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Hartz IV and the Decline of German Unemployment: A Macroeconomic Evaluation[†]

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Abstract

This paper proposes a new approach to evaluate the macroeconomic effects of the "Hartz IV" reform, which reduced the generosity of long-term unemployment benefits. We propose a model with different unemployment durations, where the reform initiates both a partial effect and an equilibrium effect. We estimate the relative importance of these two effects and the size of the partial effect based on the IAB Job Vacancy Survey. Our approach does not hinge on an external source for the decline in the replacement rate for long-term unemployed. 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 provide direct empirical evidence on labor selection, one potential dimension of recruiting intensity.

1 Introduction

Since the implementation of an unemployment benefits reform in the year 2005 (dubbed as "Hartz IV"), which reduced the average benefit replacement rate for long-term unemployed, the German unemployment rate has declined from 12 percent in 2005 to 5 percent in 2019 (see Figure 1). The existing literature has not yet reached a consensus on how much the Hartz IV reform has contributed to this decline. On the one hand, there are microeconometric studies (in particular, Price, 2019) that show a significant increase of the unemployment-to-employment transition rate for those close to the expiration of short-term unemployment benefits. However, these studies remain silent about the potential equilibrium effects of the reform. On the other hand, quantitative results in macroeconomic models of the labor market depend strongly on the assumed reduction of the replacement rate for long-term unemployed. However, because of the complex structure of the unemployment benefit system, there is no consensus on the overall reduction of the replacement rate (see Section 2). Due to different assigned values for the reduction of the replacement rate, the quantitative unemployment effects in existing macroeconomic studies (Krause and Uhlig, 2012, Krebs and Scheffel, 2013, Launov and Wälde, 2013) range from close to zero to a 2.8 percentage point reduction of steady state unemployment.

Figure 1: Registered unemployment rate in Germany, 1970-2019.
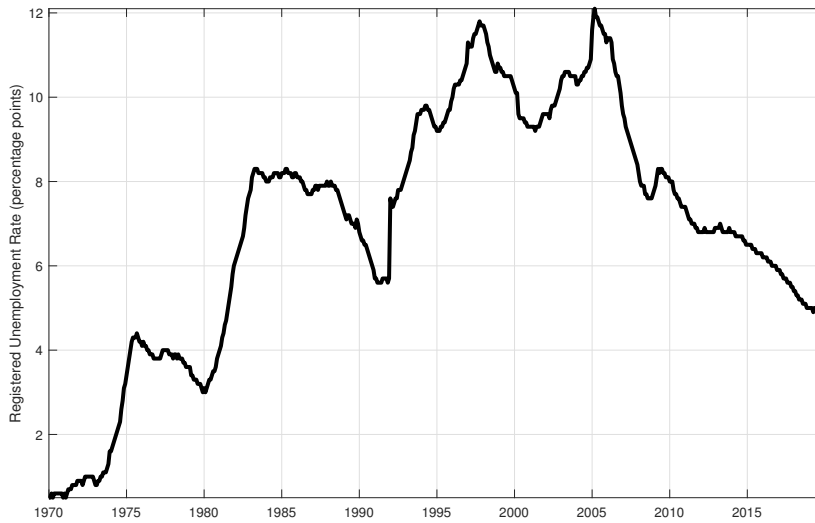
Our paper offers a solution to this disagreement. We propose a labor market model with a rich unemployment duration structure. In our model, hiring is a two-stage process where firms have to establish a contact and select a certain fraction of workers. A benefit reform triggers two effects. First, it stimulates vacancy posting, thereby increases market tightness and aggregate contacts between workers and firms. We call this the equilibrium effect. Second, firms select a larger fraction of workers (i.e. they are less selective), which we call the partial effect. We can measure this worker-firm-level effect directly in the data. For this purpose, we propose a new proxy for the selectivity of firms based on the IAB Job Vacancy Survey and estimate the increase of the selection rate (share of selected suitable applicants) due to Hartz IV.[1] In addition, we use the IAB

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[1] Descriptively, the selection rate increased from 46 percent (1992-2004) before the Hartz IV reform to 53 percent

Job Vacancy Survey to estimate the relative importance of partial and equilibrium effects over the business cycle, which we find to be equally important. The estimation results discipline the partial and the equilibrium effect in our macroeconomic model of the labor market. Our quantitative exercise yields a 2.1 percentage point decline of unemployment. This strategy replaces the conventional approach to assume a certain decline of the replacement rate, which is based on an exogenous source.

We validate our model along three dimensions (see Appendix C for details). First, the time series properties of our model are in line with the data in various dimensions. Second, our partial effects are similar to the causal microeconometric results from Price (2019). Third, there is supportive evidence for our simulated aggregate outcomes. The aggregate unemployment effects are in line with time series evidence by Klein and Schiman (2021). In addition, consonant with descriptive evidence, the job-finding rate for short-term unemployed reacts more strongly than for long-term unemployed. Furthermore, the model-based Beveridge Curve movement in response to the Hartz IV reform is similar to the actual movement of the Beveridge Curve in the years after the reform.[2]

Our model combines three ingredients: First, workers and firms meet randomly. Meetings are driven by a Cobb-Douglas constant returns contact function (Mortensen and Pissarides, 1994).[3] Second, upon meeting, only a certain fraction of contacts is hired. All meetings draw an idiosyncratic training cost shock from a stable density function (see Chugh and Merkl, 2016 or Sedláček, 2014). Firms only select workers with an expected positive present value. Third, as Hartz IV was a reform of long-term unemployment benefits, we model a detailed deterministic unemployment structure. In our model, less generous long-term unemployment benefits trigger two effects. First, vacancy posting is stimulated (due to lower negotiated wages) and thereby the probability of making a contact with a firm increases for all workers. This equilibrium effect would not be measured in microeconometric studies. Second, less generous long-term unemployment benefits will affect the surplus of employment relative to unemployment. Due to this higher joint surplus, a larger fraction of applicants will be selected by firms upon meeting (partial effect).

Our paper contributes to the existing literature in several dimensions. First, we complement the microeconomic literature on unemployment benefits reform and estimate the partial effect at the firm level. For our empirical exercise, we use the IAB Job Vacancy Survey, which is a representative survey among up to 14,000 establishments. We find that the average share of applicants that firms selected after the reform increased by 11 percent when controlling for business cycle effects. The estimated increase of the selection rate is similar at different aggregation levels and when controlling for composition of the unemployment pool. This establishes the partial effect of an unemployment benefit reform at the firm level.[4] Reassuringly, our quantitative results for the partial effect are in a similar order of magnitude as the ones by Price (2019), who causally

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(2005-2019) afterwards.

[2] Both model simulation and data show a strong increase of vacancies. This is remarkably different from the United States around the time of the Great Recession (e.g. Benati and Lubik, 2014).

[3] Krause and Lubik (2014) argue that the search and matching model is both suitable for analyzing macroeconomic dynamics of the labor market and the effects of policy reforms.

[4] Typically, partial effects are estimated at the worker level. See Krueger and Meyer, 2002 for a survey or Card, Johnston, Leung, Mas, and Pei, 2015a; Card, Lee, Pei, and Weber, 2015b for more recent examples.

estimates the partial effect of the reform at the worker level based on individual administrative data (see Appendix C.2 for details).

Second, we contribute to the literature on the relative importance of individual (partial) and equilibrium effects of unemployment benefit reforms. Similar to Hagedorn, Karahan, Manovskii, and Mitman (2019) and Karahan, Mitman, and Moore (2019), our framework allows us to decompose the job-finding rate into a market-level (equilibrium) and an individual 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,[5] we refer to the latter as partial effect. We estimate the relative size of partial and equilibrium effects based on business cycle fluctuations. We find that these two effects are equally important.

Our methodology to determine the relative size of the partial and the equilibrium effect is completely different and thereby complementary to Chodorow-Reich, Coglianese, and Karabarbounis (2019), Hagedorn et al. (2019), and Karahan et al. (2019). While these authors identify the macro effects based on data revisions, a county-border discontinuity, and an unexpected cut in the duration of unemployment insurance, we use the firm-level variation of the selection rate over the business cycle to identify the relative importance of the equilibrium effect. One additional key difference is that we evaluate the macroeconomic implications of a permanent reform of long-term benefits, while they evaluate temporary changes of benefits that were triggered during the Great Recession.

We further contribute to the literature on the effects of the German Hartz IV reform. In contrast to the existing literature on the macroeconomic labor market effects of the Hartz IV reform (Krause and Uhlig, 2012, Krebs and Scheffel, 2013, Launov and Wälde, 2013), we do not use an exogenous source to determine the decline of the replacement rate on the job-finding rate. By contrast, we choose the decline of the replacement rate endogenously to target the partial effects from our estimation. One strength of this approach is that it determines the estimated partial effect independently of the chosen bargaining regime (individual bargaining vs. collective bargaining). Our approach is highly complementary to a recent paper by Hartung, Jung, and Kuhn (2020). While these authors put particular emphasis on the role of the separation rate, our paper proposes a new approach how to pin down the increase of the job-finding rate due to Hartz IV. Their paper and our paper both find that the Hartz IV reform played an important role for the decline of German unemployment.

Finally, our paper looks into the black box of recruiting intensity for Germany. Davis, Faberman, and Haltiwanger (2013) and Gavazza, Mongey, and Violante (2018) argue (in the context of the

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[5] 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.

Great Recession for the United States) that firms use other channels beyond vacancy posting. However, these channels are difficult to pin down for the United States due to a lack of suitable micro datasets. Our empirical exercise shows that the selection rate in Germany is strongly pro-cyclical over the business cycle and drives one half of the fluctuations of the job-finding rate over the business cycle. Thus, our paper identifies one important dimension of recruiting intensity: firms use time-varying hiring standards. Our time series evidence complements existing cross-sectional evidence on firms' hiring standards and, thus, closes an important research gap. Based on the Employment Opportunity Pilot Project (EOPP), Barron, Bishop, and Dunkelberg (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, Mueller, Şahin, and Topa (2017) show – based on a supplement to the Survey of Consumer Expectations – that only a fraction of worker-firm contacts translate to job offers.[6]

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 strategy. 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 benefit system consisted of three layers.[7] Upon beginning of a new unemployment spell, workers received short-term unemployment benefits ("Arbeitslosengeld"), which amounted to 60-67% of the previous net earnings[8] were paid up to 32 months. After the expiration of short-term benefits, the unemployed received long-term unemployment benefits ("Arbeitslosenhilfe"), which amounted to 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.

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[6] Our paper is further complementary to a recent literature on the cross-sectional implications of labor selection (see Baydur, 2017, Carrillo-Tudela, Gartner, and Kaas, 2020, and Lochner, Merkl, Stüber, and Gürtzgen, 2021).

[7] Hartz IV was part of a sequence of labor market reforms (the Hartz reforms), for details on the Hartz reforms, see Appendix A.

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

Figure 2: Illustration of the Hartz IV reform for single households.
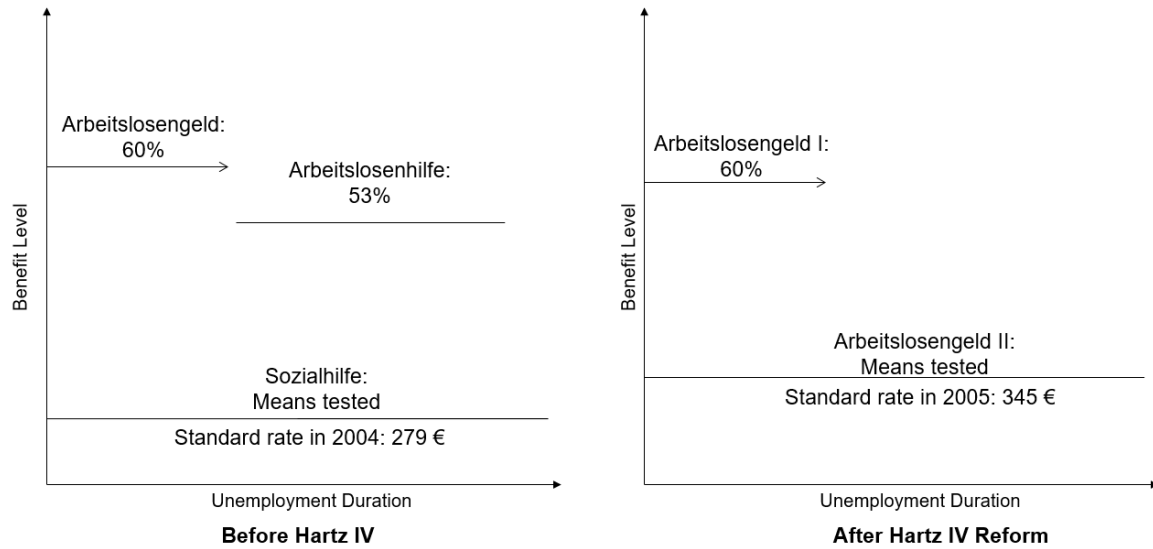
As a rule of thumb, the cut of benefits for long-term unemployed was larger for high income and high wealth households. The former faced a large drop because in the new system transfers are no longer proportional to prior earnings and high spousal income may lead to ineligibility. The latter faced a large drop because they 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 replacement 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 low-skilled workers are overrepresented in the pool of long-term unemployed and their benefits changed the least with the reform.

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 directly estimate the partial effect and the relative size of partial and equilibrium effect. The estimated effects are used as calibration targets for the model.

3 The Model

Our model economy consists of a unit mass of infinitely lived, risk neutral, atomistic multi-worker firms and infinitely lived, risk neutral workers.[9] 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. In the second stage, firms only select a certain fraction of all contacts because each worker-firm pair is hit by an idiosyncratic training costs shock. 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
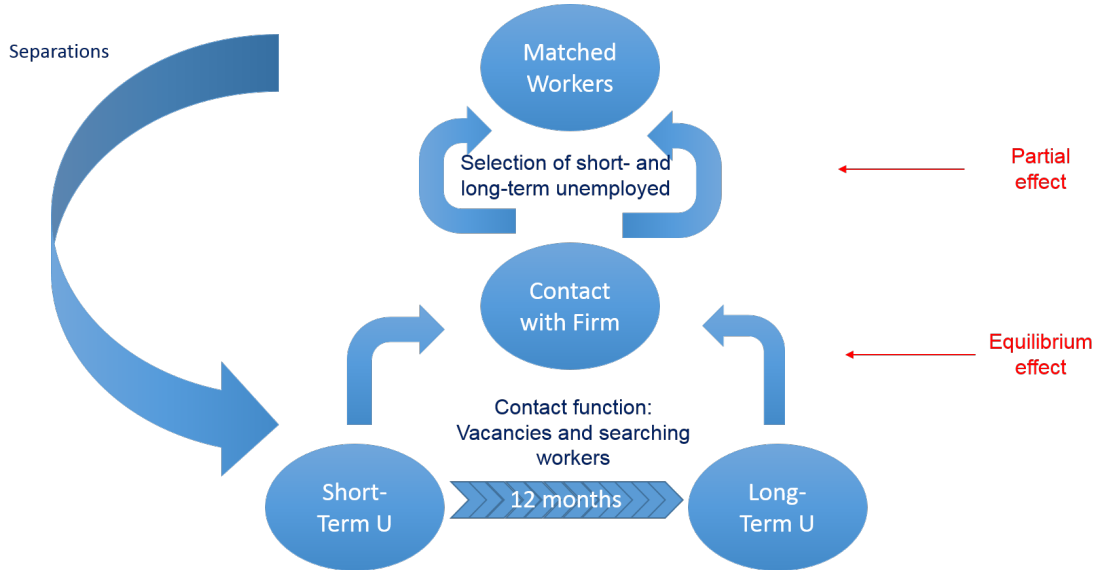
Figure 3 illustrates the main features of the model. Our model is similar to that in Kohlbrecher, Merkl, and Nordmeier (2016), to the stochastic job matching model (Pissarides, 2000, chapter 6) or many of the endogenous separation models (e.g. Krause and Lubik, 2007). Chugh and Merkl (2016), Lechthaler, Merkl, and Snower (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

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[9] For better comparison with establishment-level data, we solve a multi-worker firm's optimization problem. However, under constant returns to scale, the results are identical to a one worker-one firm setup.

Vacancy Survey does not provide sufficient 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.
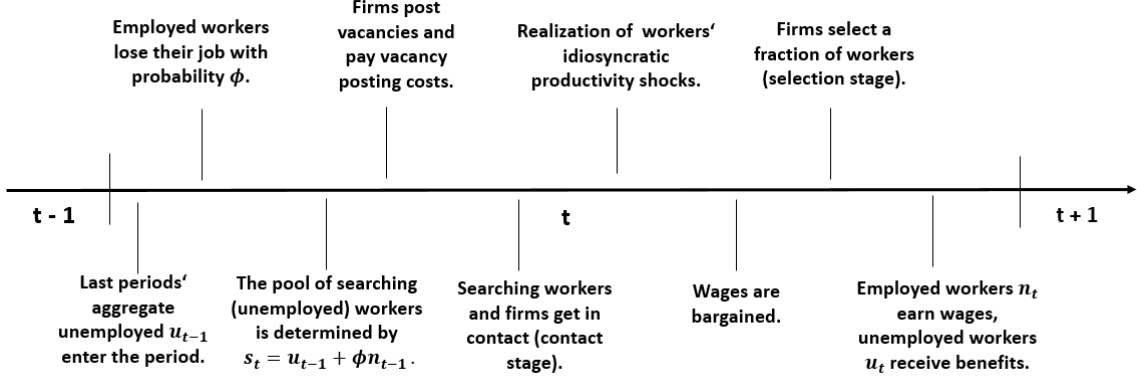
Figure 4 shows the timing of the model. Separated workers can immediately be re-matched. Therefore, the pool of searching workers, s_t, consists of unemployed workers from the previous period and workers that are separated at the beginning of the period.

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. Their 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 ϱ_t^d, the contact probability within this duration group q_t^d (which depends on aggregate market tightness), and the firm's selection rate, $\eta_{it}^d = \eta(\tilde{\varepsilon}_{it}^d)$, which depends on the 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:

$$E_0\left\{\sum_{t=0}^{\infty}\delta^t\Big[\ a_t n_{it} - w_{it}^I(1-\phi)n_{i,t-1} - \kappa v_{it} - v_{it}\sum_{d=0}^{12}\varrho_t^d q_t^d \eta_{it}^d(\bar{w}_{it}^d + \bar{H}_{it}^d + tc^d)\ \Big]\right\}, \qquad (3.1)$$

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:

$$n_{it} = (1-\phi)n_{i,t-1} + v_{it}\sum_{d=0}^{12}\varrho_t^d q_t^d \eta_{it}^d, \qquad (3.2)$$

$$\eta_{it}^d = \int_{-\infty}^{\tilde{\varepsilon}_{it}^d} f(\varepsilon)d\varepsilon \quad \forall d, \qquad (3.3)$$

$$\bar{H}_{it}^d = \frac{\int_{-\infty}^{\tilde{\varepsilon}_{it}^d}\varepsilon f(\varepsilon)d\varepsilon}{\eta_{it}^d} \quad \forall d, \qquad (3.4)$$

$$\bar{w}_{it}^d = \frac{\int_{-\infty}^{\tilde{\varepsilon}_{it}^d} w(\varepsilon)f(\varepsilon)d\varepsilon}{\eta_{it}^d} \quad \forall d. \qquad (3.5)$$

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

$$\tilde{\varepsilon}_{it}^d = a_t - \tilde{w}_{it}^d - tc^d + \delta(1-\phi)E_t\pi_{it+1}^I \quad \forall d, \qquad (3.6)$$

and

$$\kappa = \sum_{d=0}^{12}\varrho_t^d q_t^d \eta_{it}^d \bar{\pi}_{it}^d, \qquad (3.7)$$

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:

$$\pi_{it}^I = a_t - w_{it}^I + \delta(1-\phi)E_t\pi_{it+1}^I, \qquad (3.8)$$

$$\bar{\pi}_{it}^d = a_t - \bar{w}_{it}^d - \bar{H}_{it}^d - tc^d + \delta(1-\phi)E_t\pi_{it+1}^I. \qquad (3.9)$$

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 con-

ditional on hiring (i.e. the evaluation of the variable at the conditional mean of idiosyncratic training costs and/or wages). Intuitively, equation (3.6) 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 (3.7) 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 ϱ_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

$$c_t^d = \mu_t^d v_t^\gamma s_t^{1-\gamma} \quad \forall d, \tag{3.10}$$

where s_t are the number of searching workers, 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.

Different contact efficiencies for different duration groups mean that (at a given market tightness) some duration groups may have an exogenously lower probability to meet with a firm than others. This is in line with the well-known fact that the job-finding rate is lower for longer-term unemployed than for shorter-term unemployed. As the job-finding rate is the combination of contacts and selection, we calibrate the contact efficiency based on the survey responses whether certain unemployment groups received a job interview or not.

The contact probability for a firm and a worker in a given duration group are therefore:

$$q_t^d(\theta_t) = \mu_t^d \theta_t^{\gamma-1}, \tag{3.11}$$

and

$$p_t^d(\theta_t) = \mu_t^d \theta_t^\gamma \quad \forall d, \tag{3.12}$$

with aggregate market tightness defined as

$$\theta_t = \frac{v_t}{s_t}. \tag{3.13}$$

Intuitively, more searching workers generate the usual market externality for other workers and more searching vacancies for other firms. Note the market externality happens at the level of all workers.

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:

$$U_t^d = b^s + \delta E_t \left[p_{t+1}^{d-1} \eta_{t+1}^{d-1} \bar{V}_{t+1}^{d-1} + (1 - p_{t+1}^{d-1} \eta_{t+1}^{d-1}) U_{t+1}^{d-1} \right]. \tag{3.14}$$

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:

$$U_t^0 = b_t^l + \delta E_t \left[p_{t+1}^0 \eta_{t+1}^0 \bar{V}_{t+1}^0 + (1 - p_{t+1}^0 \eta_{t+1}^0) U_{t+1}^0 \right]. \tag{3.15}$$

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:

$$V_t^d(\varepsilon_t) = w_t^d(\varepsilon_t) + \delta E_t \left[(1 - \phi) V_{t+1}^I + \phi U_{t+1}^I \right] \quad \forall d. \tag{3.16}$$

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:

$$V_t^I = w_t^I + \delta E_t \left[(1 - \phi) V_{t+1}^I + \phi \left(p_t^{12} \eta_t^{12} \bar{V}_t^{12} + (1 - p_t^{12} \eta_t^{12}) U_t^{12} \right) \right]. \tag{3.17}$$

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:

$$w_t^d(\varepsilon_t) \in \arg\max \left(V_t^d(\varepsilon_t) - U_t^d \right)^\alpha \left(\pi_t^d(\varepsilon_t) \right)^{1-\alpha} \quad \forall d \tag{3.18}$$

for newly hired workers with prior duration index d and

$$w_t^I \in \arg\max \left(V_t^I - U_t^I \right)^\alpha \left(\pi_t^I \right)^{1-\alpha} \tag{3.19}$$

for an incumbent worker.

Maximizing the Nash product yields the following wage equations for incumbents

$$w_t^I = \alpha\big[a_t + \delta p_t^{12}\eta_t^{12}E_t\pi_{t+1}^I\big] \tag{3.20}$$
$$+ (1-\alpha)\big[-p_t^{12}\eta_t^{12}E_t\big[(\delta V_{t+1}^I - \bar{V}_t^{12})\big] - (1-p_t^{12}\eta_t^{12})E_t\big[(\delta U_{t+1}^I - U_t^{12})\big]\big],$$

and different duration groups of entries:

$$w_t^d(\varepsilon_t) = \alpha\big[a_t - \varepsilon_t - tc^d + \delta E_t\big[p_{t+1}^{d-1}\eta_{t+1}^{d-1}\pi_{t+1}^I\big]\big] \tag{3.21}$$
$$+ (1-\alpha)\big[b^S - \delta E_t p_{t+1}^{d-1}\eta_{t+1}^{d-1}(V_{t+1}^I - \bar{V}_{t+1}^{d-1}) - \delta E_t\big[(1-p_{t+1}^{d-1}\eta_{t+1}^{d-1})(U_{t+1}^I - U_{t+1}^{d-1})\big]\big]$$
$$\forall d \in \{1,\dots,12\},$$

$$w_t^0(\varepsilon_t) = \alpha\big[a_t - \varepsilon_t - tc^0 + \delta E_t\big[p_{t+1}^0\eta_{t+1}^0\pi_{t+1}^I\big]\big] \tag{3.22}$$
$$+ (1-\alpha)\big[b_t^L - \delta E_t p_{t+1}^0\eta_{t+1}^0(V_{t+1}^I - \bar{V}_{t+1}^0) - \delta E_t\big[(1-p_{t+1}^0\eta_{t+1}^0)(U_{t+1}^I - U_{t+1}^0)\big]\big].$$

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 $E_t U_{t+1}^0$. 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 depends on the individual idiosyncratic training cost realization.[10] In D.3 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 ($d \in \{0,\dots,12\}$):

$$u_t = \sum_{d=0}^{12} u_t^d. \tag{3.23}$$

Unemployment in period t is thus given by

$$u_t = 1 - n_t. \tag{3.24}$$

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:

$$u_t^{12} = \phi(1 - p_t^{12}\eta_t^{12})n_{t-1}. \tag{3.25}$$

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[10] The wage for entrants is smaller than the wage for incumbents in our bargaining set-up due to training costs, and because longer-term unemployment is associated with higher fixed training costs in our calibration. 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.

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

$$u_t^d = (1 - p_t^d \eta_t^d) u_{t-1}^{d+1}, \tag{3.26}$$

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:

$$u_t^0 = (1 - p_t^0 \eta_t^0)(u_{t-1}^1 + u_{t-1}^0). \tag{3.27}$$

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

$$s_t = \phi n_{t-1} + u_{t-1}. \tag{3.28}$$

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

$$\varrho_t^{12} = \frac{\phi n_{t-1}}{s_t}, \tag{3.29}$$

for newly separated workers,

$$\varrho_t^d = \frac{u_{t-1}^{d+1}}{s_t}, \tag{3.30}$$

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

$$\varrho_t^0 = \frac{u_{t-1}^1 + u_{t-1}^0}{s_t} \tag{3.31}$$

for long-term unemployed.

3.5 Labor Market Equilibrium

Given initial values for all states of unemployment u_{t-1}^d with $d \in \{0,\ldots,12\}$ and employment n_{t-1} as well as processes for productivity, long-term unemployment benefits, and the spell-dependent contact efficiency $\{a_t, b_t^l, \mu_t^d\}_{t=0}^{+\infty}$, the labor market equilibrium is a sequence of allocations $\{n_t, u_t, s_t, v_t, \theta_t, u_t^d, \varrho_t^d, p_t^d, q_t^d, \tilde{\varepsilon}_t^d, \eta_t^d, \bar{H}_t^d, \pi_t^I, \bar{\pi}_t^d, V_t^I, \bar{V}_t^d, \tilde{V}_t^d, U_t^I, U_t^d, w_t^I, \bar{w}_t^d, \tilde{w}_t^d\}_{t=0}^{+\infty}$ for all durations $d \in \{0,\ldots,12\}$ that satisfy the following equations: the definition of employment (3.24), unemployment (3.23) and searching workers (3.28), market tightness (3.13), unemployment (3.25) - (3.27), the shares of searching workers (3.29) - (3.31), the contact rates for workers (3.12) and firms (3.11), the free-entry condition for vacancies (3.7), the hiring cutoffs (3.6), selection rates (3.3), conditional expectation of idiosyncratic hiring costs (3.4) and wages (3.5)[11], the definition of profits for entrants (3.9) and incumbents (3.8), the value of a job for entrants (3.16) and incumbents (3.17), the value of unemployment (3.14), and (3.15), and wages (3.20), (3.21) and (3.22).

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

4 Empirical Results

While there is agreement that the Hartz IV reform reduced the generosity of long-term unemployment benefits, there is no consensus on the exact quantitative decline of the replacement rate (see Section 2 for details). This poses a challenge for any model-based macroeconomic evaluation. In order to circumvent this problem, we propose a new strategy based on the IAB Job Vacancy Survey.

Our empirical strategy is based on measuring three objects: (i) the permanent shift of selection in response to the reform, (ii) the elasticity of the selection rate w.r.t. market tightness and (iii) the elasticity of the job-finding rate w.r.t. market tightness. The first object pins down the partial effect in our framework. The second and third objects determine the relative size of the partial and equilibrium effects. We use the estimated results as calibration targets for our quantitative exercise (see Section 5).

4.1 Measuring Selection

In our model, a multi-worker firm can adjust the workforce along two margins. First, it can change the number of posted vacancies. Depending on aggregate market tightness, this determines the number of applicants (vacancies multiplied with q_t) that get in contact with the firm. Second, the firm selects a certain fraction of these applicants, η_t. In our model, the selection rate corresponds to the number of hires divided by the number of contacts ($\frac{\eta_t v_t q_t}{v_t q_t}$).

Why does the selection rate increase when unemployment benefits decline? Lower unemployment benefits increase the joint surplus of a match. Therefore, a larger fraction of workers, η_t, is selected upon contact. Assume for illustration purposes that a multi-worker firm chose 33% of contacts before the reform and 50% of contacts after the reform. In this case, it had on average three contacts per hire before the reform and two contacts per hire after the reform. Thus, our model predicts that the selection rate rises and the number of contacts per hire decrease in response to the reform (see Appendix D.1 for an illustration).

We use the IAB Job Vacancy Survey to proxy the selection rate. The IAB Job Vacancy Survey is an annual representative survey of up to 14,000 German establishments (for more information on the dataset, see Appendix B.1). Ideally, we would measure the selection rate as the ratio of all hires over all applicants ($\frac{\eta_t v_t q_t}{v_t q_t}$). However, in the IAB Job Vacancy Survey, we only have information on firms' recruiting situation for the most recent hire. Firms are asked about the number of suitable applicants,[12] which we use as a proxy for the selection rate by taking the inverse. Hence, from the perspective of an applicant, this represents his or her probability of being selected (upon contact) by the firm. Both our choice of "suitable applicants" and the focus on the most recent hire require some more discussion.

We believe that the notion of "suitable applicants" is well in line with our model.[13] First, as firms consider these applicants as suitable, they must have screened them in some way (e.g. by check-

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[12] While this information is available for a long-time period, the total number of applicants is only available from 2005 onwards and therefore not suitable for our question at hand.

[13] To provide intuition, we illustrate the model reaction of the total number of contacts and contacts per hire in response to a reduction of the replacement rate for long-term unemployed in Appendix D.1 for our baseline calibration.

ing the CV or by inviting them to an interview). Thus, through the lens of our model, the contact function established a contact between firms and applicants. Second, when firms answer how many suitable applicants they had for a given position, we expect that these applicants were suitable in terms of observable characteristics (e.g. skills or experience.) This is also in line with our model where we abstract from observable characteristics. Ex-ante homogeneous applicants are ex post heterogeneous in terms of an idiosyncratic shock realization in our model. Although applicants may appear suitable in terms of their qualifications or their CV, a more detailed screening may uncover that the candidates have unfavorable characteristics (i.e. high training costs in terms of our model).[14]

Given that we only have information for the most recent hire (and not all hires), it is important to ensure representativity. Several comments are in order: First, when employers made several simultaneous hirings, the survey asks about the hired individual whose surname comes first in alphabetical order (i.e. randomness is imposed). Second, while a given vacancy may not be representative at a particular firm (e.g. if the firm hires one high-skilled and one low-skilled worker, the firm reports on the most recent hire), we use representative survey weights to ensure representativity at higher aggregation levels (e.g., state, industry, national). The last step allows us to obtain a panel dimension at the industry and state level. As the IAB Job Vacancy Survey is a repeated cross section, we can only perform panel regressions at an aggregated level in any case. Using our measure of the selection rate, we construct annual time series on the national (West Germany as a baseline and entire Germany as robustness[15]), state and industry level from 1992 to 2019.

Figure 5 shows the time series of the job-finding rate (defined as matches over unemployment), selection rate and market tightness (defined as vacancies over unemployment) from 1992 to 2019 (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 (see Appendix C for a discussion).

4.2 Estimation Strategy

As the aggregate contact and selection rate are roughly multiplicative in our model ($jfr_t \approx p_t \eta_t$),[16] we can express the job-finding rate, $\hat{jfr}_t$, as the sum of the contact rate, $\hat{p}_t$, and the selection rate, $\hat{\eta}_t$, in terms of log-deviations (denoted with hats):

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[14] 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 and lowers the measured selection rate, which would bias our estimate for the selection rate downward, i.e. we would obtain a lower bound.

[15] 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, Jost, Kaufmann, Ludewig, and Weyh, 2018). Second, the number of establishments in the sample is very small in the early 1990s.

[16] 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.

Figure 5: German labor market dynamics, 1992-2019.
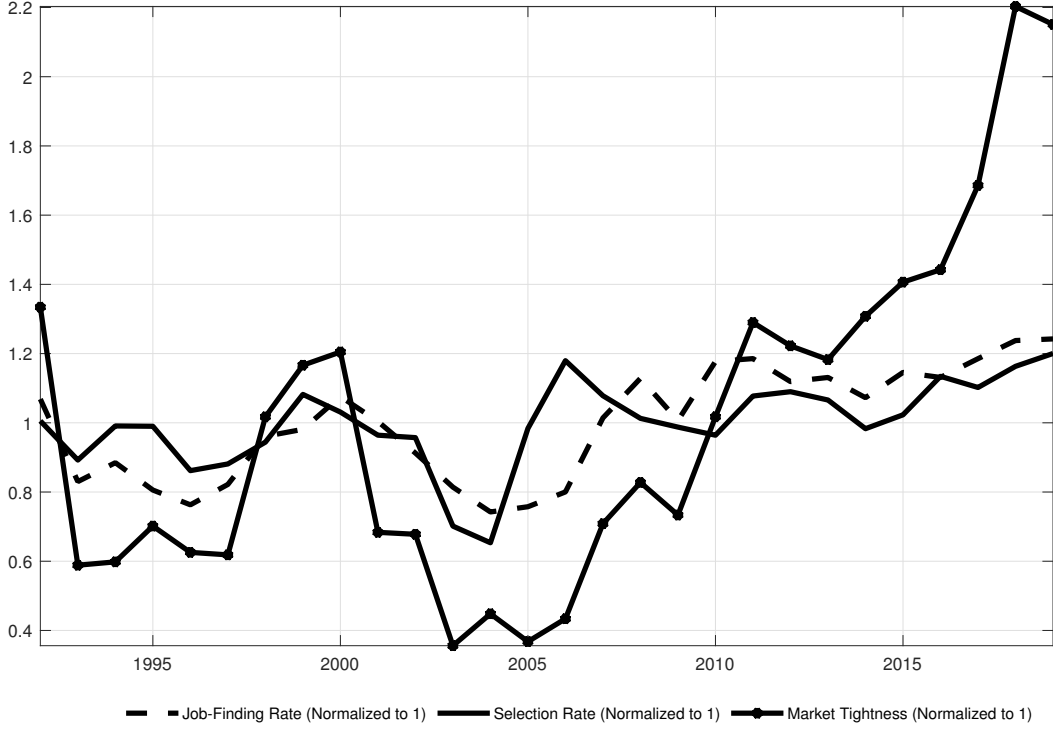
Note: For visibility, we normalized all three time series to mean one. The job-finding rate (matches over unemployment) and market tightness (vacancies over unemployment) are constructed with the corrected unemployment series (as described in Appendix E.3). For details on the data sources, see Table B.1 in Appendix B.

$$\hat{jfr}_t \approx \hat{p}_t + \hat{\eta}_t. \tag{4.1}$$

We refer to the response of the selection rate to a benefit change as the partial effect and the response of the contact rate as equilibrium effect.

In order to quantify the macroeconomic reaction of the job-finding rate with respect to changes in long-term unemployment benefits, we proceed in two steps. First, we estimate the reaction of the selection rate with respect to changes of benefits ($\partial \hat{\eta}_t / \partial \hat{b}_t$), i.e. the partial effect. Second, we use the connection that the overall macroeconomic effect can be disentangled into the equilibrium effect ($\partial \hat{p}_t / \partial \hat{b}_t$) and the partial effect:

$$\frac{\partial \hat{jfr}_t}{\partial \hat{b}_t} \approx \frac{\partial \hat{p}_t}{\partial \hat{b}_t} + \frac{\partial \hat{\eta}_t}{\partial \hat{b}_t}. \tag{4.2}$$

Estimating the effects of benefits on labor selection has two advantages compared to estimating the effects of benefits on the job-finding rate. First, selection happens after contacts were established. Thus, there is no direct effect of improved matching efficiency on selection. This is important because of the reform the Federal Employment Agency in 2003 (Hartz III), which according to Launov and Wälde (2016) improved matching efficiency.[17] Second, in contrast to

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[17] In our setting, this would be equivalent to an increase in the contact efficiency. We analyze the effect of an increase in the contact efficiency in Appendix D.4 and show that this would bias our results downwards.

the time series for the job-finding rate (which is calculated as matches over unemployed), the selection rate does not suffer from a severe statistical break in 2005 (see Appendix E for a detailed discussion). Against this background, we use a simple shift dummy (which is one from 2005 onwards) to determine the effects of the Hartz IV reform on selection. In addition, we control for business cycle and composition effects.

In a second step, we determine the relative importance of the equilibrium effect. For this purpose, we use the following relationship from our model:

$$\frac{\partial \hat{p}_t / \partial \hat{b}_t}{\partial \hat{\eta}_t / \partial \hat{b}_t} \approx \frac{\partial \hat{p}_t / \partial \hat{\theta}_t}{\partial \hat{\eta}_t / \partial \hat{\theta}_t}. \tag{4.3}$$

We can show numerically that this feature holds approximately in our full quantitative model.[18] Note that selection, job finding and market tightness positively comove with aggregate productivity in our model. Up to a first-order Taylor approximation, there is a linear comovement between productivity and tightness.

The relative importance of the partial and the equilibrium effect are the same for a labor market reform and over the business cycle. Intuitively, both an increase of productivity and a reduction of unemployment benefits increase the joint surplus of a match. Thus, they have equivalent effects on the contact and selection margin. Empirically, market tightness is a better indicator for the labor market stance than productivity. Therefore, we use this measure to determine the relative importance of the partial and equilibrium effects.

The contact rate is not observable in the data. But the elasticity of the contact rate and selection rate approximately add up to the elasticity of the job-finding rate with respect to market tightness:

$$\frac{\partial \hat{jfr}_t}{\partial \hat{\theta}_t} \approx \frac{\partial \hat{p}_t}{\partial \hat{\theta}_t} + \frac{\partial \hat{\eta}_t}{\partial \hat{\theta}_t}. \tag{4.4}$$

Therefore, we can estimate the comovement of the job-finding rate and the selection rate with market tightness. We impose these elasticities in our calibrated model and thereby discipline the relative size of the partial and equilibrium effects by their business cycle movements.

4.3   Baseline Results

To estimate the partial effect, the logarithm of the selection rate is regressed on a shift dummy that takes the value of one from 2005 onward ($D_t^{Hartz\ IV}$). The shift dummy measures the permanent differences of the selection rate before and after Hartz IV. To disentangle the partial effect from the equilibrium effects, we control for business cycle fluctuations (either in terms of value added or market tightness):

$$\log \eta_t = \beta_0 + \beta_1 D_t^{Hartz\ IV} + \beta_2 X_t + \nu_t, \tag{4.5}$$

where X_t denotes a business cycle control (such as value added growth or market tightness). Due to data availability, we perform the baseline estimation on an annual basis for the sample range

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[18]   For an analytical illustration in the context of a simplified model see Appendix D.2.

1992 to 2019 for West Germany.

Columns 1 and 2 in Table 1 show that conditional on value added or market tightness fluctuations the aggregate selection rate has increased by 15% or 11% respectively after the Hartz IV reform. The estimated coefficients are statistically significant at the 5% level.[19] We take the more conservative second value as calibration target. Although our baseline result is based on very simple specification, we show in the next subsection that it is robust in various dimensions (e.g. controlling for composition, testing for the time structure, disaggregating to the regional or industry level).

Table 1: Regression Results for West Germany, 1992-2019.

| | *Dependent variable:* | | |
| | log(selection rate) | | log(job-finding rate) |
	(1)	(2)	(3)
Hartz IV-Dummy	0.15***	0.11**	0.09***
	(0.04)	(0.04)	(0.03)
Value Added Growth	1.21*		
	(0.64)		
log(Market Tightness)		0.13**	0.26***
		(0.05)	(0.03)
Constant	−0.80***	−0.60***	−2.64***
	(0.04)	(0.07)	(0.04)
Observations	28	28	28
R^2	0.38	0.54	0.85
Adjusted R^2	0.33	0.51	0.84

Note: Estimation by OLS with robust standard errors; *p<0.1; **p<0.05; ***p<0.01. For details on the data sources, see Table B.1 in Appendix B.

To determine the relative importance of partial and equilibrium effects, we estimate the comovement of the job-finding rate and the selection rate with market tightness.[20] Column 2 in Table 1 shows that the estimated elasticity for the selection rate is 0.13. We perform the same regression with the logarithm of the job-finding rate as dependent variable. Column 3 in Table 1 shows an estimated elasticity of 0.26 (which is well in line with existing matching function estimations[21]). By targeting both estimated elasticities in a dynamic business cycle simulation of our model, we can uniquely pin down the contact elasticity.

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[19] We use heteroskedasticity-consistent estimators, with a small sample correction proposed by MacKinnon and White (1985)

[20] Although we also estimate a shift dummy for the job-finding rate, we do not interpret it (due to the statistical break). The key purpose of this estimation is the estimation of the elasticity of the job-finding rate with respect to market tightness for determining the size of the equilibrium effect.

[21] See e.g. Hertweck and Sigrist (2013) (based on data from the German Socio-Economic Panel) and Kohlbrecher et al. (2016) (based on detailed administrative data).

4.4 Robustness Checks and Discussion

The estimated partial effects are robust in various dimensions:

In columns 1 and 2 of Table 2, we show that the results are very similar if we disaggregate by state and industry level and use a panel fixed-effects estimator. Our estimation of the shift dummy may be misinterpreted if the selection rate moves due to composition over time (e.g. as unemployed with different skills or durations may have a different selection rate). 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 3 and 4 in Table 2). Appendix E shows further robustness checks with additional controls for the pool of applicants and unemployment along the gender and age dimension (see Table E.2). In addition, controlling for market tightness instead or in addition to value added growth (see Table E.3) does not change the estimated coefficient on the Hartz IV-dummy by much.

Table 2: Robustness

	Dependent variable:			
	log(selection rate)			
	panel linear		*OLS*	
	State Level	Industry Level	Aggr.: Low Qualification	Aggr.: Long-term U
	(1)	(2)	(3)	(4)
Hartz IV-Dummy	0.15***	0.13***	0.16***	0.18**
	(0.02)	(0.03)	(0.03)	(0.07)
Value Added Growth	0.51	0.34	0.92	0.86
	(0.88)	(0.46)	(0.65)	(0.78)
log(Low Qualification)			0.48***	
			(0.13)	
log(Share long-term U)				0.26
				(0.31)
Constant			0.17	−0.56*
			(0.26)	(0.32)
Observations	140	168	28	22
R^2	0.19	0.18	0.69	0.41
Adjusted R^2	0.15	0.14	0.65	0.31

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 - 2019. *p<0.1; **p<0.05; ***p<0.01. For details on the data sources, see Table B.1 in Appendix B.

Figure F.1 in F shows that the increase of the selection rate between 2004 and 2005 was largest for workers in the middle of the skill distribution.[22] This is in line with our expectations. Compared

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[22] The question on skills is only available from 2004 onwards. Therefore, we can only provide anecdotal evidence for

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.[23]

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 at the 5 percent level 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 and remains on a higher level from then onwards. Thus, the shift of the selection rate cannot be attributed to long-run trends such as the wage moderation that started in the early 2000s.

Figure 6: Alternative shift-dummies starting in each respective year (controlling for value added growth).
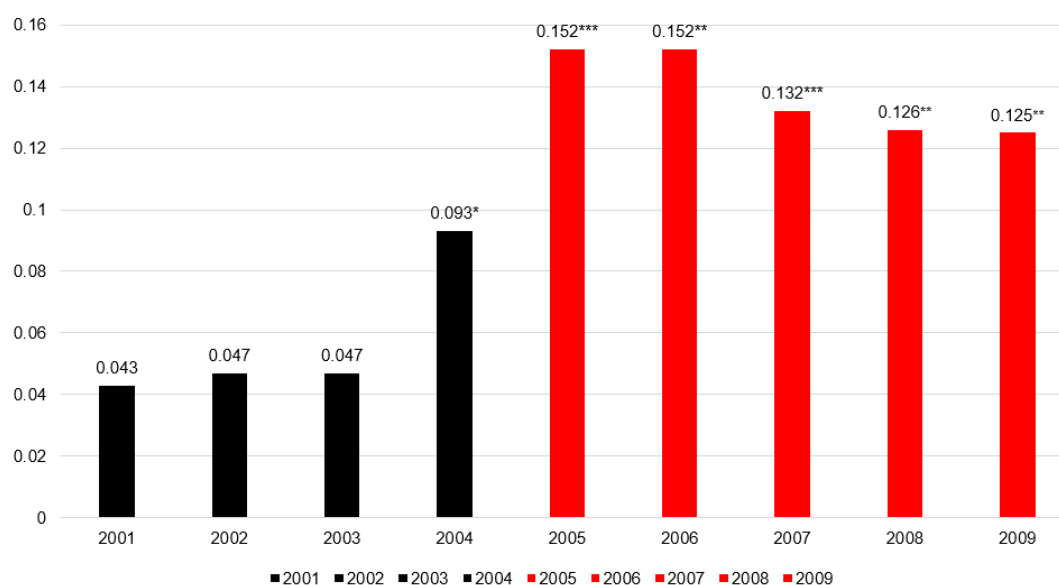
Note: The red bars denote significant dummy estimates at the 1 percent (***) and 5 percent (**) significance level.

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 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

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the upward shift between 2004 and 2005.

[23] While it would be desirable to study the cross-sectional 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.

parliamentary discussions about the Hartz IV reform started in 2003. The Hartz IV law passed both chambers in 2004. If anticipation effects played a role, our dummy estimate for 2005 would constitute a lower bound.

As further reassuring evidence, we can compare our results to Price (2019) who uses German administrative worker-level data to analyze the partial effect of Hartz IV. He estimates the causal microeconometric effects of Hartz IV. In Section C, we show that our and Price's (2019) partial effects are of a similar order of magnitude despite of different data source and very different methodologies. The benefit of our framework is that we provide complementary evidence and can make statements on the size of the equilibrium effect on top of the partial effect.

5 Calibration

We calibrate our model to West-German data.[24] Table 3 illustrates the parameters and targets for our quantitative exercise. 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

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[24] 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 E.2).

Table 3: Parameters and targets for calibration.

Parameter	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	Targeted unemployment rate
Elasticity of contact rate w.r.t. vacancies	0.13	Empirical matching and selection functions
Target	Value	Source
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
Unemployment rate	0.09	Average unemployment rate before the reform
Market tightness	0.25	IEB and Job Vacancy Survey
Selection rate	0.46	Job Vacancy Survey
$\partial \log \eta / \partial \log \theta$	0.13	IEB and Job Vacancy Survey
$\partial \log jfr / \partial \log \theta$	0.26	IEB and Job Vacancy Survey

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 that all short-term unemployed face the same contact, selection, and job-finding rate,[25] our calibration implies that the fixed training costs component increases every month with the duration of unemployment.[26]

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 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 0.13 in the estimation.[27] 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 0.26 in the data. The resulting weight on vacancies in the contact function is 0.13.

We solve for the transition to the post-reform steady state under perfect foresight with a fully nonlinear solution algorithm. The business cycle simulations to match certain moments from the estimations are performed with a second-order Taylor approximation.

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[25] While we observe different job-finding rates per month of short-term unemployment duration in the data, we cannot compute the corresponding contact rates. We use the IAB PASS survey to determine the pre-reform steady state contact rates and the IAB Job Vacancy Survey to measure the selection rate. Note that due data availability the analogy is not perfect as the former are based on interviews, while the selection rate is based on suitable candidates.

[26] 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.

[27] The resulting scale parameter of the distribution is 4.8. 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 the existing literature.

6.1 Partial Effects

Our empirical estimations in Section 4.3 has shown that the reform resulted in a 11% increase of 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% in our baseline scenario (individualistic bargaining).

Due to the importance of collective bargaining at the time of the reform implementation, we study the effects under a collective bargaining regime as a robustness check. In 2004, 68% of employees in West Germany were covered by a collective bargaining regime, see Elguth and Kohaut (2005). Under the collective bargaining scenario, the required drop of unemployment benefits (to match the estimated selection rate shift) is 23% (see Appendix D.3). 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 D.3). 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 modeling choices. This does not only apply to the wage bargaining regime. In a recent paper, Hartung et al. (2020) argue that separation rates declined sharply as a result of the Hartz IV reform which contributed to the decline of the unemployment rate. Lower separation rates have a feedback effect on the selection rate, as they affect the net present value of a match. Given that we estimate and target the effect on the selection rate, the simulated partial effect is robust to a potential endogenous separation channel. However, a declining separation rate would further increase the effects on the unemployment rate (beyond the effect via the job-finding rate). Thus, our quantitative results on unemployment constitute a lower bound.

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

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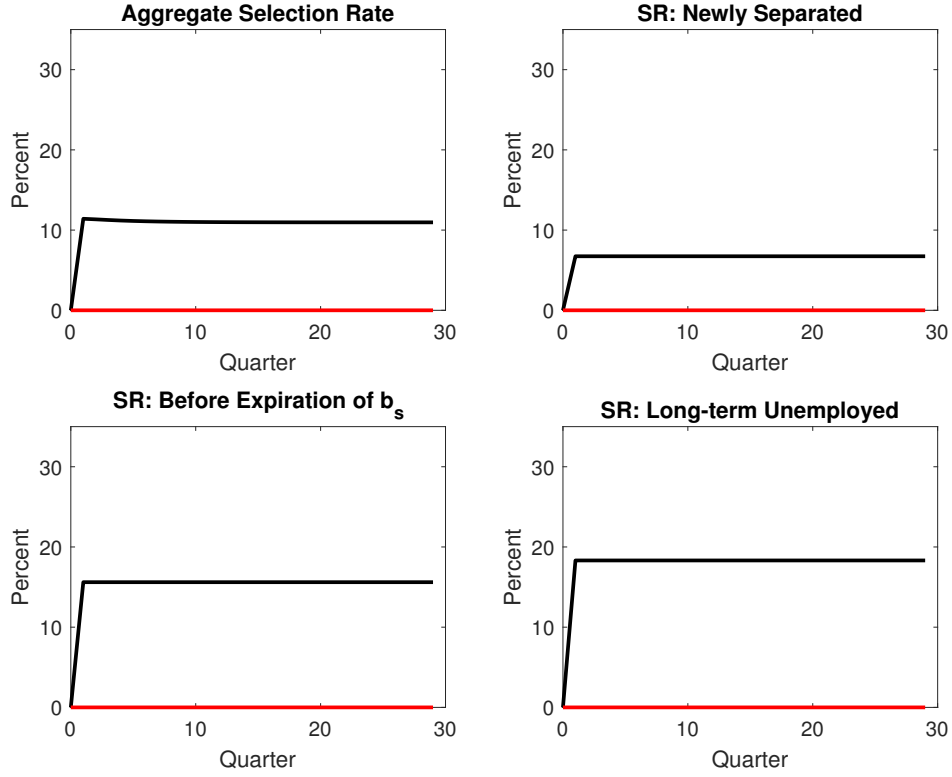

impact for all groups of searching workers due to a lower outside option. However, the effect is larger, the closer the unemployed get to the expiration of the more generous short-term benefits. For workers who have just been separated from a job (upper right panel in Figure 7), the reduction of long-term unemployment benefits affects their present value of unemployment by the least because their benefits only drop 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 16%. This is in line with Price (2019), who finds that unemployed workers' incremental increase of the transition rate (from unemployment to employment) due to the Hartz IV reform increases with duration 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 18%. 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 long-run 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 re-

maining 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: Long-run responses of selection rate to a decline of long-term unemployment benefits by remaining months of short-term benefit entitlement.
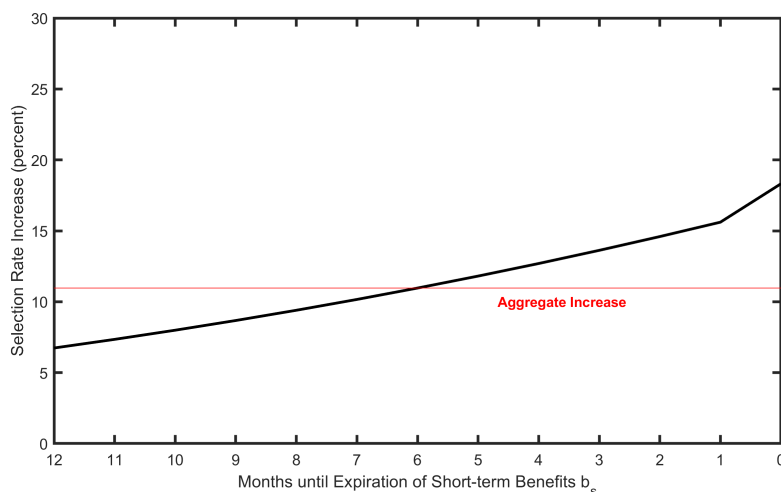
In our model, the average wage over the employment spell for a reemployed worker who exhausted short-term benefits falls by 2.5% due to Hartz IV. As shown in Appendix C, this and other orders of magnitude are similar to Price (2019).

The average wage effect in our model is smaller than the wage effect of workers who exhausted their benefits. Averaged over all unemployment durations, the average reemployment wage drops by only 1.1%.[28]

The wage effects are 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, von Wachter, and Bender (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 more painful which might explain why Price (2019) 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 (2019) 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, 2019) and firm survey data (this study).

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[28] 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%.

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 4.3.

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 9.7% on impact.[29] The overall aggregate job-finding rate, which is the product of both the contact and the selection rates, increases by 19.6% on impact (lower right panel of Figure 9). Therefore, one half of the initial response of the job-finding rate is due to the equilibrium effect. Note that the response of the selection rate (10.1% increase on impact, 9.3% 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 22.6%. This corresponds to a decrease of the steady state unemployment rate by 2.1 percentage points in our calibration.

Figure 9: Impulse responses to a decline in long-term unemployment benefits.
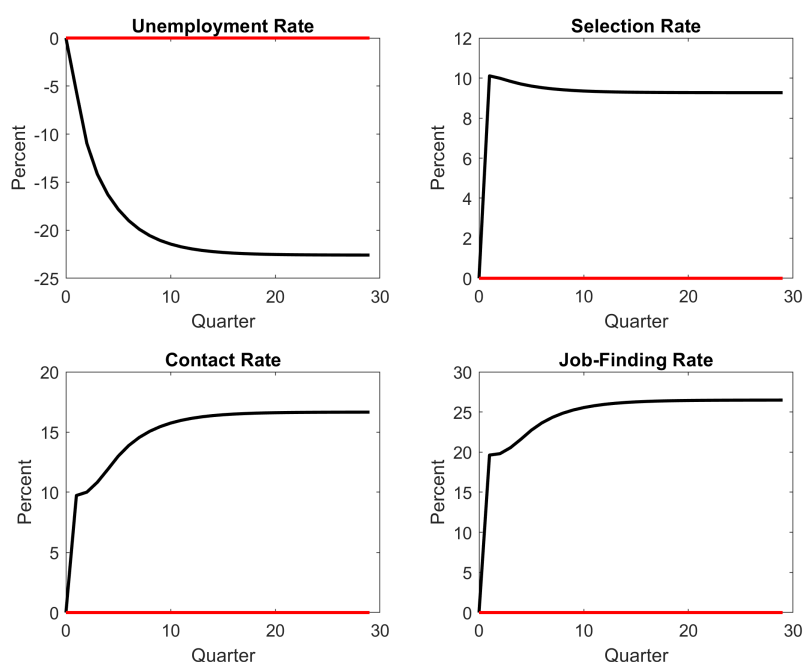
When the economy adjusts to a new steady state, the composition of the pool of unemployed changes. This can be seen in the adjustment dynamics of the contact and job-finding rate, which increase quite sluggishly. The aggregate contact and job-finding rates are a weighted average for

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[29] 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.

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.[30]

In the long run, the job-finding rate in our simulated model increases by 26.5 percent. The average job-finding rate for short-term unemployed increased by more than for long-term unemployed, which is in line with what we observe in the data. For a discussion of the orders of magnitude and for an intuition, see Appendix C.

Our results show that there are quantitatively 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, Michaillat, and Saez, 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,[31] we abstain from extending our framework in this respect. Hence, we regard the effects of our baseline model as a lower bound.

Finally, we 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.[32] Unemployment sequentially declines to a permanently lower level in the data. The movements are not only qualitatively comparable,

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[30] 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.

[31] 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 methodology to isolate the pure search effort effects.

[32] 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.

but the quantitative reactions (as percent deviations) are also similar.

Figure 10: Beveridge Curve generated by the model during the first three years after the shock.
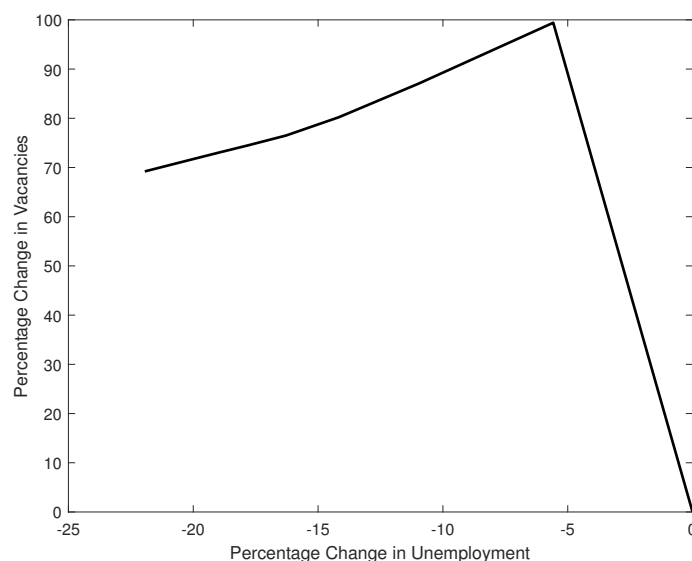
Figure 11: West German Beveridge Curve from 2005-2007.
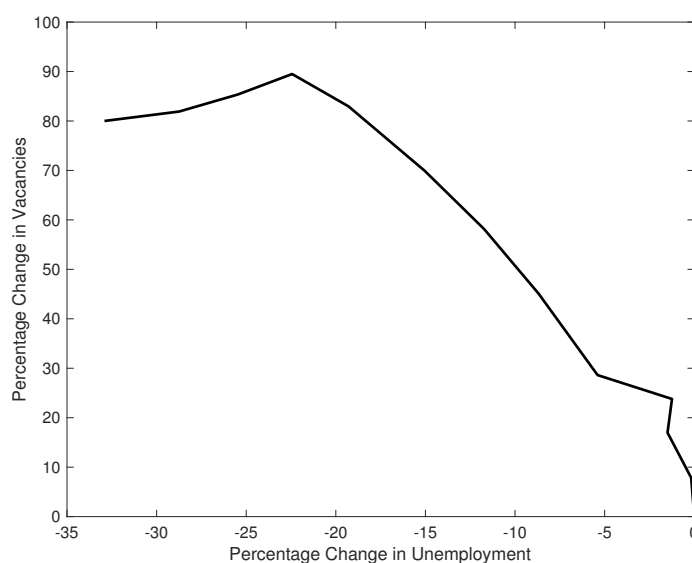
Note: We use the corrected unemployment series (as described in Appendix E.3).

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, Launov, and Robin (2021) and Rothe and Wälde (2017) document that relatively few unemployed workers directly transitioned to regular (full-time) employment after Hartz IV. Indeed, Carrillo-Tudela et al. (2021) 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[33] 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.

These results by Carrillo-Tudela et al. (2021) contrast with the findings by Price (2019). 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 (2021) 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 may overestimate 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 modeling participation decisions in quantitative models, the work by Carrillo-Tudela et al. (2021) 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.

Besides contributing to the scientific debate on the macroeconomic effects of unemployment benefit reforms, our paper also contributes to the literature on recruiting intensity. Our approach is closely related to Davis et al. (2013) and Gavazza et al. (2018). Davis et al. (2013) argue in the context of the Great Recession that firms in the United States use additional margins on top of vacancy posting to adjust hiring over the business cycle. Labor selection represents one of these margins. We are the first to construct an aggregate time series on labor selection. We show that labor selection behaves strongly procyclical over the business cycle and generates plausible patterns in the cross section (see C for a brief discussion. Through the lens of our search and matching model with labor selection, one half of job findings is driven by labor selection, while the

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[33] The rise in part-time employment around the time of the Hartz reforms is also documented in a recent paper by Burda and Seele (2020).

other half is driven by the vacancy margin. Thus, our paper opens up the black box of recruiting intensity in the time dimension.

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 the 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 fact, such as the joint business cycle dynamics of the selection rate, the job-finding rate, and market tightness. Furthermore, the simulated model generates a realistic trajectory of the Beveridge Curve in response to the reform. Overall, our results show that 2.1 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 2005/06 (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 centers. Those local job centers 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, Anne, Rebien, and Vogler-Ludwig (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, Burghardt, and Trenkle, 2013).[34] 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.[35]

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 2019. 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. The Job Vacancy Survey contains too few observations for small

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[34] Status quo of the data as of 2020.
[35] This corresponds to the available pre-Hartz period.

federal states in order to be representative. Therefore, we restrict our sample to federal states with at least 6 million inhabitants.[36]

B.2   Details on the IAB PASS Survey

Furthermore, we use data of the IAB Panel Study Labor Market and Social Security (PASS)[37] 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 benefits 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.[38] 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, Beste, Bethmann, and Müller (2013).

B.3   Data Sources

Table B.1: Data Sources for Empirical Estimations

Variable Name	Details	Source
Unemployment rate	Registered unemployment rate for entire Germany	Federal Employment Agency
Job-finding rate	Matches over unemployed (quarterly averages of monthly data, 3rd quarter)	IAB: Integrated Employment Biographies (IEB)
Selection rate	Inverse of the number of suitable applicants (aggregated)	IAB: Job Vacancy Survey (JVS)
Vacancies	Number of vacancies, 3rd quarter	IAB: Job Vacancy Survey (JVS)
Value added growth	Annual growth in value added	Statistisches Bundesamt (Destatis): volkswirtschaftliche Gesamtrechnung der Länder
Low qualification	Share of vacancies for jobs with low qualification	IAB: Job Vacancy Survey (JVS)
Share long-term u.	Share of long-term unemployed workers	IAB: Integrated Employment Biographies (IEB)
Female hires	Share of female hires as of total hires	IAB: Job Vacancy Survey (JVS)
Female unemp	Share of female unemployed as of total unemployed	OECD; unemployed population: Aged 15 and Over: Females for Germany
Share unemp over 50	Share of unemployed workers over the age of 50 as of total unemployed	Statistisches Bundesamt (Destatis): Mikrozensus
Export growth	Export growth (year-on-year), 3rd quarter	OECD: exports of goods and services in Germany

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[36] As of December 2020. Hence, we include Baden-Wuerttemberg, Bavaria, North-Rhine Westphalia, Lower Saxony plus Bremen and Hessen.

[37] 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.

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

C   Model Validation

This Appendix analyzes the validity of the model from three perspectives. First, we analyze the general business cycle and cross-sectional properties of our model. Second, we compare non-targeted dimensions of the partial effects to results from Price (2019). Third, we compare equilibrium outcomes of our model to time series evidence by Klein and Schiman (2021) and descriptive results.

C.1   Business Cycle and Cross-Sectional Properties

Although we target our model to important business cycle properties, it is worthwhile analyzing whether the general time series properties of our model are in line with the time series properties of the new dataset on selection. This is analogous to empirical research that analyzes whether the Cobb-Douglas or constant returns properties of matching functions typically hold (see e.g. Petrongolo and Pissarides, 2001 for a survey). These are important prerequisites for targeting the model to the data.

Kohlbrecher et al. (2016) show analytically and numerically that a labor selection model has the following properties. First, when the model economy is hit by aggregate shocks, it generates a positive comovement between market tightness and selection, which (up to a first order Taylor approximation) can be described by the following equation (see their Appendix B.1):

$$\frac{\partial \log \eta}{\partial \log \theta} = \frac{f(\tilde{\varepsilon})}{\eta}\left(\tilde{\varepsilon} - \frac{\int_{-\infty}^{\tilde{\varepsilon}} \varepsilon f(\varepsilon) d\varepsilon}{\eta}\right) = \frac{\partial \frac{\int_{-\infty}^{\tilde{\varepsilon}} \varepsilon f(\varepsilon) d\varepsilon}{\eta}}{\partial \tilde{\varepsilon}} \tag{C.1}$$

Kohlbrecher et al. (2016) show numerically for various idiosyncratic distributions (see their Figure 1) that the log-log-comovement between labor selection and market tightness is constrained to be in between 0 and 1.

Property 1: *The elasticity of the selection rate with respect to market tightness is bounded between 0 and 1:*

$$0 \le \frac{\partial \log \eta}{\partial \log \theta} \le 1 \tag{C.2}$$

Second, when a traditional contact function (with weight on vacancies larger than zero) and the selection mechanism are combined, the estimated elasticity between the job-finding rate and market tightness is larger than the estimated elasticity between the selection rate and market tightness.

Property 2: *The elasticity of the job-finding rate w.r.t. market tightness is at least as large as the elasticity of the selection rate w.r.t. market tightness:*

$$\frac{\partial \log jfr}{\partial \log \theta} \ge \frac{\partial \log \eta}{\partial \log \theta} \tag{C.3}$$

Intuitively, both the contact function and the selection mechanism generate a positive weight on vacancies. The estimation results in Table 1 show that these two model properties hold empirically. One contribution of our paper is to show that the time series evidence for our selection

proxy is in line with the model. This allows calibrating the model to time series data.

In addition, our used selection measure shows plausible patterns in the cross section. Lochner et al. (2021) show patterns for our selection measure as a function of the employment growth distribution. First, the selection rate is an increasing function of employment growth in the positive part of the employment growth distribution. In different words, firms that grow more select a larger fraction of applicants (i.e. they become less selective). This fact is in line with the selection model by Baydur (2017). Second, Lochner et al. (2021) show that churn and selection are connected. The more workers firms lose to other firms (via job-to-job transitions), the higher is their selection rate due to replacement hiring.

C.2 Partial Effects

We estimate the permanent upward-shift of the selection rate (controlling for business cycle effects and heterogeneity) which pins down the partial effect in our model. This highlights the advantage of our two-stage matching model: In a standard search and matching model, where the selectivity margin is absent, we could not calibrate to this dimension of the data. When simulating the partial effects, we compare various dimension of our model's reaction to Price (2019). He uses a completely different methodology and dataset to identify causal partial effects in a microeconometric sense. All of these effects are in a similar order of magnitude as ours.

First, as discussed in Section 6.1, analogous to our Figure 7, Price (2019) finds that the incremental partial effects of the Hartz IV reform increase with unemployment duration up to long-term unemployment. Second, the overall partial effect on unemployment is similar in Price (2019) and in our paper (roughly one percentage point in both cases). Third, Price (2019) 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 2.7 percentage points, which is smaller but close to Price's (2019) results. Fourth, 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 (2019) finds that those workers accept 4% - 8% lower wages on reemployment after the reform and conditional on jobless duration.[39]

C.3 Aggregate Effects

Klein and Schiman (2021) identify aggregate macroeconomic effects of different labor market and aggregate shocks in a structural VAR with sign restrictions. Their wage bargaining shock, which is closest to the Hartz IV reform that changed the fall-back option for long-term unemployed, leads to a cumulative effect on unemployment of 3.5 percentage points. Although this number is larger than our aggregate results (2.1 percentage points), we consider their results as corroborating evidence. Our approach does not include separation rate effects as in Hartung et al. (2020), which increase the aggregate effects on unemployment.

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

Furthermore, we compare our aggregate results to descriptive changes after the Hartz reforms. Obviously, these comparisons should be taken with a grain of salt, as the descriptives may be affected by other reforms, shocks, or trends. As shown in Section 6.2, our model-simulated Beveridge Curve and the Beveridge Curve in the data from 2005-07 show very similar dynamics. In addition, we compare the simulated change of the job-finding rate to the long-run movement in the data. The job-finding rate in our simulated model increases by 26.5%. On average, the (corrected) job-finding rate in our data is 21% higher in the post-Hartz compared to pre-Hartz period. However, the comparison is quite sensitive to the chosen time period. If we choose the same time period as Hartung et al. (2020) (i.e. 1993-2002 and 2008-2014), we obtain an increase of around 24%. 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. (2021) suggest that this was indeed an important adjustment channel.

Finally, we compare the relative movement of the job-finding rate for short-term to long-term unemployed (comparing 2005-2017 (post-reform) to 1998-2004 (before reform) for data availability reasons). Interestingly, the job-finding rate for short-term unemployed has gone up by about 17% more for short-term unemployed than for long-term unemployed. The aggregate increase of the job-finding rate in our simulated model for short-term unemployed is about 15% larger than for long-term unemployed. At first sight, this seems to stand in contradiction to the partial effect of the selection rate (which is larger for longer duration groups). However, it is not for two reasons: First, in the full equilibrium model, when the aggregate job-finding rate goes up, more workers are in unemployment groups with shorter duration. This composition effects pushes up the aggregate job-finding rate for short-term unemployed by more. A similar effect is absent for long-term unemployed in our model (which all belong to the same group). Second, long-term unemployed have a lower contact efficiency in our calibration. Therefore, they benefit less from the aggregate increase of the market tightness and their duration-specific contact rate due to the reform.

D Model Details and Robustness

D.1 Total Contacts vs. Contacts per Hire

Figure D.1 shows the response of the total number of contacts in the model economy and the number of contacts for each hire in response to a negative benefit shock in a model simulation.[40] The total number of contacts first goes up after the decline of benefits because workers'

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[40] 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.

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. 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 D.1: 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.
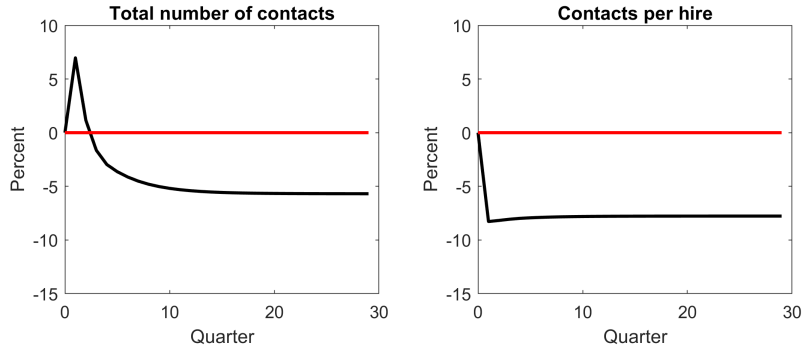
D.2 Equivalence of Productivity and Business Cycle Shocks

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[41] the model can be described by the following four equations:

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

$$\theta = (1-\alpha)\frac{p\eta}{\kappa}\left(\tilde{\varepsilon} - \frac{\int_{-\infty}^{\bar{\varepsilon}}\varepsilon f(\varepsilon)d\varepsilon}{\eta}\right) \tag{D.2}$$

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[41] 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 $w^I - \alpha\tilde{\varepsilon}$.

$$\eta = \int_{-\infty}^{\bar{\varepsilon}} f(\varepsilon) d\varepsilon \tag{D.3}$$

$$p = \mu \theta^{\gamma} \tag{D.4}$$

Using the implicit function theorem, it can be shown that

$$\frac{\partial \eta}{\partial b} = -\frac{\partial \eta}{\partial a} \tag{D.5}$$

and

$$\frac{\partial p}{\partial b} = -\frac{\partial p}{\partial a}. \tag{D.6}$$

It follows that:

$$\frac{\partial \hat{p}/\partial \hat{b}}{\partial \hat{\eta}/\partial \hat{b}} = \frac{\partial \hat{p}/\partial \hat{a}}{\partial \hat{\eta}/\partial \hat{a}}. \tag{D.7}$$

D.3 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 2004 (prior to the reform implementation), 68% of all West German employees in the private sector were covered by collective bargaining (see e.g. Elguth and Kohaut, 2005). 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 11% 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 D.2 shows that otherwise the model reaction is virtually unchanged.

D.4 Contact Efficiency Shock

Figure D.3 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. A higher contact efficiency leads to a larger market tightness, which improves workers fall-back option. Thereby, bargained wages increase and the selection rate falls.

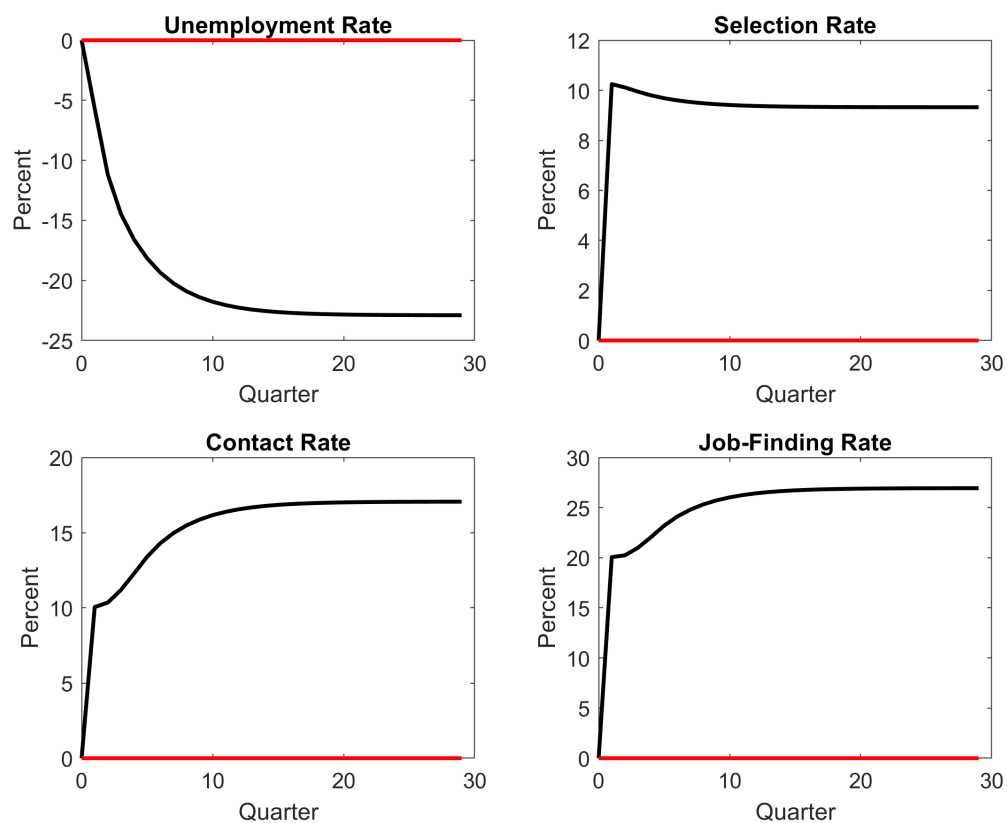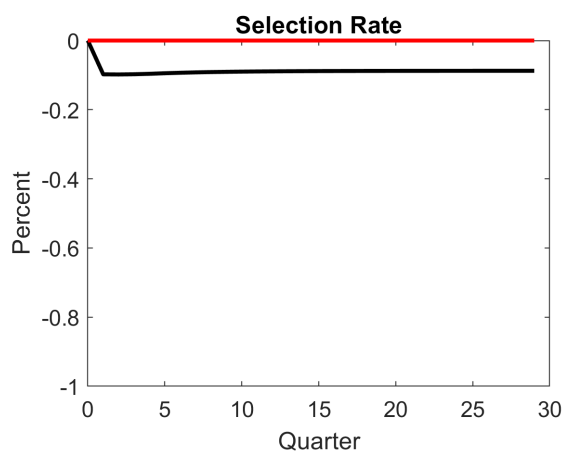**Figure D.2:** Impulse responses to a 23% drop of long-term unemployment benefits under collective bargaining.

Figure D.3: Response of the selection rate to a 1% positive shock to the contact efficiency.

E Robustness of Empirical Strategy

E.1 Robustness regarding the Partial Effect

Table E.1: Robustness: Further Controls

	Dependent variable:					
	log(selection_rate)					
	Female Hires	Share Female Unemp.	Lag(VA Growth)	Lag(Market Tightness)	Export Growth	Share Unemp. over 50
	(1)	(2)	(3)	(4)	(5)	(6)
Hartz IV-Dummy	0.13***	0.19***	0.15***	0.10**	0.13***	0.17***
	(0.04)	(0.07)	(0.04)	(0.04)	(0.04)	(0.06)
Value Added Growth	1.54*	1.22*	1.25**		3.61***	0.79
	(0.84)	(0.64)	(0.59)		(1.28)	(0.78)
Female Hires	0.75					
	(0.74)					
Female Unemp		0.88				
		(0.87)				
Lag(VA Growth)			0.02**			
			(0.01)			
log(Market Tightness)				0.15**		
				(0.07)		
Lag(log(Market Tightness))				-0.02		
				(0.08)		
Export Growth (yoy)					-0.80*	
					(0.41)	
Share Unemp. over 50						1.20
						(1.33)
Constant	-1.10***	-1.22***	-0.83***	-0.60***	-0.78***	-1.15***
	(0.32)	(0.44)	(0.05)	(0.07)	(0.04)	(0.41)
Observations	28	28	27	28	28	28
R^2	0.42	0.41	0.48	0.54	0.43	0.40
Adjusted R^2	0.34	0.34	0.41	0.49	0.36	0.33

Note: Estimation by OLS with robust standard errors. Due to data availability, the regression with the lag of value added growth covers the time span 1993 - 2019. *p<0.1; **p<0.05; ***p<0.01. The lags of value added and market tightness correspond to annual lags. For details on the data sources, see Table B.1 in Appendix B.

Table E.2: Robustness: Controlling for Market Tightness.

	panel linear		*OLS*	
			Dependent variable:	
			log(selection rate)	
	State Level	Industry Level	Aggr.: Low Qualification	Aggr.: Long-term U
	(1)	(2)	(3)	(4)
Hartz IV-Dummy	0.10***	0.09***	0.14***	0.12**
	(0.02)	(0.03)	(0.04)	(0.05)
log(Market Tightness)	0.11***	0.07***	0.07	0.13**
	(0.01)	(0.02)	(0.04)	(0.05)
log(Low Qualification)			0.40***	
			(0.15)	
log(Share long-term U)				0.35
				(0.21)
Constant			0.11	−0.27
			(0.27)	(0.21)
Observations	140	168	28	22
R^2	0.30	0.28	0.71	0.59
Adjusted R^2	0.26	0.25	0.67	0.52

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 - 2019. $^*p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$. For details on the data sources, see Table B.1 in Appendix B.

Table E.3: Robustness: Controlling for Market Tightness and Value Added Growth.

	panel linear		OLS	
	State Level	Industry Level	Aggr.: Low Qualification	Aggr.: Long-term U
	(1)	(2)	(3)	(4)
Hartz IV-Dummy	0.10***	0.09***	0.14***	0.12**
	(0.02)	(0.03)	(0.03)	(0.06)
log(Market Tightness)	0.11***	0.07***	0.06	0.13**
	(0.01)	(0.02)	(0.04)	(0.06)
Value Added Growth	0.01	−0.05	0.75	0.21
	(0.84)	(0.33)	(0.64)	(0.84)
log(Low Qualification)			0.40***	
			(0.14)	
log(Share long-term U)				0.32
				(0.23)
Constant			0.09	−0.30
			(0.26)	(0.25)
Observations	140	168	28	22
R^2	0.30	0.28	0.72	0.59
Adjusted R^2	0.26	0.25	0.67	0.50

Dependent variable: log(selection rate)

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 - 2019. $^*p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$. For details on the data sources, see Table B.1 in Appendix B.

E.2 Including East Germany

In this robustness check, we extend our sample to entire Germany (including East Germany). Table E.4 shows the estimation results. The coefficient on the Hartz IV-dummy in the specification with market tightness (column 3) is unaffected when including East Germany (0.11). 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.

Table E.4: Results for entire Germany (including East Germany)

	Dependent variable:			
	log(selection rate)			log(job-finding rate)
	(1)	(2)	(3)	(4)
Hartz IV-Dummy	0.15***	0.11***	0.11***	0.11***
	(0.03)	(0.03)	(0.04)	(0.03)
log(Market Tightness)		0.08**	0.09**	0.23***
		(0.04)	(0.04)	(0.03)
Value Added Growth	1.40**	1.03		
	(0.59)	(0.70)		
Constant	−0.79***	−0.64***	−0.61***	−2.64***
	(0.03)	(0.07)	(0.07)	(0.05)
Observations	28	28	28	28
R^2	0.54	0.65	0.63	0.86
Adjusted R^2	0.50	0.61	0.60	0.85

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

E.3 Correction of Unemployment Series

In 2005, there is a severe structural break in the unemployment series as a direct consequence of the reform. 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 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) and it reduces the market tightness (which is defined as vacancies divided by unemployment).

We correct for the structural break in terms of the level shift and use the corrected series for estimating dynamics over the business cycle.

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.[42] Figure E.1 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.[43]

It is also noteworthy that the estimated elasticities of the selection rate and the job-finding rate with respect to market tightness in Table E.5 are identical to our baseline estimation (see Table 1).

Table E.5: Estimation results without unemployment correction, 1992-2019

	Dependent variable:		
	log(selection rate)		log(job-finding rate)
	(1)	(2)	(3)
Hartz IV-Dummy	0.11***	0.11***	0.04
	(0.04)	(0.04)	(0.03)
log(Market Tightness)	0.13**	0.13**	0.26***
	(0.05)	(0.05)	(0.03)
Value Added Growth	0.74		
	(0.77)		
Constant	−0.61***	−0.60***	−2.64***
	(0.07)	(0.07)	(0.04)
Observations	28	28	28
R^2	0.55	0.54	0.82
Adjusted R^2	0.50	0.51	0.80

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

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[42] 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.

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

Figure E.1: Clearing for the break in the unemployment series, 1990Q1-2019Q4.
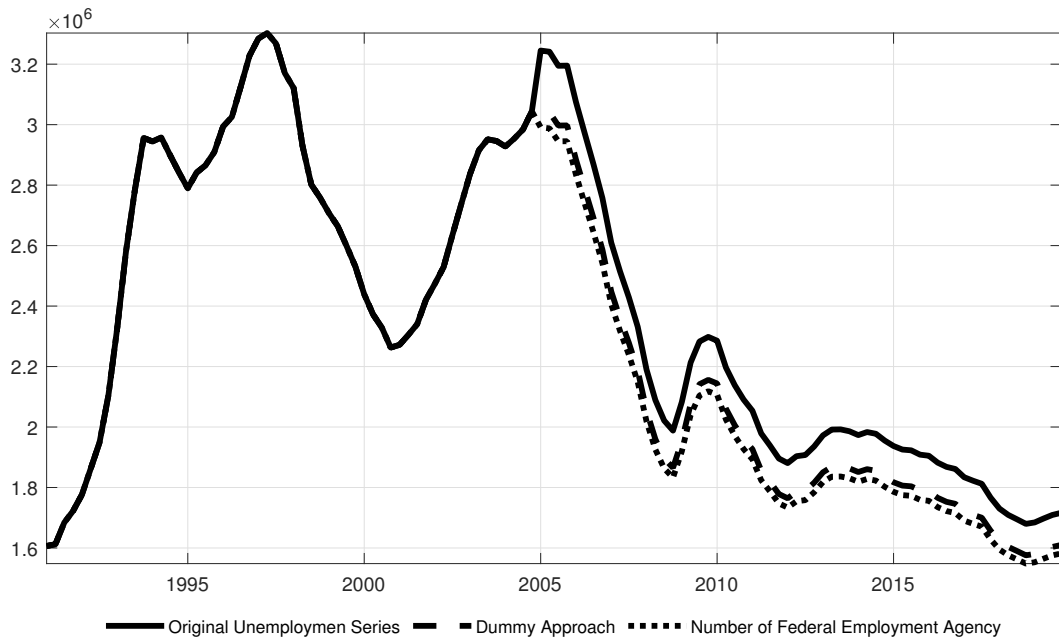
F Further Evidence

In addition, a closer look at the change of the selection rate by skill group from 2004 to 2005[44] 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.[45]

Figure F.2 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.

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

[45] 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.

Figure F.1: Increase of selection rate by skill group, 2004-2005
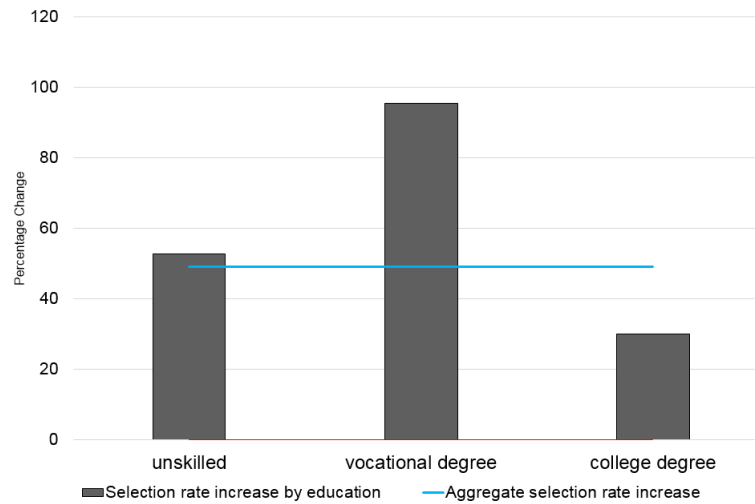

Figure F.2: The willingness of unemployed applicants to ... (% of establishments who gave the respective answer).

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