The Determinants of Displaced Workers' Wages: Sorting, Matching, Selection, and the Hartz Reforms*

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We present a simple new method to decompose the wage effects of displacement into components due to differences in the way that displaced and non-displaced workers are sorted across higher- and lower-paying employers (a sorting effect), differences in the quality of worker-employer matches they enter into (a matching effect), and differences in their unobservable characteristics (a selection effect). In an extended application, we apply our decomposition to understand how the determinants of displaced workers' wages in Germany changed following the 2003-2005 Hartz reforms. We find that the wages of displaced workers fell substantially after the reforms, and that over 80 percent of the decline was because they found re-employment at lower-paying employers. Sorting into worse matches explains a smaller 5-9 percent of the wage decline experienced by men, and 12-23.5 percent of the female wage decline. Collectively, the sorting and matching channels explain almost all of the post-reform decline in displaced workers' wages, and selection played little role.

Keywords: Displacement, wages, fixed effects, decomposition, Hartz reforms

JEL Codes: J31, J63, J65, C23

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1 Introduction

Workers experience large and highly persistent wage losses when they are displaced from employment. While a large literature has documented and quantified these losses, the underlying mechanisms are less well understood.[1] Long-standing hypotheses about the cause of wage losses center on the loss of valuable industry-, job-, or occupation-specific human capital, or match-specific rents, at displacement.[2] Several recent studies have focused on the role of employers in explaining wage losses, and have relied on employer-specific fixed effects in wages to quantify how much of the wage loss is attributable to moving from a higher-paying employer to a lower-paying employer after displacement. The employer-specific fixed effects, or employer wage premia, are estimated from a wage model in the style of Abowd, Kramarz, and Margolis (1999; AKM hereafter). Among these recent studies, Schmieder et al. (2019) and Fackler et al. (2021) conclude that most of the wage loss at displacement is attributable to the loss of employer-specific wage premia in Germany, while Lachowska et al. (2020) and Moore and Scott-Clayton (2019) find that employer-specific factors play a minor role in wage losses in the United States. Lachowska et al. (2020) conclude instead that the loss of match-specific wage premia is the main source of wage losses at displacement.

We contribute to this recent literature with a simple new decomposition of wage changes following displacement. Our decomposition distinguishes between a *sorting* effect that arises if displaced workers are employed in lower-paying establishments after displacement, a *matching* effect that arises if displaced workers find re-employment in lower-paying worker-employer matches, and a *selection* effect that arises if displaced workers have unobserved characteristics that earn lower labor market returns than non-displaced workers. The distinction between these channels is natural, and reflects the possibility that displaced workers may earn low wages because they are employed at low-paying establishments, because they enter into poor quality matches, or because they have poor unobserved characteristics. More importantly, quantifying the relative magnitude of these effects helps to illuminate the specific channels that generate wage losses at displacement. This is especially valuable for understanding the effects of UI reforms, re-training programs, job search assistance, and other policies that affect the wage and employment outcomes of displaced workers, as we show

[1] See, among many others, Topel (1990), Jacobson et al. (1993), Farber (1993, 2017), Couch and Placzek (2010), Davis and von Wachter (2011).

[2] Topel (1990), Neal (1995), Poletaev and Robinson (2008).

in an application to the Hartz reforms. It is also important for designing policy to mitigate the adverse consequences of displacement. For example, if post-displacement wage losses are primarily because workers return to employment at lower-wage employers, then targeted wage subsidies that increase hiring of displaced workers at higher-wage employers may be highly effective at mitigating wage losses. On the other hand, if post-displacement wage losses are primarily because displaced workers enter into lower-quality matches with employers when they return to work, then policies that facilitate search and improve matching outcomes may be more effective.

Beyond quantifying the importance of these three channels in displaced workers' wages, our decomposition makes several additional contributions. First, it formalizes the approach of recent studies like Schmieder et al. (2019), Moore and Scott-Clayton (2019), Lachowska et al. (2020), and Fackler et al. (2021) that rely on AKM employer wage premia to quantify the role of employers in wage losses at displacement, and shows that estimators of the type used in those papers are subject to two kinds of bias. The first bias arises when employer wage premia are estimated separately from the wage effects of displacement, or equivalently, when the AKM specification does not control for displacement. We show that this may bias estimators of the employer-specific component of the wage loss at displacement (akin to our sorting effect). The bias is non-zero if displaced and non-displaced workers sort systematically into different employers before or after displacement. Our application provides evidence that they do. Other studies mitigate such bias by excluding displaced workers from the AKM estimation sample. However, this might not eliminate all bias, because those studies rely on narrow definitions of displacement, so that some workers who lose a job and experience large wage declines remain in the AKM sample.[3] Moreover, sample exclusions based on displacement may introduce other selection biases in the estimated AKM wage components. Indeed, Moore and Scott-Clayton (2019) find that their estimates are sensitive to the exclusion rule used to define their AKM sample.[4]

[3]For example, Lachowska et al. (2020) define displaced workers as individuals with at least six years of job tenure who lose their job within four quarters of a mass layoff event. Consider a displaced worker and a co-worker with five years of job tenure who loses her job at the same time. The co-worker does not meet the definition of a displaced worker and hence would not be excluded from the AKM sample, but probably experiences a similar wage loss. That wage loss isn't controlled for in the AKM specification and will bias estimated establishment wage premia if displaced workers sort systematically into particular employers.

[4]When they exclude displaced workers' post-displacement observations from their AKM sample, their estimated employer wage premia explain 24 percent of wage losses at displacement. When they exclude all observations on displaced workers and a comparable control group of non-displaced workers from the AKM sample, as in Lachowska et al. (2020), their estimated employer wage premia explain only 16 percent of wage losses.

The second kind of bias arises in studies that do not account for match effects. These are time-invariant factors that are specific to the worker-employer match and influence wages, such as match quality or match-specific human capital. Lachowska et al. (2020) show that lost match effects are the main source of wage losses of displaced workers in Washington, but other studies don't account for their contribution to wage losses. We show that omitting match effects from the equation used to estimate AKM wage components will bias estimators of the employer-specific component of the wage loss if displaced and non-displaced workers sort systematically into different quality matches. This would be the case if displaced workers systematically find re-employment in lower-quality matches, which is indeed what Lachowska et al. (2020) find.

Thus, a second contribution is to address both kinds of bias by simultaneously estimating the wage effects of displacement and AKM-style wage components in a framework that accounts for match effects. Specifically, our decomposition is based on the match effects model of Woodcock (2008, 2015), which extends the AKM framework to include match-specific fixed effects in wages. Our specification controls for the time-invariant unobserved characteristics of workers via individual fixed effects, time-invariant employer-specific factors via employer fixed effects, match effects, and the wage effects of displacement. Our decomposition is based on the difference between the estimated wage effects of displacement in that specification, and in a specification that omits controls for the unobserved characteristics of individuals, their employers, and matches. We show, via the omitted variables bias formula, that the difference between the estimated wage effects of displacement in these two specifications can be decomposed into three components: one attributable to differences in the way that displaced and non-displaced workers are sorted across higher- and lower-wage employers (the sorting effect), one attributable to differences in the way they are sorted across higher- and lower-wage matches with their employers (the matching effect), and one attributable to differences in their unobserved characteristics (the selection effect).

Our decomposition is related to the Pendakur and Woodcock (2010) "Glass Door effect" and the Gelbach (2016) decomposition, both of which rely on the omitted variables bias formula to decompose the difference between a short regression and a long regression. Gelbach (2016) focuses on quantifying the contribution of different observable characteristics to the wage gap between two groups, whereas our focus is on unobservables. Pendakur and Woodcock (2010) quantify how differences in the way that immigrants and natives are sorted across employers contributes to the wage gap between them. That is similar to the sorting effect that we develop here, but ignores the roles of matching and selection. Our decomposition approach thus extends the insights and framework developed in those two papers to encompass multiple dimensions of unobserved heterogeneity.

Our decomposition embeds conveniently in a difference-in-differences framework. We exploit this in an extended application to the 2003-2005 Hartz reforms in Germany. These reforms were designed to increase labor market flexibility and reduce long-term unemployment through a collection of initiatives to deregulate the labor market, increase job search assistance, and reform the UI system by imposing stricter job search requirements and reducing the generosity of long-term unemployment benefits. We do not attempt to identify the effect of specific initiatives within the suite of reforms, but rather to understand how the determinants of recently displaced workers' wages changed from the period before the reforms to the period after. The net effect of the reforms on displaced workers' wages is theoretically ambiguous: Schmieder et al. (2016) and Nekoei and Weber (2017) note that UI generosity has an ambiguous effect on post-UI earnings;[5] and while initiatives to facilitate search and improve matching might improve post-displacement wages, deregulation probably had the opposite effect. Empirically, Price (2016) estimates that the reforms reduced the wages of workers who exhausted short-term unemployment benefits by 4-8 percent, while Engbom et al. (2015) reports a larger 10 percent wage loss after displacement.[6] However, neither study quantifies how the reforms changed the way that displaced workers are sorted across employers and matches, and how this affected wages, as we do.

We find that recently displaced men experienced a 14 log point wage decline relative to non-displaced men after the reforms. Recently displaced women experienced a smaller 5 log point decline.[7] For both sexes, our decomposition shows that sorting explains the lion's share – roughly 84 percent – of the wage decline. That is, the wages of recently displaced workers fell after the reforms predominantly because they increasingly found re-employment at establishments that paid their employees lower wages, all else equal. A smaller portion of the post-reform wage decline –

[5]In other settings, Card et al. (2007), Lalive (2007), and van Ours and Vodopivec (2008) find no relationship between the duration of unemployment benefits and re-employment wages. Schmieder et al. (2016) find a negative effect of UI generosity on wages, whereas Nekoei and Weber (2017) find a positive effect.

[6]Krause and Uhlig (2012), Krebs and Scheffel (2013), and Launov and Waelde (2013) find that the UI reforms reduced unemployment; Fahr and Sunde (2009), Klinger and Rothe (2012) and Hertweck and Sigrist (2012) find that the reforms improved matching efficiency; and Dlugosz et al. (2013) find that they reduced transitions between employment and unemployment.

[7]These estimates apply to in an individual's primary job, and do not account for the possibility that the reforms may have affected the uptake of secondary jobs.

between five and nine percent for men, and between 12 and 23.5 percent for women – is because displaced workers entered into worse matches with employers after the reforms. Changes in the distribution of the unobserved characteristics of workers selected into displacement and re-employment accounted for none of the female wage decline, and slightly increased wages of displaced men. That is, men who were displaced after the Hartz reforms and eventually returned to work had unobserved characteristics that earned higher labor market returns than men displaced prior to the reforms, and this slightly increased their wages. Collectively, these three channels explain almost all of the decline in displaced workers' wages after the Hartz reforms. 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 finding that sorting into employment at low-wage establishments is an important determinant of post-displacement wages in Germany aligns with Schmieder et al. (2019) and Fackler et al. (2021). To better understand this phenomenon and why it strengthened after the reforms, we characterize the low-wage establishments that employ recently displaced workers. In so doing, we document a dramatic increase in post-displacement employment in the temporary employment sector after the reforms, and present evidence that this was an important contributor to recently displaced workers' wage declines. 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 in the four quarters after displacement. These are very low wage jobs: the average employer in the temporary employment sector has a 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 temporary employment and established an infrastructure for placing unemployed workers into temporary work via newly-legislated "Staff Service Agencies" (*Personal-Service-Agentur*, or PSAs).

Relative to most of the displacement literature, our decomposition focuses on a slightly different estimand and definition of job displacement. Most studies focus on a treatment group of displaced workers who are highly attached to the labor market and to their pre-displacement employer, and who are displaced from employment during a mass layoff. Those studies estimate wage losses by comparing the wage changes of displaced workers before and after a single displacement event to an otherwise similar control group of non-displaced workers, controlling for observable characteristics and individual fixed effects. Most studies focus on mass layoff events because the data do not identify the reason for separations; individuals who separate during a mass layoff are likely to have been involuntarily displaced, rather than being quits or dismissals for cause (Jacobson, Lalonde, and Sullivan, 1993; JLS hereafter). In contrast, we exploit features of the German data and UI benefit eligibility rules to identify involuntarily displaced workers from the timing of UI benefit receipt. As a consequence, our sample of displaced workers encompasses a broader set of those who lose their jobs involuntarily than most studies, including those who are displaced outside of a mass layoff, those who experience multiple displacements, and those who return to their pre-displacement employer after a spell of UI benefit receipt. Our estimates apply to this more broadly defined population and definition of job displacement. We also rely on our match effects specification to control for unobserved individual, employer, and match heterogeneity in wages and to ensure matched comparisons between displaced and non-displaced workers,[8] instead of propensity score matching (Schmieder et al., 2019; Fackler et al., 2021) or imposing strong restrictions on the employment and benefit histories of treatment and control group members (Lachowska et al., 2020). Furthermore, our matching effect is identified from within-match wage changes around a displacement event, which departs from the within-person variation that identifies most wage loss estimates in the literature. Because of these differences, we undertake robustness checks based on alternate specifications and definitions of displacement, and report event study estimates similar to those estimated by others, to ensure comparability between our estimates and other studies.

2 Accounting for Selection, Sorting, and Matching

To begin, consider a basic specification for wages:

$$y_{it} = \mathbf{x}_{it}'\beta_0 + \mathbf{d_{it}}'\delta_0 + \tau_{0,t} + \epsilon_{0,it} \tag{1}$$

[8]This specification explains roughly 90 percent of observed wage variation in our data, so we are confident that it controls for essential sources of wage heterogeneity.

where $i = 1, ..., N$ indexes individuals, $t = 1, ..., T$ indexes time (quarters, in our application), y_{it} is the logarithm of i's wage in t, $\mathbf{x}_{it}$ is a vector of observable characteristics with returns β_0, $\mathbf{d}_{it}$ is a vector of displacement indicators with coefficients δ_0, $\tau_{0,t}$ is a fixed time effect, and $\epsilon_{0,it}$ is statistical error. We have in mind that δ_0 is the object of primary interest in eq. (1), and we refer to δ_0 as the *gross wage effect* of displacement. The precise interpretation of δ_0 will depend on how the vector $\mathbf{d}_{it}$ is specified, and different specifications of $\mathbf{d}_{it}$ will be of interest in different applications. In the simplest case, $\mathbf{d}_{it}$ is a binary indicator that equals one if i has recently been displaced from employment, in which case δ_0 will measure the difference between the average wages of recently displaced workers and all others, conditional on $\mathbf{x}_{it}$. A richer specification that is more in keeping with the displacement literature follows JLS and defines:

$$\mathbf{d_{it}}'\delta_0 = \sum_{k=-b}^{a} \delta_{0,k} d_{k,it} \tag{2}$$

where each $d_{k,it}$ is a binary indicator that equals one if the worker is observed in period k relative to displacement and zero otherwise ($k = 0$ is the period of displacement, and indicators are defined between b periods before and a periods after displacement). In this case, the $\delta_{0,k}$ measure differences between wages of displaced and non-displaced workers before displacement ($k < 0$), in the period of displacement ($k = 0$), and after displacement ($k > 0$), conditional on $\mathbf{x}_{it}$.

The prior literature has established that individuals experience substantial wage losses when they are displaced from employment. This corresponds to post-displacement elements of δ_0 being negative. We posit three possible mechanisms for this. As noted, one is selection: if displaced individuals have unobserved characteristics that earn lower returns in the labor market than non-displaced workers, then all else equal $\delta_0 < 0$. The second mechanism is sorting: if displaced workers systematically work at establishments that pay lower AKM-style establishment wage premia than non-displaced workers, then post-displacement elements of $\delta_0 < 0$ also. The third mechanism is matching: if displaced workers enter into systematically lower quality matches (i.e., matches that pay a lower match-specific wage premium in the sense of Woodcock (2008, 2015)), then we would also find post-displacement elements of $\delta_0 < 0$ in eq. (1).

To quantify the magnitude of these potential determinants of the gross wage effect of displacement, we begin by defining $\mathbf{z}_{it}'\eta_0 = \mathbf{x}_{it}'\beta_0 + \tau_{0,t}$ so that we can we rewrite our baseline specification more compactly as:

$$y_{it} = \mathbf{z}_{it}'\eta_0 + \mathbf{d_{it}}'\delta_0 + \epsilon_{0,it}. \tag{3}$$

Let θ_i denote unobserved characteristics of individual i; 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 $\theta_i, \psi_{J(i,t)}$, and $\phi_{iJ(i,t)}$ are all time-invariant. In the interest of making minimal assumptions about the relationship between unobservables, let $\Phi\left(\theta_i, \psi_{J(i,t)}, \phi_{iJ(i,t)}\right)$ denote their combined effect on wages. For the moment, we only assume that $\Phi(\cdot)$ is additively separable from the observable determinants of wages and ϵ_{it}. With these assumptions, we have the following expression for wages:

$$y_{it} = \mathbf{z}_{it}^{'}\eta + \mathbf{d_{it}}'\delta + \Phi\left(\theta_i, \psi_{J(i,t)}, \phi_{iJ(i,t)}\right) + \epsilon_{it} \tag{4}$$

where we assume that the error satisfies $E\left[\epsilon_{it}|\mathbf{z}_{it}, \mathbf{d}_{it}, \Phi(\cdot)\right] = 0$.[9]

If the unobserved characteristics of displaced workers, the establishments at which they work, or the matches that they enter into differ from those of non-displaced workers, then $\delta \neq \delta_0$. In fact the difference between δ_0 and δ estimates the net effect of displacement on the distribution of $\Phi\left(\theta_i, \psi_{J(i,t)}, \phi_{iJ(i,t)}\right)$ in wages. This is the central insight of the Pendakur and Woodcock (2010) "glass door effect"[10] and the Gelbach (2016) decomposition.[11] 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 unobservables is fixed for the duration of an employment spell. Consequently, we can replace the function $\Phi\left(\theta_i, \psi_{J(i,t)}, \phi_{iJ(i,t)}\right)$ with a fixed effect for the match between worker i and establishment $J(i,t)$:

$$y_{it} = \mathbf{z}_{it}'\eta + \mathbf{d}_{it}'\delta + \Phi_{iJ(i,t)} + \epsilon_{it}. \tag{5}$$

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

[10]Pendakur and Woodcock (2010) do not consider the case of displacement, and $\Phi(\cdot) = \psi_{J(i,t)}$ in their application. They define the glass door effect in the context of the immigrant-native wage gap, where $\mathbf{d}_{it}$ is an indicator for immigrant status. In this case δ_0 measures the economy-wide wage gap between immigrants and natives and δ measures the within-firm gap. They show that $\delta_0 - \delta$ estimates a regression-adjusted difference between the average firm wage premium of immigrants and natives, which summarizes how the sorting of immigrants and natives across higher- and lower-wage firms contributes to the overall wage gap between them.

[11]Gelbach's (2016) motivating example is the black-white wage gap, so that $\mathbf{d}_{it}$ is an indicator for race, and $\Phi(\cdot) = \mathbf{w}_{it}'\mathbf{\Gamma}$ is a vector of observable characteristics whose distribution might differ between black and white workers. In that context, Gelbach (2016) shows that $\delta_0 - \delta$ estimates the contribution of differences between the observable characteristics of black and white workers to the overall black-white wage gap.

In eq. (5), δ is identified from within-match wage changes around displacement events. The data required to identify δ will depend on the precise definition of $\mathbf{d}_{it}$, but observing pre- and post-displacement wages at the same employer, which is only possible for displaced individuals who eventually return to work at the establishment that displaced them, will be helpful. In our application, for example, we estimate specifications where $\mathbf{d}_{it}$ is an indicator that equals one in the four quarters following displacement and zero otherwise. In this case, δ is identified from the difference between pre- and post-displacement wages in the subset of individuals that return to work at the displacing establishment, and from the within-match difference in wages between the four quarters after displacement and later quarters. For a JLS-style definition of $\mathbf{d}_{it}$, the data requirement will be somewhat stronger since there are more coefficients to identify, but the same intuition applies: the level difference between pre- and post-displacement elements of δ will be identified from the difference between pre- and post-displacement wages among individuals that return to work at their pre-displacement establishment. In Section 3, we revisit identification of δ for the precise specifications of $\mathbf{d}_{it}$ that we consider in our application.

The match 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 displacement indicators $\mathbf{d}_{it}$. If we could observe $\Phi_{iJ(i,t)}$ directly, we could estimate how differences between the distribution of these unobserved characteristics among displaced and non-displaced workers contribute to the overall wage difference between them via the regression:

$$E\left[\Phi_{iJ(i,t)}|\mathbf{z}_{it}, \mathbf{d}_{it}\right] = \mathbf{z}_{it}'\eta_\Phi + \mathbf{d}_{it}'\delta_\Phi \tag{6}$$

where δ_Φ is the regression-adjusted difference between the average unobserved worker, establishment, and match characteristics of displaced and non-displaced workers. Although it is infeasible to estimate eq. (6) directly because $\Phi_{iJ(i,t)}$ is unobserved, the following proposition shows that the difference between OLS estimates of δ_0 and δ is an unbiased estimator of δ_Φ.

Proposition 1. *Let $\hat{\delta}_0$ and $\hat{\delta}$ denote the OLS estimators of δ_0 and δ in eqs. (3) and (5), respectively, and assume that ϵ_{it} in eq. (5) has zero conditional mean. Then $\left(\hat{\delta}_0 - \hat{\delta}\right)$ is an unbiased estimator of δ_Φ in the infeasible regression (6).*

Proof. Rewriting eqs. (3) and (5) in matrix notation, we have:

$$\mathbf{y} = \mathbf{Z}\eta_0 + \mathbf{D}\delta_0 + \epsilon_0 \tag{7}$$

$$\mathbf{y} = \mathbf{Z}\eta + \mathbf{D}\delta + \mathbf{G}\mathbf{\Phi} + \epsilon \tag{8}$$

where $\mathbf{y}$ is the $N^* \times 1$ vector of wage observations, $\mathbf{Z}$ is the $N^* \times k$ matrix of observables with rows $\mathbf{z}'_{it}$, $\mathbf{D}$ is the $N^* \times l$ matrix of displacement indicators with rows $\mathbf{d}'_{it}$, 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 errors, N^* is the number of observations, and M is the number of worker-employer matches. The OLS estimator of δ in eq. (8) is $\hat{\delta} = \left(\mathbf{D}'\mathbf{M}_{[\mathbf{Z}\ \mathbf{G}]}\mathbf{D}\right)^{-1}\mathbf{D}'\mathbf{M}_{[\mathbf{Z}\ \mathbf{G}]}\mathbf{y}$ where $\mathbf{M_A} \equiv \mathbf{I} - \mathbf{A}\left(\mathbf{A}'\mathbf{A}\right)^{\mathbf{-1}}\mathbf{A}'$. Given $E\left[\epsilon|\mathbf{Z},\mathbf{D},\mathbf{G}\right] = 0$, we have $E\left[\hat{\delta}|\mathbf{Z},\mathbf{D},\mathbf{G}\right] = \delta$. Premultiplying both sides of eq. (8) by $\left(\mathbf{D}'\mathbf{M_Z}\mathbf{D}\right)^{-1}\mathbf{D}'\mathbf{M_Z}$, we obtain

$$\left(\mathbf{D}'\mathbf{M_Z}\mathbf{D}\right)^{-1}\mathbf{D}'\mathbf{M_Z}\mathbf{y} = \delta + \left(\mathbf{D}'\mathbf{M_Z}\mathbf{D}\right)^{-1}\mathbf{D}'\mathbf{M_Z}\mathbf{G}\mathbf{\Phi} + \left(\mathbf{D}'\mathbf{M_Z}\mathbf{D}\right)^{\mathbf{-1}}\mathbf{D}'\mathbf{M_Z}\epsilon \tag{9}$$

because $\mathbf{M_Z}\mathbf{Z} = \mathbf{0}$. The left side of eq. (9) is the OLS estimator of δ_0 in eq. (7), $\hat{\delta}_0$. Consequently,

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

which is the OLS estimator of δ_Φ in eq. (6). □

The intuition underlying Proposition 1 is straightforward. Because eq. (3) does not control for unobserved individual, establishment, or match heterogeneity, $\hat{\delta}_0$ estimates a *gross* wage difference between displaced and non-displaced workers that includes differences in the distribution of these unobserved characteristics between the two groups. In contrast, eq. (5) controls for all three dimensions of unobserved heterogeneity via $\Phi_{iJ(i,t)}$. Consequently, $\hat{\delta}$ estimates the *net effect* of displacement on wages, i.e., the residual effect of displacement when we hold unobservables constant. The difference $\hat{\delta}_0 - \hat{\delta}$ isolates the effect of displacement on wages that operates through the selection, sorting, and matching channels. At the population level, this is measured by δ_Φ.

In the finite sample, we have the exact result $\hat{\delta}_0 - \hat{\delta} = \left(\mathbf{D}'\mathbf{M_Z}\mathbf{D}\right)^{-1}\mathbf{D}'\mathbf{M_Z}\mathbf{G}\hat{\mathbf{\Phi}}$ where $\hat{\mathbf{\Phi}}$ is

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

the OLS estimator of $\mathbf{\Phi}$.[13] This gives another way to estimate δ_Φ: via an auxiliary regression of $\hat{\Phi}_{iJ(i,t)}$ on $\mathbf{z}_{it}$ and $\mathbf{d}_{it}$. While the circumstances in which this would be preferred to simply taking the difference $\hat{\delta}_0 - \hat{\delta}$ are limited, this result is helpful for operationalizing the decompositions that we develop below. Asymptotically, plim$\hat{\delta} = \delta$ and plim$\hat{\delta}_0 = \delta_0$ under standard OLS regularity conditions, so that plim $\left(\hat{\delta}_0 - \hat{\delta}\right) = \delta_\Phi$. This consistency result holds even though each of the estimated match effects $\hat{\Phi}_{iJ(i,t)}$ is inconsistent (but unbiased) in a fixed-length panel.

Inference about δ_Φ is straightforward via Hausman-type tests about $\hat{\delta}_0 - \hat{\delta}$, as established in Proposition 2. It is worth noting that failing to reject $H_0 : \delta_\Phi = 0$ does not imply the absence of unobserved heterogeneity in wages, nor does it imply that match effects $\Phi_{iJ(i,t)}$ do not belong in the model. It is simply evidence that the difference between the average wages of displaced and non-displaced workers is not determined by differences in the distribution of their unobserved individual, establishment, and match characteristics.

Proposition 2. *Under the null hypothesis* $H_0 : \delta_\Phi = 0$, $Var\left[\hat{\delta}_0 - \hat{\delta}|\mathbf{Z},\mathbf{D}\right] = Var\left[\hat{\delta}|\mathbf{Z},\mathbf{D}\right] - Var\left[\hat{\delta}_0|\mathbf{Z},\mathbf{D}\right]$ *so that* $Q = \left(\hat{\delta}_0 - \hat{\delta}\right)' \left(\hat{Var}\left[\hat{\delta}\right] - \hat{Var}\left[\hat{\delta}_0\right]\right)^{-1} \left(\hat{\delta}_0 - \hat{\delta}\right) \overset{a}{\sim} \chi^2_l$, *where* $\hat{Var}\left[\hat{\delta}_0\right]$ *and* $\hat{Var}\left[\hat{\delta}\right]$ *are consistent estimates of* $Var\left[\hat{\delta}_0|\mathbf{Z},\mathbf{D}\right]$ *and* $Var\left[\hat{\delta}|\mathbf{Z},\mathbf{D}\right]$, *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 combined effect of displacement on wages via the selection, sorting, and matching channels admits many possible relationships between unobservables. This includes nonlinear and non-separable specifications, 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 displacement on the distribution of these characteristics, $\hat{\delta}_0 - \hat{\delta}$, is invariant to the functional form of $\Phi(\cdot)$. That said, it is clearly useful to decompose the combined effect into selection, sorting, and matching components so that we can understand the mechanisms through which displacement reduces wages. Any such decomposition requires additional assumptions about the functional form of $\Phi(\cdot)$. We now develop two alternative decompositions that rely on different assumptions about the relationships between unobservables.

[13] Letting $\hat{\eta}$ denote the OLS estimator of η and $\mathbf{e}$ denote the OLS residual, we can rewrite eq. (8) as $\mathbf{y} = \mathbf{Z}\hat{\eta} + \mathbf{D}\hat{\delta} + \mathbf{G}\hat{\mathbf{\Phi}} + \mathbf{e}$. Premultiplying both sides by $\left(\mathbf{D}'\mathbf{M_Z}\mathbf{D}\right)^{-1}\mathbf{D}'\mathbf{M_Z}$, we obtain $\hat{\delta}_0 = \hat{\delta} + \left(\mathbf{D}'\mathbf{M_Z}\mathbf{D}\right)^{-1}\mathbf{D}'\mathbf{M_Z}\mathbf{G}\hat{\mathbf{\Phi}}$ because $\mathbf{M_Z}\mathbf{Z} = \mathbf{0}$ and $\mathbf{e}$ is orthogonal to $\mathbf{D}$ and $\mathbf{Z}$.

2.1 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 is now:

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

where θ is an $N \times 1$ vector of individual effects θ_i, ψ is a $J \times 1$ vector of establishment effects $\psi_{J(i,t)}$, ϕ is an $M \times 1$ vector of match effects $\phi_{iJ(i,t)}$, $\mathbf{P}$ 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, and J is the number of establishments. 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 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 specifications (which omit the match effect from eq. (10)), $\psi_{J(i,t)}$ is usually characterized as an establishment wage premium. The match effects $\phi_{iJ(i,t)}$ measure persistent differences in average wages between worker-employer matches, which we loosely characterize as match quality, conditional on observable characteristics and workers' and establishments' time-invariant unobserved characteristics.

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

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

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

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

[14] $\mathbf{P} = [\mathbf{p}_1, \mathbf{p}_2, \ldots, \mathbf{p}_N]$ where the i^{th} column $\mathbf{p}_i$ 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 establishment j.

[15] Eqs. (8) and (10) provide the same estimate of δ because $\mathbf{P}$ 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 $\mathbf{P}$; if we sum the columns of $\mathbf{G}$ for each employer, we obtain $\mathbf{F}$.

which is a regression-adjusted estimate of the difference between the average value of displaced and non-displaced workers' unobserved characteristics θ_i. Likewise, we call $(\mathbf{D}'\mathbf{M_Z}\mathbf{D})^{-1}\mathbf{D}'\mathbf{M_Z}\mathbf{F}\psi$ the Decomposition 1 sorting effect. It is the OLS estimator of δ_ψ in the regression:

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

which is a regression-adjusted estimate of the difference between the average value of the wage premia $\psi_{J(i,t)}$ of the establishments that employ displaced workers, and those that employ others. Finally, we call $(\mathbf{D}'\mathbf{M_Z}\mathbf{D})^{-1}\mathbf{D}'\mathbf{M_Z}\mathbf{G}\phi$ the Decomposition 1 matching effect. It is the OLS estimator of δ_ϕ in the regression:

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

which is a regression-adjusted estimate of the difference between the average value of unobserved match characteristics $\phi_{iJ(i,t)}$ of displaced and non-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. However, $\hat{\delta}_0-\hat{\delta}=(\mathbf{D}'\mathbf{M_Z}\mathbf{D})^{-1}\mathbf{D}'\mathbf{M_Z}\mathbf{P}\hat{\theta}+(\mathbf{D}'\mathbf{M_Z}\mathbf{D})^{-1}\mathbf{D}'\mathbf{M_Z}\mathbf{F}\hat{\psi}+(\mathbf{D}'\mathbf{M_Z}\mathbf{D})^{-1}\mathbf{D}'\mathbf{M_Z}\mathbf{G}\hat{\phi}$ in the finite sample, where $\hat{\theta},\hat{\psi},\hat{\phi}$ are OLS estimates. So we can operationalize the decomposition with OLS estimates in sample counterparts of eqs. (11)-(13). However, this requires additional identifying assumptions, because eq. (10) is overparameterized.[16] In our application, we estimate eq. (10) using the orthogonal match effects estimator developed in Woodcock (2008, 2015), which defines $\phi_{iJ(i,t)}$ to be orthogonal to 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 $\mathbf{d}_{it}$; see Woodcock (2015) for a detailed discussion.

We also make the standard identifying assumption:

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

[16] 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.

(orthogonality would suffice). This requires that employment mobility is conditionally exogenous: it can depend on observable characteristics and time-invariant 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}.

To summarize, Decomposition 1 can be implemented as follows. First, estimate eqs. (3) and (5) to obtain OLS estimates $\hat{\delta}_0$, $\hat{\delta}$, and $\hat{\Phi}_{iJ(i,t)}$. Second, 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 and normalize the fixed effects to have zero sample mean. Finally, estimate sample counterparts of (11)-(13). The resulting 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 matrix calculations that are cumbersome when there are many workers, establishments, and matches, so we base inferences on bootstrap standard errors clustered at the individual level in our application.

2.2 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 to define the selection and sorting effects. That specification is:

$$y_{it} = \mathbf{z}_{it}'\eta_2 + \mathbf{d}_{it}'\delta_2 + \theta_{2,i} + \psi_{2,J(i,t)} + \epsilon_{2,it} \tag{15}$$

or in matrix notation,

$$\mathbf{y} = \mathbf{Z}\eta_2 + \mathbf{D}\delta_2 + \mathbf{P}\theta_2 + \mathbf{F}\psi_2 + \epsilon_2. \tag{16}$$

Estimates based on eq. (15) will differ, in general, from those in eqs. (3) and (10) because the three specifications identify δ and other model parameters using different variation and under slightly different identifying assumptions. In particular, eq. (15) identifies δ_2 from both within-worker and within-employer wage variation around displacement events. The most natural characterization is that δ_2 is identified using within-person wage changes around the displacement event, holding $\mathbf{z}_{it}$ and the unobserved characteristics of the pre- and post-displacement employers constant. Because δ_2 is identified more squarely from within-worker wage variation around a displacement event, instead of within-match variation, it is more closely aligned with traditional estimands of post-displacement wage losses based on JLS-style event studies than our within-match estimator δ is.[17] Specifically, while Decomposition 2 relies on within-worker and within-firm variation to identify the selection and sorting effects, it only relies on within-match variation to identify the matching effect. This may be a reason to prefer Decomposition 2 over Decomposition 1 in some applications.

Eq. (15) further requires:

$$E\left[\epsilon_2|\mathbf{Z},\mathbf{D},\mathbf{P},\mathbf{F}\right]=\mathbf{0} \tag{17}$$

(again, orthogonality would suffice). As above, this can be interpreted as an exogenous mobility assumption: the AKM specification admits employment mobility that depends on observable characteristics and time-invariant characteristics of workers and establishments as captured by $\theta_{2,i}$ and $\psi_{2,J(i,t)}$, but not $\epsilon_{2,it}$. This is stronger than eq. (14) because mobility that depends on unobserved match heterogeneity violates eq. (17).[18] This may be one reason to prefer Decomposition 1 over Decomposition 2. That said, Card et al. (2013) estimate an AKM specification on the IEB data (of which the LIAB used in our application are a subset) and find considerable support for eq. (17); other authors have reached the same conclusion in various data.[19]

Letting $\hat{\delta}_2$ denote the least squares estimator of δ_2,

$$E\left[\hat{\delta}_0-\hat{\delta}_2|\mathbf{Z},\mathbf{D},\mathbf{P},\mathbf{F}\right]=\left(\mathbf{D}'\mathbf{M_Z}\mathbf{D}\right)^{-1}\mathbf{D}'\mathbf{M_Z}\mathbf{P}\theta_2+\left(\mathbf{D}'\mathbf{M_Z}\mathbf{D}\right)^{-1}\mathbf{D}'\mathbf{M_Z}\mathbf{F}\psi_2. \tag{18}$$

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, $\theta_{2,i}$ and $\psi_{2,J(i,t)}$ are based on eq. (15) which does not. We expect the two decompositions to yield similar estimates of the selection and sorting effects, since both assume that the wage returns to individual and establishment unobserved heterogeneity are additively separable from each other and from observable determinants of wages.

[17]See the discussion in Section 2.3.

[18]See Woodcock (2015) for an extended discussion of exogenous mobility and match effects.

[19]Lachowska et al. (2020) in Washington State data, Moore and Scott-Clayton (2019) in Ohio data, Dauth et al. (2021) in the IEB data, among others.

The extent to which they differ will depend on two things: the relative importance of match-specific unobserved heterogeneity in wages; and whether within-match wage changes around displacement differ meaningfully from within-person wage changes that hold the unobserved characteristics of pre- and post-displacement employers constant.

Our definition of the Decomposition 2 matching effect relies on eq. (5), which does not impose any structure on the relationship between unobserved worker, establishment, and match heterogeneity. Proposition 3 summarizes the key result.

Proposition 3. *Let $\hat{\delta}$ and $\hat{\delta}_2$ denote the OLS estimators of δ and δ_2 in eqs. (8) and (16), respectively, and assume that $E\left[\epsilon|\mathbf{Z},\mathbf{D},\mathbf{G}\right]=\mathbf{0}$. Then $\left(\hat{\delta}_2-\hat{\delta}\right)$ is an unbiased estimator of $\delta_{2,\Phi}$ in the infeasible regression:*

$$E\left[\Phi_{iJ(i,t)}|\mathbf{z}_{it},\mathbf{d}_{it},\mathbf{p}_i,\mathbf{f}_i\right]=\mathbf{z}'_{it}\eta_{2,\Phi}+\mathbf{d}'_{it}\delta_{2,\Phi}+\theta_{2,\Phi,i}+\psi_{2,\Phi,J(i,t)} \tag{19}$$

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

Proof. Recalling $\hat{\delta}=\left(\mathbf{D}'\mathbf{M}_{[\mathbf{Z}\ \mathbf{G}]}\mathbf{D}\right)^{-1}\mathbf{D}'\mathbf{M}_{[\mathbf{Z}\ \mathbf{G}]}\mathbf{y}$, premultiplying both sides of eq. (8) by $\left(\mathbf{D}'\mathbf{M}_{[\mathbf{Z}\ \mathbf{P}\ \mathbf{F}]}\mathbf{D}\right)^{-1}\mathbf{D}'\mathbf{M}_{[\mathbf{Z}\ \mathbf{P}\ \mathbf{F}]}$ we obtain:

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

because $\left(\mathbf{D}'\mathbf{M}_{[\mathbf{Z}\ \mathbf{P}\ \mathbf{F}]}\mathbf{D}\right)^{-1}\mathbf{D}'\mathbf{M}_{[\mathbf{Z}\ \mathbf{P}\ \mathbf{F}]}\mathbf{Z}=\mathbf{0}$. The left-hand side of eq. (20) is the OLS estimator of δ_2 in eq. (16), $\hat{\delta}_2$. Since $E\left[\epsilon|\mathbf{Z},\mathbf{D},\mathbf{G}\right]=0$ implies $E\left[\hat{\delta}|\mathbf{Z},\mathbf{D},\mathbf{G}\right]=\delta$, we have:

$$E\left[\hat{\delta}_2-\hat{\delta}|\mathbf{Z},\mathbf{D},\mathbf{G}\right]=\left(\mathbf{D}'\mathbf{M}_{[\mathbf{Z}\ \mathbf{P}\ \mathbf{F}]}\mathbf{D}\right)^{-1}\mathbf{D}'\mathbf{M}_{[\mathbf{Z}\ \mathbf{P}\ \mathbf{F}]}\mathbf{G}\boldsymbol{\Phi}. \tag{21}$$

The right-hand side of (21) is the OLS estimator of $\delta_{2,\Phi}$ in eq. (19). □

We call $\delta_{2,\Phi}=\left(\mathbf{D}'\mathbf{M}_{[\mathbf{Z}\ \mathbf{P}\ \mathbf{F}]}\mathbf{D}\right)^{-1}\mathbf{D}'\mathbf{M}_{[\mathbf{Z}\ \mathbf{P}\ \mathbf{F}]}\mathbf{G}\boldsymbol{\Phi}$ the Decomposition 2 matching effect.[20] The infeasible regression on which $\delta_{2,\Phi}$ is based, eq. (19), is similar to eq. (6) except that it holds

[20] Figueiredo et al. (2014) propose a similar measure to estimate whether industrial clusters improve worker-firm matching. Their estimator has the same mathematical structure, but in their application $\mathbf{D}$ is a measure of industrial agglomeration and $\delta_{2,\Phi}$ measures the contribution of match quality to the agglomeration wage premium.

individual- and establishment-specific unobservables constant. The Decomposition 2 matching effect is thus the regression-adjusted difference between the average unobserved worker, establishment, and match characteristics of displaced and non-displaced workers, 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 additively-separable components of worker and establishment heterogeneity to wage losses, and the matching effect captures the contribution of any remaining match-specific heterogeneity (e.g., interactions between worker and establishment effects, nonlinearities, separable match effects, etc.). This is a broader definition of match 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 selection, sorting, and matching on wages, i.e., the estimated selection, sorting, and matching effects sum to $\hat{\delta}_0 - \hat{\delta}$ in both decompositions.[21]

Proposition 3 assumes $E[\epsilon|\mathbf{Z}, \mathbf{D}, \mathbf{G}] = \mathbf{0}$. This is equivalent to eq. (14) since $\mathbf{P}$ 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.

Decomposition 2 is implemented as follows. First, estimate eq. (16) to obtain OLS estimates $\hat{\delta}_2$, $\hat{\theta}_{2,i}$, and $\hat{\psi}_{2,J(i,t)}$, with the latter two effects normalized to have zero mean in the sample; and estimate (5) to obtain the OLS estimate $\hat{\delta}$. Then estimate the Decomposition 2 selection and sorting effects from sample counterparts of (11) and (12) and estimate the Decomposition 2 matching effect $\delta_{2,\Phi}$ directly from $\hat{\delta}_2 - \hat{\delta}$. The estimated Decomposition 2 selection and sorting effects are unbiased and consistent under (17), and the estimated matching effect is unbiased and consistent under $E[\epsilon|\mathbf{Z}, \mathbf{D}, \mathbf{G}] = \mathbf{0}$. In our application, we base inferences about all three components on bootstrap standard errors clustered at the individual level.

2.3 Comparison to Alternative Approaches

Several recent studies (Fackler et al. (2021); Lachowska et al. (2020); Moore and Scott-Clayton (2019); Schmieder et al. (2019)) estimate the contribution of employers to wage losses at displacement. While there are methodological differences between those papers, they all quantify employers'

[21] This may not be immediately apparent in the case of Decomposition 2, but note that $\left(\hat{\delta}_0 - \hat{\delta}_2\right) + \left(\hat{\delta}_2 - \hat{\delta}\right) = \hat{\delta}_0 - \hat{\delta}$.

contribution to wages via an AKM model:

$$y_{it} = \mathbf{z}_{it}'\eta^* + \theta_i^* + \psi_{J(i,t)}^* + \epsilon_{it}^* \tag{22}$$

that is estimated on a large sample of observations representative of the broad labor market; and estimate the wage effects of displacement from JLS-style event study regressions:

$$y_{it} = \mathbf{z}_{it}'\eta^{**} + \alpha_i^{**} + \sum_{k=-b}^{a} \delta_k^{**} d_{k,it} + \epsilon_{it}^{**} \tag{23}$$

$$\hat{\psi}_{J(i,t)}^* = \mathbf{z}_{it}'\eta_\psi^{**} + \alpha_{\psi,i}^{**} + \sum_{k=-b}^{a} \delta_{\psi,k}^{**} d_{k,it} + \epsilon_{\psi,it}^{**} \tag{24}$$

that are estimated on a narrowly-defined treatment group of displaced workers and a carefully selected control group.[22] In those papers, the vector δ^{**} (with elements δ_k^{**}) is the total wage loss from displacement that the authors seek to explain, and the vector δ_ψ^{**} (with elements $\delta_{\psi,k}^{**}$) estimates the importance of employer wage premiums in explaining that wage loss, so that $\delta^{**} - \delta_\psi^{**}$ is the net (or direct) wage loss from displacement.

There are some notable differences between that approach and our decomposition. First, because those authors estimate employer wage premia from eq. (22) which omits the wage effects of displacement ($\mathbf{D}\delta$) and match effects, their estimates of the employer contribution to wages losses at displacement, $\hat{\delta}_\psi^{**}$, and of the "net" effect of displacement, $\hat{\delta}^{**} - \hat{\delta}_\psi^{**}$, may be biased.[23] We derive the bias in Appendix B. That derivation shows that the bias will be non-zero if displaced and non-displaced workers sort systematically into different quality matches or into different employers. Our application provides evidence that they do. Other authors address this bias by excluding some or

[22] Fackler et al. (2021) define a treatment group of workers with at least three years of job tenure who separate from employment within one year of their employer declaring bankruptcy, and a control group by randomly selecting non-displaced workers with at least 3 years ob tenure. Schmieder et al. (2019) define a treatment group of workers with at least 3 years job tenure, who separate within one year of a mass-displacement event, and who are not employed at the displacing employer during the subsequent 10 years; their control group is constructed via propensity score matching. The Lachowska et al. (2020) treatment group consists of workers with at least 6 years of consecutive employment at their primary employer, who separate from that employer within four quarters of a mass-displacement event, and who have at least one quarter of positive earnings in each of the next five years. Their control group consists of workers who satisfy the same tenure requirement and remain continuously employed at the primary employer through the end of the sample, but collect UI at least once during a 13 year period. The Moore and Scott-Clayton (2019) treatment group consists of workers with at least 3 years of job tenure, who separate within one year of a mass layoff event, do not return to employment at that firm during the sample period, and have positive earnings in at least 25% of post-displacement quarters; their control group consists of continuously employed individuals.

[23] In some instances, those authors also include a smaller set of controls in their AKM specification than in their JLS-style regressions, which may introduce additional bias that we ignore here.

all observations on displaced individuals from their AKM estimation sample. However such sample exclusions may introduce additional selection biases into estimated AKM wage components, and they are unlikely to completely eliminate the bias derived in Appendix B because they are based on narrow definitions of displacement that comprise only a subset of workers that lose their jobs and experience wage losses. Indeed, Moore and Scott-Clayton (2019) report that their estimates are quite sensitive to such sample exclusion restrictions.

Second, most of these papers do not account for match quality in wages and hence do not identify a matching effect. Lachowska et al. (2020) is the exception, and they find that matching explains most of the wage loss at displacement. They estimate match effects from an equation similar to (22) but including a fixed match effect, and estimate the contribution of matching to wage losses from a JLS-style event study with the estimated match effect as dependent variable. However, because the equation that they use to estimate match effects omits $\mathbf{D}\delta$, their estimate of the matching effect may be subject to bias similar to that described above, as we show in Appendix B.

Third, the specification defined by eqs. (1) and (2) mirrors eq. (23), except that the latter includes an individual fixed effect α_i^{**}. As a consequence, δ^{**} is identified from within-person wage changes around displacement. In contrast, eq. (1) identifies δ_0 from both within- and between-individual differences in wages around displacement. We exclude individual fixed effects from eq. (1), precisely so that we can identify selection effects in the wage loss from displacement, which eq. (23) does not. This is important in applications like ours in which a policy reform might change which workers are selected into displacement or re-employment following displacement. Similarly, eq. (5) identifies δ from within-match wage changes around displacement events. Thus, in most applications δ will be at least partly identified from temporary displacements where workers return to their pre-displacement employer. In contrast, estimates based on JLS-style specifications like eq. (23) usually exclude temporary displacements. Since the wage losses from temporary and permanent displacements may differ, this may contribute to differences in the concept of displacement underlying δ and δ^{**}. This is less of a concern for our Decomposition 2, which does not rely on within-match wage variation to identify sorting and selection effects.

Finally, because eq. (24) includes an individual fixed effect $\alpha_{\psi,i}^{**}$, the estimated employer contribution to post-displacement wage losses, δ_{ψ}^{**}, is net of individual heterogeneity in the average change in employer wage premia following displacement. This is in contrast to the sorting effects that we define above, which include person-average changes in employer wage premia.[24] This distinction is generally ignored, and is not identified, in applications that estimate specifications like (24) on a treatment group of individuals who experience a single displacement and a never-displaced control group. However the distinction matters in applications like ours, and more generally to understand the wage effects of displacement in the broad population of workers, since individuals may experience multiple displacements and there may be differences between individuals in average wage losses at displacement.

3 Application: The Hartz Reforms

We apply our decomposition approach to understand how the determinants of displaced workers' wages changed after the introduction of the Hartz reforms. These reforms were introduced in Germany in several phases between 2003 and 2005, and were broadly targeted at making the labor market more flexible and reducing long-term unemployment. The first phases (Hartz I-III, introduced in 2003 and 2004) included initiatives to deregulate temporary work, dismissal, and fixed-term contracts; new measures to restructure and increase the effectiveness of local employment agencies; introduced new subsidies for entrepreneurs and vocational training; defined "mini jobs" that are exempt from most social security taxes; and created provisions to reduce unemployment benefits if individuals refused reasonable job offers. The centerpiece of the reforms, Hartz IV, came into effect in January 2005 and was squarely targeted at reducing long-term unemployment. This phase significantly restructured unemployment and social assistance benefits, reducing the amount and duration of benefits for most recipients, and making them conditional on stricter job search and acceptance requirements. See Online Appendix D for additional details.

We don't attempt to identify the separate effects of individual components of the reforms because they were complex, multi-dimensional, and applied to all regions and all workers. There is arguably no control group of workers that was entirely unaffected by the reforms. However, because the reforms were primarily targeted at job search and unemployment benefits, they likely had the greatest impact on the behavior and outcomes of unemployed individuals who were actively

[24] As we show in Appendix B, and ignoring the omitted variable biases described above, $E\left[\hat{\delta}_{\psi}^{**}\right] = \left(\mathbf{D}'\mathbf{M}_{[\mathbf{Z}\ \mathbf{P}]}\mathbf{D}\right)^{-1}\mathbf{D}'\mathbf{M}_{[\mathbf{Z}\ \mathbf{P}]}\mathbf{F}\psi$, which sweeps out individual-level means of $\psi_{J(i,t)}$. In contrast our Decomposition 1 sorting effect is $\left(\mathbf{D}'\mathbf{M}_{\mathbf{Z}}\mathbf{D}\right)^{-1}\mathbf{D}'\mathbf{M}_{\mathbf{Z}}\mathbf{F}\psi$, which does not.

searching for work, and had a lesser effect on the wages of continuously employed individuals. Thus our empirical strategy is to embed our decomposition in a difference-in-differences framework that estimates how the difference between the wages of displaced and non-displaced workers changed from the period before the reforms to the period after. Engbom et al. (2015) rely on a similar difference-in-differences strategy, and find that the Hartz reforms reduced the wages of displaced workers by roughly 10 percent relative to non-displaced workers. However they do not estimate the effect of the reforms on the selection of workers into displacement, or sorting across employers and matches, which leaves the underlying mechanism unexplained.

3.1 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). Individuals are included in the LIAB if they were employed at an establishment in the IAB Establishment Panel for at least one day between 2002 and 2012. The LIAB data comprise each individual's complete history of employment subject to Social Security, marginal part-time employment, and receipt of short-term unemployment benefits between 1993 and 2014. The employment records include spell start and end dates, individual characteristics, their employment earnings, and a unique identifier for the establishment at which they were employed. We use the establishment identifiers to link employment records to the Establishment History Panel (BHP) to obtain employer characteristics including geography, industry, and years of operation.

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 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 Online Appendix C. The main departure is that we undertake our analysis at the quarterly level instead of annually because we prefer to have finer resolution of the elapsed time since displacement.

We compute each individual's total earnings at each establishment in each quarter and designate the one at which they earned the most as their main job 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 per quarter (the average number of jobs per quarter is 1.03 for men, and 1.04 for women), so we believe the restriction to main jobs 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, and convert wages to real 2010 euros using the CPI.

3.1.1 Defining Displacement

The IEB records specify the date a job ends, 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 use this eligibility rule to identify involuntarily displaced workers. Specifically, we 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.

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.[25] As shown in Table 1, roughly 2.5 percent of male wage observations meet this definition of recent displacement. Women are slightly more likely than men to be recently displaced. 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.[26] As shown in Figure 1, our displacement measure was generally stable in the four years prior to the introduction of the Hartz reforms, increased in early 2003, and remained elevated thereafter.

3.1.2 Additional Restrictions

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

[25]In Online Appendix E we show that our estimates are robust to alternate definitions of recent displacement in the preceding eight, twelve, or twenty quarters.

[26]Education is reported by employers, and consequently is more missing in the LIAB more often than most surveys.

comparison group of workers who were not recently displaced and had at least 24 months tenure with their current employer.[27] Since 24 months is at the lower end of the range of tenure restrictions in the displacement literature, we impose stricter minimum tenure requirements in robustness checks.

Table 2 presents summary statistics for our estimation sample. Columns 1 and 4 present sample means for the full sample of men and women, respectively, and columns 2 and 5 present the corresponding means after imposing the minimum tenure restriction. As expected, those satisfying the tenure restriction earn slightly more, have longer tenure with their current employer, and are less likely to be recently displaced than the full sample.

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 2 reports summary statistics for this set. It comprises about 97 percent of observations that satisfy the 24 month tenure restriction for men, slightly less for women, with sample means and proportions very similar to the overall sample. We focus our attention on the largest connected set for the remainder of this article.

3.2 Event Studies

To facilitate comparisons with other recent studies, we begin by estimating event study regressions similar to eq. (23). We estimate separate regressions before and after the reforms for a treatment group of displaced workers relative to a control group, between 20 quarters before displacement and 20 quarters after. The pre-Hartz treatment group consists of workers who were involuntarily displaced for the first time in 1998 or 1999, had at least 24 months job tenure at the time of displacement, and met our other sample restrictions above.[28] The post-Hartz treatment group is defined similarly for individuals whose first displacement occurred in 2005 or 2006. Our pre-Hartz control group consists of individuals who were never displaced between 1993 and 2014, had at least 24 months job tenure in at least one quarter in 1998 or 1999, and met our other sample restrictions

[27]To clarify, our estimation sample consists of individuals' entire work histories over the 1993-2014 period, subject to the sample restrictions defined above. In quarters when an individual is recently displaced, they are retained in the sample if they had at least 24 months job tenure at displacement. In quarters when an individual is not recently displaced, they are retained in the sample if they have at least 24 months job tenure at their current employer.

[28]We select 1998-1999 for the pre-Hartz displacement window to ensure a complete 20 quarter pre-displacement history for all treatment group members, with as much of the post-displacement history occurring prior to the introduction of the Hartz I-II reforms as possible.

above. The post-Hartz control group is defined similarly, except that we require these individuals to have at least 24 months job tenure in at least one quarter in 2005 or 2006. Individuals who satisfied both the pre- and post-Hartz control group definitions were randomly assigned to one of the two control groups. We also randomly assign each control group member to a reference quarter in which they satisfy the tenure restriction, and include a full set of dummy variables for quarter relative to the reference quarter in the event study regressions.[29] This follows Schmieder et al. (2019) and is important because the tenure restriction imposes hump-shaped earnings and employment profiles around the reference quarter that cannot be captured by time fixed effects. Additional controls in the event study regressions include a full set of quarterly time fixed effects τ_t^{**}, individual fixed effects α_i^{**}, a cubic polynomial in age, and age interacted with education (collectively, $\mathbf{z}_{it}$).

We plot estimates of δ_k^{**} for men and women in Figures 2 and 3, respectively. As shown in Panel A, displaced workers experienced wage declines relative to non-displaced workers in the quarters preceding displacement, both before and after the reforms. Upon displacement, men's wages fell by about 5 log points prior to the reforms, which is similar to what Schmieder et al. (2019) and Fackler et al. (2021) report, and women's wages fell by around 3 log points. Following the reforms, the initial wage loss expanded to roughly 20 log points for men and 12 log points for women, which is somewhat larger than Engbom et. al.'s (2015) difference-in-differences estimate of the post-Hartz increase in wage losses at displacement. Notably, the wage loss is effectively permanent: there's no evidence that wages recover in the 20 quarters following displacement for men or women, before or after the reforms. This mirrors Schmieder et al. (2019), Lachowska et al. (2020), and others who also find that log wages experience very little recovery in the five years following displacement. Of course these estimates are conditional on workers returning to employment (quarters of zero employment earnings are excluded from the Panel A regressions), and are consequently subject to

[29] Our event study specification mirrors Schmieder et al. (2019):

$$y_{it} = \mathbf{z}_{it}'\eta^{**} + \alpha_i^{**} + \sum_{k=-20}^{20} \gamma_k^{**}\mathbf{1}\left(t = c + k\right) + \sum_{k=-20}^{20} \delta_k^{**}\mathbf{1}\left(t = c + k\right) \times DISP_i + \tau_t^{**} + \epsilon_{it}^{**} \tag{25}$$

where c is the reference quarter, $\mathbf{1}\left(t = c + k\right)$ is an indicator that equals one if $t = c + k$ and zero otherwise, and $DISP_i$ is an indicator that equals one if i belongs to the treatment group of displaced workers and zero if they belong to the control group. In the notation of eq. (23), $d_{k,it} = \mathbf{1}\left(t = c + k\right) \times DISP_i$ where c is the quarter of displacement and $k = 0$ in that quarter; this is a binary indicator that equals one in period k relative to displacement. For the control group, $\mathbf{1}\left(t = c + k\right)$ is a binary indicator that equals one in quarter k relative to their randomly assigned reference quarter c. We normalize $\gamma_{-20}^{**} = \delta_{-20}^{**} = 0$ and omit the time dummy for the first quarter of 1993 to avoid collinearity. The δ_k^{**} can thus be interpreted as the difference in outcomes between displaced and non-displaced workers, relative to the difference 20 quarters before displacement.

selection – particularly if the reforms affected who returned to work, and when.

In Panel B we report estimates for real daily wages, including zeros in quarters of unemployment or non-employment. Here there is more evidence of recovery in the years following displacement which parallels displaced workers' return to employment (Panel C): in both cases, there is a sharp recovery in the first four quarters following displacement (when short-term unemployment benefits end for most workers), and a slower recovery thereafter.[30] Again, however, there is clear evidence that post-displacement outcomes worsened after the reforms. In the pre-Hartz period, men experienced an immediate daily wage loss of approximately 55 euros upon displacement, eventually recovering to 27 euros after 20 quarters. Following the reforms, the initial wage loss expanded to 97 euros and recovered more slowly, so that the wage loss remained approximately 51 euros after 20 quarters. Women's wage losses followed a similar path, but were smaller. The probability of employment in the quarter following displacement fell by roughly 18 percentage points following the reforms for men and 8 percentage points women, and remained substantially below pre-Hartz levels even after 20 quarters. That slower return to employment following the reforms mirrors an increase in short-term unemployment benefit receipt (Panels D and E). The increased probability of receiving benefits in the quarter after displacement is roughly 15 percentage points for men and 2 percentage points for women,[31] and men receive benefits for an additional 17 days in that quarter (6 for women). Notably, the probability of receiving UI benefits and the number of days of receipt remained above the pre-Hartz level for the first 12 quarters after displacement, even though a primary objective of the reforms was to reduce long-term unemployment.

In Online Appendix E we show that our event study estimates are robust to a stricter tenure requirement (60 months vs. 24 months) and a stricter definition of displacement due to establishment closure. Overall, they provide considerable evidence that the wage, employment, and unemployment outcomes of displaced workers changed significantly after the reforms. This raises questions about the underlying mechanism, and specifically whether the reforms changed which workers are selected

[30] Displaced and non-displaced workers also exhibit much more similar pre-displacement wage growth in Panel B than in Panel A. This is because individuals with more continuous pre-displacement work histories also experienced slower pre-displacement wage growth, relative to the non-displaced, than individuals with less continuous work histories in our event study sample. (Lachowska et al., 2020) report a similar pattern of wage growth, though slightly less pronounced than we observe in Germany, likely because they impose stronger restrictions on the pre-displacement work histories of their treatment and control groups.

[31] Despite our definition of displacement, the probability of receiving UI benefits in the first quarter following displacement is less than one because some displaced individuals collect UI benefits in the displacement quarter ($k = 0$) and have already returned to work by the following quarter ($k = 1$).

into displacement and re-employment, and how they were sorted across establishments and match quality, all of which motivates our decomposition.

3.3 Decomposing Wage Declines

We embed our decompositions in a difference-in-differences framework that estimates the difference between the wages of displaced and non-displaced workers, before vs. after the Hartz reforms. We focus on log daily wages. Since our event study regressions indicate that post-displacement log wage losses are effectively permanent, we specify our decomposition using a simple indicator $d_{it} = 1$ if the individual was displaced in the preceding four quarters and zero otherwise.

For the baseline eq. (3), we define:

$$\mathbf{d_{it}}'\delta_0 = \delta_0^B d_{it} + \delta_0^D d_{it} \times DURING_{it} + \delta_0^H d_{it} \times HARTZ_{it} \tag{26}$$

where $DURING_{it}$ is an indicator variable that equals one if the displacement occurred in 2002, 2003, or 2004; and $HARTZ_{it}$ is an indicator variable that equals one if the displacement occurred in 2005 or later. We include the interaction term $d_{it} \times DURING_{it}$ because the reforms were introduced in stages between 2003 and 2005, and thus displacements between 2002 and 2004 are likely to have been partially exposed to the Hartz reforms.[32] Including this interaction ensures that we are able to make clean comparisons between the pre- and post-reform periods.

The gross wage gap δ_0 is identified from within- and between-individual differences in wages in the four quarters after displacement versus all other quarters. Of primary interest, δ_0^H is a difference-in-differences (DiD) estimate of the post-Hartz change in the gross wage effect of displacement. It has a causal interpretation if a parallel trends assumption is satisfied. That assumption requires that the wage gap between recently displaced and non-displaced workers would have remained constant in the absence of the reforms. In support of that assumption, Figure 1 plots the difference between the log wages of recently displaced and non-displaced workers between 1999 and 2008. There is no discernible trend in the four years prior to the reforms, and a clear decline that begins in the first quarter of 2003. Nevertheless, we do not insist on a causal interpretation of our estimates.

[32] Because most workers were entitled to at least 12 months of short-term unemployment benefits, unemployment spells that began in early 2002 would have been eligible for benefits continuing into 2003, when Hartz I-II were introduced. Only unemployment spells that began after Hartz IV was introduced on January 1 2005 were exposed to the full set of reforms for the full duration of the unemployment spell.

Similarly, for the within-match eq. (5), we define:

$$\mathbf{d_{it}}'\delta = \delta^{B} d_{it} + \delta^{D} d_{it} \times DURING_{it} + \delta^{H} d_{it} \times HARTZ_{it}. \tag{27}$$

In this case δ is identified from within-match differences in wages between the four quarters following displacement and all other quarters. Two kinds of wage differences contribute to identification: within-match differences between pre- and post-displacement wages in the subset of individuals that return to work at the displacing establishment within four quarters of displacement; and within-match differences between wages in the first four quarters following displacement and later quarters. In our estimation sample, slightly less than one fifth of displacements contribute to identification via the first channel,[33] whereas more than 80 percent contribute via the second.[34] δ^H is a DiD estimate of the pre- vs. post-reform change in the within-match wage gap between recently displaced and non-displaced workers. It is separately identified from other elements of δ by variation in the within-match wage gap around the time period cutoffs in 2002 and 2005. Again, it has a causal interpretation if a parallel trends assumption holds, which requires that the within-match wage gap would have remained constant in the absence of the reforms.

Given these definitions, it is straightforward to apply our decomposition approach to understand the mechanisms underlying the expansion of the post-displacement wage loss after the reforms. $\hat{\delta}_0^H - \hat{\delta}^H$ is a DiD estimate of the post-reform change in the distribution of unobserved worker, establishment, and match characteristics in the wages of recently displaced workers, relative to non-displaced workers. Defining $\mathbf{d}'_{it}\delta_\theta, \mathbf{d}'_{it}\delta_\psi$ and $\mathbf{d}'_{it}\delta_\phi$ in eqs. (11)-(13) analogously to $\mathbf{d_{it}}'\delta_0$ and $\mathbf{d_{it}}'\delta$ in eqs. (26) and (27), then δ_θ^H, δ_ψ^H, and δ_ϕ^H are DiD estimates of the post-Hartz change in the selection, sorting, and matching components of the gross wage effect of displacement.

[33] 23 percent of displaced men and 20 percent of displaced women in our estimation sample eventually return to work at the displacing establishment. The vast majority (80 percent of men and women alike) do so within four quarters. The mean time to return is 3.3 quarters for men, and 3.5 quarters for women.

[34] Prior to the reforms, 87 percent of displaced men and women who returned to work within four quarters remained employed at their post-displacement establishment for more than four quarters. This fell to 80 percent for men following the reforms, and 78 percent for women, so that the proportion of post-reform matches contributing to identification of δ^H via the second channel is smaller than the pre-reform proportion contributing to δ^B. We have no reason to think that these matches, or the workers or establishments that enter into them, are systematically different than prior to the reforms, and $\theta_i, \psi_{J(i,t)}$ and $\phi_{iJ(i,t)}$ control for any such heterogeneity.

3.4 Results

Column 1 of Table 3 presents estimated coefficients on the displacement indicators in our baseline specification, eqs. (3) and (26).[35] These indicate that prior to the Hartz reforms, recently displaced workers earned substantially less than their non-displaced counterparts: 23.5 log points for men and 25.5 log points for women. The gross wage effect of displacement increased substantially after the reforms: by 14.3 log points for men, and by 5.1 log points for women. This is very similar to our event study estimate of the expanded wage loss for men, though somewhat smaller than the estimate for women. These are also larger than Engbom et al. (2015) and Price's (2016) estimates of the reforms' effects on the wages of recently displaced workers. This might be due to the longer time horizon considered in this paper, differences between our definition of wages and displacement and theirs, or our inclusion of the $d_{it} * DURING_{it}$ interaction, which makes for a cleaner comparison between the pre- and post-reform periods.[36]

Column 2 presents coefficient estimates from eq. (5). The difference between columns 1 and 2 measures the combined effect of selection, sorting, and matching on the wages of recently displaced workers. Prior to the reforms, these channels collectively explained almost all of the difference between displaced and non-displaced workers' wages: 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 wage gap. After the reforms, the within-match wage gap 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 gross wage effect of displacement 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 recently displaced women, all of the 5.1 log point wage decline is attributable to these selection, sorting, and matching channels. For both men and women, we easily reject

[35]We estimate all regression specifications separately for men and women. Our controls for observable characteristics include education (5 categories), a cubic polynomial in age, and the interaction between age and education; and all specifications include a full vector of fixed time effects.

[36]Engbom et al. (2015) estimate their model on monthly data and define the wage in month t as the average monthly earnings between t and $t + 12$. We restrict our attention to individuals involuntarily displaced from employment, whereas Engbom et al. (2015) define an individual as displaced if they collect unemployment benefits. Their measure will include individuals who quit voluntarily and collect unemployment benefits.

the null hypothesis that the combined effect of the reforms via these channels is zero using the Hausman-type test developed in Proposition 2.[37]

Columns 3-5 and 6-8 decompose the selection, sorting, and matching effects via Decompositions 1 and 2, respectively. The two decompositions yield very similar estimates.[38] Prior to the reforms, the lion's share of the measured wage difference between recently displaced and non-displaced workers was due to selection. Recently displaced men earned 15.4 log points less than their non-displaced counterparts because they had unobserved characteristics that earned lower returns in the labor market; this comprised 66 percent of the pre-reform wage difference. Among women, selection accounted for 11.9 log points (47 percent) of the pre-reform gross wage effect of displacement. However, selection accounts for none of the wage decline that displaced women experienced after the reforms, and actually increased the wages of displaced men by about 2 log points. That is, recently displaced men had slightly higher-earning unobserved characteristics after the reforms than before, relative to their non-displaced counterparts, and this slightly reduced the wage gap between them. We present additional evidence that individuals were more positively selected into displacement after the reforms in Online Appendix E.

In line with Schmieder et al. (2019) and Fackler et al. (2021), we find that much of the gross wage effect of displacement is explained by sorting into employment at establishments with relatively low wage premia. This accounted for more than 30 percent (7.4 log points) of the pre-reform gross effect for men, and more than 40 percent (10.9 log points) for women. However almost all of the increased wage gap following the Hartz reforms – about 12 log points for men and over 4 log points for women – is because recently displaced workers sorted increasingly into employment at lower-paying establishments. This accounts for 84 percent of the post-reform increase in the wage gap. It's clear that sorting is the primary channel via which wage losses of displaced workers increased after the reforms.

Our two decomposition approaches yield different, though in both cases small and negative, estimates of the matching effect. Prior to the reforms, entering into relatively low quality matches

[37] $Q = 1039$ for men and $Q = 79.2$ for women; in each case the corresponding p-value is < 0.00001.

[38] In the Online Appendix we present estimates of the AKM specification on which Decomposition 2 is based. The AKM specification provides an alternate estimate of the wage effect of displacement that more closely aligns with estimates in other studies, because it identifies the wage effects of displacement from within-person variation in wages around a displacement event, instead of within-match variation. In our application, this distinction matters little: the estimated wage effects of displacement Table E.1 are very close to the within-match estimates in column (2) of Table 3.

explained 0.2-0.5 log points of the gross wage effect of displacement. However, this more than doubled for men and roughly tripled for women after the reforms. Decomposition 2, which imposes fewer restrictions on the relationship between unobservables and relies on a broader definition of match heterogeneity, yields estimates that are roughly twice as large as Decomposition 1, accounting for 9 percent of the wage decline that displaced men face following the Hartz reforms, and 23.5 percent of displaced women's wage decline.

Table 4 further summarizes the distribution of individual, establishment, and match effects estimated from eq. (10).[39] Individual fixed effects comprise the largest component of observed variation in wages (roughly 80 percent for both men and women). The average value of $\hat{\theta}_i$ among recently displaced individuals is more than 14 log points below the average of non-displaced men and 9.8 log points below the average for non-displaced women. 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 $\hat{\psi}_{J(i,t)}$ among recently displaced men and women is roughly 15 log points below the non-displaced. However, displaced workers also face considerably more dispersion in $\hat{\psi}_{J(i,t)}$ than non-displaced workers, so it is clear that they are not uniformly employed in low-wage establishments.

3.5 Robustness

Most recent studies define displacement as employment separations that occur during a mass layoff event.[40] While we prefer our definition of displacement based on the timing of UI benefit receipt, an alternative definition that is closer to the literature facilitates comparisons with other studies. In Table 5, we present comparable estimates based on a stricter definition of 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 because in some

[39] Andrews et al. (2008), Bonhomme et al. (2019), Kline et al. (2020) and others note in the context of AKM specifications that the sample variances of $\hat{\theta}_i$ and $\hat{\psi}_{J(i,t)}$ and the sample correlation between them are biased estimators of the underlying variance components and $Corr(\theta_i, \psi_{J(i,t)})$ when employment mobility is low. Those authors propose alternate variance estimators that address the bias. Those alternatives, and the limited-mobility bias critique in general, focus specifically on the AKM model without match effects. While similar biases may exist in the model with match effects, we do not attempt to correct for such bias for two reasons. First, the bias is primarily a short-panel phenomenon, and ours is much longer (22 years) than those authors consider in their applications. Second, we are primarily interested in differences between recently displaced and non-displaced workers and we have no reason to think that limited-mobility bias is likely to affect these two groups differently.

[40] Typically, these are defined as 30% reductions in employment and include establishment closures.

instances it may conflate closure with ownership changes.[41,42] However, focusing on establishment closure does provide 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 bias due to misclassification. In fact, we find that it has very little effect on our estimates, which alleviates concerns that our UI-based definition of displacement might inhibit direct comparisons with the broader literature. Indeed, Table 5 estimates for men are virtually identical to those in Table 3, although the estimated selection effect is no longer statistically significant. The stricter definition of displacement yields a slightly larger estimate of the wage decline following the Hartz reforms – nearly 9 log points – for women, that is entirely attributable to sorting into lower-paying establishments after displacement.

In Online Appendix E we show that our estimates are also robust to less strict definitions of recent displacement (displacement in the preceding 8, 12, or 20 quarters), a stricter 60 month tenure restriction, and are unlikely to be driven by the effects of the 2008 financial crisis, the lingering effects of German reunification, different pre-policy trends for displaced and non-displaced workers, important omitted variables, or increasing returns to job tenure.

3.6 Unpacking the sorting effect

Sorting was the primary mechanism underlying the decline in recently displaced workers' wages after the reforms. To better understand why, we further decompose the sorting effect using observable establishment characteristics. Our objective is to characterize the low wage establishments where displaced workers were increasingly employed after the reforms. To this end, we estimate an augmented version of eq. (12), in which we regress $\hat{\psi}_{J(i,t)}$ on the observable characteristics and displacement indicators from our baseline specification ($\mathbf{z}_{it}$ and $\mathbf{d}_{it}$), and a vector of additional

[41] Establishments are identified via a unique ID number. Card et al. (2013) note that an establishment is issued a new ID number if it changes ownership. Using data on worker flows between establishments, Schmieder and Hethey (2010) estimate that only about half of establishment ID "deaths" in the IEB are true establishment closings. 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.

[42] 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 establishment wage premium since a new owner might change the structure of compensation. In cases where a new establishment ID is assigned to a continuing business enterprise, there is no bias from treating the old and new IDs as different establishments, but there is a potential loss of efficiency if the old and new establishments have the same wage structure.

establishment characteristics.[43] The coefficient on the interaction between the recent displacement and post-Hartz indicators in this regression, δ_{ψ}^{H}, provides a revised estimate of the sorting effect that is net of the contribution of establishment characteristics to the post-Hartz wage decline, and allows us to measure the contribution of establishment characteristics to the sorting effect.

As shown in Table 6, establishment characteristics explain about 40 percent (4.5 log points) of the post-reform increase in the sorting effect for men, and about 25 percent (1.1 log points) for women. The only characteristic that explains a meaningful portion of this (over 40 percent for both genders) is industry sector. Displaced men's wages fell by 5 log points after the reforms because they sorted into lower-wage sectors; the remaining 7 log points of the sorting effect is due to sorting into lower-wage establishments within sectors. Among women, sorting into lower-wage sectors and sorting into lower-wage establishments within sector each explain roughly 2 log points of the sorting effect.

How did the sectoral allocation of displaced workers change following the reforms? To answer this question, Table 7 present the top pre- and post-displacement sectors before and after the Hartz reforms, along with the average establishment fixed effect in each sector. Unsurprisingly, displaced workers are predominantly employed in low-wage sectors, both before and after displacement and before and after the reforms.[44] 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 sector (a high-wage sector where workers received above-average establishment wage premia) after the reforms. However, very few displaced workers found re-employment in this sector in the four quarters following displacement, which contributed to wage declines explained by reallocating into lower-paying sectors.

The most striking feature of Table 7, 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 1998 and 2001, 7.6 percent of men and 5 percent of women were employed in this sector in the four quarters following displacement. By the 2005-2009 period, this sector had grown to account for the largest share of post-displacement employment by

[43] Establishment characteristics are industry sector (202 categories), establishment birth year (40 categories), establishment size (8 categories), geography (10 state categories), and the share of employees who work full-time.

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

a wide margin: in our sample, roughly 21 percent of men and 17 percent of women were employed in this sector in the four quarters following displacement.[45] 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. The large number of displaced workers who move into employment in this sector after the reforms, and the large negative wage premium they receive upon doing so, is an important reason for the post-reform wage decline of displaced workers.

As shown in Figure 4, 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 females at the 2012 maximum – it grew steadily between 1994 and 2014.[46] For both men and women, the rate of that growth clearly accelerated following the introduction of Hartz I-II in 2003.

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.[47] This was a short-lived phenomenon, however: the legislation mandating PSAs was weakened after 2005, and repealed entirely in 2009, following widespread criticism that PSAs failed to re-integrate unemployed workers into permanent employment (Mosley, 2006).[48] Second, 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

[45]As shown in Table E.9 in the Online Appendix, this trend is even more pronounced over the full 1994-2014 period of our sample.

[46]The share of employment in Sector 74.5 in our sample is comparable to that reported by Antoni and Jahn (2009), Spermann (2011), Hirsch and Mueller (2012), and Goldschmidt and Schmieder (2017) in other samples. Consistent with our findings, Fackler et al. (2019) report that displacement increased the probability of temporary employment in a sample of older displaced German workers between 2008 and 2013.

[47]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).

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

(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: 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 stipulated relatively low wages.[49] This effectively deregulated temporary employment in Germany, and began a period of sustained employment growth in the sector. Table 7 shows that the growth in temporary employment was especially pronounced among displaced workers, and contributed to their lower wages after the reforms.

Our estimates are also consistent with the hypothesis that some post-reform displacements were caused by employers substituting away from permanent job arrangements toward temporary work. Indeed Goldschmidt and Schmieder (2017) document a large increase in outsourcing over a similar period and show that workers outsourced to temporary employment agencies experience highly persistent wage declines. It is unclear how many workers identified as outsourced in Goldschmidt and Schmieder (2017) would also meet our definition of displacement,[50] but the magnitude of the wage declines that they report are similar to ours. While establishments' increasing reliance on outsourcing and a broader shift toward temporary work could have altered which workers are selected into displacement, and which establishments employ them before and after displacement, our within-match specification for wages addresses this by holding constant the unobserved characteristics of workers and establishments.

4 Conclusion

The Hartz reforms were sweeping, multi-dimensional, and affected many aspects of German employment and social benefits. Following the reforms, the wages of recently displaced men and women declined by about 14 log points and 5 log points, respectively, relative to non-displaced workers. Because the reforms encompassed so many changes in such a short time frame, and because they

[49] 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.

[50] Goldschmidt and Schmieder (2017) identify outsourced workers from large employment flows between one establishment (the "mother") and another establishment (the "daughter"), subject to various restrictions on size, sector, and timing. Workers who collect UI benefits within 12 weeks of separating from the mother, and are re-employed at the daughter within four quarters would meet our definition of recent displacement.

potentially affected all workers, it is unrealistic to expect that we can determine which specific provisions of the reforms were responsible for the wage declines. However our decomposition approach helps to understand the underlying mechanism. Specifically, our decomposition reveals that the wages of recently displaced workers fell primarily because they increasingly found re-employment in low-wage establishments. They also entered into worse matches, which contributed to a smaller degree. For men, this was partly offset by increasingly positive selection into displacement.

The Hartz IV package is likely a key contributor to the wage decline because it gave unemployed workers strong incentives to return to work quickly, even if it meant accepting a relatively low wage offer. This may also explain the offsetting effect of selection following the reforms if workers with higher-earning unobserved characteristics faced stronger incentives to return to work quickly. This seems likely, since unemployment is more costly for higher-earning individuals.

Provisions of Hartz I-II that deregulated temporary work arrangements likely accelerated a shift away from permanent work arrangements that began in the early 1990s. The substantial negative wage premia among employers in the temporary work sector and the large number of displaced workers who found work there after the reforms partly explain the decline in displaced workers' wages. The ongoing shift toward temporary work might also explain the positive selection effects we find after the reforms. In particular, it's possible that workers with the "worst" unobserved characteristics were the first to be displaced into temporary work, before the reforms, because they were the most costly to employ via permanent contracts, so that those displaced later had "better" unobserved characteristics on average.

Finally, as noted at the outset, there is an interesting discrepancy in the estimated importance of sorting in recent American and German studies. While our decomposition and analysis do not resolve the discrepancy, they do at least eliminate one possible cause: that failing to account for match effects in wages might explain why previous German studies found sorting to be the main determinant of wage losses at displacement. On the contrary, our application shows that sorting is the dominant cause of wage losses in Germany, even controlling for match effects.

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Appendix A Proofs

Proof of Proposition 2. We provide a direct proof for the case of spherical errors, $E\left[\epsilon\epsilon'|\mathbf{Z},\mathbf{D}\right]=\sigma^2\mathbf{I}$. 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. (6) is $\hat{\delta}_\Phi=\left(\mathbf{D}'\mathbf{M_Z}\mathbf{D}\right)^{-1}\mathbf{D}'\mathbf{M_Z}\mathbf{G}\boldsymbol{\Phi}=\delta_\Phi+\left(\mathbf{D}'\mathbf{M_Z}\mathbf{D}\right)^{-1}\mathbf{D}'\mathbf{M_Z}\epsilon_\Phi$ where ϵ_Φ is the error term in eq. (6) satisfying $E\left[\epsilon_\Phi|\mathbf{Z},\mathbf{D}\right]=\mathbf{0}$ and $E\left[\epsilon_\Phi\epsilon'|\mathbf{Z},\mathbf{D}\right]=\mathbf{0}$. We can write $\hat{\delta}_0=\delta+\left(\mathbf{D}'\mathbf{M_Z}\mathbf{D}\right)^{-1}\mathbf{D}'\mathbf{M_Z}\mathbf{G}\boldsymbol{\Phi}+\left(\mathbf{D}'\mathbf{M_Z}\mathbf{D}\right)^{-1}\mathbf{D}'\mathbf{M_Z}\epsilon=\delta+\hat{\delta}_\Phi+\left(\mathbf{D}'\mathbf{M_Z}\mathbf{D}\right)^{-1}\mathbf{D}'\mathbf{M_Z}\epsilon$ and $\hat{\delta}=\delta+\left(\mathbf{D}'\mathbf{M}_{[\mathbf{Z}\ \mathbf{G}]}\mathbf{D}\right)^{-1}\mathbf{D}'\mathbf{M}_{[\mathbf{Z}\ \mathbf{G}]}\epsilon$. Under H_0, $\hat{\delta}_\Phi=\left(\mathbf{D}'\mathbf{M_Z}\mathbf{D}\right)^{-1}\mathbf{D}'\mathbf{M_Z}\epsilon_\Phi$ and consequently $\hat{\delta}_0=\delta+\left(\mathbf{D}'\mathbf{M_Z}\mathbf{D}\right)^{-1}\mathbf{D}'\mathbf{M_Z}\left(\epsilon_{\boldsymbol{\Phi}}+\epsilon\right)$, so that:

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

because $\mathbf{D}'\mathbf{M_Z}\mathbf{M}_{[\mathbf{Z}\ \mathbf{G}]}=\mathbf{D}'\mathbf{M}_{[\mathbf{Z}\ \mathbf{G}]}$. It follows that $Var\left[\hat{\delta}_0-\hat{\delta}|\mathbf{Z},\mathbf{D}\right]=Var\left[\hat{\delta}_0|\mathbf{Z},\mathbf{D}\right]+Var\left[\hat{\delta}|\mathbf{Z},\mathbf{D}\right]-2Cov\left[\hat{\delta}_0,\hat{\delta}|\mathbf{Z},\mathbf{D}\right]=Var\left[\hat{\delta}|\mathbf{Z},\mathbf{D}\right]-Var\left[\hat{\delta}_0|\mathbf{Z},\mathbf{D}\right]$. Under standard regularity conditions, $\hat{\delta}_0$ and $\hat{\delta}$ are asymptotically normal and hence so is their difference, so that

$$Q^*=\left(\hat{\delta}_0-\hat{\delta}\right)'\left(Var\left[\hat{\delta}|Z,\mathbf{D}\right]-Var\left[\hat{\delta}_0|\mathbf{Z},\mathbf{D}\right]\right)^{-1}\left(\hat{\delta}_0-\hat{\delta}\right)\overset{a}{\sim}\chi^2_l. \qquad \text{(A.2)}$$

Since $\hat{Var}\left[\hat{\delta}_0\right]$ and $\hat{Var}\left[\hat{\delta}\right]$ are consistent estimates of $Var\left[\hat{\delta}_0|\mathbf{Z},\mathbf{D}\right]$ and $Var\left[\hat{\delta}|\mathbf{Z},\mathbf{D}\right]$, Q^* and Q have the same asymptotic distribution. □

Appendix B Bias in Alternative Approaches

Abstracting from the fact that Fackler et al. (2021), Lachowska et al. (2020), Moore and Scott-Clayton (2019), and Schmieder et al. (2019), estimate their AKM wage specifications (22) on a broader sample of observations than their JLS-style regressions (23) and (24), we rewrite those equations in matrix notation as:

$$\mathbf{y} = \mathbf{Z}\eta^* + \mathbf{P}\theta^* + \mathbf{F}\psi^* + \epsilon^* \tag{B.1}$$

$$\mathbf{y} = \mathbf{Z}\eta^{**} + \mathbf{P}\alpha^{**} + \mathbf{D}\delta^{**} + \epsilon^{**} \tag{B.2}$$

$$\mathbf{F}\hat{\psi}^* = \mathbf{Z}\eta^{**}_{\psi} + \mathbf{P}\alpha^{**}_{\psi} + \mathbf{D}\delta^{**}_{\psi} + \epsilon^{**}_{\psi}. \tag{B.3}$$

In this framework, the wage loss of displacement is the OLS estimator of δ^{**} in eq. (B.2), $\hat{\delta}^{**} = \left(\mathbf{D}'\mathbf{M}_{[\mathbf{Z}\,\mathbf{P}]}\mathbf{D}\right)^{-1}\mathbf{D}'\mathbf{M}_{[\mathbf{Z}\,\mathbf{P}]}\mathbf{y}$, and the employer contribution to the wage loss is the OLS estimator of δ^{**}_{ψ} in eq. (B.3),

$$\hat{\delta}^{**}_{\psi} = \left(\mathbf{D}'\mathbf{M}_{[\mathbf{Z}\,\mathbf{P}]}\mathbf{D}\right)^{-1}\mathbf{D}'\mathbf{M}_{[\mathbf{Z}\,\mathbf{P}]}\mathbf{F}\hat{\psi}^* = \left(\mathbf{D}'\mathbf{M}_{[\mathbf{Z}\,\mathbf{P}]}\mathbf{D}\right)^{-1}\mathbf{D}'\mathbf{M}_{[\mathbf{Z}\,\mathbf{P}]}\mathbf{F}\left(\mathbf{F}'\mathbf{M}_{[\mathbf{Z}\,\mathbf{P}]}\mathbf{F}\right)^{\mathbf{-1}}\mathbf{F}'\mathbf{M}_{[\mathbf{Z}\,\mathbf{P}]}\mathbf{y}.$$

If wages are determined according to eq. (10) and $E\left[\epsilon|\mathbf{Z},\mathbf{D},\mathbf{P},\mathbf{F}\right] = \mathbf{0}$ then

$$E\left[\hat{\delta}^{**}\right] = \delta + \left(\mathbf{D}'\mathbf{M}_{[\mathbf{Z}\,\mathbf{P}]}\mathbf{D}\right)^{-1}\mathbf{D}'\mathbf{M}_{[\mathbf{Z}\,\mathbf{P}]}\left(\mathbf{F}\psi + \mathbf{G}\phi\right)$$

where the second term is a regression-adjusted difference between the average establishment and match effects of displaced and non-displaced workers. Similarly,

$$E\left[\hat{\delta}^{**}_{\psi}\right] = \left(\mathbf{D}'\mathbf{M}_{[\mathbf{Z}\,\mathbf{P}]}\mathbf{D}\right)^{-1}\mathbf{D}'\mathbf{M}_{[\mathbf{Z}\,\mathbf{P}]}\mathbf{F}\psi + \left(\mathbf{D}'\mathbf{M}_{[\mathbf{Z}\,\mathbf{P}]}\mathbf{D}\right)^{-1}\mathbf{D}'\mathbf{M}_{[\mathbf{Z}\,\mathbf{P}]}\mathbf{F}\left(\mathbf{F}'\mathbf{M}_{[\mathbf{Z}\,\mathbf{P}]}\mathbf{F}\right)^{\mathbf{-1}}\mathbf{F}'\mathbf{M}_{[\mathbf{Z}\,\mathbf{P}]}\left(\mathbf{D}\delta + \mathbf{G}\phi\right).$$

The first term in this expression is similar to our Decomposition 1 sorting effect, except that $\mathbf{M}_{[\mathbf{Z}\,\mathbf{P}]}$ differences out person-level means of $\psi_{J(i,t)}$. The second term is bias that arises from omitting the wage effects of displacement, $\mathbf{D}\delta$, and match effects from eq. (B.1). Specifically, $\left(\mathbf{D}'\mathbf{M}_{[\mathbf{Z}\,\mathbf{P}]}\mathbf{D}\right)^{-1}\mathbf{D}'\mathbf{M}_{[\mathbf{Z}\,\mathbf{P}]}\mathbf{F}\left(\mathbf{F}'\mathbf{M}_{[\mathbf{Z}\,\mathbf{P}]}\mathbf{F}\right)^{\mathbf{-1}}\mathbf{F}'\mathbf{M}_{[\mathbf{Z}\,\mathbf{P}]}\mathbf{D}\delta$ is a regression-adjusted difference between displaced and non-displaced workers in employer-level averages of displacement effects, $\mathbf{D}\delta$. It will be non-zero if displaced and non-displaced workers sort systematically into different employers, e.g., if workers are systematically displaced from specific employers (as they are in mass displacement events), or systematically find re-employment in particular employers after displacement (as we find in our application). Likewise, $\left(\mathbf{D}'\mathbf{M}_{[\mathbf{Z}\,\mathbf{P}]}\mathbf{D}\right)^{-1}\mathbf{D}'\mathbf{M}_{[\mathbf{Z}\,\mathbf{P}]}\mathbf{F}\left(\mathbf{F}'\mathbf{M}_{[\mathbf{Z}\,\mathbf{P}]}\mathbf{F}\right)^{\mathbf{-1}}\mathbf{F}'\mathbf{M}_{[\mathbf{Z}\,\mathbf{P}]}\mathbf{G}\phi$ is a regression-adjusted difference between displaced and non-displaced workers in employer-level averages of match effects. It will be non-zero if displaced and non-displaced workers sort systematically into different quality matches, e.g., if displaced workers find re-employment in lower-quality matches.

The estimated "net" effect of displacement in this framework satisfies

$$
\begin{aligned}
E\left[\hat{\delta}^{**}-\hat{\delta}^{**}_{\psi}\right] &= \delta-\left(\mathbf{D}'\mathbf{M}_{[\mathbf{Z}\,\mathbf{P}]}\mathbf{D}\right)^{-1}\mathbf{D}'\mathbf{M}_{[\mathbf{Z}\,\mathbf{P}]}\mathbf{F}\left(\mathbf{F}'\mathbf{M}_{[\mathbf{Z}\,\mathbf{P}]}\mathbf{F}\right)^{\mathbf{-1}}\mathbf{F}'\mathbf{M}_{[\mathbf{Z}\,\mathbf{P}]}\mathbf{D}\delta \\
&\quad+\left(\mathbf{D}'\mathbf{M}_{[\mathbf{Z}\,\mathbf{P}]}\mathbf{D}\right)^{-1}\mathbf{D}'\mathbf{M}_{[\mathbf{Z}\,\mathbf{P}]}\left(\mathbf{I}-\mathbf{F}\left(\mathbf{F}'\mathbf{M}_{[\mathbf{Z}\,\mathbf{P}]}\mathbf{F}\right)^{\mathbf{-1}}\mathbf{F}'\mathbf{M}_{[\mathbf{Z}\,\mathbf{P}]}\right)\mathbf{G}\phi
\end{aligned}
$$

where the bias terms have been defined previously, and arise because the wage effects of displacement and match effects are omitted from the AKM-style wage equation (B.1).

Lachowska et al. (2020) additionally estimate match effects from a specification like:

$$
\mathbf{y}=\mathbf{Z}\eta^{*}+\mathbf{P}\theta^{*}+\mathbf{F}\psi^{*}+\mathbf{G}\phi^{*}+\epsilon^{*} \tag{B.4}
$$

and estimate the contribution of matching to post-displacement wage losses from the OLS estimator of δ^{**}_{ϕ} in:

$$
\mathbf{G}\hat{\phi}^{*}=\mathbf{Z}\eta^{**}_{\phi}+\mathbf{P}\alpha^{**}_{\phi}+\mathbf{D}\delta^{**}_{\phi}+\epsilon^{**}_{\phi}.
$$

That estimator is:

$$
\hat{\delta}^{**}_{\phi}=\left(\mathbf{D}'\mathbf{M}_{[\mathbf{Z}\,\mathbf{P}]}\mathbf{D}\right)^{-1}\mathbf{D}'\mathbf{M}_{[\mathbf{Z}\,\mathbf{P}]}\mathbf{G}\hat{\phi}^{*}=\left(\mathbf{D}'\mathbf{M}_{[\mathbf{Z}\,\mathbf{P}]}\mathbf{D}\right)^{-1}\mathbf{D}'\mathbf{M}_{[\mathbf{Z}\,\mathbf{P}]}\mathbf{G}\left(\mathbf{G}'\mathbf{M}_{[\mathbf{Z}\,\mathbf{P}\,\mathbf{F}]}\mathbf{G}\right)^{\mathbf{-1}}\mathbf{G}'\mathbf{M}_{[\mathbf{Z}\,\mathbf{P}\,\mathbf{F}]}\mathbf{y}
$$

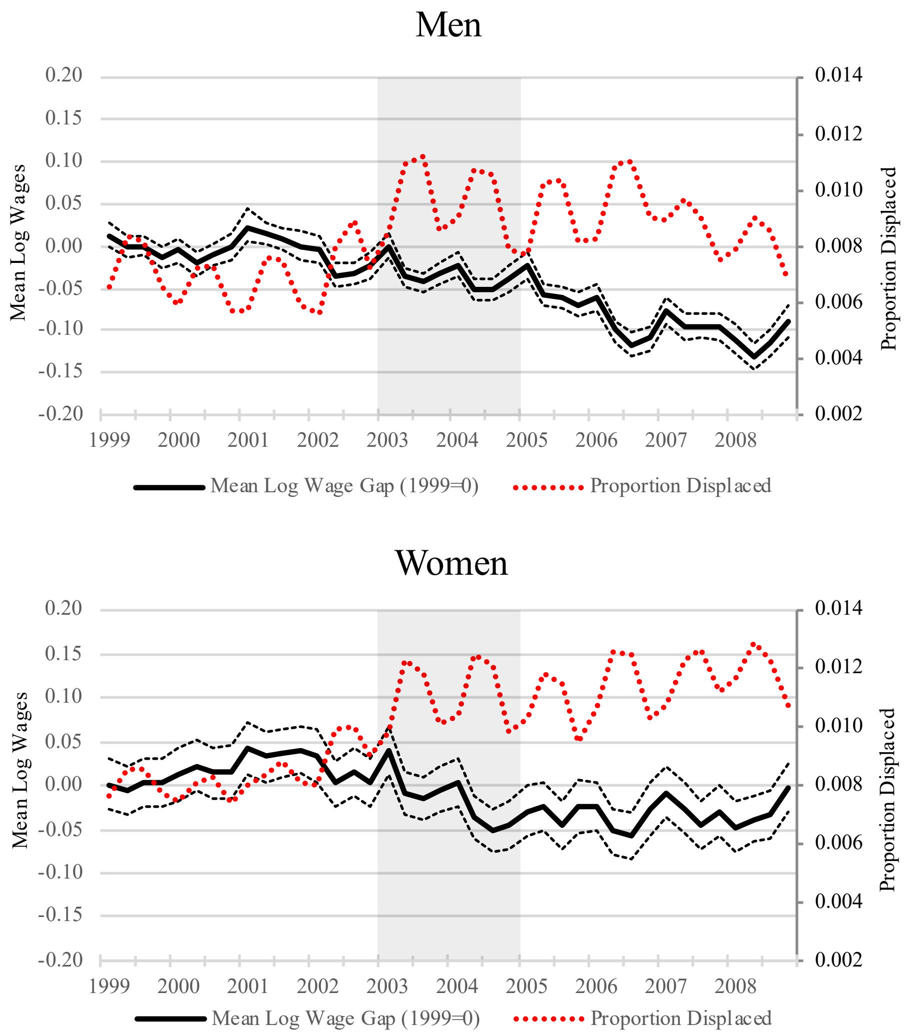
so that

$$
E\left[\hat{\delta}^{**}_{\phi}\right]=\left(\mathbf{D}'\mathbf{M}_{[\mathbf{Z}\,\mathbf{P}]}\mathbf{D}\right)^{-1}\mathbf{D}'\mathbf{M}_{[\mathbf{Z}\,\mathbf{P}]}\mathbf{G}\phi+\left(\mathbf{D}'\mathbf{M}_{[\mathbf{Z}\,\mathbf{P}]}\mathbf{D}\right)^{\mathbf{-1}}\mathbf{D}'\mathbf{M}_{[\mathbf{Z}\,\mathbf{P}]}\mathbf{G}\left(\mathbf{G}'\mathbf{M}_{[\mathbf{Z}\,\mathbf{P}\,\mathbf{F}]}\mathbf{G}\right)^{-1}\mathbf{G}'\mathbf{M}_{[\mathbf{Z}\,\mathbf{P}\,\mathbf{F}]}\mathbf{D}\delta.
$$

The first term is similar to our Decomposition 1 matching effect, except that $\mathbf{M}_{[\mathbf{Z}\,\mathbf{P}]}$ differences out person-level means of $\phi_{iJ(i,t)}$. The second term is bias that arises because $\mathbf{D}\delta$ is omitted from eq. (B.4). It is a regression-adjusted difference between displaced and non-displaced workers in match-level averages of displacement effects, $\mathbf{D}\delta$. It will be non-zero if $\delta \neq 0$. The resulting "net" effect of displacement satisfies:

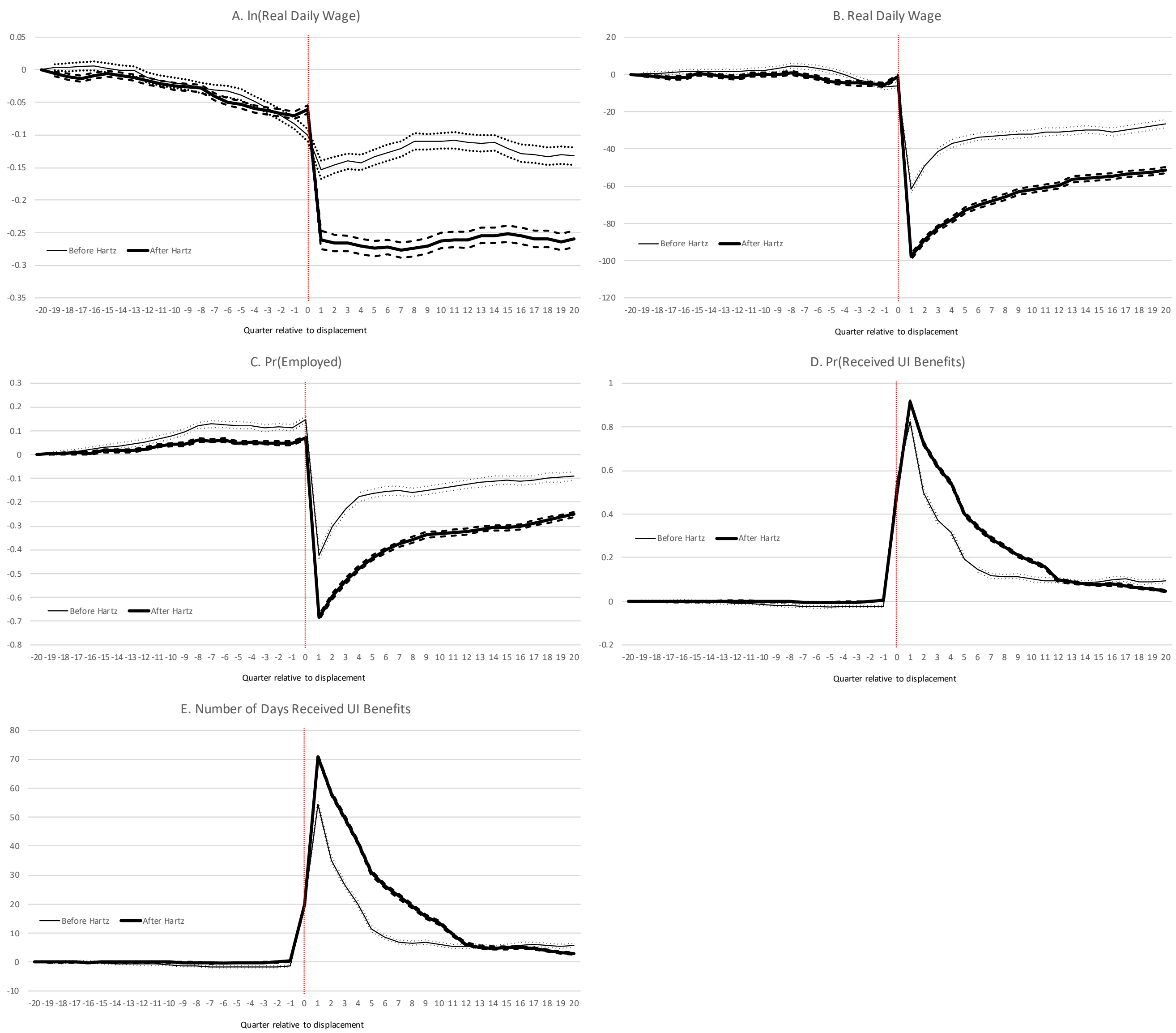
$$
\begin{aligned}
E\left[\hat{\delta}^{**} - \hat{\delta}_{\psi}^{**} - \hat{\delta}_{\phi}^{**}\right] = \delta &- \left(\mathbf{D}'\mathbf{M}_{[\mathbf{Z}\,\mathbf{P}]}\mathbf{D}\right)^{-1}\mathbf{D}'\mathbf{M}_{[\mathbf{Z}\,\mathbf{P}]}\mathbf{F}\left(\mathbf{F}'\mathbf{M}_{[\mathbf{Z}\,\mathbf{P}]}\mathbf{F}\right)^{\mathbf{-1}}\mathbf{F}'\mathbf{M}_{[\mathbf{Z}\,\mathbf{P}]}\left(\mathbf{D}\delta + \mathbf{G}\phi\right) \\
&- \left(\mathbf{D}'\mathbf{M}_{[\mathbf{Z}\,\mathbf{P}]}\mathbf{D}\right)^{-1}\mathbf{D}'\mathbf{M}_{[\mathbf{Z}\,\mathbf{P}]}\mathbf{G}\left(\mathbf{G}'\mathbf{M}_{[\mathbf{Z}\,\mathbf{P}\,\mathbf{F}]}\mathbf{G}\right)^{\mathbf{-1}}\mathbf{G}'\mathbf{M}_{[\mathbf{Z}\,\mathbf{P}\,\mathbf{F}]}\mathbf{D}\delta.
\end{aligned}
$$

Figure 1: Displacement Rates and Mean Log Wage Gap



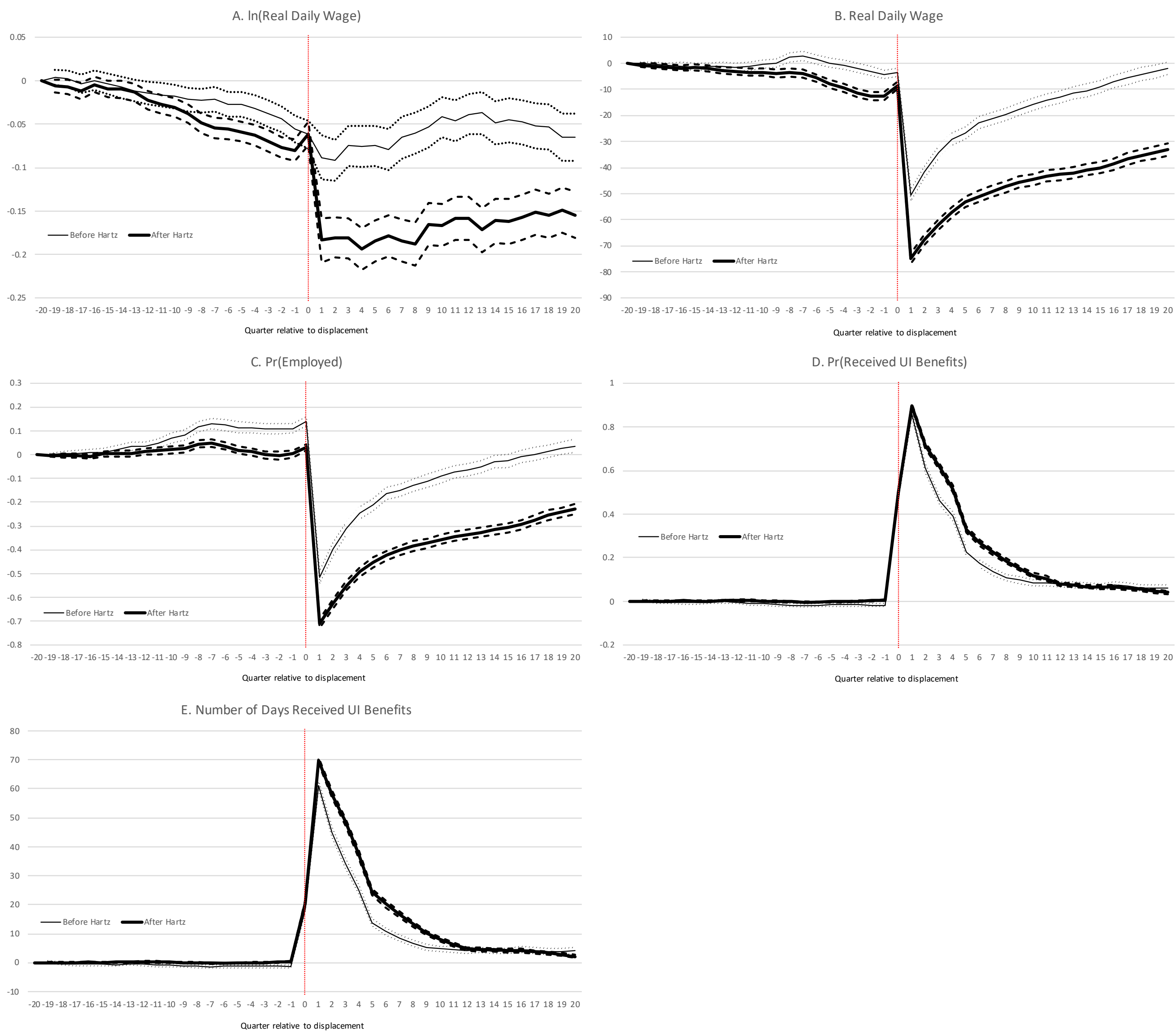
Notes: The red line in each panel shows the proportion of individuals in our estimation sample who were displaced from employment in the preceding four quarters ("recently displaced"). The solid black line in each panel shows the difference between mean log wages of recently displaced workers and all others, normalized to zero in 1999. The dotted black lines indicate 95 confidence intervals, clustered at the individual level. The shaded area indicates the period during which Hartz I-IV was implemented.

Figure 2: Change in Men's Outcomes Due to Displacement, Before vs. After the Hartz Reforms



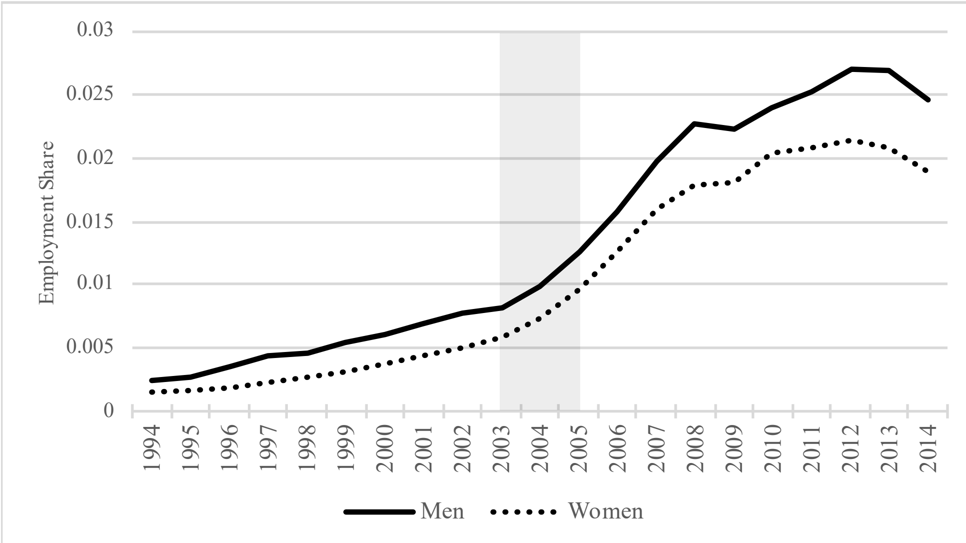
Notes: In each panel we plot estimates of δ_k^{**} from eq. (25). Each panel plots estimates from two regressions. Estimates marked "Before Hartz" are estimated on a treatment group of men displaced from employment in 1998 or 1999 who had at least 24 months tenure with their employer at the time of displacement and met our other sample restrictions; and a control group of men who were never displaced between 1993 and 2014, had at least 24 months tenure with their employer between 1998 and 1999 and met our other sample restrictions. Estimates marked "After Hartz" are estimated on a similarly defined treatment group of individuals displaced in 2005 or 2006, and comparable control group. Dotted and dashed lines indicate 95% confidence intervals, clustered by individual. The outcome in Panel A is the logarithm of real daily wages, excluding zeros, resulting in an unbalanced panel. The outcome in Panel B is real daily wages (2010 Euros); quarters in which individuals are unemployed or missing from the LIAB are set to zero and included in the regressions. The outcome in Panel C is an indicator for employment; quarters in which individuals have no employment record in the LIAB are set to zero and included in the regressions. The outcome in Panel D is an indicator for collecting unemployment benefits at any time during the quarter, and the outcome in Panel E is the number of days that the individual collected unemployment benefits; quarters in which individuals are missing from the LIAB are set to zero and included in the regressions. In Panel A, N = 6,139,024 in the Before Hartz regression and N = 6,266,989 in the After Hartz regression. In the other panels, N = 6,871,067 in the Before Hartz regression and N = 7,202,716 in the After Hartz regression. All regressions control for a cubic in age, age interacted with education, and individual and quarter fixed effects.

Figure 3: Change in Women's Outcomes Due to Displacement, Before vs. After the Hartz Reforms



Notes: In each panel we plot estimates of δ_k^{**} from eq. (25). Each panel plots estimates from two regressions. Estimates marked "Before Hartz" are estimated on a treatment group of women displaced from employment in 1998 or 1999 who had at least 24 months tenure with their employer at the time of displacement and met our other sample restrictions; and a control group of women who were never displaced between 1993 and 2014, had at least 24 months tenure with their employer between 1998 and 1999 and met our other sample restrictions. Estimates marked "After Hartz" are estimated on a similarly defined treatment group of individuals displaced in 2005 or 2006, and comparable control group. Dotted and dashed lines indicate 95% confidence intervals, clustered by individual. The outcome in Panel A is the logarithm of real daily wages, excluding zeros, resulting in an unbalanced panel. The outcome in Panel B is real daily wages (2010 Euros); quarters in which individuals are unemployed or missing from the LIAB are set to zero and included in the regressions. The outcome in Panel C is an indicator for employment; quarters in which individuals have no employment record in the LIAB are set to zero and included in the regressions. The outcome in Panel D is an indicator for collecting unemployment benefits at any time during the quarter, and the outcome in Panel E is the number of days that the individual collected unemployment benefits; quarters in which individuals are missing from the LIAB are set to zero and included in the regressions. In Panel A, N = 1,813,058 in the Before Hartz regression and N = 1,896,463 in the After Hartz regression. In the other panels, N = 2,408,586 in the Before Hartz regression and N = 2,554,218 in the After Hartz regression. All regressions control for a cubic in age, age interacted with education, and individual and quarter fixed effects.

Figure 4: Employment in "Labour Recruitment and Personnel Provision" Sector



Notes: The figure plots the share of employment in sector 74.5, "Labour recruitment and personnel provision," in our estimation sample between 1994 and 2014. See notes to Table 2 for definition of our estimation sample.

Table 1
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
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

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 2
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
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 3
Estimated Effect of the Hartz Reforms on Wages of Recently Displaced Workers

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
			Decomposition 1			Decomposition 2		
	Gross Effect	Net Effect	Selection	Sorting	Matching	Selection	Sorting	Matching
Panel A: Full-time Men (N = 28,236,539)								
Recently displaced	-0.235***	-0.003***	-0.154***	-0.074***	-0.003***	-0.155***	-0.071***	-0.005***
	(0.002)	(0.001)	(0.005)	(0.004)	(0.001)	(0.005)	(0.004)	(0.001)
Recently displaced × during Hartz	-0.074***	-0.024***	0.022***	-0.069***	-0.003***	0.023***	-0.069***	-0.004***
	(0.003)	(0.002)	(0.004)	(0.003)	(0.001)	(0.004)	(0.003)	(0.001)
Recently displaced × after Hartz	-0.143***	-0.032***	0.018***	-0.121***	-0.007***	0.022***	-0.120***	-0.013***
	(0.003)	(0.002)	(0.004)	(0.004)	(0.001)	(0.004)	(0.004)	(0.001)
R-squared	0.342	0.895	0.394	0.031	0.001	0.763	0.030	
RMSE of Residual	0.346	0.142	0.264	0.186	0.054	0.264	0.185	
Panel B: Full-time Women (N = 7,986,586)								
Recently displaced	-0.255***	-0.025***	-0.119***	-0.109***	-0.002***	-0.119***	-0.108***	-0.004***
	(0.004)	(0.002)	(0.008)	(0.008)	(0.001)	(0.009)	(0.008)	(0.001)
Recently displaced × during Hartz	-0.044***	-0.002	0.001	-0.041***	-0.002	0.002	-0.040***	-0.003
	(0.007)	(0.004)	(0.007)	(0.008)	(0.001)	(0.007)	(0.008)	(0.002)
Recently displaced × after Hartz	-0.051***	0.000	0.000	-0.044***	-0.006***	0.003	-0.042***	-0.012***
	(0.006)	(0.003)	(0.007)	(0.008)	(0.001)	(0.007)	(0.007)	(0.002)
R-squared	0.203	0.889	0.291	0.025	0.001	0.794	0.025	
RMSE of Residual	0.389	0.150	0.303	0.258	0.054	0.303	0.258	
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, eqs. (3) and (26). Column (2) reports OLS estimates of eqs. (5) and (27). Columns (3), (4), and (5) 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 (6) and (7) 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 (8) reports the difference between estimated coefficients in the AKM specification eq. (15) and eq. (5). See Table E.1 for estimates of eq. (15). 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 (3), (4), (5), (6), (7), and (8) 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 2 for the number of observations, workers, establishments, and matches in the largest connected sets of men and women, and additional information about sample composition.

Table 4
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 (N = 28,236,539)						
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 (N = 7,986,586)						
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 2 for the number of observations, workers, establishments, and matches in the largest connected sets of men and women, and additional information about sample composition.

Table 5
Decomposition Estimates Based on a Stricter Definition of Involuntary Displacement due to Establishment Closure

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				Decomposition 1			Decomposition 2	
	Gross Effect	Net Effect	Selection	Sorting	Matching	Selection	Sorting	Matching
Panel A: Full-time Men (N = 27,942,825)								
Recently displaced	-0.258***	-0.004	-0.162***	-0.090***	-0.002	-0.164***	-0.088***	-0.003
	(0.007)	(0.005)	(0.007)	(0.007)	(0.002)	(0.007)	(0.008)	(0.003)
Recently displaced × during Hartz	-0.050***	-0.011	0.018	-0.049***	-0.009**	0.019	-0.047***	-0.012**
	(0.013)	(0.008)	(0.014)	(0.012)	(0.004)	(0.014)	(0.012)	(0.006)
Recently displaced × after Hartz	-0.148***	-0.024***	0.002	-0.117***	-0.008**	0.005	-0.115***	-0.013**
	(0.011)	(0.006)	(0.010)	(0.011)	(0.004)	(0.010)	(0.011)	(0.006)
R-squared	0.338	0.894	0.393	0.025	0.001	0.765	0.024	
RMSE of Residual	0.346	0.142	0.265	0.186	0.054	0.264	0.185	
Panel B: Full-time Women (N = 7,853,530)								
Recently displaced	-0.295***	-0.049***	-0.143***	-0.098***	-0.005	-0.142***	-0.096***	-0.007
	(0.018)	(0.009)	(0.019)	(0.018)	(0.003)	(0.020)	(0.018)	(0.005)
Recently displaced × during Hartz	-0.038	0.035***	-0.015	-0.058	0.000	-0.016	-0.056	-0.001
	(0.032)	(0.015)	(0.045)	(0.037)	(0.007)	(0.045)	(0.036)	(0.012)
Recently displaced × after Hartz	-0.088***	0.015	-0.016	-0.086***	-0.001	-0.015	-0.085***	-0.002
	(0.025)	(0.013)	(0.023)	(0.023)	(0.004)	(0.023)	(0.023)	(0.008)
R-squared	0.200	0.887	0.289	0.021	0.001	0.793	0.021	
RMSE of Residual	0.388	0.150	0.304	0.259	0.053	0.304	0.258	
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, eqs. (3) and (26). Column (2) reports OLS estimates of eqs. (5) and (27). Columns (3), (4), and (5) 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 (6) and (7) 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 (8) reports the difference between estimated coefficients in the AKM specification eq. (15) and eq. (5). 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 (3), (4), (5), (6), (7), and (8) 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 3 because the treatment and control groups depend on different definitions of displacement.

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	Net Sorting Effect	Industry Sector	Estab. Cohort	Estab. Size	State	Share Full-Time	Sorting	Net Sorting Effect	Industry Sector	Estab. Cohort	Estab. Size	State	Share Full-Time
Panel A: Full-time Men (N = 28,236,539)														
Recently displaced	-0.074***	0.020***	-0.044***	0.000	-0.046***	-0.001	-0.003***	-0.071***	0.022***	-0.043***	0.000	-0.046***	-0.001	-0.003***
	(0.004)	(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)
Recently displaced × during Hartz	-0.069***	-0.045***	-0.021***	-0.005***	0.003***	0.000	-0.001***	-0.069***	-0.045***	-0.021***	-0.005***	0.003***	0.000	-0.001***
	(0.003)	(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)
Recently displaced × after Hartz	-0.121***	-0.075***	-0.050***	-0.006***	0.010***	0.000	0.000	-0.120***	-0.073***	-0.050***	-0.007***	0.009***	0.000	0.000
	(0.004)	(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)
R-squared	0.031	0.527	0.020	0.013	0.038	0.003	0.095	0.030	0.525	0.020	0.014	0.038	0.003	0.095
RMSE of Residual	0.186	0.130	0.107	0.023	0.036	0.011	0.024	0.185	0.130	0.106	0.023	0.035	0.011	0.024
Panel B: Full-time Women (N = 7,986,586)														
Recently displaced	-0.109***	-0.008	-0.053***	-0.002	-0.044***	-0.003**	0.000	-0.108***	-0.007	-0.053***	-0.002	-0.044***	-0.003**	0.000
	(0.008)	(0.006)	(0.003)	(0.001)	(0.002)	(0.001)	(0.000)	(0.008)	(0.006)	(0.003)	(0.001)	(0.002)	(0.001)	(0.000)
Recently displaced × during Hartz	-0.041***	-0.027***	-0.014***	-0.003***	0.002	0.000	0.000	-0.040***	-0.026***	-0.014***	-0.003***	0.002***	0.000	0.000
	(0.008)	(0.007)	(0.003)	(0.001)	(0.002)	(0.001)	(0.001)	(0.008)	(0.007)	(0.003)	(0.001)	(0.002)	(0.001)	(0.001)
Recently displaced × after Hartz	-0.044***	-0.033***	-0.020***	-0.003***	0.008***	0.001*	0.002***	-0.042***	-0.030***	-0.019***	-0.003***	0.008***	0.001*	0.002***
	(0.008)	(0.007)	(0.003)	(0.001)	(0.002)	(0.001)	(0.000)	(0.008)	(0.007)	(0.003)	(0.001)	(0.002)	(0.001)	(0.000)
R-squared	0.025	0.374	0.027	0.011	0.031	0.003	0.084	0.025	0.371	0.027	0.011	0.031	0.003	0.084
RMSE of Residual	0.258	0.207	0.111	0.028	0.062	0.018	0.027	0.258	0.207	0.110	0.028	0.062	0.019	0.027
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 (4) and (7), respectively, of Table 3. Columns (2) and (9) 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. Columns (3)-(7) report estimates of the Gelbach decomposition of the difference between columns (1) and (2). Columns (10)-(14) report estimates of the Gelbach decomposition of the difference between columns (8) and (9). 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 2 for the number of observations, workers, establishments, and matches in the largest connected sets of men and women, and additional information about sample composition.

Table 7
Top Pre- and Post-Displacement Sectors, 1998-2009

Pre-Displacement Sector	(1) Share	(2) Estab. Effect	Post-Displacement Sector	(3) Share	(4) Estab. Effect
Panel A: Men 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
Panel B: Men 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
Panel C: Women 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
Panel D: Women 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

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 establishments. Panel A is based on 15,647 displacements and 11,157 post-displacement jobs held by men between 1998 and 2001. Panel B is based on 44,360 displacements and 21,890 post-displacement jobs held by men between 2005 and 2009. Panel C is based on 5,723 displacements and 3,996 post-displacement jobs held by women between 1998 and 2001, and Panel D is based on 15,536 displacements and 8,708 post-displacement jobs held by women between 2005 and 2009 See notes to Table 2 for information about sample composition.

Online Appendix to:

The Determinants of Displaced Workers' Wages: Sorting, Matching, Selection, and the Hartz Reforms

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Appendix C Data Appendix

C.1 Overview of the LIAB and data processing

The LIAB sample frame is based on the IAB Establishment Panel, which is a representative sample of German establishments. Establishments 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 are called "panel cases." 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 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 employment records cover all of an individual's employment spells between 1993 and 2014.

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 in that year (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.

Table C.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 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. In line with Card et al. (2013), wage dispersion increased for both men and women between 1993 and 2010, though that trend appears to have reversed in the last few years of our sample.

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-employer-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-employer-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-employer-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.

C.2 Imputing Censored Wages

A limitation of wage data based on the IEB, including 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 C.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 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. [51]

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, 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 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 C.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

[51] 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 $y^* = \mathbf{x}'\hat{\beta} + \hat{\sigma}\Phi^{-1}\left[q + u(1-q)\right].$

female employees; number of full-time female employees; number of low-, medium- and high-skilled employees; and the median wage of full-time employees);[52] a dummy variable that equals one if the current job was the individual's main job in this calendar year; 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). 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.

Figure C.1 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

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

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 preserves the relative share of wage variation attributable to within-establishment and within-match variation, even at very high censoring rates.

C.3 Other Key Variable Definitions

C.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-employer-year.

C.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-employer-year. We only use this occupation measure for the robustness checks reported in Table E.8.

C.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. 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.

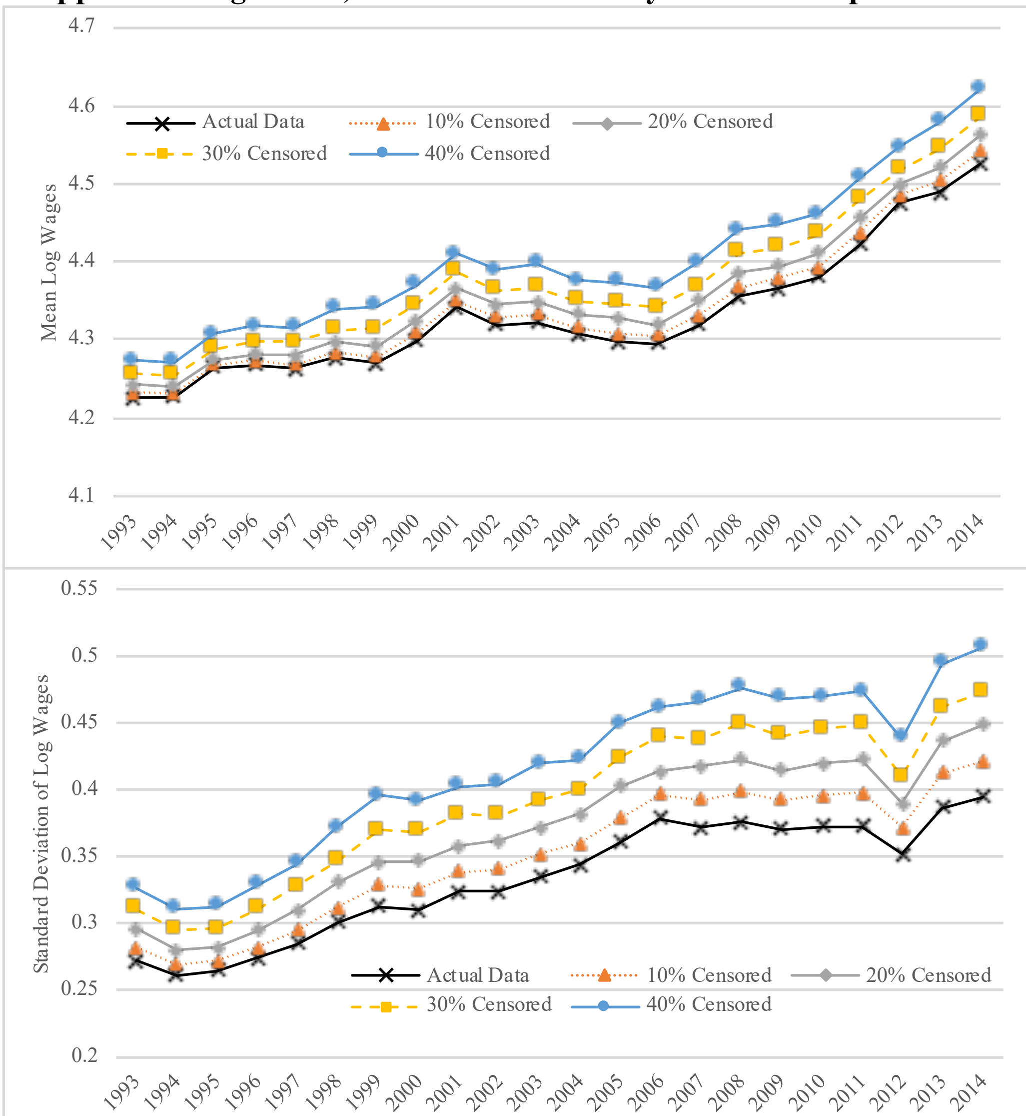
We define an individual as recently displaced in quarter t if they were displaced from employment in the preceding m quarters. In our main analysis, we set $m = 4$. In robustness checks, we relax this definition and estimate specifications for $m = 8, 12, 20$. As shown in Table C.2, the characteristics of recently displaced workers are similar under all of these definitions. 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 for any specification.

C.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 60 months.

Figure C.1: 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.

Table C.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 C.2
Summary Statistics: Recently Displaced Workers vs. Others

	(1)	(2)	(3)	Education (%) (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.

Appendix D The Hartz Reforms

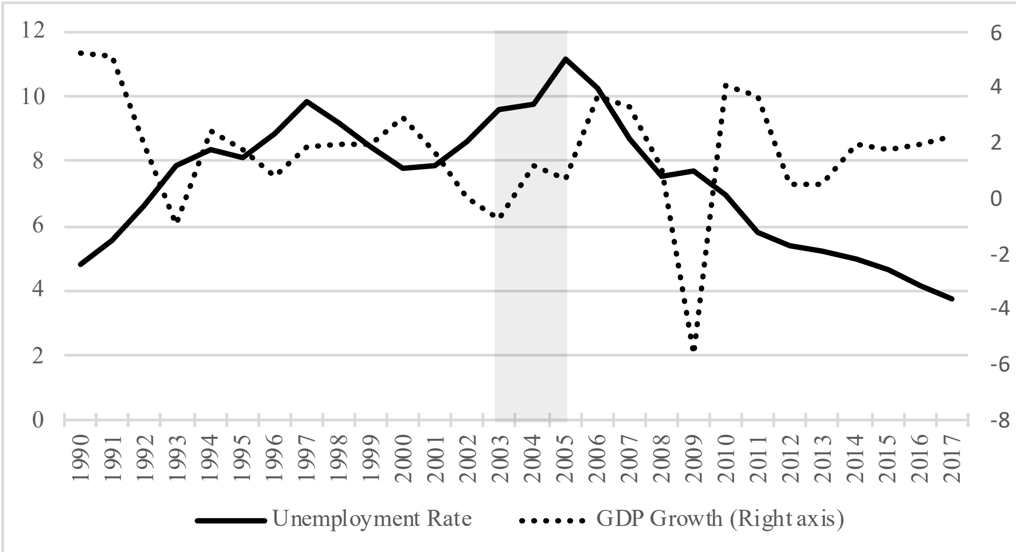
Following reunification, the German economy entered an extended period of slow growth and increasing unemployment (Figure D.1). Pressure for reform led to the creation of the Hartz Commission in 2002, which was tasked with proposing reforms to labour market institutions. The Commission's recommendations 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 Hartz IV reforms came into effect on January 1 2005 and were targeted specifically 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 at least 12 months of employment experience in the preceding three years 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.

Figure D.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.

Appendix E Robustness

In this Appendix we present additional estimation results that support the analysis in the main text, and demonstrate the robustness of our main estimates to alternative specifications, sample restrictions, and definitions of key variables.

E.1 Event Studies

We establish the robustness of our event study estimates in two ways. First, we re-estimate our event study specifications on treatment and control groups that satisfy a stricter 60-month minimum tenure requirement, which is at the upper end of the range found in the literature. Figures E.1 and E.2 plot the estimates; they are very similar to Figures 2 and 3. The most notable difference is that the estimated pre-displacement gaps are generally closer to zero, suggesting that the 60 month tenure restriction yields treatment and control groups that are more similar prior to displacement. Perhaps for that reason, the estimated post-displacement gaps are slightly larger than in our main sample, and expand slightly further following the reforms. Second, we re-estimate our event studies on an alternative treatment group of workers displaced due to establishment closure. This alternative definition of displacement, which we discuss in greater detail in Section 3.5 below, is more similar to studies that define displacement using mass-displacement events. Estimates for this treatment group are plotted in Figures E.3 and E.4. They are similar to Figures 2 and 3, but are much less precisely estimated because only about one tenth of displacements in our data coincide with establishment closure.[53]

E.2 Selection into Displacement and Re-employment

A possible concern is that the small post-reform selection effects we have estimated might be the net result of several offsetting factors that determine which workers were selected into displacement, and which were selected into re-employment within four quarters. On the one hand, we know that German firms increasingly outsourced workers in low-skilled occupations over this period (Goldschmidt and Schmieder, 2017) which may have changed which workers were selected into layoff. At

[53]The Figure 2 and 3 event studies are estimated on treatment groups of 14,630 displaced men and 6,591 women, whereas Figures E.3 and E.4 are estimated on treatment groups comprising 1,697 men and 667 women displaced due to establishment closure.

the same time, less generous UI benefits after the reforms gave workers incentives to return to work more quickly (Schmieder et al., 2016) which may have changed which workers were selected into re-employment and the duration of their unemployment spells.

To investigate this further, Figure E.5 plots the incidence of re-employment within 4 quarters by displacement quarter. It indicates that individuals returned to work more slowly following the reforms, which is consistent with the event study estimates. The proportion of men that return to work within four quarters declined from about 49 percent in the four years prior to the reforms to 41 percent in the four years after, and the average number of quarters worked in the four quarters after displacement declined from 1.4 to 1.1. We see similar, though slightly smaller, declines for women. Figure E.6 plots average individual fixed effects, $\hat{\theta}_i$, of workers displaced in each quarter. These clearly increase after the reforms, indicating that individuals with increasingly valuable unmeasured skills were being selected into displacement. In the case of men, however, this looks to be part of a trend that predates the reforms. Also plotted in Figure E.6 is the difference between the average value of $\hat{\theta}_i$ among displaced workers who return to work within four quarters, and the average $\hat{\theta}_i$ among all workers displaced that quarter. This difference is small and positive for men, indicating that those who return to work within four quarters are positively selected from the pool of displaced, and this relationship is very stable over time. The difference among women after the reforms is slightly larger than among men, indicating slightly stronger positive selection among those who returned to work quickly. However, the precisely estimated zero selection effect in Table 3 implies that this stronger positive selection into re-employment mirrored an equal increase in the value of unmeasured skills among their non-displaced counterparts.

E.3 Additional Robustness Checks

In Tables E.2 and E.3, we present additional 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 3 though generally slightly smaller in magnitude. The fact that the wage effects of displacement vary so little depending on whether we use the 4, 8, 12, or 20 quarter measure aligns with what we found in the event study, namely that the wage losses following displacement are highly persistent both before and after the reforms.

In Table E.4 we tighten our tenure restriction from 24 months to 60 months. Again, this has no meaningful effect on our estimates for men except to slightly reduce the magnitude of some parameter estimates. For women, the stricter tenure requirement yields estimates more similar to what we observe for men following the reforms: a slightly larger wage decline, more positive selection into displacement, and a stronger sorting effect.

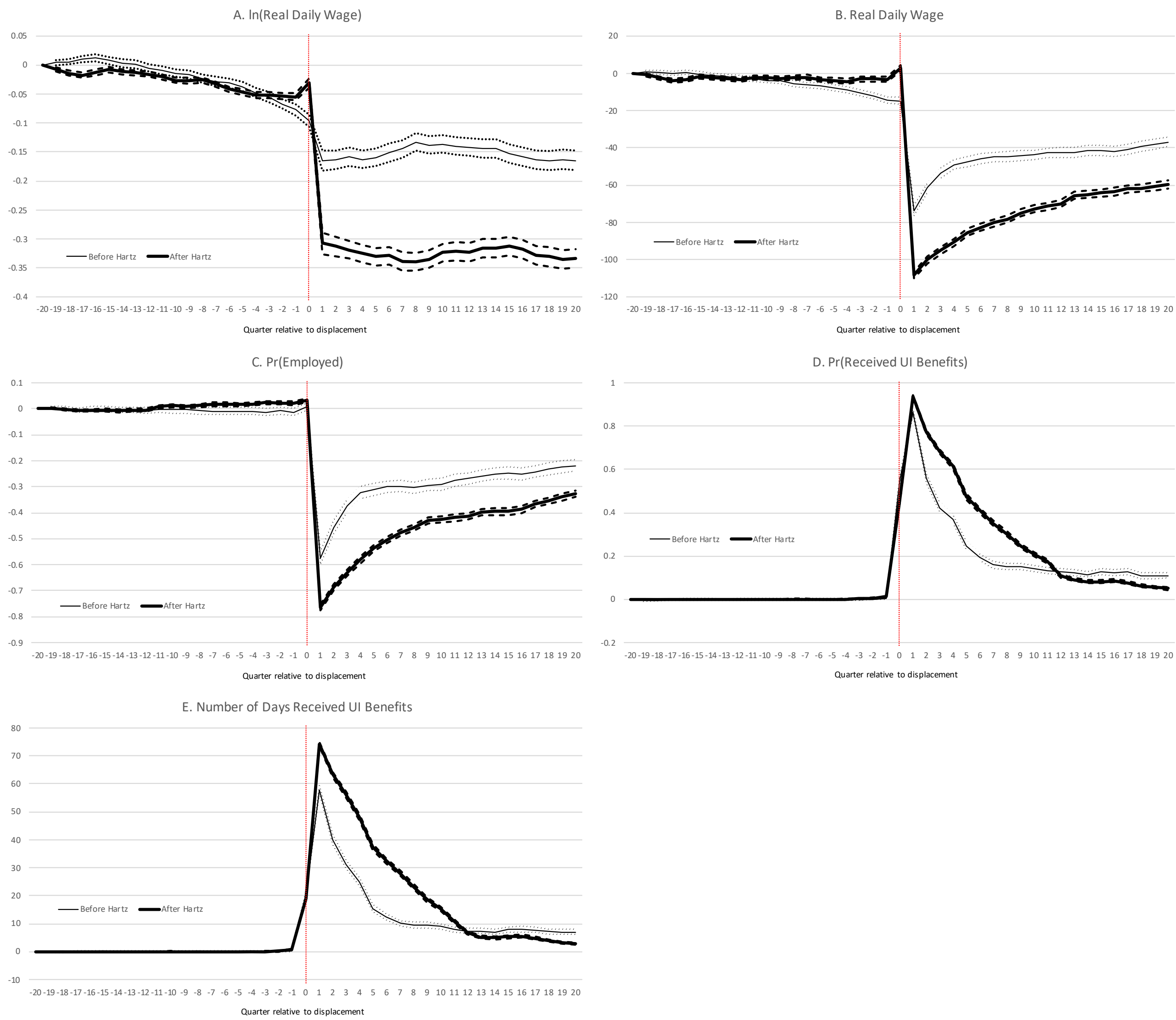
A possible source of concern is that estimates in Table 3 capture not only the effect of the Hartz reforms, but also of the subsequent financial crisis. To address this concern, we restrict our sample to the period 1993-2008 and re-estimate our decompositions. The resulting estimates, in Table E.5, are again very similar to those in Table 3. A related concern is that the early part of our sample period could be influenced by labor market dislocation in the early years following re-unification. To address this concern and concerns about the effects of the financial crisis simultaneously, we further restrict our sample to the period 1998-2008. The results, in Table E.6, are again very similar to those in Table 3. The only notable difference is that the estimated post-reform wage decline is somewhat smaller for recently displaced 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 wage decline). The reverse is true for recently displaced women: the estimated post-reform wage decline 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 decline because the estimated selection effect is now small and positive, though it remains statistically insignificant. 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 influenced by different pre-policy trends for displaced and non-displaced workers, we replace the vector of unrestricted time effects that are common to both groups with separate linear time trends for displaced and non-displaced workers. The resulting estimates are presented in Table E.7. Again, this has little effect on estimates. The specification with linear trends yields a smaller decline in displaced men's wages following the introduction of the Hartz reforms, roughly 8 log points vs. 14 log points in Table 3. The sorting effect is about the same magnitude, while the selection and matching effects are somewhat larger than in Table 3. The overall pattern, that sorting into lower-paying establishments accounts for the lion's share of the post-reform wage decline, while matching plays a smaller role and selection works in the opposite direction, remains unchanged. For recently displaced women, the specification with linear trends yields a substantially larger estimate of the wage decline following the reforms: 15.7 log points vs. 5.1 log points in Table 3. 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 3). 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 34, though the magnitudes are somewhat larger and more similar in magnitude to what we observe for men.

A final potential source of concern is that our results could be driven by important omitted variables. Table E.8 presents estimates from an alternative specification that addresses such concerns. Specifically, our Table 3 estimates do 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 omitted variables that vary at the level of the worker-establishment match, such as occupation. In Table E.8, 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). Estimates of that specifications are presented in column 2. Although these controls substantially reduce the estimated pre-reform wage effect 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 decline in wages. That is, the post-reform decline in displaced workers' wages remains largely unexplained, in sharp contrast to our estimates in column 2 of Table 3. Thus our main results are clearly not an artifact of failing to adequately control for tenure, sector, or occupation in the baseline specification.

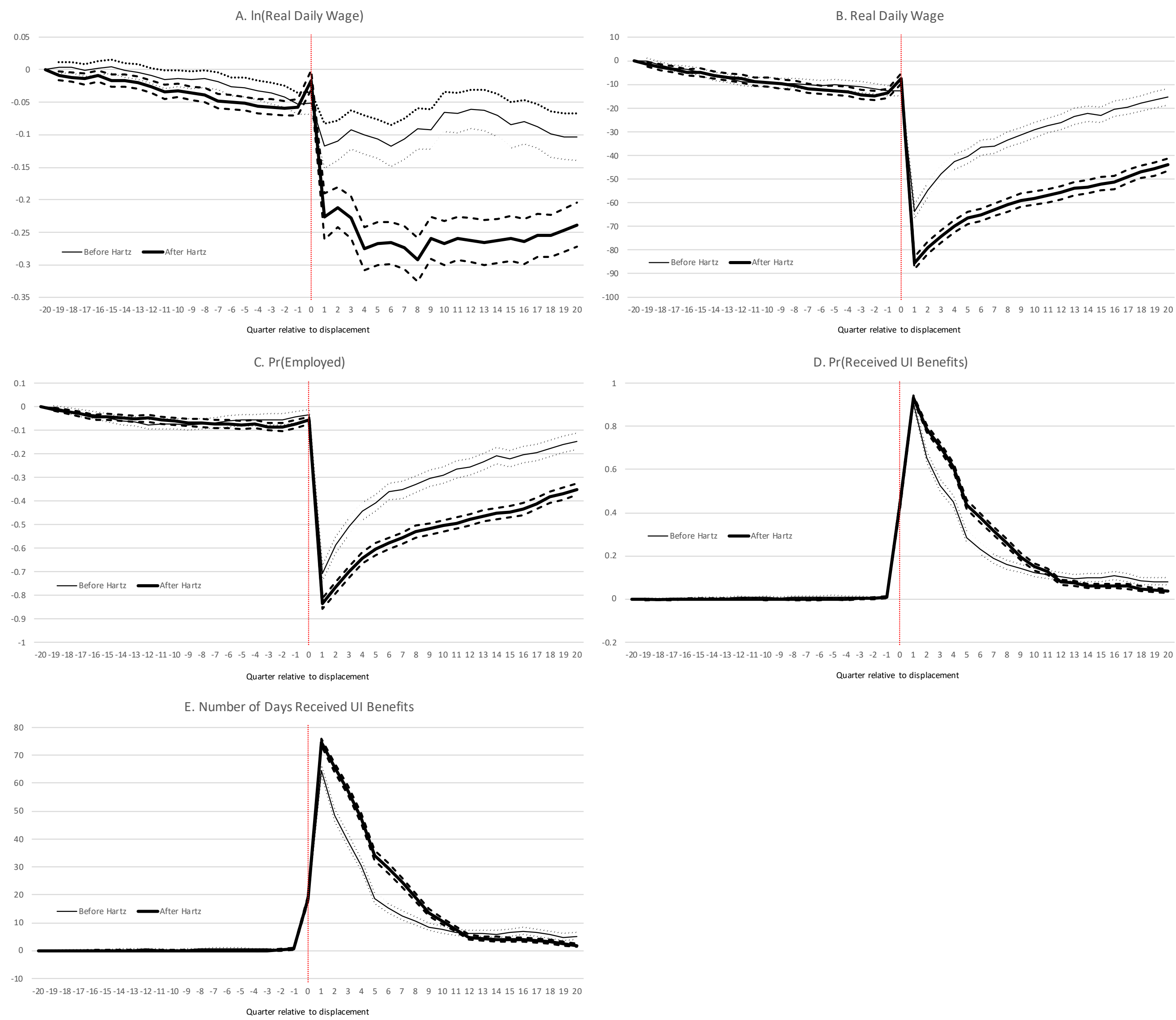
In columns 3-5 of Table E.8 we perform a simple Gelbach (2016) decomposition to assess the relative importance of the additional controls in explaining the reforms' effect on displaced workers' wages. Of these, industry sector is most important, explaining 4.9 log points (34 percent) of displaced men's post-reform wage decline, and 1.7 log points (31 percent) for women. This indicates that our estimated sorting effect partly reflects differences in the way that displaced and non-displaced workers are sorted across sectors.

Figure E.1: Change in Men's Outcomes Due to Displacement, Before vs. After the Hartz Reforms



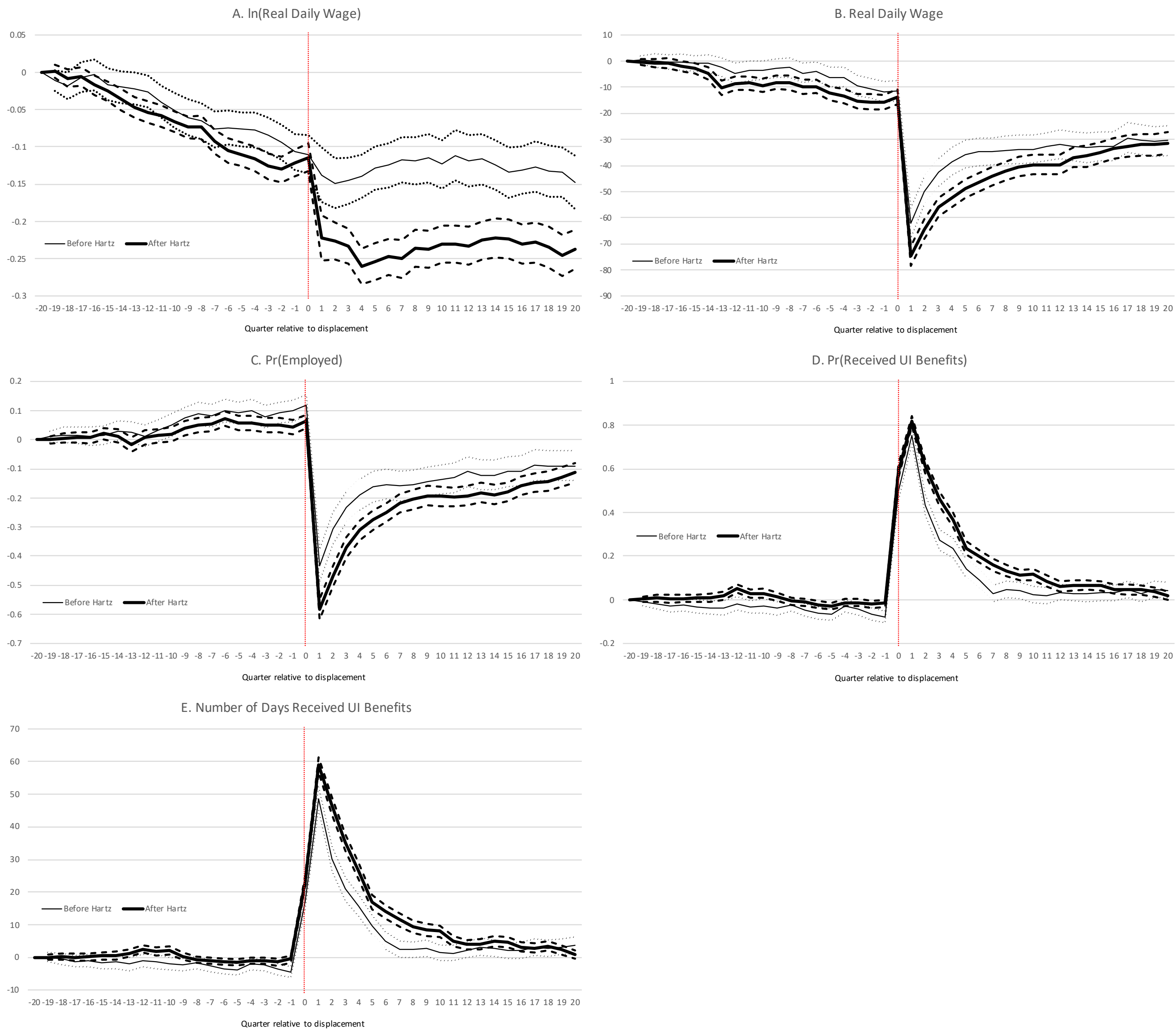
Notes: In each panel we plot estimates of δ_k^{**} from eq. (25). Each panel plots estimates from two regressions. Estimates marked "Before Hartz" are estimated on a treatment group of men displaced from employment in 1998 or 1999 who had at least 60 months tenure with their employer at the time of displacement and met our other sample restrictions; and a control group of men who were never displaced between 1993 and 2014, had at least 60 months tenure with their employer between 1998 and 1999 and met our other sample restrictions. Estimates marked "After Hartz" are estimated on a similarly defined treatment group of individuals displaced in 2005 or 2006, and comparable control group. Dotted and dashed lines indicate 95% confidence intervals, clustered by individual. The outcome in Panel A is the logarithm of real daily wages, excluding zeros, resulting in an unbalanced panel. The outcome in Panel B is real daily wages (2010 Euros); quarters in which individuals are unemployed or missing from the LIAB are set to zero and included in the regressions. The outcome in Panel C is an indicator for employment; quarters in which individuals have no employment record in the LIAB are set to zero and included in the regressions. The outcome in Panel D is an indicator for collecting unemployment benefits at any time during the quarter, and the outcome in Panel E is the number of days that the individual collected unemployment benefits; quarters in which individuals are missing from the LIAB are set to zero and included in the regressions. In Panel A, N = 5,393,557 in the Before Hartz regression and N = 5,770,909 in the After Hartz regression. In the other panels, N = 5,889,076 in the Before Hartz regression and N = 6,376,812 in the After Hartz regression. All regressions control for a cubic in age, age interacted with education, and individual and quarter fixed effects.

Figure E.2: Change in Women's Outcomes Due to Displacement, Before vs. After the Hartz Reforms



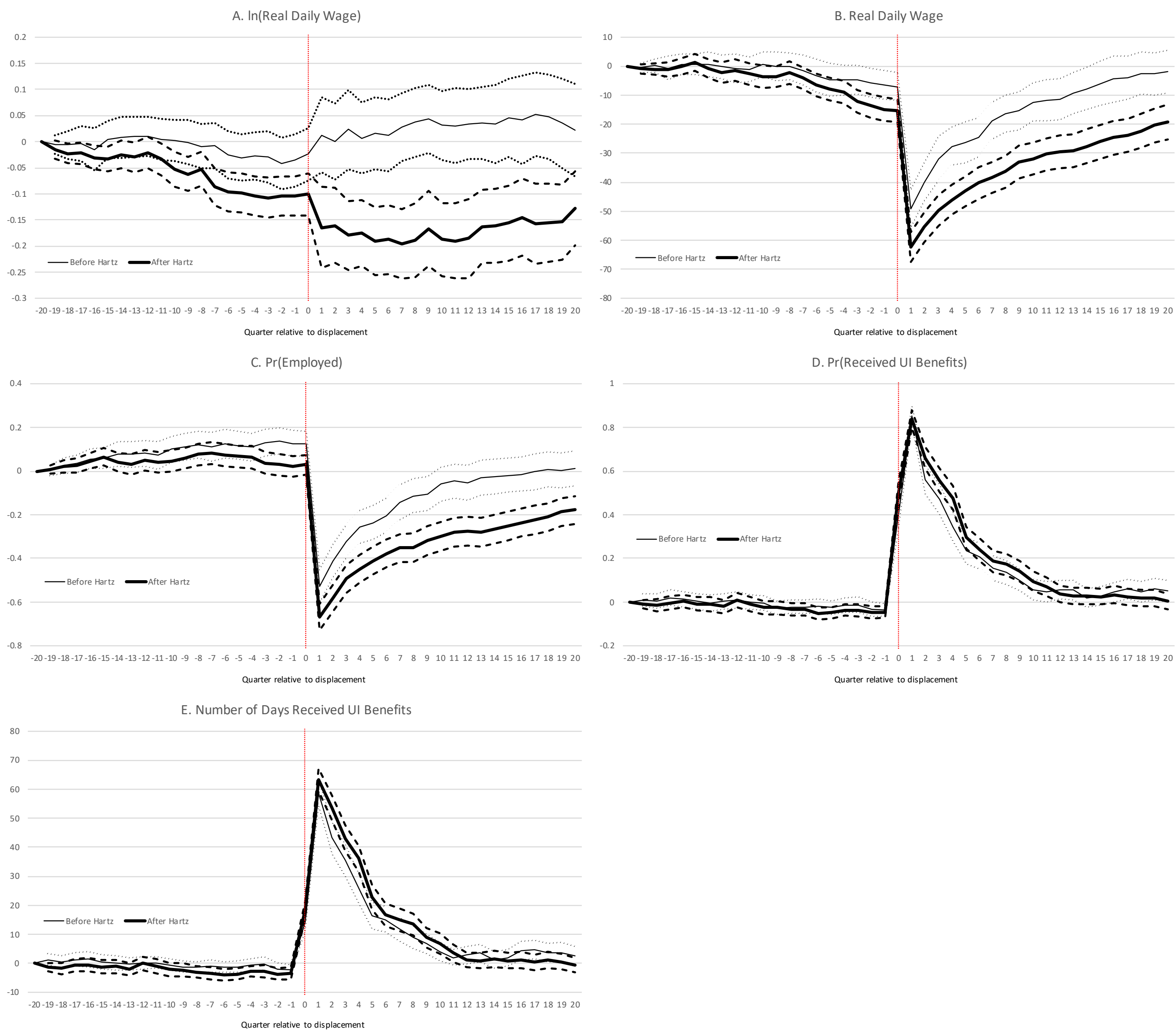
Notes: In each panel we plot estimates of δ_k^{**} from eq. (25). Each panel plots estimates from two regressions. Estimates marked "Before Hartz" are estimated on a treatment group of women displaced from employment in 1998 or 1999 who had at least 60 months tenure with their employer at the time of displacement and met our other sample restrictions; and a control group of women who were never displaced between 1993 and 2014, had at least 60 months tenure with their employer between 1998 and 1999 and met our other sample restrictions. Estimates marked "After Hartz" are estimated on a similarly defined treatment group of individuals displaced in 2005 or 2006, and comparable control group. Dotted and dashed lines indicate 95% confidence intervals, clustered by individual. The outcome in Panel A is the logarithm of real daily wages, excluding zeros, resulting in an unbalanced panel. The outcome in Panel B is real daily wages (2010 Euros); quarters in which individuals are unemployed or missing from the LIAB are set to zero and included in the regressions. The outcome in Panel C is an indicator for employment; quarters in which individuals have no employment record in the LIAB are set to zero and included in the regressions. The outcome in Panel D is an indicator for collecting unemployment benefits at any time during the quarter, and the outcome in Panel E is the number of days that the individual collected unemployment benefits; quarters in which individuals are missing from the LIAB are set to zero and included in the regressions. In Panel A, N = 1,813,058 in the Before Hartz regression and N = 1,896,463 in the After Hartz regression. In the other panels, N = 2,408,586 in the Before Hartz regression and N = 2,554,218 in the After Hartz regression. All regressions control for a cubic in age, age interacted with education, and individual and quarter fixed effects.

Figure E.3: Change in Men's Outcomes Due to Displacement at Closure, Before vs. After the Hartz Reforms



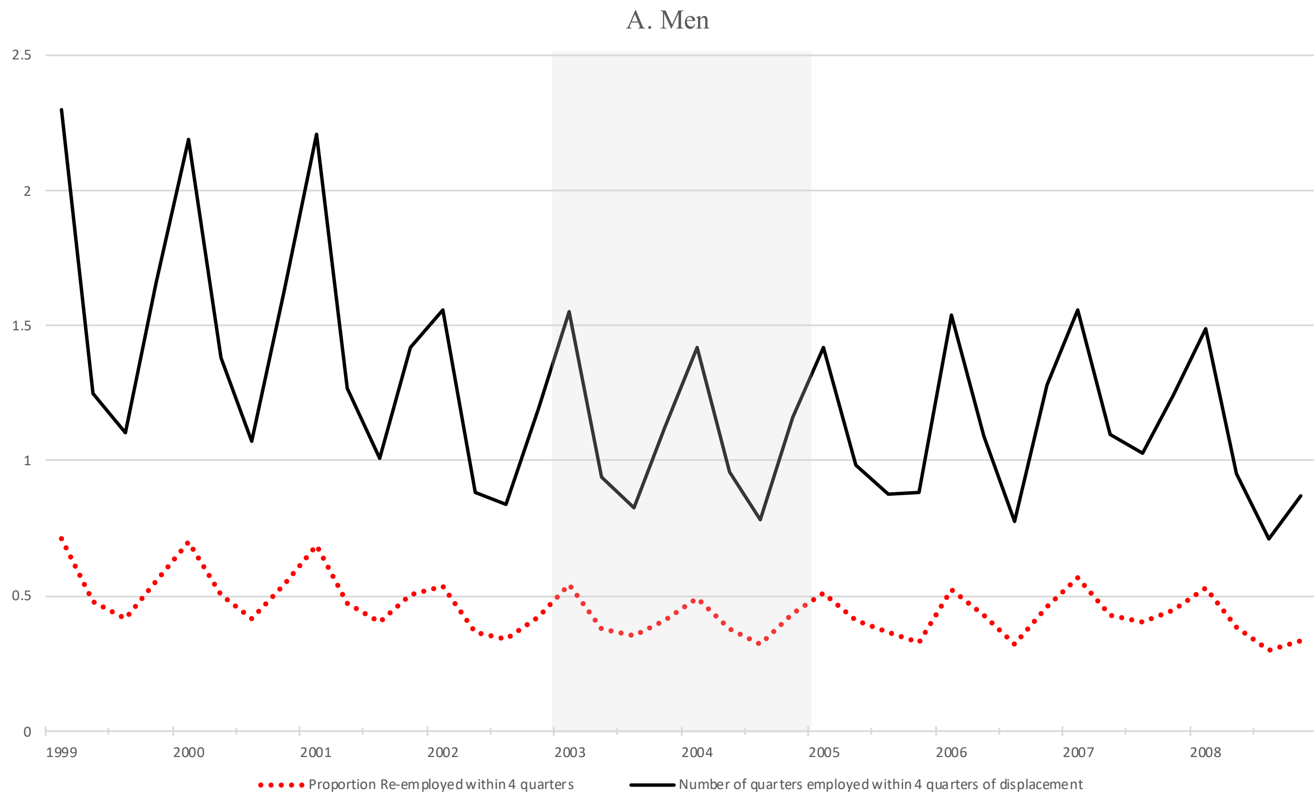
Notes: In each panel we plot estimates of δ_k^{**} from eq. (25). Each panel plots estimates from two regressions. Estimates marked "Before Hartz" are estimated on a treatment group of men displaced from employment in 1998 or 1999 due to establishment closure, who had at least 24 months tenure with their employer at the time of displacement, and met our other sample restrictions; and a control group of men who were never displaced between 1993 and 2014, had at least 24 months tenure with their employer between 1998 and 1999 and met our other sample restrictions. Estimates marked "After Hartz" are estimated on a similarly defined treatment group of individuals displaced in 2005 or 2006, and comparable control group. Dotted and dashed lines indicate 95% confidence intervals, clustered by individual. The outcome in Panel A is the logarithm of real daily wages, excluding zeros, resulting in an unbalanced panel. The outcome in Panel B is real daily wages (2010 Euros); quarters in which individuals are unemployed or missing from the LIAB are set to zero and included in the regressions. The outcome in Panel C is an indicator for employment; quarters in which individuals have no employment record in the LIAB are set to zero and included in the regressions. The outcome in Panel D is an indicator for collecting unemployment benefits at any time during the quarter, and the outcome in Panel E is the number of days that the individual collected unemployment benefits; quarters in which individuals are missing from the LIAB are set to zero and included in the regressions. In Panel A, N = 6,017,679 in the Before Hartz regression and N = 6,037,351 in the After Hartz regression. In the other panels, N = 6,699,769 in the Before Hartz regression and N = 6,843,761 in the After Hartz regression. All regressions control for a cubic in age, age interacted with education, and individual and quarter fixed effects.

Figure E.4: Change in Women's Outcomes Due to Displacement at Closure, Before vs. After the Hartz Reforms



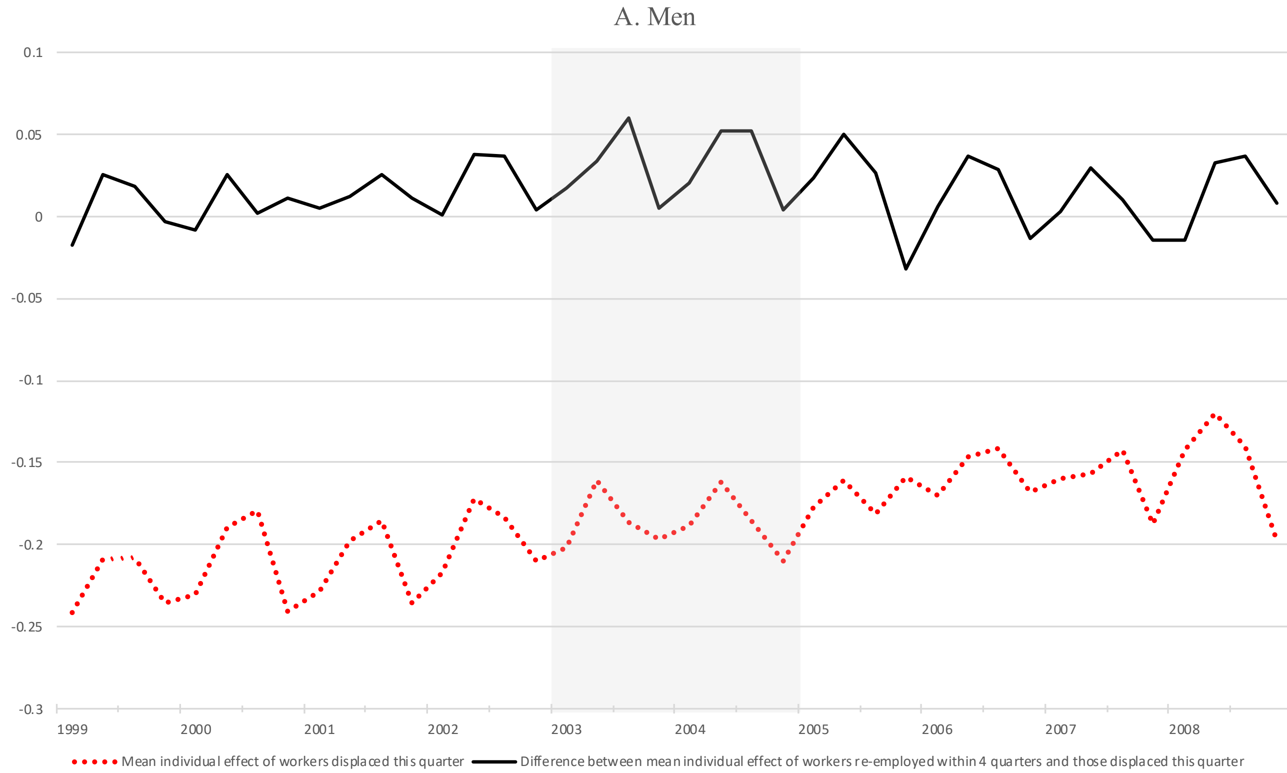
Notes: In each panel we plot estimates of δ_k^{**} from eq. (25). Each panel plots estimates from two regressions. Estimates marked "Before Hartz" are estimated on a treatment group of women displaced from employment in 1998 or 1999 due to establishment closure, who had at least 24 months tenure with their employer at the time of displacement, and met our other sample restrictions; and a control group of women who were never displaced between 1993 and 2014, had at least 24 months tenure with their employer between 1998 and 1999 and met our other sample restrictions. Estimates marked "After Hartz" are estimated on a similarly defined treatment group of individuals displaced in 2005 or 2006, and comparable control group. Dotted and dashed lines indicate 95% confidence intervals, clustered by individual. The outcome in Panel A is the logarithm of real daily wages, excluding zeros, resulting in an unbalanced panel. The outcome in Panel B is real daily wages (2010 Euros); quarters in which individuals are unemployed or missing from the LIAB are set to zero and included in the regressions. The outcome in Panel C is an indicator for employment; quarters in which individuals have no employment record in the LIAB are set to zero and included in the regressions. The outcome in Panel D is an indicator for collecting unemployment benefits at any time during the quarter, and the outcome in Panel E is the number of days that the individual collected unemployment benefits; quarters in which individuals are missing from the LIAB are set to zero and included in the regressions. In Panel A, N = 1,759,107 in the Before Hartz regression and N = 1,811,972 in the After Hartz regression. In the other panels, N = 2,316,418 in the Before Hartz regression and N = 2,403,502 in the After Hartz regression. All regressions control for a cubic in age, age interacted with education, and individual and quarter fixed effects.

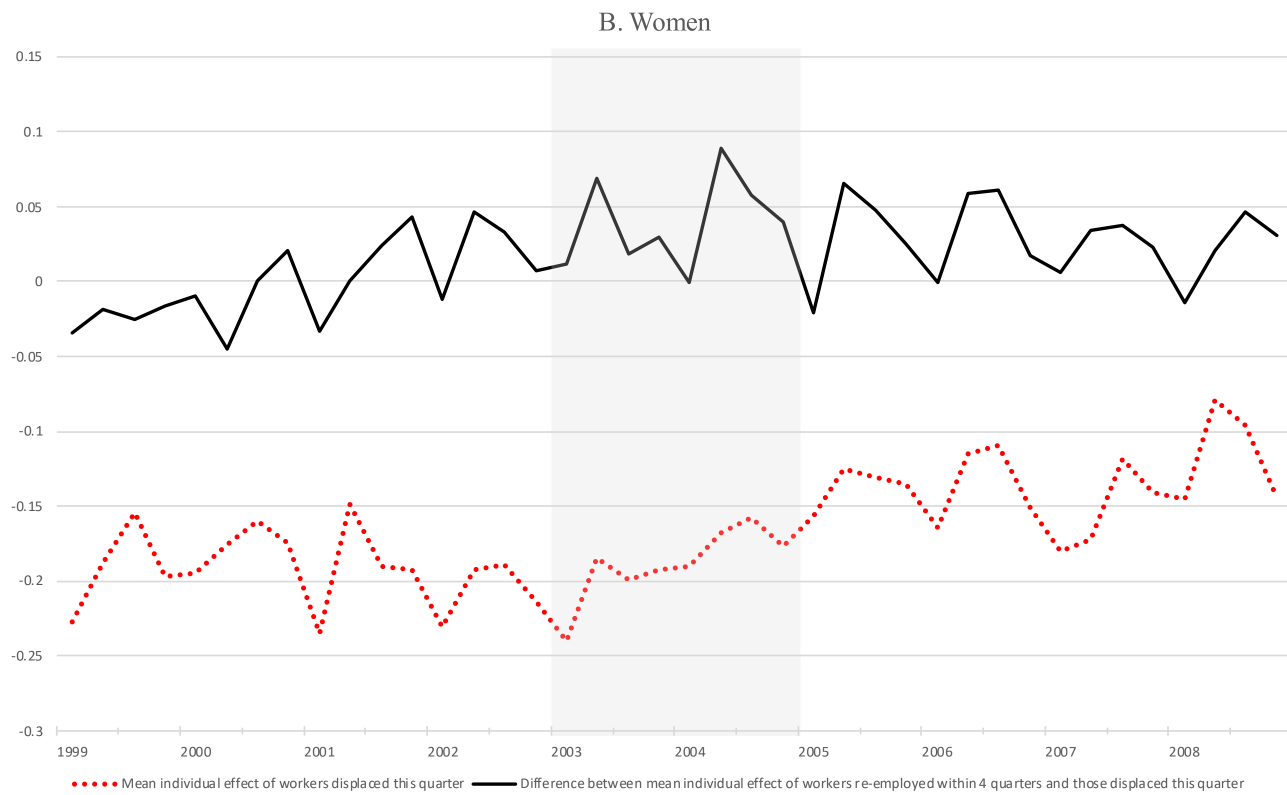
Figure E.5: Incidence of Re-employment Within Four Quarters of Displacement



Notes: The red line in each panel plots the proportion of individuals displaced in each quarter who return to full-time employment at any employer within four quarters of displacement. The black line in each panel plots the average number of quarters worked by each displaced individual in the four quarters following displacement, by displacement quarter. See notes to Table 2 for definition of the estimation sample.

Figure E.6: Mean Individual Fixed Effect of Displaced Workers





Notes: The red line in each panel plots the average individual fixed effect of workers displaced in each quarter. The black line in each panel plots the difference between the average individual fixed effect of displaced workers who return to full-time employment within four quarters, and the average individual fixed effect among all workers displaced that quarter. See notes to Table 2 for definition of the estimation sample.

Table E.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 2 for additional information about sample composition using the four quarter displacement measure.

Table E.2
Decomposition Estimates for Alternate Definitions of Recent Displacement, Full-Time Men

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
			Decomposition 1			Decomposition 2		
	Gross Effect	Net Effect	Selection	Sorting	Matching	Selection	Sorting	Matching
Panel A: 8 quarter displacement measure (*N* = 26,972,184)								
Recently displaced	-0.241***	-0.003***	-0.150***	-0.085***	-0.003***	-0.150***	-0.082***	-0.005***
	(0.002)	(0.001)	(0.003)	(0.003)	(0.000)	(0.003)	(0.003)	(0.001)
Recently displaced × during Hartz	-0.069***	-0.021***	0.022***	-0.067***	-0.003***	0.023***	-0.066***	-0.005***
	(0.003)	(0.002)	(0.003)	(0.003)	(0.001)	(0.003)	(0.003)	(0.001)
Recently displaced × after Hartz	-0.129***	-0.026***	0.016***	-0.111***	-0.008***	0.021***	-0.108***	-0.016***
	(0.003)	(0.002)	(0.003)	(0.003)	(0.001)	(0.003)	(0.003)	(0.001)
R-squared	0.344	0.896	0.388	0.036	0.002	0.751	0.035	
Panel B: 12 quarter displacement measure (*N* = 25,537,770)								
Recently displaced	-0.240***	0.001	-0.149***	-0.089***	-0.003***	-0.149***	-0.086***	-0.005***
	(0.002)	(0.001)	(0.003)	(0.003)	(0.000)	(0.003)	(0.003)	(0.001)
Recently displaced × during Hartz	-0.067***	-0.017***	0.020***	-0.067***	-0.004***	0.023***	-0.065***	-0.007***
	(0.003)	(0.002)	(0.003)	(0.003)	(0.001)	(0.003)	(0.003)	(0.001)
Recently displaced × after Hartz	-0.121***	-0.025***	0.014***	-0.103***	-0.007***	0.021***	-0.099***	-0.018***
	(0.003)	(0.002)	(0.003)	(0.003)	(0.001)	(0.003)	(0.003)	(0.001)
R-squared	0.347	0.897	0.399	0.039	0.002	0.725	0.037	
Panel C: 20 quarter displacement measure (*N* = 22,559,316)								
Recently displaced	-0.226***	0.009***	-0.146***	-0.087***	-0.002***	-0.145***	-0.085***	-0.005***
	(0.002)	(0.001)	(0.003)	(0.002)	(0.000)	(0.003)	(0.002)	(0.001)
Recently displaced × during Hartz	-0.069***	-0.017***	0.014***	-0.064***	-0.003***	0.017***	-0.061***	-0.008***
	(0.003)	(0.002)	(0.004)	(0.003)	(0.001)	(0.004)	(0.003)	(0.001)
Recently displaced × after Hartz	-0.120***	-0.028***	0.007***	-0.093***	-0.006***	0.018***	-0.087***	-0.024***
	(0.003)	(0.002)	(0.004)	(0.003)	(0.000)	(0.003)	(0.003)	(0.001)
R-squared	0.351	0.896	0.406	0.045	0.002	0.375	0.043	
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, eqs. (3) and (26). Column (2) reports OLS estimates of eqs. (5) and (27). Columns (3), (4), and (5) 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 (6) and (7) 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 Table C.1 in the Online Appendix for estimates of eq. (15). Column (8) reports the difference between estimated coefficients in the AKM specification eq. (15) and eq. (5). 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 (3), (4), (5), (6), (7), and (8) 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. See Table 3 and Table C.1 for the number of observations, workers, establishments, and matches in the largest connected set; and notes to Table 2 for information about sample composition.

Table E.3
Decomposition Estimates for Alternate Definitions of Recent Displacement, Full-Time Women

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
			Decomposition 1			Decomposition 2		
	Gross Effect	Net Effect	Selection	Sorting	Matching	Selection	Sorting	Matching
Panel A: 8 quarter displacement measure (N = 7,624,296)								
Recently displaced	-0.250***	-0.021***	-0.120***	-0.107***	-0.001***	-0.120***	-0.106***	-0.003***
	(0.004)	(0.002)	(0.006)	(0.006)	(0.001)	(0.006)	(0.006)	(0.001)
Recently displaced × during Hartz	-0.045***	-0.003	0.005	-0.045***	-0.002**	0.006	-0.043***	-0.005**
	(0.007)	(0.004)	(0.008)	(0.009)	(0.001)	(0.008)	(0.008)	(0.002)
Recently displaced × after Hartz	-0.045***	0.002	0.006	-0.048***	-0.006***	0.010	-0.045***	-0.013***
	(0.005)	(0.003)	(0.006)	(0.007)	(0.001)	(0.006)	(0.006)	(0.002)
R-squared	0.206	0.890	0.276	0.027	0.001	0.787	0.026	
Panel B: 12 quarter displacement measure (N = 7,187,498)								
Recently displaced	-0.242***	-0.018***	-0.119***	-0.104***	-0.001**	-0.119***	-0.103***	-0.003**
	(0.004)	(0.002)	(0.006)	(0.006)	(0.001)	(0.006)	(0.006)	(0.001)
Recently displaced × during Hartz	-0.050***	-0.002	0.006	-0.053***	-0.002**	0.008	-0.051***	-0.005**
	(0.007)	(0.004)	(0.008)	(0.007)	(0.001)	(0.007)	(0.007)	(0.002)
Recently displaced × after Hartz	-0.043***	0.005	0.004	-0.047***	-0.005***	0.009*	-0.044***	-0.014***
	(0.005)	(0.003)	(0.006)	(0.006)	(0.001)	(0.006)	(0.006)	(0.002)
R-squared	0.210	0.891	0.284	0.028	0.001	0.774	0.027	
Panel C: 20 quarter displacement measure (N = 6,305,438)								
Recently displaced	-0.216***	-0.008***	-0.117***	-0.091***	-0.001***	-0.116***	-0.090***	-0.003***
	(0.004)	(0.002)	(0.005)	(0.005)	(0.000)	(0.005)	(0.005)	(0.001)
Recently displaced × during Hartz	-0.063***	0.000	-0.002	-0.060***	-0.001	-0.001	-0.059***	-0.004*
	(0.006)	(0.004)	(0.009)	(0.007)	(0.001)	(0.009)	(0.007)	(0.002)
Recently displaced × after Hartz	-0.057***	0.004	-0.008	-0.050***	-0.003***	0.000	-0.047***	-0.014***
	(0.005)	(0.003)	(0.008)	(0.006)	(0.001)	(0.007)	(0.006)	(0.002)
R-squared	0.216	0.892	0.287	0.028	0.001	0.746	0.027	
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, eqs. (3) and (26). Column (2) reports OLS estimates of eqs. (5) and (27). Columns (3), (4), and (5) 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 (6) and (7) 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 Table C.1 in the Online Appendix for estimates of eq. (15). Column (8) reports the difference between estimated coefficients in the AKM specification eq. (15) and eq. (5). 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 (3), (4), (5), (6), (7), and (8) 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. See Table 3 and Table C.1 for the number of observations, workers, establishments, and matches in the largest connected set; and notes to Table 2 for information about sample composition.

Table E.4
Decomposition Estimates Based on 60 Month Tenure Restriction

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
			Decomposition 1			Decomposition 2		
	Gross Effect	Net Effect	Selection	Sorting	Matching	Selection	Sorting	Matching
Panel A: Full-time Men (N = 20,251,483)								
Recently displaced	-0.240***	-0.001	-0.160***	-0.074***	-0.006***	-0.158***	-0.070***	-0.011***
	(0.003)	(0.003)	(0.005)	(0.005)	(0.001)	(0.005)	(0.005)	(0.001)
Recently displaced × during Hartz	-0.078***	-0.030***	0.014***	-0.063***	0.001	0.014**	-0.063***	0.001
	(0.005)	(0.004)	(0.006)	(0.005)	(0.001)	(0.006)	(0.005)	(0.001)
Recently displaced × after Hartz	-0.138***	-0.053***	0.031***	-0.115***	-0.001	0.033***	-0.113***	-0.005***
	(0.005)	(0.004)	(0.005)	(0.005)	(0.001)	(0.005)	(0.005)	(0.002)
R-squared	0.356	0.879	0.377	0.014	0.000	0.503	0.014	
RMSE of Residual	0.324	0.143	0.269	0.178	0.036	0.269	0.178	
Panel B: Full-time Women (N = 4,806,006)								
Recently displaced	-0.276***	-0.025***	-0.126***	-0.125***	-0.001	-0.125***	-0.124***	-0.003***
	(0.008)	(0.006)	(0.011)	(0.010)	(0.001)	(0.011)	(0.010)	(0.002)
Recently displaced × during Hartz	-0.065***	-0.012	0.035**	-0.086***	-0.002	0.037**	-0.084***	-0.005
	(0.013)	(0.011)	(0.017)	(0.017)	(0.002)	(0.017)	(0.017)	(0.004)
Recently displaced × after Hartz	-0.077***	0.024***	0.017	-0.114***	-0.005**	0.023*	-0.109***	-0.016***
	(0.011)	(0.009)	(0.013)	(0.013)	(0.002)	(0.013)	(0.013)	(0.003)
R-squared	0.211	0.870	0.172	0.010	0.000	0.776	0.010	
RMSE of Residual	0.358	0.149	0.386	0.342	0.037	0.321	0.341	
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, eqs. (3) and (26). Column (2) reports OLS estimates of eqs. (5) and (27). Columns (3), (4), and (5) 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 (6) and (7) 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 (8) reports the difference between estimated coefficients in the AKM specification eq. (15) and eq. (5). 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 (3), (4), (5), (6), (7), and (8) 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 2, except restricted to individuals with at least 60 months of tenure at their current employer (if they were not displaced from eployment in the preceding 4 quarters) or at least 60 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 20,251,483 observations on 483,507 individuals and 63,938 establishments. Sample size for Panel B is 4,806,006 observations on 164,990 individuals and 21,978 establishments. See notes to Table 2 for additional information about sample composition.

Table E.5
Decomposition Estimates for the Restricted Sample Period, 1993-2008

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
			Decomposition 1			Decomposition 2		
	Gross Effect	Net Effect	Selection	Sorting	Matching	Selection	Sorting	Matching
Panel A: Full-time Men (*N* = 19,211,401)								
Recently displaced	-0.238***	-0.011***	-0.143***	-0.080***	-0.004***	-0.143***	-0.077***	-0.007***
	(0.002)	(0.001)	(0.004)	(0.004)	(0.000)	(0.004)	(0.004)	(0.001)
Recently displaced × during Hartz	-0.073***	-0.025***	0.021***	-0.068***	-0.002**	0.023***	-0.069***	-0.003***
	(0.004)	(0.002)	(0.004)	(0.003)	(0.001)	(0.004)	(0.004)	(0.001)
Recently displaced × after Hartz	-0.130***	-0.040***	0.020***	-0.105***	-0.005***	0.025***	-0.103***	-0.012***
	(0.004)	(0.002)	(0.004)	(0.004)	(0.001)	(0.004)	(0.004)	(0.002)
R-squared	0.348	0.902	0.345	0.017	0.001	0.414	0.017	
RMSE of Residual	0.317	0.126	0.256	0.167	0.041	0.256	0.166	
Panel B: Full-time Women (*N* = 5,136,780)								
Recently displaced	-0.244***	-0.027***	-0.111***	-0.104***	-0.002***	-0.111***	-0.101***	-0.004***
	(0.005)	(0.002)	(0.010)	(0.011)	(0.001)	(0.010)	(0.011)	(0.001)
Recently displaced × during Hartz	-0.041***	-0.001	0.000	-0.038***	-0.002*	0.002	-0.038***	-0.005**
	(0.007)	(0.004)	(0.011)	(0.009)	(0.001)	(0.011)	(0.009)	(0.002)
Recently displaced × after Hartz	-0.063***	0.001	0.003	-0.060***	-0.006***	0.010	-0.057***	-0.017***
	(0.008)	(0.004)	(0.010)	(0.009)	(0.001)	(0.010)	(0.009)	(0.003)
R-squared	0.205	0.891	0.209	0.017	0.001	0.776	0.017	
RMSE of Residual	0.360	0.137	0.312	0.248	0.042	0.311	0.246	
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, eqs. (3) and (26). Column (2) reports OLS estimates of eqs. (5) and (27). Columns (3), (4), and (5) 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 (6) and (7) 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 (8) reports the difference between estimated coefficients in the AKM specification eq. (15) and eq. (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 (3), (4), (5), (6), (7), and (8) 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 2 for information about sample composition.

Table E.6
Decomposition Estimates for the Restricted Sample Period, 1998-2008

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
			Decomposition 1			Decomposition 2		
	Gross Effect	Net Effect	Selection	Sorting	Matching	Selection	Sorting	Matching
Panel A: Full-time Men (N = 14,403,586)								
Recently displaced	-0.268***	-0.013***	-0.161***	-0.091***	-0.003***	-0.161***	-0.089***	-0.005***
	(0.003)	(0.002)	(0.007)	(0.007)	(0.001)	(0.007)	(0.007)	(0.001)
Recently displaced × during Hartz	-0.044***	-0.017***	0.025***	-0.051***	-0.001	0.026***	-0.051***	-0.002
	(0.004)	(0.002)	(0.005)	(0.005)	(0.001)	(0.005)	(0.005)	(0.001)
Recently displaced × after Hartz	-0.103***	-0.032***	0.023***	-0.089***	-0.005***	0.028***	-0.087***	-0.012***
	(0.004)	(0.003)	(0.005)	(0.005)	(0.001)	(0.005)	(0.004)	(0.002)
R-squared	0.343	0.910	0.349	0.018	0.001	0.350	0.018	
RMSE of Residual	0.327	0.125	0.270	0.171	0.036	0.270	0.170	
Panel B: Full-time Women (N = 3,761,034)								
Recently displaced	-0.247***	-0.030***	-0.119***	-0.095***	-0.002**	-0.119***	-0.094***	-0.004***
	(0.006)	(0.003)	(0.017)	(0.015)	(0.001)	(0.016)	(0.015)	(0.002)
Recently displaced × during Hartz	-0.039***	0.003	-0.006	-0.035***	-0.001	-0.006	-0.034***	-0.002
	(0.008)	(0.004)	(0.014)	(0.012)	(0.001)	(0.014)	(0.012)	(0.002)
Recently displaced × after Hartz	-0.059***	0.006	0.004	-0.064***	-0.004***	0.009	-0.062***	-0.012***
	(0.009)	(0.005)	(0.015)	(0.013)	(0.001)	(0.015)	(0.013)	(0.003)
R-squared	0.211	0.900	0.202	0.016	0.000	0.721	0.016	
RMSE of Residual	0.369	0.136	0.335	0.253	0.036	0.335	0.253	
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, eqs. (3) and (26). Column (2) reports OLS estimates of eqs. (5) and (27). Columns (3), (4), and (5) 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 (6) and (7) 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 (8) reports the difference between estimated coefficients in the AKM specification eq. (15) and eq. (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 (3), (4), (5), (6), (7), and (8) 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 2 for information about sample composition.

Table E.7

Decomposition Estimates with Separate Linear Time Trends for Displaced and Non-Displaced Workers

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
			Decomposition 1			Decomposition 2		
	Gross Effect	Net Effect	Selection	Sorting	Matching	Selection	Sorting	Matching
Panel A: Full-time Men (N = 28,236,539)								
Recently displaced	-0.010	0.078***	-0.113***	0.050***	-0.025***	-0.122***	0.057***	-0.024***
	(0.020)	(0.012)	(0.017)	(0.019)	(0.006)	(0.017)	(0.019)	(0.009)
Recently displaced × during Hartz	-0.036***	-0.010***	0.031***	-0.048***	-0.008*	0.031***	-0.047***	-0.009***
	(0.004)	(0.003)	(0.004)	(0.004)	(0.001)	(0.004)	(0.004)	(0.002)
Recently displaced × after Hartz	-0.081***	-0.022***	0.037***	-0.083***	-0.013***	0.038***	-0.081***	-0.016***
	(0.007)	(0.004)	(0.006)	(0.005)	(0.002)	(0.006)	(0.005)	(0.003)
R-squared	0.341	0.893	0.350	0.031	0.001	0.333	0.031	
RMSE of Residual	0.347	0.143	0.264	0.188	0.055	0.263	0.187	
Panel B: Full-time Women (N = 7,986,586)								
Recently displaced	-0.554***	-0.079***	-0.168***	-0.261***	-0.046***	-0.163***	-0.249***	-0.064***
	(0.039)	(0.023)	(0.052)	(0.051)	(0.009)	(0.053)	(0.051)	(0.016)
Recently displaced × during Hartz	-0.091***	-0.004	-0.008	-0.068***	-0.010	-0.006	-0.065***	-0.015***
	(0.009)	(0.005)	(0.012)	(0.011)	0.002***	(0.012)	(0.011)	(0.003)
Recently displaced × after Hartz	-0.157***	-0.027***	-0.013	-0.097***	-0.020***	-0.008	-0.091***	-0.030***
	(0.013)	(0.007)	(0.017)	(0.018)	(0.003)	(0.017)	(0.018)	(0.005)
R-squared	0.201	0.888	0.351	0.025	0.001	0.358	0.024	
RMSE of Residual	0.390	0.150	0.303	0.260	0.054	0.303	0.259	
Linear Time Trends	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: Columns (1)-(8) reproduce estimates of specifications from the corresponding columns of Table 3, 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 (3), (4), (5), (6), (7), and (8) 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 2 for the number of observations, workers, establishments, and matches in the largest connected sets of men and women; and notes to Table 2 for information about sample composition.

Table E.8
Estimates for Specification with Tenure, Sector, and Occupation Controls

	(1)	(2)	(3)	(4)	(5)
			Gelbach Decomposition		
	Gross Effect	Net Effect	Tenure	Sector	Occupation
Panel A: Full-time Men (*N* = 27,941,270)					
Recently displaced	-0.235***	-0.081***	-0.020***	-0.095***	-0.040***
	(0.002)	(0.002)	(0.000)	(0.001)	(0.001)
Recently displaced × during Hartz	-0.075***	-0.053***	-0.010***	-0.020***	0.008***
	(0.003)	(0.003)	(0.000)	(0.002)	(0.002)
Recently displaced × after Hartz	-0.144***	-0.083***	-0.020***	-0.049***	0.008***
	(0.003)	(0.002)	(0.000)	(0.001)	(0.001)
R-squared	0.347	0.603	0.293	0.025	0.400
RMSE of Residual	0.344	0.268	0.032	0.133	0.159
Panel B: Full-time Women (*N* = 7,857,486)					
Recently displaced	-0.258***	-0.091***	-0.038***	-0.088***	-0.041***
	(0.004)	(0.004)	(0.001)	(0.003)	(0.002)
Recently displaced × during Hartz	-0.046***	-0.037***	-0.009***	-0.015***	0.016***
	(0.007)	(0.006)	(0.001)	(0.003)	(0.003)
Recently displaced × after Hartz	-0.055***	-0.044***	-0.010***	-0.017***	0.016***
	(0.006)	(0.005)	(0.001)	(0.003)	(0.002)
R-squared	0.213	0.495	0.242	0.026	0.303
RMSE of Residual	0.380	0.305	0.043	0.151	0.148
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 3 on the subset of observations with non-missing occupation and employer tenure. Column (2) 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 (3), (4), and (5) report estimates of the Gelbach decomposition of the difference between the baseline model in column (1) and the full specification in column (2). 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 (3), (4), and (5) 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.

Table E.9
Top Pre- and Post-Displacement Sectors, Additional Time Periods

Pre-Displacement Sector	(1) Share	(2) Estab. Effect	Post-Displacement Sector	(3) Share	(4) Estab. Effect
Panel A: Men 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
Panel B: Men 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
Panel C: Women 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
Panel D: Women 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

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 duing the indicated time period. Panel A is based on 16,327 displacements and 13,254 post-displacement jobs held by men between 1994 and 1997. Panel B is based on 34,010 displacements and 21,657 post-displacement jobs held by men between 2010 and 2014. Panel C is based on 5,750 displacements and 3,785 post-displacement jobs held by women between 1994 and 1997, and Panel D is based on 12,070 displacements and 7,509 post-displacement jobs held by women between 2010 and 2014. See notes to Table 2 for information about sample composition.