The Effect of the Hartz Labor Market Reforms on Post-unemployment Outcomes, Sorting, and Matching

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We use linked longitudinal data on employers and employees to estimate how the 2003-2005 Hartz reforms affected the wages of displaced German workers after they returned to work. We also present a simple new method to decompose the wage effects into components attributable to selection on unobservables, and to changes in the way that displaced workers are sorted across firms and worker-firm matches upon re-employment. We find that the Hartz reforms substantially reduced the wages of displaced workers after their return to work. Women experience smaller wage losses than men. For both sexes, nearly two thirds of the increased wage loss arises because displaced workers increasingly find re-employment in low-wage firms. An additional twenty percent is because displaced workers sort into worse matches with employers. Collectively, these two channels explain almost all of the Hartz reforms' effect on post-displacement wages. Robustness checks indicate that our findings aren't explained by reallocation across sectors or occupations, by changes to the returns to employer-specific human capital, or by the subsequent financial crisis.

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1 Introduction

Between 2003 and 2005, the German goverment introduced a series of labor market reforms known collectively as the Hartz reforms. Introduced in response to a decade of high unemployment and weak economic growth, they were designed to increase labor market flexibility and reduce long-term unemployment. Many-pronged, the reforms exempted certain low-wage part-time jobs (so-called "Mini jobs") from social security taxes, introduced grants for entrepreneurs and new supports for vocational training, eased regulation on temporary work agencies, relaxed firing restrictions, increased job search assistance, tightened search obligations for the unemployed, and most significantly, they made long-term unemployment benefits substantially less generous for most workers. In the decade that followed the reforms, Germany stood apart from many other advanced economies, with steadily declining unemployment and strong economic growth. Although controversial, the Hartz reforms are cited by many as an important component of Germany's recent economic success.

Despite their scope and high profile, surprisingly little is known about the reforms' effects on labour market outcomes. In this paper, we use linked longitudinal data on employers and employees to estimate the reforms' effect on the wages of displaced workers after they return to work. We also present a simple new method to decompose the wage effects of the reforms. Our decomposition distinguishes between *sorting* and *matching* effects that arise due to changes in the way that displaced workers are sorted across firms and worker-firm matches upon re-employment, and a *selection* effect that arises due to changes in the distribution of unobserved characteristics of workers selected into displacement and re-employment. Our decomposition is closely related to the Pendakur and Woodcock (2010) "Glass Door effect," the Gelbach (2016) decomposition, and Figueiredo et al. (2014) estimates of the effect of industrial agglomeration on worker-firm matching.

The Hartz reforms affected all regions and applied to all workers. There is therefore no control group of German workers who were not exposed to the reforms. This has limited the credibility of previous evaluations. However, because the reforms were primarily targeted at policies surrounding job search and unemployment assistance, we would expect them to have had the greatest impact on unemployed individuals who were searching for work, and little or no effect on the continuously employed. Thus our basic empirical strategy, elaborated below, is a "difference-in-differences" (DD) analysis in which we compare the post-unemployment outcomes of individuals who were displaced from employment and collected unemployment benefits to individuals who were continuously employed, before vs. after the Hartz reforms. Such comparisons allow us to isolate the effect of the reforms on displaced workers who received unemployment benefits.

Standard search and matching models (see Rogerson et al. (2005)for a survey) predict that a reduction in the generosity of unemployment benefits will reduce an unemployed worker's reservation wage, and thus reduce the expected wage upon re-employment. And to the extent that wages have persistent firm and match-specific components (see Abowd et al. (1999) and Woodcock (2015) for supporting empirical evidence), we would therefore expect less generous unemployment benefits to make unemployed workers more willing to accept employment at low-wage firms and/or in "worse" matches.[1] Despite the clear predictions of canonical models, however, the empirical evidence on the relationship between unemployment benefits and re-employment wages has been decidedly mixed.[2] This, coupled with the fact that the other components of the Hartz reforms were designed to facilitate search and matching and hence could have had opposing effects on re-employment wages, arguably made the net effect of the reforms on re-employment wages an open question.

In keeping with canonical search and matching models, we find that the Hartz reforms substantially increased the wage loss of displaced workers when they returned to work. The increased wage loss was larger for men than women. For both sexes, nearly two thirds of the increased post-displacement wage loss arises because displaced workers increasingly find re-employment in low-wage firms: firms that pay *all* of their employees substantially less than a typical German employer, conditional on their characteristics. An additional twenty percent of the increased post-displacement wage loss under Hartz arises because displaced workers sort into worse matches with employers. Changes in the distribution of the unobserved characteristics of workers selected into displacement and re-employment explain only about 8 percent of the wage loss for men, and around 25 percent for women. Collectively, these three channels explain almost all of the Hartz reforms' effect on post-displacement wages. Robustness checks indicate that our findings aren't explained by reallocation across sectors or occupations, by changes to the returns to employer-specific human

[1] Woodcock (2010) presents a matching model with heterogeneous workers and firms that makes precisely this prediction.

[2] For example, Card et al. (2007), Lalive (2007), and van Ours and Vodopivec (2008) find no statistically significant relationship between the duration of unemployment insurance and re-employment wages. Schmieder et al. (2016) find a statistically significant negative effect on wages, whereas Nekoei and Weber (2017) find a positive effect. The latter authors also present a search model with duration dependence that reconciles these disparate findings.

capital, or by the subsequent financial crisis.

The remainder of this paper is organized as follows. Section 2 describes the reforms more fully and summarizes previous studies of their effects. Section 3 describes our data. Section 4 describes our empirical strategy and our decomposition into sorting, matching, and selection effects. Section 5presents the empirical results, and Section 6concludes.

2 The Hartz Reforms

Following reunification, the German economy entered an extended period of slow growth and increasing unemployment (Figure 1). Pressure for reform led to the creation of the Hartz Commission in 2002, which was tasked with proposing reforms to labour market institutions. The Commission's recommendations were approved in 2002-2003, and implemented in phases between January 2003 and January 2005.

The first three phases of the reforms, dubbed Hartz I-III, sought to improve the efficiency of job search and increase employment flexibility. This included measures to deregulate the temporary work sector, introduce a subsidy for one-person companies, introduce "mini jobs" that are exempt from most social security taxes, and relax layoff rules for small firms. The centerpiece of the reforms, dubbed Hartz IV, came into effect on January 1 2005. These significantly restructured the unemployment and social assistance system to make benefits less generous for most unemployed individuals, by reducing both the amount and duration of benefits and making them conditional on stricter job search and acceptance requirements. Hartz IV was squarely targeted at reducing long-term unemployment.

Prior to 2005, workers with sufficient pre-unemployment experience were entitled to an unemployment benefit (UB) that replaced 60-67 percent of their pre-unemployment net earnings. The duration of the UB entitlement was limited to 12 months for workers under 45 years of age, but could be as long as 36 months for older workers, depending on the claimant's work history. Individuals that exhausted their UB entitlement were eligible for additional unemployment assistance (UA; *Arbeitslosenhilfe*) that replaced 53-57 percent of their pre-unemployment net earnings. There was no limit on the duration of UA entitlement, but benefits were means-tested and claimants were subject to an annual review. Individuals that did not qualify for UB or UA (e.g., because of an insufficient employment history) but who met a means-test could receive social assistance benefits (SA; *Sozialhilfe*). The SA benefit was a lump-sum payment that did not depend on the pre-unemployment level of earnings, and was consequently less generous than UB or UA for most unemployed individuals. This three-layered benefits system provided Germany's long-term unemployed with relatively generous income support compared to many other advanced economies.

Hartz IV reduced the generosity of the benefits available to most of Germany's long-term unemployed. UB was replaced by a new but very similar short-term unemployment benefit (UB I; *Arbeitslosengeld I*) that maintained the same replacement rate and the 12 month maximum benefit duration for younger workers. However older workers saw a reduction in the maximum duration of benefits to which they were entitled, to 15 months for workers over age 50, 18 months for workers over age 55, and 24 months for workers over age 58. UA and SA were collectively replaced by the new unemployment benefit II (UB II; *Arbeitslosengeld II*). UB II most closely resembles the pre-reform SA benefit: it is means-tested and recipients receive a lump sum similar in value to the previous SA benefit. As a consequence of the Hartz IV reforms, many workers would have exhausted their short-term unemployment benefits sooner, and experienced a sharper reduction in benefits when they did so, than prior to the reforms.

Most of the existing literature on the effects of the Hartz reforms has focused on unemployment and matching outcomes. Krause and Uhlig (2012), Krebs and Scheffel (2013), and Launov and Waelde (2013) use calibrated search models to simulate the effect of the reforms and conclude that the reduced generosity of unemployment benefits significantly reduced unemployment. Fahr and Sunde (2009), Klinger and ROthe (2012) and Hertweck and Sigrist (2012) estimate matching functions from time series data and find that Hartz I-III improved matching efficiency. Dlugosz et al. (2013) estimate transition rates between employment and unemployment and find that the reforms materially reduced transitions – especially for older workers, whose experienced deeper cuts to benefits.

Relatively few authors have considered the effects of the Hartz reforms on wage outcomes. Arent and Nagel (2011) test for a structural break in wages following the reforms, and argue that they reduced wages economy-wide. Price (2016)exploits age-related heterogeneity in the timing of the effective onset of Hartz IV to estimate how long-term benefit reductions affected unemployment duration and re-employment wages, and finds that it reduced both.

The paper most closely related to this one is Engbom et al. (2015). They also rely on a difference-in-differences framework to estimate the effect of the Hartz reforms on post-displacement wages, and find that it reduced re-employment wages by roughly 10 percent. Unlike the current paper, however, those authors are unable to identify individuals' employers either before or after displacement.[3] As a consequence, they are unable to identify or estimate the extent to which the reforms changed the way that displaced workers are sorted across firms and matches. This is an important innovation, and in Section 4 we show how to identify these sorting and matching effects – as well as a selection effect that arises if the reforms changed the distribution of unobserved characteristics of the workers selected into displacement and re-employment. Note that the distinction between these channels matters, and it is important to understand which underlies the reforms' effects on post-displacement wages.

3 Data

We use linked employer-employee data from the German Institute for Employment Research (IAB), called the LIAB. The LIAB link establishment data from annual waves of the IAB Establishment Panel with individual-level data from the IAB's Integrated Employment Biographies (IEB). The IAB Establishment Panel is a representative sample of German establishments. Firms are sampled from the population of all German establishments with at least one employee subject to social security; stratified by industry, size, and federal state. The subset of establishments that appear in the IAB Establishment Panel in multiple years, or go out of business, between 2003 and 2011 (so-called "panel cases") form the basis of the LIAB. The specific version of the LIAB used in this paper (the 2014 LIAB Longitudinal Model) comprises all individuals that were employed in one of the "panel case" establishments for at least one day between 2002 and 2012. The LIAB include each individual's complete history of employment subject to Social Security, marginal part-time employment, or receipt of short-term unemployment benefits between 1993 and 2014. Employment and benefit receipt is recorded at a daily level of detail. The employment records include key information about

[3]There are a number of other less important distinctions. We study a different earnings measure, and our treatment group is restricted to involuntarily displaced workers instead of all individuals who receive short-term unemployment benefits. Our sample includes a longer post-Hartz period, which allows us to estimate the policy's longer-term effects. Our data also allow us to better control for unobserved heterogeneity that may be correlated with selection into displacement.

individuals, their employment earnings, and a unique identifier for their employer. Notably, the employment records include identifiers for *all* of an individual's employers – even those that are not part of the IAB Establishment Panel – between 1993 and 2014. This makes it possible to control for unobserved firm and match heterogeneity as described in Section 4. It also makes it possible to link employment records to the Establishment History Panel (BHP), a more comprehensive (but less detailed) 50% sample of all establishments in Germany with at least one employee. We obtain some key employer characteristics, such as geography, industry, and final year of operation, from the BHP.

We focus our analysis on daily wages at full-time jobs covered by social security held by individuals 25-65 years of age and working in the former West Germany.[4] We further exclude mini-jobs (which are only included in the IEB after 1999) and jobs held by trainees and interns. Our sample construction mostly follows Card et al. (2013) and is described in more detail in the Data Appendix. The main departure is that we undertake our analysis at the quarterly level instead of annually. Because we are estimating the effects of the Hartz reforms on post-displacement wage outcomes, we prefer to have finer resolution of the elapsed time since displacement than is possible with annual data.

We sum the earnings received by each individual from each establishment in each quarter and designate the one that paid an individual the most as their main job for that quarter. We restrict our sample to main jobs. The vast majority of full-time workers in our sample are employed at only one establishment in any quarter (the average number of jobs per quarter in our sample is 1.03), so we believe the restriction to one job per quarter is innocuous. We calculate the average daily wage in each quarter by dividing total earnings by the duration of the job spell (including weekends and holidays) in that quarter. We convert wages to real 2010 euros using the CPI.

Table 1 provides basic characteristics of the wage data in our sample. The sample comprises roughly 1.3-1.9 million quarterly wage observations on full-time men in each year, and roughly one third that number on full-time women. The trends in male and female average wages over the 1993-2014 period are remarkably similar: increasing by roughly 6 percent between 1993 and 2003, then declining by roughly 5 percent until 2008, and increasing again thereafter. For men, the 2008-2014

[4] A future version will also estimate the effects of the Hartz reforms on the propensity to work full-time versus part-time.

increase is about 7 percent, whereas for women it is a substantially larger 13 percent. The gap between male and female mean wages was around 25 log points for most of the sample period, but narrowed to around 19 log points following the substantial wage growth that women experienced after 2008. Paralleling the trend investigated in Card et al. (2013), wage dispersion increased for both men and women between 1993 and 2014, with the standard deviation of real daily wages increasing by about 14 percent for men and 12 percent for women over that period. That trend appears to have reversed since 2010, with the standard deviation of wages falling by about 3 percent for men and 5 percent for women between 2010 and 2014.

A limitation of wage data based on the IEB, and this includes the LIAB, is that reported earnings are censored at a maximum value dictated by reporting requirements of the social security system. As shown in Table 1, 12 to 17 percent of male wage observations and 3 to 7 percent of female wage observations are censored each year. To address this problem we follow Card et al. (2013) and Dustmann et al. (2009), and use Tobit models to stochastically impute the censored upper tail of the wage distribution. As described in detail in Appendix A, we estimate separate Tobit models by sex, 10-year age category, year, and education (5 categories). The imputation procedure is designed to capture the patterns of within-person and within-establishment wage variation in the data because our econometric methodology, described in Section 4, relies on models that include worker and establishment fixed effects, or worker-establishment match effects. Specifically, our Tobit models for a given year include the worker's earnings and censoring rate in all other years, and the mean earnings and censoring rate of his or her coworkers in that year. We use the estimated Tobit parameters to replace each censored wage value with a random draw from the upper tail of the appropriate conditional wage distribution (see Appendix A for details). In Appendix B we present the results of a validation exercise which demonstrates that our imputation method does a good job of replicating the upper tail of the wage distribution, and a good job of preserving the relative share of within-establishment and within-match wage variation.

Unsurprisingly, our imputation procedure increases both the mean and standard deviation of the wage distribution (relative to the censored data), as illustrated in Table 1. The increase is larger for men than women – about 6 percent vs. 2 percent for the mean, and 10 percent vs. 4 percent for the standard deviation – reflecting the higher male censoring rate. For both genders, the magnitude of the increase varies from year to year and is larger in years when the censoring rate is higher.

There is no discernible trend prior to about 2008, but there is a clear increase for both men and women since 2009.

As described in Section 4, our econometric framework compares the outcomes of workers who have recently been displaced from employment to other workers, before vs. after the Hartz reforms. We focus on displaced workers, rather than voluntary job changers, primarily to reduce concerns about the possible endogeneity of job change (and hence selection into treatment). The IEB employment records indicate the date of employment termination, but not the reason. However we also observe the start date of short-term unemployment benefits, to which all unemployed workers with at least 12 months of employment experience in the preceding three years are entitled. Individuals who are involuntarily displaced from employment may collect short-term benefits immediately following the end of employment. Those who quit voluntarily, however, must wait 12 weeks before collecting benefits. We therefore define an individual who separates from employment as being involuntarily displaced if they begin receiving unemployment benefits within 12 weeks of their last day of work. This is conservative: under this definition we will misclassify some genuinely displaced workers as non-displaced if they find a new job before their last day of work and never collect short-term benefits; as well as those who change jobs voluntarily in anticipation of being laid off. This will tend to bias our estimates of the wage effect of displacement toward zero. However this is true both before and after the Hartz reforms, and we have no reason to think that the reforms would change the likelihood of mis-classification.[5] Thus we do not expect it to bias our difference-in-differences estimates.

For our main analysis, we define an individual as *recently* displaced from employment if they were involuntarily displaced from employment in the preceding four quarters. For robustness, we also report estimates using looser definitions of recent displacement; namely, involuntary displacement from employment in the preceding 8, 12, or 20 quarters. As shown in Table 2, roughly 2.5 percent of men were involuntarily displaced in the preceding four quarters, increasing steadily to 11 percent for the 20 quarter measure. Women are more likely to have experienced a recent displacement from employment: 2.9 percent in the preceding four quarters and 12.9 percent in the preceding 20 quarters. Unsurprisingly, the recently displaced earn considerably less than other workers (47

[5]Indeed, the since the main thrust of the reforms was to reduce the generosity of long-term unemployment benefits, which for most workers did not commence until 12 months or more after employment termination, the reforms probably had little effect on behavior prior to employment termination.

log points for men, 33 log points for women), are younger, less likely to have an upper secondary certificate or university degree, and are more likely to have missing education data.[6] As shown in Figure 2, our displacement measure increases steadily throughout the early part of the sample period, reaching a peak of nearly 4 percent for both men and women in 2010, before declining steadily thereafter. The 2010 peak reflects the abnormally large number of displacements at the height of the financial crisis, in 2009.

4 Empirical Strategy

The Hartz reforms affected all regions and applied to all workers. There is therefore no control group of workers that was unaffected by the reforms. However, because the reforms were mostly targeted at job search and unemployment benefits, we would expect them to have the greatest impact on unemployed individuals who are actively searching for work, and to have little or no effect on continuously employed individuals. Thus our basic empirical strategy is to estimate the effect of the Hartz reforms in a difference-in-differences framework that compares the pre- vs. post-reform change in wages for recently displaced workers to the comparable change in wages for other workers.

Consider a basic difference-in-differences estimator

$$y_{it} = \mathbf{x}_{it}^{'}\beta_0 + \alpha_0 DISP_{it} + \delta_0 DISP_{it} * HARTZ_{it} + \tau_{0,t} + \epsilon_{0,it} \tag{1}$$

where $i = 1, ..., N$ indexes individuals, $t = 1, ..., T$ indexes time periods, y_{it} is the logarithm of i's daily wage, $\mathbf{x}_{it}$ is a vector of observable characteristics that earn returns β_0, $DISP_{it}$ is an indicator variable that equals one if i has recently been displaced from employment, $HARTZ_{it}$ is an indicator variable that equals one if the displacement occurred in 2005 or later, $\tau_{0,t}$ is a fixed time effect, α_0 and δ_0 are coefficients to be estimated, and $\epsilon_{0,it}$ is statistical error. In this specification, δ_0 is the coefficient of primary interest. Provided the parallel trends assumption is satisfied, it measures the causal effect of the Hartz reforms on post-displacement wages. The causal effect is identified from the pre- vs. post-reform change in the wage gap between individuals who were recently displaced

[6] Education is reported by employers, and consequently is more likely to be missing in the LIAB than in typical survey data.

from employment and those who were not. In this context, the parallel trends assumption requires that the wage gap between the recently displaced and other workers would have remained constant in the absence of the reforms.

In support of the parallel trends assumption, Figure 2 plots the regression-adjusted wage gap between recently displaced workers and all others in each year of our sample. To obtain the plotted values, for each year of our sample we estimated a regression of log real wages on an indicator for displacement in the preceding four quarters, indicators for calendar quarter and education (5 categories), a cubic polynomial in age, and age interacted with education. The plotted values are the coefficients on each year's displacement indicator. While we do see evidence of a mild pre-policy trend for men, that trend breaks in the opposite direction of our estimated policy effect in 2006, and hence cannot explain our findings. There is no evidence of a pre-policy trend for women.

Equation (1) is very similar to that proposed by Engbom et al. (2015).[7] We depart from that specification in several ways. First, as described in Section 3, we restrict our attention individuals involuntarily displaced from employment. In contrast, Engbom et al. (2015) define an individual as displaced if they are collecting unemployment benefits. Their measure will include some individuals who quit voluntarily, since they are also entitled to collect unemployment benefits after a 12 week waiting period.[8] Second, to reflect the fact that the Hartz reforms were introduced in stages between 2003 and 2005, we introduce an additional interaction term $\gamma_0 DISP_{it} * DURING_{it}$ where $DURING_{it}$ is an indicator variable that equals one if the displacement occurred between 2002 and 2004. Displacements in this window were likely to have been partially exposed to the Hartz reforms.[9]Including this interaction term therefore ensures that δ_0 measures the pre- vs. post-reform change in post-displacement wages, free of potential contamination from partially exposed displacements.

More importantly, we rely on the linked employer-employee structure of the LIAB to generalize

[7]Engbom et al. (2015) estimate their model on monthly data and use a different wage measure. Their wage measure in month t is the average monthly earnings between t and $t + 12$.

[8]Engbom et al. (2015)further restrict their definition to individuals who were continuously employed for 36 months prior to collecting unemployment benefits, which they argue reduces the likelihood of misclassifying voluntary quitters as displaced and hence is likely to reduce the bias resulting from misclassification as well.

[9]Both before and after the reforms, most workers were entitled to at least 12 months of short-term unemployment benefits. Thus unemployment spells that began in early 2002 would have been eligible for benefits continuing into 2003, when the first two phases of the Hartz reforms were introduced. The Hartz IV reforms were introduced on January 1 2005, so unemployment spells that began before this date could not have been exposed to the full set of reforms for their entire duration.

eq. (1) by controlling for unobserved worker-, firm-, and match-level heterogeneity. This is important for several reasons. First, if displacement – and specifically, the timing of displacement – is systematically related to unobserved characteristics, then controlling for unobserved heterogeneity will help to disentangle the effect of the policy reforms from the effect of selection into displacement. Second, controlling for unobserved heterogeneity allows us to quantify the reforms' effects on the way that workers are sorted across firms and matches.

Consider

$$\begin{aligned} y_{it} &= \mathbf{x}_{it}'\beta_1 + \alpha_1 DISP_{it} + \gamma_1 DISP_{it} * DURING_{it} + \delta_1 DISP_{it} * HARTZ_{it} + \tau_{1,t} + \theta_{1,i} + \epsilon_{1,it} \\ &= \mathbf{z}_{it}'\eta_1 + \delta_1 h_{it} + \theta_{1,i} + \epsilon_{1,it} \end{aligned} \tag{2}$$

where we have introduced a fixed worker effect $\theta_{1,i}$ and rewritten $\mathbf{z}_{it}'\eta_1 = \mathbf{x}_{it}'\beta_1 + \alpha_1 DISP_{it} + \gamma_1 DISP_{it} * DURING_{it} + \tau_{1,t}$ and $h_{it} = DISP_{it} * HARTZ_{it}$ for compactness. The coefficient δ_1 continues to measure the effect of the Hartz reforms on post-displacement wages. Unlike δ_0, however, δ_1 is conditional on workers' time-invariant unobserved characteristics. If the Hartz reforms changed the distribution of the unobserved characteristics (θ_i) of workers that were selected into displacement – or into re-employment, following displacement – then δ_0 and δ_1 will differ. We can think of δ_0 as the *total effect*, including the reforms' effect on selection into displacement and re-employment. In contrast, δ_1 holds the distribution of workers' unobserved characteristics constant, and hence is net of the reforms' effect on selection into displacement and re-employment.

It is straightforward to isolate the reforms' effect on post-displacement wages via selection. Let $\hat{\delta}_0$ and $\hat{\delta}_1$ denote the OLS estimators of δ_0 and δ_1, respectively. The estimated selection effect is $\hat{\delta}_0 - \hat{\delta}_1$. To see this, rewrite eqs. (1) and (2) in matrix notation as follows:

$$\mathbf{y} = \mathbf{Z}\eta_0 + \delta_0\mathbf{h} + \epsilon_0 \tag{3}$$

$$\mathbf{y} = \mathbf{Z}\eta_1 + \delta_1\mathbf{h} + \mathbf{D}\theta_1 + \epsilon_1 \tag{4}$$

where $\mathbf{y}$ is the $N^* \times 1$ vector of wage outcomes, $\mathbf{Z}$ is the $N^* \times k$ matrix of observable characteristics including time and displacement indicators, $\mathbf{h}$ is the $N^* \times 1$ vector of displacement indicators interacted with the Hartz indicator, $\mathbf{D}$ is the $N^* \times N$ design matrix of the fixed worker effects,[10] ϵ_0 and ϵ_1 are $N^* \times 1$ vectors of errors, and $\eta_0, \eta_1, \delta_0, \delta_1$, and θ_1 are conformable vectors of parameters. Suppose that eq. (4) is correctly specified, but we estimate eq. (3); and let $\mathbf{M_A} = \mathbf{I} - \mathbf{A}\left(\mathbf{A}'\mathbf{A}\right)^{\mathbf{-1}}\mathbf{A}'$ denote the standard "annihilator matrix" that projects onto the column null space of an arbitrary matrix $\mathbf{A}$. Then we have

$$E\left[\hat{\delta}_0 - \hat{\delta}_1\right] = \left(\mathbf{h}'\mathbf{M_Z}\mathbf{h}\right)^{-1}\mathbf{h}'\mathbf{M_Z}\mathbf{D}\theta_1$$

which is an unbiased estimator of δ_θ in the hypothetical regression:

$$\theta_{1,i} = \mathbf{z}'_{it}\eta_\theta + \delta_\theta h_{it} + \epsilon_{\theta,it}.$$

The coefficient δ_θ measures the difference between the average worker effect of individuals displaced after the Hartz reforms and the average worker effect of workers displaced before the reforms. It will be non-zero if the reforms changed the distribution of workers selected into displacement. Note that this estimate of the reforms' effect on selection is closely related to the Pendakur and Woodcock (2010) "Glass Door effect" and is a special case of the Gelbach (2016) decomposition. It is straightforward to test the hypothesis of no selection effect, $H_0 : \delta_\theta = 0$, via a Hausman test; see Proposition 2 of Pendakur and Woodcock (2010) and its proof.

The preceding generalizes naturally to account for the reforms' effects on the way that workers are sorted across firms and matches. Following Abowd et al. (1999) and others, we introduce a firm effect to the wage specification:

$$y_{it} = \mathbf{z}'_{it}\eta_2 + \delta_2 h_{it} + \theta_{2,i} + \psi_{2,J(i,t)} + \epsilon_{2,it} \tag{5}$$

where the function $J(i,t) = j$ denotes the firm j at worker i was employed in period t, and $\psi_{2,J(i,t)}$ is the fixed firm effect. In matrix notation,

$$\mathbf{y} = \mathbf{Z}\eta_2 + \delta_2\mathbf{h} + \mathbf{D}\theta_2 + \mathbf{F}\psi_2 + \epsilon_2 \tag{6}$$

where $\mathbf{F}$ is the $N^* \times J$ design matrix of the firm effects.[11] The firm effects $\psi_{2,J(i,t)}$ measure inter-firm

[10] $\mathbf{D} = [\mathbf{d}_1, \mathbf{d}_2, \ldots, \mathbf{d}_N]$ where the i^{th} column $\mathbf{d}_i$ is an indicator variable for worker i.
[11] $\mathbf{F} = [\mathbf{f}_1, \mathbf{f}_2, \ldots, \mathbf{f}_J]$ where the j^{th} column $\mathbf{f}_j$ is an indicator for employment at firm j.

differences in average wages, conditional on observable characteristics $\mathbf{z}_{it}$ and workers' unobserved characteristics $\theta_{2,i}$. They thus provide a useful summary of firm-level wage outcomes, and it is of interest to understand whether and how the Hartz reforms may have changed the way that workers are sorted across high- vs. low-wage firms.

In eqs. (5) and (6), δ_2 measures the effects of the Hartz reforms on post-displacement wages, conditional on observable characteristics and the unobserved characteristics of workers and their employers. If the reforms changed the way that workers are sorted across employment in high- vs. low-wage firms, then estimates of δ_2 will be net of this effect but estimates of δ_0 and δ_1 will not. Analogous to the above, we can therefore isolate the reforms' effect on inter-firm sorting from the difference between estimated policy effects that do and do not control for firm effects $\hat{\delta}_1 - \hat{\delta}_2$. Specifically, if eq. (6) is correctly specified but we estimate eq. (4), then

$$E\left[\hat{\delta}_1 - \hat{\delta}_2\right] = \left(\mathbf{h}'\mathbf{M}_{[\mathbf{Z}\ \mathbf{D}]}\mathbf{h}\right)^{-1}\mathbf{h}'\mathbf{M}_{[\mathbf{Z}\ \mathbf{D}]}\mathbf{F}\psi_2$$

which is an unbiased estimator of δ_ψ in the hypothetical regression:

$$\psi_{2,J(i,t)} = \mathbf{z}_{it}'\eta_\psi + \delta_\psi h_{it} + \theta_{\psi,i} + \epsilon_{\psi,it}.$$

The coefficient δ_ψ measures the difference between the average post-displacement firm effect of individuals displaced after the Hartz reforms vs. those displaced before the reforms, conditional on their observable and unobserved characteristics. It will be non-zero if the reforms changed the way that displaced workers are sorted across high- and low-wage firms once they find re-employment. Again, it is straightforward to test the hypothesis of no sorting effect, $H_0 : \delta_\psi = 0$, via a Hausman test.

In addition to its effects on selection and inter-firm sorting, the Hartz reforms may have also affected the way that displaced workers are sorted across "good" and "bad" employer-employee matches. Following Woodcock (2008) and Woodcock (2015), consider augmenting our wage specification with a match effect:

$$y_{it} = \mathbf{z}_{it}'\eta_3 + \delta_3 h_{it} + \theta_{3,i} + \psi_{3,J(i,t)} + \phi_{3,iJ(i,t)} + \epsilon_{3,it}. \tag{7}$$

The match effects $\phi_{3,iJ(i,t)}$ measure persistent differences in average wages between worker-firm matches, conditional on observable characteristics $\mathbf{z}_{it}$, and workers' and firms' time-invariant unobserved characteristics $\theta_{3,i}$ and $\psi_{3,J(i,t)}$. They thus provide a useful summary of match-level wage outcomes, and it is of interest to understand whether and how the Hartz reforms may have changed the way that workers are sorted across better vs. worse matches.

As discussed in detail in Woodcock (2015), separately identifying the worker, firm, and match effects requires potentially strong assumptions. However, our objective here is simply to isolate the effect of the Hartz reforms post-displacement matching outcomes and for this it is unnecessary to make further identifying assumptions. Instead, we replace the person, firm, and match effects in (7) with a combined match-specific fixed effect,

$$y_{it} = \mathbf{z}_{it}^{'}\eta_3 + \delta_3 h_{it} + \Phi_{3,iJ(i,t)} + \epsilon_{3,it}. \tag{8}$$

Note that eq. (8) nests eq. (7), in the case where $\Phi_{3,iJ(i,t)} = \theta_{3,i} + \psi_{3,J(i,t)} + \phi_{3,iJ(i,t)}$, as well as other potentially interesting specifications with worker-, firm-, and/or match-specific heterogeneity in wages, e.g., $\Phi_{3,iJ(i,t)} = \theta_{3,i} + \psi_{3,J(i,t)} + f\left(\theta_{3i}, \psi_{3J(i,t)}\right)$.

Similar to the above, we can isolate the effect of the Hartz reforms on the way that workers are sorted across worker-firm matches from the difference between an estimate of δ_3 and δ_2. To see what this measures, we rewrite eq. (8) in matrix notation as

$$\mathbf{y} = \mathbf{Z}\eta_3 + \delta_3\mathbf{h} + \mathbf{G}\mathbf{\Phi}_3 + \epsilon_3 \tag{9}$$

where $\mathbf{G}$ is the $N^* \times M$ design matrix of the combined match effects, and $M \geq N + J$ is the total number of observed worker-firm matches.[12] Then we have

$$E\left[\hat{\delta}_2 - \hat{\delta}_3\right] = \left(\mathbf{h}'\mathbf{M}_{[\mathbf{Z}\ \mathbf{D}\ \mathbf{F}]}\mathbf{h}\right)^{-1}\mathbf{h}'\mathbf{M}_{[\mathbf{Z}\ \mathbf{D}\ \mathbf{F}]}\mathbf{G}\mathbf{\Phi}_3$$

[12]The M columns of $\mathbf{G}$ are indicator variables, one for each worker-firm match. If we sum the columns of $\mathbf{G}$ for each worker, we obtain $\mathbf{D}$. Likewise, if we sum the columns of $\mathbf{G}$ for each firm, we obtain $\mathbf{F}$. In other words, both $\mathbf{D}$ and $\mathbf{F}$ are contained within the column space of $\mathbf{G}$. In this sense the specifications given by eqs. (4) and (6) are nested within the specification in eq. (9).

which is an unbiased estimator of δ_Φ in the hypothetical regression:

$$\Phi_{3,iJ(i,t)} = \mathbf{z}_{it}'\eta_\Phi + \delta_\Phi h_{it} + \theta_{\Phi,i} + \psi_{\Phi,J(i,t)} + \epsilon_{\Phi,it}.$$

Figueiredo et al. (2014) propose a similar measure to estimate whether industrial clusters improve worker-firm matching. The coefficient δ_Φ measures the difference between the average post-displacement match effect of individuals displaced after the Hartz reforms vs. those displaced before the reforms, conditional on their observable characteristics $\mathbf{z}_{it}$ and workers' and firms' time-invariant unobserved characteristics. It will be non-zero if the reforms changed the way that displaced workers are sorted across better vs. worse matches once they find re-employment. Again, it is straightforward to test the hypothesis that the reforms had no effect on matching, $H_0 : \delta_\Phi = 0$, via a Hausman test.

The identifying assumptions for the specifications with person, firm, and/or match effects are standard. Of these, the specification with person and firm effects arguably has the strictest identifying assumptions. For that model we require:

$$E\left[\epsilon_2 | \mathbf{Z}, \mathbf{h}, \mathbf{D}, \mathbf{F}\right] = \mathbf{0}$$

(a slightly weaker assumption based on orthogonality would suffice). This embodies a parallel trends assumption, conditional on $\mathbf{Z}, \mathbf{D}$, and $\mathbf{F}$, as well as an assumption that employment mobility is "conditionally exogenous," i.e., employment mobility can depend on observable characteristics ($\mathbf{Z}$ and $\mathbf{h}$) and time-invariant observable and unobservable characteristics of workers and firms as captured by $\theta_{2,i}$ and $\psi_{2,J(i,t)}$, but not the errors $\epsilon_{2,it}$. Card et al. (2013) find considerable support for the exogenous mobility assumption in IEB data, of which the LIAB data comprise a sample. As discussed in Woodcock (2015), the specification with match effects relaxes the exogenous mobility assumption by allowing employment mobility to depend on time-invariant observable and unobservable match-specific characteristics (e.g., match quality) as well.

Abowd et al. (1999) and Abowd et al. (2002) show that the worker and establishment effects in eq. (5) are only identified within a "connected set" of establishments that are linked by worker mobility. To simplify estimation and ensure comparability of our estimates across specifications, we restrict our analysis to the largest connected set of establishments. Table 3 reports summary statistics for the full sample and the largest connected set. For men, the largest connected set comprises over 99 percent of observations in the full sample of full-time men, representing 97 percent of individuals and 94 percent of establishments. For women, the largest connected set of comprises a slightly portion of the full sample of full-time women: about 95.5 percent of observations, representing 90 percent of individuals and 82 percent of establishments. For both sexes, means and sample proportions of observable characteristics in the largest connected set are indistinguishable from the full sample. Consequently, there is little or no loss in focusing attention on the largest connected group for the remainder of this article.

5 Results

We estimate eqs. (1), (2), (5), and (8) separately for men and women. Table 4 presents estimates of α, γ, and δ – the coefficients on our displacement indicator and its interactions with the Hartz indicators – for each of these specifications, conditional on education (5 categories), a cubic polynomial in age, and the interaction between age and education. Estimates in column (1) indicate that prior to the Hartz reforms, recently displaced male and female workers faced a steep wage loss of nearly 25 log points upon finding re-employment. The Hartz reforms substantially increased the magnitude of the wage loss: by 15 log points for men, and by 8 log points for women. As shown in column (2), almost all of the pre-Hartz wage loss resulting from displacement was explained by selection: once we control for unobserved individual heterogeneity, the recently displaced men earned almost exactly the same as their non-displaced counterparts; while recently displaced women faced a wage gap of about 2 log points. However very little of the post-Hartz increase in the wage loss from displacement – only about 1.5 log points – is accounted for by a selection effect. Most of the increased wage loss, nearly 10 log points for men and 5 log points for women, is because recently displaced workers sort into employment in lower-paying firms following the Hartz reforms. Most of the remainder, 2.7 log points for men and 1.6 log points for women, is attributable to recently displaced workers sorting into lower-paying matches. The selection, sorting, and matching effects entirely account for the 8 log point increase in the wage loss that women faced following the Hartz reforms, and account for all but 1.3 log points of the 15 log point wage loss faced by men.

To better understand these findings it is helpful to further investigate the individual components of wages, and how they have evolved over the sample period. Table 5 summarizes the distribution of individual and establishment effects estimated from eq. (5), and the distribution of combined individual-establishment match effects estimated from eq. (8). The average value of the individual fixed effect, θ_i, among recently displaced individuals is substantially below the sample average: more than 16 log points for men, which is roughly half of a standard deviation of the distribution of θ_i; and 11 log points for women (roughly one third of a standard deviation). This helps to explain why controlling for unobserved individual heterogeneity reduces the pre-Hartz estimate of the post-displacement wage loss in Table 4 nearly to zero: on average, recently displaced individuals earn relatively low wages in all of their jobs – and not only following displacement. The recently displaced are also employed in firms that pay all of their workers below-average wages: the average establishment effect, $\psi_{J(i,t)}$, among recently displaced individuals is nearly 20 log points below the sample average for men, and 17 log points below the mean for women. **<NTD: is this also true before displacement?>**

Figures 3 and 4 plot the mean wage components for recently displaced workers vs. all others over the 1994-2014 period. **<TBD when results released>**

The estimates in Table 4 are robust to alternate definitions of recent displacement. In Appendix Table 1, we present comparable estimates based on less strict definitions of recent displacement; specifically, involuntary displacement from employment in the preceding 8, 12, or 20 quarters. In every case, the estimates are extremely similar to those presented in Table 5. The only notable exception is that estimated selection effects are slightly smaller than in Table 4 (approximately zero for men, and around one log point for women), and the estimated matching effects are slightly larger (between 3 and 4 log points for men, and roughly 2 log points for women). The fact that the wage losses from displacement vary so little depending on whether we use the 4, 8, 12, or 20 quarter measure suggests that the wage effect of displacement are very persistent, at least over a five year horizon.

In Appendix Table 2, we present comparable estimates based on a stricter definition of displacement; namely displacement due to establishment closure. We define an individual as displaced due to closure if they were displaced in the final year that the establishment appears in our data. This measure is imperfect. Establishments are identified via a unique ID number. However, as noted by

Card et al. (2013), an establishment is issued a new ID number if it changes ownership. As a consequence, some of our establishment "closures" are really just ownership changes. Indeed, using data on worker flows between establishments, Schmieder and Hethey (2010) estimate that only about half of firm ID "deaths" in the IEB are true establishment closings.[13] Nevertheless, this provides a much more conservative definition of involuntary displacement (only about 6 percent of displacements in our data meet our definition of displacement due to establishment closure). If our main measure of displacement erroneously classifies some voluntary job changes as involuntary displacement, then this more conservative measure should reduce any bias due to misclassification. In fact, we find that it has almost no effect on our estimates, as show in Appendix Table 2. Estimates for men in that table are virtually identical to those in Table 4. However the stricter definition of displacement yields a slightly larger estimate of the overall wage loss following the Hartz reforms – around 12 log points – for women. The estimated selection, sorting, and matching effects are each around 1 log point larger than in Table 4 as a consequence.

A possible source of concern is that our estimates in Table 4 capture not only the effect of the Hartz reforms, but also of the subsequent financial crisis. To address this concern, we restrict our sample to the period 1993-2008 and re-estimate the specifications underlying Table 4. The results, in Table 6, are virtually identical to those in Table 4. A related concern is that even though we restrict our sample to the former West Germany, the early part of our sample period could be affected by the turmoil of the early years re-unification. To address this concern and concerns about the effects of the financial crisis simultaneously, we restrict our sample further to the period 1998-2008 and re-estimate the specifications underlying Table 4. The results, in Table 7, are once again virtually identical to those in Table 4. We conclude that our estimates are driven neither by the lingering effects of re-unification, nor the financial crisis.

A final source of concern is that our results could be driven by important omitted variables. Table 8 presents estimates from several alternative specifications that address such concerns. Our empirical specification in Table 4 does not control for employer tenure. If the wage cost of displacement

[13] For the purposes of estimating wage models with fixed establishment effects, we believe it is appropriate to treat an ownership change as a potential change in the firm-specific component of wages, even when a establishment remains open. A new owner, for example, might introduce a bonus system that changes firm-specific component of wages. In cases where a new establishment ID is assigned to a continuing business enterprise, there is no bias in treating the old and new new IDs as different establishments. However there is a potential loss of efficiency because the old and new establishments might have the same wage structure.

is substantially due to the loss of accumulated match-specific human capital represented by the return to job tenure, then we may be overestimating the post-displacement wage loss. And if the return to job tenure increased over the sample period, we might erroneously attribute resulting the increase in the cost of displacement to the Hartz reforms. Column (1) of Table 8 replicates the specification from column (1) of Table 4, but with the addition of a control for employer tenure and its interactions with our Hartz dummies. This substantially reduces the pre-Hartz estimate of the gross wage loss of displacement, but only slightly reduces the estimated post-Hartz effect. A similar concern is that establishment effects might simply be capturing wage differences between industrial sectors, and consequently that our estimated sorting effect might simply capture the effect of post-displacement sorting across sectors rather than establishments *per se*. To address this, column (2) of Table 8 replicates the specification from column (1) of Table 4, but with the addition of 222 sector fixed effects. While this further reduces the estimated post-displacement wage loss, both before and after Hartz, the magnitude of the reduction is a fraction of that obtained by introducing establishment effects. This implies that some of our estimated sorting effect reflects re-allocation across sectors, but most of the sorting effect occurs between establishments within sectors. **NTD: can quantify this.** A closely related concern is that match effects might simply capture wage differences attributable to observables that vary at the level of the worker-establishment match. Occupation is the primary suspect. Column (3) adds fixed effects for 342 occupation categories to the specification from column (1) of Table 4. Again, we see a slight reduction in the estimated wage loss of displacement, but not enough to cause concern about the specification in Table 4. Column (4) of Table 8 adds all three additional controls – tenure, sector, and occupation – and column (5) adds all three plus individual fixed effects. These additional controls fail to fully explain the increase in the post-displacement wage loss following the introduction of the Hartz reforms, and thus do not change our conclusion that Hartz increased the cost of displacement, principally by changing the way that displaced workers are sorted across firms.

JLS <TBD when results released>

6 Conclusion

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A Data Appendix

To be completed.

- 02.01: create education categories per CHK, identify east/west based on establishment geography, & top-coded records

- 02.02: create annual totals of earnings & days worked for each job, maximal reported education & occupation for the job-year, identify the main job for the year (highest total earnings in that year)

- 02.03: construct variables needed for imputation: age categories (6); leave-out individual means of annual earnings & censoring rate (= overall annual averages if individual only appears once & add indicator for this); leave-out firm means of same, and proportion with a university degree (= overall annual averages if firm appears only once); grab a bunch of additional firm characteristics from the BHP (replace within-firm median wage with mean when missing, and add dummy for this)

- 02.04: imputation; method follows Card et al. (2013). at the annual level to avoid spurious quarter-to-quarter variation. separate Tobit models by: sex, east/west, regular (subject to Social Security and not a trainee/intern), FT, age, year, education. control variables are age, leave-out individual means of annual earnings & censoring rate, total number of full-time employees at firm, total number of female employees at firm, total number of full-time female employees at firm, total number of #high/medium/low skill employees at firm, leave-out firm means of annual earnings & censoring rate & proportion with university degree, indicators for individuals and firms that appear only once in our sample, firm's median daily wage & an indicator when missing, indicator for Berlin, indicator for main job this year; if group contains $<$ 500 obs, collapse into one of 20 supergroups by sex, east/west & education – remaining group variables get dummied out & added to controls, and interact control variables (minus person & firm censoring rates & firm skill measures) with age & FT; estimate tobit model and impute with draw from the upper tail, conditional on observables.

- 02.05: collapse wage records to quarterly frequency. create quarterly measures of employer tenure & experience. identify main job in the quarter as the one where earned the most. We calculate the average daily wage in that quarter by dividing total earnings by the duration of the job spell (including weekends and holidays). restrict sample to main job.

- 02.06: validate imputation per Card et al. (2013)

- 02.07 & 02.08: identify separations from employment. separation is deemed displacement if it is followed by a UI spell within 12 weeks. A displacement is due to closure if it occurs in the last year that the establishment appears in the data

- 03.01 real daily wage. restrict to largest mobility group. compute quarters since displacement (0 in quarter of displacement), and dummies for displacement in last Q quarters. estimate main models.

B Imputation Validation

To be completed.

To evaluate the quality of our imputation procedure, we follow Card et al. (2013) and undertake a validation exercise in which we artificially censor the upper tail of the wage distribution for a group of workers with a very low censoring rate, and then apply our imputation procedure to the artificially censored data. The validation group is male workers age 20-29 with an apprenticeship education. These workers have an average censoring rate of **XXX%** between 1993 and 2014. We artificially censor their wages at 60th, 70th, 80th, 90th percentiles of wages, and then impute the artificially censored wages using the algorithm described in Appendix A.

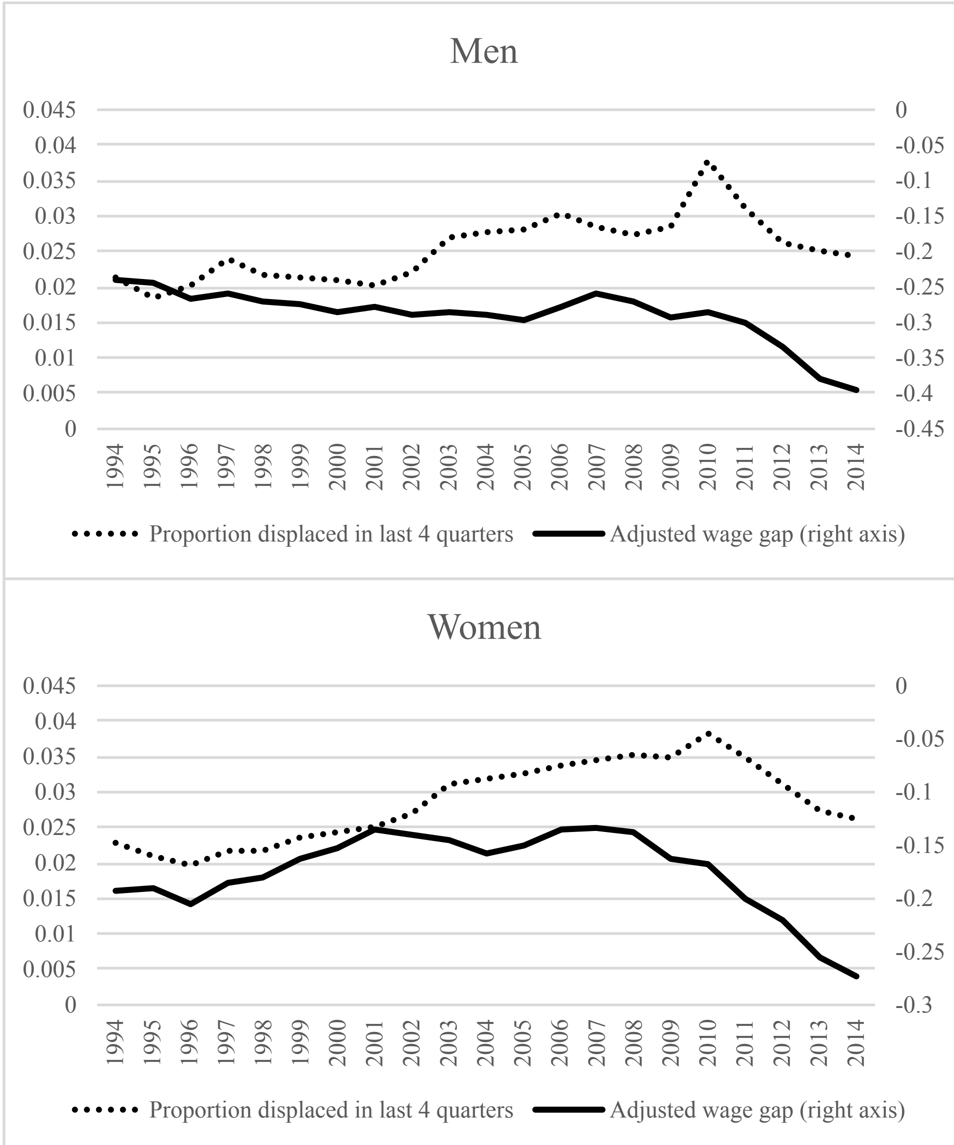
Appendix Table **XXX** shows the actual mean and standard deviation of wages of the validation group, and the means and standard deviations from the artificially censored and imputed samples. The means and standard deviations are **uniformly higher than the actual data values, with a larger upward bias at higher censoring rates**. **For example ... trend ... this leads us to conclude ...**

To ensure that our imputation preserves the proportion of within- vs. between-establishment wage variation, we also fit simple linear regression models with fixed year and establishment effects to the raw data and the artificially censored and imputed data. The sample has 1,296,409 quarterly observations on the employees of 155,673 establishments. R^2 in the actual data is 0.656, vs. 0.645 when the artificial censoring rate is 10 percent, 0.638 when it is 20%, 0.630 when it is 30%, and 0.620 when it is 40%. Thus the imputation method successfully maintains the relative share of within-establishment wage variation.

Figure 1: Unemployment and GDP Growth

Notes: The dotted line shows the year-over-year percentage change in Gross Domestic Product, as reported by the OECD (doi: 10.1787/b86d1fc8-en, Accessed on 08 June 2018). The solid line shows annual averages of the unemployment rate, as reported by the OECD (doi: 10.1787/997c8750-en, Accessed on 08 June 2018).

Figure 2: Recent Displacement from Employment



Notes: The dotted line in each panel shows the proportion of observations in each year in which the worker was displaced from employment in the preceding four quarters. The solid line in each panel shows the estimated wage difference between individuals displaced in the preceding four quarters and all others, conditional on education and age. To obtain values on the solid line, we regressed log real wages on an indicator for displacement in the preceding four quarters, calendar quarter, education (5 categories), a cubic polynomial in age, and age interacted with education, in each year of our data; the plotted points are the estimated coefficients on the displacement indicator in each year's regression.

Table 1
Summary of Wage Data

	Full-time Men						Full-time Women					
		Log real wage, unallocated			Log real wage, allocated			Log real wage, unallocated			Log real wage, allocated	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Number of Observations	Mean	Std. Dev	Percent censored	Mean	Std. Dev	Number of Observations	Mean	Std. Dev	Percent censored	Mean	Std. Dev
All years	36,151,699	4.71	0.359	14.5	4.77	0.462	11,457,161	4.47	0.449	4.57	4.49	0.488
1993	1,284,336	4.69	0.272	13.4	4.74	0.355	385,787	4.42	0.379	2.97	4.44	0.407
1994	1,317,052	4.67	0.278	12.6	4.72	0.362	399,171	4.41	0.373	2.65	4.42	0.395
1995	1,386,050	4.70	0.284	12.9	4.75	0.366	419,301	4.44	0.375	2.81	4.46	0.405
1996	1,426,502	4.70	0.289	12.6	4.73	0.359	433,680	4.44	0.375	2.53	4.45	0.392
1997	1,460,550	4.69	0.301	13.3	4.73	0.388	441,168	4.44	0.387	2.91	4.45	0.408
1998	1,535,485	4.71	0.311	13.0	4.75	0.392	459,756	4.45	0.399	3.13	4.47	0.426
1999	1,496,963	4.71	0.320	15.9	4.78	0.435	470,844	4.46	0.417	4.14	4.48	0.450
2000	1,652,113	4.72	0.318	15.1	4.78	0.413	504,029	4.47	0.423	4.21	4.48	0.454
2001	1,693,825	4.72	0.322	14.6	4.76	0.389	521,587	4.47	0.430	4.36	4.48	0.457
2002	1,694,164	4.72	0.328	17.5	4.80	0.449	527,099	4.46	0.440	5.59	4.49	0.486
2003	1,684,530	4.75	0.357	12.4	4.79	0.432	521,845	4.48	0.455	3.46	4.49	0.481
2004	1,662,648	4.73	0.364	13.0	4.79	0.456	515,993	4.47	0.463	3.87	4.49	0.493
2005	1,653,128	4.73	0.372	12.9	4.78	0.461	517,607	4.47	0.473	3.94	4.48	0.504
2006	1,681,582	4.72	0.394	13.6	4.77	0.481	537,539	4.46	0.485	4.24	4.47	0.517
2007	1,738,260	4.70	0.400	13.7	4.75	0.491	564,650	4.44	0.494	4.38	4.45	0.527
2008	1,785,958	4.69	0.405	15.2	4.76	0.515	591,481	4.43	0.500	5.02	4.45	0.541
2009	1,755,589	4.70	0.404	14.1	4.75	0.487	596,659	4.46	0.498	4.86	4.47	0.535
2010	1,778,496	4.70	0.416	14.9	4.76	0.519	612,138	4.46	0.501	5.32	4.48	0.546
2011	1,837,904	4.70	0.406	16.3	4.77	0.516	595,255	4.50	0.474	6.45	4.53	0.525
2012	1,871,645	4.72	0.397	16.7	4.79	0.520	611,649	4.52	0.456	6.93	4.55	0.515
2013	1,879,135	4.74	0.391	15.9	4.83	0.558	615,767	4.55	0.449	6.50	4.58	0.513
2014	1,875,784	4.76	0.389	16.0	4.85	0.557	614,156	4.57	0.447	6.67	4.60	0.511

Notes: Sample includes full-time employees working in non-marginal jobs in the former West Germany, age 25-65. Data are aggregated to quarterly frequency. Real wage is based on average daily earnings at the full-time job with the highest total earnings that quarter, adjusted for inflation using the 2010 Consumer Price Index. Unallocated wage data in columns (2), (3), (8), and (9) are based on raw daily wages as reported in the LIAB, which are censored at the social security maximum for the corresponding year. The percentage of observations censored at this threshold is shown in columns (4) and (10). Censored wage observations have been stochastically imputed using Tobit models to produce the allocated wage data in columns (5), (6), (11), and (12) have had c

Table 2
Summary Statistics: Recently Displaced Workers vs. Others

				Education				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Percent	Log real wage	Age	Missing	No vocational qualification	Vocational qualification	Upper secondary certificate (Abitur)	University degree
Panel A: Men								
Displaced in last 4 quarters	2.52	4.32	37.2	24.2	13.0	51.9	3.55	7.38
Not displaced in last 4 quarters	97.5	4.79	41.1	11.9	11.0	56.3	5.4	15.4
Displaced in last 8 quarters	4.94							
Displaced in last 12 quarters	7.12							
Displaced in last 20 quarters	11.0							
Panel B: Women								
Displaced in last 4 quarters	2.86	4.18	37.3	24.8	12.6	44.4	7.18	11.0
Not displaced in last 4 quarters	97.1	4.52	39.4	12.8	13.5	51.6	9.83	12.3
Displaced in last 8 quarters	5.74							
Displaced in last 12 quarters	8.35							
Displaced in last 20 quarters	12.9							

Notes: Sample includes full-time employees working in non-marginal jobs in the former West Germany, age 25-65, aggregated to quarterly frequency. Column (5) reports the sample percent with less than an upper secondary school certificate, and no vocational qualification. Column (6) reports the sample percent with less than an upper secondary school certificate, and a vocational qualification. Column (8) reports the sample percent with a degree from a Fachhochschule or university.

Table 3

Summary Statistics for Overall Sample and Individuals in the Largest Connected Set

	Full-Time Men		Full-Time Women	
	(1)	(2)	(3)	(4)
	Full Sample	Largest Connected Set	Full Sample	Largest Connected Set
ln(real daily wage)	4.77	4.78	4.49	4.51
Employer Tenure (months)	122	122	97.7	99.0
Age (years)	41.0	41.0	39.2	39.3
Number of jobs this quarter	1.03	1.03	1.04	1.04
Year	2004	2004	2004	2004
Quarter	2.50	2.50	2.50	2.50
Displaced in last 4 quarters (proportion)	0.025	0.025	0.029	0.028
Displaced in last 8 quarters (proportion)	0.049	0.049	0.057	0.057
Displaced in last 12 quarters (proportion)	0.071	0.071	0.084	0.082
Displaced in last 20 quarters (proportion)	0.110	0.109	0.129	0.127
Education				
Missing	0.124	0.122	0.136	0.131
No upper secondary, no vocational certificate	0.110	0.110	0.132	0.135
No upper secondary, with vocational certificate	0.561	0.561	0.516	0.514
Upper secondary certificate (Abitur)	0.054	0.054	0.096	0.098
Degree from Fachhochschule or university	0.152	0.152	0.121	0.122
Number of observations	36,151,699	35,839,183	11,457,161	10,937,896
Number of individuals	764,634	741,838	377,759	341,328
Number of establishments	441,846	413,850	261,173	214,702
Number of individual-establishment matches		2,094,649		813,234
Mean number of matches/individual		2.98		2.74
Mean number of matches/establishment		7,467		986
Proportion of individuals with only one match		0.355		0.360
Proportion of establishments with only one match		0.046		0.087

Notes: Sample includes full-time employees working in non-marginal jobs in the former West Germany, age 25-65, aggregated to quarterly frequency. Daily wages are deflated to 2010 euros using the CPI, and censored values are imputed using a Tobit model. "Connected set" refers a group of observations connected by worker mobility (see Abowd, Creecy, and Kramarz 2002 for details).

Table 4

Estimated Effect of the Hartz Reforms on Post-Displacement Wages

	(1)	(2)	(3)	(4)
Panel A: Full-time Men				
Recently displaced	-0.248***	0.006***	0.003***	-0.004**
	(0.001)	(0.001)	(0.001)	(0.001)
Recently displaced × during Hartz	-0.088***	-0.086***	-0.025***	-0.010***
	(0.002)	(0.001)	(0.001)	(0.001)
Recently displaced × after Hartz	-0.150***	-0.138***	-0.040***	-0.013***
	(0.002)	(0.001)	(0.001)	(0.001)
Selection effect		-0.012***		
		[0.000]		
Sorting effect			-0.098***	
			[0.000]	
Matching effect				-0.027***
				[0.000]
R-squared	0.342	0.824	0.879	0.908
RMSE of Residual	0.374	0.195	0.163	0.142
Panel B: Full-time Women				
Recently displaced	-0.233***	-0.021***	-0.011***	-0.016***
	(0.003)	(0.002)	(0.001)	(0.001)
Recently displaced × during Hartz	-0.057***	-0.050***	-0.013***	-0.002
	(0.004)	(0.003)	(0.002)	(0.002)
Recently displaced × after Hartz	-0.081***	-0.064***	-0.016***	0.000
	(0.003)	(0.002)	(0.002)	(0.001)
Selection effect		-0.016***		
		[0.000]		
Sorting effect			-0.048***	
			[0.000]	
Matching effect				-0.016***
				[0.000]
R-squared	0.197	0.783	0.873	0.904
RMSE of Residual	0.428	0.222	0.174	0.152
Year & Quarter effects	YES	YES	YES	YES
Age & Education controls	YES	YES	YES	YES
Individual Effects		YES	YES	
Establishment Effects			YES	
Match Effects				YES

Notes: Columns (1) reports OLS estimates based on eq. (1). Columns (2), (3), and (4) report OLS estimates based on eqs. (2), (5) and (8), respectively. Individuals are defined as recently displaced if they were displaced from employment in the previous four quarters. Standard errors are clustered by individual and reported in parentheses. Hausman test p-values are reported in square brackets. *** indicates statistical significance at the 1 percent level, ** indicates significance at the 5 percent level, and * indicates significance at the 10 percent level. Panels A and B are based on separate regressions of full-time men and women in the largest connected sets. See Table 3 for the number of observations, workers, establishments, and matches in the largest connected sets of men and women; and notes to Table 3 for information about sample composition.

Table 5
Summary of Wage components

	All		Recently Displaced		Non-Displaced	
	(1)	(2)	(3)	(4)	(5)	(6)
	Mean	Std Dev	Mean	Std Dev	Mean	Std Dev
Panel A: Men						
ln(real daily wage)	4.78	0.460	4.32	0.414	4.79	0.456
Individual Effect (θ)	0.000	0.320	-0.165	0.271	0.004	0.320
Establishment Effect (ψ)	0.000	0.209	-0.198	0.279	0.005	0.205
Correlation (θ,ψ)	0.075		0.033		0.065	
Combined Match Effect (Φ)	0.000	0.411	-0.370	0.411	0.009	0.407
Panel B: Women						
ln(real daily wage)	4.51	0.477	4.18	0.461	4.52	0.474
Individual Effect (θ)	0.000	0.349	-0.110	0.335	0.003	0.349
Establishment Effect (ψ)	0.000	0.274	-0.165	0.326	0.005	0.271
Correlation (θ,ψ)	-0.020		-0.065		-0.024	
Combined Match Effect (Φ)	0.000	0.453	-0.279	0.467	0.008	0.450

Notes: Person and firm effects are based on OLS esitmates of eq. (5). Combined match effects are based on OLS esimates of eq. (8). The individual, establishment, and match effects are all normalized to have zero mean in the sample. Individuals are defined as recently displaced if they were displaced from employment in the previous four quarters. Panels A and B are based on separate regressions of full-time men and women in the largest connected sets. See Table 3 for the number of observations, workers, establishments, and matches in the largest connected sets of men and women; and notes to Table 3 for information about sample composition.

Table 6
Model Estimates for the Restricted Sample Period, 1993-2008

	(1)	(2)	(3)	(4)
Panel A: Full-time Men				
Recently displaced	-0.248***	-0.013***	-0.007***	-0.008***
	(0.001)	(0.001)	(0.001)	(0.001)
Recently displaced × during Hartz	-0.087***	-0.085***	-0.025***	-0.011***
	(0.002)	(0.001)	(0.001)	(0.001)
Recently displaced × after Hartz	-0.129***	-0.136***	-0.045***	-0.018***
	(0.002)	(0.002)	(0.001)	(0.001)
Selection effect		0.007***		
		[0.000]		
Sorting effect			-0.091***	
			[0.000]	
Matching effect				-0.027***
				[0.000]
R-squared	0.343	0.841	0.890	0.912
RMSE of Residual	0.346	0.172	0.144	0.128
Panel B: Full-time Women				
Recently displaced	-0.225***	-0.026***	-0.015***	-0.017***
	(0.003)	(0.002)	(0.001)	(0.001)
Recently displaced × during Hartz	-0.054***	-0.047***	-0.009***	-0.001
	(0.004)	(0.003)	(0.002)	(0.002)
Recently displaced × after Hartz	-0.076***	-0.071***	-0.020***	-0.001
	(0.004)	(0.003)	(0.002)	(0.002)
Selection effect		-0.005***		
		[0.090]		
Sorting effect			-0.050***	
			[0.000]	
Matching effect				-0.019***
				[0.000]
R-squared	0.193	0.808	0.885	0.908
RMSE of Residual	0.406	0.202	0.157	0.140
Year & Quarter effects	YES	YES	YES	YES
Age & Education controls	YES	YES	YES	YES
Individual Effects		YES	YES	
Establishment Effects			YES	
Match Effects				YES

Notes: Columns (1) reports OLS estimates based on eq. (1). Columns (2), (3), and (4) report OLS estimates based on eqs. (2), (5) and (8), respectively. Individuals are defined as recently displaced if they were displaced from employment in the previous four quarters. Standard errors are clustered by individual and reported in parentheses. Hausman test p-values are reported in square brackets. *** indicates statistical significance at the 1 percent level, ** indicates significance at the 5 percent level, and * indicates significance at the 10 percent level. Sample size for Panel A is 24,620,170 observations on 610,109 individual, 273,879 establishments, in 1,310,773 individual-establishment matches. Sample size for Panel B is 7,227,326 observations on 255,395 individuals, 133,465 establishments, in 486,180 individual-establishment matches. See notes to Table 3 for information about sample composition.

Table 7
Model Estimates for the Restricted Sample Period, 1998-2008

	(1)	(2)	(3)	(4)
Panel A: Full-time Men				
Recently displaced	-0.268***	0.002***		-0.006***
	(0.002)	(0.001)		(0.001)
Recently displaced ✕ during Hartz	-0.064***	-0.076***		-0.009***
	(0.002)	(0.001)		(0.001)
Recently displaced ✕ after Hartz	-0.107***	-0.132***		-0.017***
	(0.002)	(0.002)		(0.001)
Selection effect		0.025***		
		[0.000]		
Sorting effect			n/a	
			[n/a]	
Matching effect				n/a
				[n/a]
R-squared	0.345	0.866		0.924
RMSE of Residual	0.362	0.166		0.125
Panel B: Full-time Women				
Recently displaced	-0.229***	-0.017***		-0.017***
	(0.003)	(0.002)		(0.001)
Recently displaced ✕ during Hartz	-0.052***	-0.045***		-0.001
	(0.004)	(0.003)		(0.002)
Recently displaced ✕ after Hartz	-0.074***	-0.069***		-0.001
	(0.005)	(0.003)		(0.002)
Selection effect		-0.005***		
		[0.096]		
Sorting effect			n/a	
			[n/a]	
Matching effect				n/a
				[n/a]
R-squared	0.204	0.839		0.920
RMSE of Residual	0.419	0.193		0.137
Year & Quarter effects	YES	YES	YES	YES
Age & Education controls	YES	YES	YES	YES
Individual Effects		YES	YES	
Establishment Effects			YES	
Match Effects				YES

Notes: Columns (1) reports OLS estimates based on eq. (1). Columns (2), (3), and (4) report OLS estimates based on eqs. (2), (5) and (8), respectively. Individuals are defined as recently displaced if they were displaced from employment in the previous four quarters. Standard errors are clustered by individual and reported in parentheses. Hausman test p-values are reported in square brackets. *** indicates statistical significance at the 1 percent level, ** indicates significance at the 5 percent level, and * indicates significance at the 10 percent level. Sample size for Panel A is 17,770,133 observations on 593,805 individual, **XXX establishments**, in 1,103,202 individual-establishment matches. Sample size for Panel B is 5,199,447 observations on 231,364 individuals, **XXX establishments,** in 399,718 individual-establishment matches. See notes to Table 3 for information about sample composition.

Table 8
Robustness to Controls for Tenure, Sector, and Occupation

	(1)	(2)	(3)	(4)	(5)
Panel A: Full-time Men					
Recently displaced	-0.177***	-0.116***	-0.165***	-0.075***	0.004***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Recently displaced × during Hartz	-0.073***	-0.052***	-0.093***	-0.044***	-0.052***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)
Recently displaced × after Hartz	-0.122***	-0.086***	-0.152***	-0.068***	-0.077***
	(0.002)	(0.001)	(0.002)	(0.001)	(0.001)
R-squared	0.386	0.494	0.508	0.615	0.847
RMSE of Residual	0.360				
Panel B: Full-time Women					
Recently displaced	-0.128***	-0.119***	-0.163***	-0.066***	-0.009***
	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)
Recently displaced × during Hartz	-0.058***	-0.033***	-0.064***	-0.035***	-0.033***
	(0.004)	(0.003)	(0.003)	(0.003)	(0.002)
Recently displaced × after Hartz	-0.083***	-0.047***	-0.088***	-0.047***	-0.040***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)
R-squared	0.265	0.385	0.377	0.505	0.827
RMSE of Residual					
Year & Quarter effects	YES	YES	YES	YES	YES
Age & Education controls	YES	YES	YES	YES	YES
Employer Tenure controls	YES			YES	YES
Sector controls		YES		YES	YES
Occupation controls			YES	YES	YES
Individual Effects					YES

Notes: Columns (1) reports OLS estimates based on eq. (1). Columns (2), (3), and (4) report OLS estimates based on eqs. (2), (5) and (8), respectively. Individuals are defined as recently displaced if they were displaced from employment in the previous four quarters. Standard errors are clustered by individual and reported in parentheses. Hausman test p-values are reported in square brackets. *** indicates statistical significance at the 1 percent level, ** indicates significance at the 5 percent level, and * indicates significance at the 10 percent level. Sample size for Panel A is 17,770,133 observations on 593,805 individual, **XXX establishments**, in 1,103,202 individual-establishment matches. Sample size for Panel B is 5,199,447 observations on 231,364 individuals, **XXX establishments,** in 399,718 individual-establishment matches. See notes to Table 3 for information about sample composition.

Appendix Table 1

Regression Estimates for Alternate Definitions of Recent Displacement

	Full-Time Men				Full-Time Women			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: 8 quarter displacement measure								
Recently displaced	-0.254***	-0.002***	0.002***	-0.003***	-0.231***	-0.020***	-0.008***	-0.013***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)
Recently displaced × during Hartz	-0.096***	-0.096***	-0.028***	-0.009***	-0.065***	-0.060***	-0.014***	-0.001
	(0.002)	(0.001)	(0.001)	(0.001)	(0.003)	(0.002)	(0.002)	(0.001)
Recently displaced × after Hartz	-0.143***	-0.140***	-0.044***	-0.010***	-0.078***	-0.068***	-0.019***	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.003)	(0.002)	(0.001)	(0.001)
R-squared	0.353	0.825	0.879	0.908	0.205	0.783	0.873	0.904
Panel B: 12 quarter displacement measure								
Recently displaced	-0.252***	-0.007***	0.003***	0.001***	-0.226***	-0.020***	-0.006***	-0.010***
	(0.001)	(0.001)	(0.001)	(0.000)	(0.002)	(0.001)	(0.001)	(0.001)
Recently displaced × during Hartz	-0.100***	-0.099***	-0.030***	-0.009***	-0.068***	-0.063***	-0.014***	0.001
	(0.002)	(0.001)	(0.001)	(0.001)	(0.003)	(0.002)	(0.001)	(0.001)
Recently displaced × after Hartz	-0.141***	-0.141***	-0.049***	-0.012***	-0.079***	-0.069***	-0.021***	0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.003)	(0.002)	(0.001)	(0.001)
R-squared	0.362	0.826	0.879	0.908	0.211	0.790	0.873	0.904
Panel C: 20 quarter displacement measure								
Recently displaced	-0.244***	-0.016***	0.004***	0.007***	-0.212***	-0.017***	-0.001	-0.001
	(0.001)	(0.001)	(0.000)	(0.000)	(0.002)	(0.001)	(0.001)	(0.001)
Recently displaced × during Hartz	-0.103***	-0.097***	-0.034***	-0.010***	-0.073***	-0.064***	-0.019***	-0.003***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.003)	(0.002)	(0.001)	(0.001)
Recently displaced × after Hartz	-0.151***	-0.148***	-0.063***	-0.019***	-0.095***	-0.078***	-0.031***	-0.007***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)	(0.001)
R-squared	0.374	0.827	0.879	0.903	0.220	0.790	0.873	0.904
Year & Quarter effects	YES	YES	YES	YES	YES	YES	YES	YES
Age & Education controls	YES	YES	YES	YES	YES	YES	YES	YES
Individual Effects		YES	YES			YES	YES	
Establishment Effects			YES				YES	
Match Effects				YES				YES

Notes: Columns (1) and (5) report OLS estimates based on eq. (1). Columns (2) and (6) report OLS estimates based on eq. (2). Columns (3) and (7) report estimates based on eq. (5) and columns (4) and (8) present estimates based on eq. (8). In Panel A, individuals are defined as recently displaced if they were displaced from employment in the previous eight quarters. In Panels B and C, individuals are defined as recentlt displaced if they were displaced from employment in the previous 12 and 20 quarters, respectively. Standard errors are clustered by individual and reported in parentheses. *** indicates statistical significance at the 1 percent level, ** indicates significance at the 5 percent level, and * indicates significance at the 10 percent level. See Table 3 for the number of observations, workers, establishments, and matches in the largest connected sets of men and women; and notes to Table 3 for information about sample composition.

Appendix Table 2
Regression Estimates Based on Stricter Definition of Involuntary Displacement

	(1)	(2)	(3)	(4)
Panel A: Full-time Men				
Recently displaced due to establishment closure	-0.244***	0.004	0.005**	-0.003
	(0.004)	(0.003)	(0.002)	(0.002)
Recently displaced due to establishment closure ✕ during Hartz	-0.071***	-0.081***	-0.024***	-0.008**
	(0.008)	(0.005)	(0.004)	(0.003)
Recently displaced due to establishment closure ✕ after Hartz	-0.148***	-0.134***	-0.040***	-0.011***
	(0.006)	(0.004)	(0.003)	(0.003)
Selection effect		-0.013***		
		[0.002]		
Sorting effect			-0.094***	
			[0.000]	
Matching effect				-0.029***
				[0.000]
R-squared	0.329	0.824	0.879	0.907
RMSE of Residual	0.377	0.195	0.163	0.142
Panel B: Full-time Women				
Recently displaced due to establishment closure	-0.229***	-0.007	-0.010**	-0.022***
	(0.010)	(0.007)	(0.005)	(0.004)
Recently displaced due to establishment closure ✕ during Hartz	-0.065***	-0.052***	-0.013	0.005
	(0.016)	(0.011)	(0.008)	(0.007)
Recently displaced due to establishment closure ✕ after Hartz	-0.118***	-0.092***	-0.027***	0.001
	(0.013)	(0.009)	(0.006)	(0.006)
Selection effect		-0.026***		
		[0.006]		
Sorting effect			-0.066***	
			[0.000]	
Matching effect				-0.027***
				[0.000]
R-squared	0.187	0.782	0.873	0.904
RMSE of Residual	0.430	0.222	0.174	0.152
Year & Quarter effects	YES	YES	YES	YES
Age & Education controls	YES	YES	YES	YES
Individual Effects		YES	YES	
Establishment Effects			YES	
Match Effects				YES

Notes: Columns (1) reports OLS estimates based on eq. (1). Columns (2), (3), and (4) report OLS estimates based on eqs. (2), (5) and (8), respectively. Individuals are defined as recently displaced if they were displaced from employment due to establishment closure in the previous four quarters. Standard errors are clustered by individual and reported in parentheses. Hausman test p-values are reported in square brackets. *** indicates statistical significance at the 1 percent level, ** indicates significance at the 5 percent level, and * indicates significance at the 10 percent level. Panels A and B are based on separate regressions of full-time men and women in the largest connected sets. See Table 3 for the number of observations, workers, establishments, and matches in the largest connected sets of men and women; and notes to Table 3 for information about sample composition.