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DISCUSSION PAPER SERIES

IZA DP No. 13300

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ABSTRACT

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

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 experienced smaller wage losses than men. For both sexes, over 80 percent of the increased wage loss was because displaced workers found re-employment in lower-wage firms after the reforms. A disproportionate share of these low-wage firms offer temporary employment services to other firms, and we document a large increase in post-displacement employment in the temporary work sector after the reforms. Sorting into worse matches with employers explains a smaller 5-9 percent of the wage loss experienced by men, and 12.5-23 percent of the female wage loss. Collectively, the sorting and matching channels explain almost all of the Hartz reforms' effect on post-displacement wages.

JEL Classification: J65, J64, J62, J68, J63, J31, C23

Keywords: Hartz reforms, displacement, unemployment insurance, reallocation, sorting, matching, selection, linked employer-employee data, fixed effects

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1 Introduction

Between 2003 and 2005, the German government 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 regulations on temporary work, 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.[1]

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. Quantifying the relative magintude of selection, sorting, and matching effects helps to illuminate the specific channels through which the reforms affected the re-employment wages of displaced workers. Our decomposition approach is closely related to the Pendakur and Woodcock (2010) "Glass Door effect," the Gelbach (2016) decomposition, and the approach taken by Figueiredo et al. (2014) to estimate the effect of industrial agglomeration on worker-firm matching. It is also related to recent papers by Lachowska et al. (2018), Fackler et al. (2017), and others that seek to understand the role of employer-specific factors in the wage losses of displaced workers.

[1] Rinne and Zimmermann (2013), for example, argue that the reforms are primarily responsible for Germany's economic succcess since 2005. In contrast, Dustmann et al. (2014) argue that the reforms were a relatively minor contributor to that success.

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 wages of the continuously employed. Thus our basic empirical strategy is a difference-in-differences (DD) analysis in which we compare the post-unemployment wages of individuals who were displaced from employment and collected unemployment benefits to the wages of individuals who were continuously employed, before vs. after the Hartz reforms. Such comparisons allow us to isolate the effect of the reforms on the wages of displaced workers who received unemployment benefits.

The centerpiece of the reforms, known as Hartz IV, made unemployment benefits broadly less generous. This was especially true for the long-term unemployed. Standard search and matching models (see Rogerson et al. (2005) for a survey) predict that reducing the generosity of unemployment benefits will reduce an unemployed worker's reservation wage, and thus reduce the expected wage upon re-employment. To the extent that wages have persistent firm- and match-specific components (see Abowd et al. (1999, AKM hereafter) and Woodcock (2015) for supporting empirical evidence), we would therefore expect that less generous unemployment benefits will make unemployed workers more willing to accept employment at low-wage firms and/or in "worse" matches.[2] Despite the clear predictions of canonical models, however, the empirical evidence on the relationship between unemployment benefits and re-employment wages is decidedly mixed.[3] 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 a positive effect on re-employment wages, have 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, the lion's share of the increased post-

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

[3] 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 present a search model with duration dependence that reconciles these disparate findings.

displacement wage loss – roughly 84 percent – 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. A smaller portion of the increased post-displacement wage loss under Hartz – between five and nine percent for men, and between 12.5 and 23 percent for women – arises because displaced workers sort into worse matches with employers. Changes in the distribution of the unobserved characteristics of workers selected into displacement account for none of the wage loss experienced by women, and actually improved wage outcomes for displaced men. That is, we find evidence that men displaced after the Hartz reforms had unobserved characteristics that earned higher returns in the labor market than men displaced prior to the reforms, relative to their non-displaced counterparts, and this slightly increased their average post-displacement wage after the reforms. Collectively, these three channels explain almost all of the Hartz reforms' effect on post-displacement wages. Robustness checks indicate that our findings are not sensitive to specification or sample definition, are not explained by the subsequent financial crisis or the lingering effects of German re-unification, by reallocation across occupations, or changes to the returns to employer-specific human capital.

Our estimates reveal that sorting into employment at low-wage establishments was the primary cause of the post-reform increase in the cost of displacement. Roughly 40-45 percent of this sorting effect arises because displaced workers sort into employment in lower-wage sectors after the reforms. Chief among these is the temporary employment sector. We document a dramatic increase in post-displacement employment in this sector following the reforms, and present evidence that this was an important contributor to the increased post-displacement wage losses. In the last five years of our sample, for example, a startling 26 percent of men and 19 percent of women find employment at establishments that offer temporary employment services to other firms in the four quarters following displacement. These are very low wage jobs. The average job in this sector is associated with a firm-specific wage premium roughly two standard deviations below the overall mean in male employment, and more than one standard deviation below the mean in female employment. The rapid growth of temporary employment after displacement was almost certainly a direct consequence of the Hartz reforms, which largely deregulated the temporary employment sector and established an infrastructure for placing unemployed workers into temporary work via newly-legislated "Staff Service Agencies" (*Personal-Service-Agentur*, or PSAs).

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 5 presents the empirical results, and Section 6 concludes.

2 The Hartz Reforms and Related Literature

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: deregulating temporary work, dismissal, and fixed-term contracts; new measures to restructure and increase the effectiveness of local employment agencies; new "Staff Service Agencies" (*Personal-Service-Agentur*, or PSAs) that place unemployed workers in temporary work assignments; a new subsidy for entrepreneurs ("Me, Inc."); additional support for further vocational training; newly-defined "mini jobs" that are exempt from most social security taxes; and provisions to reduce unemployment benefits if an individual refused a "reasonable" job offer. The centerpiece of the reforms, dubbed Hartz IV, came into effect on January 1 2005 and was squarely targeted at reducing long-term unemployment. This phase of the reforms significantly restructured the unemployment and social assistance system. Hartz IV made benefits less generous for most unemployed individuals by reducing the amount and duration of benefits, and by making them conditional on stricter job search and acceptance requirements.

Prior to 2005, workers with sufficient pre-unemployment experience were entitled to an unemployment benefit (UB; *Arbeitslosengeld*) 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 the 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 (and thus smaller than the old UA benefit for most individuals). As a consequence of the Hartz IV reforms, therefore, 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, who 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.[4] 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. 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. Distinguishing between these channels is important for designing policy to mitigate the adverse consequences of job displacement, and for understanding the mechanism that underlies the reforms' effects on post-displacement wages. For example, if post-displacement wage losses are primarily due to selection on workers' unmeasured characteristics, then policies that target worker skills (e.g., retraining) may be most effective for mitigating wage losses. On the other hand, if post-displacement wage losses are primarily because workers return to employment at lower-wage firms, or enter into lower-quality matches with firms, then policies that facilitate search and improve matching outcomes may be more effective.

The decomposition approach that we develop in Section 4 is also related to recent papers in the displacement literature by Fackler et al. (2017), Lachowska et al. (2018), and Schmieder et al. (2018). These papers seek to explain the sources of post-displacement wage losses (in Germany and Washington state) using linked employer-employee data. All three papers highlight the importance of unobserved firm-specific characteristics, as captured by firm-specific fixed effects in wages, in explaining wage losses after displacement. Our decomposition approach formalizes this idea, characterizes it as a sorting effect, and extends the approach taken in those papers by controlling for unobserved match-specific heterogeneity as well. This allows us to further quantify the importance

[4]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.

of selection and matching in explaining post-displacement wage losses.

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 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 years 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 (excluding Berlin). We further exclude mini-jobs (which are only included in the IEB after 1999), jobs held by trainees and interns, and jobs in agriculture, mining, forestry, and fishing. 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 annualized data.

We compute each individual's total earnings at each establishment in each quarter and designate the establishment at which they earned 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 for men, and 1.04 for women), so we believe the restriction to one job per quarter is innocuous. We calculate the average daily wage in each quarter by dividing total quarterly 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 for full-time women. The trends in male and female average wages over the 1993-2014 period are remarkably similar: increasing by roughly 6 percentage points between 1993 and 2003, then declining by roughly 5 percentage points until 2008, and increasing again thereafter. For men, the 2008-2014 increase is 6.7 percentage points, whereas for women it is a substantially larger 13.5 percentage points. 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. In line with Card et al. (2013), wage dispersion increased for both men and women between 1993 and 2010, with the standard deviation of real daily wages increasing by about 14 log points for men and 12 log points for women over that period. That trend appears to have reversed since 2010, with the standard deviation of wages falling by 2.7 log points for men and 5.4 log points 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, 12.5 to 16.7 percent of male wage observations and 2.5 to 6.9 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 B, 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 mean log earnings and censoring rate in all other years, and the mean log 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 B for details). In Appendix B.2 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 percentage points vs. 2 percentage points for the mean, and 10 percentage points vs. 4 percentage points 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 selection into unemployment benefit receipt, which we use to define 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 male wage observations meet our definition of involuntary displacement 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: 2.8 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 (46 log points less for men, 32 log points less 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 is generally increasing throughout the early part of the sample period, reaching a peak of 3.8 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.

Finally, in keeping with the displacement literature, we restrict our sample to observations on recently displaced workers who had at least 24 months tenure with their employer at displacement, and a control group of workers who were not recently displaced and had at least 24 months tenure with their current employer. In robustness checks, we impose a stricter 36 month minimum tenure requirement.

Table 3 presents summary statistics for our estimation sample. Columns 1 and 4 present sample means for the full sample of LIAB observations on men and women, respectively, and columns 2 and

[5] Indeed, 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.

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

5 present the corresponding means after imposing the 24 month tenure restriction. As expected, those satisfying the tenure restriction earn slightly more and have longer tenure with their current employer than in the full sample. Perhaps mostly importantly, the incidence of displacement is lower among observations satisfying this restriction (about one percent) than in the full sample (about 2.5 percent) because most displacements occur early in the employment relationship. In robustness checks not reported here but available on request, we have verified that eliminating the minimum tenure restriction does not substantially affect our estimation results.

4 Empirical Strategy

The Hartz reforms affected all regions and applied to all workers. There is consequently no control group of workers that was unaffected by the reforms. However, because the reforms were primarily targeted at job search and unemployment benefits, we would expect them to have had the greatest impact on the behavior and outcomes of unemployed individuals who were actively searching for work, and to have had little or no effect on the wages of 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 wage change of recently displaced workers to the pre- vs. post-reform wage change of 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 with 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. As long as a 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 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 mean wages of recently displaced vs. non-displaced workers in each year of our sample. For both men and women, mean wages of displaced workers were essentially flat in the pre-reform period 1994-2002, and began falling when the first phase of the reforms was introduced in 2003. Mean wages of non-displaced workers, on the other hand, increased slightly during the pre-reform period (7.9 percent for men and 7.2 percent for women). While this does suggest that displaced and non-displaced workers experienced different pre-policy trends in wages, it is reassuring that we see no decline in the mean wages of displaced workers prior to the reforms, followed by a clear decline that coincides with the implementation of the reforms. Nevertheless, to assess the robustness of our estimates to violations of the parallel trends assumption, we report estimates of specifications that allow different linear trends in wages for displaced and non-displaced workers in Section 5.

Equation (1) is very similar to that estimated 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 collect unemployment benefits. Their measure will therefore 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 in 2002, 2003, or 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-

[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 of displaced individuals to those who were continuously employed for 36 months prior to collecting unemployment benefits, which they argue reduces the likelihood of misclassifying voluntary quitters as displaced. In contrast, we restrict our sample to individuals with at least 24 months tenure with their current or pre-displacement employer, which is likely to have a similar effect, *and* identify displaced individuals from the timing of their benefit receipt. In robustness checks, we extend our minimum employer tenure restriction to 36 months.

[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 only unemployment spells that began after this date could have been exposed to the full set of reforms for the full duration of the unemployment spell.

reform change in post-displacement wages, free of potential contamination from partially exposed displacements.

Our baseline specification is:

$$y_{it} = \mathbf{x}_{it}^{'}\beta_0 + \alpha_0 DISP_{it} + \gamma_0 DISP_{it} * DURING_{it} + \delta_0 DISP_{it} * HARTZ_{it} + \tau_{0,t} + \epsilon_{0,it}.$$

Defining $\mathbf{z}_{it}'\eta_0 = \mathbf{x}_{it}^{'}\beta_0 + \alpha_0 DISP_{it} + \gamma_0 DISP_{it} * DURING_{it} + \tau_{0,t}$ and $h_{it} = DISP_{it} * HARTZ_{it}$, we rewrite the baseline specification more compactly as:

$$y_{it} = \mathbf{z}_{it}'\eta_0 + \delta_0 h_{it} + \epsilon_{0,it}. \tag{2}$$

4.1 Identifying Potential Mechanisms

To preview our results, we find compelling evidence that $\delta_0 < 0$. This is consistent with Engbom et al. (2015) and Price (2016), and indicates that the Hartz reforms increased the wage losses that displaced workers experienced upon finding re-employment. We posit three possible mechanisms that could underlie these wage losses. The first is selection: if individuals displaced after the reforms had different unobserved characteristics than individuals displaced prior to the reforms, this will be reflected in δ_0. Specifically, if workers displaced after the reforms had "worse" unobserved characteristics (i.e., characteristics that earned lower returns in the labor market) than workers displaced prior to the reform, then all else equal we would find $\delta_0 < 0$ in eq. (2). The second mechanism is sorting, which would arise if the reforms changed the distribution of unobserved characteristics of the establishments at which displaced workers found re-employment. Specifically, if they increasingly found re-employment at establishments that paid systematically lower wages conditional on worker characteristics (e.g., establishments that paid a lower AKM-style establishment wage premium), then we would likewise find $\delta_0 < 0$ in eq. (2). The third mechanism is matching, which would arise if the reforms changed the distribution of the unobserved characteristics of worker-establishment matches that displaced workers entered into. In this case, if displaced workers entered into systematically lower quality matches (e.g., matches that paid a lower match-specific wage premium in the sense of Woodcock (2008) or Woodcock (2015)), then we would also find $\delta_0 < 0$ in eq. (2).

To quantify the relative contribution of these potential mechanisms to the increased cost of displacement following the Hartz reforms, we now develop a simple framework and two decomposition approaches. Let θ_i denote unobserved characteristics of individual i that influence wages; let $\psi_{J(i,t)}$ denote the unobserved characteristics of the establishment $J(i,t)$ at which individual i was employed in period t; and let $\phi_{iJ(i,t)}$ denote the unobserved characteristics (e.g., "match quality") of the match between individual i and establishment $J(i,t)$. For tractability, we assume that unobserved characteristics $\theta_i, \psi_{J(i,t)}$, and $\phi_{iJ(i,t)}$ are all time-invariant. In the interest of making minimal assumptions about the relationship between unobserved characteristics, let $\Phi\left(\theta_i, \psi_{J(i,t)}, \phi_{iJ(i,t)}\right)$ denote their combined effect on wages. We simply assume that $\Phi(\cdot)$ is additively separable from the observable determinants of wages, $\mathbf{z}_{it}$ and h_{it}, and from the error ϵ_{it}. With these minimal assumptions, we have the following expression for wages:

$$y_{it} = \mathbf{z}_{it}'\eta + \delta h_{it} + \Phi\left(\theta_i, \psi_{J(i,t)}, \phi_{iJ(i,t)}\right) + \epsilon_{it} \tag{3}$$

where we assume that the error satisfies $E\left[\epsilon_{it}|\mathbf{z}_{it}, h_{it}, \Phi(\cdot)\right] = 0$.[10]

If the Hartz reforms changed the distribution of θ_i among workers selected into displacement, the distribution of $\psi_{J(i,t)}$ among the establishments at which they found re-employment, or the distribution of $\phi_{iJ(i,t)}$ in the matches that they entered into, then $\delta \neq \delta_0$. In fact the difference between δ_0 and δ estimates the net effect of the Hartz reforms on the distribution of $\Phi\left(\theta_i, \psi_{J(i,t)}, \phi_{iJ(i,t)}\right)$ in re-employment wages. This is the central insight of the Pendakur and Woodcock (2010) "glass door effect"[11] and the Gelbach (2016) decomposition. To see this, note that $\Phi(\cdot)$ depends only on $\theta_i, \psi_{J(i,t)}$, and $\phi_{iJ(i,t)}$, and that the value of all three unobserved heterogeneity terms is fixed for the duration of individual i's employment spell at establishment $J(i,t)$. Consequently, we can replace the nonparametric function $\Phi\left(\theta_i, \psi_{J(i,t)}, \phi_{iJ(i,t)}\right)$ in eq. (3) with a fixed effect for the match between worker i and establishment $J(i,t)$:

$$y_{it} = \mathbf{z}_{it}'\eta + \delta h_{it} + \Phi_{iJ(i,t)} + \epsilon_{it}. \tag{4}$$

[10] A slightly weaker assumption based on orthogonality would also suffice.

[11] Pendakur and Woodcock (2010) define the glass door effect as the difference between the economy-wide return to an observable characteristic (such as race, gender, or immigrant status) and the average within-firm return. It is mathematically equivalent to the difference between δ_0 and δ in the absence of unobserved worker and match heterogeneity, i.e., when $\Phi(\cdot)$ is a function of $\psi_{J(i,t)}$ only. It measures the contribution of inter-firm sorting to the economy-wide return to an observable characteristic.

The match-specific fixed effect $\Phi_{iJ(i,t)}$ measures between-match differences in average wages that arise because of differences in the unobserved characteristics of workers, establishments, and the matches that they enter into, conditional on $\mathbf{z}_{it}$ and the post-Hartz displacement indicator h_{it}. If we could observe $\Phi_{iJ(i,t)}$ directly, we could summarize the effect of the reforms on the distribution of $\Phi_{iJ(i,t)}$ in re-employment wages from the regression:

$$E\left[\Phi_{iJ(i,t)}|\mathbf{z}_{it}, h_{it}\right] = \mathbf{z}_{it}'\eta_{\Phi} + \delta_{\Phi}h_{it}. \tag{5}$$

The coefficient δ_{Φ} is a difference-in-differences estimate of the Hartz reforms' effect on the distribution of unobserved worker, establishment, and match characteristics in re-employment wages. That is, it measures the pre- vs. post-reform change in the average value of $\Phi_{iJ(i,t)}$ among recently displaced workers, relative to the pre- vs. post-reform change for all other workers.

Of course, it is infeasible to directly estimate eq. (5) because $\Phi_{iJ(i,t)}$ is unobserved. However, the following proposition shows that we can obtain an unbiased estimate of δ_{Φ} from the difference between OLS estimates of δ_0 and δ.

Proposition 1. *Let $\hat{\delta}_0$ and $\hat{\delta}$ denote the OLS estimators of δ_0 and δ in eqs. (2) and (4), respectively, and assume that ϵ_{it} in eq. (4) has zero conditional mean. Then $\left(\hat{\delta}_0 - \hat{\delta}\right)$ is an unbiased estimator of δ_{Φ} in the infeasible regression (5).*

Proof. Rewriting eqs. (2) and (4) in matrix notation, we have:

$$\mathbf{y} = \mathbf{Z}\eta_0 + \delta_0\mathbf{h} + \epsilon_0 \tag{6}$$

$$\mathbf{y} = \mathbf{Z}\eta + \delta\mathbf{h} + \mathbf{G\Phi} + \epsilon \tag{7}$$

where $\mathbf{y}$ is the $N^* \times 1$ vector of wage observations, $\mathbf{Z}$ is the $N^* \times k$ matrix of observables with rows $\mathbf{z}_{it}'$, $\mathbf{h}$ is the $N^* \times 1$ vector of indicators for displacement after the Hartz reforms, η_0, δ_0, η and δ are conformable coefficient vectors, $\mathbf{\Phi}$ is the $M \times 1$ vector of match fixed effects with $N^* \times M$ design matrix $\mathbf{G}$,[12] ϵ and ϵ_0 are $N^* \times 1$ vectors of errors, N^* is the number of observations, and M is the number of worker-establishment matches. The OLS estimator of δ in eq. (7) is $\hat{\delta} = \left(\mathbf{h}'\mathbf{M}_{[\mathbf{Z}\ \mathbf{G}]}\mathbf{h}\right)^{-1}\mathbf{h}'\mathbf{M}_{[\mathbf{Z}\ \mathbf{G}]}\mathbf{y}$ where $\mathbf{M_A} = \mathbf{I} - \mathbf{A}\left(\mathbf{A}'\mathbf{A}\right)^{\mathbf{-1}}\mathbf{A}'$ is the usual annihilator matrix that

[12]The M columns of $\mathbf{G}$ are indicator variables, one for each worker-firm match: $\mathbf{G} = [\mathbf{g}_1, \mathbf{g}_2, \ldots, \mathbf{g}_M]$.

projects onto the column null space of a matrix $\mathbf{A}$. Given $E\left[\epsilon|\mathbf{Z},\mathbf{h},\mathbf{G}\right]=0$, we have $E\left[\hat{\delta}|\mathbf{Z},\mathbf{h},\mathbf{G}\right]=\delta$. Premultiplying both sides of eq. (7) by $\left(\mathbf{h}'\mathbf{M_Z}\mathbf{h}\right)^{-1}\mathbf{h}'\mathbf{M_Z}$, we obtain

$$\left(\mathbf{h}'\mathbf{M_Z}\mathbf{h}\right)^{-1}\mathbf{h}'\mathbf{M_Z}\mathbf{y}=\delta+\left(\mathbf{h}'\mathbf{M_Z}\mathbf{h}\right)^{-1}\mathbf{h}'\mathbf{M_Z}\mathbf{G}\mathbf{\Phi}+\left(\mathbf{h}'\mathbf{M_Z}\mathbf{h}\right)^{-\mathbf{1}}\mathbf{h}'\mathbf{M_Z}\epsilon \tag{8}$$

because $\mathbf{M_Z}\mathbf{Z}=\mathbf{0}$. The left hand side of eq. (8) is the OLS estimator of δ_0 in eq. (6), $\hat{\delta}_0=\left(\mathbf{h}'\mathbf{M_Z}\mathbf{h}\right)^{-1}\mathbf{h}'\mathbf{M_Z}\mathbf{y}$. Consequently,

$$E\left[\hat{\delta}_0-\hat{\delta}|\mathbf{Z},\mathbf{h},\mathbf{G}\right]=\left(\mathbf{h}'\mathbf{M_Z}\mathbf{h}\right)^{-1}\mathbf{h}'\mathbf{M_Z}\mathbf{G}\mathbf{\Phi}$$

which is the OLS estimator of δ_Φ in the regression:

$$E\left[\mathbf{G}\mathbf{\Phi}|\mathbf{Z},\mathbf{h}\right]=\mathbf{Z}\eta_\Phi+\delta_\Phi\mathbf{h} \tag{9}$$

which is simply eq. (5) rewritten in matrix notation. $\square$

The intuition underlying Proposition 1 is straightforward. Because the baseline specification eq. (2) does not control for unobserved individual, establishment, or match-specific heterogeneity, $\hat{\delta}_0$ estimates the *total effect* of the Hartz reforms on the re-employment wages of recently displaced workers. This includes the reforms' effect on the distribution of the unobserved characteristics of recently displaced workers (if the reforms changed who was selected into displacement), on the unobserved characteristics of the firms at which they found re-employment (if the reforms changed the way that displaced workers were sorted across establishments), and on the unobserved characteristics of their employment matches (if the reforms changed the quality of the matches that they entered into). In contrast, eq. (4) controls for all three dimensions of unobserved heterogeneity via $\Phi_{iJ(i,t)}$. Consequently, $\hat{\delta}$ estimates the *net effect* of the Hartz reforms on re-employment wages, holding these unobserved characteristics constant. The difference $\hat{\delta}_0-\hat{\delta}$ isolates the reforms' effect on re-employment wages that operates through the selection, sorting, and matching channels. At the population level, this is measured by δ_Φ.

Proposition 1 is an application of Gelbach's (2016) decomposition approach. Although Gelbach (2016) develops that approach in the context of measuring the "importance" of observable covariates, Proposition 1 demonstrates that it also applies to measuring the contribution of unobservables to observed outcomes.

In the finite sample, we have the exact result $\hat{\delta}_0 - \hat{\delta} = (\mathbf{h}'\mathbf{M_Z h})^{-1}\mathbf{h}'\mathbf{M_Z G}\hat{\mathbf{\Phi}}$ where $\hat{\mathbf{\Phi}}$ is the OLS estimator of $\mathbf{\Phi}$.[13] This gives an alternate way to estimate δ_Φ: via an auxiliary regression of $\hat{\Phi}_{iJ(i,t)}$ on $\mathbf{z}_{it}$ and h_{it}. While the circumstances in which this would be preferred to simply taking the difference $\hat{\delta}_0 - \hat{\delta}$ are probably limited, this result turns out to be helpful for operationalizing the decompositions that we develop below. Asymptotically, $\text{plim}\hat{\delta} = \delta$ and $\text{plim}\hat{\delta}_0 = \delta_0$ under standard regularity conditions, so that $\text{plim}\left(\hat{\delta}_0 - \hat{\delta}\right) = \delta_\Phi$. It is worth noting that this consistency result holds even though each of the estimated match fixed effects $\hat{\Phi}_{iJ(i,t)}$ is inconsistent (but unbiased) in a fixed-length panel.

Inference about δ_Φ is straightforward via a Hausman-type test based on $\hat{\delta}_0 - \hat{\delta}$, as established in Proposition 2. It is worth noting that failing to reject $H_0 : \delta_\Phi = 0$, or equivalently $H_0 : \delta_0 = \delta$, does not imply the absence of unobserved heterogeneity in wages, nor does it imply that the match fixed effects $\Phi_{iJ(i,t)}$ do not belong in the model. Rather, it is simply evidence that the reforms did not affect the average wages of displaced workers via changes to the distribution of their unobserved characteristics, the characteristics of the establishments at which they found re-employment, or the characteristics of the matches that they entered into.

Proposition 2. *Under the null hypothesis* $H_0 : \delta_\Phi = 0$, $Var\left[\hat{\delta}_0 - \hat{\delta}|\mathbf{Z}, \mathbf{h}\right] = Var\left[\hat{\delta}|\mathbf{Z}, \mathbf{h}\right] - Var\left[\hat{\delta}_0|\mathbf{Z}, \mathbf{h}\right]$ *so that* $Q = \left(\hat{\delta}_0 - \hat{\delta}\right)'\left(\hat{Var}\left[\hat{\delta}\right] - \hat{Var}\left[\hat{\delta}_0\right]\right)^{-1}\left(\hat{\delta}_0 - \hat{\delta}\right) \overset{a}{\sim} \chi^2_1$, *where* $\hat{Var}\left[\hat{\delta}_0\right]$ *and* $\hat{Var}\left[\hat{\delta}\right]$ *are consistent estimates of* $Var\left[\hat{\delta}_0|\mathbf{Z}, \mathbf{h}\right]$ *and* $Var\left[\hat{\delta}|\mathbf{Z}, \mathbf{h}\right]$*, respectively.*

Proof. See Appendix A. □

Because we have left the functional form of $\Phi\left(\theta_i, \psi_{J(i,t)}, \phi_{iJ(i,t)}\right)$ unspecified, our estimate of the reforms' combined effect on re-employment wages via the selection, sorting, and matching channels admits a wide array of possible relationships between the unobserved heterogeneity components. Notably, this includes possibly nonlinear and non-separable relationships, e.g., $\Phi(\cdot) = \theta_i + \psi_{J(i,t)} + \theta_i\psi_{J(i,t)}$ or $\Phi(\cdot) = \phi_{iJ(i,t)}\left(\theta_i + \psi_{J(i,t)}\right)$. Indeed, our estimator of the combined effect of the reforms on the distribution of these characteristics in re-employment wages, $\hat{\delta}_0 - \hat{\delta}$,

[13] Letting $\hat{\eta}$ denote the OLS estimator of η and $\mathbf{e}$denote the OLS residual, we can rewrite eq. (7) as $\mathbf{y} = \mathbf{Z}\hat{\eta} + \hat{\delta}\mathbf{h} + \mathbf{G}\hat{\mathbf{\Phi}} + \mathbf{e}$. Premultiplying both sides by $(\mathbf{h}'\mathbf{M_Z h})^{-1}\mathbf{h}'\mathbf{M_Z}$, we obtain $\hat{\delta}_0 = \hat{\delta} + (\mathbf{h}'\mathbf{M_Z h})^{-1}\mathbf{h}'\mathbf{M_Z G}\hat{\mathbf{\Phi}}$ because $\mathbf{M_Z Z} = \mathbf{0}$ and $\mathbf{e}$ is orthogonal to $\mathbf{h}$ and $\mathbf{Z}$ by construction.

is invariant to the particular functional form of the unobserved heterogeneity. That said, it is clearly useful to decompose the combined effect of the reforms into selection, sorting, and matching components so that we can understand the mechanism through which the reforms reduced the re-employment wages of displaced workers. Any such decomposition requires us to make additional assumptions about the functional form of $\Phi\left(\theta_i, \psi_{J(i,t)}, \phi_{iJ(i,t)}\right)$. We now develop two alternative decomposition approaches that rely on different assumptions about the relationships between the unobserved heterogeneity components.

4.1.1 Decomposition 1

Our first decomposition assumes that unobserved individual, establishment, and match heterogeneity are additively separable, so that $\Phi\left(\theta_i, \psi_{J(i,t)}, \phi_{iJ(i,t)}\right) = \theta_i + \psi_{J(i,t)} + \phi_{iJ(i,t)}$. In matrix notation, the full specification for wages becomes:

$$\mathbf{y} = \mathbf{Z}\eta + \delta\mathbf{h} + \mathbf{D}\theta + \mathbf{F}\psi + \mathbf{G}\phi + \epsilon \tag{10}$$

where θ is an $N \times 1$ vector of individual fixed effects θ_i, ψ is a $J \times 1$ vector of establishment fixed effects $\psi_{J(i,t)}$, ϕ is an $M \times 1$ vector of match-specific effects $\phi_{iJ(i,t)}$, $\mathbf{D}$ and $\mathbf{F}$ are $N^* \times N$ and $N^* \times J$ design matrices of the individual and establishment effects, respectively,[14] N is the number of individuals, J is the number of establishments, and all other terms are as defined previously. The individual effects θ_i measure persistent differences in wages between individuals, holding constant their observable characteristics and the unobserved characteristics of their employers and the employment matches. Likewise, the establishment effects $\psi_{J(i,t)}$ measure persistent differences in average wages between employers, holding observable characteristics and the unobserved characteristics of their employees and matches constant. In the context of AKM-type specifications (which have the same structure as eq. (10) but omit the match effect $\phi_{iJ(i,t)}$), $\psi_{J(i,t)}$ is usually characterized as an establishment wage premium that is common to all employees. The match effects $\phi_{iJ(i,t)}$ measure persistent differences in average wages between worker-firm matches, conditional on observable characteristics and workers' and establishments' time-invariant unobserved characteristics. They provide a useful summary of match-level wage outcomes, which we can loosely characterize as match quality.

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

Following the same method of proof as in Proposition 1, it is straightforward to show that:

$$E\left[\hat{\delta}_0 - \hat{\delta}|\mathbf{Z},\mathbf{h},\mathbf{D},\mathbf{F},\mathbf{G}\right] = \left(\mathbf{h}'\mathbf{M_Z}\mathbf{h}\right)^{-1}\mathbf{h}'\mathbf{M_Z}\mathbf{D}\theta + \left(\mathbf{h}'\mathbf{M_Z}\mathbf{h}\right)^{-1}\mathbf{h}'\mathbf{M_Z}\mathbf{F}\psi + \left(\mathbf{h}'\mathbf{M_Z}\mathbf{h}\right)^{-1}\mathbf{h}'\mathbf{M_Z}\mathbf{G}\phi$$

where $\hat{\delta}$ is the OLS estimator of δ in eq. (10).[15] We call the first term, $(\mathbf{h}'\mathbf{M_Z}\mathbf{h})^{-1}\mathbf{h}'\mathbf{M_Z}\mathbf{D}\theta$, the Decomposition 1 selection effect. It is the OLS estimator of δ_θ in the infeasible regression:

$$E\left[\theta_i|\mathbf{z}_{it}, h_{it}\right] = \mathbf{z}'_{it}\eta_\theta + \delta_\theta h_{it} \tag{11}$$

and therefore provides a difference-in-differences estimate of the Hartz reforms' effect on the average value of recently displaced workers' unobserved characteristics θ_i. We call the second term, $(\mathbf{h}'\mathbf{M_Z}\mathbf{h})^{-1}\mathbf{h}'\mathbf{M_Z}\mathbf{F}\psi$, the Decomposition 1 sorting effect. It is the OLS estimator of δ_ψ in the regression:

$$E\left[\psi_{J(i,t)}|\mathbf{z}_{it}, h_{it}\right] = \mathbf{z}'_{it}\eta_\psi + \delta_\psi h_{it} \tag{12}$$

which provides a difference-in-differences estimate of the Hartz reforms' effect on the average value of the unobserved characteristics $\psi_{J(i,t)}$ of the establishments employing recently displaced workers. Finally, we call the third term, $(\mathbf{h}'\mathbf{M_Z}\mathbf{h})^{-1}\mathbf{h}'\mathbf{M_Z}\mathbf{G}\phi$, the Decomposition 1 matching effect. It is likewise the OLS estimator of δ_ϕ in the regression:

$$E\left[\phi_{iJ(i,t)}|\mathbf{z}_{it}, h_{it}\right] = \mathbf{z}'_{it}\eta_\phi + \delta_\phi h_{it} \tag{13}$$

and is therefore a difference-in-differences estimate of the Hartz reforms' effect on the average value of unobserved worker-establishment match characteristics $\phi_{iJ(i,t)}$ of recently displaced workers.

Of course $\theta_i, \psi_{J(i,t)}$ and $\phi_{iJ(i,t)}$ aren't directly observed and hence eqs. (11)-(13) aren't directly estimable. To operationalize the decomposition, we therefore need to estimate the unobserved heterogeneity components. This, in turn, requires additional identifying assumptions, because eq. (10) is overparameterized.[16] We estimate eq. (10) using the orthogonal match effects estimator

[15] Note that eqs. (7) and (10) provide exactly the same estimate of δ. because $\mathbf{D}$ and $\mathbf{F}$ lie within the column space of $\mathbf{G}$. That is, 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 $\mathbf{F}$.

[16] Intuitively, there are $N + J + M$ individual, establishment, and match fixed effects to estimate, but only M match-specific means, $\Phi_{iJ(i,t)}$ in our notation, from which to estimate them.

developed in Woodcock (2008) and Woodcock (2015), which defines $\phi_{iJ(i,t)}$ to be orthogonal to the individual and establishment fixed effects, θ_i and $\psi_{J(i,t)}$. The orthogonality condition, while restrictive, solves the overparameterization problem without resorting to more restrictive random effect assumptions. That is, while it imposes a restriction on the relationship between $\phi_{iJ(i,t)}$ and the worker and establishment effects, it does not impose restrictions on the relationship between any of the unobserved heterogeneity components and the observables $\mathbf{z}_{it}$ and h_{it}; see Woodcock (2015) for a detailed discussion. This is important, because we use our estimates of $\theta_i, \psi_{J(i,t)}$ and $\phi_{iJ(i,t)}$ in sample counterparts of eqs. (11)-(13) to estimate the selection, sampling, and matching effects, $(\delta_\theta, \delta_\psi, \delta_\phi)$.

The other identifying assumptions for our implementation of eq. (10) are standard. Specifically, we require:

$$E\left[\epsilon \middle| \mathbf{Z}, \mathbf{h}, \mathbf{D}, \mathbf{F}, \mathbf{G}\right] = \mathbf{0} \tag{14}$$

(a slightly weaker assumption based on orthogonality would suffice). This embodies a parallel trends assumption, conditional on $\mathbf{Z}, \mathbf{D}$, $\mathbf{F}$, and $\mathbf{G}$, as well as an assumption that employment mobility is conditionally exogenous. Specifically, employment mobility can depend on observable characteristics ($\mathbf{Z}$ and $\mathbf{h}$) and time-invariant observable and unobservable characteristics of workers, establishments, and matches as captured by $\theta_i, \psi_{J(i,t)}$, and $\phi_{iJ(i,t)}$, but not the errors ϵ_{it}. Card et al. (2013) find considerable support for a stronger exogenous mobility assumption in IEB data, of which the LIAB data comprise a sample.[17]

To summarize, we implement Decomposition 1 as follows. We estimate (10) in two steps. In the first step, we estimate eq. (4) to obtain OLS estimates $\hat{\delta}$ and $\hat{\Phi}_{iJ(i,t)}$. In the second step, we decompose $\hat{\Phi}_{iJ(i,t)}$ into OLS estimates $\hat{\theta}_i, \hat{\psi}_{J(i,t)}$ and $\hat{\phi}_{iJ(i,t)}$ via the orthogonal match effects estimator described above. We normalize all three of $\hat{\theta}_i, \hat{\psi}_{J(i,t)}$ and $\hat{\phi}_{iJ(i,t)}$ to have zero mean within sample. Finally, we use those estimates in sample counterparts of (11)-(13) to estimate $\delta_\theta, \delta_\psi$ and δ_ϕ. Our estimates satisfy $\hat{\delta}_0 - \hat{\delta} = \hat{\delta}_\theta + \hat{\delta}_\psi + \hat{\delta}_\phi$ in the finite sample, and are unbiased and consistent estimates of $\delta_\theta, \delta_\psi$ and δ_ϕ subject to the orthogonality condition. Asymptotic inference about $(\delta_\theta, \delta_\psi, \delta_\phi)$ can be based on results in Gelbach (2016) Appendix B. However Gelbach's variance estimator involves

[17] Card et al. (2013) estimate a model that omits match effects, with corresponding exogenous mobility assumption $E\left[\epsilon \middle| \mathbf{Z}, \mathbf{h}, \mathbf{D}, \mathbf{F}\right] = \mathbf{0}$. This assumption is stronger than (14) because it is violated if employment mobility depends on match-specific unobserved heterogeneity. See Woodcock (2015) for an extended discussion of exogenous mobility in the context of models with and without match effects.

matrix calculations that are cumbersome for a problem of the dimension considered here, so our inferences are based instead on bootstrap standard errors clustered at the individual level.

4.1.2 Decomposition 2

Our second decomposition imposes fewer restrictions on the relationship between unobserved heterogeneity components. To do so, it relies on an intermediate AKM specification with additively-separable worker and establishment fixed effects to define the selection and sorting effects. That specification is:

$$y_{it} = \mathbf{z}_{it}'\eta_1 + \delta_1 h_{it} + \theta_{1,i} + \psi_{1,J(i,t)} + \epsilon_{1,it} \tag{15}$$

or in matrix notation,

$$\mathbf{y} = \mathbf{Z}\eta_1 + \delta_1 \mathbf{h} + \mathbf{D}\theta_1 + \mathbf{F}\psi_1 + \epsilon_1. \tag{16}$$

We have subscripted the parameters, including the worker and establishment effects, to emphasize that parameter estimates based on eq. (15) will differ, in general, from those in eqs. (2) and (10). This is because the three specifications identify δ and other model parameters using different variation[18] and under slightly different identifying assumptions. In particular, eq. (15) requires:

$$E\left[\epsilon_1 | \mathbf{Z}, \mathbf{h}, \mathbf{D}, \mathbf{F}\right] = \mathbf{0} \tag{17}$$

(again, a slightly weaker assumption based on orthogonality would suffice). As above, this embodies both a parallel trends assumption (conditional on $\mathbf{Z}, \mathbf{D}$,and $\mathbf{F}$), and an exogenous mobility assumption. Specifically, the AKM specification admits employment mobility that depends on observable characteristics ($\mathbf{Z}$ and $\mathbf{h}$) and time-invariant unobserved characteristics of workers and establishments as captured by $\theta_{1,i}$ and $\psi_{1,J(i,t)}$. However, employment mobility that depends on unobserved match-specific heterogeneity violates eq. (17), and this may be one reason to prefer Decomposition 1 over Decomposition 2. That said (and as noted in footnote 17), Card et al. (2013) estimate an AKM specification on the full IEB data (of which the LIAB are a subset) and find considerable support for the exogenous mobility assumption (17).

[18] Eq. (10) identifies δ from within-match variation over time, whereas eq. (2) identifies δ_0 from a combination of within- and between-match variation. In contrast, eq. (15) identifies δ_1 from both within-worker and within-firm variation.

Letting $\hat{\delta}_1$ denote the least squares estimator of δ_1,

$$E\left[\hat{\delta}_0 - \hat{\delta}_1 | \mathbf{Z}, \mathbf{h}, \mathbf{D}, \mathbf{F}\right] = \left(\mathbf{h}'\mathbf{M_Z}\mathbf{h}\right)^{-1}\mathbf{h}'\mathbf{M_Z}\mathbf{D}\theta_1 + \left(\mathbf{h}'\mathbf{M_Z}\mathbf{h}\right)^{-1}\mathbf{h}'\mathbf{M_Z}\mathbf{F}\psi_1. \tag{18}$$

We call the first and second terms in eq. (18) the Decomposition 2 selection and sorting effects, respectively. Note that they have the same functional form as the Decomposition 1 selection and sorting effects, but the definition of the worker and establishment effects on which they are based differs. In Decomposition 1, θ_i and $\psi_{J(i,t)}$ are based on eq. (10) which holds unobserved match-specific heterogeneity constant; whereas in Decomposition 2, $\theta_{1,i}$ and $\psi_{1,J(i,t)}$ are based on eq. (15) which does not. We therefore expect the two decompositions to yield similar estimates of the selection and sorting effects, since both assume that the wage returns to worker- and establishment-specific unobserved heterogeneity are additively separable from each other and from observable determinants of wages. The extent to which they differ will depend on the relative importance of match-specific unobserved heterogeneity in wages.

To wit, our definition of the Decomposition 2 matching effect relies on eq. (4), which does not impose any structure on the relationship between unobserved worker, establishment, and match heterogeneity.[19] Proposition 3 summarizes the key result underlying our definition of the Decomposition 2 matching effect.

Proposition 3. *Let $\hat{\delta}$ and $\hat{\delta}_1$ denote the OLS estimators of δ and δ_1 in eqs. (7) and (16), respectively, and assume that $E\left[\epsilon|\mathbf{Z}, \mathbf{h}, \mathbf{G}\right] = \mathbf{0}$. Then $\left(\hat{\delta}_1 - \hat{\delta}\right)$ is an unbiased estimator of δ^*_Φ in the infeasible regression:*

$$E\left[\Phi_{iJ(i,t)} | \mathbf{z}_{it}, h_{it}, \mathbf{d}_i, \mathbf{f}_i\right] = \mathbf{z}'_{it}\eta^*_\Phi + \delta^*_\Phi h_{it} + \theta^*_{\Phi i} + \psi^*_{\Phi J(i,t)} \tag{19}$$

where $\mathbf{d}_i$ and $\mathbf{f}_i$ are the rows of $\mathbf{D}$ and $\mathbf{F}$, respectively, corresponding to individual i.

Proof. Recall that $\hat{\delta} = \left(\mathbf{h}'\mathbf{M}_{[\mathbf{Z}\ \mathbf{G}]}\mathbf{h}\right)^{-1}\mathbf{h}'\mathbf{M}_{[\mathbf{Z}\ \mathbf{G}]}\mathbf{y}$. Premultiplying both sides of eq. (7) by

[19] Recall that the OLS estimators of δ in eqs. (4) and (10) are exactly the same.

$\left(\mathbf{h}'\mathbf{M}_{[\mathbf{Z}\ \mathbf{D}\ \mathbf{F}]}\mathbf{h}\right)^{-1}\mathbf{h}'\mathbf{M}_{[\mathbf{Z}\ \mathbf{D}\ \mathbf{F}]}$, we obtain:

$$\begin{aligned}\left(\mathbf{h}'\mathbf{M}_{[\mathbf{Z}\ \mathbf{D}\ \mathbf{F}]}\mathbf{h}\right)^{-1}\mathbf{h}'\mathbf{M}_{[\mathbf{Z}\ \mathbf{D}\ \mathbf{F}]}\mathbf{y} = \delta + \left(\mathbf{h}'\mathbf{M}_{[\mathbf{Z}\ \mathbf{D}\ \mathbf{F}]}\mathbf{h}\right)^{-1}\mathbf{h}'\mathbf{M}_{[\mathbf{Z}\ \mathbf{D}\ \mathbf{F}]}\mathbf{G}\mathbf{\Phi} \\ + \left(\mathbf{h}'\mathbf{M}_{[\mathbf{Z}\ \mathbf{D}\ \mathbf{F}]}\mathbf{h}\right)^{-1}\mathbf{h}'\mathbf{M}_{[\mathbf{Z}\ \mathbf{D}\ \mathbf{F}]}\epsilon\end{aligned} \tag{20}$$

because $\left(\mathbf{h}'\mathbf{M}_{[\mathbf{Z}\ \mathbf{D}\ \mathbf{F}]}\mathbf{h}\right)^{-1}\mathbf{h}'\mathbf{M}_{[\mathbf{Z}\ \mathbf{D}\ \mathbf{F}]}\mathbf{Z} = \mathbf{0}$. The left-hand side of eq. (20) is the OLS estimator of δ_1 in eq. (16), $\hat{\delta}_1 = \left(\mathbf{h}'\mathbf{M}_{[\mathbf{Z}\ \mathbf{D}\ \mathbf{F}]}\mathbf{h}\right)^{-1}\mathbf{h}'\mathbf{M}_{[\mathbf{Z}\ \mathbf{D}\ \mathbf{F}]}\mathbf{y}$. Since $E\left[\epsilon|\mathbf{Z},\mathbf{h},\mathbf{G}\right] = 0$ implies $E\left[\hat{\delta}|\mathbf{Z},\mathbf{h},\mathbf{G}\right] = \delta$, we have:

$$E\left[\hat{\delta}_1 - \hat{\delta}|\mathbf{Z},\mathbf{h},\mathbf{G}\right] = \left(\mathbf{h}'\mathbf{M}_{[\mathbf{Z}\ \mathbf{D}\ \mathbf{F}]}\mathbf{h}\right)^{-1}\mathbf{h}'\mathbf{M}_{[\mathbf{Z}\ \mathbf{D}\ \mathbf{F}]}\mathbf{G}\mathbf{\Phi} \tag{21}$$

where $\mathbf{\Phi}$ is the $M \times 1$ vector of worker-establishment match fixed effects defined in the Proof of Proposition 1. The right-hand side of (21) is the OLS estimator of δ^*_Φ in the regression:

$$E\left[\mathbf{G}\mathbf{\Phi}|\mathbf{Z},\mathbf{h},\mathbf{D},\mathbf{F}\right] = \mathbf{Z}\eta^*_\Phi + \delta^*_\Phi\mathbf{h} + \mathbf{D}\theta^*_\Phi + \mathbf{F}\psi^*_\Phi$$

which is the matrix equivalent of eq. (19). $\square$

We call $\delta^*_\Phi = \left(\mathbf{h}'\mathbf{M}_{[\mathbf{Z}\ \mathbf{D}\ \mathbf{F}]}\mathbf{h}\right)^{-1}\mathbf{h}'\mathbf{M}_{[\mathbf{Z}\ \mathbf{D}\ \mathbf{F}]}\mathbf{G}\mathbf{\Phi}$ the Decomposition 2 matching effect. Figueiredo et al. (2014) propose a similar measure to estimate whether industrial clusters improve worker-firm matching. The infeasible regression on which δ^*_Φ is based, eq. (19), is similar to the infeasible regression eq. (5) except that it holds individual- and establishment-specific unobservables constant. The Decomposition 2 matching effect δ^*_Φ thus provides a difference-in-differences estimate of the Hartz reforms' combined effect on unobserved worker-, establishment-, and match-specific heterogeneity in re-employment wages, net of the additively-separable and time-invariant component of worker- and establishment-specific heterogeneity. In essence, the Decomposition 2 selection and sorting effects capture the contribution of the additively-separable components of worker- and establishment-specific unobserved heterogeneity to re-employment wages, and the matching effect captures the contribution of any remaining match-specific heterogeneity (e.g., interactions between worker- and establishment components, nonlinearities, separable match effects, etc.). This is a broader definition of match-specific unobserved heterogeneity than is captured by Decomposition 1, and may be a reason to prefer Decomposition 2. We stress, however, that both decompositions yield the same estimate of the total effect of the reforms on re-employment wages via selection, sorting, and matching.[20]

Proposition 3 assumes $E[\epsilon|\mathbf{Z},\mathbf{h},\mathbf{G}]=\mathbf{0}$. For completeness, we note that this is equivalent to eq. (14) since $\mathbf{D}$ and $\mathbf{F}$ are contained within the column space of $\mathbf{G}$ (see footnote 15), and weaker than eq. (17) since it admits employment mobility that is correlated with match-specific unobserved heterogeneity.

To summarize, we implement Decomposition 2 as follows. We estimate eq. (16) via the Abowd et al. (2002) conjugate gradient algorithm to obtain OLS estimates $\hat{\delta}_1$, $\hat{\theta}_{1,i}$, and $\hat{\psi}_{1,J(i,t)}$; and we estimate (4) to obtain the OLS estimate $\hat{\delta}$. We normalize both $\hat{\theta}_{1,i}$, and $\hat{\psi}_{1,J(i,t)}$ to have zero mean within our sample. Then we use the estimated AKM individual and establishment effects in sample counterparts of (11) and (12) to estimate the Decomposition 2 selection and sorting effects, and we estimate the Decomposition 2 matching effect δ^*_Φ directly from $\hat{\delta}_1-\hat{\delta}$. As in the case of Decomposition 1, the estimated selection, sorting, and matching effects sum to $\hat{\delta}_0-\hat{\delta}$ in the finite sample. The estimated Decomposition 2 selection and sorting effects are unbiased and consistent under (17), and the estimated matching effect is unbiased and consistent under $E[\epsilon|\mathbf{Z},\mathbf{h},\mathbf{G}]=\mathbf{0}$. We base inferences about all three components on bootstrap standard errors clustered at the individual level.

Abowd et al. (2002) show that the worker and establishment effects in eq. (15) 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. Columns 3 and 6 of Table 3 reports summary statistics for the largest connected set. For men, the largest connected set comprises about 97 percent of observations in the full sample of full-time men who satisfy the 24 month tenure restriction, representing 94 percent of individuals, 79 percent of establishments, and 95 percent of worker-establishment matches. For women, the largest connected set of comprises a slightly smaller portion of the sample: about 90.5 percent of observations, representing 82 percent of individuals, 59 percent of establishments, and 85 percent of worker-establishment matches. Among men, sample means and proportions of observable characteristics in the largest connected set are indistinguishable from the larger sample.

[20] It may not be immediately apparent that the Decomposition 2 selection, sorting, and matching effects sum to the combined effect defined in Proposition 1. The simplest way to see that they do is to note that $\left(\hat{\delta}_0-\hat{\delta}_1\right)+\left(\hat{\delta}_1-\hat{\delta}\right)=\hat{\delta}_0-\hat{\delta}$ in the finite sample.

Women in the largest connected set also have characteristics very similar to the broader sample, although they earn slightly more and are slightly less likely to have missing education data. We focus our attention on the largest connected set for the remainder of this article.

5 Results

We estimate all regression specifications separately for men and women. Our controls for observable characteristics ($\mathbf{x}_{it}$) include education (5 categories), a cubic polynomial in age, and the interaction between age and education.

Column 1 of Table 4 presents estimated coefficients on the displacement indicators in our baseline specification, eq. (2). These indicate that prior to the Hartz reforms, recently displaced workers faced a steep wage loss of nearly 23.5 log points (men) or 25.5 log points (women) upon finding re-employment. The Hartz reforms substantially increased the magnitude of the wage loss: by 14.3 log points for men, and by 5.1 log points for women. These estimates of the reforms' effects on post-displacement wage losses are larger than reported by Engbom et al. (2015) and Price (2016). This might be due to the longer time horizon considered in this paper, differences between our definition of displacement and theirs, or our inclusion of the $DISP_{it} * DURING_{it}$ interaction, which makes for a cleaner comparison between the pre- and post-reform periods.

Column 8 presents coefficient estimates from eq. (4), in which fixed match effects control for unobserved worker, establishment, and match heterogeneity. As made precise in Proposition 1, the difference between estimates in columns 1 and 8 measures the combined effect of selection, sorting, and matching on re-employment wages. Prior to the reforms, these channels collectively explained almost all of the post-displacement wage loss: holding unobservables constant, recently displaced men earned only 0.3 log points less than their non-displaced counterparts, whereas recently displaced women faced a 2.5 log point wag gap. After the reforms, the wage loss expanded modestly to 3.2 log points for men but declined to zero for women. This implies that of the 14.3 log point total increase in post-displacement wage losses that men experienced following the Hartz reforms, 11.1 log points is accounted for by changes in the distribution of the unobserved characteristics of individuals selected into displacement, the unobserved characteristics of the employers where they found re-employment, and the unobserved characteristics of the matches that they entered into. For women, all of the

5.1 log point increase in post-displacement wage losses is attributable to these selection, sorting, and matching channels. For both men and women, we easily reject the null hypothesis that the combined effect of the reforms via these channels is zero using the Hausman-type test developed in Proposition 2.[21]

Columns 2-4 and 5-7 decompose the post-displacement wage loss into selection, sorting, and matching components via Decompositions 1 and 2, respectively.[22] The two decompositions yield very similar estimates. Prior to the reforms, most of the measured wage difference between recently displaced and non-displaced workers was due to selection. Recently displaced men earned roughly 15.5 log points less than their non-displaced counterparts because they had unobserved characteristics that earned lower returns in the labor market. This constitutes about 66 percent of the 23.5 log point pre-reform wage difference between displaced and non-displaced workers. Among women, selection accounted for 11.9 log points (47 percent) of the pre-reform wage loss. However, selection accounts for none of the increased wage loss that displaced women experienced after the reforms, and actually shrank the wage loss experienced by displaced men by about 2 log points. That is, men displaced after the reforms actually had slightly higher-earning unobserved characteristics than men displaced prior to the reforms, relative to their non-displaced counterparts, and this slightly reduced the wage gap between them.

Most of the increased wage loss following the reforms, about 12 log points for men and over 4 log points for women, is because recently displaced workers sort increasingly into employment at lower-paying firms after the Hartz reforms. This accounts for roughly 84 percent of the increased wage loss that displaced men and women experience after the reforms. It's clear that sorting is the primary channel via which the Hartz reforms expanded the wage loss of displacement, and so in Section 5.2 we more thoroughly investigate how the reforms changed the way that individuals are sorted across firms after displacement.

Our two decomposition approaches yield different, though in both cases small and negative, estimates of the matching effect. Decomposition 1 attributes 0.7 log points of the post-Hartz increase in the wage cost of displacement faced by men to sorting into lower-paying matches. For women, the Decomposition 1 matching effect is a very similar 0.6 log points. Decomposition 2,

[21] The value of the test statistic is 1039 for men and 79.2 for women; in each case the corresponding p-value is < 0.00001.

[22] Appendix Table 1 presents estimates of the AKM specification on which Decomposition 2 is based.

which imposes fewer restrictions on the relationship between unobservables and relies on a broader definition of match-specific heterogeneity, yields estimates that are roughly twice as large: 1.3 log points for men, and 1.2 log points for women. This accounts for between 5 and 9 percent of the increased wage loss that displaced men face following the Hartz reforms, and between 12.5 and 23 percent of the increased wage loss for women.

To better understand these results it is helpful to further investigate the individual components of wages, and how they differ between recently displaced and non-displaced workers. Table 5 summarizes the distribution of individual, establishment, and match effects estimated from eq. (10). Individual fixed effects, θ_i, comprise the largest component of observed variation in wages (roughly 80 percent for both men and women). The average value of θ_i among recently displaced individuals is more than 14 log points below the average of non-displaced men (0.42 standard deviations of the distribution of θ_i), and 9.8 log points below the average for non-displaced women (0.27 standard deviations). Recently displaced workers are also employed at establishments that pay their employees substantially below-average wages given their observed and unobserved characteristics: the average establishment effect $\psi_{J(i,t)}$ among recently displaced men and women is roughly 15 log points below the non-displaced (roughly 0.81 standard deviations below the mean for men, and 0.56 standard deviations for women). However, displaced workers also face considerably more dispersion in $\psi_{J(i,t)}$ than non-displaced workers, so it is clear that they are not uniformly employed in low-wage firms. Overall, the Table 5 estimates help to explain why controlling for unobserved 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, not only following displacement, and are employed in firms that pay *all* of their workers below-average wages.

5.1 Robustness

The estimates in Table 4 are robust to a variety of alternate definitions of recent displacement. In Appendix Tables 2 and 3, 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 4, though generally slightly smaller in magnitude. 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 losses following displacement are highly persistent, both before and after the Hartz reforms.

In Appendix Table 4, 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.[23] The rate of misclassification of establishment closure in our data is almost certainly lower than this, however, because the closures that we identify are all associated with involuntary displacement from employment. In any event, focusing on establishment closures 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 very little effect on our estimates, as show in Appendix Table 4. Estimates for men in that table are virtually identical to those in Table 4, although the estimated selection effect is no longer statistically significant. However the stricter definition of displacement yields a slightly larger estimate of the overall wage loss following the Hartz reforms – nearly 9 log points – for women, and is entirely attributable to sorting into lower-paying establishments after displacement.

In Appendix Table 5 we tighten our tenure restriction from 24 months to 36 months. Again, this has no meaningful effect on our estimates except to slightly reduce the magnitude of some parameter estimates.

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

[23]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.

sample to the period 1993-2008 and re-estimate the specifications underlying Table 4. The resulting estimates, in Appendix Table 6, are again very similar to those in Table 4. A related concern is that the early part of our sample period could be influenced by the turmoil of the early years following 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 Appendix Table 7, are again very similar to those in Table 4. The only notable difference is that the estimated post-reform wage loss following displacement is somewhat smaller for men (10.3 log points), as is the sorting effect (slightly less than 9 log points, which remains about 85 percent of the post-Hartz increase in the wage loss following displacement). The reverse is true for women: the estimated post-reform increase in wage losses is a slightly larger 5.9 log points, and the estimated sorting effect is now a somewhat larger 6.4 log points. This exceeds the total wage loss because the estimated selection effect is now small and positive, though it remains statistically significant. One the whole, we conclude that our estimates are driven neither by the lingering effects of re-unification, nor the financial crisis.

To assess whether our estimates might be a consequence of different pre-policy trends for displaced and non-displaced workers, we replace the vector of unrestricted year and quarter effects that are common to both groups with separate linear time trends for displaced and non-displaced workers. The resulting estimates are presented in Appendix Table 8. The results for men are very similar to our main estimates in Table 4. The specification with linear trends yields a smaller increase in the wage loss from displacement following the introduction of the Hartz reforms, roughly 8 log points vs. 14 log points in Table 4. The sorting effect is about the same magnitude, while the selection and matching effects are somewhat larger than in Table 4. The overall pattern, that sorting into lower-paying establishments accounts for the lion's share of the post-reform increase in wage losses, while matching plays a smaller role and selection works in the opposite direction, remains unchanged. For women, the specification with linear trends yields a substantially larger estimate of the increased wage loss following the reforms: 15.7 log points vs. 5.1 log points in Table 4. The estimated sorting effect also roughly doubles in size to over 9 log points, though it now comprises a smaller share of the total post-reform increase (roughly 60 percent, vs. 84 percent in Table 4). The matching effect is also somewhat larger, 2-3 log points depending on decomposition, and the selection effect remains statistically significant. Thus, the overall pattern of estimates remains the same as Table 4, though the magnitudes are somewhat larger and more similar in magnitude to what we observe for men.

A final source of concern is that our results could be driven by important omitted variables. Appendix Table 9 presents estimates from an alternative specification that addresses such concerns. Specifically, our empirical specification in Table 4 does not control for employer tenure, sector, or occupation. If the wage cost of displacement is substantially due to the loss of accumulated match-specific human capital as represented by the return to job tenure, and if the return to job tenure increased over the sample period for reasons unrelated to the reforms, then our specification might erroneously attribute the resulting increase in the cost of displacement to the Hartz reforms. Another concern is that establishment effects might simply capture wage differences between industrial sectors rather than establishments, so that our estimated sorting effect reflects changes in the way that displaced workers are sorted across sectors rather than establishments *per se*. A related concern is that match fixed effects might simply capture wage differences due to observables that vary at the level of the worker-establishment match, such as occupation. In Appendix Table 9, therefore, we estimate a version of our baseline specification that includes controls for employer tenure and its interactions with our Hartz dummies, fixed effects for 3-digit industry sector (202 categories), and occupation (341 categories). The estimates are presented in column 5. Although including these controls substantially reduces the estimated pre-reform wage loss of displacement to 8.1 log points for men and 9.1 log points for women, it does not substantially reduce the estimated post-reform increase in the wage loss. That is, the post-reform increase in the wage loss of displacement remains largely unexplained, in sharp contrast to our estimates in column 8 of Table 4. Thus our main results are clearly not an artifact of failing to adequately control for tenure, sector, or occupation in our baseline specification.

In columns 2-4 of Appendix Table 9 we perform a simple Gelbach (2016) decomposition to assess the importance of the additional controls in explaining the reforms' effect on post-displacement wage losses. Of these, industry sector is most important, explaining 4.9 log points (34 percent) of the increased cost of displacement for men, and 1.7 log points (31 percent) for women. This indicates that our estimated sorting effect partly reflects re-allocation across sectors following displacement. We investigate this further in Section 5.2 below.

5.2 Unpacking the sorting effect

It's clear from the preceding that sorting was the primary channel through which the Hartz reforms increased the wage losses of displaced workers. To better understand this phenomenon, we further decompose the sorting effect using observable establishment characteristics. Our objective is to characterize the low wage firms into which displaced workers increasingly sorted after the reforms. To this end, we estimate an augmented version of eq. (12), in which we regress estimated establishment fixed effects on the observable characteristics and Hartz indicators from our baseline specification ($\mathbf{z}_{it}$ and h_{it}), and a vector of additional establishment characteristics. The coefficient on the Hartz indicator h_{it} in this regression provides a revised estimate of the sorting effect, net of the contribution of observable establishment characteristics to the post-Hartz increase in wage losses. It also allows us, via an additional Gelbach (2016) decomposition, to measure the contribution of specific establishment characteristics to the sorting effect reported in Table 4.

The establishment characteristics that we consider include industry sector (202 categories), establishment birth year (40 categories), establishment size (8 categories), geography (state, 10 categories), and the share of employees who work full-time. The estimates are presented in Table 6. Together, these characteristics explain less than half of the sorting effect. For men, observables explain about 40 percent (4.5 log points) of the 12 log point wage loss that displaced workers suffer following the reforms as a consequence of sorting into lower-wage firms, and the remaining 7.5 log points is attributed to establishments' unobserved characteristics. Observables explain even less of the sorting effect for women: about 25 percent (1.1 log points). The only characteristic that explains a substantial portion of the sorting effect (over 40 percent for both genders) is industry sector. Displaced men experience a 5 log point wage loss following the reforms as a consequence of sorting into lower-wage sectors, and the remaining 7 log points of the sorting effect is due to sorting into lower-wage establishments within sectors. Sorting into lower-wage sectors increases the wage loss of displaced women by about 2 log points, while about 2.2 log points is due to sorting into lower-wage establishments within sector.

How did the sectoral allocation of displaced workers change following the reforms? To answer this question, Tables 7 and 8 present the top pre- and post-displacement sectors over roughly 5-year intervals before and after the Hartz reforms, along with the average establishment fixed effect,

$\hat{\psi}_{J(i,t)}$, in each sector. Unsurprisingly, displaced workers are predominantly employed in low-wage sectors, both before and after displacement and before and after the reforms.[24] For the most part, displaced workers are employed in the same sectors before and after displacement. There are notable exceptions, however. For example, a large number of men were displaced from the auto manufacturing and auto parts manufacturing sectors in the years after the reforms. These are high-wage sectors that pay their workers above-average wage premia. However very few displaced workers find re-employment in these sectors in the four quarters following displacement, and this certainly contributes to the post-displacement wage loss associated with reallocating into lower-paying sectors after the reforms.

The most striking feature of Tables 7 and 8, however, is the dramatic rise in post-displacement employment in sector 74.5, "Labour recruitment and personnel provision," which consists primarily of temporary employment agencies. Between 1994 and 1997, 3.6 percent of men and 2.2 percent of women were employed in this sector in the four quarters following displacement. By the 2010-2014 period, this sector grew to account for the largest share of post-displacement employment by a wide margin: in our sample, roughly 26 percent of men and 19 percent of women were employed in this sector in the four quarters following displacement. Notably, wages in this sector are also very low: on average, men employed in this sector receive an establishment wage premium roughly 2 standard deviations (36 log points) below the overall mean, while women receive an average establishment wage premium roughly one standard deviation (28 log points) below the overall mean. Given the large proportion of displaced workers who move into employment in this sector following the reforms, and the large negative wage premium they receive upon doing so, it is clear that this sector plays a major role in the post-reform wage losses that displaced workers experience upon changing industry sector.[25]

As shown in Figure 3, the post-reform increase in temporary employment after displacement has parallels in the broader economy. Although the temporary employment sector represents a small share of total employment in our sample – only 2.7 percent of male employment and 2.1 percent of

[24] Recall that estimated establishment wage effects $\hat{\psi}_{J(i,t)}$ are normalized to have zero mean in the sample. Thus establishments with $\hat{\psi}_{J(i,t)} < 0$ pay their employees less than we would expect given their observable characteristics $\mathbf{z}_{it}$ and unobserved personal and match heterogeneity.

[25] This is consistent with Fackler et al. (2019) who report that displacement increases the probability of temporary employment in a sample of older German workers who were displaced due to bankruptcy between 2008 and 2013.

females at the 2012 maximum – it has grown steadily between 1994 and 2014.[26] For both men and women, the rate of that growth clearly accelerated following the introduction of Hartz I-II in 2003. Indeed, the post-2003 growth in temporary employment was almost certainly a direct consequence of the reforms.

The Hartz I-II reforms encouraged growth in temporary employment in two ways. First, the reforms established new "Staff Service Agencies" (*Personal-Service-Agentur*, or PSAs), that were created specifically to place unemployed workers into temporary work assignments. At the outset of the reforms, each local employment office was required to establish a PSA, either internally or by contracting with a private agency (Jacobi and Kluve, 2007). PSAs, along with other private temporary employment agencies, are classified in sector 74.5. This partly explains the rapid growth in employment in that sector beginning in 2003. However, the PSA experiment was short-lived. PSAs were widely criticized for failing to integrate unemployed workers back into the labor force, in particular due to concern over lock-in effects that prevented temporary workers from transitioning to permanent employment (Mosley, 2006). As a consequence, the legislation mandating PSAs was weakened after 2005, and repealed entirely in 2009.[27] Thus PSAs are unlikely to account for the continued growth in temporary employment in the latter part of our sample.

Perhaps more importantly, therefore, the Hartz I-II reforms largely deregulated temporary agency work (Rinne and Zimmermann, 2013). Prior to the reforms, regulations limited how long an employee of a temporary employment agency (a "temp") could be continuously assigned to a client firm, limited the number of times that temps could be rehired by the same agency, and stipulated that temp workers were entitled to the same remuneration and working conditions as permanent employees if the temp assignment exceeded 12 months (Antoni and Jahn (2009), Hirsch and Mueller (2012), Hirsch (2016)). Hartz I-II exempted temporary employment agencies from all of these regulations if they signed a sectoral collective agreement. Nearly all of them did so: prior to 2002, there were no collective agreements in the temporary employment sector; by the end of 2003, nearly 97% of temporary employment agencies had signed a sectoral collective agreement (Antoni and Jahn, 2009). Unsurprisingly, the new collective agreements in the temporary employment sector

[26] The share of employment in Sector 74.5 in our sample is comparable to that reported by Spermann (2011), Antoni and Jahn (2009), and Hirsch and Mueller (2012) in other samples.

[27] *Sozialgesetzbuch* III, Section 37c, Article 1, December 2005, p. 3676; *Sozialgesetzbuch* III, Section 37c, Article 1, December 2008, p. 2917

stipulated relatively low wages.[28] This effectively deregulated temporary employment in Germany, and began a period of sustained employment growth in the sector. Tables 7 and 8 clearly illustrate that the growth in temporary employment was especially pronounced among displaced workers, and contributed to the wage losses they experienced after the reforms.

6 Conclusion

The Hartz reforms were sweeping, multi-dimensional, and affected many aspects of German employment and social benefits. We find that the combined effect of the suite of reforms was to reduce re-employment wages of displaced men by about 14 log points, and displaced women by about 5 log points. Because the reforms encompassed so many changes in such a short time frame, and because they potentially affected all German workers, it is unrealistic to expect that we can determine which specific provisions of the reforms were responsible for the increased wage loss. However the Hartz IV package, which substantially reduced the generosity of benefits for the long-term unemployed, is almost certainly a key contributor because it gave unemployed workers strong incentives to return to work quickly, even if it meant accepting a relatively low wage offer.

Provisions of the Hartz I-II package, which largely deregulated temporary employment and encouraged its early growth through the creation of PSAs, are almost certainly another key contributor. We show that post-displacement employment in the temporary work sector grew dramatically after the reforms, and that employers in this sector pay very low wages, even after accounting for the observable and unobserved characterstics of individuals and the matches that they enter into. The large number of displaced workers finding re-employment in this sector after the reforms, and the very low wages that they earn there, are a perhaps unexpected dimension of the Hartz reforms' effects on the wages of displaced workers.

[28] Jahn (2010) documents that the wage gap between temps and permanent employees expanded by roughly 3 percent shortly after the Hartz I-II reforms were introduced.

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A Proofs

Proof of Proposition 2. We provide a direct proof for the case of spherical errors, $E[\epsilon\epsilon'|\mathbf{Z},\mathbf{h}] = \sigma^2\mathbf{I}$. The result holds under more general conditions as long as the conditions of Lemma 2.1 of Hausman (1978) are satisfied.

The OLS estimator of δ_Φ in eq. (9) is $\hat{\delta}_\Phi = (\mathbf{h}'\mathbf{M_Z}\mathbf{h})^{-1}\mathbf{h}'\mathbf{M_Z}\mathbf{G}\boldsymbol{\Phi} = \delta_\Phi + (\mathbf{h}'\mathbf{M_Z}\mathbf{h})^{-1}\mathbf{h}'\mathbf{M_Z}\epsilon_\Phi$ where ϵ_Φ is the error term in eq. (9) satisfying $E[\epsilon_\Phi|\mathbf{Z},\mathbf{h}] = \mathbf{0}$ and $E[\epsilon_\Phi\epsilon'|\mathbf{Z},\mathbf{h}] = \mathbf{0}$. We can write $\hat{\delta}_0 = \delta + (\mathbf{h}'\mathbf{M_Z}\mathbf{h})^{-1}\mathbf{h}'\mathbf{M_Z}\mathbf{G}\boldsymbol{\Phi} + (\mathbf{h}'\mathbf{M_Z}\mathbf{h})^{-1}\mathbf{h}'\mathbf{M_Z}\epsilon = \delta + \hat{\delta}_\Phi + (\mathbf{h}'\mathbf{M_Z}\mathbf{h})^{-1}\mathbf{h}'\mathbf{M_Z}\epsilon$ and $\hat{\delta} = \delta + \left(\mathbf{h}'\mathbf{M}_{[\mathbf{Z}\ \mathbf{G}]}\mathbf{h}\right)^{-1}\mathbf{h}'\mathbf{M}_{[\mathbf{Z}\ \mathbf{G}]}\epsilon$. Under H_0, $\hat{\delta}_\Phi = (\mathbf{h}'\mathbf{M_Z}\mathbf{h})^{-1}\mathbf{h}'\mathbf{M_Z}\epsilon_\Phi$ and consequently $\hat{\delta}_0 = \delta + (\mathbf{h}'\mathbf{M_Z}\mathbf{h})^{-1}\mathbf{h}'\mathbf{M_Z}(\epsilon_{\boldsymbol{\Phi}} + \epsilon)$, so that:

$$\begin{aligned}
Cov\left[\hat{\delta}_0, \hat{\delta}|\mathbf{Z},\mathbf{h}\right] &= E\left[\left(\mathbf{h}'\mathbf{M_Z}\mathbf{h}\right)^{-1}\mathbf{h}'\mathbf{M_Z}(\epsilon_{\boldsymbol{\Phi}} + \epsilon)\epsilon'\mathbf{M}_{[\mathbf{Z}\ \mathbf{G}]}\mathbf{h}\left(\mathbf{h}'\mathbf{M}_{[\mathbf{Z}\ \mathbf{G}]}\mathbf{h}\right)^{-1}|\mathbf{Z},\mathbf{h}\right] \\
&= \sigma^2\left(\mathbf{h}'\mathbf{M_Z}\mathbf{h}\right)^{-1}\mathbf{h}'\mathbf{M_Z}\mathbf{M}_{[\mathbf{Z}\ \mathbf{G}]}\mathbf{h}\left(\mathbf{h}'\mathbf{M}_{[\mathbf{Z}\ \mathbf{G}]}\mathbf{h}\right)^{-1} \\
&= \sigma^2\left(\mathbf{h}'\mathbf{M_Z}\mathbf{h}\right)^{-1} = Var\left[\hat{\delta}_0|\mathbf{Z},\mathbf{h}\right]
\end{aligned}$$

because $\mathbf{h}'\mathbf{M_Z}\mathbf{M}_{[\mathbf{Z}\ \mathbf{G}]} = \mathbf{h}'\mathbf{M}_{[\mathbf{Z}\ \mathbf{G}]}$. The intuition for this result is straightforward. $\mathbf{h}'\mathbf{M_Z}$ gives the residuals from the regression of $\mathbf{h}$ on $\mathbf{Z}$. Regressing these on $\mathbf{Z}$ and $\mathbf{G}$ gives residuals $\left(\mathbf{h}'\mathbf{M_Z}\mathbf{M}_{[\mathbf{Z}\ \mathbf{G}]}\right)$ that are the same as those obtained from the regression of $\mathbf{h}$ on $\mathbf{Z}$ and $\mathbf{G}$ directly $\left(\mathbf{h}'\mathbf{M}_{[\mathbf{Z}\ \mathbf{G}]}\right)$. It follows that $Var\left[\hat{\delta}_0 - \hat{\delta}|\mathbf{Z},\mathbf{h}\right] = Var\left[\hat{\delta}_0|\mathbf{Z},\mathbf{h}\right] + Var\left[\hat{\delta}|\mathbf{Z},\mathbf{h}\right] - 2Cov\left[\hat{\delta}_0, \hat{\delta}|\mathbf{Z},\mathbf{h}\right] = Var\left[\hat{\delta}|\mathbf{Z},\mathbf{h}\right] - Var\left[\hat{\delta}_0|\mathbf{Z},\mathbf{h}\right]$. Under standard regularity conditions, $\hat{\delta}_0$ and $\hat{\delta}$ are asymptotically normal and hence so is their difference, so that $Q^* = \left(\hat{\delta}_0 - \hat{\delta}\right)'\left(Var\left[\hat{\delta}|Z,\mathbf{h}\right] - Var\left[\hat{\delta}_0|\mathbf{Z},\mathbf{h}\right]\right)^{-1}\left(\hat{\delta}_0 - \hat{\delta}\right) \overset{a}{\sim} \chi^2_1$. Since $\hat{Var}\left[\hat{\delta}_0\right]$ and $\hat{Var}\left[\hat{\delta}\right]$ are consistent estimates of $Var\left[\hat{\delta}_0|\mathbf{Z},\mathbf{h}\right]$ and $Var\left[\hat{\delta}|\mathbf{Z},\mathbf{h}\right]$, Q^* and Q have the same asymptotic distribution. □

B Data Appendix

B.1 Overview of the LIAB and data processing

The LIAB comprises several linked data modules. For our purposes, the most important module is the Individual Data, which is extracted from the Integrated Employment Biography (IEB) database. This consists of records of individuals' employment and benefit receipt.

The employment records are derived from employment notifications filed by the employer and are the primary data source for our analysis. Employment notifications are filed at the start and end of an employment spell, and annually for ongoing spells. Each notification specifies the first day of work at this employer in the calendar year associated with this employment spell (e.g., January 1 or the start date of the employment spell), the corresponding last day of work at this employer in the calendar year (e.g., December 31 or the end date of the employment spell), the reason for the notification (job start, job end, job interruption, annual update, etc.), the average daily wage earned by the employee during the period covered by the notification (censored at the Social Security maximum), characteristics of the job (full-time/part-time, legal status, etc.), characteristics of the employee (gender, birth date, educational qualification), and unique identifiers for the individual and employer.

The benefit records are derived from various administrative sources. Each record corresponds to a single spell of benefits received during the calendar year and indicates the first day of benefit receipt (e.g., January 1 or the start date of the benefit spell), the last day of benefit receipt (e.g., December 31 or the end date of the benefit spell), and the type of benefits received (unemployment benefits, training benefits, etc.). We use the benefit records together with the employment records to identify recently displaced individuals based on the elapsed time between the end of a job spell and the start of a spell of short-term unemployment benefit receipt.

The second important module of the LIAB for our analysis is the Establishment File, which is extracted from the Establishment History Panel (BHP). This consists of annual records that describe characteristics of the employing establishment (geography, industry, number of employees, date of establishment birth and death, etc.). We link these to employment records to determine the set of individuals employed in West Germany, to determine the employing establishment's industry, to identify individuals that were displaced due to establishment closure, and to control for employer characteristics in our imputation regressions.

We process the data in several steps. First, we impute wage observations that are censored at the Social Security maximum. Second, we collapse all of an individual's full-time employment spells at the same employer in a given quarter into a single person-firm-quarter record. In doing so, we compute the individual's average daily wage by dividing their total earnings at the employer in that quarter by total days worked at the employer in that quarter (including weekends and holidays). Other characteristics of the spell – when they vary across records for the same person-firm-quarter – are assigned from the record with the highest total earnings. Third, we identify and date all displacement events for each individual to determine the quarters in which individuals meet our definition of being recently displaced. Fourth, we select one observation per person per quarter by selecting the person-firm-quarter record with the highest total earnings that quarter. Finally, we impose all remaining sample restrictions before determining the largest connected set of establishments, and restricting the sample to observations in the largest connected set. Additional information about key data processing steps follows.

B.2 Imputing Censored Wages

As shown in Table 1, nearly 15 percent of male wage observations in our sample, and nearly 5 percent of female wage observations, are censored at the Social Security maximum. We follow Card et al. (2013) and Dustmann et al. (2009) and impute wages for the censored observations using Tobit models fit to log daily wages. Imputed values are randomly drawn from the upper tail of the distribution implied by the Tobit model.[29]

Our imputation models are designed to preserve, to the extent possible, the individual, establishment, and match-specific components of wages. To that end, we construct, for each employment notification, the mean of the individual's daily wage in all other employment notifications and the proportion of other notifications in which the individual's wage was censored (i.e., "leave-out means" of individual wages and censoring). For individuals who are only observed once, we set the leave-out mean of individual wages equal to the overall mean of daily wages in the current year, and the leave-out mean censoring rate equal to the overall mean censoring rate in the current year, and include a dummy in the imputation model for individuals observed only once. We similarly construct the mean log wage of the individual's same-sex coworkers in the current year (i.e., for men this would be the leave-out mean of log wages of all male employees of the establishment associated with this employment notification in the current year), the fraction of same-sex coworkers whose wages are censored in the current year, and the fraction of coworkers with a university degree in the current

[29]Specifically, suppose log daily wages, y, satisfy $y \sim N(\mathbf{x}'\beta, \sigma)$ and wages are censored above c. Let $q = \Phi\left[(c - \mathbf{x}'\beta)/\sigma\right]$ where Φ is the standard normal CDF, let $u \sim U[0,1]$ denote a uniformly distributed random variable, and let $\hat{\beta}, \hat{\sigma}$ denote Tobit estimates of β and σ. For each censored observation $y \geq c$ we impute a value y^* from the upper tail of the log wage distribution using $y^* = \mathbf{x}'\hat{\beta} + \hat{\sigma}\Phi^{-1}\left[q + u(1-q)\right]$.

year. For establishments that are only observed once, we set the mean of coworker wages equal to the overall mean of daily wages in the current year, the coworker censoring rate equal to the overall mean censoring rate in the current year, and the coworker proportion of university graduates equal to the overall mean, and include a dummy in the imputation model for establishments observed only once.

We then form 1100 imputation groups by sex, 10-year age category (under 29, 30-39, 40-49, 50-59, over 60), year, and education (5 categories; see Section B.3.1 for definitions), and estimate a separate Tobit model for each imputation group controlling for: age; the leave-out means of individual wages and censoring; same-sex coworker mean wages and censoring rates; the coworker proportion with a university degree; other establishment characteristics (number of full-time employees; number of female employees; number of full-time female employees; number of low-, medium- and high-skilled employees; and the median wage of full-time employees);[30] a dummy variable that equals one if the current job was the individual's main job in this calendar year;[31] and dummies for individuals and establishments observed only once. Imputation groups that contained fewer than 500 observations were collapsed into ten "supergroups" by gender and education category, in which case we fully interacted the Tobit control variables with age category and added additional dummy variables for age category and year.

To evaluate the effect of our imputation procedure on the distribution of log daily wages, we undertake a validation exercise that follows Card et al. (2013) closely. Specifically, we artificially censor the upper tail of the wage distribution for a group of workers with a very low censoring rate in our data, and then stochastically impute the upper tail of the wage distribution using the procedure described above. We then compare various features of the distribution of log daily wages to the distribution in the artificially censored and imputed sample. We select male workers age 20-29 with an apprenticeship education for this purpose (the censoring rate in this group is 0.5 percent in our data). We undertake separate experiments in which we artificially censor the distribution of wages at the 60th, 70th, 80th, and 90th percentile of this group's observed wages in each year. We apply our imputation procedure separately to each of the artificially censored samples.

[30]The within-firm median wage measure is sometimes missing in the Establishment File, in which case we replace it with the overall mean of within-firm median wages in that year, and include a dummy in the imputation model for establishments with missing median wages.

[31]We define an individual's main job in year t as the job at which they earned the most in year t.

Appendix Figure A1 shows the actual mean and standard deviation of log real wages in the validation sample, as well means and standard deviations in the artificially censored/imputed samples. The means and standard deviations in the imputed series are uniformly higher than in the raw data, with a larger upward bias at higher censoring rates. Card et al. (2013) report a similar result. For both the mean and standard deviation, the upward bias is small but increases slightly over the sample period. For example, when the censoring rate is 40 percent, the upward bias in the mean increases from about 1 percent in the early part of the sample to 2 percent in the later part of the sample; in the case of the standard deviation, the upward bias increases from about 20 percent in 1993 to 28 percent in 2014. The bias is uniformly smaller for lower censoring rates. Fortunately for our purposes, the bias increases smoothly over time and doesn't coincide with the Hartz reforms. This leads us to conclude that the Tobit imputation procedure performs well, even at very high censoring rates.

A potential concern is that our imputation procedure might alter the relative shares of wage variation within vs. between establishments, or within vs. between worker-establishment matches. To investigate this, we fit linear regressions with year dummies and establishment or match effects to observations in our validation sample. This sample has 1,296,409 observations over the 1993-2014 period on individuals employed at 155,673 establishments in 451,339 distinct worker-establishment matches. For the regression with establishment effects, the R-squared coefficient was 0.656 in the actual data, vs. 0.645 with 10% censoring, 0.638 with 20% censoring, 0.630 with 30% censoring, and 0.620 with 40% censoring. For the regression with match effects, R-squared was 0.838 in the actual data, vs. 0.829 with 10% censoring, 0.817 with 20% censoring, 0.802 with 30% censoring, and 0.788 with 40% censoring. This demonstrates that the imputation procedure successfully preserves the relative share of wage variation attributable to within-establishment and within-match variation, even at very high censoring rates.

B.3 Other Key Variable Definitions

B.3.1 Education

Educational and vocational qualifications in the LIAB Individual Data are reported with four categories of vocational training, three categories of educational qualification prior to 2010, and four categories of educational qualification for 2010 and later. We group these into five time-consistent categories that mirror the definitions in CHK as closely as possible: (1) missing; (2) primary/lower secondary or intermediate school leaving certificate, or equivalent, with no vocational qualification; (3) primary/lower secondary or intermediate school leaving certificate, or equivalent, with a vocational qualification; (4) upper secondary school certificate (Abitur) with or without a vocational certificate; and (5) degree from Fachhochschule or university. For individuals with multiple employment notices from the same employer in the same year, we assign them the highest education category reported for that person-firm-year.

B.3.2 Occupation

Each employment notification includes information about an individual's occupation. Individuals with multiple notifications from the same employer in the same year were assigned the highest occupation category that person-firm-year. Note that we only use this occupation measure for the robustness checks reported in Appendix Table 9.

B.3.3 Displacement Measures

Each employment notification indicates the reason that the employer filed the notification (*grund*). One such reason is because the employment spell terminated, and we use this to identify job separations. Employment and benefit notifications also include a status code (*erwstat*) that indicates whether the individual is employed, collecting unemployment benefits, etc. We use this to identify spells of short-term unemployment benefit receipt. For reasons noted in the main text, we define an employment separation as an involuntary displacement if the elapsed time between the date of the job separation and the start date of the next spell of short-term unemployment benefit receipt is less than 85 days. We define a displacement as being due to establishment closure if the displacement event occurs in the same calendar year as the establishment's final reporting year (*lzt_jahr*) in the Establishment File, and the final reporting year is prior to 2014.

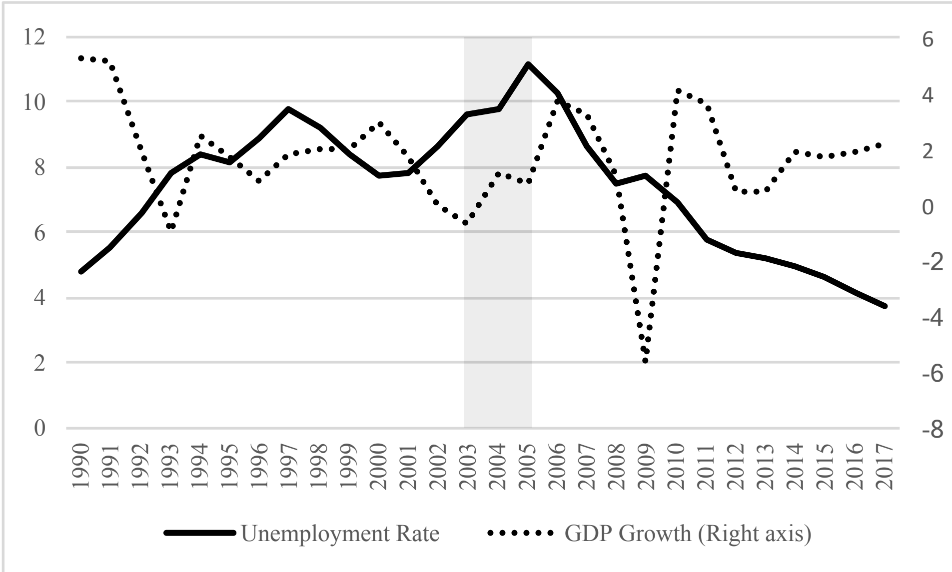
We define an individual as recently displaced in quarter t if they were displaced from employment in the preceding m quarters. In our main analysis, we set $m = 4$. In robustness checks, we relax this definition and estimate specifications for $m = 8, 12, 20$. We only observe displacements in 1993 or later, so to ensure that our displacement indicators are consistently defined across years we restrict our estimation sample to years $1993 + m/4$ or later. This ensures that our measure of recent displacement is not left-censored in any year.

B.3.4 Employer Tenure

We measure an individual's tenure with their employer (establishment) in months. For left-censored employment spells, we begin incrementing tenure from the level reported on the earliest observed employment notification at this establishment. For all other spells, we begin incrementing tenure from the observed start date of the spell. In either case, we increment the individual's tenure by one month for each calendar month that the individual is reported as employed at the establishment.

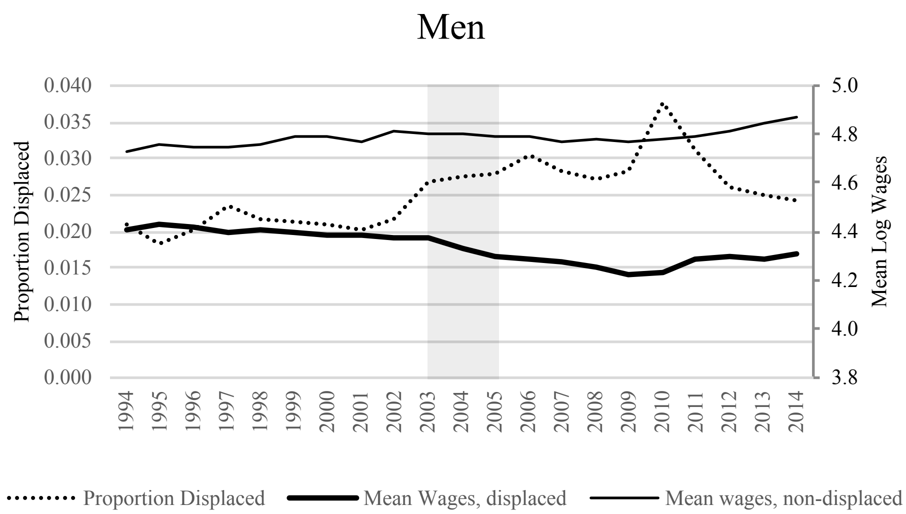
In our main analysis, we restrict the sample to person-quarter observations that satisfy one of two conditions: (1) the individual did not meet the definition of recently displaced in the current quarter and had at least 24 months tenure with their current employer; or (2) the individual did meet the definition of recently displaced in the current quarter and had at least 24 months tenure with their employer at the time of displacement. In robustness checks we increase the tenure requirement to 36 months.

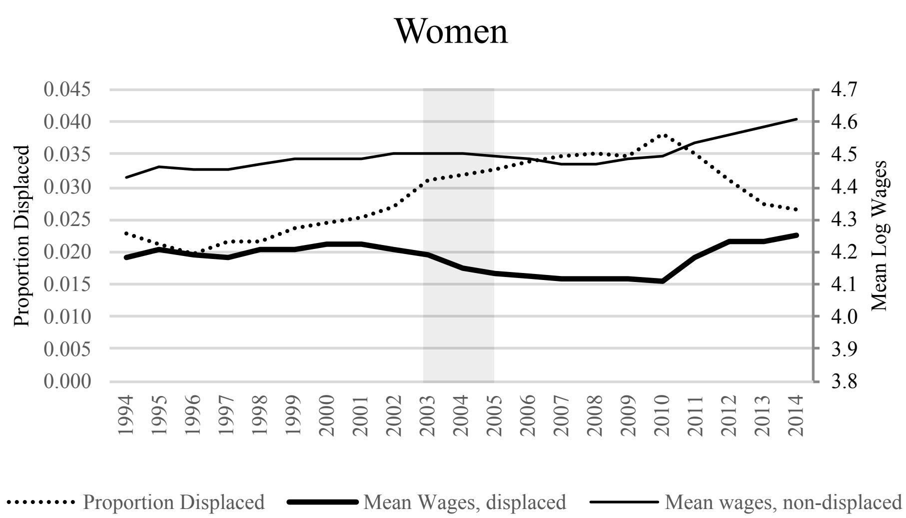
Figure 1: Unemployment and GDP Growth



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). The shaded area indicates the period during which the Hartz reforms were implemented.

Figure 2: Displacement Rates and Mean Log Wages





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 heavy solid line in each panel shows the mean log wages of workers who had been displaced from employment in the preceding four quarters. The light solid line in each panel shows the mean log wages of all other workers.

Figure 3: Employment in "Labour Recruitment and Personnel Provision" Sector

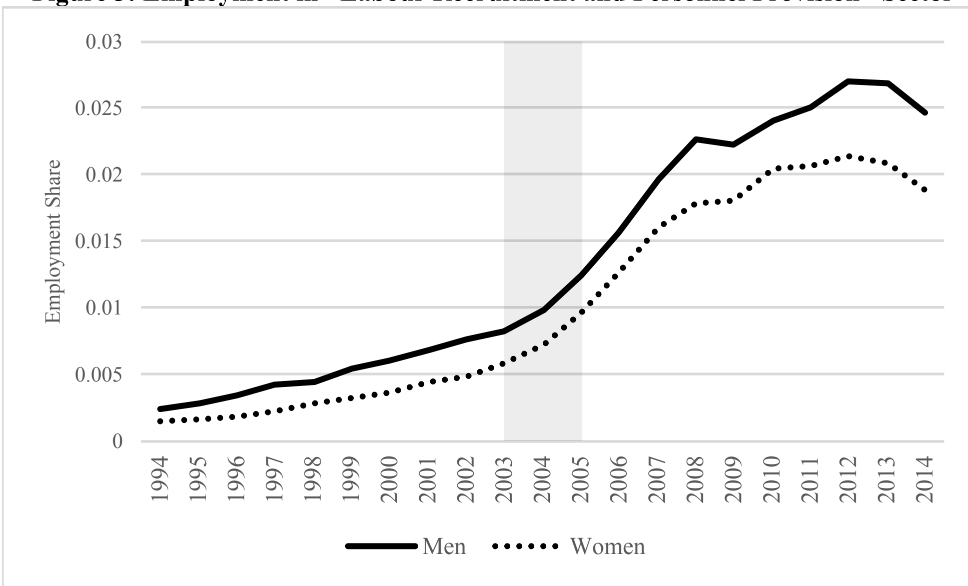


Table 1
Summary of Wage Data

	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	35,695,539	4.72	0.359	14.6	4.78	0.462	11,406,755	4.47	0.449	4.58	4.49	0.487
1993	1,262,718	4.69	0.271	13.6	4.75	0.355	384,025	4.42	0.378	2.98	4.44	0.406
1994	1,296,630	4.68	0.277	12.8	4.72	0.362	397,321	4.41	0.372	2.66	4.42	0.394
1995	1,364,970	4.70	0.284	13.0	4.75	0.366	417,428	4.44	0.374	2.82	4.46	0.404
1996	1,405,099	4.70	0.288	12.8	4.74	0.359	431,702	4.44	0.374	2.54	4.45	0.391
1997	1,439,982	4.69	0.301	13.4	4.74	0.388	439,133	4.44	0.386	2.92	4.45	0.407
1998	1,514,769	4.71	0.310	13.1	4.75	0.392	457,714	4.45	0.398	3.14	4.47	0.425
1999	1,476,416	4.71	0.320	16.1	4.78	0.435	468,679	4.46	0.416	4.15	4.48	0.449
2000	1,631,538	4.73	0.317	15.2	4.78	0.414	501,775	4.47	0.422	4.22	4.48	0.453
2001	1,673,666	4.72	0.322	14.7	4.76	0.389	519,300	4.47	0.429	4.38	4.48	0.456
2002	1,674,324	4.73	0.328	17.6	4.80	0.449	524,711	4.47	0.439	5.61	4.49	0.485
2003	1,665,405	4.75	0.357	12.5	4.79	0.432	519,560	4.48	0.454	3.47	4.50	0.480
2004	1,644,149	4.74	0.364	13.1	4.79	0.457	513,839	4.47	0.462	3.88	4.49	0.493
2005	1,635,001	4.73	0.372	13.0	4.78	0.461	515,535	4.47	0.472	3.94	4.48	0.503
2006	1,663,149	4.72	0.394	13.7	4.77	0.482	535,352	4.46	0.484	4.25	4.48	0.516
2007	1,719,079	4.70	0.400	13.8	4.76	0.491	562,333	4.44	0.493	4.40	4.45	0.527
2008	1,765,768	4.70	0.404	15.3	4.76	0.515	589,029	4.43	0.499	5.03	4.46	0.540
2009	1,734,475	4.70	0.404	14.2	4.75	0.487	594,085	4.46	0.497	4.87	4.48	0.534
2010	1,755,525	4.70	0.415	15.0	4.76	0.520	609,284	4.46	0.501	5.33	4.48	0.545
2011	1,814,357	4.71	0.406	16.4	4.77	0.517	592,658	4.50	0.473	6.46	4.53	0.525
2012	1,848,204	4.72	0.397	16.9	4.79	0.521	608,883	4.52	0.455	6.94	4.55	0.515
2013	1,855,664	4.74	0.391	16.1	4.83	0.559	612,904	4.55	0.448	6.52	4.58	0.512
2014	1,854,651	4.76	0.388	16.1	4.86	0.557	611,505	4.57	0.446	6.68	4.60	0.510

Notes: Sample includes full-time employees working in non-marginal jobs in the former West Germany, age 25-65, in all sectors except agriculture, mining, forestry, and fishing. 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).

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	2.48	4.32	37.2	24.2	12.7	52.0	3.6	7.5
Not displaced in last 4 quarters	97.5	4.79	41.1	12.0	10.9	56.1	5.5	15.5
Displaced in last 8 quarters	4.89	4.34	37.2	24.0	12.5	51.5	3.9	8.2
Displaced in last 12 quarters	7.08	4.36	37.3	23.7	12.1	51.5	4.1	8.6
Displaced in last 20 quarters	10.9	4.39	37.3	22.9	11.6	52.3	4.4	8.8
Panel B: Women								
Displaced in last 4 quarters	2.84	4.17	37.2	25.2	12.1	44.6	7.2	10.9
Not displaced in last 4 quarters	97.2	4.50	39.2	13.2	13.2	51.8	9.7	12.1
Displaced in last 8 quarters	5.71	4.19	37.2	24.3	11.5	45.4	7.5	11.4
Displaced in last 12 quarters	8.32	4.21	37.2	23.8	11.0	45.9	7.8	11.5
Displaced in last 20 quarters	12.9	4.24	37.2	23.0	10.5	47.1	8.1	11.3

Notes: Sample includes full-time employees working in non-marginal jobs in the former West Germany, age 25-65, in all sectors except agriculture, mining, forestry, and fishing, 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)	(5)	(6)
	Full Sample	Employer Tenure ≧ 24 months	Largest Connected Set	Full Sample	Employer Tenure ≧ 24 months	Largest Connected Set
ln(real daily wage)	4.78	4.83	4.84	4.49	4.54	4.58
Employer Tenure (months)	122	143	144	97.8	119	123
Age (years)	41.0	41.9	42.0	39.2	40.1	40.3
Number of jobs this quarter	1.03	1.02	1.02	1.04	1.03	1.02
Year	2004.0	2004.5	2004.4	2004.4	2004.6	2004.6
Quarter	2.50	2.51	2.51	2.50	2.51	2.51
Displaced in last 4 quarters (proportion)	0.025	0.009	0.008	0.028	0.011	0.010
Displaced in last 8 quarters (proportion)	0.049	0.017	0.016	0.057	0.021	0.019
Displaced in last 12 quarters (proportion)	0.071	0.024	0.023	0.083	0.031	0.028
Displaced in last 20 quarters (proportion)	0.109	0.042	0.040	0.129	0.055	0.050
Education (percent)						
Missing	12.3	11.0	10.7	13.6	11.7	10.6
No upper secondary, no vocational certificate	11.0	10.8	10.9	13.2	13.5	14.1
No upper secondary, with vocational certificate	56.0	57.7	57.9	51.6	53.8	53.7
Upper secondary certificate (Abitur)	5.4	5.4	5.4	9.6	9.9	10.2
Degree from Fachhochschule or university	15.3	15.1	15.2	12.1	11.1	11.4
Number of observations	35,695,539	28,898,758	28,236,539	11,406,755	8,826,630	7,986,586
Number of individuals	758,895	680,735	636,506	376,601	320,377	263,668
Number of establishments	430,602	244,964	193,752	258,349	151,683	89,253
Number of individual-establishment matches	2,095,894	1,242,318	1,184,177	859,553	507,429	432,753
Mean number of matches/individual			1.91			1.78
Mean number of matches/establishment			8,519			1,143
Proportion of individuals with only one match			0.473			0.511
Proportion of establishments with only one match			0.037			0.060

Notes: Columns (1) and (4) comprise the full sample of full-time employees working in non-marginal jobs in the former West Germany age 25-65, in all sectors except agriculture, mining, forestry, and fishing, aggregated to quarterly frequency. Columns (2) and (5) restrict the sample to individuals with at least 24 months of tenure at their current employer (if they were not displaced from employment in the preceding 4 quarters) or at least 24 months of tenure in the month of displacement (if they were displaced from employment in the preceding 4 quarters). Columns (3) and (6) further restrict the sample to the largest set of observations connected by worker mobility (see Abowd, Creecy, and Kramarz 2002 for details). Daily wages are deflated to 2010 euros using the CPI, and censored values are imputed using a Tobit model.

Table 4
Estimated Effect of the Hartz Reforms on Post-Displacement Wages

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
		Decomposition 1			Decomposition 2			
	Gross Effect	Selection	Sorting	Matching	Selection	Sorting	Matching	Net Effect
Panel A: Full-time Men								
Recently displaced	-0.235***	-0.154***	-0.074***	-0.003***	-0.155***	-0.071***	-0.005***	-0.003***
	(0.002)	(0.005)	(0.004)	(0.001)	(0.005)	(0.004)	(0.001)	(0.001)
Recently displaced ✕ during Hartz	-0.074***	0.022***	-0.069***	-0.003***	0.023***	-0.120***	-0.004***	-0.024***
	(0.003)	(0.004)	(0.003)	(0.001)	(0.004)	(0.003)	(0.001)	(0.002)
Recently displaced ✕ after Hartz	-0.143***	0.018***	-0.121***	-0.007***	0.022***	-0.120***	-0.013***	-0.032***
	(0.003)	(0.004)	(0.004)	(0.001)	(0.004)	(0.004)	(0.001)	(0.002)
R-squared	0.342	0.394	0.031	0.001	0.763	0.030		0.895
RMSE of Residual	0.346	0.264	0.186	0.054	0.264	0.185		0.142
Panel B: Full-time Women								
Recently displaced	-0.255***	-0.119***	-0.109***	-0.002***	-0.119***	-0.108***	-0.004***	-0.025***
	(0.004)	(0.008)	(0.008)	(0.001)	(0.009)	(0.008)	(0.001)	(0.002)
Recently displaced ✕ during Hartz	-0.044***	0.001	-0.041***	-0.002	0.002	-0.040***	-0.003	-0.002
	(0.007)	(0.007)	(0.008)	(0.001)	(0.007)	(0.008)	(0.002)	(0.004)
Recently displaced ✕ after Hartz	-0.051***	0.000	-0.044***	-0.006***	0.003	-0.042***	-0.012***	0.000
	(0.006)	(0.007)	(0.008)	(0.001)	(0.007)	(0.007)	(0.002)	(0.003)
R-squared	0.203	0.291	0.025	0.001	0.794	0.025		0.889
RMSE of Residual	0.389	0.303	0.258	0.054	0.303	0.258		0.150
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	
Establishment Effects							YES	
Match Effects								YES

Notes: Column (1) reports OLS estimates of our baseline specification, eq. (2). Columns (2), (3), and (4) report OLS estimates of eqs. (11), (12), and (13), respectively, where the dependent variables are estimated individual, establishment, and match effects from the orthogonal match effects model , eq. (10). Columns (5) and (6) report OLS estimates of eqs. (11) and (12), where the dependent variables are estimated individual and establishment effects from the AKM specification eq. (15). Column (7) reports the difference between estimated coefficients in the AKM specification eq. (15) and eq. (4). See Appendix Table 1 for estaimates of eq. (15). Column (8) reports OLS estimates of eq. (4). 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; standard errors in columns (2), (3), (4), (5), (6), and (7) are based on 50 block-bootstrap replications, clustered by individual. *** 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)	4.84	0.427	4.44	0.395	4.84	0.426
Individual Effect (θ)	0.000	0.339	-0.142	0.332	0.001	0.339
Establishment Effect (ψ)	0.000	0.189	-0.154	0.277	0.001	0.188
Orthogonal Match Effect (ϕ)	0.000	0.055	-0.008	0.103	0.000	0.054
Correlation (θ,ψ)	-0.023		-0.121		-0.025	
Panel B: Women						
ln(real daily wage)	4.58	0.436	4.28		4.58	
Individual Effect (θ)	0.000	0.360	-0.098	0.401	0.001	0.360
Establishment Effect (ψ)	0.000	0.262	-0.148	0.331	0.001	0.261
Orthogonal Match Effect (ϕ)	0.000	0.054	-0.006	0.101	0.000	0.053
Correlation (θ,ψ)	-0.136		-0.208		-0.136	

Notes: Estimates are based on OLS esimates of eq. (10), decomposed via the orthogonal match effect estimator. The individual, establishment, and match effects are all normalized to have zero mean in the largest connect set. 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
The Role of Observable Establishment Characteristics in Sorting

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
	Decomposition 1							Decomposition 2						
	Sorting	Industry Sector	Estab. Cohort	Estab. Size	State	Share Full-Time	Net Sorting Effect	Sorting	Industry Sector	Estab. Cohort	Estab. Size	State	Share Full-Time	Net Sorting Effect
Panel A: Full-time Men														
Recently displaced	-0.074***	-0.044***	0.000	-0.046***	-0.001	-0.003***	0.020***	-0.071***	-0.043***	0.000	-0.046***	-0.001	-0.003***	0.022***
	(0.004)	(0.002)	(0.000)	(0.001)	(0.001)	(0.000)	(0.004)	(0.004)	(0.002)	(0.000)	(0.001)	(0.001)	(0.000)	(0.004)
Recently displaced ✕ during Hartz	-0.069***	-0.021***	-0.005***	0.003***	0.000	-0.001***	-0.045***	-0.069***	-0.021***	-0.005***	0.003***	0.000	-0.001***	-0.045***
	(0.003)	(0.002)	(0.000)	(0.001)	(0.000)	(0.000)	(0.003)	(0.003)	(0.002)	(0.000)	(0.001)	(0.000)	(0.000)	(0.003)
Recently displaced ✕ after Hartz	-0.121***	-0.050***	-0.006***	0.010***	0.000	0.000	-0.075***	-0.120***	-0.050***	-0.007***	0.009***	0.000	0.000	-0.073***
	(0.004)	(0.002)	(0.000)	(0.000)	(0.000)	(0.000)	(0.004)	(0.004)	(0.002)	(0.000)	(0.000)	(0.000)	(0.000)	(0.004)
R-squared	0.031	0.020	0.013	0.038	0.003	0.095	0.527	0.030	0.020	0.014	0.038	0.003	0.095	0.525
RMSE of Residual	0.186	0.107	0.023	0.036	0.011	0.024	0.130	0.185	0.106	0.023	0.035	0.011	0.024	0.130
Panel B: Full-time Women														
Recently displaced	-0.109***	-0.053***	-0.002	-0.044***	-0.003**	0.000	-0.008	-0.108***	-0.053***	-0.002	-0.044***	-0.003**	0.000	-0.007
	(0.008)	(0.003)	(0.001)	(0.002)	(0.001)	(0.000)	(0.006)	(0.008)	(0.003)	(0.001)	(0.002)	(0.001)	(0.000)	(0.006)
Recently displaced ✕ during Hartz	-0.041***	-0.014***	-0.003***	0.002	0.000	0.000	-0.027***	-0.040***	-0.014***	-0.003***	0.002***	0.000	0.000	-0.026***
	(0.008)	(0.003)	(0.001)	(0.002)	(0.001)	(0.001)	(0.007)	(0.008)	(0.003)	(0.001)	(0.002)	(0.001)	(0.001)	(0.007)
Recently displaced ✕ after Hartz	-0.044***	-0.020***	-0.003***	0.008***	0.001*	0.002***	-0.033***	-0.042***	-0.019***	-0.003***	0.008***	0.001*	0.002***	-0.030***
	(0.008)	(0.003)	(0.001)	(0.002)	(0.001)	(0.000)	(0.007)	(0.008)	(0.003)	(0.001)	(0.002)	(0.001)	(0.000)	(0.007)
R-squared	0.025	0.027	0.011	0.031	0.003	0.084	0.374	0.025	0.027	0.011	0.031	0.003	0.084	0.371
RMSE of Residual	0.258	0.111	0.028	0.062	0.018	0.027	0.207	0.258	0.110	0.028	0.062	0.019	0.027	0.207
Year & Quarter effects	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Age & Education controls	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Establishment Characteristics controls							YES							YES

Notes: Columns (1) and (8) replicate the estimates from columns (3) and (6), respectively, of Table 4. Columns (7) and (14) augment those specifications with additional controls for establishment characteristics: industrial sector (202 categories), establishment birth year (40 categories), establishment size (8 categories), state (10 categories), and the share of employees who work full-time. Collumns (2)-(6) report estimates of the Gelbach decomposition of the difference between columns (1) and (7). Columns (9)-(13) report estimates of the Gelbach decomposition of the difference between columns (8) and (14). Individuals are defined as recently displaced if they were displaced from employment in the previous four quarters. Standard errors are based on 50 block-bootstrap replications, clustered by individual, and are 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. 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 7
Top Pre- and Post-Displacement Sectors, Men

Pre-Displacement Sector	(1) Share	(2) Estab. Effect	Post-Displacement Sector	(3) Share	(4) Estab. Effect
Panel A: 1994-1997					
45.2: Construction & civil engineering	12.2	0.048	45.2: Construction & civil engineering	16.0	0.048
45.3: Construction trades	4.75	-0.059	45.4: Construction finishing	5.20	-0.015
45.4: Construction finishing	3.36	-0.015	45.3: Construction trades	4.09	-0.059
52.4: Retail sales ex. pharmacy, food & beverage	2.62	-0.124	74.5: Labour recruitment and personnel provision	3.59	-0.305
25.2: Manufacturing, plastic products	2.32	-0.012	75.1: Public Administration	2.70	-0.180
51.4: Wholesale of household goods	2.16	-0.049	25.2: Manufacturing, plastic products	2.26	-0.012
36.1: Manufacturing, furniture	2.09	0.037	60.2: Land transport ex. railways and pipelines	2.04	-0.202
75.1: Public Administration	2.08	-0.180	52.4: Retail sales ex. pharmacy, food & beverage	2.01	-0.124
29.5: Manufacturing, other special purpose mach.	2.05	0.040	34.1: Manufacturing, motor vehicles	1.91	0.190
60.2: Land transport ex. railways and pipelines	1.86	-0.202	63.4: Other transport agencies	1.89	-0.098
Panel B: 1998-2001					
45.2: Construction & civil engineering	10.7	0.028	45.2: Construction & civil engineering	9.99	0.028
45.3: Construction trades	3.96	-0.088	74.5: Labour recruitment and personnel provision	7.57	-0.341
45.4: Construction finishing	3.93	-0.045	34.1: Manufacturing, motor vehicles	4.50	0.187
74.5: Labour recruitment and personnel provision	2.98	-0.341	45.4: Construction finishing	4.28	-0.045
52.4: Retail sales ex. pharmacy, food & beverage	2.49	-0.138	45.3: Construction trades	3.21	-0.088
63.4: Other transport agencies	2.30	-0.126	25.2: Manufacturing, plastic products	2.36	-0.015
25.2: Manufacturing, plastic products	2.17	-0.015	75.1: Public Administration	2.16	-0.185
36.1: Manufacturing, furniture	2.13	0.034	60.2: Land transport ex. railways and pipelines	2.14	-0.211
74.2: Architecture & engineering	1.89	-0.080	63.4: Other transport agencies	2.12	-0.126
60.2: Land transport ex. railways and pipelines	1.87	-0.211	52.4: Retail sales ex. pharmacy, food & beverage	2.02	-0.138
Panel C: 2005-2009					
74.5: Labour recruitment and personnel provision	10.3	-0.368	74.5: Labour recruitment and personnel provision	21.3	-0.368
34.1: Manufacture of motor vehicles	5.41	0.187	45.2: Construction & civil engineering	5.39	-0.024
45.2: Construction & civil engineering	4.31	-0.024	63.4: Other transport agencies	3.12	-0.233
63.4: Other transport agencies	3.41	-0.233	60.2: Land transport ex. railways and pipelines	2.82	-0.242
25.2: Manufacturing, plastic products	2.61	-0.039	45.4: Construction finishing	2.12	-0.144
34.3: Auto parts manufacturing	2.20	0.066	45.3: Construction trades	2.10	-0.116
60.2: Land transport ex. railways and pipelines	2.20	-0.242	74.2: Architecture & engineering	2.07	-0.080
45.3: Construction trades	1.84	-0.116	52.4: Retail sales ex. pharmacy, food & beverage	1.96	-0.201
52.4: Retail sales ex. pharmacy, food & beverage	1.84	-0.201	74.6: Investigation and security services	1.75	-0.381
45.4: Construction finishing	1.66	-0.144	34.1: Manufacturing, motor vehicles	1.69	0.187
Panel D: 2010-2014					
74.5: Labour recruitment and personnel provision	13.3	-0.358	74.5: Labour recruitment and personnel provision	25.9	-0.358
45.2: Construction & civil engineering	3.06	-0.053	45.2: Construction & civil engineering	3.18	-0.053
63.4: Other transport agencies	2.69	-0.266	60.2: Land transport ex. railways and pipelines	2.71	-0.260
60.2: Land transport ex. railways and pipelines	2.40	-0.260	63.4: Other transport agencies	2.53	-0.266
25.2: Manufacturing, plastic products	2.31	-0.058	74.2: Architecture & engineering	2.43	-0.044
34.1: Manufacturing, motor vehicles	2.31	0.196	74.1: Professional and consulting services	2.10	0.020
34.3: Manufacturing, auto parts	2.23	0.057	52.4: Retail sales ex. pharmacy, food & beverage	1.75	-0.220
52.4: Retail sales ex. pharmacy, food & beverage	1.89	-0.220	45.3: Construction trades	1.72	-0.150
80.3: Higher education	1.88	-0.217	25.2: Manufacturing, plastic products	1.72	-0.058
74.1: Professional and consulting services	1.80	0.020	75.1: Public Administration	1.57	-0.210

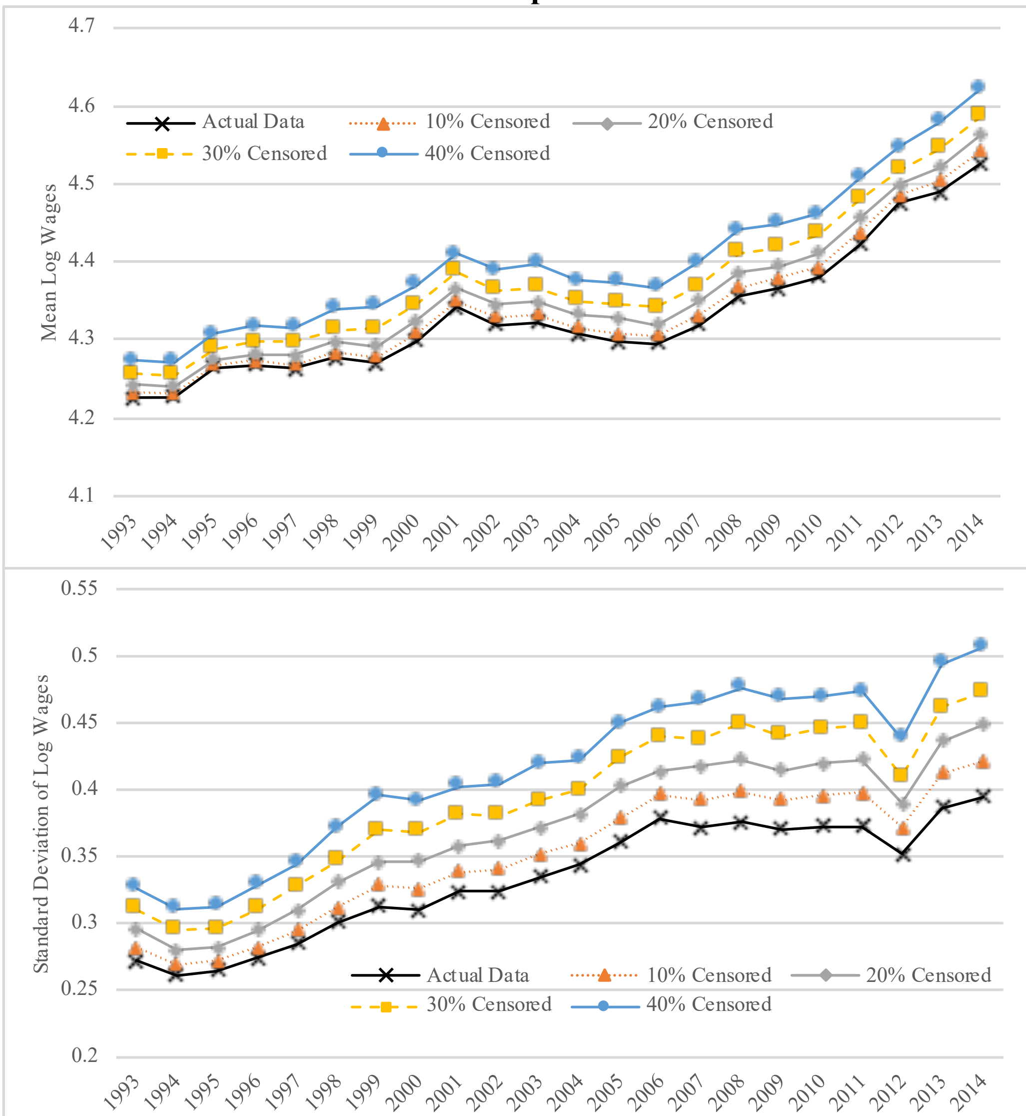
Notes: Column (1) reports the sector shares of displacements during the indicated time period. Column (3) reports sectors shares of employment in the four quarters following displacement. Columns (2) and (4) report the mean value of establishment wage fixed effects among those establishemtns operating during the indicated time period. Panel A is based on 16,327 displacements and 13,254 post-displacement jobs between 1994 and 1997. Panel B is based on 15,647 displacements and 11,157 post-displacement jobs between 1998 and 2001. Panel C is based on 44,360 displacements and 21,890 post-displacement jobs between 2005 and 2009, and Panel D is based on 34,010 displacements and 21,657 post-displacement jobs between 2010 and 2014. See notes to Table 3 for information about sample composition.

Table 8
Top Pre- and Post-Displacement Sectors, Women

Pre-Displacement Sector	(1) Share	(2) Estab. Effect	Post-Displacement Sector	(3) Share	(4) Estab. Effect
Panel A: 1994-1997					
85.1: Healthcare	6.73	-0.071	85.1: Healthcare	7.03	-0.071
85.3: Social Work	4.80	-0.055	85.3: Social Work	5.79	-0.055
52.4: Retail sales ex. pharmacy, food & beverage	4.26	-0.125	52.4: Retail sales ex. pharmacy, food & beverage	4.07	-0.125
51.4: Wholesale of household goods	3.10	-0.032	75.1: Public Administration	3.54	-0.078
18.2: Garment manufacturing, ex. Leather	2.96	-0.010	15.8: Manufactuting, other food products	3.01	-0.095
75.1: Public Administration	2.73	-0.078	55.3: Restaurants	2.83	-0.339
15.8: Manufactuting, other food products	2.21	-0.095	52.1: Retail sales, non-specialized stores	2.38	-0.039
55.3: Restaurants	2.19	-0.339	74.5: Labour recruitment and personnel provision	2.19	-0.254
74.1: Professional and consulting services	2.10	-0.077	18.2: Garment manufacturing, ex. Leather	2.11	-0.010
25.2: Manufacturing, plastic products	2.02	-0.014	55.1: Hotels	2.06	-0.302
Panel B: 1998-2001					
85.1: Healthcare	8.04	-0.077	85.1: Healthcare	8.26	-0.077
85.3: Social Work	6.38	-0.083	85.3: Social Work	6.33	-0.083
52.4: Retail sales ex. pharmacy, food & beverage	3.27	-0.132	74.5: Labour recruitment and personnel provision	4.98	-0.272
15.8: Manufactuting, other food products	3.11	-0.105	15.8: Manufactuting, other food products	3.08	-0.105
74.1: Professional and consulting services	2.90	-0.073	52.1: Retail sales, non-specialized stores	2.78	-0.043
92.3: Entertainment ex. film, television, and radio	2.55	-0.113	52.4: Retail sales ex. pharmacy, food & beverage	2.75	-0.132
75.1: Public Administration	2.48	-0.079	74.1: Professional and consulting services	2.55	-0.073
55.3: Restaurants	2.27	-0.323	75.1: Public Administration	2.38	-0.079
91.3: Religious, political, and other organizations	2.15	-0.066	91.3: Religious, political, and other organizations	1.98	-0.066
74.2: Architecture & engineering	1.94	-0.087	75.3: Compulsory social security services	1.95	0.005
Panel C: 2005-2009					
74.5: Labour recruitment and personnel provision	6.56	-0.309	74.5: Labour recruitment and personnel provision	16.9	-0.309
85.1: Healthcare	5.84	-0.103	85.1: Healthcare	6.78	-0.103
85.3: Social Work	4.70	-0.149	85.3: Social Work	6.17	-0.149
74.1: Professional and consulting services	3.39	-0.040	75.3: Compulsory social security services	5.00	-0.012
52.4: Retail sales ex. pharmacy, food & beverage	2.85	-0.183	74.1: Professional and consulting services	3.31	-0.040
34.1: Manufacture of motor vehicles	2.81	0.308	52.4: Retail sales ex. pharmacy, food & beverage	2.47	-0.183
25.2: Manufacturing, plastic products	2.59	-0.069	75.1: Public Administration	2.40	-0.077
15.8: Manufactuting, other food products	2.22	-0.113	15.8: Manufactuting, other food products	2.25	-0.113
74.7: Industrial cleaning	2.13	-0.458	73.1: R&D in natural sciences	1.87	0.145
52.1: Retail sales, non-specialized stores	2.07	-0.070	74.8: Misc. business activities	1.75	-0.119
Panel D: 2010-2014					
74.5: Labour recruitment and personnel provision	8.12	-0.267	74.5: Labour recruitment and personnel provision	18.9	-0.267
85.1: Healthcare	6.49	-0.103	85.1: Healthcare	7.03	-0.103
85.3: Social Work	4.50	-0.165	85.3: Social Work	4.51	-0.165
74.1: Professional and consulting services	3.65	-0.001	74.1: Professional and consulting services	3.98	-0.001
15.8: Manufactuting, other food products	2.51	-0.116	75.1: Public Administration	3.48	-0.079
52.4: Retail sales ex. pharmacy, food & beverage	2.41	-0.182	75.3: Compulsory social security services	2.89	-0.018
80.3: Higher education	2.37	-0.111	15.8: Manufactuting, other food products	2.29	-0.116
52.1: Retail sales, non-specialized stores	2.34	-0.097	52.4: Retail sales ex. pharmacy, food & beverage	2.29	-0.182
75.3: Compulsory social security services	2.19	-0.018	80.3: Higher education	2.25	-0.111
74.8: Misc. business activities	2.12	-0.121	74.8: Misc. business activities	2.00	-0.121

Notes: Column (1) reports the sector shares of displacements during the indicated time period. Column (3) reports sectors shares of employment in the four quarters following displacement. Columns (2) and (4) report the mean value of establishment wage fixed effects among those establishemtns operating during the indicated time period. Panel A is based on 5,750 displacements and 3,785 post-displacement jobs between 1994 and 1997. Panel B is based on 5,723 displacements and 3,996 post-displacement jobs between 1998 and 2001. Panel C is based on 15,536 displacements and 8,708 post-displacement jobs between 2005 and 2009, and Panel D is based on 12,070 displacements and 7,509 post-displacement jobs between 2010 and 2014. See notes to Table 3 for information about sample composition.

Appendix Figure A1: Trends in Mean and Standard Deviation of Log Wages, Male Apprentices Age 20-29, Actual and Artificially Censored/Imputed Data



Note: Actual data has censoring rate of between 0.3% and 0.9% in each year. Data are artificially censored at the 60th, 70th, 80th, or 90th percentile of log real wages in each year. Then Tobit models are fit separately by year, using the same specification as the main imputation model, and upper tail observations are randomly imputed using the same procedure as in our main imputation model.

Appendix Table 1
Estimates of AKM Specification

	(1) 4 Quarter Displacement Measure	(2) 8 Quarter Displacement Measure	(3) 12 Quarter Displacement Measure	(4) 20 Quarter Displacement Measure
Panel A: Full-time Men				
Recently displaced	-0.009***	-0.008***	-0.005***	0.005***
	(0.002)	(0.001)	(0.001)	(0.001)
Recently displaced ✕ during Hartz	-0.028***	-0.026***	-0.024***	-0.025***
	(0.002)	(0.002)	(0.002)	(0.001)
Recently displaced ✕ after Hartz	-0.045***	-0.042***	-0.043***	-0.052***
	(0.002)	(0.002)	(0.002)	(0.001)
R-squared	0.879	0.880	0.882	0.883
RMSE of Residual	0.149	0.149	0.148	0.149
Number of observations	28,236,539	26,972,184	25,537,770	22,559,316
Number of individuals	636,507	632,616	627,810	616,830
Number of establishments	193,752	186,287	177,685	160,409
Panel B: Full-time Women				
Recently displaced	-0.028***	-0.024***	-0.020***	-0.011***
	(0.003)	(0.002)	(0.002)	(0.002)
Recently displaced ✕ during Hartz	-0.006	-0.008**	-0.007*	-0.003
	(0.004)	(0.004)	(0.004)	(0.003)
Recently displaced ✕ after Hartz	-0.012***	-0.010***	-0.009***	-0.010***
	(0.003)	(0.003)	(0.003)	(0.003)
R-squared	0.873	0.875	0.877	0.880
RMSE of Residual	0.155	0.155	0.154	0.154
Number of observations	7,986,586	7,624,296	7,187,498	6,305,438
Number of individuals	263,668	259,354	254,090	241,978
Number of establishments	89,253	85,115	80,367	71,389
Year & Quarter effects	YES	YES	YES	YES
Age & Education controls	YES	YES	YES	YES
Individual Effects	YES	YES	YES	YES
Establishment Effects	YES	YES	YES	YES

Notes: The table reports OLS estimates of the AKM specification, eq. (15). In column 1, individuals are defined as recently displaced if they were displaced from employment in the previous four quarters. In columns 2, 3, and 4, individuals are defined as recently displaced if they were displaced from employment in the previous 8, 12, and 20 quarters, respectively. Standard errors are based on 50 block-bootstrap replications, clustered by individual, and are 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. The number of observations varies across columns because treatment and control group definitions depend on the displacement measure. See Table 3 for additional information about sample composition using the four quarter displacement measure.

Appendix Table 2
Regression Estimates for Alternate Definitions of Recent Displacement, Full-Time Men

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
		Decomposition 1			Decomposition 2			
	Gross Effect	Selection	Sorting	Matching	Selection	Sorting	Matching	Net Effect
Panel A: 8 quarter displacement measure								
Recently displaced	-0.241***	-0.150***	-0.085***	-0.003***	-0.150***	-0.082***	-0.005***	-0.003***
	(0.002)	(0.003)	(0.003)	(0.000)	(0.003)	(0.003)	(0.001)	(0.001)
Recently displaced ✕ during Hartz	-0.069***	0.022***	-0.067***	-0.003***	0.023***	-0.066***	-0.005***	-0.021***
	(0.003)	(0.003)	(0.003)	(0.001)	(0.003)	(0.003)	(0.001)	(0.002)
Recently displaced ✕ after Hartz	-0.129***	0.016***	-0.111***	-0.008***	0.021***	-0.108***	-0.016***	-0.026***
	(0.003)	(0.003)	(0.003)	(0.001)	(0.003)	(0.003)	(0.001)	(0.002)
R-squared	0.344	0.388	0.036	0.002	0.751	0.035		0.896
Panel B: 12 quarter displacement measure								
Recently displaced	-0.240***	-0.149***	-0.089***	-0.003***	-0.149***	-0.086***	-0.005***	0.001
	(0.002)	(0.003)	(0.003)	(0.000)	(0.003)	(0.003)	(0.001)	(0.001)
Recently displaced ✕ during Hartz	-0.067***	0.020***	-0.067***	-0.004***	0.023***	-0.065***	-0.007***	-0.017***
	(0.003)	(0.003)	(0.003)	(0.001)	(0.003)	(0.003)	(0.001)	(0.002)
Recently displaced ✕ after Hartz	-0.121***	0.014***	-0.103***	-0.007***	0.021***	-0.099***	-0.018***	-0.025***
	(0.003)	(0.003)	(0.003)	(0.001)	(0.003)	(0.003)	(0.001)	(0.002)
R-squared	0.347	0.399	0.039	0.002	0.725	0.037		0.897
Panel C: 20 quarter displacement measure								
Recently displaced	-0.226***	-0.146***	-0.087***	-0.002***	-0.145***	-0.085***	-0.005***	0.009***
	(0.002)	(0.003)	(0.002)	(0.000)	(0.003)	(0.002)	(0.001)	(0.001)
Recently displaced ✕ during Hartz	-0.069***	0.014***	-0.064***	-0.003***	0.017***	-0.061***	-0.008***	-0.017***
	(0.003)	(0.004)	(0.003)	(0.001)	(0.004)	(0.003)	(0.001)	(0.002)
Recently displaced ✕ after Hartz	-0.120***	0.007***	-0.093***	-0.006***	0.018***	-0.087***	-0.024***	-0.028***
	(0.003)	(0.004)	(0.003)	(0.000)	(0.003)	(0.003)	(0.001)	(0.002)
R-squared	0.351	0.406	0.045	0.002	0.375	0.043		0.896
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	
Establishment Effects							YES	
Match Effects								YES

Notes: Column (1) reports OLS estimates of our baseline specification, eq. (2). Columns (2), (3), and (4) report OLS estimates of eqs. (11), (12), and (13), respectively, where the dependent variables are estimated individual, establishment, and match effects from the orthogonal match effects model , eq. (10). Columns (5) and (6) report OLS estimates of eqs. (11) and (12), where the dependent variables are estimated individual and establishment effects from the AKM specification eq. (15). See Appendix Table 1 for estimates of eq. (15). Column (7) reports the difference between estimated coefficients in the AKM specification eq. (15) and eq. (4). Column (8) reports OLS estimates of eq. (4). In Panel A, individuals are defined as recently displaced if they were displaced from employment in the previous eight quarters. Individuals in panels B and C are defined as recently displaced if they were displaced from employment in the previous 12 or 20 quarters, respectively. Standard errors are clustered by individual and reported in parentheses; standard errors in columns (2), (3), (4), (5), (6), and (7) are based on 50 block-bootstrap replications, clustered by individual. *** 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 and Appendix Table 1 for the number of observations, workers, establishments, and matches in the largest connected set; and notes to Table 3 for information about sample composition.

Appendix Table 3
Regression Estimates for Alternate Definitions of Recent Displacement, Full-Time Women

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
		Decomposition 1			Decomposition 2			
	Gross Effect	Selection	Sorting	Matching	Selection	Sorting	Matching	Net Effect
Panel A: 8 quarter displacement measure								
Recently displaced	-0.250***	-0.120***	-0.107***	-0.001***	-0.120***	-0.106***	-0.003***	-0.021***
	(0.004)	(0.006)	(0.006)	(0.001)	(0.006)	(0.006)	(0.001)	(0.002)
Recently displaced ✕ during Hartz	-0.045***	0.005	-0.045***	-0.002**	0.006	-0.043***	-0.005**	-0.003
	(0.007)	(0.008)	(0.009)	(0.001)	(0.008)	(0.008)	(0.002)	(0.004)
Recently displaced ✕ after Hartz	-0.045***	0.006	-0.048***	-0.006***	0.010	-0.045***	-0.013***	0.002
	(0.005)	(0.006)	(0.007)	(0.001)	(0.006)	(0.006)	(0.002)	(0.003)
R-squared	0.206	0.276	0.027	0.001	0.787	0.026		0.890
Panel B: 12 quarter displacement measure								
Recently displaced	-0.242***	-0.119***	-0.104***	-0.001**	-0.119***	-0.103***	-0.003**	-0.018***
	(0.004)	(0.006)	(0.006)	(0.001)	(0.006)	(0.006)	(0.001)	(0.002)
Recently displaced ✕ during Hartz	-0.050***	0.006	-0.053***	-0.002**	0.008	-0.051***	-0.005**	-0.002
	(0.007)	(0.008)	(0.007)	(0.001)	(0.007)	(0.007)	(0.002)	(0.004)
Recently displaced ✕ after Hartz	-0.043***	0.004	-0.047***	-0.005***	0.009*	-0.044***	-0.014***	0.005
	(0.005)	(0.006)	(0.006)	(0.001)	(0.006)	(0.006)	(0.002)	(0.003)
R-squared	0.210	0.284	0.028	0.001	0.774	0.027		0.891
Panel C: 20 quarter displacement measure								
Recently displaced	-0.216***	-0.117***	-0.091***	-0.001***	-0.116***	-0.090***	-0.003***	-0.008***
	(0.004)	(0.005)	(0.005)	(0.000)	(0.005)	(0.005)	(0.001)	(0.002)
Recently displaced ✕ during Hartz	-0.063***	-0.002	-0.060***	-0.001	-0.001	-0.059***	-0.004*	0.000
	(0.006)	(0.009)	(0.007)	(0.001)	(0.009)	(0.007)	(0.002)	(0.004)
Recently displaced ✕ after Hartz	-0.057***	-0.008	-0.050***	-0.003***	0.000	-0.047***	-0.014***	0.004
	(0.005)	(0.008)	(0.006)	(0.001)	(0.007)	(0.006)	(0.002)	(0.003)
R-squared	0.216	0.287	0.028	0.001	0.746	0.027		0.892
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	
Establishment Effects							YES	
Match Effects								YES

Notes: Column (1) reports OLS estimates of our baseline specification, eq. (2). Columns (2), (3), and (4) report OLS estimates of eqs. (11), (12), and (13), respectively, where the dependent variables are estimated individual, establishment, and match effects from the orthogonal match effects model , eq. (10). Columns (5) and (6) report OLS estimates of eqs. (11) and (12), where the dependent variables are estimated individual and establishment effects from the AKM specification eq. (15). See Appendix Table 1 for estimates of eq. (15). Column (7) reports the difference between estimated coefficients in the AKM specification eq. (15) and eq. (4). Column (8) reports OLS estimates of eq. (4). In Panel A, individuals are defined as recently displaced if they were displaced from employment in the previous eight quarters. Individuals in panels B and C are defined as recently displaced if they were displaced from employment in the previous 12 or 20 quarters, respectively. Standard errors are clustered by individual and reported in parentheses; standard errors in columns (2), (3), (4), (5), (6), and (7) are based on 50 block-bootstrap replications, clustered by individual. *** 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 and Appendix Table 1 for the number of observations, workers, establishments, and matches in the largest connected set; and notes to Table 3 for information about sample composition.

Appendix Table 4
Regression Estimates Based on a Stricter Definition of Involuntary Displacement

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
		Decomposition 1			Decomposition 2			
	Gross Effect	Selection	Sorting	Matching	Selection	Sorting	Matching	Net Effect
Panel A: Full-time Men								
Recently displaced	-0.258***	-0.162***	-0.090***	-0.002	-0.164***	-0.088***	-0.003	-0.004
	(0.007)	(0.007)	(0.007)	(0.002)	(0.007)	(0.008)	(0.003)	(0.005)
Recently displaced ✕ during Hartz	-0.050***	0.018	-0.049***	-0.009**	0.019	-0.047***	-0.012**	-0.011
	(0.013)	(0.014)	(0.012)	(0.004)	(0.014)	(0.012)	(0.006)	(0.008)
Recently displaced ✕ after Hartz	-0.148***	0.002	-0.117***	-0.008**	0.005	-0.115***	-0.013**	-0.024***
	(0.011)	(0.010)	(0.011)	(0.004)	(0.010)	(0.011)	(0.006)	(0.006)
R-squared	0.338	0.393	0.025	0.001	0.765	0.024		0.894
RMSE of Residual	0.346	0.265	0.186	0.054	0.264	0.185		0.142
Panel B: Full-time Women								
Recently displaced	-0.295***	-0.143***	-0.098***	-0.005	-0.142***	-0.096***	-0.007	-0.049***
	(0.018)	(0.019)	(0.018)	(0.003)	(0.020)	(0.018)	(0.005)	(0.009)
Recently displaced ✕ during Hartz	-0.038	-0.015	-0.058	0.000	-0.016	-0.056	-0.001	0.035***
	(0.032)	(0.045)	(0.037)	(0.007)	(0.045)	(0.036)	(0.012)	(0.015)
Recently displaced ✕ after Hartz	-0.088***	-0.016	-0.086***	-0.001	-0.015	-0.085***	-0.002	0.015
	(0.025)	(0.023)	(0.023)	(0.004)	(0.023)	(0.023)	(0.008)	(0.013)
R-squared	0.200	0.289	0.021	0.001	0.793	0.021		0.887
RMSE of Residual	0.388	0.304	0.259	0.053	0.304	0.258		0.150
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	
Establishment Effects							YES	
Match Effects								YES

Notes: Column (1) reports OLS estimates of our baseline specification, eq. (2). Columns (2), (3), and (4) report OLS estimates of eqs. (11), (12), and (13), respectively, where the dependent variables are estimated individual, establishment, and match effects from the orthogonal match effects model , eq. (10). Columns (5) and (6) report OLS estimates of eqs. (11) and (12), where the dependent variables are estimated individual and establishment effects from the AKM specification eq. (15). Column (7) reports the difference between estimated coefficients in the AKM specification eq. (15) and eq. (4). Column (8) reports OLS estimates of eq. (4). Individuals are defined as recently displaced if they were displaced from employment in the previous four quarters due to establishment closure. Standard errors are clustered by individual and reported in parentheses; standard errors in columns (2), (3), (4), (5), (6), and (7) are based on 50 block-bootstrap replications, clustered by individual. *** 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. The sample of full-time men comprises 27,942,825 observations on 630,294 individuals employed at 180,700 establishments. The sample of full-time women comprises 7,853,530 observations on 258,869 individuals employed at 82,226 establishments. These observation counts differ from Table 4 because the treatment and control groups in the two tables depend on different definitions of displacement.

Appendix Table 5
Regression Estimates Based on 36-Month Employer Tenure Restriction

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
		Decomposition 1			Decomposition 2			
	Gross Effect	Selection	Sorting	Matching	Selection	Sorting	Matching	Net Effect
Panel A: Full-time Men								
Recently displaced	-0.234***	-0.151***	-0.077***	-0.005***	-0.152***	-0.073***	-0.008***	-0.001
	(0.003)	(0.006)	(0.006)	(0.000)	(0.006)	(0.006)	(0.001)	(0.002)
Recently displaced ✕ during Hartz	-0.077***	0.020***	-0.068***	-0.001	0.021***	-0.068***	-0.001	-0.029***
	(0.004)	(0.006)	(0.006)	(0.001)	(0.006)	(0.006)	(0.001)	(0.003)
Recently displaced ✕ after Hartz	-0.139***	0.023***	-0.115***	-0.003***	0.025***	-0.114***	-0.007***	-0.044***
	(0.004)	(0.005)	(0.005)	(0.001)	(0.005)	(0.005)	(0.002)	(0.003)
R-squared	0.347	0.391	0.021	0.001	0.469	0.021		0.888
RMSE of Residual	0.336	0.265	0.181	0.047	0.265	0.181		0.142
Panel B: Full-time Women								
Recently displaced	-0.265***	-0.124***	-0.113***	-0.003***	-0.123***	-0.111***	-0.005***	-0.026***
	(0.006)	(0.013)	(0.014)	(0.001)	(0.013)	(0.014)	(0.002)	(0.003)
Recently displaced ✕ during Hartz	-0.045	0.004	-0.045***	-0.001	0.005	-0.044***	-0.003	-0.002
	(0.009)	(0.013)	(0.013)	(0.002)	(0.013)	(0.013)	(0.004)	(0.006)
Recently displaced ✕ after Hartz	-0.046***	-0.003	-0.046***	-0.004***	0.000	-0.044***	-0.010***	0.007
	(0.007)	(0.013)	(0.012)	(0.001)	(0.013)	(0.012)	(0.003)	(0.005)
R-squared	0.207	0.259	0.016	0.001	0.776	0.016		0.881
RMSE of Residual	0.376	0.321	0.271	0.047	0.321	0.271		0.150
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	
Establishment Effects							YES	
Match Effects								YES

Notes: Column (1) reports OLS estimates of our baseline specification, eq. (2). Columns (2), (3), and (4) report OLS estimates of eqs. (11), (12), and (13), respectively, where the dependent variables are estimated individual, establishment, and match effects from the orthogonal match effects model , eq. (10). Columns (5) and (6) report OLS estimates of eqs. (11) and (12), where the dependent variables are estimated individual and establishment effects from the AKM specification eq. (15). Column (7) reports the difference between estimated coefficients in the AKM specification eq. (15) and eq. (4). See Appendix Table 1 for estaimates of eq. (15). Column (8) reports OLS estimates of eq. (4). Individuals are defined as recently displaced if they were displaced from employment in the previous four quarters due to establishment closure. Standard errors are clustered by individual and reported in parentheses; standard errors in columns (2), (3), (4), (5), (6), and (7) are based on 50 block-bootstrap replications, clustered by individual. *** 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. Sample definition is the same as described in Table 3, except restricted to individuals with at least 36 months of tenure at their current employer (if they were not displaced from eployment in the preceding 4 quarters) or at least 36 months of tenure in the month of displacement (if they were displaced from employment in the preceding 4 quarters). Sample size for Panel A is 24,999,483 observations on 581,786 individual, 131,084 establishments. Sample size for Panel B is 6,691,108 observations on 225,963 individuals, 54,395 establishments. See notes to Table 3 for additional information about sample composition.

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
		Decomposition 1			Decomposition 2			
	Gross Effect	Selection	Sorting	Matching	Selection	Sorting	Matching	Net Effect
Panel A: Full-time Men								
Recently displaced	-0.238***	-0.143***	-0.080***	-0.004***	-0.143***	-0.077***	-0.007***	-0.011***
	(0.002)	(0.004)	(0.004)	(0.000)	(0.004)	(0.004)	(0.001)	(0.001)
Recently displaced × during Hartz	-0.073***	0.021***	-0.068***	-0.002**	0.023***	-0.069***	-0.003***	-0.025***
	(0.004)	(0.004)	(0.003)	(0.001)	(0.004)	(0.004)	(0.001)	(0.002)
Recently displaced × after Hartz	-0.130***	0.020***	-0.105***	-0.005***	0.025***	-0.103***	-0.012***	-0.040***
	(0.004)	(0.004)	(0.004)	(0.001)	(0.004)	(0.004)	(0.002)	(0.002)
R-squared	0.348	0.345	0.017	0.001	0.414	0.017		0.902
RMSE of Residual	0.317	0.256	0.167	0.041	0.256	0.166		0.126
Panel B: Full-time Women								
Recently displaced	-0.244***	-0.111***	-0.104***	-0.002***	-0.111***	-0.101***	-0.004***	-0.027***
	(0.005)	(0.010)	(0.011)	(0.001)	(0.010)	(0.011)	(0.001)	(0.002)
Recently displaced × during Hartz	-0.041***	0.000	-0.038***	-0.002*	0.002	-0.038***	-0.005**	-0.001
	(0.007)	(0.011)	(0.009)	(0.001)	(0.011)	(0.009)	(0.002)	(0.004)
Recently displaced × after Hartz	-0.063***	0.003	-0.060***	-0.006***	0.010	-0.057***	-0.017***	0.001
	(0.008)	(0.010)	(0.009)	(0.001)	(0.010)	(0.009)	(0.003)	(0.004)
R-squared	0.205	0.209	0.017	0.001	0.776	0.017		0.891
RMSE of Residual	0.360	0.312	0.248	0.042	0.311	0.246		0.137
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	
Establishment Effects							YES	
Match Effects								YES

Notes: Column (1) reports OLS estimates of our baseline specification, eq. (2). Columns (2), (3), and (4) report OLS estimates of eqs. (11), (12), and (13), respectively, where the dependent variables are estimated individual, establishment, and match effects from the orthogonal match effects model , eq. (10). Columns (5) and (6) report OLS estimates of eqs. (11) and (12), where the dependent variables are estimated individual and establishment effects from the AKM specification eq. (15). Column (7) reports the difference between estimated coefficients in the AKM specification eq. (15) and eq. (4). See Appendix Table 1 for estaimates of eq. (15). Column (8) reports OLS estimates of eq. (4). 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; standard errors in columns (2), (3), (4), (5), (6), and (7) are based on 50 block-bootstrap replications, clustered by individual. *** 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. Sample size for Panel A is 19,211,401 observations on 516,579 individuals employed at 128,274 establishments. Sample size for Panel B is 5,136,780 observations on 190,532 individuals employed at 52,048 establishments. See notes to Table 3 for information about sample composition.

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
		Decomposition 1			Decomposition 2			
	Gross Effect	Selection	Sorting	Matching	Selection	Sorting	Matching	Net Effect
Panel A: Full-time Men								
Recently displaced	-0.268***	-0.161***	-0.091***	-0.003***	-0.161***	-0.089***	-0.005***	-0.013***
	(0.003)	(0.007)	(0.007)	(0.001)	(0.007)	(0.007)	(0.001)	(0.002)
Recently displaced ✕ during Hartz	-0.044***	0.025***	-0.051***	-0.001	0.026***	-0.051***	-0.002	-0.017***
	(0.004)	(0.005)	(0.005)	(0.001)	(0.005)	(0.005)	(0.001)	(0.002)
Recently displaced ✕ after Hartz	-0.103***	0.023***	-0.089***	-0.005***	0.028***	-0.087***	-0.012***	-0.032***
	(0.004)	(0.005)	(0.005)	(0.001)	(0.005)	(0.004)	(0.002)	(0.003)
R-squared	0.343	0.349	0.018	0.001	0.350	0.018		0.910
RMSE of Residual	0.327	0.270	0.171	0.036	0.270	0.170		0.125
Panel B: Full-time Women								
Recently displaced	-0.247***	-0.119***	-0.095***	-0.002**	-0.119***	-0.094***	-0.004***	-0.030***
	(0.006)	(0.017)	(0.015)	(0.001)	(0.016)	(0.015)	(0.002)	(0.003)
Recently displaced ✕ during Hartz	-0.039***	-0.006	-0.035***	-0.001	-0.006	-0.034***	-0.002	0.003
	(0.008)	(0.014)	(0.012)	(0.001)	(0.014)	(0.012)	(0.002)	(0.004)
Recently displaced ✕ after Hartz	-0.059***	0.004	-0.064***	-0.004***	0.009	-0.062***	-0.012***	0.006
	(0.009)	(0.015)	(0.013)	(0.001)	(0.015)	(0.013)	(0.003)	(0.005)
R-squared	0.211	0.202	0.016	0.000	0.721	0.016		0.900
RMSE of Residual	0.369	0.335	0.253	0.036	0.335	0.253		0.136
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	
Establishment Effects							YES	
Match Effects								YES

Notes: Column (1) reports OLS estimates of our baseline specification, eq. (2). Columns (2), (3), and (4) report OLS estimates of eqs. (11), (12), and (13), respectively, where the dependent variables are estimated individual, establishment, and match effects from the orthogonal match effects model , eq. (10). Columns (5) and (6) report OLS estimates of eqs. (11) and (12), where the dependent variables are estimated individual and establishment effects from the AKM specification eq. (15). Column (7) reports the difference between estimated coefficients in the AKM specification eq. (15) and eq. (4). See Appendix Table 1 for estaimates of eq. (15). Column (8) reports OLS estimates of eq. (4). 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; standard errors in columns (2), (3), (4), (5), (6), and (7) are based on 50 block-bootstrap replications, clustered by individual. *** 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. Sample size for Panel A is 14,403,586 observations on 501,002 individuals employed at 97,438 establishments. Sample size for Panel B is 3,761,034 observations on 170,162 individuals employed at 37,438 establishments. See notes to Table 3 for information about sample composition.

Appendix Table 8
Model Estimates for Specification with Linear Time Trends

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
		Decomposition 1			Decomposition 2			
	Gross Effect	Selection	Sorting	Matching	Selection	Sorting	Matching	Net Effect
Panel A: Full-time Men								
Recently displaced	-0.010	-0.113***	0.050***	-0.025***	-0.122***	0.057***	-0.024***	0.078***
	(0.020)	(0.017)	(0.019)	(0.006)	(0.017)	(0.019)	(0.009)	(0.012)
Recently displaced × during Hartz	-0.036***	0.031***	-0.048***	-0.008*	0.031***	-0.047***	-0.009***	-0.010***
	(0.004)	(0.004)	(0.004)	(0.001)	(0.004)	(0.004)	(0.002)	(0.003)
Recently displaced × after Hartz	-0.081***	0.037***	-0.083***	-0.013***	0.038***	-0.081***	-0.016***	-0.022***
	(0.007)	(0.006)	(0.005)	(0.002)	(0.006)	(0.005)	(0.003)	(0.004)
R-squared	0.341	0.350	0.031	0.001	0.333	0.031		0.893
RMSE of Residual	0.347	0.264	0.188	0.055	0.263	0.187		0.143
Panel B: Full-time Women								
Recently displaced	-0.554***	-0.168***	-0.261***	-0.046***	-0.163***	-0.249***	-0.064***	-0.079***
	(0.039)	(0.052)	(0.051)	(0.009)	(0.053)	(0.051)	(0.016)	(0.023)
Recently displaced × during Hartz	-0.091***	-0.008	-0.068***	-0.010	-0.006	-0.065***	-0.015***	-0.004
	(0.009)	(0.012)	(0.011)	0.002***	(0.012)	(0.011)	(0.003)	(0.005)
Recently displaced × after Hartz	-0.157***	-0.013	-0.097***	-0.020***	-0.008	-0.091***	-0.030***	-0.027***
	(0.013)	(0.017)	(0.018)	(0.003)	(0.017)	(0.018)	(0.005)	(0.007)
R-squared	0.201	0.351	0.025	0.001	0.358	0.024		0.888
RMSE of Residual	0.390	0.303	0.260	0.054	0.303	0.259		0.150
Year & Quarter effects	YES	YES	YES	YES	YES	YES	YES	YES
Age & Education controls	YES	YES	YES	YES	YES	YES	YES	YES
Linear Time Trends	YES	YES	YES	YES	YES	YES	YES	YES
Individual Effects							YES	
Establishment Effects							YES	
Match Effects								YES

Notes: Columns (1)-(8) reproduce estimates of specifications from the corresponding column of Table 4, with the addition of separate linear quarterly trends for recently displaced and non-displaced workers. 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; standard errors in columns (2), (3), (4), (5), (6), and (7) are based on 50 block-bootstrap replications, clustered by individual. *** 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.

Appendix Table 9
Model Estimates for Specification with Tenure, Sector, and Occupation Controls

	(1)	(2)	(3)	(4)	(5)
		Gelbach Decomposition			
	Gross Effect	Tenure	Sector	Occupation	Net Effect
Panel A: Full-time Men					
Recently displaced	-0.235***	-0.020***	-0.095***	-0.040***	-0.081***
	(0.002)	(0.000)	(0.001)	(0.001)	(0.002)
Recently displaced × during Hartz	-0.075***	-0.010***	-0.020***	0.008***	-0.053***
	(0.003)	(0.000)	(0.002)	(0.002)	(0.003)
Recently displaced × after Hartz	-0.144***	-0.020***	-0.049***	0.008***	-0.083***
	(0.003)	(0.000)	(0.001)	(0.001)	(0.002)
R-squared	0.347	0.293	0.025	0.400	0.603
RMSE of Residual	0.344	0.032	0.133	0.159	0.268
Panel B: Full-time Women					
Recently displaced	-0.258***	-0.038***	-0.088***	-0.041***	-0.091***
	(0.004)	(0.001)	(0.003)	(0.002)	(0.004)
Recently displaced × during Hartz	-0.046***	-0.009***	-0.015***	0.016***	-0.037***
	(0.007)	(0.001)	(0.003)	(0.003)	(0.006)
Recently displaced × after Hartz	-0.055***	-0.010***	-0.017***	0.016***	-0.044***
	(0.006)	(0.001)	(0.003)	(0.002)	(0.005)
R-squared	0.213	0.242	0.026	0.303	0.495
RMSE of Residual	0.380	0.043	0.151	0.148	0.305
Year & Quarter effects	YES	YES	YES	YES	YES
Age & Education controls	YES	YES	YES	YES	YES
Employer Tenure controls		YES	YES	YES	YES
Sector Controls		YES	YES	YES	YES
Occupation Controls		YES	YES	YES	YES

Notes: Column (1) replicates column (1) of Table 4 on the subset of observations with non-missing occupation and employer tenure. Column (5) augments that specification with additional controls for employer tenure (fully interacted with indicators for the periods during and after the Hartz reforms), industrial sector (202 categories), and occupation (341 categories). Columns (2), (3), and (4) report estimates of the Gelbach decomposition that comparres the baseline model in column (1) to the full specification in column (5). 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; standard errors in columns (2), (3), and (4) are based on 50 block-bootstrap replications, clustered by individual. *** 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. Sample size for Panel A is 27,941,270 observations on 635,341 individuals employed at 192,652 establishments. Sample size for Panel B is 7,857,486 observations on 261,262 individuals employed at 87,646 establishments.