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 often overlooked the role of job sorting following displacement in contributing to recursive unemployment. This study aims to estimate the decline in job security associated with the occupations 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 occupation: 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 occupations. I find that displacement generates an increased risk of dismissal intrinsic to the post-displacement occupations of about 2.38 percentage points and a lower expected tenure of around 156 days. This is equal to approximately a 13% decline in job security compared to pre-displacement averages.

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1 Introduction

The idea 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). Yet, one aspect that still remains underexplored is the role of the search and matching process that follows displacement in contributing to the generation of the cycles of recurring unemployment observed in dismissed workers' careers.

This study starts from the intuition that displaced workers' heightened exposure to recurring unemployment may be closely related to the occupations 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 job positions. This can happen, for instance, because more attractive job vacancies open up less frequently and take longer to find, making them de facto difficult to attain for recently dismissed workers with bad outside options. If the set of accessible jobs to dismissed workers tends to include the generally less desirable ones, then these jobs may not only offer lower wages but also come with reduced job security. Consequently, sorting into these occupations may, in and of itself, constitute 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 occupations that dismissed workers find 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 detailed information was crucial to the development of two job security indicators that I link to each sector-specific occupation in the sample (hereafter loosely referred to as ”occupations” for simplicity): one conveying the risk of unemployment and the other the expected tenure. Several reasons may explain differing levels of job security across occupations. 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. Additionally, entry-level job positions may 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 certain sectors, such as manufacturing or hospitality, may be more susceptible to business cycle fluctuations. Lastly, there are some sectors that, by their own nature, entail a higher share of seasonal or temporary work. The job security measures developed for the analysis should be able to capture this heterogeneity among occupations, which is the ultimate focus of this study.

I estimate the effects of displacement on job security using 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 subject to 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 offers an important advantage over prior designs that employ mass layoffs as exogenous shocks. While both types of events are firms' workforce readjustments driven by economic or organizational reasons (and are, therefore, independent of dismissed workers' inner abilities), collective dismissals are typically smaller-scale shocks compared to mass layoffs.[2] This is a valuable feature, as the 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 leads to a significant decline in the job security of the subsequent occupations found by displaced workers after dismissal. In particular, these occupations feature a significantly higher inherent risk of unemployment of approximately 2.38 percentage points and a reduced expected tenure of about 156 days. To give a sense of the magnitude of these effects, the increase in unemployment risk corresponds to a 13% rise over the average value before displacement, while the drop in expected tenure is approximately equal to a 13.4% decline with respect to pre-displacement averages. Notably, these effects seem to be relatively stable and persistent over time.

Moreover, when looking 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

1 Nonetheless, I also conducted a supplementary analysis using a more conventional approach 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 Section 2.1 provides an extensive discussion of collective dismissal procedures. In short, these procedures apply to companies with more than 15 employees that 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 (see, for example, Bertheau et al., 2023) usually defines mass layoffs as a 30% reduction in a firm's workforce within a year. Hence, collective dismissal may also entail firm-level shocks that are 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, which address the following issues: i) first, I considered the possibility that a small number of outlier observations might be responsible for driving the results; ii) a second concern may arise from having a limited number of observations for some occupations and sectors in the sample, which could potentially compromise the accuracy of computed indicators of job security; iii) 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 beginning or the 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; iv) 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. The first three exercises yield estimates that are close to those obtained with the main specification, while the placebo test confirms that these estimates reflect a fundamental change occurring in correspondence with the year of treatment.

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 documented the negative consequences of unemployment on workers' career trajectories (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 following displacement 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 final outcome, the underlying idea of this study is closer in spirit to the latter mechanism, suggesting that sorting into an occupation with an inherent heightened unemployment risk increases the chances of further displacement episodes in the future, thus giving rise to a job-ladder-type chain of events. However, differently from these previous works, I empirically estimate the drop in job security using a research design that exploits collective dismissals as exogenous variations and key information on work spells that allow me to construct outcome measures of job security at the occupational level. Investigating the heterogeneity in job security at the occupational level is particularly interesting because, as shown by 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 finding suggests that demand-side frictions in the labor markets can, on their own, explain why displaced workers may face adverse consequences in their labor market outcomes even many years after a displacement episode. This, in turn, implies that sorting into certain occupations may increase workers' exposure to a heightened risk of recursive unemployment.

This paper is structured as follows: Section 2 describes the data, Section 3 outlines in detail how I constructed the two indicators of job security, Section 4 motivates the empirical strategy, the results are presented in Section 5, followed by robustness checks and placebos in Section 6, and, finally, Section 7 concludes.

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. Since smaller firms tend to offer less stable 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). This helps explain why low-job-security firms typically also offer lower salaries. In this paper, I do not measure job security at the firm level but rather at the sectoral and occupational levels. Nevertheless, as shown in Haltiwanger et al. (2022), most of the wage (and thus, given the above-mentioned correlation, most of the job security) dispersion occurs at the industrial level. Therefore, the job security indicators presented in this work should accurately reflect this heterogeneity in the job security levels among different occupations.

2 Data

This study relies on a sample extracted from the "Comunicazioni Obbligatorie" database, which records 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, each row reports 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 occupation, the sector of employment, and notably, the reason for job termination, which is an essential element for 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 employment.[7] Next, I extracted the sample of interest, which consists of individuals who experienced only one collective dismissal during the considered period. I limited the analysis to workers with a single collective dismissal to avoid complications in the interpretation of the estimates that may arise in the presence of multiple treatments.[8] Additionally, I excluded workers who started a new job before their prior contract's official termination with the dismissing firm, those who were dismissed from a job that was not identified as the main one, as well as those who returned to the same firm that had previously 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 individuals aged at least 30 in 2010 and not older than 54 in 2022. This choice was motivated by the fact that young and senior workers may respond differently to displacement with respect to prime-age workers: younger individuals, early in their careers, frequently move across many jobs regardless of

4 More specifically, the name of the sample is "Campione Integrato delle Comunicazioni Obbligatorie," in short, CICO.

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), I restricted the sample to keep a single dominant record per year. However, as earnings are not the focus of this study, I defined the main job as the one with the longest employment spell within each year. To identify main jobs, I sequentially retained the employment record that displayed: 1. the highest number of employment days within the year; 2. the longest employment spell overall (spanning throughout the whole sampled period); 3. full-time jobs were preferred to part-time ones; 4. highest income. When 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 sensibly change the results, see Figure B1. This 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).

dismissal, while older workers may instead react to displacement by anticipating their retirement.[9] Finally, to extract a balanced dataset, I also removed individuals who had gap years in their reported employment history (i.e., a full calendar year without a single employment day), which implies eliminating the long-term unemployed from the sample. The main rationale for excluding these types of workers is that individuals who remain unemployed for an extended period of time may carry unobservable characteristics that deeply differentiate them from workers with more stable employment histories, possibly introducing major confounders into the analysis.[10] After making all these adjustments, and after excluding those individuals who experienced a collective dismissal within the first three years of the studied period (to allow for at least three pre-treatment years), the final sample accounts for 8,256 treated individuals observed over 13 years, which corresponds to a total of 107,328 observations.[11] Summary statistics pertaining to this sample are shown in Appendix A from Table A1 through Table A5. 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 A6 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 by 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 based on a series of observable characteristics such that they can constitute suitable comparison units for the extrapolation of untreated potential outcomes. To this purpose, I made use of a propensity score matching procedure 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,[12] and temporary and part-time work dummies. I use this set of variables to assign to each untreated unit a score representing its probability of belonging to the treatment group. 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

9 For example, Farber et al. (2019), who studies the role of employment and unemployment histories in callbacks to job applications, 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 generally be 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.

10 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". Furthermore, Eriksson and Rooth (2014), for example, finds that potential employers attach a stigma to contemporary unemployment spells that last at least nine months, which negatively affects their hiring decisions of long-term unemployed individuals.

11 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, described in Section 3, are time-invariant indicators of job security and, thus, only vary when workers change employment. 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.

12 In this case, I categorized economic sectors using alphabetical NACE codes.

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.[13] 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 A7 in Appendix A for a detailed summary of the 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 Section 4. Nonetheless, as identification strategies that employ never-treated units have been the standard, the charts and the tables 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. These procedures 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 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

13 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.

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.

3 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 sector-specific occupation[14] in the sample: one assessing the risk of unemployment and the other measuring expected tenure. These indicators were first calculated for 2131 sectors and 627 occupations separately,[15] and then averaged to derive a single measure of job security for each sector-specific occupation (hereafter occasionally loosely referred to as ”occupations” for the sake of simplicity). 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, observing the annual survival rates of each individual job, I categorized termination events, assigning a value of one to involuntary separations and a value of zero to voluntary job transitions and ongoing employment spells.[16] Subsequently, I computed the share of involuntary separations for each occupation and sector separately, which I then averaged to finally get a time-invariant measure that captures the risk of unemployment attached to any given sector-specific occupation. This process is summarized by the formula below:

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

where y is the unemployment risk indicator pertaining to each occupation, defined as the combination of sector j and occupation 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

14 Sectors refer to 6-digit ATECO codes, while occupations are defined as ”the set of activities that an individual must perform in carrying out their job” (as defined by the Italian National Institute of Statistics). These activities imply knowledge and skills and are codified and regulated.

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

16 To conduct this operation, employment spells that reported a termination reason that left unclear the categorization into voluntary or involuntary separations, such as firm closures, were discarded. In the case of firm closures, for example, it was not possible to determine whether the firm closure entailed an actual shutdown of the firm, with all its employees being made redundant, or whether the enterprise was acquired by another company resulting, therefore, only in a transfer of employment from one employer to another. Eliminating such cases discards about 4% of all the employment spells in the sample utilized for the computation of the job security indicators.

one when the worker is subject to a layoff.

I developed the second indicator, conveying a measure of expected tenure, in a similar fashion. However, contrary to the previous case, constructing this indicator did not require an inspection of jobs' annual survival rates, but it was sufficient to calculate their length knowing the hiring and separation dates provided in the data. Nevertheless, information on the separation reasons was still useful, as I needed to filter out from the computation employment spells that ended with voluntary job moves.[17] Once I obtained these mean values pertaining to each occupation and sector separately, I averaged these employment durations to get a single measure for the expected tenure attached to each sector-specific occupation, as illustrated before for the other indicator. The formula used to compute this second measure, in fact, closely resembles the previous one:

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

where y is now the expected tenure indicator for each occupation, while d denotes the average number of days individuals spent working in that occupation within the available time period.[18]

Both indicators should reflect the heterogeneity in job security associated with different occupations. 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 thus convey the level of job security for each occupation for the whole considered time span. Table 1 shows mean values of both outcome variables before and after treatment, while Figure 1 displays the distribution of the two outcome variables' values in the final sample, regardless of the treatment period.

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

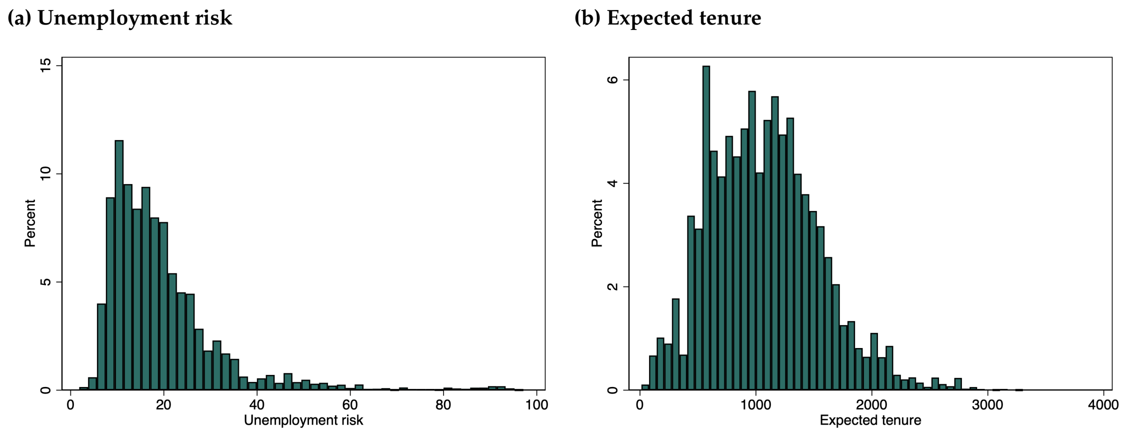
	Unemployment risk	Expected tenure
Before treatment	18.41	1163.28
	(11.19)	(496.30)
After treatment	20.47	997.94
	(13.57)	(475.33)

Notes: Standard errors in parentheses. The table displays the average value for the two outcome variables (unemployment risk and expected tenure) before and after displacement takes place. The figures refer to the 8256 individuals in the final sample of treated units.

17 Employment spells with unclear separation reasons, as mentioned before, were also excluded.

18 Note that the original dataset also includes employment spells that began before the first sampled year. I included these work spells in the calculation of both indicators.

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



Notes: The two charts show the distribution of the two outcome variables' values in the final sample of treated units. The charts, therefore, display values belonging to both pre- and post-treatment periods.

4 Identification and Empirical Strategy

This analysis aims to assess the impact of displacement on the job security of the occupations obtained by dismissed individuals after job loss. 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 economic and organizational needs. Therefore, in the context of mass layoffs and firm closures, all kinds of workers may face displacement, regardless of their individual attributes.[19] However, Cederlöf (2021) critiques the use of these types of shocks, arguing that they might 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 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 lack information on the reason for employment termination, and thus, they 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

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, approaches.

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.

dataset that does contain this information and thus allows for disentangling between these two separation reasons, which constitutes a clear advantage with respect to previous research. Furthermore, collective dismissals, while preserving the nature of being firm-level shocks motivated by economic or organizational reasons like firm closures and mass layoffs, are generally of a smaller scale, which alleviates concerns about spillover effects being too prominent in local labor markets.

The following analysis employs a difference-in-differences event study, where the control group consists of individuals who have not yet been displaced in 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 derive counterfactual potential outcomes can be presumed to be less vulnerable to biases stemming from unobservable time-varying factors, as these types of confounders are more likely to be implicitly netted out. In the next 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 and regulations. 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, leveraging the "no anticipation" assumption, employs every cohort that is yet to be treated. This feature of the CS estimator is particularly valuable given that the dataset includes fewer treated units in later periods. It needs to be remarked 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 then the following: conditional on a set of observable characteristics, treated and control units followed similar job security paths prior to dismissal; in the counterfactual scenario where displacement would not have occurred, these trajectories would have remained unchanged. Given this conceptual framework, the main specification is:

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

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 is a vector of pre-treatment ($e = -1$) time-invariant covariates (including 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, and 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 the occupations found by dismissed workers following dismissal.

Furthermore, to benchmark the results, I analogously performed the presented estimation (although using an unconditional specification) employing a stacking-by-event methodology as in Deshpande and Li (2019) or Cengiz et al. (2019).

5 Results

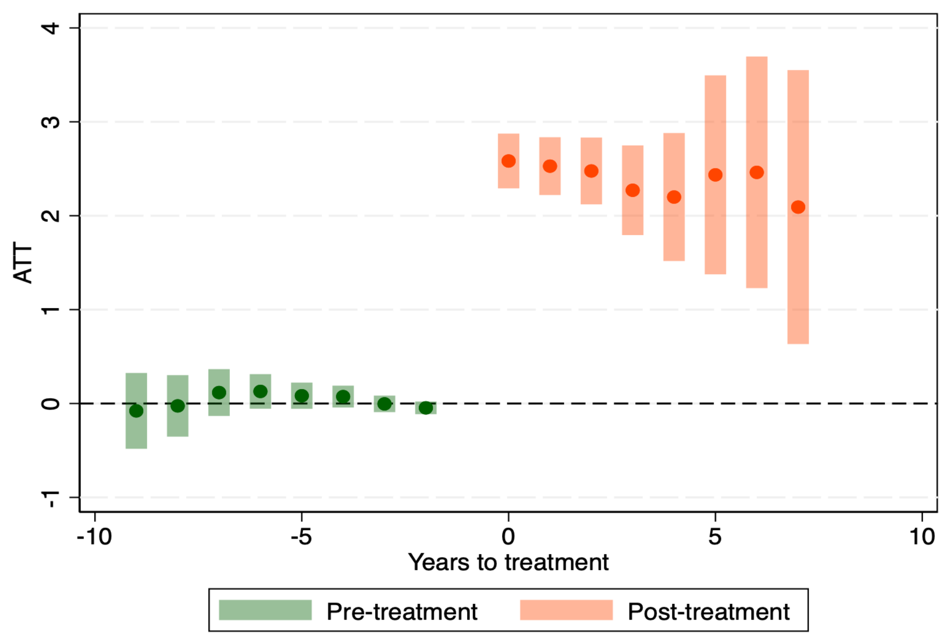
Figure 2 presents the results obtained from estimating equation (1) for the first of the two outcome variables, the unemployment risk. The chart displays the expected change in the unemployment risk linked to the occupations held by treated units compared to the variation in the control group before and after the treatment. In other words, post-treatment estimates reflect the average treatment effect of displacement on the dismissed workers' expected unemployment risk associated with the occupations that they find after being dismissed.

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 relatively 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 omits the value corresponding to $e = 8$, which is also excluded from all regression results presented next. 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 A6 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 this estimate, I preferred to leave it out from the presented results. An analogous motivation applies to two pre-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 controls (shown in Figure B2 of Appendix

B) are similar to those retrieved with not-yet-treated units, although coefficients are slightly smaller in magnitude and display a mild (but noticeable) recovery over time. Additionally, a further distinction between the results derived from the two different specifications is that, while the average of the pre-treatment estimates is still close to zero (0.123), it is instead jointly positive and significant for the never-treated case (even though this result is mainly driven by the coefficients that are farther away from the treatment period). Taking stock of these findings, while using never-treated units as controls yields post-treatment estimates that are comparable to those obtained with not-yet-treated units, an examination of pre-trends suggests that, as hypothesized in the previous section, the latter type of control units is more appropriate for the empirical setting under consideration.

Figure 2: Unemployment risk



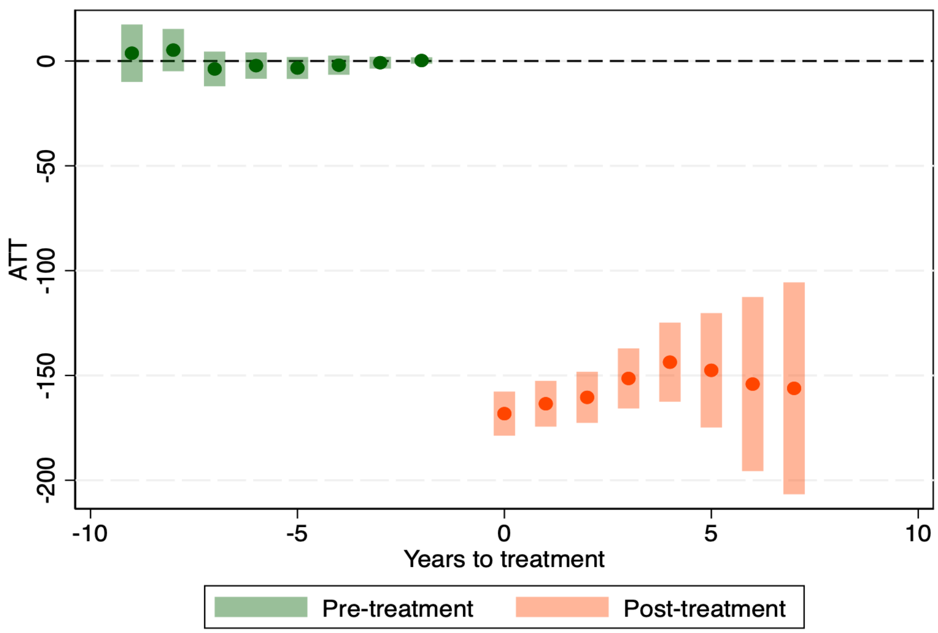
Note: The chart displays estimates of the average treatment effect of displacement on the unemployment risk associated with the main occupations in which "treated" individuals are employed each year. Estimates are obtained using not-yet-treated units as a control group. Confidence intervals, represented 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 B3 in Appendix B. Similarly to what I observed in the estimates obtained for the unemployment risk indicator, the magnitude of these coefficients is generally inferior with respect to estimates shown in Figure 3.

This is mainly due to the progressive recovery over time that treated individuals seem to experience when never-treated units are used to derive counterfactual potential outcomes. In addition, like before, pre-treatment coefficients are jointly significant (even though relatively close to zero) and have the same sign as the post-treatment coefficients. I interpret these findings as further evidence that not-yet-treated units provide better counterfactual potential outcomes in this empirical framework.

Figure 3: 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 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.

Moreover, the analysis has been replicated by means of a stacking by event specification. As mentioned in the previous section, this specification does not feature covariates. Yet, the findings are in line with those from the main specification and are reported in Table A8 of Appendix A. These results can also be compared with those derived using unconditional specifications through the CS methodology. These estimates are plotted in Figures B4 and B5 of Appendix B. The main takeaway from these latter results is that adding covariates with the aim of retrieving credible conditional parallel trends seems to only matter in one case, which is the one relative to the specification of the expected tenure indicator that uses never-treated units as controls.

Overall, all these specifications unequivocally suggest that dismissal entails a substantial drop in the job security attached to the subsequent occupations found by dismissed individuals. To give a sense of the magnitude of these effects, the increase in unemployment risk corresponds to a 13% rise over the pre-displacement average value, while the reduction in expected tenure represents approximately a 13.4% decline compared to pre-displacement means.

On a final note, cohort-specific results reveal a substantial heterogeneity in the impact of dismissal across different cohorts. The 2019 cohort is the most affected, with post-treatment averages of 4.23 pp for the unemployment risk indicator and negative 260 days for the expected tenure one. In contrast, the 2017 cohort is the least affected, with post-treatment averages of 1.11 pp and negative 130 days, respectively.[21]

5.1 Heterogeneity of results

The findings in the previous section document a significant and persistent impact of displacement on the job security associated with the occupations attained by dismissed workers after being laid off. A natural question that arises is whether these adverse 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.[22] Therefore, in what follows, I will examine the impact of job loss on all of these subgroups separately to ascertain the presence of possible heterogeneous treatment effects.

First, is there a gender differential in the impact of job loss on the job security of subsequent occupations? Based on the evidence disseminated in the literature, the answer to this question is not straightforward. In fact, on one hand, some studies have shown that women tend to sort into jobs with lower wage premiums (Card et al., 2016) but also a reduced risk of dismissal (Wilkins and Wooden, 2013; see, also, Table A5). On the other hand, some recent analyses have revealed that job loss leads to more substantial and persistent reductions in earnings for women (Illing et al., 2021). Putting together these findings, two hypotheses could explain the observed gender gap in the cost of job loss on earnings. First, compared to their male counterparts, women may end up accepting lower-paid jobs with limited wage growth potential (e.g., part-time jobs) after dismissal. This hypothesis would be consistent with a scenario where the observed gender gap in earnings due to job loss comes primarily from wages or hours worked. Second, displacement may erode women's relative advantage over men in terms of job security. In this latter case, the gender gap in earnings due to displacement would originate from a larger decline in job security, reflecting effects on the extensive margin (employment). Naturally, while both mechanisms could be at work at the same time, the results that will be presented next only test the presence of the latter between these two mechanisms.

Figure 4 presents estimates[23] 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 average increase in unemployment risk of approximately 2.73 pp, while males (on the top left corner of the chart), instead, face a comparatively smaller average increase of about 2.13 pp. Declines in expected tenure align with these results,

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

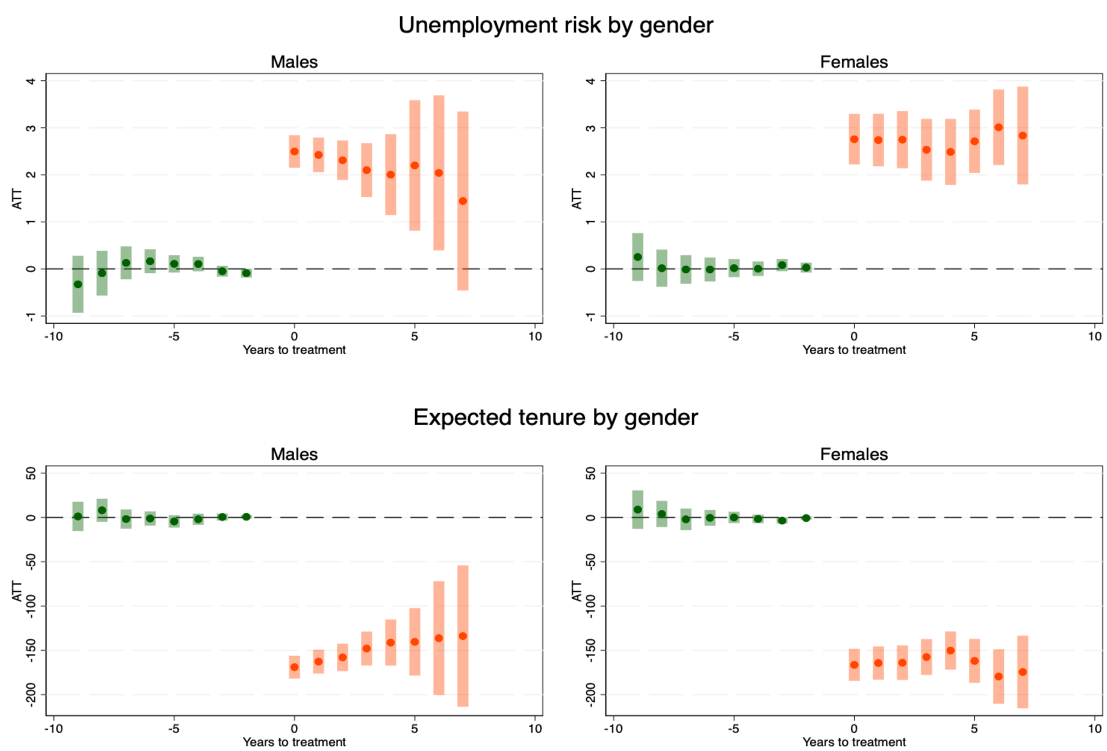
22 For the regressions based on the region of work as a grouping categorization, individuals are classified according to the macro-region (namely, South, Center, and North of Italy) where they were employed during the year before the treatment occurred (denoted as: e=-1).

23 These estimates, like those shown in Figures 5 and 6, are derived without including the educational level as a covariate. This decision was made because, when splitting the sample to estimate the model on separate subgroups, the sample size of certain cohorts was not sufficiently large to accommodate all the covariates in the original model. Nevertheless, excluding education from the model does not seem to alter substantially the coefficient of interest. For instance, by omitting this covariate from the main specification, the estimate relative to the unemployment risk indicator barely declines from 2.38 to 2.33.

amounting to about 165 days for females and 149 days for males. These averages, however, mask rich dynamics in post-treatment effects that are critical to understanding gender differences in these outcomes. In the immediate aftermath of displacement, the drop in job security seems to affect to a similar extent both men and women. However, over time, these negative effects get moderately worse for women, while the situation gradually improves for men instead.

Estimates derived using never-treated units (shown in Figure B6 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.[24]

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 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.

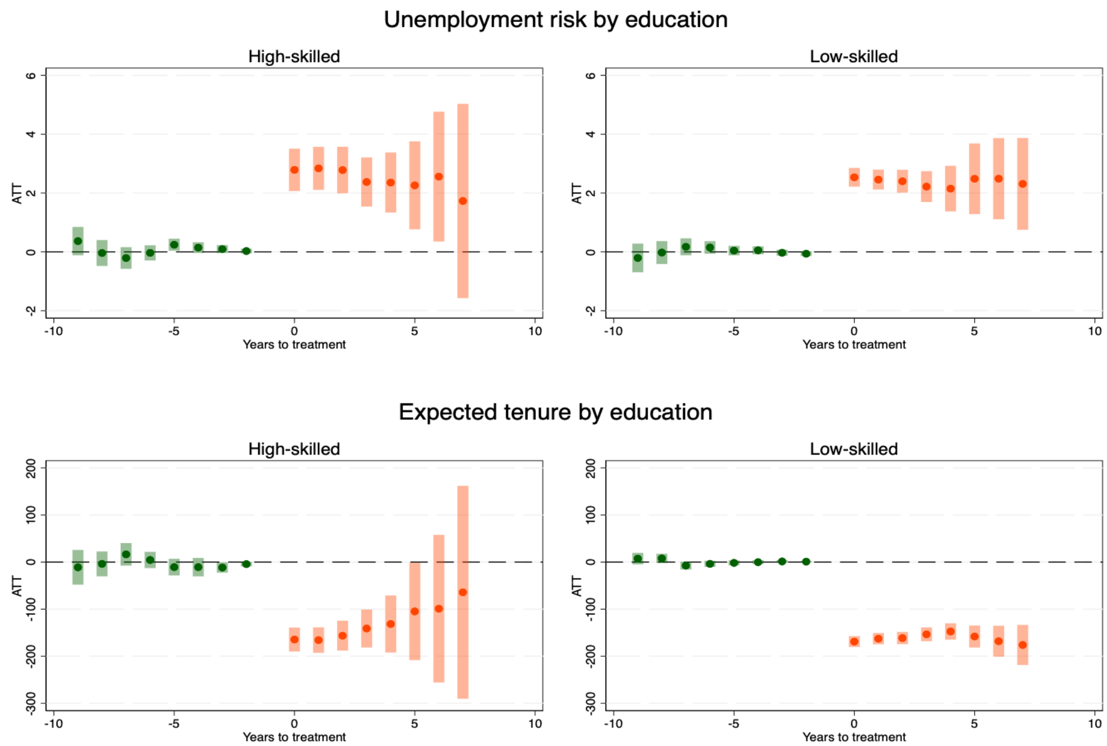
It is important to emphasize, nonetheless, that these considerations solely apply when point estimates are taken into account in isolation. In fact, confidence intervals are too wide to establish any statistically significant differences between men and women. Altogether, these results, therefore, do

24 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 their recovery is slower-paced and less pronounced than that of males.

not provide sufficient evidence to support the hypothesis that women, after dismissal, end up losing their edge over men in terms of job security, even though point estimates do indicate that this might be happening to some moderate degree.

Next, I examine the presence of heterogeneous effects by educational level. To do so, I categorized individuals as high-skilled if they obtained at least a college degree and low-skilled otherwise. The results are displayed 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 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.

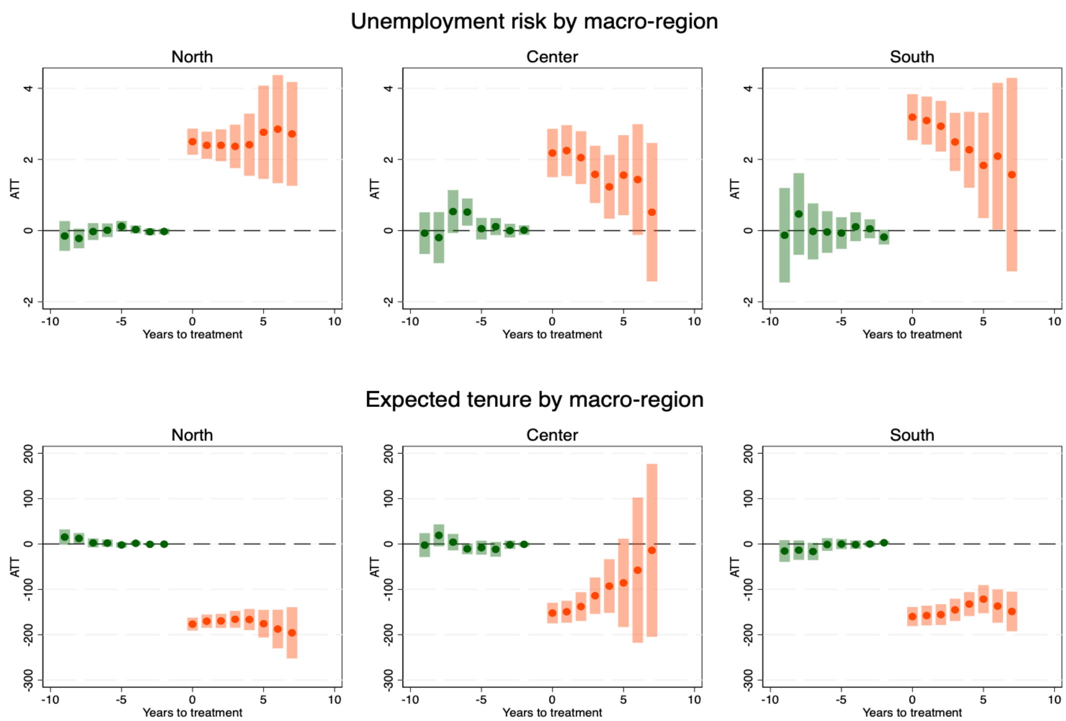
Differences between higher-educated and lower-educated individuals seem to emerge only in later periods and exclusively for the expected tenure indicator. On average, the expected tenure of lower-educated individuals decreases by approximately 162 days after displacement, while the decline is less pronounced for higher-educated individuals, at around 128 days. This substantial difference between these post-treatment averages primarily reflects the recovery observed among higher-educated individuals beginning in the sixth year following displacement. In contrast, notable differences cannot be spotted between these two subgroups for the other indicator, which shows an average increase of about 2.4 pp across both subgroups.[25] As for the previously analyzed case, confi-

25 Although I observe that the last post-treatment estimate pertaining to highly educated individuals is not significant, I attribute this result to a lack of statistical power.

dence intervals are too wide to assert the presence of statistically significant differences between these two categories. Furthermore, Figure B7 in Appendix B, reporting results obtained with never-treated units, does not clarify whether higher-educated individuals recover faster than lower-educated ones from the negative effects of displacement. While the expected tenure indicator suggests a faster recovery, the other indicator points to the opposite conclusion. By weighing all these elements, there seems to be a very mild indication that high-skilled individuals might reverse the negative effects of displacement over time. However, I conclude that no significant differences can be established between 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 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 subgroups because of wide confidence intervals. Nevertheless, judging solely from an examination of the point estimates, individuals employed in the North of Italy at the time of 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. An inspection of pre-treatment averages (shown in Table A3), however, reveals that individuals working in the northern regions of

Italy are generally employed in jobs with a substantially lower risk of dismissal (about three percentage points less than in the Center and almost five percentage points less than in the South) and a considerably higher expected tenure (about 12% higher than in the Center, and 24% higher than in the South). These pre-treatment differences suggest that individuals working in the North have more to lose from dismissal compared to their counterparts in other regions, potentially explaining, at least partially, the patterns observed 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 "stigma effects" theorized in the literature (i.e., Omori, 1997, which shows that workers who experience "nonemployment" when few workers are nonemployed are more severely stigmatized).

Finally, it is important to highlight that the narrative conveyed by these estimates is not consistent with the one depicted by the estimates retrieved using never-treated units as the control group, as shown in Figure B8 in Appendix B. In this latter case, the evidence regarding heterogeneous treatment effects across different macro-areas in Italy appears, in fact, more mixed.[26] Since confidence intervals do not allow for making definitive statements on the regional heterogeneity of the examined effects, and given the aforementioned inconsistency between the results of the two types of specifications, these findings should be viewed with caution.

6 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 B9 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

26 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).

thousand observations. The results, shown in Figure B10 in Appendix B, are similar to those retrieved by the main specification.

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.[27] 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,[28] 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 B11 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.[29] 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

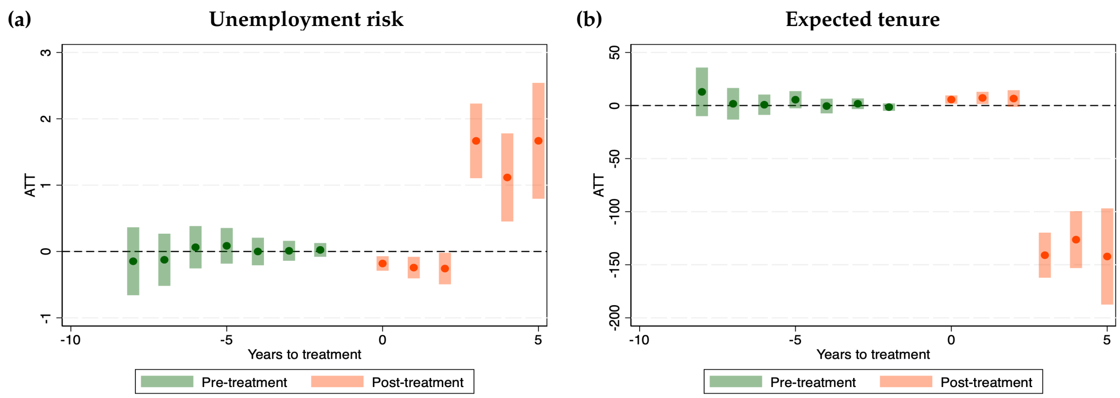
27 This covariate was not included in the main specification because it might work as a "bad control," as it is likely to be endogenous to the treatment variable and potentially bias the estimates. As a matter of fact, local unemployment rates have been frequently used in the literature as instruments 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.

28 In this case, the benchmark results are the ones retrieved using the methodology as in Cengiz et al. (2019), reported in Table A8 in Appendix A.

29 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 B12 in Appendix B.

B12 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 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 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).

7 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 occupations found by displaced workers after displacement. 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, exploiting collective dismissals as exogenous variations. While previous research has extensively investigated the effects of displacement on workers' earnings, wages, and future unemployment, this study narrows the focus of analysis to the types of employment found by dismissed workers after they lose their jobs. Concentrating on job security variations across occupations is particularly interesting 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 may embed a heightened likelihood of repeated unemployment spells, which ultimately points to potential 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 occupations 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 indicate a gradual, albeit modest, recovery from these negative effects over time. However, I do not find conclusive evidence to claim the existence of differential effects 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), which may be able to alleviate the human capital losses arising once employer-employee ties are broken, may not be as effective in tackling the other kinds of labor market deficiencies that generate the long-lasting negative effects illustrated in this paper. As a matter of fact, changing jobs may require not only acquiring new skills but also spending an extensive period of time going through screening and trial periods before settling into the new work environment. These new occupations may be thus less stable than those previously held, at least initially. This calls for active labor market policies and regulations that have the ability to limit this kind of labor market frictions that effectively represent the root causes of recursive unemployment.

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A Appendix

Table A1: 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 A2: 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 A3: 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 A4: 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	16.30	1254.17
	(10.89)	(471.58)

Notes: Standard errors in parentheses.

Table A5: 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 A6: 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 A7: 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 A8: 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 A9: 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 A10: 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.

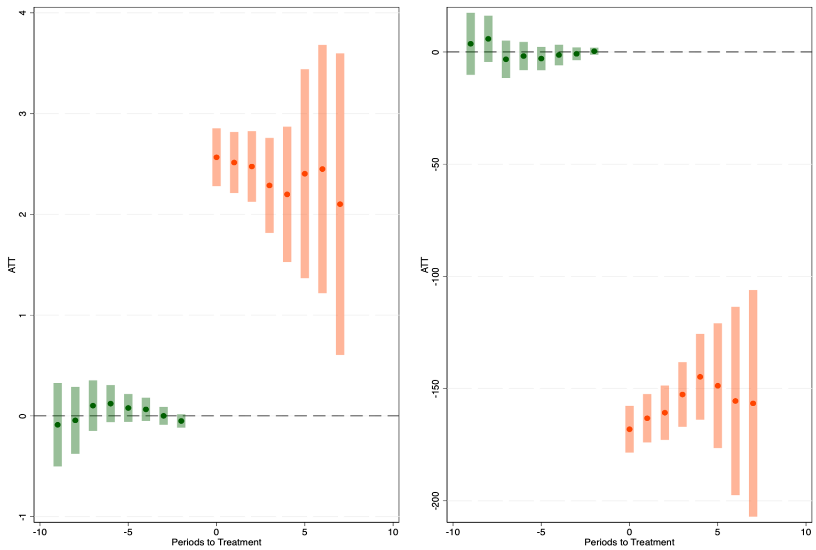
Table A11: Robustness, spillover effects on regional unemployment rates: Unconditional regression, stacking à la Cengiz et al. (2019) - control group: not-yet-treated units

	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: $***p < 0.01$, $**p < 0.05$, $*p < 0.1$.

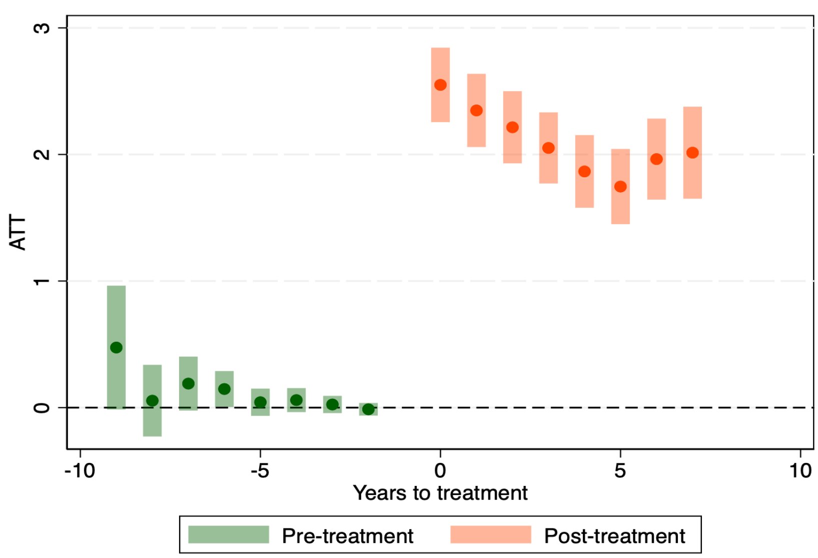
B Appendix

Figure B1: Conditional regression à la Callaway and Sant'Anna (2021) - including workers who lost their jobs more than once due to a collective dismissal procedure



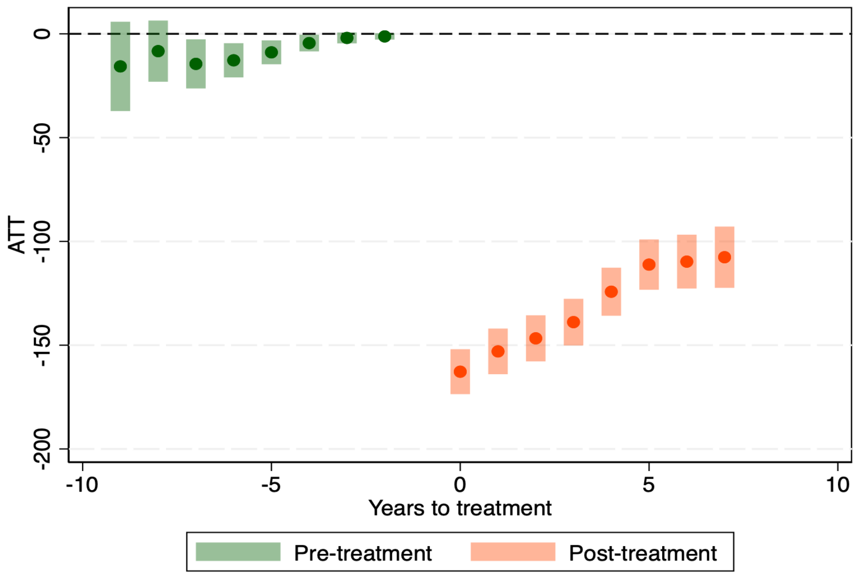
Note: The chart displays estimates of the average treatment effect of displacement on the unemployment risk (left) and the expected tenure (right) associated with the main occupations in which "treated" individuals are employed each year. Estimates are obtained using not-yet-treated units as a control 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 B2: Unemployment risk - control group: never-treated units



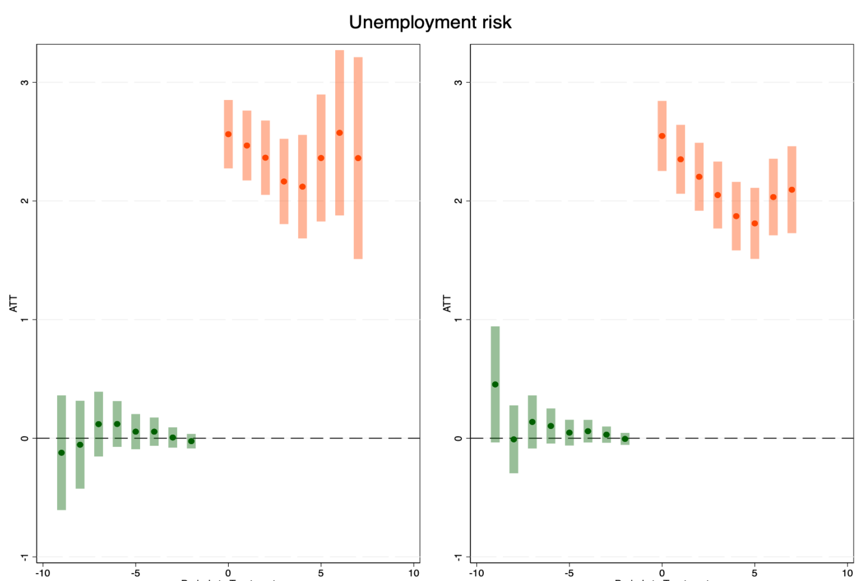
Note: The chart displays estimates of the average treatment effect of displacement on the unemployment risk associated with the main occupations in which "treated" individuals are employed each year. Estimates are obtained using never-treated units as a control group. Confidence intervals, represented 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 B3: Expected tenure - control group: never-treated units



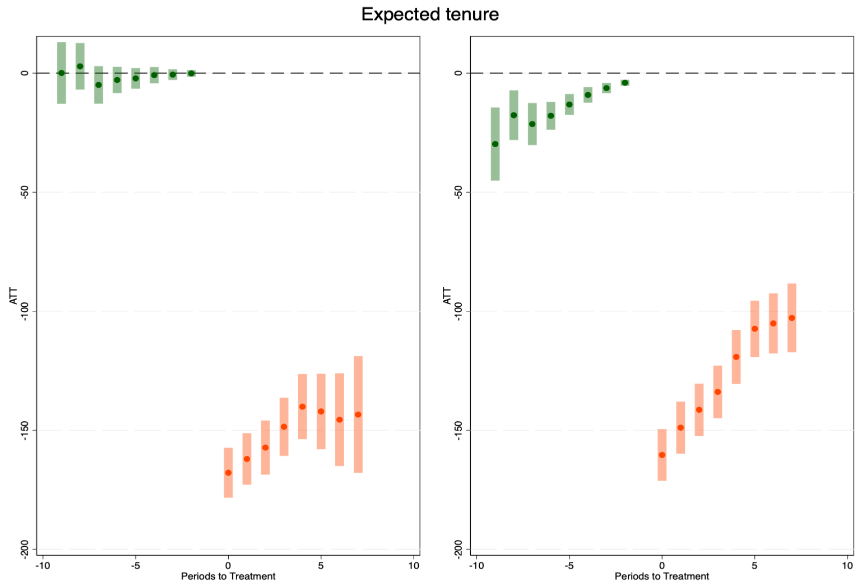
Note: The chart displays estimates of the average treatment effect of displacement on the expected tenure associated with the main occupations in which "treated" individuals are employed each year. Estimates are obtained using never-treated units as a control group. Confidence intervals, represented 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: Unconditional regression à la Callaway and Sant'Anna (2021) - Unemployment risk



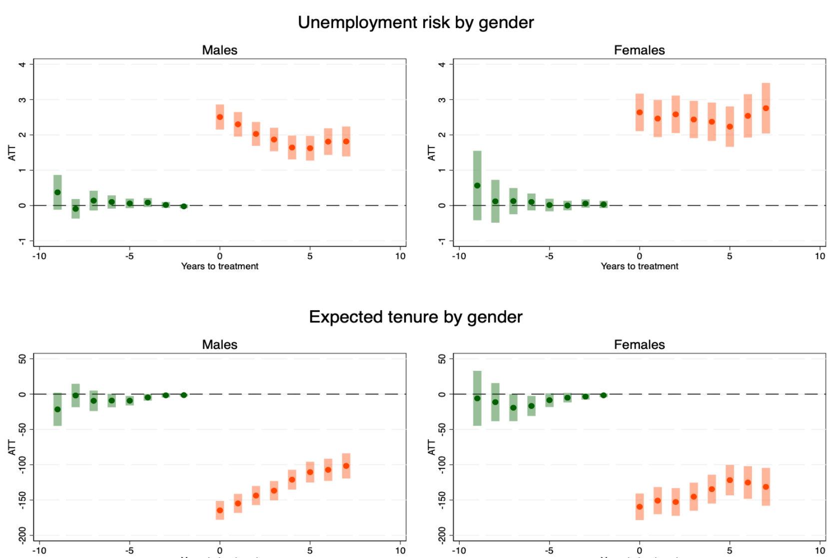
Note: The chart displays estimates of the average treatment effect of displacement on the unemployment risk associated with the main occupations in which "treated" individuals are employed each year. Estimates on the left panel are obtained using not-yet-treated units as a control group, whereas estimates 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 B5: Unconditional regression à la Callaway and Sant'Anna (2021) - Expected tenure



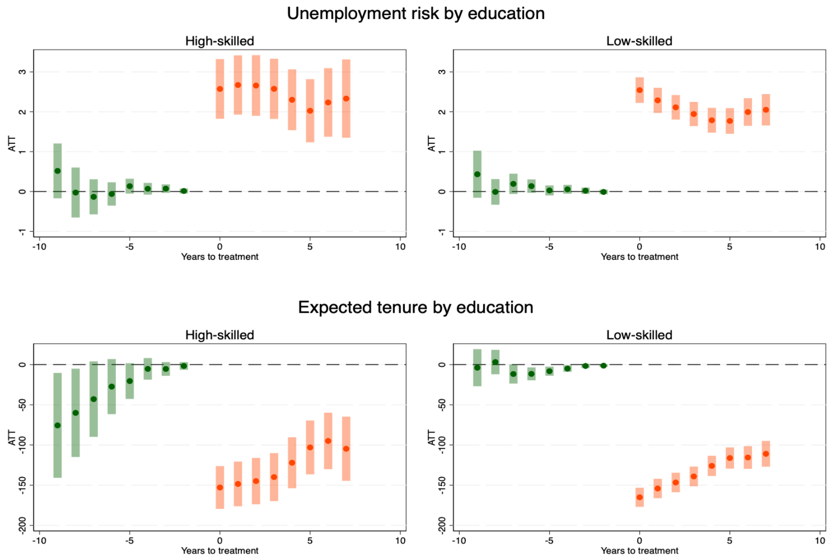
Note: The chart displays estimates of the average treatment effect of displacement on the expected tenure associated with the main occupations in which "treated" individuals are employed each year. Estimates on the left panel are obtained using not-yet-treated units as a control group, whereas estimates 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 B6: Conditional regression à la Callaway and Sant'Anna (2021) by gender - control group: never-treated units



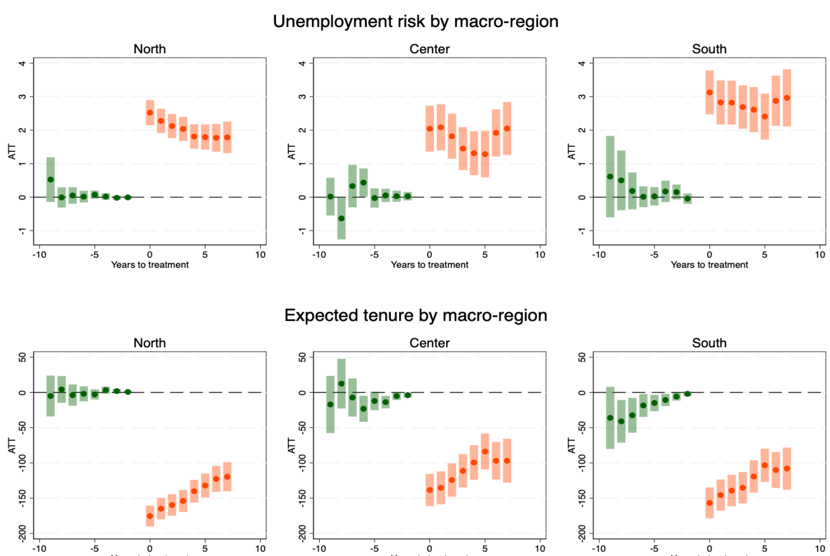
Note: The chart displays estimates of the average treatment effect of displacement on the unemployment risk (top) and the expected tenure (bottom) associated with the main occupations in which "treated" individuals are employed each year. The charts on the left show estimates obtained for the males in 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 B7: Conditional regression à la Callaway and Sant'Anna (2021) by educational level - control group: never-treated units



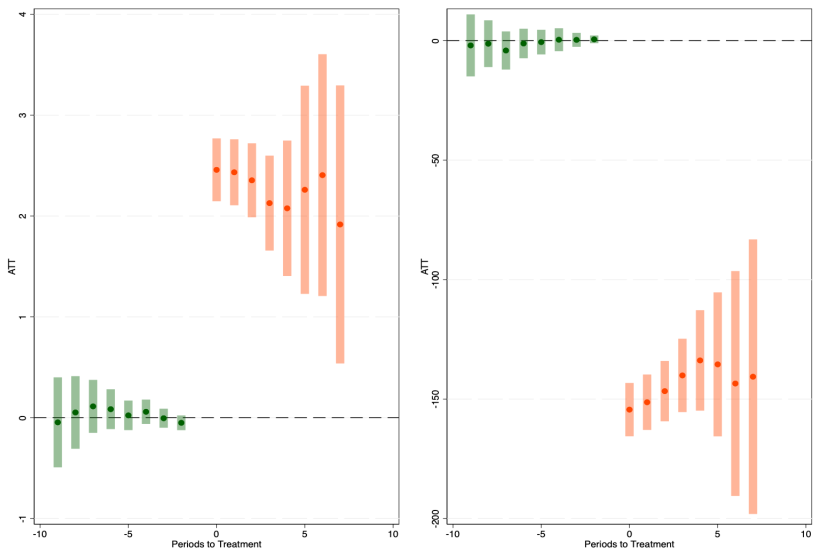
Note: The chart displays estimates of the average treatment effect of displacement on the unemployment risk (top) and the expected tenure (bottom) associated with the main occupations in which "treated" individuals are employed each year. The charts on the left show estimates obtained for individuals 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 B8: Conditional regression à la Callaway and Sant'Anna (2021) by macro-region of work - control group: never-treated units



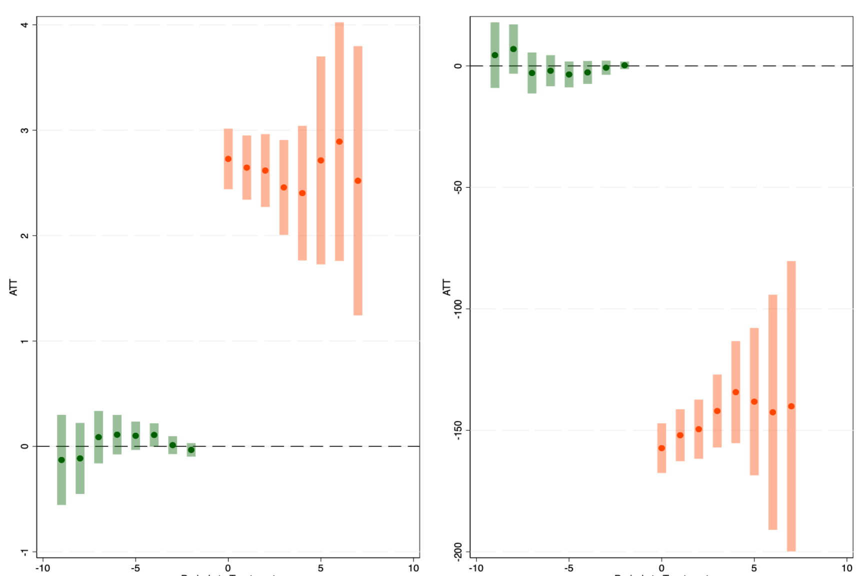
Note: The chart displays estimates of the average treatment effect of displacement on the unemployment risk (top) and the expected tenure (bottom) associated with the main occupations in which "treated" individuals are employed each year. The charts on the left, the center, and the right show estimates 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 B9: Conditional regression à la Callaway and Sant'Anna (2021) - robustness: excluding occupations and sectors with <100 observations in the original sample



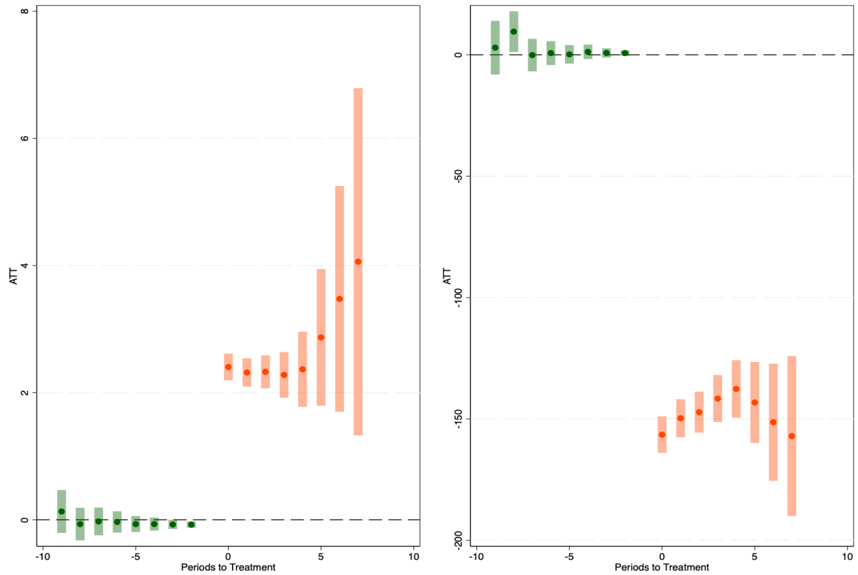
Note: The chart displays estimates of the average treatment effect of displacement on the unemployment risk (left) and the expected tenure (right) associated with the main occupations in which "treated" individuals are employed each year. Estimates are obtained using not-yet-treated units as a control 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: excluding outlier observations in the bottom and top 1% of the outcome variables' distributions



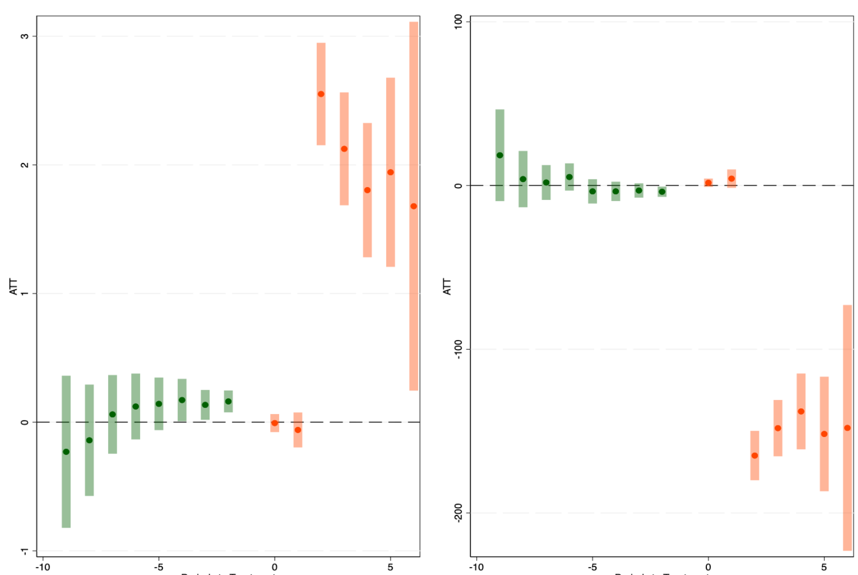
Note: The chart displays estimates of the average treatment effect of displacement on the unemployment risk (left) and the expected tenure (right) associated with the main occupations in which "treated" individuals are employed each year. Estimates are obtained using not-yet-treated units as a control 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) - 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 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) - 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 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).