Job Loss and Job Prospects:
Estimating the Impact of Displacement on Job Security

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Abstract

Existing research on the cost of job loss has typically overlooked the role of job sorting after displacement in the perpetuation of recursive unemployment. This study aims to estimate the decline in job security associated with the type of jobs matched by displaced employees following their dismissals. Using a dataset containing the employment histories of about four million individuals working in Italy that allows for disentangling voluntary from involuntary job moves, I construct two indicators of job security attached to each specific type of job: one that captures the risk of dismissal and the other that conveys a measure of expected tenure. Then, employing an identification strategy that exploits collective dismissals as exogenous variations and a difference-in-differences methodology that uses not-yet-treated units as a control group, I estimate the impact of job loss on the expected job security of subsequent job types. I find that displacement generates an increased risk of dismissal intrinsic to the post-displacement type of jobs of about 2.38 percentage points and a lower expected tenure of around 156 days.

1 Introduction

The notion that unemployment can have long-lasting repercussions on workers' careers has been around for several decades. Since Heckman and Borjas (1980), who first delineated the methodological challenges involved in the estimation of the impact of job loss on workers' labor market outcomes, an extensive body of literature has emerged to ascertain the presence and evaluate the extent of displacement's effects on earnings (Jacobson et al., 1993), wages (Ruhm, 1991; Arulampalam, 2001), and prospective unemployment (Gregg, 2001). However, one aspect that still remains to be explored is the role that the search and matching process following displacement may play in generating the cycles of recurring unemployment observed in dismissed workers' careers.

This study departs from the intuition that there might exist an underlying connection between displaced workers' increased exposure to recurring unemployment and the type of jobs they find after displacement. In particular, displacement is a disruptive event that may confine workers into a labor market with limited opportunities, where they can only compete for lower-quality occupations. This can happen, for instance, because more attractive job positions are usually filled by stronger candidates, thus being de facto unattainable for weaker candidates' profiles, such as those of dismissed workers. If the set of accessible job positions to dismissed workers includes only types of jobs that are generally less desirable, these occupations, besides offering a lower wage, may also be characterized by reduced job security. In this case, sorting into these occupations may constitute, in and of itself, a fundamental driver for the propagation of the adverse effects of displacement.

The proposed analysis tests, therefore, the hypothesis that displacement may lead to a persistent decline in the job security associated with the type of jobs found by dismissed workers after being displaced. To test this hypothesis, I use a dataset containing the employment histories of about four million individuals working in Italy. An essential feature of this dataset is the inclusion of information on the reason for each job termination and on the start and end dates of each employment spell, which allows for disentangling voluntary from involuntary job moves and precisely determining the duration of each job tenure. This information was crucial to the development of two indicators of job security that I link to each type of job (defined by the combination of the sector of work and the professional qualification of each occupation) in the sample: one conveying the risk of unemployment and the other the expected tenure. These indicators should credibly reflect the intrinsic job security attached to different job types. There are several reasons why some types of jobs may entail a higher latent risk of dismissal. First, unions may have a stronger presence in certain sectors and occupations and may be more effective in protecting tenured workers rather than new hires, making entry-level positions more vulnerable to the risk of dismissal. Furthermore, entry-level job positions may also be subject to less stringent employment protection laws. Second, some occupations may be more at risk of being replaced by new technological advancements or being offshored in countries where the cost of labor is relatively lower. Third, occupations in some sectors, such as manufacturing or hospitality, may be more susceptible to business cycle fluctuations. Finally, there are sectors that, by their own

nature, entail a higher share of seasonal or temporary workers. The measures of job security that I constructed should be able to capture this heterogeneity among job types, which is the focus of this study.

I estimate the effects of displacement on job security with a difference-in-differences analysis that leverages collective dismissals as exogenous variations and the staggered treatment timing at which they occur to define treatment and control groups. The treatment group then consists of workers who have already experienced a collective dismissal, while the control group includes those workers who will experience one at a later stage. The motivation behind choosing a control group of later-treated individuals over one composed of untreated units relies on the assumption that workers who undergo a collective dismissal, albeit at different points in time, are more likely to share similar unobservable characteristics, making them more suitable control units.[1] To the best of my knowledge, this approach to the estimation of the costs of job loss is a novelty in the literature. Moreover, using collective dismissals as instruments for unemployment occurrence represents an improvement over previous research designs that exploit mass layoffs as exogenous variations. While collective dismissals preserve the nature of being firms' workforce readjustments driven by economic or organizational reasons (and are thus independent of dismissed workers' inner abilities), they are shocks that are generally smaller in size compared to those generated by mass layoffs.[2] This is a valuable feature, as the potential concern that these shocks may have substantial spillover effects in local labor markets, as argued in regards to mass layoffs in Cederlöf (2021), is considerably mitigated.

The main findings indicate that displacement causes a substantial drop in the job security of the subsequent types of jobs found by displaced workers after dismissal. In particular, these types of jobs feature a significantly higher inherent risk of unemployment of approximately 2.38 percentage points and a reduced expected tenure of about 156 days. These effects seem to be relatively constant and persistent over time.

Moreover, when delving into the heterogeneity of these findings across different subgroups, such as gender, level of education, and geographical region of employment (north, center, and south of Italy), I find estimates that suggest a larger decline in job security for women and individuals working in the northern regions of Italy. Estimates for workers with different levels of educational attainment, instead, do not delineate a clear-cut picture, although the negative effects of displacement do seem more persistent for lower-educated individuals. However, it must be remarked that wide confidence intervals do not allow for claiming statistically significant gaps among any of these subgroups.

More in detail, examining average post-treatment effects (i.e., the mean value of all post-treatment

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1 Nonetheless, I have simultaneously conducted the analysis using a more standard identification that makes use of never-treated individuals as comparison units. In this case, I selected the workers in the control group through a propensity score matching procedure that exploits available workers' observable characteristics as predictors of their likelihood of being part of the treatment group. I use these results as a benchmark for those of the main specification.

2 An extensive discussion of collective dismissal procedures is carried out in Section 2.1. In short, these procedures apply to companies with more than 15 employees who intend to dismiss at least five employees. This implies, for instance, that if a company of 50 employees wants to dismiss 10% of its workforce (i.e., five employees), this would qualify as a collective dismissal. On the other hand, the literature usually defines mass layoffs as a 30% reduction in the workforce of a firm within a year. Therefore, collective dismissal may entail firm-level shocks that are generally smaller in size with respect to mass layoffs.

periods estimates), I find that men experience a 2.13 pp increase in unemployment risk, while women seem to endure more severe consequences of displacement, with an increase of 2.73 pp. The decline in expected tenure is in line with these results, with men and women facing reductions of approximately 165 and 149 days, respectively. Furthermore, looking at the dynamics of these effects, the decline in job security seems to remain relatively constant over time for women, whereas men's job security appears to improve gradually. Differences between higher-educated (those holding at least a college degree) and lower-educated individuals seem instead to arise only in later periods and can only be spotted for the expected tenure indicator. On average, the expected tenure of lower-educated individuals decreases after displacement by about 162 days, while it falls more moderately for higher-educated individuals by about 128 days. This marked difference is almost entirely due to the recovery that higher-educated workers exhibit in later periods. Finally, individuals who were working in the north of Italy at the time of dismissal seem to be more affected by displacement than individuals from the other regions, reporting a rise in unemployment risk of about 2.55 pp and a reduction in expected tenure of about 176 days. Moreover, these effects seem also to be more persistent for employees in the north of Italy.

These findings are robust to a battery of sensitivity checks and placebo tests. First, I considered the possibility that a small number of outlier observations might be responsible for driving the results. To ensure this was not the case, I identified outliers (values in the first or last percentile of the distribution) for both outcome variables in the year preceding treatment. Then, after excluding all employment records associated with these extreme values from the sample, I re-estimated the model using the remaining subset of observations. Interestingly, average treatment effects are, in this case, even slightly stronger than those of the main specification. A second concern may arise from having a limited number of observations for some types of jobs in the sample, which could potentially compromise the accuracy of computed indicators of job security. To attenuate this concern, I left out from the analysis job types (and, thus, individuals employed therein) with fewer than one hundred observations available in any given year. The estimates derived using this restricted sample remarkably mirror the main results, being only mildly smaller in absolute value. Third, since I obtained the main estimates from a sample of workers aged between 30 and below 54 (which was motivated by the fact that individuals at the start or end of their careers might react substantially differently to displacement compared to prime-age workers), I extended the sample to include all individuals aged between 20 and 64. This extended sample yields estimates that are broadly in line with those of the main analysis for the expected tenure indicator and even larger in magnitude for the unemployment risk indicator. Lastly, to verify that a significant variation in the outcome variable happened in correspondence with the treatment timing, I arbitrarily reassigned the treatment to three years earlier relative to its actual occurrence. Estimating the model with this placebo treatment, I find that prior to the actual one, workers were gradually moving towards occupations with higher job security. Dismissal interrupts this favorable trajectory, repositioning displaced individuals on a less favorable job security path.

This study contributes to the literature on the costs of job loss by estimating the causal effects of displacement on the job security attached to the occupations found by dismissed workers following dismissal. While previous research has thoroughly assessed the negative consequences of unemployment on the evolution of workers' careers (Ruhm, 1991; Jacobson et al., 1993; Stevens, 1997; Arulampalam et al., 2000; Arulampalam, 2001; Gregg, 2001; Burgess et al., 2003; Gregg et al., 2004; Gangl, 2006; Eliason and Storrie, 2006; Raaum and Røed, 2006; Mroz and Savage, 2006; Von Wachter and Bender, 2006; Oreopoulos et al., 2012), this analysis focuses instead on the possible role of the search and matching process that follows dismissal in generating these observed negative outcomes. The central idea advanced in this study is that displacement may force displaced workers into a situation where they are bound to accept lower-quality occupations. Four possible mechanisms can help explain why this may unfold: i) human capital models suggest that periods of unemployment may erode workers' human capital, thereby reducing the marketability of their skills to potential employers (Mincer and Ofek, 1982; Pissarides, 1992); ii) unemployment may come along with a stigma (Vishwanath, 1989; Gibbons and Katz, 1991; Omori, 1997), which translates into a negative signal of lower labor productivity to potential employers; iii) negative effects may arise from untying strong employer-employee bonds (match effects), especially when workers' abilities and employers' skill requirements are strongly aligned; iv) the final mechanism relates to job ladder models, which predict that workers try to "climb" towards higher-paying and more secure jobs throughout their career (Burdett and Mortensen, 1998), but displacement may destroy their progress, pushing them into less-secure and lower-paid jobs (Pinheiro and Visschers, 2015; Jarosch, 2023).[3] Being dismissed, thus, entails falling down to the bottom of the job ladder, where rungs are more slippery, which makes it more likely for dismissed workers to experience repeated unemployment spells. While all these mechanisms may simultaneously be at play to determine the main results, the underlying idea of this study is closer in spirit to the one exposed in the latter mechanism. In other words, sorting into an occupation with an inherent heightened unemployment risk will increase the chances of experiencing further displacement episodes in the future, giving rise to a job-ladder-type chain of events. Furthermore, investigating the heterogeneity in job security at the occupational level is particularly interesting because, as shown in Pinheiro and Visschers (2015), unemployment scarring (i.e., the persistent negative effects of displacement on workers' careers) can occur even when heterogeneity is assumed to be present only on the employers' side (thus imposing homogeneity in workers' inner characteristics). This implies that frictional labor markets alone can explain why displaced workers may experience negative effects on their labor market outcomes even many years after a displacement

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3 The positive correlation between job security and wages has been extensively discussed in the literature on the lack of compensating differentials (Mayo and Murray, 1991; Winter-Ebmer, 2001; Bonhomme and Jolivet, 2009). Mayo and Murray (1991), for example, finds that workers sort into large or small firms according to their unobservable qualities. However, since smaller firms tend to offer unstable employment prospects and lower wages, they also tend to attract employees with unstable work histories (who, in the context analyzed by this study, would be displaced workers), which helps explain why low-job-security firms also offer lower salaries. In this paper, I do not measure job security at the firm level but rather at the sectoral and professional qualification level. Nevertheless, as shown in Haltiwanger et al. (2022), most of the wage (and thus, given the aforementioned correlation, most of the job security) dispersion occurs at the industrial level. Therefore, the job security indicators presented in this work should accurately convey this heterogeneity in job security levels among different occupations.

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episode.

This paper is structured as follows: Section 2 will describe the data, Section 3 will outline the empirical strategy, the results will be presented in Section 4, followed by robustness checks and placebos in Section 5, and, finally, Section 6 will be dedicated to some concluding remarks.

2 Data

This study relies on a sample extracted from the database of the "Comunicazioni Obbligatorie," which are mandatory notifications that Italian employers (or their intermediaries) are required to submit to the competent regional or national authority regarding important workforce variations (such as hirings, firings, or conversions to a different type of contract).[4] The sample accounts for around 22 million observations, which are obtained from a random selection of 4 million individuals employed within the Italian labor market, both in the private and public sectors, from 2010 to 2022.[5] The data contains the complete employment histories of the selected individuals throughout the considered time frame. In its original structure, the dataset reports in each row information on one single employment spell, which includes the starting and ending dates, the contract type (temporary or permanent, full-time or part-time), the professional qualification, the sector of employment, and notably, the reason for job termination, which will key to the computation of the outcome variables. The dataset also includes several time-invariant individual characteristics such as gender, year and region of birth, and education.[6]

To prepare it for the analysis, I restructured the dataset into a yearly panel format, where every year reports each individual's main occupation.[7] Next, I extracted the sample of interest, which consists of individuals who experienced only one collective dismissal within the specified period. The motivation for this choice was to refrain from complications in the interpretation of the estimates that are obtained from units that may have been treated multiple times.[8] Additionally, I excluded workers who started a new job before the official termination of their contract with the firm involved in the collective dismissal, those who were dismissed in an occupation that was not identified as their main one, as well as those who returned to the same firm that had dismissed them. This last cut was done to avoid the possibility of leaving in the sample cases of workers' reinstatements when the collective dismissal is later ruled illegitimate by a court. To further homogenize the sample, I restricted it to

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4   More specifically, the name of the sample is "Campione Integrato delle Comunicazioni Obbligatorie," in short, CICO. The sample was made available by the Italian Ministry of Labor and Social Policies.

5   The random selection is performed by extracting every individual born on the 1st, the 9th, the 10th, and the 11th day of any month and any year.

6   This is a non-trivial caveat, as it is not possible to know whether individuals upgraded their education once they started working. The reported level of education in the dataset refers to the one at the time of the last registered employment spell.

7   Similarly to Card et al. (2013), the sample was restricted to keep a single dominant record for any given year. However, as the focus of this paper is not earnings, the main occupation was defined by selecting instead the longest employment spell in every year. To do so, I sequentially kept the employment record that displayed: 1. the highest number of employment days within the year; 2. the longest spell overall (spanning throughout the whole sampled period); 3. full-time occupations were preferred to part-time ones; 4. highest income. If two or more employment spells were found to be equal in every one of these dimensions, I picked one at random.

8   However, including individuals who were treated more than once does not change the results, see Figure B12, which may be due to the relatively low number of individuals who experienced a collective dismissal more than once in the sampled period (only 1.7% of the total).

individuals who were at least 30 years old in 2010 and not older than 54 in 2022. The reason for this choice is that young and senior workers may react very differently to displacement with respect to prime-age workers, as young workers usually switch jobs very frequently at the beginning of their careers, while senior workers may, instead, react to displacement by anticipating their retirement.[9] Finally, to extract a balanced dataset, I removed individuals who reported gap years (i.e., years with no work spells) in the considered sampled period, which also eliminates the long-term unemployed (in this case, those staying at least one full year out of work) from the analysis. The main rationale for excluding these types of workers is twofold. First, individuals who stay unemployed for a long period of time may have latent unobservable characteristics that deeply differentiate them from the rest, and thus bring into the analysis major confounders.[10] Second, the alternative approach to this one would have entailed either forgoing the possibility of working with a balanced panel (complicating the interpretation of the results) or completely changing the methodological strategy.[11] After all these adjustments, and after excluding individuals who experienced the collective dismissal in the first three years of the sample to allow for at least three years of pre-treatment, the final sample accounts for 8,256 treated individuals, observed over 13 years, which corresponds to a total of 107,328 observations.[12] Summary statistics pertaining to this sample are shown in Appendix A from Table A3 through Table A7. Most of the workers in the sample are employed in northern Italy ($\approx 58.4\%$ of the total), have a high school diploma ($\approx 52\%$), are male ($\approx 67\%$), and, on average, are about 42 years old.

The size of all treatment cohorts is sufficiently large, although earlier cohorts, from 2013 to 2017, are considerably larger than later ones. The distribution of individuals across treatment cohorts is reported in Table A1 in Appendix A. The main specification will only make use of this group of units, exploiting later-treated units to build control groups that vary accordingly for each cohort and calendar time. Nevertheless, the analysis can likewise be performed by employing a group of never-treated individuals as control units. In this case, untreated units must be selected such that, according to a series of observable characteristics, they constitute suitable comparison units to extrapolate untreated potential outcomes. To this purpose, I made use of a propensity score matching procedure

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9 For example, Farber et al. (2019) find that younger and older applicants have a lower callback rate than prime-aged applicants. Furthermore, also in Bertheau et al. (2023) only workers who are at most 50 years old in the year preceding displacement are retained to limit the influence of early-retirement programs. Differently from this approach, however, I preferred to further homogenize the age composition of the sample, given that later-treated units are used in this work to construct counterfactual potential outcomes and thus might be generally slightly older than already-treated units. In Bertheau et al. (2023), instead, the authors employ a more standard identification strategy that exploits never-treated units selected through propensity score matching (e.g., as in Schmieder et al., 2023) as controls. In order to benchmark the main results in this paper I also replicated the analysis following this alternative and more standard approach. I present the results in Appendix B.

10 Potential employers attach a stigma to contemporary unemployment spells lasting at least nine months Eriksson and Rooth (2014), which negatively conditions their hiring decisions of long-term unemployed individuals.

11 In addition, other papers in the literature, such as Ruhm (1991), have followed this approach of considering only "individuals with fairly strong attachments to the labor force".

12 As opposed to what is common practice in the literature on the cost of displacement (e.g., see Jacobson et al., 1993), the sample was not restricted to include only individuals with at least three years of tenure in the firing firm at the time of dismissal. This choice was motivated by the fact that outcome variables are time-invariant indicators of job security and, thus, only vary when workers change occupations. Hence, keeping only individuals who have not changed firm over the three years preceding the collective dismissal would have implied ending up with a complete absence of variation in the outcome variable for those periods, which would have made these pre-treatment coefficients impossible to compute.

that employed the values in the pre-treatment year of the following variables: age, education, gender, macro-region of work (north, center, and south and islands), sector,[13] and temporary and part-time work dummies. I employ this set of variables to attach a score to each untreated unit in the dataset that conveys its probability of belonging to the treatment group instead. The resulting disjoint set of untreated units is then attached to the set of treated units described above. After the exclusion of individuals treated before 2013 or after 2019 (which was done to minimize compositional changes that can confound the interpretation of the estimates), the resulting sample totaled 413,140 observations and 31,780 individuals, of which 7,449 were the treated ones.[14] The samples of treated and untreated individuals exhibit fairly similar observable characteristics in terms of age, education, share of females, and temporary workers. However, the control group exhibits a higher share of part-time workers (see Table A2 in Appendix A to check the exact numbers for these descriptive statistics of the two samples).

In the following sections of this paper, I will mainly focus on estimates obtained with the sample including only treated units, which will be the preferred specification. The motivation for this choice will be outlined in the next section. Nonetheless, as identification strategies that employ never-treated units have been the standard until very recently, the charts and table showing the results pertaining to the specifications that make use of the untreated units' sample will be contextually reported in Appendix B, serving the purpose of being a benchmark for the main results.

2.1 The Italian institutional setting of collective dismissals

The collective dismissal (*licenziamento collettivo*) procedure in Italy is the process through which a company can dismiss a substantial number of employees because of economic, organizational, or structural reasons. In the dataset that I employ for this analysis, 0.62% of all job separations are filed as collective dismissals.

Collective dismissal procedures are regulated by the Law 223/1991, which was further refined by the legislative decree n.151 of the 26th of May 1997, with the intention to homogenize the legislation on the subject across the European Union's member countries. They apply to companies with more than 15 employees who intend to dismiss at least five employees within 120 days and involve several steps. First, the company must send a written notification to the trade unions and the relevant labor authorities, providing detailed information about the reasons for the dismissals, the number and roles of employees affected, and the previewed timeline. Then, within seven days, trade unions may propose to re-examine the reasons for the collective dismissal and discuss possible alternative solutions with the company, such as temporary layoffs, retraining, or reduced working hours. This consultation phase lasts a maximum of 45 days (or about 22 days in the case the procedure involves less than ten employees). If no agreement is reached by the deadline, administrative authorities may initiate a second consultation phase, which must end within 30 days (15 in case the procedure

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13 I used alphabetical NACE codes to categorize economic sectors.

14 The sample of treated units shrinks because, for these regressions, I also excluded individuals treated from 2020 onwards to minimize compositional changes in the treatment group.

involves less than ten employees). Once both consultation phases are over, the company can proceed to dismiss the workers that it deems to be redundant. To do so, the company must apply criteria that had been previously determined with union agreements. Typically, these criteria include the length of service (newcomers shall leave the company first, similar to the "last-in-first-out" rule present in the labor market legislation of other European countries), the presence of family responsibilities, and the company's operational needs. Finally, dismissed employees are entitled to severance pay and may qualify for unemployment benefits. Furthermore, the law provides that firms that hire workers who were previously displaced in the course of a collective dismissal are entitled to substantial tax breaks.

One important caveat is that, while younger workers may have fewer family responsibilities and shorter tenure, which are factors that might increase their likelihood of being subject to collective dismissals, the "pensionability" criterion is among the most commonly used in such procedures. This consideration partly explains why I chose to exclude both younger and older workers from the analyzed sample, as discussed in the data section.

2.2 Measuring occupations' job security

Taking advantage of the richness of the original dataset, I developed two indicators to gauge the level of job security associated with each occupation in the sample: one assessing the risk of unemployment and the other measuring expected tenure. These indicators were first calculated for 2131 sectors (referring to 6-digit ATECO codes) and 627 professional qualification separately.[15] Subsequently, I averaged these two values to derive a single measure of job security for each sector-specific professional qualification. For the sake of simplicity, I will refer to the combination of these sector-specific professional qualifications as "occupations" for the remainder of this study. This process yields an indicator of job security for 9445 distinct occupations present in the final dataset of treated units.

To construct the two job security measures, I followed slightly different procedures. For the unemployment risk indicator, I exploited a crucial piece of information in the dataset, which is the reason behind each job termination. Then, looking at the annual survival rates of individual jobs, I categorized each termination event, assigning a value of one to involuntary separations and a value of zero to voluntary job transitions or ongoing employment spells.[16] Subsequently, I computed the share of involuntary separations for each professional qualification and sector separately, which I then averaged to finally get a time-invariant measure that captures the risk of unemployment attached to any

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15 The total number of 6-digit ATECO codes is 2150, while there are 629 different professional qualification. However, the original dataset, from which I extrapolate the indicators, only accounts for the number of professional qualification and sectors mentioned above.

16 To conduct this operation, I had to discard employment spells that reported a reason for their termination that leaves unclear the categorization into voluntary and involuntary terminations. This is the case, for example, of jobs that ended due to firm closures. In this latter case, it was not possible to ascertain the root causes of the closure. The firm could have been actually shut down, with all its employees being dismissed, or it could have been acquired by another company, in which case its employees would have simply changed employers while retaining their jobs. Eliminating such cases discards about 4% of all the employment spells in the sample.

given occupation. This process is summarized by the formula below:

$$y = \frac{\sum L_{itj}}{N_j} + \frac{\sum L_{itk}}{N_k}$$

where y is the unemployment risk indicator pertaining to each occupation, defined as the combination of sector j and position of employment k of individual i, and L is a dummy variable that takes the value zero for voluntary job moves and ongoing work spells within the considered year t or the value of one when the worker is subject to a layoff.

I constructed the second indicator, which conveys a measure of expected tenure, in a similar fashion. However, contrary to the previous case, there was no need to consider the evolution of the years of individual jobs' tenure, but it was enough to compute their length by exploiting the information on hiring and separation dates included in the data. To do so, however, I still needed to retain the information on the separation reason for each job, as I needed to exclude those employment spells that ended with voluntary job moves.[17] Once I got these metrics for each professional qualification and sector separately, I averaged these employment durations to get a single measure for the expected tenure attached to each occupation, as illustrated before for the other indicator. The following formula to compute this second indicator, in fact, resembles very closely the one above:

$$y = \frac{\sum d_{ij}}{N_j} + \frac{\sum d_{ik}}{N_k}$$

where y is now the expected tenure indicator pertaining to each occupation, while d is the average number of days spent working in that occupation by individuals within the considered time framework.[18] Both indicators convey measures of the degree of job security associated with each occupation. To facilitate the comprehension of these measures, I give some examples of occupations characterized by high or low job security. As examples of high job security occupations, I find professionals who work in the banking sector and engineers/designers who work in the automotive or aircraft production sector. As examples of low job security occupations, instead, we find day laborers who work in the farming sector and professionals who work in the cinematographic industry or theaters.

Finally, it is important to remark that both measures of job security are time-invariant and convey the level of job security for each occupation for the whole considered time span. The distribution of the values of the two outcome variables is shown in Figure 1, while Table 1 shows instead mean values of both outcome variables before and after the treatment's occurrence.

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17 Or, with an unclear categorization, as explained in the previous footnote.
18 The original dataset encompasses employees who were holding their jobs from years before the first sampling year. These working spells were included in the computation of both indicators.

Figure 1: Distribution of the outcome variables' values in the sample of treated units

(a) Unemployment risk
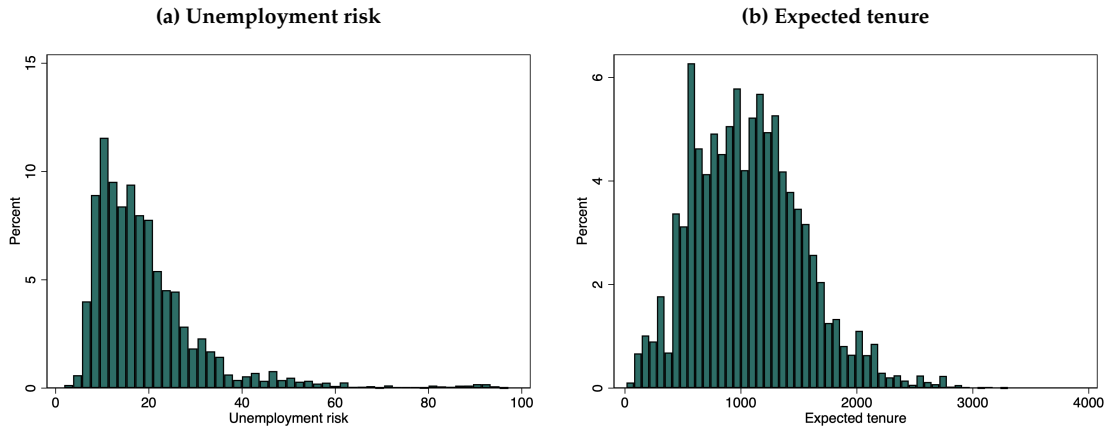
(b) Expected tenure

Table 1: Mean value of the outcome variables before and after the treatment's occurrence

	Unemployment risk	Expected tenure
Before treatment	18.41	1163.28
	(11.19)	(496.30)
After treatment	20.47	997.94
	(13.57)	(475.33)

Notes: Standard errors in parentheses.

3 Identification and Empirical Strategy

This analysis aims to assess the impact of displacement on the job security of the occupations obtained by dismissed individuals following dismissal. This task poses substantial methodological challenges, as dismissals may be correlated with unobservable workers' characteristics that may influence their future job prospects. Partialling out these features is, therefore, the fundamental obstacle that needs to be overcome to get causal estimates of the effects of displacement on workers' labor market outcomes. To tackle this issue, researchers have typically resorted to firm-level exogenous shocks, such as firm closures or mass layoffs, as instruments for unemployment occurrence. The rationale is that these shocks are considered exogenous to workers' characteristics as, in these contexts, firms do not select the pool of workers to be dismissed based on their job performance but rather according to different considerations. Therefore, in the context of mass layoffs and firm closures, all types of workers may face displacement, regardless of their individual attributes.[19] However, Cederlöf (2021) critiques the use of firm closures and mass layoffs, arguing that these shocks may generate significant spillover effects at the local, sectoral, or industrial level, which can thereby exacerbate the negative effects of displacement. This concern is clearly more relevant when the size of the mass layoff represents

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[19] One of the earliest published articles in the literature adopting this approach was Jacobson et al. (1993), which was followed by a long list of works that used the same, or somewhat similar, methods.

a substantial proportion of the local labor market. Studies that employ mass layoffs as exogenous variations usually define them as a reduction of at least 30% of a firm's workforce within a given year. Nevertheless, these works are often based on data that do not contain information on the reason for employment termination, and thus, they might need to resort to large firm downsizes to correctly discriminate between job separations due to economic reasons and individual layoffs.[20] This study, instead, uses a dataset that does contain this information and thus allows for disentangling between these two, which constitutes a clear advantage with respect to previous research.

The following analysis employs a difference-in-differences event study, where control units consist of individuals who are yet to be displaced as part of a collective dismissal. The motivation for choosing not-yet-treated units as a control group lies in the idea that workers who undergo a collective dismissal, even though at different points in time, are more likely to share more similar unobservable traits with each other rather than with those who were never treated. Hence, estimates obtained using not-yet-treated units to extract counterfactual potential outcomes can be presumed to be less vulnerable to unobservable time-varying factors that may bias the estimates, as these confounders would be implicitly netted out. In the upcoming section, I will therefore only present results obtained from regressions that exploit not-yet-treated units as a comparison group, but to benchmark these estimates, I contextually carried out the analysis using never-treated individuals as control units, which were selected through a propensity score matching procedure making use of observed workers' characteristics. These results will be reported in Appendix B.

The chosen methodology to retrieve the event-study estimates is the estimator proposed by Callaway and Sant'Anna (2021) (henceforth CS). This methodology is particularly fitting for this empirical framework for a couple of reasons. First, it is robust to heterogeneous treatment effects, which is crucial in this setting given that workers in the sample are employed in very diverse occupations and face dismissals at different points in time, therefore encountering different contingent labor market conditions. Second, this estimator maximizes the use of not-yet-treated units as part of the control group. This is a distinctive advantage over alternative estimators designed to deal with heterogeneous treatment effects that have been devised in recent years. For instance, Sun and Abraham (2021) only exploits the last-treated cohort as a control group, while the CS estimator employs every cohort that is yet to be treated, leveraging the "no anticipation" assumption. This feature of this estimator is particularly valuable given that the dataset includes fewer treated units in later periods.

It needs to be remarked at this point that the CS estimator employs covariates to generate a propensity score that matches units based on a defined set of time-invariant observable characteristics fixed at their pre-treatment year values. The parallel trends assumption required for identification is as follows: conditional on a set of observable characteristics, treated and control units followed similar job security trajectories prior to dismissal. In the counterfactual scenario where displacement would not have occurred, these trajectories would have remained unchanged.

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[20] This is, for instance, explicitly outlined in Bertheau et al. (2023), where to correctly specify the treatment group they must impose a 30% annual drop in a given firm's workforce in order to avoid mischaracterizing voluntary separations as layoffs.

Given this setup, the main specification is the following[21]:

$$y_{itg} = \sum_{e, e \neq -1} \beta_e \times D_i \times \mathbb{1}(t = g + e) + \pi_{e=-1} X_i + \gamma + \varepsilon_{itg} \qquad (1)$$

Where y is the unemployment risk indicator or the expected tenure in a given occupation, faced by individual i of cohort g (the cohort is defined by the treatment year) in year $t \in \{2010, ..., 2022\}$, D is a dummy equal to 1 in the year of the collective dismissal, e is event time (with $e = 0$ corresponding to the year of treatment), X are pre-treatment ($e = -1$) time-invariant covariates (full-time and temporary employment status, macro-region of work, age, gender and education), γ is a comprehensive term that includes id, year, and cohort fixed-effects, including all of their interactions. Standard errors are clustered at the individual level and the chosen estimation method is the doubly robust difference-in-differences estimator based on stabilized inverse probability weighting and ordinary least squares as in Sant'Anna and Zhao (2020). The parameter of interest is β, and it can be interpreted as the effect of displacement on the subsequent job security (either in terms of unemployment risk or expected tenure) attached to occupations found by dismissed workers following dismissal.

Furthermore, I also performed this analysis using a stacking by event specification as in Deshpande and Li (2019), or Cengiz et al. (2019). Results are reported in Appendix A.

4 Results

Figure 2 presents the results from estimating equation (1) for the first of the two outcome variables: unemployment risk. The chart displays the expected variation in the unemployment risk of treated units compared to the control group's variation before and after the treatment's occurrence. More precisely, post-treatment estimates illustrate the average treatment effect on the treated, which is the dismissal's impact on the expected unemployment risk associated with the subsequent job matches of dismissed individuals. All pre-treatment coefficients are close to zero and not significant, which suggests that the parallel trend assumption holds. Post-treatment estimates are, instead, all positive, significant, and pretty stable over time, ranging between 2.58 pp in the first period after the treatment and 2.09 pp in the last period.

Note that the chart does not show the value corresponding to $e = 8$, which is also discarded from every one of the following regression results. The reason for not showing this estimate is that its value is obtained by exploiting only one treated cohort, that of individuals treated in 2013, and one not-yet-treated cohort as a control group, that of those treated in 2022. However, as reported in Table A1 in Appendix A, the 2013 cohort accounts for 1165 individuals, while the cohort of 2022 counts only 236 individuals. This implies that there is about one control unit for every five treated ones. Given the limited size of the last-treated cohort and the relative imprecision in the computation of the estimate, I preferred to leave it out of the chart below. An analogous motivation applies to two pre-

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21 Notice that, since I am using the methodology by Callaway and Sant'Anna (2021), regression 1 has only an illustrative purpose.

treatment coefficients that have also been discarded from the chart, which are those corresponding to the periods $e = -10$ and $e = -11$.

Estimates obtained using never-treated individuals as control units display similar results. However, in this case, the coefficients' magnitude is slightly inferior to that of the estimates shown in Figure 2, and point estimates also exhibit a mild recovery over time. Furthermore, the pre-treatment estimates' average, although close to zero (0.123), is jointly positive and significant, even though this finding is mainly driven by coefficients that are farther away from the treatment period. Nevertheless, analyzing these results, all in all, they seem to hint that not-yet-treated units constitute, indeed, a better control group, as claimed in the previous section. These results are reported in Figure B3 of Appendix B.
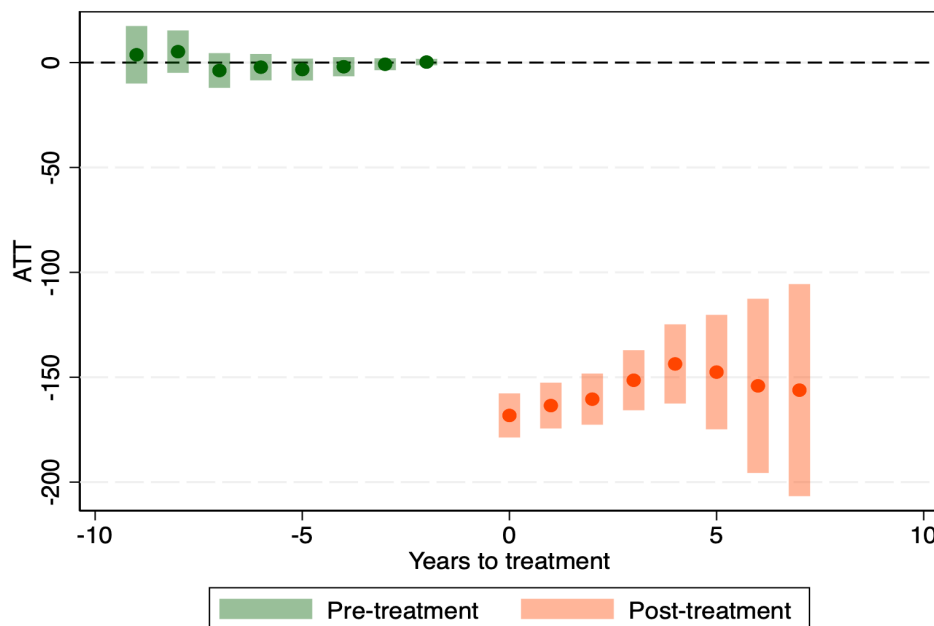
Figure 2: Unemployment risk
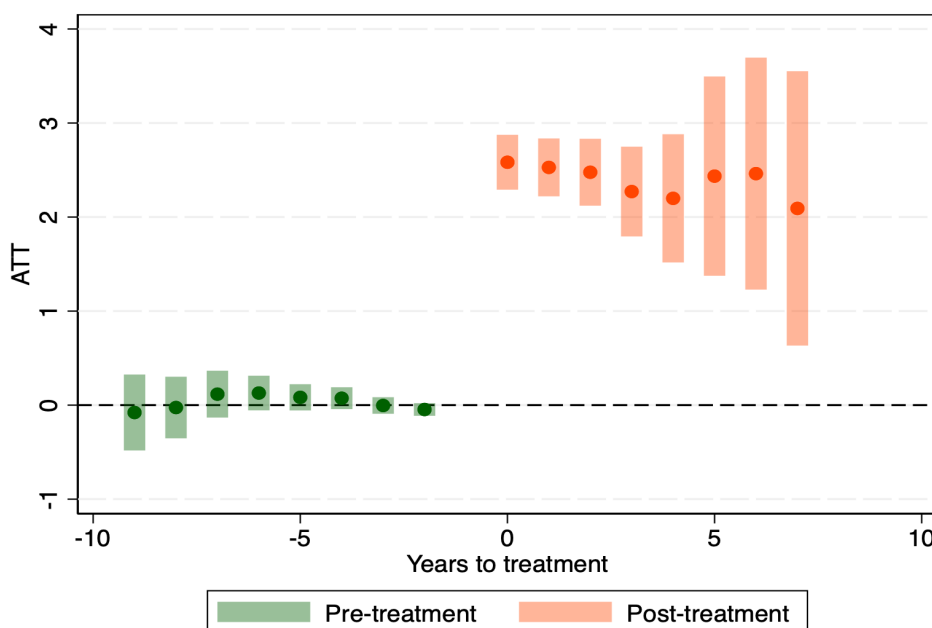
Note: The chart displays estimates of the average treatment effect of displacement on the unemployment risk associated with the main job occupations in which "treated" individuals are employed each year. Estimates are obtained using not-yet-treated units as a control group. Confidence intervals, represented by the extent of the bars, are computed using a 95% confidence level.

The interpretation of the estimates shown in Figure 3 is identical to the one of those portrayed in Figure 2. However, this time, the outcome variable of interest is the expected tenure. Again, all pre-treatment coefficients are not significant and close to zero, suggesting that the parallel trends assumption holds. Post-treatment estimates are, instead, negative, significant, and relatively steady over time, ranging from a maximum of 168 days of reduced expected tenure in the year right after the dismissal and a minimum of 144 days five years from the dismissal.

Estimates obtained using never-treated units as a control group are shown in Figure B4 in Appendix B. Similarly to the estimates obtained for the unemployment risk indicator, the magnitude of

the coefficients is inferior with respect to estimates in Figure 3. This is due mainly to the progressive recovery over time that treated individuals seem to experience when never-treated units are used to derive counterfactual potential outcomes, which is instead less pronounced when estimates are obtained by using not-yet-treated units as controls. In addition, as before, the pre-treatment period average is significant (even though relatively closer to zero with respect to post-treatment estimates, being approximately 8.5) and has the same sign as the post-treatment coefficients. Once again, I take this as a clue that not-yet-treated units are a better fit as a control group.

Figure 3: Expected tenure

Note: The chart displays estimates of the average treatment effect of displacement on the expected tenure associated with the main job occupations in which "treated" individuals are employed each year. Estimates are obtained using not-yet-treated units as a control group. Confidence intervals, represented by the extent of the bars, are computed using a 95% confidence level.

Finally, the analysis has been replicated by means of stacking by event specification and using not-yet-treated units as comparison ones. As mentioned in the previous section, no covariates were included in the model in this case. The findings are very close to those presented above and are reported in Table A9 of Appendix A.

Overall, both indicators clearly suggest that dismissal entails a substantial drop in the job security attached to the subsequent job found by dismissed individuals.

Dissecting cohort-specific results, I notice that the impact of dismissal is wildly heterogeneous across cohorts. The 2019 cohort is the most impacted by dismissal, with post-treatment averages of 4.23 and minus 260 days for the unemployment risk and the expected tenure indicators, respectively. The 2017 cohort seems to be instead the less severely impacted one, with post-treatment averages

equal to 1.11 and minus 130 days.[22]

Finally, results obtained with unconditional specifications can be found in Figures B1 and B2 of Appendix B. The main takeaway from these results is that adding covariates seems to matter for retrieving a credible conditional parallel trends assumption only in one case, which is the one relative to the specification of the expected tenure indicator that uses never-treated units as controls.

4.1 Heterogeneity of results

The findings in the previous section document a significant and persistent impact on the job security associated with the occupations joined by dismissed workers after being laid off. Next, one might wonder whether these negative effects on job security are homogeneous across various subgroups, such as gender, education, and region of work, or whether they vary across some of these dimensions.[23] Therefore, in what follows, I will examine the impact of job loss on all of these subgroups separately to investigate the presence of heterogeneous treatment effects.

First, is there a gender differential in the impact of job loss on the subsequent occupation's job security? Taking stock of the evidence disseminated in the literature, the answer to this question does not appear to be straightforward. In fact, on the one hand, studies have shown that women generally tend to sort into occupations characterized by lower wage premiums (Card et al., 2016) but reduced risk of dismissal (Wilkins and Wooden, 2013). On the other hand, some recent analyses reveal that job loss causes women to endure more substantial and persisting reductions in earnings (Illing et al., 2021). Putting these findings together, two hypotheses may explain the observed gender gap in the cost of job loss: first, vis-à-vis their male counterparts, women may end up taking lower-paid jobs with limited wage growth prospects (e.g., part-time work) after dismissal. This hypothesis would be consistent with a scenario where the observed gender gap in earnings due to job loss comes primarily from the intensive margin (wages or hours worked); second, displacement might push women into a situation where they lose their edge over men in terms of job security. In this case, the gender gap in the cost of job loss would originate from a larger loss in job security coming from the extensive margin (employment). Naturally, both mechanisms might be working at the same time. The results that will be presented next test the presence of the latter between these two mechanisms.

Figure 4 shows estimates[24] for males on the left and females on the right. The chart in the top right part of the panel illustrates that females experience an increase in unemployment risk equal, on average, to about 2.73%. Males, instead, bear a cost from job loss that appears to be less severe, being, on average, approximately 2.13%. Declines in expected tenure align with these results, totaling about

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22 These results are not included in the Appendix and are available on request.

23 As regards the regressions using the region of work as a grouping categorization, individuals are categorized according to the macro-region (namely, South, Center, and North of Italy) where they were employed in the year before the treatment occurred (using notations: e=-1).

24 These estimates, as those shown in Figures 5 and 6, are obtained without including the educational level as a covariate. This was done because, for some cohorts, I do not have enough individuals to slice the sample into too many bins, considering the wide array of covariates already employed. Nevertheless, by removing education from the main specification, I observe that the coefficient of interest does not change substantially, for instance, decreasing from 2.38% to 2.33% for the unemployment risk indicator.

165 days for females and about 149 days for males. These means, however, hide rich dynamics in post-treatment effects that are key to understanding the main difference between men and women on these outcomes. In the immediate aftermath of displacement, the drop in job security seems to impact similarly men and women. However, this effect appears to get slightly worse over time for women, while the situation gradually improves for men instead.

Figure 4: Heterogeneous effects of displacement on job security by gender
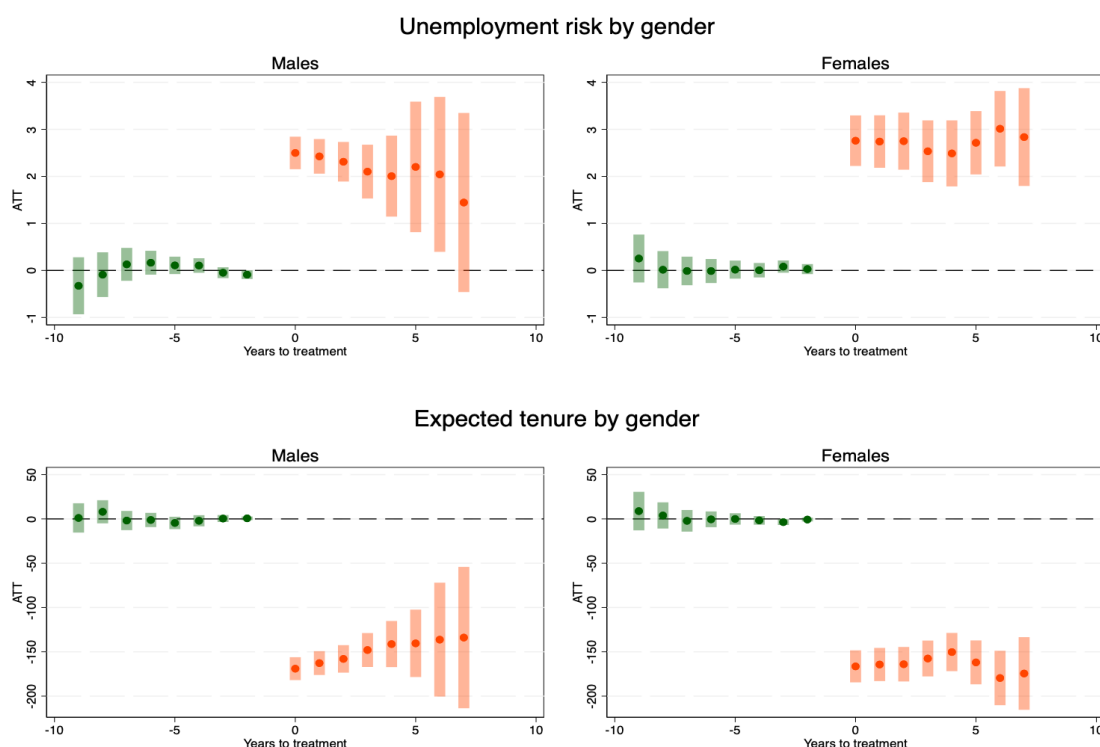
Note: The chart displays estimates of the average treatment effect of displacement on the unemployment risk associated with the main job occupations in which "treated" individuals are employed each year. Estimates are obtained using not-yet-treated units as a control group. Confidence intervals, represented by the extent of the bars, are computed using a 95% confidence level.

Estimates derived using never-treated units (shown in Figure B5 in Appendix B) are quantitatively similar to those presented in Figure 4 for the periods immediately after the treatment, then mildly diverge afterward, as individuals seemingly recover at a faster pace. Nevertheless, both specifications do convey a very similar takeaway message: while the negative impact of dismissal on both outcomes is initially almost equivalent in magnitude for men and women, effects gradually attenuate over time for males while, instead, slightly intensify for females.[25]

It is important to stress, nonetheless, that these considerations solely apply when point estimates are taken into account in isolation. In fact, confidence intervals are too wide to establish any significant differences among men and women. Altogether, these results do not show enough evidence to

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[25] More precisely, in the never-treated case, females appear to modestly recover from the negative effects of dismissal over time when looking at the expected tenure indicator, but at a slower pace and lesser extent than men.

support the hypothesis that women, after dismissal, end up losing their edge over men in terms of job security, even though point estimates might indicate that this might be happening to some moderate degree.

Next, I am going to examine the presence of heterogeneous effects with respect to the educational level. To do so, I categorized individuals as high-skilled if they obtained at least a college degree and low-skilled otherwise. Results are presented in Figure 5.

Figure 5: Heterogeneous effects of displacement on job security by educational level
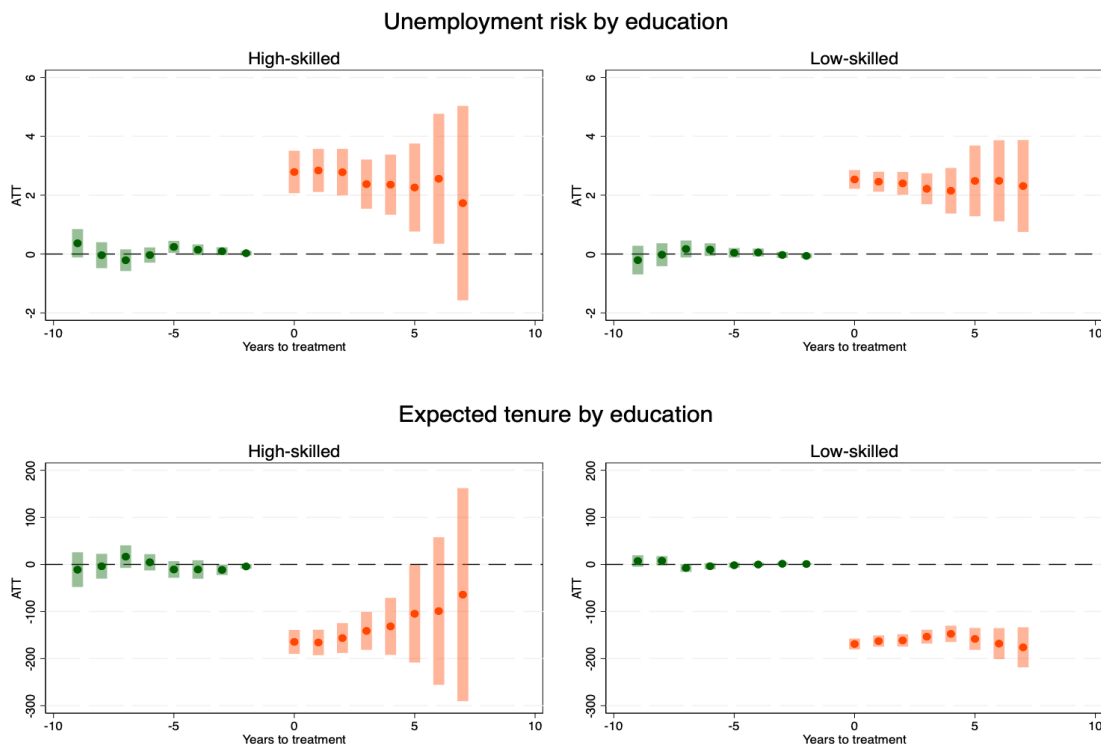
Note: The chart displays estimates of the average treatment effect of displacement on the unemployment risk associated with the main job occupations in which "treated" individuals are employed each year. Estimates are obtained using not-yet-treated units as a control group. Confidence intervals, represented by the extent of the bars, are computed using a 95% confidence level.

Differences between higher-educated and lower-educated individuals seem to materialize only in later periods and just for the expected tenure indicator. The expected tenure of lower-educated individuals on average decreases after displacement by about 162 days, while it declines less dramatically for higher-educated individuals by approximately 128 days. This substantial difference between these post-treatment averages derives mainly from the recovery that higher-educated individuals start showing from the sixth year after displacement. On the other hand, notable differences cannot be spotted between these two subgroups when I shift the attention to the other indicator, for which I register an average increase of about 2.4 pp.[26] As for the previously analyzed case, confidence

26 While I observe that the last post-treatment estimate pertaining to highly educated individuals is not significant, I interpret this as being the result of a lack of statistical power.

intervals are too large to assert the presence of significant differences between these two categories. Furthermore, looking at Figure B6 in Appendix B, which reports results obtained with never-treated units, it is unclear whether higher-educated individuals do indeed recover faster from the negative effects of displacement. There seems to be a faster-moving recovery in the expected tenure indicator, but the other indicator suggests instead the opposite story. By weighing all these elements, I conclude that no significant differences can be claimed among different educational groups.

Lastly, Figure 6 displays the results from separate regressions with respect to the macro-region of work.

Figure 6: Heterogeneous effects of displacement on job security by macro-region
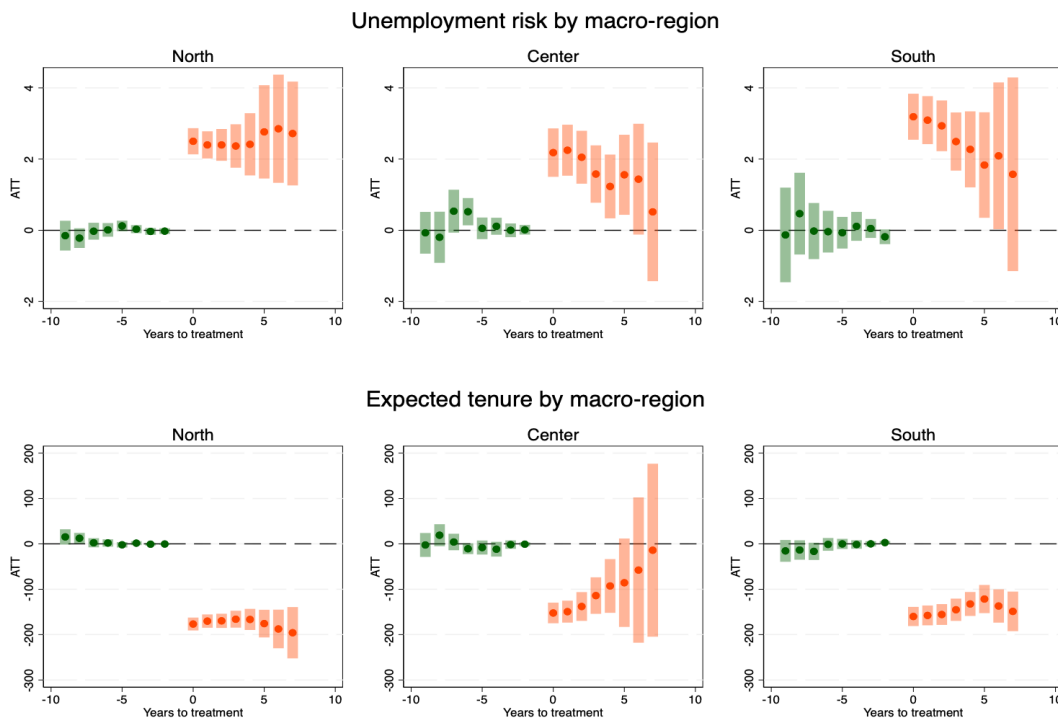
Note: The chart displays estimates of the average treatment effect of displacement on the unemployment risk associated with the main job occupations in which "treated" individuals are employed each year. Estimates are obtained using not-yet-treated units as a control group. Confidence intervals, represented by the extent of the bars, are computed using a 95% confidence level.

Once again, it is not possible to detect statistically significant differences among these three sub-groups because of wide confidence intervals. Judging solely from an examination of the point estimates, people working in the North of Italy at the time of the dismissal seem to suffer harsher and more persistent consequences from job loss, reporting a rise in unemployment risk of about 2.55 pp and a reduction in expected tenure of about 176 days. By inspecting pre-treatment averages (displayed in Table A5), however, I noticed that people working in northern regions of Italy are generally employed in jobs with a much lower risk of involuntary dismissal (about three percentage points less than in the Center and almost five percentage points less than in the South), and a substantially

higher expected tenure (about 24% higher than in the South, and 12% higher than in the Center). This means that people working in the North have more to lose from dismissal compared to people working in other regions of Italy, which can potentially, at least partially, explain the findings in Figure 6. Furthermore, given that unemployment rates in the north of Italy are historically lower than in the rest of the country, these results are consistent with the findings presented in Omori (1997), which show that workers who experience "nonemployment" when few workers are nonemployed are more severely stigmatized.

Furthermore, it should be stressed at this point that the story told by these estimates is not consistent with the one depicted by the estimates retrieved using never-treated units as a control group (reported in Figure B7 in Appendix B). In this latter case, the evidence regarding heterogeneous treatment effects across different macro-areas in Italy is, in fact, rather mixed.[27]

Since confidence intervals do not allow for making definitive statements on the regional heterogeneity of the examined effects, and since I find the aforementioned inconsistency between the two types of specification results, I suggest taking these findings with a grain of salt.

5   Robustness and placebo

As a final step, the solidity of the presented results is evaluated through a series of robustness checks, sensitivity analyses, and placebo testing. I will begin by discussing the former.

In the first one, I address potential concerns arising from the fact that for some sectors and professional qualifications, I could only rely on a smaller number of observations to compute the outcome variables. To mitigate these concerns, I refined the outcome variables by excluding sectors and professional qualifications with fewer than 100 available observations per year in the original sample. This approach aimed to retain only those occupations that ensured a precise computation of the outcome measures. Consequently, individuals employed in these excluded occupations at any point during the considered period were also discarded. After these operations, the sample dimension shrank to about 94 thousand observations. The estimates resulting from this refined sample are plotted in Figure B8 in Appendix B. These estimates closely resemble those from the main specification, differing only slightly in magnitude.

Secondly, one may be worried that outlier observations might be the ones driving the estimates. To address this concern, I excluded occupations that fall in the top or the bottom 1% of the distributions for either unemployment risk or the expected tenure indicator. Again, by doing this, I am also excluding all individuals employed in these occupations at any point in time during the sampled period. This operation removes about 3% of the total sample, reducing the sample size to about 104 thousand observations. The results, shown in Figure B9 in Appendix B, are similar to those retrieved by the main specification.

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27 However, unlike education and gender, which are time-invariant workers' characteristics in this dataset, the region of work might change over time. This creates an issue for the CS estimator because the dataset is considered, in this case, unbalanced given the set of covariates and the other restrictions imposed on the estimation (i.e., the same macro-region of work in the year preceding treatment for both treated and untreated units).

A third source of concern might be related to the use of collective dismissals as a treatment variable. More specifically, it is possible that large-scale mass layoffs could generate significant spillover effects throughout the local labor market, potentially amplifying the measured effects. For instance, widespread mass layoffs in a particular region might have significant repercussions on regional labor demand, making it more difficult for unemployed workers to find stable jobs. To alleviate these concerns, I added regional unemployment rates to the main specification to control for these potential spillovers and re-estimated the model.[28] As the CS estimator does not work well with time-varying covariates, I added this control to the regression specification carried out with the methodology introduced by Cengiz et al. (2019). The results of this exercise are remarkably close to those generated by the main specification,[29] and are reported in Table A11 in Appendix B.

Finally, I expanded the sample to include both senior and young workers, encompassing all individuals aged 20 to 64 years. This increased the sample size to approximately 195 thousand observations. The rationale for focusing only on prime-age workers in the main analysis was based on the fact that both young and senior workers might be affected by displacement very differently compared to prime-age workers. Young workers, whose careers are just beginning, might suffer more from unemployment because it prevents them from building up the relevant human capital that employers demand. Conversely, senior workers, being closer to retirement, might face significant challenges in finding new employment, as available opportunities may require modern competencies that they have not developed over their careers. The estimates of this exercise are displayed in Figure B10 in Appendix B. Overall, these estimates corroborate the findings presented throughout this study. However, the unemployment risk indicator behaves quite differently, with treatment effects intensifying in the later periods.

Finally, as a placebo test, I reallocated the treatment three years prior to its actual occurrence. The specific number of years was arbitrarily chosen and could have been set in any other year preceding the actual treatment.[30] Figure 7 shows the results originating from this exercise.

The chart on the left depicts a very small (about 0.23 pp on average) yet statistically significant negative effect for the three periods following the placebo treatment. However, while these estimates are all significant, they also are of the opposite sign with respect to the ones pertaining to the true treatment. This, if anything, may suggest that individuals, prior to the treatment occurrence, were on a positive trajectory, and thus gaining job security. Moreover, these small negative effects vanish when the placebo treatment is moved, for example, to two years before the real treatment (see Figure in Appendix B). The same reasoning applies to the second outcome variable shown in the chart on the right of Figure 7. In both cases the real effect emerges three years after the placebo treatment, aligning

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28 This covariate was not included in the main specification because it might work as a "bad control," as it is likely to be endogenous to the treatment variable and potentially bias the estimates. As a matter of fact, local unemployment rates have been frequently used in the literature as instruments for individual layoffs in lieu of collective dismissals (see, for instance, Gregg, 2001). Nevertheless, for the specific purpose of assessing the incidence of the potential spillovers generated by mass layoffs on the main results, concerns about causality can be set momentarily aside.

29 In this case, the benchmark results are the ones retrieved using the methodology as in Cengiz et al. (2019), reported in Table A9 in Appendix A.

30 Indeed, I replicated this placebo test, retiming the treatment to two years before the actual one. The results of this exercise are shown in Figure B11 in Appendix B.

with the onset of the actual treatment. These results reinforce the hypothesis that the findings in the previous section capture a fundamental change occurring in conjunction with the year of the treatment.

Figure 7: Placebo treatment
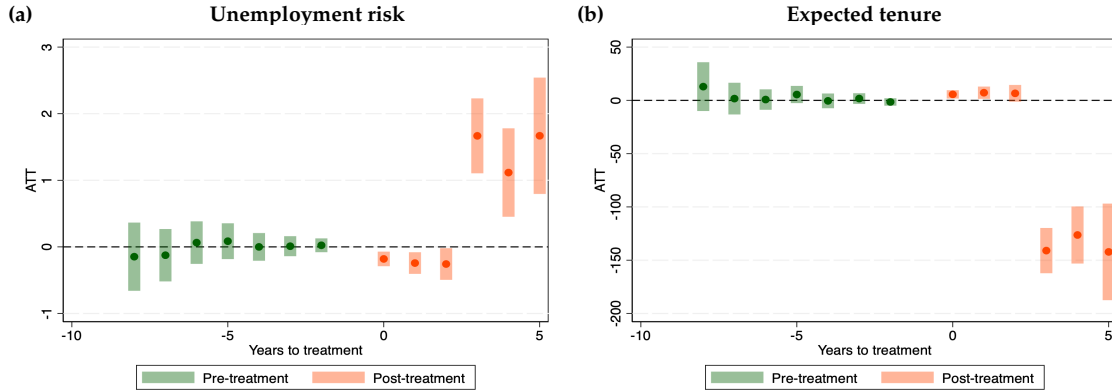
Note: The chart displays estimates of the average treatment effect of the placebo displacement on the unemployment risk (left) and the expected tenure (right) associated with the main job occupations in which "treated" individuals are employed each year. Estimates are obtained using not-yet-treated units as a control group. Confidence intervals, represented by the extent of the bars, are computed using a 95% confidence level. Estimates were retrieved using the methodology suggested by Callaway and Sant'Anna (2021).

6   Conclusions

This paper aims to provide new insights into the literature on the negative effects of job loss by analyzing how displacement affects the job security attached to the type of jobs found by displaced workers. To this purpose, I construct two measures of job security at the occupational level and empirically estimate the causal impact of displacement on these outcomes by exploiting collective dismissals as exogenous variations. While previous research has thoroughly investigated workers' earnings, wages, and employment reductions induced by displacement, this study narrows the focus to the analysis of the types of employment found by dismissed workers after they lose their jobs. Focusing on the job security variations between occupations is a particularly interesting endeavor for a couple of reasons: i) it quantifies the extent of the fall down into the job ladder in terms of job security caused by displacement; ii) it suggests that the underlying characteristics of a job can embed a heightened likelihood of repeated unemployment spells, which ultimately hints at possible demand-side deficiencies in labor markets that may be concomitantly responsible for generating persistent effects of job loss.

This study documents a significant fall in job security associated with the types of jobs found by dismissed workers. These occupations are characterized by an increased unemployment risk of about 2.38 percentage points and a decreased expected tenure of about 156 days. The dynamics of the estimated treatment effects on the treated point to a gradual but mild recovery from these negative

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effects over time. I do not find instead conclusive evidence to claim the existence of a differential effect across individuals of different genders, educational levels, and regions of work.

In light of these findings, consequential policy implications can be drawn. Papers on the scarring effects of unemployment have traditionally recommended vocational training as a solution to help dismissed workers recover from their human capital losses and get back on track. However, even on-the-job training programs that teach generic types of skills (as opposed to firm-specific skills that may not be in demand when employees leave the firm) that may alleviate human capital losses once employer-employee ties are broken may not be effective in addressing the other kinds of deficiencies described in this paper. As a matter of fact, changing jobs requires not only acquiring new skills but also settling into new work environments and going through screening and trial periods. These new occupations may be thus less stable than previously held job positions, at least initially. This calls for active labor market policies and regulations that may limit these labor market frictions that effectively represent the underlying causes of recursive unemployment.

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A Appendix

Table A1: Distribution of observations across cohorts of treated individuals

Year of treatment	Number of treated ids	Percentage
2013	1165	14.11
2014	1459	17.67
2015	1742	21.10
2016	1218	14.75
2017	915	11.08
2018	539	6.53
2019	411	4.98
2020	367	4.45
2021	204	2.47
2022	236	2.86
Total	8256	100.00

Table A2: Comparison among observable characteristics between treated and untreated units

	age	educ	female	temp	part-time
treated	42.36	3.83	0.33	0.13	0.11
untreated	43.46	3.90	0.34	0.12	0.05

Notes: The table displays from left to right the average age, education, and shares of female, temporary, and part-time workers among the treated and untreated groups for the entire dataset. Time-varying characteristics, such as part-time and temporary shares of workers in the treated sample, are calculated by only taking averages in the pre-treatment year.

Table A3: Distribution of individuals in the treatment group across macro-regions of Italy

Macro-region	N	Percent
North	4822	58.41
Center	1681	20.36
South and Islands	1753	21.23

Table A4: Distribution of individuals in the treatment group across educational levels

Level of education	N	Percent
None	140	1.70
Elementary school	32	0.39
Middle school	2353	28.50
High school	4309	52.19
University degree or more	1422	17.22

Table A5: Mean value of the outcome variables in the treatment group across macro-regions of Italy in the treatment group in pre-treatment years

Macro-region	Unemployment risk	Expected tenure
North	16.78	1239.97
	(9.32)	(486.30)
Center	19.83	1110.45
	(12.55)	(507.91)
South and Islands	21.62	999.10
	(13.52)	(465.81)

Notes: Standard errors in parentheses.

Table A6: Mean value of the outcome variables in the treatment group across educational levels in the treatment group in pre-treatment years

Level of education	Unemployment risk	Expected tenure
None	23.11	909.27
	(11.83)	(433.65)
Elementary school	30.39	769.99
	(20.99)	(415.44)
Middle school	20.89	1047.59
	(11.58)	(505.62)
High school	17.57	1204.88
	(10.63)	(486.79)
University degree or more	16.3	1254.17
	(10.89)	(471.58)

Notes: Standard errors in parentheses.

Table A7: Mean value of the outcome variables in the treatment group by gender in the treatment group in pre-treatment years

Gender	Unemployment risk	Expected tenure
Male	18.53	1166.52
	(11.08)	(508.38)
Female	18.17	1156.92
	(11.41)	(471.54)

Notes: Standard errors in parentheses.

Table A8: Results: conditional regression à la Callaway and Sant'Anna (2021) - control group: not-yet-treated units

	Unemployment risk	Expected tenure
Pre avg	-0.009	1.347
Post avg	2.134***	-155.096***
Tm9	-0.078	3.725
Tm8	-0.025	5.217
Tm7	0.117	-3.773
Tm6	0.129	-2.180
Tm5	0.083	-3.326
Tm4	0.074	-1.979
Tm3	-0.004	-0.794
Tm2	-0.046	0.250
Tp0	2.583***	-168.224***
Tp1	2.528***	-163.513***
Tp2	2.477***	-160.447***
Tp3	2.271***	-151.435***
Tp4	2.199***	-143.667***
Tp5	2.435***	-147.552***
Tp6	2.462***	-154.118***
Tp7	2.092***	-156.144***
Observations	107,328	107,328

Notes: The table reports the point estimates displayed in Figures 1 and 2 relative to the specifications run with not-yet-treated individuals as control units.

Table A9: Results: Unconditional regression, stacking à la Cengiz et al. (2019) - control group: not-yet-treated units

	Unemployment risk	Expected tenure
Tm11	-0.039	6.707
Tm10	-0.201	4.639
Tm9	-0.188	14.375**
Tm8	-0.147	17.231***
Tm7	0.213	0.652
Tm6	0.239**	-3.220
Tm5	0.072	-1.683
Tm4	0.017	0.166
Tm3	0.012	-0.615
Tm2	-0.023	-0.153
Tp0	2.567***	-167.119***
Tp1	2.437***	-160.350***
Tp2	2.297***	-155.763***
Tp3	2.161***	-150.902***
Tp4	2.159***	-146.683***
Tp5	2.269***	-147.225***
Tp6	2.334***	-146.611***
Tp7	2.260***	-145.509***
Tp8	2.294***	-145.011***
Observations	279,924	279,924

Notes: The table above displays estimates obtained via a stacking by event specification à la Cengiz et al. (2019). Stars indicate p-values, with: $***p < 0.01, **p < 0.05, *p < 0.1$.

Table A10: Results: Unconditional regression, stacking à la Cengiz et al. (2019) - control group: never-treated units

	Unemployment risk	Expected tenure
Tm9	-0.218	10.074
Tm8	-0.345*	9.330
Tm7	0.262*	-13.632**
Tm6	0.304***	-18.340***
Tm5	0.096	-12.679***
Tm4	0.035	-8.281***
Tm3	0.031	-6.275***
Tm2	-0.005	-4.036***
Tp0	2.547***	-160.566***
Tp1	2.350***	-149.068***
Tp2	2.203***	-141.559***
Tp3	2.050***	-134.013***
Tp4	1.908***	-121.560***
Tp5	1.855***	-111.291***
Tp6	1.987***	-107.706***
Tp7	1.947***	-102.522***
Tp8	1.902***	-94.503***
Tp9	1.798***	-90.453***
Observations	2,310,958	2,310,958

Notes: The table above displays estimates obtained via a stacking by event specification à la Cengiz et al. (2019). Stars indicate p-values, with: $***p < 0.01, **p < 0.05, *p < 0.1$.

Table A11: Robustness, spillover effects on regional unemployment rates: Unconditional regression, stacking à la Cengiz et al. (2019) - control group: not-yet-treated units

	Unemployment risk	Expected tenure
Tm11	-0.070	6.874
Tm10	-0.204	4.144
Tm9	-0.171	13.762**
Tm8	-0.156	17.103***
Tm7	0.209	0.364
Tm6	0.238**	-3.447
Tm5	0.075	-1.958
Tm4	0.013	0.260
Tm3	0.011	-0.624
Tm2	-0.021	-0.176
Tp0	2.584***	-167.305***
Tp1	2.454***	-160.426***
Tp2	2.322***	-155.869***
Tp3	2.187***	-150.919***
Tp4	2.179***	-146.618***
Tp5	2.292***	-147.180***
Tp6	2.355***	-146.425***
Tp7	2.283***	-145.487***
Tp8	2.353***	-145.127***
Observations	279,924	279,924

Notes: The table above displays estimates obtained via a stacking by event specification à la Cengiz et al. (2019). Stars indicate p-values, with: $***p < 0.01, **p < 0.05, *p < 0.1$.
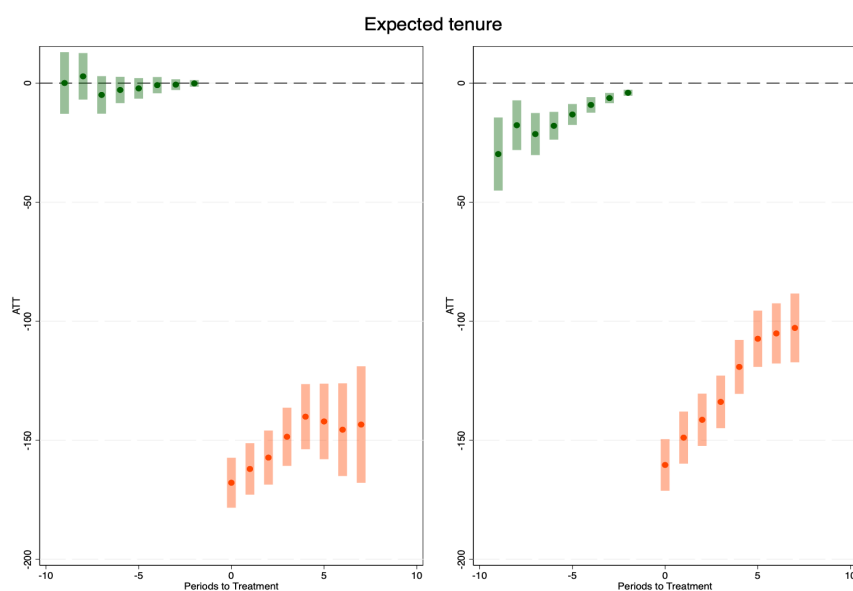
B Appendix

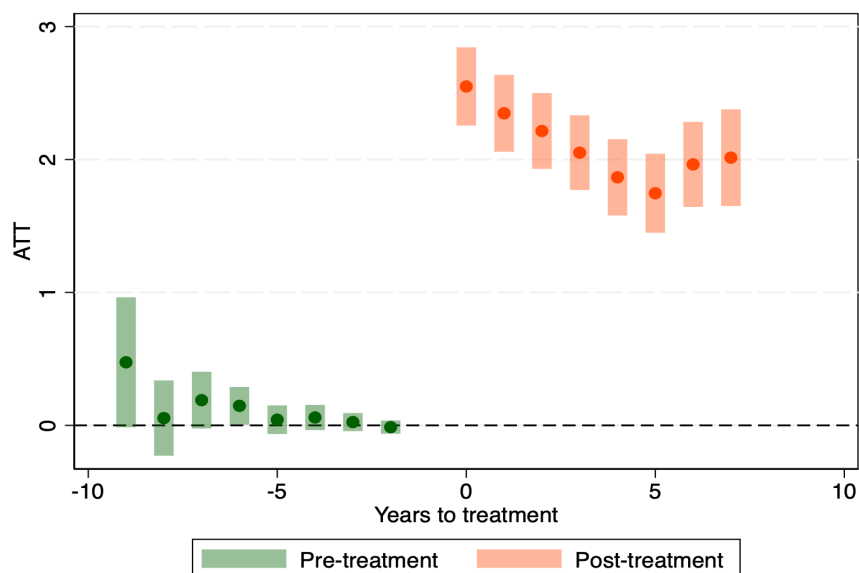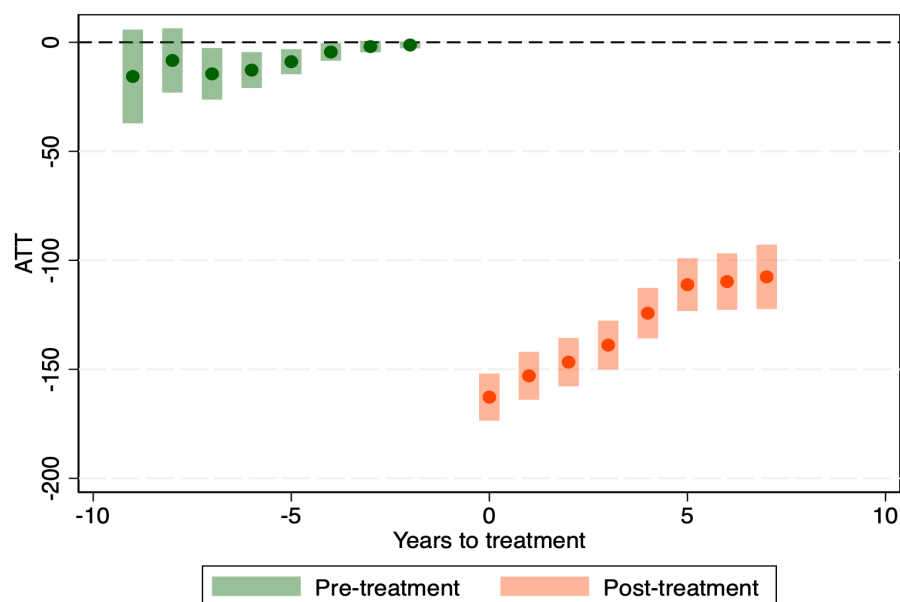**Figure B1: Unconditional regression à la Callaway and Sant'Anna (2021) - Unemployment risk**
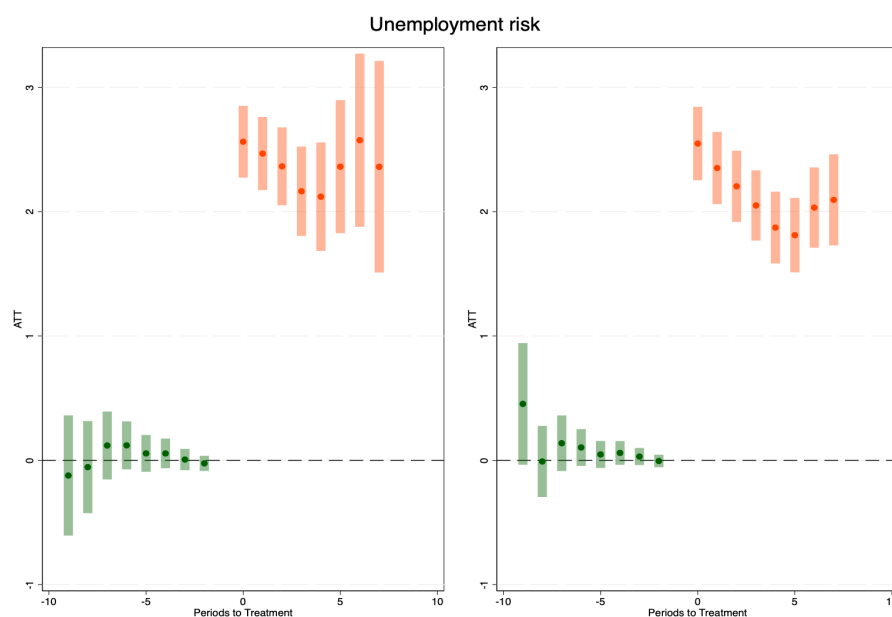
Note: The chart displays estimates of the average treatment effect of displacement on the unemployment risk associated with the main occupations in which "treated" individuals are employed each year. Estimates on the left panel are obtained using not-yet-treated units as a control group, whereas estimates on the right are obtained using never-treated units. Confidence intervals, represented by the extent of the bars, are computed using a 95% confidence level.

Figure B2: Unconditional regression à la Callaway and Sant'Anna (2021) - Expected tenure

Note: The chart displays estimates of the average treatment effect of displacement on the expected tenure associated with the main occupations in which "treated" individuals are employed each year. Estimates on the left panel are obtained using not-yet-treated units as a control group, whereas estimates on the right are obtained using never-treated units. Confidence intervals, represented by the extent of the bars, are computed using a 95% confidence level.

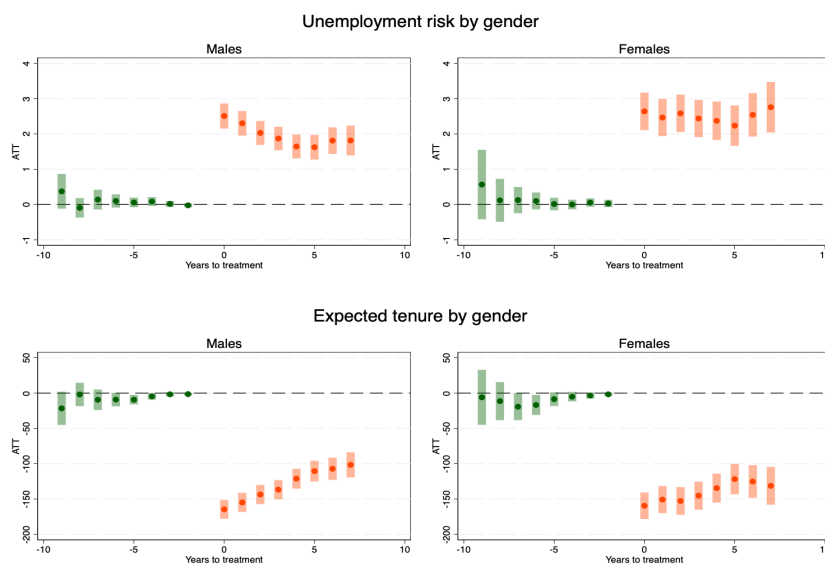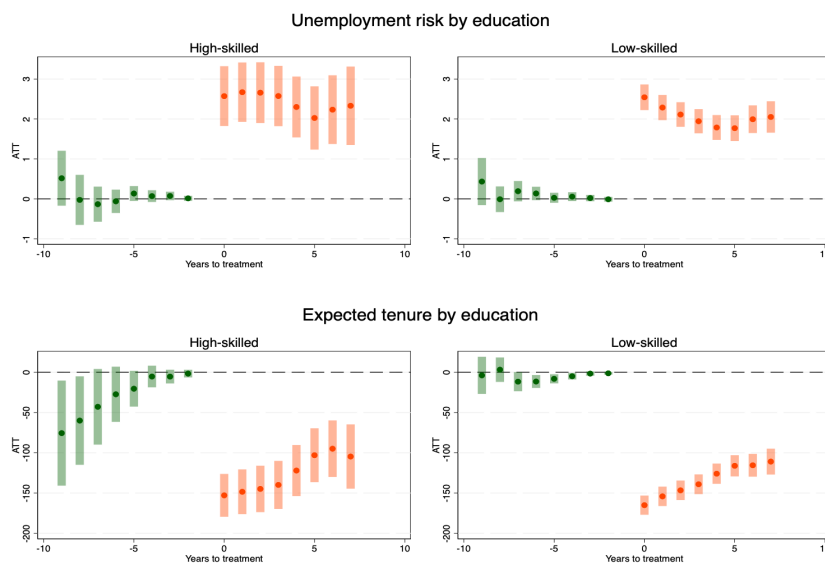**Figure B3: Unemployment risk - control group: never-treated units**

Note: The chart displays estimates of the average treatment effect of displacement on the unemployment risk associated with the main job occupations in which "treated" individuals are employed each year. Estimates are obtained using never-treated units as a control group. Confidence intervals, represented by the extent of the bars, are computed using a 95% confidence level. Estimates were retrieved using the methodology suggested by Callaway and Sant'Anna (2021).

Figure B4: Expected tenure - control group: never-treated units

Note: The chart displays estimates of the average treatment effect of displacement on the expected tenure associated with the main job occupations in which "treated" individuals are employed each year. Estimates are obtained using never-treated units as a control group. Confidence intervals, represented by the extent of the bars, are computed using a 95% confidence level. Estimates were retrieved using the methodology suggested by Callaway and Sant'Anna (2021).
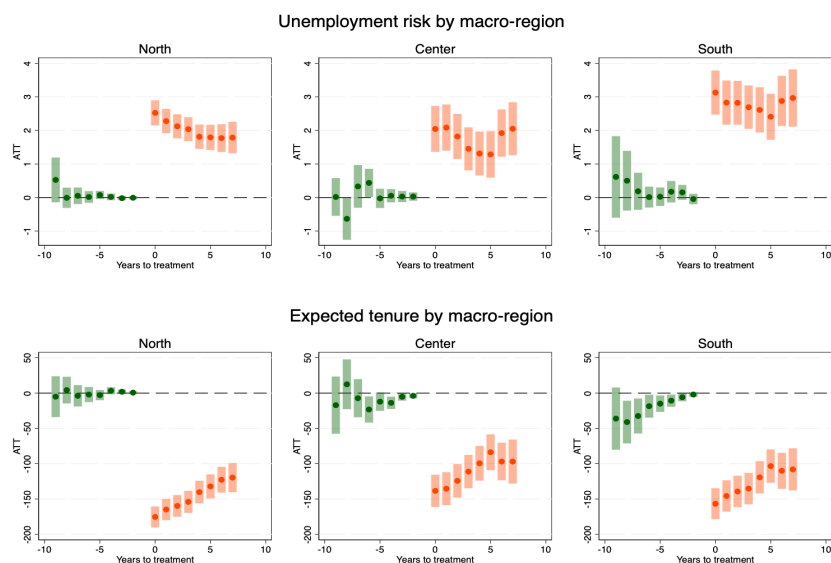
Figure B5: Conditional regression à la Callaway and Sant'Anna (2021) by gender - control group: never-treated units

Note: The chart displays estimates of the average treatment effect of displacement on the unemployment risk (top) and the expected tenure (bottom) associated with the main job occupations in which "treated" individuals are employed each year. The charts on the left show estimates obtained for the males in the sample, whereas the charts on the right display estimates for the females. Estimates are obtained using never-treated units as a control group. Confidence intervals, represented by the extent of the bars, are computed using a 95% confidence level. Estimates were retrieved using the methodology suggested by Callaway and Sant'Anna (2021).

Figure B6: Conditional regression à la Callaway and Sant'Anna (2021) by educational level - control group: never-treated units

Note: The chart displays estimates of the average treatment effect of displacement on the unemployment risk (top) and the expected tenure (bottom) associated with the main job occupations in which "treated" individuals are employed each year. The charts on the left show estimates obtained for individuals with higher education (having at least a college degree) in the sample, whereas the charts on the right display estimates for the lower educated. Estimates are obtained using never-treated units as a control group. Confidence intervals, represented by the extent of the bars, are computed using a 95% confidence level. Estimates were retrieved using the methodology suggested by Callaway and Sant'Anna (2021).
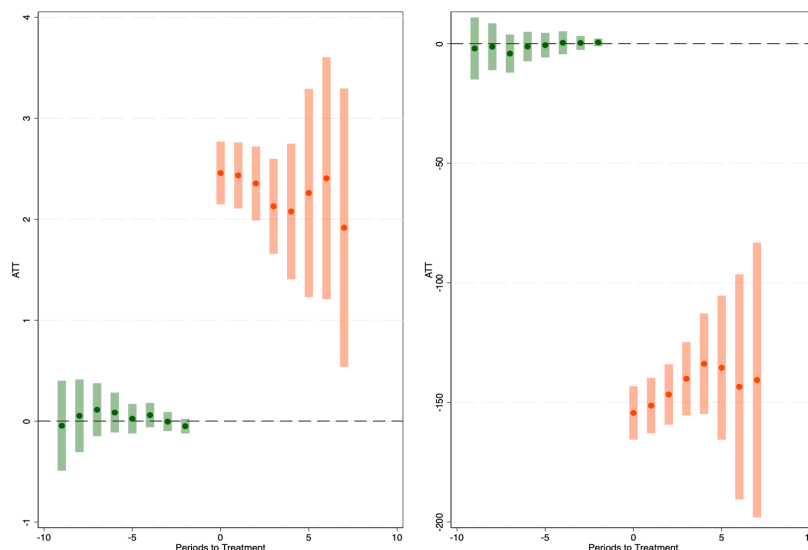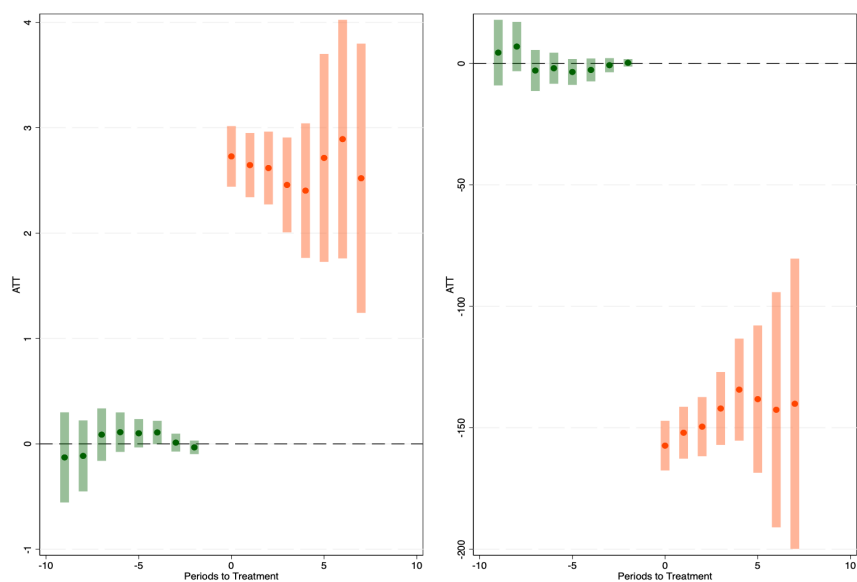
Figure B7: Conditional regression à la Callaway and Sant'Anna (2021) by macro-region of work - control group: never-treated units

Note: The chart displays estimates of the average treatment effect of displacement on the unemployment risk (top) and the expected tenure (bottom) associated with the main job occupations in which "treated" individuals are employed each year. The charts on the left, the center, and the right show estimates obtained for individuals who worked respectively in the North, Center, and South of Italy at the time of dismissal. Estimates are obtained using never-treated units as a control group. Confidence intervals, represented by the extent of the bars, are computed using a 95% confidence level. Estimates were retrieved using the methodology suggested by Callaway and Sant'Anna (2021).

Figure B8: Conditional regression à la Callaway and Sant'Anna (2021) - robustness: excluding job types with <100 observations in the original sample
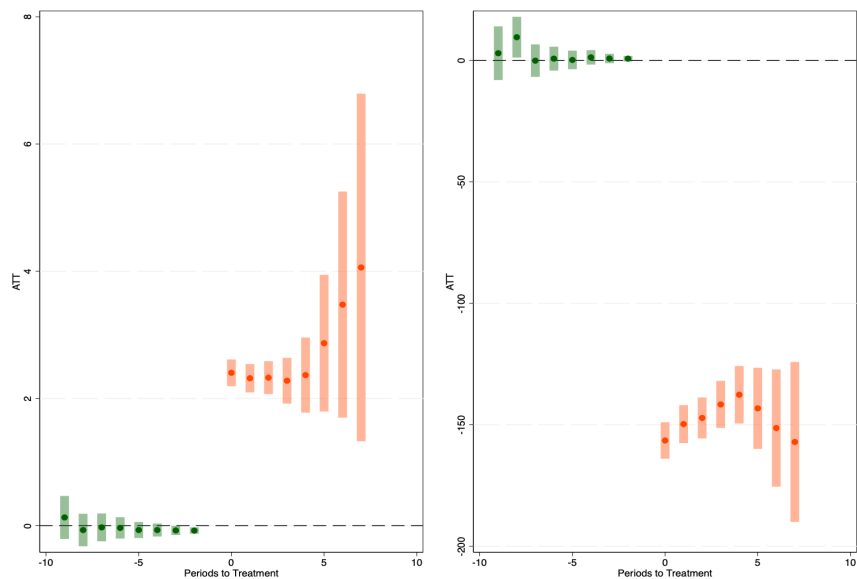

Note: The chart displays estimates of the average treatment effect of displacement on the unemployment risk (left) and the expected tenure (right) associated with the main job occupations in which "treated" individuals are employed each year. Estimates are obtained using not-yet-treated units as a control group. Confidence intervals, represented by the extent of the bars, are computed using a 95% confidence level. Estimates were retrieved using the methodology suggested by Callaway and Sant'Anna (2021).

Figure B9: Conditional regression à la Callaway and Sant'Anna (2021) - robustness: excluding outlier observations in the bottom and top 1% of the outcome variables' distributions
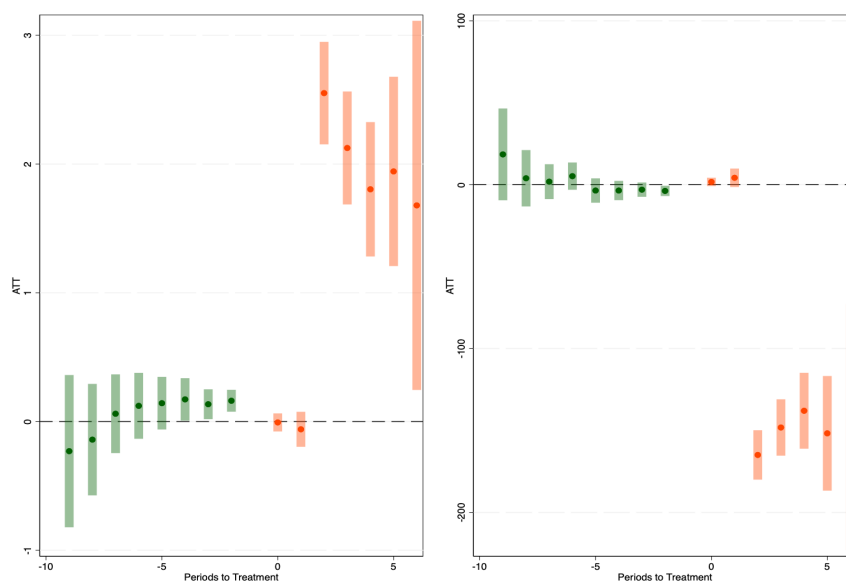

Note: The chart displays estimates of the average treatment effect of displacement on the unemployment risk (left) and the expected tenure (right) associated with the main job occupations in which "treated" individuals are employed each year. Estimates are obtained using not-yet-treated units as a control group. Confidence intervals, represented by the extent of the bars, are computed using a 95% confidence level. Estimates were retrieved using the methodology suggested by Callaway and Sant'Anna (2021).

Figure B10: Conditional regression à la Callaway and Sant'Anna (2021) - robustness: including individuals aged 20 to 64
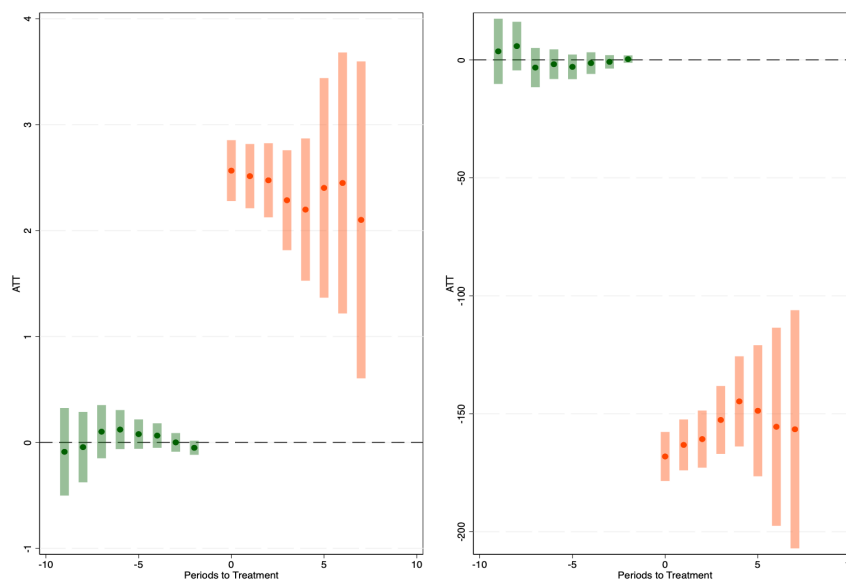

Note: The chart displays estimates of the average treatment effect of displacement on the unemployment risk (left) and the expected tenure (right) associated with the main job occupations in which "treated" individuals are employed each year. Estimates are obtained using not-yet-treated units as a control group. Confidence intervals, represented by the extent of the bars, are computed using a 95% confidence level. Estimates were retrieved using the methodology suggested by Callaway and Sant'Anna (2021).

Figure B11: Conditional regression à la Callaway and Sant'Anna (2021) - placebo: treatment reallocated two years prior the real one

Note: The chart displays estimates of the average treatment effect of the placebo displacement on the unemployment risk (left) and the expected tenure (right) associated with the main job occupations in which "treated" individuals are employed each year. Estimates are obtained using not-yet-treated units as a control group. Confidence intervals, represented by the extent of the bars, are computed using a 95% confidence level. Estimates were retrieved using the methodology suggested by Callaway and Sant'Anna (2021).

Figure B12: Conditional regression à la Callaway and Sant'Anna (2021) - including workers who lost their jobs more than once due to a collective dismissal procedure

Note: The chart displays estimates of the average treatment effect of displacement on the unemployment risk (left) and the expected tenure (right) associated with the main job occupations in which "treated" individuals are employed each year. Estimates are obtained using not-yet-treated units as a control group. Confidence intervals, represented by the extent of the bars, are computed using a 95% confidence level. Estimates were retrieved using the methodology suggested by Callaway and Sant'Anna (2021).