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

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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 ten percentage points smaller under the more coordinated wage bargaining system. The attenuation stems from faster re-employment and a greater likelihood that displaced workers match to higher-paying firms, indicating that a compressed wage-offer distribution shortens search and limits long-run scarring.

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, 2021; 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., 2022).

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). 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 if and how employment dynamics may drive 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. Specifically, over five years post-displacement, their average employment rate is 4.6 percent lower than that of non-displaced workers, compared to an average decrease of 7.5 percent observed in the more flexible regime. After this period, gaps in employment rates narrow but do not disappear, with both groups ultimately exhibiting a sustained decline relative to non-displaced workers after ten years.

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. We show that the larger earnings losses we find among displaced workers under the flexible pay system are not driven by the business cycle. The cyclical pattern of earnings losses is concentrated in the short term, while the pace at which earnings losses fall is driven by the degree of flexibility in the wage-setting system.

To explain the observed reduction in earnings losses among displaced workers following the reform, we identify several potential mechanisms and provide supporting empirical evidence. First, by estimating our primary model using firm rank based on pre-displacement wages, we demonstrate 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). It might also suggest that the imposition of a uniform wage-growth ceiling across industries and firms disproportionately benefits higher-productivity firms. These firms, which already ranked higher in terms of wage distributions prior to displacement, become relatively more profitable under the reform, thereby enhancing their attractiveness as employment destinations for displaced workers (Bhuller et al., 2022; Kampelmann et al., 2018).

Second, we draw on the reasoning presented by Janssen (2018), which suggests that in a more coordinated wage-setting environment, displaced workers have fewer incentives to prolong their job search in pursuit of higher wages. 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. In support of this mechanism, we show that high-wage displaced workers are more likely to find new employment quickly and experience faster earnings recovery relative to non-displaced workers following the introduction of more coordinated collective wage bargaining. This indicates that in a coordinated wage-setting environment, high-wage or high-productivity workers are less likely to delay re-employment, resulting in quicker job matches and more favorable post-displacement earnings outcomes compared to those in a more flexible wage-setting system. In line with this interpretation, we also compare the distributions of pre- and post-displacement wage changes before and after the reform. We find that wage differences became less dispersed following the reform, consistent again with a narrowing of the wage-offer distribution under a more coordinated wage bargaining system.

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., 2022). Instead, 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 and manufacturing workers largely unchanged.

Overall, these results support the mechanism through which the Wage Norm influenced the earnings trajectories of displaced workers—primarily by accelerating their likelihood of re-employment

in the short term. This effect operates through changes in the incentives to search for higher-paying jobs, as well as shifts in the relative productivity-to-wage ratio of high-productivity firms following the reform.

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., 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, hereafter). The standard AKM model imposes the assumption that firm effects—the contribution of

[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.

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, 2021).[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 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

[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, 2021), 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.

[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.

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 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 their 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 (i.e., the "favourability principle"). 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). This margin depends essentially on forecast pay trends in the three reference countries—France, Germany, and the Netherlands. 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 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 law aimed at increasing coordination among social partners when bargaining the national collective agreement to avoid excessive wage increases. 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. The wage norm has been largely adhered to. During the period 1997-2006, the accumulated increase in the Belgian labour costs (24.7%) was broadly in line with the accumulated

increase by the wage norm (24.4%) (Van Gyes, 2009).

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 — then applied to all workers — 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

Descriptive evidence lines up with the institutional narrative. 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. Although the descriptive evidence shows contraction of wage dispersion after 1996, establishing a causal link is difficult. The Wage Norm ceiling was introduced simultaneously for every sector and firm, leaving no counterfactual group whose wages evolved outside the new rules.

The ideal experiment would randomly assign some wages to remain subject to firm-level negotiation while shielding others from the Wage Norm and then compare labour-market outcomes across the two regimes. In Belgium, however, the 1996 ceiling applied economy-wide, so no direct counterfactual group exists. Sectors differed in how often wages were bargained at firm level, which means the reform probably bound more tightly in high-wage-premium industries. However, we lack micro data that identify which individual workers were negotiating at that margin or how much of any employer-specific premium was forfeited.

To assess whether and where the ceiling "bites," we turn to mobility. In a flexible system, workers who change employer voluntarily can renegotiate upwards, while displaced workers typically

lose firm-specific premiums; with a compressed wage structure that margin should narrow. Our sample has workers with three types of earnings trajectories: (i) workers moving job-to-job without a spell of unemployment, (ii) workers displaced by mass layoffs, and (iii) non-movers. If the Wage Norm curbed the scope for wage renegotiation at the top, we expect (i) to gain less relative to (iii) after 1996, and displaced workers in (ii) to recover a larger share of their pre-displacement wage gap than they did before 1996. A continuing rise in job-to-job movers' wages despite the ceiling would instead suggest firms reallocated available pay increases toward strategically valuable workers, a redistribution that could also cushion high-skill displaced workers who land good post-layoff matches.

We would expect firms and sectors with high wage premiums before the reform to be the most affected by the Wage Norm, but we do not directly observe which workers or firms were exposed to these premiums.[5] Since the wage growth ceiling was applied at the firm level, employers could in principle adjust in at least two ways: (1) by limiting wage growth at the top of the distribution, thereby reducing firm-level wage premiums; or (2) by slowing wage growth for low- and middle-paid workers to preserve headroom for top earners and continue attracting high-skill workers. In the first case, we would expect compression at the upper end of the distribution; in the second, a reallocation of constrained wage increases toward higher earners. Without direct data on wage bargaining or internal firm pay-setting practices, identifying which of these responses dominated remains an open empirical question.

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

[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).

have access to a 10 percent representative sample.[6] This data consists of complete information on earnings and days work 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. We use employer-employee data spans over twenty years from 1990-2010.

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

	Mass-layoff events before 1996		Mass-layoff events after 1996	
	Displaced Workers	Non-displaced Workers	Displaced Workers	Non-displaced Workers
	(1)	(2)	(3)	(4)
Age (yrs)	33.94	34.58	34.00	34.89
	(5.98)	(5.79)	(5.94)	(5.92)
Tenure (yrs)	2.53	2.89	4.21	5.08
	(1.19)	(1.22)	(2.64)	(2.56)
Experience in employment (yrs)	13.27	14.16	12.32	13.91
	(7.67)	(7.25)	(7.73)	(7.39)
Blue collar	0.65	0.57	0.58	0.56
	(0.48)	(0.50)	(0.49)	(0.50)
Annual Earnings main job	21560.81	26752.72	23596.48	27122.84
	(16158.72)	(13714.79)	(15770.56)	(15513.82)
Firm size $<$ 500	0.74	0.54	0.85	0.58
	(0.44)	(0.50)	(0.36)	(0.49)
Firm size $>=$ 500	0.26	0.46	0.15	0.42
	(0.44)	(0.50)	(0.36)	(0.49)
Manufacturing	0.25	0.38	0.30	0.36
	(0.43)	(0.48)	(0.46)	(0.48)
Sales	0.13	0.09	0.10	0.09
	(0.34)	(0.29)	(0.30)	(0.28)
Services	0.41	0.37	0.44	0.39
	(0.49)	(0.48)	(0.50)	(0.49)
Transportation	0.15	0.12	0.13	0.12
	(0.36)	(0.33)	(0.33)	(0.33)
Observations	2911	14217	2283	10957

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.

[6]Self-employment and civil servants (except contract workers) are not covered in this data. The lack of self-employment is common in the job displacement literature. We keep civil servants employment and earnings information to account potential for job transitions, but no mass-layoff event is defined using the public sector.

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 with at least one year of tenure 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, and has at least one year of tenure at their main job in a private sector establishment with 20 or more employees.[7] 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 to define an appropriate control group of workers who did not suffer job displacement.[8] 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,

[7] 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). 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.

[8] 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).

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.[9]

Our main sample comprises 4,381 displaced male workers and 25,134 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. The data does not provide information on working hours, but we have information on worked days over the year for each employer-employee pair. We can also distinguish between part-time and full-time employment. For each individual, we collect yearly observations in the three years before and ten years after the displacement. We define reference year c by the year in which the individual is last employed before the mass-layoff event.

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

[9]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., 2021). 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).

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.

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 $\delta_0, ..., \delta_{10}$ 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.[10]

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 sample 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.[11] 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 significant differences between displaced workers' earnings losses across pre- and post-reform years. Following Janssen (2018); 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 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 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

[10] 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).

[11] 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.

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.[12] 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 δ_{-3}^R,...,δ_{-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 under the Wage Norm (i.e, post-reform years).

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 lay-off 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.

[12] 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.

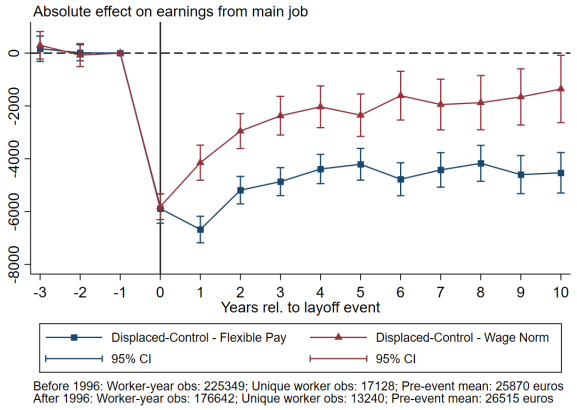
Workers displaced during the Flexible-Pay system (blue squares) continue to earn 15–25 percent less than their non-displaced peers for a full decade, whereas those displaced under the Wage Norm (red triangles) see their losses shrink to about 10 percent after the initial shock. Panel 1b plots the coefficients δ_k^R from equation 2, which directly measure the effect of the wage-setting reform on earnings. In the lay-off 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.

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-Wage 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 2,600 euros (10 percent) under the Wage Norm, yielding a long-run gain of about 3,000 euros.

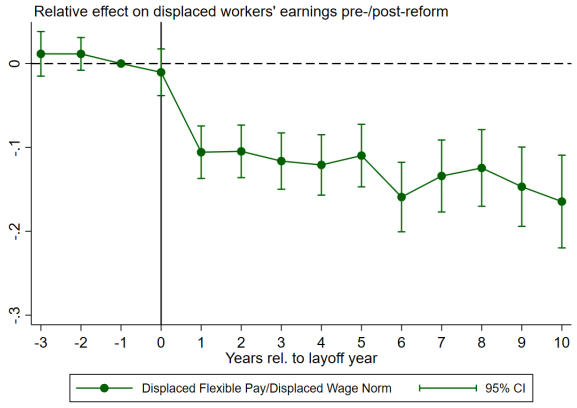
To investigate the source of these earnings losses and the role of the Wage Norm in shaping the earnings trajectories of displaced workers, we examine their employment profiles next. The event study in Figure 1c shows short-term gap in employment rates across wage-setting systems. In the first year after displacement about 10 percent of Flexible-Pay workers remain out of work, compared with 5 percent of their Wage-Norm counterparts. Averaged over the first five years (Table 2, Panel B), the employment shortfalls are 7.5 percent and 4.6 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 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 35 fewer days of work per year, whereas Wage-Norm workers lose about 30 days—a gap of roughly one working week. The same five-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.

In sum, non-employment is a plausible source of earnings losses. The re-employment rates of

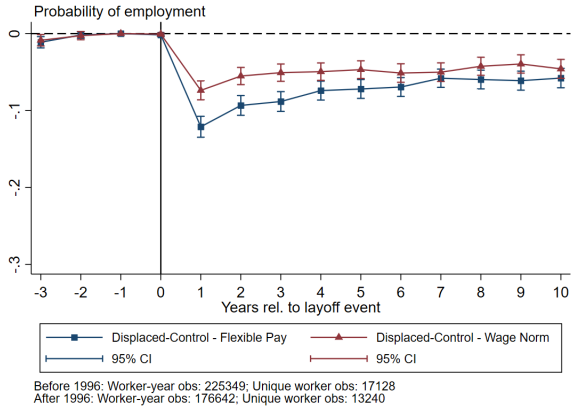
Figure 1: The Effect of Job Loss on Annual Earnings from Main Job



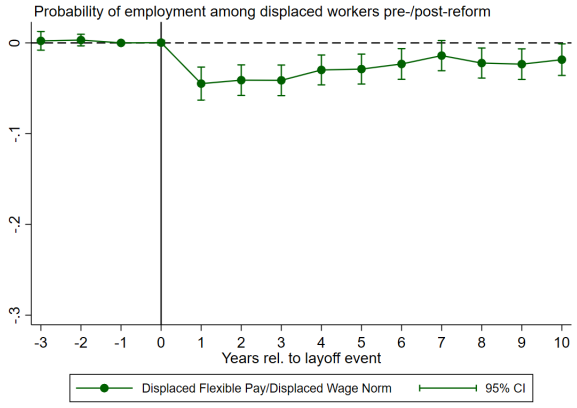
(a)



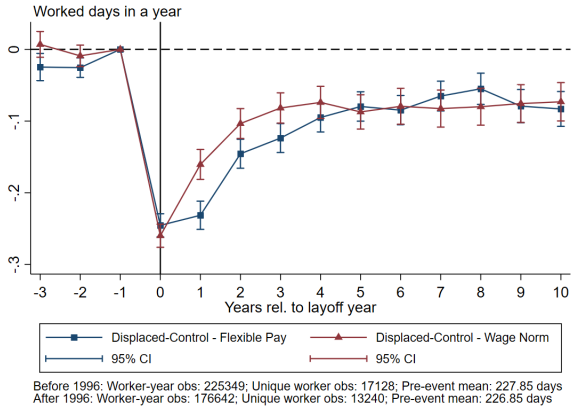
(b)



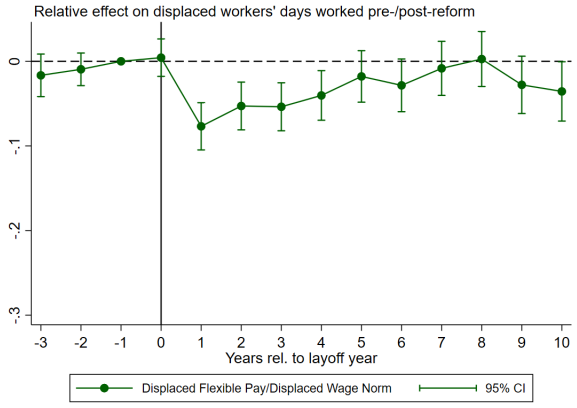
(c)



(d)



(e)



(f)

Notes: This figure shows the effect of displacement on yearly earnings (in euros, 2004 prices), employment, and days worked. 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 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)	-5201.3***	-3277.9***	-2447.2***
	(221.6)	(283.7)	(348.4)
Average event-time effect (0–10 yrs)	-4882.2***	-2556.4***	-3049.3***
	(251.9)	(350.3)	(414.6)
Baseline mean outcome	25870.3	26514.8	26151.3
Panel B: Employment			
Average event-time effect (0–5 yrs)	-0.075***	-0.046***	-0.031***
	(0.004)	(0.004)	(0.006)
Average event-time effect (0–10 yrs)	-0.069***	-0.046***	-0.026***
	(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)	-35.0***	-29.0***	-9.0***
	(1.8)	(2.0)	(2.6)
Average event-time effect (0–10 yrs)	-26.7***	-23.9***	-6.9**
	(1.9)	(2.1)	(2.8)
Baseline mean outcome	227.9	226.9	227.4
Observations	225349	176642	401991

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.

workers displaced under Flexible Pay and under the Wage Norm evolve similarly in the long term compared to non-displaced workers of their respective control groups. The 5 percentage point gap in re-employment probabilities in the first year after displacement may reflect the fact that the objective of the Wage Norm aimed at preserving Belgium's competitiveness and promoting employment by avoiding "excessive wage increases". In practice, the wage-setting reform reduced the scope of employer-specific bargaining by setting an upper limit to wage negotiations. 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. If persistent earnings

losses among displaced workers arise from 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.

Pinning down which of these channels dominates is empirically difficult; in the next sections, we present evidence on these potential mechanisms. In section 5.1 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 5.2, we examine workers mobility patterns across wage-setting systems, asking whether the wage norm influences match quality and wage outcomes. In section 5.3, we explore whether the wage norm changes earnings and employment trajectories of displaced workers similarly across sectors of employment and skills.

5.1 The effect of job loss over the business cycle

The early job displacement literature documents that workers who are displaced during recessions face even higher earnings losses (Davis and von Wachter, 2011; Couch and Placzek, 2010). More recent work focuses on the sources behind the larger losses during economic downturns. For example, Schmieder et al. (2023) show that the strong cyclical pattern of wages during the 1982, 1993, and 2003 German recessions is driven by losses in days worked, which indicates that an important part of the cyclicality of earnings losses at displacement are driven by employment losses.

We next examine to what extent the effects of job loss on earnings are driven by fluctuations over the business cycle. We observe earnings and employment trajectories of displaced and non-displaced workers between 1990 and 2010. During these two decades, Belgium experienced three recession episodes: 1993, 2001, and 2009. The latter, the great recession in 2009, is too close to the end of our sample to have had any impact in our results so we do not consider it.[13]

[13] See National Bank of Belgium (2015) for a description of the three other recessionary periods between 1970 and 2014—the first oil shock in the 1970s, 1980-1981 which follows the second oil shock, the great recession in 2009.

During the 1993 recession the GDP growth rate was -1% and the unemployment rate increased, from 6-7% in the early 1990s to 8.6% in 1993. Belgium entered a period of prolonged high unemployment rates (above 9% overall, and above 7% for men) until 1998. The unemployment rate dropped to 6.9% (5.6% for men) in year 2000. In 2001, the Belgian economy experienced a short recession that lasted 3 quarters (2nd, 3rd and 4th). The real GDP contracted by just 0.3% and the unemployment rate increased slightly, from 6.1% of the labor force in March 2001 to 7.4% in December 2001. During the subsequent six-and-a-half-year recovery, the unemployment rate kept on rising. It peaked at 8.7% of the labor force in April 2006 (Bodart et al., 2018).

To observe earnings trajectories for the full pre- and post-displacement window over 1990-2010, we consider workers who were displaced in any year between 1992-1995 (i.e., Flexible Pay) and 1996-1999 (i.e., Wage Norm). Therefore, the displacement events that we define only span over the 1993 recession.[14] A priori, this asymmetry represents a challenge in identifying the effects of the Wage Norm reform as opposed to just reflecting cyclicality present in our pre-reform period.[15]

In appendix B we examine whether there is a strong cyclical pattern in earnings and employment by plotting earnings losses of displaced workers separately by year of displacement as in Schmieder et al. (2023). We find a cyclical pattern in employment probabilities and short-term earnings losses for those displaced during the 1993 recession. However, in the German case in earnings trajectories of those displaced during expansions and recessions narrow in the long run and is much smaller than the gap in earnings that we document after the passage of the Belgian wage norm. The results this sensitivity check to allays concerns regarding the possibility of the 1993 recession driving the earnings dynamics that we document pre-reform. Moreover, when estimating Equation 2 we pool all displacement event years to test for significant differences in displaced workers' earnings and employment losses across wage-setting systems as in Janssen (2018). Pooling pre- and post-reform samples of displaced and non-displaced workers also allows us to control for global trends across all displacement years and better capture macroeconomic shocks potentially affecting earnings losses.

All in all, the long-term earnings and employment trajectories that we document under two different wage-setting systems are not driven by the business cycle. This finding is in line with cross-

[14] Moreover, the 2001 episode would not be considered a recession defined as a year of negative GDP growth because the contraction was of 0.3% and it lasted only from the second quarter to the end of year 2001.

[15] 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).

country evidence of job losses under different institutional settings. Bertheau et al. (2022), which shows that earnings losses differences 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 setting allows us to connect the results from the job loss literature with the literature on the role of employers and collective bargaining in wage setting, providing within-country causal evidence.

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. It remains hard to track all possible paths that workers can take when losing their job. Do they switch to another sector of employment? Do they wait for a better match? How is their job mobility affected? We try to answer these questions in the next sections.

5.2 Effect of job loss on career trajectory

Job loss may affects workers earnings through several channels. For example, workers may change careers, fall down the job ladder, or sort into lower quality employers. To examine the effects of job loss on workers' career trajectory, we follow Salvanes et al. (2024) to assess whether displaced workers sort into firms similarly ranked to their previous employer, conditional on finding a new job. 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 top-paying firm receives 1. We estimate equations 1 and 2 using using this normalized firm rank as a measure of employer quality.

Table 3 shows that wage-setting institutions shape the quality of firms that displaced workers are able to access. Under the flexible-pay regime (Column 1), involuntary job loss reduces the rank of the hiring firm by 2.4 percentage points in the first five post-displacement years and, although the penalty attenuates, it remains 1.1 points lower than the comparison group even a decade later. These statistically significant drops represent roughly 3.5 percent downward shift relative to baseline in the medium term and 1.6 percent below baseline in the long term.

By contrast, workers displaced after the reform do not slip down the firm-wage ladder. Over the

first five years their average firm rank is essentially unchanged, and by year 10 it even rises by 0.015 (2.2 percent). The triple-difference estimate in Column 3—which nets out both displacement status and pay regime—indicates a persistent 2.8 percentage-point disadvantage for displaced workers in flexible-pay settings relative to their wage-norm counterparts in both the short run and the long run. These results imply 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 more rigid wage-norm framework somewhat shields workers from such long-term scarring.[16]

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***	0.002	-0.028***
	(0.005)	(0.005)	(0.007)
Average event-time effect (0–10 yrs)	-0.011**	0.015***	-0.028***
	(0.005)	(0.006)	(0.008)
Baseline mean outcome	0.673	0.681	0.677
Observations	205155	161413	366568

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.

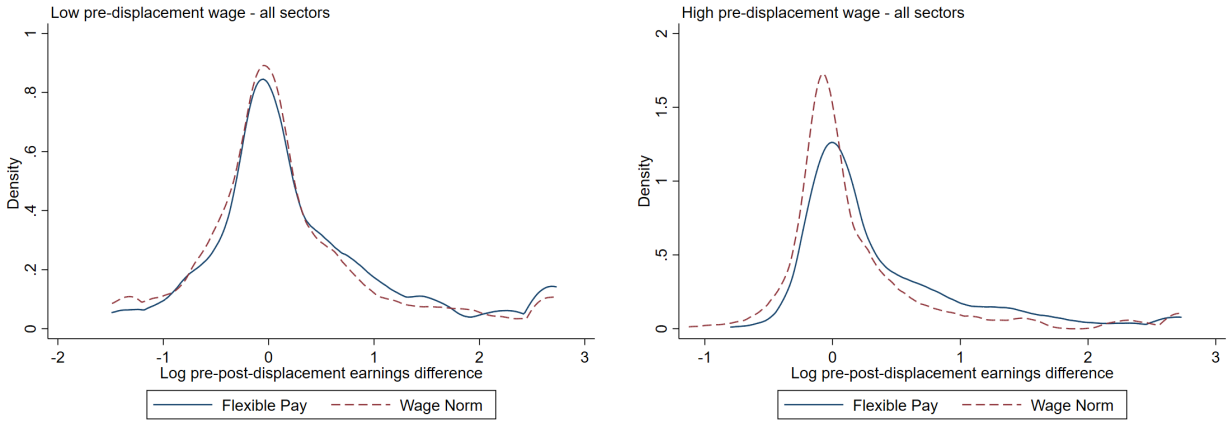
Another explanation for the short-term re-employment probabilities is that voluntary job shopping became less important in response to a more compressed wage distribution (Janssen, 2018). Under the wage norm, job-to-job transitions may play a less critical role in shaping workers' wage trajectories, as firm-specific wage premiums are more compressed. As a result, being displaced may carry smaller long-term wage penalties, since the foregone gains from upward voluntary mobility are more limited. In addition, displaced workers may have fewer incentives to prolong their job search, as the expected wage gains from waiting for a better offer are smaller relative to the costs

[16]This result may reflect that uniform wage ceilings disproportionately benefit high-productivity firms, enhancing their profitability and attractiveness to displaced workers (Bhuller et al., 2022; Kampelmann et al., 2018).

of unemployment. If displaced workers indeed respond to a less dispersed wage-offer distribution, realized pre–post-displacement wage differences should also become less dispersed after the reform. If workers reenter employment at lower wages the distribution would shift to the left.

Figure 2 plots kernel-density estimates of the log difference between pre- and post-displacement earnings under flexible pay and under the wage norm. Because the wage norm restricts wage growth, we expect the reform to bite mainly at the top of the earnings distribution and much less at the bottom. Thus, 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).

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).

Pre–post-displacement earnings differences are centered near zero for both groups, indicating that the median displaced worker did not accept post-displacement wages that were substantially below his pre-displacement level. Moreover, these earnings differences become much less dispersed for workers with high pre-displacement earnings. Panel (b) also provides suggestive evidence that high-wage displaced workers responded to the narrower wage-offer distribution by accepting lower wages under the wage norm. Overall, the results align with the idea that job shopping became less important for high earners after the reform.

5.3 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 sectoral bargain, 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, 2021). In this section, we examine the relationship between pay flexibility and the costs of job loss across sectors of employment—mainly, manufacturing and services—and across type of jobs—blue-collar and white-collar.[17]

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 placebo, and Helm et al. (2022), which takes into account the secular trend of declining manufacturing jobs as a possible driver of earnings losses for those workers who switch to low-pay service sector. Heterogeneous effects across sectors and secular growth of the service sector are important dimensions to consider in our setting for two reasons. First, if the average effects that we document are 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, if the earnings trajectories of displaced workers across wage-setting systems are 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 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 less ex-ante constraints to set wage premiums.[18]

[17] 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 (2021) 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.

[18] Bormans and Theodorakopoulos (2020) document larger increases in wage dispersion in the service sector compared to manufacturing in 14 European economies. Bormans and Theodorakopoulos (2020) find that firms in industries with limited product market competition pass on fewer productivity gains to wages compared to more competitive

Table 4 shows that 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.[19] Panel (a) shows that displaced manufacturing sector workers experienced a 18-20 percent loss in earnings during the first 5 years after displacement compared to non-displaced manufacturing sector workers. This loss in earnings remains at 17 percent on average 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 from 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 the manufacturing and service sector displaced workers could arise from switching sectors conditional on re-employment after job loss. For example, Helm et al. (2022) find that sectoral switching (out of manufacturing to low-knowledge service sector) accounts for 40 to 45 percent in establishment premium loss over time in Germany. A priory, industry switches from manufacturing to 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

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.

[19] Worker i belongs 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)	-5384.8***	-5208.1***	-586.9
	(383.7)	(433.5)	(561.6)
Average event-time effect (0–10 yrs)	-4711.0***	-4915.2***	-481.5
	(438.9)	(527.1)	(665.6)
Baseline mean outcome	27202.8	28408.1	27725.8
Observations	81464	63212	144676
Panel B: Service sector workers			
Average event-time effect (0–5 yrs)	-5583.4***	-2009.0***	-4282.4***
	(405.7)	(494.4)	(618.1)
Average event-time effect (0–10 yrs)	-5722.7***	-935.4	-5633.4***
	(462.3)	(636.9)	(753.2)
Baseline mean outcome	25666.5	26163.0	25890.7
Observations	82867	69050	151917

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.

employment is increasing and 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 a vis those who do not. We define a workers as a sectoral switcher if we observe them in a different sector any time in relative time 0-5, that is, if a workers is employed in a different sector from the reference year -1 within 5 years after displacement. We focus on Manufacturing and Services because they are the largest groups and to check if in our setting 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., 2022). 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) displaced 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.1 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.

The evidence presented in table A.1 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.2 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.

Low-wage 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—finding new jobs more quickly and at better-ranked firms. This pattern is consistent with the idea that, over the life cycle, low-wage workers primarily advance by moving to firms with higher baseline pay, while high-wage workers tend to progress through better match quality, as highlighted by Haltiwanger et al. (2018). It also supports the mechanism proposed by Janssen (2018), in which coordinated wage-setting compresses the wage-offer distribution, reducing the returns to prolonged job search and thereby shortening unemployment durations.

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 over an 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.

The smaller earnings losses under the wage norm stems mainly from swifter re-employment. Consistent with a more compressed wage-offer distribution, high-wage workers accepted new jobs earlier and overall earnings differences became less dispersed after the reform. Workers displaced under the wage norm find jobs at higher-ranked firms, indicating an improvement in the employee-employer quality match.

These gains were not uniform across the economy. In manufacturing, earnings scarring are severe and persistent regardless of bargaining regime. In services, however, the ten-year earnings loss plummeted from roughly 22 percent under flexible pay to near zero under the Wage Norm, with the largest improvements concentrated in high-knowledge subsectors.

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 income losses. Our findings are consistent with the increasing literature documenting the existence of firm-specific wage components and their role in explaining career trajectories of workers. Our study contributes to the analysis of the welfare implications of labor market institutions, such as collective bargaining and firm pay policies. We provide additional evidence of how actual labor market policies may affect displaced workers ability to get back on the job ladder.

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A Additional Tables

Table A.1: 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)	-5312.8***	-4840.2***	-1067.8
	(513.3)	(527.7)	(727.0)
Average event-time effect (0–10 yrs)	-4795.9***	-4504.7***	-1191.4
	(588.7)	(628.9)	(846.7)
Baseline mean outcome	27826.4	28909.2	28289.9
Observations	71223	53901	125124
Panel B: Service sector workers			
Average event-time effect (0–5 yrs)	-6362.5***	-2717.0***	-4300.4***
	(449.9)	(533.8)	(677.6)
Average event-time effect (0–10 yrs)	-6529.7***	-1737.8**	-5578.7***
	(507.5)	(695.0)	(828.2)
Baseline mean outcome	25746.6	26431.6	26055.6
Observations	75491	62919	138410
SWITCHERS			
Panel C: Manufacturing workers			
Average event-time effect (0–5 yrs)	-1949.1***	-3738.9***	2402.0**
	(687.7)	(1034.7)	(987.9)
Average event-time effect (0–10 yrs)	-1538.9*	-4060.6***	3216.3***
	(816.6)	(1170.9)	(1202.8)
Baseline mean outcome	22957.7	25537.2	24171.9
Observations	10241	9311	19552
Panel D: Service sector workers			
Average event-time effect (0–5 yrs)	-1356.1	1282.2	-4220.8***
	(931.2)	(1257.9)	(1441.7)
Average event-time effect (0–10 yrs)	-1559.9	2704.8*	-6153.4***
	(1161.2)	(1573.9)	(1749.6)
Baseline mean outcome	24810.3	23342.5	24141.8
Observations	7376	6131	13507

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 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.2: 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)	-7040.8***	-2130.3**	-6084.3***
	(620.9)	(833.5)	(976.5)
Average event-time effect (0–10 yrs)	-7754.1***	-1088.7	-8186.3***
	(709.4)	(1091.2)	(1215.7)
Baseline mean outcome	29728.7	31422.4	30474.2
Observations	36063	28352	64415
Panel B: Less-knowledge intensive services			
Average event-time effect (0–5 yrs)	-4400.2***	-3110.7***	-1664.5**
	(423.8)	(523.5)	(648.3)
Average event-time effect (0–10 yrs)	-4031.1***	-2486.5***	-2126.2***
	(480.9)	(604.3)	(732.6)
Baseline mean outcome	25599.5	25360.1	25493.9
Observations	50589	40832	91421
Panel C: Other services			
Average event-time effect (0–5 yrs) avg5_star	-6508.6***	-3057.0***	-4036.0***
	(1088.5)	(923.4)	(1426.6)
Average event-time effect (0–10 yrs)	-5959.3***	-1783.5	-4835.2***
	(1227.6)	(1187.2)	(1690.1)
Baseline mean outcome	22443.3	22796.4	22605.9
Observations	24726	21544	46270

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

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. For presentation purposes, we only show the first four years after job displacement in Panel (a). To facilitate the visualization of the long-term effects adding 95% confidence intervals, we plot the estimates separately for mass-layoff events that take place before the Wage Norm went into effect (i.e., 1992-1995) in Panel (b) and after the reform (i.e., 1996-1999) in Panel (c). Figure B.1 reveals some cyclicality in the loss of annual earnings from job loss in the year of displacement and in the following year. The annual earnings losses are about 7000 euros in the displacement year (i.e., a 30 percent loss with respect to mean pre-displacement earnings) and about 8000 euros in the year that follows for workers displaced during mass layoffs that take place in 1993. Earnings losses are between 5000 and 6000 euros in the displacement year for non-recession years in our sample.

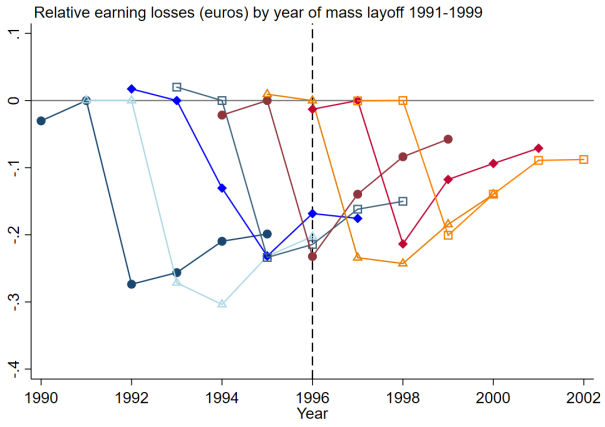
Turning to employment losses, Figure B.2 shows a cyclical pattern for the probability of employment (left panels) and days worked (right panels). The largest short-term losses are for workers who lose their jobs during the 1993 recession, which indicates that an important part of the cyclicality of earnings losses at displacement during recession years are driven by losses in days worked. While employment rates recover fast, earnings and days worked losses recover sluggishly.

To place our results within the literature, we compare the magnitude of our estimates to those found in Germany over the same period.[20] This comparison is also relevant because the GDP and unemployment rates evolve similarly in Belgium and Germany during those years. Schmieder et al. (2023) 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 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.

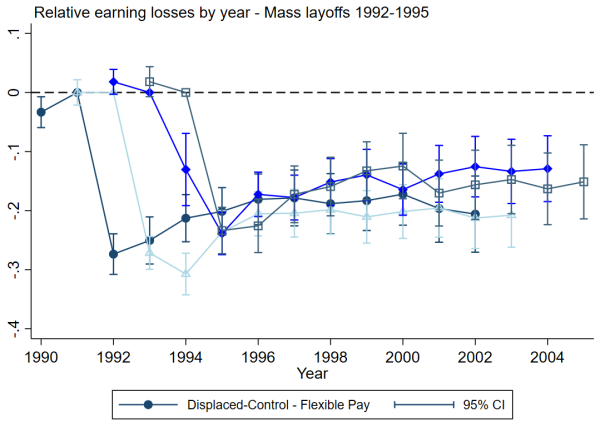
Our finding that workers losing their jobs in the 1993 recession experience larger and longer

[20] See Figures 2 and 3 in Schmieder et al. (2023).

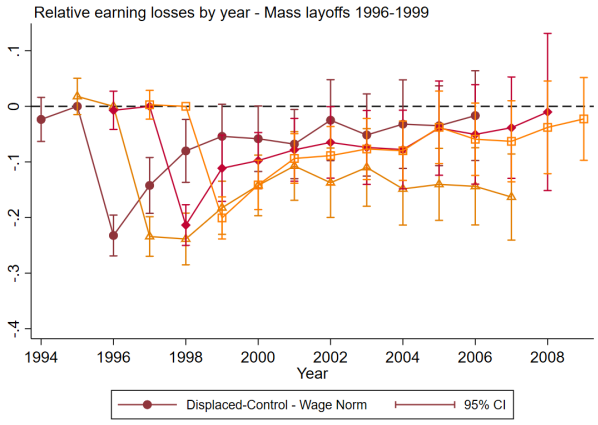
Figure B.1: The Effects of Job Loss on Earnings by Year of Job Loss



Panel (a)



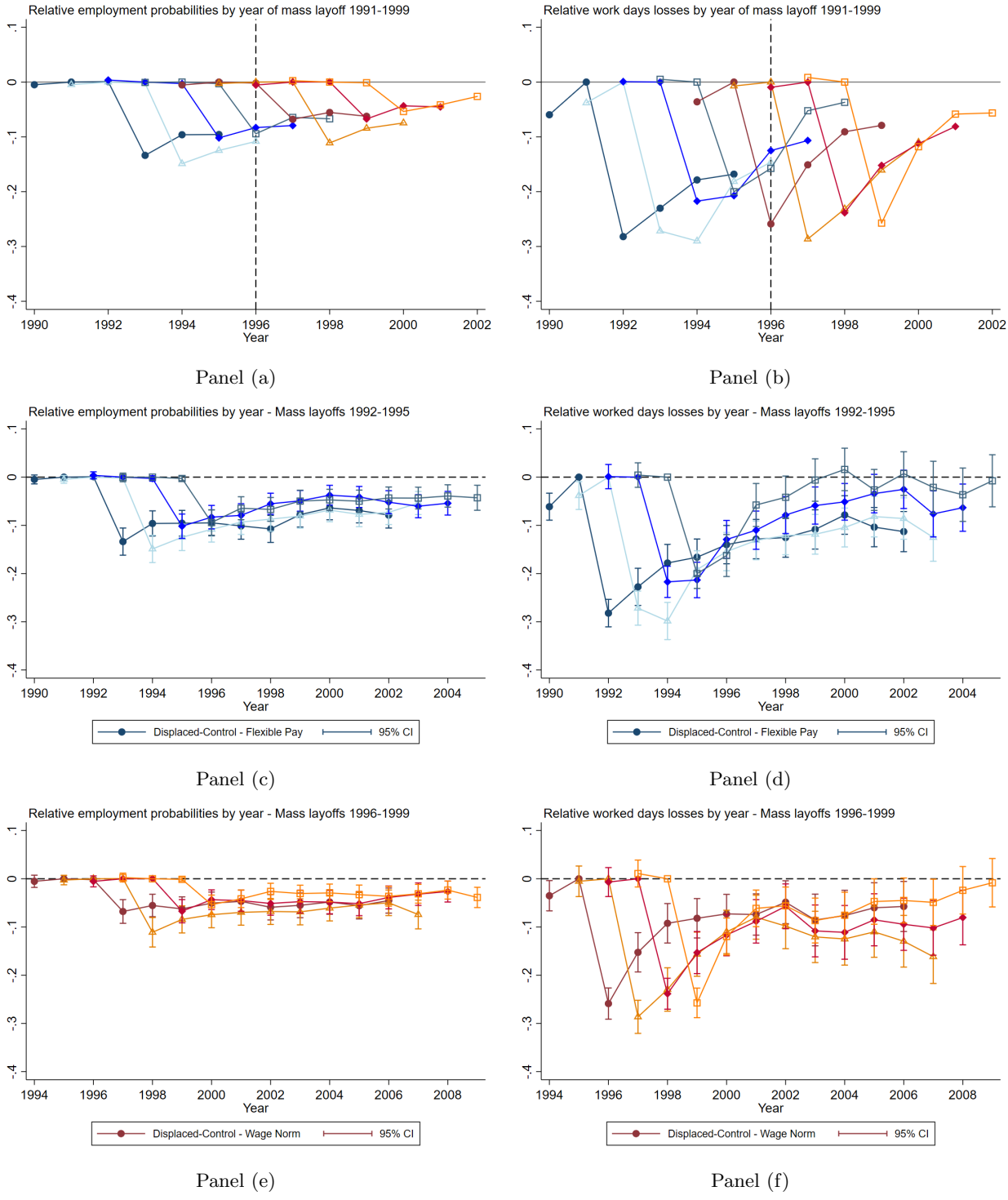
Panel (b)



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.

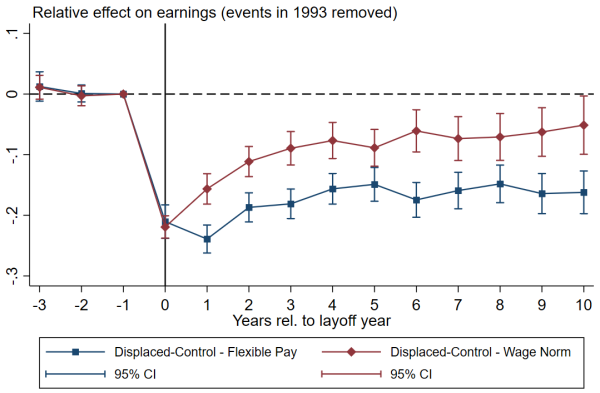
Figure B.2: The Effects of Job Loss on Employment Status and Days Worked by Year of Job Loss



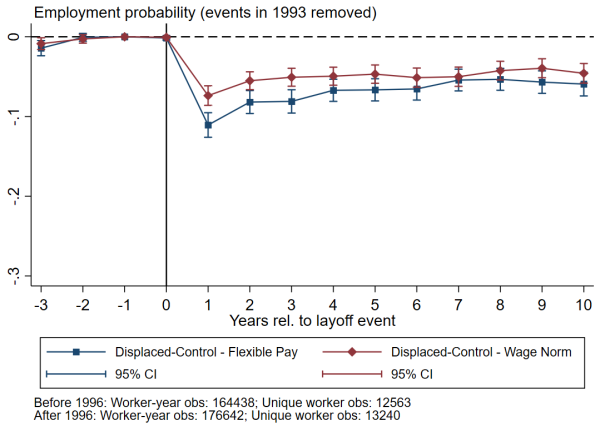
Notes: This figure shows the effect of job loss on employment probability (left panels) and on days worked (right panels) separately by year of displacement obtained by estimating equation 1 for each displacement year (i.e., 1992-1999). For presentation purposes, we only show four years after job displacement 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. where we show all displacement years. 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.

lasting earnings losses (Panel (b) in Figure B.1) driven by losses in days worked (Panel (d) in Figure B.2), is in line with the German evidence. The gap in earnings trajectories of those displaced during expansions and recessions narrows in the long run in the German case, and it is much smaller than the gap in earnings trajectories that we document after the passage of the Wage Norm in Belgium. 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.3. Comparing these estimates to those in Figure 1 allays concerns on the business cycle driving our main results. In Figure B.4 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 similar number of unique worker observations. In Panels (b) and (d) 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 pre-event information from workers displaced that early in our sample. All in all, the estimation results remain similar across samples.

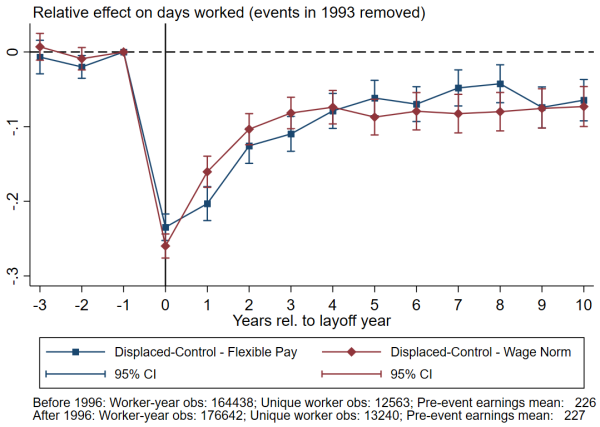
Figure B.3: The Effect of Job Loss on Earnings - Excluding Displacements during 1993 Recession



Panel (a)



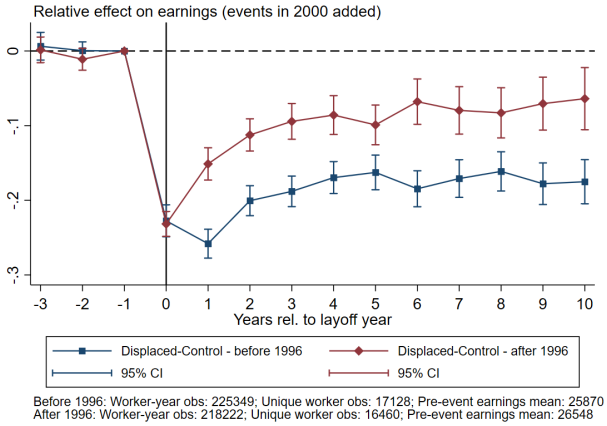
Panel (b)



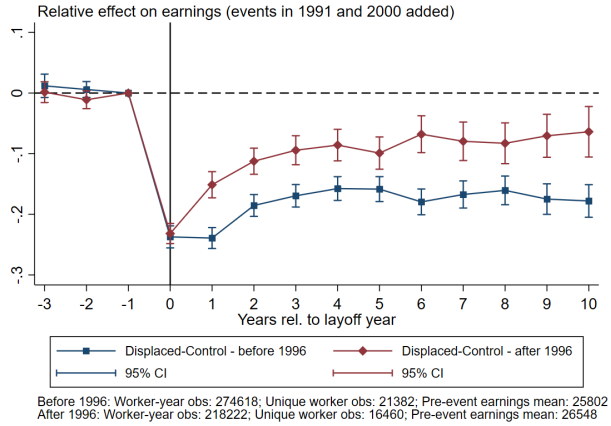
Panel (c)

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.

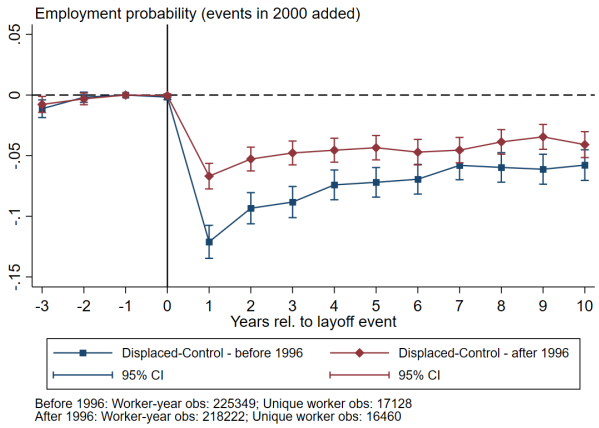
Figure B.4: The Effect of Job Loss on Earnings - Changing Event Years Considered



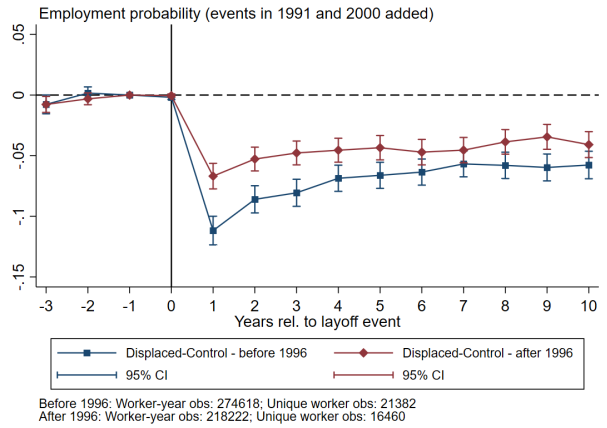
Panel (a)



Panel (b)



Panel (c)



Panel (d)

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) and (c) estimate equation 1 adding mass layoff that occur in year 2000, Panels (b) and (d) add mass layoffs in year 1991.