The (Unequal) Job Security Scars of Displacement

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March 28, 2025

Abstract

We investigate how liquid wealth affects job security after displacement, leveraging a Dutch policy that makes only some workers eligible for a lump-sum transfer. Our findings show that displaced workers experience sizable and persistent losses in job security, with important negative effects on their earnings. However, eligibility for the lump-sum transfer significantly offsets these losses. This positive impact is more pronounced among workers with low liquid wealth at the time of displacement, consistent with credit constraints. We conclude that cash-on-hand post-displacement can attenuate the wage cost of job loss, especially for the poor.

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1 Introduction

Losing a long-term job entails large and lasting losses in earnings (Jacobson et al., 1993). Recently, Jarosch (2023) argues that such costly effects stem from the loss in job security after displacement, that hinders skill accumulation on the job. In this paper, we demonstrate how access to cash-on-hand can significantly mitigate the negative impact of displacement on job security. In doing so, we uncover a novel channel through which liquid wealth—the primary self-insurance mechanism during a jobless spell—affects search outcomes of the unemployed. By allowing workers to return to stable jobs, we find that it attenuates wage losses from displacement, revealing a new feedback mechanism of wealth inequality on the wage distribution. These findings highlight the importance of considering individuals' ability to smooth consumption when designing labor market policies, such as the unemployment insurance.

To causally investigate the importance of cash-on-hand access on job security after displacement, we exploit a policy in the Netherlands that makes only certain workers eligible to a lump-sum transfer upon job loss. Specifically, those who lose their job following firm bankruptcy are eligible for a lump-sum transfer, while those laid-off due to economic reasons (i.e. downsizing) are not. The average transfer is equivalent to approximately one month's salary, allowing individuals to top up the unemployment insurance to 100% of their prior wage for about 3.5 months. Crucially, we show that the two groups of displaced workers are otherwise identical along all other observable characteristics. One reason is because downsizing firms must follow layoff rules that mostly avoid selective layoff behaviour, something we validate with our data. Also, we show that in the years before workers are displaced, voluntary separations remain small and stable in both downsizing and bankrupt firms, suggesting there is no negative selection into treatment from workers with better outside options quitting in anticipation of job loss.

We measure job security using information on the type of labor contract a worker holds. In our context, as in many European countries, there are two types of contracts whose difference in job security is highly salient to workers: open-ended contracts with large separation costs (henceforth, permanent employment) and fixed-term contracts of short duration. Us-

ing an event-study design, we start by showing that permanent employment decreases by 19% for displaced workers the year after displacement, compared to a control group of non-displaced workers. Within five years, permanent employment still remains 12% lower for the average displaced worker. Importantly, we find that eligibility to the lump-sum transfer significantly offsets losses in job security: one year after displacement, non-eligible workers experience a 25% decrease in permanent employment, whereas eligible workers a drop of 16%. This difference persists five years after.

Differences in permanent employment after job loss between workers eligible for the lump-sum transfer are not explained by sector, region, or timing differences that might affect displaced workers' job market prospects. Our results also hold if we restrict focus to economic layoffs in firms undergoing a mass layoff—defined as workforce reduction of more than 30%, demonstrating that differences in permanent employment are not driven by negative signalling about workers' ability inferred from the cause of displacement (Gibbbons and Katz, 1991).

To further isolate the role of binding liquidity constraints, we exploit detailed information on pre-displacement household assets and investigate whether the eligibility effect differs between displaced workers in the bottom and top quintiles of the liquid wealth distribution. The underlying assumption is that, if liquidity constraints is a driving mechanism, the lump-sum transfer should have a larger effect on those with lower liquid wealth upon displacement. Alternative explanations would not predict such heterogeneity along the liquid wealth distribution.

In line with our hypothesis, we find that the sensitivity of permanent employment to cash-on-hand one year after job loss is twice as large for workers likely to be liquid-constrained (bottom 20% of the liquid wealth distribution) compared to unconstrained workers (top 20%). These effects hold irrespective of individuals' illiquid assets. Furthermore, we show that across different population groups (old or young workers, migrant or native workers, high or low wage workers) the eligibility effect is consistently larger for low-liquidity workers. This suggests that differences in the eligibility effect between high- and low-liquidity workers are not explained by differences in other dimensions.

These results point to liquidity constraints significantly affecting a displaced worker's

ability to return to a permanent job, which protects against experiencing unstable employment relationships in the years following displacement. While we use the type of labor contract held by the worker as a primary measure of job security, we show that the risk of unemployment—the probability of separation into non-employment—after displacement is also higher for non-eligible workers, particularly among those in the bottom 20% of the liquid wealth distribution. This result corroborates the view that the two types of labor contracts entail differences in job security.

Next, we examine how much the loss of job security contributes to the wage cost of displacement. To do so, we first compute individual-level estimates of wage loss five years after displacement, as in Schmieder et al. (2023). Consistent with prior work, we find that displaced workers suffer a large and persistent drop in wages of around 14%. However, workers who are not eligible for a lump-sum payment experience a loss larger by 5pp than eligible ones. Using these individual-level estimates, we offer two novel insights. First, we show that temporary workers experience average wage losses 21% larger than permanent job holders five years after displacement. Importantly, this is after accounting for other explanatory factors emphasized by the literature such as losses in employer-specific wage premium (Schmieder et al., 2023; Bertheau et al., 2023; Moore and Scott-Clayton, 2019; Helm et al., 2022) and losses in sector and firm-specific skills (Jacobson et al., 1993; Neal, 1995; Huckfeldt, 2022). Second, using an Oaxaca-Blinded decomposition, we show that differences in job security after displacement explain one-fifth of the difference in wage loss between eligible and non-eligible workers, on average. This implies that a relatively small lump-sum transfer at displacement can significantly mitigate the cost of job loss by improving long-term employment stability.

Our findings provide new insights into the relationship between wealth and wages after displacement. While a large literature has focused on understanding how wages differences contribute to wealth inequality, our results suggest that wealth inequality feeds back into the wage distribution through its effects on post-displacement outcomes. In particular, liquid wealth allows workers to return to secure employment after displacement, reducing wage losses after displacement. This suggests a novel motivation for asset-dependent unemployment insurance in order to help low-wealth individuals to search for safer jobs (Rendahl,

2012; Koehne and Kuhn, 2015) .

Contribution This paper contributes to the job loss literature. Since Jacobson et al. (1993), many papers have documented long-term losses in wages and employment after displacement (e.g. Couch and Placzek, 2010; Davis and Von Wachter, 2011). A growing literature has examined the sources of such costly effects of displacement. Some have focused on the loss of industry- and occupation-specific skills (Jacobson et al., 1993; Neal, 1995; Huckfeldt, 2022), others on the importance of losses in wage premiums that firms pay to their workers (Bertheau et al., 2023; Schmieder et al., 2023). We add to this work by showing that losses in job security, a dimension so far overlooked, are an important determinant of the wage cost of displacement, consistent with Jarosch (2023)'s theoretical framework. Moreover, this paper brings new insights into recent work showing that poor workers suffer disproportionately larger earnings losses when compared to relative to rich workers. (Kaila et al., 2021; Griffy, 2021). Specifically, our findings suggest that such differences stem, in part, from differences in job security after displacement.

We also contribute to our understanding of how wealth affects future labor market outcomes of unemployed workers. Prior work has established that liquidity-constraints shorten unemployment duration (Chetty, 2008; Basten et al., 2014; Card et al., 2007; Griffy, 2021; Fontaine et al., 2023). We complement this knowledge in two ways. First, we provide novel evidence that liquid wealth protects workers from experiencing more pronounced losses in job security after displacement. Second, we show that, through this effect, liquid wealth attenuates the wage cost of job loss. As such, we uncover a new channel through which wealth affects the search outcomes of the unemployed. While the positive eligibility effect on the wage cost of displacement is in line with Griffy (2021), it contrasts with earlier studies that found no effect of cash-on-hand on post-unemployment wages (Card et al., 2007; Basten et al., 2014; Fontaine et al., 2023). These different findings can be reconciled through the lens of a directed search framework with risk-averse workers, as in Griffy (2021) and Eeckhout and Sepahsalari (2023). In such a setting, the effect of liquid wealth on subsequent wages is not necessarily positive, as it is determined by two countervailing forces. More liquidity allows displaced workers to be more selective in their search, raising post-displacement wages,

but it also prolongs unemployment spell, which negatively affect wages. This theoretically implies that the wealth-wage effect will depend on the relative importance of the selectivity and search margins across different populations.

Roadmap The rest of the paper proceeds as follows. The next section presents the data. Section 3 provides institutional context and introduces our identification strategy. Section 4 presents the main results. Section 5 shows that job security contributes to the cost of job loss. Section 6 concludes.

2 Data

2.1 Sources and Main Variables

Sources We rely on several administrative data sources provided by Statistics Netherlands (CBS), covering the universe of residents of the Netherlands between 2004 to 2015. Our starting point is a linked employer-employee dataset that reports the start and end date of a job, earnings and days worked in a year, firm sector and size, contract type (temporary or permanent), and hours relative to full-time equivalent. Using unique identifiers, we add information on the reasons behind job terminations, worker demographic characteristics —age, gender, and migration background (i.e. whether at least one parent is foreign-born)—and detailed information on household wealth. On the assets side, we observe savings accounts, stock and bond holdings, as well as owned property value. Regarding liabilities, the data reports separately the mortgage value for primary residences, student loans, and a category labeled "others", that includes credit card debts and second-home mortgages. This information is collected by tax authorities, and is thus highly reliable.

Sample We focus on full-time male workers earning more than €1000 per month.[1] Given their higher labor market attachment, focusing on men minimizes selection issues driven by transitions in and out of the labor force.[2]

[1] This is around 70% of the Dutch minimum wage in the analysis period.

[2] This restriction follows largely the existing literature on job loss that almost exclusively studies the labor market outcomes of men, easing comparability.

Job Security The coexistence of secure jobs (open-ended contracts with high firing cost or low turnover jobs) with unstable ones (short-term work arrangements) has given rise to a two-tier labor market in most advanced economies. In European countries, including the Netherlands, dualism is mostly driven by labor market institutions, where rigid employment protection legislation for permanent jobs coexists with lenient regulations for temporary contracts (Bentolila et al., 2019). But dualism can also emerge in the absence of such institutional structures, as in the US (Ahn et al., 2023). Using information on the type of labor contract a worker holds, we define our main dependent variable as a binary variable that equals 1 for permanent workers and 0 for temporary workers. The key distinction between both is the degree of job protection. Permanent workers are employed under a contract with no termination date and firms need to pay high firing costs if they wish to separate from such workers. These include not only lump-sum compensations to the employee, but also the implicit costs of lengthy layoff procedures. For instance, in the Netherlands, firms need to ask for dismissal permission, which is only granted on valid grounds such as economic reasons (e.g. firm downsizing or relocation), worker's performance, prolonged illness, disturbed employment relationship or dysfunctional behavior. By contrast, when a temporary contract reaches its termination date, the employer can dismiss the worker without incurring any costs. Their incidence is significantly higher among young and low-skilled workers (OECD, 2014)[3]. Similarly, temporary workers in our sample are, on average, younger, more likely to have a migrant background, and have lower annual earnings when compared to permanent workers (Appendix Table A.1).

Liquid Wealth Since individuals' ability to smooth consumption is not observed, we follow the previous literature (e.g. Chetty, 2008; Griffy, 2021), and use liquid wealth as a proxy for the degree to which a worker is constrained upon entering unemployment. To distinguish between low (constrained) and high-liquidity (unconstrained) workers, we use household's liquid wealth, defined as the total value of financial asset holdings (saving accounts, stocks, and bonds). Workers in the bottom 20% of the liquid wealth distribution before displacement

[3]The case of the Netherlands is particularly interesting because it has the largest incidence of short-term employment in Europe (27.7% in 2022 according to OECD data, compared to 11.3% on average across all OECD countries).

are categorized as low-liquidity, and those in the top 20% as high-liquidity. We do not consider liquid debt because we cannot distinguish between liquid and non-liquid liabilities in the data. Nonetheless, our results remain similar when we use a proxy for net liquid wealth (liquid assets minus the total value of debt on credit card and mortgages for second homes or other real estate).

2.2 Definition of Treated and Control Workers

Treated workers Following existing literature on job loss, we focus on workers who involuntarily separate from their jobs due to exogenous shocks, such as firm closure or downsizing. To differentiate between involuntary separations from those that are voluntary (quits) or due to poor performance, previous studies typically focus on separations from firms experiencing a significant reduction in their workforce. Instead, we use detailed information about the reasons behind job separations, allowing us to precisely identify workers not at fault for their job loss.[4] Specifically, we focus on workers who lost their job due to firm closure following bankruptcy and workers who were laid-off due to economic reasons (i.e. downsizing). This ensures there is minimal relationship between worker's characteristics and the displacement event. Important for the exogeneity assumption, firms in the Netherlands must follow the "last-in-first-out" rule within job type and 10-year age brackets in cases of economic layoffs (van den Berge, 2016). As firms have limited discretion on whom they layoff, the cause of displacement—bankruptcy versus layoff—is unlikely to signal any unfavorable information about the worker that might impact their employment trajectories after displacement (Gibbons and Katz, 1991). Furthermore, economic layoffs are likely unexpected: firms can only inform workers after the dismissal request is approved and only need to provide a one-month notice period before termination.

We consider workers as "displaced" if in year t they lost their job following a bankruptcy or an economic layoff, are aged between 25 and 50, and have more than three years of employer tenure. Prior work also restricts firm size to between 10 and 500 workers in or-

[4] Birinci et al. (2023) show that using information on the reason for job termination is crucial for accurately assessing the effects of displacement, as many separations labeled as involuntary by conventional methods are not truly involuntary.

der to ensure that the firm's workforce reduction—one of the criteria to identify displaced workers—does not reflect small-number variability. Although we overcome this using information about the reasons for job separations, we also impose this restriction and relax it in a robustness check. We focus on workers displaced once between 2007 and 2014. For each displacement year d, we construct a sample including observations for the pre- and post-displacement years for the treatment group that experienced displacement in year d and a control group which did not.

Control workers Our sample of potential control workers consists of non-displaced workers in a given year that meet the same age, tenure and firm size restrictions as displaced workers. Importantly, we do not restrict workers in the control group to stay at the same firm between t and $t+1$, i.e. we allow them to separate for reasons other than displacement such as voluntary job changes. To obtain a control sample comparable to displaced workers, we apply coarse exact matching and match each displaced worker to a control with similar observable characteristics prior to displacement. Specifically, we match on age, job tenure, sector (15 sectors), firm size quintile, annual earnings decile and liquid wealth quintile. When displaced workers match with multiple non-displaced workers, we randomly select one as a control.

2.3 Descriptive Statistics

Table 1 describes the matched sample. Using the matching procedure previously described, we successfully match 78% of displaced workers.[5] This leaves us with 38,925 displacements, out of which 57% are workers that lost their job following a bankruptcy.[6] Columns 1 and 2 show that our matching procedure works well, as matched displaced workers are nearly identical to their control counterparts in their observed pre-displacement characteristics (Columns 3 reports the normalized difference between the two groups). Prior to displacement, they are, on average, 38 years old and have worked at the same firm for 9 years. Almost all work under a permanent contract (99%).

[5]The proportion of workers matched is similar to Britto et al. (2022), who use an equivalent matching procedure. The matched and full sample are very similar (Appendix Table A.2).

[6]Appendix Figure A.1 plots the yearly distribution of events.

Table 1: Descriptive Statistics, Matched sample

	Full Sample			Displaced Sample			Top 20%		Bottom 20%	
	Non-displaced (1)	Displaced (2)	Norm Diff (3)	Bankruptcies (4)	Layoffs (5)	Norm Diff (6)	Non-displaced (7)	Displaced (8)	Non-displaced (10)	Displaced (11)
Age (years)	38.2	38.2	-.001	37.9	38.7	-.104	39.6	39.6	36.8	36.8
Migrant Background (%)	17.4	19.5	.052	17.8	21.7	-.096	6.7	7.7	30.4	33.5
Wealth (000 Euros)										
Savings accounts	25.9	25.0	-.013	25.4	24.5	.014	122.1	116.0	0.9	0.8
Liquid wealth	32.7	30.8	-.014	31.7	29.7	.022	167.7	153.6	0.9	0.8
Net illiquid wealth	62.5	59.7	-.018	62.2	56.3	.036	182.9	169.4	11.1	10.2
Job										
Tenure (years)	9.3	9.3	-.001	9.2	9.5	-.056	10.1	10.1	7.9	7.9
Annual wage (000 Euros)	42.3	42.0	-.015	43.1	40.6	.120	55.4	54.5	35.2	35.1
Firm size	95.2	89.0	-.058	79.2	101.8	-.217	94.3	88.4	96.0	89.3
Industry (%)										
Manufacturing	33.8	34.1	.004	30.6	38.6	-.169	30.9	31.0	34.0	34.3
Wholesale and retail trade	22.4	22.9	.012	23.5	22.1	.033	24.3	24.7	19.9	20.4
Construction	17.8	17.3	-.013	17.7	17.4	.008	16.7	16.3	17.2	16.7
Real Estate	16.1	15.9	-.005	17.1	14.3	.074	20.1	19.9	16.6	16.2
Transportation	5.8	5.7	.000	7.2	3.8	.149	3.4	3.4	7.4	7.4
Permanent employment (%)										
1-year before displacement	98.9	98.8	-.009	98.4	99.4	-.095	99.6	99.6	97.5	97.6
1-year after displacement	98.6	79.4		82.8	74.5		99.4	87.2	97.0	70.0
Observations	38,396	38,925		22,004	16,921		4,995	5,052	9,112	9,282

Notes: The table reports average characteristics of matched displaced and non-displaced workers one year before displacement, unless stated otherwise. *Bankruptcies* refer to displaced workers that experienced a firm closure. *Layoffs* refer to displaced workers who lost their job due economic reasons. Top (bottom) 20% corresponds to individuals in top (bottom) 20% of the liquid wealth distribution. *Migrant Background* refers to individuals with at least one foreign-born parent, including first and second-generation immigrants. *Liquid wealth* is the total value of financial asset holdings (saving accounts, stocks, and bonds). *Net illiquid wealth* corresponds the value of owned real estate minus the mortgage in the primary residence. Column 3 reports the normalized difference between displaced and non-displaced workers and column 6 the normalized difference between bankruptcies and layoffs. Wealth and earnings are deflated and measured in 2015 Euros. Sample defined in Section 2.

However, one year after displacement, treated workers are, on average, 19% less likely to be permanent workers. This decline is more pronounced for low-liquidity workers than high-liquidity ones, a gap that persists after 5 years (Appendix Figure A.2). Wealth is not randomly assigned, hence we cannot interpret this heterogeneity as causal. In the next section, we describe our strategy to isolate the causal link between liquid wealth and job security after displacement.

3 Identification Strategy

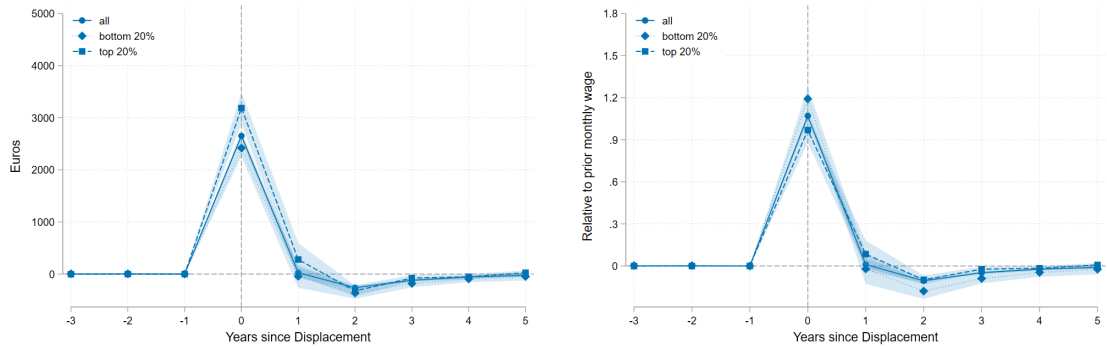
Identifying the causal effect of liquid wealth on permanent employment after job loss is challenging. Losing a job and liquid wealth holdings upon displacement likely depend on other factors, such as age or labor market experience, that correlate with job security through other channels. To address this, we exploit eligibility rules to lump-sum transfers in the Netherlands, which create exogenous variation in liquid wealth among displaced workers.[7]

Quasi-experimental variation in liquidity The Dutch unemployment insurance scheme covers all workers who lose their job due to exogenous shocks, such as firm closure or downsizing. In the period of analysis, the rules stipulate that displaced workers receive 75% of the previous wage in the first two months and 70% thereafter, for a maximum of 38 months (Cammeraat et al., 2023). On top of this, some displaced workers are entitled to a lump-sum transfer. Eligibility for such compensation hinges on whether job loss resulted from a layoff due to economic reasons or firm closure following bankruptcy. To layoff workers, firms need to ask permission to the Public Employment Office (UWV) or the civil court and provide valid reasons for the dismissal, such as economic reasons (e.g. restructuring or company relocation), long-term sickness or poor performance of the worker.[8] Once the dismissal request is approved, workers can not appeal the decision and firms were not required to pay any lump-sum compensation. In contrast, workers who lose their jobs due to bankruptcy are entitled to a lump-sum transfer from the UWV, covering salaries over the notice period

[7] Card et al. (2007) and Basten et al. (2014) use a similar identification strategy.

[8] UWV mainly deals with economic layoffs, while the civil court manages mostly termination requests due to a disturbed employment relationship (Frenk, 2013).

Figure 1: Difference in Yearly Transfers between Eligible and Non-eligible Workers



Notes: Figures plot estimated difference in the transfer amount received in a year by eligible and non-eligible workers, conditional on unemployment insurance. Estimates are obtained from the following regression: $transfer_{ij} = \beta_j \cdot eligible_i + controls_{ij} + \epsilon_{ij}$, $j \in [-3, ..., 5]$, where $transfer_{ij}$ is the average monthly amount received on unemployment insurance and lump-sum payments in year j since displacement and $eligible_i$ is a dummy that equals one for eligible workers and 0 for non-eligible. Controls include age, its square, a migrant background dummy, pre-displacement tenure and monthly wage, as a proxy for unemployment insurance, and displacement-year fixed effects. β_j is the difference in the average monthly benefit received in year j since displacement between eligible and non-eligible workers conditional on unemployment insurance, thus capturing the lump-sum transfer. The left panel plots $\hat{\beta}_j$ multiplied by the number of months eligible workers receive such benefits in a year, on average, and the right panel plots this value normalized to the average monthly salary prior to displacement. Shaded area are 95% confidence intervals. Sample of displaced workers as defined in Section 2.

or delayed salaries. This institutional setting creates quasi-experimental variation in liquid wealth for workers facing a similar unexpected shock to employment, allowing us to identify the causal relationship between liquid wealth and permanent employment after job loss.

We do not observe directly the transfer received by workers. However, we can infer it using data on the average monthly transfer workers receive throughout the year, along with the duration of such payments. The reported amounts include payments relative to unemployment insurance and any lump-sum transfer as compensation for involuntary job loss. Figure 1 plots the estimated gap in the transfer received by eligible and non-eligible workers before and after displacement. We control for the wage prior to displacement, ensuring that we compare eligible and non-eligible workers with similar unemployment insurance. Eligible workers receive around €2.600 more than non-eligible workers in the displacement year only. This is equivalent to one month's salary, allowing the average worker to supplement unemployment insurance to 100% of their previous wage for about 3.5 months. We exploit this one-time liquidity shock as exogenous variation in liquid wealth upon job loss.

Empirical Design We compare post- to pre-displacement changes in permanent employment of workers displaced in a given year d to changes in permanent employment of workers who were not displaced in that same year. To identify how the impact of displacement on permanent employment varies with liquid wealth at the time of displacement, we estimate the following event-study model separately for workers eligible for the lump-sum transfer and those non-eligible:

$$y_{idt} = \alpha_i + \gamma_t + \gamma_d + \sum_{j=-3}^{5} \delta_j D_{t=d+j} + \beta' X_{idt} + \epsilon_{idt} \quad (1)$$

where y_{idt} equals one if worker i displaced in year d is a permanent worker at time t or 0 if they are a temporary worker. $D_{t=d+j}$ are dummy variables indicating whether an individual was displaced in year d=t-j, t being the observation year. X_{idt} is a control vector including age and its square. Equation 1 also includes displacement-year (γ_d), calendar-year dummies (γ_t) and worker fixed effects (α_i) to control for time-invariant worker characteristics. Standard errors are clustered by individual i to allow for the correlation of the error term ϵ_{idt} across different time periods.

The coefficients of interest are $\{\delta_{-3}, ..., \delta_5\}$, which measure differences in permanent employment of displaced workers relative to non-displaced workers in pre- and post-displacement years $j \in [-3, \ldots, 5]$, with $t-1$ being the baseline omitted period. Differences in the estimated parameters between eligible and non-eligible displaced workers capture the effect of being eligible for the lump-sum transfer, or equivalently the intention-to-treat effect of the lump-sum compensation on permanent employment after displacement.[9]

Identification The causal interpretation relies on three main assumptions, namely that: (i) permanent employment would have followed a similar path for both displaced and non-displaced workers in the absence of displacement, (ii) workers did not anticipate layoffs nor firm closure, which would otherwise lead to negative selection into displacement, and (iii) eligible and non-eligible workers do not differ in other dimensions than the lump-sum transfer.

[9]Comparisons always occur between treated and never-treated individuals, ensuring that our estimates are not biased due to staggered treatment and heterogeneous treatment effects (Sun and Abraham, 2021; Goodman-Bacon, 2021; Callaway and Sant'Anna, 2021).

Appendix Figure A.3 presents evidence supporting the plausibility of the parallel trends assumption underlying our empirical design. Before the displacement shock, permanent employment of non-displaced workers and both types of displaced workers (eligible and non-eligible) were evolving in a very similar manner. This suggests that, in the absence of the displacement event, their employment trajectories would have followed a similar trajectory. On the other hand, Appendix Figure A.4 provides evidence against negative selection into treatment. The average number of separations, whether voluntary or involuntary, are small and stable in the years before a displacement event. This suggests that workers with better outside options do not begin leaving the firm in anticipation of job loss. Importantly, Appendix Figure A.4 shows that such anticipation effects are absent in both firms that go bankrupt and those that undergo economic layoffs.

One concern is that eligible and non-eligible displaced workers differ in characteristics that could affect their labor market outcomes differently after displacement. This would confound the interpretation that any differences in permanent employment after displacement are due to eligibility for a lump-sum transfer. Columns 4 and 5 in Table 1 show that eligible and non-eligible workers are similar in terms of pre-displacement observables. For all variables, the magnitude of the normalized difference (Table 1, column 5) is smaller than the 0.25 threshold commonly used as a rule of thumb indication of imbalance (Imbens and Rubin, 2015). Robustness checks show that our results are stable when using a matched sample of eligible and non-eligible workers, obtained through coarse matching on pre-displacement observables (as described in subsection 2.2) or estimated matching weights that equalize the mean of pre-displacement covariates between the weighted sample of non-eligible workers and eligible workers (Hainmueller, 2012).

To further address concerns about differences between eligible and non-eligible workers beyond the lump-sum transfer, and isolate the role of liquid wealth, we also leverage information on liquid holdings at the moment of displacement. If differences in permanent employment after displacement are due to liquidity constraints, then the same payment should have a smaller impact on those with higher pre-displacement liquid wealth compared to those with lower pre-displacement liquid wealth.

4 Results

Figure 2 shows estimates from Equation 1. Note that we observe no difference in permanent employment between displaced and non-displaced workers before displacement, supporting the parallel trends assumption, as previously discussed. Appendix Table A.3 reports corresponding point estimates and standard errors.

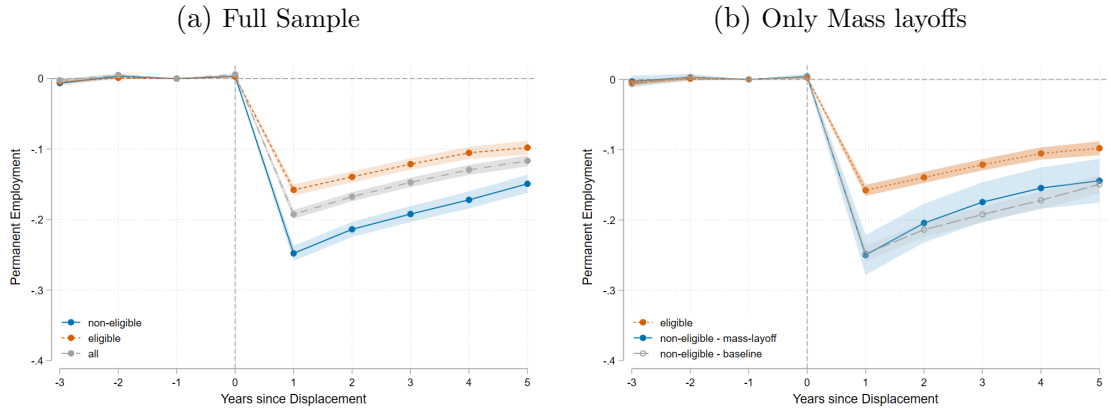
Panel (a) shows that the average displaced worker suffers a substantial decrease in permanent employment relative to non-displaced workers. One year after displacement, permanent employment is 19% lower. This effect is persistent: after 5 years, displaced workers still experience a 12% drop in permanent employment probability relative to the control group of non-displaced workers. However, workers non-eligible for a lump-sum transfer fare much worse than those eligible. Permanent employment declines by 25% one year after displacement for displaced workers non-eligible to the lump-sum, which compares to a 16% drop for those eligible. This gap persists after five years. Thus, a lump-sum compensation worth around 1 month of salary partially protects displaced workers from suffering more pronounced scars in job security.

Perhaps losing a job through an economic layoff versus firm closure impacts individuals' job market prospects differently either because they happen in different sectors, regions or at different times. To explore this hypothesis, we account for pre-displacement characteristics such as sector and region in a framework similar in spirit to a triple difference. Specifically, we estimate

$$y_{idt} = \mathbb{1}(eligible = 1) + \sum_{j=-3}^{5} \delta_j D_{t=d+j} + \sum_{j=-3}^{5} \delta_j^E D_{t=d+j} \cdot \mathbb{1}(eligible = 1) + \alpha_i + \gamma_t + \gamma_d + + \beta' X_{idt} + \epsilon_{idt} \quad (2)$$

where terms are defined as in Equation 1, except that now we pool the eligible and non-eligible samples together. $\mathbb{1}(eligible = 1)$ is an indicator variable for both displaced workers eligible for the lump-sum transfer and their matched control counterparts. The parameters δ_j^E capture the impact of displacement on permanent employment for displaced workers eligible for a lump-sum transfer relative to non-eligible displaced workers in pre- and post-

Figure 2: The Impact of Displacement on Permanent Employment by Eligibility



Notes: Each panel plots estimates of δ_j obtained from Equation 1, estimated separately for displaced workers eligible for a lump-sum transfer upon displacement (orange line) and those non-eligible (blue line). Panel (a) uses all displacement events, while panel (b) focuses on all bankruptcy events and only economic layoffs occurring in firms that have undergone a mass layoff, defined as a workforce reduction of more than 30%. The outcome variable is a categorical variable that equals 1 for permanent workers and 0 for temporary workers j years since displacement. All regressions control for a quadratic polynomial in age, displacement-year fixed effects, calendar-year fixed effects and individual fixed effects. Shaded areas correspond to 99% confidence intervals. Appendix Table A.3 reports estimates and standard errors clustered at the individual level. Sample defined in Section 2.

displacement years. We separately add as controls pre-displacement sector and region-event-year fixed effects. Appendix Figure A.5 shows that the significant differences between eligible versus non-eligible remain. Therefore, our findings are not driven by the fact that economic layoffs tend to happen in different sectors, regions and at different times when compared to firm closures.

We also estimate a simple difference-in-difference version of Equation 2, in which event study dummies are collapsed into a single displacement indicator:

$$y_{idt} = \delta_1 D + \delta_2 Post + \delta_3 D{\cdot}Post + (\delta_0^E + \delta_1^E D + \delta_2^E Post + \delta_3^E D{\cdot}Post) \cdot \mathbb{1}(eligible = 1) \\ \alpha_i + \gamma_t + \gamma_d + + \beta' X_{idt} + \epsilon_{idt}, \quad (3)$$

where D is a dummy equal to 1 for displaced workers, $Post$ is equal to 1 in the 5 years after a displacement has occurred both for individuals who are actually displaced as well as those in the same event year who are not displaced. Thus, $D \cdot Post$ is the "treatment" of job loss on job security. The parameter of interest is δ_3^E, which captures the effect of job loss on job security for workers eligible to the lump-sum transfer. The difference-in-differences estimate of the eligibility effect is shown in the bottom right corner of Appendix Figure A.5: $\hat{\delta}_3^E$ is

positive and statistically significant, indicating that eligible workers experience a smaller decline in permanent employment due to displacement.

Given the definition of the main outcome variable, the reported results are *conditional* on finding a job in the years following displacement, which is the case for the majority of individuals in our sample period—83%, similar to findings in Card et al. (2007). This still raises the potential concern that selection into employment, whichever the contract type, could drive these differences between eligible and non-eligible workers. To address this issue, we redefine the outcome variable as equal to 1 for permanent workers and 0 for temporary workers and those without a job. Our results show that this concern is unfounded. The resulting *unconditional* difference-in-difference estimate of the effect for eligible workers is positive and statistically significant. Specifically, the decline in permanent employment is 6.5 percentage points smaller for eligible workers, compared to the conditional estimate of 4.6 percentage points, as shown in Appendix Figure A.5.[10]

Layoffs as a negative signal If firms have discretion over whom to retain or layoff, layoffs may provide a negative signal about worker's ability in the labor market, as hypothesized by (Gibbbons and Katz, 1991). Assuming that no such negative inference is warranted if workers are displaced due to firm closure, the observed differences in permanent employment between eligible and non-eligible workers after displacement might reflect differences in firms' inferences about their quality rather than a liquidity effect.

As discussed in subsection 2.2, firms in the Netherlands must follow the "last-in-first-out" rule when deciding whom to layoffs on economic grounds. This alleviates concerns that firms might selectively lay off low-ability workers. Nevertheless, as a robustness check, we restrict the analysis to mass layoffs, defined as firms with a workforce reduction of more than 30% within a year. These layoffs typically result from a negative external shocks at the firm level, rather than the characteristics or behavior of dismissed workers. Consequently, they are less "stigmatizing" for a worker, as adverse selection into the treatment group plays less of a role. The left panel in Figure 2 shows that our results remain unchanged, suggesting that

[10]We also consider a specification that again only uses the job finders before the end of the sampling period but this time to estimate the impact of eligibility for the lump-sum transfer on the probability of obtaining a permanent contract in the *first* job after displacement. The results are again consistent: eligible workers are more likely to secure a permanent contract in their first post-displacement job.

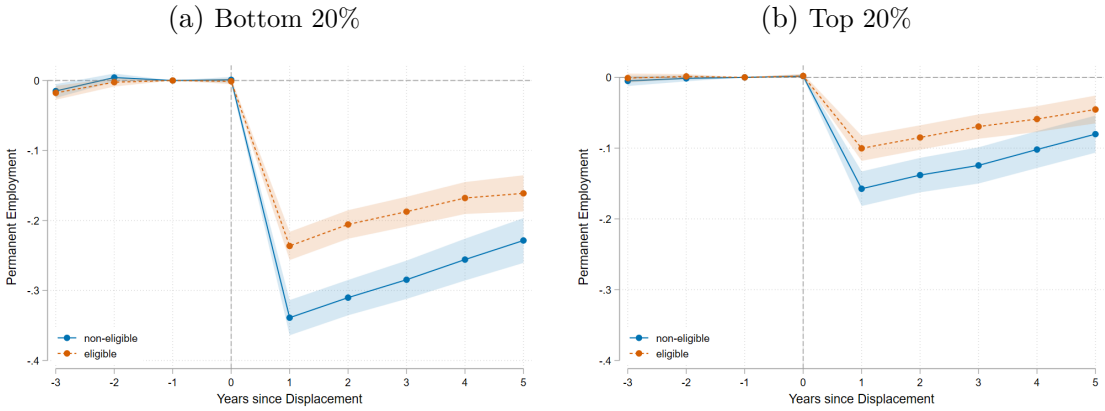
differences in permanent employment between eligible and non-eligible after displacement are not driven by negative signalling about ability.

Isolating the liquidity channel To further isolate the role of liquidity, we investigate whether the estimated eligibility effect differs between two groups: individuals with the highest 20% of liquid wealth, as defined in Section 2, and those with the lowest 20%. For both groups, the lump-sum transfer is approximately equal to one month salary (Figure 1). However, when compared to pre-displacement liquid wealth, the liquidity shock is notably larger for those in the lowest quintile. For the latter, the lump-sum transfer represents more than twice their liquid holdings, while among high-wealth individuals, this payment represents only 2% of liquid wealth. If liquidity constraints are driving the differential impact of displacement on job security between eligible and non-eligible workers, one would expect a lump-sum transfer to have a larger impact on those with low liquid wealth prior to displacement, and more likely to be liquidity-constrained.[11] The underlying assumption is that alternative explanations reflecting potential differences in unobserved characteristics between eligible and non-eligible workers, such as negative signalling, job search technology or risk aversion, would not predict a different effect of eligibility between high- and low-wealth workers.

Panel (a) and (b) of Figure 3 replicate Panel (a) of Figure 2 for both groups. Among those in the lowest quintile of liquid wealth, eligibility to a lump-sum transfer upon displacement reduces the loss in permanent employment by 10 pp within a year (Panel (a)): non-eligible workers experience a decline of 34%, compared to 24% for those eligible. In contrast, for high-wealth individuals, eligibility only reduces the drop in permanent employment by 5.7 pp (Panel (b)). For those with low liquid wealth, the sensitivity of permanent employment to cash-on-hand is thus nearly twice as high. This difference becomes more (less) pronounced with narrower (wider) wealth bands (Appendix Figure A.6). The gap in permanent employment between eligible and non-eligible workers persists among the bottom 20% until year 5 after displacement. This contrasts with high liquid wealth individuals

[11] Average liquid wealth of displaced workers at the bottom 20% of the liquid wealth distribution is around 850 euros, suggesting that many may not be in a position to smooth consumption while unemployed. This contrasts with an average of 160 thousand euros among those at the top 20%.

Figure 3: The Impact of Displacement on Permanent Employment by Eligibility and Wealth



Notes: Each panel plots estimates of δ_j obtained from Equation 1, estimated separately for displaced workers eligible for a lump-sum transfer upon displacement (orange line) and those non-eligible (blue line). Panel (a) and (b) restrict focus to individuals in the bottom and top 20% of the liquid wealth distribution, respectively. The outcome variable is a categorical variable that equals 1 for permanent workers and 0 for temporary workers j years since displacement. All regressions control for a quadratic polynomial in age, displacement-year fixed effects, calendar-year fixed effects and individual fixed effects. Shaded areas correspond to 99% confidence intervals. Appendix Table A.3 reports estimates and standard errors clustered at the individual level. Sample defined in Section 2.

for whom the gap in permanent employment disappears within five years of displacement. Difference-in-difference of the eligibility effect using Equation 3 for both groups show similar results. For the bottom 20%, eligibility reduces the drop in permanent employment by 5.9 percentage points in post-displacement period versus a 3.1 percentage point drop for the top 20%. Both estimates are significant at the 95% confidence level and significantly different from each other.[12]

It is possible that displaced workers with little liquid wealth tap into their illiquid wealth, specifically home equity, as an alternative income source (e.g. they can sell their property and relocate to a rental or smaller owned home). Appendix Figure A.7 shows that illiquid wealth, defined as the value of owned real estate assets minus mortgage owed, does not play a significant role in explaining future job security. Irrespective of the level of *illiquid* wealth, the impact of eligibility for a lump-sum transfer on permanent employment is high for workers with little liquid wealth (Panel (a) and (b)), but small for highly liquid workers

[12]While our baseline estimation uses a linear probability model, employing a logit model instead does not alter our main findings. Specifically, for the bottom 20% of the liquid wealth distribution, eligibility reduces the drop in permanent employment by 4.8 percentage points in the post-displacement period, whereas for the top 20%, the reduction is 2.4 percentage points. This implies that the eligibility effect is twice as large for workers in the bottom 20%, consistent with our baseline results.

(Panel (c) and (d)). Thus, consistent with Ebner (2010), home equity is not used as a buffer against negative shocks. These results add to recent findings on the the contrasting role of wealth 'type' after job loss. For instance, Fontaine et al. (2023) show that liquid assets affect reemployment probability, but not illiquid assets or liabilities, and Andersen et al. (2023) find that liquid savings are the most important mechanism unemployed workers use to compensate for lost income.

Together, our findings suggest that eligibility to a lump-sum transfer protects workers from severe scars in job security after displacement, reducing drops in permanent employment. However, this effect is nearly twice as large among those with little liquid wealth prior to job loss. Such heterogeneity suggests that the eligibility effect on job security is primarily driven by liquidity-related effects rather than other systematic worker differences. To further support this interpretation, Appendix Figure A.8 shows how the the impact of displacement in permanent employment varies with migrant background (at least one foreign-born parent), age and workers quality separately for those in the bottom versus top 20% of the liquid wealth distribution. The difference in the eligibility effect between low- and high-liquidity workers is similar within different population groups: native and migrant, old and young, and high and low-quality groups. This suggests that the difference in the eligibility effect between the bottom and the top 20% of the liquid wealth distribution does not reflect differences in these other dimensions.

Type of contract, Unemployment risk and Job Stability Thus far, we have used the type of labor contract held by the worker as a proxy for job security. In our context, as in most European labor markets, the difference in job security between permanent and temporary jobs is very salient to workers. As discussed in Section 2, permanent jobs have no termination date and entail high firing costs, whereas temporary jobs have a limited duration and involve negligible firing costs. An alternative way to measure job security is by examining the probability of separation into non-employment after job loss. Previous work has shown that displaced workers experience unstable employment relationships after job loss, with the probability of losing a job increasing in post-displacement years (e.g. Stevens, 1997; Jarosch, 2023).

In line with this, displaced workers in our sample are on average 14 percentage points more likely to experience a transition to non-employment when compared to non-displaced workers in post-displacement years. Given a yearly rate of job loss of around 4.7% in the whole sample of men working full-time, this amounts to a significant increase in the unemployment risk due to displacement. Consistent with our main findings, Appendix Figure A.9 shows that workers eligible for a lump-sum transfer upon job loss experience smaller losses in job security. Specifically, after displacement, they suffer a smaller decline in permanent employment and a smaller increase in the probability of separation into non-employment compared to non-eligible workers. The positive impact of eligibility is significantly larger for those at the bottom of the liquid wealth distribution across the various measures of job security used.

Mechanism A plausible explanation for our findings is that liquidity changes search behavior. Workers with low liquid wealth are likely to run into liquidity constraints sooner while unemployed, and a lump-sum transfer allows them to prolong job search, resulting in a more secure job. Several studies provide evidence of such mechanism (e.g. Card et al., 2007; Chetty, 2008; Basten et al., 2014; Griffy, 2021). Consistently, Appendix Table A.5 shows that eligibility to a lump-sum transfer prolongs average non-employment duration by two weeks. Importantly, this effect is driven by low-wealth individuals, where eligibility increases non-employment duration by more than two weeks (significant at 1%), while it is statistically insignificant among high-wealth individuals.[13]

Robustness We perform several robustness checks of our results. Results hold when we split the sample only in terms of holdings in savings accounts; net liquid wealth (liquid wealth minus debt excluding the mortgage value in the primary residence), and liquid wealth relative to the prior wage, which approximates how long workers can sustain previous expenditure level (Appendix Figure A.10). Appendix Figure A.11 shows that our results remain unchanged if we use a (more) balanced sample obtained by matching eligible and non-eligible workers on pre-displacement characteristics (Panel (a)) or using weights such that pre-displacement means of pre-displacement characteristics are equal between eligible

[13]We find similar results using a Cox regression, as shown in Columns 5 to 8 of Appendix Table A.5.

and non-eligible workers, as in Hainmueller (2012) (Panel (b)). Our results are also robust to alternative ways to build our sample. We relax firm size restrictions, use a random sample of control workers, focus on displacements occurring before 2010 with at least 5 years of post-displacement outcomes, and include workers experiencing multiple displacements, considering only the first event (Appendix Figure A.12). Overall, these robustness checks suggest that regardless of how we approach the data, we observe similar differences in permanent employment after displacement between eligible and non-eligible, which are mainly driven by those at the bottom of the liquid wealth distribution.

5 Implications for the Cost of Job Loss

Measuring the Wage Cost of Displacement To understand the implications of our findings for the cost of job loss, we first construct an individual-level estimate of the wage loss from displacement as in Schmieder et al. (2023). While we readily observe post-displacement wages, estimating the wage loss from displacement at the individual level requires a measure of counterfactual wages for each displaced worker. Leveraging on the fact that each displaced worker in our sample is matched with a non-displaced control worker, who closely resembles them in terms of pre-job loss observables, we compute an individual-level estimate of the wage impact of displacement for each treated-control worker pair, following Schmieder et al. (2023):

$$\Delta w_{idj} \;=\; \Delta_d w_{idj} \;-\; \Delta_{nd} w_{idj} \tag{4}$$

where Δw_{idj} is the the estimate of the individual treatment effect from job loss after j years. $\Delta_d w_{idj}$ is the difference between pre-displacement wage and the wage j years after job loss for worker i displaced in year d and $\Delta_{nd} w_{idj}$ is the wage change for the matched non-displaced worker.

Using the log daily wage, we compute individual-level job loss effects on wages 1 and 5 years after displacement relative to 1 year before displacement. Consistent with prior work, the average wage loss in the year following displacement is 17%. After five years, wages remain 14% lower. However, displaced workers non-eligible for the lump-sum transfer experience a substantially larger loss relative to those eligible. In particular, within 5 years of

displacement, eligibility attenuates the wage loss in 5 pp (Table 2, Column 1). Mimicking the patterns documented for permanent employment in Section 4, the eligibility effect matters most for low-liquidity workers. Among displaced workers at the bottom 20% of the liquid wealth distribution, a lump-sum transfer worth around 1 month of salary reduces wage loss by 7.7 pp, while for those at the top 20% this gap is not statistically significant.

Our estimates suggest that the scarring effects of displacement are more severe for workers with low liquid wealth upon job loss. While this is consistent with findings of Griffy (2021) and Kaila et al. (2021), it seem at odds with other studies that reported no effect of cash-on-hand on post-displacement wages (e.g. Card et al., 2007; Basten et al., 2014; Fontaine et al., 2023). This discrepancy can be reconciled if one takes into account that liquid wealth likely diminishes during a jobless spell, in the spirit of Nekoei and Weber (2017).[14] In a direct search framework with risk averse workers, as in Griffy (2021) or Eeckhout and Sepahsalari (2023), the relationship between wealth and post-displacement wages results from two opposing forces. On the one hand, higher liquid wealth allows workers to be more selective in their job search, leading to higher subsequent wages. On the other hand, higher liquid wealth can result in longer periods of unemployment, which reduces subsequent wages. Therefore, variations in the wealth-wage effect may arise from differences in the relative importance of the selectivity and search margins across different populations.

Next, we explore how much the loss of job security contributes to the wage cost of displacement, and to what extent the differences between eligible and non-eligible workers are driven by the differences in post-displacement job security uncovered in the previous section.

Job Security and Wage Loss To investigate the role of job security loss in the wage cost of displacement, we adopt the method proposed by Schmieder et al. (2023) and estimate:

$$\Delta w_{idj} = \beta \cdot temporary_{ij} + \delta\, \hat{\alpha}_i + \gamma\, \hat{\psi}_{J(id)} + \xi\, \Delta\hat{\psi}_{J(id)} + \theta' X_i + \pi_1\, t + \pi_2\, t^2 + \epsilon_{ijd}, \quad (5)$$

where Δw_{idj} is the individual-level estimate of the wage loss j years after displacement for worker i displaced in year d, and $temporary_{ij}$ is an indicator variable for displaced workers

[14] For instance, Andersen et al. (2023) show that liquid savings are the primary source of self-insurance during unemployment.

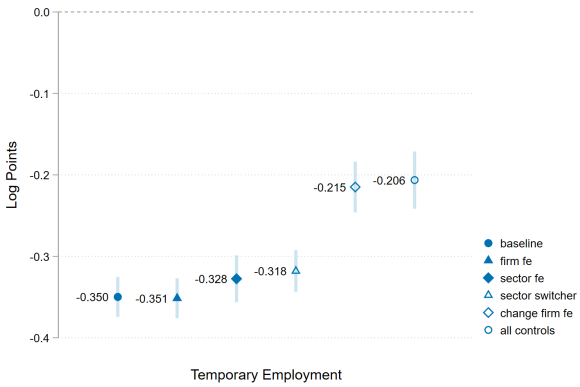
being in a temporary job or not j years after displacement. To control for differences in worker characteristics, we add an estimate of worker fixed effect, $\hat{\alpha}_i$, a migrant background dummy and quadratic polynomials for age and prior job tenure, that captures the potential loss in firm-specific skills (Topel, 1990; Jacobson et al., 1993). We also control for other potential explanatory factors behind the cost of job loss. First, we control for both the estimated fixed effect of the displacing firms, $\hat{\psi}_{J(id)}$, and the change of the firm effect after job loss relative to the control group, $\Delta\hat{\psi}_{J(id)}$ (Helm et al., 2022; Bertheau et al., 2023; Schmieder et al., 2023). We obtain firm and worker fixed effects from a standard AKM model using spells of full-time male workers aged 20 to 55 years old, and compute $\Delta\hat{\psi}_{J(id)}$ in the same way as Δw_{ijd}.[15] Additionally, we add an indicator variable for switching sector after displacement to capture losses in sector-specific skills (Neal, 1995; Huckfeldt, 2022), and post-displacement sector fixed effects (Jacobson et al., 1993). We also include a quadratic time trend.

The parameter of interest, β, measures wage loss differences between workers with a temporary contract j years after displacement and those with a permanent one. Given the negative mean of Δw_{idj}, if $\beta < 0$ workers with a temporary contract after displacement have larger wage losses than permanent workers. Figure 4 reports the $\hat{\beta}$'s from specifications of equation 5 which sequentially includes controls.

Starting from a specification including only worker-level controls, we find that being a temporary worker is associated with a wage loss 35% larger 5 years after displacement. Controlling for the estimated fixed effect of the displacing firm, post-displacement sector fixed effects and a sector switcher dummy barely changes the estimated coefficient. However, once we introduce the change in employer-specific wage premiums, $|\hat{\beta}|$ becomes smaller. This suggests that part of the wage loss effect of losing job security can be attributed to workers reallocating into lower-paying firms. The estimated coefficient changes little when we reintroduce all other controls and the effect of losing job security on wage losses remains statistically and economically significant. The wage loss is on average 21% higher for dis-

[15]Since job loss may affect the premiums firms pay their employees, we exclude post-displacement observations of displacement firms, displaced workers, and corresponding control workers. Moreover, we mitigate concerns regarding limited mobility bias by partitioning firms in the bottom 10% of the firm size distribution into 20 distinct clusters of firms with a similar wage distribution (i.e. the discretized empirical CDF of their log wages) using the K-means clustering algorithm (Bonhomme et al., 2019).

Figure 4: Losses in Job Security and the Cost of Job Loss



Notes: Figure displays estimates of β obtained from Equation 5. The dependent variable is the individual-level estimate of wage loss Δw_{idj}, i.e., the change in the log daily wage compared to a matched control observation 5 years after displacement relative to the year prior to displacement (Equation 4). Baseline controls include quadratic polynomial in pre-displacement age and job tenure, a migrant background dummy, estimated worker fixed effects and a quadratic time trend. The remaining specifications introduce the specified control: *firm fe* adds the estimated fixed effect of the displacing firm, *sector fe* adds post-displacement sector fixed effects, *sector switcher* adds a sector switcher dummy, *change firm fe* adds the change in employer-specific wage premiums. Mean of dependent variable equals -14%. Sample defined in Section 2.

placed workers with temporary contract 5 years after job loss, relative to an average wage loss of 14% for all displaced workers. Reassuringly, we obtain a similar estimate if displaced workers are instead assigned a random control observation who satisfies the same tenure, firm size and age restrictions (Appendix Table A.4).

Decomposing the Wage Loss Gap In light of these findings, we investigate the extent to which the gap in wage losses between eligible and non-eligible can be attributed to differences in job security after displacement, as documented in Section 4. We address this question by employing an Oaxaca-Blinder decomposition which allows us to disentangle the percentage of the difference in wage loss between these two groups that can be attributed to observable characteristics from the part that remains unexplained (Kaila et al., 2021; Bertheau et al., 2023). We first regress the individual-level job loss effect (Δw_{id}) on a vector of worker and employer observables X, including a quadratic polynomial in prior job tenure and age, a migrant background dummy, estimated worker and displacing firm fixed effects, post-displacement sector fixed effects, a sector switcher dummy, the change in firm fixed effects after job loss ($\Delta\hat{\psi}_{J(id)}$), and a binary variable for the contract type (permanent versus temporary). Using the estimated coefficients, we decompose the average gap in wage loss 5

Table 2: Decomposition of Differences in the Wage Loss between Eligible and Non-eligible Workers

	Wage Loss	Explained		Unexplained
	Gap	Job Security	Others	Total
Full Sample	-4.97 (0.81)	-1.03	-1.88	-2.06
Bottom 20%	-7.72 (1.93)	-1.04	-4.97	-1.70
Top 20%	-0.96 (2.25)	–	–	–

Notes: Column 1 shows the wage loss gap between eligible and non-eligible workers five years after displacement, with standard errors in parenthesis. Columns 2-5 report the part that is explained by differences in job security loss, captured by an indicator variable that equals one if the individual is a temporary worker 5 years after displacement and 0 otherwise (Column 2), changes in firm fixed effect (Column 3), other post-displacement observables (sector fixed effects and sector change) (Column 4) and pre-displacement observables (quadratic polynomial in prior job tenure and age, a migrant background dummy, estimated worker fixed effects, estimated fixed effects of the displacing firm) (Column 5). Column 6 reports the part unexplained by average differences in observables. Estimates are based on Equation 6. Magnitudes are reported in percentage points. Sample of displaced workers defined in Section 2.

years after displacement between eligible (e) and non-eligible (ne) workers into the explained and unexplained portions as follows:

$$\Delta_j = \underbrace{\sum_{x \in X} \hat{\beta}_x^* (E[x_{i,e}] - E[x_{i,ne}])}_{\text{Explained}} + \underbrace{\sum_{x \in X} (\hat{\beta}_x^e - \hat{\beta}_x^*) E[x_{i,e}] + (\hat{\beta}_x^{ne} - \hat{\beta}_x^*) E[x_{i,ne}]}_{\text{Unexplained}} \tag{6}$$

where $\hat{\beta}_x^*$ is coefficient associated with observable x_i from the pooled regression over both groups (Neumark, 1988).

Table 2 reports the results. Observable differences in job security (Column 2) explain a significant portion of the wage loss gap between eligible and non-eligible workers (Column 1). Specifically, out of a 5 pp gap, 21% is due to differences in job security after displacement (Column 2). For low-wealth individuals, differences in job security account for 13% of the 7.7 pp wage loss gap.[16] Overall, we conclude that being eligible to a lump-sum transfer worth one month of salary protects displaced workers against more severe scars in job security, significantly reducing the wage cost of job loss. The results also suggest that eligibility helps workers in finding jobs in high-quality firm, specially those with low-wealth (Column 3).

[16] As reported in the last row of Table 2, this gap is small and not statistically significant for high liquid-wealth individuals so we do not decompose its effects.

6 Conclusion

Using rich administrative data from the Netherlands and quasi-experimental variation in liquid wealth upon job loss, we investigate the impact of displacement on job security and the role of liquidity. Within a year of displacement, the likelihood of having a permanent job drops by 19%. Five years later, displaced workers still experience lower job security. Importantly, eligibility to a lump-sum transfer when displaced mitigates losses in job security, reducing the drop in permanent employment by 9 percentage points. This effect is mainly driven by liquid-constrained workers, while we find no heterogeneity in other dimensions. Our findings thus suggest that cash-on-hand is a key mechanism linking displacement to job security.

We also show that losing job security plays an important role in the wage cost of displacement, explaining a significant fraction of the wage loss gap between eligible and non-eligible workers. Policy-wise, our findings highlight the role of social transfers in protecting against negative job security shocks —and its associated large wage costs —that are large and lasting following job loss, particularly for those that are more likely to be financially constrained.

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Appendix

Table A.1: Descriptive Statistics, Permanent vs. Temporary Workers

	Temporary	Permanent
Age (years)	31.8	40.2
Migrant Background (%)	41.8	17.7
Tenure (years)	2.2	9.4
Annual wage (000 Euros) (000 euros)	12.5	41.4

Notes: The table reports average characteristics of temporary and permanent workers. Earnings are deflated and measured in 2015 Euros. The sample consists of full-time male workers whose earnings are higher than €1000 per month between 2004 and 2015.

Table A.2: Descriptive Statistics

	Non-displaced Workers		Displaced Workers	
	Matched	Random	Matched	Full
Age (years)	38.2	38.5	38.2	38.6
Migrant Backgroumd (%)	17.5	14.7	19.5	19.3
Wealth (000 Euros)				
Savings	25.9	34.2	25.0	25.6
Liquid wealth	32.7	43.8	30.8	31.6
Net illiquid wealth	62.5	72.4	59.7	60.5
Job				
Tenure (years)	9.3	10.4	9.3	9.7
Permanent Employment (%)	98.9	99.1	98.8	98.8
Annual wage (000 Euros)	42.3	48.4	42.0	41.7
Firm size	95.2	129.5	89.0	85.8
Industry				
Manufacturing (%)	33.8	25.8	34.1	30.3
Wholesale and retail trade (%)	22.4	11.6	22.9	23.0
Construction (%)	17.8	19.6	17.3	16.3
Real Estate (%)	16.1	16.7	15.9	15.2
Transportation (%)	5.8	7.6	5.7	6.8
Observations	38,396	45,308	38,925	49,618

Notes: The table reports average characteristics of displaced and non-displaced workers one year before displacement. Column 1 and 2 display characteristics of matched non-displaced and a random sample of non-displaced workers that satisfy the same age, tenure and firm size restrictions as displaced workers. The sample in column 3 and 4 correspond, respectively, to displaced workers that are successfully matched to control workers using the matching process described in Section 2 and the full sample. All wealth variables and wages are deflated and measured in 2015 Euros. Sample defined in Section 2

Table A.3: The Effect of Displacement on Permanent Employment

	Full Sample			Bottom 20%		Top 20%	
	All	Non-Eligible	Eligible	Non-Eligible	Eligible	Non-eligible	Eligible
-3	-0.0023	-0.0064	-0.0046	-0.0150	-0.0177	-0.0050	-0.0010
	(0.0009)	(0.0014)	(0.0013)	(0.0038)	(0.0038)	(0.0028)	(0.0025)
-2	0.0052	0.0035	0.0011	0.0042	-0.0024	-0.0015	0.0013
	(0.0006)	(0.0008)	(0.0008)	(0.0022)	(0.0024)	(0.0016)	(0.0013)
-1	0	0	0	0	0	0	0
	0	0	0	0	0	0	0
0	0.0060	0.0032	0.0027	0.0010	-0.0011	0.0017	0.0020
	(0.0004)	(0.0006)	(0.0006)	(0.0016)	(0.0016)	(0.0010)	(0.0011)
1	-0.1926	-0.2479	-0.1578	-0.3389	-0.2364	-0.1573	-0.1002
	(0.0025)	(0.0042)	(0.0031)	(0.0097)	(0.0078)	(0.0095)	(0.0069)
2	-0.1676	-0.2139	-0.1394	-0.3102	-0.2057	-0.1381	-0.0849
	(0.0025)	(0.0041)	(0.0031)	(0.0098)	(0.0079)	(0.0094)	(0.0067)
3	-0.1473	-0.1922	-0.1214	-0.2847	-0.1874	-0.1244	-0.0695
	(0.0026)	(0.0044)	(0.0031)	(0.0106)	(0.0082)	(0.0099)	(0.0067)
4	-0.1294	-0.1720	-0.1053	-0.2558	-0.1679	-0.1020	-0.0589
	(0.0027)	(0.0047)	(0.0033)	(0.0116)	(0.0088)	(0.0101)	(0.0071)
5	-0.1167	-0.1493	-0.0979	-0.2286	-0.1612	-0.0802	-0.0454
	(0.0030)	(0.0050)	(0.0038)	(0.0124)	(0.0100)	(0.0102)	(0.0076)
N	826,797	360,968	468,187	84,321	105,906	47,246	61,693

Notes: Table reports event time coefficients underlying Panel (a) in Figure 2 and Panels (a) and (b) in Figure 3. Estimates are obtained from Equation 1. The outcome variable is a binary variable that takes value one if an individual is employed under a permanent contract and 0 if the individual has a temporary contract. All regressions control for a quadratic polynomial in age, displacement year, calendar year and individual fixed effects. Standard errors clustered at the individual level in parentheses. Sample defined in Section 2.

Table A.4: Losses in Job Security and the Cost of Job Loss

	Dependent variable: Wage Loss 5 years after Displacement					
	Baseline (1)	Firm FE (2)	Switcher (3)	Sector FE (4)	Change Firm FE (5)	All (6)
Panel A: Matched Controls						
Temporary$_i$	-0.350***	-0.351***	-0.318***	-0.328***	-0.215***	-0.206***
	(0.015)	(0.015)	(0.016)	(0.018)	(0.019)	(0.021)
Change Firm FE					0.532***	0.538***
					(0.046)	(0.057)
Observations	10,115	10,115	10,115	10,115	10,115	10,115
R^2	0.086	0.095	0.094	0.108	0.200	0.214
Panel B: Random Controls						
Temporary$_i$	-0.324***	-0.325***	-0.297***	-0.317***	-0.172***	-0.174***
	(0.014)	(0.014)	(0.014)	(0.016)	(0.018)	(0.020)
Change Firm FE					0.609***	0.638**
					(0.0447)	(0.0592)
Observations	12,933	12,933	12,933	12,933	12,933	12,933
R^2	0.089	0.100	0.096	0.107	0.212	0.225

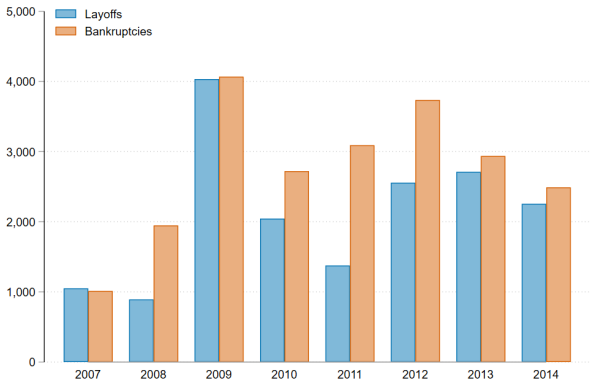
Notes: Table reports estimates of β obtained from Equation 5. $Temporary_i$ is a binary variable equal to 1 if the displaced worker is a temporary worker 5 years after job loss. In Panel A, displaced workers are matched with a similar control, following the matching procedure described in Section 2, while in Panel B they are matched with a random control. All columns include quadratic polynomials in pre-displacement age and job tenure, a migrant background dummy (an indicator variable for individuals with at least one parent foreign born), estimated worker fixed effects and a quadratic time trend. Columns 2 to 5 add the control referred in the header to the baseline. Column 5 includes all controls. Mean of the dependent variable is -14%. Robust standard errors in parenthesis. Sample consists of displaced workers as defined in Section 2.***, ** and * represent statistical significance at 1%, 5% and 10% levels, respectively.

Table A.5: The Effect of Eligibility on Duration

	Completed Duration				Cox Proportional Model			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Eligible_i	1.881***	2.182***			-0.058***	-0.075***		
	(0.251)	(0.254)			(0.019)	(0.019)		
Bottom 20% · Eligible_i			2.137***	2.137***			-0.090**	-0.082**
			(0.525)	(0.532)			(0.038)	(0.039)
Top 20% · Eligible_i			1.213*	1.094			-0.032	-0.021
			(0.675)	(0.675)			(0.053)	(0.053)
Controls		X		X		X		X
Observations	11,467	11,467	4,091	4,091	11,465	11,465	4,091	4,091

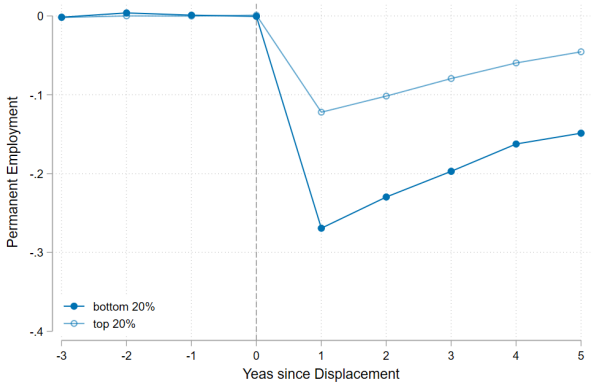
Notes: In columns 1 to 4, the dependent variable is defined as completed duration (in weeks) until re-employment, censored after 1 year. Columns 5 to 8 report estimates from a Cox hazard model. *Eligible*$_i$ is a indicator variable for displaced workers eligible to receive a lump-sum transfer upon job loss. Bottom (Top) 20% · Eligible_i captures the difference between eligible and non-eligible among the bottom (top) 20%. Controls include quadratic polynomials in age and job tenure, a migrant background dummy, pre-displacement sector fixed effects and displacement year-fixed effects. Robust standard errors in parenthesis. Sample consists of displaced workers that took unemployment insurance within 1 year after job loss (as in Chetty, 2008) and found a job within a year after displacement. ***, ** and * represent statistical significance at 1%, 5% and 10% levels, respectively.

Figure A.1: Displacements per Year, 2007-2014



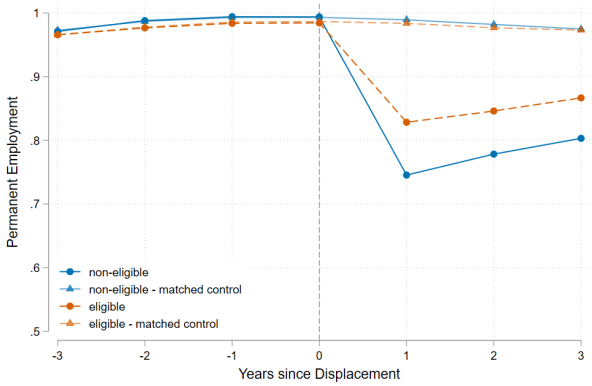
Notes: Figure displays number of displacements per year in the matched sample. *Bankruptices* correspond to workers that lost their job due to firm closure as a result of a bankruptcy. $Layoffs$ correspond to workers dismissed due to other economic reasons. Sample defined in Section 2

Figure A.2: Permanent Employment after Displacement by Wealth: Descriptive Evidence



Notes: Figure displays the difference in permanent employment between displaced and non-displaced workers 3 years before and 5 years after the job loss by pre-displacement liquid wealth. Bottom 20% and Top 20% consist of displaced and non-displaced workers in the bottom and top 20% of the liquid wealth distribution, respectively. Sample defined in Section 2.

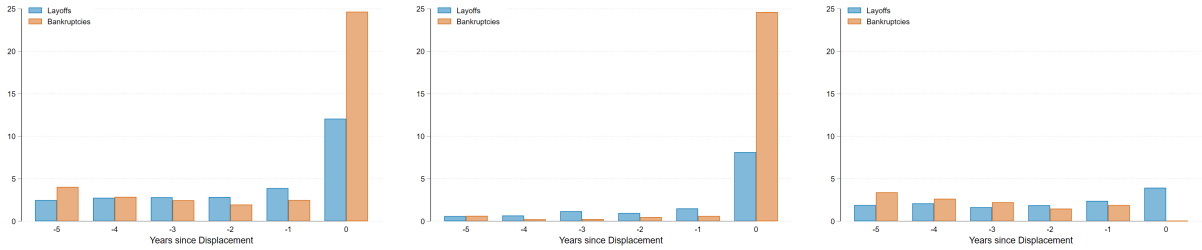
Figure A.3: Raw patterns of Permanent Employment



Notes: Figure displays the average permanent employment rates for eligible and non-eligible displaced workers, both before and after displacement, alongside the corresponding matched control workers. Sample defined in Section 2.

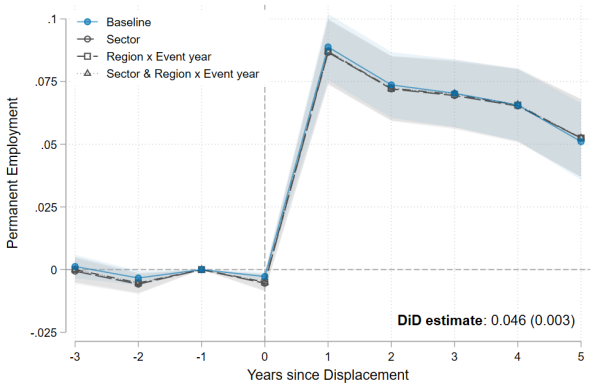
Figure A.4: Separations in firms in pre-displacement years

(a) Total (b) Involuntary (c) Voluntary



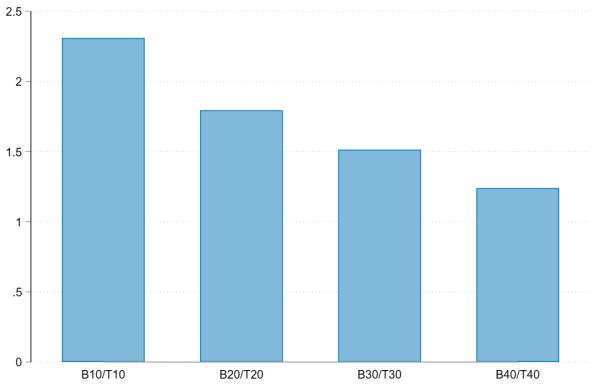
Notes: Figure displays the average number of job separations during pre-displacement years in firms undergoing economic layoffs or closure at time 0. It focuses on separations of male workers aged 25-50 who have been with the firm for at least three years, matching the criteria for displaced workers.. Panel (a) shows the average total separations. Panel (b) shows terminations from layoffs due to economic reasons or firm closure, these are *involuntary* separations. Panel (c) covers all other separations, including quits and involuntary terminations due to reasons like long-term illness, which we label *voluntary*.

Figure A.5: Eligibility Effect on Permanent Employment



Notes: Each panel plots estimates of δ_j^E obtained from Equation 2. The baseline controls include for a quadratic polynomial in age, displacement-year fixed effects, calendar-year fixed effects and individual fixed effects. Shaded areas correspond to 99% confidence intervals. Standard errors clustered at the individual level. Sample defined in Section 2.

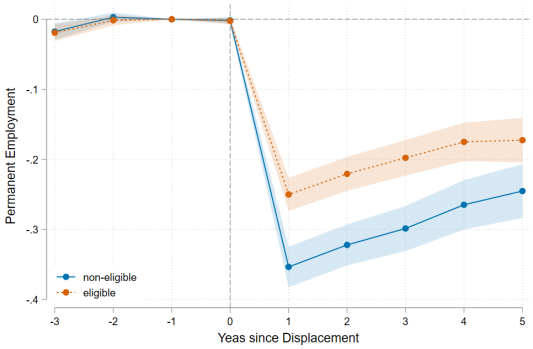
Figure A.6: Eligibility Effect on Permanent Employment 1 Year after Displacement: Bottom vs. Top 10%, 20%, 30% and 40%



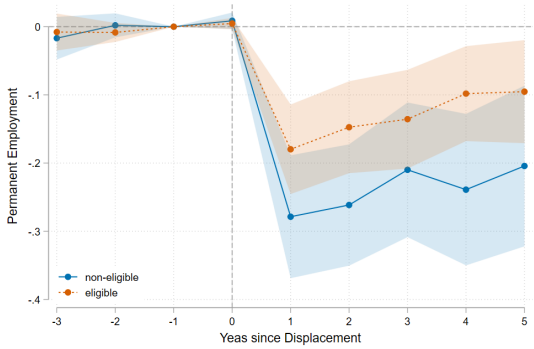
Notes: Figure displays the eligibility effect, that is the difference in the impact of job loss on permanent employment between non-eligible and eligible workers, in the bottom of the liquid wealth distribution relative to the top for different wealth bands. Sample defined in Section 2.

Figure A.7: Eligibility effect by Liquid and Illiquid Wealth

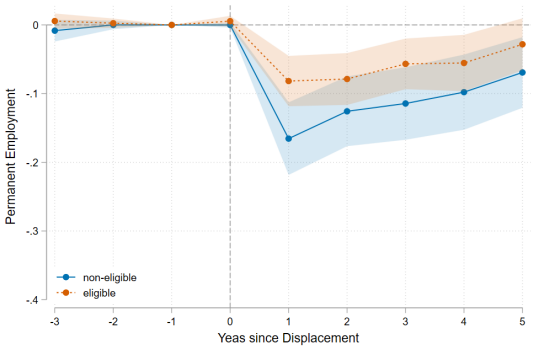
(a) Low Liquidity: Low Illiquid Wealth



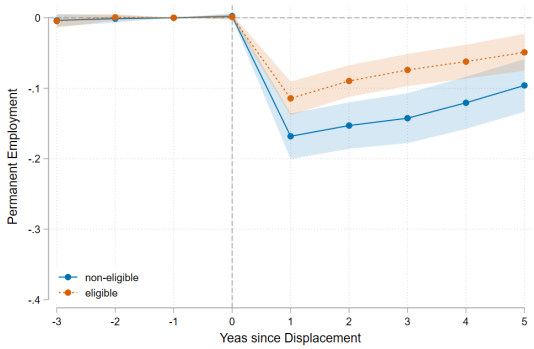
(b) Low Liquidity: High Illiquid Wealth



(c) High Liquidity: Low Illiquid Wealth

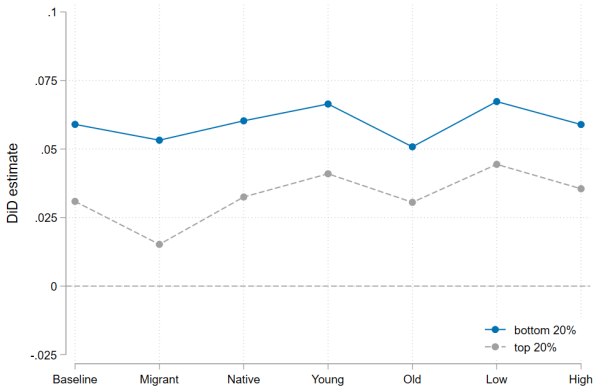


(d) High Liquidity: High Illiquid Wealth



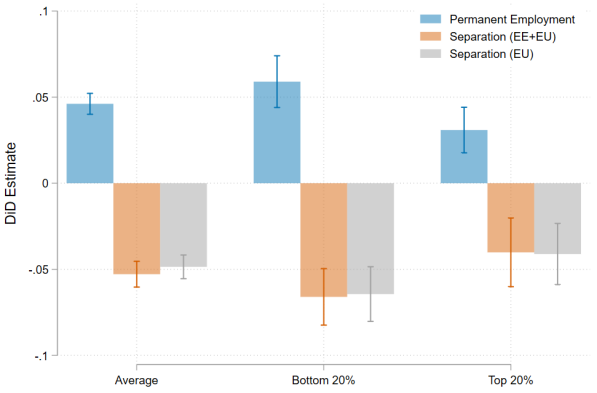
Notes: Panel (a) and (b) show, respectively, the eligibility effect on permanent employment after displacement for displaced workers in the bottom 20% of liquid wealth and in the bottom and top 30% of the net illiquid wealth distribution. Panel (c) and (d) show, respectively, the eligibility effect on permanent employment after displacement for displaced workers in the top 20% of liquid wealth and in the bottom and top 30% of the net illiquid wealth distribution. Shaded areas correspond to 99% confidence intervals. Sample defined in Section 2.

Figure A.8: Eligibility Effect: Bottom vs Top 20% along Different Dimensions



Notes: Figure shows the eligibility effect, that is the difference in the impact of displacement on permanent employment between eligible and non-eligible workers in the bottom and top 20% of the liquid wealth distribution for different groups. *Migrant* are workers with at least one foreign-born parent, *Native* are workers with native parents, *Low* and *High* are workers who fall into the top and bottom 40% of the distribution of estimated worker fixed effects using a standard AKM regression. Results are based on DiD estimates obtained from Equation 3, estimated separately for each group. Standard errors are clustered at the individual level. Sample defined in Section 2.

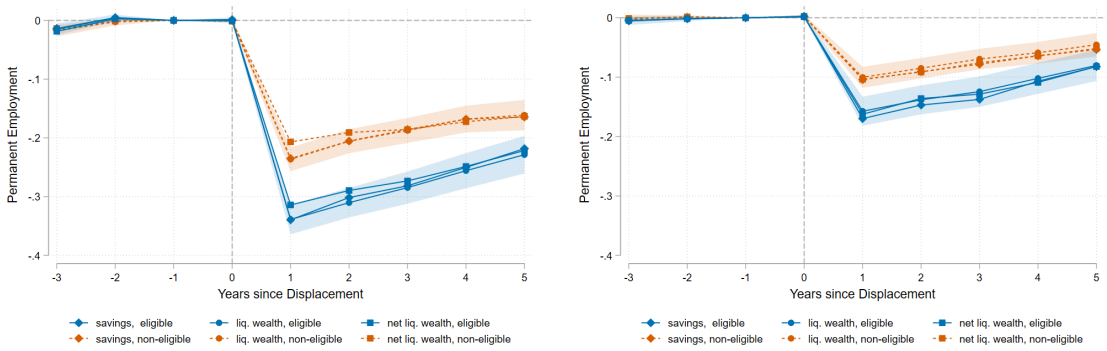
Figure A.9: Eligibility Effect: Permanent Employment vs. Likelihood of Separation



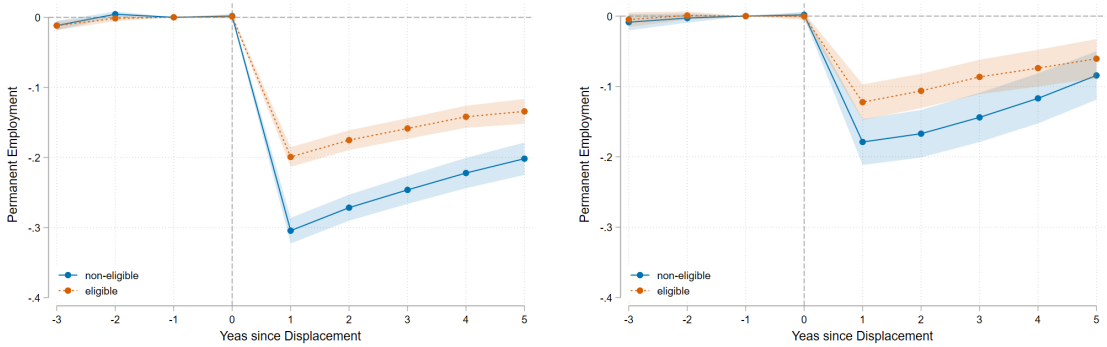
Notes: Figure shows the eligibility effect, that is the difference in the impact of displacement on job security between eligible and non-eligible workers. Job security is proxied by the likelihood of having a permanent contract (our main outcome variable), likelihood of job separation (including job-to-job transitions (EE) and employment to non-employment transitions (EU)) and likelihood of separation to non-employment (EU). Results are based on DiD estimates obtained from Equation 3, estimated using the whole sample and separately for the bottom and top 20% of the liquid wealth distribution. Standard errors are clustered at the individual level. Sample defined in Section 2.

Figure A.10: Robustness Checks: Alternative Definitions

(a) Savings. Liquid Wealth & Net Liquid Wealth



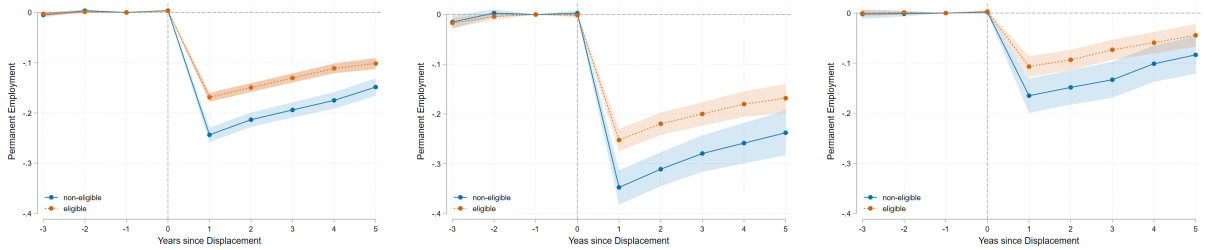
(b) Liquid Wealth relative to Monthly Wage



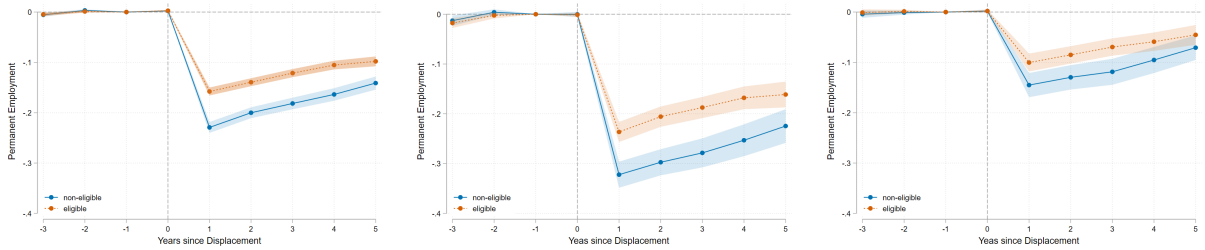
Notes: Each panel replicates Panel (a) (left) in Figure 2, and Panels (a) (middle) and (b) (right) in Figure 3. In Panel (a), the sample is split into high- and low-wealth on the basis of whether households' savings account, liquid wealth (savings accounts + bonds + stocks), and net liquid wealth (liquid wealth - other debt, including debt in credit cards and mortgages related to real estate that is not primary residence of the household) are in the bottom or top quintile of their respective distribution. In Panel (b), the sample is split according to the number of pre-displacement monthly wages covered by pre-displacement liquid wealth. The left panel corresponds to displaced workers for which pre-displacement liquid wealth does not cover more than two monthly salaries, the right panel restricts the sample to displaced workers for which pre-displacement wealth covers more than two years of monthly wages. Shaded areas correspond to 99% confidence intervals. Sample defined in Section 2.

Figure A.11: Robustness Checks: Balanced Sample of Eligible and Non-eligible Workers

(a) Coarse Exact Matching



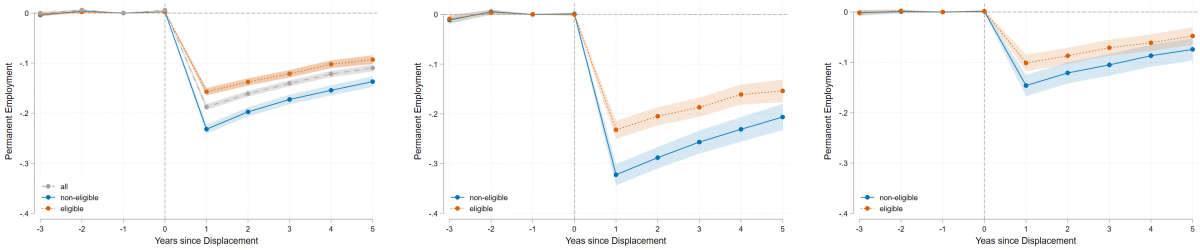
(b) Entropy Balancing



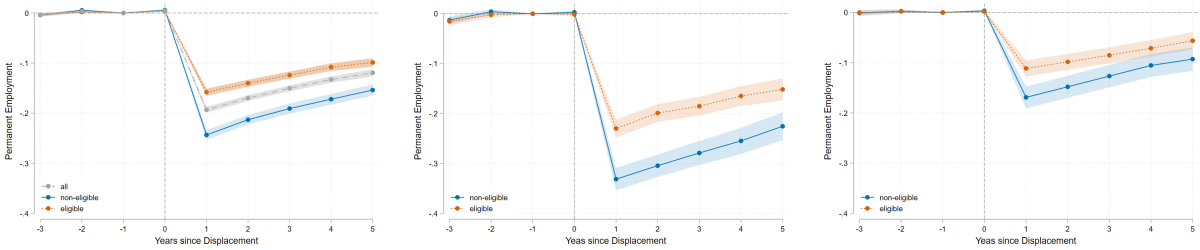
Notes: Each panel replicates Panel (a) (left) in Figure 2, and Panels (a) (middle) and (b) (right) in Figure 3 using a balanced sample of eligible and non-eligible workers. Panel (a) uses a balaced sample obtained by matching eligible and non-eligible on a set of pre-displacement characteristics as outlined in subsection 2.2. Panel (b) uses matching weights to estimate Equation 1. Non-negative matching weights are estimated beforehand so that the mean of pre-displacement covariates for the weighted sample of non-eligible workers is identical to that of the eligible workers (Hainmueller, 2012). Shaded areas correspond to 99% confidence intervals. Sample defined in Section 2.

Figure A.12: Robustness Checks: Alternative Samples

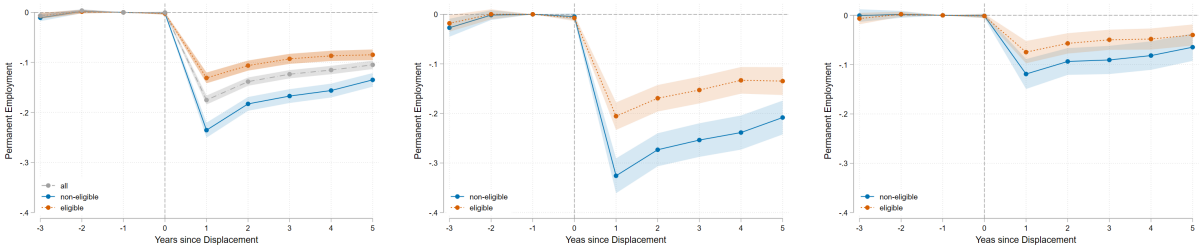
(a) No Firm Size Restriction



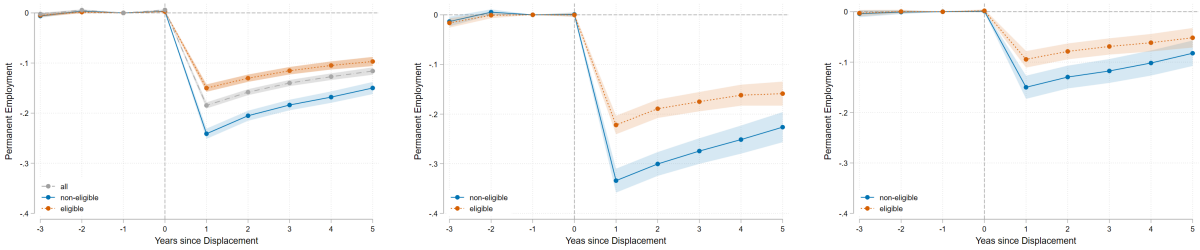
(b) Random Control Sample



(c) Displacements between 2007 and 2010



(d) More than one Displacement



Notes: Each panel replicates Panel (a) (left) in Figure 2, and Panels (a) (middle) and (b) (right) in Figure 3. Panel (a) relaxes the firm size restriction, Panel (b) uses a random sample of control workers; Panel (c) restricts the sample to displacements between 2007 and 2010 and Panel (d) includes also workers that experience more than one displacement over the analysis period, but considering only the first displacement as an event in the analysis. Shaded areas correspond to 99% confidence intervals. Sample defined in Section 2.