Collective Wage Co-ordination and the Costs of Job Displacement[a]

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March, 2026

Abstract

This paper investigates whether a higher level of co-ordination in collective wage bargaining affects the wage costs of job displacement. We use quasi-exogenous variation in the timing of job loss due to mass layoffs spanning an institutional reform that introduced national ceilings to wage agreements negotiated at sectoral- and firm-level—the 1996 Belgian Wage Norm. We find that average earnings losses over a ten-year period after displacement are roughly half as large under the more coordinated wage bargaining system. While business cycle conditions may contribute to some earnings differences, several factors suggest that cyclical effects are unlikely to be the primary driver of our results. The attenuation stems from faster re-employment and improved sorting into higher-ranked firms, consistent with wage compression facilitating quicker job transitions and limiting downward mobility after job loss. These effects are concentrated among high-skill service workers.

Keywords: Job displacement, Coordinated wage bargaining
JEL codes: J31, J51, J63

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1 Introduction

How to ease the burden of layoffs and unemployment on displaced workers and their families is a central topic of policy discussion. Extensive research shows that the earnings losses for displaced workers are severe, long-lasting, and countercyclical (Jacobson et al., 1993; Couch and Placzek, 2010; Schmieder and Von Wachter, 2010). While short-term consequences of unemployment can often be successfully addressed with policies such as unemployment insurance benefits, designing government policies that reduce the costs of long-term adjustment remains challenging.

A growing body of literature investigates the significant costs of job loss and the relative contributions of labor market forces and institutions in shaping the employment trajectories of displaced workers. On the labor market forces side, a common hypothesis posits that displaced workers lose firm-, industry-, or occupation-specific skills (Lachowska et al., 2020; Huckfeldt, 2022; Raposo et al., 2021). Recent research also emphasizes persistent firm-level wage differentials as key determinants of job loss costs (Schmieder et al., 2023; Fackler et al., 2021; Bertheau et al., 2023).

On the institutional side, a revitalized literature highlights the role of unions and collective bargaining in mitigating inequality (Farber et al., 2021; Biasi and Sarsons, 2022; Kauhanen, 2024; Jäger et al., 2025). Empirical evidence indicates that country-specific institutional features—especially the prevalence of sector-level bargaining agreements—help explain why job-loss costs are higher in the United States than in many European countries. Complementary research links trade-union coverage to both the risk of job loss and workers' perceptions of job insecurity (Blanchflower et al., 2022). However, causal evidence on the impact of changes in wage-setting institutions on job loss costs remains scarce, particularly evidence derived from within-country analyses.

This paper examines how collective-bargaining structures shape the wage costs of job loss, exploiting Belgium's Wage Norm (*norme salariale*)—an institutional reform that capped sector- and firm-level wage premia and strengthened national coordination by imposing an economy-wide ceiling on wage growth. We label the period before the reform the "Flexible Pay" regime and the period after the reform the "Wage Norm" regime. Leveraging plausibly exogenous variation in the timing of mass-layoff-induced job separations across the two regimes, we compare earnings and employment losses for displaced workers under these distinct wage-setting systems. The analysis draws on Belgian social security microdata spanning two decades of job displacements, offering

detailed information on male workers' employment transitions, earnings trajectories, and firm- and worker-level characteristics.

We find that displaced workers incur sizable earnings losses under both wage-setting regimes, but the losses are consistently smaller when the Wage Norm is in place. Under the pre-reform Flexible Pay system, annual earnings fall by an average of 19 percent over the ten years following displacement. Under the post-reform Wage Norm system, the average loss is roughly 10 percent—about half as large. Earnings in the Wage Norm regime also show signs of recovery: the penalty shrinks from about 22 percent in the layoff year to roughly 5 percent a decade later. By contrast, in the Flexible Pay regime the initial 23 percent drop plateaus at around 15–18 percent below the pre-displacement level and shows no meaningful rebound after ten years.

We then explore potential mechanisms underlying these findings, examining whether and how employment dynamics contribute to the observed earnings differences between the two wage-setting regimes. Our analysis indicates that workers displaced under the more coordinated Wage Norm regime experience less severe employment losses. On the extensive margin, over the first five years post-displacement, their average employment rate is 4.5 percent lower than that of non-displaced workers, compared to a decrease of 7.6 percent observed in the more flexible regime. On the intensive margin, Flexible Pay workers average about 36 fewer days of work per year over the same period, whereas Wage Norm workers lose about 29 days—a gap of roughly one working week. After this period, gaps in both employment rates and days worked narrow but do not disappear, with both groups ultimately exhibiting sustained declines relative to non-displaced workers after ten years.

Beyond employment dynamics, we identify several additional mechanisms that may contribute to the earnings differences across wage-setting regimes. First, by estimating our primary model using firm rank based on pre-displacement wages, we find that workers displaced after the reform are more likely than non-displaced workers to transition into higher-ranked firms. These results suggest that the rigid wage-norm framework offers some protection against long-term scarring (Raposo et al., 2021; Schmieder et al., 2023; Fackler et al., 2021). This improvement in match quality is consistent with both demand- and supply-side mechanisms. On the demand side, uniform wage ceilings may disproportionately benefit high-productivity firms by enhancing their relative profitability, making them both more willing to hire and more attractive destinations for displaced

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workers (Bhuller et al., 2022; Kampelmann et al., 2018). On the supply side, workers facing a more compressed wage-offer distribution have fewer incentives to prolong job search, as the potential gains from waiting for a better offer diminish (Janssen, 2018).

Second, the supply-side channel may be particularly salient for high-wage workers, who typically advance through better employer matches rather than higher wages (Haltiwanger et al., 2018; Helm et al., 2023). We explore this mechanism further by examining how post-displacement wage changes differ for low- and high-wage workers across wage-setting regimes. For low-wage workers, the wage distributions are very similar across wage-setting systems. In contrast, the earnings distribution of high-wage workers is more concentrated and shifts slightly to the left under the Wage Norm. This suggests that some high-wage displaced workers accept positions with lower wages under a more coordinated wage bargaining system. This narrower distribution of wage outcomes combined with the faster re-employment documented earlier supports the supply-side mechanism. Our findings are consistent with matching models (Mortensen, 1986; Lazear, 1986), which predict that a more compressed wage-offer distribution leads to shorter unemployment durations.

Third, we study whether the effects of the reform are heterogeneous across sectors. Manufacturing workers experience broadly similar post-displacement losses regardless of the wage-setting regime, but displaced service-sector workers suffer markedly deeper and more persistent earnings declines when pay is decentralized. We further rule out composition effects: switches from manufacturing into (lower-paid) services do not account for our results (Helm et al., 2023). Instead, disaggregating the service sector by skill level reveals that high-skill service workers recover their earnings more quickly under the Wage Norm, while low-skill service workers continue to face persistent losses regardless of the wage-setting regime. These patterns align with broader structural trends in labor market polarization (Autor and Dorn, 2013), suggesting that the reform's effects interact with underlying shifts in demand for different types of service jobs. Our results point to tighter limits on firm-level wage premia as the key channel through which the reform mitigates long-run earnings losses for high-skill service workers while leaving outcomes for low-skill service workers and manufacturing workers largely unchanged.

Our findings suggest that by constraining sectoral and employer-specific wage premiums and establishing a coordinated national ceiling for future wage growth, the Wage Norm reform facilitated faster re-employment recovery for displaced workers compared to a more flexible pay system,

thereby influencing earnings outcomes over the long term. While business cycle conditions may contribute to some earnings differences, several factors suggest that cyclical effects are unlikely to be the primary driver of our results. The symmetric exposure to high unemployment across pre- and post-reform periods, the inclusion of year fixed effects in our specifications, the persistence of earnings gaps well beyond short-term cyclical patterns, and the robustness of our findings to excluding various displacement years all support the interpretation that the wage-setting reform played a substantive role in shaping displaced workers' earnings trajectories.

Our results contribute to the extensive literature on job displacement by providing a setting that integrates insights from both the job loss literature and research on the role of employers and collective bargaining in wage determination. While recent work has made substantial progress in identifying the sources of earnings losses following displacement, the evidence remains mixed (Domínguez and Gutiérrez, 2004; Plasman et al., 2007). For instance, studies from Germany emphasize persistent firm-level wage differentials as a key driver of post-displacement wage losses (Fackler et al., 2021; Schmieder et al., 2023). In contrast, evidence from the United States suggests that establishment-specific wage effects account for only a moderate share of the average earnings decline following job loss (Lachowska et al., 2020; Moore and Scott-Clayton, 2019). A common interpretation is that countries with less prevalent collective bargaining tend to exhibit greater firm-specific pay premiums and, consequently, higher wage inequality. However, cross-country comparisons are complicated by institutional and structural differences that may confound the interpretation of divergent findings. In this context, our paper is among the first to provide within-country causal evidence on the role of collective wage-setting systems in shaping earnings losses after job displacement. We exploit a nationwide reform that curtailed the influence of employers in the wage-setting process to show that long-term earnings losses for displaced workers are significantly larger under a flexible wage-setting system—that is, one characterized by unrestricted sectoral and employer-specific wage premiums.[1]

Related to our first contribution, recent studies focused on the role of employers in explaining wage losses in the U.S. and Germany (Schmieder et al., 2023; Fackler et al., 2021; Lachowska et al.,

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[1]Earlier work by Card et al. (2013) argue that a potential explanation for the increasing dispersion of the wage premiums at new German establishments in the mid-nineties was a rise in the fraction of plants that opted out of the traditional collective bargaining system. However, the authors acknowledge that it is difficult to assign a causal role to collective bargaining, because firms in Germany could choose whether to adopt some form of collective bargaining.

2020; Moore and Scott-Clayton, 2019) rely on employer-specific fixed effects in wages to quantify the role of employers in wage losses at displacement. The employer wage premiums are estimated following the variance decomposition method proposed by Abowd et al. (1999) (AKM). The standard AKM model imposes the assumption that firm effects—the contribution of a given firm's pay policies to workers' wages—are time invariant.[2] In our setting, the assumption of time-invariant firm pay policies would not hold because the Wage Norm explicitly targeted firm wage policies. The policy shift in wage formation that we exploit allows us to examine the role of the variability of firm pay policies in explaining earnings trajectories of displaced workers. Hence, we provide evidence on the role of employer-specific wage premiums on earnings losses using an alternative approach to the growing literature that examines the sources of such costly and persistent effects of job loss. To our knowledge, there are no other papers in the job displacement literature that can exploit a reform specifically targeting nationwide and sector-wide firm pay policies.

Our results also relate to the literature on the determinants of wage inequality. Previous work documents a negative relationship between unionization and income inequality in the U.S. (Card, 1996; DiNardo et al., 1996; Farber et al., 2021). Although the U.S. has experienced a stronger and more persistent increase in inequality than many continental European countries, recent work incorporates firms as important determinants of wage inequality both in the U.S. (Song et al., 2019; Bonhomme et al., 2019; Autor et al., 2020) and Europe (Card et al., 2013; Cardoso and Portugal, 2005; Card and Cardoso, 2022; Dahl et al., 2013; Dustmann et al., 2014).[3] Here, we use the variation in the timing of job loss due to mass layoffs and a policy change to the wage formation process that reduced the scope of firm-level wage bargaining in the entire Belgian economy. Combining these two sources of variation we show that wage premiums lead to higher wage dispersion than that of bargained wages. The reform limits the ability of firms to deviate from the wage floor agreed upon at the collective bargaining, thus making it easier for job losers to catch up upon re-employment.

The most closely related study is Janssen (2018), which exploits a reform to the wage bargaining

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[2]Lachowska et al. (2023) points out that the assumption that firms effects are time invariant justifies pooling many time periods to increase the number of observed worker transitions, reducing the sampling error and alleviating the limited mobility bias (that arises when there are not enough individuals moving between jobs). However, assuming that firm pay policies are time invariant, risks understating the true variability of firm pay policies.

[3]While some studies emphasize the role employer-specific pay premiums (or *wage cushions*) (Card et al., 2013; Cardoso and Portugal, 2005; Card and Cardoso, 2022), other studies focus on sorting patterns between workers and firms (Bonhomme et al., 2019; Song et al., 2019; Raposo et al., 2021) while analyzing the contribution of firms to inequality.

system in the Danish manufacturing sector and finds that displaced workers in this sector experience larger income losses under decentralized wage bargaining.[4] Our empirical setting extends their analysis to the entire economy, enabling a broader assessment of how both the level of coordination and the degree of centralization in wage bargaining systems influence earnings losses following displacement. While our findings corroborate the direction of Janssen's results, they reveal substantially larger impacts of job displacement both before and after the reform. This suggests that focusing solely on the degree of (de)centralization may be insufficient for understanding the relationship between job loss earnings and collective bargaining systems, as the level of coordination also plays a critical role (Bhuller et al., 2022).

This paper proceeds as follows. In the next section we provide background on Belgium's institutional setting. Section 3 describes the data. In Section 4, we describe our empirical strategy. Section 5 presents the results. Section 6 concludes.

2 Institutional setting

In this section, we provide background information on the Belgian wage-setting system.

2.1 Wage-setting system in Belgium

Wage bargaining in Belgium takes place every two years at the national, sectoral, and firm levels, following a hierarchical structure. First, the national collective agreement is adopted by the National Labour Council and establishes a national minimum wage, which sets the floor for wage increases over the next two years and applies across the entire country.

Second, building on the national agreement, sectoral-level agreements are negotiated within Joint Committees, which are permanent bodies at the industry level comprising representatives from employers' associations and trade unions. While only 54 percent of employees are union members, 96 percent are covered by a collective agreement (Garnero et al., 2020). The Ministry of Employment, Labour and Social Dialogue determines which Joint Committee a firm falls under, based on the firm's principal economic activity. These Joint Committees establish sector-wide

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[4]The income losses reported in Janssen (2018) are relatively modest in magnitude: displaced manufacturing workers face income declines of 6–7% under flexible pay and about 1% under a more rigid bargaining system, relative to their pre-displacement income.

standards applicable to all covered workers, including minimum wages by worker category, resulting in highly detailed pay scales. Most Joint Committees oversee one occupation per sector. In those covering blue-collar workers, pay scales are primarily defined by job description, while for white-collar workers, scales also vary by tenure (Rusinek and Tojerow, 2014). Moreover, nearly all sectoral collective agreements are extended by royal decree, meaning they apply mandatorily to all companies and workers in the sector—regardless of membership in the signatory employer organizations or trade unions.

Third, multi-employer bargaining (at the national and/or sectoral level) can be complemented with single-employer bargaining. The wages bargained at the firm level can only be greater or equal to the wage set at the sectoral level and apply to the entire firm workforce covered by the agreement (i.e., the "favourability principle").[5] The wage premium associated with a firm-level collective agreements (with respect to higher-level agreements) is generally estimated at between 3 and 7 percent (Garnero et al., 2020). Belgium also has an extensive automatic index-linking for setting wages, that is, pay and social security benefits are linked to the consumer price index. In practice, this automatic indexation mechanism imposes a floor for wage increases.

2.2   The 1996 Wage Norm

In July 1996, Belgium introduced a law enabling the national collective agreement to define a margin of wage increases that may be bargained at lower levels: *"Loi relative à la promotion de l'emploi et à la sauvegarde préventive de la compétitivité"* (Moniteur Belge, 1996). The law requires that the growth of nominal hourly labor costs for enterprises in a period of two years should not exceed a "Wage Norm" (*norme salariale*): a weighted average of the projected increases in labor costs in Belgium's three major trading partners.

The Secretariat of the Central Economic Council (CCE/CRB) estimates the nominal wage norm as the weighted average of the projected increases in nominal labor costs in Germany, France, and

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[5]A firm-level survey conducted by the Belgian National Bank reports that 98 percent of surveyed firms were subject to sectoral agreements via their joint committees, while about 26 percent of the firms had collective wage agreements concluded at the firm level. This means that the dominant sectoral negotiations certainly do not preclude supplementary agreements at firm level. The survey results clearly show that pay agreements at firm level are, as expected, more common in the case of larger firms: 67% of firms employing 200 or more staff have such an agreement, compared to just 9% of firms with between 5 and 19 employees. Partly as a result of the concentration of large firms in some sectors, collective pay agreements at firm level appear relatively common in the energy sector, manufacturing industry and financial institutions, and less so in construction, trade and business services (Druant et al., 2008).

the Netherlands. These projections are based on data published by the countries' respective central banks and the OECD's Economic Outlook, adjusted for average working hours. The wage norm is defined in addition to the automatic indexation mechanism described above, which is explicitly excluded from its scope. Nonetheless, the indexation can still affect the wage norm indirectly: indexation-driven labor-cost increases feed into the Central Economic Council's competitiveness assessment, which in turn determines the margin for wage growth in the next round. This automatic indexation mechanism has remained essentially unchanged both before and after the introduction of the 1996 wage-norm legislation.

The law aimed at increasing coordination among social partners when bargaining the national collective agreement to avoid excessive wage increases. Importantly, the reform did not involve any other institutional changes to Belgium's labor market or collective bargaining framework. The law was first applied in January 1997, with the government setting a 6.1% wage increase ceiling for the 1997-1998 bargaining period. In practice, the 1996 legislation enabled the government to monitor the wage bargaining process more closely. In addition to the minimum wages, the national collective agreement was enabled to set an upper limit for wage negotiations at all levels. During the period under study, the law stipulated an advisory report on the "available margin for wage negotiations" to be prepared by the Central Economic Council. In practice, the outcome of this study almost always led to the wage norm that was accepted by the social partners (Vandekerckhove et al., 2018). However, during the Great Recession, the government used its right to declare the wage norm legally enforceable in the absence of an agreement, and imposed real wage freezes in a number of consecutive bargaining rounds.[6] The wage norm has been largely adhered to overall. During the period 1997-2006, the accumulated increase in Belgian labor costs (24.7%) was broadly in line with the accumulated increase indicated by the wage norm (24.4%) (Van Gyes, 2009). Between 1996 and 2018, the growth of nominal hourly wages in Belgium (+60.2%) evolved similarly to the weighted average of the three reference countries (+60.5%), with growth slightly lower than in France (67.1%) and the Netherlands (71.6%), but higher than in Germany (53.1%) (CCE, 2022).

It is important to note that Belgium's wage-setting system represents a distinctive institutional model. Under the 1996 Wage Norm, the government establishes a ceiling on wage growth based on

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[6]Prior to 2007, Belgian wage growth largely aligned with its neighbors, with Belgium occasionally experiencing slower growth (e.g., -1% in 1999-2000 and -1.4% in 2003-2004). The main divergence occurred after 2007, when Belgian wages exceeded the norm by 2.3% in 2007-2008 and 0.9% in 2009-2010 (Federal Planning Bureau, 2010).

projected labor cost increases in Belgium's three major trading partners. However, social partners retain responsibility for coordinating wage negotiations within this constraint through the national collective agreement. If social partners fail to reach agreement within the mandated ceiling, the government has the authority to intervene and impose the wage norm. This approach differs from other coordinated wage-setting models, particularly the Nordic system, where wage anchors emerge from bargaining in export-oriented sectors (Mogstad et al., 2025; Calmfors, 2025; Bhuller et al., 2022).[7] While this institutional specificity may limit the direct generalizability of our findings to other contexts (such as the Nordic system), it provides valuable insights into how constraints on firm-level wage setting affect displaced workers' labor market outcomes.

After 1996, the scope for sectoral- and firm-level wage bargaining was curtailed, as the national collective agreement gained greater authority to impose wage ceilings under the framework of existing legislation. In other words, wage increases negotiated at the sectoral and firm levels became subject to a binding national wage norm. This reform of Belgium's wage-setting system provides a unique opportunity to study the relationship between wage flexibility and the costs of job displacement by leveraging the substantial constraints placed on collective bargaining at the sectoral and firm levels.

2.3   Wage dispersion

Using harmonised micro-data from the Socio-Economic Panel (SEP; 1992 and 1997 waves), the European Community Household Panel (ECHP; 1998-2001) and the European Union Statistics on Income and Living Conditions (EU-SILC; 2004-2010), Capéau et al. (2024) document an inverted-U profile for Belgian hourly-wage inequality: the 90:10 ratio rises from about 3.1 in 1992 to a peak of roughly 3.3 in 1995-96, then falls to just under 3.0 by 2010, while the Gini coefficient drops from around 0.25 to 0.22 over the same horizon. Most of this compression occurs in the upper half of the distribution—the 90th-to-median gap narrows—whereas the gap between the median and the 10th percentile remains essentially flat. However, these trends should be interpreted with

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[7]In the Nordic model, wage increases in the internationally competitive tradables sector (primarily manufacturing) establish a norm that is then followed by the sheltered nontradables sector (Calmfors, 2025). The basic idea is that price and productivity increases in the tradables sector determine the room for wage increases to be followed by sectors sheltered from international competition. This coordination occurs horizontally across sectors through voluntary adherence to the pattern set by the first-mover sector (Mogstad et al., 2025). In the Belgian case, the coordination also occurs horizontally across sectors; if the social partners do not reach an agreement, the government can always intervene. However, the wage norm has been almost always adhered to as described above.

caution, as reliable hourly-wage dispersion series for Belgium cannot be drawn from a single source over 1990-2010, requiring the splicing of datasets that differ in sampling, wage measurement, and measurement error. These data constraints make analyzing aggregate wage inequality trends in Belgium particularly complex.

While administrative data lack hours worked information necessary to construct hourly wages, it allows for continuous measurement of annual earnings dispersion over longer periods. Figure A.1 presents earnings dispersion for full-time male workers using both social security records (1990–2010) and aggregate data from StatBel (1999–2010). The figure shows stable inequality ratios in the StatBel data, while the 90:10 earnings ratio based on social security records rises gradually over the period from 2.9 to 3.3. The 90:50 and 50:10 ratios remain relatively stable throughout. These patterns of stable earnings inequality highlight the empirical challenges in assessing the Wage Norm's aggregate effects on wage dispersion. This motivates our focus on displacement and mobility patterns, where the reform's constraints on wage renegotiation should be more directly observable.

3 Data

Our empirical analysis is based on combined data from several administrative registers collected by the Belgian Crossroad Bank for Social Security (CBSS). This is a linked employer-employee database that covers the universe of Belgian workers in the private sector from 1990 onward; we have access to a 10 percent representative sample, stratified by year.[8] For each individual in the extracted sample, we can link to the employing firm (but not the establishment), when employed, using an anonymous firm identifier. The link between the individual identifier and the firm identifier allows us to observe when an individual leaves a given firm and whether they subsequently move to another firm. This link also allows us to connect each individual to information on firm size included in the dataset. Using this information, we can associate an individual layoff with a mass-layoff event at the firm level.

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[8]Self-employment and civil servants (permanent public sector employees with special legal status) are not covered in this data. However, public sector contract workers, that is government employees without civil servant status and therefore with a contract equivalent to a private one, are included. At baseline, we include only private sector workers, as the public sector cannot experience a mass-layoff event by definition. We allow public sector contract employment in subsequent periods to account for job transitions.

All in all, this data consists of complete information on earnings and days worked in each employment spell along with an employer identifier. In addition, the data includes information on basic demographic characteristics including age, gender, marital status, household composition, and place of birth provided by the National Registry.

3.1 Measuring job displacement at mass layoffs

We use the linked employer-employee structure of the CBSS data to identify mass layoffs. Following the existing literature, we define job displacement as an event when a worker leaves a job at its main employer in the course of a mass layoff. We define a mass-layoff event by identifying large drops in firm size (i.e., at least 30 percent of employment) in year c—the year of the job displacement event. We exclude cases in which, based on worker-flow, displaced workers appear in connection with an employer identification number change, merger, acquisition, spin off or break up, following the literature (Lachowska et al., 2020; Halla et al., 2020). In our sample, we consider all mass layoffs between reference years 1992 and 1999, and we follow workers using the data covering 1990-2010.

3.2 Baseline restrictions on the sample of displaced and non-displaced workers

We define a set of baseline restrictions. The individual is male, between ages 25 and 45, works full time and has at least one year of tenure at their main job in a private sector establishment with 20 or more employees.[9] We define an individual as displaced if the establishment has a mass layoff in year c, and the individual leaves the establishment in year c (and is no longer employed at the establishment in subsequent years). We only consider the first displacement event for each worker, as subsequent outcomes might be influenced by the first displacement.

The construction of the sample allows us to use the information on the pre-displacement period

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[9]Setting the baseline age 25-45 means that we can follow workers until they are 55 years old. Over this time, Belgium had one of the EU's lowest employment rates for workers aged over 55 (22%) and an increasing unemployment rate of people aged over. Workers 55 and older are often targeted by prevention and reintegration into professional activity via financial incentives and preventive personnel management policies. Also, since 1992 workers aged 55 or older who lose their jobs are offered an early retirement (pre pension) (Delbar, 2000; Moniteur Belge, 1996). In Belgium, male part-time employment was very low during our period of analysis; going from 2.2% in 1990 to 5% in 2000 (Statbel. https://statbel.fgov.be/en/themes/work-training/labour-market/part-time-employment. Accessed 6 Oct. 2025.) The job loss literature uses 1 to 3 years of tenure as baseline restriction to capture labor market attachment. We can only calculate tenure starting in 1990 (i.e., the variable tenure is left-censored). Thus, we set a baseline restriction of at least 1 year because we would exclude workers who satisfy the tenure restriction in the early years otherwise. See Table 1.

to define an appropriate control group of workers who did not suffer job displacement.[10] We focus on workers fulfilling the same baseline restrictions as our displaced sample. The comparison group contains workers employed at mass-layoff firms at the mass-layoff date who do not lose their jobs, and workers who are employed at any reference year from 1992 to 1999 at firms that do not experience a mass-layoff event. Because the latter is a large group, we draw a 10 percent random sample. We then assign to controls a placebo dismissal date equal to the layoff date of the treated workers who satisfy the same baseline restrictions and compare outcomes for the two groups at different time intervals relative to the layoff date.[11]

Our main sample comprises 5,025 displaced male workers and 24,251 non-displaced male workers. We focus our main analysis on men to facilitate comparisons with the earlier literature investigating the sources of displaced workers earnings losses, which has typically focused on men because their higher labor force attachment leads to less selection issues between in and out of the labor force (Schmieder et al., 2023; Fackler et al., 2021; Janssen, 2018).

3.3 Outcome variables and sample characteristics

The main outcome variables considered in our analysis are employment and earnings. We organize individual observations at yearly level and define employment by an indicator equal to one if the individual is employed at least some portion of each calendar year. Earnings refer to the annual real earnings in euros (2004 prices) with the main employer. Our earnings variable captures compensation for which social security contributions (salaries and bonuses) have been paid. However, it does not cover unemployment benefits or other social allowances.[12] The data does not provide

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[10]In our setting, control workers are not dismissed in the mass-layoff year but may be dismissed in subsequent years following the approach used in recent related work (Britto et al., 2022; Schmieder et al., 2023; Lachowska et al., 2020). An alternative approach used in earlier work restricts the control group to workers who are continuously employed through the whole period (Jacobson et al., 1993; Couch and Placzek, 2010). We follow the former approach because the latter could lead to an overstatement of displaced workers' losses as pointed out in Krolikowski (2018).

[11]The presence of never-treated workers in the analysis allays concerns raised by the recent methodological literature on staggered difference-in-differences designs, such as the presence of negative weights attached to some treated units when averaging heterogeneous treatment effects in typical two-way fixed effects regressions (de Chaisemartin and D'Haultfœuille, 2020; Sun and Abraham, 2021; Callaway and Sant' Anna, 2021; Goodman-Bacon, 2021; Borusyak et al., 2024). Also, the strategy of stacking treatment and control groups for each displacement year is very similar to the estimator proposed by Callaway and Sant' Anna (2021) as noted in Schmieder et al. (2023).

[12]Throughout this paper, we use the terms "wages" and "earnings" when discussing concepts and mechanisms from the literature. However, it is important to note that our data contains only annual earnings (total compensation from the main employer in a calendar year) and annual days worked. We do not observe hourly wages, hours worked per day, or any measure that would allow us to separate wage rates from work intensity. Therefore, when we refer to our empirical results or our own data, "wages" should be understood as annual earnings.

information on working hours, but we have information on days worked over the year for each employer-employee pair.

Table 1: Sample Characteristics of Displaced Workers Workers One Year Prior to Displacement - pre/post Wage Norm Reform

	Flexible Pay		Wage Norm	
	(1)	(2)	(3)	(4)
	Displaced	Non-Displaced	Displaced	Non-Displaced
Age	33.0 (6.0)	33.6 (5.8)	33.0 (5.9)	33.9 (5.9)
Tenure	2.6 (1.2)	2.9 (1.2)	4.3 (2.6)	5.1 (2.5)
Experience	13.3 (7.7)	14.2 (7.2)	12.3 (7.7)	13.9 (7.4)
Earnings	21642.3 (16050.5)	27010.2 (13673.6)	23814.0 (15813.8)	27480.8 (15502.8)
Married	67.2%	75.9%	61.3%	69.1%
Children	56.8%	64.7%	48.4%	55.7%
Blue collar	64.7%	56.8%	58.5%	55.6%
Industry				
Manufacturing	24.9%	38.3%	31.3%	37.0%
Sales	12.7%	8.7%	9.7%	8.4%
Services	41.0%	36.6%	43.2%	39.0%
Transportation	15.4%	12.0%	12.4%	11.8%
Firm size				
20-49	16.0%	10.2%	22.7%	13.0%
50-99	20.9%	12.3%	25.0%	13.5%
100-199	18.4%	12.9%	18.2%	12.7%
200-499	17.8%	16.6%	18.3%	16.7%
500-999	8.6%	11.7%	4.2%	10.6%
>=1000	18.4%	36.2%	11.7%	33.5%
Workers	2,818	13,730	2,207	10,521

Notes: Characteristics of displaced and non-displaced workers in year prior to displacement year. Workers satisfy the following restrictions: age 25 to 45, have at least one year of tenure, and establishment of at least 20 employees. The differences in average tenure between the two periods is due to the fact that we can only calculate tenure starting in 1990 (i.e., the variable tenure is left-censored). The drop of at least 30% employment used to define a mass-layoff event means that the likelihood of identifying mass layoffs events is larger in smaller firms. Mechanically, this also affects pre-event firm size among displaced and non-displaced workers.

In our sample, we consider all mass layoff events between 1992 and 1999, which allows us to have at least two years of pre-displacement data and at least ten years of post-displacement information about workers labor market trajectories. In addition to examining the dynamic effects of job displacement, we are interested in understanding whether and how flexible pay affects the costs of job displacement. To examine heterogeneous effects of job loss under different wage-setting systems, we study the effect of job loss across groups of workers who were displaced between 1992 and 1995 (i.e., pre-reform years) and groups of workers who were displaced between 1996 and 1999 (i.e., post-reform years). Table 1 presents the pre-layoff summary statistics of displaced and non-displaced workers. Columns 1-2 and 3-4 list the pre-reform sample (i.e., mass layoffs between 1992

and 1995) and post-reform sample (i.e., mass layoffs between 1996 and 1999), respectively.[13]

4   Empirical Strategy

We use variation in the timing of job loss due to mass layoffs spanning over an institutional reform that restricted single-employer bargaining, the passage of the Belgian Wage Norm in 1996. We measure the effects of job displacement by comparing outcome variables at the individual level for the displaced and control workers in the years before and after the reference date. To examine heterogeneous effects of job loss under different wage-setting systems, we study the effect of job loss across groups of workers who were displaced under Flexible Pay (i.e., 1992-1995) and under the Wage Norm (i.e., 1996-1999). We provide estimates of the effect of job loss on a variety of outcomes using an event study analysis. Following the job displacement literature (e.g., Schmieder et al. (2023)), we estimate the following regression model:

$$Y_{itc} = \sum_{k=-3;k\neq-1}^{10} \delta_k I(t=c+k) \times Disp_i + \sum_{k=-3}^{10} \gamma_k I(t=c+k) + \pi_t + \alpha_i + \epsilon_{itc} \tag{1}$$

where Y_{itc} is the labor market outcome of worker i, with baseline year c observed in calendar year t. $Disp_i$ is an indicator variable for whether the worker was displaced between year c and $c+1$, or belongs to the control group. The coefficients of interest are δ_k, which measure the change in a labor market outcome (e.g., earnings) of displaced workers with respect to the baseline year (c), relative to the evolution of the same outcome among non-displaced workers. Thus, coefficients

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[13]Table A.1 shows that overall worker-level displacement rates remained stable across the reform (17.0% vs. 17.3%, p-value of difference=0.485), indicating no change in aggregate layoff volumes. This allays concerns related to the reform itself affecting layoff decisions. Table A.2 shows that firm-level displacement rates by industry and firm size did not significantly change before and after the reform, except for a decline in displacement rates in firms with 200-499 employees and an increase in firms with 20-29 employees. These tables show that the composition of firms did not change substantially with the reform. Nevertheless, our difference-in-differences design (equation 1) does not require that the composition of layoff firms remain constant across periods to identify the causal effect of job displacement within each wage-setting regime. The key identifying assumption is that displaced and non-displaced workers would have experienced similar trends in outcomes (e.g., earnings and employment) in the absence of displacement. We visually assess this parallel-trends assumption in Figure 1 panels a, c, and e, which show that pre-displacement earnings and employment trends were similar for both groups in both periods. We also present raw means of the main outcome variables in Figure A.2. To identify causal effects across wage-setting regimes, we turn to the triple-difference regression in equation 2 . Here, the key identifying assumption is that the displaced/non-displaced difference in earnings and employment between the pre- and post-reform periods would have been stable in the absence of the wage-setting reform. If this assumption holds, workers displaced before the reform represent a valid counterfactual for those displaced after the introduction of the wage norm. Figure 1 panels b, d, and f allow us to visually assess the parallel-trends assumption in this triple-difference framework, showing no differential pre-trends in outcomes between those displaced under the wage norm and under flexible pay.

$\delta_0, ..., \delta_{10}$ identify dynamic treatment effects, δ_{-1} is the baseline omitted period, and $\delta_{-3}, ..., \delta_{-2}$ estimate anticipation effects. The specification includes individual fixed effects α_i, calendar year effects (π_t), and "year relative to baseline year" fixed effects, γ_k, where $k = [-3, +10]$ measures the number of years relative to the reference year. I(.) is an indicator function that equals 1 when the argument is true. Standard errors are clustered at worker level to allow for the correlation of the error terms, ϵ_{itc}, across different time periods t and base years c for individual i.[14]

Using a regression saturated in cohort c and relative period k indicators ensures that the comparison in outcomes of displaced and non-displaced workers in the same baseline year c and with the same relative distance k to the baseline year. Also, due to the tenure restriction in the baseline year c both displaced and non-displaced workers might be on an upward earnings profile around the baseline year event that cannot be captured by the calendar year fixed effects alone.[15] To avoid collinearity, the specification omits δ_{-1} (i.e., normalizing relative to the period prior to treatment) and one of the year dummies.

Estimating Equation 1 for the Flexible-Pay and Wage-Norm cohorts allows us to examine earnings and employment profiles of displaced and non-displaced workers under the two wage-setting systems. However, analyzing the costs of job loss in separate samples does not allow us to test for statistically significant differences between displaced workers' earnings losses across pre- and post-reform years. Following Janssen (2018) and Bennett (2022), we pool all mass-layoff event years pre- and post- reform (i.e., 1992-1999) to estimate:

$$Y_{itc} = \sum_{k=-3; k \neq -1}^{10} \delta_k^R I(.) \times Disp_i \times FlexiblePay + \sum_{k=-3}^{10} \gamma_k I(.) \times Disp_i + \pi_t + \alpha_i + \epsilon_{itc} \qquad (2)$$

where $Disp_i \times FlexiblePay$ equals one if the workers is displaced under Flexible Pay (pre-reform). The coefficients of interest, δ_k^R, measure the change in earnings of displaced workers under Flexible

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[14]In the baseline specification we do not include any time-varying control variables which could be represented by adding X_{it} in Equation 1. However, our main results do not change if we include time-varying characteristics such as age polynomials or when we allow for time-varying shocks specific to industry, type of job, or employer size, by including interaction terms between time dummies and 1-digit industry dummies, white-collar job indicators, or employer size classes, where the i-th worker is employed at the reference year (i.e., pre-event).

[15]Schmieder et al. (2023) show that the tenure restriction leads to hump-shaped earnings profiles in both displaced and non-displaced workers. After year c there is no restriction on labor force attachment; thus earnings might go down from the upward earnings profile they exhibit due to the tenure condition imposed for the baseline year.

Pay with respect to the baseline year (c), relative to earnings profiles among displaced workers under the Wage Norm.

This approach allows us to test for statistically significant differences in displaced workers' earnings and employment losses across wage-setting systems. Pooling pre- and post-reform samples of displaced and non-displaced workers also allows us to control for global trends π_t across all years and better capture macroeconomic shocks potentially affecting earnings losses. Equation 2 is in essence a triple difference regression, where δ_k^R correspond to the difference in the impact of displacement on earnings and employment profiles under Flexible Pay and under the Wage Norm, both relative to their respective non-displaced counterparts. However, the $FlexiblePay$ dummy is multicollinear with the time dummies and time-constant for most displaced workers. Thus, we cannot include the $FlexiblePay$ dummy, and the interactions $FlexiblePay \times Disp_i$ and $FlexiblePay \times \pi_t$ separately as in the triple difference framework. In our specification, π_t and α_i jointly pick up the isolated wage-setting reform effect.[16] We can estimate Equation 2 under the assumption that the displaced/non-displaced difference in earnings between pre-/post-reform would be stable in the absence of the reform. If this assumption holds, then those displaced under Flexible pay represent a valid counterfactual for those displaced under the Wage Norm. While this assumption is untestable, the coefficients $\delta_{-3}^R, ..., \delta_{-1}^R$ allow us to visually assess the lack of differential pre-trends in employment and earnings between future displaced and non-displaced workers before and after the reform prior to the mass-layoff event.

Alongside the event studies, we present average post-treatment effects over the medium term (0–5 years post-displacement) and long term (0–10 years post-displacement), computed as the mean of the estimated event-time coefficients.

5 The Effect of Job Loss on Earnings and Employment

In this section, we provide estimates of the effect of job displacement on labor market outcomes under two different wage-setting systems: Flexible Pay (i.e., pre-reform years) and the Wage Norm (i.e., post-reform years).

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[16]See Bennett (2022) and Janssen (2018) for other examples of a triple difference approach to compare displaced workers earnings losses before and after reforms—which expanded a second chance education scheme in Norway and which decentralized wage bargaining in the manufacturing sector in Denmark, respectively.

5.1 Average labor market outcomes of displaced workers

We begin by examining how job displacement shapes earnings and employment up to ten years after job loss under the two wage-setting regimes in Figure 1. Panel 1a compares the earnings paths of displaced workers with those non-displaced. Earnings plunge by about 24 percent—roughly 6,000 euros (in 2004 euros)—in the layoff year, and this short-run drop is virtually identical under Flexible Pay and the Wage Norm. From year 1 onward, however, the trajectories diverge sharply. Workers displaced during the Flexible-Pay system (blue squares) continue to earn 15–25 percent less than their non-displaced peers for a full decade, whereas those displaced under the Wage Norm (red triangles) see their losses shrink to about 10 percent after the initial shock.[17] Panel 1b plots the coefficients δ_k^R from Equation 2, which directly measure the effect of the wage-setting reform on earnings. In the layoff year the difference between regimes is negligible, but from the first post-event year onward earnings losses for the Flexible-Pay cohort exceed those of the Wage-Norm cohort by 10–20 percentage points.[18]

While the event-study plots reveal year-by-year dynamics, Table 2 aggregates those paths into medium-term (five-year) and long-term (ten-year) averages, giving a clearer sense of the sustained impact of displacement. Panel (a) shows that displacement under the pre-reform Flexible-Pay regime reduces annual earnings by about 5,200 euros—roughly 20 percent—in the first five years, whereas the Wage-Norm cohort loses only 3,300 euros (12 percent). The triple-difference implies the reform "allowed" the displaced to recover approximately 2,400 euros of those short-run losses. A decade on, earnings remain lower by 4,900 euros (19 percent) for Flexible-Wage workers but by just 2,550 euros (10 percent) under the Wage Norm, yielding a long-run gain of about 3,000 euros.

To investigate the source of these earnings losses and the role of the Wage Norm in shaping the earnings trajectories of displaced workers, we examine their employment profiles next. The event study in Figure 1c shows a short-term gap in employment rates across wage-setting systems. In the first year after displacement about 10 percent of Flexible-Pay workers remain out of work,

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[17]Results are presented using absolute values (in Euros, 2004 prices) instead of relative means in Figure A.3.

[18]As a robustness check, we replicate our main results in a matched sample in Appendix C.1. In Figure C.1 we present the same event studies as in Figure 1 on a matched sample. We match displaced and non-displaced workers on pre-displacement characteristics (age, tenure, experience, earnings, marital status, children, firm size, and industry) using propensity score matching. All regressions are weighted using the propensity score weights to ensure balance between treatment and control groups. Results are extremely similar. Table C.1 shows the covariate balance before and after matching. Table C.2 replicates Table 2 on the matched sample.
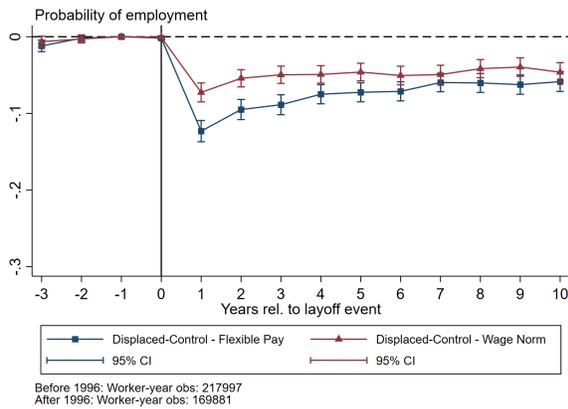
Figure 1: The Effect of Job Loss on Annual Earnings, Employment and Days Worked
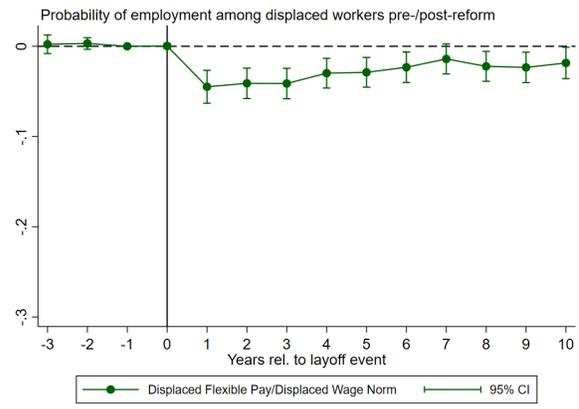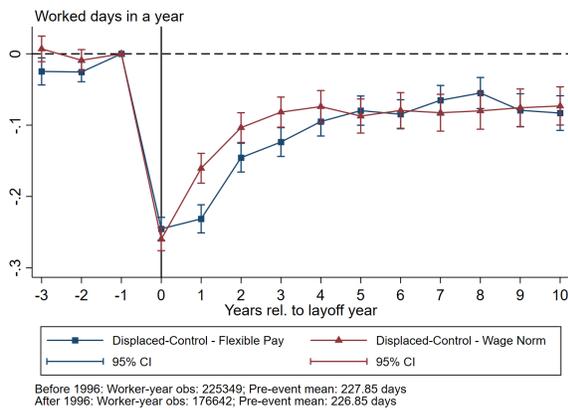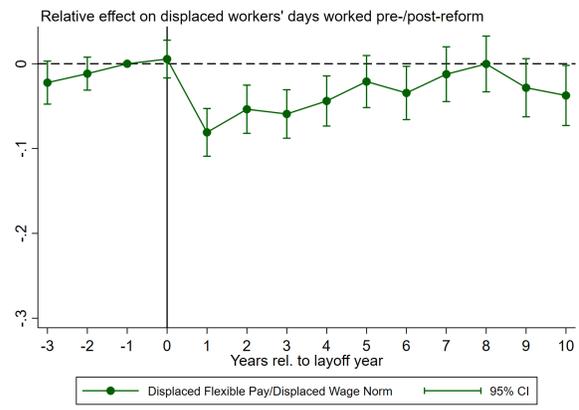

(a)

(b)

(c)

(d)

(e)

(f)
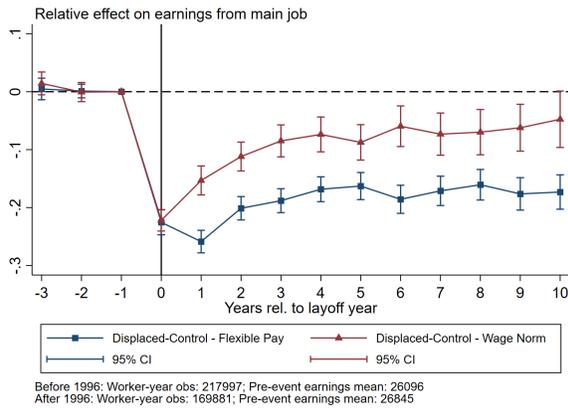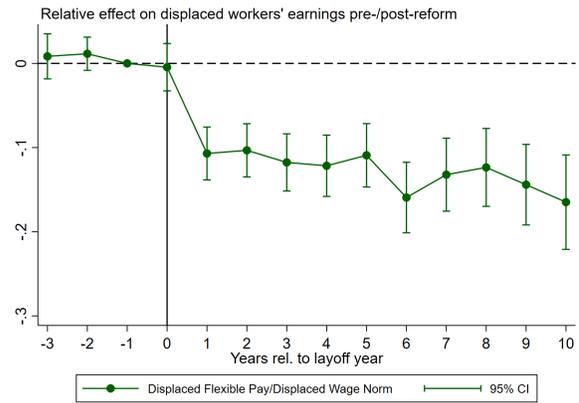
Notes: This figure shows the effect of displacement on yearly earnings and employment relative to the pre-displacement earnings mean, employment, and days worked. In Panels (a), (c) and (e) blue squares correspond to the coefficients from equation 1 under Flexible Pay, while red triangles correspond to those under the Wage Norm. Panels (b), (d), and (f) plot coefficients from equation 2, which show the change in earnings and employment of displaced workers under Flexible Pay relative to displaced workers under the Wage Norm.

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compared with 5 percent of their Wage-Norm counterparts. Averaged over the first five years (Table 2, Panel b), the employment shortfalls are 7.6 percent and 4.5 percent, implying that the reform lifts employment by roughly three percentage points. A decade later the gap narrows yet persists—around 7 percent for the Flexible-Pay cohort versus 4.5 percent for the Wage-Norm cohort—confirming better job-finding prospects under the Wage Norm. Turning to the intensive margin (in Figures 1e, 1f and Table 2 Panel c), both groups work roughly one-quarter fewer days in the layoff year. Over the next five years Flexible-Pay workers average about 36 fewer days of work per year, whereas Wage-Norm workers lose about 29 days—a gap of roughly one working week. A three-day edge in favor of the Wage Norm remains when the horizon is extended to ten years, pointing to a modest but sustained improvement in annual employment.[19]

In sum, non-employment is a plausible source of earnings losses. The 5 percentage point gap in re-employment probabilities in the first year after displacement may reflect both supply- and demand-side responses to the Wage Norm. On the supply side, workers facing a more compressed wage-offer distribution have fewer incentives to prolong job search, as the potential gains from waiting for a better offer diminish (we explore this mechanism in Section 5.3). On the demand side, the Wage Norm aimed at preserving Belgium's competitiveness and promoting employment by avoiding "excessive wage increases", which may have reduced hiring costs and expanded firms' willingness to hire, particularly among high-productivity firms whose relative profitability increased under uniform wage constraints.

In practice, the wage-setting reform reduced the scope of employer-specific bargaining by setting an upper limit to wage negotiations. This constraint could have affected displaced workers' earnings trajectories through two channels. Firms may have adapted to the wage norm by either limiting wage growth at the top—narrowing firm-specific pay premiums—or by slowing wage progression for low- and middle-level earners in order to maintain more generous compensation for high-skilled workers within the overall cap.[20] If persistent earnings losses among displaced workers arise from

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[19]In Appendix C.2 we also assess whether displacement effects differ by worker and firm characteristics. Specifically, following the approach suggested in Meekes and Hassink (2022), we include interaction terms between a vector of worker and firm characteristics—age, tenure, marital status, children, blue collar status, firm size, industry, and year of displacement—and our displacement and event time dummies. Figure C.2 indicates that these characteristics are unlikely to drive the differences we document within regimes. Finally, we estimate a version of Equation 2 that additionally controls for the same vector of worker and firm characteristics. Figure C.3 confirms that the relative gap in earnings losses across wage setting systems remains when accounting for these characteristics.

[20]A possible explanation for the difference between the pre- and post-reform periods is some substitution between wage and non-wage components. However, any such substitution is likely limited. The 1996 Wage Norm caps labor

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Table 2: The Effects of Job Loss on Earnings and Employment

	Flexible Wage	Wage Norm	Triple Difference
	(1)	(2)	(3)
Panel A: Annual earnings from main job (1000 euros)			
Average event-time effect (0–5 yrs)	-5240.3***	-3280.6***	-2450.1***
	(224.2)	(289.2)	(354.8)
Average event-time effect (0–10 yrs)	-4915.7***	-2552.0***	-3054.3***
	(254.9)	(358.3)	(422.9)
Baseline mean outcome	26096.1	26845.0	26421.7
Panel B: Employment			
Average event-time effect (0–5 yrs)	-0.076***	-0.045***	-0.033***
	(0.004)	(0.004)	(0.006)
Average event-time effect (0–10 yrs)	-0.070***	-0.045***	-0.028***
	(0.004)	(0.004)	(0.006)
Baseline mean outcome	1.0	1.0	1.0
Panel C: Days worked			
Average event-time effect (0–5 yrs)	-35.7***	-28.9***	-9.7***
	(1.9)	(2.0)	(2.7)
Average event-time effect (0–10 yrs)	-27.2***	-23.5***	-7.6***
	(1.9)	(2.1)	(2.8)
Baseline mean outcome	228.8	228.1	228.5
Observations	217997	169881	387878

Notes: This table shows the medium- and long-run earnings and employment losses post-displacement. The dependent variable is earnings (in thousands of euros, 2004 prices) in Panel A, employment rate in Panel B, and days worked in Panel C. Columns 1 and 2 correspond to displacement events under flexible pay and under the wage norm, averaging over the coefficients obtained estimating equation 1, i.e. $\frac{1}{6}\sum_{k=0}^{5}\delta_k$ and $\frac{1}{11}\sum_{k=0}^{10}\delta_k$ to obtain medium and long-term average effects 5 and 10 years post displacement. The coefficients δ_k from equation 1 measures the average difference in earnings between displaced and non-displaced workers. Column 3 shows medium and long term averages of δ_k^R, which are triple difference estimates from equation 2. The second to last row in each panel show the mean of the dependent variable in the reference year.

substantial firm wage premiums that the average displaced worker cannot recoup in the next job, restricted employer-specific wage premiums may have helped displaced workers catch up to non-displaced workers' earnings trajectories. Alternatively, if firms preserved wage premiums for top earners by constraining wage growth further down the distribution, displaced workers may have continued to face large earnings losses—unless they were able to re-enter at high-paying firms offering those preserved premiums.

Before examining these mechanisms in detail, we address two alternative interpretations of

cost growth at the firm level and defines labor costs broadly to include all forms of remuneration, benefits in kind, and employer social-security contributions. Firms therefore could not easily circumvent the norm by shifting from wage to non-wage compensation, since almost all components enter the regulated labor cost aggregate.

our findings. First, smaller earnings losses under the Wage Norm could reflect a mechanical effect whereby workers in a compressed wage distribution simply "lose less" when displaced. For example, if the control group itself is affected by the reform—specifically, if the Wage Norm constrained wage growth for non-displaced workers—the narrower gap between displaced and non-displaced workers could stem from slower control-group earnings growth rather than improved outcomes for displaced workers. However, Figure A.1 shows that non-displaced workers (dashed lines) follow similar trajectories before and after the reform, with no evidence of meaningfully slower post-reform wage growth. Moreover, we document substantial heterogeneity across sectors and skill levels (Section 5.4), and displaced workers under the Wage Norm sort into higher-ranked firms (Section 5.3)—suggesting improvements in match quality rather than a purely mechanical compression effect. Second, broader macroeconomic improvements in Belgium after 1996 could contribute to better displaced worker outcomes. We address this concern directly in Section 5.2 by examining business cycle conditions.

To identify which channels drive our findings, in the next sections we present evidence on these potential mechanisms. In Section 5.2 we rule out that the long-term earnings and employment losses we observe are purely driven by the business cycle. Given that long-run re-employment probabilities are only weakly cyclical in our data, the remaining differences across wage-setting regimes must stem from who finds jobs, where they land, and how quickly they accept offers. In Section 5.3, we examine workers' mobility patterns across wage-setting systems, asking whether the wage norm influences match quality and wage outcomes. In Section 5.4, we explore whether the wage norm changes earnings and employment trajectories of displaced workers similarly across sectors of employment and skills.

5.2 The effect of job loss over the business cycle

Prior research shows that displacement during recessions generates larger earnings losses than displacement during expansions, with employment losses driving much of this cyclical variation (Davis and von Wachter, 2011; Couch and Placzek, 2010; Schmieder et al., 2023). We next examine to what extent the effects of job loss on earnings in our setting are driven by fluctuations over the business cycle. To assess cyclical conditions, we consider both GDP growth and unemployment

rates.[21]

Defining recessions by negative GDP growth, Belgium experienced three episodes during our sample period: 1993, 2001, and 2009.[22] By this measure, our pre-reform displacement years (1992-1995) include one recession year, while our post-reform displacement years (1996-1999) include none, raising potential concerns about cyclical asymmetry. However, examining unemployment rates reveals greater symmetry across the two periods. The unemployment rate increased from 6-7% in the early 1990s to 8.6% in 1993 and remained persistently high (above 9% overall, and above 7% for men) through 1998, then dropped to 6.9% (5.6% for men) by 2000. Thus, three displacement years in the pre-reform period (1993-1995) and three in the post-reform period (1996-1998) all occurred under similarly elevated unemployment, mitigating concerns about cyclical asymmetry between the two periods.

While the asymmetry in GDP-defined recessions could pose an identification challenge, several factors support our interpretation that the wage-setting reform, rather than cyclical conditions alone, drives the earnings differences we observe. First, as noted above, both periods experienced similarly elevated unemployment. Second, our triple-difference specification in Equation 2 includes year fixed effects that control for aggregate economic conditions affecting all workers in a given year (see Figure 1, panels 1b, 1d, and 1f).

Third, we conduct extensive robustness checks in Appendix B to assess the sensitivity of our results to business cycle effects. Figure B.1 and Figure B.2 show earnings and employment losses, respectively, of displaced workers separately by year of displacement obtained by replicating the regression in Equation 1 for each displacement year between 1992 and 1999. Table B.1 presents the point estimates for these earnings losses. These estimates reveal some short-term cyclicality for those displaced during the 1993 recession, which indicates that an important part of the cyclicality of earnings losses at displacement during recession years is driven by losses in days worked.[23]

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[21]Schmieder et al. (2023) argue that unemployment rates (or changes therein) often provide a better measure of cyclical variation than GDP growth alone, particularly in contexts with high long-term unemployment, as the unemployment rate is more strongly correlated with business cycle conditions.

[22]In 1993, GDP growth was -1%. The negative economic conditions that employers were facing in 1993 were to some extent compensated by the *Maribel bis* scheme in July 1993 and by a series of selected reductions in employers' social security contributions (National Bank of Belgium, 2015). The 2001 episode would not qualify as a recession because the contraction was only 0.3% and lasted only from the second quarter to the end of 2001. The 2009 great recession is too close to the end of our sample to affect our results, so we do not consider it. See National Bank of Belgium (2015) for a description of the three recessionary periods between 1970 and 2014.

[23]To place our results within the literature, we compare the magnitude of our estimates to those found in Germany over the same period (see Figures 2 and 3 in Schmieder et al. (2023)). This comparison is also relevant because the

The year-by-year event studies (Figure B.1, Table B.1) show estimated earnings losses for each displacement cohort, but comparing coefficients visually across separate regressions does not reveal whether the differences between cohorts are statistically significant—overlapping confidence intervals do not necessarily imply that two coefficients are statistically indistinguishable. Figure B.3 takes the analysis a step further by presenting formal pairwise tests of the difference in earnings losses across all displacement year combinations at three time horizons: one year after displacement (Panel a), five years after displacement (Panel b), and ten years after displacement (Panel c). One year after displacement (Panel a), within-period comparisons—both during the flexible pay era (1992–1995, gray squares) and the wage norm era (1996–1999, orange triangles)—show few statistically significant differences and no systematic pattern of losses, while cross-period comparisons (navy circles) consistently show that pre-reform cohorts experienced significantly larger losses. This pattern persists and strengthens at longer horizons (Panels b and c): within-period differences remain largely insignificant or show no systematic pattern, while cross-period differences are consistently negative and statistically significant, indicating that pre-reform workers experienced persistently larger earnings losses. If the 1993 recession were driving our results, we would expect systematic heterogeneity among early 1990s cohorts, which we do not observe.

Furthermore, the different trajectories in earnings losses remain when we exclude the displacement events that take place during the 1993 recession year in Figure B.4. Comparing these estimates to those in Figure 1 allays concerns about the business cycle driving our main results. In Figure B.5 we estimate Equation 1 for different mass-layoff event years. Panels (a) and (c) estimate Equation 1 adding mass layoffs that take place in 2000, which allows us to compare pre- and post-reform samples with a similar number of unique worker observations. In Panels (b) and (d) we add mass layoffs in year 1991, which allows us to have additional non-recession years in the pre-reform period, with the caveat that we have only one year of pre-event information from workers displaced that early in our sample. All in all, the estimation results remain similar across samples. Finally, in Figure B.6 we remove other sets of years: Panels (a), (c), and (d) exclude mass layoffs in years 1994

GDP and unemployment rates evolve similarly in Belgium and Germany during those years. Schmieder et al. (2023); Fackler et al. (2021) find that earnings losses of displaced workers during the 1982 and 1993 German recessions doubled with respect to non-recession periods (1979-1980 and 1983-1992). They also document cyclical losses associated with periods of high unemployment in the mid 1990s to early 2000s. We find similar short-term cyclicality during the 1993 recession, but we do not observe the same cyclical pattern for the high unemployment period in the late 1990s. The authors find that what changes over the business cycle is mainly the short run effect, upon which a common recovery path follows.

and 1995, while panels (b), (d) and (f) exclude mass layoffs in years 1996 and 1997. All results remain robust to these exclusions.

The robustness checks in Appendix B provide several pieces of evidence suggesting that cyclical effects are unlikely to be the primary driver of our results. These findings are consistent with cross-country evidence of job losses under different institutional settings. Bertheau et al. (2023) shows that differences in earnings losses across displaced workers in European countries are driven by differences in re-employment probabilities (with workers in southern European countries being much less likely to re-enter the labor market post-displacement compared to northern European countries); our within-country evidence complements this cross-country variation.

Given that the long-run re-employment probabilities are only weekly cyclical in our data, the remaining differences across wage-setting regimes must then stem from who finds jobs and where they land. In the following sections, we investigate these mechanisms by examining sectoral mobility, match quality (measured by the ranking of firms where workers are reemployed), and wage changes between pre- and post-displacement jobs.

5.3   The effect of job loss on career trajectory

Our main results show that displaced workers experience smaller earnings losses under the Wage Norm compared to Flexible Pay. These earnings differences operate in part through higher re-employment probabilities: displaced workers under the Wage Norm return to employment more quickly and maintain higher employment rates over the ten-year period following displacement. In this section, we study additional mechanisms affecting earnings upon re-employment by examining whether the quality of firms displaced workers sort into differs across wage-setting regimes and how the distribution of post-displacement earnings varies.

5.3.1   Firm quality and employer sorting

Job loss may push workers down the job ladder, causing them to sort into lower-quality employers. To examine whether wage-setting institutions affect employer sorting patterns, we follow Salvanes et al. (2024) and assess whether displaced workers sort into firms similarly ranked to their previous employer. For each calendar year we rank all firms by their average market wage and then divide that rank by the number of firms in that year, so a firm at the very bottom receives 0 and the

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top-paying firm receives 1. We estimate equations 1 and 2 using this normalized firm rank as a measure of employer quality.

Table 3: The Effects of Job Loss on Career Trajectory

	Flexible Wage	Wage Norm	Triple Difference
	(1)	(2)	(3)
Dependent variable: Firm Rank, average wages			
Average event-time effect (0–5 yrs)	-0.024***	0.003	-0.028***
	(0.005)	(0.005)	(0.007)
Average event-time effect (0–10 yrs)	-0.011**	0.016***	-0.029***
	(0.005)	(0.006)	(0.008)
Baseline mean outcome	0.680	0.689	0.684
Observations	199320	156112	355432

Notes: This table shows the effect of job loss on career trajectories. For each calendar year we rank all plants by their average market wage and then divide that rank by the number of firms in that year, so a firm at the very bottom receives 0 and the top-paying firm receives 1. We estimate equations 1 and 2 using using this normalized firm rank as a measure of employer quality. Columns 1 and 2 correspond to displacement events under flexible pay and under the wage norm, averaging over the coefficients obtained estimating equation 1, i.e. $\frac{1}{6}\sum_{k=0}^{5}\delta_k$ and $\frac{1}{11}\sum_{k=0}^{10}\delta_k$ to obtain medium and long-term average effects 5 and 10 years post displacement. The coefficients δ_k from equation 1 measures the average difference in earnings between displaced and non-displaced workers. Column 3 shows medium and long term averages of δ_k^R, which are triple difference estimates from equation 2. The second to last row in each panel show the mean of the dependent variable in the reference year.

Table 3 shows that wage-setting institutions shape the quality of firms that displaced workers are able to join upon reemployment. Under Flexible Pay (Column 1), involuntary job loss reduces the rank of the hiring firm by 2.4 percentage points in the first five post-displacement years—a 3.5 percent decline relative to baseline. This penalty attenuates over time but persists: displaced workers remain at firms ranked 1.1 percentage points (1.6 percent) lower than the comparison group even a decade later.

By contrast, workers displaced under the Wage Norm do not slip down the firm-wage ladder. Over the first five years, their average firm rank remains essentially unchanged, and by year 10 it even rises by 1.6 percentage points (2.3 percent). The triple-difference estimate in Column 3 indicates a persistent 2.8-2.9 percentage-point advantage for workers displaced under the Wage Norm relative to those displaced under Flexible Pay, in both the medium and long run.

These results indicate that flexible wage bargaining amplifies the downward employer sorting that accompanies displacement (Raposo et al., 2021; Schmieder et al., 2023; Fackler et al., 2021),

while the Wage Norm mitigates this scarring effect. This improvement in match quality is consistent with both demand- and supply-side mechanisms. On the demand side, uniform wage ceilings may disproportionately benefit high-productivity firms by enhancing their relative profitability, making them both more willing to hire and more attractive destinations for displaced workers (Bhuller et al., 2022; Kampelmann et al., 2018). On the supply side, workers facing a more compressed wage-offer distribution have fewer incentives to prolong job search, as the potential gains from waiting for a better offer diminish (Janssen, 2018). This channel may be particularly relevant for high-wage workers, who typically advance through better employer matches rather than higher wages (Haltiwanger et al., 2018; Helm et al., 2023). We explore this mechanism further by examining how post-displacement wage changes differ for low- and high-wage workers across wage-setting regimes.

5.3.2 The distribution of post-displacement wages

The improved employer sorting under the Wage Norm indicates that displaced workers find better matches and return to employment more quickly. Recent evidence shows that the earnings penalty from job displacement is strongly mediated by the length of the jobless spell after displacement, with prolonged joblessness generating large and persistent earnings losses and leading workers to move to lower-paying firms (Fallick et al., 2025). As discussed above, workers facing a more compressed wage distribution under the Wage Norm may have fewer incentives to prolong their job search. We now examine how the distribution of wage changes from pre- to post-displacement jobs differs across wage-setting regimes.

Figure 2 plots kernel-density estimates of the log difference between pre- and post-displacement earnings under both wage-setting regimes. To examine whether the effect of wage compression on post-displacement wages differs for low- versus high-wage workers, we present results separately for workers with low pre-displacement wages (below the median, Panel a) and high pre-displacement wages (at or above the median, Panel b).

The distribution of wage changes is centered near zero for both groups, indicating that the median displaced worker does not experience wages substantially below their pre-displacement level upon re-employment. For low-wage workers (Panel a), the distributions look similar across regimes. However, the dispersion of wage changes differs across regimes for high-wage workers, consistent with the Wage Norm primarily constraining the upper tail of the wage distribution.

Figure 2: Distribution of pre-post-displacement wage differences
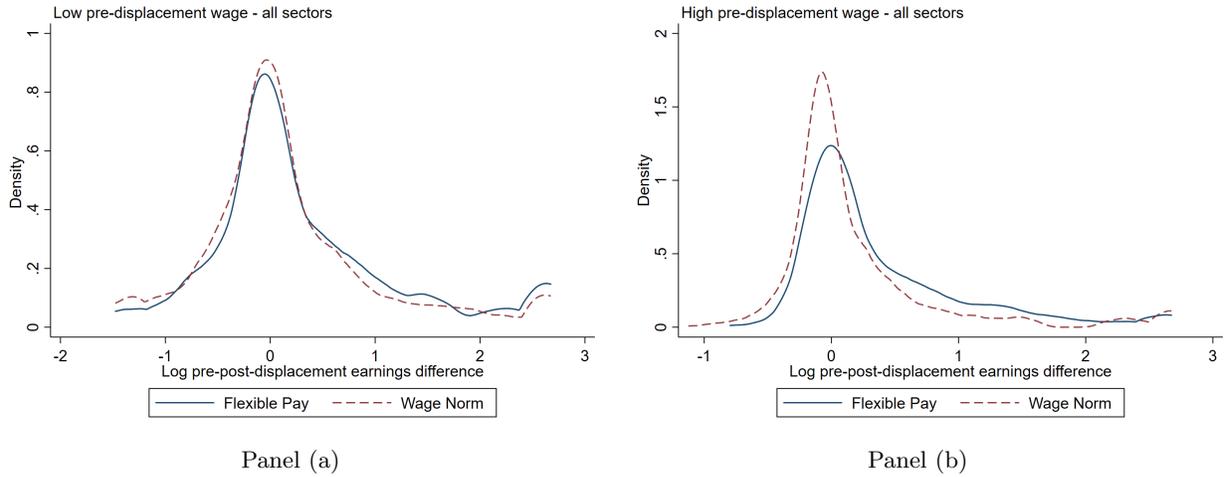
Panel (a)

Panel (b)

Notes: This figure plots kernel-density estimates of the log difference between pre- and post-displacement earnings under flexible pay and under the wage norm. Panel (a) presents the results for workers with low pre-displacement wages (below the median), and Panel (b) shows the results for workers with high pre-displacement wages (at or above the median).

Panel (b) shows that the distribution is more concentrated under the Wage Norm (dashed line) compared to Flexible Pay (solid line), consistent with a compressed wage-offer distribution. A slight shift to the left also suggests that some high-wage displaced workers accept positions with lower wages under the Wage Norm, though the median change remains close to zero.

Overall, these results are consistent with wage compression under the Wage Norm reducing the dispersion of wage offers and realized wage changes, particularly for high-wage workers. This narrower distribution of wage outcomes combined with the faster re-employment documented earlier supports the supply-side mechanism proposed by Janssen (2018): under a more compressed wage distribution, the returns to extended search diminish, facilitating quicker labor market transitions for displaced workers. Importantly, the Unemployment Insurance (UI) system remained unchanged during our period of analysis, suggesting that changes in wage structure rather than UI policy drive these results.[24]

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[24]During our period of analysis, workers who experienced an involuntary job loss were entitled to unemployment benefits of unlimited duration under the Belgian UI. While the disincentive effects of UI payments have been well documented in the literature (Marinescu and Skandalis, 2021), the Belgian UI system's features that facilitate extended job search are orthogonal to the wage-setting system. Unemployment benefits were earnings-related but declined over the spell, which relaxed liquidity constraints after displacement while maintaining search incentives. From 2004 onwards, a systematic approach to employment reactivation was implemented following the European Employment Strategy. For more details on the Belgian UI system, see Fradkin et al. (2019) and Lievens and Vandenbroucke (2015).

While these mechanisms operate on average across all displaced workers, the effects of wage compression may vary across sectors and skill levels, which we explore in the next section.

5.4 Are there sectoral differences in post-displacement earnings losses?

Are the effects of the Wage Norm on earnings dynamics driven by a specific group of workers? Recent research shows that wage premiums set by the employer, rather than by sectoral bargaining, introduce an important source of wage flexibility both to the cross-sectional wage distribution at a point in time and to changes in wages for individuals and groups (i.e., gender, education, age, and between more and less profitable employers) over time (Card and Cardoso, 2022). In this section, we examine the relationship between pay flexibility and the costs of job loss across two large sectors of employment—manufacturing and services—and across skill levels.[25]

Most papers in the job loss literature provide estimates for all displaced workers regardless of their sector of employment. Two clear exceptions are Janssen (2018), which looks at the effect of decentralization in the manufacturing sector and uses other sectors as a placebo, and Helm et al. (2023), which takes into account the secular trend of declining manufacturing jobs as a possible driver of earnings losses for those workers who switch to the low-pay service sector. More recently, Barreto et al. (2025) and OECD (2024) examine displacement costs across high- versus low-emission sectors, providing additional evidence on how sectoral differences shape earnings losses. Heterogeneous effects across sectors and the secular growth of the service sector are important dimensions to consider in our setting for two reasons. First, the average effects that we document may be driven by a specific sector, for example, if the reform bound more strongly in a given sector due to pre-reform higher employer-specific premiums (Druant et al., 2008). Second, the earnings trajectories of displaced workers across wage-setting systems may be explained by workers switching sectors after displacement.

The Belgian wage-setting reform did not target specific sectors of employment. In practice, though, it affected industries with higher wage growth rates—such as domestic-oriented industries within the service sector (CCE, 2022). On the contrary, Belgian export-oriented manufacturing

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[25]Ideally, we would directly calculate changes in wage premiums by category of workers. Unfortunately, we do not have information on the joint committee workers belong to, which is necessary to calculate the difference between the contractual wage bargained by each collective agreement and the actual wage the workers received. Card and Cardoso (2022) find that the *wage cushion* (e.g., the difference between the actual and the contractual wage) has a de-equalizing impact on the wage distribution, as it leads to higher wage dispersion than that of bargained wages.

industries were already more constrained in setting wage premiums and had lower wage dispersion (Du Caju et al., 2012). Thus, we would expect the effects of the Belgian wage-setting reform to be concentrated in sectors with fewer ex-ante constraints to set wage premiums.[26]

Table 4 shows that displaced manufacturing sector workers suffer similar earnings losses compared to non-displaced workers regardless of the year when the mass layoff takes place, whereas service sector workers displaced under Flexible Pay suffer a severe long-term impact compared to those laid off under the Wage Norm.[27] Panel (a) shows that displaced manufacturing sector workers experienced a 18-20 percent loss in earnings during the first five years after displacement compared to non-displaced manufacturing sector workers. This loss in earnings remains at about 17 percent in the long run and is similar regardless of the timing of the mass layoff event with respect to the wage-setting reform. The triple difference coefficients are small in magnitude and not statistically significant, implying that the wage-setting system does not have heterogeneous effects on manufacturing earnings losses. Panel (b) shows that service sector workers displaced under a Flexible Pay system suffer a strong and persistent 22 percent loss in annual earnings both in the medium and long term. However, service-sector workers displaced under the Wage Norm suffer a 7 percent loss in the medium term, and catch up to non-displaced workers' earnings in the long run. The triple difference coefficients in column 3 show that service workers displaced under a decentralized system suffer larger and statistically significant losses compared to workers displaced under the wage norm.

Our findings are in line with the economic intuition that restrictions on employer-specific wage premiums introduced by the reform should have an effect on workers in industries with higher wage growth and wage dispersion associated with wage premiums. Contrary to what Janssen (2018) documents for Denmark, in Belgium, the earnings trajectories of manufacturing displaced workers look very similar across wage-setting systems. Moreover, we do not find evidence showing that displaced workers' earnings recovery is better after the reform due to switching sectors either.

The difference in earnings losses between manufacturing and service sector displaced workers

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[26]Bormans and Theodorakopoulos (2023) document larger increases in wage dispersion in the service sector compared to manufacturing in 14 European economies. The authors find that firms in industries with limited product market competition pass on fewer productivity gains to wages compared to more competitive industries. Berlingieri et al. (2017) find that manufacturing- and service-sector wages at the 90th percentile of the wage distribution, were on average 3.4 and 5.8 times higher than those at the bottom decile, respectively.

[27]Worker i belongs to a given sector if he is employed in that sector at the reference year c before a mass layoff. We do not impose restrictions on workers switching sector of employments afterwards. We focus our cross-sectoral analysis on the manufacturing sector and the services sector because these comprise 75 percent of the workers in our sample.

Table 4: The Effects of Job Loss on Earnings (1000 euros), by sector of employment

	Flexible Wage	Wage Norm	Triple Difference
	(1)	(2)	(3)
Panel A: Manufacturing workers			
Average event-time effect (0–5 yrs)	-5406.3***	-5216.1***	-599.2
	(389.4)	(435.3)	(566.5)
Average event-time effect (0–10 yrs)	-4732.3***	-4925.9***	-493.1
	(446.0)	(529.3)	(671.4)
Baseline mean outcome	27323.1	28541.4	27852.4
Observations	80197	62341	142538
Panel B: Service sector workers			
Average event-time effect (0–5 yrs)	-5614.0***	-1892.8***	-4370.9***
	(408.4)	(505.9)	(630.4)
Average event-time effect (0–10 yrs)	-5747.3***	-763.0	-5770.2***
	(466.1)	(655.6)	(771.4)
Baseline mean outcome	25841.6	26473.6	26126.1
Observations	79762	66078	145840

Notes: This table shows the medium- and long-run earnings losses post-displacement by initial sector of employment. The effects of displacement on workers from the manufacturing sector are in Panel A and from the service sector in Panel B. The dependent variable is earnings (in thousands of euros, 2004 prices). Columns 1 and 2 correspond to displacement events under flexible pay and under the wage norm, averaging over the coefficients obtained estimating equation 1, i.e. $\frac{1}{6}\sum_{k=0}^{5}\delta_k$ and $\frac{1}{11}\sum_{k=0}^{10}\delta_k$ to obtain medium and long-term average effects 5 and 10 years post displacement. The coefficients δ_k from equation 1 measures the average difference in earnings between displaced and non-displaced workers. Column 3 shows medium and long term averages of δ_k^R, which are triple difference estimates from equation 2. The second to last row in each panel show the mean of the dependent variable in the reference year.

could arise from switching sectors conditional on re-employment after job loss. For example, Helm et al. (2023) find that sectoral switching (out of manufacturing to the low-knowledge service sector) accounts for 40 to 45 percent of the establishment premium loss over time in Germany. A priori, industry switches from manufacturing to the service sector are a plausible channel explaining the differences in earnings dynamics that we document, especially in the early 2000s when the share of service sector employment is increasing and the share of manufacturing sector employment is decreasing in Belgium (Bodart et al., 2018). However, we do not find evidence supporting this channel.

We next explore whether earnings losses are different across workers who switch sectors vis-à-vis those who do not. We define a worker as a sectoral switcher if we observe them in a different sector any time in relative time t=0-5, that is, if a worker is employed in a different sector from the reference year t=-1 within five years after displacement. We focus on manufacturing and services

because they are the largest groups and to check whether in our setting the secular decline in the manufacturing sector plays a potential role in explaining earnings losses of displaced workers who switch to (low-wage) service sector jobs (Helm et al., 2023). In our sample, the switching rates are rather low: only 13.3 (8.6) percent of manufacturing (service) workers switch to the service (manufacturing) sector. However, displaced workers are more likely to switch sectors compared to non-displaced workers across sectors. Displaced manufacturing (service) workers are 26.7 (7.3) percentage points more likely to switch sectors relative to non-displaced workers (p-value<0.001 for both types). Table A.3 shows that the earnings losses documented thus far are not driven by workers who switch sectors. Panel (a) shows that manufacturing workers who remain in the manufacturing sector suffer similar losses across wage setting periods, while Panel (c) shows that displaced workers who switch to the service sector under the wage norm suffer larger losses compared to those who switch during the flexible-pay period. Panel (b) shows that service sector workers who remain employed in the same sector suffer larger losses under flexible pay.

5.4.1 Skill-level heterogeneity within the service sector

The evidence presented in Table A.3 rules out a manufacturing-to-low-pay-services story in our setting. Our results thus far indicate that the average differences in earnings losses we observe are driven by service-sector workers. Yet the service sector is very heterogeneous. Disaggregating by skill level reveals that it is displaced workers in high-skill service jobs who recover their earnings more quickly and close the gap with their non-displaced peers, while those in low-skill roles continue to face persistent losses. Panel (a) in Table A.4 shows that high-skilled displaced workers losses are 25 percent smaller under the wage norm relative to flexible pay. For less-skilled service subsectors (Panel b), the losses are smaller under Wage Norm by about 8 percent.

Less-skilled workers within the service sector continue to experience meaningful earnings losses after displacement, even under the Wage Norm. This suggests that any gains in employment opportunities at the lower end of the wage distribution were limited. Instead, it is high-skilled service workers who benefit most. These differential effects across skill levels within the service sector may reflect broader structural changes in labor markets beyond the wage-setting reform itself. In particular, the patterns we observe are consistent with the labor market polarization documented by Autor and Dorn (2013), in which routine-task jobs decline while employment grows at both the

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high-skill, high-wage end (e.g., professional services) and the low-skill, low-wage end (e.g., personal services) of the occupational distribution. In our setting, high-skilled service workers may have benefited from increased demand for non-routine cognitive tasks during this period, making it easier for them to find good matches after displacement. Meanwhile, low-skilled service workers face a labor market segment characterized by lower wage floors and less favorable employment opportunities, limiting their ability to recover earnings even under more centralized wage-setting. While a full decomposition of these structural trends is beyond the scope of this paper, our findings suggest that the wage-setting regime interacts with these underlying labor market dynamics to shape post-displacement outcomes.

6 Conclusion

This paper uses administrative data from Belgium covering two decades to explore the relationship between collective wage coordination and the wage costs of job loss. We use variation in the timing of job loss due to mass layoffs spanning a nation- and sector-wide institutional reform that restricted firm-level wage growth to compare the earnings losses of displaced workers under different wage-setting systems. Our data allows us to distinguish between losses in employment and losses in earnings over a ten-year period after a mass-layoff event.

We find that Belgium's 1996 Wage Norm—by capping sector- and firm-level wage growth and tightening national coordination—substantially softened the long-run earnings penalty of involuntary job loss. Workers displaced just before the reform, when wages were still bargained flexibly, lost about 19 percent of annual earnings over the subsequent decade. The comparable loss for those displaced immediately after the reform was roughly 10 percent, and it shrank steadily to around 5 percent within ten years, whereas the flexible-pay penalty remained around 15–18 percent.

To understand the sources of these earnings differences, we examine employment dynamics across both the extensive and intensive margins. The smaller earnings losses under the wage norm stem in part from swifter re-employment: displaced workers under the Wage Norm are more likely to return to employment and remain employed throughout the ten-year period. On the intensive margin, they also work more days per year, maintaining a roughly one-week advantage over workers displaced under Flexible Pay in the medium term. Several factors suggest that business

cycle conditions are unlikely to be the primary driver of these results. The symmetric exposure to high unemployment across pre- and post-reform periods, the inclusion of year fixed effects in our specifications, the persistence of earnings gaps well beyond short-term cyclical patterns, and the robustness of our findings to excluding various displacement years all support the interpretation that the wage-setting reform played a substantive role in shaping displaced workers' earnings trajectories.

Beyond employment dynamics, we identify several mechanisms through which the reform affected post-displacement outcomes. Workers displaced under the Wage Norm sort into higher-ranked firms, suggesting the reform offers protection against long-term earnings scarring. This improvement in match quality is consistent with both demand- and supply-side mechanisms. On the demand side, uniform wage ceilings enhanced the relative profitability of high-productivity firms, making them more willing to hire displaced workers. On the supply side, compressed wage distributions reduced incentives to prolong job search. Examining post-displacement wage changes, we find that high-wage workers face more concentrated earnings outcomes under the Wage Norm, with some accepting positions at slightly lower wages and returning to employment earlier.

The reform's effects vary substantially across sectors and skill levels. Displaced manufacturing workers experience similar earnings losses across wage-setting regimes. In contrast, service-sector workers displaced under Flexible Pay suffer larger losses relative to those displaced during the Wage Norm. Within services, high-skill workers drive the earnings recovery, while low-skill workers continue to face persistent losses under both regimes. These patterns suggest that the reform benefited workers in high-skill service occupations where firm-level wage premiums were larger before the reform.

Taken together, the evidence shows that when collective bargaining constrains employer-specific wage premia, displaced workers return to work sooner, enter higher-quality firms, and suffer markedly smaller long-term earnings losses. Our findings are consistent with the literature documenting the existence of firm-specific wage components and their role in explaining workers' career trajectories. The mechanisms we identify—faster re-employment through compressed wage distributions, improved match quality, and heterogeneous effects across sectors and skill levels—offer broader insights into how wage-setting institutions shape the welfare costs of job displacement and how labor market policies may affect displaced workers' ability to get back on the job ladder.

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A   Additional Tables and Figures

Figure A.1: Aggregate earnings inequality
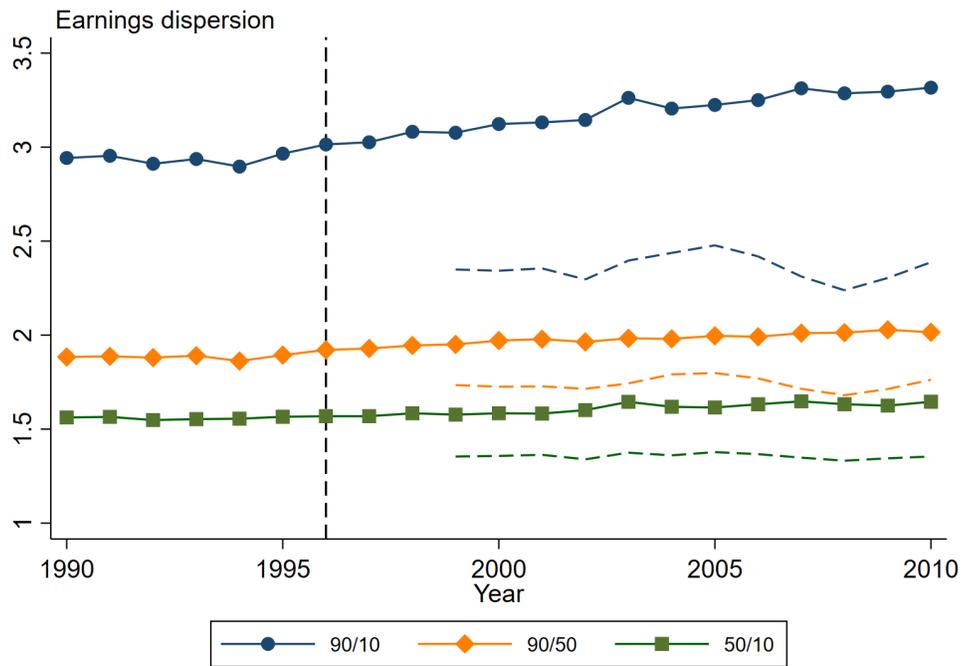
Notes: Authors own elaboration based on yearly earnings reported in social security data (CBSS). Dashed lines show trends based on deciles calculated by Statbel using monthly gross salaries. Statbel data is only available from 1999.

Table A.1: Displacement rates across wage-setting systems

	Pre-reform (1992-1995)	Post-reform (1996-1999)	Difference	P-value
All workers	17.0%	17.3%	0.3pp	0.485
	(0.29)	(0.34)	(0.44)	
Observations	16,548	12,728	29,276	

Notes: This table reports overall displacement rates before and after the reform, with p-value from a two-sample t-test. We define job displacement as an event when a worker leaves a job at its main employer in the course of a mass layoff. We define a mass-layoff event by identifying large drops in firm size.

Table A.2: Firm-level displacement rates by sector of employment and firm size

	Pre-reform (1992-1995)	Post-reform (1996-1999)	Difference	p-value
Panel A: Industry				
Manufacturing	19.6%	19.4%	-0.2pp	0.904
	(1.0)	(1.0)	(1.4)	
	[n=1647]	[n=1517]		
Sales	28.5%	25.8%	-2.7pp	0.274
	(1.7)	(1.8)	(2.5)	
	[n= 716]	[n= 582]		
Services	27.1%	27.4%	0.4pp	0.798
	(1.0)	(1.0)	(1.4)	
	[n=2040]	[n=2061]		
Transportation	24.3%	23.9%	-0.4pp	0.842
	(1.5)	(1.5)	(2.1)	
	[n= 861]	[n= 834]		
Panel B: Firm size				
20-49 employees	26.9%	30.7%	3.8pp	0.035
	(1.3)	(1.3)	(1.8)	
	[n=1244]	[n=1279]		
50-99 employees	30.0%	31.3%	1.3pp	0.462
	(1.3)	(1.3)	(1.8)	
	[n=1342]	[n=1227]		
100-199 employees	24.8%	22.5%	-2.4pp	0.214
	(1.3)	(1.4)	(1.9)	
	[n=1055]	[n= 922]		
200-499 employees	19.9%	15.5%	-4.4pp	0.015
	(1.3)	(1.2)	(1.8)	
	[n= 934]	[n= 869]		
500-999 employees	13.3%	11.7%	-1.6pp	0.518
	(1.7)	(1.7)	(2.5)	
	[n= 398]	[n= 341]		
≥1000 employees	16.5%	14.5%	-2.0pp	0.458
	(1.9)	(1.9)	(2.7)	
	[n= 389]	[n= 359]		

Notes: This table reports overall displacement rates by sector of employment and firm size before and after the reform, with p-value from a two-sample t-test. Observations are firms. We define job displacement as an event when a worker leaves a job at its main employer in the course of a mass layoff. We define a mass-layoff event by identifying large drops in firm size.
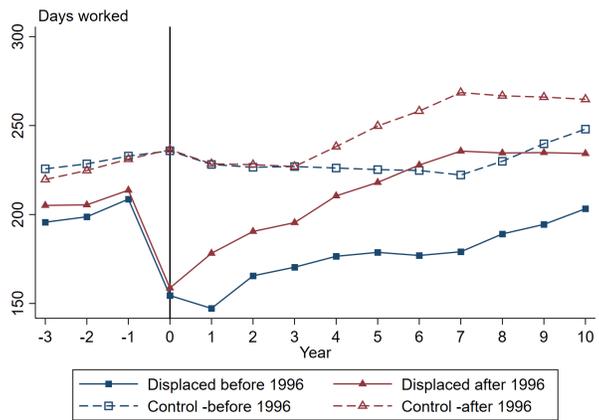
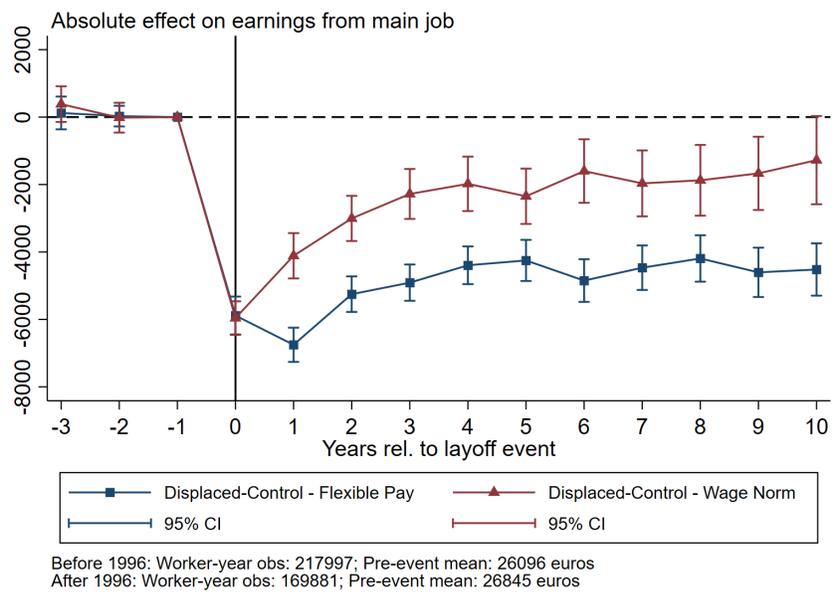## Figure A.2: Raw Means - Earnings, Employment, Days Worked
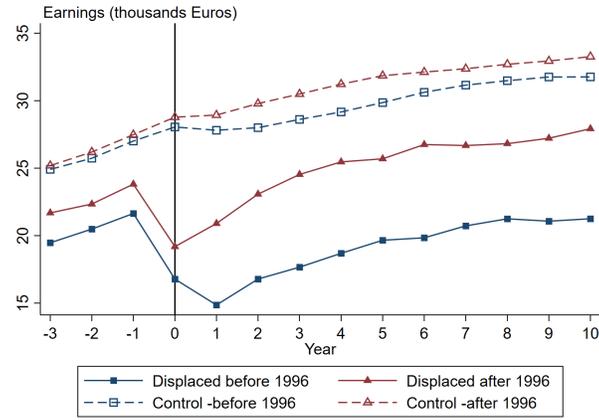
Panel (a)
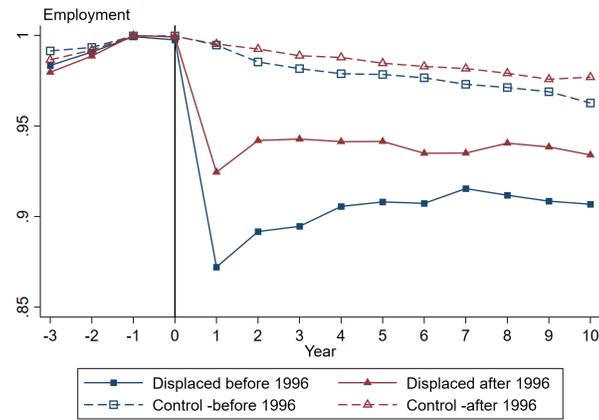
Panel (b)

Panel (c)

Notes: This figure shows raw means for the main outcome variables.

Figure A.3: Absolute effect of Job Loss on Annual Earnings

Before 1996: Worker-year obs: 217997; Pre-event mean: 26096 euros
After 1996: Worker-year obs: 169881; Pre-event mean: 26845 euros

Notes: This figure shows the effect of displacement on yearly earnings (in euros, 2004 prices). The blue squares correspond to the coefficients from equation 1 under Flexible Pay, while red triangles correspond to those under the Wage Norm.

Table A.3: The Effects of Job Loss on Earnings (1000 euros)

	Flexible Wage	Wage Norm	Triple Difference
	(1)	(2)	(3)
NON-SWITCHERS			
Panel A: Manufacturing workers			
Average event-time effect (0–5 yrs)	-5331.4***	-4827.3***	-1110.6
	(521.4)	(529.6)	(733.9)
Average event-time effect (0–10 yrs)	-4809.8***	-4495.4***	-1227.0
	(598.8)	(631.0)	(854.9)
Baseline mean outcome	27950.0	29007.4	28403.4
Observations	70209	53271	123480
Panel B: Service sector workers			
Average event-time effect (0–5 yrs)	-6358.8***	-2561.2***	-4385.3***
	(453.6)	(547.1)	(692.0)
Average event-time effect (0–10 yrs)	-6524.9***	-1545.2**	-5697.8***
	(512.9)	(715.7)	(849.2)
Baseline mean outcome	25876.8	26739.1	26264.6
Observations	72818	60335	133153
SWITCHERS			
Panel C: Manufacturing workers			
Average event-time effect (0–5 yrs)	-2029.7***	-3710.3***	2459.9**
	(700.5)	(1058.1)	(999.8)
Average event-time effect (0–10 yrs)	-1628.3*	-4034.4***	3255.3***
	(833.5)	(1193.4)	(1217.4)
Baseline mean outcome	23014.2	25832.8	24339.8
Observations	9988	9070	19058
Panel D: Service sector workers			
Average event-time effect (0–5 yrs)	-1401.7	1310.6	-4417.0***
	(933.9)	(1290.5)	(1470.6)
Average event-time effect (0–10 yrs)	-1557.7	2915.5*	-6465.0***
	(1168.2)	(1627.5)	(1791.0)
Baseline mean outcome	25456.9	23615.3	24621.6
Observations	6944	5743	12687

Notes: This table shows the medium- and long-run earnings losses post-displacement by initial sector of employment and switcher status after job loss. The effects for displaced workers from the manufacturing (service) sector who do not switch to another sector post-displacement are in Panel A (Panel B). Panel C (Panel D) shows the effect of job loss on earnings of manufacturing (service) workers who switch to another sector. The dependent variable is earnings (in thousands of euros, 2004 prices). Columns 1 and 2 correspond to displacement events under flexible pay and under the wage norm, averaging over the coefficients obtained estimating equation 1, i.e. $\frac{1}{6}\sum_{k=0}^{5}\delta_k$ and $\frac{1}{11}\sum_{k=0}^{10}\delta_k$ to obtain medium and long-term average effects 5 and 10 years post displacement. The coefficients δ_k from equation 1 measures the average difference in earnings between displaced and non-displaced workers. Column 3 shows medium and long term averages of δ_k^R, which are triple difference estimates from equation 2. The second to last row in each panel show the mean of the dependent variable in the reference year.
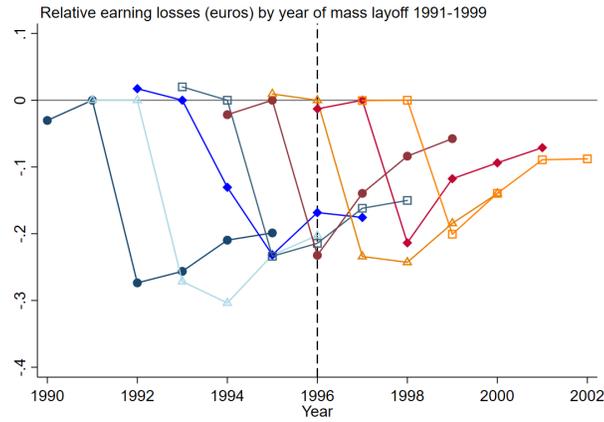
Table A.4: The Effects of Job Loss on Earnings in Service sub-sectors

	Flexible Wage	Wage Norm	Triple Difference
	(1)	(2)	(3)
Panel A: High-knowledge intensive services			
Average event-time effect (0–5 yrs)	-6918.4***	-2002.3**	-5958.6***
	(616.6)	(839.5)	(982.7)
Average event-time effect (0–10 yrs)	-7618.6***	-946.7	-8050.1***
	(710.1)	(1107.4)	(1230.3)
Baseline mean outcome	29738.1	31469.4	30500.4
Observations	34920	27496	62416
Panel B: Less-knowledge intensive services			
Average event-time effect (0–5 yrs)	-4481.2***	-3018.3***	-1783.3***
	(432.0)	(543.3)	(668.9)
Average event-time effect (0–10 yrs)	-4136.5***	-2407.4***	-2261.0***
	(488.4)	(627.4)	(754.9)
Baseline mean outcome	25907.6	26008.0	25951.3
Observations	48380	38075	86455
Panel C: Other services			
Average event-time effect (0–5 yrs)	-7052.7***	-3123.7***	-4528.1***
	(1142.7)	(963.1)	(1496.0)
Average event-time effect (0–10 yrs)	-6557.7***	-1761.4	-5442.6***
	(1281.9)	(1248.1)	(1772.6)
Baseline mean outcome	22752.4	23025.2	22878.1
Observations	23615	20541	44156

Notes: This table shows the medium- and long-run earnings losses post-displacement for service subsectors. Panel A: Information, Finance, Professional and Scientific; Panel B: Retail, Transport, Accommodation and Food; Panel C: Administrative support, Education, Health. The dependent variable is earnings (in thousands of euros, 2004 prices). Columns 1 and 2 correspond to displacement events under flexible pay and under the wage norm, averaging over the coefficients obtained estimating equation 1, i.e. $\frac{1}{6}\sum_{k=0}^{5}\delta_k$ and $\frac{1}{11}\sum_{k=0}^{10}\delta_k$ to obtain medium and long-term average effects 5 and 10 years post displacement. The coefficients δ_k from equation 1 measures the average difference in earnings between displaced and non-displaced workers. Column 3 shows medium and long term averages of δ_k^R, which are triple difference estimates from equation 2. The second to last row in each panel show the mean of the dependent variable in the reference year.

B Estimates of the Cyclicality of Earnings Losses
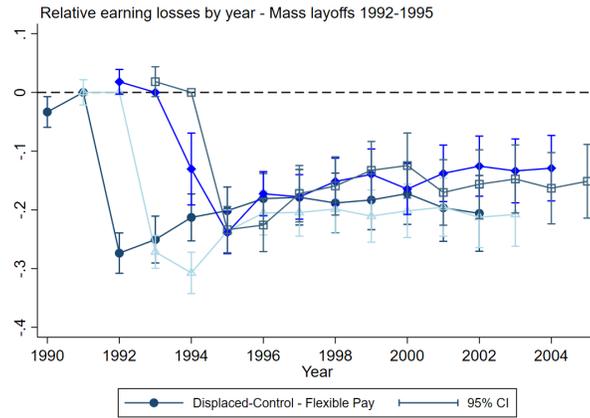
Table B.1: Relative Earnings Losses by Displacement Year

Lag	1992	1993	1994	1995	1996	1997	1998	1999
-2	-0.033**	0.000	0.018*	0.018	-0.024	0.018	-0.007	0.003
	(0.013)	(0.011)	(0.011)	(0.013)	(0.020)	(0.017)	(0.018)	(0.013)
	(reference period)							
0	-0.274***	-0.272***	-0.130***	-0.234***	-0.232***	-0.234***	-0.214***	-0.201***
	(0.018)	(0.014)	(0.031)	(0.020)	(0.019)	(0.018)	(0.019)	(0.019)
1	-0.251***	-0.307***	-0.239***	-0.226***	-0.142***	-0.239***	-0.111***	-0.141***
	(0.020)	(0.018)	(0.018)	(0.023)	(0.026)	(0.024)	(0.030)	(0.023)
2	-0.213***	-0.236***	-0.172***	-0.172***	-0.080***	-0.182***	-0.098***	-0.094***
	(0.020)	(0.019)	(0.019)	(0.024)	(0.029)	(0.024)	(0.026)	(0.023)
3	-0.201***	-0.205***	-0.178***	-0.159***	-0.054*	-0.142***	-0.078***	-0.089***
	(0.021)	(0.019)	(0.019)	(0.025)	(0.029)	(0.028)	(0.029)	(0.027)
4	-0.181***	-0.204***	-0.152***	-0.133***	-0.058*	-0.107***	-0.065**	-0.077***
	(0.022)	(0.021)	(0.020)	(0.025)	(0.030)	(0.031)	(0.033)	(0.028)
5	-0.179***	-0.199***	-0.139***	-0.125***	-0.068**	-0.137***	-0.074**	-0.080***
	(0.024)	(0.021)	(0.022)	(0.028)	(0.032)	(0.032)	(0.034)	(0.027)
6	-0.188***	-0.211***	-0.165***	-0.170***	-0.025	-0.110***	-0.078**	-0.038
	(0.026)	(0.023)	(0.022)	(0.028)	(0.037)	(0.036)	(0.036)	(0.033)
7	-0.183***	-0.202***	-0.138***	-0.156***	-0.052	-0.148***	-0.039	-0.059*
	(0.026)	(0.023)	(0.025)	(0.030)	(0.038)	(0.033)	(0.043)	(0.033)
8	-0.172***	-0.195***	-0.126***	-0.147***	-0.032	-0.140***	-0.050	-0.063*
	(0.027)	(0.025)	(0.026)	(0.030)	(0.041)	(0.033)	(0.046)	(0.037)
9	-0.196***	-0.213***	-0.134***	-0.163***	-0.035	-0.144***	-0.038	-0.038
	(0.029)	(0.026)	(0.028)	(0.031)	(0.037)	(0.035)	(0.046)	(0.043)
10	-0.206***	-0.208***	-0.129***	-0.151***	-0.017	-0.163***	-0.010	-0.023
	(0.033)	(0.028)	(0.028)	(0.032)	(0.041)	(0.040)	(0.072)	(0.038)
Observations	58,594	56,537	52,902	45,367	43,096	40,124	39,873	41,015

Notes: This table shows earnings losses of displaced workers separately by year of displacement obtained by estimating equation 1 for each displacement year (i.e., 1992-1999).
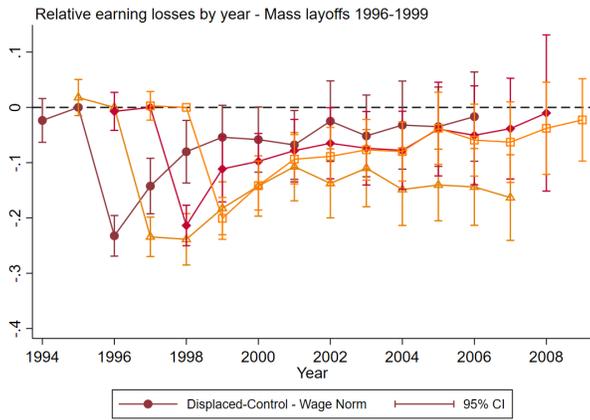
Figure B.1: The Effects of Job Loss on Earnings by Displacement Year

Relative earning losses (euros) by year of mass layoff 1991-1999

Panel (a)

Relative earning losses by year - Mass layoffs 1992-1995

Displaced-Control - Flexible Pay   95% CI

Panel (b)

Relative earning losses by year - Mass layoffs 1996-1999

Displaced-Control - Wage Norm   95% CI

Panel (c)

Notes: This figure shows earnings losses of displaced workers separately by year of displacement obtained by estimating equation 1 for each displacement year (i.e., 1992-1999). The three panels provide a relative comparison to the corresponding pre-event level of earnings. The blue lines plot the effect given by the difference between displaced and non-displaced workers under Flexible Pay. The red lines plots the effect given by the difference between displaced and non-displaced workers under the Wage Norm. For presentation purposes, we only show four years after job displacement in Panel (a) where we show all displacement years. In Panels (b) and (c) we show the effect of displacement on earnings losses pre- and post-reform, respectively, ten years after job displacement adding 95% confidence intervals.

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Figure B.2: The Effects of Job Loss on Employment Status and Days Worked by Displacement Year

Panel (a)

Panel (b)

Panel (c)

Panel (d)
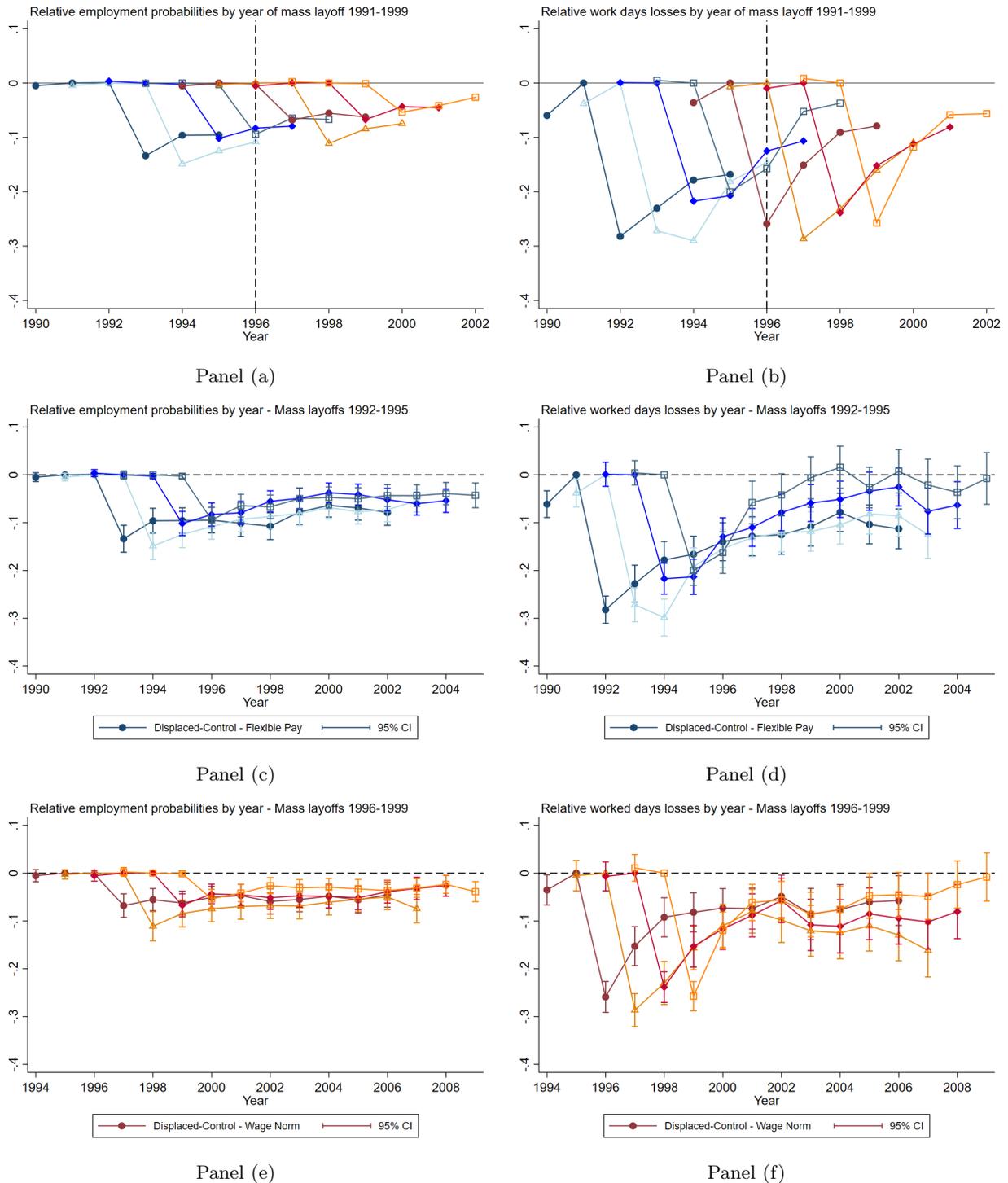
Panel (e)

Panel (f)

Notes: This figure shows the effect of job loss on employment probability (left panels) and on days worked (right panels) separately by year of displacement obtained by estimating equation 1 for each displacement year (i.e., 1992-1999). For presentation purposes, we only show four years after job displacement in Panels (a) and (b). The blue lines plot the effect given by the difference between displaced and non-displaced workers under Flexible Pay. The red lines plots the effect given by the difference between displaced and non-displaced workers under the Wage Norm. We show the effect of displacement on employment and days worked losses pre-reform (Panels c and d) and post-reform (Panels e and f) ten years after job displacement adding 95% confidence intervals.

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Figure B.3: Pairwise Comparisons of Earnings Losses Across Displacement Years
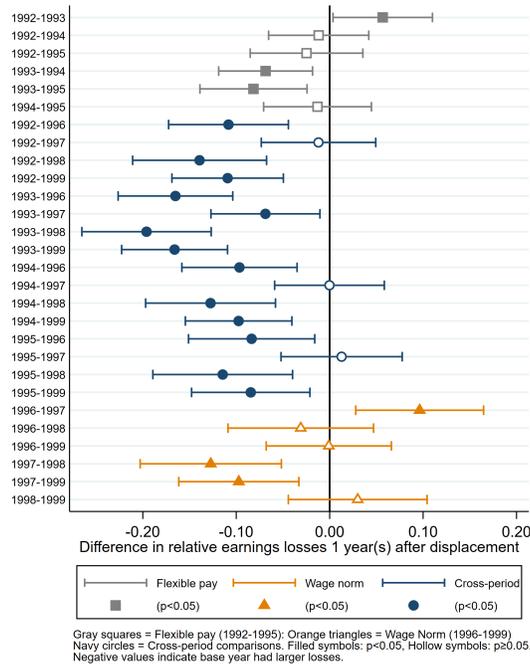
Panel (a) - One year after displacement
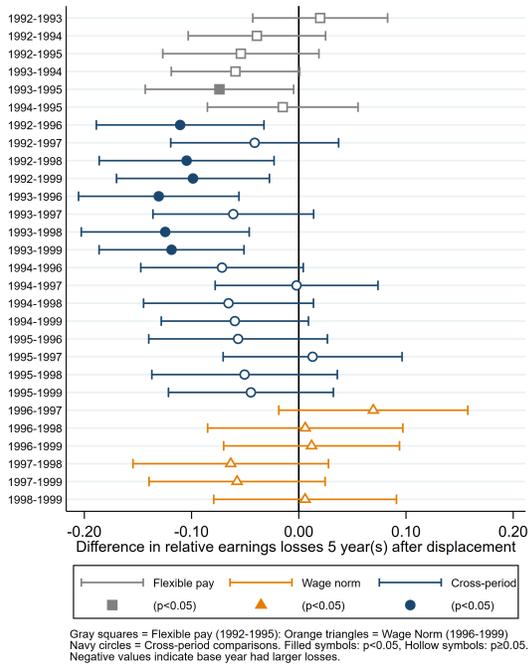
Panel (b) - Five years after displacement
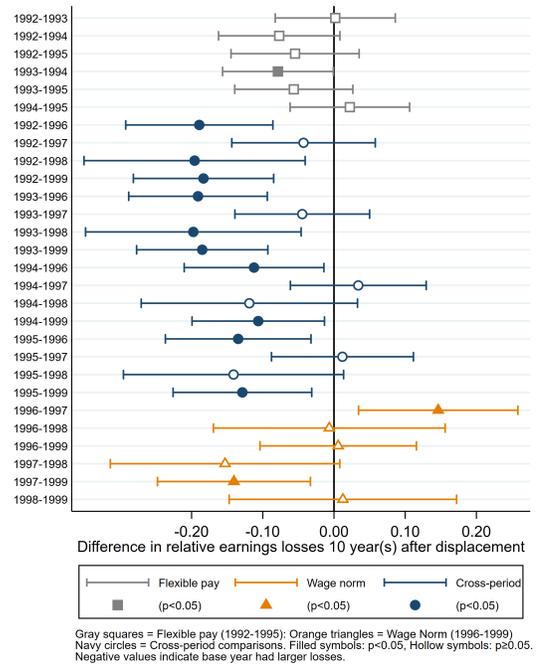
Panel (c) - Ten years after displacement

Notes: This figure shows pairwise tests of differences in earnings losses between displacement cohorts at one year (Panel a), five years (Panel b), and ten years (Panel c) after displacement. Gray squares represent within-period comparisons for Flexible Pay (1992–1995), orange triangles for Wage Norm (1996–1999), and navy circles for cross-period comparisons. Filled symbols indicate p<0.05; hollow symbols indicate p≥0.05. See Table B.1 for underlying estimates.

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Figure B.4: The Effect of Job Loss on Earnings - Excluding Displacements during 1993 Recession
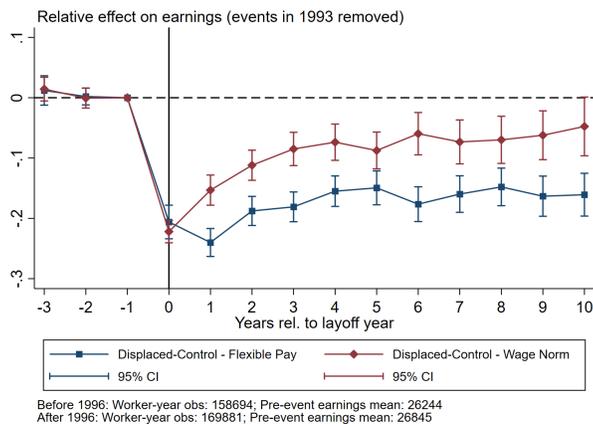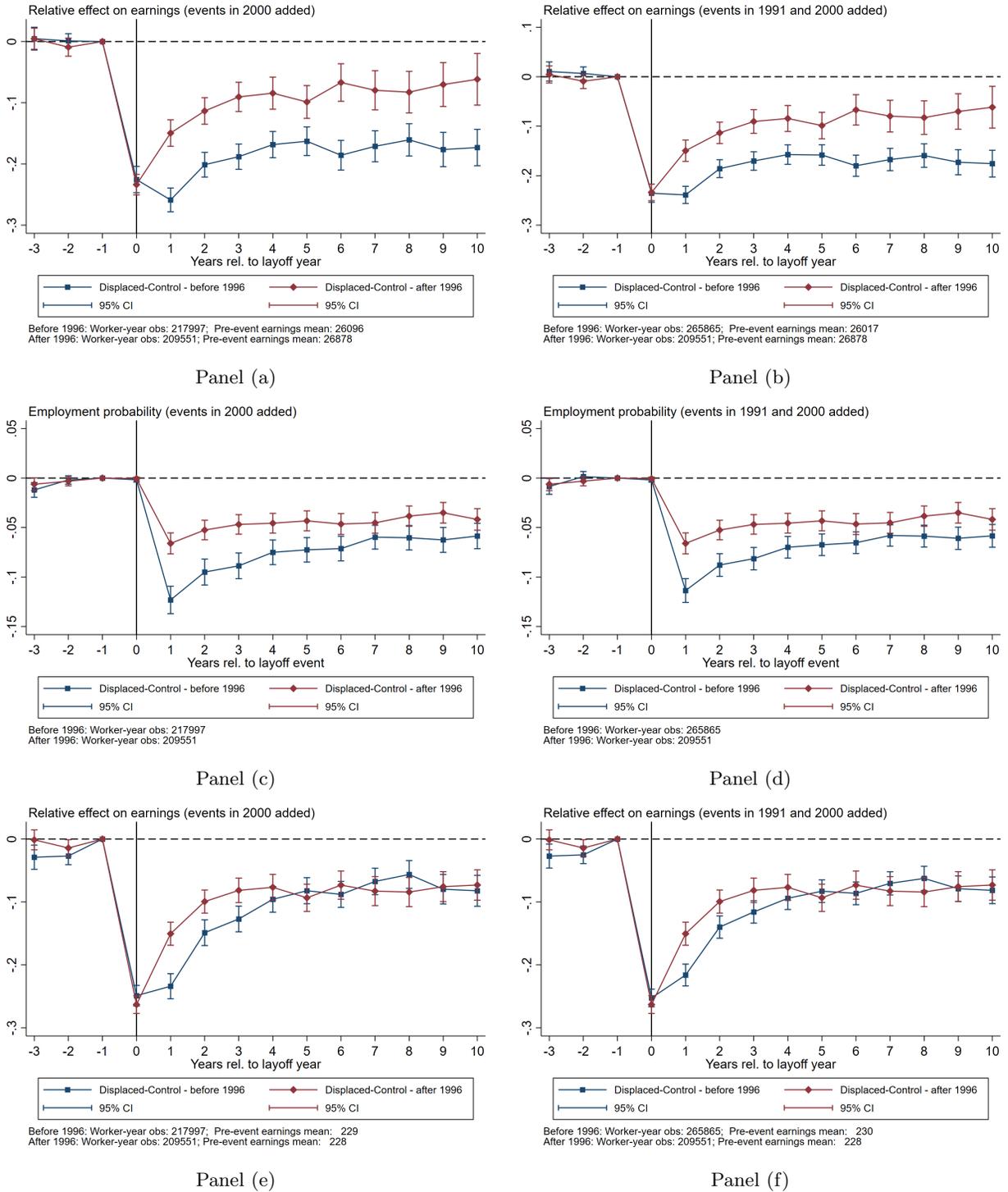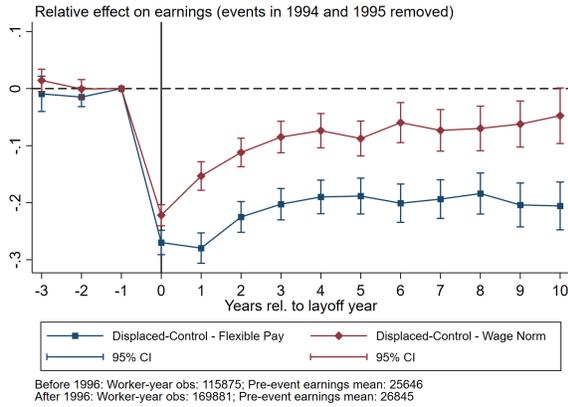

Panel (a)
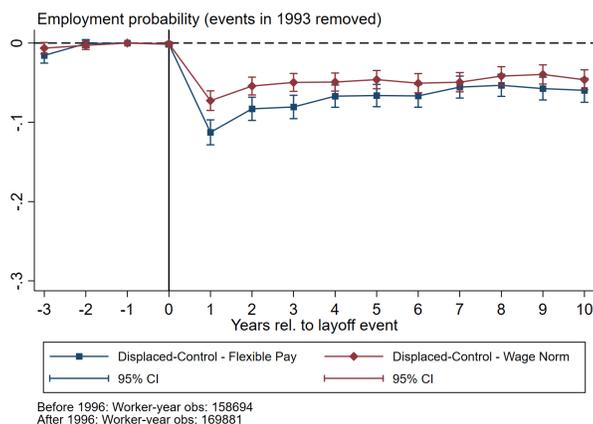
Panel (b)
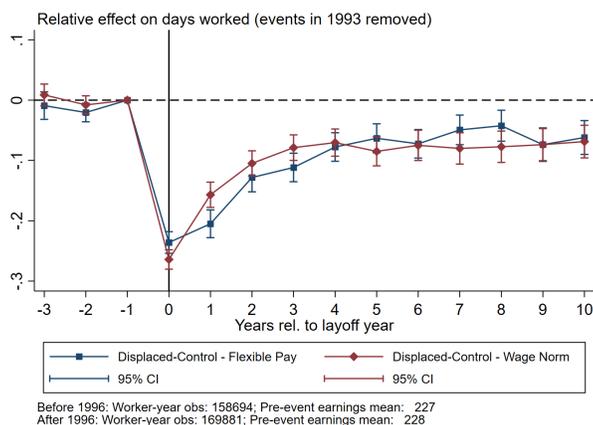
Panel (c)

Notes: This figure shows the effect of displacement on earnings (Panel a) employment rates (Panel b) and days worked (Panel c). We exclude displacement year 1993 from the regression to shows that the recession episode does not drive our results. The blue line (square) plots the effect given by the difference between displaced and non-displaced workers under Flexible Pay. The red line (triangle) plots the effect given by the difference between displaced and non-displaced workers under the Wage Norm. The panels provides a relative comparison to the corresponding pre-event level of days worked.

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Figure B.5: The Effect of Job Loss on Earnings - Changing Event Years Considered (adding 1991 and 2000)

Panel (a)

Panel (b)

Panel (c)

Panel (d)
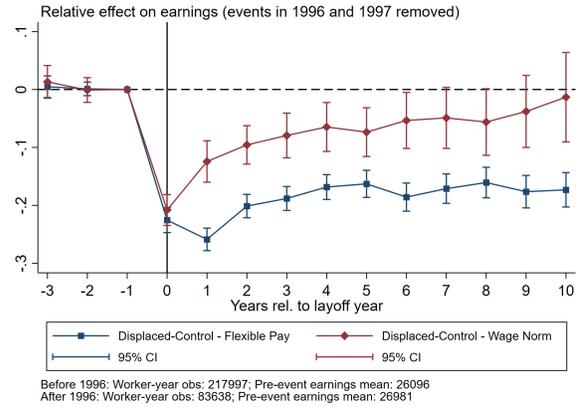

Panel (e)

Panel (f)

Notes: This figure show event study plots obtained estimating equation 1 on different samples as a robustness check to the main results (see Figure 1 notes). The top panels show the relative effect on earnings, and the bottom panels show the effect on employment rates. Panels (a), (c) and (e) estimate equation 1 adding mass layoff that occur in year 2000, Panels (b), (d) and (f) add mass layoffs in year 1991.
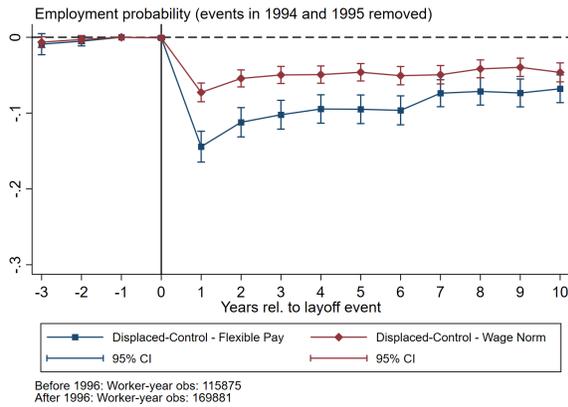
Figure B.6: The Effect of Job Loss on Earnings, Employment and Days Worked - Excluding Other Displacements Years (1994-1995 and 1996-1997)
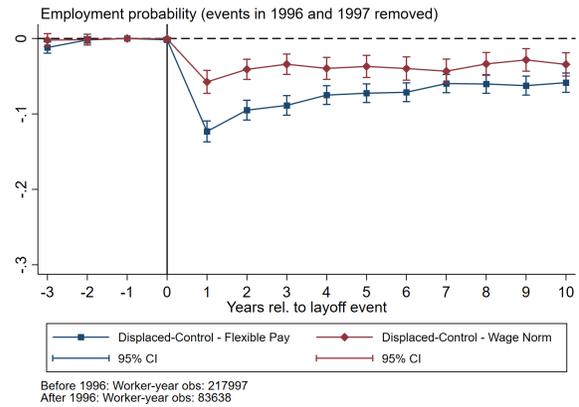

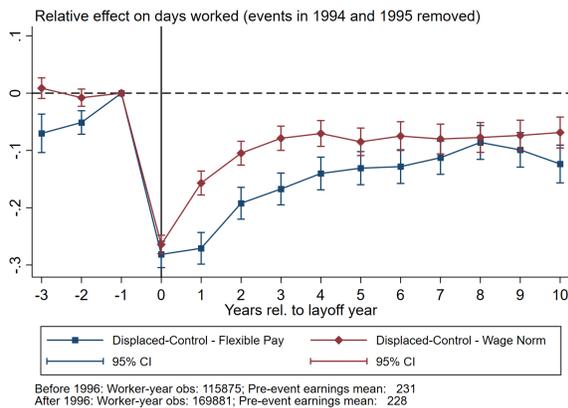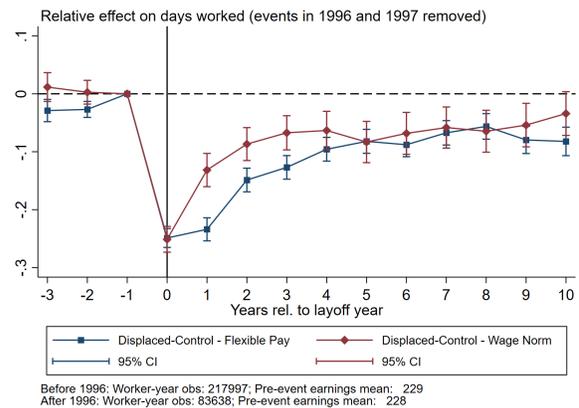Relative effect on earnings (events in 1994 and 1995 removed)

Before 1996: Worker-year obs: 115875; Pre-event earnings mean: 25646
After 1996: Worker-year obs: 169881; Pre-event earnings mean: 26845

(a)

Relative effect on earnings (events in 1996 and 1997 removed)

Before 1996: Worker-year obs: 217997; Pre-event earnings mean: 26096
After 1996: Worker-year obs: 83638; Pre-event earnings mean: 26981

(b)

Employment probability (events in 1994 and 1995 removed)

Before 1996: Worker-year obs: 115875
After 1996: Worker-year obs: 169881

(c)

Employment probability (events in 1996 and 1997 removed)

Before 1996: Worker-year obs: 217997
After 1996: Worker-year obs: 83638

(d)

Relative effect on days worked (events in 1994 and 1995 removed)

Before 1996: Worker-year obs: 115875; Pre-event earnings mean: 231
After 1996: Worker-year obs: 169881; Pre-event earnings mean: 228

(e)

Relative effect on days worked (events in 1996 and 1997 removed)

Before 1996: Worker-year obs: 217997; Pre-event earnings mean: 229
After 1996: Worker-year obs: 83638; Pre-event earnings mean: 228

(f)

Notes: This figure show event study plots obtained estimating equation 1 on different samples as a robustness check to the main results (see Figure 1 notes). The top panels show the relative effect on earnings, the middle panels show the effect on employment rates, and the bottom panels show the effect on days worked. Panels (a), (c) and (e) estimate equation 1 removing mass layoffs that occur in years 1994 and 1995, Panels (b), (d) and (f) remove mass layoffs in years 1996 and 1997.

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C Additional Robustness Checks

C.1 Matched Difference-in-differences

Displaced and non-displaced workers may differ in ways that make them difficult to compare. In this section, we follow the standard approach in the job displacement literature and use propensity score matching to construct a comparable group of non-displaced workers (Schmieder et al., 2023; Fackler et al., 2021). Starting with our baseline restrictions, we match displaced and non-displaced workers on the following pre-displacement characteristics: age (25-45), tenure (years in the same firm), experience (i.e., years since entry to the labor market), earnings (in 2000 euros), marital status (=1 if married), children (=1 if has any children), firm size (categories: 20-49, 50–99, 100–199, 200–499, 500–999, and 1000+ employees), and industry (Manufacturing, services, sales, transportation). This allows us to construct a reweighted comparison group of non-displaced workers who are observationally similar to displaced workers at baseline (one year prior to displacement), addressing any concerns about potential selection into treatment. The resulting matching weights are applied in the difference-in-differences regressions to account for observable differences between displaced and non-displaced workers in pre-treatment characteristics. This procedure yields a group of displaced workers and an observationally comparable set of non-displaced workers with similar individual and firm characteristics. This approach is closely related to Abadie (2005), who proposes propensity score reweighting to construct a control group with similar pre-treatment characteristics, for which the parallel trends assumption is more likely to be satisfied.

Table C.1 shows the covariate balance before and after matching. In Figure C.1 we present the same event studies as in Figure 1 on the matched sample. Table C.2 replicates Table 2 on the matched sample. Results are extremely similar.

Table C.1: Covariate Balance - Matched Sample

	(1) Displaced	(2) Non-displaced	(3) Std Diff
Panel A: Flexible pay			
Age	32.994	33.020	-0.004
Tenure	2.619	2.678	-0.049
Experience	13.459	13.600	-0.019
Earnings	22076.684	22548.373	-0.032
Married	0.688	0.688	0.001
Children	0.579	0.580	-0.002
Blue collar	0.635	0.638	-0.006
Manufacturing	0.265	0.255	0.022
Sales	0.125	0.137	-0.037
Services	0.405	0.404	0.001
Transportation	0.150	0.154	-0.012
Firm size 50-99	0.195	0.192	0.007
Firm size 100-199	0.181	0.196	-0.040
Firm size 200-499	0.183	0.194	-0.028
Firm size 500-999	0.091	0.088	0.009
Firm size > 1000	0.196	0.172	0.054
Observations	2,622	10,015	
Panel B: Wage Norm			
Age	33.129	33.098	0.005
Tenure	4.431	4.432	-0.000
Experience	12.779	12.871	-0.012
Earnings	24366.260	24170.458	0.013
Married	0.637	0.638	-0.002
Children	0.504	0.510	-0.011
Blue collar	0.579	0.592	-0.025
Manufacturing	0.324	0.332	-0.018
Sales	0.103	0.105	-0.005
Services	0.410	0.407	0.006
Transportation	0.130	0.126	0.012
Firm size 50-99	0.233	0.236	-0.006
Firm size 100-199	0.185	0.191	-0.017
Firm size 200-499	0.195	0.190	0.013
Firm size 500-999	0.045	0.045	0.000
Firm size > 1000	0.126	0.127	-0.003
Observations	2,059	7,353	

Notes: The table reports means for displaced and non-displaced workers after propensity score matching. Matching is conducted on characteristics measured one year prior to displacement. Column 3 shows the standardized difference after matching (balanced when |Std Diff| < 0.10).
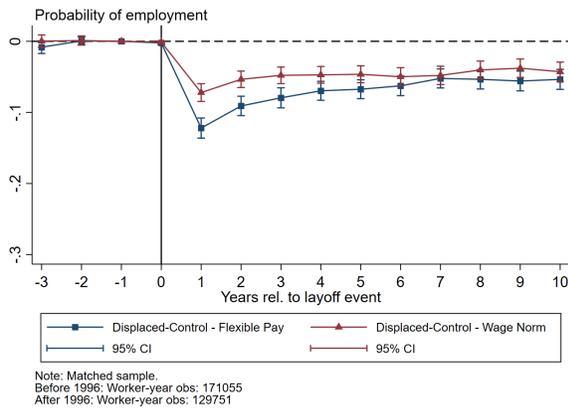
Figure C.1: The Effect of Job Loss on Earnings, Employment and Days Worked - Matched sample
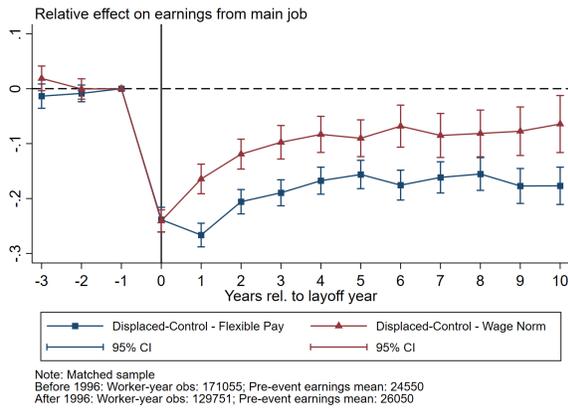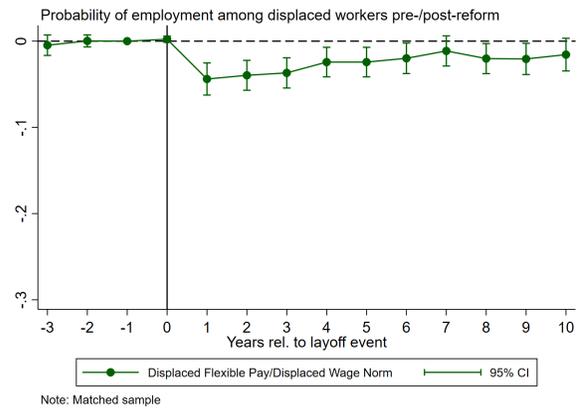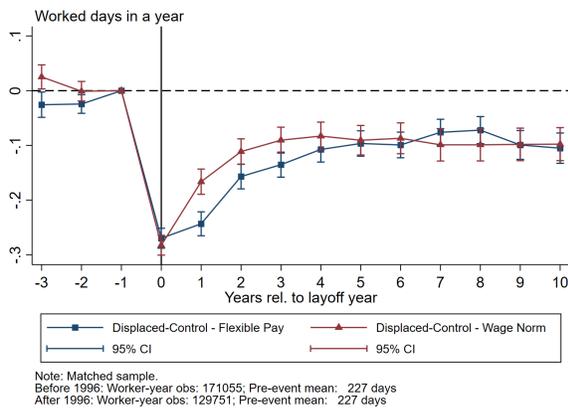

(a)
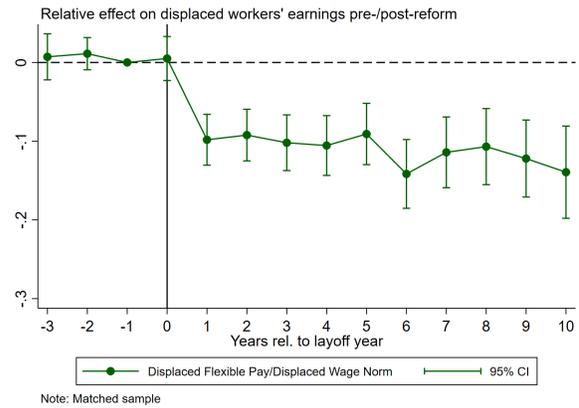
(b)

(c)

(d)

(e)

(f)

Notes:This figure shows the effect of displacement on yearly earnings (in euros, 2004 prices), employment, and days worked using our matched sample. In Panels (a), (b) and (c) blue squares correspond to the coefficients from equation 1 under Flexible Pay, while red triangles correspond to those under the Wage Norm. Panels (b), (d), and (f) plot coefficients from equation 2, which show the change in earnings and employment of displaced workers under Flexible Pay relative to displaced workers under the Wage Norm.

Table C.2: The Effects of Job Loss on Earnings and Employment - Matched Sample

	Flexible Wage	Wage Norm	Triple Difference
	(1)	(2)	(3)
Panel A: Annual earnings from main job (1000 euros)			
Average event-time effect (0–5 yrs)	-5278.159***	-3515.515***	-2085.847***
	(252.345)	(314.829)	(364.328)
Average event-time effect (0–10 yrs)	-4870.126***	-2825.800***	-2605.367***
	(289.830)	(388.303)	(432.990)
Baseline mean outcome	24550.2	26049.6	25192.0
Panel B: Employment			
Average event-time effect (0–5 yrs)	-0.072***	-0.045***	-0.028***
	(0.005)	(0.004)	(0.006)
Average event-time effect (0–10 yrs)	-0.064***	-0.044***	-0.023***
	(0.005)	(0.004)	(0.006)
Baseline mean outcome	1.0	1.0	1.0
Panel C: Days worked			
Average event-time effect (0–5 yrs)	-38.311***	-31.152***	-7.945***
	(2.108)	(2.200)	(2.747)
Average event-time effect (0–10 yrs)	-30.250***	-26.904***	-6.017**
	(2.204)	(2.376)	(2.910)
Baseline mean outcome	226.8	227.0	226.9
Observations	171055	129751	300806

This table shows the medium- and long-run earnings and employment losses post-displacement using our matched sample. The dependent variable is earnings (in thousands of euros, 2004 prices) in Panel A, employment rate in Panel B, and days worked in Panel C. Columns 1 and 2 correspond to displacement events under flexible pay and under the wage norm, averaging over the coefficients obtained estimating equation 1, i.e. $\frac{1}{6}\sum_{k=0}^{5}\delta_k$ and $\frac{1}{11}\sum_{k=0}^{10}\delta_k$ to obtain medium and long-term average effects 5 and 10 years post displacement. The coefficients δ_k from equation 1 measures the average difference in earnings between displaced and non-displaced workers. Column 3 shows medium and long term averages of δ_k^R, which are triple difference estimates from equation 2. The second to last row in each panel show the mean of the dependent variable in the reference year.

C.2 Controlling for worker and firm heterogeneity

In this section, we assess whether displacement effects differ by worker and firm characteristics. First, following the approach suggested in Meekes and Hassink (2022), we include interaction terms between a vector of worker characteristics—age, tenure (years in the same firm), marital status (=1 if married), children (=1 if has any children), white-/blue-collar status, firm size (categories: 20-49, 50–99, 100–199, 200–499, 500–999, and 1000+ employees), industry (manufacturing, services, sales, transportation), and year of displacement—and our displacement and event time dummies. Figure C.2 plots the coefficients δ_k of the triple interaction in the following equation:

$$Y_{itc} = \sum_{k=-3; k \neq -1}^{10} \delta_k I(t = c + k) \times Disp_i \times X_{itc} + \sum_{k=-3}^{10} \gamma_k I(t = c + k) \times X_{itc} + \pi_t + \alpha_i + \epsilon_{itc} \quad \text{(C.1)}$$

where the vector X_{itc} contains the worker characteristics and firm characteristics described above, and all other variables are as defined in Equation 1. The estimates show the relative changes in earnings of displaced workers relative to those of non-displaced workers, while also identifying and controlling for differences in displacement effects among workers with different characteristics. Panel (a) shows the estimates of the three-way interaction term Service Sector$\times$ Displaced$\times$ Years rel. to layoff. The reference category is Manufacturing. Panel (b) shows the estimates of the three-way interaction term Large firm ($>=200$ employees) $\times$ Displaced$\times$ Years rel. to layoff. The reference category includes firms with fewer than 200 employees.[28] These results indicate that worker and firm characteristics are unlikely to drive the differences in displacement effects that we document within regimes.

Finally, to assess whether the displacement effects differ by worker and firm characteristics across regimes, we estimate Equation C.2:

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[28]When estimating equation C.1 in Panel (a) we use two broad categories of industry to facilitate interpretation of the figure and because manufacturing and services represent about 75 percent of our sample. In this case we control for all disaggregated categories of firm size. Similarly, when estimating equation C.1 in Panel (b) we use all sectors as controls, but we construct two aggregated categories of firm size (large/small) to plot the parameter estimates. Figures plotting separate lines for each sector and firm size category are noisier due to smaller cell sizes. Nevertheless, they show no statistically significant differences in displacement effects across categories and are available upon request.

$$Y_{itc} = \sum_{k=-3;k\neq-1}^{10} \delta_k^R I(.) \times Disp_i \times FlexiblePay + \sum_{k=-3}^{10} \beta_k^R I(.) \times Disp_i \times X_{itc} +$$

$$\sum_{k=-3}^{10} \gamma_k I(.) \times Disp_i + X_{itc} + \pi_t + \alpha_i + \epsilon_{itc} \tag{C.2}$$

This is a version of Equation 2 that additionally controls for a vector of worker and firm characteristics—age, tenure (years in the same firm), marital status (=1 if married), children (=1 if has any children), white-/blue-collar status, firm size (categories: 20-49, 50–99, 100–199, 200–499, 500–999, and 1000+ employees), and industry (manufacturing, services, sales, transportation). Figure C.3 confirms that the relative gap in earnings losses across wage setting systems remains when accounting for these characteristics. The results confirm that our main findings are robust to controlling for compositional differences in worker characteristics across wage-setting regimes.

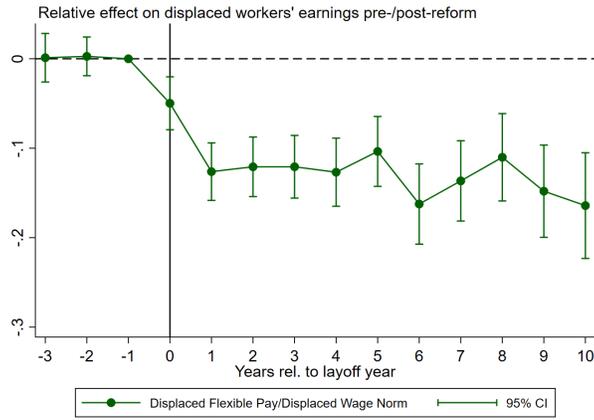Figure C.2: Effects of job displacement on earnings by worker characteristics
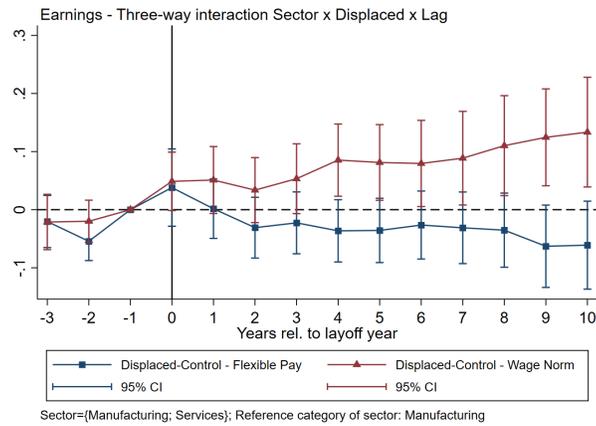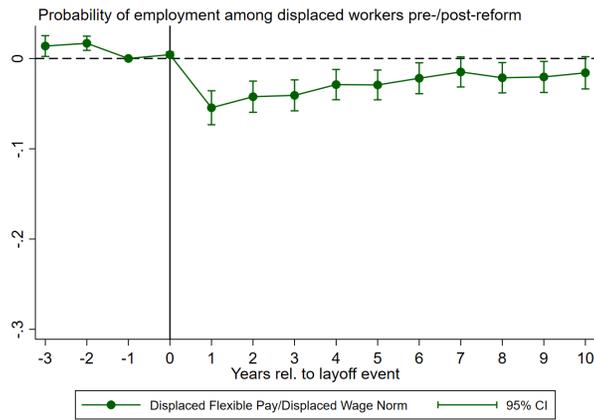

Panel (a) - Sector
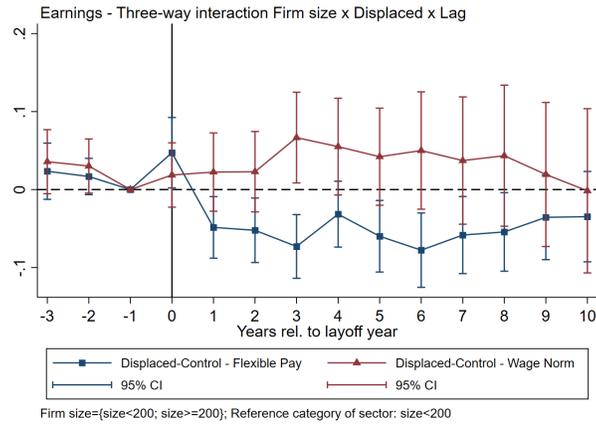
Panel (b) - Firm Size

Notes: This figure plots the coefficients δ_k of the triple interaction in the following equation: $Y_{itc} = \sum_{k=-3;k\neq-1}^{10} \delta_k I(t = c + k) \times Disp_i \times X_{itc} + \sum_{k=-3}^{10} \gamma_k I(t = c + k) \times X_{itc} + \pi_t + \alpha_i + \epsilon_{itc}$ Where the vector X contains worker characteristics—age, tenure, marital status, children, blue-/white-collar status, and year of displacement—and firm characteristics—firm size and industry (Meekes and Hassink, 2022). The estimates are the relative changes in earnings of displaced workers relative the changes in earnings of non-displaced workers but also identify and control for differences in displacement effects among workers with different individual characteristics. Panel (a) shows the estimates of the three-way interaction term Service Sector× Displaced× Years rel. to layoff. The reference category is Manufacturing. Panel (b) shows the estimates of the three-way interaction term Large firm (>=200 employees) × Displaced× Years rel. to layoff. The reference contains firms with fewer than 200 employees.
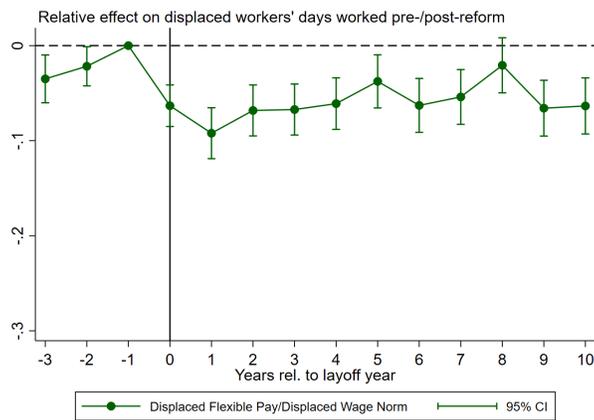
Figure C.3: The Effect of Job Loss on Earnings - Additional controls

Relative effect on displaced workers' earnings pre-/post-reform

Years rel. to layoff year

Displaced Flexible Pay/Displaced Wage Norm   95% CI

Panel (a)

Probability of employment among displaced workers pre-/post-reform

Years rel. to layoff event

Displaced Flexible Pay/Displaced Wage Norm   95% CI

Panel (b)

Relative effect on displaced workers' days worked pre-/post-reform

Years rel. to layoff year

Displaced Flexible Pay/Displaced Wage Norm   95% CI

Panel (c)

Notes: This figure shows triple-difference estimates from equation C.2 of the effect of displacement on yearly earnings, employment and days worked relative to the pre-displacement mean. Estimates show the change in earnings and employment of displaced workers under Flexible Pay relative to displaced workers under the Wage Norm, additionally controlling for a vector of worker and firm characteristics.