation by OLS with robust standard errors; *p<0.1; **p<0.05; ***p<0.01*

E. Further Evidence

In addition, a closer look at the change of the selection rate by skill group from 2004 to 2005[41] reveals that the increase in the selection rate was highest for workers with a vocational degree. This is exactly what we would expect: While medium-skilled workers suffered on average a larger drop in the replacement rate due to higher wages compared to low-skilled workers, they also faced a higher risk of unemployment compared to high-skill workers.[42]

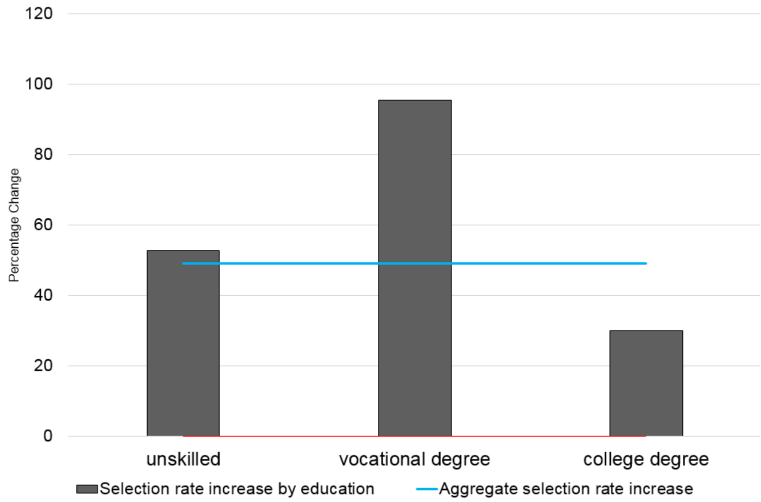
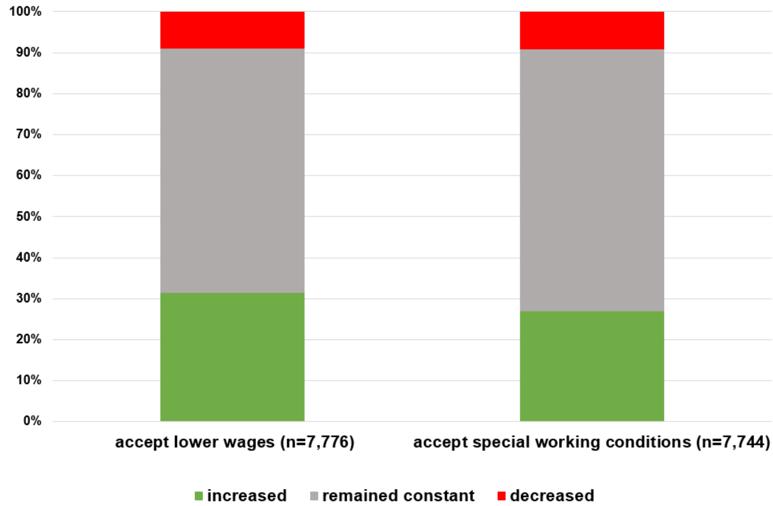


Figure E.4: Increase of selection rate by skill group, 2004-2005

Figure E.5 shows evidence from a special survey on Hartz IV of the IAB Job Vacancy survey in which establishments were asked about their perception of changes in applicants' reservation wages and their willingness to accept special working conditions due to the Hartz IV reform. The results indicate that on average, establishments perceived that reservation wages of unemployed applicants had dropped and that their willingness to accept special working conditions had increased.

[41]Unfortunately, the IAB Job Vacancy Survey provides information on the last realized hire by skill group only from 2004 onward.

[42]Unskilled workers may even have benefited from the Hartz IV reform because the standard rate for social assistance ("Sozialhilfe") was even lower than the standard rate for "Hartz IV". On the other hand, high-skilled workers with a college degree face only a very small probability to fall into the pool of long-term unemployed in the first place.



Source: IAB Job Vacancy Survey (Special survey on Hartz IV 2005/2006).

Figure E.5: The willingness of unemployed applicants to ... (% of establishments who gave the respective answer.)