Flexible Wages and the Costs of Job Displacement

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Abstract

This paper investigates whether flexible pay increases the wage costs of job displacement. We use quasi-exogenous variation in the timing of job loss due to mass layoffs spanning over an institutional reform that restricted single-employer bargaining, the Belgian Wage Norm in 1996. We find that average earnings losses over a ten-year period after displacement are 10 percentage points larger under flexible pay. Workers displaced from jobs with higher employer-specific wage premiums—service sector and white-collar—benefit the most from restricted single-employer bargaining as their earnings fully converge to non-displaced workers' earnings within three years. We show that the differences in earnings losses across wage-setting systems are not driven by fluctuations in the business cycle. Our results suggest that reduced pay flexibility may help displaced workers catch up faster to non-displaced workers' pay premium ladder conditional on re-employment.

Keywords: Job displacement, wage flexibility, 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). Similarly, young adults entering the labor market during a recession suffer persistent earnings losses (Kahn, 2010; Oreopoulos et al., 2012, 2021; Schwandt and Von Wachter, 2019). While short-term consequences of unemployment can be often successfully addressed with policies such as unemployment insurance benefits, designing government policies that reduce the costs of long-term adjustment remains challenging.

There is a growing literature exploring what drives the large costs of job loss, and the role of labor market forces versus institutions in shaping the employment trajectories of affected workers. On the market forces side, a common hypothesis is that displaced workers lose firm, industry, or occupation specific skills (Lachowska et al., 2020; Huckfeldt, 2021; Raposo et al., 2021). For young workers, initial labor market economic conditions impact the most active period in their career in terms of job mobility and wage growth (Haltiwanger et al., 2018a; von Wachter, 2020). On the institutions side, recently re-emerged literature highlights the role of unions and collective bargaining in reducing inequality (Farber et al., 2021; Biasi and Sarsons, 2022) vis-à-vis recent research documenting persistent firm-level wage differentials as determinants of the cost of job loss (Schmieder et al., 2022; Fackler et al., 2021; Bertheau et al., 2022). Some evidence points to country-specific differences in institutions to explain larger costs of job loss in the U.S. compared to European countries, where sectoral bargaining agreements are more common. However, causal evidence on the effect of institutional changes in the wage-setting process on the costs of job loss is scant.

This paper uses the passage of Belgium's Wage Norm (*norme salariale*)—an institutional reform that restricted employer-specific wage premiums—to explore the relationship between flexible pay and the wage costs of job loss. The Wage Norm reform reduced the scope of firm-level bargaining by fixing a nationwide maximum permitted wage cost increase margin. Using plausibly exogenous variation in the timing of job loss due to mass layoffs spanning over the reform, we compare earnings and employment losses of displaced workers under two different wage-setting systems. We refer to

the pre-reform period as Flexible Pay, and to the post-reform period as the Wage Norm. The Belgian social security data we use covers two decades of job displacements with detailed information about employment transitions, earnings, and firm and worker characteristics. We focus our analysis on early-career workers, a group with high job mobility and wage growth for whom interruptions in the job-to-job ladder towards better quality and higher-paying jobs has potentially dire consequences in their career trajectories (Topel and Ward, 1992; Haltiwanger et al., 2018b; Von Wachter and Bender, 2006).

Using a sample of early-career male workers, we obtain several key findings. First, we find that displaced workers lose on average 24 percent of earnings the first year after displacement. While the initial drop in earnings is similar among workers displaced across wage-setting systems, the decreasing trend in earnings losses diverges thereafter. Workers displaced under Flexible Pay (i.e., before the reform) lost on average 18 percent of their annual earnings over a ten-year period after displacement, while those who were laid off under the Wage Norm (i.e., after the reform) lost on average 8 percent of their annual earnings. Job loss under Flexible Pay leads to long-lasting earnings losses that have not faded away ten years after job displacement, whereas under the Wage Norm earnings losses fade away after five years. In the short run, earnings losses are driven by employment losses and are similar across wage-setting systems. However, employment rates and days worked evolve similarly across wage-setting systems in the long run. Hence, they cannot explain the divergence in earnings trajectories. Our results imply that, by limiting employer-specific wage premiums, the Wage Norm reform made it easier for displaced workers to catch up when they became re-employed.

Second, we examine whether these differences in earnings losses are heterogeneous across sectors of employment and category of jobs. We find that workers displaced from the manufacturing sector suffer similar long-term earnings losses (of around 15-20 percent yearly) with respect to the non-displaced in both wage-setting systems. Service Sector workers displaced under Flexible Pay also suffer a strong and persistent 20 percent loss in annual earnings relative to non-displaced workers. On the contrary, earnings of service sector workers displaced under the Wage Norm fully converge to those of the non-displaced within three years. We also study the earnings and employment trajectories of blue-collar and white-collar workers; we find that they evolve similarly than those of manufacturing and service displaced workers, respectively.

Third, we show that the larger long-term earnings losses we find among displaced workers under Flexible Pay 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.

Our results imply that the primary mechanism through which the Wage Norm affected earnings and employment losses of displaced workers is via a reduction in the gap between contractual wages and actual wages—the wage premium or wage cushion. The impact of the reform was negligible in earnings losses of workers displaced from sectors exposed to international competition (already moderating wage growth pre-reform)—broadly, manufacturing jobs—and joint committees with relatively higher wage floors bargained by collective agreements (thus, lower wage dispersion)—broadly, blue-collar jobs. On the contrary, the reform impacted firm-level wage formation policies in white-collar jobs that pay a lower bargained wage (which pre-reform had more maneuver to pay firm-level wage premiums), and in service sector jobs with less exposure to international competition.

This paper makes two main contributions. First, we provide empirical evidence that displaced workers' long-term earnings losses are larger under a flexible wage-setting system (i.e., unrestricted employer-specific premiums). The recent job displacement literature has made great strides in documenting the sources behind costs of job displacement. However, studies often struggle to reconcile their findings with those of contemporary studies. While recent evidence from Germany points to persistent firm-level wage differentials as the main source of wage losses (Fackler et al., 2021; Schmieder et al., 2022), several contemporary studies in the U.S. find that establishment wage effects play a moderate role in explaining the average cost of job loss (Lachowska et al., 2020; Moore and Scott-clayton, 2019). A common explanation is that countries where collective bargaining is less common usually have higher firm-specific pay premiums, and hence higher wage inequality. However, a direct comparison of estimates on the effects of job loss on earnings from studies across countries with different institutional settings is challenging due to the other country-specific characteristics possibly explaining the opposite results in these studies. We provide within-country causal evidence of the role of employer-specific wage premiums in earnings losses after job displacement by exploiting a nationwide reform that restricted the role of employers in the wage

formation process.[1]

Second, we contribute to the re-emerging 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).[2] 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. 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.

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., 2022; 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 a given firm's pay policies to workers' wage—are time invariant. 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.

[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.

[2] 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.

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. Using this change in firm pay policy allows us to avoid the potential biases that the estimators obtained using standard AKM models are subject to in the job loss literature (Lachowska et al., 2022; Woodcock, 2022).[3] 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.

The most closely related paper is Janssen (2018) which leverages a reform to the wage bargaining system in the Danish manufacturing sector and finds that displaced workers in the manufacturing sector experience larger income losses under decentralized wage bargaining.[4] Our empirical setting allows us to explore whether displaced workers' earnings losses vary across sectors of employment and types of jobs given the variation in ex-ante heterogeneity in firm-level wage premiums bargained across sectors. We find that manufacturing and blue-collar workers suffer similar earnings losses compared to non-displaced workers regardless of the year when the mass layoff takes place with respect to the wage-setting reform. Service sector and white-collar workers displaced under Flexible Pay suffer a severe long-term impact compared to those laid off under the Wage Norm, whose earnings recover in the medium term.

More broadly, our analysis relates to an extensive body of work on the short- and long-term effects of job displacement on an array of socio-economic outcomes such as couples' fertility decisions (Huttunen and Kellokumpu, 2016), regional mobility (Huttunen et al., 2018), spousal labor supply (Halla et al., 2020), health (Black et al., 2015; Schaller and Stevens, 2015), mortality (Sullivan and Wachter, 2009), crime (Britto et al., 2022; Rege et al., 2019), retirement (Chan and Stevens, 2001) and the children of displaced workers (Fradkin et al., 2019; Lindo, 2011). Here, our results

[3] Woodcock (2022) shows that AKM estimations used in the job displacement literature suffer two kinds of bias. First, when employer wage premia are estimated separately from the wage effects of displacement. This bias is non-zero if workers sort systematically into different employers before and after displacement. Second, omitting match effects from the equation used to estimate matched components bias the estimators of the employer-specific component if the wage loss if displaced and non-displaced workers sort systematically into different quality matches. Lachowska et al. (2022) 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.

[4] The economic magnitude of the effect of job loss on Danish manufacturing workers' income losses documented in Janssen (2018) is relatively modest both before and after the wage bargaining reform: displaced manufacturing workers suffer income losses of between 6% and 7% with respect to their pre-displacement income under flexible pay, and 0.09% and 0.5% under a rigid bargaining system.

contribute to a better understanding of the implications of job loss for younger workers over a period of high job mobility, in contrast to the more mature workers studied in those papers whose average mobility is lower and cyclical downgrading may be permanent. By relying on mass-layoff events as a source of exogenous variation to young workers' job ladder, our results also contribute to the empirical evidence on job-to-job transitions as determinants of wage growth and job mobility into better quality jobs (Topel and Ward, 1992; Haltiwanger et al., 2018b; Forsythe, 2020).[5]

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 occurs at national-, sectoral-, and firm-level. First, the national collective agreement is adopted by the National Labour Council and defines minimum wages that cover the entire country. Second, sectoral-level agreements are negotiated within Joint Committees, which are permanent bodies at the industry level in which employer's associations and trade unions are represented. While 54 percent of employees are members of a union, 96 percent are covered by a collective agreement (Garnero et al., 2020). The Ministry of Employment, Labour and Social Dialogue decides to which Joint Committee a firm belongs to based on the main economic activity of the firm. These Joint Committees set sector-wide standards for all workers covered by them, including minimum wages by category of workers.[6] 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.,

[5] See von Wachter (2020) for a summary of the findings of the growing empirical literature on the persistent effects of initial labor market conditions for young adults and their sources.

[6] The provisions of collective agreements made compulsory by Royal Decree are concluded under the Act on collective agreements of 5 December 1968. Most Joint Committees are responsible for one occupation in each sector. In Joint Committees for blue-collar workers, pay scales are primarily defined in relation to the job description. For white-collar workers, the pay scale also varies by tenure (Rusinek and Tojerow, 2014).

the "favourability principle").[7] 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.[8]

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.[9] 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 labour 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 labour costs in Belgium's three major trading partners.[10]

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.[11]

After 1996, the scope for firm-level wage bargaining was reduced with the national collective agreement's increased ability to set wage ceilings within the purview of the legislation. This reform in the wage-setting system in Belgium allows us to exploit the substantial restrictions on collective bargaining at sector- and firm-level to study the relationship between wage flexibility and the costs of job displacement.

[7] 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).

[8] In 1989 the state tried to balance this automatic indexing of wages with a Competitiveness Law, which authorized government intervention if the average overall wage increases result in an upsurge of labour costs potentially deteriorating external performance of the private sector.

[9] "Loi relative à la promotion de l'emploi et à la sauvegarde préventive de la compétitivité" (Moniteur Belge, 1996)

[10] The Central Economic Council (CCE/CRB) estimates the nominal wage norm as the weighted average of the expected increase in nominal labour costs in Germany, France, and the Netherlands, according to projections published by the OECD's Economic Outlook and corrected for average working hours.

[11] 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).

3 Data

Our empirical analysis is based on combined data from several administrative registers collected by the Belgian Crossroad Bank for Social Security (CBSS). This is a linked employer-employee database that covers the universe of Belgian workers in the private sector from 1990 onward; we have access to a 10 percent representative sample.[12] 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.

3.1 Measuring job displacement at mass layoffs

We make use of the linked employer-employee structure of the CBSS data to identify mass layoffs. We follow the existing literature and 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) between the base year c and $c+1$, but exclude events in which a large number of employees moves to the same employer identifier.[13] 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 denote the year prior to displacement the "baseline year" c and we choose for each baseline year all workers that satisfy a set of restrictions. The individual is male, between age 25 and 35, and has at least one year of tenure at their main job in a private sector establishment with 20 or more employees.[14] We define an individual as displaced if the establishment has a mass layoff between

[12] 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.

[13] 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).

[14] Increasing this threshold to 50 employees does not affect our results. Because mass layoffs are defined by percentage changes in employment, small employers may be counted as having a mass layoff with only a small absolute change in employment. Results are also similar if we follow Halla et al. (2020) mass layoff definition (see footnote 6), which only considers plants with more than 11 employees and apply the following rules for size reductions: at least 3

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)	29.79	30.12	29.79	30.52
	(3.18)	(3.09)	(3.13)	(2.92)
Tenure (yrs)	2.44	2.77	3.65	4.61
	(1.16)	(1.21)	(2.46)	(2.54)
Experience in employment (yrs)	8.81	9.23	7.75	9.00
	(4.95)	(4.56)	(4.57)	(4.23)
Blue collar	0.63	0.58	0.57	0.57
	(0.48)	(0.49)	(0.50)	(0.49)
Employed full time	0.90	0.94	0.92	0.94
	(0.30)	(0.23)	(0.27)	(0.24)
Number of jobs	1.41	1.26	1.40	1.33
	(0.73)	(0.60)	(0.69)	(0.67)
Annual Earnings main job	19825.59	24344.81	21098.71	24250.15
	(11555.86)	(11065.26)	(11326.36)	(11673.21)
Annual Earnings second job	3060.48	3400.66	3185.98	3480.02
	(3433.42)	(3848.35)	(3508.44)	(4168.14)
Firm size	328.25	520	231.3	472.3
	(807.35)	(931)	(670.7)	(908.55)
Manufacturing	0.25	0.38	0.27	0.37
	(0.43)	(0.49)	(0.45)	(0.48)
Sales	0.12	0.09	0.10	0.08
	(0.33)	(0.28)	(0.30)	(0.28)
Services	0.43	0.37	0.45	0.40
	(0.50)	(0.48)	(0.50)	(0.49)
Transport	0.15	0.12	0.14	0.12
	(0.36)	(0.33)	(0.35)	(0.33)
Observations	1854	7603	1362	5444

Notes: Characteristics of displaced and non-displaced workers in year prior to displacement year. Workers satisfy the following restrictions: age 25 to 35, have at least one year of tenure, and establishment of at least 20 employees.

year c and $c+1$, and the individual leaves the establishment between year c and $c+1$ (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.[15] We focus on workers fulfilling the same baseline restrictions as our displaced sample. The comparison group contains workers employed at mass-layoff firms at the mass-layoff date who do not lose their jobs, and workers who are employed at any reference year from 1992 to 1999 at firms that do not experience a mass-layoff event. Because the latter is a large group, we draw a 10 percent random sample. We then assign to controls a placebo dismissal date equal to the layoff date of the treated workers who satisfy the same baseline restrictions in year c and compare outcomes for the two groups at different time intervals relative to the layoff date.[16]

Our main sample comprises 3,216 displaced male workers and 13,047 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., 2022; 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

individuals in plants with 11-20 employees, at least 5 individuals in plants with 21-100 employees, at least 5 percent in plants with 100-600 employees, at least 30 employees in plants with more than 600 employees.

[15] 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., 2022; 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).

[16] 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. (2022).

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 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.[17]

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. (2022)), we estimate the following regression model:

[17]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.

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

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.[18]

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.[19] To avoid collinearity, the specification omits δ_{-1} (i.e., normalizing relative to the period prior to treatment) and one of the year dummies.

To quantify the displacement effects, we also average the difference between displaced and non-displaced workers relative to the reference data over 10 years after displacement. We estimate the following regression model:

[18] In the baseline specification we do not include any time-varying control variables represented by 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).

[19] Schmieder et al. (2022) 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.

$$Y_{itc} = \delta^{post} * Disp_i I(t > c) + \sum_{k=-3}^{-1} \delta_k I(t = c+1+k) * Disp_i + \sum_{k=-3}^{10} \gamma_k I(t = c+1+k) + \pi_t + \alpha_i + \epsilon_{itc} \quad (2)$$

where $\delta^{post} * Disp_i$ is the difference between displaced and control workers relative to the reference date averaged over 10 years after displacement.

5 The Effect of Job Loss on Earnings and Employment

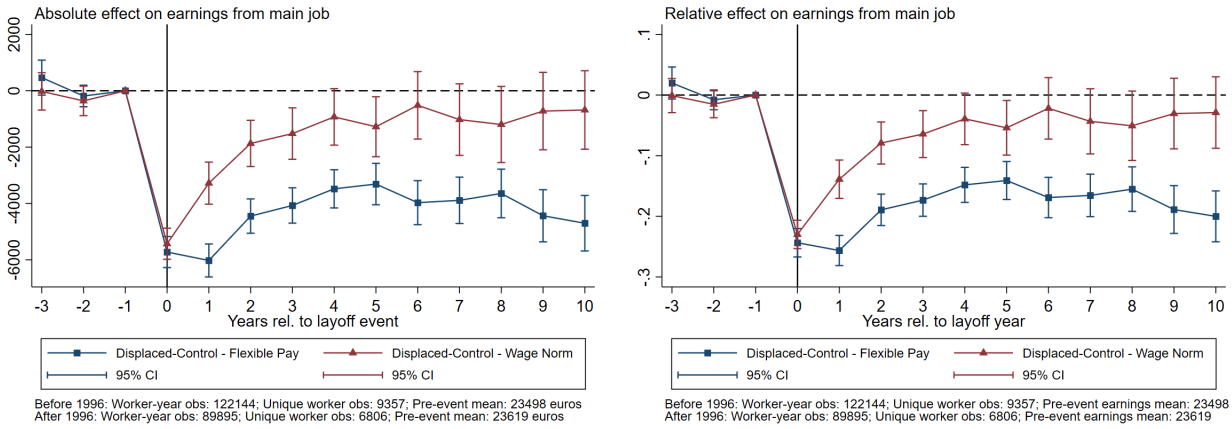
In this section, we provide estimates of the long-term effect of job displacement on labor market outcomes under Flexible Pay (i.e., pre-reform years) and under the Wage Norm (i.e, post-reform years). We begin by investigating the overall effect of job loss on early-career male workers in section 5.1. In section 5.2, we turn to examining heterogeneous effects of job loss on earnings and employment based on the workers' sector of employment and job type at the time of the mass layoff. In section 5.3, we examine the effect of job loss on earnings and employment over the business cycle.

5.1 Labor market outcomes of displaced workers under different wage-setting systems

We start by investigating the overall effect of the job displacement on earnings and employment up to ten years after job loss under Flexible Pay and the Wage Norm. Figure 1 compares earnings profiles in the displaced group and in the control group. The left panel plots the treatment effect in absolute terms (i.e., in euros, 2004 prices), and the right panel provides a relative comparison to the corresponding pre-event level of individuals' earnings from their main job. The initial drop in earnings in the year of the layoff event is 24 percent (6000 euros) compared to the pre-event earnings mean. These short-term earnings losses are similar across workers displaced under either wage-setting systems. However, the decreasing trend in earnings losses diverges thereafter. Workers displaced under Flexible Pay suffer a severe long-term impact compared to those laid off under the Wage Norm. The blue line (square) shows that displaced workers who lost their jobs between 1991 and 1995 experienced substantial earnings losses relative to their comparison group of non-displaced workers, ranging from 15 to 25 percent annually with respect to the pre-event mean

earnings. The red line (triangle) shows that displaced workers who lost their jobs under the Wage Norm experienced earnings losses of around 10 percent compared to non-displaced workers in their control group.

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



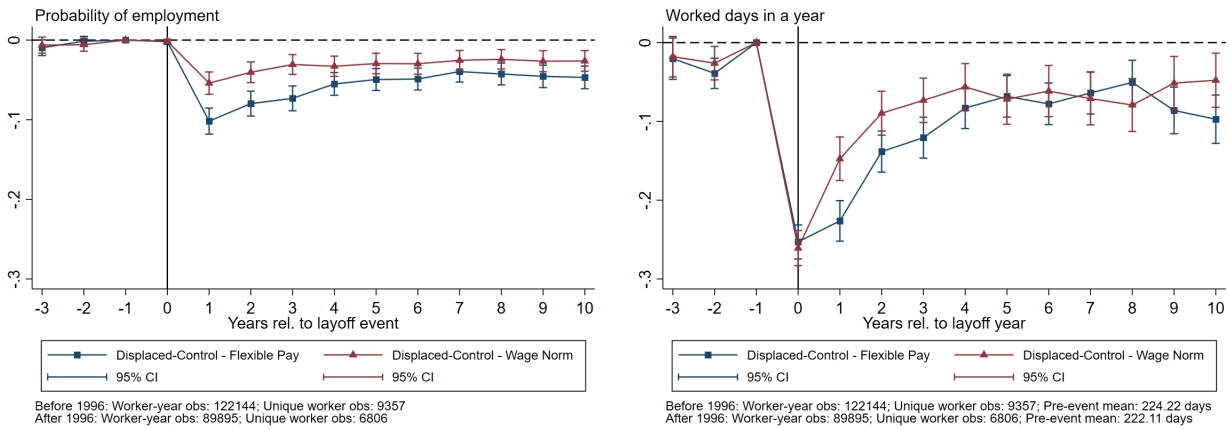
Panel (a) Panel (b)

Notes: This figure shows the effect of displacement on yearly earnings (in euros, 2004 prices). 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 left panel plots the treatment effect in absolute terms, and the right panel provides a relative comparison to the corresponding pre-event level of earnings.

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. Figure 2 compares annual employment rates and days worked before and after job displacement for workers in the displaced group and in the control group. Panel (a) shows that in the first year post-layoff, 10 percent of workers displaced under Flexible Pay are still not employed compared to 5 percent of those displaced under the Wage Norm. The gap in employment probabilities between the two groups narrows 3 years after displacement.

The employment probability is one by construction for years c and $c+1$ (i.e., years -1 and 0 in Panel a) because of the baseline tenure restriction that we imposed. Panel (b), though, allows us to quantify the effect of job displacement on days worked, another measure of employment dynamics. In the year of displacement, displaced individuals work 57 days less relative to the non-displaced under both wage setting systems. This represents a 25 percent decline in days worked compare to

Figure 2: The Effect of Job Loss on Employment Rates and Days Worked



Panel (a) Panel (b)

Notes: This figure shows the change in employment rates (Panel a) and days worked (Panel b) of displaced workers with respect to the baseline year (-1), relative to the employment profile among non-displaced workers. 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. Both panels provides a relative comparison to the corresponding pre-event mean employment rate and number of days worked in a year.

their pre-event days worked. The first year after job loss, workers displaced under Flexible Pay (blue line, square) work 51 days less relative to non-displaced workers, while individuals displaced under the Wage Norm (red line, triangle) work 33 days less relative to their control group (i.e., 23 and 15 percent with respect to the pre-displacement mean, respectively). This initial gap in days worked between displaced workers before and after the reform also narrows three years after the mass-layoff event. Yet, despite an initial recovery, the reduction in days work is permanent for both groups of displaced workers, who work on average 20 days less per year compared to non-displaced workers.

All in all, non-employment is a plausible source of earnings losses in the short term under both wage-setting systems, but it does not explain the larger long-term losses under Flexible Pay compared to the Wage Norm. The re-employment rates of 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.[20] On the contrary, average earnings losses over a ten-year period

[20]The 5 percentage point gap in re-employment probabilities in the first year after displacement, although small in magnitude, 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".

after displacement are 10 percentage points larger under Flexible Pay. Our results thus far imply that wage flexibility is associated with larger earnings losses after job displacement. This evidence is in line with the increasing literature documenting the existence of firm-specific wage components (Card et al., 2013; Song et al., 2019) and their role in explaining the long-term costs of job loss (Schmieder et al., 2022; Fackler et al., 2021; Bertheau et al., 2022).

In practice, the wage-setting reform reduced the scope of employer-specific bargaining by setting an upper limit to wage negotiations. 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. In the next sections, we examine to what extent the earnings losses are driven by wage reductions conditional on re-employment. First, we explore whether the effects of the reform are concentrated in workers displaced from jobs with higher ex-ante employer-specific wage premiums, such as service-sector or white-collar workers. Second, we investigate to what extent our results are affected by the business cycle.

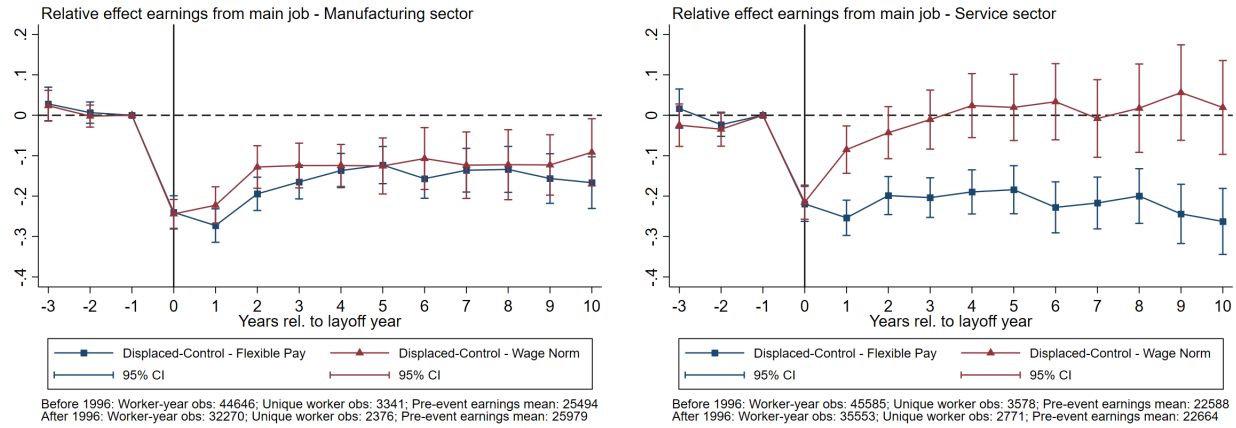
5.2 Heterogeneous effects by sector of employment and type of job

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.[21]

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

[21] 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.

Figure 3: The Effect of Job Loss on Annual Earnings: Manufacturing Sector and Service Sector



Panel (a) Panel (b)

Notes: This figure shows the effect of displacement on yearly earnings (in euros, 2004 prices) for workers who are employed in the manufacturing sector (Panel a) and the service sector (Panel b) at the reference date. 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 provide a relative comparison to the corresponding pre-event mean level of earnings.

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.[22]

Figure 3 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.[23] Panel (a) shows that displaced manufacturing sector workers experienced a 25 percent loss in earnings during the first few years after displacement compared to non-displaced manufacturing sector workers. This loss in earnings remains at 15 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

[22] 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 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.

[23] 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.

reform. Panel (b) shows that service sector workers displaced under a Flexible Pay system suffer a strong and persistent 20 percent loss in annual earnings (blue line, square), while service sector displaced workers' earnings catch up to those of the non-displaced group in about three years after a mass layoff event taking place under the Wage Norm (red line, triangle).

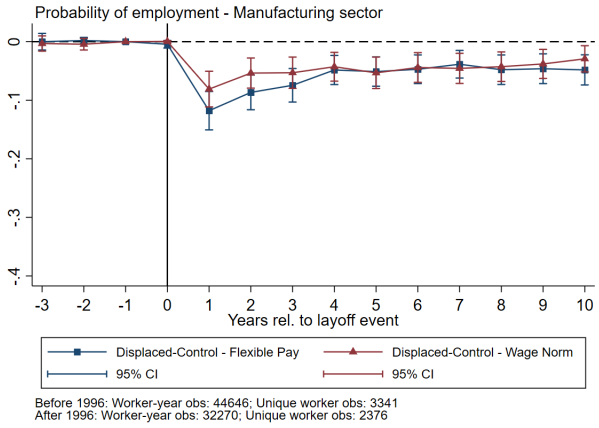
Our findings are in line with the economic intuition that restrictions on employer-specific wage premiums introduced by the reform should have an effect on workers from in industries with higher wage growth and wage dispersion associated with wage premiums. We take this results as suggestive evidence that conditional on reemployment, restricting employer-specific wage premiums, made it easier for displaced workers to catch up to non-displaced workers' earnings profiles.

Figure 4 presents the employment profiles of displaced workers in the manufacturing sector (top panels) and the service sector (bottom panels), relative to the evolution of employment profiles of non-displaced workers in each sector. Panels (a) and (b) show that, in both sectors, the employment rate drops 10 percentage points the year after displacement under Flexible Pay and remains around 5 percent lower compared to the control group for the following 10 years. Manufacturing workers laid off under the Wage Norm follow a similar employment trajectory. Displaced and non-displaced service sector workers employment rates are not statistically significant different in the medium term. Panels (b) and (c) show similar patterns in days worked across sectors and wage-setting systems. The initial drop in days worked is larger in the manufacturing sector (i.e., about 80 days or 30 percent with respect to the pre-event mean) than in the service sector (i.e., about 50 days or 20 percent with respect to the pre-event mean), gradually recovering and stalling at 20 to 30 days per year in both sectors.

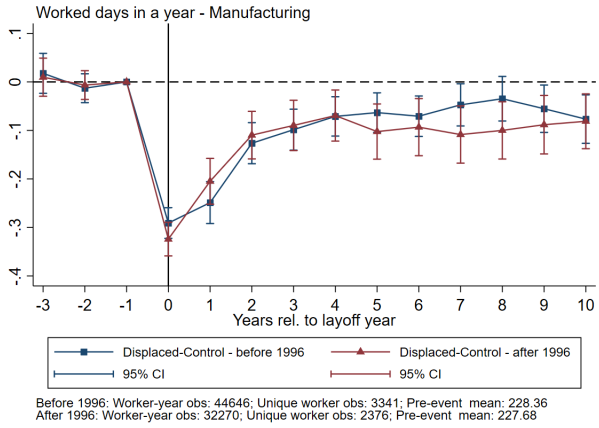
All in all, the relative magnitude of the earnings loss from job displacement only partially mirrors individuals probability of re-employment after displacement, which indicates that employment rates are not the main driver of earnings drops. This result is consistent with findings in Schmieder et al. (2022) and in Lachowska et al. (2020) for Germany and Washington State, respectively, who show that long-term earnings losses are to an important extent driven by losses in wages.

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

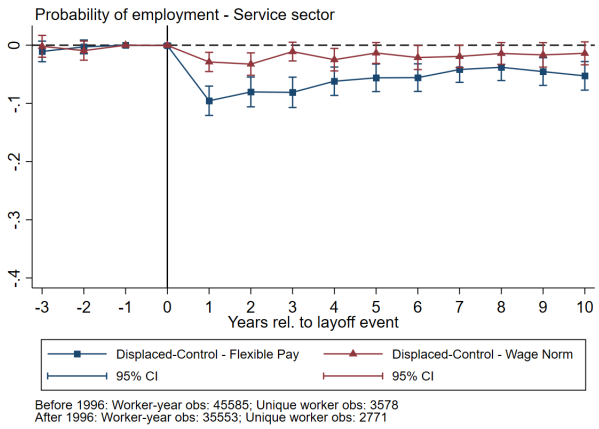
Figure 4: The Effect of Job Loss on Employment and Days Worked: Manufacturing Sector and Service Sector



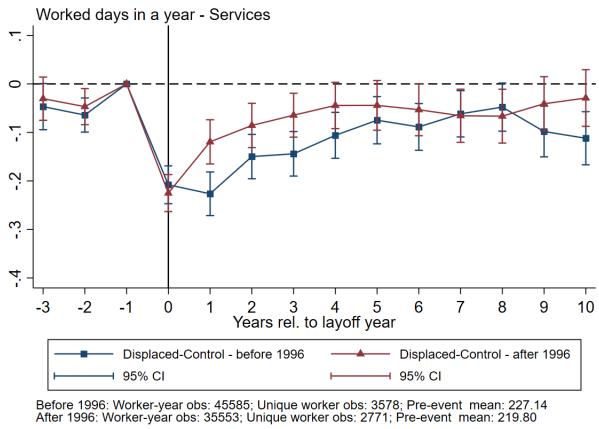
Panel (a)



Panel (b)



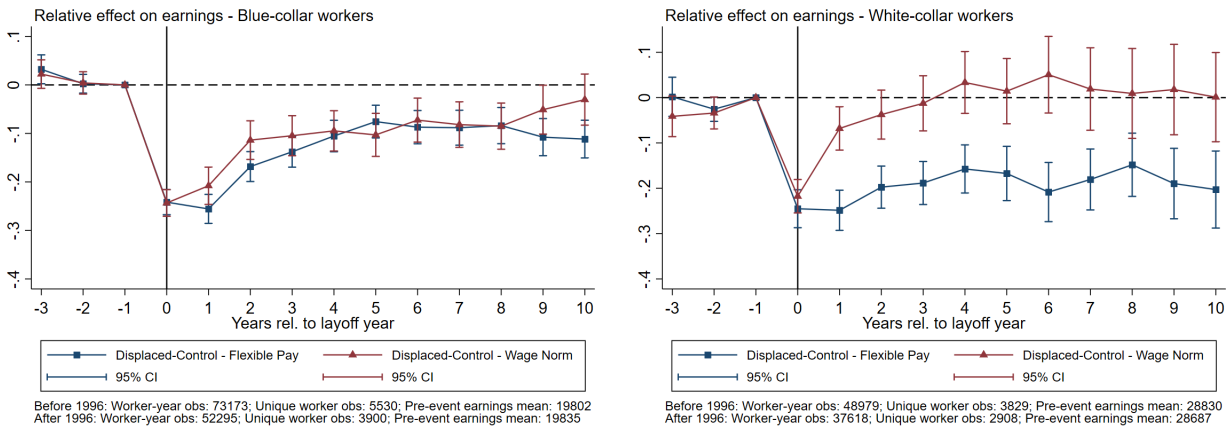
Panel (c)



Panel (d)

Notes: This figure shows the effect of displacement on employment rates (left) and days worked (right) in a year for workers who are employed in the manufacturing sector (top panels) and the service sector (bottom panels) at the reference date. 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 5: The Effects of Job Loss on Earnings: Blue-collar and White-collar Employees



Panel (a) Panel (b)

Notes: This figure shows the effect of displacement on yearly earnings (in euros, 2004 prices) for workers who are employed in blue-collar jobs (Panel a) and in white-collar jobs (Panel b) at the reference date. 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 provide a relative comparison to the corresponding pre-event mean level of earnings.

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 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.

Figure A.1 shows the coefficient obtained from estimating Equation 1 for the subset of displaced and non-displaced workers who do not switch to a different sector of employment within five years of a displacement event (i.e., we exclude sector switchers).[24] The estimation results for workers in the manufacturing sector Panel (a) and the service sector in Panel (b) confirm that the effect of job loss on earnings is not driven by workers who switch sectors. Our finding that industry switches play no role in explaining earnings losses is consistent with a large empirical literature showing that reallocation between industries or occupations does not appear to be a major source of employment fluctuation over the business cycle (Abraham and Katz, 1986; Aaronson and Christopher, 2004; Rothstein, 2017).

Thus far we have documented that long-run earnings losses are not driven by employment

[24]Most of the workers in our sample remain in the same sector they were employed at in the reference year c.

losses or by switching sectors of employment. We next explore to what extent earnings losses are the consequence of lower re-employment wages due to changes in within-sector wage premium losses related to job types. Previous literature linking union-wage setting to wage inequality find that industries or professional categories with higher average bargained wages are associated with lower dispersion in actual wages and wage premiums (Cardoso and Portugal, 2005). In Belgium, wage rates in the private sector are settled collectively by industry in joint committees consisting of trade unions and employers' federations. In this context, we would expect the effects of the Wage Norm on earnings to be concentrated in professional categories (or joint committees) that pay a lower bargained wage (say, with weak bargaining power) and have more room to maneuver to pay wages above the contractual wage pre-reform (Rycx, 2003; Vandekerckhove et al., 2020).

Our data does not contain detailed information on professional categories (that determine joint committees representation), but it allows us to organize employment records measuring the number of blue-collar and white-collar employees at the reference year c. Often, there is a separate joint committee for white-collar and blue-collar workers within an economic branch (Vandekerckhove et al., 2020).[25]

Figure 5 shows that in the year of displacement both blue- and white-collar displaced workers suffer a similar drop (around 20 percent) in earnings across wage-setting systems. Panel (a) shows that the wage-setting reform does not have any heterogeneous impact on the long-term earnings losses of blue-collar workers. After the initial 20 percent drop, blue-collar displaced workers earnings stay 10 percent lower than non-displaced counterparts. Panel (b) shows a different story for white-collar workers. Under Flexible Pay, earnings losses for white-collar displaced workers stall at 20 percent yearly over the ten-year period following the mass layoff compared to non-displaced white-collar workers. Under the Wage Norm, the difference between white-collar displaced and non-displaced workers is not statistically significant two years after displacement. Figure A.2 confirms that employment rates and days worked do not explain the different earnings trajectories.

Our results are in line with previous evidence on the relationship between collective bargaining,

[25] A non-exhaustive list of blue-collar joint committees: clothing, textile cleaning, car maintenance, glass manufacturing, chemical manufacturing, food manufacturing, food distribution, textile manufacturing, cleaning, construction, carpetenrs, publishing, paper industry, transport and logistics, gardening, metal and related industries. White-collar joint committees include: independent stores, food trade, chemical industry, metal industry, petrol industry, textile industry, clothing, various service industries, food industry. See Vandekerckhove et al. (2018) and Plasman (2015) for a discussion on wage dispersion across joint committees.

employer bargaining, and wage dispersion. Cardoso and Portugal (2005) show that the returns to collective bargaining power are offset by firm-specific arrangements (in the form of a wage premium) in the manufacturing and service private sector in Portugal.[26] Dahl et al. (2013) find that when wages are bargained at firm level (as opposed to collective bargaining at sectoral level) smaller wage premiums are found in the lower part of the wage distribution, while larger premiums are found in the upper part. Regarding results in the job displacement literature specifically, Raposo et al. (2021) show that sorting into firms plays an important role in driving wage losses because firms often deviate from collectively bargained wage floors for each occupational categories.

The estimates of Equation 1 of the effect of job displacement on earnings (in 2004 Euros), employment probabilities, and days worked are presented in Table A.1, Table A.2, and Table A.3, respectively. These three tables show the dynamic earnings and employment trajectories in the overall economy (columns 1-2), in the manufacturing and service sector (columns 3-6), and in blue-collar and white-collar jobs (columns 7-10). In this way, we summarize all the results in the figures presented thus far. Table A.4 presents the estimation results for the mean effects on job displacement on annual earnings (Panel A), employment (Panel B), and days worked (Panel C) based on Equation 2. The estimated coefficients of *Displaced* x *Post* report the difference between displaced and control individuals relative to the reference date averaged over ten years after displacement.

Our results imply that the primary mechanism through which the Wage Norm affected earnings losses of displaced workers is via a reduction in the gap between contractual wages and actual wages—the wage premium or wage cushion. The impact of the reform was negligible in earnings losses of workers displaced from sectors exposed to international competition (already constraining wage growth pre-reform)—broadly, manufacturing jobs—and joint committees with relatively higher wage floors bargained by collective agreements (thus, lower wage dispersion)—broadly, blue-collar jobs. On the contrary, the reform impacted firm-level wage formation policies in white-collar jobs that pay a lower bargained wage (which pre-reform had more maneuver to pay firm-level wage premiums), and in service sector jobs with less ex-ante exposure to international competition.

[26] Cardoso and Portugal (2005) show that professional categories with higher bargained wages tend to present a lower dispersion of the wage cushion. At the level of the collective bargaining agreement, higher average bargained wages are associated with lower dispersion of both the wage cushion and the actual wage.

5.3 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. (2022) 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.[27]

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.[28] 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.[29]

[27] 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.

[28] 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.

[29] 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).

In what follows we present results from two main sensitivity checks to allay concerns regarding the possibility of the 1993 recession driving the earnings dynamics that we document pre-reform. First, 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. (2022). Second, 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.

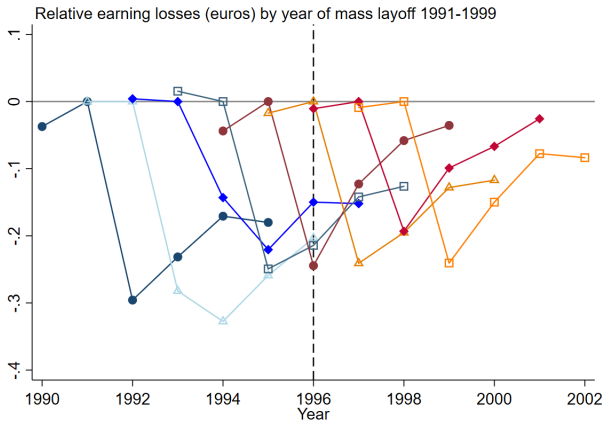
5.3.1 Estimates of the Cyclicality of Earnings Losses

Figure 6 and Figure 7 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 6 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.

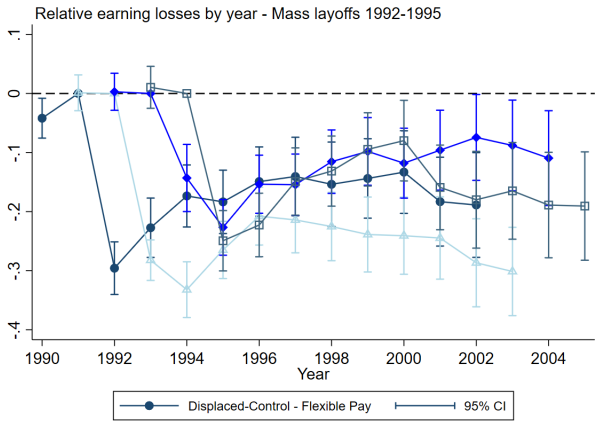
Turning to employment losses, Figure 7 shows a cyclical pattern for the probability of employment (left panels) and days worked (right panels). The largest short-term losses are for workers who lose their jobs during the 1993 recession, which indicates that an important part of the cyclicality of earnings 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

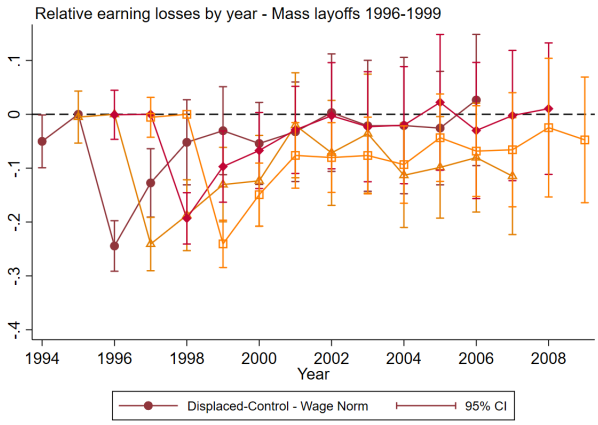
Figure 6: The Effects of Job Loss on Earnings by Year of Job Loss



Panel (a)



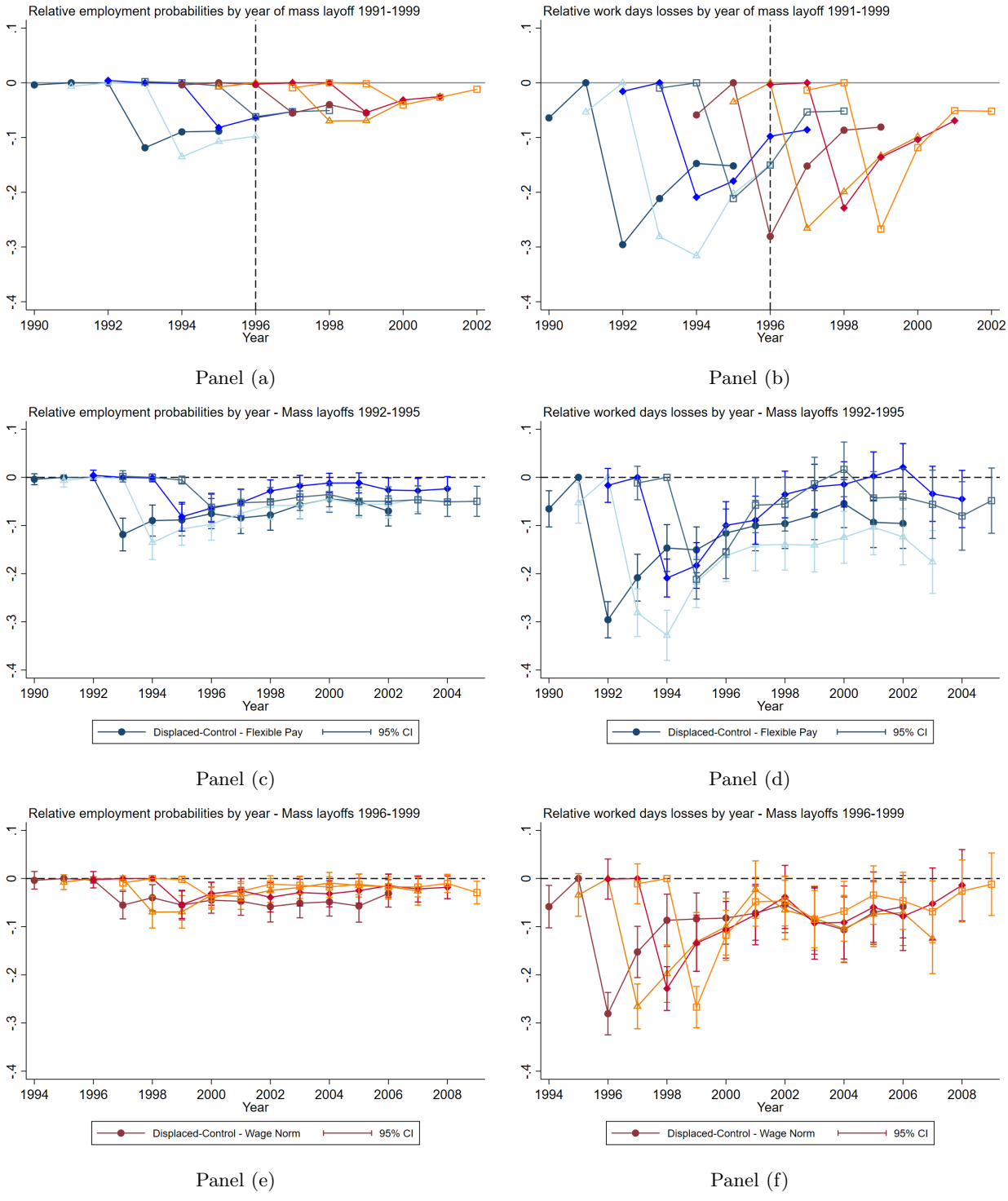
Panel (b)



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 7: 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.

found in Germany over the same period.[30] This comparison is also relevant because the GDP and unemployment rates evolve similarly in Belgium and Germany during those years. Schmieder et al. (2022) 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 lasting earnings losses (Panel (b) in Figure 6) driven by losses in days worked (Panel (d) in Figure 7), 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 A.3. Comparing these estimates to those in Figure 1 allays concerns on the business cycle driving our main results.[31]

5.3.2 The effects of job loss among displaced workers (pooled sample)

Thus far, we showed diverging earnings 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), we pool all mass-layoff event years pre- and post-reform (i.e., 1992-1999) to estimate:

[30] See Figures 2 and 3 in Schmieder et al. (2022).

[31] In Figure A.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.

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

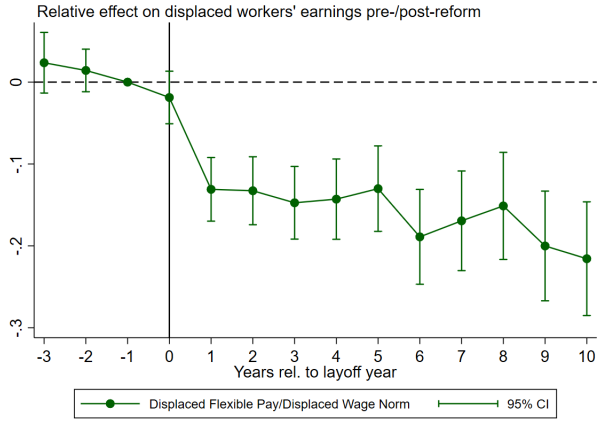
where $Disp_i * FlexiblePay$ equals one if the workers is displaced under Flexible Pay (pre-reform). The coefficients of interest, δ_k^R, measure the change in earnings of displaced workers under Flexible Pay with respect to the baseline year (c), relative to earnings profiles among displaced workers 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 capture macroeconomic shocks potentially affecting earnings losses.[32] We can estimate Equation 3 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 showing no 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.

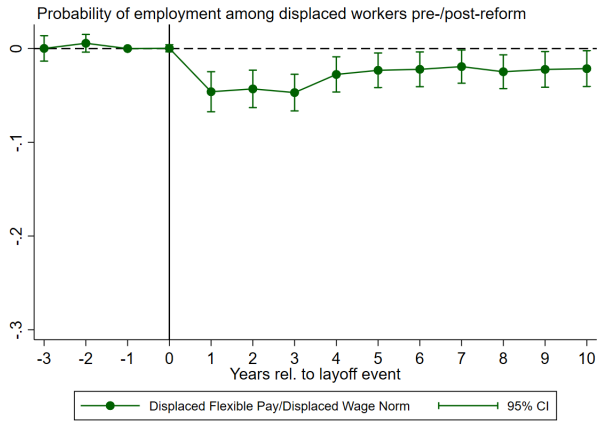
Figure 8 plots the coefficients δ_k^R, which directly measure the effect of the wage-setting reform on earnings (Panel (a)), employment rates (Panel (b)), and days worked (Panel (c)). In the year of displacement, the difference in earnings losses between workers displaced under either wage-setting system are not statistically significant. However, each year thereafter, workers displaced under Flexible Pay experience earnings losses 10 to 20 percentage points larger compared to workers displaced under the wage norm. The difference in employment rates between workers displaced pre- and post-reform is 4 percentage points in the short term and 2 percentage points in the long

[32] Equation 3 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 $FlexiblePay * Disp_i$ and $FlexiblePay * \pi_t$ separately as in the triple difference framework. In our specification, π_t and α_i jointly pick up the isolated wage-setting reform effect. (See Equation (2) in Janssen (2018)). See Bennett (2022) for another example of a triple difference approach to compare displaced workers earnings losses before and after a reform, which expanded a second chance education scheme in Norway.

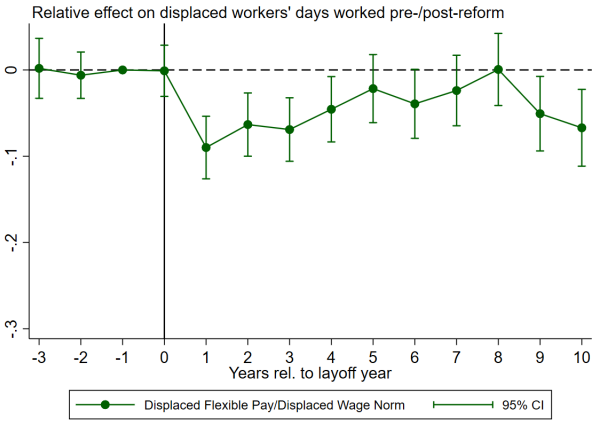
Figure 8: The effects of job loss on earnings, employment, and days work among displaced workers pre-/post reform



Panel (a)



Panel (b)



Panel (c)

Notes: This figure shows estimates of δ_k^R in Equation 3, which measure the change in earnings and employment of displaced workers under Flexible Pay with respect to the baseline year (c), relative to earnings and employment profiles among displaced workers under the Wage Norm. The outcome variables are earnings in Panel (a), employment rates in Panel (b), and days worked in Panel (c).

term. Days worked for those displaced under Flexible Pay drop 10 percentage points more than for those displaced under the Wage Norm in the short term but then recover to similar levels.

Figure A.5 the effect of the reform among displaced workers in the manufacturing sector (Panel (a)), in the service sector (Panel (b)), in blue-collar jobs (Panel (c)), and white-collar jobs (Panel (d)). The figure shows that there are no statistically significant differences among displaced manufacturing [blue-collar] workers across wage-setting systems. However, service sector [white-collar] workers displaced under Flexible Pay experience earnings losses 20 to 40 percentage points larger compared to service sector workers displaced under the Wage Norm.

All in all, pooling the samples of those displaced between 1992-1995 and 1996-1999 to test differences in earnings and employment trajectories among displaced workers confirms our previous results.

6 Conclusion

This paper uses administrative data from Belgium covering two decades to explore the relationship between flexible pay and the wage costs of job loss. We use variation in the timing of job loss due to mass layoffs spanning over an institutional reform that restricted employer-specific wage premiums, 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 focus our analysis on early-career workers, a group with high job mobility and wage growth for whom interruptions in the job-to-job ladder towards better quality and higher-paying jobs has potentially dire consequences in their career trajectories.

We obtain several key findings. First, we find that displaced workers lose on average 24 percent of earnings the first year after displacement. While the initial drop in earnings is similar across workers displaced before and after the wage setting-reform, the decreasing trend in earnings losses diverges thereafter. Workers displaced before the Belgian Wage Norm went into effect lost on average 18 percent of their annual earnings over a 10-year period after displacement, while those who were laid off after the passage of the law lost on average 8 percent. In the short run, earnings losses are driven by losses in employment and days worked. However, employment rates and days worked recover similarly across wage-setting systems in the long run. Hence, they do not explain

the divergence in earnings trajectories. Our results imply that, by limiting employer-specific wage premiums, the Wage Norm made it easier for displaced workers to catch up when the upon re-employment.

Second, we examine whether these differences in earnings losses are heterogeneous across two sectors of sectors of employment—manufacturing and service—and across two occupational categories of jobs—white-collar and blue-collar. Our results imply that the primary mechanism through which the Wage Norm affected earnings losses of displaced workers is via a reduction in the gap between contractual wages and actual wages—the wage premium or wage cushion. The impact of the reform was negligible in earnings losses of workers displaced from sectors exposed to international competition (already constraining wage growth pre-reform)—broadly, manufacturing jobs—and joint committees with relatively higher wage floors bargained by collective agreements (thus, lower wage dispersion)—broadly, blue-collar jobs. On the contrary, the reform impacted firm-level wage formation policies in white-collar jobs that pay a lower bargained wage (which pre-reform had more maneuver to pay firm-level wage premiums), and in service sector jobs with less ex-ante exposure to international competition.

Finally, 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.

Our findings are consistent with the increasing literature documenting the existence of firm-specific wage components and their role in explaining career trajectories of young 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 (in thousands of euros, 2004 prices)

Dependent variable: Annual earnings from main job (1000 euros)										
	Overall		**Manufacturing**		**Service**		**Blue-collar**		**White-collar**	
	Pre-96	Post-96	Pre-96	Post-96	Pre-96	Post-96	Pre-96	Post-96	Pre-96	Post-96
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Pre-event										
δ_{-3}	0.466	-0.022	0.714	0.611	0.373	-0.557	0.637**	0.443	0.051	-1.186*
	(0.317)	(0.338)	(0.539)	(0.504)	(0.558)	(0.608)	(0.301)	(0.297)	(0.635)	(0.654)
δ_{-2}	-0.185	-0.360	0.166	-0.054	-0.527	-0.782	0.048	0.082	-0.736*	-0.975*
	(0.195)	(0.268)	(0.342)	(0.362)	(0.332)	(0.487)	(0.195)	(0.233)	(0.395)	(0.515)
Year of mass-layoff event										
δ_0	-5.724***	-5.430***	-6.125***	-6.340***	-4.954***	-4.874***	-4.786***	-4.827***	-7.067***	-6.240***
	(0.281)	(0.282)	(0.535)	(0.474)	(0.498)	(0.488)	(0.261)	(0.278)	(0.616)	(0.539)
Post-event										
δ_1	-6.024***	-3.279***	-6.958***	-5.784***	-5.730***	-1.927***	-5.062***	-4.124***	-7.165***	-1.952***
	(0.298)	(0.381)	(0.538)	(0.606)	(0.502)	(0.678)	(0.302)	(0.389)	(0.651)	(0.699)
δ_2	-4.448***	-1.869***	-4.957***	-3.325***	-4.488***	-0.976	-3.333***	-2.257***	-5.693***	-1.074
	(0.311)	(0.418)	(0.535)	(0.698)	(0.542)	(0.744)	(0.311)	(0.403)	(0.684)	(0.791)
δ_3	-4.071***	-1.520***	-4.206***	-3.231***	-4.602***	-0.242	-2.729***	-2.077***	-5.431***	-0.362
	(0.320)	(0.466)	(0.546)	(0.733)	(0.565)	(0.844)	(0.320)	(0.417)	(0.698)	(0.890)
δ_4	-3.481***	-0.928*	-3.485***	-3.226***	-4.283***	0.540	-2.086***	-1.879***	-4.537***	0.958
	(0.346)	(0.512)	(0.549)	(0.688)	(0.629)	(0.915)	(0.328)	(0.421)	(0.781)	(0.998)
δ_5	-3.312***	-1.277**	-3.143***	-3.259***	-4.160***	0.439	-1.497***	-2.043***	-4.825***	0.409
	(0.375)	(0.543)	(0.598)	(0.918)	(0.685)	(0.947)	(0.341)	(0.449)	(0.880)	(1.054)
δ_6	-3.972***	-0.518	-3.999***	-2.781***	-5.145***	0.759	-1.724***	-1.438***	-6.007***	1.449
	(0.399)	(0.611)	(0.630)	(1.014)	(0.727)	(1.090)	(0.346)	(0.459)	(0.960)	(1.237)
δ_7	-3.889***	-1.024	-3.466***	-3.209***	-4.902***	-0.180	-1.747***	-1.623***	-5.207***	0.543
	(0.420)	(0.647)	(0.703)	(1.090)	(0.738)	(1.110)	(0.363)	(0.475)	(0.988)	(1.331)
δ_8	-3.645***	-1.198*	-3.416***	-3.176***	-4.514***	0.397	-1.661***	-1.686***	-4.271***	0.264
	(0.440)	(0.690)	(0.739)	(1.148)	(0.780)	(1.264)	(0.376)	(0.482)	(1.025)	(1.451)
δ_9	-4.437***	-0.721	-3.990***	-3.192***	-5.512***	1.272	-2.132***	-1.012**	-5.466***	0.509
	(0.472)	(0.701)	(0.798)	(0.990)	(0.844)	(1.365)	(0.387)	(0.510)	(1.143)	(1.459)
δ_{10}	-4.701***	-0.682	-4.250***	-2.380**	-5.933***	0.435	-2.211***	-0.601	-5.848***	0.029
	(0.503)	(0.710)	(0.832)	(1.101)	(0.941)	(1.343)	(0.393)	(0.533)	(1.249)	(1.440)
Mean t-1	23	24	25	26	23	23	20	20	29	29
Worker-year Obs	122,144	89,895	44,646	32,270	45,585	35,553	73,173	52,295	48,979	37,618

Notes: This table shows the effect of job loss on displaced workers' earnings (in thousands of euros, 2004 prices) at a yearly level based on Equation 1. The coefficient δ_k measures the average difference in earnings between displaced and non-displaced workers. We present estimation results for workers who at the reference date are employed in any sector (columns 1-2), in the manufacturing sector (columns 3-4), in the service sector (5-6), in blue-collar jobs (columns 7-8), and in white-collar jobs (columns 9-10). Odd [even] columns present the estimation results using the sample of displaced and non-displaced workers during events that occur before [after] the wage-setting reform. The second to last row shows the pre-event mean, which refers to the mean of the dependent variable in the year before the reference date.

Table A.2: The Effects of Job Loss on Employment

Dependent variable: Probability of Employment										
	Overall		**Manufacturing**		**Service**		**Blue-collar**		**White-collar**	
	Pre-96	Post-96	Pre-96	Post-96	Pre-96	Post-96	Pre-96	Post-96	Pre-96	Post-96
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Pre-event										
δ_{-3}	-0.010*	-0.006	-0.000	-0.003	-0.011	-0.002	-0.007	-0.006	-0.013	-0.008
	(0.005)	(0.005)	(0.007)	(0.007)	(0.009)	(0.010)	(0.006)	(0.006)	(0.009)	(0.009)
δ_{-2}	-0.001	-0.006	0.002	-0.004	-0.003	-0.009	0.001	-0.004	-0.006	-0.007
	(0.003)	(0.004)	(0.003)	(0.005)	(0.006)	(0.008)	(0.003)	(0.005)	(0.006)	(0.007)
Year of mass-layoff event										
δ_0	-0.002	-0.001	-0.004	0.000	-0.001	-0.000	-0.003	-0.001	0.000	-0.002
	(0.001)	(0.001)	(0.003)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.000)	(0.002)
Post-event										
δ_1	-0.102***	-0.054***	-0.118***	-0.081***	-0.096***	-0.029***	-0.105***	-0.072***	-0.093***	-0.028***
	(0.008)	(0.007)	(0.017)	(0.016)	(0.013)	(0.008)	(0.010)	(0.011)	(0.014)	(0.008)
δ_2	-0.080***	-0.040***	-0.087***	-0.054***	-0.080***	-0.032***	-0.089***	-0.058***	-0.058***	-0.016**
	(0.008)	(0.007)	(0.015)	(0.013)	(0.013)	(0.010)	(0.010)	(0.010)	(0.012)	(0.007)
δ_3	-0.073***	-0.031***	-0.074***	-0.053***	-0.081***	-0.011	-0.078***	-0.046***	-0.059***	-0.009
	(0.008)	(0.006)	(0.015)	(0.014)	(0.013)	(0.008)	(0.010)	(0.010)	(0.012)	(0.006)
δ_4	-0.055***	-0.033***	-0.048***	-0.043***	-0.062***	-0.025**	-0.060***	-0.045***	-0.040***	-0.016**
	(0.007)	(0.006)	(0.013)	(0.013)	(0.012)	(0.010)	(0.010)	(0.010)	(0.010)	(0.007)
δ_5	-0.050***	-0.029***	-0.051***	-0.053***	-0.056***	-0.013	-0.052***	-0.038***	-0.039***	-0.017**
	(0.007)	(0.007)	(0.013)	(0.014)	(0.012)	(0.009)	(0.009)	(0.009)	(0.010)	(0.008)
δ_6	-0.049***	-0.030***	-0.047***	-0.044***	-0.056***	-0.021**	-0.053***	-0.043***	-0.033***	-0.010
	(0.007)	(0.007)	(0.012)	(0.013)	(0.012)	(0.011)	(0.009)	(0.010)	(0.009)	(0.008)
δ_7	-0.040***	-0.025***	-0.039***	-0.046***	-0.042***	-0.019*	-0.040***	-0.033***	-0.032***	-0.014*
	(0.007)	(0.006)	(0.012)	(0.013)	(0.011)	(0.010)	(0.009)	(0.009)	(0.009)	(0.008)
δ_8	-0.043***	-0.024***	-0.048***	-0.043***	-0.038***	-0.014	-0.048***	-0.030***	-0.024**	-0.015*
	(0.007)	(0.006)	(0.013)	(0.013)	(0.012)	(0.010)	(0.009)	(0.009)	(0.009)	(0.008)
δ_9	-0.046***	-0.027***	-0.046***	-0.038***	-0.045***	-0.017	-0.055***	-0.028***	-0.019**	-0.025**
	(0.007)	(0.007)	(0.013)	(0.013)	(0.012)	(0.011)	(0.010)	(0.009)	(0.009)	(0.010)
δ_{10}	-0.047***	-0.026***	-0.048***	-0.029**	-0.053***	-0.014	-0.050***	-0.024***	-0.035***	-0.029***
	(0.007)	(0.007)	(0.013)	(0.011)	(0.013)	(0.010)	(0.009)	(0.009)	(0.011)	(0.010)
Mean t-1	1	1	1	1	1	1	1	1	1	1
Worker-year Obs	122,144	89,895	44,646	32,270	45,585	35,553	73,173	52,295	48,979	37,618

Notes: This table shows the effect of job loss on employment rates at a yearly level based on Equation 1. The coefficient δ_k measures the average difference in employment probabilities between displaced and non-displaced workers. We present estimation results for workers who at the reference date are employed in any sector (columns 1-2), in the manufacturing sector (columns 3-4), in the service sector (5-6), in blue-collar jobs (columns 7-8), and in white-collar jobs (columns 9-10). Odd [even] columns present the estimation results using the sample of displaced and non-displaced workers during events that occur before [after] the wage-setting reform. The second to last row shows the pre-event mean, which refers to the mean of the dependent variable in the year before the reference date.

Table A.3: The Effects of Job Loss on Days Worked in a Year

Dependent variable: days worked in a year										
	Overall		**Manufacturing**		**Service**		**Blue-collar**		**White-collar**	
	Pre-96	Post-96	Pre-96	Post-96	Pre-96	Post-96	Pre-96	Post-96	Pre-96	Post-96
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Pre-event										
δ_{-3}	-4.577	-3.962	4.039	2.212	-10.642*	-6.683	0.247	-0.613	-14.396**	-9.473*
	(2.999)	(2.915)	(4.794)	(4.562)	(5.511)	(5.001)	(3.293)	(3.469)	(5.742)	(5.053)
δ_{-2}	-8.748***	-5.795**	-2.967	-1.474	-14.576***	-10.285**	-3.924	-2.279	-17.756***	-10.144**
	(2.198)	(2.409)	(3.466)	(3.447)	(4.078)	(4.175)	(2.426)	(2.880)	(4.209)	(4.110)
Year of mass-layoff event										
δ_0	-56.707***	-57.927***	-66.434***	-73.774***	-47.224***	-49.451***	-53.105***	-60.779***	-61.647***	-54.230***
	(2.470)	(2.519)	(3.681)	(4.023)	(4.518)	(4.281)	(2.634)	(3.099)	(4.947)	(4.142)
Post-event										
δ_1	-50.739***	-32.735***	-56.824***	-46.637***	-51.436***	-26.236***	-47.777***	-41.311***	-53.186***	-20.359***
	(2.940)	(3.138)	(5.025)	(5.483)	(5.210)	(5.113)	(3.354)	(4.111)	(5.659)	(4.724)
δ_2	-31.008***	-19.897***	-28.848***	-24.975***	-34.023***	-18.821***	-27.306***	-21.174***	-35.502***	-17.471***
	(2.984)	(3.160)	(4.930)	(5.696)	(5.298)	(5.121)	(3.465)	(4.220)	(5.629)	(4.743)
δ_3	-27.054***	-16.272***	-22.480***	-20.414***	-32.735***	-14.170***	-21.504***	-17.130***	-35.044***	-14.271***
	(2.977)	(3.193)	(4.922)	(6.026)	(5.312)	(5.073)	(3.528)	(4.380)	(5.428)	(4.572)
δ_4	-18.604***	-12.517***	-16.233***	-15.785***	-24.075***	-9.758*	-13.717***	-16.625***	-25.498***	-5.643
	(2.980)	(3.372)	(4.724)	(6.116)	(5.489)	(5.346)	(3.506)	(4.593)	(5.510)	(4.828)
δ_5	-15.305***	-15.961***	-14.449***	-23.310***	-16.999***	-9.722*	-7.811**	-20.010***	-27.904***	-9.229*
	(3.009)	(3.605)	(4.742)	(6.608)	(5.639)	(5.751)	(3.531)	(4.953)	(5.598)	(5.087)
δ_6	-17.435***	-13.667***	-16.152***	-21.239***	-20.133***	-11.683*	-8.579**	-16.391***	-33.318***	-9.868*
	(3.016)	(3.688)	(4.860)	(6.837)	(5.588)	(6.014)	(3.531)	(5.082)	(5.645)	(5.142)
δ_7	-14.321***	-15.781***	-10.800**	-24.693***	-13.988**	-14.429**	-8.064**	-19.434***	-24.736***	-10.059*
	(3.071)	(3.786)	(5.065)	(6.851)	(5.531)	(6.140)	(3.606)	(5.229)	(5.720)	(5.290)
δ_8	-11.339***	-17.591***	-7.909	-22.756***	-10.861*	-14.604**	-7.307*	-20.076***	-17.011***	-13.880***
	(3.220)	(3.805)	(5.356)	(6.853)	(5.745)	(6.222)	(3.827)	(5.229)	(5.886)	(5.364)
δ_9	-19.296***	-11.425***	-12.618**	-20.078***	-22.282***	-8.998	-14.520***	-12.123**	-27.180***	-9.918*
	(3.388)	(3.845)	(5.687)	(7.001)	(6.057)	(6.270)	(4.103)	(5.226)	(5.974)	(5.560)
δ_{10}	-21.807***	-10.604***	-17.509***	-18.480***	-25.443***	-6.397	-18.432***	-10.333**	-27.389***	-10.260*
	(3.511)	(3.897)	(5.818)	(6.566)	(6.346)	(6.548)	(4.235)	(5.265)	(6.266)	(5.668)
Observations	122,144	89,895	44,646	32,270	45,585	35,553	73,173	52,295	48,979	37,618
Mean t-1	224	222	228	228	227	220	208	208	248	241

Notes: This table shows the effect of job loss on days worked at a yearly level based on Equation 1. The coefficient δ_k measures the average difference between displaced and non-displaced workers. We present estimation results for workers who at the reference date are employed in any sector (columns 1-2), in the manufacturing sector (columns 3-4), , in the service sector (5-6), in blue-collar jobs (columns 7-8), and in white-collar jobs (columns 9-10). Odd [even] columns present the estimation results using the sample of displaced and non-displaced workers during events that occur before [after] the wage-setting reform. The second to last row shows the pre-event mean, which refers to the mean of the dependent variable in the year before the reference date.

Table A.4: Average Effect of Job Loss on Earnings, Employment, and Days Worked

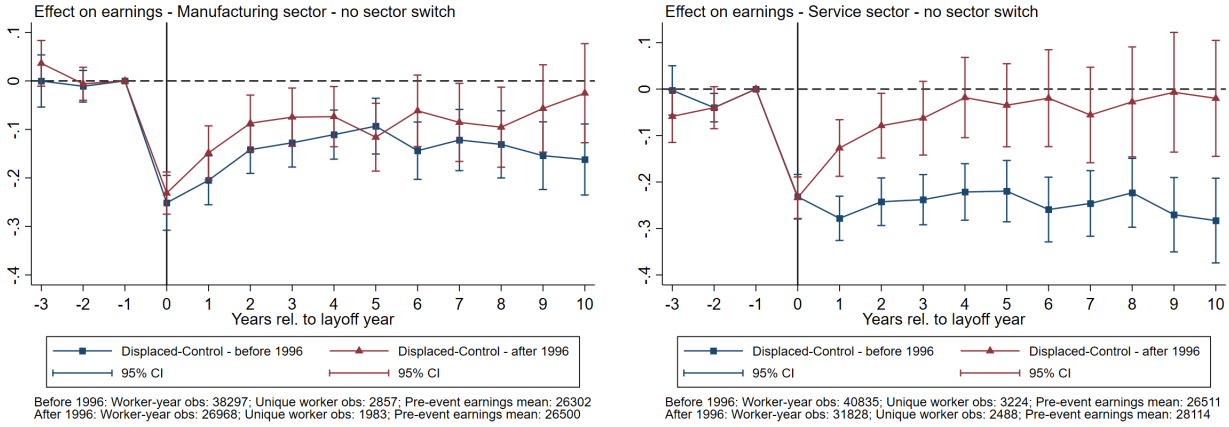
	Overall		**Manufacturing**		**Service**		**Blue-collar**		**White-Collar**	
	Pre-96	Post-96	Pre-96	Post-96	Pre-96	Post-96	Pre-96	Post-96	Pre-96	Post-96
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: Annual earnings (1000 euros)										
Displaced x Post	-4.428***	-1.912***	-4.455***	-3.754***	-4.934***	-0.779	-2.726***	-2.284***	-5.766***	-0.960
	(0.294)	(0.422)	(0.500)	(0.689)	(0.515)	(0.735)	(0.267)	(0.340)	(0.646)	(0.800)
Mean t-1	23.5	23.6	25.5	26.0	22.6	22.7	19.8	19.8	28.8	28.7
Panel B: Probability of Employment										
Displaced x Post	-0.052***	-0.028***	-0.055***	-0.043***	-0.053***	-0.017***	-0.057***	-0.037***	-0.037***	-0.015***
	(0.005)	(0.004)	(0.009)	(0.009)	(0.007)	(0.006)	(0.006)	(0.006)	(0.006)	(0.005)
Mean t-1	1.0	1.0	1.0	1.0	1.0	1.0	1.0	1.0	1.0	1.0
Panel C: Days worked in a year										
Displaced x Post	-27.771***	-22.657***	-26.388***	-30.241***	-29.252***	-19.475***	-21.998***	-24.999***	-36.684***	-18.863***
	(2.368)	(2.607)	(3.709)	(4.907)	(4.329)	(4.212)	(2.759)	(3.544)	(4.393)	(3.788)
Mean t-1	224.2	222.1	228.4	227.7	227.1	219.8	207.7	207.9	248.2	241.2
Worker-year Obs	122,144	89,895	44,646	32,270	45,585	35,553	73,173	52,295	48,979	37,618

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: This table displays the impact of job displacement on worker's labor market outcomes based on Equation 2. We present estimation results for workers who at the reference date are employed in any sector (columns 1-2), in the manufacturing sector (columns 3-4), in the service sector (5-6), in blue-collar jobs (columns 7-8), and in white-collar jobs (columns 9-10). Odd [even] columns present the estimation results using the sample of displaced and non-displaced workers during events that occur before [after] the wage-setting reform. In Panel A, the dependent variable is the yearly earnings in thousands of euros (2004 prices), with zeros for those not employed. In Panel B, it equals to one if the individual is employed at a given year. The outcome is the number of days worked in Panel C. Displaced x Post measures the average difference in the outcome variable between the displaced and the control group relative to the reference date in the ten years after the reference year. The bottom row of each panel shows the pre-event mean, which refers to the mean of the dependent variable in the year before the reference date.

B Additional Figures

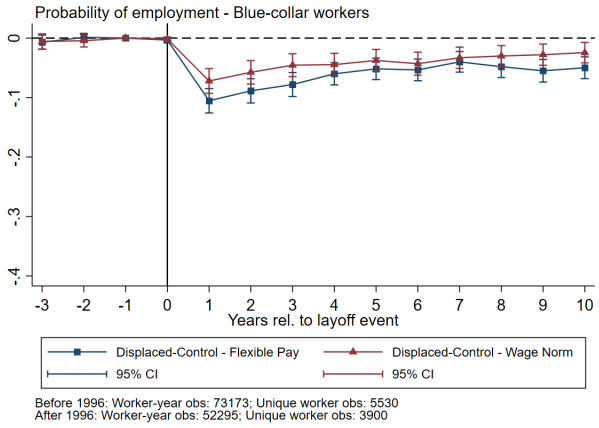
Figure A.1: The Effects of Job Loss on Earnings for Non-Switchers: Manufacturing and Services



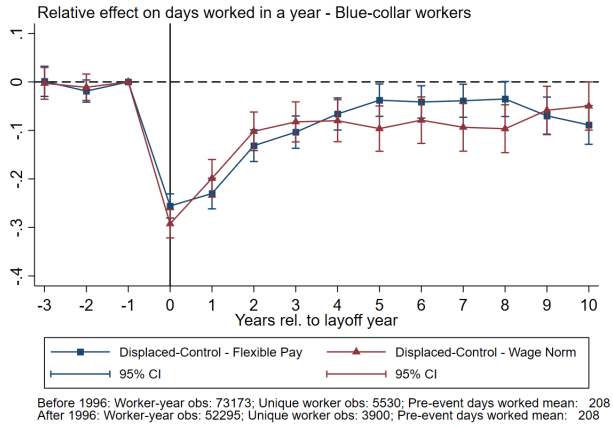
Panel (a) Panel (b)

Notes: This figure shows the effect of displacement on yearly earnings (in euros, 2004 prices) for workers who at the reference date are employed in the manufacturing sector (Panel a) and the service sector (Panel b), and who do not switch sectors after displacement conditional on re-employment. The blue line (square) plots the effect given by the difference between displaced and non-displaced workers *before* the passage of the Wage Norm. The red line (triangle) plots the effect given by the difference between displaced and non-displaced workers *after* the wage-setting reform. The graphs provide a relative comparison to the corresponding pre-event level of earnings.

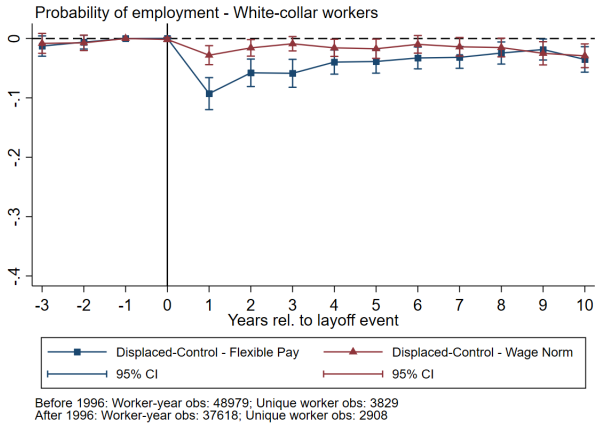
Figure A.2: The Effects of Job Loss on Employment and Days Worked: Blue-collar and White-collar Employees



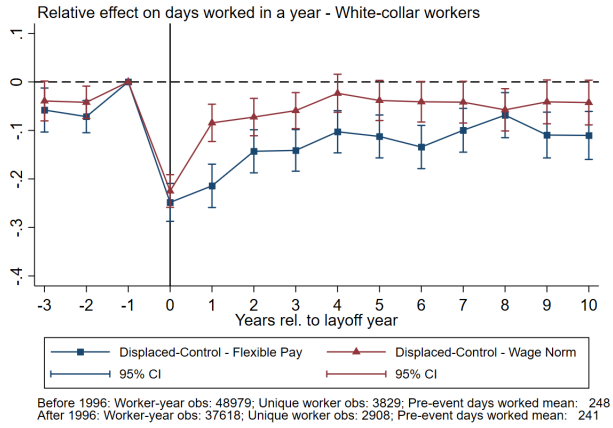
Panel (a)



Panel (b)



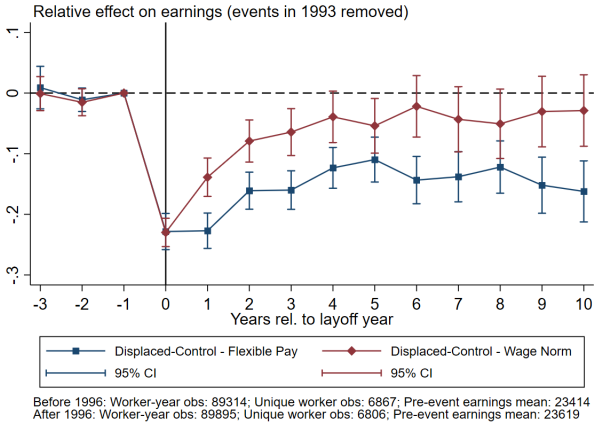
Panel (c)



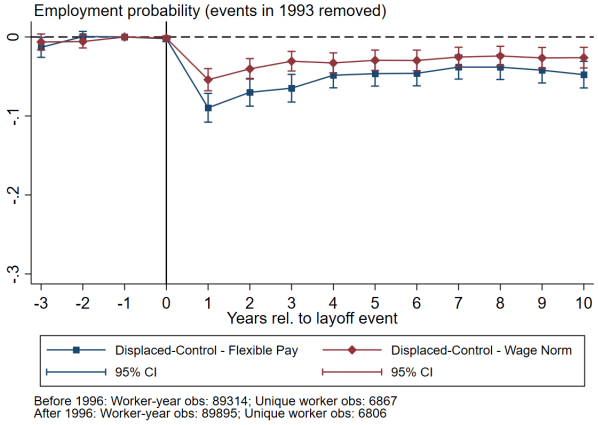
Panel (d)

Notes: This figure shows the effect of displacement on employment rates (left) and days worked (right) for workers who are employed in blue-collar jobs (top panels) and white-collar jobs (bottom panels) at the reference date. 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.

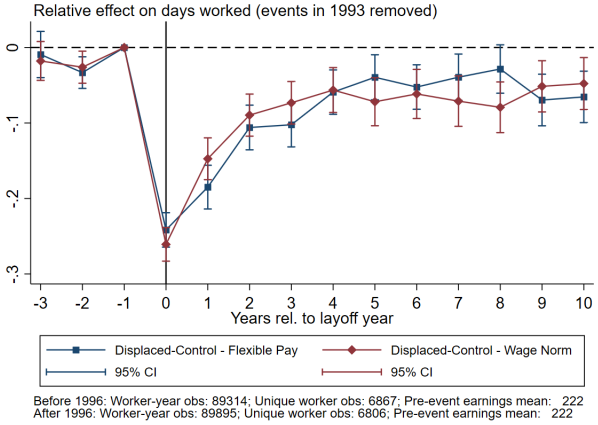
Figure A.3: The Effect of Job Loss on Earnings - Excluding Displacements during 1993 Recession



Panel (a)



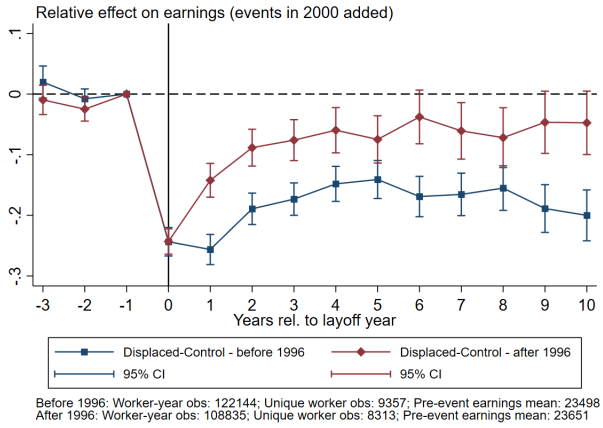
Panel (b)



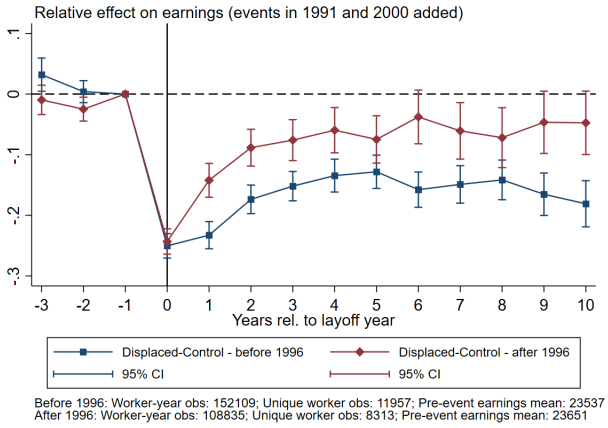
Panel (c)

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.

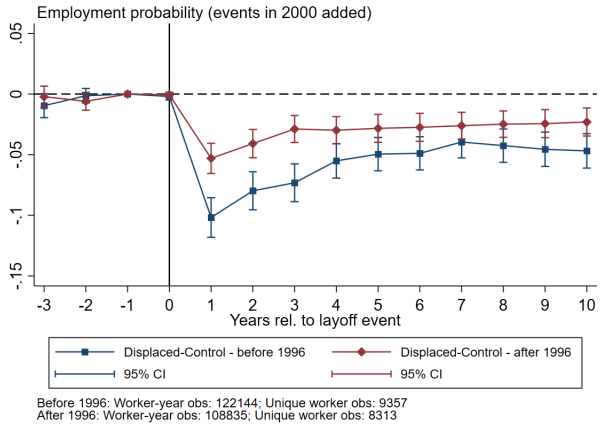
Figure A.4: The Effect of Job Loss on Earnings - Changing Event Years Considered



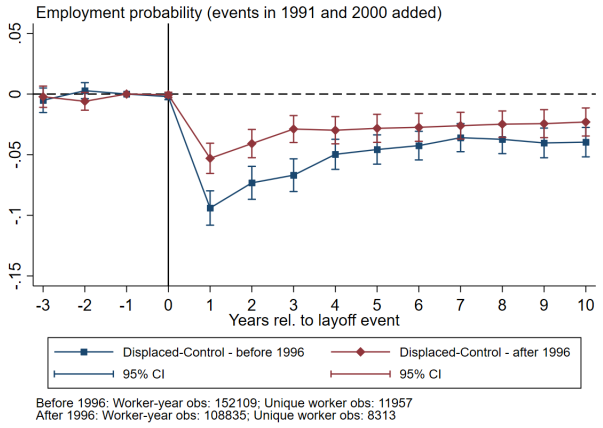
Panel (a)



Panel (b)



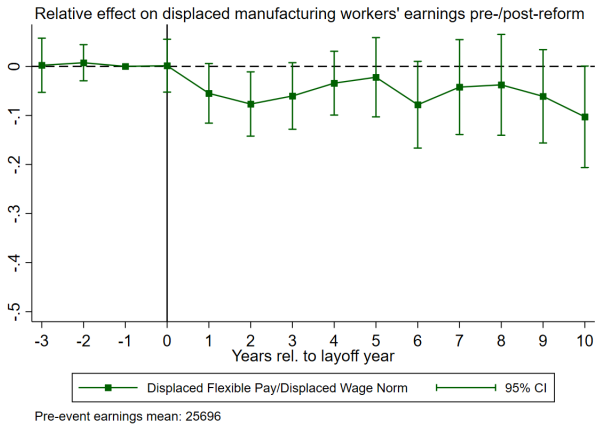
Panel (c)



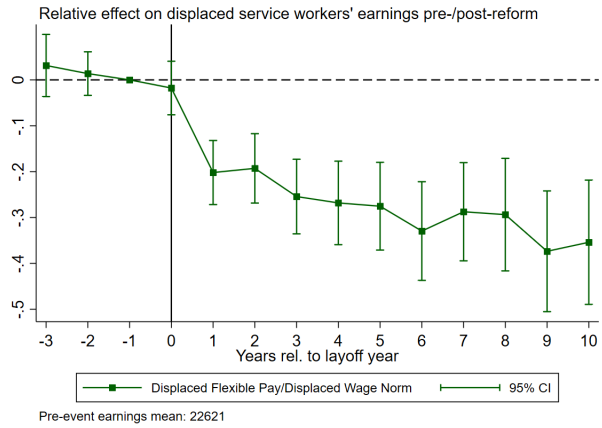
Panel (d)

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.

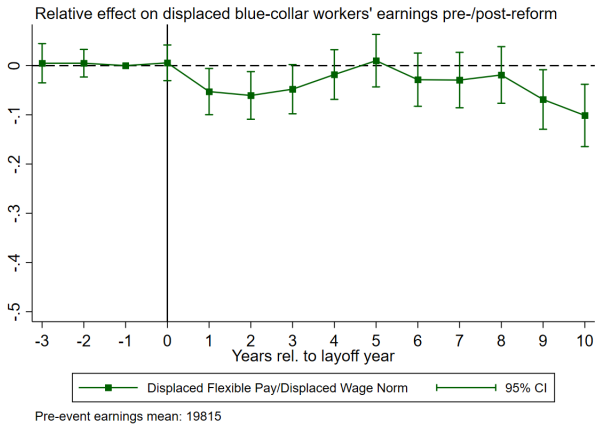
Figure A.5: The Effect of job loss on earnings among displaced workers by sector and type of job



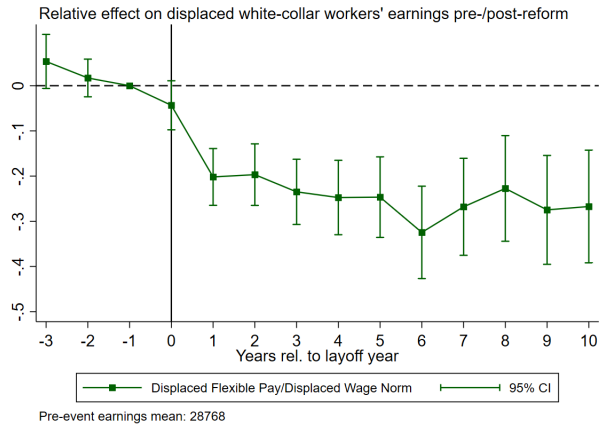
Panel (a): Manufacturing



Panel (b): Service



Panel (c): Blue-collar



Panel (d): White-collar

Notes: This figure shows estimates of δ_k^R in Equation 3, which 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. The figure shows the effect of the reform among displaced workers in the manufacturing sector (Panel (a)), in the service sector (Panel (b)), in blue-collar jobs (Panel (c)), and white-collar jobs (Panel (d)).