IMF Working Paper

The German Labor Market Reforms and Post-Unemployment Earnings

by Niklas Engbom, Enrica Detragiache, and Faezeh Raei

INTERNATIONAL MONETARY FUND

IMF Working Paper

European Department

The German Labor Market Reforms and Post-Unemployment Earnings

Prepared by Niklas Engbom, Enrica Detragiache, and Faezeh Raei[1]

Authorized for distribution by Enrica Detragiache

July 2015

Abstract

In 2003–05, Germany undertook extensive labor market reforms which were followed by a large and persistent decline in unemployment. Key elements of the reforms were a drastic cut in benefits for the long-term unemployed and tighter job search and acceptance obligations. Using a large confidential data set from the German social security administration, we find that the reforms were associated with a fall in the earnings of workers returning to work from short-term unemployment relative to workers in long-term employment of about 10 percent. We interpret this as evidence that the reforms strengthened incentives to return to work but, in doing so, they adversely affected post re-entry earnings.

Contents   Page

I. INTRODUCTION

After a decade of high unemployment and weak growth leading up to the turn of the 21th century, Germany embarked on a significant labor market overhaul. The reforms, collectively known as the Hartz reforms, were put in place in three steps between January 2003 and January 2005. They eased regulation on temporary work agencies, relaxed firing restrictions, restructured the federal employment agency, and reshaped unemployment insurance to significantly reduce benefits for the long-term unemployed and tighten job search obligations.

Figure 1. Unemployment Rate in G7 Countries
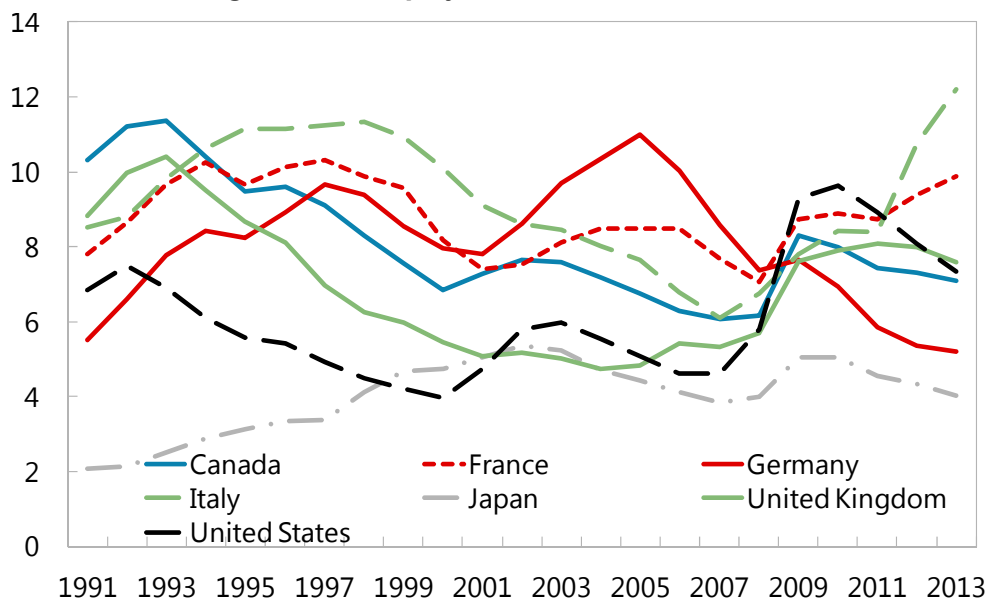
Source: Eurostat (harmonized ILO definition).

Subsequent to the reforms, the unemployment rate declined steadily from a peak of almost 11 percent in 2005 to five percent at the end of 2014, the lowest level since reunification. In contrast, following the Great Recession other advanced economies — particularly in the euro area — experienced a marked and persistent increase in unemployment (Figure 1). The strong labor market helped Germany consolidate its public finances, as lower outlays on unemployment benefits resulted in lower spending while stronger taxes and social security contribution pushed up revenues.

What should other countries do to emulate the German labor market success? Should they put in place reforms similar to the Hartz reforms, as suggested by some experts (e.g., Kirkegaard, 2014)?

To answer these questions it is important to gain a thorough understanding of the effects of the Hartz reforms on the German labor market. The purpose of this paper is to contribute to this effort by documenting how the earnings of workers returning to work from unemployment changed following the reforms. Specifically, we will show that the 'cost of displacement' — the reduction in earnings post-unemployment — significantly increased following the reforms (controlling for worker observables and unobservables as well time trends). We interpret this as evidence that the Hartz reforms succeeded in their goal of strengthening incentives for the unemployed to take up work. The large decline in re-entry earnings, however, indicates that the reforms also made becoming unemployed more onerous for individuals.

To provide a pre-view of the data, Figure 2 plots the normalized log earnings of two groups of workers: those who at some point in the previous 12 months claimed unemployment benefits (displaced workers) and those who remained continuously employed. To ease comparison, each series is normalized to average to zero over the 1992–2002 period. Until 2003, the earnings of the two groups closely tracked each other, but then started deviating sharply, with earnings of displaced workers falling sharply especially in 2005 and remaining lower through the end of the sample period in 2010. This provides *prima facie* evidence that the reforms had an important effect on the labor market outcomes of the unemployed.

This evidence is explored more rigorously in the rest of the paper. Specifically, using a regression framework, we show that before the reforms the cost of displacement was about 20 percentage points after controlling both for observable and unobservable individual differences and other factors. After the reforms, the penalty increased by another 10 percentage points. If workers re-entering the labor market as part-time workers are also included, the penalty becomes even larger. We also show that the result is robust to a number of changes in the methodology.

An important caveat is that we cannot reliably identify which element of the reform package was responsible for its effects, though this would be very interesting to policymakers. This is particularly difficult because the various elements of the Hartz package were designed to complement one another and were introduced within a relatively short period of time. Tests relying on the precise timing of the policy changes would be polluted by anticipatory effects and, possibly, reaction lags, as workers and firms learned about the new system.

The paper is related to various strands of literature: (i) empirical studies documenting sizable and persistent earnings losses for displaced workers returning to work (for example, Jacobson et al., 1993, Couch and Placzek, 2010, Davis and von Wachter, 2013); (ii) empirical studies of the effects of unemployment benefits on workers behavior (for a recent contribution, see Hagedorn, Manovskii, and Mitman, 2015); (iii) theories of high unemployment in Europe; (iv) and studies

of the effects of the Hartz reforms on the German labor market. We briefly review the latter two strands in the next section.

Figure 2. Normalized Earnings of Workers in Stable Employed (Blue) and Displaced Workers (Red)
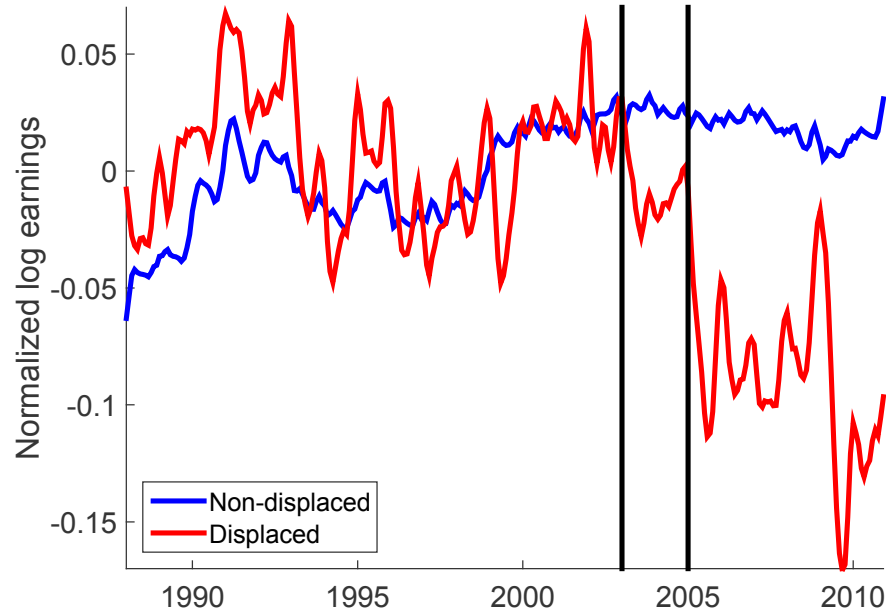
Note: Full-time employed males age 25-62. Displaced workers had spent at least three years in continuous full-time employment prior to unemployment, transited from a full-time job to unemployment at some point 12 months ago and are currently full-time employed. Non-displaced workers have spent at least four years in full-time employment. Log monthly real labor income (12-month moving average).
Sources: Sample of Integrated Labor Market Biographies (SIAB).

The paper is organized as follows. The next section provides some background on the employment and wage developments in Germany, describes the main elements of the Hartz reforms, and provides a brief review of the literature on the effects of the Hartz reforms. Section 4 introduces our data and empirical strategy. Section 5 presents results, and section 6 concludes.

II. BACKGROUND

A. The Rise and Fall in Germany's Unemployment

The German economy has gone through a salient transformation in the last decade, especially with regard to its labor market. As in other European countries, beginning in the mid-1970s there was a gradual rise in the unemployment rate as sharp increases during cyclical downturns were only partially reversed in the subsequent recoveries (Figure 3). This long-term upward trend was reversed at the time of completion of the Hartz reforms in 2005, when the unemployment rate began to decline steadily, pausing only briefly during the sharp (but

short-lived) 2009 downturn. As Figure 4 shows, the period of the Hartz reforms coincided with a prolonged phase of negative growth in average wages.

Figure 3. Unemployment Rate and GDP Growth
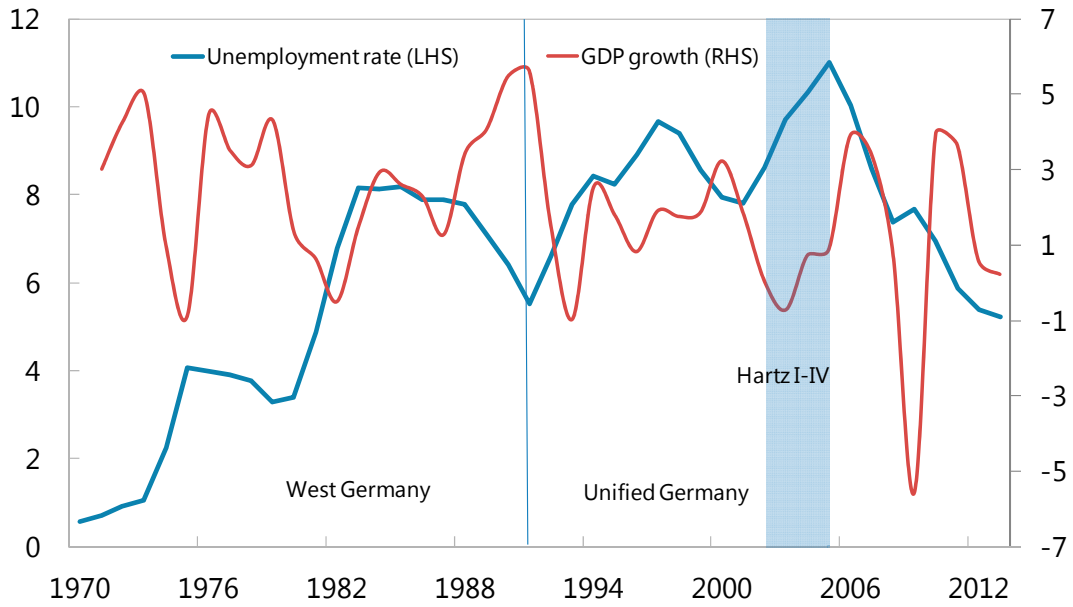
Source: Federal Statistical Office (National and ILO definition).

Figure 4. Compensation per Employee (year over year growth)
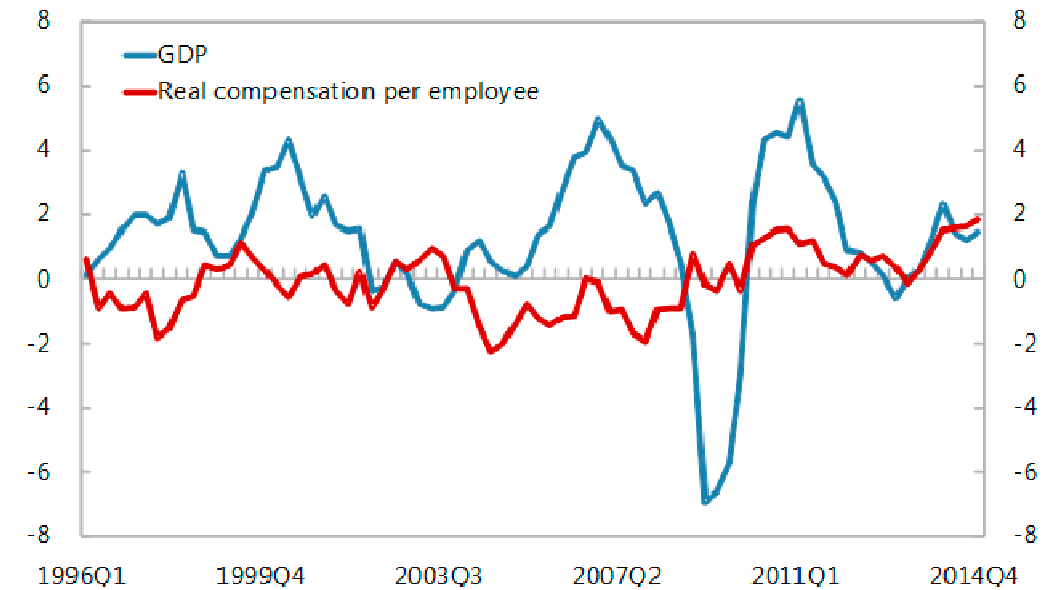
Source: Haver Analytics.

An early literature proposed several theories to explain the rise in European unemployment in the 1970s and 1980s. Some authors focus on labor demand factors, such as high hiring and firing costs (Bentolila and Bertola, 1990) or insider-outsider dynamics (Lindbeck and Snower, 1988). [2] Ljunqvist and Sargent (1998), on the other hand, emphasize labor supply factors: in their model, shocks to the economy create the need for workers to move across sectors. This leads to a loss of job-specific skills, so that displaced workers can find work only at reduced wages. With generous long-term unemployment benefits linked to past-earnings, however, staying unemployed is the more attractive so the unemployment rate stays high even after the economy recovers. An implication of this paper is that to reduce unemployment it is necessary to provide less generous benefits to the unemployed because their wages need to adjust downwards. The model is consistent with the large cost of displacement observed by Jacobson et al. (1993) and others for U.S. workers, and interprets the penalty as reflecting a skill loss from structural changes.

To any policy-maker inspired by the Ljunqvist-Sargent theory, Germany's system of unemployment insurance, as it stood before the Hartz reforms, must have seemed particularly problematic: while the benefits for short-term unemployed were in line with those of the average of OECD countries, those for the long-term unemployed were extremely generous (Figure 5).

Faced with low growth and mounting unemployment with the 2001-03 recession and famously dubbed by the Economist magazine "The Sick Man of Europe", Germany set out to drastically reshape its approach to unemployment insurance.

---

[2] Mortensen and Pissarides (1999) attributes differences in unemployment rates (and wage inequality) between European and other OECD countries to skill-biased technological progress, high unemployment benefits, and large firing costs. Blanchard et al. (1997) links the high European unemployment in the 70s to adverse labor supply shifts (lack of adjustment of wages to declining productivity) and to subsequent labor demand shifts in the 80s (technological bias against labor) that led to an increase in capital share and continued rise in unemployment.

Figure 5. Net Replacement Rates for Short- and Long-Term Unemployment in Germany and the OECD (percent of previous earnings)
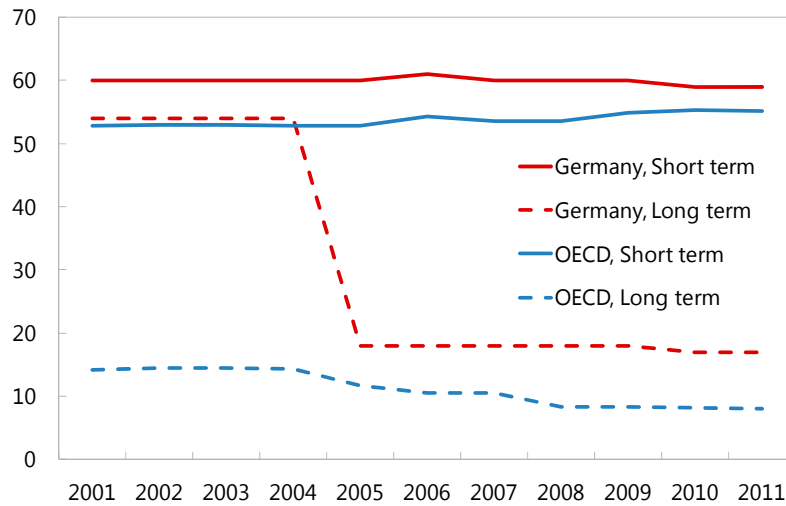
Note: The replacement rates correspond to that of a household earning the average income. OECD number is the simple average across all OECD countries. Short-term refers to initial phase of benefits, the length of which may vary across country.
Source: OECD Tax Benefit Models.

B. The Hartz Reform Package

The Hartz I–IV legislative package was approved in 2002–03 and implemented gradually from January 2003 to January 2005 (Table 1). The first three stages of the reforms (Hartz I–III) sought to improve job search efficiency and employment flexibility. They included deregulation of the temporary work sector to give individual employers more flexibility to vary employment levels without incurring hiring or firing costs, as well as a restructuring of the federal labor agency in order to improve training and matching efficiency of job searchers. The final set of reforms (Hartz IV), implemented on January 1, 2005, entailed a major restructuring of the unemployment and social assistance system that considerably reduced the size and duration of unemployment benefits and made them conditional on tighter rules for job search and acceptance.

Table 1. Brief Description of Hartz Reforms in Germany

Law	Adoption of law	Effective date	Measures
Hartz I	Dec 1, 2002	Jan 1, 2003	Setting up of new Personnel Service Agencies Support for further vocational education from the German Federal Labor Agency Deregulation of temporary work sector
Hartz II	Dec 1, 2002	Jan 1, 2003 and April 1, 2003	Introduction of subsidy for one-person companies (Me-inc); Introduction of low paid jobs (mini and midi-jobs) exempt from most social security taxes Threshold size for firms subject to layoff rules raised from five to ten workers
Hartz III	Dec 1, 2003	Jan 1, 2004	Restructuring of the Federal Labor Office
Hartz IV	Dec 1, 2003	Jan 1, 2005	Shortening of the duration of unemployment benefits. Merging of unemployment assistance and social assistance, with benefit set at the lower level of social benefits (unemployment benefit II); A new definition of acceptable jobs with sanctions for refusal of an acceptable job.

Sources: Eichhorst and Marx (2011), Dlugosz and Wilke (2013).

Before the reforms, the German unemployment insurance system consisted of three layers. The first layer, unemployment benefits (UB), was available to workers who had acquired a sufficient number of working years prior to unemployment. Workers in UB received a benefit equal to 60 percent of their previous net earnings (67 percent for parents with dependent children). For workers younger than 45 the benefit was limited to 12 months, while older workers were eligible for up to 32 months. Workers who remained unemployed after exhausting UB were eligible for the second layer of support, unemployment assistance (UA), with a replacement rate of 53 percent of previous net earnings (57 percent for parents with dependent children). [3] UA benefits could be claimed indefinitely subject to a means test and an annual review. The final step of support was social assistance (SA), a means-tested lump-sum transfer available for those who did not qualify for UB or UA. It provided the least

---

[3] Prior to 2000, someone could receive UA without previous eligibility for UB, but this was abolished in 2000.

generous support.[4] This system resulted in very generous benefits for the long-term unemployed compared to other advanced countries.

The reforms collapsed the system of benefits into two layers. The first layer, unemployment benefit I (UB I), was in practice UB relabeled. The replacement rate was unchanged and for many workers the duration of the benefits was left intact at 12 months (some older workers saw a reduction in the duration). The major change was the introduction of unemployment benefit II (UB II), which replaced the previous UA and SA. The structure of UB II was essentially that of SA: it was a means-tested, lump-sum benefit paying an amount similar to the old SA. Therefore, after the reforms most workers who would have qualified for UA under the old system would experience a drastic cut in benefits if they remained unemployed after running out of the short-term benefit.

The literature on the effects of the Hartz reforms

There is a considerable literature studying the consequences of the Hartz reforms. Krause and Uhlig (2012), Krebs and Scheffel (2013), and Launov and Waelde (2013) calibrate macroeconomic search models to the German economy and simulate the effect of the reforms. All three studies conclude that the cuts in unemployment benefits introduced by the Hartz reforms significantly reduced unemployment. Fahr and Sunde (2009), Klinger and Rothe (2012) and Hertweck and Sigrist (2013) estimate matching functions using aggregate time series and find important positive effects of the earlier parts of the reforms (Hartz I and III) on matching efficiency.

Using administrative data, Dlugosz et al. (2014) find a considerable decline in transition rates from employment to unemployment after the reforms, particularly for older workers, whose benefits were cut more markedly. Arent and Nagl (2011) test for a structural break in wage equations in the reform years, and argue that average wages fell after Hartz. Giannelli et al. (2013) study how the quality of new jobs, including job duration and wages, evolved in Germany in 1998-2010. In this context, they show that the median wage of workers re-entering from unemployment declined in the post-Hartz years.

While these papers support the view that the Hartz reforms had important effects, Dustmann et al. (2014) provide and alternative explanation of the German labor market success and explicitly challenge the conventional wisdom that Hartz played a key role. According to this perspective, the threat of off-shoring jobs to recently opened-up central and eastern European countries in the early 1990s together with the decentralized nature of employer-union negotiations allowed German firms to successfully push for limited wage growth, thus

---

[4] UB or UA benefits below the SA benefit level were topped up.

improving competitiveness. They also argue that greater flexibility in wage bargaining and in employment contracts helped German firms better weather the Great Recession.

In this paper, we use a large, administrative data set with daily information on changes in employment status and associated earnings. We ask: did the reforms reduce the re-entry earnings of workers who lost their job? Evidence of an increased "displacement penalty" after the reforms would be consistent with frictional models of the labor market such as Mortensen and Pissarides (1994) and Ljunqvist and Sargent (1998). These models predict that lower unemployment benefits reduce the reservation wage of workers searching for a job and lower post-unemployment earnings.

III. DATA AND EMPIRICAL STRATEGY

A. Data

We use the Sample of Integrated Labor Market Biographies (SIAB), a two percent random sample from German administrative social security records (sampled to preserve the panel structure). The data cover 1975 to 2010 (starting in 1992 for former East Germany) and include all workers who are subject to social security contributions (i.e., it excludes the self-employed, civil servants, and those serving in the military). People in "mini-jobs" — social security exempt jobs paying less than €400 a month — are included starting in 1999. Overall, the data covers approximately 80 percent of the German workforce.

Previous studies of German labor markets and the Hartz reforms have primarily relied on the German Socio-Economic Panel (GSEOP), a household-level panel survey similar to the U.S. Panel Study of Income Dynamics. For several reasons, the SIAB is better suited than the GSOEP for the purposes of this paper. First, the GSOEP is conducted annually, whereas many unemployment spells last less than a year.[5] In the SIAB, labor market transitions and associated income changes are reported on a daily basis, thus minimizing the risk of misclassifying labor market flows.[6] Secondly, firms report on behalf of their employees (under the threat of punishment by law) and unemployment agencies report for unemployed workers, likely reducing measurement error compared to household survey data. Thirdly, the GSOEP contains approximately 2,000 households each year. For studying transitions in and out of unemployment at the monthly level, this is barely sufficient, and conditioning on covariates is near impossible. In contrast, the SIAB contains almost 100 times as many

---

[5] Although the GSOEP asks for labor market status in each of the previous 12 months, this is arguably subject to significant recall bias. Moreover, income variables are only recorded at the annual level.

[6] For instance, with annual data we might misclassify an employment-unemployment-employment transition as a job-to-job transition if the intermediary unemployment spell falls entirely within the year.

observations, enabling a precise empirical analysis conditional on background variables.

The main drawbacks of the SIAB are that earnings are right-censored at the social security contribution limit and that information on hours worked is limited to full-time/part-time. Although our study focuses on workers returning from unemployment who tend to earn less than the social security cutoff, we contrast these workers with workers in steady employment, who are more affected by top coding. To address this, we follow approaches commonly used in U.S. survey data to impute censored wages. To deal with the lack of information on hours worked, we primarily focus on workers in full-time employment. The robustness section further investigates the sensitivity of our results to these issues.

B. Sample Selection and Variable Definitions

We restrict attention to male workers aged 25 and older to avoid issues related to secular and life-cycle changes in female labor force participation as well as issues related to the timing of entry into the labor market.[7] In order to attain as consistent as possible a data set, we use only data from states in former West Germany. We use data from 1988 to 1991 to condition on previous employment history and earnings, and start our main analysis after reunification in 1992. The last year of the sample is 2010. We set the pre-reform period to 1992–2002 and the post-reform period to 2005–2009 (because we use data from t to t + 11 to construct earnings at t, this uses also data from 2003 and 2010). As previously mentioned, we make no attempt to separately identify the impact of each of the four Hartz reforms based on their different implementation years, as anticipation effects and phase-in provisions in some of the measure would likely confound the effects.

A problem with the SIAB is that the Hartz reforms changed how information on long-term unemployment benefit recipients was collected. This resulted in a temporary glitch in the system, which impaired the data on long-term unemployment beneficiaries during 2005–2006 to the extent that they essentially cannot be used during this period. We circumvent this data limitation by conditioning only on employment or short-term unemployment.

The SIAB reports changes in employment status on a daily basis, as well as average daily gross nominal labor income, calculated as the annual gross income paid by the employer divided by the number of days worked at that job. The number of days worked is based on the reported start and end date of the employment relationship. For computational purposes, we convert the data to monthly frequency by calculating the number of days a spell is active and the associated total earnings during the month. As multiple spells may be active in the same month, we define a unique observation for a month using the following selection

---

[7] Workers older than 62 are excluded from the sample since they are not covered by the SIAB.

criteria:

1. We drop non-employment spells if an employment spell is active;
2. If one or more employment spells are active, we define as the unique observation of that individual-month the spell paying the highest amount in that month;
3. If no employment spell is active, we define a worker as short-term unemployed if he/she receives UB before the reforms or UB I after the reforms, and as long-term unemployed if he or she receives UA before the reforms or UB II after the reforms (but did not receive UB/UB I);
4. Finally, we define a worker as not in the labor force if there is no valid observation in that month.

Subsequently, we convert earnings to real 2013 values using the CPI and take the logarithm of this value. Finally, we assign as our earnings measure in month t the average log monthly real earnings from t to $t + 11$. This reduces the effect of any initial decline in earnings post displacement that is quickly recovered (the robustness section further investigates this), as well as noise in our earnings measure.[8]

As noted above, earnings are subject to right-censoring at the social security contribution limit. To deal with this, we follow approaches commonly used to impute top-coded values in U.S. survey data. We assume that the right tail of the distribution of log earnings follows a Pareto distribution and estimate its shape using the top decile of non-top coded earnings. Subsequently, we assign top coded values the conditional mean above the top coded threshold. We have also experimented with either assigning top coded values a random draw from the estimated Pareto distribution, keeping top coded observations at their top coded value, and dropping all top coded observations. Neither approach has any material effect on our baseline results, but as we discuss later top coding remains an issue when we analyze the impact of the reforms within occupations or sectors.

Unemployment spells are reported from a different agency than employment spells, and these reports do not contain information about education. We define the education of an individual to be constant over his career, and equal to the maximum reported education level. Similarly, we cannot tell the geographic location of an unemployed worker. Hence we define a worker to be in the East if any of his employment records stems from a state who was part of former East Germany.

---

[8] With earnings data averaged across overlapping time periods, regression residuals are serially correlated by construction, but this is not a problem since we are clustering residuals at the level of the individual worker.

We define someone as recently displaced in month t if he satisfies the following conditions:

1. Was full-time employed at time t − 13 and had been so for at least 36 months;
2. Was short-term unemployed at time t − 12;
3. Is full-time employed at time t but at a different firm than at t − 13.

Thus, the definition classifies as recently displaced at t all workers who experienced unemployment for at least one month between t and t-12 and were back at work at t+13. A worker is classified as non-displaced at time t if he has been employed full-time in the previous four years. Evaluating earnings 12 months after displacement strikes a balance between giving displaced workers some time to return to work and the short data series we have available after the reforms (the robustness section discusses this further). In a robustness test we alter the selection rule and examine workers who reenter the labor market after 24 months. The restriction on re-entering at a different firm eliminates seasonal workers.

The SIAB data do not contain information on the reason for separation, and the fact that the worker was receiving unemployment benefits does not completely rule out voluntary separations as in Germany reduced benefits are available to workers who quit. The restriction to workers with a significant previous work history, which follows the literature on the cost of displacement (Jacobson et al., 1993), arguably maximizes the chances that these workers indeed separated involuntarily. Another possible source of misclassification are workers who switch jobs in anticipation of being laid off, or laid off workers who return to full-time work within a month. These workers are not classified as non-displaced under our definition. These sources of misclassification would bias our results towards finding no impact of the reforms, so our estimates of the cost of displacement can be viewed as lower bounds.

An additional advantage to restrict attention to workers with long employment history is that they were all eligible for at least 12 months of short-term unemployment benefits, which allows us to define displacement based on a claim for short-term unemployment benefits without worrying that this introduces significant compositional bias in who we define as displaced (as might be the case if only some were eligible for short-term benefits). The robustness section loosens the previous work requirement restriction (and finds a very similar effect of the reforms).

Although a reduction in hours worked after displacement might be considered a loss from displacement, earnings changes due to fewer hours worked is arguably distinct from changes due to lower compensation per unit of time. Given that we do not have access to hours worked, we cannot construct a measure of compensation per unit of time worked. Hence we focus on full-time employees only. The robustness section documents an increase in the probability of returning to part-time employment after the reforms, but the proportion of part-

time workers remains small also after the reforms.

C. A Regression Framework to Identify the Impact of the Reforms on Earnings

We investigate the impact of the Hartz reforms by comparing the earnings of recently displaced workers to those of non-displaced workers. As long as any shocks to the economy affected both groups similarly, this strategy allows us to differentiate the impact of the Hartz reforms from other changes in the economic environment. To the extent that the reforms exerted some wage pressure also on workers in stable employment, for instance by expanding aggregate labor supply or reducing the bargaining power of non-displaced workers, our estimates provide a lower bound on the effect of the reforms on post-unemployment earnings. We further address concerns that the assumption of parallel trends is violated below.

Let earnings_{it} be average log monthly real earnings over month t to $t + 11$, let Disp_{it} indicate whether the worker is recently displaced (as defined above), and let Hartz_t take value one if the year is 2005–2009, and zero if it 1992–2002.[9] Consider the regression of average log monthly real earnings on the displacement dummy, the post-reform dummy and their interaction:

$$\text{earnings}_{it} = \beta \mathbf{X}_{it} + \gamma_0 \text{Disp}_{it} + \gamma_1 \text{Hartz}_t + \gamma_2 \text{Disp}_{it} * \text{Hartz}_t + \varepsilon_{it}$$

where $\mathbf{X}_{it}$ is a set of controls that we discuss further below. We estimate this regression by ordinary least squares (OLS) with standard errors clustered at the individual level. Given the well-documented cost of displacement in the literature, we expect γ_0 to be negative (Jacobson et al., 1993, Couch and Placzek, 2010). In addition, we hypothesize that γ_2 is negative, i.e. that the adverse effect of displacement on earnings is stronger after the Hartz reform, as the cut in long-term unemployment benefits and the tightening of job search requirements induced workers to accept lower-paying jobs.

We gradually increase the number of controls in $\mathbf{X}_{it}$ to first only include a constant, then education dummies (three), a cubic in age, linear interactions between age and each of the education dummies, interactions between each education group and the displacement dummy, and a linear interaction between age and the displacement dummy. This controls for differences in earnings between displaced and non-displaced workers along observable dimensions. Subsequently, we control for compositional changes over time along observable and unobservable dimensions as well as differences in secular and business cycle trends for displaced workers. We do so by including an individual's

---

[9] To construct earnings at t we need data from t to $t + 11$, so we use also 2003 and 2010.

average log monthly real earnings over months $t − 13$ to $t − 48$ as a control for unobservable differences in earnings power, and separate time trends for each education group, a linear interaction between age and time, a separate linear time trend for displaced workers, and a linear interaction between quarterly GDP growth and the displacement dummy. We also include year and month dummies (the Hartz dummy is excluded as it would be co-linear with the year dummies). Finally, in an alternative specification we also control for the occupation or sector of employment. There are 120 occupations and nine sectors in the SIAB.

Although this empirical framework allows us to control for changes in the composition of the displaced along the observable or unobservable dimensions, it cannot be excluded that the two groups could have changed in other ways around the time of the reforms. A particular worry is that after the reforms employers may have had a greater ability to separate workers who received a bad shock to their individual productivity, possibly as a result of a loosening of firing restrictions. To get a better, more exogenous measure of displacement we would ultimately like to follow the approach of Jacobson et al. (1993) to focus on a "mass-layoff" subsample. However, our current data set does not allow us to do so because it lacks firm identifiers.

IV. RESULTS

We first show summary statistics for displaced and non-displaced workers before and after the reforms. Secondly, we present results from our regression framework comparing earnings of the two groups before and after the reform.

A. Summary Statistics

Table 2 presents summary statistics for displaced and non-displaced workers for the period prior to the reforms, 1992–2002, and the period after the reforms, 2005–2009.[10] The sample contains 27 million individual-month observations for almost three hundred thousand individuals. The group of displaced workers is younger than the non-displaced by about four years before the reforms. The former is about two years older after the reforms and the latter about a year older. The non-displaced group has a higher fraction of university graduates (9.9 versus 6.5 percent) and both groups are better educated after the reforms. A significant fraction of the sample is top coded — more than 20 percent of non-displaced and 11 percent of displaced workers — where we define an observation as top-coded at t if monthly earnings are top coded in any month t to $t + 11$ or $t − 48$ to $t − 13$ (as we explain below earnings in

---

[10] Since we use data for months t to $t + 11$ to construct the measure of earnings at date t, our pre-Hartz group includes data through 2003 and our post-Hartz group data through 2010.

these months are used in our baseline regressions). The earnings of the displaced are more than 30 log points lower than those of the non-displaced before the reforms; this difference widens to 40 log points after the reforms. In the next section we employ a regression framework to investigate whether this relative drop in earnings of the displaced after the reforms remains after controlling for compositional changes.

Table 2. Summary Statistics Before and After the Reforms

	Before Hartz		After Hartz	
	Displaced	**Non-displaced**	**Displaced**	**Non-displaced**
Age	38.22	42.59	40.69	43.93
Upper secondary (%)	17.79	15.39	14.55	13.36
University (%)	6.54	9.85	10.85	13.74
Average monthly earnings (logs)	7.86	8.18	7.78	8.20
Fraction top coded (%)	11.24	22.97	11.19	21.35
Individual-Months	11,236	17,893,823	5,209	7,750,372
Individuals	10,922	240,475	5,182	171,066

Note: Weighed by individual-months. An observation is top coded at t if earnings are top coded at any point t to $t + 11$ or $t - 13$ to $t - 48$.

B. The Impact of the Reforms on Earnings

Table 3 presents results from estimating our main difference-in-difference earnings regression. All standard errors are clustered at the individual level. Unless otherwise noted, all estimates are statistically significant at any reasonable level of confidence.

Column 1 shows results without any controls. The earnings of non-displaced workers are almost two log points higher in the post-reform period. Recently displaced workers, on the other hand, are paid markedly less: the raw earnings differential is almost 32 log points prior to the reforms. Moreover, this increases a further 10 log points after the reforms. Although all estimates are statistically significant at all reasonable levels, the small R-squared value indicates that displacement alone cannot explain much of the variation in earnings in the data.

Column 2 shows results controlling for demographics, where we include in $\mathbf{X}_{it}$ three education dummies, a cubic in age, linear interactions between age and each education dummy, interactions between the education groups and the displacement dummy, and an interaction between age and the displacement dummy. The displayed coefficients are for a worker with the median age and education. We confirm standard findings in the literature that earnings are increasing in age but at a declining rate. Those that went to college earn significantly more. Controlling for changes in composition, earnings of non-displaced workers are lower after the reforms by about two log points. Prior to the reforms, displaced workers earned about 28 log points less than workers in long-term

employment. Although smaller than without demographic controls, this differential is still sizable. The cost of displacement is larger for older workers and decreasing in education. The estimate of the impact of the reforms on the earnings of recently displaced workers remains largely unchanged at about 10 log points. The R-squared of the regression is .26, confirming standard findings in the literature that even flexible controls for worker observables cannot explain much of the variation in earnings.

In column 3, we also include past earnings (measured 1–4 years prior to the observation), separate linear time trends for each education group, a linear interaction between age and time, a separate linear time trend for displaced workers, an interaction between quarterly GDP growth and the displacement dummy, and year and month dummies. This additional variables control for changes in composition along observable and unobservable dimensions and allow for a secular trend in the cost of displacement as well as fluctuations over the business cycle. All estimates are for a worker with the median education and age evaluated at the middle date of the sample period and at the (unweighed) average growth in GDP. In the new specification, the explanatory power of the regressions is greatly increased: the R-squared value jumps to .86. Several of the covariates reverse sign: for instance, controlling for past earnings, earnings are lower for older workers, indicating greater earnings growth for young people. Earnings are highly autocorrelated: the coefficient on past earnings is .995. The estimate of the separate time trend for the displaced is positive but economically small: on average earnings of the recently displaced grow by .3 log points a year relative to the non-displaced. The estimate of the difference in the effect of the business cycle is statistically insignificant. Interestingly, the estimated loss from displacement remains sizable (almost 22 log points) prior to the reforms, while the increase in this loss after the reforms remains close to 10 log points as in the other regression specifications.

Column 4 presents results excluding top coded observations. This reduces the sample by 22 percent of all individual-month observations and 14 percent of all individuals. The autocorrelation of earnings drops and so does the R-squared value of the regression. Although the degree of top coding is fairly high (particularly among the non-displaced), our results excluding top coded observations suggest that top coding does not affect our results, as the coefficient of interest changes very little.

Table 3. Earnings Regression

	(1)	(2)	(3)	(4)
Hartz	.01775*	-.02264*		
	(.00097)	(.00086)		
Disp	-.31666*	-.28135*	-.22213*	-.20959*
	(.00387)	(.00452)	(.02807)	(.02860)
Disp x Hartz	-.09645*	-.10378*	-.09703*	-.10211*

	(.00801)	(.00707)	(.01124)	(.01160)
Disp x time			.00029*	.00031*
			(.00008)	(.00008)
Disp x GDP growth			-.00250	-.00114
			(.00315)	(.00322)
Average past earnings			.99469*	.96612*
			(.00060)	(.00094)
Demographic controls	No	Yes	Yes	Yes
Past earnings and time	No	No	Yes	Yes
Include top-coded	Yes	Yes	Yes	No
Occupation controls	No	No	No	No
Sector controls	No	No	No	No
R squared	.0009	.2610	.8657	.7624
Individual-months	25,660,640	25,660,640	25,660,640	19,901,846
Individuals	280,742	280,742	280,742	240,309

* p-value < .001; standard errors clustered at the individual level; displayed estimates
are for a median-age, median- education worker evaluated at the middle date of the
sample at average GDP growth.

Our German data thus confirm findings in the literature of a significant decline in earnings after displacement among workers with a long previous work history (Jacobson et al. 1993). Moreover, they indicate that the loss from displacement increased markedly after the Hartz reforms.

Additional tests

We conduct three additional robustness exercises, presented in Table 4. First, we include also workers who return to types of employment other than full-time. Secondly, we investigate the degree of catch up in earnings after re-entry from displacement. Finally, we loosen the requirement that, to be included in the sample, a worker has to have been full-time employed for at least three years prior to unemployment.

Column 1 presents regression results including also workers who return to part-time or vocational training after unemployment.[11] This neither affects our estimates of the cost of displacement prior to the reforms nor the increase in this cost after the reforms.

Next, we investigate whether post-unemployment earnings workers recover over time. In their seminal paper Jacobson et al. (1993) find an immediate loss of 40 percent of earnings and a long-term impact of 25 percent after six years for workers displaced in the United States.

---

[11] We do not include those who return to mini-jobs since data for these workers are only available from 1999 onwards (results including them for the later years are similar, though).

Couch and Placzek (2010) estimate smaller but persistent losses of about 30 percent in short term and 15 percent after six years, also using U.S. data. Schmieder et al. (2010) show that the cost of displacement in Germany in the 1982 recession was significant and lasted for at least 15 years.

To explore this issue with our sample, column 2 averages earnings over a three-year period instead of a one-year period. If earnings quickly recover after unemployment, we would expect the estimated cost of displacement averaged over this longer time horizon to be smaller. This is in fact the case, as the estimated cost of displacement is now 14.5 log points compared to 22.2 in the baseline regression. However, in line with previous findings in the literature, this recovery is slow. The estimated impact of the reforms is reduced to seven log points from 10.[12]

Column 3 loosens the previous work requirement to one year instead of three. This has only a marginal impact on the estimated loss from displacement prior to the reforms and the additional loss after the reforms. Column 4 changes the selection rule to look at workers who are back in work after two years (rather than one year) from the month in which they entered unemployment. In this variant, both the cost of displacement is somewhat smaller and its increment after the Hartz reform are a somewhat smaller than in the baseline, but still important.

Finally, columns 5–6 include controls for occupation-year and sector-year, respectively (120 occupations and nine sectors). By controlling for the average earnings in the occupation or sector in the year, we filter out declines in post-displacement earnings due to the fact that a displaced worker switches into an on average worse-paid occupation or sector. Our estimates of the cost of displacement, however, are only marginally affected, both before and after the reforms. Thus, we conclude that it does not appear that the fact that workers return to lower-paying occupations or sectors plays a major role in driving the cost of displacement.

---

[12] Because we now need three years of subsequent data for each observation, for this regression we exclude 2008–2010 from the estimation. Excluding this period from the previously reported baseline regression does not affect our earlier results, so the observed differences are not due to difference in the sample period.

Table 4. Robustness Earnings Regression

	(1)	(2)	(3)	(4)	(5)	(6)
	All Employment Types	3-year Average	Prior employment 1-year	Re-employment after 2 years	Controlling for occupation-year	Controlling for sector-year
Disp	-.22118*	-.14556*	-.25924*	-.14274*	-.22013**	-.22583**
	(.03002)	(.02679)	(.02439)	(.02874)	(.02711)	(.02751)
Disp x Hartz	-.10120*	-.06730*	-.09358*	-.07130*	-.10008**	-.10167**
	(.01215)	(.01042)	(.00995)	(.01179)	(.01108)	(.01124)
R squared	.8605	.8723	.8206	.8356	.8688	.8650
Individual-months	25,671,003	20,064,397	28,840,027	22,368,762	24,789,632	24,789,632
Individuals	280,860	240,309	308,983	244,913	272,123	272,123

* p-value < .001; standard errors clustered at the individual level; displayed estimates are for a median-age, median- education worker evaluated at the middle date of the sample at average GDP growth.

Sorting out which element of the reform was at work

As discussed in the introduction, an important limitation in our study is that we cannot perfectly infer which elements of the reform package were most important for what we observe. The sharp reduction in long-term unemployment benefits and tighter job search/acceptance requirements are both consistent with the evidence of a higher hazard rate and displacement penalty. The liberalization of temporary agency work could also have played a role by allowing employers to offer lower-paying jobs. If displaced workers had always been willing to return to work at a significantly lower wage but institutional constraints prior to the reforms prohibited employers from offering such jobs, a relaxation of such constraints could produce a reduction in earnings of recently displaced workers, as we find in the data. Indeed as Figure 6 shows, temporary work accounted for a sizable fraction of new job creation in some of the post-Hartz sample years. Our data, however, do not allow us to infer whether a worker worked in a temporary job, and hence we cannot provide evidence on how important this part of the reforms was.

Concerning the reform of the employment agency, such a reform would have improved matching efficiency, which is supported by the existing literature. Standard models might be consistent with both improved matching efficiency and lower post-unemployment earnings if, after Hartz, displaced workers' ability to extract surplus was curtailed, which might have been the case in light of the other elements of the reform. Hence, although we believe that the final, Hartz IV package was a critical component of the reforms, we caution that the other dimensions of the reforms might also have contributed to the facts we document.

Figure 6. Temporary Work in Total Employment
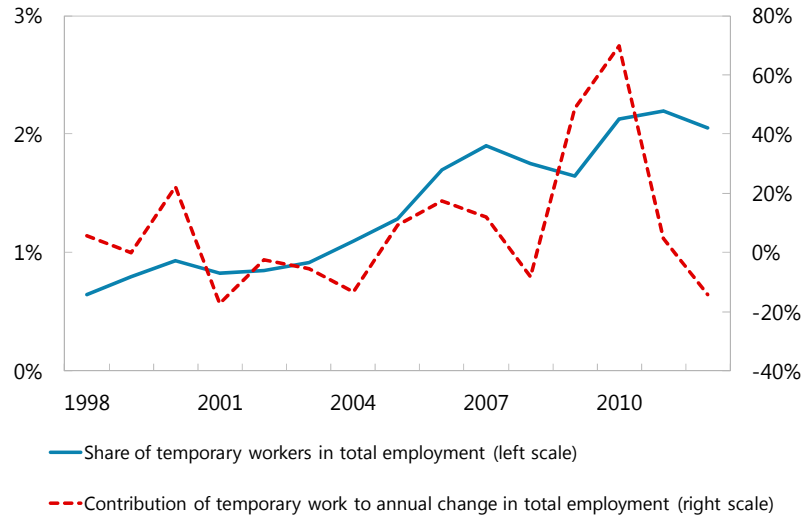
Sources: Federal Employment Agency, Eurostat, and Authors' calculations.

V. CONCLUSION

The unemployment rate in Germany gradually increased for three decades before peaking at over 11 percent in the early 2000s. It has since fallen dramatically to currently stand at five percent, its lowest level since reunification. As the peak in the unemployment rate coincided with the extensive labor market reforms known as the Hartz reforms, the importance of these reforms for the improvement in German labor markets has been the source of a lively academic and policy debate.

In this paper we document that the earnings of workers recently displaced from a full-time job fell dramatically relative to those who remained in full-time employment after the Hartz reforms. Using a difference-in-difference framework and controlling for worker observable and unobservable characteristics, we estimate a 10 log point additional reduction in the earnings of displaced workers relative to similar workers who remain employed in the post-reform years. Our results are consistent with search-theoretic models of labor markets, which argue that lowering unemployment benefits reduces the reservation wage of displaced workers. As a result, post-unemployment earnings drop. From this perspective, while the reform was successful at reducing Germany's unemployment, it came at a significant cost to workers experiencing unemployment not only in terms of reduced benefits but also through lower earnings post unemployment. Although our paper does not offer a structural model of the economy and hence cannot address the welfare implications of the reforms, its results indicate that attempts to evaluate the welfare consequences of the reforms must take into account the effect on workers' earnings after they return to employment.

The findings in this paper can be extended along numerous dimensions. For instance, it would be interesting to study how post-unemployment outcomes such as migration, the probability of subsequent displacement, the probability of switching occupation or sector, volatility of earnings, or future earnings growth change following the Hartz reforms. Most importantly, additional research would be desirable to better establish the causal impact of the reforms to address worries that the differential effects documented in this paper may be driven by shocks contemporaneous but not related to the Hartz reforms. The latter can be achieved by exploiting the differential impact of the reforms across subpopulations of workers. An initial exploration of this line of inquiry suggests that workers more affected by the reforms disproportionately increased their hazard rate of returning to work and suffered larger post-unemployment earnings losses after the reforms. This is further evidence that the Hartz reforms were behind the results presented in this paper.

References

Arent, Stefan, and Wolfgang Nagl, 2011, Unemployment Benefit and Wages: The Impact of the Labor Market Reform in Germany on (Reservation) Wages, Ifo Working Paper No. 101.

Bentolila, Samuel, and Giuseppe Bertola, 1990, Firing Costs and Labor Demand: How Bad is Eurosclerosis?, Review of Economic Studies, Vol. 57, pp. 381-402.

Burda, Michael C., and Jennifer Hunt, 2011, What Explains the German Labor Market Miracle in the Great Recession?, NBER Working Paper No.17187.

Blanchard, Olivier J., William D. Nordhaus, and Edmund S. Phelps, 1997, The Medium Run, *Brookings Papers on Economic Activity*, Vol. 1997 No. 2, pp.89-158.

Couch, Kenneth. A., and Dana W. Placzek, 2010, Earnings Losses of Displaced Workers Revisited, *American Economic Review*, Vol. 100, pp. 572-589.

Davis, Steven J., and Til Von Wachter, 2011, Recessions and the Costs of Job Loss, *Brookings Papers on Economic Activity*, Vol. 43, pp. 1-72.

Dlugosz, Stephan, Gesine Stephan, and Ralph A. Wilke, 2013, Fixing the Leak: Unemployment Incidence Before and After a Major Reform of Unemployment Benefits in Germany, *German Economic Review,* Vol. 15, pp. 329-352.

Dustmann, Christian, Bernd Fitzenberger, Uta Schönberg, and Alexandra Spitz-Oener, 2014, From Sick Man of Europe to Economic Superstar: Germany's Resurgent Economy. *The Journal of Economic Perspectives*, Vol. 28, pp. 167-188.

Fahr, Rene' and Uwe Sunde, 2009, Did the Hartz Reforms Speed-Up the Matching Process? A Macro-Evaluation Using Empirical Matching Functions, *German Economic Review*, Vol. 10, pp. 284-316.

Funk Kirkegaard, Jakob, 2014, Making Labor Market Reforms Work for Everyone: Lessons from Germany, Peterson Institute on International Economics Policy Brief 1-14.

Giannelli, Gianna G., Ursula Jaenichen, and Thomas Rothe, 2013, Doing Well in Reforming the Labour Market? Recent Trends in Job Stability and Wages in Germany, IZA Discussion Paper No. 7580.

Hertweck, Mattias S., and Oliver Sigrist, 2012, The Aggregate Effects of the Hartz Reforms in Germany, SOEP papers on Multidisciplinary Panel Data Research No. 532.

Jacobson, Louis, Robert Lalonde, and Daniel Sullivan, 1993, Earnings Loss of Displaced Workers, *American Economic Review*, Vol. 83, pp. 685-709.

Klinger, Sabine, and Thomas Rothe, 2012, The Impact of Labour Market Reforms and Economic Performance on the Matching of the Short-term and the Long-Term Unemployed. *Scottish Journal of Political Economy*, Vol. 59, pp. 90-114.

Krause, Michael U., and Harald Uhlig, 2012, Transitions in the German Labour Market: Structure and Crisis", *Journal of Monetary Economics*, Vol. 59, pp. 64-79.

Krebs, Tom, and Scheffel, Martin, 2013, Macroeconomic Evaluation of Labor Market Reform in Germany. *IMF Economic Review*, Vol. 61, pp. 664-701.

Launov, Andrey, and Klaus Waelde, 2013, Estimating Incentive and Welfare Effects of Non-Stationary Unemployment Benefits, International Economic Review.

Lindbeck, Assar, and David Snower, 1988, *The Insider-Outsider Theory of Employment and Unemployment*, (Cambridge, Mass.: MIT Press).

Ljunqvist, Lars, and Sargent Thomas J. Sargent, 1996, The European Unemployment Dilemma, *Journal of Political Economy,* Vol. 106, pp. 514-550.

Mortensen, Dale T., and Christopher A. Pissarides, 1994. Job Creation and Job Destruction in the Theory of Unemployment, *Review of Economic Studies*, Vol. 61, pp. 397-415.

Mortensen, Dale T., and Christopher. A. Pissarides, 1999, Job Reallocation, Employment Fluctuations, and Unemployment, in John B. Taylor and Michael Woodford (Eds.), Chapter 18 in *Handbook of Macroeconomics*, (Amsterdam: Elsevier).

Schmieder, Johannes, F., Til von Wachter, and Stefan Bender, 2010, The Long-Term Impact of Job Displacement in Germany during the 1982 Recession on Earnings, Income, and Employment, IAB discussion paper No. 1.

Von Wachter, Til, Jae Song, and Joyce Manchester, 2011, Long-Term Earnings Losses due to Mass Layoffs During the 1982 Recession: An Analysis Using Longitudinal Administrative Data from 1974 to 2008, mimeo, Columbia University.